NBER WORKING PAPER SERIES

DO TEMPORARY WORKERS EXPERIENCE ADDITIONAL EMPLOYMENT AND EARNINGS RISK AFTER WORKPLACE INJURIES?

Nicholas Broten
Michael Dworsky
David Powell

Working Paper 25989
http://www.nber.org/papers/w25989

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2019, Revised March 2022

We thank conference attendees at the 2018 Conference of the American Society of Health Economists and the 2018 Disability Research Consortium Meeting for helpful discussions and comments, especially our discussants Nicolas Ziebarth and David Mann. Findings from an earlier version of this study were presented to the California Commission on Health and Safety and Workers' Compensation in September 2019; we are also thankful to the commissioners for providing helpful feedback. Roald Euller at RAND provided invaluable programming support. This research was supported by the U.S. Social Security Administration through grant #5 DRC12000002-06 to the National Bureau of Economic Research as part of the SSA Disability Research Consortium. The findings and conclusions expressed are solely those of the authors and do not represent the views of SSA, any agency of the Federal Government, or the NBER. The authors are grateful to the California Department of Industrial Relations for providing the data used in this study.

Do Temporary Workers Experience Additional Employment and Earnings Risk After Workplace Injuries?

Nicholas Broten, Michael Dworsky, and David Powell

NBER Working Paper No. 25989
June 2019, Revised March 2022
JEL No. J28,J81

ABSTRACT

Do temporary workers face more employment and earnings risk than direct-hire workers? We link administrative workers' compensation claims to earnings records to measure the risk posed by workplace injuries, comparing employment and earnings outcomes between temporary and direct-hire workers injured doing the same job. We implement two complementary empirical strategies to account for underlying differences in labor market attachment. Despite evidence that injury severity does not vary between the two sets of workers, temporary workers suffer larger reductions in employment and more severe earnings losses, persisting at least three years after injury, relative to similar direct-hire workers. The additional earnings losses suffered by temporary workers are partially offset by workers' compensation benefits, but the income loss gap is still large even after accounting for these benefits.

Nicholas Broten
RAND Corporation
1776 Main Street
P.O. Box 2138
Santa Monica, CA 90407
nbroten@rand.org

David Powell
RAND Corporation
1776 Main Street
P.O. Box 2138
Santa Monica, CA 90407
dpowell@rand.org

Michael Dworsky
RAND Corporation
1776 Main Street
P.O. Box 2138
Santa Monica, CA 90407
mdworsky@rand.org

1 Introduction

A growing number of jobs are 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 (Jackson et al., 2017; Katz and Krueger, 2019). Research has debated whether temporary work, in particular, acts as a stepping-stone to permanent and higher-wage employment for entry-level workers (Autor and Houseman, 2010; Pallais, 2014).[1] However, there is less evidence evaluating the economic risks faced by temporary workers. The earnings and employment risk differentials between workers in alternative work arrangements ("nonstandard workers") and more traditional jobs are potentially quite large.

We study earnings and employment risk faced by both direct-hire workers and workers hired through temporary help agencies in the context of workplace injuries. Workplace injuries represent substantial income and health shocks to workers and households (Leigh, 2011). Temporary workers represent a significant and growing component of the nonstandard workforce (Katz and Krueger, 2019) and have been of independent interest to the economic literature and to policymakers. We use a unique data set from California to compare outcomes after workplace injuries between temporary workers and direct-hire workers who are injured doing similar jobs. The data set includes administrative records from the universe of California workers' compensation claims from 2005 to 2012. 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 across different categories of workers.

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. Given that we have panel data on employment and earnings, we are able to condition on pre-injury earnings histories to compare workers with similar employment and earnings trajectories prior to their injuries. We use an event study framework to estimate temporary worker labor outcomes relative to direct-hire labor outcomes by quarter relative to the time of injury. To further account

[1] Researchers have argued that temporary work fails to promote career progression since workers may fail to develop skills and lack opportunities for promotion. Early studies on temporary work were broadly consistent with the stepping-stone theory (Lane et al., 2003; Heinrich et al., 2005). Autor and Houseman (2010) and Autor et al. (2016) find evidence that temporary work failed to increase employment at the end of the initial job assignment.

for possible confounders, we condition on the type of job in which the worker was injured performing and the nature and cause of the injury, also letting this information predict the full outcome trajectory of workers.

As complementary evidence, we implement a triple-differences design, using *medical-only* workers' compensation claims as an additional control group. Medical-only claims are claims for minor injuries that do not result in payment of indemnity (i.e., cash) benefits. By using workers with the same type of work arrangement as an additional control group, this strategy helps distinguish the differential impact of a workplace injury on temporary workers from other differences in employment dynamics between temporary and direct-hire workers.

We find that temporary workers suffer significantly larger reductions in employment and earnings than direct-hires. In the first year after injury, the employment propensities of temporary workers decrease by 9.6 percentage points more than those of direct-hires. This gap closes gradually over the second and third years, but the difference remains large and statistically significant even at 12 quarters post-injury. Earnings losses for temporary workers also persist through the end of the third year after injury. Given our rich information on type and cause of injury, we are able to account for observable differences in injury severity across work arrangements. We find no evidence of meaningful differences that might explain the labor outcome results. We also estimate similar results when we focus on traumatic injuries, suggesting that differential claiming behavior cannot explain the results since there is less scope for such behavioral responses to these types of injuries.

The 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. 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 15.6% reduction in labor earnings (for the 3 years post-injury) attributable to temporary status falls to 12.9% when temporary disability and permanent disability benefits are included, and to 10.2% when settlements are included. In general, workers' compensation provides only partial insurance against earnings losses, and our estimates suggest that the incremental earnings risk associated with temporary work is only partially insured by workers' compensation.

Previous studies have documented that temporary work is associated with worse occupational health (Underhill and Quinlan, 2011; Quinlan, 2015) and 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 injured on the job have not previously been examined. 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. 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 challenges for workers' compensation policy, most notably the growth of non-covered employment.

Different proposals have been advanced for how to provide social insurance to nonstandard workers. Harris and Krueger (2015) proposed to create a new legal classification–the *independent worker*–such that independent contractors would gain coverage under selected labor laws and social insurance arrangements without establishing a direct-hire relationship. Others have urged the elimination of exclusions from workers' compensation coverage requirements (American Public Health Association, 2017). Our study represents, to our knowledge, the first attempt to assess the suitability of an existing social insurance program for the risks faced by a group of nonstandard workers. While our results may not generalize to independent contractors and other alternative work arrangements, evidence on differences in income risk and workers' compensation benefit adequacy between workers in different work arrangements may be constructive for the wider debate over how nonstandard workers should be covered by social insurance systems.

2 Background

2.1 Workplace Injuries and Workers' Compensation

In 2019, there were 2.8 million reported nonfatal occupational injuries and illnesses in the U.S. (Bureau of Labor Statistics, 2020). To help protect workers against the financial and health risks of workplace injury and illness, state workers' compensation (WC) systems ensure that injured workers have access to medical care and rehabilitation services. In 2016, state workers' compensation systems paid a total of $62.9 billion in medical and cash benefits to injured workers (Griffin Murphy and Boden, 2020). Outside of Texas, where employers can opt out of the workers' compensation system (Cabral et al., 2019), workers' compensation is nearly universal for wage and salary employees (McLaren and Baldwin, 2017).

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 disability and providing additional permanent disability benefits to workers whose injuries result in long-term impairment. 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 (which is experience-rated for most employers) or through self-insurance (which is effectively experience-rated in that the risk of losses is mostly borne by employers).[2] In contrast to voluntary employee benefits such as health insurance that are typically more generous at higher-wage employers, the generosity of workers' compensation benefits is standardized by state law and is uniform across all covered workers: Cash benefits are determined by the same statutory formulas for temporary and direct-hire workers, and both groups of workers are provided medical care for their injuries with no patient cost-sharing.

Temporary workers are covered by the workers' compensation system even though they are not direct-hires. Independent contractors are typically not covered by workers' compensation–even if they are working for businesses whose employees must be covered by workers' compensation. Our setting thus provides an opportunity to understand an important dimension of income risk associated with one type of alternative work arrangements.

2.2 Temporary Work and Workplace Safety

Temporary 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. Because they are employees of a labor intermediary, temporary 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. Legally, both the temporary agency and the host employer are responsible for providing safe working conditions (Howard, 2016).

[2]In California, injury claims filed by long-term employee leasing arrangements, including temporary agencies, should be counted against the host employer's claims experience, while injury claims filed by short-term temporary workers may be counted against the temporary agency's claims experience. Source: WCIRB of California (2020) "California's Experience Rating System." Available at `https://www.wcirb.com/guide-to-workers-compensation/experience-rating` as of July 30, 2020.

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 A1 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.

3 Data

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 California DIR for years 2005 to 2012.[3] California law requires workers' compensation claims administrators (insurers, self-insured employers, and third-party administrators) to report new 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 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 Disability (TD) or Permanent Disability (PD), and to observe the total amount of payments to date. See Appendix C.2 for more details.

We also 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 richer 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 employees

[3]Further information about the WCIS is available at `https://www.dir.ca.gov/dwc/wcis.htm`. Please also see Appendix Section C.1 for more details.

within workplaces.[4]

Our research design is made possible by the fact that class codes for temporary workers are defined based on the work activities and level of risk exposure at the host employer, not the firm that employs the worker.[5] This distinction is important in this context because the employer of record for temporary workers differs from the host employer.

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. 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-identified and assigned an ID number that could be used to match to the WCIS data.[6] To produce estimates representative for the population of cases reported to the WCIS, we constructed sampling weights (see Appendix C.2 for details). We reweight 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.

We used the wage records to create a measure of total quarterly wage and salary

[4]We do not have information about relative injury risk within class codes and whether this varies by temporary work status. 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.

[5]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.

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

earnings by summing earnings over all employers in each quarter.[7] We also created an indicator variable for employment, which we define as having quarterly earnings from all employers greater than $200 (in 2014 dollars). We constructed a balanced panel containing 17 quarters of earnings and employment data (from four quarters prior to injury to twelve quarters after injury).

