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DISSERTATION

Reducing the Economic Burden of Work-Related Injuries

Christopher F. McLaren

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Christopher F. McLaren

This document was submitted as a dissertation in April 2014 in partial fulfillment of the requirements of the doctoral degree in public policy analysis at the Pardee RAND Graduate School. The faculty committee that supervised and approved the dissertation consisted of Seth A. Seabury (Chair), Robert T. Reville, and John Mendeloff.

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Table of Contents

List of Figures

List of Tables

Introduction

Despite workplace safety and health improvements in recent years, occupational injuries and illnesses continue to be a prominent public health concern. In 2012 there were approximately three million non-fatal workplace injuries and illnesses reported by private industry employers in the U.S., with almost one-third of them involving days away from work.[1] The costs associated with workplace injuries can be substantial for both injured workers and employers.[2] Workers suffer economic and noneconomic hardships that often persist for years. Employers must pay medical and indemnity benefits, lose worker productivity, and often bear the costs of replacing the lost worker. Estimates suggest that the costs of injuries amount to hundreds of billions of dollars per year.[3]

This dissertation consists of three distinct papers that address topics related to reducing the economic burden of work-related injuries. The primary goal is to provide policymakers, employers, and workers with information that may be used to improve occupational health and safety outcomes. Broadly, the research questions addressed are:

- How effective are employer return to work programs?
- Are chiropractic care and physical therapy cost-effective medical treatments?
- What is the impact of exposure to occupational hazards on disease rates and costs among the elderly?

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[1] Bureau of Labor Statistics, U.S. Department of Labor, *Injuries, Illnesses, and Fatalities,* retrieved January 25, 2014, from http://www.bls.gov/iif.

[2] “Injuries and illnesses” will be referred to simply as “injuries” for brevity unless otherwise specified.

[3] *See:* Leigh, J. Paul, Steven Markowitz, Marianne Fahs, and Philip Landrigan, 2000. *Costs of Occupational Injuries and Illnesses:* The University of Michigan Press. Leigh, J. Paul, 2011. “Economic Burden of Occupational Injury and Illness in the United States.” *The Milbank Quarterly,* Vol. 89, No. 4, pp. 728-772.

The first paper evaluates the effectiveness of return to work policies that are designed to reduce post-injury costs for both employers and injured workers. While there is some evidence that these policies are associated with lower duration of injury-related absences, there is little consensus as to whether the impact is large enough to justify the intervention costs. This paper examines the issue using a unique dataset that combines information from an employer-level survey about disability management and return to work practices with worker's compensation claims and five years of post-injury employment outcomes for roughly 15,000 injured workers in California from 1991-1995.

Various estimation techniques, including discrete-time logistic duration models, are used to assess the effectiveness of return to work programs in reducing the duration of injury-related absences. Estimates suggest that return to work programs significantly reduce the duration of injury-related absences. Having a program in place is associated with about a 3.6 week reduction in number of weeks away from work for the median injured worker. For workers with a permanent disability, programs reduce the number of weeks out of work by 12.6 weeks. The effects appear to be dominated by males, though unobserved variation in occupation by gender may be driving this result. The largest reduction in injury duration is associated with the use of modified equipment. Using back-of-the-envelope estimates of program costs, this paper finds that return to work programs are cost-effective for large, self-insured employers.

The second paper addresses the impact of California workers' compensation reforms that limit two controversial medical treatments: chiropractic care and physical therapy. This paper analyzes the Medical Expenditure Panel Survey (MEPS) and reviews the literature to assess treatment utilization rates and identify the proportion of injured workers who may be affected by the reform. Additionally, a systematic literature review is performed to evaluate the effectiveness of chiropractic and physical therapy treatments, particularly long-term treatments, with respect to health, cost-effectiveness, and return to work.

This paper finds that utilization rates in California decreased significantly after the 2004 reforms and rates fell more in line with national averages of about 10 to 14 visits for most conditions. Roughly 10 percent of individuals who seek chiropractic or physical therapy treatment go for more than 24 visits, which suggest that the treatment cap may be binding for some injured workers. Relatively few studies document a substantial benefit of chiropractic care or physical therapy on employment or health outcomes for injured workers relative to conventional physician treatment. Evidence focusing on return to work outcomes and cost-effectiveness is mixed and weak. Existing clinical evidence on the effectiveness and utilization of chiropractic and physical therapy treatment provides little support for the idea that the reforms should have a substantially negative impact on outcomes for injured workers.

The third paper estimates the impact of exposure to occupational hazards on disease rates and costs among the elderly. Occupational disease poses a number of challenges, primarily because there are often long latency periods after exposure until a disease manifests and many diseases have multiple potential causes, including lifestyle factors, which make it difficult to establish if a disease is work-related. Evidence suggests that workers' compensation misses the vast majority of work-related illnesses which results in significant cost-shifting and leads to sub-optimal safety precautions and inadequate compensation. Studies have investigated the impact of occupational exposures to hazards on disease however the full impact is largely unknown.

This paper uses individual-level data from the Health and Retirement Study (HRS) to evaluate the impact of various occupational hazard exposures on prevalence and incidence of six diseases while controlling for a number of potentially confounding socio-economic factors. It finds that disease prevalence in the first wave of the HRS is significantly higher for workers exposed to occupational hazards, and the effect is concentrated among workers with long-term exposures. Estimates of new incidence of disease finds that exposure is only significantly associated with lung disease and cancer. Population attributable risks (PARs) for lung disease and cancer fall in line with previous research at 11 and 10 percent, respectively. Back-of-the-envelope estimates of total costs, both direct and indirect, of disease attributable to workplace exposure to hazards are roughly $17.9 billion for lung disease, and $18.5 billion for cancer.

The rest of this dissertation is organized as follows. Chapter I assesses the effectiveness of return to work programs, chapter II evaluates chiropractic and physical therapy treatments, and chapter III estimates the impact of occupational exposure to hazards on disease prevalence, incidence, and costs.[4]

[4] Each chapter is treated as a stand-alone document – all references, tables, and figures for each paper are at the end of the related chapter.

I. How Effective are Employer Return to Work Programs?[5]

Abstract

This study examines the effectiveness of employer return to work programs in reducing the average duration of absences from work-related injuries. It finds that workers injured with an employer return to work program in place return to sustained employment approximately 1.38 times sooner after an injury. The biggest reductions in work-injury absence are experienced by men and by workers with a permanent disability. Modifying work equipment is associated with the greatest reductions in injury durations relative to other program components. Back-of-the-envelope calculations indicate that these programs are cost effective for large, self-insured employers.

Keywords: workers' compensation, injuries and employee return to work, workplace injuries and employer self-insurance

[5] An earlier version of this paper was published as: McLaren, Christopher F., Seth A. Seabury, and Robert T. Reville, 2010. *How Effective are Employer Return to Work Programs?* Santa Monica, Calif.: RAND Corporation, WR-745-CHSWC.

Introduction

A key metric for the impact of occupational injuries for both workers and employers is the duration of injury-related absences. Early return after an injury is associated with significant declines in long-term earnings losses for disabled workers, even conditional on injury severity, presumably by minimizing the extent of career disruption (Reville et al., 2005; Campolieti and Krashinsky, 2006). Extended injury duration also poses direct and indirect costs on employers: the direct cost of higher disability indemnity benefit payments, and indirectly through the value of lost productivity.[6] These costs associated with the duration of work absences make the promotion of early return to work for workers' compensation claimants an important priority for policymakers.

Many policy initiatives that are intended to improve return to work for injured or disabled workers operate through employers. For instance, some states offer subsidies to offset the costs to employers of hiring, retaining or accommodating disabled workers. These policies are adopted, however, with relatively little consensus among the scientific literature as to the effectiveness of these employer-based efforts. While there is some evidence that these policies are associated with lower duration of injury-related absences, there is little consensus as to whether the impact is large enough to justify the intervention costs.

This paper combines information from an employer-level survey about disability management and return to work practices—which we loosely term "return to work

[6] Nicholson et al. (2006) estimate the indirect cost of worker absenteeism to employers, encompassing the lost productivity of the worker, the loss of specific human capital, and any replacement or retraining costs. While the estimates vary by occupation, they find that a welder, for example, who is out of work for 2 weeks costs the employer about 133 percent of his or her daily wage, about $1,604 total.

programs"—with worker's compensation claims and five years of post-injury employment outcomes for roughly 15,000 injured workers in California from 1991-1995. We use various estimation techniques to assess the effectiveness of these programs in reducing the duration of injury-related absences. A key feature of our analysis is that some employers adopt a program during the period over which we observe workplace injuries, allowing us to use employer fixed effects to eliminate employer heterogeneity. Our data also allow us to examine the impact of these practices on long-term worker outcomes. To inform the policy debate surrounding return to work programs, we seek to answer four primary research questions:

- What is the impact of return to work policies on the duration of work-injury absence?
- What return to work program components are most effective in returning injured workers back to work?
- Is return to work program participation associated with better long term employment outcomes for injured workers?
- Are return to work programs cost-effective for employers?

Our estimates suggest that return to work programs significantly reduce the duration of injury-related absences. Having a program in place is associated with about a 3.6 week reduction in number of weeks away from work for the median injured worker. For workers with a permanent disability, programs reduce the number of weeks out of work by 12.6 weeks. The effects appear to be dominated by males, though this might be due to unobserved variation in occupation by gender. The largest reduction in injury duration is associated with the use of modified equipment. Using back-of-the-envelope estimates of program costs, we find that return to work programs are cost-effective for large, self-insured employers.

In the next section of this paper we provide some background on workers' compensation policy and return to work practices. In Section III, we outline our empirical framework and present our data and explanatory variables as well as results from the employer return to work survey, and in Section IV, we present the results of our analysis. Section V concludes with a discussion of the policy implications of our findings.

Background

Workers' Compensation and Self-Insurance

Workers' compensation laws dictate the compensation provided to workers who suffer an occupational injury or illness in the course of their employment. First appearing in the U.S. in the early 20th century, workers' compensation laws created a no-fault system making employers responsible for providing injured workers with full medical and partial indemnity benefits. Prior to workers' compensation, workers were only due compensation in the event of an occupational injury if they could demonstrate in the tort system that their employers were negligent. By offering lower benefits than were available in the tort system but without having to show fault, workers' compensation was a compromise policy providing (at least in theory) increased cost certainty for employers and greater security for workers (Fishback and Kantor, 1998).

The details of workers' compensation policies vary by state, including the generosity of benefits, the methods of insurance allowed and the extent of coverage. Employers are typically required to cover all medical expenses and pay two-thirds of pre-injury earnings, subject to a cap, for an extended period subsequent to the injury. Indemnity benefits are tax free, and the dollar amounts provided differ according to whether or not the injury resulted in a temporary or permanent disability. Temporary total disability (TTD) benefits are paid weekly, usually for as long as an injured worker remains out of work.[7] Thus, the employers' costs associated with a workers' compensation claim are directly associated with the length injury-related absences.

Workers' compensation policies fix employer obligations and regulate how they guarantee those obligations will be met. The majority of employers purchase insurance to cover their workers' compensation liabilities. That insurance may be purchased from the private market or in some states from a state fund, which may or may not compete with private insurers. However the obligations are met, participation in the system is mandatory in every state except for Texas, which allows employers to opt out. The vast majority of workers are covered, though in some states very small employers are not required to offer coverage.

The data we use in our analysis consists of injured workers at self-insured employers in California. California employers wishing to self-insure must be approved by the Department of Industrial Relations (DIR).[8] To be approved, an employer must meet minimum thresholds for revenue and net worth, and must demonstrate an ability to meet their expected liabilities from workers' compensation claims. These requirements for self-insurance and the implications of choosing to self-insure generate differences between self-insured and privately insured employers, which has implications for the generalizability of our findings. The most glaring difference is the size of self-insured

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[7] Temporary benefits also usually end for workers who do not return to work but have reached maximal medical improvement (MMI).
[8] See Reville et al. (2001) for more detail on the requirements for self-insurance in California.

firms; for example, in 2007 self-insured employers represented less than one percent of all employers in California but represented approximately 26 percent of all employment.[9]

Whether an employer is insured or self-insured has potential implications for the duration of work-injury absences. While privately insured employers pay a premium based on their expected workers' compensation liability, many employers are imperfectly experience rated (particularly small employers). Self-insured employers, on the other hand, bear the full cost of their workers' compensation claims and are, thus, perfectly experience rated. Therefore, self-insured employers may be more likely to adopt a return to work program than privately insured employers because they have stronger incentives to minimize the cost of a given claim.

Empirically, self-insurance appears to correlate with the duration of injury-related work absences. Reville et al. (2001) finds that time out of work in the first three months after an occupational injury is 15 percent lower at self-insured employers than at insured employers in California. Similarly, Kruger (1990) finds that workers at self-insured employers in Minnesota have lower duration of absences after an injury. Seabury et al. (2012) find that self-insurance is associated with significantly improved short-term and long-term employment of injured workers, but the effect is focused on injured workers at larger employers. Evidence is mixed, however, on the extent to which these differences are due to the incentives of self-insured firms or other factors (such as their size).[10] For example, a study by Butler and Park (2005) finds no effect of self-insurance once industry and occupation are held constant.

[9] Self-insured employer data is from the California Self Insurance Plans (SIP), and total employment data is from the California Employment Development Department (EDD).
[10] Most research has concludes workers injured at larger employers generally experience less time off work than workers injured at smaller employers (Galizzi and Boden, 1996; Cheadle et al., 1994).

Strategies for Improving Return to Work

Some specific return to work policies found to be effective in previous studies include light duty assignment, modified work and equipment, reduced hours, and ergonomic interventions (e.g. Baldwin et al., 1996; Butler et al., 1995). Other studies analyzing different types of return to work interventions find they are typically associated with lower injury durations and claim frequencies (Butler and Park, 2005; Hunt et al. 1993). More generally, Franche et al. (2005) and Krause et al. (1998) systematically review the return to work literature and report that different interventions reduce injury durations by a factor ranging from roughly 1.9 to 2.5, and 1 to 4 times, respectively. Tompa et al. (2008) review studies examining the economic implications of disability management interventions and find credible evidence that programs provide financial benefits. These reviews generally conclude, however, that the lack of evidence on causality and on program costs makes it difficult to assess the cost-effectiveness of these programs.

Differences in benefits, health care, workplace factors, and demographic characteristics also play important roles in successfully returning injured workers back to work (Krause et al. 2001, Baldwin et al, 1996). Past studies consistently find that the duration of injury-related absences are increasing in workers' compensation benefit levels (see e.g. Meyer et. al, 1995; Neuhauser and Raphael, 2004; Galizzi and Boden, 1996; Butler and Worral, 1985; Johnson and Ondrich, 1990). Other studies find that older workers are less likely to return to work and are out of work for longer periods after an injury than younger workers (Cheadle et al, 1994; MacKenzie et al, 1988; Tate, 1992), and workers with higher education, higher income and job seniority are more likely to return to work faster (MacKenzie et al, 1988; Tate, 1992). There are gender differences as well, as Boden and Galizzi (2003, 1999) and Johnson and Ondrich (1990) report that women are more likely to take longer to return to work and experience greater income losses compared to men.

Return to Work Practices Used by Employers in California

To examine the impact of return to work practices used by employers in California during our study period, we use the results of a survey conducted by RAND in 2000. RAND collected information from 40 large, private self-insured employers with either a telephone or written survey assessing the most common features of the employer's return to work program and disability management practices (if any).[11] The process for collecting the survey data is described in greater detail below, but here we summarize a few key results in order to describe the kinds of practices we expect are most widely used.

The survey asked employers to provide information about methods used to return injured employees to work, how often they are used, and the subjective importance of each method in relation to the overall effectiveness of the program (as of the time of the survey, 2000).[12] Table 1 reports the most common transitional work accommodation characteristics of the programs in our sample and the perceived level of importance as

[11] In 1980 only one employer in our sample had adopted a program but there is a positive trend in adoption rates from 1980 to 2000, including a significant rise after 1990, probably because of the implementation of the Americans with Disabilities Act (ADA). Just 9 employers (25 percent) report having adopted a program prior to 1990, while 21 (58 percent) adopt a program by 1996. Four employers did not report a year of program adoption and are dropped from the analysis.

[12] It is important to note that these surveys focus solely on methods used to return injured workers back to work after an injury, not to prevent injuries. As an empirical check, we estimate the impact of the programs at firms in our sample on injury claim rates which we describe in detail later in the paper. A number of studies evaluate programs that include injury-prevention measures (see e.g. LaTourette and Mendeloff, 2008; Hunt et al. (1993); Butler and Park (2005)).

reported by each employer. The four primary characteristics we report are modified work tasks, providing a modified workstation or modified equipment, reduced time and work schedule changes, and providing a different job in either the same or a different department.[13]

Modified work tasks are the most common among employers in our sample, with 82 percent of the employers reporting that they use this method frequently or quite often. Roughly half of the sample report providing a modified workstation or modified equipment frequently, or most of the time. Reduced time and work schedule changes are fairly common with 45 percent of the sample reporting use, and 32 percent of the employers report providing a different job in either the same or a different department as used frequently or quite often.

The rightmost column of Table 1 lists the perceived level of importance, as reported by employers, of each return to work method. Employers were given the option to choose whether they believed a method is extremely important, quite important, moderately important, of limited importance, or no importance at all. To quantify these answers, we use a scale from 1-5, with 5 being extremely important. Not surprisingly, the perceived level of importance of each method fall in line with the proportion of employers that report use of each characteristic as frequent or quite often. Also not surprisingly, relatively minor accommodations (such as modifying tasks) are used more frequently than more disruptive changes (such as relocating the employee to a different job).

[13] Modified work is any temporary change in work tasks or functions, modified workstations and modified equipment allow injured workers to perform work functions while recovering from an injury, and reduced time/work schedules and providing a different job are examples of actions employers may take to facilitate the return of an injured worker to the workplace.

Empirical Framework

The primary goal of our empirical work is to assess the effectiveness of return to work programs over a number of different evaluation criteria and to answer the research questions set forth in the introduction. In the following section, we describe the data, outline our empirical approach, and discuss estimation issues.

Data

We use workers' compensation data collected from the employers in our sample linked to administrative earnings data from the Base Wage file maintained by the California Employment Development Department (EDD). Every quarter, employers covered by Unemployment Insurance (UI) in California are required to report the quarterly earnings of every employee to the EDD. The EDD provided the quarterly earnings of all workers' compensation claimants at the employers in our sample for five years (20 quarters) after injury.

To collect the workers' compensation data, RAND contacted a sample of 150 private, self-insured employers (out of a total of 466) and 150 public self-insured employers (out of a total of 432) and requested data on all indemnity claims from 1991 through 1996. The sample of contacted employers was based on their number of claims; thus, the resulting data was representative of the sample of claims (as opposed to employers), and the sample was stratified by employer size to increase the probability of selection for small self-insured employers.[14] Data was requested on paid and incurred

[14] Several randomization validity tests were performed on the final data set and Table 2 reports the response rate for employers in the sample (see Reville et al. 2001). The biggest source of non-randomness in the response appears to be based on industry. In particular, public utilities are more likely to respond, and transportation firms are less likely to respond. Reville et al. (2001) tests whether controlling for the likelihood of response has any impact on estimated post-injury employment outcomes and finds that the

benefits, injury dates, and individual identifiers to facilitate linking to earnings data maintained by the EDD.

Reville, et al. (2001) describes the full data collection process. Ultimately RAND collected useable data on workers' compensation claims from 68 employers. The 68 employers used in the final dataset represent 15 percent of self-insured, private employers, and 30 percent of indemnity claims at self-insured employers over the period of interest. These 68 employers provided the sample for survey collecting information on return to work practices. Ultimately, 40 employers responded to the survey, of which 33 had sufficient non-missing information on workers' compensation, earnings and return to work practices to include in the sample.

The simplest measure of the duration of injury-related absence available to use is the number of weeks that an injured worker receives temporary total disability (TTD) benefits. It is possible, however, that a worker runs out of TTD benefits before they actually return to work. To better measure the time until actual return to work, we compute the number of weeks until a worker attains "sustained" return to work—defined as the number of weeks until we observe positive wages for an injured worker for at least two consecutive quarters after TTD benefits had been exhausted.[15] We only designate

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overall impact is minor. Another test compares pre-injury employee earnings and the probability of response and finds no clear relationship, although we still control for pre-injury earnings in our empirical models.

[15] Our wage data are quarterly, meaning that we did not observe the actual number of weeks until return to work. Let rtw_{2q} be the number of quarters after injury until we observe positive wages for at least two consecutive quarters, and let ttd_q be the number of quarters of TTD benefits received. If $ttd_q \geq rtw_{2q}$, then weeks to sustained return to work is set equal to weeks of TTD benefits received. We make this assumption because if a worker is still receiving TTD benefits, even if they have positive wages, then they have not fully returned to work. Or, they might return for one or more temporary spells, and receive UI wages during that spell, but then have to leave work and go back on TTD. On the other hand, if $ttd_q < rtw_{2q}$, we compute weeks to sustained return as the weeks from the initial date of injury to the midpoint of the first quarter in which they are observed back.

return to work as having two consecutive quarters of earnings to avoid cases with delayed wages or other benefits recorded that did not truly indicate a successful return to work.[16]

These metrics represent the relatively short-term outcomes of injuries on the employment of injured workers. Focusing exclusively on short-term outcomes may overstate the effect if workers in a program experience worse long term outcomes. This could happen, for example, if they return to work too soon after an injury and have worse subsequent health and lower productivity as a result. Alternatively, long term outcomes could be better if, for example, the program helps better coordinate health care with the injured workers' physicians. To assess these possibilities, we estimate the impact of return to work programs on the likelihood of being employed five years after an injury.[17]

Summary Statistics

Table 3 summarizes some characteristics of injured workers in our sample by return to work program status at the time of injury. Our sample includes 14,888 workers injured on the job corresponding to 17,312 workers' compensation claims between the years 1991-1995.[18] There are significant differences in the mean number of weeks to sustained return to work between workers based on program participation. The mean number of weeks for injured workers in a program is 24.1, compared to 42.1 for workers not in a program, a 43 percent difference. The median weeks to sustained return to work are 14 percent higher for injured workers not in a program.

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[16] Butler et al. (1995) note that return to work is a process, not an event in time, and there are many potential instances of work spells and absences.

[17] We also estimate the likelihood of a subsequent injury. However, data limitations decrease the reliability of these estimates. We discuss the results and our concerns in the results section.

[18] Though the original data collection effort collected information on claims through 1996, only injuries though 1995 are included in the sample because the wage data include up to 5 years of post-injury earnings.

The large differences in mean durations may, in part, be due to the skewed nature of our data. The majority of workers return to work within a month after an injury, but some workers take up to 200 weeks, or longer. When comparing the two groups, a larger proportion of workers not in a program are censored at 200 weeks.[19] The difference in long-term employment rates by program status is just 1.3 percentage points.[20]

The key worker characteristics in our data are weekly wage, PPD payments, gender, and age. Wages are higher for injured workers in a program and almost 40 percent of our sample collects any PPD payments. The proportion of workers who obtain PPD payments is higher for injured workers in a program (46 to 32 percent, respectively), which corresponds with higher average PPD payouts for that group as well. More than half (58 percent) of our overall sample is female, but an even larger proportion (63 percent) of workers not in a program are female.

Some employer characteristics of interest include indemnity and medical costs per injury, injury rates, industry, and size. Average indemnity and medical costs per case are higher for injured workers in a program, although injury rates are relatively similar across the groups. The majority of workers come from employers in the service and transportation industries with a much smaller proportion from manufacturing and trade. Table 3 also illustrates the large size of the employers in our sample; the majority of workers (52 percent) come from employers with 1,001-35,000 employees, and 47 percent

[19] Eberwein et al. (2002) note that median durations are likely to be a more robust descriptive statistic than mean durations since medians give less weight to outlying durations and rely much less heavily on high and out-of-sample durations or on extreme values for any potential unobserved heterogeneity. Therefore, while we estimate mean differences, our central conclusions focus on differences in estimated medians between the two groups.
[20] These return to work rates are similar to those of injured workers in Ontario studied by Butler et al (1995).

of the workers come from employers with more than 35,000 employees.[21] Taken together those findings suggest clear differences between employers with and without a program. Below we discuss how we deal with these differences in our empirical work.

Estimation Approach

Our first research question is to identify the impact of return to work programs on work-injury absence. However, there are a number of factors that could influence the duration of injury-related absences. Suppose workers have uninjured marginal productivity of π_t^* in time *t*, receive an offer wage of w_t and face reservation wage of w_r. Given a competitive labor market, workers accept employment if $w_t = \pi_t^* \geq w_r$, i.e., if offer wages exceed reservation wages. Injuries act as a negative shocks to marginal productivity, so $\pi_t^* \geq \pi_t$ after an injury. For a fixed reservation wage, the lower productivity makes it less likely the wage offer is high enough to induce work.

