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ABSTRACT

Alternative work arrangements, including temporary and contract work, have become more widespread. There is interest in understanding the effects of these types of arrangements on employment and earnings risk for workers and the potential for existing social insurance programs to address this risk. We study employment and earnings risk in the context of workplace injuries among temporary and contract workers. We link administrative workers' compensation claims to earnings records to measure the employment and earnings risk posed by workplace injuries, comparing labor market outcomes after injury between temporary and contract workers and direct-hire workers injured doing the same job. We use a triple-difference identification strategy to isolate the effect of alternative work arrangements. We find that temporary workers have significantly lower probabilities of employment post-injury relative to similar direct-hire workers; temporary workers also have more severe earnings losses, which persist for at least three years after injury. This difference in income risk cannot be explained by differences in employment dynamics between temporary and direct-hire workers. We find evidence that the additional earnings losses suffered by temporary workers are offset by workers' compensation benefits, suggesting that the program provides insurance for the incremental risk faced by temporary workers.

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1 Introduction

A growing number of jobs are being filled through various types of alternative work arrangements, including independent contracting, employment through temporary help agencies, or participation in the "gig economy" of jobs arranged through online intermediaries such as Uber and TaskRabbit (Katz and Krueger, 2019).[1] Workers in alternative work arrangements—who we also refer to as *nonstandard workers*—are unlikely to be eligible for employee benefits such as employer-sponsored health insurance or to be covered under social insurance programs such as unemployment insurance or workers' compensation. Observers have thus raised concerns that alternative work arrangements shift economic risks from employers and social insurance programs to workers and their families. However, there is little evidence directly comparing the economic risks faced by nonstandard and direct-hire workers or examining the suitability of traditional social insurance arrangements for workers employed through alternative work arrangements.

In this paper, we study earnings and employment risk faced by both direct-hire workers and nonstandard workers hired through temporary help agencies or contract firms in the context of workplace injuries. Unlike independent contractors, workers employed by temporary help agencies or other contract firms are covered by workers' compensation, and so administrative records from the workers' compensation system create a rare opportunity to examine the effects of alternative work arrangements on an important form of income risk for a fast-growing segment of the nonstandard worker population. We use a unique data set from California to provide the first evidence comparing employment risk after workplace injury between workers in alternative work arrangements and direct-hire workers who are injured doing similar jobs. The data set includes administrative records from the universe

[1] An earlier paper by Katz and Krueger (2016) suggested even higher rates of growth in alternative work arrangements. The 2019 paper concludes that there has been a "modest upward trend."

of California workers' compensation claims from 2005 to 2011. We linked workers to administrative Unemployment Insurance (UI) earnings records to provide pre- and post-injury earnings and employment outcomes, permitting us to compare the trajectory of labor market outcomes through workplace injury across different categories of workers.

In our data, we can identify direct-hire workers as well as temporary workers and contract workers. These workers do not encompass the entire nonstandard workforce, as independent contractors generally are not covered by workers' compensation and their earnings may not be captured in administrative records on wage and salary income since they are unlikely to participate in state Unemployment Insurance programs. In contrast, temporary and contract workers are covered by both workers' compensation and Unemployment Insurance so it is possible to observe injuries and post-injury labor market outcomes for these groups of nonstandard workers. The outcomes of temporary workers can thus shed light on how income risk after injury and the generosity of the existing workers' compensation system compare between workers who are and who are not direct-hires. Because we are able to control for job tenure at injury and the type of work being done at the establishment where the injury took place, we are able to identify differences in employment risk due to differences in work arrangements while holding constant confounding factors like job tenure, demographics, and the type of work.

A major challenge in studying the labor market outcomes of temporary workers is that temporary workers are likely to have different employment trajectories than direct-hire workers even in the absence of a workplace injury. Our preferred analysis accordingly uses *medical-only* workers' compensation claims as an additional control group for more severe, *lost-time* injuries. Medical-only claims are claims for minor injuries that do not result in payment of indemnity (or cash) benefits for either temporary or permanent disability; in California, this means that the worker experienced no more than three days of work absence due to the injury. We estimate a triple-difference regression specification that compares the post-injury labor market outcomes of workers with lost-time injuries to workers with medical-only injuries who were hurt doing a similar job and who had the same type of work arrangement (direct-hire or temporary worker). By using other workers with the same type of work arrangement as an additional control group, this identification strategy allows us to distinguish the effect of alternative work arrangements on post-injury labor market outcomes from other differences in employment dynamics between temporary and direct-hire workers. Event-study estimates of these triple-differences models indicate that this strategy eliminates differential pre-injury trends in labor market outcomes, suggesting that our triple-difference identification strategy may capture the causal effect of temporary work on post-injury labor market outcomes.

We find that temporary workers have significantly lower post-injury employment and earnings than we would expect if these workers were direct hires at the time of injury instead. One year after injury, temporary workers are 4.3 percentage points less likely to be employed. This gap in employment closes gradually over the second year after injury and disappears by the end of the third year after injury. Even though reductions in employment appear to be transitory, earnings losses for temporary workers continue through the end of the third year after injury, indicating that these workers experience some combination of slower wage growth and reduced hours even after returning to work: temporary workers with a lost-time injury earn 9.1 percent less than they otherwise would have over the three years following injury. This pattern of results holds even when we compare temporary (at time of injury) workers who were recently employed as direct hire workers to direct hire workers who were recently working as temporary workers, further suggesting that the results are not simply indicative of unobserved differences between temporary and direct-hire workers. To sum up, our findings show that temporary workers face a larger magnitude of earnings and employment risk after a lost-time workplace injury than they would if they were working as direct-hire employees at the time of injury.

This difference in earnings risk raises the question of whether workers' compensation benefits are designed in a way that provides temporary workers with the additional compensation needed to offset their greater earnings losses, an important question given concerns about the adequacy of existing social insurance programs if expanded to nonstandard workers. Since we observe a set of nonstandard workers eligible for workers' compensation, we are able to address this question by adapting our triple-difference regression model for labor earnings to estimate the effect of alternative work arrangements on a measure of income that includes both labor earnings and workers' compensation benefits. The goal of this exercise is to examine whether the difference in earnings losses results in a difference in uncompensated losses net of workers' compensation benefits. The workers' compensation system in California provides separate benefits for temporary disability and permanent disability; lump-sum settlements are also important. We add these different types of benefits to labor earnings sequentially in order to provide evidence on which components of the overall workers' compensation benefit structure are most important for offsetting the greater risk faced by temporary workers.

We find that temporary and permanent disability benefits reduce, but do not eliminate, the gap in post-injury income between temporary and direct-hire workers; the 9.1% reduction in labor earnings attributable to temporary status falls to 6.3% when temporary total disability benefits are included and to 4.0% when permanent partial disability benefits are included. When lump-sum settlements are added to the income measure, however, the decline in post-injury income due to temporary worker status is eliminated. Our findings suggest that, even though temporary workers face greater earnings risk after injury than do direct-hire workers, the current workers' compensation system in California is more or less successful – on average – at providing benefits that are sufficient to offset this higher level of risk.

This study provides the first empirical estimates of how the shift from direct-hire employment to alternative work arrangements affects an important dimension of household income risk: earnings and employment loss due to workplace injury. Previous studies have documented that temporary work is associated with worse occupational health (Underhill and Quinlan, 2011; Quinlan, 2015) and substantially higher rates of workers' compensation claiming (Park and Butler, 2001; Smith et al., 2010; Zaidman, 2017). However, the employment and earnings outcomes of temporary workers who are injured on the job have not previously been examined. Besides facing a higher risk of experiencing a workplace injury, our findings indicate that temporary workers face more severe economic consequences when workplace injuries occur. Beyond the question of the health and safety implications of alternative work arrangements, our study adds to our understanding of the impact of alternative work arrangements on job quality, an area identified by Bernhardt et al. (2016) as a major gap in knowledge. Our study also represents, to our knowledge, the first attempt to assess the suitability of an existing social insurance program for the risks faced by nonstandard workers. As such, our analysis comparing losses to benefits may help to inform ongoing state and federal debates about worker classification and coverage of social insurance for independent contractors and other types of nonstandard workers.

The remainder of this paper is organized as follows. Section 2 provides background and a literature review on workers' compensation and temporary help agencies. Section 3 describes our data sources. Section 4 describes our empirical strategy. Section 5 presents our results. Section 6 discusses policy implications and suggests priorities for future research.

2 Background

In 2015, there were 2.9 million reported nonfatal occupational injuries and illnesses in the U.S. (Bureau of Labor Statistics, 2017). These injuries impose a substantial economic cost on workers and the economy as a whole: Leigh (2011) estimates that the total social cost of workplace injuries in the United States was $250 billion per year as of 2007. In order to help protect workers against the financial and health risks arising from workplace injury and illness, workers' compensation (WC) systems have been established in every state to ensure that injured workers have access to needed medical care and rehabilitation services. In 2016, state workers' compensation systems paid a total of $61.9 billion in medical and cash benefits to injured workers (McLaren et al., 2018). Cash benefits in workers' compensation are untaxed and designed to provide partial insurance against earnings losses due to injury, typically paying workers two-thirds of their weekly wage at the time of injury (subject to a minimum and maximum benefit) during an initial spell of temporary total disability and providing additional permanent disability benefits to workers whose injuries result in long-term impairment. Workers' compensation provides only partial wage replacement, a design feature that is justified by strong evidence that disability duration (and thus the cost of providing benefits) is responsive to the level of benefits or the wage replacement rate (Meyer et al., 1995; Butler et al., 2013).

When injured workers experience severe disability following injury, they are likely to use social insurance programs beyond the workers' compensation system: Leigh and Marcin (2012) estimated that occupational injuries result in about $1 of federal costs due to increased Social Security Disability Insurance (SSDI), Medicare, and Medicaid benefits for every $2 paid by state WC systems. In a study suggesting that such cost spillovers could be quite widespread, Reville and Schoeni (2004) estimated that 1 in 3 SSDI recipients over the age of 50 reported being disabled due to a work-related injury. The most compelling evidence on the link between occupational injury and federal disability program participation comes from a series of studies involving SSA researchers that linked workers' compensation claims from New Mexico to SSA records on earnings, SSDI receipt, and mortality. An important finding from this work is that lost-time injuries result in highly persistent earnings losses up to ten years after injury; even workers with moderately severe injuries (defined to include those with less than 8 weeks of total disability and no permanent disability benefits) experienced long-term earnings losses of 10% or more. O'Leary et al. (2012) confirms that workers with lost-time workers' compensation claims face an elevated risk of SSDI receipt over the subsequent decade, while a follow-up study shows that injured workers even face elevated mortality risk (Boden et al., 2016).

State workers' compensation laws operate by establishing a statutory responsibility for employers to provide specified wage replacement and medical benefits to injured workers. Employers typically meet these obligations by purchasing workers' compensation insurance or through self-insurance. The role of employers in financing and arranging workers' compensation coverage for their employees reflects the historical origins of workers' compensation as an alternative to tort liability for employers. By expanding the ranks of workers who do not have a traditional, direct-hire employment relationship with their employers, the rise of outsourcing and alternative work arrangements in the United States has created a number of challenges for workers' compensation policy. The most visible challenge for workers' compensation posed by the rise of alternative work arrangements is the growth of non-covered employment. Outside of Texas, where participation in the workers' compensation system is optional for employers, workers' compensation is essentially universal for wage and salary employees in all states, with 97.2 percent of UI-covered workers covered by workers' compensation (McLaren and Baldwin, 2017). Independent contractors, who are not legally classified as employees, are typically not covered by workers' compensation even if they are working for businesses whose direct-hire employees must be covered by workers' compensation.

2.1 Temporary Work, Workplace Safety, and Income Dynamics

Unlike gig economy workers and other independent contractors, temporary and contract employees are classified as wage and salary employees. However, they are not wage and salary employees of the host employer—the firm that is using the worker's labor to produce goods and services—but instead are employees of a temporary agency or contract firm. Because they are employees of a labor intermediary, temporary and contract workers are covered by mandatory employee benefits such as workers' compensation and unemployment insurance, and are subject to the same labor laws and regulations that govern direct-hire workers. Workers' compensation coverage is typically provided by the temporary agency rather than the host employer. Temporary work is commonly used for many low-skilled occupations as well as higher-skilled jobs which require less firm-specific specialization, such as nursing and computer programming (Kilcoyne, 2004). Table 1 shows the occupations in California with the highest shares of temporary workers using data from the Bureau of Labor Statistics Occupational Employment Statistics (OES) program.

Legally, both the temporary agency and the host employer are responsible for providing safe working conditions (Howard, 2016). In practice, however, the temporary agency may have little or no control over the temporary worker's activities and work environment at the job site, and a number of studies have found that temporary workers have higher workers' compensation injury claim rates than direct-hires in the same industries (Park and Butler, 2001; Smith et al., 2010; Zaidman, 2017). These studies have reached differing conclusions about the extent to which higher claim rates reflect moral hazard or actual safety differences. Park and Butler (2001) argue that temporary workers may be less deterred from filing claims by the threat of retaliation than direct-hire employees (for whom there is an implicit contract offering continued employment), resulting in greater claim-filing moral hazard (i.e., higher claim-filing rates conditional on injury occurrence and severity). While such a mechanism is likely to affect claiming behavior, a growing literature has also provided evidence for important safety and health differences between temporary and direct-hire workers (Benavides et al., 2006; Underhill and Quinlan, 2011). Many of these differences are attributable to observable differences in risk factors such as age and job tenure, so unconditional differences in claim rates should not be interpreted as the causal effect of the work arrangement (Zaidman, 2017). However, the type of work arrangement is also likely to have an independent effect on safety due to lower access to safety training and worse communication with coworkers (Foley, 2017). Analysis of the injury mix for temporary workers bolsters the idea that poor working conditions and worse hazard communication contribute to higher injury risk: "struck by or against" and "caught in" injuries are more common among temporary and contract workers (Smith et al., 2010).