UI earnings records are frequently used to study temporary workers (Lane et al., 2003; Heinrich et al., 2005; Autor and Houseman, 2010; Autor et al., 2016) as they are often the best available source of data on individual earnings. However, there are limitations since we only observe UI-eligible earnings in California. We do not observe other sources of earnings or earnings from work in other states. Our results should be interpreted as the effects on UI earnings/employment in California. In Section 5.3, we discuss why our findings are not consistent with differential migration or transitions to unreported employment.

3.3 Sample Definition and Ascertainment of Temporary Status

Our treated group consists of injured workers receiving indemnity benefits, indicating that the injury led to a temporary or permanent work disability, whose employer reported NAICS industry code 56132 (temporary help services) to EDD. We observe 12,874 injured workers in this NAICS category and refer to these workers as *temporary workers*. We assume that all other injured workers are direct-hires.[8]

Many class codes have very few injuries among temporary workers. Table 1 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 previously observed in Table A1. In particular, class codes likely to involve material moving, packaging, and assembly have high rates of temporary employment among injured workers.

[7] To avoid results being driven by very high earners, outliers were removed. The outlier threshold was defined by calculating the 99.8^{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 $79,721 per quarter (in 2014 dollars), or $318,884 in annualized terms. If an injured or control worker's CPI-adjusted total earnings exceeded this value in any quarter (either pre- or post-injury), they were classified as an outlier and removed. 0.9% of injured workers in the merged data met this criterion and were excluded.

[8] Our sample excludes workers in NAICS industry code 56133, professional employer organizations (PEOs). PEOs, like temporary help agencies, are labor intermediaries. PEO workers are often counted as nonstandard workers, but PEOs sometimes provide personnel services to employees who were originally direct hires, and the structure of the relationship between a PEO, a host employer, and a worker is ambiguous compared to temporary help agencies. An earlier version of this paper that included PEO workers along with temporary workers in our treatment was circulated as NBER Working Paper No. 25989. The overall results were similar with or without PEOs included in the sample as part of the treatment group.

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 2005-2012 with 20 or more injuries among temporary workers. As discussed below, we include separate class code-quarter of injury fixed effects in our regression models, ensuring that temporary workers are compared to direct-hires who are injured at the same time doing the same job. The sample restriction leaves us with 62 class codes represented in our analysis sample (see Appendix B for a list of all included class codes).

Using the linked earnings data, Table 2 compares earnings (in 2014 dollars) and employment across our different categories. The table shows that temporary workers have worse post-injury labor market outcomes. However, direct-hires also have much higher earnings and employment than temporary workers prior to the injury. These pre-injury differences motivate our use of an empirical strategy which accounts for these differences. The unadjusted percentage reduction in earnings and employment relative to pre-injury outcomes is actually smaller for temporary workers. Table A2 provides differences in demographics, job characteristics, and injury mix between direct-hire workers and temporary workers. Overall, 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 (injuries resulting from repeated exposure), which is consistent with less tenure at the firm.[9]

4 Empirical Strategy

The goal of the empirical strategy is to isolate differences in the employment and earnings response to workplace injuries for temporary workers relative to direct-hire workers. We condition on worker fixed effects given pre-existing differences in labor outcomes for these two set of workers and estimate difference-in-differences event studies. We model the expected value of the outcome as

$$E[y_{icdqt}] = F\left(\alpha_i + \mu_{cqt} + \sum_{s=-4}^{12} \beta_s 1(d = \text{temporary}) \times \mathbf{1}\{t = s\} + \sum_{s=-4}^{12} X_i' \gamma_s^X \mathbf{1}\{t = s\}\right), \tag{1}$$

where y_{icdmqt} is a labor outcome for individual i in class c, and contract type (work arrangement) d at time-relative-to-injury t for an injury incurred in calendar year-quarter q.

[9] 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.

We condition on worker fixed effects (α_i), which account for differences between temporary workers and direct-hires, and time-relative-to-injury fixed effects. We permit these time-relative-to-injury fixed effects to also vary based on class of worker c and time of injury t (μ_{cqt}).[10] 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.

We plot the β_s estimates, representing the (normalized) differences in the outcome between temporary workers and direct-hires, normalizing $\beta_{-1} = 0$. We also include individual characteristics (X_i) interacted with time-to-event indicators as control variables to permit the observable characteristics to predict the full trajectory of outcomes.[11] We interact indicators based on the following variables with quarter-relative-to-injury indicators: age group-gender interactions,[12] job tenure, type of injury, geographic region within California,[13] full-time/part-time status, and the weekly wage as reported on the workers' compensation claim. The weekly wage reported on the workers' compensation claim determines the level of indemnity benefits and can be viewed as a way to control for differences in post-injury incentives due to differences in the wage replacement rate.[14]

In addition, we are concerned that – even in the absence of an injury – these sets of workers may have different labor trajectories on average. To account for differences in labor market attachment and earnings propensities between the two groups, we also construct a measure of labor supply, categorizing workers as earning \$0, \$0.01-\$4,999.99, \$5000-\$9,999.99, \$10,000-\$14,999.99, \$15,000+ by quarter (pre-injury). We create bins based on the interaction of the four pre-injury quarters (i.e., assigning workers to one of the 625 groups based on the pre-injury earnings profile), and then interact indicators for these bins with the full set of time-to-event indicators $\left(\sum_{s=-4}^{12} \mathbf{1}\{t = s\}\right)$ included in equation 1 above. By allowing the counterfactual earnings trajectory to vary with a worker's pre-injury earnings profile, including these variables controls flexibly for differences in dynamics between workers

[10] For example, there is a full profile of time-relative-to-injury ($t = -4, -3, \ldots 12$) 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.

[11] 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).

[12] 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.

[13] 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`.

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

with different earnings histories. The assumption of this specification is that after adjusting for the fixed effects and covariates in the model, the direct-hires provide an appropriate counterfactual for the labor outcomes of the injured temporary workers.

When analyzing employment as the outcome variable, we estimate equation 1 as a linear probability model using OLS. We estimate the specification for earnings using Poisson regression (i.e., $F(\cdot) = \exp(\cdot)$). We use Poisson regression for earnings given the skewed nature of this variable (Santos Silva and Tenreyro, 2006) and the frequency of observations with zero earnings.[15] In addition, we are interested in percentage changes in earnings since workers responding to an injury by working fewer hours in a quarter or by switching to a lower-paying job would experience earnings reductions proportional to their baseline. We will also provide results using alternative functional form assumptions.

Given recent concerns about staggered adoption in difference-in-differences designs (Goodman-Bacon, 2021), we note that the fixed effects in equation (1) are interacted with calendar quarter-of-injury dummies, implementing a stacked difference-in-differences design. This approach should be robust to the concerns expressed in the literature as the comparison group for each treated worker was injured at the same time. The one exception is that many of our control variables are assumed to have a constant effect regardless of the quarter-of-injury. We will show sensitivity analyses which suggest that this assumption is not driving the results.

We will also summarize our estimates by presenting averages of the event studies estimates. We calculate the average effect in the first year as $\frac{1}{4}\sum_{s=1}^{4}\hat{\beta}_s$, and the average effects in the second and third years similarly. We weight all analyses by sample weights. Standard errors are adjusted for clustering by individual.

5 Results

We present unadjusted employment propensities (Panel A) and earnings (Panel B) for direct-hires and temporary workers in Figure A1. The trend lines peak mechanically at the quarter

[15] Unlike log-linear regression using 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 Santos Silva and Tenreyro (2006) and Wooldridge (1999). Consistency of alternative estimators of the exponential conditional mean model, such as negative binomial regression, require 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 Santos Silva and Tenreyro (2006) and Ciani and Fisher (2019) for more details). While we only have 17 observations per worker, incidental parameters should not be a concern with Poisson regression (Lancaster, 2002).

of injury because workers must be working to experience a workplace injury.[16] We observe very different post-injury dynamics between the direct-hires and temporary workers. The pre-injury trends for these groups, however, are also very different. We proceed by estimating models which should account for these different dynamics.

5.1 Difference-in-Differences Estimates

Figure 1 presents our main difference-in-differences estimates. While the raw time series suggests evidence of pre-existing differences between temporary and direct-hire workers, we now condition on a large set of potential predictors of these differences. Each point represents the estimated conditional difference between temporary workers and direct-hires for that quarter-relative-to-injury, relative to baseline. We find little evidence of systematic pre-injury trajectories. Panel A shows the employment effects. We estimate large employment reductions for temporary workers relative to direct-hires after injury. The immediate effect is about a 10 percentage point decrease. Over time, this effect moderates, though we still estimate large and statistically significant effects 12 quarters after injury. Panel B provides the earnings estimates. We observe a similar pattern – a sharp immediate reduction and large long-term effects. We estimate short-term earnings reductions of about 20% for temporary workers relative to direct-hires with long-term magnitudes of about 13%.

We summarize the results by year relative to injury in Table 3. All regressions include individual fixed effects and class codes*time-relative-to-injury*calendar time fixed effects. We also include our pre-injury earnings controls. In Column 1, we find that temporary work predicts incremental employment reductions of 9.3 percentage points in the first year, 5.8 in the second year, and 3.6 in year 3. The incremental earnings loss is 17.6% in year 1, 12.3% in year 2, and 8.1% in year 3. In Column 2, we add our baseline controls.[17] Coefficients are slightly more negative, but they are similar. In dollar terms (2014$), the Column 2 earnings estimates imply annual reductions of $2,958; $2,114; and $1,587, respectively.[18]

In Column 3, we provide unweighted results. They are similar. Columns 4 and 5 stratify based on year to consider effects prior to the Great Recession and after the Great

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

[17]Quarter-relative-to-injury indicators interacted with age group-gender indicators, job tenure, type of injury, geographic region within California, full-time/part-time status, and the weekly wage as reported on the workers' compensation claim.