Further, suppose that injured workers experience a natural rate of recovery such that π_t increases over time, i.e., $\frac{\partial(\pi_t^* - \pi_t)}{\partial t} < 0$. The recovery rate is affected by factors such as the type of injury, the quality of the health care, demographic characteristics and by accommodations offered by the employer. Employer accommodations may include a bundle of activities such as those described before. Ultimately, however, the purpose of an accommodation is to bring marginal productivity for injured workers closer to its pre-injury level. Let a denote the level of investment in accommodations, where

[21] Note that the summary statistics are at the worker level. Thus, part of the over-representation of certain industries is due to the fact that these industries have the largest firms in our sample. Similarly, the firm size distribution is skewed more towards large firms than it would be if evaluated at the firm level.

$\pi_t(a)-\pi_t(0)\geq 0$. This implies that the impact of workplace accommodations on recovery is given by:

(1)
$$\frac{\partial^2(\pi_t^* - \pi_t)}{\partial t \partial a} < 0.$$

We estimate the magnitude of (1) by examining differences in instantaneous and cumulative probabilities of returning to work, as defined by the hazard rate and cumulative hazard rate, based on program participation. The hazard rate represents the instantaneous probability a worker returns to work in a time period $t+\Delta t$, given they were not working in the previous time period t and the cumulative hazard rate, $H(t)$, is the integral of the hazard rates over the given duration. For a given level of investment in return to work policies, denoted a as above, the instantaneous probability that an injured worker returns to work in time $t+\Delta t$ is:

(2)
$$h_{ijt}(\pi(t,a)) = \lim_{\Delta t \to 0} \Pr\left(\frac{\pi(t+\Delta t, a) \geq w_r \mid \pi(t,a) < w_r}{\Delta t}\right)$$

Examining the cumulative and instantaneous hazard rates by program participation provides evidence as to the effectiveness of programs over time. Figure 1 displays some of the potential outcome scenarios. However, while visually comparing differences in h_{ijt} and $H(t)$ by program status and testing for equality of the hazard functions is informative, it fails to control for worker and employer heterogeneity.[22] We estimate several duration models that control for observable heterogeneity in our sample, and provide point estimates that indicate the effectiveness of return to work programs.

[22] We test the equality of the hazard functions by program participation by computing the log-rank test statistic.

These estimates also allow us to evaluate the net-benefit of programs, which we explain in more detail later in the paper.

Since our outcome variables are measured in weeks, and to allow for flexibility in the specification of the hazard function, we estimate discrete time logistic hazard models of the form:

(3) $$h_{ijt} = (1 + \exp(-weeks_{ij}(t)))^{-1}$$

where

(4) $$weeks_{ij}(t) = \beta_0 \ln(t) + \beta_1 a_{jt} + \beta_2 x'_{ijt} + \beta_3 z'_{jt} + \delta_t + \lambda_j .$$

The baseline hazard is the log of weeks to return to work, which is represented as $\ln(t)$ in equation (4).[23] Using the earlier notation, a_{jt} represents investments in accommodations, which we measure using an indicator that equals one if employer j has a program in place in time t and zero otherwise. The coefficient β_1 represents the change in hazard ratios for workers in the "treatment" group—those injured at an employer with a program in place—compared to the controls. By including a program participation indicator, we assume that the effect on the probability of returning to work, and the subsequent durations, is proportional over time. This is a stringent assumption, however based on specification checks, and the characteristics of our data, it appears to be reasonable.

We include year of injury fixed effects, δ_t, and control for worker heterogeneity by including, x'_{ijt}, a vector of characteristics such as the log of pre-injury weekly wage,

[23] We estimate a number of alternative specifications including a flexible piece-wise constant discrete time duration model which simply assumes that the baseline hazard is constant over the specified week dummy variables. Additional information regarding our alternative specifications is in the Appendix.

age, gender and PPD payments (which we use to proxy for injury severity).[24] We control for observable employer heterogeneity by including, z'_{jt}, a vector of characteristics such as employer size, industry, annual number of injuries per 100 workers, average medical cost per case, and average indemnity cost per case.

While our estimate of β_1 is consistent, there is a correlation of observations within each firm cluster. In this setting, conventional OLS standard errors could exhibit a downward bias causing significant over-rejection of the null hypothesis that our point estimates are significantly different from zero (see e.g. Moulton (1986, 1990); Wooldridge (2002)). To deal with this issue, we adjust all standard errors to allow for clustering within employers (Liang and Zeger (1986)).

To address research question number two and evaluate the effectiveness of individual return to work program components, we use a similar approach as described above. The functional form of our specification is the same as in equation (3), however instead of only including an indicator for program participation, we enter each component separately in our specification. Specifically, we estimate:

$$weeks_{ij}(t) = \beta_0 \ln(t) + \sum_{i=1}^{4} \beta_i a_{ijt} + \beta_5 x'_{ijt} + \beta_6 z'_{jt} + \delta_t + \lambda_j . \tag{5}$$

All of the variables in equation (5) are the same as described in equation (4), except for the program components which are defined as: a_i = {modified work, modified equipment, different job within the same firm, and scheduling accommodations}. Each

[24] Key to our analysis is the fact that PPD benefits in California are determined according to a physician's evaluation of injury severity, and do not depend on employment status. Thus, they are not endogenously influenced by return to work, and we can use them to proxy for injury severity in cases with permanent disability. We recode PPD payments into four categorical variables, split by quartile, and in comparison to workers who do not collect any PPD payments.

program component indicator is equal to one when a worker is injured at firm j in time t with a program in place that includes the specific program characteristic.

Evaluating the association between injured worker outcomes and program participation is valuable. However, even if program participation is associated with better injured-worker outcomes in the short-run, it is possible that workers may have worse outcomes in the long-run if, for instance, they return to work too soon after an injury. To assess this possibility, and to address research question three, we evaluate the impact of program participation on employment outcomes five years after injury. Specifically, we estimate a series of ordinary least squares linear probability models. The dependent variable is an indicator equal to one if an injured worker is employed five years after injury and zero otherwise. We include a number of additional control variables that are described in equation (4).

Identification

Given the observed heterogeneity in workers and employers across program status, we are naturally concerned about the possibility of unobserved heterogeneity that could confound our estimates. In particular, we assume that any unobserved heterogeneity in our sample, including starting and censoring times, is independent of the observed covariates. Two areas of concern are injury severity and employer behavior.

We assume a random, unbiased distribution of injury types and severity across workers and by program status. We include PPD payments as a proxy for injury severity for workers with a permanent disability but there is no control for injury severity for workers with a temporary disability. However, since we are only concerned with determining the effect of programs on the mean and median durations of injured workers, explicitly modeling any unobserved heterogeneity is not compelling because it does not change the mean or median effects, only the error distribution (Wooldridge, 2002).

Additionally, we are concerned that employer behavior differs in other ways that affects return to work even in the absence of a program. We control for observable firm characteristics as mentioned above, but we do not have time-varying information on these characteristics. We address this issue by testing separate specifications that drop these characteristics and include employer fixed effects, λ_j.

These controls will work well and our model will be identified as long as any unobserved heterogeneity correlated with program status is time invariant. If program adoption is spurred by changes in unobservable characteristics, however, then clearly we will not be identified. The factors that drive program adoption are the marginal product of adoption, the risk of injury and the relative cost of a program compared to the cost of injury duration. The marginal productivity of adoption and the risk of injury are likely driven by factors such as employer size and production technology, while the marginal cost of injury duration is driven by replacement costs and firm-specific human capital. None of these factors are likely to change significantly over a small period of time, supporting the use of employer fixed effects.

The relative cost of accommodations versus injury duration could be affected by policy changes, such as workers' compensation reform. The only significant policy changes to California's workers' compensation system over our period of interest, however, were reforms to the insurance system. Assembly bill 110 in California was passed in 1993 and took effect January 1, 1995, and it opened the California workers' compensation market to competitive pricing. Because the employers in our sample were self-insured during this time period it seems unlikely that this reform would have had a significant impact on return to work.

The reform most likely to have impacted employer incentives to adopt return to work programs is the Americans with Disabilities Act (ADA). Since 1992, the ADA requires employers with more than 15 employees to make “reasonable” accommodations for disabled workers. The threat of exposure to civil suits under the ADA if they are unwilling to accommodate disabled workers may have made employers in our sample more willing to undergo the cost of a return to work program. In our data there is a sharp increase in adoption rates after ADA passage in 1990.[25] If that is so, then adoption is likely to be uncorrelated with underlying trends in return to work. That is not to say that adoption is random, as there could be differences in the employers who were more likely to adopt after ADA enactment.

Ultimately, our discussion of the impact of the ADA on program adoption by employers is conjecture. We lack sufficient data to formally test whether it was a decisive factor for employers. We did, however, test whether pre-adoption trends in average return to work rates of employers is correlated with adoption. Ultimately, we find almost no evidence to suggest that average return to work rates in the pre-adoption period has any effect on the likelihood of adoption. Thus, we believe that any underlying impact of selection bias on our findings of program effectiveness is likely small.[26]

[25] The survey asks firms whether the ADA was a contributing factor in the decision to adopt a return to work program after 1990, and 7 of 17 employers (41 percent) said it was at least “moderately” important.
[26] Additional information regarding these regression results are in the Appendix.

Results

Descriptive Findings

Figure 2 illustrates the differences in return to work rates for injured workers by program participation (using weeks until sustained return to work as the duration measure). The left panel shows the cumulative hazard rates for injured workers by program participation for up to 52 weeks after injury. The cumulative rate, which indicates the fraction of workers returning by that date, displays a noticeable difference after five weeks, subsequent to which injured workers in a program have a higher cumulative return to work rate at each week.[27] Further visual analysis shows that once the rates diverge after five weeks, the gap widens and persists up to 52 weeks after injury.[28]

The right panel of Figure 2 illustrates the instantaneous hazard rate—the proportion of workers that return to work in time t, given that they haven't returned yet. In the first week after injury, roughly 21 percent of injured workers in a program return to work compared to 20 percent of injured workers not in a program. There is a significant decline in both curves after the first week, but the decline is sharper for injured workers not in a program. After about four weeks, there is a persistent difference such that injured workers in a program are about two percentage points more likely to return in a given week. Additionally, the shape of the instantaneous hazard rate suggests that time

[27] To test the statistical significance of the differences in the cumulative hazard rates for Figures 1-4, we perform a log-rank test of equality of the corresponding survivor functions, with the null hypothesis that the functions are equal. We find that the differences are significant in all cases.
[28] We show each cumulative and instantaneous hazard rate graph up to 52 weeks after injury for space considerations but the trends in rates continue to 200 weeks after injury.

out of work exhibits negative duration dependence, with the conditional probability of returning to work falling as time from injury increases.

Figure 3 illustrates the cumulative and instantaneous hazard rates for men only. Graphically, the effect of program status is stronger for men. Both the cumulative and instantaneous hazard rates diverge after about five weeks, such that injured workers in a program are significantly more likely to return to work. Furthermore, the instantaneous hazard displays a consistent two to three percentage point higher probability of returning to work for men in a program. Figure 4 compares the cumulative and instantaneous hazard rates by program status for women. The effect of having a program in place is considerably weaker for women, with little observed difference in return to work rates based on program status.

Figure 5 compares hazard rates for workers in our sample who receive PPD benefits and shows that, while overall return to work rates are lower, there is still a pronounced difference by program status. The cumulative hazard is more than 20 percentage points higher for injured workers in a program one-year after injury. The instantaneous hazard rate is significantly higher for workers in a program even for the first several weeks after an injury. This suggests that the benefits of a program to the most severely disabled workers can have immediate effect. In fact, the difference appears greatest for the first 15-20 weeks in this sample; still, a clear difference by program status persists throughout the 52 week period.

Duration Model Findings

Table 4 reports the estimated results from our discrete-time hazard models. Each column in the table represents a separate regression specification, varying by the sample used and the presence of employer fixed effects. All standard errors are adjusted to allow for clustering within employers.

The estimated hazard ratios on the treatment indicator suggest that, even controlling for individual and employer heterogeneity, the presence of a program is associated with a significant reduction in the duration of work- injury absences. Using weeks until sustained return to work as the dependent variable, the hazard ratios for the entire sample are 1.36 and 1.38 with and without fixed effects, respectively, and both estimates are significant at the one percent level. Consistent with the descriptive analysis, the program effect is large and statistically significant when we restrict the sample to men and to workers with permanent disability.

There are several possible explanations as to why the estimated effect is so much larger for men. We do not have any controls for occupation, so it could be that men work in more physically demanding jobs that require greater accommodations in order to continue working after an injury. Alternatively, it could be that men work in riskier jobs and so are targeted more heavily when an employer adopts a program. We lack sufficient data to try and uncover the precise mechanism that makes the programs so much more effective for the men in our sample. We also note that the estimated hazard ratios follow a similar pattern when we use the weeks receiving TTD benefits as the dependent variable.

We estimate the median and mean differences in the duration of work-injury absences associated with program participation, and report the findings in Table 5.[29]

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[29] To calculate mean duration, we estimate the weekly hazard rate, $h(t) = (1 + \exp(-weeks(t))^{-1}$ as described before, by program status. Next, we use our hazard rate estimates to calculate the survivor

Column one reports that the median number of weeks until return to sustained work is 9.0. The mean number of weeks is 41.1, reflecting the skewed nature of injury absences. Our estimates suggest that having a program in place reduces the median number of weeks that a worker is absent by 3.8, a difference of 42 percent. Looking at the mean difference, we see that workers return 15.7 weeks sooner on average, a 38 percent drop. These estimates are similar with or without employer fixed effects.

The estimated program effects are skewed somewhat by large differences in injury severity among the workers in our sample. Workers with permanent disability represent 40 percent of our sample, and the table reports that the median injury duration for a worker with permanent disability is 39.7 weeks (the mean is 69.5 weeks). The impact of the program reduces the median duration for those with a permanent disability by 18.8 weeks, or 47 percent. The effect is somewhat smaller if we include fixed effects and look at the mean difference, but this still represents a drop of 27 percent. This suggests that much of the program effect is driven by the large reduction in injury duration for the most severely injured workers. As before, the estimated effects are similar using the number of weeks on TTD as the dependent variable.

Table 6 reports the results from our discrete-time hazard models with each program element evaluated separately. We include four primary elements in our models:

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function, $S(t) = \exp\{\sum_{t=1}^{200} \ln(1 - h(t))\}$ for each week. The mean duration is defined by $\sum_{t=1}^{200} S(t)$. Median duration is defined as the point where the survivor function $S(t) = 0.5$. In each case, the survivor function is never exactly equal to 0.5, so we impute the median using this formula provided by Eberwein, Ham, and LaLonde (2002) as: $\frac{S(t+1) - 0.5}{S(t+1) - S(t)} * (t) + \frac{0.5 - S(t)}{S(t+1) - S(t)} * (t+1)$. In this case, if the median duration of time to return to weeks is between 6 and 7 weeks, then $(t) = 6, (t+1) = 7, S(t)$ is the value of the survivor function when $(t) = 6,$ and $S(t+1)$ is the value of the survivor function when $(t) = 7.$

modified work tasks, providing a modified workstation or modified equipment, providing a different job in the same or a different department, and reduced time and work schedule changes. Each column in the table represents a separate regression specification, varying by the sample used and the presence of employer fixed effects. We only report the results using weeks until sustained return to work as the dependent variable because it is our preferred outcome measure. All standard errors are adjusted to allow for clustering within employers.

The estimated hazard ratios for each program element vary in size and significance but they follow a similar patter to our overall program effect results shown in Table 4. The estimates are mostly positive and significant and the impact is strongest for men and permanently disabled workers. However, the magnitudes of the estimates vary depending on whether employer fixed effects are included or not. This imprecision is likely the result of insufficient variation when we break down each program component to evaluate separately.

The estimated hazard ratios of the impact of providing modified work tasks for the entire sample are 1.37 and 1.27 with and without fixed effects, respectively, and both estimates are significant at the one percent level. Providing modified work tasks is by far the most common program characteristic, therefore it is not surprising that these estimates fall in a similar range to our overall program effect estimates. While the estimated hazard ratios are largest for men and permanently disabled workers, the estimates are also significant and positive for women. Modifying work stations and/or equipment is the second most common program characteristic and found in 50 percent of firms with a program in place. For all workers, the estimated hazard ratios are 1.16 and

1.50 with and without fixed effects, respectively, but only significant without fixed effects. However, the ratios for men and permanently disabled workers are large and significant.

Scheduling accommodations are the third most common program element, and present in 45 percent of the firms with programs. The estimated hazard ratios for scheduling accommodations are mostly positive, but largely insignificant. Finally, about one-third of firms with a program offer the opportunity for an injured worker to work in a different job within the firm. Surprisingly, the estimated hazard ratios are negative suggesting that finding new jobs for injured workers is associated with longer disability durations relative to workers injured at firms that do not provide that return to work opportunity. One possible explanation for this is that it takes time to find a suitable new job, which could delay return to work.

Table 7 reports the estimated mean and median durations of weeks to sustained return to work by program element. The mean and median weeks until sustained return to work for injured workers not participating in a program are 41.6 and 9.1, respectively, with fixed effects. These estimated mean and medians are roughly the same compared to our overall program impact estimates. Our estimates suggest that workers injured at firms providing modified work tasks return to work between 29 and 37 percent faster, on average, than workers injured at firms that do not provide modified work tasks. The estimated median reduction in time to return to work associated with modified work is between 3 and 4 weeks, or a reduction of roughly 34 to 40 percent.

The largest reduction in injury duration is associated with the use of modified equipment. We estimate that workers injured at a firm providing modified work equipment return to work about 50 to 65 percent faster (in both mean and median durations) than workers who do not have the same opportunities.

Long-Term Employment Outcomes

We estimate a series of linear probability models to identify the impact of program participation on three and five year employment outcomes. Table 8 reports the impact of programs on long-term employment. We find that the probability of working five years after injury is between two and four percentage points higher for workers in a program. However, these estimates are not significant at standard confidence levels. The estimates for our other populations fall in a similar range compared to the entire sample, ranging from roughly a two percentage point to a 5.5 percentage point higher probability of being employed. The probability of working three years after injury is generally positive and of roughly the same magnitude as the probability of working five years after injury. The only significant estimate is when we restrict the sample to injured men and we use employer fixed effects. These estimates suggest that return to work programs are associated with an immediate impact on employment for injured workers, and potentially have long-term benefit on health. However, there appears to be relatively little impact on the long-term employment of injured workers.[30]

[30] Additionally, we estimate the probability of re-injury by program participation. We find that program status is associated with approximately a 15 percentage point lower probability of subsequent injuries that result in a new workers' compensation claim. This effect is slightly more pronounced for men who have about a 17 percentage point lower probability of re-injury. Workers with a permanent disability have roughly a 10 percentage point lower probability of subsequent injury. However, the reliability of these estimates is questionable because of data limitations. For instance, we are only able to identify whether a worker filed for a new workers' compensation claim. It is not clear whether the claim was for a new injury or if it is a separate claim for the original injury. Therefore, while these results suggest that programs are effective in preventing another claim, we are hesitant in making conclusive statements.

The Net Benefit of Programs

Our estimates indicate that the employer return to work programs in our sample reduce mean and median durations of injury absences. The magnitudes are significant, but sometimes the accommodations required can be quite costly. From the perspective of promoting the use of return to work programs as a policy initiative, we are interested in whether the benefits from improved return to work outweigh the costs to implement and maintain the programs. We conduct a back-of-the envelope benefit-cost analysis to address research question four and assess the net benefit of program adoption.

It is difficult to define the average cost of a return to work program because there can be fixed costs (e.g., the cost of hiring a disability case manager, or building a wheelchair accessible ramp in the entryway), variable costs (e.g., installing ergonomic modifications for an injured worker) or indirect costs (e.g., if the accommodations only partly overcome the injured workers disability, then the opportunity cost of lost productivity from simply rehiring a new worker is a program cost). Some limited information on program costs were recorded in the RAND survey. In particular, employers were asked to indicate the annual cost of their programs in the survey year (2000). Only 12 employers reported an annual cost, ranging from $40,000 to $6,000,000. Dividing the total reported annual program cost by the number of injured workers at the same employer indicates that the average program cost per injured worker in our sample is $1,174, but the range varies from $500 to $3,000. We feel that this number probably underestimates the total costs, because indirect costs are unlikely to be included, and it is unclear whether or not certain types of fixed costs are included.

For our estimate of program benefits we use the dollar savings on TTD payments from shorter injury durations. The weekly benefit level is equal to two-thirds of the employer's average weekly wage, which averages $438 per week in our sample. We evaluate benefits ranging from the 25th percentile at $347 in our sample up to the maximum weekly benefit of $757, to consider how the benefit of a program compares to changes in the cost. While these dollar amounts represent the direct benefits to employers, they almost certainly understate the true benefits of a program. In particular, these benefits do not account for reductions in replacement costs and higher productivity levels from experienced workers.[31] In addition, we ignore the benefits to injured workers of getting back to work sooner and reducing the adverse economic impacts of an injury. While these indirect benefits and benefits to workers are instructive for evaluating the programs from a social perspective, the direct benefits and costs likely have the strongest impact on employer behavior.

Table 9 reports the number of weeks of injury duration a program must reduce in order for the program to break-even. For example, in the low-benefit, low-cost scenario, the break-even estimate is equal to 1.4 weeks and any additional reductions in average durations generate a net benefit for the employer. Comparing our treatment effect estimates from Table 5 of between 3 and 3.8 weeks, with the break-even numbers in Table 9, the programs generate net benefits for all but the most expensive programs when wages (and thus weekly benefits) are high. With average wages, the programs are beneficial when the program cost per injured worker is below $1,500, and with low wages, the programs are beneficial when the program cost per injured worker is below

[31] Nicholson et al. (2006) estimate that a two-week absence costs employers 133 percent of the weekly wage in indirect costs.

$1,000.[32] Compared to our sample average program cost per injured worker of $1,174, virtually all of the treatment effect and wage scenarios deem the programs to be beneficial to employers. Simply examining the net benefits using our sample average weekly TTD benefit of $438 and program cost per injured worker of $1,174 yields net benefits per injured worker of between $140 and $490 based on our range of treatment effect estimates, which corresponds to a range of benefit-cost ratios between 1.12 and 1.42.[33]

Conclusion

A common theme in workers' compensation policy discussions is how to improve the return to work of injured workers. In this paper we examine the effectiveness of employer-based return to work programs adopted by a sample of large, private, self-insured employers in California. We find that the programs led to significant reductions in the duration of work-injury absences. Having a return to work program in place at the time of injury is associated with a 3-4 week reduction in the median duration of injury-related absences. The largest reduction in injury duration is associated with the use of

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[32] Note that we use the median effects rather than the mean, because we feel that the median effects better represent the gains the employer observes in the highest number of cases. If we focus on the mean differences, the programs would be cost-effective in this example in all cases.

[33] Assessing the program impact per injured worker, instead of the impact per worker, may be misleading if programs have a significant impact on claim rates. For instance, if programs reduce claim rates as Butler and Park (2005) and Hunt et al (1993) find, then our net-benefit estimates would understate the overall program impact on costs. On the other hand, if programs increase claim rates, perhaps by encouraging the reporting of relatively minor injuries, then our estimates may overstate the program impacts on costs.

To investigate this concern, we identify the program impact on claim rates by collapsing our data to the employer-year level and estimating a series of OLS linear probability models where the dependent variable is yearly claim rate (defined as the number of claims divided by the number of workers). We find that claim rates are 1.5 to 2 percentage points higher after program adoption in our sample, with and without employer fixed effects, respectively; however none of the estimates are significant. This result suggests that the primary cost saving mechanism with which the programs in our sample operate through is a loss reduction (duration) effect as opposed to a loss prevention (claim rate) effect or some combination thereof.

modified equipment. We also find no significant differences in long-term employment outcomes between injured workers by program participation which suggests that the short-term program benefits are not nullified by long-term adverse outcomes. Together these findings suggest that the use of employer programs that promote return to work can have sustained, positive effects on worker outcomes. Our estimates also suggest the programs are cost-effective to employers, with our conservative estimates of the benefit-to-cost ratio ranging from 1.12 to 1.42.