Temporary and contract workers are sharply distinguished from other nonstandard workers such as independent contractors by their status as wage and salary employees. Nevertheless, temporary workers share certain vulnerabilities with independent contractors. In the absence of linked injury claims and earnings data covering independent contractors, it is worth noting the structural similarities between temporary workers and independent contractors in order to assess the external validity of our findings for independent contractors and other groups of nonstandard workers.

The defining feature of all these alternative work arrangements is that workers are likely to have weaker connections—in the sense that they lack an implicit contract promising continued employment—with their host employers than would be the case if they were direct-hire workers. To the extent that the gaps in safety training and hazard communication identified by Foley (2017) reflect privately optimal employer behavior in settings where returns to firm-specific human capital are limited and turnover costs are low, then we would expect to find similar safety challenges for independent contractors and other alternative work arrangements. Researchers have also identified loopholes in workers' compensation experience-rating that weaken employers' financial incentives to provide safe working conditions to temporary workers (MacEachen et al., 2016). Independent contractors, as non-covered workers, are also likely to present host employers with weak or non-existent financial incentives for safety.

Beyond occupational health and safety, a literature in labor economics has examined the broader question of whether temporary employment is beneficial to workers' career development, or whether job experience as a temporary worker somehow results in less skill development. This has been a difficult question to answer because different workers are likely to sort into temporary versus direct-hire employment on the basis of productivity, labor market experience, or labor supply preferences, and because it is unclear whether the right counterfactual for temporary employment is direct-hire employment in a similar job or, perhaps, unemployment.

In general, there have been two different views of temporary work in the labor economics literature. Some researchers and observers have viewed temporary agency work as a stepping-stone to permanent and higher-wage employment for entry-level workers such as young adults or mothers transitioning off of the Temporary Assistance for Needy Families program after welfare reform. Other researchers, however, have argued that temporary work fails to promote career progression since workers may fail to develop skills and lack opportunities for promotion. To the extent that temporary work is associated with greater health and safety risks as well as reduced access to retirement accounts and other benefits, it might even be seen as an obstacle to accumulating wealth or human capital. Early studies on temporary work were broadly consistent with the stepping-stone theory (Lane et al., 2003; Heinrich et al., 2005). However, Autor and Houseman (2010) revisited this question with an instrumental variables strategy that leveraged quasi-random variation in job placement for welfare leavers.[2] They found that temporary work failed to increase employment at the end of the initial job assignment, suggesting that temporary workers were very likely to revert to non-employment rather than using the job to build a career.

These findings are not likely to be directly applicable to our setting since many of the temporary workers in our sample are likely to have more employment experience than the welfare leavers that have largely been the focus of public economics and labor economics literature on temporary workers. Rather, these findings are relevant because they suggest that employment dynamics are likely to be systematically different for temporary and direct-hire workers, a pattern that we find in our data for workers incurring workplace injuries. We address these concerns by focusing on a triple-difference specification that uses temporary workers with less severe injuries to control for temporary workers with lost-time injuries, as we discuss below.

3 Data

In this paper, we use administrative records on workers' compensation claims reported to the California Department of Industrial Relations (DIR). These data were linked to administrative earnings data maintained by the state Employment Development Department, which administers California's Unemployment Insurance (UI) program. We briefly discuss these data sets below as well as the variables that we constructed for our analysis.

3.1 Workers' Compensation Claims

Our primary data source for identifying injured workers in California is the Workers' Compensation Information System (WCIS), an all-payer database of workers' compensation claims collected and maintained by the Division of Workers' Compensation (DWC) in the

[2] Autor et al. (2016) studies the distributional consequences of temporary job placement using quantile regression methods.

California DIR for years 2005 to 2011.[3] California law requires workers' compensation claims administrators (insurers, self-insured employers, and third-party administrators) to report new workers' compensation claims to the WCIS, and to submit additional reports reflecting material changes in the status of the claim, such as claim closure or the start or end of benefit payments. The WCIS contains basic demographic information about injured workers (age at injury and gender). We also rely on codes for the *Nature, Origin, and Cause of Injury* to classify injuries based on initial characteristics observed as of the date of injury. The workers' compensation data also allow us to identify workers who receive settlements or benefit payments for Temporary Total Disability (TTD) or Permanent Partial Disability (PPD), and to observe the total amount of payments to date.[4]

In addition to these variables, we use workers' compensation classification codes (referred to as *class codes*) to proxy for the level of job demand and injury risk faced by workers within an industry. Class codes in California are developed by the Workers' Compensation Insurance Rating Bureau (WCIRB) to allow efficient risk segmentation in the pricing of workers' compensation coverage. Class codes are meant to group workplaces on the basis of risk and expected cost for workers' compensation insurers. Class codes thus are not directly comparable to either industry or occupation codes, but are likely to incorporate information about both industry and occupation that affects injury risk and disability costs across workplaces and, in cases where low-risk workers are employed at high-risk workplaces, between

[3]Further information about the WCIS is available at `https://www.dir.ca.gov/dwc/wcis.htm`.

[4]We casewise-deleted cases with missing data on any variables used in our analysis: out of 4.61 million cases with a first report of injury for injury years 2005-2011 that had been reported as of the time of data collection, 19% were casewise-deleted due to missing WCIS data, leaving 3.7 million complete records workers' compensation claims. After linkage to the EDD base wage file and exclusion of workers with inconsistent names over time in the EDD data (a sign of potentially contaminated data), we were left with about 3.1 million complete records workers' compensation claims linked to usable EDD earnings histories. Our analytic sample for this paper was further limited based on industry and class code, as described below. Additional details on data collection and construction methods are presented in Chapter 3 of Dworsky et al. (2016).

employees within workplaces.[5]

Our research design is made possible by the fact that class codes for temporary and contract workers are defined based on the work activities and level of risk exposure at the host employer, not the firm that employs the worker.[6] This distinction is critical in this context because the employer of record for temporary and contract workers differs from the host employer, which is not the case for direct-hire workers. We are thus able to identify the type of work that the worker engaged in and the corresponding risk of injury and disability. As we discuss below, we rely on class codes to compare temporary and direct-hire workers injured doing the same type of job.

3.2 Earnings and Employment Outcomes

We linked the WCIS data with administrative earnings records. The EDD base wage file captures all quarterly wage and salary income earned by UI-covered workers in California. Under an interagency agreement between DIR and EDD, we submitted programs for EDD staff to link individuals appearing in the WCIS to their earnings histories in the Base Wage File. Linkage was performed primarily using the injured worker's Social Security Number (SSN). 6-digit NAICS codes reported to EDD were provided to identify the industry of employers appearing in the linked data. After identifying injured workers and excluding individuals with inconsistent name information in the Base Wage File, the data were de-

[5] In general, the highest-risk classification present at an establishment is the *governing classification* that is used to set workers' compensation premiums. However, California and other states allow for covered payroll to be divided into higher- and lower-risk classifications under some circumstances. The most important *standard exception* is for clerical employees at high-risk workplaces (e.g., a receptionist at a shipping warehouse) whose work is physically separated from the high-risk production processes. See Workers' Compensation Insurance Rating Bureau of California (2018) for further details.

[6] Rule 8 of Part 3, Section IV of the *California Workers' Compensation Uniform Statistical Reporting Plan*, which provides workers' compensation insurers with instructions for the determination of class codes, states that "the classification of workers provided to a client under any type of employee leasing arrangement (temporary or otherwise) shall be determined as though the workers are employees of the client. The limitations and conditions of the classification(s) so assigned and all Standard Classification System rules pertaining thereto shall be applicable." See Workers' Compensation Insurance Rating Bureau of California (2018) for further details.

identified and assigned an ID number that could be used to match to the WCIS data.[7] To produce estimates that are representative for the population of cases reported to the WCIS, we constructed sampling weights, which we used in all our estimates.[8] We show that weighted and unweighted estimates are very similar.

We used the wage records to create a measure of total quarterly wage and salary earnings by summing earnings over all employers in each quarter.[9] We also created an indicator variable for employment, which we define as having quarterly earnings from all employers greater than $200. We are especially interested in understanding the full trajectory of post-injury outcomes as well as testing for observable differences prior to the injury. We constructed a balanced panel containing, for each injured worker, 17 quarters of earnings and employment data (from four quarters prior to injury to twelve quarters after injury).

3.3 Sample Definition and Ascertainment of Temporary Status

Our treated group consists of workers whose employer reported NAICS industry code 56132 (temporary help services) or 56133 (professional employer organizations [PEOs]) to EDD. We observe 49,669 injured workers in these categories, with the majority (80%) employed by temporary help agencies; for convenience, we refer to employers of either temporary agencies or PEOs as *temporary workers*. Nationally, these categories of workers are estimated to constitute 22% to 31% of the nonstandard workforce, depending on the survey used (Katz

[7] Additional details on earnings data collection can be found in Dworsky et al. (2018), specifically Chapter 2 and the Appendix.

[8] Specifically, we reweighted the sample of complete records WCIS claims with matched EDD wage data so that the joint distribution of age, gender, geographic region within California, year and quarter of injury, firm size, and quartiles of annual pre-injury earnings matches the joint distribution of these variables among all observations with complete records on these variables.

[9] To avoid results being driven by very high earners, outliers were removed. The outlier threshold was defined by calculating the 99.8$^{\text{th}}$ percentile of CPI-adjusted total earnings for each quarter in the analysis (before, during, and after the injury) for injured workers only, and taking the minimum value across quarters relative to injury. The resulting outlier threshold in the merged sample was $72,540 per quarter, or $290,160 in annualized terms. If an injured or control worker's CPI-adjusted total earnings exceeded this value in any quarter, they were classified as an outlier and removed. 1.3% of injured workers in the merged data met this criterion.

and Krueger, 2019).[10] We assume that all other injured workers are direct-hires.

The prevalence of temporary and contract work varies widely across industries and occupations, and many class codes have very few injuries among temporary workers. Table 2 presents temporary worker shares by class code in our data. The categories with the highest shares, while not directly comparable to occupation codes, suggest overlap with the occupations observed in Table 1. In particular, class codes likely to involve material moving, packaging, and assembly have high rates of temporary employment among injured workers.

Because we are interested in comparing temporary and direct-hire workers injured doing similar jobs, class codes without substantial temporary/contract employment do not contribute to our empirical strategy. We therefore limit our analysis sample to class codes with a sufficiently large number of injuries among both temporary and direct-hire workers. Specifically, we tabulated the number of temporary and direct-hire injuries by class code and restricted the sample to class codes in which there was at least one calendar quarter between 2005-2011 with 20 or more injuries among temporary or contract workers. As discussed below, we include separate fixed effects for class code-quarter of injury interactions in our regression models, ensuring that temporary workers are always compared to direct-hires who are injured at roughly the same time doing the same type of job.

This sample restriction leaves us with 62 class codes represented in our analysis sample (see Appendix A for a list of all included class codes). The included class codes correspond closely to the occupational distribution shown above in Table 1. As suggested

[10] It has proven challenging to measure trends in alternative work arrangements using household surveys, as discussed in a recent working paper by Katz and Krueger (2019). Despite this ambiguity, establishment survey estimates from the BLS Current Employment Statistics (CES) indicate that headcount employment by temporary agencies and PEOs has grown much faster than total nonfarm employment in recent decades, from 1.3 million employees in 1990 to 3.3 million employees in 2017. Source: BLS Series ID CEU6056132001 (All employees, thousands, temporary help services, not seasonally adjusted) and CEU6056133001 (All employees, thousands, professional employer organizations, not seasonally adjusted). Available from https://data.bls.gov/PDQWeb/ce, accessed January 23, 2019.

by Table 2, transportation and warehousing, low-wage construction, and manufacturing are well-represented, as are some higher-wage occupations with a high temporary/contract worker prevalence (such as health care professions and computer programming).

Tables 3 and 4 show summary statistics for injured workers who received cash benefits ("indemnity claims"), which indicates that their injuries resulted in temporary disability beyond three days, permanent disability, or both. Table 3 shows differences in demographics, job characteristics, and injury mix between direct-hire workers and temporary workers with indemnity claims. Overall, the temporary workers have lower weekly wages, have shorter tenures at the at-injury firm, and are less likely to work full-time. They are also less likely to be female and are younger on average. Temporary workers also have fewer cumulative injuries, which is consistent with less tenure at the firm. Cause of injury differences between temporary and direct-hire workers resemble the patterns highlighted by Foley (2017), with temporary workers disproportionately likely to be injured by being caught in, being rubbed by, striking, or being struck by external objects.