[18]Using our estimates, we calculate each worker's post-injury earnings assuming that they were direct-hires (i.e., adding back in the differential impact experienced by temporary workers). The dollar estimates above refer to the implications of the estimated percentage change given this baseline.

Recession. In general, the magnitudes are larger for workers injured in 2005-2008, but effect sizes are still large and statistically different from zero for workers injured in 2009-2012.

One concern is that, conditional on an injury or injury severity, temporary workers may be less (or possibly more) likely to file workers' compensation claims. To consider the possibility of selection concerns, we replicate our analysis using only traumatic injuries, defined as injuries caused by external forces.[19] Relative to cumulative injuries 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. We present the results for this sample in Column 6. We estimate larger employment and earnings reductions for this sample. The stability of the estimates suggests that differential selection is not driving the main results.

We are not explicitly using a matching estimator, which may induce problems due to mean reversion in difference-in-differences analyses (Daw and Hatfield, 2018). However, our approach permits post-injury outcomes to depend flexibly on pre-period outcomes, which shares many of the properties (and potential concerns) of a matching estimator. To test for the possible importance of mean reversion, we repeat our main analysis, but we do not use information from the quarter immediately prior to injury. The results are shown in Column 7 and are similar to the main estimates (the employment magnitudes tend to be larger while the earnings magnitudes are smaller).

So far, we have studied pre-tax earnings. Because workers' compensation benefits are tax-exempt, we need a measure of after-tax earnings to evaluate uncompensated earnings losses (in Section 5.4 below). The administrative data do not include spousal income, non-

[19]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)
- 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)

In a sample of injuries for which we have medical spending data, injured workers with traumatic injuries and indemnity benefits had slightly higher average total medical spending in the six months after the injury ($4,132) than those workers with non-traumatic injuries ($3,935). Workers with traumatic injuries were also more likely to have emergency-department related expenses (19% versus 11%) and had higher average inpatient expenses ($412 versus $216).

labor income, and other information needed to calculate income taxes at the individual level. We therefore impute tax rates using data from the CPS ASEC. For a sample of working California residents in each year from 2005 to 2012, we calculate state and federal income tax liability using variables provided by IPUMS (based on a tax model used by the Census Bureau) (Flood et al., 2021). We also account for payroll taxes, including OASDI, Medicare, and California State Disability Insurance. We then calculate the average tax rate by year and ventile of the labor earnings distribution and merge these average tax rates into our data based on calendar year and workers' labor earnings to calculate after-tax earnings.[20] We present the after-tax earnings results in Column 8 of Panel B (the employment outcomes in Panel A are unaffected by taxes). Estimates using after-tax income are nearly identical to those using pre-tax income, suggesting that the proportional effects are unaffected by adjusting (or not adjusting) for taxes.

While our baseline specification implements a stacked difference-in-differences design, we have assumed that the relationship between most of our covariates and our outcomes do not vary by quarter-of-injury in a way that biases our main results. However, the Column 1 estimate is generated from a specification which interacts all fixed effects and the included controls with quarter-of-injury indicators. In addition, we replicate our Figure 1 results but interact all of the covariates with quarter-of-injury indicators. These results are presented in Figure A3 are similar to the main results.

In addition, we consider the role of functional form assumptions. We estimate the earnings effect using OLS. The results are presented in Panel A of Figure A4 and have a similar pattern as our Poisson estimates. We also estimate the employment effect using Poisson estimation, which has been recommended for binary choice models Zou (2004), especially given high-dimensional fixed effects, and found to be useful (Abaluck et al., 2021). The pattern of estimates, provided in Panel B of Figure A4, is similar to the main OLS estimates.

Direct-hires and temporary workers may differ in unobserved ways that predict different employment trends. To further guard against bias due to these unobservables, we replicate our analysis for a subsample of workers who were employed *both* as direct-hires and as temporary workers at some point in the two years before injury. Given that these workers

[20]Measurement error in the tax rates is a potential concern since we are assuming that injured workers have the same tax rates as the full (sampled) universe of workers with similar labor earnings. Any measurement error will be problematic only if temporary workers experience differential *changes* in this measurement error post-injury relative to direct-hires.

had all recently selected into temporary work, we may consider it quasi-random which work arrangement they had at the time of injury. Notably, these sets of workers had similar time spent in temporary jobs. Workers in this sample injured as direct-hire workers had spent 2.1 quarters as temporary workers in the two years prior to injury; workers injured as temporary workers had also spent 2.1 quarters as temporary workers in the two years prior to injury (see Table A3 for more detail).

A limitation of this exercise is that some benefits of direct-hire work arrangements, such as opportunities for promotion within the firm, may accrue over time and be limited for direct-hire workers who recently transitioned out of temporary work. Thus, this test is conservative since we are eliminating much of the effect that we are interested in. Results are presented in Column 9. As predicted, the estimates are smaller in magnitude. However, we still observe large and statistically significant incremental employment and earnings losses for workers injured while employed by temporary help agencies. These findings are consistent with important work arrangement-specific effects associated with temporary work.

5.2 Triple-Difference Estimates

As a further sensitivity analysis, we present triple-differences event study estimates[21] that use medical-only injuries to establish underlying labor trajectory differences between injured temporary and direct-hire workers.[22] This empirical strategy has pros and cons compared to the difference-in-differences approach. The primary advantage is that the medical-only injuries provide another way to control for differences between injured temporary and direct-hire workers. This third "difference" reduces the need to condition on pre-injury earnings histories: our main triple-differences analysis will not use these control variables. To the extent that mean reversion was a concern due to these controls, the triple-differences analysis will not be subject to this issue. The disadvantage is that it is likely that the medical-only injured workers, despite not missing enough work to receive indemnity benefits, may

[21] Our triple-difference event-study specification can be represented by

$$E[y_{icdmqt}] = F\left(\alpha_i + \mu_{cdqt} + \delta_{mt} + \sum_{s=-4}^{12} \beta_s 1(m = \text{indemnity}) \times 1(d = \text{temporary}) \times \mathbf{1}\{t = s\} + \sum_{s=-4}^{12} X_i' \gamma_s^X \mathbf{1}\{t = s\}\right),$$

where y_{icdmqt} is a labor outcome for individual i in class c, contract type (work arrangement) d, and injury category (indemnity or medical-only) m at time-relative-to-injury t for an injury incurred in calendar year-quarter q. The μ_{cdqt} fixed effects 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.

[22] Tables A2 and A4 include basic summary statistics about medical-only injuries. Figure A2 provides raw time series for the two outcomes.

experience some income loss due to their injuries. This should attenuate our estimates since the medical-only injuries are also "treated."

We present our results graphically, showing the full trajectory of labor outcomes, in Figure 2. Panel A presents the employment differences while Panel B provides the earnings differences. For both outcomes, we observe patterns similar to those observed in the difference-in-differences analysis, though the magnitudes are attenuated, as expected.

We summarize these results and explore their sensitivity in Table 4. In Column 1, we estimate a model with only our baseline fixed effects (see footnote 21). We estimate a 6.1 percentage point relative reduction in employment for temporary workers relative to direct-hires in Year 1 followed by a 2.2 reduction in Year 2. This difference disappears by Year 3. The earnings reductions persist – 14% in the first year, 11% in the second year, and 6% in Year 3. These estimates are smaller in magnitude than the difference-in-differences results, but still indicate economically meaningful incremental losses for temporary workers.

In Column 2, we add our baseline covariates, and the estimates are not materially affected. Column 3 adds the pre-injury earnings controls used in the previous section. The estimates are similar, suggesting that the medical-only injured workers are adequately accounting for underlying differences between temporary and direct-hire workers (and also suggesting that mean reversion is not a concern in the difference-in-differences analyses).

Workers suffering from medical-only injuries also potentially experience some impact. To explore this possibility, we used data on medical bill payments to exclude medical-only claims with high levels of medical spending since these injuries are likely more severe. Medical spending data are available for only about half of our analysis sample (759,949 out of 1,552,513 injured workers).[23] To address concerns that the sample with medical care spending data may be different from our main analysis sample, we replicate our employment and earnings results on this sub-sample and provide the results in Column 4. We find relatively similar estimates compared to those presented in Column 2, suggesting that we are not working with a systematically different sample.

[23]Claims administrators were not required to submit medical bills to the WCIS until September 2006, so medical claims are unavailable or incomplete for earlier injury dates. Even after collection of medical bills began, some claims administrators regularly submitted billing data that were missing direct identifiers (such as the patient name or SSN) needed to enable linkage to other components of the workers' compensation claim. This problem was widespread, affecting around 44% of medical bills in recent years for which DIR has published data on the extent of the problem. Source: California Department of Industrial Relations (2019). Table 1.2: Distribution of Errors by Error Code and Error Messages. Available at `https://www.dir.ca.gov/dwc/wcis/WCIS_tables/MedicalTables/Table1/Table1-2.pdf` as of May 18, 2020.

In Column 5, we exclude high-severity injuries, defined as injuries with medical spending over the first 6 months post-injury above the median for our sample.[24] We observe evidence that the triple-differences estimates are attenuated as the magnitudes generally increase when the high severity injuries are excluded. The earnings results are similar to the Table 3 results while the employment results are still smaller in magnitude. The similarity of these triple-differences results with the difference-in-differences estimates suggests the latter are not driven by unmeasured differences between direct-hires and temporary workers.

5.3 Alternative Explanations

We consider whether temporary workers suffer more severe injuries, possibly explaining the worse post-injury outcomes. We found that controlling for the type of injury (Column 2 of Table 3) had minimal impact on the estimates (compared to Column 1), suggesting that differences in the type of injury are not driving the results. However, it is possible that temporary workers experience worse health or more disability conditional on injury type.

To examine injury severity, we use the data on total medical spending discussed above.[25] For the sample with medical care data, we compare differences in cumulative medical care spending between direct-hires and temporary workers over the first 24 months after injury.[26] There are no institutional differences in access to workers' compensation medical care between temporary workers and direct-hires: all injured workers with a workers' compensation claim receive medical care for their injuries with no patient cost-sharing. We estimate a Poisson regression with a temporary worker indicator and fixed effects based on class code*quarter of injury. We also include additional worker- and injury-specific control variables when noted. The results are presented in Table A5.