These findings have important implications for the role of using workers' compensation policy to promote workplace accommodations by employers. If the gains to workers are imperfectly passed on to employers—say, if wage rigidities prevent compensating wage differentials from fully adjusting to reflect the improved outcomes for injured workers—then employer adoption incentives might be sub-optimal.[34] This argues in favor of promoting the use of these kinds of programs more generally, say with subsidies to accommodations or with insurance premium discounts. Our results offer some simple guides as to which employers would most need such subsidies. In particular, employers will be less willing to adopt policies for low wage workers, even though these workers are harder hit by workplace injuries (Reville et al., 2001). Similarly, employers with workforces with less ability to transfer skills and human capital across different tasks or jobs—that is, employers with less ability to modify the

[34] There are many reasons why compensating wage differentials may not fully adjust, meaning that the full gains to workers will not be recognized by employers when deciding whether or not to adopt. Workers may be uncertain about the economic impact of disability, and probably lack sufficient information to estimate the gains associated with a return to work program. Workers could also have time-inconsistent preferences, and may not themselves fully value the gains *ex ante*. The empirical evidence offers little guidance, as economists have traditionally found it difficult to estimate compensating wage differentials for job risks, particularly non-fatal job risks (c.f., Viscusi, 1993).

required activities of injured workers—will find return to work programs less profitable. This almost certainly applies to smaller employers.

The issue of small employers highlights some of the limitations of our study, which affect the generalizability of our findings. Ultimately, our findings suggest that return to work programs are highly effective when adopted at large, self-insured employers. It is by no means obvious that programs would be as effective if adopted by a different set of employers. Small employers, in particular, would likely find it difficult to offer the kinds of modifications that are prevalent in the return to work programs we study. The employers we study are all extremely large, at least when compared to the average or median employer, so we do not have a means with our data to identify any kind of threshold below which the programs are ineffective. Evaluating the impact of return to work programs, as well as injury and fatality prevention, for small employers is especially important because over 55 percent of Americans are employed in businesses with fewer than 100 workers and evidence suggests the smallest business establishments have high fatality rates (Mendeloff et al, 2006). Future work should study whether and how return to work initiatives provide a cost-effective means of improving employment outcomes for disabled workers at small employers.

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Appendix

Alternate Empirical Specifications

To check the robustness of our results, we estimate a number of additional discrete time hazard model specifications with different baseline hazard assumptions. Specifically, we estimate a discrete-time flexible piece-wise constant baseline hazard model, a continuous hazard model with a Weibull hazard distribution, and a Cox proportional hazard model (CPH).

One of the benefits of estimating a discrete time hazard model with a flexible piecewise-constant proportional hazard is that the hazard is allowed to be different (albeit constant) over each time interval (Wooldridge, 2002). We try a number of specifications, including weekly dummy variables, and by varying the groups of the piece-wise components of the baseline hazard. In each specification the estimate of interest does not vary significantly so we report the most parsimonious specification with quarterly time dummy variables.

Although our duration data is measured discretely in weeks, we also estimate a continuous time model assuming that time is Weibull distributed conditional on the socioeconomic characteristics of the worker, firm characteristics, and program status. The hazard rate is parameterized as $h(t) = \alpha t^{(\alpha-1)} \exp(x'\beta)$ and the duration of time an injured worker is out of work directly affects the hazard rate.[35] The greatest benefit of

[35] For instance, when $\alpha = 1$, there is no duration dependence and the length of time away from work does not have an effect on the hazard rate so we are simply estimating an exponentially distributed hazard function. However, when $0 \leq \alpha < 1$, there is negative duration dependence which means that the longer a worker is out of work, the lower the instantaneous conditional probability they will return to work in a

estimating a Weibull distributed hazard model is the ease with which we may estimate mean and median durations based on program status. However, the cost is that we must make a strong assumption that time is Weibull distributed. Based on graphical representations of our data, the Weibull distribution does appear to fit reasonably well.

The CPH model classifies the hazard rate as $h(t) = h_o(t)\exp(x'\beta)$, where $h_o(t)$ is the baseline hazard function, and we adjust for heterogeneity by including $\exp(x'\beta)$. In each case, $x'\beta$ includes all control variables from equation (4). The benefit of the CPH estimation procedure is that the baseline hazard is estimated non-parametrically, so we do not have to assume any distribution. However, we do have to assume that the difference in hazard rates by program status is proportional and constant over time. Furthermore, the greatest drawback of this empirical specification is that we are unable to quantify the impact of participating in a program on expected durations because to do this we need to parameterize the baseline hazard, $h_o(t)$. Therefore, we can only estimate the relative impact of participating in a program on duration of time to return to work using this specification (Butler and Worrall, 1985).

Selection Regressions

Because of the potential bias we face due to the nonrandom assignment of programs discussed earlier, we estimate a series of selection regressions that predicts the probability that a firm adopts a return to work program in time t, based on previous average levels of both of our duration measures and other firm level characteristics. To estimate these equations, we collapse the data to the firm, year level for the years 1991-

given time period. When $\alpha > 1$, there is positive duration dependence, which means the longer a worker is out of work, the higher the instantaneous conditional probability they return.

1995. Since we are estimating the probability of program adoption based on the previous year average measure of return to work, we drop 1991 because we only have injury duration information dating back to 1991. Therefore, we analyze year of program adoption from 1992 to 1996. We estimate two equations, each with and without firm fixed effects, which are shown below:

(6) $$\Pr(\mathit{program})_{jt} = \mathit{weeks}_{jt-1}\phi + \delta_t + \bar{x}'_j\beta + \bar{z}'_j\gamma + \lambda_j + \varepsilon_{jt}$$

(7) $$\Pr(\mathit{program})_{jt} = \mathit{program}_{jt-2}\alpha + \mathit{weeks}_{jt-1}\phi + \delta_t + \bar{x}'_j\beta + \bar{z}'_j\gamma + \lambda_j + \varepsilon_{jt}$$

In each of these models, *weeks* is equal to both TTD weeks and weeks to sustained return to work. Including a lagged program adoption variable causes auto correlation with the error term and fixed effects, so we include a twice lagged program adoption variable, firm and average individual characteristics, and run the models with and without time invariant firm fixed effects. Significant estimates on the lagged return to work duration measures would indicate possible selection biases in the types of firms adopting programs.

Alternate Specification Results

Table 10 displays the results of our alternate specification estimations. Columns 1 and 2 report the results for all workers in our sample. Columns 3 and 4, and 5 and 6, report the results when we limit the sample to men and women only, respectively. Finally, columns 7 and 8 report our results for workers with collected PPD benefits. In this table, for brevity, we simply report the program effect estimate for each alternate specification.

The first estimates we report are from the discrete time logistic hazard model with a piece-wise constant baseline hazard. These results are very similar to our main discrete time hazard model results that include the log of time to estimate the baseline hazard. This is not surprising given the similarities of our raw data with the Weibull distribution. In the discrete time hazard model the estimates for all workers and men are significant at the one percent level, however there is no significant effect on women.

The final two alternate specifications we list are the continuous time Weibull hazard model and the CPH model. In both specifications, the results fall in line with our other estimations, with a range of differences in hazard ratios by program status ranging from 1.36 to 1.46 for the entire sample, 1.58 to 1.77 for men, and no statistically significant effect on women.

Selection Regression Results

Table 11 displays the results from our selection regressions. Columns 1 and 2 include regressions with a lag of both of our duration measures, with and without firm fixed effects and other controls, and columns 3 and 4 include regressions with a lag of our duration measures and a twice-lagged program indicator variable. The first row includes estimates when we use TTD weeks, and the second row includes estimates when we use weeks to sustained return to work.

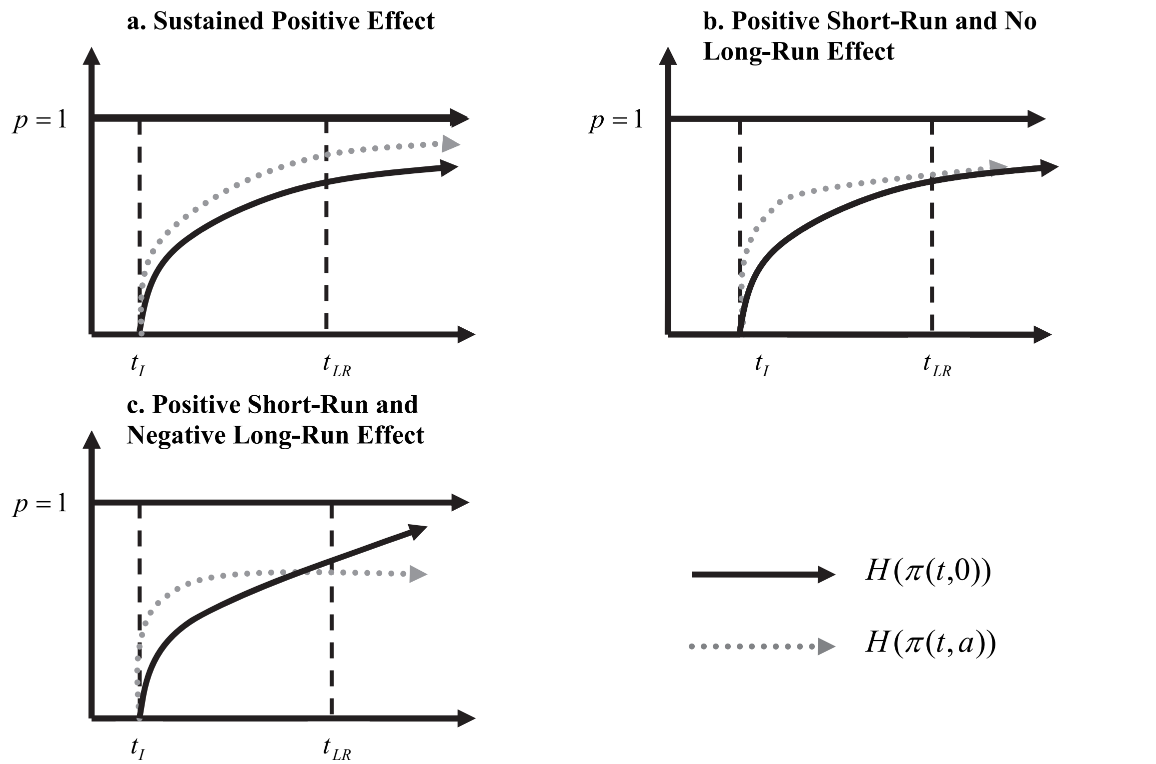
None of the estimates in the table are statistically significant however there are differences between the duration measures we analyze, and depending on whether we include firm fixed effects. In the first row, we report the estimates when we include TTD weeks and all the estimates are negative, and when we include firm fixed effects the estimates are between 3 and 7 times greater in absolute magnitude. Negative estimates predict that any potential bias may be for firms that are more safety-conscious and have lower workers' compensation liabilities. The second row of the table analyzes weeks to sustained return to work and paints a slightly different picture from the first row. In this row, all of the estimates are insignificant but unlike the results from the first row, the point estimates are positive which suggests that firms with higher workers compensation liabilities may be more likely to adopt a program. These estimates are once again a degree of order smaller than our main results.

Although none of these selection regression estimates are significant, it is entirely possibly that we do not have enough variation to identify a significant relationship after collapsing our data to the firm/year level. This suggests that we may still have a selection bias problem which we are not adequately dealing with in our primary specifications, however even if we did find significance in the selection regression estimates, the magnitude in absolute terms of the largest coefficients are of a degree of order smaller than our main results. For instance, our point estimate of -0.0029 indicates than an increase of one in the average firm level TTD weeks in time t decreases the probability the firm will adopt a program in time $t+1$ by roughly 0.3 percentage points.

These selection regression results provide us with a clearer understanding of the possibility of selection bias affecting our main results. Overall, the coefficients are small and insignificant, and while it is possible that the significance is lacking because of the small sample size, it appears that given the magnitude of the coefficients, even if the estimates were significant, they are not driving our main results.

Figures

Figure 1. Theoretical Cumulative Hazard Rates by Program Participation



Notes: t_I represents the time of injury, t_{LR} represents the long-term, $H(\pi(t,0))$, represents a theoretical cumulative hazard rate for workers who are injured and do not participate in any type of return to work program, and $H(\pi(t,a))$ represents a theoretical cumulative hazard rate for workers who are injured and do participate in a program.

Figure 2. Cumulative and Instantaneous Hazard Rates by Return to Work Program Participation, All Injured Workers

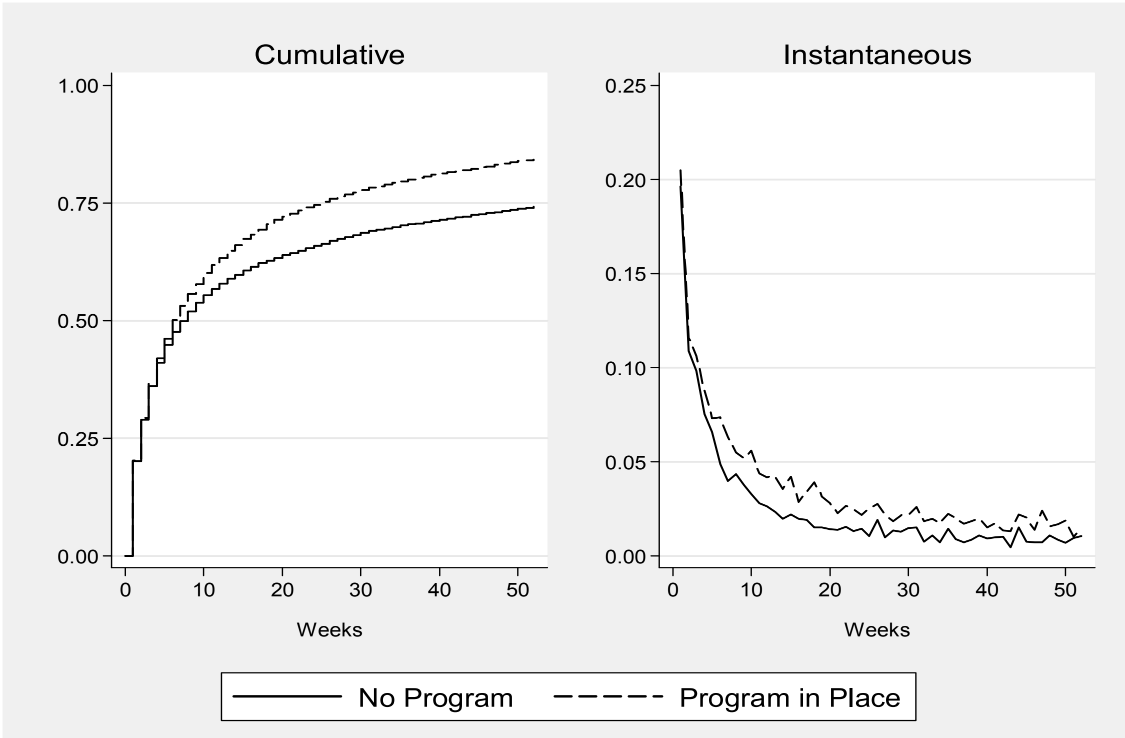


Figure 3. Cumulative and Instantaneous Hazard Rates by Return to Work Program Participation, Male Injured Workers

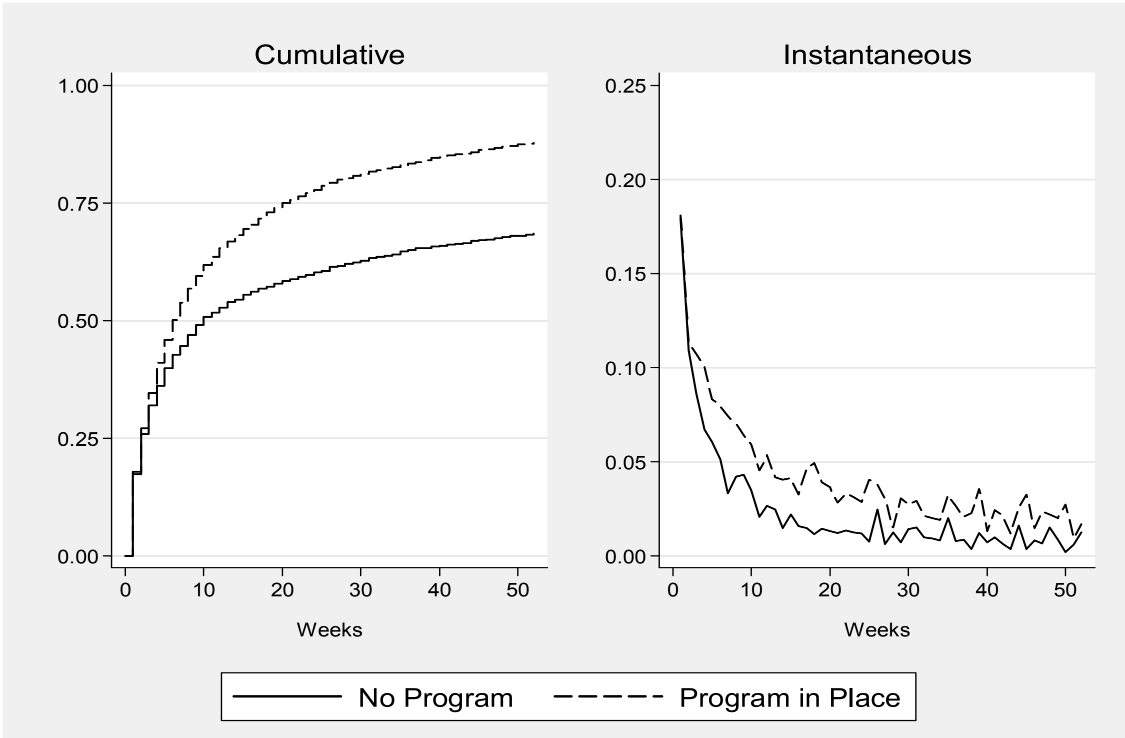


Figure 4. Cumulative and Instantaneous Hazard Rates by Return to Work Program Participation, Female Injured Workers

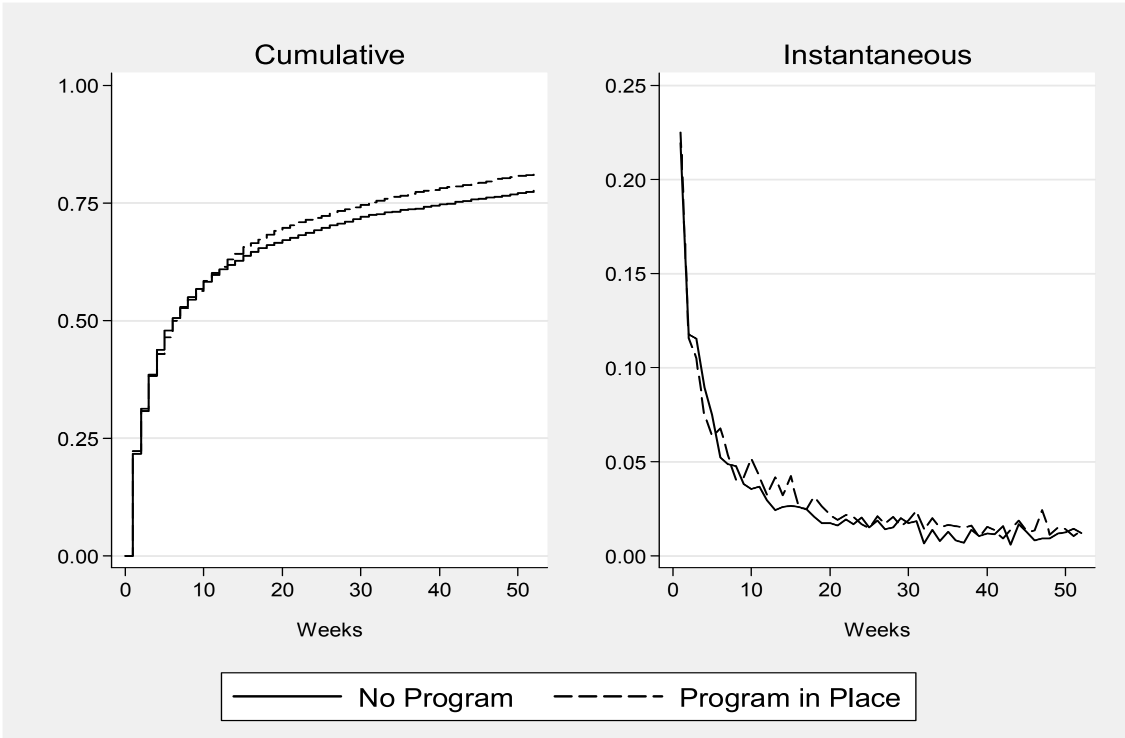
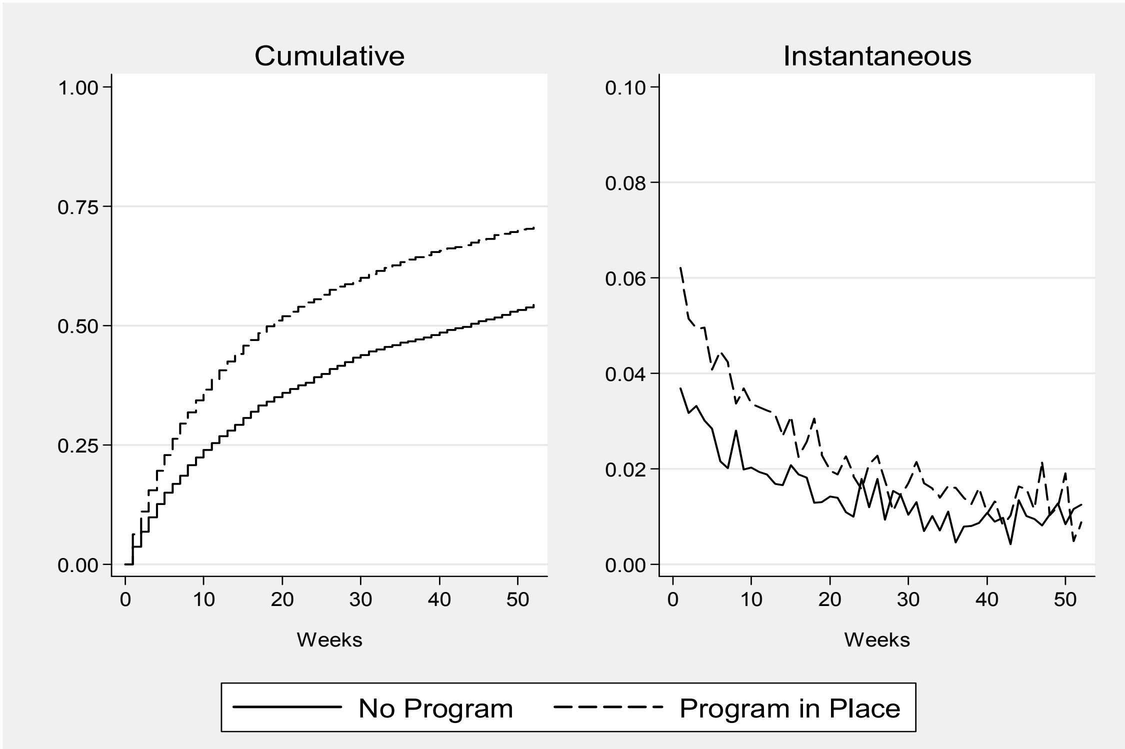


Figure 5. Cumulative and Instantaneous Hazard Rates by Return to Work Program Participation, Permanently Disabled Workers



Tables

Table 1. Perceived Importance and Frequency of Use of Leading Methods for Transitioning Injured Employees Back to the Workplace

Method	Used Frequently or Quite Often	Used Occasionally	Used Rarely or Not at All	Perceived Importance Level: Scale 1-5, 5=Very Important
Modified work tasks	82%	14%	5%	4.68
Modified work station/equipment	50%	27%	18%	4.10
Reduced time/work schedule change	45%	27%	18%	3.86
Different job in same or different department	32%	41%	23%	3.71

Notes: Table reports the results from a survey of return to work and disability management practice of 40 large, self-insured employers in California.

Table 2. Response-Rate for Self-Insured Employers

Variable	Parameter Estimate	Standard Error	Adjusted T-Stat
Cases (in logs)	-0.060	0.027	-2.195
Number of employees (in logs)	0.065	0.030	2.196
Number of administrative changes	-0.095	0.085	-1.108
SIC 0 Agriculture, Forestry, and Fishing	0.051	0.287	0.179
SIC 1 Mining and Construction	0.017	0.403	0.043
SIC 2 Manufacturing	0.166	0.145	1.143
SIC 3 Manufacturing	-0.089	0.142	-0.625
SIC 4 Transportation	-0.213	0.162	-1.309
SIC 4 Communication, Power, Water	0.722	0.140	5.151
SIC 6-7 Banks, Insurance, Hotels, Entertainment	-0.097	0.138	-0.706
SIC 8 Health Care Services	0.201	0.114	1.756
Self-administered	-0.312	0.096	-3.246
Combination administered	0.150	0.152	0.990
Southern California Headquarters	0.190	0.091	2.095
Outside California Headquarters	0.398	0.101	3.947
Payroll per employee (in thousands)	0.003	3.820	0.777
Total indemnity per employee (in thousands)	-0.009	3.629	-0.247

Notes: The table reports a regression using data on the characteristics of the population of self-insured firms from the State California Self-Insured Plans as reported in Reville et al. (2001). They note that these data were used to select the sample that was contacted for the survey. The regression examines the determinants of a positive response to the request for data and thus which characteristics lead to a greater probability that a firm would be one of the 68 firms that provided data. R-squared = 0.3449, Omitted SIC 5 category: Retail, Wholesale Trade. The regression is weighted by the inverse of the sampling probability for the firm and standard errors are heteroskedasticity consistent.