Using the linked earnings data, Table 4 compares earnings and employment across our different categories. The unadjusted changes in means presented in Table 4 show that temporary workers – proportional to pre-injury outcomes – have worse post-injury outcomes. This gap appears to shrink over time. The post-injury employment of temporary workers sinks to 75.7% of pre-injury levels in the first year post-injury, compared to 80.2% for direct-hires. However, Table 4 also shows that direct-hires have much higher earnings and employment than temporary workers prior to the injury. These pre-injury differences motivate our use of a triple-difference empirical strategy which accounts for these differences.

4 Empirical Strategy

The goal of our empirical strategy is to isolate the effect of work arrangements on post-injury worker outcomes while controlling for the observable differences between direct-hire and temporary workers documented above, as well as any unobservable differences between these groups of workers that would affect post-injury employment dynamics. Our data permit us to compare outcomes after an injury to labor outcomes before the injury, and we will study quarterly outcome changes for temporary workers receiving indemnity benefits due to missed work days to direct-hires also receiving indemnity benefits. The focus on outcome *changes* is motivated by the differences between direct hire and temporary employees even prior to injury. Given the nature of these different types of work arrangements, it is not surprising that temporary workers have lower employment propensities and earnings before the injury. We will show the full trajectory of these outcomes to analyze outcome changes, accounting for level differences across work arrangement types.

4.1 Difference-in-Differences

A natural research design in this setting is a difference-in-differences approach comparing changes in employment between temporary and direct-hire workers with indemnity benefits. The obvious concern with this empirical strategy is that temporary workers may have different employment and earnings trajectories compared to direct hires even in the absence of an injury. Some of these differences can be explained by differences in observable characteristics between the different categories of workers. Younger workers or workers in certain industries and occupations should not necessarily be expected to have the same future employment trajectories, regardless of whether they are currently direct-hires or temporary workers. Given our data, we are able to account for a rich set of observable characteristics which predict future labor outcomes.

We begin by estimating difference-in-differences event studies for workers receiving indemnity benefits after incurring a workplace injury. This specification is represented by

$$
\begin{aligned}
y_{icqt} =& \alpha_{cqt} + \sum_{s=-4}^{12} \beta_s^T T_i \mathbf{1}\{t-q=s\} \qquad \textit{(temp vs. direct hire)} \\
&+ \sum_{s=-4}^{12} X_i' \beta_s^X \mathbf{1}\{t-q=s\} \qquad \textit{(observable characteristics)} \\
&+ \varepsilon_{icqt}.
\end{aligned} \tag{1}
$$

where y_{icqt} is a labor outcome for individual i in class c injured at time-relative-to-injury t for an injury incurred in calendar year-quarter q. The fixed effects α_{cqt} account for the employment propensity of a worker in class c injured at time q and time-relative-to-injury t.[11] We note that, due to the inclusion of these fixed effects, all comparisons are made between workers injured in the same quarter at the same type of job. T_i is an indicator variable that is equal to 1 if the worker was a temporary worker and zero if the worker was a direct-hire. We have not included the temporary worker indicator T_i separately, but instead include all the event-study interaction terms $\beta_s^T T_i \mathbf{1}\{t-q=s\}$ so the model fully saturates differences between direct-hire and temporary workers. We do not omit any of the quarter relative to injury indicators, and these estimates, consequently, are *not* normalized to the difference in a specific quarter relative to injury. Instead, the β_s^T coefficients trace out the difference, s quarters after the injury, in the probabilities of employment for temporary workers compared to direct-hire workers.[12]

[11] For example, there is a full profile of time-relative-to-injury ($t = -4, -3, \dots 12$) effects for workers injured in Landscape Gardening (Class Code 0042) in 2005Q1, corresponding to labor market outcomes from 2004Q1 to 2008Q1. There is a separate profile for workers in Landscape Gardening injured in the second quarter of 2005, and so on for all possible combinations of injury date and class code.

[12] To interpret equation (1) as a more traditional difference-in-differences specification, the α_{cqt} fixed effect can be thought of as the "calendar time" fixed effect. Here, it is interacted with time-relative-to-injury and class code to more flexibly account for secular trends and underlying outcome trajectories. A traditional difference-in-differences specification would then interact a "post" dummy with a temporary worker (i.e, treatment) dummy. We trace out the trajectory by interacting the temporary worker dummy with a full set of time-relative-to-injury indicators, permitting pre- and post-injury comparisons.

We also include individual characteristics (X_i) interacted with time-to-event indicators as control variables. This specification permits observable characteristics to have different effects based on time relative to injury, which is recommended in difference-in-differences designs (Jaeger et al., 2018). In this setting, we wish to avoid attributing labor market dynamics associated with differences in age, job tenure, or other observable differences between temporary and direct-hire workers, to the work arrangement. We interact indicators based on the following variables with quarter-relative-to-injury indicators: age group-gender interactions,[13] job tenure, type of injury, geographic region within California,[14] full-time/part-time status, and the weekly wage as reported on the workers' compensation claim. We note that the weekly wage reported on the workers' compensation claim determines the level of indemnity benefits and thus can be viewed as a way to control for differences in post-injury labor supply incentives due to differences in the wage replacement rate.[15]

4.2 Triple-Difference Model

In addition to concerns about observable differences, direct hire and temporary workers may also be different on unobserved dimensions that affect post-injury employment dynamics. Our preferred specification, therefore, uses "medical-only" workers' compensation claims as an additional comparison group to account for differential underlying trends between direct-hires and temporary workers. "Medical-only" claims are injury claims which result in no more than three days of lost work; these workers receive medical care through the workers' compensation system, but do not receive any indemnity benefits or settlements for either permanent or total disability. We use these claims to account for natural differences in labor

[13] We include indicators for the following age bins, with age measured at the time of injury: 16-25; 26-35; 36-45; 46-55; 56-65; 66-70.

[14] DWC groups the 58 counties in California into 10 regions for system monitoring purposes. Definitions of these regions (e.g., "Los Angeles," "Inland Empire," "Central Valley") are available at `https://www.dir.ca.gov/dwc/wcis/WCIS_tables/TABLE-7/WCIS_Reports-Table7.html`.

[15] The weekly wage varies independently of earnings due to variation in hours.

outcomes before and after workplace injuries between direct-hires and temporary workers unrelated to the injury. Thus, we rely on a triple difference specification in which the main variables are the interactions of (1) a temporary worker indicator; (2) an injured worker receiving indemnity benefits indicator; and (3) time-relative-to-injury indicators. We also include indicators controlling for two-way interactions of these categories in the specification. This approach results in an event-study analog of a triple differences empirical strategy, tracing out the relative labor outcomes in each quarter-relative-to-injury. Thus, we can test for systematic differences before the injury and study the timing of post-injury employment effects.

The disadvantage of the triple-differences approach is that the "medical-only" claims may include part of the effect that we are interested in if temporary workers with medical-only claims also causally suffer relatively worse post-injury outcomes. Consequently, our estimates are likely biased against finding an effect and should represent lower bounds (in terms of the absolute magnitude of the coefficients) on the true effects of temporary work on post-injury outcomes. The differences-in-differences estimates, meanwhile, are likely represent upper bounds on the absolute magnitude of the effect of temporary work on post-injury outcomes due to the potential for differences in employment dynamics that would violate the parallel trends assumption.

In our triple-differences specifications, we also include fixed effects for four-way interactions of categorical variables for the following worker and injury characteristics: class codes, calendar time of injury, time-relative-to-injury, and contract type (direct hire or temporary). By including these fixed effects, we are comparing temporary and direct hire employees working in the same type of job at the same time (both in calendar time and time-relative-to-injury). As in the difference-in-differences specification introduced above, we also control for observable differences between temporary and direct-hire workers by including time-varying controls for worker, job, and injury characteristics.

Our triple-difference event-study specification can be represented by

$$
\begin{aligned}
y_{icdqt} = & \mu_{cdqt} + \sum_{s=-4}^{12} \beta_s^I I_i \mathbf{1}\{t-q=s\} \quad \textit{(indemnity difference from medical-only)} \\
& + \sum_{s=-4}^{12} \beta_s^{TI} T_i I_i \mathbf{1}\{t-q=s\} \qquad \textit{(indemnity X temp triple-difference)} \\
& + \sum_{s=-4}^{12} X_i' \beta_s^X \mathbf{1}\{t-q=s\} \qquad \textit{(observable characteristics)} \\
& + \varepsilon_{icdqt},
\end{aligned}
\tag{2}
$$

where y_{icdqt} is a labor outcome for individual i in class c and contract type (work arrangement) d injured at time-relative-to-injury t for an injury incurred in calendar year-quarter s. The μ_{cdqt} interactions account for the trajectory of employment over time relative to injury, while flexibly allowing these trajectories to vary by calendar time, class code, and for whether the worker is a direct-hire or temporary worker. As before, T_i is equal to 1 if the worker was a temporary worker when injured while I_i is equal to 1 for indemnity claims (0 for medical-only claims). Thus, the β_s^I estimates capture differences in employment patterns (relative to time of injury) for all (medical-only and indemnity benefit) injured temporary workers.

The coefficients of primary interest are the β_s^{TI} terms, which trace out the incremental difference in the probability of employment for injured temporary workers receiving indemnity benefits relative to what would be expected if the employment effects of temporary work and indemnity injury were additive. We study the relative trajectory of labor outcomes for 4 quarters prior to injury up to 8 quarters after injury, plotting the estimates of β_s^{TI}. As with the difference-in-differences event-study specification introduced in Equation 1 above, the interaction $T_i I_i$ between temporary work and indemnity benefit receipt is fully absorbed by (i.e., does not appear separately from) the event-study coefficients and so there is no normalization or omitted category for these event-study coefficients.

Equation 2 is analogous to a triple-difference regression specification in that all two-way interactions are included in the model (or are spanned by more flexible controls). Interactions between time relative to injury and temporary worker status are absorbed by the fixed effects μ_{cdqt}, while interactions between time relative to injury and indemnity benefit receipt are absorbed by the event-study terms $\beta_s^I I_i \mathbf{1}\{t-q=s\}$. A flexible triple-difference model would include a time-invariant two-way interaction $T_i I_i$ between indemnity benefit receipt and temporary worker status, and the triple-difference treatment effect would be identified by an interaction between this two-way interaction term and an indicator for time periods after the injury. In Equation 2, the two-way interaction $T_i I_i$ is fully absorbed by the event-study terms $\beta_s^{TI} T_i I_i \mathbf{1}\{t-q=s\}$. We use this specification to capture the dynamics of temporary worker employment before and after a lost-time injury, and to evaluate the credibility of a triple-difference design: coefficients of zero for the pre-injury event-study variables $\beta_s^{TI}, s<0$ indicate that any pre-injury differences in employment trajectories between temporary workers with lost-time injuries and those with medical-only injuries are indistinguishable from the difference in employment trajectories between direct-hire workers with lost-time and medical-only injuries. Throughout this paper, standard errors are adjusted for clustering based on class code, and we signify statistical significance at the 5%, 1%, and 0.1% levels. We include this lower threshold given the size of our data.

After estimating Equation 2, we use a more conventional triple-difference model to obtain our best estimates of the effect of temporary work on post-injury outcomes at various time horizons after injury, summarizing the overall effects. In this specification, we replace the event-study terms $\beta_s^{TI} T_i I_i \mathbf{1}\{t-q=s\}$ with a fixed effect $T_i I_i$ for injured workers with indemnity benefits and an interaction between this term and post-injury indicators corresponding to outcomes one, two, and three years post-injury ($s=4,8,12$ quarters post-injury). To reduce computational burden, we limited the sample to one pre-injury quarter ($s = -4$) and these three post-injury quarters ($s = 4, 8, 12$):

$$\begin{aligned} y_{icdqt} =& \mu_{cdqt} + \beta^I I_i + \sum_{s=4,8,12} I_i \beta_s^I \mathbf{1}\{t - q = s\} \\ &+ \beta^{TI} T_i I_i + \sum_{s=4,8,12} \beta_s^{TI} T_i I_i \mathbf{1}\{t - q = s\} \\ &+ \sum_{s=-4,4,8,12} \beta_s^X X_i \mathbf{1}\{t - q = s\} + \varepsilon_{icdqt}. \end{aligned} \tag{3}$$

With the addition of the two-way interaction fixed effect $T_i I_i$ to the model, the β_s^{TI} terms become triple-difference coefficients for $s = 4, 8, 12$ quarters post-injury, providing estimated effects relative to the pre-period ($s = -4$). We report the β^{TI} coefficient as a measure of pre-injury differences.

We also report the triple-difference estimates and test for pairwise equality between the β_s^{TI} estimates to one another to assess whether the post-injury dynamics observed in the event-study model are statistically significant. When analyzing employment as the outcome variable, we estimate Equations 1, 2, and 3 above as linear probability models using OLS. We are also interested in quantifying earnings dynamics after injury, and so we also use the specifications above in Poisson regressions, exponentiating the right-hand side, for quarterly labor earnings. We rely on the sparser model (equation (3)) when estimating earnings dynamics given the computational burden required for Poisson regression with the size of our data and rich set of interactions in the full model. We use Poisson regression for earnings given the skewed nature of this variable (Silva and Tenreyro, 2006).