We find little evidence of medical spending differences by work arrangement. Temporary workers receive slightly *less* medical care spending. However, we might expect that direct-hires and temporary workers could demand different amounts of medical care conditional on injury so we replicate this analysis for our triple-differences sample. The medical-only injuries play a similar role here as in our main triple-differences analyses, accounting

[24] California had a medical fee schedule in place limiting most payments for medical services to 120% of the Medicare rate, a rate low enough that it is generally binding. Prices should therefore be similar, if not identical, between temporary and direct-hire workers, making spending a reasonable measure of utilization in this setting.

[25] Research has suggested that additional medical care can improve future labor outcomes for injured workers (Powell and Seabury, 2018) so differences in medical care could also be problematic for this reason.

[26] Results are similar if we study medical care spending at 6 or 12 months.

for underlying differences between the two groups. We estimate a Poisson regression with a temporary worker indicator, an indemnity indicator (interacted with class code and quarter of injury), and the interaction as the main explanatory variable. The results are presented in Panel B of Table A5. Point estimates are close to zero and precisely estimated.[27]

Temporary workers and direct-hires could face different incentives to work, as TD benefits are progressive and temporary workers have lower earnings. To explore this issue, we calculate each worker's wage replacement rate. Temporary workers have a replacement rate that is 0.015 higher than direct-hire workers (on a base of 0.71). However, as we add control variables, this difference disappears. In our triple-differences sample, temporary workers have lower replacement rates than direct-hires (relative to the medical-only differential). This difference also shrinks as additional controls are included in the model.

Additionally, we doubt that differential incentives in the system are driving the results since temporary workers had slightly *shorter* temporary disability duration (i.e., time receiving TD benefits).[28] While return to work incentives do not appear to drive our results, we cannot rule out different *responses* to the same incentives, and we consider this part of the effect that we are estimating.

In addition, we found similar employment and earnings losses for workers with traumatic injuries, suggesting that differential selection into workers' compensation is not driving our results (Column 6 of Table 3). Finally, it is worth highlighting that our outcome variable is limited to UI employment and earnings within California. If temporary workers are more or less likely to leave the state after an injury, this would look like differences in employment to us. The triple-differences design should account for different propensities to migrate out of state. We also doubt that the large immediate relative reduction is due to differential out-migration. A migration effect in response to a workplace injury would likely cause the effect to grow over time, which is the opposite of what we see. Additionally, administrative data sources do not capture all earnings. Again, the triple-differences specification should account for underlying trajectories to transition to work not reported to UI. Notably, we

[27]It is possible that workers returning to work more quickly receive less care due to higher opportunity costs of their time (Cabral and Dillender, 2020). However, we find that temporary workers are *less* likely to be employed immediately after injury so this possibility would operate in the opposite direction suggested by the results.

[28]Mean temporary disability duration among workers with indemnity benefits was 25.2 weeks for temporary workers, 3.1 percent shorter than the mean of 26.0 weeks for direct-hire workers in our sample. A Poisson regression for TD duration including all covariates (μ_{cqt} and X_i) included in our regression models shows that temporary workers' disability duration was 4.7% shorter conditional on observables.

observe larger effects in the early part of our sample (Table 3, Column 4), consisting of injury cohorts less exposed to the gig economy. The relative magnitudes suggest that shifts to other sources of income are also not driving our findings as we would expect the effects then to grow as the gig economy became more established.

Overall, we interpret our results as the differential effects on UI-reported earnings/employment, outcomes which are of interest for policy and social insurance systems more broadly. However, the evidence suggests that these movements reflect true changes in labor supply.

5.4 Benefits and Uncompensated Earnings Losses

We find greater income risk after injury for temporary workers. This raises the question of whether workers' compensation cash benefits reduce—or perhaps magnify—this gap in outcomes. To analyze how uncompensated income loss net of workers' compensation benefits varies across work arrangements, we construct a total income measure that includes both earnings and benefits. We aggregate the year before injury period into one pre-injury time period and the three years after injury into one post-injury time period. This aggregation is necessary since we do not observe the timing of some of the benefits.

We use Poisson regression to compare earnings between the one-year pre-injury period and the three-year post-injury period without annualizing post-injury earnings; differences in the time periods being compared are absorbed by the (multiplicative) time effects in the model. We use after-tax earnings because benefits are tax-exempt. We examine five categories of payments (see Table A6 for summary statistics): Temporary Disability (TD) Benefits, Permanent Disability (PD) Benefits, Disability Benefit Settlements, Unspecified Settlements, and Medical Settlements. Pre-injury income equals after-tax earnings, as workers do not receive benefits before injury. Post-injury income measures are constructed by sequentially adding the five benefit types to after-tax earnings.

The first three benefit types unambiguously represent indemnity benefits. The fourth category of benefits, unspecified settlements, is more ambiguous since it represents some unknown combination of indemnity and medical benefits.[29] The last category, medical

[29]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. 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.

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. We include medical settlements to provide indirect evidence on the relative importance of medical and indemnity benefits in the unspecified benefits category.

Table 5 reports estimates representing the incremental uncompensated income loss associated with temporary work. Column 1 indicates that temporary workers experience an additional earnings loss of 15.6% over three years post-injury. TD and PD benefits reduce the incremental post-injury income loss experienced by temporary workers to 13.8% when TD benefits are included and to 12.9% when PD benefits are added. Indemnity settlements do not appear to have a differential effect on temporary workers. When unspecified settlements are added, the incremental income loss associated with temporary work declines to 10.2%. We cannot know with certainty if these unspecified settlement payments reflect settlements for indemnity benefits or medical payments, but it is plausible that these settlements primarily reflect indemnity benefits, since adding medical settlements actually increases the magnitude of the income loss.

The estimates in Table 5 suggest that indemnity benefits and settlements provided by the workers' compensation system reduce, but do not eliminate, the incremental earnings risk faced by temporary workers who are injured on the job. Given that benefits are progressive and temporary workers have lower earnings, this might be surprising. However, the results in Table 5 reflect differences within groups of workers who, given the control variables, have similar pre-injury earnings. To the extent that temporary workers have lower earnings than direct-hires, then the unconditional (on pre-injury earnings) insurance value of benefits is different for the two groups due to the progressivity of the benefit schedule. Our results speak more to differences in benefits associated with work arrangements and less about the benefit schedule, which we consider a separate question. Workers' compensation is designed to provide partial, not full, insurance for both temporary and permanent earnings losses. If these incremental losses are only partially insured, then it is unlikely that workers' compensation would fully eliminate the gap in post-injury earnings losses.

6 Conclusion

We examine the labor market outcomes of temporary workers after workplace injury. Using two complementary empirical strategies to account for underlying differences in labor market attachment between temporary workers and direct-hire workers, we find robust evidence that temporary workers suffer larger reductions in employment and labor earnings after a work-

place injury. Over the three years after injury, temporary workers experience 15.6 percent larger earnings (in after-tax dollars) reductions relative to observably identical direct-hires injured doing the same job. This greater income risk faced by temporary workers partially reflects lower employment. While temporary work is known to be associated with greater risks to health and safety and higher workers' compensation injury rates, this study provides the first evidence on how income risk conditional on injury varies across work arrangements. We find that, even after accounting for workers' compensation benefits, workplace injuries have a more negative effect on the labor earnings and income of temporary workers.

Selection into temporary work is not random, and we cannot be certain our results are due purely to work arrangements rather than differential behavioral responses. We provide some evidence that the work arrangement matters, though: when comparing workers who had recently been both direct-hires and temporary workers, we find substantial incremental earnings losses for workers injured while employed as temporary workers.

Even if part of the effect is due to selection, social insurance policymakers may also find it useful to know that specific job arrangements predict additional income risk. Information about heterogeneity in income risk permits the targeting of resources to improve the consumption smoothing benefits of workers' compensation, or to potentially mitigate income risk by improving the post-injury work capacity of injured workers for whom injury predicts greater income risk. Workers' compensation programs generally provide vocational rehabilitation benefits, and some state workers' compensation systems have adopted active labor market programs (such as targeted wage subsidies or funding for workplace accommodations) to promote re-employment of disabled workers: state policymakers might consider targeting outreach for these programs more heavily on injured temporary workers. Moreover, evidence that the effect is partially related to the job and not the worker suggests that policy has a place for mitigating risk for certain types of jobs.

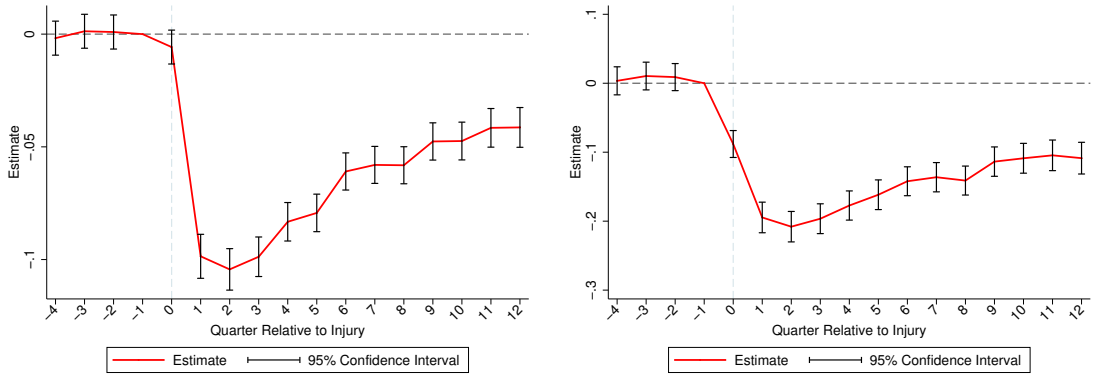
We acknowledge that a full welfare analysis of such targeted interventions would call for stronger evidence than we are able to provide on differences in labor supply behavior (and resulting moral hazard) between temporary and direct-hire workers. While we lacked appropriate identifying variation in benefit generosity between temporary and direct-hire workers, future research on separating income risk from moral hazard in this context would be an important input for analyzing optimal benefit design.