Table 3. Summary Statistics

	Total Sample N = 17,312	Program in Place N = 8,082	No Program N = 9,230
Worker Characteristics			
TTD Weeks	29.50	20.44	37.44
	(51.90)	(34.99)	(62.01)
Returned to Work	0.94	0.96	0.93
	(.23)	(.19)	(.26)
Weeks to Sustained Return to Work	33.68	24.13	42.05
	(56.04)	(40.90)	(65.32)
Sustained Return to Work	0.91	0.93	0.89
	(.29)	(.25)	(.31)
Re-Injury Rate	0.13	0.08	0.18
	(.34)	(.27)	(.38)
Employed 5 Years After Injury	0.707	0.712	0.699
	(.455)	(.453)	(.458)
Weekly Wage	601.21	690.18	523.31
	(429.21)	(295.30)	(506.16)
Positive PPD	0.39	0.46	0.32
	(.49)	(.50)	(.47)
PPD Payments	5,861.30	6,864.40	4,984.33
	(18,898.59)	(19,284.29)	(18,394.99)
Female	0.58	0.53	0.63
	(.49)	(.50)	(.48)
Age	41.57	42.19	41.02
	(10.44)	(9.73)	(10.99)
Employer Characteristics			
Indemnity Cost Per Case ($)	4,008.38	4,454.63	3,617.63
	(1,501.80)	(1,411.08)	(1,469.68)
Medical Cost Per Case ($)	3,415.73	4,130.08	2,790.23
	(1,271.48)	(1,370.16)	(741.89)
Injury Rate	0.11	0.11	0.10
(# cases/ # employees)	(.03)	(.02)	(.03)
Transportation	0.33	0.36	0.31
	(.47)	(.48)	(.46)
Trade	0.01	0.01	0.00
	(.08)	(.17)	(.04)
Service	0.60	0.51	0.68
	(.49)	(.50)	(.46)
Manufacturing	0.06	0.12	0.00
	(.23)	(.33)	(.05)
Employer Size			
Number of employees	20,937	20,221	21,583
	(10,485)	(10,403)	(10,517)
Less than 1,000 employees	0.01	0.02	0.00
	(.09)	(.13)	(.04)
1,001-35,000 employees	0.52	0.43	0.60
	(.50)	(.50)	(.49)
More than 35,000 employees	0.47	0.55	0.40
	(.50)	(.50)	(.49)

Notes: The table reports sample means with standard deviations in parentheses.

Table 4. Discrete Time Hazard Model Results

	All Workers		Men		Women		Positive PPD	
	1	2	3	4	5	6	7	8
Dependent Variable: Weeks to Sustained Return to Work								
Program Effect	1.38** [0.321] (.065)	1.36** [0.309] (.023)	1.48** [0.393] (.088)	1.52** [0.417] (.047)	1.26** [0.228] (.058)	1.18** [0.161] (.028)	1.56** [0.446] (.097)	1.35** [0.300] (.073)
Dependent Variable: TTD Weeks								
Program Effect	1.42** [0.348] (.053)	1.39** [0.33] (.032)	1.64** [0.492] (.070)	1.64** [0.494] (.051)	1.11** [0.105] (.050)	1.06 [0.057] (.036)	1.66** [0.507] (.085)	1.47** [0.382] (.081)
Employer Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table reports estimated hazard rate ratios, coefficients, and standard errors from discrete time logistic hazard models that include the log of duration of time to return to work as the baseline hazard as well as other covariates. The first two columns report the estimates for all injured workers, columns three and four report estimates for males, columns five and six report estimates for females, and columns seven and eight report estimates for workers with positive PPD benefits. Hazard ratios are reported with coefficients in brackets. Robust standard errors are presented in parentheses, adjusted to allow for clustering by employer. A * or ** represents statistical significance at the 10 percent or 5 percent or better levels, respectively.

Table 5. Estimated Change in Median and Mean Weeks to Return to Work with a Program in Place

	All Workers		Positive PPD	
	1	2	3	4
Weeks Until Sustained Return to Work				
Weeks Until Return to Work: No Program	9.0 (41.1)	8.9 (40.8)	39.7 (69.5)	35.5 (65.2)
Difference with Program	-3.8 (-15.7)	-3.6 (-15.1)	-18.8 (-25.9)	-12.6 (-17.6)
Weeks Receiving TTD Benefits				
Weeks Until Return to Work: No Program	7.8 (35.3)	7.7 (35.0)	32.8 (60.1)	30.1 (56.8)
Difference with Program	-3.2 (-15.1)	-3.0 (-14.3)	-16.0 (-26.3)	-12.0 (-20.0)
Employer Fixed Effects	No	Yes	No	Yes

Notes: The table reports estimated median weeks to return to work for workers not in a program, and the difference compared to workers in a program. Estimated mean number of weeks to return to work for workers not in a program, and the difference compared to workers in a program are reported in parentheses. Differences are based on the fixed-effect models reported in Table 4.

Table 6. Discrete Time Hazard Model Results – Program Components

	All Workers		Men		Women		Positive PPD	
	1	2	3	4	5	6	7	8
Dependent Variable: Weeks to Sustained Return to Work								
Modified Work	1.27*** [.239] (.074)	1.37*** [.312] (.025)	1.44*** [.365] (.074)	1.50*** [.402] (.050)	1.13** [.123] (.052)	1.19*** [.175] (.032)	1.35*** [.300] (.078)	1.36*** [.307] (.075)
Modified Equipment	1.50*** [.404] (.108)	1.16 [.147] (.121)	1.54*** [.434] (.122)	2.62*** [.961] (.172)	1.04 [.035] (.160)	1.07 [.069] (.071)	1.21* [.190] (.101)	2.22*** [.799] (.146)
Different Job	0.70** [-.362] (.173)	0.79** [-.243] (.106)	0.74** [-.308] (.124)	0.42*** [-.868] (.176)	0.92 [-.079] (.175)	0.73*** [-.317] (.048)	0.82 [-.196] (.125)	0.40*** [-.917] (.123)
Scheduling Accommodations	1.22 [.196] (.371)	1.54 [.432] (.268)	1.40 [.333] (.306)	1.71** [.534] (.216)	0.64 [-.448] (.515)	1.06 [.055] (.259)	1.16 [.144] (.484)	1.48 [.388] (.439)
Employer Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table reports estimated hazard rate ratios, coefficients, and standard errors from discrete time logistic hazard models that include the log of duration of time to return to work as the baseline hazard as well as other covariates. The first two columns report the estimates for all injured workers, columns three and four report estimates for males, columns five and six report estimates for females, and columns seven and eight report estimates for workers with positive PPD benefits. Hazard ratios are reported with coefficients in brackets. Robust standard errors are presented in parentheses, adjusted to allow for clustering by employer. A * or ** represents statistical significance at the 10 percent or 5 percent or better levels, respectively.

Table 7. Estimated Change in Median and Mean Weeks to Return to Work with a Program in Place by Specific Program Element

	All Workers	
Weeks Until Sustained Return to Work		
	1	2
Weeks Until Return to Work: No Program	9.2 (41.6)	9.1 (41.6)
Modified Work	-3.1 (-12.2)	-3.7 (-15.5)
Modified Equipment	-5.8 (-26.9)	-4.8 (-21.2)
Different Job	2.5 (7.2)	-1.1 (-3.8)
Scheduling Accommodations	-4.7 (-20.3)	-6.2 (-29.5)
Employer Fixed Effects	No	Yes

Notes: The table reports estimated median weeks to return to work for workers not in a program, and the difference compared to workers in a program with each specific program characteristic. Estimated mean number of weeks to return to work for workers not in a program, and the difference compared to workers in a program are reported in parentheses. Differences are based on the fixed-effect models reported in Table 4.

Table 8. Worker Re-Injury Rates and Long-Term Employment Outcomes

	All Workers		Men		Women		Positive PPD	
	1	2	3	4	5	6	7	8
Dependent Variable: Employed 3 years after injury								
Probability of Working 3 Years After Injury	0.017 (0.025)	-0.011 (0.14)	0.025 (0.025)	0.027** (0.011)	0.022 (0.028)	-0.021 (0.020)	0.030 (0.037)	-0.012 (0.023)
Dependent Variable: Employed 5 years after injury								
Probability of Working 5 Years After Injury	0.023 (.020)	0.041 (.034)	0.019 (.018)	0.036* (.020)	0.028 (.031)	0.053 (.052)	0.021 (.035)	0.055 (.055)
Employer Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table reports estimated coefficients and standard errors from linear probability models that include covariates to control for worker and employer heterogeneity as well as year of injury effects. The first two columns report the estimates for all injured workers, columns three and four report estimates for males, columns five and six report estimates for females, and columns seven and eight report estimates for workers with positive PPD benefits. Robust standard errors are presented in parentheses, adjusted to allow for clustering by employer. A * or ** represents statistical significance at the 10 percent or 5 percent or better levels, respectively.

Table 9. Break-Even Return to Work Program Treatment Effects, Measured in Weeks to Sustained Return to Work

			Program Cost per Injured Worker					
			$ 500	$ 1,000	$ 1,500	$ 2,000	$ 2,500	$ 3,000
	Low:	$ 347	1.4	2.9	4.3	5.8	7.2	8.6
Weekly Wage	Medium:	$ 438	1.1	2.3	3.4	4.6	5.7	6.8
	High:	$ 757	0.7	1.3	2.0	2.6	3.3	4.0

Notes: This table reports break-even program treatment effects, measured in weeks to sustained return to work across different program cost and weekly wage scenarios. The average program cost per injured worker in our sample is $1,174 and the average indemnity cost per week is $438. We estimate that the impact of adopting a program is a reduction of between 3 and 4 weeks in median duration of work absence therefore the programs are cost-effective in any scenario with a break-even effect below that level.

Table 10. Treatment Effect Estimates: Alternate Specifications

	All Workers		Men		Women		Positive PPD	
	1	2	3	4	5	6	7	8
Dependent Variable: TTD Weeks								
Discrete Time Piece-wise Constant	1.41*** [0.35] (0.068)	1.40*** [0.34] (0.043)	1.64*** [0.50] (0.087)	1.66*** [0.51] (0.060)	1.04 [0.04] (0.058)	0.99 [-0.01] (0.042)	1.66*** [0.51] (0.086)	1.48*** [0.39] (0.088)
Cox Proportional Hazard	1.37*** [0.31] (0.057)	1.36*** [0.31] (0.035)	1.58*** [0.46] (0.068)	1.60*** [0.47] (0.045)	1.04 [0.04] (0.049)	1.00 [0.00] (0.033)	1.62*** [0.48] (0.084)	1.45*** [0.37] (0.087)
Weibull Hazard	1.46*** [0.38] (0.057)	1.43*** [0.36] (0.036)	1.77*** [0.57] (0.085)	1.77*** [0.57] (0.060)	1.03 [0.03] (0.059)	0.98 [-0.02] (0.047)	1.68*** [0.52] (0.089)	1.48*** [0.39] (0.085)
Dependent Variable: Weeks to Sustained Return to Work								
Discrete Time Piece-wise Constant	1.36*** [0.31] (0.073)	1.35*** [0.30] (0.025)	1.54*** [0.43] (0.105)	1.60*** [0.48] (0.058)	1.09 [0.08] (0.068)	1.01 [0.01] (0.038)	1.57*** [0.45] (0.096)	1.36*** [0.31] (0.070)
Cox Proportional Hazard	1.37*** [0.31] (0.057)	1.36*** [0.31] (0.035)	1.58*** [0.46] (0.068)	1.59*** [0.47] (0.045)	1.04 [0.04] (0.049)	1.00 [0.00] (0.033)	1.51*** [0.41] (0.089)	1.34*** [0.29] (0.067)
Weibull Hazard	1.46*** [0.38] (0.057)	1.43*** [0.36] (0.036)	1.77*** [0.57] (0.085)	1.77*** [0.057] (0.060)	1.03 [0.03] (0.059)	0.98 [-0.02] (0.047)	1.57*** [0.45] (0.100)	1.34*** [0.29] (0.074)
Employer Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table reports estimated coefficients from three separate specifications: a discrete time piecewise-constant hazard model, a continuous time Cox proportional hazard model, and a continuous time Weibull hazard model. The first two columns report the estimates for all injured workers, columns three and four report estimates for males, columns five and six report estimates for females, and columns seven and eight report estimates for workers with positive PPD benefits. Hazard rate ratios for the program effect are reported with coefficients in brackets. Robust standard errors are presented in parentheses, adjusted to allow for clustering by firm. A *, ** or *** represents statistical significance at the 10, 5 or 1 percent levels, respectively.

Table 11. Selection Regressions

	1	2	3	4
TTD Weeks	-0.0008 (0.0023)	-0.0029 (0.0020)	-0.0004 (0.0012)	-0.0028 (0.0022)
Weeks to Sustained Return to Work	0.0026 (0.0029)	0.0014 (0.0023)	0.0018 (0.0022)	0.0016 (0.0023)
Employer Fixed Effects	No	Yes	No	Yes

Notes: The table reports estimated coefficients from OLS linear probability regressions of the probability a firm adopts a return to work program in a given time period against lags of the variables illustrated as well as other covariates. The first two columns report the estimates for models that include a lagged duration variable, and columns three and four report estimates for models that include a lagged duration variable and a twice-lagged program indicator variable. Robust standard errors are presented in parentheses, adjusted to allow for clustering by firm. A *, ** or *** represents statistical significance at the 10, 5 or 1 percent levels, respectively.

II. Chiropractic Care and Physical Therapy: Evaluating Effectiveness and the Impact of California Workers' Compensation Reforms[36]

Abstract

This study addresses the impact of California workers' compensation reforms that limit two controversial medical treatments: chiropractic care and physical therapy. It finds that utilization rates in California decrease significantly after the reforms and fall more in line with national averages of about 10 to 14 visits for most conditions. Relatively few studies document a substantial benefit of chiropractic care or physical therapy on employment or health outcomes for injured workers relative to conventional physician treatment. Evidence focusing on return to work outcomes and cost-effectiveness is mixed and weak. Clinical evidence on the effectiveness and utilization of chiropractic and physical therapy treatment provides little support for the idea that these workers' compensation reforms should have a substantially negative impact on outcomes for injured workers.

Keywords: workers' compensation, California workers' compensation reforms, utilization and effectiveness of physical therapy and chiropractic care treatment

[36] An earlier version of this paper was published as part of: Seabury, Seth A., and Christopher F. McLaren., 2010. *The Frequency, Severity, and Economic Consequences of Musculoskeletal Injuries to Firefighters in California*. Santa Monica, Calif.: RAND Corporation, MG-1018-CHSWC.

Introduction

Workers' compensation costs in California rank among the highest in the nation and in the early 2000's they were spiraling out of control (Sengupta et al., 2012).[37] In response to these skyrocketing costs, California policymakers introduced sweeping reforms in 2003 and 2004 aimed at improving benefit delivery to injured workers and controlling the growth of medical utilization and costs. Part of the reforms targeted doctor of chiropractic care (DC) and physical therapy (PT) treatments, which are two of the most controversial treatments in the workers' compensation system, by imposing a limit of 24 visits for the life of a workers' compensation claim unless the employer authorizes additional treatments.[38,39,40]

For the most common injuries treated by chiropractors and physical therapists, American College of Occupational and Environmental (ACOEM) guidelines are well under the 24-visit cap. For instance, ACOEM guidelines recommend two or less PT visits and 12 DC visits for a soft-tissue low back injury, both of which are well below the new limit. But, the treatment cap may be binding for some injured workers. For instance,

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[37] From 1994 to 2003, the average estimated cost for an indemnity claim rose an alarming 230 percent (Swedlow, 2005a). In 2003, medical, cash and total benefits in California were the highest in the nation by a wide margin and benefits per $100 of covered wages were the third highest in the nation, just below Montana and West Virginia (Sengupta et al., 2006).

[38] In 2003, DC and PT treatments comprised over 33 percent of all classified physician costs in the California workers' compensation system (Wynn et al., 2011).

[39] The specific reform to limit physical medicine treatments, Labor Code §4604.5, was adopted as part of California Senate Bill (SB) 228 and later in SB 899. It applied to workers' compensation claims occurring on or after January 1, 2004.

[40]Labor Code §4062.9 also shifted decision-making beyond the 24-visit cap from the medical care provider to the employer by repealing the presumption of correctness of the treating physician and Labor Code §4610 required employers to adopt utilization review systems consistent with the American College of Occupational and Environmental (ACOEM) guidelines or some other approved set of guidelines. More recently in 2013, California SB 863 was signed into law and additional limits have been placed on DC treatment. Labor Code §4600 prevents chiropractors from acting as Primary Treating Physician's (PTPs) after an injured worker has received the maximum number of visits allowed by Labor Code §4604.5. Additionally, Labor Code §139.2 mandates that chiropractors be certified in California and that the certification program include instruction on disability evaluation report writing.

80 to 90 percent of patients with acute low back pain (LBP), one of the most prevalent work-related musculoskeletal disorders (MSDs) and the most common reason for visiting a DC or PT (Hurwitz, Morgenstern, Harber, et al., 2002; Torstensen et al., 1998), will recover and be back to work within six to eight weeks after injury (Frank et al., 1996).[41] However, 8 to 10 percent of those with acute pain will end up with chronic LBP (Frank et al., 1996). In these cases, long-term treatment past the 24-visit cap might be necessary.

Shortly after the enactment of these provisions, the number of visits and costs for both DC and PT treatments in the California workers' compensation system decreased roughly 40 to 60 percent from just before the reforms in 2002 until after the reforms in 2004 (Swedlow, 2005a).[42] But the overall social-welfare effects of the cap are unclear, particularly for workers with higher MSD risk who may be more likely to seek DC and PT treatment.[43] First, MSDs often lack organic evidence of disability (Guidotti, 2002) which might prevent some workers from receiving beneficial treatment above and beyond the 24-visit limit because of a lack of evidence to justify additional treatment. Second, because MSDs are often a cumulative injury, it is more challenging to provide evidence documenting the injury within the standard timeline for discovery. It is clear

[41] The most common MSDs treated by DCs and PTs involve back injuries. Patients with chronic LBP represent a significant proportion of back-injury treatments and dominate the total proportion of those patients seeking long-term care. Other common MSDs treated by DCs and PTs are neck pain, carpal-tunnel syndrome, and other injuries caused by sprains, strains, and tears.

[42] Costs decreased significantly after the implementation of the reforms in 2003 and 2004. However, workers' compensation costs in California have been increasing steadily since 2005. Yang (2011) reports that California's medical costs per workers' compensation claim increased 8 percent per year from 2005 to 2009. Yang attributes the recent cost increase to a number of factors including moderate increases in services per visit for physical medicine (DC and PT treatments included). Despite the increase in costs in recent years, the reforms still appear to have reduced costs significantly, particularly the cap on physical medicine visits and the repeal of the primary treating physician presumption of correctness.

[43] Given the high proportion of MSDs experienced by firefighters relative to other occupations, a cap on DC and PT treatments might be disproportionately felt by this sector of the workforce (Seabury and McLaren, 2010) simply because they are more likely to go to a DC or PT.

that we need more evidence about the effectiveness of DC and PT treatments, particularly long-term treatments, to make informed policy decisions.

This paper has two primary goals. The first goal is to estimate average utilization rates for DC and PT treatments in California and the rest of the US. To accomplish this, a literature review is conducted along with an analysis of the Medical Expenditure Panel Survey (MEPS). Estimating average utilization rates provides a basis for determining how many injured workers are expected to be affected by the cap if utilization in California workers' compensation cases is comparable to that of the general population. The second goal is to assess the existing evidence on the effectiveness of DC and PT treatments, with a focus on long-term treatments, compared to alternative methods. To accomplish this goal, a systematic literature review is performed. This analysis provides insight into whether the reforms limiting treatment may adversely affect injured workers who will not be able to receive long-term treatments.

Based on the MEPS analysis and literature review, I find utilization rates in California decreased significantly after the 2004 reforms and fell more in line with national averages of about 10 to 14 visits for most conditions. Roughly 10 percent of individuals who sought chiropractic or physical therapy treatment went for more than 24 visits, which suggests that the treatment cap may be binding for some injured workers. Relatively few studies document a substantial benefit of DC or PT on employment or health outcomes for injured workers relative to conventional physician treatment. Further, evidence focusing on return to work outcomes and cost-effectiveness is mixed and weak. Clinical evidence on the effectiveness and utilization of chiropractic and physical therapy treatment provides little support for the idea that the workers' compensation reforms should have a substantially negative impact on outcomes for injured workers.

The rest of the paper is organized as follows. The next section describes the methods, section III analyzes the results, and section IV concludes with a discussion of the findings and policy implications.

Methods

To evaluate the existing evidence on average utilization and effectiveness of DC and PT treatments, a systematic literature review is conducted. The search focuses on eight high-quality journals within MEDLINE using search terms chosen to identify relevant papers by addressing type of care, type of injury, and outcome measures. Table 12 reports the specific journals and terms used and the total number of papers searched. Other papers found through secondary searches or by reviewing separate papers are included in the summary as necessary. In total, the search identified 5,797 possible papers.

In addition to the literature review, the Medical Expenditure Panel Survey (MEPS) is used to estimate average utilization rates among individuals who went to a DC or PT.[44] MEPS is a nationally representative survey conducted by the Agency for Healthcare Research and Quality (AHRQ) designed to measure health care insurance, utilization, and expenditures. MEPS is composed of a number of data files that contain a breadth of

[44] MEPS is a set of large-scale surveys of families and individuals, their medical providers, and employers across the United States. MEPS collects data on the specific health services that Americans use, how frequently they use them, and the cost of these services, as well as insurance and personal well-being measures. The household component collects data from a sample of families and individuals in selected communities across the United States, drawn from a nationally representative subsample of households that participated in the prior year's National Health Interview Survey. The medical-condition file is a subset of the individuals included in the full-year file and includes detailed information on specific medical conditions.

health utilization measures, including the number of visits to a particular type of doctor, as well as detailed injury and demographic information. While the MEPS is a nationally representative sample and not specific to California, it still allows for the estimation of utilization rates for DC and PT services across injury types and workers' compensation status at the national level.

Chiropractic Care and Physical Therapy Utilization

When reviewing utilization studies, the focus is on the type of care provided; the sample analyzed, including injury type and geographic location; and mean and median utilization rates. Table 13 summarizes the final 12 papers selected. Of the 12 papers selected, five focus on DC utilization, three on DC and PT, and four on PT. The papers include data from U.S. nationally representative samples; data from specific states, including California, as well as international samples in Canada and Taiwan; and workers' compensation utilization rates. Sample sizes range from 160 to more than 600,000.

The dataset used in this analysis is constructed by combining the full-year consolidated and medical-condition files from the household component of MEPS for the years 2002–2007. All individuals with an injury and an identifiable ICD-9 code who went to a DC or PT at least once are included. The drawbacks of using this data are (1) the sample is done at the national level and state-specific data are not publicly available, and (2) the occupation codes do not allow us to evaluate whether the new reforms may disproportionately impact high risk groups such as firefighters.[45] Nevertheless, the

[45] MEPS does have individual data containing more detailed information on geography and occupation. Given the relatively small sample sizes at the state level, however, it is unlikely there is sufficient information on firefighters in California to conduct a meaningful analysis with the individual data.

estimates do provide valuable information to help evaluate average utilization rates and determine how binding the cap on DC and PT treatments in California may be if care follows standard practices.

Chiropractic Care and Physical Therapy Effectiveness

To evaluate the effectiveness of DC and PT treatments, this study reviews the literature for evidence on three classes of outcomes: (1) health outcomes (e.g., disability or impairment), (2) return to work, and (3) cost-effectiveness. Assessing the health effects of DC and PT treatment is important in order to identify whether treatments improve health outcomes for workers with MSDs more than treatments by alternative options, such as a general practitioner, particularly for long-term treatments. However, despite accepted definitions of *clinical significance* (see, e.g., Goldby et al., 2006, and Cherkin, Deyo, et al., 1998), interpreting changes in commonly used health outcomes, such as the Oswestry Disability Index or the Roland-Morris Disability Questionnaire, in a policy-evaluation framework is challenging. Therefore, papers assessing return to work outcomes for workers who receive DC and PT treatments are evaluated because returning injured workers to work is of primary importance. Finally, papers that assess the cost-effectiveness of DC and PT treatments relative to other methods are included because cost-effectiveness is one of the most useful metrics with which to evaluate this policy from a social-welfare perspective.