5 Results

5.1 Employment Dynamics Before and After Injury

We first present unadjusted employment propensities for four groups of injured workers: direct-hires with indemnity benefits, medical only direct-hires, temporary workers with indemnity benefits, and medical only temporary workers. All four trend lines – presented in Figure 1 – peak mechanically at the quarter of injury because workers must be working to experience a workplace injury but were not necessarily working in the other quarters.[16] We observe very different post-injury dynamics between the direct-hires and temporary workers. However, we find that the pre-injury employment trends for these groups are also very different. Four quarters prior to injury, under 70% of the temporary workers are working while nearly 90% of direct-hires are working. Accounting for these differential employment patterns is necessary to distinguish the differential effects of workplace injuries from variation in unobserved attachment to the labor force.

However, Figure 1 also suggests that workers with medical only injuries have similar pre-injury employment patterns as workers with lost-time injuries. The pre-employment levels and trends are similar between injury types within both work arrangements, suggesting that the medical-only groups potentially provide a useful counterfactual for understanding future employment patterns in the absence of an injury requiring missed work.

Figure 1 provides some preliminary, unadjusted evidence of the differential effects of workplace injuries for temporary workers. We see that temporary workers have much worse post-injury outcomes than direct-hires. Some of this difference is due to underlying differences in employment trajectories between the two groups, as we observe from the medical

[16] Since we define "working" as earning more than $200 in a quarter, these propensities are slightly less than 100% in the quarter of injury.

only injuries. However, there are still large differences between the two groups. We discuss magnitudes more formally in the next sections.

5.2 Difference-in-Differences Estimates

Our difference-in-differences strategy compares the relative employment propensities of temporary workers to direct-hires in each quarter relative to injury. We examine these differences for workers receiving indemnity benefits and then, as a way to understand our triple-differences estimates in the next section, for medical only injuries. We estimate equation (1) for both sets of injured workers and present the results graphically.

Figure 2 presents the estimates for those receiving indemnity benefits. In panel A, we include only fixed effects for quarter of injury, time relative to injury, and class codes. Introducing controls for job tenure, age, region, type of injury, wage quartiles, and full- or part-time status in panel B significantly moderates the pre- and post-injury trends. Nonetheless, both panels show a rise in employment before the injury, followed by a sharp relative decline for temporary workers after the injury. In the adjusted figure, this gap is more than 12 percentage points in the third quarter after injury and moderates to 7 percentage points three years after injury.

Figure 3 presents the corresponding difference-in-differences figures for the medical-only claims. Again, we see that introducing controls significantly moderates the pre-injury trend and adjusts post-injury relative employment upward. The employment dynamics depicted in Figure 3 can be thought of as defining the counterfactual used in the triple-difference specification below.[17] Here, we also observe pre-existing differential increases in employment propensities. One advantage of the triple-differences specification is that we account for these underlying differences between temporary and direct-hire workers that exist even prior

[17] We note that the results in the next section are not mechanically equal to the differences between Figures 2 and 3 given the inclusion of control variables in the model which are not fully interacted.

to injury. We observe similar pre-injury trends for both the medical only and indemnity benefit groups, suggesting that these trends represent the natural (differential) progression of employment rates prior to a workplace injury for each work arrangement.

In Figure 3, we also observe a large relative decrease in employment post-injury (between 6 and 8 percentage points), but we do not observe the same recovery in relative employment over the three years post-injury that we saw for indemnity injuries. However, the medical only group also provides a useful counterfactual for the differences in employment trajectories that we might expect between temporary workers and direct-hires: as suggested by the employment trajectories leading up to injury depicted in Figure 1, substantial post-injury differences in employment between temporary and direct-hire workers should be expected due solely to mean-reversion (since there is a mechanical employment peak at time-of-injury) and different employment propensities even if there were no effect of injury on labor market outcomes. The evidence in this section also shows that our triple-difference estimates below are not driven by differentially *positive* employment experiences for temporary workers with medical only claims. Since our triple-difference estimates difference out the losses depicted in Figure 3, they will likely represent a lower bound (in magnitude) on the true effect.

5.3 Triple-Difference Estimates

In the previous section, we provided difference-in-difference estimates for workers suffering injuries requiring lost work time. Those estimates strongly suggested that temporary workers and direct-hires would have different employment trajectories even in the absence of injuries, so we present triple-differences estimates from the specification represented by equation 2. As with our event-study difference-in-difference models, we present our results graphically, showing the full trajectory of relative employment outcomes. Our main results are shown in Figure 4. Panel A presents the unadjusted differences, estimated from a specification without additional covariates (X_i). Panel B includes the control variables interacted with time-relative-to-injury indicators. Coefficient estimates for the control variables are reported in Appendix A.

Figure 4 shows little indication of differential pre-injury employment trends or levels. In fact, we cannot reject the null hypothesis that pre-employment differences remain zero throughout the pre-period. Upon injury, however, we observe large decreases in employment for temporary workers. We estimate that employment falls by 7.1 percentage points more for temporary workers than for direct-hires (relative to their medical only differential) in the first quarter after injury. Over time, however, we find that these differences converge. The differential shrinks to 1.8 percentage points by seven quarters after injury, a meaningful difference but roughly one-quarter the size of the original differential. The gap continues to shrink, eventually disappearing around 3 years after the injury. Overall, we can statistically reject that temporary and direct-hire workers experience the same employment effects throughout the post-injury period. This effect disappears in the long term, but only after substantial short- and medium-term effects. These results are consistent with very large differential employment reductions immediately after injury, followed by gradual adjustments.

We also observe little difference between the two set of estimates depending on whether we condition on a rich set of observable characteristics. The similarity of the Panel A and Panel B estimates suggest that the medical-only injuries are providing an appropriate counterfactual for the underlying employment differences between temporary workers and direct-hires. This contrasts with our difference-in-difference event-study estimates shown in Figures 2 and 3, which were highly sensitive to the inclusion of control variables accounting for observable differences.

To summarize the event-study estimates, we estimate equation 3, which uses our triple-difference identification strategy to estimate the incremental employment and earnings losses due to temporary work at three post-injury time horizons: 4, 8, and 12 quarters after the quarter of injury (i.e., at the end of the first, second, and third full years after the quarter of injury). As discussed in Section 4 above, the baseline (pre-injury) period in this specification is four quarters prior to injury. Equation 3 imposes the assumption that there are no differential pre-injury trends, an assumption corroborated by the event-study estimates presented above.

We also use this triple-difference specification to estimate the effect of temporary worker status on post-injury earnings. When the outcome is quarterly earnings, we estimate an exponential conditional mean specification using Poisson regression due to the heavily skewed nature of the earnings variable and the frequency of observations with zero earnings (13% in the fourth-quarter before injury and 29% in the 12th quarter after injury). Unlike log-linear regression using Ordinary Least Squares (OLS), Poisson regression provides a functional form capable of handling both zero and positive observations while avoiding reliance on the restrictive statistical assumptions implied by log-linear OLS, as discussed in Silva and Tenreyro (2006). Consistency of alternative estimators of the exponential conditional mean model, such as negative binomial regression, requires strong assumptions on the form of conditional heteroskedasticity; Poisson regression, in contrast remains consistent under arbitrary heteroskedasticity as long as the error term is mean-independent of the explanatory variables (see Silva and Tenreyro (2006) and Ciani and Fisher (2019) for more details).

For the full sample, we estimate that temporary workers incur earnings losses of 11.5% in the fourth quarter after injury. This earnings gap shrinks but remains large three years after injury: tests for equality of coefficients across different time horizons confirm that employment losses are significantly different at each time horizon examined, while we fail to reject equality of the earnings losses across adjacent years. Interestingly, while we find no evidence of employment differences three years after injury, we still estimate large earnings differences. This combination of results suggests that temporary workers in the long-term may not experience a disproportionate disemployment effect due to injury, but they may return to lower-paying jobs due to fewer hours (e.g., part-time work) or lower wages because of reduced productivity (or other mechanisms).[18]

In short, we observe rather striking evidence of a large employment reduction for temporary workers relative to a counterfactual based on direct-hires injured in the same job at the same time. Despite the convergence of these post-injury trends over time, we find large effects even two years after the date of the injury. At one year post-injury, we estimate a 3.9 percentage point employment reduction, equivalent to a 6% decrease relative to the baseline employment percentage of direct-hires four quarters after injury. In contrast, the difference-in-differences event-study estimates (Figure 2) suggested an effect that is twice as large.[19] Given the employment risk associated with workplace injuries in general, this evidence suggests that temporary and contract workers are disproportionately affected by workplace injuries even though this effect eventually disappears.

5.4 Sensitivity Analyses

Before examining how workers' compensation benefit payments differ between temporary and direct-hire workers, we discuss a series of sensitivity analyses chosen to assess various threats to our identification strategy.[20]

[18] Unfortunately, we do not observe hours worked or wages, only quarterly earnings.

[19] In the event-study model underlying Figure 2, we estimated that the employment gap between temporary and direct-hire workers was 3.1% four quarters before injury ($\beta^T_{-4} = -0.0312$) and 10.8% four quarters after injury ($\beta^T_{-4} = -0.1084$), implying a differences-in-differences effect of -7.7% instead of our best estimate of -3.9%.

[20] In the Appendix, we also test whether our choice to use survey weights throughout our analyses is materially affecting the results. The estimates in Table A2 show that the unweighted results are similar.

5.4.1 Selection into Workers' Compensation

We only observe injured workers who file a workers' compensation claim, and therefore the interpretation of our results as the effect of injury on employment outcomes requires an assumption that claiming propensities are similar across groups. Injured workers claiming workers' compensation benefits are likely different from non-claiming injured workers. This may present a threat to both the internal and external validity of our study.

If claiming behavior among temporary workers is systematically more (or less) responsive to future labor market outcomes than is claiming behavior among direct-hires, then we might have concerns about internal validity. We note that claiming behavior will only affect our results if it is systematically different across direct-hires and workers in alternative work arrangements, and these differences are not adequately controlled for by differences observed in the medical only sample. In addition, if workers who claim are systematically different from workers who are injured but do not claim, then it may be difficult to extrapolate from our study to characterize the effect of work arrangements on income risk due to injuries not reported to workers' compensation. While limited external validity for workers who do not file workers' compensation claims is an inherent limitation of research based on administrative workers' compensation data, it is important that we consider the threat to internal validity posed by differential selection into injury across work arrangements.

To address this threat to internal validity, we replicate our analysis using only traumatic injuries, defined as injuries caused by external forces.[21] Relative to cumulative injuries

[21] Specifically, we define traumatic injuries to include those with the following IAIABC cause of injury codes reported to the WCIS

- Caught In or Between: machine or machinery (10), object handled (12), collapsing materials (20), caught in, under, or between, not otherwise classified (NOC) (13)
- Striking Against or Stepping On: moving parts of machine (65); object being lifted or handled (66); sanding, scraping, cleaning operations (67); stationary object (68); stepping on sharp object (69); striking against or stepping on, NOC (70)

and other injury types, there is less scope for reporting differences for traumatic issues so selection should be less of a concern for this sample. Figure 5 reproduces our triple-difference event-study estimates for the full sample (Panel A) alongside estimates for traumatic injuries (Panel B). While noisier due to the smaller sample, the results are very similar to the main estimates. In fact, we estimate larger magnitudes for this sample. The stability of the estimates suggests that differential selection is not driving the main results.

5.4.2 Selection into Temporary Work

A primary motivation of our triple-differences analysis is that direct-hires and temporary workers are possibly different in unobserved ways which predict different employment trends. To further account for these possible differences, we replicate our triple-differences analysis while selecting on workers who were *both* direct-hires and temporary workers at some point in the two years prior to their injury. These workers are more similar to each other as they each have recently (as of the time of injury) selected into temporary work. We study the effect of a workplace injury for this sample based on the type of arrangement at the time of injury.

A caveat to this exercise is that many of the benefits of direct-hire work arrangements, such as higher tenure or greater opportunities for advancement, are likely reduced for this direct-hire population. Thus, we consider this test rather conservative since we are potentially eliminating much of the effect that we are interested in. To assess balance across groups in this sample, we examined the average number of quarters before injury that the four categories of switchers spent in direct-hire employment, temporary employment,

- Struck or Injured by: fellow worker, patient, or other person (74), falling or flying object (75), hand tool or machine in use (76), motor vehicle (77), moving parts of machine (78), object being lifted or handled (79), object handled by others (80), struck or injured, NOC (81), animal or insect (85), explosion or flare back (86) or non-employment over the two years preceding the quarter of injury.[22] Switchers who are temporary workers in the quarter of injury have slightly fewer quarters as temporary or direct-hire workers. Instead, they spend an additional half of one quarter in non-employment. Pre-injury employment histories across the groups are very similar and are nearly identical for medical-only and indemnity injuries within each work arrangement.

We present our estimates for the switcher sample in Figure 6, Panel B. Panel A repeats our main estimates. As expected, there are smaller employment effects for the "switchers." However, even for this population, we estimate large and statistically significant immediate reductions in employment for temporary workers relative to similar direct-hire workers. In the first quarter after the injury, temporary workers have an employment rate 4.2 percentage points lower than their direct-hire peers. This gap disappears by two years after the injury. While the relative reductions in employment are smaller for this population, we take this overall post-injury effect as a conservative lower bound on the true effect and as support for the broader evidence that temporary workers experience greater employment reductions than equivalent direct-hires due to workplace injuries. Despite the similarities in these populations in terms of attachment to their firm (and other factors), we estimate large employment disparities after workplace injuries.