Notwithstanding our limited capacity for welfare analysis in this setting, our analysis of uncompensated earnings losses provides insight into the extent to which workers' com-

pensation insures temporary workers against the incremental earnings risk they appear to face. Given that workers' compensation provides only partial insurance against income loss, it is unsurprising that workers' compensation does not fully insure against the incremental income risk associated with temporary work. Research studying how other alternative work arrangements modify income risk–whether due to workplace injury, non-occupational health shocks, or other varieties of bad luck–should also be a priority for future work.

Figures

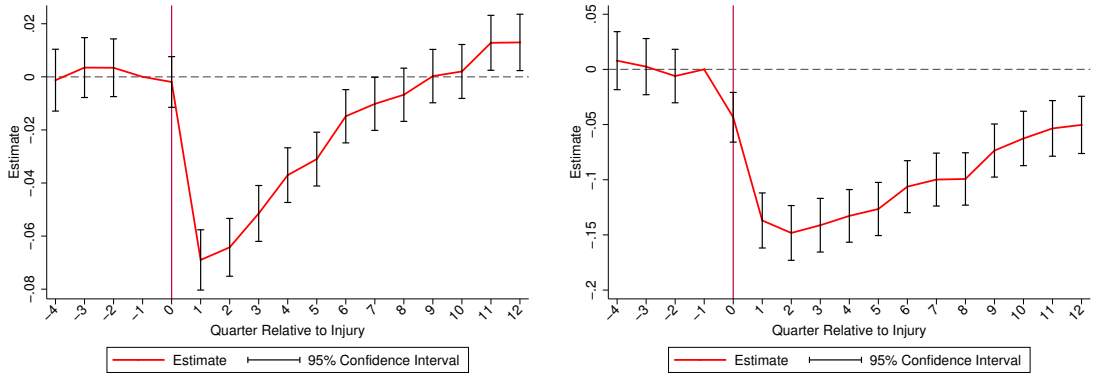
Figure 1: Difference-in-Differences Event Study Estimates



A: Employment B: Earnings

Notes: N = 397,160 injured workers. Point estimates from difference-in-differences event study plotted along with 95% confidence intervals. Baseline period is quarter prior to injury. Individual fixed effects plus fixed effects based on class code*calendar time of injury*time-relative-to-injury. We also interact time-relative-to-injury indicators with 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. Finally, we interact time-relative-to-injury indicators with quarter of injury indicators and measures of pre-injury earnings (see text for details). Confidence intervals adjusted for clustering by class code. OLS is used for Panel A; Poisson is used for Panel B.

Figure 2: Triple Differences Event Study Estimates



A: Employment B: Earnings

Notes: N = 1,552,513 injured workers. Point estimates from triple-differences event study plotted along with 95% confidence intervals. Baseline period is quarter prior to injury. Sample includes medical-only workers' compensation claims as an additional comparison source. All regressions include individual fixed effects, time-relative-to-injury*injury classification (indemnity vs. medical-only) fixed effects, and time-relative-to-injury*calendar time*work arrangement*injury classification fixed effects. We also interact the following controls with time-relative-to-injury indicators: age group-gender interactions, job tenure, type of injury, geographic region within California, full-time/part-time status, and the weekly wage as reported on the workers' compensation claim. Confidence intervals adjusted for clustering by individual. OLS is used for Panel A; Poisson is used for Panel B.

Tables

Table 1: Temporary Work Shares among Injured Workers

Name	Total Injury Count	Temporary Injury Count	Share Involving Temporary Workers
Warehouses - General Merchandise	30178	7655	25.4%
Fruit - Dried Fruit Packing	1630	351	21.5%
Carpentry (NOC) - Low Wage	7477	1158	15.5%
Garbage, Ashes or Refuse Dump Operations	3207	381	11.9%
Printed Circuit Board Assembling	1265	149	11.8%
Stores - Clothing/Dry Goods Wholesale	4107	478	11.6%
Fruit - Citrus Fruit Packing	2085	238	11.4%
Warehouses - Self Storage	1836	208	11.3%
Medical Instrument Manufacturing Electronic	2807	318	11.3%
Electrical Apparatus Manufacturing	3758	416	11.1%

Source: Authors' calculations, 2005-2012 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 industry 56132. NOC = not otherwise classified.

Table 2: 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	36,592.68	100.00%	17,444.79	100.00%
Employed 1 Year (4Q) Before Injury?	86.84%	100.00%	63.37%	100.00%
Post-Injury Earnings				
1st Year (4Q) Post-Injury Earnings	27,373.73	74.81%	12,567.26	72.04%
2nd Year (8Q) Post-Injury Earnings	26,141.43	71.44%	12,615.80	72.32%
3rd Year (12Q) Post-Injury Earnings	25,330.06	69.22%	13,076.14	74.96%
Post-Injury Employment				
1st Year (4Q) Post-Injury Employment	68.60%	79.00%	47.77%	75.38%
2nd Year (8Q) Post-Injury Employment	63.24%	72.82%	45.92%	72.46%
3rd Year (12Q) Post-Injury Employment	60.59%	69.77%	45.95%	72.50%

Source: Authors' calculations, 2005-2012 WCIS-EDD. N = 384,286 injured direct-hire workers and N = 12,874 injured temporary workers. Temporary workers defined as those with a payroll employer in NAICS industry 56132. Earnings adjusted to real 2014$ using the CPI-U.

Table 3: Difference-in-Differences Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: 1(Working)									
Year 1 (Q1-Q4)	-0.093***	-0.096***	-0.096***	-0.098***	-0.094***	-0.102***	-0.102***		-0.058***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.009)	(0.004)		(0.008)
Year 2 (Q5-Q8)	-0.058***	-0.064***	-0.064***	-0.073***	-0.055***	-0.081***	-0.070***		-0.015*
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.010)	(0.005)		(0.009)
Year 3 (Q9-Q12)	-0.036***	-0.044***	-0.044***	-0.058***	-0.030***	-0.054***	-0.051***		-0.007
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.010)	(0.005)		(0.009)
Controls	Earnings Only	Baseline	Baseline	Baseline	Baseline	Baseline	Fewer		Baseline
Weighted?	Yes	Yes	No	Yes	Yes	Yes	Yes		Yes
Years	2005-2012	2005-2012	2005-2012	2005-2008	2009-2012	2005-2012	2005-2012		2005-2012
Set of Workers:	Full	Full	Full	Full	Full	Traumatic	Full		Switchers
Panel B: Earnings									
Year 1 (Q1-Q4)	-0.176***	-0.194***	-0.194***	-0.193***	-0.195***	-0.203***	-0.170***	-0.193***	-0.135***
	(0.011)	(0.011)	(0.011)	(0.014)	(0.016)	(0.021)	(0.011)	(0.010)	(0.019)
Year 2 (Q5-Q8)	-0.123***	-0.145***	-0.145***	-0.156***	-0.132***	-0.187***	-0.116***	-0.146***	-0.044**
	(0.012)	(0.012)	(0.013)	(0.017)	(0.018)	(0.026)	(0.013)	(0.012)	(0.022)
Year 3 (Q9-Q12)	-0.081***	-0.109***	-0.108***	-0.137***	-0.076***	-0.152***	-0.078***	-0.109***	-0.035
	(0.013)	(0.013)	(0.013)	(0.018)	(0.018)	(0.026)	(0.013)	(0.012)	(0.023)
Controls	Earnings Only	Baseline	Baseline	Baseline	Baseline	Baseline	Fewer	Baseline	Baseline
Weighted?	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Years	2005-2012	2005-2012	2005-2012	2005-2008	2009-2012	2005-2012	2005-2012	2005-2012	2005-2012
Set of Workers:	Full	Full	Full	Full	Full	Traumatic	Full	Full	Switchers
Adjusted For Taxes?	No	No	No	No	No	No	No	Yes	No
N (observations)	6,751,720	6,751,720	6,751,720	3,364,572	3,387,148	1,027,021	6,751,720	6,751,720	351,713
N (workers)	397,160	397,160	397,160	197,916	199,244	60,413	397,160	397,160	20,689

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Employment effects estimated using OLS. Earnings effects estimated using Poisson regression. Standard errors in parentheses adjusted for clustering by worker. Estimates represent temporary worker outcomes relative to direct-hires. Baseline period is quarter prior to injury, and reported estimates are the averages of four quarterly estimates from the event studies. All regressions include individual fixed effects and class code*time-relative-to-injury*calendar time fixed effects. "Earnings Only" controls are the pre-period earnings categories interacted with time-relative-to-injury*quarter-of-injury indicators. The "Baseline" controls include these plus age group-gender interactions, job tenure, type of injury, geographic region within California, full-time/part-time status, and the weekly wage as reported on the workers' compensation claim. See text for more details. These controls are also interacted with time-relative-to-injury indicators. "Traumatic" implies that the sample is limited to workers incurring traumatic injuries. "Switchers" are workers which worked both as temporary and direct-hire workers within the two years prior to injury.