When reviewing studies to assess the effectiveness of chiropractic care and physical therapy, the focus is on a number of factors: type of care provided and the nature of the intervention, study type and quality (e.g., randomized controlled trial (RCT) or quasi-experimental design), sample size and data description (including injury type and demographic characteristics), outcome measures, incremental results (to help identify understand how the effectiveness of treatment varies over time and by number of treatments), and overall results. Papers that clearly compare DC or PT to some other treatment (most commonly to a placebo or general practitioner (GP)) are included and preference is given to studies that perform RCTs and observational studies with a strong methodology. Since the workers' compensation reform cap on DC and PT treatments is set at 24 visits, preference is also given to studies that evaluate long-term care, although there are few studies evaluating the effectiveness of such extended periods of care.

More than 5,700 papers met the initial criteria, and these are narrowed to 17 papers, which Table 15 summarizes. Of the 17 papers, 12 include an evaluation of physical or manual therapy,[46] and 8 evaluate DC care. The primary comparison group is care provided by a GP (9 papers), and the majority of the papers implement an RCT design (12), in addition to five observational studies. The main injury type is back pain, with 13 of the 17 papers evaluating patients with some type of back pain (acute, sub-acute, chronic), three papers focus on patients with neck pain, one on sciatica, and one on osteoarthritis of the knee. Virtually all of the papers (15 of 17) include health outcomes; eight evaluate return to work outcomes; and six include cost analyses. Only two papers evaluate long-term treatments.

Results

Chiropractic Care and Physical Therapy Utilization: Literature Review

Shekelle and Brook (1991) provide the first set of population-based estimates of chiropractor utilization using data from the RAND Health Insurance Experiment. They

[46] Manual therapy is a specialization within physical therapy that provides comprehensive and conservative management for pain in the spine and extremities.

estimate a median and mean number of visits per year for individuals who went to a DC at least one time at 7 and 11.5, respectively. A significant finding of this paper is that DC use has a substantial tail to the right. Specifically, they find that two percent of the sample consumes ten percent of the total number of visits. They also note that chiropractors account for about twice as many visits for back pain as physicians when compared to a previous study analyzing the National Medical Ambulatory Care Survey between 1975 and 1980 (Murt et al., 1986).

In subsequent work, Shekelle, Markovich, and Louie (1995b) further utilize RAND Health Insurance Experiment data and estimate mean and median visits to a chiropractor of 10.4 and 5, respectively. In another study that provides nationally representative estimates of chiropractic utilization, Eisenberg, Kessler, et al. (1993) find that 10 percent of their sample utilized chiropractic services in the previous month with a mean number of visits per user of 13.

Hurwitz, Coulter, et al. (1998) utilize data from 131 chiropractors sampled from five U.S. sites and one Canadian site and find that, of the 1,916 patients, more than 40 percent with LBP have acute (less than three weeks) episodes, while about 20 percent have chronic (more than six months) episodes. The dominant therapeutic intervention delivered is spinal manipulative therapy and is received by 80 percent of patients with LBP. The median length of an episode for LBP is more than twice the median length for other episodes of care (29 days versus 14 days), and there are almost twice as many visits during episodes of care for LBP (a median of seven visits versus four visits). Their estimate of the mean number of visits for all episodes of care is 12.4, significantly higher than the median number of visits. They note, as did Shekelle and Brook (1991), that a small proportion of patients are frequent or long-term users of chiropractic services causing the distribution of visits and episode length to be skewed to the right.

Swedlow (2005b) analyzes a sample of 610,371 workers' compensation claims in California that involve physical therapy or chiropractic manipulation from the Industry Claims Information System (ICIS).[47] This paper reports that after the implementation of the workers' compensation reform cap on DC and PT treatments, the average number of PT visits at nine months after the date of injury fell 45 percent from 20 to 11. The average number of chiropractic manipulations recorded nine months after injury also declined 55 percent, from 29 to 13 visits. While the reductions in visits and costs are not surprising given the cap, the pre-cap averages are quite high compared to other estimates of utilization rates. The post-cap averages are more in line with the estimates provided by Shekelle and Brook (1991); Shekelle, Markovich, and Louie (1995a); Eisenberg, Kessler, et al. (1993); and Hurwitz, Coulter, et al. (1998).

In another study utilizing workers' compensation data, Wasiak and McNeely (2006) find that utilization and costs of chiropractic care vary significantly across workers' compensation jurisdictions. They report median chiropractic-care utilization rates ranging from 5 to 14.5 across 7 workers' compensation jurisdictions (Florida, Idaho, Illinois, Maryland, New Hampshire, New York, and Pennsylvania). They also find that states with more restrictive payment policies have lower costs of chiropractic care for work-related LBP and lower numbers of services per visit. The authors note that the way

[47] The ICIS database encompasses transaction-level data and, at the time of this study, included information on more than 3.5 million California workers' compensation claims contributed by large and midsize national regional insurers and self-insured employers for claims with dates of injury from 1993 through 2004.

in which the pattern of chiropractic care relates to duration of work disability and return to work remains largely unknown.

Two studies focus on utilization for chiropractic and PT services for chronic LBP using samples of adults from North Carolina. In the first, Carey, Garett, et al. (1995) find that chiropractors are major providers of care for chronic LBP and the people for whom they care are significantly less impaired than those who seek care from medical doctors. They note that the improved health status might be the result of care given by the DC, but it is also likely that the more severely disabled patients seek care from medical doctors who have the ability to prescribe medication and admit to the hospital and have ready access to medical subspecialists. Compared to Shekelle and Brook (1991), who find that the mean number of annual visits is 11.5 for visits of any cause, this study finds an average of 15 but, this study is condition-specific to one of the most disabling musculoskeletal problems—chronic back pain. In a more recent study, Carey, Freburger, et al. (2009) survey 732 patients in North Carolina with LBP and find that the mean number of visits to a DC is 21 and the mean number of visits to a PT is 16.

In studies focusing on PT utilization, Mielenz et al. (1997) use the same cohort as the Carey, Garett, et al. (1995) study and estimate the mean number of PT visits to be 8.5. They also find that patients who saw a PT for LBP tended to have more severe conditions. Tsauo et al. (2009) provide the first study reporting the average number of treatment sessions and duration of PT for work-related MSDs in Taiwan. Overall, they estimate mean and median numbers of treatments of 8 and 6, respectively. However, a small sample size and differences in the health care delivery system prevent us from assigning much weight to these estimates.

Other estimates of PT utilization range from 11 (Jette et al., 1994) to as low as 6 (Akpala et al., 1988). On the high side, Ehrmann-Feldman et al. (1996) report an average of 25 visits to PTs per episode of LBP for workers' compensation claimants in Canada. They do find that patients who are referred earlier tended to return to work sooner than those who were referred later.

Based on the utilization literature review, rates for DC and PT treatments vary by type of injury, workers' compensation status, and geographic location. Rates tend to be higher for patients with chronic LBP and workers' compensation claimants, and there is significant variation by geographic region. Estimates from nationally representative samples of DC utilization fall in the range of 10 to 13 visits per year for any condition, whereas the majority of median rates range from 5 to 7. The median rates are significantly lower due to a small proportion of patients consuming a large majority of visits (Shekelle and Brook, 1991; Shekelle, Markovich, and Louie, 1995b; Hurwitz, Coulter, et al., 1998). Studies focusing on patients with chronic LBP reported higher averages, ranging between 15 and 21 treatments per year (Carey, Freburger, et al., 2009; Carey, Garett, et al., 1995). Physical-therapy utilization rates fall in the same general range as DC rates, with averages as low as six to a high of 16 for patients with chronic pain.

Studies focusing on workers' compensation claimants report the highest utilization rates, and there is significant variation by geographic region. The highest averages reported are from California workers' compensation claimants prior to the recent reforms. Average utilization rates for DC and PT treatments prior to the reform are 29 and 20, respectively (Swedlow, 2005b), though the post-cap rates fell significantly to

17 and 11, respectively. A more recent study finds that the average number of visits to a chiropractor per claim has decreased dramatically from 40 visits in 2002-2003 to 11 visits in 2007-2008 (Wynn et al., 2011). The same study also finds that total medical payments for chiropractor and physical therapy treatments from 2003 to 2009 reduced 81 and 34 percent, respectively. After the implementation of the cap in California, utilization rates became more comparable to national estimates and costs have reduced significantly.

Chiropractic Care and Physical Therapy Utilization: MEPS Analysis

Table 14 reports utilization rates for chiropractic and physical- and occupational-therapy services from the MEPS. As noted earlier, this paper analyzes a sample of individuals who experienced an injury and sought care from a DC or PT at least once. Utilization rates are averaged over the years 2002–2007 and reported for the 25th, 50th, and 75th percentile. Averages are also reported for the entire sample of injured persons, as well as by segmenting those with an injury at work, those with a MSD injury, and those with a MSD injury at work. Finally, the proportion of each group that exceeds 24 visits is included.

The average chiropractic rates fall in line with much of the literature using nationally representative samples, at 10 visits per year. For all injured persons, the 25th percentile is 2, the median number of visits is 6, and the 75th percentile is 13. This falls in line with Shekelle and Brook (1991) and Hurwitz, Coulter, et al. (1998) who find that a small fraction of individuals account for a large portion of overall visits. The average number of visits for individuals injured at work is slightly higher than the overall average, at 11, but the difference is not significant. Roughly 10 percent of each group exceeds 24 visits to a DC or PT.

In the lower panel of Table 14, utilization rates for persons with an injury who went to a physical or occupational therapist at least once are reported. The average utilization rate is about 12. Interestingly, these utilization rates are higher than for chiropractic services, unlike in the literature; however, this might be because physical and occupational therapy is grouped together in the MEPS. The average utilization rate is significantly higher for those with a work-related injury than the rate for the entire sample. Not surprisingly, given these higher utilization rates, this group has the highest proportion of individuals exceeding 24 visits.

Chiropractic and Physical-Therapy Effectiveness: Health Outcomes

The evidence on the health outcomes of patients treated by a DC or a PT is mildly positive. Most studies find that, relative to GP care, DC and PT treatments provide benefits, particularly with patients experiencing back pain, but the benefits are small and not likely to be cost-effective. Relative to placebo therapies, DC and PT treatments fare much better, and patients generally show significant health improvements. Patients receiving DC care also report higher levels of treatment satisfaction.

In a widely cited paper, Cherkin, Deyo, et al. (1998) conduct a randomized control trial comparing DC and PT treatments with an educational booklet for patients with back pain. They find a positive association between the number of treatments (contact with providers) and improvement of back-related symptoms. Further, they find that some outcomes for the DC and PT groups are superior to those of the booklet group; however, the differences are small, and, after adjustment, they are significant only for the bothersomeness of symptoms at four weeks and the subjects' satisfaction with care at one and four weeks. This study concludes that, given the limited benefits and high costs of

DC and PT treatment, it seems unwise to refer all patients with LBP to a DC or PT, although these results are limited because of the relatively short duration of treatment studied.

As part of the University of California, Los Angeles (UCLA) LBP study, Hurwitz, Morgenstern, Harber et al. (2002) perform an RCT on 681 patients who are assigned to medical care (GP), medical care with physical therapy, or chiropractic care. They find that DC and GP treatments are comparable for LBP in their effectiveness after six months of follow-up and that PT is marginally more effective than GP for reducing disability, but the possible benefit is small. In an 18-month follow up with the same group, Hurwitz, Morgenstern, Kominski, et al. (2006) find no differences in outcomes between medical and chiropractic care, although chiropractic care might result in a greater likelihood of perceived improvement. The authors attribute this to either reflecting satisfaction or insufficient blinding of persons involved in the experiment. They do find that PT might be more effective than medical care alone for some patients.

Other studies focusing on LBP find mildly positive results for DC and PT treatments. Pengel et al. (2007) find that physiotherapist-directed exercise and advice are more effective in treating patients with subacute LBP, compared to placebo exercises and advice. Their effect is significant at 6 weeks but diminish 12 months after treatment. Goldby et al. (2006) follows subjects with chronic back pain in a spinal-stabilization rehabilitation group, manual therapy, or a minimal-intervention group. The authors find significant improvements in the spinal-stabilization group, but only reductions in Oswestry Disability Index and medication use are significant after 12 months. They note that spinal stabilization appears to be more effective than manual therapy, which is, in turn, more effective than a simple education booklet.

Koes et al. (1992) finds a decrease in severity of complaints for patients treated with PT; however, the authors note that a substantial part of the effect appeared to be due to nonspecific (placebo) effects. Finally, in a nonrandomized observational study, Haas et al. (2004) finds that most improvement with DC treatment is seen by three months after treatment and that the benefits are usually sustained for up to a year; however, exacerbation is seen thereafter.

Additional studies evaluate treatments for osteoarthritis and neck pain. Deyle et al. (2000) finds significant impacts of manual therapy on patients with osteoarthritis of the knee; however, there is no cost-effectiveness analysis. Hoving et al. (2002) examines manual therapy performed by physical therapists and finds improved outcomes for patients with neck pain over a seven-week duration compared to patients receiving GP care. Finally, in a comparison of manual therapy and a minimal intervention, Walker et al. (2008) finds that manual therapy is more effective in reducing pain, reducing disability, and increasing perceived treatment success in patients with mechanical neck pain.

This paper also reviews two meta-analyses that evaluate spinal manipulation, one of the most common treatments provided by chiropractors. Cherkin et al. (2002) reviews 26 RCTs evaluating spinal manipulation for acute and chronic back pain and reports that manipulation is superior to sham therapies and therapies judged to have no evidence of a benefit but is not superior to effective conventional treatments. Further, Assendelft et al. (2003) performs a metaregression analysis of 39 RCTs and finds that spinal manipulation is superior to other forms of treatment for acute or chronic LBP only when compared with sham therapies or therapies known to be ineffective, such as traction or bed rest.

Chiropractic and Physical-Therapy Effectiveness: Return to Work and Cost-Effectiveness

The evidence on return to work outcomes and cost for injured workers utilizing DC or PT treatments is mixed. Most studies do not find a significant effect on return to work rates and find that DC and PT treatments are not as cost-effective as more conventional therapies administered by a GP. However, there is one paper that does address long-term treatment and finds a positive cost-effectiveness for PT. Three of the papers evaluating return to work or cost implement an RCT design, whereas four papers are observational studies.

In one of the only long-term treatment studies, Torstensen et al. (1998) evaluate 208 injured persons in Norway with chronic LBP. The treatment group in this study receives 36 treatments of one of two types of physiotherapy: conventional physiotherapy or medical exercise therapy, while the control group undergoes self-exercise therapy. The authors find significant improvements for the patients in the physiotherapy group compared to the self-exercise group, but they do not find any difference in return to work rates between the groups. However, they do note that the two PT groups record fewer sick-leave days during treatment. After incorporating this into the cost-benefit analysis, they find that both PT groups have lower costs than the self-exercise group.

Two additional RCT studies evaluate the cost-effectiveness of PT and DC treatments. Niemistö et al. (2005) finds that combined manipulative treatment, stabilizing exercises, and physician consultation did not yield more cost-effective outcomes than physician consultation alone. This study finds that consultation alone was more cost-effective for both health care use and work absenteeism and led to equal improvement in disability and health-related quality of life. In another study, Luijsterburg et al. (2007) evaluate 135 patients with sciatica in the Netherlands and compare the effect of nine PT and GP treatments on a number of outcomes. While the authors find a significant difference on perceived recovery at the one-year follow-up in favor of the PT group, they find no incremental effect on quality of life or a statistically significant difference in days to return to work. Furthermore, they conclude that the PT treatment is not more cost-effective than GP.

In an observational study evaluating cost-effectiveness and return to work, Carey, Garett, et al. (1995) finds no significant difference in time to functional recovery, return to work, or complete recovery from LBP based on six provider types (orthopedic surgeon, HMO, primary care rural, primary care urban, chiropractic urban, and chiropractic rural). The study does find significantly higher costs for chiropractic services and higher utilization than other provider types; however, there is greater satisfaction reported for patients seen by chiropractors.

Johnson, Baldwin, and Butler (1999) estimate differences in costs, outcomes, and return to work between patients treated by a DC and those treated by a GP. They find that costs for an average work-related back claim are lower for DC patients than for GP patients because more DC patients return to work within the three-day waiting period, and those with temporary-disability claims return to work more quickly. However, once they control for unobserved heterogeneity between the groups, the significance disappears. They say that this might be because DC treatment returns injured workers to their jobs more quickly by providing continuing care after workers return. Another explanation they posit is that chiropractors treat less severe back injuries, on average, than do physicians. In an earlier study focusing on work-related sprains and strains, Johnson, Schultz, and Ferguson (1989) compare chiropractic, medical, and osteopathic care. They find that claimants who go to chiropractors lose one less day of work, on average, than claimants who see other practitioners (eight compared with nine days of work lost). However, only one-third of claimants responded to the survey, and no adjustments were made for baseline demographic characteristics or clinical severity. In addition, claimants selected providers themselves.

Wasiak, Kim, and Pransky (2007) use workers' compensation claim data from 4 states that cover 10 percent of the private market to analyze outcomes of workers who experience occupational LBP and see a chiropractor at least once. They analyze utilization and return to work outcomes and find that, after controlling for multiple factors, shorter chiropractic-care duration is significantly associated with a lower likelihood of work-disability recurrence and shorter work-disability duration. Workers initiating and concluding chiropractic care shortly after an injury have significantly fewer chiropractic visits and a lower share of DC visits in overall utilization than those who delay treatment. However, as the authors note, there are limitations in interpreting their results because it is not possible to determine whether more DC care leads to more work disability or whether work disability leads to more DC care.

Zigenfus et al. (2000) evaluate cost-effectiveness and return to work while studying the effects of initiating early physical-therapy treatment. They find that workers receiving treatment either the day of injury or the day after experience significantly fewer physician visits, fewer restricted workdays, fewer days away from work, and shorter case duration than the other groups. This conclusion assumes that initiating early physical therapy has a causal effect of improving outcomes, but there may be underlying differences in the groups examined (e.g. injury severity or unobservable characteristics) that explain the differences in outcomes. However, this study finds no significant differences among the three groups in injury severity which does improve the credibility of the results.

In a study focusing on treatment costs, Shekelle, Markovich, and Louie (1995a) compare the costs between provider types of episodes of back-pain care. Analyzing RAND Health Insurance Experiment data, they find that chiropractors serve as the primary providers for 40 percent of the back-pain episodes and have the highest mean outpatient costs. Although chiropractors charge less each episode than other practitioners, they see patients an average of 10 times per episode, approximately twice as often as any other type of provider. GPs see their patients only 2 times per episode and have the lowest mean outpatient and overall costs. Finally, in a review of cost-effectiveness studies of medical and chiropractic care for occupational LBP, Baldwin et al. (2001) find that chiropractors and physicians provide equally effective care for occupational LBP but that chiropractic patients are more satisfied with their care. Furthermore, they report that evidence on relative costs of medical and chiropractic care is conflicting.

Conclusion

Based on a review of the literature and analysis of the MEPS, this study finds that average utilization rates for DC and PT treatments are 10 to 14 visits for most conditions, with slightly higher averages for patients with chronic LBP and for workers' compensation claimants. These estimates fall in line with ACOEM recommendations of roughly 7 to 12 visits to a DC or PT per injury episode. In line with previous research, this study finds large difference in mean and median rates, indicating that there is a skewed distribution of visits. The highest utilization rates are for workers' compensation claimants in California prior to the recent reforms; however, since the reforms, utilization rates have fallen more in line with national averages. Furthermore, based on an analysis of the MEPS, this study finds that roughly 10 percent of individuals who seek DC or PT treatment will go more than 24 times.

This study also assesses existing studies that evaluate the effectiveness of DC and PT treatments with respect to health outcomes, return to work, and cost-effectiveness. This review provides little support for the idea that utilization review should have a substantially negative impact on outcomes for injured workers. Relatively few studies document a substantial benefit of DC or PT on employment or health outcomes for injured workers relative to conventional physician treatments. Most studies evaluating the health effects of DC and PT treatments find mildly positive results relative to GP care and significant improvements relative to placebos. The evidence regarding return to work outcomes and cost-effectiveness is mixed and weak. While some studies do find that DC and PT treatments are marginally cost-effective and return injured workers to work faster, they are sparse, and many studies find that alternative treatments from a GP are more cost-effective.

It is important to note that the existing literature tends to focus on much less intensive levels of treatment (that is, fewer visits per injury) than is seen in California's workers' compensation system prior to the reforms. While, on the one hand, this suggests that the levels of utilization in California's system exceed standard practice, it makes it difficult to apply the existing literature to assess whether the reforms will negatively affect worker outcomes.

Some conclusions can be drawn from this study's findings as to the overall impact on injured worker outcomes of the recent cap on DC and PT treatments. ACOEM guidelines suggest that virtually all injury conditions treated by a DC or PT can be treated well within the new 24-treatment cap, and utilization estimates confirm that most individuals do not require that much treatment. Given this, what does the literature suggest about the potential effect of the cap on workers who are denied the additional treatment? Based on the review of the effectiveness of DC and PT treatments, it does not appear that outcomes will dramatically worsen. DC and PT treatment do not appear to be correlated with significantly better health, return to work, or cost-effectiveness outcomes for injured workers than treatment from a GP. There is also no evidence that workers in high risk jobs would experience differential effects of treatment. That said, some of this lack of evidence is due to limitations in the existing evidence base.

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Tables

Table 12. Systematic Literature Review Search Terms and Journal List

Search Term	JAMA	NEJM	AJPH	AIM	JOEM	JOR	AJIM	*Spine*	Total
Treatment									
Chiropractic	34	42	18	19	2	2	1	114	232
Physical therapy	491	312	65	278	20	34	18	741	1,959
Occupational therapy	72	37	16	19	97	57	64	99	461
Spinal manipulation	10	13	5	26	0	1	0	144	199
Injury									
Musculoskeletal disorder	12	0	8	7	50	67	97	56	297
Chronic pain	138	128	9	117	34	53	20	803	1,302
Soft tissue injury	10	2	0	2	5	0	6	56	81
Low back pain	70	59	14	49	54	72	50	0	368
Outcomes and occupation									
Return to work	16	5	7	9	47	109	40	187	420
Absenteeism	36	14	47	26	151	15	38	64	391
Firefighters	0	8	6	1	39	1	32	0	87
Total	889	620	195	553	499	411	366	2,264	5,797

Notes: JAMA = *Journal of the American Medical Association*. NEJM = *New England Journal of Medicine*. AJPH = *American Journal of Public Health*. AIM = *Annals of Internal Medicine*. JEOM = *Journal of Occupational and Environmental Medicine*. JOR = *Journal of Occupational Rehabilitation*. AJIM = *American Journal of Industrial Medicine*.

Table 13. Chiropractic and Physical Therapy Utilization Studies

Study	Treatment	Injury	Data	Sample Size	Utilization Estimates
Hurwitz, Coulter, et al. (1998)	DC	All injuries treated by a DC	Chiropractors sampled from 5 U.S. and 1 Canadian site	131 chiropractors and 1,916 patients	Mean no. of visits for LBP range from 10.5 to 21.3, with medians ranging from 4 to 10. Mean no. of visits for all conditions range from 9.6 to 16.4, with an overall mean of 12.4, and medians range from 4 to 9.5, with an overall median of 7.
Shekelle and Brook (1991)	DC	All injuries treated by a DC	RAND Health Insurance Experiment	5,279	395 different persons use 7,873 chiropractic services for a visit rate of 41 per 100 person-years and rate of use of 7.5 percent. The median and mean numbers of visits per year are 7 and 11.5, respectively.
Eisenberg, Kessler, et al. (1993)	DC	All	Telephone interviews in a national sample of adults 18 years of age or older in 1990	1,539	10% of the sample use DC services in the previous month, with a mean number of visits per user of 13.
Tsauo et al. (2009)	DC	Work-related MSDs	Subjects from a medical center and regional hospital in Taiwan who were 18–65 years old with a recent MSD injury	160	53% of the sample receives PT. Of those, 70.7% receive short-term treatment (<30 days) and 29.3% receive long-term treatment (≥30 days). The number of sessions for the treatment groups is 7.8 ± 9.0, with a median of 6. Subjects in the short-term group receive 3.6 ± 2.9 sessions, and those in the long-term group receive 18.0 ± 12.9 sessions.
Shekelle, Markovich, and Louie (1995b)	DC	Back-pain care	RAND Health Insurance Experiment	3,105; 686 with episodes of back-pain care	Mean no. of visits to a DC per episode of back pain is 10.4, with a median of 5 visits.