5.5 Workers' Compensation Benefits and Uncompensated Earnings Losses

The results presented above indicate that temporary workers face greater earnings losses after workplace injury than do similar direct-hire workers. This difference in income risk conditional on injury raises the question of whether workers' compensation benefits reduce or magnify the gap in outcomes between workers in different work arrangements. In this section,

[22] See Appendix Table A3 for estimates.

we directly examine how adding different categories of workers' compensation benefits to wage and salary income affects this gap.

We construct a total income measure by adding workers' compensation benefits to earnings: the post-injury change in this total income measure captures earnings losses that remain uncompensated by workers' compensation benefits. We use a triple-difference specification similar to Equation 3 to test for differences in uncompensated losses between temporary and direct-hire workers. However, we depart slightly from the framework used in our main earnings and employment estimates by aggregating the total income measures described above to a single post-injury time period covering the full three years after injury. That is, we sum all labor earnings over the first through twelfth quarters after injury and then add benefit payments to this three-year earnings measure.[23] We also sum labor earnings over the year preceding the quarter of injury ($s = -4$ to -1), resulting in a data set with two observations per worker: one capturing annual earnings the year before the injury and one capturing annual earnings plus benefits in the three years after the injury. We work with these aggregated income measures due to the lack of information of benefit payment timing in our data. Although the WCIS provides accurate data on total benefit payments by type of benefit (e.g., TTD, PPD, settlements), the information on benefit timing extracted for this study is not sufficiently detailed to allow us to observe the exact timing of benefit payments at the quarterly frequency.

As above, we use Poisson regression and an exponential conditional mean specification; as a consequence of this specification, we can compare earnings between the one-year pre-injury period and the three-year post-injury period without annualizing post-injury earnings—differences in the length of the periods being compared are absorbed by the (mul-

[23]We do not discount labor earnings because we do not have detailed information on the timing of benefit payments. Similarly, we do not calculate after-tax income due to a lack of information about family structure, non-labor earnings, and other variables needed to impute marginal tax rates given labor earnings.

tiplicative) time effects included in the model. Aggregating the data allows us to abstract from the timing of benefit payments and analyze streams of benefit payments and lump-sum payments in the same estimation framework.[24]

We consider five distinct categories of benefit payments. See Table A1 for summary statistics on these measures.

1. Temporary Disability (TD) Benefits (including Temporary Total and Temporary Partial disability benefits, as well as benefits paid by the employer)
2. Permanent Disability (PD) Benefits (including Permanent Partial Disability, Permanent Partial Disfigurement, and Permanent Total Disability Benefits)
3. Disability Benefit Settlements (including settlement payments specifically labeled as Temporary or Permanent Disability Benefits, including employer-paid settlements)
4. Unspecified Settlements (excluding settlement payments specifically labeled as Medical)
5. Medical Settlements (settlement payments specifically earmarked for future medical payments)

All pre-injury income measures are simply equal to earnings because workers do not receive benefits before injury. Post-injury income measures are constructed by sequentially adding each of the five benefit categories above to earnings.

Ideally, we would be able to compare the differential earnings losses experienced by temporary workers directly to the total amount of indemnity benefits provided. The first three benefit categories unambiguously represent indemnity benefits. As such, they are intended to compensate workers for lost earnings and are not problematic to include in our income measure. The fourth category of benefits, unspecified settlements, is more ambiguous since it represents some unknown combination of indemnity and medical benefits.

[24] This choice was also motivated by a data limitation, which is that we observe the earliest and latest dates of benefit payment but cannot observe the timing of interruptions or changes in benefit payment rates. Instead, we focus on the cumulative amount paid to the worker. Because we use data on 2005-2011 injuries that was collected in 2016, we think right-censoring of benefit payments is likely to affect only a very small minority of cases.

In addition, claims administrators have the option of using this category of benefits to report total settlements without providing further detail on how much of the settlement is attributable to specific benefit categories.[25]

The last category, medical settlements, comprises payments meant to discharge the insurer or employer's statutory obligation to pay for medical care needed over the remainder of the worker's lifetime. It would be inappropriate to compare medical settlements to earnings losses since our data do not contain information about current or future medical spending arising out of the worker's injury: even if medical settlements offset the incremental earnings losses associated with temporary work, we would need to assume—with no evidence—that temporary workers have lower future medical spending needs than observably similar direct-hires with the same injuries in order to conclude that some portion of the medical settlement represents a windfall that could be used to remedy earnings losses. Given the evidence that temporary work is associated with worse health along dimensions other than injury risk (Virtanen et al., 2005), it seems at least as plausible that the opposite would be the case. While the income measure including medical settlements is not an appropriate measure of the workers' compensation system's generosity, we include medical settlements in our estimates in order to provide some indirect evidence on the relative importance of medical and indemnity benefits in the unspecified benefits category.

Table 6 presents estimates of the triple-difference model specified above. We report the interaction of the indicators for temporary workers and indemnity benefits as a test of pre-existing differences. Our estimate of interest is the differential post-injury effect for this group (labeled *Temporary X Indemnity X Post-Injury*). However, the *Indemnity X Post-Injury* interaction provides evidence on the degree to which different categories of benefits

[25] Although the default is for disability benefits to be paid as a stream of biweekly payments, lump-sum settlements are commonly used and thus need to be included when considering benefit adequacy. Settlements can be used to resolve disputes between the worker and the employer, and they can also be issued in the absence of a dispute when the worker is facing financial hardship or otherwise needs access to liquidity.

diminish uncompensated earnings losses for direct-hire workers. The remaining coefficients identify pre-injury differences in earnings between workers with indemnity and medical-only injuries. These estimates will remain constant across models as we add benefits in the post period.

The estimates in the first column of Table 6 indicate that direct-hire workers included in our sample can expect earnings losses of 20.1% over the three years following the injury. The last row of the first column, meanwhile, indicates that temporary workers with indemnity injuries can expect an additional 9.1% reduction in labor earnings. Reading across the last row of the table, we see that temporary disability and permanent disability benefits are effective in reducing the incremental post-injury income loss experienced by temporary workers: this incremental income loss falls from 9.1% to 6.3% when temporary benefits are added and to 4.0% when permanent disability benefits are added. Adding settlement amounts for disability benefits has a small impact on the gap, reducing it to 3.4%.

When unspecified settlements are added, the incremental income loss associated with temporary work disappears, becoming insignificant with a point estimate of 0.59%. While we caveat that we cannot know with certainty if these unspecified settlement payments reflect settlements for indemnity benefits or medical payments, we think it is plausible that these settlements would primarily reflect indemnity benefits, since the final column of Table 6 indicates that adding medical settlements has no impact on our regression estimates for temporary workers. In fact, the main estimate and its standard error are identical with and without medical settlements. If the impact of unspecified settlements was driven by medical settlements, then we might expect to see that temporary workers also received differentially large medical settlements, which was not the case. We also note that DIR counts workers with unspecified settlements as indemnity cases in aggregated reports based on WCIS data.[26]

Taken together, the estimates in Table 6 suggest that indemnity benefits and settlements provided by the workers' compensation system are sufficient to completely undo the incremental pre-tax income losses experienced by temporary workers on average. In part, this pattern may reflect the progressivity of workers' compensation benefits: temporary workers are a lower-wage group than direct-hires, and so weekly benefits as a percentage of wages may be higher for temporary workers even after controlling for quartiles of the weekly wage. In addition to the progressivity of benefits, our findings might also suggest that variation in workers' compensation benefits is driven by differences in earnings losses, which would indicate that workers' compensation is effective at insuring workers against their earnings losses. This might be the case if additional losses for temporary workers are driven by higher temporary disability duration or more severe permanent disability.

As we discuss in the Conclusion, these estimates cannot be interpreted as an indication of benefit adequacy or even a comparison of benefit adequacy between temporary and direct-hire workers: workers' compensation benefits are tax-exempt, and data limitations have prevented us from accounting for differences in tax rates between these two groups. Indeed, direct-hire workers are likely to face higher marginal tax rates than temporary workers, in which case the income differences documented in Table 6 would overestimate differences in after-tax wage replacement rates between the two groups.

6 Conclusion

In this paper, we examined the labor market outcomes of temporary and contract workers after workplace injury. By using a triple-difference identification strategy, we were able to

[26]Source: Department of Industrial Relations. 2018. "Table 12: Subsequent Report of Injury (SROI), with Indemnity, by Month of Injury, 2000 - 2017." Online Resource. Available at https://www.dir.ca.gov/dwc/wcis/WCIS_tables/AggregateFROISROIData/AggregateFROISROIData-Archive.html, Accessed May 10, 2019.

identify differences in post-injury labor market outcomes associated with temporary work while using workers with the same job and work arrangement to control for other differences in labor market dynamics between temporary and direct-hire workers. We found that temporary and contract workers face more substantial income risk—in terms of larger reductions in earnings and employment due to injury—than do observably similar direct-hires injured doing the same job.

Over the three years after injury, temporary workers earn 9.1 percent less than they would have if they were observably identical direct-hires injured doing the same job. This greater income risk faced by temporary workers reflects lower employment in the first years after injury. Although our event-study estimates indicate that the employment of injured temporary workers converges fully to their counterfactual level of employment by three years post-injury, a 9.1 percent difference in earnings over a three-year period represents a substantial degree of additional income risk associated with temporary work. While it is well-established that temporary work is associated with greater risk to health and safety (Benavides et al., 2006; Underhill and Quinlan, 2011) and higher workers' compensation claiming rates (Park and Butler, 2001; Smith et al., 2010; Zaidman, 2017), this study provides the first evidence on how differences in work arrangements influence the extent of income risk workers face conditional on injury.

We also examined differences between temporary and direct-hire workers in the extent of uncompensated earnings losses. We found that workers' compensation indemnity (i.e., cash) benefits—including settlements—were sufficient to offset the increased earnings risk associated with temporary work. Although temporary workers' uncompensated losses remained greater than those experienced by direct-hires when only temporary and permanent disability benefits were included, the addition of settlement payments to our income measure eliminated the disparity in uncompensated losses.

Our findings may have implications for the broader debate about whether and how workers in other alternative work arrangements not covered by many social insurance programs—most notably independent contractors—should be incorporated into existing social insurance arrangements. A proposal by Harris and Krueger (2015) would create a new legal classification that they term the *independent worker*, through which jobs that are currently filled by independent contractors would gain coverage under selected labor laws and social insurance arrangements without establishing a direct-hire relationship between the employer and the employee. Under this proposal, labor intermediaries (including online labor intermediaries) could elect to offer workers' compensation coverage to independent workers without assuming the other legal obligations of employers, effectively providing labor intermediaries with an opportunity to opt into workers' compensation. Other observers, in contrast, have urged the elimination of exclusions from workers' compensation coverage requirements—including the exclusion of independent contractors—both in order to expand access to medical care and cash benefits and in order to strengthen financial incentives for host employers to invest in workplace safety (American Public Health Association, 2017).

The welfare implications of these policy alternatives—the extension of voluntary or mandatory workers' compensation to nonstandard workers who are currently excluded from coverage—turn in large part on whether heterogeneity in risk or preferences between direct-hire and nonstandard workers would lead nonstandard workers to prefer some different insurance arrangement in lieu of workers' compensation. If so, then mandating the expansion of workers' compensation coverage to these additional groups of workers might be less welfare-enhancing than allowing employers the choice to opt in or establish other forms of accident and disability insurance. While our findings on earnings loss show that nonstandard workers face systematically greater income risk, our finding that workers' compensation is effective at equalizing uncompensated losses between temporary and direct-hire workers provides suggestive evidence that access to the workers' compensation system is likely to be valuable for nonstandard workers as they seek to offset the greater earnings risk they face after workplace injury. We caution that this policy implication is tentative, as we were not able to estimate wage replacement rates or conduct welfare analysis with the data on hand. Furthermore, our study did not examine the health and safety implications of expanding workers' compensation coverage to more nonstandard workers. MacEachen et al. (2016) clearly documented the potential for perverse incentives to arise from the use of experience-rating in the context of alternative work arrangements, and policymakers considering expansion of workers' compensation coverage would need to consider whether changes in experience-rating are necessary to promote safety.

Our findings may also be relevant to ongoing debate in California, where a major 2018 state Supreme Court decision in the matter of *Dynamex Operations West, Inc. v. Superior Court of Los Angeles* (known as the *Dynamex* decision) shifted the burden of proof in employee misclassification cases to the employer and established a simplified test for misclassification. These changes are likely to require that many current independent contractors either be reclassified as direct-hire employees or replaced with workers in other nonstandard work arrangements (such as through temporary or contract firms). Our findings at least provide some reassurance that the existing workers' compensation system appears capable of providing compensation to these workers that offsets the greater earnings risk they face in comparison to direct-hires. This analysis may be relevant to the current legislative debate over whether to codify the *Dynamex* decision, especially if, as seems likely, many workers who are currently misclassified as independent contractors would transition to temporary or contract worker status rather than becoming direct-hires.

Our study had some limitations that point to the need for further research. One such limitation is that we were unable to observe non-labor income or program participation outcomes outside of workers' compensation. Direct measurement of injured workers' take-up of other social insurance and income support programs such as Unemployment Insurance, the Supplemental Nutrition Assistance Program, or California's State Disability Insurance program is needed to document how the differences in earnings risk estimated here might translate into uninsured income risk for families and, potentially, spillover costs for state government. Longer-term earnings and employment outcomes would be of interest in follow-up research. We also caution that we did not conduct a formal welfare analysis or estimate wage replacement rates, and many more questions need to be answered to rigorously identify the best model for providing social insurance to nonstandard workers. Notwithstanding these limitations, our findings that temporary workers face additional employment and earnings risk after a workplace injury represent a first step toward addressing these questions in future work.