Table 4: Triple-Differences Estimates

	(1)	(2)	(3)	(4)	(5)
Panel A: 1(Working)					
Year 1 (Q1-Q4)	-0.061***	-0.055***	-0.057***	-0.051***	-0.066***
	(0.005)	(0.005)	(0.004)	(0.006)	(0.007)
Year 2 (Q5-Q8)	-0.022***	-0.016**	-0.019**	-0.010	-0.021***
	(0.006)	(0.005)	(0.005)	(0.007)	(0.008)
Year 3 (Q9-Q12)	-0.000	0.007	0.004	0.015**	0.010
	(0.006)	(0.005)	(0.005)	(0.007)	(0.008)
Panel B: Earnings					
Year 1 (Q1-Q4)	-0.141***	-0.140***	-0.145***	-0.161***	-0.194***
	(0.012)	(0.012)	(0.011)	(0.016)	(0.017)
Year 2 (Q5-Q8)	-0.109***	-0.108***	-0.115***	-0.138***	-0.168***
	(0.014)	(0.014)	(0.013)	(0.019)	(0.020)
Year 3 (Q9-Q12)	-0.061***	-0.060***	-0.066***	-0.072***	-0.089***
	(0.015)	(0.014)	(0.014)	(0.019)	(0.021)
Controls	No	Yes	Yes	Yes	Yes
Pre-Injury Earnings Bins	No	No	Yes	No	No
Sample	Full	Full	Full	Medical Care Data	Less Severe
N (observations)	26,392,721	26,392,721	26,392,721	12,919,333	9,816,466
N (workers)	1,552,513	1,552,513	1,552,513	759,949	577,438

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Employment effects estimated using OLS. Earnings effects estimated using Poisson regression. Standard errors in parentheses adjusted for clustering by worker. Estimates represent temporary worker outcomes relative to direct-hires. Baseline period is quarter prior to injury, and reported estimates are the averages of four quarterly estimates from the event studies. Sample includes medical-only workers' compensation claims as an additional comparison source. All regressions include individual fixed effects, time-relative-to-injury*injury classification (indemnity vs. medical-only) fixed effects, and time-relative-to-injury*calendar time*work arrangement*injury classification fixed effects. "Pre-Injury Earnings Bins" refer to controls based the pre-period earnings categories interacted with time-relative-to-injury*quarter-of-injury indicators. "Controls" are same as those listed in Table 3. "Medical Care Data" sample includes all workers with 6 months of medical cost data. "Less Severe" excludes any medical-only injuries with above median 6-month medical costs.

Table 5: Uncompensated Earnings Losses Over Three Years Post-Injury

Outcome:	**After-Tax Earnings + Benefits**					
	(1)	(2)	(3)	(4)	(5)	(6)
Temporary × Post-Injury	-0.156***	-0.138***	-0.129***	-0.133***	-0.102***	-0.122***
	(0.010)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)
Benefits Included: (Cumulative)	After-Tax Earnings	+ TD Benefits	+ PD Benefits	+ Indemnity Settlements	+ Unspecified Settlements	+ Medical Settlements

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. N = 397,160 injured workers. Poisson regression estimates. Outcomes refer to the total earnings and benefits received in the three years post-injury or the one year pre-injury. All regressions include individual fixed effects and class code*time-relative-to-injury*calendar time fixed effects (where time-relative-to-injury is pre or post). 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), quartiles of the administrative workers' compensation weekly wage variable, and pre-period earnings categories*time-relative-to-injury. Earnings refer to after-tax earnings. All benefits are added cumulatively as you move to the right of the table. Standard errors in parentheses adjusted for clustering by worker.

References

Abaluck, Jason, Laura H Kwong, Ashley Styczynski, Ashraful Haque, Md Alamgir Kabir, Ellen Bates-Jefferys, Emily Crawford, Jade Benjamin-Chung, Shabib Raihan, Shadman Rahman et al., "Impact of community masking on COVID-19: A cluster-randomized trial in Bangladesh," *Science*, 2021, p. eabi9069.

American Public Health Association, "The Critical Need to Reform Workers' Compensation," 2017.

Autor, David H and Susan N Houseman, "Do Temporary-Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from 'Work First'," *American Economic Journal: Applied*, 2010, *2* (3), 96–128.

Autor, David H., Susan N. Houseman, and Sari Pekkala Kerr, "The Effect of Work First Job Placements on the Distribution of Earnings: An Instrumental Variable Quantile Regression Approach," *Journal of Labor Economics*, 2016, *35* (1).

Bernhardt, Annette, Rosemary Batt, Susan Houseman, and Eileen Appelbaum, "Domestic Outsourcing in the U.S.: A Research Agenda to Assess Trends and Effects on Job Quality," Technical Report, U.S. Department of Labor, Chief Evaluation Office, Washington, DC February 2016.

Bureau of Labor Statistics, "Employer-related Workplace Injuries and Illnesses – 2019," 2020.

Cabral, Marika and Marcus Dillender, "The Impact of Benefit Generosity on Workers' Compensation Claims: Evidence and Implications," Technical Report, National Bureau of Economic Research 2020.

_ , Can Cui, and Michael Dworsky, "What is the Rationale for an Insurance Coverage Mandate? Evidence from Workers' Compensation Insurance," 2019.

Ciani, Emanuele and Paul Fisher, "Dif-in-dif estimators of multiplicative treatment effects," *Journal of Econometric Methods*, 2019, *8* (1).

Daw, Jamie R and Laura A Hatfield, "Matching and regression to the mean in difference-in-differences analysis," *Health Services Research*, 2018, *53* (6), 4138–4156.

Dworsky, Michael, Seth A Seabury, Frank W Neuhauser, Ujwal Kharel, and Roald Euller, *Benefits and Earnings Losses for Permanently Disabled Workers in California: Trends Through the Great Recession and Impacts of Recent Reforms*, Santa Monica, CA: RAND Corporation, 2016.

_ , Stephanie Rennane, and Nicholas Broten, "Wage Loss Monitoring for Injured Workers in California's Workers' Compensation System: 2013 Injury Year Findings (First Interim Report)," Technical Report, RAND Corporation, Santa Monica, CA 2018.

Flood, Sarah, Miriam King, Renae Rodgers, Steven Ruggles, J. Robert Warren, and Michael Westberry, "Integrated Public Use Microdata Series, Current Population Survey: Version 9.0," 2021.

Foley, Michael, "Factors underlying observed injury rate differences between temporary workers and permanent peers," *American Journal of Industrial Medicine*, 2017, *60* (10), 841–851.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 2021.

Harris, Seth D and Alan B Krueger, *A Proposal for Modernizing Labor Laws for Twenty-First-Century Work: The "Independent Worker"* 2015.

Heinrich, CJ, PR Mueser, and KR Troske, "Welfare to temporary work: implications for labor market outcomes," *Review of Economics and Statistics*, 2005, *87* (February), 154–173.

Howard, John, "Nonstandard work arrangements and worker health and safety," *American Journal of Industrial Medicine*, 2016, *10*, 1–10.

Jackson, Emilie, Adam Looney, and Shanthi Ramnath, "The Rise of Alternative Work Arrangements: Evidence and Implications for Tax Filing and Benefit Coverage," *Office of Tax Analysis Working Paper*, 2017, (114).

Jaeger, David A, Theodore J Joyce, and Robert Kaestner, "A Cautionary Tale of Evaluating Identifying Assumptions: Did Reality TV Really Cause a Decline in Teenage Childbearing?," *Journal of Business & Economic Statistics*, 2018, pp. 1–10.

Katz, Lawrence F and Alan B Krueger, "Understanding Trends in Alternative Work Arrangements in the United States," *RSF: The Russell Sage Foundation Journal of the Social Sciences*, November 2019, *5* (5), 132–146.

Kilcoyne, Patrick, "Occupations in the Temporary Help Services Industry," 2004.

Lancaster, Tony, "Orthogonal parameters and panel data," *The Review of Economic Studies*, 2002, *69* (3), 647–666.

Lane, Julia, Kelly S. Mikelson, Pat Sharkey, and Doug Wissoker, "Pathways to Work for Low-Income Workers: The Effect of Work in the Temporary Help Industry," *Journal of Policy Analysis and Management*, 2003, *22* (4), 581–598.

Leigh, J Paul, "Economic Burden of Occupational Injury and Illness in the United States," *Milbank Quarterly*, 2011, *89* (4), 728–772.

McLaren, Christopher F. and Marjorie L. Baldwin, "Workers' Compensation: Benefits, Coverage, and Costs," Technical Report, National Academy of Social Insurance, Washington, DC 2017.

Pallais, Amanda, "Inefficient hiring in entry-level labor markets," *American Economic Review*, 2014, *104* (11), 3565–99.

Park, YS and Richard J. Butler, "The safety costs of contingent work: Evidence from Minnesota," *Journal of Labor Research*, 2001, *XXII* (4), 831–849.

Patel, Elaine Weiss Griffin Murphy Jay and Leslie I. Boden, "Workers' Compensation: Benefits, Coverage, and Costs," Technical Report, National Academy of Social Insurance, Washington, DC 2020.

Powell, David and Seth Seabury, "Medical Care Spending and Labor Market Outcomes: Evidence from Workers' Compensation Reforms," *American Economic Review*, 2018, *108* (10), 2995–3027.

Quinlan, Michael, "The effects of non-standard forms of employment on worker health and safety," Technical Report 67, International Labour Organization, Geneva 2015.

Silva, JMC Santos and Silvana Tenreyro, "The log of gravity," *The Review of Economics and Statistics*, 2006, *88* (4), 641–658.

Smith, Caroline K, Barbara A Silverstein, David K Bonauto, Darrin Adams, and Z Joyce Fan, "Temporary Workers in Washington State," *American Journal of Industrial Medicine*, 2010, *53*, 135–145.

Underhill, Elsa and Michael Quinlan, "How Precarious Employment Affects Health and Safety at Work: The Case of Temporary Agency Workers," *Relations Industrielles*, 2011, *66*, 397–421.

Wooldridge, Jeffrey M, "Distribution-free estimation of some nonlinear panel data models," *Journal of Econometrics*, 1999, *90* (1), 77–97.