Chiropractic and Physical Therapy Utilization Studies, continued

Study	Treatment	Injury	Data	Sample Size	Utilization Estimates
Carey, Garett, et al. (1995)	PT and DC	Chronic LBP	North Carolina adults with LBP (random-digit-dialing sampling design)	269	Of those who see a DC, mean no. of visits is 15.7. Of those who see a PT, mean no. of visits is 17.2.
Swedlow (2005b)	PT and DC	All related injuries	ICIS	610,371	From 2002 to 2004, average no. of PT visits 9 months after date of injury fell from 20.4 to 11.2 visits; average number of DC visits recorded 9 months after injury declined from 28.5 to 12.6.
Wasiak and McNeely (2006)	DC	LBP	WC claim data from a WC carrier with a 10% share of the U.S. WC private market in 7 states	13,734	Median utilization rates vary across 7 WC jurisdictions (Fla., Idaho, Ill., Md., N.H., N.Y., Pa.) from 5 to 14.5. Fee schedules alone are not associated with lower costs and utilization; restrictive payment policies are associated with lower costs and services.
Ehrmann-Feldman et al. (1996)	PT	Back injuries	WC claim data from Quebec, Canada	2,147	PT patients tend to return to work faster. Patients receiving PT tend to have longer work-duration absences than workers not receiving PT. Average is 24.5 treatments per patient.
Jette et al. (1994)	PT	LBP	Mail survey conducted with representatives of a national probability sample of facilities providing outpatient PT services	11,584 from 2,329 facilities	Average is 11 PT visits per episode. No difference between utilization in private and public facilities; WC claims are costlier, on average.
Mielenz et al. (1997)	PT	Acute LBP	Telephone interview of PT practitioners in North Carolina	1,580 patients from 208 practitioners	Median PT treatments vary from 5 for patients whose first provider is an HMO and up to 7 for patients who see a chiropractor first. Mean number of treatments is 8.5.

Notes: CI = confidence interval. HMO = health maintenance organization.

Table 14. MEPS Chiropractic and Physical Therapy Utilization Estimates

Utilization	Mean No. of Visits (2002–2007)	25th Percentile	50th Percentile	75th Percentile	Percentage Using >24 Visits
Chiropractic care[a]					
All (2,830)	10.1	2	6	13	10.7
WC (505)	10.5	2	5	13	10.9
MSD (1,090)	10.6	2	6	14	10.8
MSD & WC (218)	11.2	2	5.5	14	11.0
Physical and occupational therapy[b]					
All (2,692)	11.7	3	7	15	11.8
WC (535)	14.0*	4	8	17	14.8
MSD (736)	11.2	3	7	15	10.6
MSD & WC (191)	13.1	4	8	17	15.7

[a] Reports descriptive statistics of the distribution of chiropractic visits for individuals who go to a DC at least once.
[b] Reports descriptive statistics of the distribution of physical- and occupational-therapy visits for individuals who go to a therapist at least once.
Notes: The number of observations in each group is shown in parentheses. WC represents individuals injured at work, and MSD represents individuals with a musculoskeletal disorder as identified through ICD-9 codes. T-tests to evaluate differences in mean number of visits are conducted (each mean is compared to the mean for all workers within each panel). * = significance at the 5 percent level or better.

Table 15. Chiropractic and Physical Therapy Effectiveness Studies

Study	Treatment	Injury	Study Type	Data and Sample Size	Outcome Measure(s)	Duration and No. of Treatments	Incremental Results	Results
Cherkin, Deyo, et al. (1998)	PT, DC, education booklet	LBP that persists >7 days after primary care visit	RCT	321	11-point bothersomeness-of-symptoms scale, level of dysfunction, return to work, and costs	One month with a maximum of 9 treatments	The DC group has significantly lower bothersomeness scores at 4 weeks compared to booklet group ($p = 0.02$) but significance decreases over time. No differences in bothersomeness between PT and booklet groups. No significant differences in Roland Disability scores.	DC and PT groups have similar outcomes that are marginally better than the minimal-intervention group. No significant differences among groups in the numbers of days of reduced activity or missed work or in recurrences of back pain.
Deyle et al. (2000)	MT, placebo	Osteoarthritis of the knee	RCT	83 patients in an outpatient physical-therapy practice of a large medical center	Distance walked in 6 minutes and the sum of the function, pain, and stiffness subscores (WOMAC™)	8 treatments over 4 weeks	At 4 weeks, distance walked by the treatment group improves 12.3% ($p < 0.05$) and 13.1% at 8 weeks ($p < 0.05$). Placebo group does not improve ($p > 0.05$). Avg. WOMAC scores 51.8% lower in the treatment group at 4 weeks ($p < 0.05$) and 55.8% lower at 8 weeks ($p < 0.05$). Scores not significantly lower in the placebo group.	Patients receiving MT show clinically significant improvements in distance walked and WOMAC scores at 4 and 8 weeks after treatment, whereas the comparison group show no improvement.
Hoving et al. (2002)	MT, PT, GP	Neck pain	RCT	183 patients in an outpatient care setting in the Netherlands	Patient-reported success, physical dysfunction, pain intensity, and disability	6 MT, 12 PT, or continued GP care over 6 weeks		At 7 weeks, success rates are 68.3% for MT, 50.8% for PT, and 35.9% for GP. MT score consistently better than PT and continued care on most outcomes.

Chiropractic and Physical Therapy Effectiveness Studies, continued

Study	Treatment	Injury	Study Type	Data and Sample Size	Outcome Measure(s)	Duration and No. of Treatments	Incremental Results	Results
Pengel et al. (2007)	PT, advice, PT + advice	Subacute LBP (>6 weeks and <3 months in duration)	RCT	259 persons at 7 university hospitals and primary care clinics in Australia and New Zealand	Primary outcomes are average pain over the past week, function, and global perceived effect at 6 weeks and 12 months.	12 PT or sham exercise sessions and 3 PT advice or sham advice sessions over 6 weeks.		PT + advice has larger significant effects on all outcomes at 6 weeks (effect on pain, –1.5 points, p = 0.001, with similar effects for other primary outcomes). At 12 months, the only significant effect is for function (1.1 points, p = 0.005).
Luijsterburg et al. (2007)	PT and GP	Sciatica	RCT	135 patients in participating GP offices in Rotterdam	Global perceived effect, quality of life, direct and indirect costs, return to work	Maximum of 9 treatments or consultations in 6 weeks	Relative risk ratios of GP+PT care to GP care only at 3, 6, 12, and 52 weeks are 1.4, 1.3, 1.1, and 1.4, respectively. Patient utility at each follow-up for both groups is not statistically different.	Significant difference in perceived recovery at 1-year follow-up in favor of the PT group. No significant difference in quality of life or return to work. PT treatment is not more cost-effective than GP.
Niemistö et al. (2005)	MT + stabilizing + GP, GP	Chronic LBP (Oswestry Disability Index >15%)	RCT	204	Pain, disability, health-related quality of life, satisfaction with care, and costs	4 sessions of MT + stabilizing exercises	Significant improvement occurs in both groups. Treatment group shows a slightly more significant reduction in pain (p = 0.01) and significantly higher patient satisfaction (p = 0.001).	GP alone is more cost-effective for both health care use and work absenteeism and leads to equal improvement in disability and health-related quality of life.

Chiropractic and Physical Therapy Effectiveness Studies, continued

Study	Treatment	Injury	Study Type	Data and Sample Size	Outcome Measure(s)	Duration and No. of Treatments	Incremental Results	Results
Hurwitz, Morgenstern, Harber et al. (2002)	GP, GP + PT, DC (with and without physical modalities)	LBP	RCT	681 participants in a large managed care facility from 1995 to 1998	Average and most severe LBP intensity in the past week and low back–related disability	From baseline to 6 months, mean DC visits are 5.7(3.3) with a range of 0–21 and median of 5. For the GP+PT group, the mean is 5.4(4.6) with a range of 1–21 and median of 4.	Mean differences in most severe pain, average pain, and disability score are measured at 0–2-week, 0–6-week, and 0–6-month intervals. Most differences are statistically insignificant. There is a significant improvement in disability for PT patients compared to GP.	After 6 months of follow-up, DC and GP for LBP are comparable in their effectiveness. PT is marginally more effective than GP for reducing disability, but the possible benefit is small.
Goldby et al. (2006)	PT (manual therapy or spinal stabilization), minimal intervention	Chronic LBP (current episode >12 weeks)	RCT	346	Intensity of LBP, disability, handicap, medication, and quality of life	A maximum of 10 spinal-stabilization treatments, 10 MT interventions, or an education booklet	Significant benefits of spinal manipulation in pain reduction at 6 mo. ($p = 0.009$); however, this benefit is not significant at 12 months.	The spinal-stabilization group experiences a significant reduction in Oswestry Disability Index scores (38.8%) ($p = 0.025$).
Torstensen et al. (1998)	Medical exercise therapy, PT, and self-exercise by walking	Chronic LBP or radicular pain (>8 wks, < 52 wks)	RCT	208	Pain intensity, functional ability, patient satisfaction, return to work, number of days on sick leave, and costs	36 treatments over 3 months	No difference is observed between the medical exercise therapy and PT groups, but both are significantly better than the self-exercise group. Patient satisfaction is highest for medical exercise. Return to work rates are equal for all 3 groups 15 months after the therapy started.	Return to work rates are similar for the 3 groups. The PT groups have fewer days on sick leave than the self-exercise group during the treatment and follow-up period. PT groups are cost-effective.

Chiropractic and Physical Therapy Effectiveness Studies, continued

Study	Treatment	Injury	Study Type	Data and Sample Size	Outcome Measure(s)	Duration and No. of Treatments	Incremental Results	Results
Carey, Garett, et al. (1995)	GP (HMO & primary care), DC, orthopedic surgeons	Acute LBP	Observational	1,633 patients enrolled with 286 practitioners in North Carolina	Use of health care services, functional status, costs, and return to work	Patients visiting a DC have the highest number of visits (13.2 for urban and 9.0 for rural) compared to visits to other providers ($p < 0.001$).	Within 25 days after initial visit, the proportion of patients with functional impairment reduces from almost 100% to roughly 20%, but there are no significant differences between the groups.	Time to functional recovery, return to work, and complete recovery from LBP are similar for all groups. Mean total estimated outpatient charges are highest for orthopedic surgeons and DCs. Satisfaction is greatest among the DC group.
Haas et al. (2004)	DC, GP	Acute and chronic ambulatory LBP	Observational	2,870 from 51 DC and 14 GP community clinics	Pain and functional disability		Pain and disability scores are significantly better for DC patients at 2 weeks and 1-, 3-, 6-, and 12-month measurements ($p < 0.001$). No significant differences are observed at 24-, 36-, or 48-month follow-ups.	DC patients experience significantly better outcomes in pain and disability, but most improvement are seen by 3 months and sustained for 1 year; any advantages are insignificant thereafter.

Chiropractic and Physical Therapy Effectiveness Studies, continued

Study	Treatment	Injury	Study Type	Data and Sample Size	Outcome Measure(s)	Duration and No. of Treatments	Incremental Results	Results
Wasiak, Kim, and Pransky (2007)	DC initiated and concluded within 30 days of beginning of episode, DC initiated within 30 days but not concluded within 30 days, DC initiated more than 30 days from beginning	Work-related LBP	Observational	6,019 patients from a workers' compensation carrier with a 10% share of the U.S. private market (Fla., Ill., N.H., Pa.)	Return to work	Mean(SD) visits for total sample is 15.5(20.1). Mean for all cases and those with lost work time are: 5.5(5.9) and 7.1(7.9) for group 1, 23.4(21.3) and 28.4(25.8) for group 2, and 21.0(30.3) and 23.8(35.6) for group 3.		DC is initiated within 30 days after the onset of OBLP by 89% of claimants. Of those claimants, 48% ended DC within the first 30 days. Shorter DC duration is significantly associated with a lower likelihood of work-disability recurrence (OR = 0.39) and 8.6% shorter work-disability duration.
Walker et al. (2008)	MT, minimal intervention	Neck pain	RCT	94 patients referred to 3 physical-therapy clinics	Neck disability indexes, pain visual analog scales, patient-perceived global rating of change, treatment success rates, and post-treatment health care utilization	Twice weekly for up to 3 weeks	Both groups experience significant decreases in neck pain disability index, cervical VAS pain scores, and upper-extremity VAS pain scores from baseline to 1 year measured at 3 weeks, 6 weeks, and 1 year. The MT group mean is significantly lower for the neck pain index at each measurement and cervical VAS at 3 and 6 weeks but not significant one year after treatment. 62% of therapy and 32% of intervention patients perceive treatment success (p = 0.004).	Significant improvements were found in short-term pain, disability, and patient-perceived recovery with the use of MT for mechanical neck pain.

Chiropractic and Physical Therapy Effectiveness Studies, continued

Study	Treatment	Injury	Study Type	Data and Sample Size	Outcome Measure(s)	Duration and No. of Treatments	Incremental Results	Results
Zigenfus et al. (2000)	PT started at either the day of or first day after injury, 2–7 days after injury, or >7 days after injury.	Acute LBP	Observational	3,867 patients randomly selected from the database of a large occupational health care provider	Physician visits, case duration, duration of restricted work, and days away from work	Average number of treatments for the early treatment group is 3.1 and significantly lower than both other groups.		The early-intervention group has significantly fewer treatments, fewer restricted workdays, fewer days away from work, and shorter case duration than the other groups.
Koes et al. (1992)	MT, PT, GP	Back and neck complaints (>6 weeks)	RCT	256	Severity of main complaint, global perceived effect, pain, and functional status	3 months; no. of treatments is unclear.	Statistically significant improvement of main complaint at 3 weeks for MT compared to GP, and significant improvement for PT compared to GP. No significant differences at 12 weeks. Differences in global perceived effects are significant between MT and GP and between PT and GP at each interval up to 12 weeks.	PT and MT decrease the severity of complaints and have a higher global perceived effect compared to GP. There are no significant differences between MT and PT.
Meade et al. (1990)	DC, GP	LBP	RCT	741	Oswestry pain, disability, results of tests of straight leg raising and lumbar flexion	Maximum of 10 DC treatments over 3 months (may extend to 1 year). Mean(SD) for DC group is 9.1(3.6) , GP is 6.3(4.8). Mean difference is significant ($p < 0.001$).	Differences in changes in Oswestry score between DC and GP groups are not significant at 6 weeks, are marginally significant at 6 months (3% difference, $p < 0.05$), not significant at 1 year, and significant at 2 years (7%, $p < 0.01$).	Marginally beneficial effects of DC compared to GP in patients with LBP.

Chiropractic and Physical Therapy Effectiveness Studies, continued

Study	Treatment	Injury	Study Type	Data and Sample Size	Outcome Measure(s)	Duration and No. of Treatments	Incremental Results	Results
William Johnson, Baldwin, and Butler (1999)	DC, GP	Back pain	Observational	850	Costs, patterns of care, and return to work	Mean nos. of office visits for medical only, temporary, and PPD cases for GP are 4.6, 5.2, and 5.9, respectively. For DC, means are 11.4, 11.5, and 11.5, respectively.		Controlling for unobserved heterogeneity between DC and GP groups yields insignificant differences in outcomes between patients treated by DC or GP.
Shekelle, Markovich, and Louie (1995a)	DC, PT, GP	Back pain	Observational	686	Costs, utilization			Chiropractors have significantly greater mean number of visits per episode than other providers. DC has the highest mean outpatient cost per episode, and GP has the lowest.
M. R. Johnson, Schultz, and Ferguson (1989)	DC, GP, OP	Work-related sprains and strains	Observational	862	Costs, return to work			Those who receive DC care have fewer lost workdays. Median provider cost is highest for DC; mean is highest for GP.

III. Estimating the Impact of Workplace Exposure to Hazards on Disease Incidence, Prevalence, and Medical Costs

Abstract

This paper estimates the impact of exposure to occupational hazards on disease rates and costs among the elderly. It finds that disease prevalence is significantly higher for workers exposed to occupational hazards, and the effect is concentrated among workers with long-term exposures. Estimates of new incidence of disease, which reduces unobserved heterogeneity in self-reported health status, are less significant and of smaller magnitudes compared to disease prevalence estimates. Population attributable risks of occupational hazard exposures for lung disease and cancer are estimated to be 11 and 10 percent, respectively. Back-of-the-envelope estimates of total costs of disease attributable to workplace exposure to hazards suggest that occupational disease is a significant public health concern.

Keywords: occupational illness, occupational hazards, work-related illness rates, duration models, population attributable risks, costs of occupational illness

Introduction

The primary goal of occupational health and safety is to achieve socially optimal safety levels and ensure workers are adequately and equitably compensated in the event of an injury or illness. However, occupational illness poses a number of unique policy challenges for both safety and compensation. There are often long latency periods after exposure until an illness manifests and many illnesses have multiple potential causes which make it difficult to establish whether the illness is work-related. Evidence suggests that workers' compensation misses the vast majority of work-related illnesses which results in significant cost-shifting and leads to sub-optimal safety levels and inadequate compensation (Leigh and Robbins, 2004; Biddle et al., 1998; Viscusi, 1984).

Studies have investigated the impact of occupational exposures to hazards on chronic disease however the full impact is largely unknown. This is primarily because no national occupational disease mortality surveillance system exists in the U.S., so any estimates of the magnitude of occupational disease must be generated from a number of different data sources and published epidemiologic studies (Steenland et al., 2003). Additionally, many primary care providers are not trained in occupational medicine and may not recognize a disease as being occupationally related which leads to underreporting (Azaroff et al, 2002; Ruser, 2008). The most recent comprehensive estimate of the number and costs of work-related injuries and illnesses are provided by Leigh (2011) who use data from a variety of sources and estimate the total costs for fatal and nonfatal occupational disease to be approximately $58 billion in 2007. However, there is a lack of research focusing on occupational causality and little information is available on the numbers of newly diagnosed diseases and medical costs (Schulte, 2005; Leigh, 2011).

This paper uses individual-level data from the Health and Retirement Study (HRS) to evaluate the impact of various occupational hazard exposures on prevalence and incidence of six diseases while controlling for a number of potentially confounding socio-economic factors. A number of empirical methods are employed including logistic regression and duration analyses. Population attributable risks (PARs) are constructed using relative risk estimates and the population prevalence of exposure in the data. Back-of-the-envelope estimates of the economic burden of occupational exposure to hazards are constructed using the estimated PARs and disease cost estimates from the literature.

The benefits of this paper stem from the richness of the data and the empirical methods. To the author's knowledge, this is the first paper to use the HRS to estimate the aggregate impact of exposure to occupational hazards on disease risk and, because the HRS is a representative sample of individuals over 51 years old in the U.S., this paper does not have to rely on various data sources like most other papers on this topic.[48] Concerns over data censoring and duration dependence are addressed by using a duration model regression approach. This paper estimates new incidence of disease to minimize unobserved heterogeneity in self-reported health status as a result of the left-censoring of the data. Additionally, a "control" disease is included in the analysis as Seabury et al. (2005) did to identify the magnitude of the potential sample selection bias and evaluate the reliability of the new incidence of disease estimates.

[48] Seabury et al. (2005) use the HRS to estimate the interaction between public and private factors on disease risk, specifically between smoking habits and exposure to asbestos.

This paper finds that disease prevalence in the first wave of the HRS is significantly higher for workers exposed to occupational hazards, and the effect is concentrated among workers with long-term exposures. Relative risk ratios are estimated from a low of about 1.3 for heart disease and cancer to a high of about 2.1 for lung disease. For new incidence of disease among workers with no disease at baseline, this paper finds that exposure is only significantly associated with lung disease and cancer, and the estimates are of a smaller magnitude compared to prevalence at baseline. Estimated PARs for lung disease and cancer fall in line with previous research at 11 and 10 percent, respectively. Back-of-the-envelope estimates of total costs of disease attributable to workplace exposure to hazards (direct and indirect) are roughly \$17.9 billion for lung disease (no asthma), and \$18.5 billion for cancer.

In the next section, the study population and methodological approach is discussed. Section III describes the results of our analyses. Section IV discusses the findings, how they fit in with the broader research body, policy implications, and limitations.

Methods

Study Population

The study population comes from the HRS which, as Seabury et al. (2005) describes, is a nationally representative panel sponsored by the National Institute of Aging and conducted by the Institute for Social Research at the University of Michigan. The Study targets individuals (and their spouses) aged 51-61 at the time of the first wave (1992), and provides information on health and retirement issues for the older community-dwelling population. Follow-up surveys were conducted biennially after

1992. The survey over-sampled Blacks and Hispanics, and includes weights that can be used to make it nationally representative for the 48 contiguous states.

The data used in this paper includes 9 separate waves, spanning 18 years from the initial survey. In the first wave, there are a series of occupational health and safety questions asked including the following:

> "Individuals are sometimes exposed to dangerous chemicals or other hazards at work. Have you ever had to breathe any kinds of dusts, fumes, or vapors, or been exposed to organic solvents or pesticides at work?"

If the individual responded affirmatively to that question, follow up questions were asked regarding the nature and duration of the exposure. The full list of workplace exposures are listed in Table 17 which are discussed in detail in the summary statistics section.

This paper evaluates the impact of exposure to each type of hazard, as well as the aggregated "any exposure to hazards" measure, on the relative risk of acquiring one of six separate diseases: lung disease (such as chronic bronchitis or emphysema, not including asthma), cancer or a malignant tumor of any kind except skin cancer, heart disease (heart attack, coronary heart disease, angina, congestive heart failure, or other heart problems), arthritis (including rheumatism), stroke or transient ischemic attack (TIA), and psychological problems (emotional, nervous, or psychiatric problems).

Exposure to hazards may increase the likelihood of lung disease, cancer, and possibly heart disease, stroke, and psychological problems. However, it is unlikely that breathing fumes or exposure to other hazards in the workplace increases the likelihood of developing arthritis. In this sense, arthritis is included as a robustness check to assess whether the estimates are being driven by correlation with unobserved variables.

Seabury et al. (2005) also includes arthritis in their analysis and finds that it is significantly associated with exposures to hazards in the workplace which suggests that there are sample-selection bias problems. Specific steps are taken to minimize unobserved heterogeneity in the analysis and are discussed in the next section.[49]

Statistical Methods

To identify the prevalence of disease by exposure status at baseline, various logistic regression models of the form below are estimated:

(8) $$\Pr(d_{it}) = (1 + \exp(-x\beta))^{-1}$$

where

(9) $$x\beta = \beta_0 + \beta_1\varepsilon_i + \beta_2 s_{it} + \beta_3 x'_{it} + \beta_4 h_{it}.$$

As written above, $d_{it} =$ {lung disease, cancer, heart disease, arthritis, stroke, psychological problem}, which are each indicator variables equal to one if respondent i has the specified disease in time t. The first term, ε_i, is the primary variable of interest and equals one if respondent i reports that they were exposed to workplace hazards as of the first wave of the study. In this analysis, ε_i is broadly estimated as any exposure to hazards in the workplace, and the analysis is expanded by identifying respondents with exposure to hazards in the workplace for one year or less, and those with exposure to hazards for more than one year. Additionally, the impact of each specific exposure type as listed in Table 17 is evaluated.

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[49] It is possible that workers who are more likely to be exposed to occupational hazards are also more likely to work in physically demanding jobs that would increase the chance of developing arthritis. This issue is explored by controlling for the physical demands of each worker's job. However, in this analysis it is only possible to control for this job characteristic for individuals who were working at the time of the survey, and only for their current job.

The indicator s_i is equal to one if a given respondent smokes however and this is also broken into two categories: individuals who are smoking at the time of the survey and individuals who had ever smoked but are not currently smoking at the time of the survey. Additionally, x'_{it} is a vector of socio-economic characteristics for the ith person in wave t that includes age, gender, race, body mass index, industry worked in longest, and highest educational attainment. Finally, h_{it} is a set of five dummy variables recording self-reported health status.[50]

There are additional variables describing job characteristics for respondents who are working at the time of the survey. Two of these variables identify whether the respondents' current job requires physical effort or is high stress. These variables are included in a separate set of regressions that limits the sample to respondents who are working at baseline (because the questions are only asked of respondents who are working at baseline).

While this paper controls for a number of potentially confounding factors that may influence disease prevalence, there are still concerns that the estimates may be biased due to unobserved correlations with exposure status. The primary concern is that there are unobserved differences in health characteristics between individuals by exposure to occupational hazards. Self-reported health status is included in the regressions but the data is left-censored – meaning nothing is observed prior to the start of the survey, so the previous health status for each respondent is unclear. Additionally, most individuals with a disease at baseline report a "poor" health status, rendering the variable largely ineffective in controlling for differences in health status.

[50] The five possible choices are: excellent, very good, good, fair, or poor

To address this concern this paper estimates new incidence of disease for respondents who do not have a disease at baseline. This step provides two benefits: 1) it provides estimates of new disease incidence among the elderly by occupational hazard exposure status and 2) it improves the reliability of the self-reported health variable. Since new incidence is estimated over 9 waves, or 18 years, taking into account duration dependence is necessary. To do this a series of logistic duration models are estimated to identify the instantaneous probability of developing a disease in wave $t+\Delta t$ given that the respondent does not have a disease in wave t. The models estimated take the form:

(10) $$(\Pr(d_j(t+\Delta t))=1 \mid \Pr(d_j(t))=0)=(1+\exp(-x\beta))^{-1}.$$

The dependent variable is a measure of the conditional probability of acquiring disease j in wave $t+\Delta t$ given that the respondent has not acquired a disease up to that point. The term $x'\beta$ represents the same vector of explanatory variables as described above, however the natural log of time (wave) is included as the baseline hazard function.