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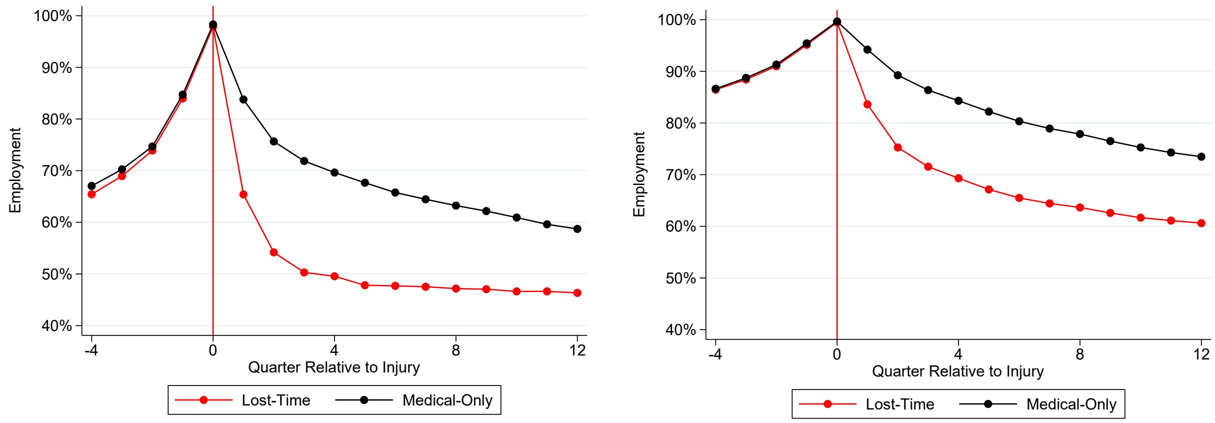
Figures

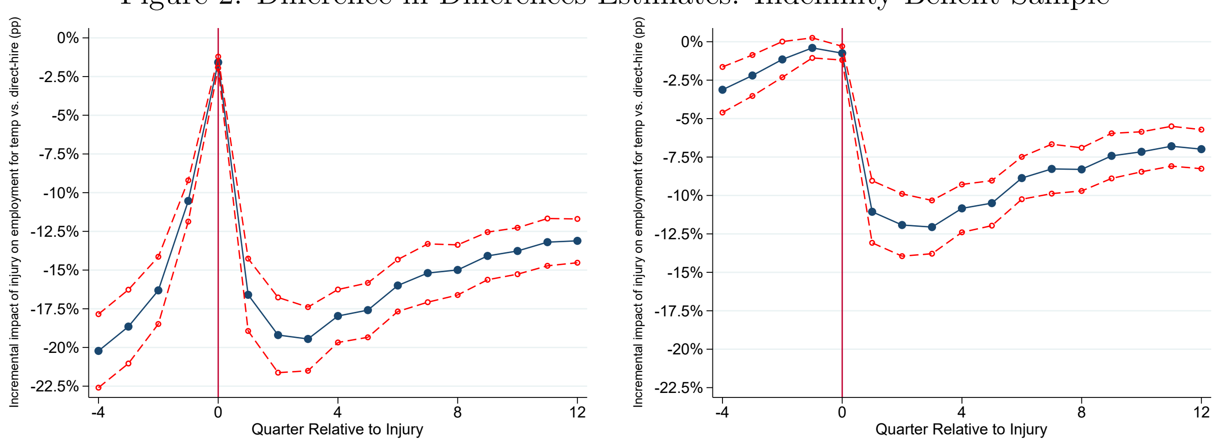
Figure 1: Employment Trends Relative to Time of Injury



A: Temporary/Contract B: Direct-Hire

Notes: Authors' calculations, 2005-2011 WCIS-EDD. N = 1,278,390 injured workers.

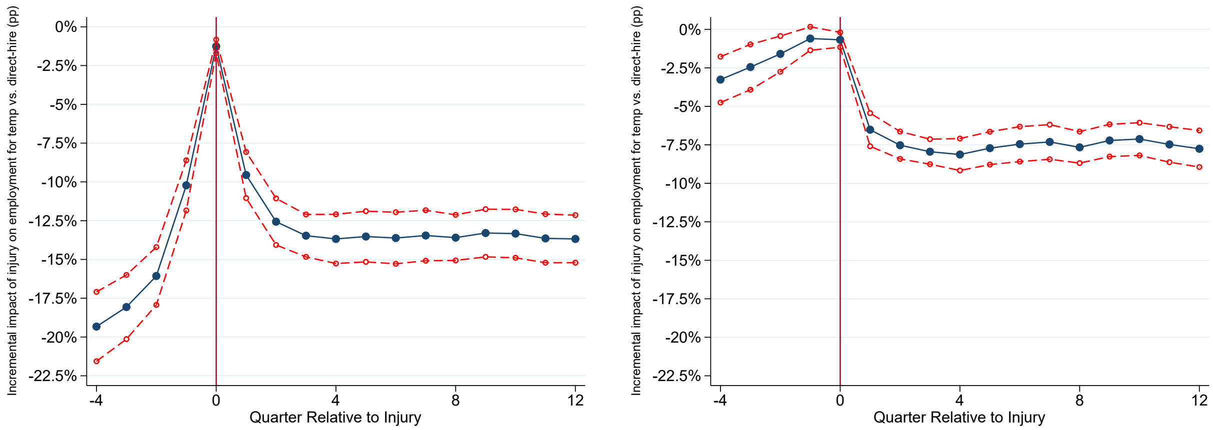
Figure 2: Difference-in-Differences Estimates: Indemnity Benefit Sample



A: Unadjusted Estimates B: Adjusted for Covariates

Notes: Point estimates from difference-in-differences event study plotted along with 95% confidence intervals. Interactions based on class code, calendar time of injury, and time-relative-to-injury included. In panel B, controls (interacted with time-relative-to-injury) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable. Confidence intervals adjusted for clustering by class code. N = 303,240 injured workers.

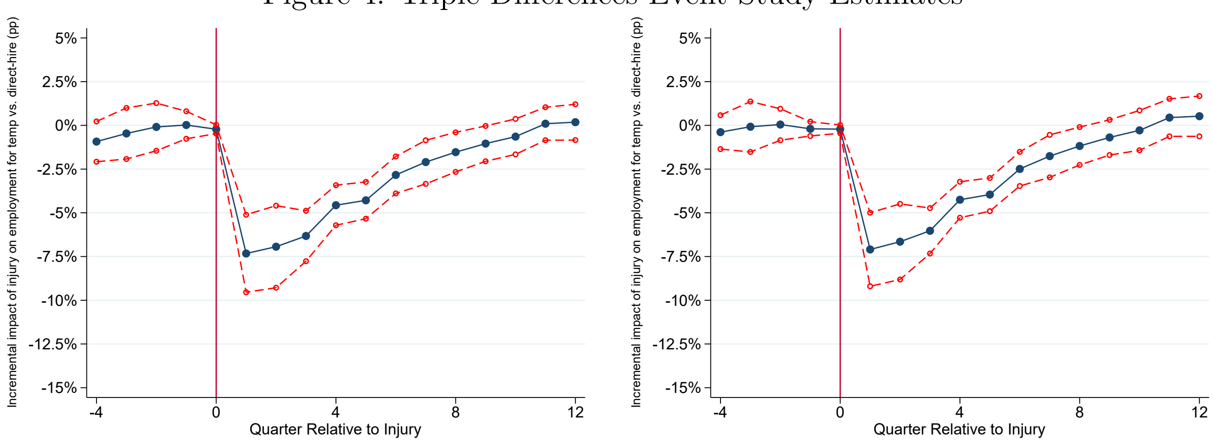
Figure 3: Difference-in-Differences Estimates: Medical Only Sample



A: Unadjusted Estimates B: Adjusted for Covariates

Notes: Point estimates from difference-in-differences event study plotted along with 95% confidence intervals. Interactions based on class code, calendar time of injury, and time-relative-to-injury included. In panel B, controls (interacted with time-relative-to-injury) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable. Confidence intervals adjusted for clustering by class code. N = 975,150 injured workers.

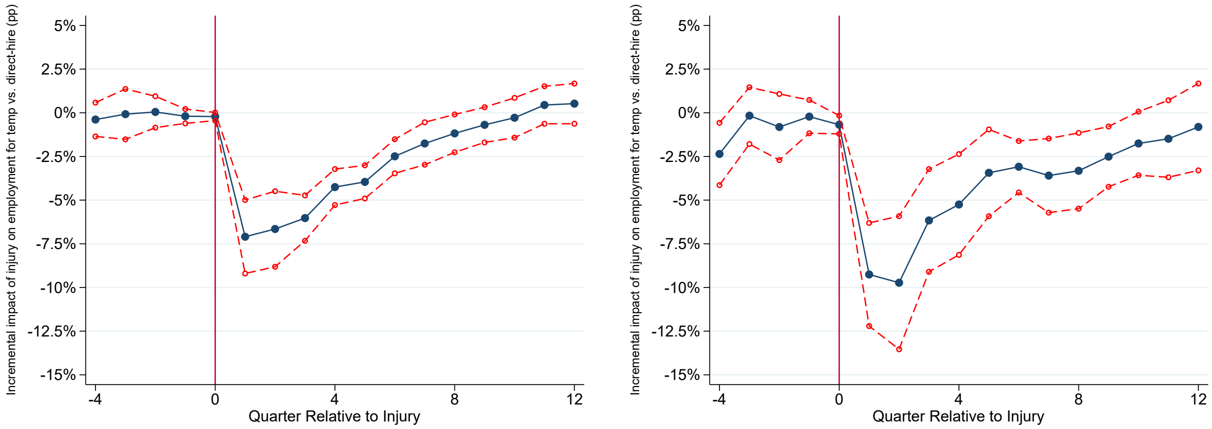
Figure 4: Triple Differences Event-Study Estimates



A: Unadjusted Estimates B: Adjusted for Covariates

Notes: Panel A shows the unadjusted difference in the employment gap between workers receiving indemnity benefits and medical-only claims as shown in Figure 1, Panel A to the same gap shown in Figure 1, Panel B. In Panel B, we show the equivalent estimates while adjusting for covariates as in equation (2). Sample includes N = 1,278,390 injured workers. Point estimates from triple-differences event study plotted along with 95% confidence intervals. Specification includes all two-way interactions between work arrangements, indemnity benefit vs. medical only sample, and time-relative-to-injury. Interactions based on class code, work arrangements, calendar time of injury, and time-relative-to-injury also included. Controls (interacted with time-relative-to-injury) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable. Confidence intervals adjusted for clustering by class code.

Figure 5: Traumatic Injuries

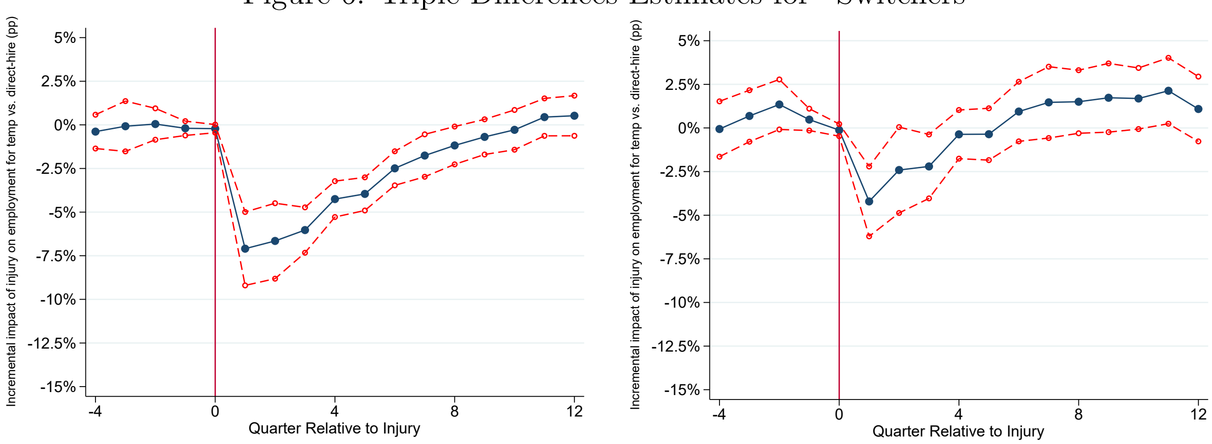


A: Full Sample

B: Traumatic Injuries

Notes: Panel A repeats the same results as shown in Figure 4, Panel B. Panel B limits the sample to workers incurring traumatic injuries. Panel A sample includes N = 1,278,390 injured workers. Panel B sample includes N = 264,702 injured workers. As in the main results, workers are categorized by their status at time of injury. In both panels, point estimates from triple-differences event study plotted along with 95% confidence intervals. Specification includes all two-way interactions between work arrangements, indemnity benefit vs. medical only sample, and time-relative-to-injury. Interactions based on class code, work arrangements, calendar time of injury, and time-relative-to-injury also included. Controls (interacted with time-relative-to-injury) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable. Confidence intervals adjusted for clustering by class code.