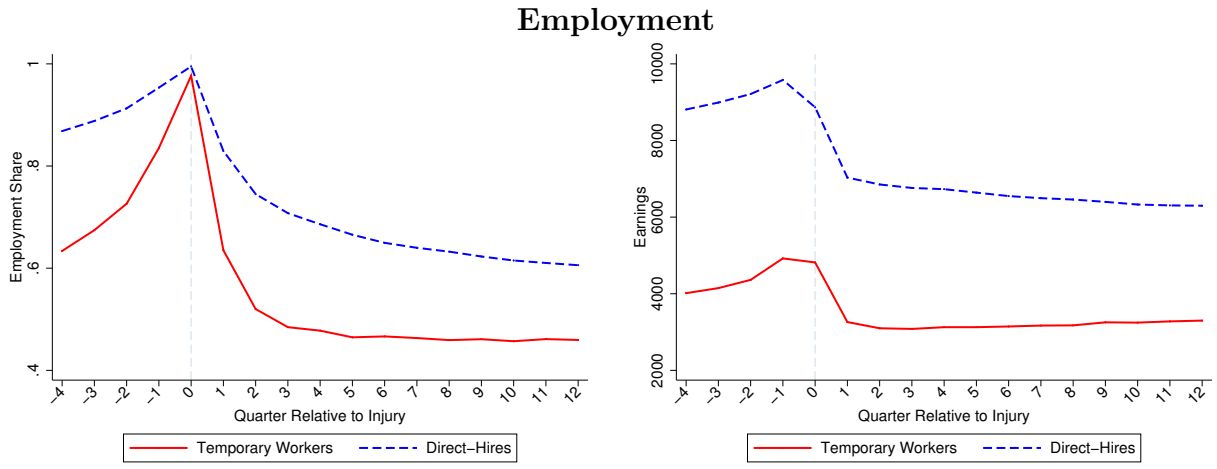
Workers' Compensation Insurance Rating Bureau of California, *California Workers' Compensation Uniform Statistical Reporting Plan — 1995*, Oakland, CA: WCIRB California, 2018.

Zaidman, Brian, "Examining the injuries of temporary help agency workers," 2017.

Zou, Guangyong, "A modified poisson regression approach to prospective studies with binary data," *American Journal of Epidemiology*, 2004, *159* (7), 702–706.

A Supplementary Tables and Figures

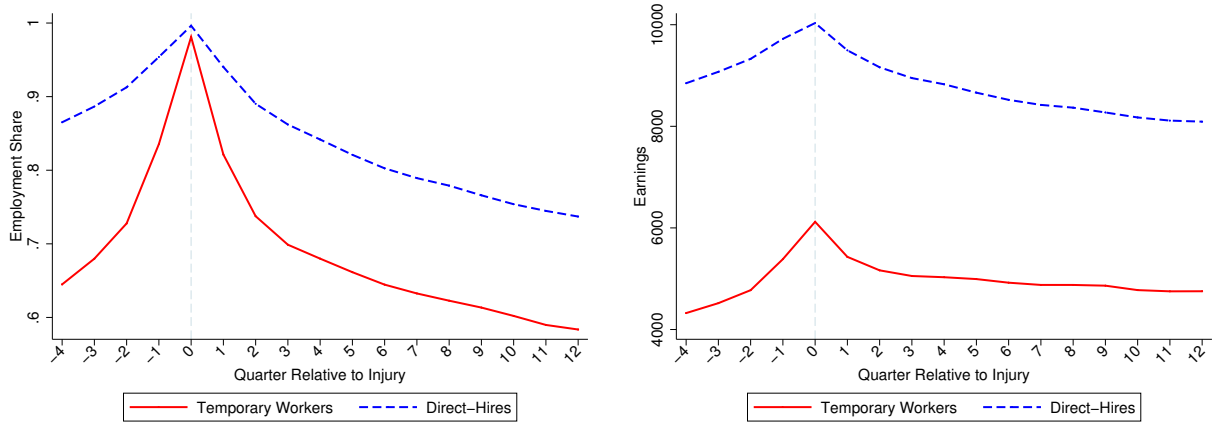
Figure A1: Raw Trends for Employment and Earnings of Injured Workers Receiving Indemnity Benefits



A: Employment B: Earnings

Notes: We present raw averages by quarter relative to injury for the indemnity benefit sample. Earnings are pre-tax and in 2014 dollars.

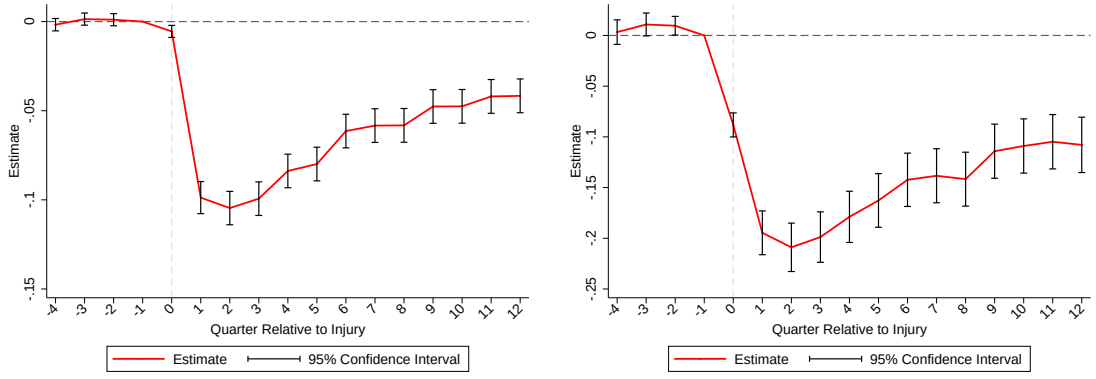
Figure A2: Raw Trends for Medical-Only Sample



A: Employment B: Earnings

Notes: We present raw averages by quarter relative to injury for the medical-only benefit sample. Earnings are pre-tax and in 2014 dollars.

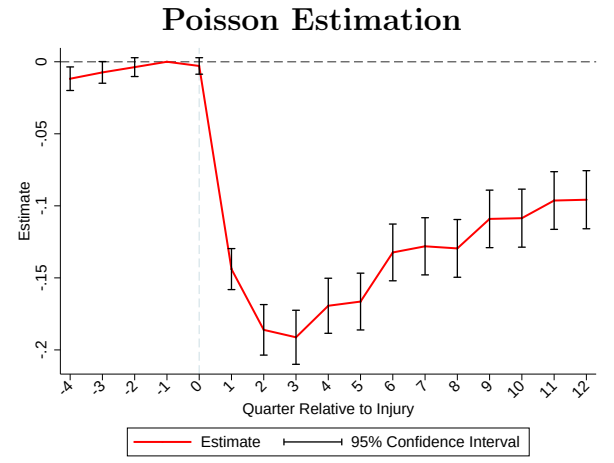
Figure A3: Stacked Difference-in-Differences Event Study Estimates



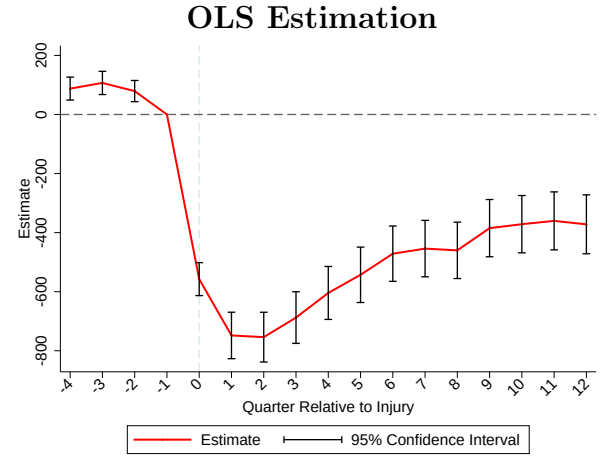
A: Employment B: Earnings

Notes: N = 397,160 injured workers. Point estimates from difference-in-differences event study plotted along with 95% confidence intervals. Baseline period is quarter prior to injury. Individual fixed effects plus fixed effects based on class code*calendar time of injury*time-relative-to-injury. We also interact time-relative-to-injury*calendar time of injury indicators with 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. Finally, we interact time-relative-to-injury indicators with quarter of injury indicators and measures of pre-injury earnings (see text for details). Confidence intervals adjusted for clustering by class code. OLS is used for Panel A; Poisson is used for Panel B.

Figure A4: Event Studies Using Different Functional Form Assumptions



A: Employment



B: Earnings

Notes: Notes: N = 397,160 injured workers. Point estimates from difference-in-differences event study plotted along with 95% confidence intervals. Estimates are analogous to those presented in Figure 1. Baseline period is quarter prior to injury. Individual fixed effects plus fixed effects based on class code*calendar time of injury*time-relative-to-injury. We also interact time-relative-to-injury indicators with 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. Finally, we interact time-relative-to-injury indicators with quarter of injury indicators and measures of pre-injury earnings (see text for details). Confidence intervals adjusted for clustering by class code. Poisson is used for Panel A; OLS is used for Panel B.

Tables

Table A1: 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 A2: Summary Statistics: Characteristics of Temporary and Direct-Hire Workers

	Indemnity Sample			Medical-Only Sample	
	Direct-Hire	**Temporary Hire**		**Direct-Hire**	**Temporary Hire**
Job			**Job**		
Weekly wage	608.02	478.03	Weekly wage	458.05	420.49
Full year tenure	68.3%	24.3%	Full year tenure	66.6%	23.5%
Full time	78.4%	66.9%	Full time	75.3%	70.2%
Demographics			**Demographics**		
Female	47.0%	40.3%	Female	47.7%	39.3%
Age	41.68	38.90	Age	37.88	35.26
Cause of Injury			**Cause of Injury**		
Caught in	2.7%	5.4%	Caught in	3.0%	4.5%
Rubbed by	0.6%	1.1%	Rubbed by	0.6%	1.2%
Striking	3.1%	5.1%	Striking	5.2%	8.0%
Struck by	9.1%	12.7%	Struck by	13.6%	14.5%
Strain	43.5%	41.7%	Strain	33.3%	31.2%
Fall	19.7%	18.7%	Fall	13.2%	11.9%
Cut	4.6%	3.9%	Cut	11.9%	11.3%
Burn	1.7%	1.4%	Burn	3.4%	3.3%
Misc.	12.0%	8.4%	Misc.	13.7%	12.9%
Crash	2.9%	1.6%	Crash	2.0%	1.1%
Nature of Injury			**Nature of Injury**		
Specific injury	87.7%	90.0%	Specific injury	90.3%	91.0%
Cumulative injury	8.6%	4.7%	Cumulative injury	5.6%	3.7%
Multiple injury	3.0%	4.7%	Multiple injury	2.4%	3.3%
Other injury	0.4%	0.4%	Other injury	1.1%	1.3%
Sample Size	384,286	12,874	**Sample Size**	1,120,322	35,031

Source: Authors' calculations, 2005-2012 WCIS. 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 industry 56132. Medical-only sample consists of claims for minor injuries that do not result in payment of indemnity benefits.