Next, using the estimates from equations (8) and (10), PARs are calculated for each disease as a result of exposure to occupational hazards. PAR methodology is commonly used in epidemiological studies and not only for studies estimating the impact of occupational exposures. The PAR (also known as attributable fractions (AF)) represents the "fraction of disease in a population that might be avoided by reducing or eliminating exposure to an etiologic agent, provided that it is causative" (Coughlin, Benichou, and Weed 1994, p. 51). The PAR calculation is shown below:

$$PAR = \frac{P(RR-1)}{1+P(RR-1)}. \qquad \textbf{(11)}$$

For the purposes of this paper, P is equal to the proportion of individuals exposed to hazards in the workplace and RR is the relative risk ratio of acquiring a disease based on occupational hazard exposure status. The interpretation of the PAR for occupational exposure to hazards on a specific disease is: if exposure to hazards is eliminated, the prevalence of that disease will decline by the same percentage. For example, if the estimated PAR for occupational exposure is 50 percent for lung disease, then the prevalence of lung disease will decline by 50 percent if that population is not exposed to occupational hazards.

The final step taken in this analysis is to extrapolate an estimate of the burden of occupational exposures to hazards on total disease costs. To accomplish this, PAR estimates for each disease are multiplied by total disease cost estimates. The cost estimates used, which include direct and indirect costs, come from the National Institute of Health (2009) and the American Cancer Society (2013).

Results

Summary Statistics

Table 16 reports summary statistics using weights reflecting the complex survey design of the HRS. A total of 8,841 workers are included in the study, roughly 40 percent report being exposed to some type of occupational hazard, and about 33 percent report being exposed to a hazard for more than one year. The majority of the sample is white, roughly 20 percent have a college degree, and about 30 percent report smoking at the time of the study. Arthritis is by far the most prevalent disease at baseline affecting roughly one-third of all respondents and heart disease is the second most common.

The table also breaks down summary statistics by exposure type and there are noticeable differences between the two groups. A larger portion of workers exposed to hazards in the workplace are male, fewer graduated from college, and a higher proportion has either smoked or is currently smoking at the time of the study. Disease prevalence at baseline is generally higher for workers exposed to hazards and overall health status is worse. Even when only comparing individuals with no disease at baseline, exposed workers have a lower overall health status. The biggest differences in industries worked in longest by exposure status could be summarized by saying that exposed workers are more likely to be in "blue-collar" type jobs, whereas those not exposed are more likely to be in "white-collar" jobs.

Table 17 reports the types of hazardous materials respondents report being exposed to in the workplace for more than one year. The most common exposure is to solvents with almost one-third of the sample reporting exposure. Exposure to fumes and dust are the second most common at about 18 percent, while asbestos and agricultural hazard exposures account for about 10 percent of exposures each. Petroleum products, biohazards, and inorganics account for about 7 percent of all exposures, each, while drugs only account for about 1 percent. Ten percent report being exposed to some other type of hazard not listed in the table.

Baseline Prevalence

Table 18 reports the estimated odds ratios of baseline prevalence of disease from logistic regression models taking into account the sampling design of the HRS. Each column in each panel represents a separate regression. Each model is estimated with any type of exposure, and then separating the types of exposures to less than one year or at least one year. Both models are estimated for each of the six dependent variables which are listed at the top of the table. Controls for respondents' age, race, gender, industry worked in the longest, body mass index, education level, and smoking history are included. Only estimates for exposure and smoking status are reported for brevity and as a comparison. The top panel reports estimated odds ratios for all respondents and the bottom panel for respondents working in wave 1.

Starting with the top panel, there are two noticeable trends. First, the estimated odds ratios are significant on most of the exposure variables and indicate higher relative disease prevalence for workers exposed to occupational hazards. Second, it appears that the effect is driven by workers with long-term exposures to hazards. Given that the majority of workers exposed to hazards in the workplace report exposure durations of more than one year, the insignificant finding for individuals with short-term exposure could be driven by a low sample size. The estimated odds ratios for workers exposed for more than one year range from a low of about 1.3 for heart disease and cancer, to a high of 2.07 for lung disease. The estimates for exposure to hazards are roughly of the same magnitude as for smoking except when looking at the relative odds ratios for lung disease for current smokers which is almost 3.7 times higher.

The bottom panel reports estimated odds ratios when the sample is restricted to respondents working in wave 1. This allows for two additional independent variables to be included indicating whether the respondents' job requires physical effort[51] and

[51] The "physical effort" variable is constructed using three of the HRS variables: "RwJPHYS," "RwJLIFT," and "RwJSTOOP." These variables indicate whether the respondents' job requires lots of physical effort,

whether it is high stress.[52] The estimated odds ratios for long and short term exposure are roughly of the same magnitude or less than for the entire sample. There is also less significance among some of the ratios for cancer, heart disease, stroke, and psychological problems. The significance and magnitude of the estimated odds ratios for lung disease and arthritis are roughly the same when compared to the whole sample. Respondents working in a job that requires physical effort have a significantly lower risk of heart disease and a higher risk of arthritis. Respondents in a high stress job have a significantly higher risk of heart disease and arthritis. The estimated odds ratios on the smoking variables are very similar when compared to the entire sample.

New Incidence

Table 19 reports the estimated hazard ratios from logistic duration regression models. The format of this table is similar to the previous table with each column in each panel representing a separate regression. The same control variables are included as in the estimates of baseline disease prevalence however these regressions are estimating new incidence of disease over 9 waves (18 years) for respondents who did not have a disease reported at baseline.

Starting with the estimated hazard ratios for the entire sample, the most obvious difference compared to baseline prevalence is that the ratios are smaller and less significant. For instance, the estimated hazard ratio for new incidence of lung disease for workers with long-term exposure is about 1.4 times higher compared to workers without

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lifting heavy loads, or stooping, kneeling or crouching, respectively. The "physical effort" variable is an indicator equal to one if a respondent indicates that his/her job requires at least one of these physical demands all or almost all of the time.

[52] The "high stress" variable is constructed using the HRS variable "RwJSTRES." The"high stress" variable is equal to one if the respondent "strongly agreed" with the statement that his/her job involves lots of stress.

exposure, whereas this ratio is more than 2 times higher at baseline. The only other significant estimates of new incidence of disease by exposure type are for cancer and psychological problems. The estimates of new incidence for heart disease, arthritis, and stroke are not statistically different from zero.

It is particularly interesting that the estimated hazard ratio for new incidence of arthritis is not significant for workers by exposure type. In the previous table, the impact of exposures to hazards on arthritis prevalence was significant, however it isn't clear that exposure to hazards should lead to higher rates of arthritis. The significant finding for baseline prevalence suggests that there is a sample selection problem as Seabury et al. (2005) concludes using a similar sample of the HRS. Unless of course, workers who are exposed to occupational hazards are more likely to develop arthritis because they work in more physically demanding jobs. But, the impact on new incidence of arthritis is much lower in magnitude compared to our baseline prevalence estimates and it is not significant suggesting that this paper has done a better job in controlling for differences in worker types by exposure.

The fact that the impact of exposure to hazards in the workplace is insignificant for many diseases when new incidence is evaluated provides some support that this approach is mitigating the sample selection problem. But, one concern could be that there are simply insufficient cases of new disease onset to detect a significant difference. To address this concern, new disease counts are compared to the number of diseases present at baseline. As Figure 6 shows, there are actually more new disease counts compared to the number of diseases present at baseline. This is most likely because the sample of respondents is getting older and individuals are simply more likely to develop some kind of condition.[53]

Prevalence and Incidence by Exposure Type

Table 20 reports both baseline prevalence and new incidence for each of the six diseases by the specific exposure type. Each column represents a separate regression and the control variables are as described in previous estimations. Starting on the left hand side of the table, asbestos has a strong and significant impact on prevalence and new incidence of lung disease as individuals exposed are roughly two times as likely to acquire the disease. Exposure to biohazards is the highest at baseline with an odds ratio of roughly 3.5, but the significance and magnitude disappear when looking at new incidence. Exposure to solvents is significant and positive for both baseline and new incidence at 1.7 and 1.4, respectively. The estimates for cancer are much less significant than they are for lung disease: there is virtually no significance across the different exposure types for baseline prevalence but greater significance for new incidence with exposures to solvents, petroleum, asbestos, and fumes all being significant and ranging from about 1.3 to 1.5.

Estimates for heart disease are similar to cancer in that there is sparse significance and none of the estimates for new incidence are significant. Baseline prevalence for arthritis is significant for all of the exposure types and there is a peculiar finding for individuals exposed to drugs who are estimated to have a much lower risk of having arthritis. This could have to do with some of the characteristics of the type of work

[53] Another aspect the figure highlights is that the total number of disease counts outnumbers the total number of respondents in the sample. This is because respondents can have more than one disease and a large majority of the respondents have arthritis. At baseline, roughly one-third of the sample has arthritis and by the end of the sample period that proportion increases to roughly two-thirds. Other cases of multiple diseases such as cancer and stroke are much less common in the data.

people do who are exposed to drugs, for instance if they do not have to do as many activities that may increase the chance of developing arthritis. It could also simply be due to chance.

When looking at new incidence of arthritis, all of the estimates lose significance except for exposure to petroleum which has a significant hazard ratio of 1.44. As is the case for the aggregate measures, this finding provides some evidence that the new incidence estimates suffer from less selection bias. It could also be that individuals exposed to petroleum are in typically more physically intensive jobs that lead to higher rates of arthritis.[54] Turning to far right portion of the table, exposures to fumes and solvents are associated with a higher baseline prevalence of stroke but the significance disappears when looking at new incidence.

Probability of Exiting the Sample by Exposure Type

Estimating the impact of exposure to hazards in the workplace on new incidence of disease could potentially be biased if sicker individuals "exit" the sample earlier (i.e. they contract the disease), leaving a healthier sample that may be over-represented. Using a duration model approach adjusts for this potential bias by estimating a baseline hazard and accounting for duration dependence.[55] However, this paper also estimates whether individuals who were exposed to hazardous chemicals are more or less likely to either die, exit the sample by simply not responding anymore, or both.

[54] While the results are not reported here, regressions limiting the sample to individuals working in wave 1 were also run. The significance of the new incidence estimates do not change, the hazard ratio for workers exposed to petroleum is roughly 1.5 and significant, and the physical effort variable is also significant and positive at roughly 1.2 indicating that respondents working in jobs that require physical effort have a higher hazard rate for arthritis.

[55] For instance, if individuals are more likely to exit the sample the longer they are in it, then there would be positive duration dependence, and if they are less likely to exit the more time goes, there would be negative duration dependence.

Table 21 shows the results of these estimations. The table is set up as in previous tables where each column in each panel represents a separate regression. Hazard ratio estimates are included for the entire sample and for only those with no disease at baseline. Starting with the top panel, there is sparse significance on the estimated hazard ratios by exposure suggesting that respondents did not die at differential rates. There is a significant estimate, where respondents with short-term exposure to hazards are actually less likely to die in a given wave. This is a slightly surprising finding but only significant at the ten percent level. Not surprising, smokers, both current and those who have ever smoked, have a higher hazard rate of dying in a given wave. When the dependent variable is "exiting the sample," there are no significant findings, even differences among smokers. When looking at "die or exit sample," respondents with short-term exposure have a lower hazard ratio compared to those with no exposure, and this finding is likely driven by the significant finding when "die" is the dependent variable. When the sample is limited to those with no disease at baseline, the findings do not change dramatically.

Population Attributable Risks

The next step taken is to use the estimated relative risk ratios along with the sample population prevalence of exposure to occupational hazards in the HRS to calculate the PARs as described earlier. Table 22 lists the PARs using the relative risks at baseline and new incidence to provide a range of estimates. Significance of the PAR estimates is indicated based on the significance of the respective odds ratios and hazard ratios. As was the case with the significance of the estimates, most of the baseline prevalence estimates are significant however significant differences in new incidence are only estimated for lung disease, cancer, and psychological problems.

The largest estimated PAR is for lung disease, with a range of 12 to 26 percent. This is of a similar magnitude as the estimated PAR for individuals in the sample who are ex-smokers. In comparison, the PAR for current smokers range from 45 to 56 percent. The estimated PARs for cancer are also significant with a range between 9 and 10 percent and for psychological problems the range is between 5 and 15 percent.

Using preferred PAR estimates (that rely on the relative risk of new incidence), the estimates fall in line with previous research. For instance, the PAR for lung disease, cancer, and psychological problems are 11, 10, and 7 percent, respectively. Steenland et al. (2003) reports PAR estimates of between 6 to 13 percent for lung cancer deaths, respectively, and Leigh et al (1997) estimate a PAR of 10 percent for chronic respiratory deaths. PAR estimates for COPD related deaths vary between 5 and 24 percent (Steenland et al., 2003; Leigh et al., 1997; Nurminen and Karjalainen, 2001). Cancer estimates vary from a low of about 4 percent (Doll and Peto, 1981; Steenland et al., 2003), to between 6 and 10 percent (Markowitz et al., 1989; Leigh et al. 1997; Nurminen and Karjalainen, 2001).

Costs Attributable to Workplace Exposure to Hazards

To estimate the cost attributable to workplace exposure to hazards, estimated PARs are multiplied by the total costs of the disease. Total disease cost estimates are obtained from the National Institute of Health (2009) and the American Cancer Society (2013). The total cost estimates from the NIH and ACS include both direct medical costs and indirect morbidity and mortality costs.[56] As is shown in Table 23, the total

[56] Costs for cancer, heart disease, lung disease, and stroke are included because these data are readily available.

estimated costs in the U.S. for the diseases listed range from a low of $71 billion for stroke up to $311 billion for heart disease.

The estimated PAR for any exposure at both baseline and new incidence for each of the four diseases listed in the table is used along with the estimated range of costs. In the far right-hand column of the table, total estimated costs adjusted for inflation to 2012 dollars are listed. As the table shows, the estimated cost attributable to workplace exposure to hazards for heart disease is as high as $25 billion, and as high as $12 billion for stroke. The range of costs for lung disease (not including asthma) is wide and between about $18 billion and $42 billion, with the lower cost estimate corresponding to the new incidence PAR estimate. For cancer, the range is much tighter, and the corresponding work-related costs are between $18.5 and $22 billion.

To put these costs into perspective, these estimates are compared to those obtained by Leigh (2011) who provides the most comprehensive estimates of occupational disease costs, counts, and deaths.[57] Leigh (2011) reports the total medical costs related to fatalities for diseases by category. He estimates total medical costs for deaths of respiratory diseases pneumoconiosis, chronic obstructive pulmonary disease (COPD), and pulmonary tuberculosis to be $4 billion and medical costs for coronary heart disease and cancer[58] to be $6 billion and $4 billion, respectively. The cost estimates in this paper are higher, but it is not surprising because the total disease costs used in this

[57] A significant portion of Leigh (2011) focuses on determining work-related disease costs whereas he uses the PAR estimates from the literature. The approach used in this paper differs in that the focus is on estimating PARs for different diseases as a result of occupational hazard exposures while total disease cost estimates are obtained from the literature.
[58] Leigh (2011) included: lung cancer, bladder cancer, mesothelioma, leukemia, laryngeal cancer, skin cancer, sinonasal cancer, nasopharynx cancer, kidney cancer, and liver cancer. This paper did not include skin cancer because that is not included in our HRS variable.

analysis come from the NIH and ACS which include direct medical costs and indirect costs for both fatal and non-fatal cases.

To make a more accurate, albeit loose, comparison with the cost estimates in this paper and those obtained by Leigh (2011), total direct and indirect costs for each specific disease are extrapolated using the relative proportions.[59] Using this approach, total costs (medical plus indirect) are extrapolated as \$15, \$15, and \$22 billion each for lung diseases, heart disease, and cancer, respectively (in 2012 inflation-adjusted dollars). These estimates fall fairly in-line with the estimates found in this paper. Leigh's estimates for the cost of lung disease are slightly lower than those in this paper but this could be because this paper includes more diseases in the "lung disease" category. His estimate for heart disease is within the range of estimates found in this paper, however as noted previously, the significance of work-related exposures on heart disease is not strong. Finally, his cost of cancer estimate is at the top of this paper's estimated range.

Discussion

This paper uses the HRS, a nationally representative dataset of individuals over the age of 51 that provides a wealth of information on workplace exposure to hazards, and a number of empirical approaches to identify the impact of workplace exposures to hazards on disease prevalence, incidence, and costs. It finds that prevalence of disease in

[59] For instance, in Leigh (2011), death related medical costs for diseases are \$17.66 billion out of a total of \$20.83 billion and therefore representing 85% of total medical costs. Additionally, total medical costs are \$20.83 billion and represent 36% of total costs of disease attributable to work-place factors (medical plus indirect) equal to \$57.8 billion. Using these proportions, total estimated costs are extrapolated as: $totalcost = \frac{totalmed}{0.36}$ where $totalmed = \frac{fatalmed}{0.85}$. Obviously this is a loose extrapolation that assumes that the relationship between medical costs for fatalities and non-fatalities are similar across diseases, and the relationship between medical costs and indirect costs is similar as well. While these assumptions are likely not to hold for precise estimates, this does allow for broad comparisons between the estimates found in this paper and those obtained by Leigh (2011).

the first wave of the HRS among workers exposed to hazards in the workplace is significantly higher than those not exposed, and the effect is strongest among workers with long-term exposures. Relative risk ratios are estimated from a low of about 1.3 for heart disease and cancer to a high of about 2.1 for lung disease.

Estimates of new incidence of disease for workers with no disease at baseline find that exposure is only significantly associated with lung disease and cancer, and the estimates are of a smaller magnitude compared to prevalence at baseline. Estimated PARs for lung disease and cancer fall in line with previous research at 11 and 10 percent, respectively. A back-of-the-envelope estimate of total costs of disease attributable to workplace exposure to hazards (direct and indirect) is roughly $17.9 billion for lung disease (no asthma), and $18.5 billion for cancer (no skin cancer).

While there are a number of benefits to this analysis, there are also a number of limitations that must be taken into consideration. First, the HRS records exposures to hazards in the workplace that happened many years ago. This allows for long-latency diseases to manifest, but the workplace has changed significantly since that time. There have been improvements in safety precautions, OSHA has increased regulations on various chemicals and toxins, and modern workers are likely to face different hazards in the workplace compared to those faced by respondents in the HRS. For instance, exposure to asbestos has been virtually eliminated in modern workplaces, but there are new hazards with unknown health risks, such as carbon nanotubes, being introduced into the workplace that cause concern (Donaldson et al., 2006).

Second, there are potentially unobservable differences in the types of people that choose to work in risky jobs that may influence our results. For instance, workers who choose to work in risky jobs may simply be more likely to contract a disease at some point in life because they make riskier private decisions or because they generally have worse health characteristics that the empirical models in this paper do not adequately control for (Seabury et al., 2005). This paper does a good job of accounting for potentially confounding socio-economic factors that other papers in this area have not accounted for, but there is still some doubt as to whether the findings are driven by selection bias. In the future, an instrumental variable approach would be beneficial.

The results from this paper, along with the results of previous research, suggest that occupational disease is a costly problem. However, it isn't necessarily obvious that government intervention is needed. Currently, the workers' compensation system covers occupational illness and provides diseased workers with full medical and partial indemnity benefits. OSHA regulations mandate minimum safety levels and, along with the experience rating of workers' compensation premiums, provide employers with incentives to reduce injuries and illnesses. This begs the next question: is there a market failure in the case of occupational disease?

Theoretically, in a perfectly competitive labor market, socially optimal levels of health risks and equitable compensation for diseased workers could be reached through compensating wage differentials.[60] In this ideal world, workers would be fully compensated for the risks they face and wage premiums would provide appropriate incentives for employers to invest in an efficient level of safety. But, in order for the labor market to generate wage premiums that fully compensate, workers must understand

[60] Health risks will be at an "efficient" level whenever the incremental cost of an extra health precaution is exactly offset by the social value of that precaution (Viscusi, 1984).

all the risks they face in all workplaces and have perfect job mobility (Hyatt and Thomason, 1998).

Given the lack of knowledge of the risks of occupational diseases and the long latency period between exposure to hazards and the resulting disease, it is unlikely compensating wage differentials and experience rating under workers' compensation will provide adequate incentives to prevent occupational diseases (Burton and Chelius, 1997).[61] In the case of these informational problems, it is likely that workers will demand lower wage premiums and less insurance than if there were no information problems, and employers will consequently have less incentive to invest in safety (Viscusi, 1984).

The empirical evidence on compensating wage premiums for occupational injuries and illnesses is mixed,[62] and Ehrenberg (1985) points out that estimates of

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[61] For instance, different demographic groups have different susceptibilities to disease (Viscusi, 1984), workers may be unaware that private health behaviors such as cigarette smoking can impact risk (Seabury et al., 2005***), and even if workers are informed, there is ample evidence suggesting that they are likely to miscalculate the odds of accidents or illnesses occurring and be unable to accurately monetize risks (see Pouliakas and Theodossiou (2013) for an extensive literature review on the subject).

[62] A number of studies focusing on occupational injury and fatality risk find evidence of compensating wage differentials (see Viscusi and Aldy, 2003). However, a number of studies do not find evidence or find that the estimates of wage differentials are highly dependent on the estimation approach (e.g. Black and Kniesner, 2003; Dorman and Hagstrom, 1998; Leigh, 1995, Jennings and Kinderman, 2003, Leigh, 1981).

Leigh (1995) and Dorman and Hagstrom (1998) provide evidence of the inter-industry wage differentials hypothesis which states that what often appears to be evidence of compensating wages is actually just industry wage premiums. But, Viscusi and Aldy (2003) argue that there is not strong enough evidence to believe the inter-industry hypothesis considering all the studies that have addressed this concern and still found risk premiums.

Three studies focus specifically on compensating wage differentials for occupational illness risk and they find evidence that wage premiums exist but additional studies are needed. Lott and Manning (2000) focus on cancer risk and find a statistically significant premium for exposure to cancer risk after controlling for injury risk. They also find that increased employer liability led to wage premiums being reduced by between 43 and 108 percent. More recently, Sandy and Elliott (2005) and Wei (2007) both investigate compensating wage differentials for occupational illnesses using data from the U.K. which incorporates a broader definition of occupational illness compared to the Lott and Manning (2000) study. Sandy and Elliott (2005) find evidence of significant compensating wage differentials for manual male workers only. Wei (2007) is the only study to construct establishment-specific risk rates as opposed to broad occupation or industry-based measures. He finds that the estimated wage compensation for one job-related illness episode per year ranges from 27% to 140% of annual earnings, depending on gender and compensating wage differentials may not be all that useful for public policy in the occupational safety area anyways because the existence of wage differentials does not imply that they fully compensate workers for the disutility associated with risk of injury.[63] Additionally, empirical evidence suggests that workers' compensation misses more than 90 percent of occupational illness deaths, over 80 percent of medical costs, and more than 92 percent of workers' compensation costs apply to injuries (Leigh and Robbins, 2004; Biddle et al., 1998), which implies that premiums do no fully reflect the costs of illness.[64] Many argue that an inefficiently high level of occupational illness is produced and the majority of the costs are passed-on to non-workers' compensation private medical insurance carriers, to Medicare, Medicaid, Social Security Disability Insurance, and to individual injured workers and their families (Leigh, 2011; Leigh and Robbins, 2004; Viscusi, 1984; Reville and Schoeni, 2004).

These shortcomings in workers' compensation and in the compensating wage differential hypothesis suggest that OSHA regulations can potentially make a major contribution in promoting workplace health (Burton and Chelius, 1997).[65] Further, the case for regulating occupational illness becomes stronger because of the complexity of occupational illness cases, fact finding requires expertise and incentives, and it is more likely that regulators have better information about inherent risks than private parties

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estimation methodology. He also finds no significant compensating wage differential for job-related non fatal injuries for both male and female workers, and no statistically significant difference between manual and non-manual male workers.

[63] He writes that, if labor markets are truly competitive, wage differentials must be fully compensating, but in the case of imperfect markets, identifying the fully compensating wage differential would be extremely difficult.

[64] There is evidence that improving experience rating of workers' compensation premiums reduces injuries (see e.g., Tompa et al., 2007; Seabury et al., 2012; Neuhauser et al., 2013).

[65] Burton and Chelius (1997, p. 283) also write that "given the evidence suggesting the relative ineffectiveness and infrequency of OSHA inspections that are largely devoted to finding violations of safety standards and the hostility of employers to these inspections, the promotion of safety can perhaps best be left to the economic incentives from workers' compensation and compensating wage differentials."