Figure 6: Triple Differences Estimates for "Switchers"



A: Full Sample

B: Switchers

Notes: Panel A repeats the same results as shown in Figure 4, Panel B. Panel B limits the sample to workers who worked both as direct-hires and as temporary/contract workers in the year before injury (N=96,630). As in the main results, workers are categorized by their status at time of injury. In both panels, point estimates from triple-differences event study plotted along with 95% confidence intervals. Specification includes all two-way interactions between work arrangements, indemnity benefit vs. medical only sample, and time-relative-to-injury. Interactions based on class code, work arrangements, calendar time of injury, and time-relative-to-injury also included. Controls (interacted with time-relative-to-injury) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable. Confidence intervals adjusted for clustering by class code.

Tables

Table 1: Temporary Work Shares by Occupation in California

Title	**SOC Code**	**Number of Workers**	**Share of Temporary Employment**	**Cumulative Share**
Laborers and Freight, Stock, and Material Movers, Hand	53-7062	89,130	23.3	23.3
Packers and Packagers, Hand	53-7064	24,850	6.5	29.8
Assemblers and Fabricators, All Other, Including Team Assemblers	51-2098	14,960	3.9	33.7
Production Workers, All Other	51-9199	14,150	3.7	37.4
Customer Service Representatives	43-4051	12,250	3.2	40.6
Packaging and Filling Machine Operators and Tenders	51-9111	11,090	2.9	43.5
Office Clerks, General	43-9061	10,530	2.8	46.2
Secretaries and Administrative Assistants, Except Legal, Medical, and Executive	43-6014	9,810	2.6	48.8
Personal Care Aides	39-9021	8,180	2.1	50.9
Industrial Truck and Tractor Operators	53-7051	7,500	2.0	52.9

Source: Bureau of Labor Statistics. 2018. "May 2017 OES Estimates." Available at `https://www.bls.gov/oes/2017/may/oes_ca.htm` as of April 17, 2019.

Table 2: Temporary Work Shares among Injured Workers

Class Code	**Total Injury Count**	**Temporary/PEO Injury Count**	**Share Involving Temporary/PEO Workers**
Warehouses - General Merchandise	27,476	7,513	27.3%
Fruit - Dried Fruit Packing	1,216	275	22.6%
Carpentry - Not Otherwise Classified - Low Wage	5,556	1,128	20.3%
Garbage, Ashes or Refuse Dump Operations	2,722	397	14.6%
Fruit - Citrus Fruit Packing	1,668	236	14.1%
Stores - Clothing, Dry Goods - Wholesale	3,472	488	14.1%
Warehouses - Self Storage	1,496	207	13.8%
Medical Instrument Manufacturing - Electronic	2,367	308	13.0%
Printed Circuit Board Assembling	1,059	122	11.5%
Instrument Manufacturing - Electronic	14,169	1629	11.5%

Source: Authors' calculations, 2005-2011 WCIS. Table lists top 10 California class codes by proportion of injuries occurring among temporary workers, defined as those with a payroll employer in NAICS industries 56132 or 56133.

Table 3: Summary Statistics: Characteristics of Temporary and Direct-Hire Workers with Indemnity Claims

	Direct-Hire	**Temporary**
Job Characteristics		
Weekly wage	619.95	477.24
Full year tenure	67.10%	25.60%
Full time	78.54%	67.29%
Demographics		
Female	45.85%	38.44%
Age	41.43	38.96
Cause of Injury		
Caught in	2.77%	5.19%
Rubbed by	0.61%	1.12%
Striking	3.26%	4.93%
Struck by	9.28%	12.19%
Strain	43.29%	41.71%
Fall	19.84%	18.86%
Cut	4.89%	4.26%
Crash	2.88%	1.95%
Burn	1.84%	1.40%
Miscellaneous	11.35%	8.39%
Nature of Injury		
Specific injury	88.25%	89.51%
Cumulative injury	8.02%	4.91%
Multiple injury	3.09%	4.98%
Other injury	0.64%	0.60%
Sample Size	290,007	13,233

Source: Authors' calculations, 2005-2011 WCIS, Indemnity injuries only. Weekly wage is nominal weekly wage variable reported on workers' compensation claim for purposes of calculating benefits. Temporary workers defined as those with a payroll employer in NAICS industries 56132 or 56133.

Table 4: Summary Statistics: Labor Market Outcomes for Temporary and Direct-Hire Workers with Indemnity Claims

	Direct-Hire		**Temporary**	
	Value	% of baseline	Value	% of baseline
Pre-injury Earnings/Employment				
Earnings in Year Before Injury	$35,911.61	100.00%	$18,792.10	100.00%
Employed 1 Year (4Q) Before Injury?	86.46%	100.00%	65.43%	100.00%
Post-Injury Earnings				
1st Year (4Q) Post-Injury Earnings	$27,280.26	75.97%	$13,699.02	72.90%
2nd Year (8Q) Post-Injury Earnings	$25,876.61	72.06%	$13,427.88	71.45%
3rd Year (12Q) Post-Injury Earnings	$24,809.93	69.09%	$13,523.35	71.96%
Post-Injury Employment				
1st Year (4Q) Post-Injury Employment	69.31%	80.17%	49.56%	75.74%
2nd Year (8Q) Post-Injury Employment	63.65%	73.62%	47.18%	72.10%
3rd Year (12Q) Post-Injury Employment	60.61%	70.11%	46.34%	70.82%

Source: Authors' calculations, 2005-2011 WCIS-EDD, Indemnity injuries only. N = 290,007 for direct-hire workers. N = 13,233 for temporary workers. Temporary workers defined as those with a payroll employer in NAICS industries 56132 or 56133. Earnings adjusted to real 2014$ using the CPI-U.

Table 5: Effects of Temporary Worker Status on Post-Injury Labor Market Outcomes: Triple-Difference Regression Estimates

Time Post-Injury	Employment	Earnings
Year 1 (4Q)	-0.0388***	-0.115***
	(0.00682)	(0.0192)
Year 2 (8Q)	-0.00815	-0.0822***
	(0.00734)	(0.0195)
Year 3 (12Q)	0.00890	-0.0519**
	(0.00576)	(0.0192)
Year 1 = Year 2		
Wald Test Statistic	28.93	2.20
p-value	0	0.1380
Year 1 = Year 3		
Wald Test Statistic	46.51	8.33
p-value	0	0.0039
Year 2 = Year 3		
Wald Test Statistic	8.98	1.89
p-value	0.0040	0.1691
N	5,113,560	5,113,560

Notes: ***Significance .1%, ** Significance 1%, * Significance 5%. Employment effects estimated using OLS. Earnings effects estimated using fixed-effects (conditional) Poisson regression. Standard errors in parentheses adjusted for clustering by class code (62 clusters). 524 observations (131 workers) were dropped from the Poisson regressions because they belonged to fixed-effects cells (4-way interactions between class code, quarter of injury, temporary worker status, and calendar time) that had all zero outcomes or contained only a single observation, and thus did not contribute to the pseudo-likelihood function. Wald test statistic for employment is distributed as $F_{1,61}$. Test statistic for earnings outcomes is distributed as χ^2_1. Specification includes all two-way interactions between work arrangements, indemnity benefit vs. medical only sample, and time-relative-to-injury. Model includes fixed effects for interactions of class code, work arrangements, calendar time of injury, and time-relative-to-injury. Additional controls (interacted with time-relative-to-injury) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable.

Table 6: Uncompensated Earnings Losses Over Three Years Post-Injury: Triple-Difference Estimates

	Forms of Income Included in Outcome Measure					
Earnings	Y	Y	Y	Y	Y	Y
TD Benefits		Y	Y	Y	Y	Y
PD Benefits			Y	Y	Y	Y
Indemnity Settlements				Y	Y	Y
Unspecified Settlements					Y	Y
Medical Settlements						Y
Indemnity	-0.0707***	-0.0707***	-0.0707***	-0.0707***	-0.0707***	-0.0707***
	(0.0019)	(0.0019)	(0.0019)	(0.0019)	(0.0019)	(0.0019)
Indemnity X Post-Injury	-0.209***	-0.125***	-0.0821***	-0.0709***	-0.0582***	-0.0440***
	(0.0040)	(0.0034)	(0.0033)	(0.0032)	(0.0033)	(0.0033)
Temporary X Indemnity	-0.00443	-0.00443	-0.00443	-0.00443	-0.00443	-0.00443
	(0.0087)	(0.0087)	(0.0087)	(0.0087)	(0.0087)	(0.0087)
Temporary X Indemnity X Post-Injury	**-0.0907*** **	**-0.0625*** **	**-0.0404** **	**-0.0340***	**0.0059**	**0.0059**
	(0.0159)	(0.0145)	(0.0140)	(0.0140)	(0.0138)	(0.0138)

Notes: ***Significance .1%, ** Significance 1%, * Significance 5%. N = 1,278,390 injured workers. Poisson regression estimates. Outcomes refer to the total earnings and benefits received in the three years post-injury ("Post-Injury") or the one year pre-injury. Specification includes all two-way interactions between work arrangements, indemnity benefit vs. medical only sample, and a post-injury indicator. Interactions based on class code, work arrangements, calendar time of injury, and post-injury. Controls (interacted with both pre and post indicators) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable. Standard errors in parentheses adjusted for clustering by class code. We note that estimates in the the first and third rows are mechanically identical because the addition of benefits has no impact on pre-injury income.

A Appendix: Supplementary Tables and Figures

Table A1 presents summary statistics on the benefit payment measures that we use to analyze uncompensated earnings losses. The sample is limited to injured workers who receive indemnity benefits, which we define as receipt of any benefit payments other than medical benefits. The first panel reports the average total benefit payment when we sequentially add different categories of benefits to our measure of total benefit payments. The second panel reports the percentage of workers with indemnity injury receiving each benefit type. Temporary/contract workers are about three times as likely as direct-hires to receive an unspecified settlement payment. Otherwise, patterns of benefit receipt are broadly similar between the two groups.

Table A1: Mean Benefit Payments and Incidence by Benefit Type and Work Arrangement

Cumulative Benefit Amounts ($)	**Direct-Hire**	**Temporary/Contract**
Temporary Disability Benefits	7,077.19	4,995.61
+ Permanent Disability Benefits	10,915.52	8,166.09
+ Disability Benefit Settlements	11,909.21	9,037.82
+ Unspecified Settlements	13,127.84	11,803.31
+ Medical Settlements	14,442.03	12,585.15
Percentage Receiving Benefits		
% with TTD benefits	87.99%	83.02%
% with PPD benefits	37.39%	34.70%
% with a TTD or PPD settlement	8.33%	7.97%
% with an unspecified settlement	6.70%	20.16%
% with a medical settlement	8.09%	5.34%
Cumulative Percentage Receiving Benefits		
% with TTD benefits	87.99%	83.02%
% with PPD benefits	96.49%	91.32%
% with a TTD or PPD settlement	98.44%	93.46%
% with an unspecified settlement	99.99%	99.99%
% with a medical settlement	100.00%	100.00%
Number of Observations	290,007	13,233

Table A2 presents additional triple-difference regression estimates to assess the sensitivity of our main results to selection into injury or to the use of sampling weights. Column 1 reproduces our main employment estimates from Column 1 of Table 5. Column 2 shows results for traumatic injuries only, as described in Section 5. The coefficients are less precisely estimated, but the point estimates are close to estimates from the full sample. Estimates for the earnings models, reported in Columns 5-6, also show that results for the traumatic injuries sample are very similar to results for the full sample. Finally, Columns 3-4 show that unweighted estimates are also very close to our weighted estimates.

Table A2: Sensitivity Analyses: Triple-Difference Regression Estimates

	Employment		Employment (unweighted)		Earnings	
Time Post-Injury	All	Traumatic	All	Traumatic	All	Traumatic
Year 1 (4Q)	-0.0388***	-0.0292*	-0.0388***	-0.0293*	-0.115***	-0.09**
(se)	(0.00682)	(0.01570)	(0.00676)	(0.01590)	(0.0192)	(0.04160)
Year 2 (8Q)	-0.00815*	-0.00989	-0.0083*	-0.00946	-0.0822***	-0.0947**
(se)	(0.00734)	(0.01070)	(0.00745)	(0.01100)	(0.0195)	(0.04250)
Year 3 (12Q)	0.0089	0.0152	0.00906	0.0144	-0.0519**	-0.0495
(se)	(0.00576)	(0.00961)	(0.00576)	(0.00983)	(0.0192)	(0.04230)
Year 1 = Year 2						
Wald Test Statistic	28.93	3.47	28.72	3.55	2.20	0.01
p-value	0	0.0673	0	0.0641	0.1380	0.9155
Year 1 = Year 3						
Wald Test Statistic	46.51	4.09	47.11	3.76	8.33	0.85
p-value	0	0.0476	0	0.0571	0.0039	0.3572
Year 2 = Year 3						
Wald Test Statistic	8.98	2.67	9.19	2.33	1.89	1.02
p-value	0.004	0.1075	0.0022	0.1322	0.1691	0.3134
N (Injured Workers)	1,278,390	264,702	1,278,390	264,702	1,278,390	264,702
N (Observations)	5,113,560	1,058,808	5,113,560	1,058,808	5,113,560	1,058,808

Notes: ***Significance .1%, ** Significance 1%, * Significance 5% Employment effects estimated using OLS. Earnings effects estimated using fixed-effects (conditional) Poisson regression. Standard errors in parentheses adjusted for clustering by class code (62 clusters). Model includes fixed effects for interactions of class code, work arrangements, calendar time of injury, and time-relative-to-injury. Additional controls (interacted with time-relative-to-injury) include age-gender interactions, job tenure, type of injury, geographic region, full-time/part-time status (pre-injury), and quartiles of the administrative workers' compensation weekly wage variable. Specification includes all two-way interactions between work arrangements, indemnity benefit vs. medical only sample, and time-relative-to-injury. Observations dropped from Poisson regressions reflect fixed-effects cells (4-way interactions between class code, quarter of injury, temporary worker status, and calendar time) that had all zero outcomes or contained only a single observation, and thus did not contribute to the pseudo-likelihood function. Test statistic for employment is distributed as $F_{1,61}$. Test statistic for earnings outcomes is distributed as χ^2_1.