Table A3: Composition of Switcher Sample Before Injury

Type of Worker	Quarters as Temp	Quarters as Direct Hire	Quarters Not Employed
Direct-hire in QOI, indemnity	2.12	4.27	1.61
Temp in QOI, indemnity	2.05	3.79	2.16

Notes: QOI = quarter of injury. N=20,689 injured workers. Sample selected on workers with *both* direct-hire and temporary work history in two years prior to injury.

Table A4: Summary Statistics: Labor Market Outcomes for Temporary and Direct-Hire Workers with Medical-Only Claims

	Direct-Hire		**Temporary**	
	Value	(% of baseline)	Value	(% of baseline)
Pre-Injury Earnings/Employment				
Earnings in Year Before Injury	36,971.21	100.00%	19,002.14	100.00%
Employed 1 Year (4Q) Before Injury?	86.52%	100.00%	64.49%	100.00%
Post-Injury Earnings				
1st Year (4Q) Post-Injury Earnings	36,434.96	98.55%	20,678.10	108.82%
2nd Year (8Q) Post-Injury Earnings	33,973.07	91.89%	19,669.56	103.51%
3rd Year (12Q) Post-Injury Earnings	32,652.39	88.32%	19,144.30	100.75%
Post-Injury Employment				
1st Year (4Q) Post-Injury Employment	84.18%	97.29%	67.99%	105.43%
2nd Year (8Q) Post-Injury Employment	77.91%	90.05%	62.28%	96.57%
3rd Year (12Q) Post-Injury Employment	73.71%	85.19%	58.36%	90.49%

Source: Authors' calculations, 2005-2012 WCIS-EDD. N = 1,120,322 injured direct-hire workers and N = 35,031 injured temporary workers. Temporary workers defined as those with a payroll employer in NAICS industry 56132. Earnings adjusted to real 2014$ using the CPI-U.

Table A5: Medical Care Spending and Replacement Rate

	A. Difference-in-Differences			
Outcome:	Medical Care Spending		Replacement Rate	
Temporary	-0.003	-0.006	0.015***	-0.000
	(0.018)	(0.018)	(0.002)	(0.000)
Covariates?	No	Yes	No	Yes
Estimator	Poisson	Poisson	OLS	OLS
N	251,364	251,364	397,160	397,160
	B. Triple-Differences			
Outcome:	Medical Care Spending		Replacement Rate	
Temporary × Indemnity	-0.003	-0.000	-0.080***	-0.005***
	(0.018)	(0.018)	(0.002)	(0.000)
Covariates?	No	Yes	No	Yes
Estimator	Poisson	Poisson	OLS	OLS
N	759,949	759,949	1,552,513	1,552,513

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Outcomes in first two columns refer to medical spending reported for the first 2 years post-injury. Replacement rate refers to weekly benefits scaled by (the administrative) pre-injury weekly wage. Heteroscedasticity-robust standard errors in parentheses. In Panel A, we control for fixed effects based on class code*quarter-of-injury. In Panel B, we control for fixed effects based on class code*quarter-of-injury*injury classification (indemnity vs. medical-only). Additional covariates are same as those listed in Table 3. The sample in the first two columns is limited to injured workers with reported medical spending data (see text for details).

Table A6 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 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 A6: Mean Benefit Payments and Incidence by Benefit Type and Work Arrangement

Cumulative Benefit Amounts ($)	**Direct-Hire**	**Temporary**
Temporary Disability Benefits	8,100.80	5,698.34
+ Permanent Disability Benefits	13,651.83	10,152.26
+ Disability Benefit Settlements	15,285.13	11,238.98
+ Unspecified Settlements	17,653.40	14,936.50
+ Medical Settlements	20,355.13	15,994.53
Percentage Receiving Benefits		
% with TD benefits	87.50%	88.00%
% with PD benefits	44.37%	41.73%
% with a TD or PD settlement	11.43%	9.01%
% with an unspecified settlement	10.06%	21.18%
% with a medical settlement	12.45%	6.08%
Cumulative Percentage Receiving Benefits		
% with TD benefits	87.50%	88.00%
% with PD benefits	97.57%	97.65%
% with a TD or PD settlement	100.00%	100.00%
% with an unspecified settlement	100.00%	100.00%
% with a medical settlement	100.00%	100.00%
Number of Observations	384,286	12,874

Notes: Benefit levels are only relevant for sample receiving indemnity benefits. Benefits in 2014 dollars.

B Online 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

C Program Details and Data Sample

C.1 Program and Data Details

The claims data used in this study (for injury dates from 2005-2012) was extracted in 2014. These data are derived from First Reports of Injury (FROI) and Subsequent Reports of Injury (SROI) reported to the WCIS by claim administrators. Claim administrators are required to submit a FROI (reporting the occurrence of a new injury) within 10 days of the date when a new claim is filed, and are required to submit SROI (reporting other events, such as the start and end of benefit payments) within 15 days of the event that triggers a required report.

To file a workers' compensation claim in California, the injured worker generally must notify the employer of the injury within 30 days of the injury date, and must complete a form to initiate the claim within one year of the injury date. (A claim can also be initiated by a doctor who treats a work-related injury.) After the claim administrator (the insurer, employer, or third party administrator) receives the claim, the worker must be notified within 14 days if the claim is being accepted, denied, or delayed pending further investigation. If the claim is delayed, it must be denied or accepted within 90 days; claims that are not denied within 90 days are legally presumed to be accepted. Aggregate statistics from the WCIS indicate that around 10 percent of claims filed are denied at some point in the claim process (within the first 14 days, the first 90 days, or at some later point). Claims that are denied may have received benefits prior to the denial, or may receive benefits after the denial is appealed and reversed.

We used reports of paid temporary disability benefits, permanent disability benefits, or settlements for these benefits to identify workers with indemnity injuries (indicating that the injury resulted either in work absence beyond 3 days or residual long-term impairment affecting the worker's earnings capacity in the long term). Temporary disability benefits in California begin on the fourth day of injury-related work absence: injury-related work absence generally occurs very soon (if not immediately) after the injury. Permanent disability benefits do not begin until the worker's recovery from the injury has reached a point where a physician determines that further recovery is unlikely, and that permanent impairment is present. Disability evaluation and the assignment of permanent disability levels can be a very slow process that may not be resolved for several years after the injury, although claim administrators are required to begin advance payment of permanent disability benefits within two weeks even if the disability evaluation process remains incomplete if

the worker's condition has stabilized and it is likely that the worker has some permanent impairment.

The claims data used in this paper, which cover 2005-2012 injury dates, were collected in September 2014. For the range of injury dates examined, the lag between the injury date and the date of data collection is sufficient for essentially all claims to have been submitted, and for all temporary disability benefits to have been paid. For claims with permanent disability benefits with injury dates at the very end of our study period (2011 or 2012), some right-censoring of paid benefit amounts is possible, and some workers who will later receive permanent disability benefits may not have received this type of benefit yet. However, the vast majority of indemnity injuries from our study should be identifiable as indemnity injuries by the time of data collection, as temporary disability benefits (which are received by 88 percent of workers with indemnity benefits) are likely to be paid out very soon after injury. We also have no reason to think that claim filing or processing timelines, or the duration of permanent disability benefit payments would differ systematically between temporary worker and direct-hires in the same types of jobs. Finally, the similarity of estimates for the 2005-2008 injury cohorts (whose benefit payments are measured at least 5 years, 9 months after injury) to those for the full sample provides reassurance that our findings are not somehow an artifact of right-censoring in the most recent injury years (see Table 3, columns 2 and 4).

C.2 Construction of Sample and Weights

Our analysis sample excludes cases that failed to match to a usable EDD earnings history for the injured worker. Cases might fail to link to EDD if the SSN used in either the EDD or WCIS data was reported incorrectly. We consider earnings histories at EDD unusable for workers with large positive outliers in quarterly earnings or who have inconsistently reported names over time in the EDD data. Consistency of names on the EDD earnings data were checked using an algorithm described in Dworsky et al. (2016) that allows for minor spelling differences, nicknames, and last name change at marriage.

Out of 5.44 million cases with a first report of injury for injury years 2005-2012, 17.1% were dropped because of failure to match to EDD data, leaving 4.51 million cases. We also casewise-deleted workers with missing WCIS data on any of the key control variables used in our analysis, including age at injury; gender; class code; part, nature, and body part of injury; weekly pre-injury wage; and full-time/part-time status. Casewise-deleting on these variables excludes another 18.2% of the total number of cases in the data, leaving 3.52 million

injury claims (64.7% of first reports of injury) with complete records and usable earnings histories. Our analytic sample for this paper was further limited based on industry and class code, as described in the main text. There was no difference (to three decimal points) between temporary and direct-hire workers in terms of the percentage dropped from the sample due to incomplete records.

To produce estimates and summary statistics that are representative of the sample with usable date of injury, geographic, and demographic data, we constructed cross-sectional weights for our analytic sample. We grouped the data into cells defined by gender, year and quarter of injury, region of the state (Los Angeles, Inland Empire, Bay Area, Central Valley, San Diego, or Rest of State), and age (10-year bins from ages 15 to 75), counted the number of injuries reported (First Reports of Injury) in each bin defined by these four variables, and then counted the number of cases with complete records and usable earnings histories. The weights for each of these cells were defined as the number of FROI divided by the number of cases in the analytic file. These weights are not critical to the internal validity of our study, and have no meaningful impact on our difference-in-differences estimates: of the six estimates (and associated standard errors) in columns 2 and 3 of Table 3, five are identical up to the third decimal place, while one estimate and one standard error are different by 0.001.