(Shleifer, 2010; Shavell, 1984). Empirically, the evidence suggests that OSHA inspections, citations, and penalties reduce injuries and exposures to toxic substances (Mendeloff, 2001), and there have been modest improvements in workplace health and safety in the U.S. as a result of OSHA regulations (Tompa et al., 2007). The World Health Organization (2009) goes as far as saying that "occupational cancers are almost entirely preventable through eliminating exposure, substituting safer materials, enclosing processes and ventilation."

There is a great deal of debate over whether OSHA-type regulations make US firms less competitive (i.e., does it kill jobs). Opponents of regulations would like to rely on private incentives to invest in workers safety (e.g., to avoid workers compensation costs), and cite evidence suggesting that OSHA inspections are infrequent, ineffective, and create hostilities with employers (Burton and Chelius, 1997). But, relying entirely on private incentives rests on the assumption that workers and firms have knowledge about and bear the full cost of adverse health outcomes. If employers have sub-optimal incentives to invest in protecting workers, pulling back regulations could lead to worse health outcomes. By demonstrating the increased risk of illness at advanced (including retirement) ages for workers, it suggests that we should be careful about pulling back on regulations or it could lead to significant increases in chronic diseases among the elderly and look to find synergies between private and public approaches to improve workplace health and safety.

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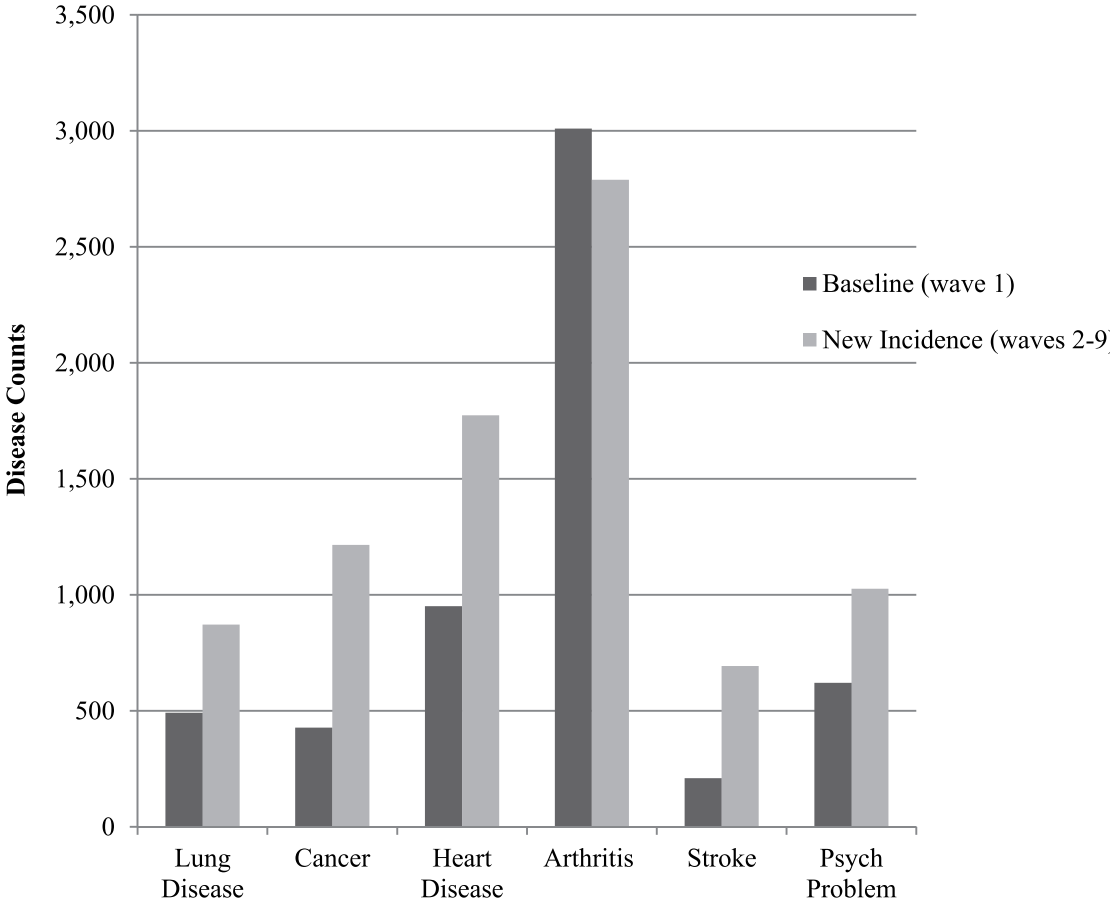
Figures

Figure 6. Disease Counts: Prevalence at Baseline and New Incidence



Tables

Table 16. Summary Statistics

	No Exposure to Hazards in Workplace	Any Exposure to Hazards in the Workplace	Total Sample
Demographic Characteristics	n = 5,399	n = 3,442	n = 8,841
Age	55.5	55.5	55.5
White	85.6	87.8	86.5
Female	59.9	32.5	49.1
College Grad	22.7	14.5	19.5
Ever Smoked	62.0	70.9	65.5
Current Smoker	28.4	33.3	30.3
Industry Worked in Longest	n = 5,399	n = 3,442	n = 8,841
Agriculture, forest, fishing	2.0	5.2	3.3
Mining and construction	4.3	9.0	6.1
Manufacturing: non-durable	6.0	9.9	7.5
Manufacturing: durable	7.5	16.2	10.9
Transportation	6.6	8.0	7.1
Wholesale	3.7	3.2	3.5
Retail	13.7	8.1	11.5
Finance, insurance, real estate	7.0	3.0	5.4
Business and repair services	5.0	5.1	5.0
Personal services	5.6	4.1	5.0
Entertainment, recreation	1.3	1.5	1.4
Professional and related services	27.9	16.2	23.4
Public administration	4.5	4.4	4.4
Other	5.1	6.4	5.6
Self-Reported Health Status (no disease in wave 1)	n = 3,044	n = 1,685	n = 4,725
Excellent	37.9	29.6	34.9
Very Good	34.8	33.9	34.5
Good	21.4	26.9	23.4
Fair	4.9	7.7	5.9
Poor	1.0	2.0	1.3
Job Characteristics (working in wave 1)	n= 4,011	n = 2,575	n = 6,586
Job requires physical effort	22.1	33.1	26.3
Job is high stress	18.4	21.0	19.4

Notes: Number of observations represents the number of observations in Wave 1 of the HRS. The total number of person-year observations in our data is 62,666. Means are calculated using weights reflecting the complex survey design of the HRS. All numbers reported are percentages except for age. Some categories within reported health status and industry worked in longest categories do not add to 100 percent due to rounding error.

Table 17. Types of Hazardous Materials Respondents Reported Being Exposed To

Exposure Type	Number	Percent
Solvents	832	29.4
Petroleum Products	202	7.1
Asbestos	293	10.3
Fumes and Dust	506	17.9
Biohazards	191	6.7
Inorganics	199	7.0
Agricultural	296	10.4
Drugs and Explosives	20	0.7
Other	295	10.4
Total	2,834	99.9

Notes: The table reports the frequency and percent of the types of exposures to hazardous materials that workers are exposed to while working, if they are exposed at all. In wave 1, the HRS includes a question asking workers if they have been exposed to any dangerous chemicals or other hazards at work. If the respondent answered "yes," follow up questions were asked to determine the specific type of exposure.

Table 18. Baseline Prevalence of Disease

	Lung Disease		Cancer		Heart Disease		Arthritis		Stroke		Psych Problem	
Total Sample												
Any Exposure	1.89***		1.24*		1.21**		1.44***		1.50**		1.56***	
	(0.000)		(0.074)		(0.019)		(0.000)		(0.014)		(0.000)	
Long-term		2.07***		1.30**		1.28***		1.45***		1.56***		1.54***
Exposure		(0.000)		(0.043)		(0.003)		(0.000)		(0.009)		(0.000)
Short-term		1.09		1.00		0.87		1.36***		1.24		1.64***
Exposure		(0.707)		(0.987)		(0.403)		(0.003)		(0.504)		(0.006)
Ever Smoked	2.09***	2.09***	1.21	1.21	1.44***	1.44***	1.20***	1.20***	1.16	1.16	1.17	1.17
	(0.000)	(0.000)	(0.143)	(0.145)	(0.000)	(0.000)	(0.003)	(0.003)	(0.454)	(0.450)	(0.193)	(0.192)
Currently Smoke	3.70***	3.72***	1.19	1.19	1.48***	1.48***	1.31***	1.31***	1.17	1.17	1.73***	1.73***
	(0.000)	(0.000)	(0.194)	(0.190)	(0.000)	(0.000)	(0.000)	(0.000)	(0.437)	(0.429)	(0.000)	(0.000)
N	8,841	8,841	8,841	8,841	8,841	8,841	8,841	8,841	8,841	8,841	8,841	8,841
Working in Wave 1												
Any Exposure	2.01***		0.96		1.12		1.37***		1.37		1.37**	
	(0.000)		(0.787)		(0.273)		(0.000)		(0.210)		(0.023)	
Long-term		2.19***		0.96		1.13		1.38***		1.41		1.29*
Exposure		(0.000)		(0.791)		(0.274)		(0.000)		(0.180)		(0.083)
Short-term		1.20		0.97		1.09		1.37***		1.16		1.75**
Exposure		(0.536)		(0.913)		(0.645)		(0.008)		(0.745)		(0.021)
Job Requires	0.80	0.79	1.10	1.10	0.75**	0.75**	1.14*	1.14*	0.83	0.83	0.89	0.90
Physical Effort	(0.143)	(0.139)	(0.510)	(0.510)	(0.019)	(0.019)	(0.060)	(0.060)	(0.543)	(0.540)	(0.484)	(0.499)
Job is High Stress	1.14	1.12	1.05	1.05	1.44***	1.44***	1.17**	1.17**	0.73	0.73	1.85***	1.86***
	(0.434)	(0.481)	(0.771)	(0.769)	(0.002)	(0.002)	(0.037)	(0.037)	(0.363)	(0.356)	(0.000)	(0.000)
Ever Smoked	1.72***	1.73***	1.26	1.26	1.29**	1.29**	1.21***	1.21***	1.03	1.03	1.23	1.23
	(0.007)	(0.007)	(0.146)	(0.146)	(0.036)	(0.036)	(0.008)	(0.008)	(0.914)	(0.911)	(0.231)	(0.226)
Currently Smoke	3.24***	3.26***	1.18	1.18	1.25*	1.25*	1.24***	1.24***	0.75	0.75	1.48**	1.48**
	(0.000)	(0.000)	(0.318)	(0.320)	(0.092)	(0.092)	(0.005)	(0.005)	(0.372)	(0.374)	(0.018)	(0.019)
N	6,586	6,586	6,586	6,586	6,586	6,586	6,586	6,586	6,586	6,586	6,586	6,586

Notes: The table reports estimated odds ratios from logistic regression models taking into account the sampling design of the HRS with p-values in parentheses. Each column in each panel represents a separate regression. The dependent variables are listed at the top of the table. Controls for respondents' age, race, gender, industry for which they worked the longest, body mass index, and education level are included. The top panel reports estimated odds ratios for all respondents and the bottom panel for respondents working in wave 1. A *** represents statistical significance at the 1 percent level, ** represents significance at the 5 percent level, * represents significance at the 10 percent level.

Table 19. New Incidence of Disease

	Lung Disease		Cancer		Heart Disease		Arthritis		Stroke		Psych Problem	
Total Sample												
Any Exposure	1.32***		1.29***		0.98		1.07		0.97		1.18**	
	(0.001)		(0.000)		(0.711)		(0.177)		(0.751)		(0.033)	
Long-term		1.40***		1.32***		1.01		1.06		1.02		1.16*
Exposure		(0.000)		(0.000)		(0.862)		(0.293)		(0.874)		(0.080)
Short-term		1.00		1.16		0.84		1.13		0.77		1.31*
Exposure		(0.998)		(0.259)		(0.134)		(0.205)		(0.198)		(0.063)
Ever Smoked	1.55***	1.55***	1.15	1.15	1.19**	1.19**	1.16***	1.16***	1.15	1.15	1.17*	1.17*
	(0.000)	(0.000)	(0.102)	(0.101)	(0.013)	(0.014)	(0.009)	(0.008)	(0.229)	(0.231)	(0.087)	(0.086)
Currently Smoke	5.20***	5.22***	1.49***	1.49***	1.45***	1.45***	1.07	1.07	1.60***	1.60***	1.45***	1.45***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.283)	(0.287)	(0.000)	(0.000)	(0.000)	(0.000)
N	56,565	56,565	56,153	56,153	51,271	51,271	31,779	31,779	58,792	58,793	54,611	54,611
Working in Wave 1												
Any Exposure	1.31***		1.25***		0.97		1.08		0.95		1.21**	
	(0.005)		(0.006)		(0.620)		(0.182)		(0.643)		(0.041)	
Long-term		1.37***		1.27***		1.03		1.06		0.96		1.20*
Exposure		(0.002)		(0.004)		(0.731)		(0.321)		(0.752)		(0.062)
Short-term		1.05		1.14		0.72**		1.16		0.89		1.24
Exposure		(0.812)		(0.370)		(0.016)		(0.172)		(0.595)		(0.197)
Job Requires	0.82	0.82	0.65***	0.65***	0.82*	0.82*	1.17**	1.17**	0.50***	0.50***	0.82	0.82
Physical Effort	(0.181)	(0.175)	(0.002)	(0.002)	(0.061)	(0.060)	(0.031)	(0.030)	(0.002)	(0.002)	(0.155)	(0.156)
Job is High Stress	0.96	0.96	0.83	0.83	1.02	1.02	1.11	1.11	0.86	0.86	1.33*	1.33*
	(0.813)	(0.818)	(0.257)	(0.254)	(0.894)	(0.893)	(0.239)	(0.236)	(0.534)	(0.530)	(0.056)	(0.056)
Ever Smoked	1.57***	1.57***	1.19*	1.19*	1.20**	1.20**	1.17**	1.17**	1.06	1.06	1.23*	1.23*
	(0.002)	(0.002)	(0.067)	(0.067)	(0.019)	(0.020)	(0.011)	(0.011)	(0.660)	(0.661)	(0.064)	(0.064)
Currently Smoke	5.50***	5.51***	1.51***	1.52***	1.50***	1.51***	1.11	1.11	1.47***	1.47***	1.70***	1.70***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.112)	(0.117)	(0.006)	(0.006)	(0.000)	(0.000)
N	43,791	43,791	42,943	42,943	39,992	39,992	25,733	25,733	45,327	45,327	42,971	42,917

Notes: The table reports estimated hazard ratios from logistic duration regression models taking into account the sampling design of the HRS with p-values in parentheses. Each column in each panel represents a separate regression. The dependent variables are listed at the top of the table. Controls for respondents' age, race, gender, industry for which they worked the longest, body mass index, education level, and self-reported health status are included. The baseline hazard is equal to the natural log of time. The top panel reports estimated hazard ratios for all respondents and the bottom panel for respondents working in wave 1. Only respondents who did not have the specified disease at baseline are included in each set of regressions. A *** represents statistical significance at the 1 percent level, ** represents significance at the 5 percent level, * represents significance at the 10 percent level.

Table 20. Baseline Prevalence and New Incidence of Disease by Exposure Type

	Lung Disease		Cancer		Heart Disease		Arthritis		Stroke		Psych Problem	
	Baseline Prevalence	**New Incidence**	**Baseline Prevalence**	**New Incidence**	**Baseline Prevalence**	**New Incidence**	**Baseline Prevalence**	**New Incidence**	**Baseline Prevalence**	**New Incidence**	**Baseline Prevalence**	**New Incidence**
Solvents	1.73***	1.39***	1.21	1.34***	1.24*	0.94	1.43***	1.11	1.52*	1.22	1.49***	1.20
	(0.001)	(0.007)	(0.326)	(0.008)	(0.084)	(0.547)	(0.000)	(0.230)	(0.093)	(0.189)	(0.008)	(0.146)
Petroleum	2.34***	1.43	1.65	1.49*	1.34	1.23	1.43**	1.44***	1.24	1.30	1.53	1.35
	(0.002)	(0.164)	(0.155)	(0.059)	(0.177)	(0.248)	(0.041)	(0.010)	(0.663)	(0.341)	(0.144)	(0.230)
Asbestos	2.64***	2.01***	1.66*	1.51**	1.21	1.19	1.40**	1.10	1.25	1.04	1.43	0.92
	(0.000)	(0.000)	(0.092)	(0.015)	(0.330)	(0.230)	(0.017)	(0.467)	(0.621)	(0.858)	(0.164)	(0.730)
Fumes	3.04***	1.46**	1.25	1.31**	1.31*	0.98	1.65***	1.03	1.94**	1.11	1.49**	0.95
	(0.000)	(0.016)	(0.378)	(0.046)	(0.073)	(0.893)	(0.000)	(0.812)	(0.020)	(0.540)	(0.043)	(0.747)
Biohazards	3.44***	0.96	1.20	1.35	1.08	0.90	1.52**	0.97	1.14	0.61	1.35	1.46*
	(0.000)	(0.888)	(0.631)	(0.156)	(0.762)	(0.598)	(0.012)	(0.870)	(0.819)	(0.169)	(0.302)	(0.080)
Inorganics	1.11	1.32	1.31	1.21	1.10	1.13	1.39*	1.18	0.64	1.27	1.67*	1.42
	(0.772)	(0.263)	(0.496)	(0.392)	(0.712)	(0.493)	(0.055)	(0.295)	(0.535)	(0.422)	(0.082)	(0.125)
Agricultural	1.29	1.30	1.13	1.13	1.03	0.94	1.35**	0.84	1.59	0.63	1.01	1.36
	(0.353)	(0.236)	(0.709)	(0.537)	(0.894)	(0.731)	(0.043)	(0.194)	(0.291)	(0.109)	(0.963)	(0.106)
Drugs		1.34		0.84	1.54	0.80	0.26*	1.33	4.02	1.39		1.40
		(0.618)		(0.789)	(0.473)	(0.641)	(0.085)	(0.463)	(0.198)	(0.632)		(0.555)
Other	1.71**	1.34	1.52	1.41**	1.52**	1.08	1.43**	0.91	1.81	0.71	1.70**	1.00
	(0.045)	(0.159)	(0.113)	(0.045)	(0.021)	(0.620)	(0.011)	(0.499)	(0.105)	(0.202)	(0.029)	(0.982)
Ever Smoked	2.07***	1.50***	1.26*	1.15	1.47***	1.24***	1.19***	1.11*	1.10	1.21	1.24*	1.14
	(0.000)	(0.001)	(0.084)	(0.120)	(0.000)	(0.003)	(0.006)	(0.064)	(0.666)	(0.112)	(0.097)	(0.169)
Currently Smoke	3.78***	5.20***	1.24	1.51***	1.51***	1.54***	1.32***	1.05	1.11	1.70***	1.74***	1.39***
	(0.000)	(0.000)	(0.115)	(0.000)	(0.000)	(0.000)	(0.000)	(0.408)	(0.608)	(0.000)	(0.000)	(0.001)
N	8,228	52,631	8,228	52,204	8,228	47,659	8,228	29,683	8,228	54,658	8,228	50,898

Notes: The table reports estimated odds ratios of disease prevalence at baseline from logistic regression models and estimated hazard ratios of new incidence of disease from logistic duration regression models taking into account the sampling design of the HRS with p-values in parentheses. Each column represents a separate regression. The dependent variables are listed at the top of the table. The same controls for baseline prevalence and new incidence of disease are included as described previously. A *** represents statistical significance at the 1 percent level, ** represents significance at the 5 percent level, * represents significance at the 10 percent level.

Table 21. Survey Non-Response Estimates

	Die		Exit Sample		Die or Exit Sample	
Total Sample						
Any Exposure	0.97 (0.580)		0.93 (0.337)		0.94 (0.189)	
Long-term Exposure		1.00 (0.998)		0.95 (0.547)		0.97 (0.577)
Short-term Exposure		0.80* (0.075)		0.82 (0.187)		0.77** (0.011)
Ever Smoked	1.53*** (0.000)	1.53*** (0.000)	0.90 (0.242)	0.90 (0.242)	1.19*** (0.006)	1.19*** (0.006)
Currently Smoke	2.76*** (0.000)	2.77*** (0.000)	1.07 (0.449)	1.07 (0.441)	1.82*** (0.000)	1.83*** (0.000)
N	61,421	61,421	61,421	61,421	61,421	61,421
No Disease at Baseline						
Any Exposure	1.03 (0.626)		0.90 (0.198)		0.97 (0.603)	
Long-term Exposure		1.07 (0.357)		0.93 (0.358)		1.01 (0.896)
Short-term Exposure		0.88 (0.306)		0.80 (0.148)		0.82* (0.051)
Ever Smoked	1.48*** (0.000)	1.48*** (0.000)	0.90 (0.215)	0.90 (0.215)	1.16** (0.024)	1.16** (0.024)
Currently Smoke	2.84*** (0.000)	2.85*** (0.000)	1.05 (0.608)	1.05 (0.600)	1.88*** (0.000)	1.88*** (0.000)
N	57309	57309	57309	57309	57309	57309

Notes: The table reports estimated hazard ratios from logistic duration regression models taking into account the sampling design of the HRS with p-values in parentheses. Each column in each panel represents a separate regression. The dependent variables are listed at the top of the table. The first dependent variable, "die" is equal to one if the respondent died at anytime during the study, "exit sample" is equal to one if the respondent was alive but didn't respond to the survey and was dropped from the study, and "die or exit sample" is equal to one if the respondent either died or exited the study. Controls for respondents' age, race, gender, industry for which they worked the longest, body mass index, education level, and self-reported health status are included. The baseline hazard is equal to the natural log of time. The top panel reports estimated hazard ratios for all respondents and the bottom panel for respondents without any disease at baseline. A *** represents statistical significance at the 1 percent level, ** represents significance at the 5 percent level, * represents significance at the 10 percent level.

Table 22. Population Attributable Risk Estimates

	Any Exposure		Long-term Exposure		Short-term Exposure		Ever Smoked		Currently Smoking	
	Baseline Prevalence	**New Incidence**	**Baseline Prevalence**	**New Incidence**	**Baseline Prevalence**	**New Incidence**	**Baseline Prevalence**	**New Incidence**	**Baseline Prevalence**	**New Incidence**
Lung Disease %	25.9***	11.1***	25.9***	11.6***	0.6	0.0	27.7***	16.2***	45.2***	56.1***
Cancer %	8.6*	10.2***	8.9**	9.5***	0.0	1.0	6.9	5.0	5.4	12.9***
Heart Disease %	7.6**	-0.8	8.4***	0.3	-0.8	-1.0	13.4***	6.3**	12.7***	12.0***
Arthritis %	14.7***	2.7	12.8***	1.9	2.3***	0.8	6.6***	5.3***	8.6***	2.1
Stroke %	16.4**	-1.2	15.5***	0.7	1.5	-1.5	5.3	5.0	4.9	15.4***
Psych Problem %	18.0***	6.6**	15.0***	5.0*	4.0***	2.0*	5.6	5.6*	18.1***	12.0***

Notes: The table includes population attributable risks that are calculated according to the following formula: $PAR = \frac{P(RR-1)}{1+P(RR-1)}$, where P = proportion of respondents in each specific category and RR = relative risk of the disease for the exposed persons compared to persons not exposed. The proportions are determined from the HRS data and the relative risks come from the estimates in Table X. PAR significance is recorded based on the significance of the relative risk ratios. A *** represents statistical significance at the 1 percent level, ** represents significance at the 5 percent level, * represents significance at the 10 percent level.

Table 23. Cost of Disease Attributable to Workplace Exposure to Hazards

	Estimated Disease Costs ($ bn)			Estimated PAR: Any Exposure (%)		Estimated Cost Attributable to Workplace Exposure to Hazards ($ bn)	
	Total	Direct	Indirect	Baseline	New Incidence	Range	2012 $
Heart Disease	311.1	189.5	121.6	7.6 **	-0.8	-2.5 : 23.6	-2.6 : 24.9
Stroke	71.2	48.2	23	16.4**	-1.2	-0.9 : 11.7	-1.0 : 12.3
Lung Disease (no asthma)	152.7	93.3	59.4	25.9***	11.1***	16.9 : 39.5	17.9 : 41.6
Cancer	201.5	77.4	124.1	8.6*	10.2***	17.3 : 20.6	18.5 : 21.9

Notes: Estimated disease costs for heart disease, stroke, and lung disease come from the National Institute of Health (2009) and represent the estimated costs in 2010. Estimated cancer costs come from the American Cancer Society and represent the estimated costs in 2008. Estimated PAR percentages come from Table 22. A *** represents statistical significance at the 1 percent level, ** represents significance at the 5 percent level, * represents significance at the 10 percent level.

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