Type of Worker	Quarters as Temp	Quarters as Direct Hire	Quarters Not Employed
Direct-hire in QOI, medical-only	2.22	4.34	1.44
Temp in QOI, medical-only	2.06	3.98	1.96
Direct-hire in QOI, indemnity	2.23	4.24	1.53
Temp in QOI, medical-only	2.14	3.81	2.05

Table A3: Composition of Switcher Sample Before Injury

Notes: QOI = quarter of injury. N=96,630 injured workers.

Table A3 describes pre-injury employment patterns for the sample of injured workers who switched work arrangements (from temporary to direct-hire or vice versa) in the two years prior to injury. Specifically, the table shows the average number of quarters before injury that the four categories of switchers spent in direct-hire, temporary, or non-employment. As described in the text, switchers who are temporary workers in the quarter of injury have slightly fewer quarters as temporary or direct-hire workers. Instead, they spend an additional half of one quarter in non-employment.

Table A4 presents Poisson regression coefficients for control variables included in our main triple-difference regression model for earnings. Control variables are fully interacted with time relative to injury. These time-varying coefficients are indicated in this table by the column header. Because control variables are fully interacted with time relative to injury, post-injury coefficients represent contemporaneous differences in earnings relative to the excluded group, not changes relative to the pre-injury period. The conditional Poisson estimator controls for four-way interactions of class code, work arrangements, calendar time of injury, and time-relative-to-injury, which are not reported.

The coefficients in Table A4 indicate plausible cross-sectional patterns of earnings, such as a gender gap between men and women and an inverse-U shape over the life cycle. Regional differences in earnings between urban and rural regions and between the Bay Area and other cities are also apparent. Cause of Injury and Nature of Injury codes are defined relative to very severe types of injuries (burn/scald and cumulative trauma), so the higher post-injury coefficients for the included categories are a reflection of more severe earnings losses in the excluded group, as reported in Dworsky et al. (2018). A similar phenomenon explains the positive coefficients on the included job tenure categories. A U-shaped relationship between pre-injury wages and earnings losses is also consistent with Dworsky et al. (2018).

Table A4: Coefficients on Control Variables, Triple-Difference Poisson Regression Models

	Pre-Injury Year	Post-Injury Years		
	Year -1 (-4Q)	Year 1 (4Q)	Year 2 (8Q)	Year 3 (12Q)
Age (Excluded: Age 15-24)				
25-34	0.425***	0.286***	0.259***	0.228***
	(0.00455)	(0.00450)	(0.00494)	(0.00509)
35-44	0.531***	0.355***	0.319***	0.278***
	(0.00617)	(0.00584)	(0.00612)	(0.00631)
45-54	0.557***	0.352***	0.301***	0.241***
	(0.00706)	(0.00652)	(0.00682)	(0.00706)
55-64	0.536***	0.254***	0.131***	-0.00132
	(0.00747)	(0.00685)	(0.00738)	(0.00785)
65-70	0.422***	0.00243	-0.219***	-0.442***
	(0.0103)	(0.0119)	(0.0141)	(0.0176)
Age-Female Interactions				
Female*25-34	-0.148***	-0.146***	-0.162***	-0.176***
	(0.00395)	(0.00443)	(0.00493)	(0.00519)
Female*35-44	-0.155***	-0.150***	-0.152***	-0.154***
	(0.00484)	(0.00550)	(0.00595)	(0.00630)
Female*45-54	-0.139***	-0.132***	-0.131***	-0.136***
	(0.00607)	(0.00709)	(0.00762)	(0.00818)
Female*55-64	-0.0979***	-0.0677***	-0.0523***	-0.0478***
	(0.00729)	(0.00783)	(0.00895)	(0.00955)
Female*65-70	-0.0745***	-0.0473**	-0.0512**	-0.0761***
	(0.0116)	(0.0147)	(0.0175)	(0.0215)
Region of State (Excluded: Bay Area)				
Central Coast	-0.0825***	-0.118***	-0.121***	-0.125***
	(0.00478)	(0.00564)	(0.00577)	(0.00603)
Central Valley	-0.167***	-0.210***	-0.212***	-0.208***
	(0.00498)	(0.00603)	(0.00632)	(0.00655)
Eastern Sierra Foothills	-0.118***	-0.162***	-0.177***	-0.187***
	(0.00618)	(0.00733)	(0.00861)	(0.00891)
Inland Empire	-0.0901***	-0.150***	-0.161***	-0.165***
	(0.00370)	(0.00409)	(0.00424)	(0.00433)
Los Angeles	-0.103***	-0.148***	-0.152***	-0.153***
	(0.00353)	(0.00403)	(0.00413)	(0.00422)
N. Sacramento Valley	-0.201***	-0.254***	-0.257***	-0.265***

	(0.00780)	(0.00913)	(0.00995)	(0.00998)
North State-Shasta	-0.246***	-0.303***	-0.323***	-0.338***
	(0.00803)	(0.00916)	(0.0101)	(0.0101)
Sacramento Valley	-0.147***	-0.179***	-0.190***	-0.196***
	(0.00517)	(0.00587)	(0.00660)	(0.00677)
San Diego	-0.115***	-0.136***	-0.144***	-0.144***
	(0.00452)	(0.00513)	(0.00536)	(0.00549)
Cause of Injury (Excluded: Burn/Scald)				
Caught In	0.0335***	0.0466***	0.0434***	0.0522***
	(0.00609)	(0.00642)	(0.00660)	(0.00685)
Crash	0.121***	0.0937***	0.0875***	0.0931***
	(0.00899)	(0.0117)	(0.0120)	(0.0121)
Cut	0.0358***	0.0571***	0.0541***	0.0575***
	(0.00575)	(0.00598)	(0.00635)	(0.00640)
Fall	0.0344***	-0.000654	-0.00763	-0.00632
	(0.00528)	(0.00587)	(0.00591)	(0.00613)
Miscellaneous	0.0435***	-0.0745***	-0.0741***	-0.0661***
	(0.00502)	(0.00649)	(0.00658)	(0.00666)
Rubbed by	0.0126	-0.0506***	-0.0408***	-0.0361**
	(0.00939)	(0.0111)	(0.0123)	(0.0140)
Strain	0.0277***	-0.0293***	-0.0303***	-0.0253***
	(0.00504)	(0.00556)	(0.00559)	(0.00570)
Striking	0.00981	-0.00383	-0.00778	-0.00491
	(0.00555)	(0.00572)	(0.00606)	(0.00634)
Struck by	0.0193***	0.00447	0.000485	0.00680
	(0.00586)	(0.00585)	(0.00612)	(0.00643)
Nature of Injury (Excluded: Cumulative Trauma)				
Infectious	0.0951***	0.344***	0.336***	0.328***
	(0.0222)	(0.0197)	(0.0208)	(0.0184)
Multiple	-0.00852	0.0541***	0.0341***	0.0318***
	(0.00543)	(0.00837)	(0.00934)	(0.00922)
Other Exposure	0.00451	0.167***	0.142***	0.144***
	(0.00895)	(0.0133)	(0.0147)	(0.0140)
Respiratory	0.0130	0.161***	0.145***	0.128***
	(0.0110)	(0.0144)	(0.0179)	(0.0172)
Specific	-0.00762	0.150***	0.137***	0.130***
	(0.00441)	(0.00768)	(0.00801)	(0.00725)
Pre-Injury Weekly Wage Quartile (Excluded: First Quartile)				
Second Quartile	-0.268***	-0.263***	-0.263***	-0.257***
	(0.00525)	(0.00539)	(0.00534)	(0.00571)

Third Quartile	-0.238***	-0.255***	-0.258***	-0.250***
	(0.00860)	(0.00960)	(0.00981)	(0.0102)
Fourth Quartile	0.260***	0.265***	0.273***	0.283***
	(0.00570)	(0.00607)	(0.00623)	(0.00646)
Full-Time/Part-Time (Excluded: Part-Time)				
Full-Time	0.175***	0.196***	0.175***	0.152***
	(0.00718)	(0.00730)	(0.00727)	(0.00685)
Job Tenure in Quarters Before Injury (Excluded: 0 Quarters)				
1	0.0901***	0.142***	0.134***	0.125***
	(0.00981)	(0.00655)	(0.00674)	(0.00644)
2	0.0990***	0.213***	0.205***	0.203***
	(0.0107)	(0.00648)	(0.00676)	(0.00681)
3	0.0182	0.261***	0.253***	0.248***
	(0.0120)	(0.00689)	(0.00686)	(0.00694)
4+	0.801***	0.460***	0.462***	0.456***
	(0.00985)	(0.00680)	(0.00678)	(0.00693)

***Significance .1%, ** Significance 1%, * Significance 5%
Table reports fixed-effects (conditional) Poisson regression coefficients for control variables from earnings model reported in Table 5 of paper. In addition to variables reported in table and triple-difference design variables reported in Table 5, model includes fixed effects for interactions of class code, work arrangements, calendar time of injury, and time-relative-to-injury. Specification includes all two-way interactions between work arrangements, indemnity benefit vs. medical only sample, and time-relative-to-injury. Standard errors in parentheses adjusted for clustering by class code (62 clusters).

B Appendix: List of Class Codes in Analysis Sample

0016:ORCHARDS - CITRUS AND DECIDUOUS FRUITS

0042:LANDSCAPE GARDENING

0050:FARM MACHINERY OPERATION

2003:BAKERIES AND CRACKER MFG

2107:FRUIT - FRESH FRUIT PACKING

2108:FRUIT - CITRUS FRUIT PACKING

2109:FRUIT - DRIED FRUIT PACKING

2111:FRUIT OR VEGETABLE PRESERVING

2142:WINERIES

2501:CLOTHING MFG

2812:CABINET MFG - WOOD

3060:DOOR OR WINDOW MFG - METAL OR PLASTIC

3179:ELECTRICAL APPARATUS MFG

3507:MACHINERY OR EQUIPMENT MFG

3572:MEDICAL INSTRUMENT MFG - ELECTRONIC

3577:PRINTED CIRCUIT BOARD ASSEMBLING

3632:MACHINE SHOPS - NOC

3681:INSTRUMENT MFG - ELECTRONIC

4299:PRINTING - ALL OTHER EMPLOYEES

4354:PRINTED CIRCUIT BOARD MFG

4478: PLASTIC GOODS MANUFACTURING

5183:PLUMBING - LOW WAGE

5190:ELECTRICAL WIRING - LOW WAGE

5201:CONCRETE WORK - SIDEWALKS - LOW WAGE

5213:CONCRETE CONSTRUCTION

5348:TILE, STONE, MOSAIC OR TERRAZZO WORK

5403:CARPENTRY - NOC - LOW WAGE

5474:PAINTING OR DECORATING - LOW WAGE

5552:ROOFING - LOW WAGE

6504:FOOD PRODUCTS MFG OR PROCESSING

7198:PARCEL DELIVERY COMPANIES

7219:TRUCKING FIRMS

7382:BUS OR LIMOUSINE OPERATIONS

7610:RADIO TELEVISION BROADCASTING STATION

8008:STORES - CLOTHING AND DRY GOODS - RETAIL

8017:STORES - RETAIL

8018:STORES - WHOLESALE

8031:STORES - MEAT, FISH OR POULTRY - RETAIL

8032:STORES - CLOTHING, DRY GOODS - WHOLESALE

8046:STORES - AUTOMOBILE ACCESSORIES

8062:STORES - COMPUTERS

8232:LUMBERYARDS - COMMERCIAL

8290:WAREHOUSES - SELF STORAGE

8291:WAREHOUSES - COLD STORAGE

8292:WAREHOUSES - GENERAL MERCHANDISE

8742:SALESPERSONS - OUTSIDE

8808:BANKS

8810:CLERICAL OFFICE EMPLOYEES

8827:HOMEMAKER SERVICES

8829:NURSING HOMES

8834:PHYSICIANS

8859:COMPUTER PROGRAMMING OR SOFTWARE DEVELOPMENT

9008:JANITORIAL SERVICES - BY CONTRACTOR

9009:BUILDING OPERATION - COMMERCIAL

9011:APARTMENT OR CONDOMINIUM COMPLEX OPERATION - ALL OTHER EMPLOYEES

9015:BUILDING OPERATION

9043:HOSPITALS

9050:HOTELS

9070:RESIDENTIAL CARE FACILITY - ELDERLY

9079:RESTAURANTS OR TAVERNS

9403:GARBAGE, ASHES OR REFUSE COLLECTING

9424:GARBAGE, ASHES OR REFUSE DUMP OPERATIONS