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ABSTRACT

Returning to Work from Injury: Longitudinal Evidence on Employment and Earnings*

New Zealand has a unique accident insurance system that pays the direct costs of all accidental injuries and compensates workers 80% of their earnings for any time post-injury that they are unable to work. Statistics New Zealand's Linked Employer-Employee Database contains monthly information on earnings, welfare benefit income, and accident-related earnings compensation for all New Zealanders from 1999-2004. Using time receiving earnings compensation as a proxy for injury severity, we estimate the effect of injuries on employment and benefit rates, and total income by comparing the observed changes in outcomes for the injured population with matched 'control' groups of non-injured individuals. We find that injuries that result in more than 3 months of earnings compensation have negative effects on future labour market outcomes. For example, individuals who receive 4 months compensation have 2% lower employment rates and 6-8% lower monthly incomes 18 months after compensation ends compared with 18 months prior to being injured than comparable non-injured workers. The magnitude of these effects increase with injury duration; individuals who receive 10-12 months of compensation have 10-15% lower employment rates, 3-4% higher benefit receipt rates, and 14-22% lower monthly incomes. We also find evidence that longer-duration injuries have larger impacts on women, older workers, and workers with lower earnings or with less stable employment histories.

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1. Introduction

Injuries can have large long-term effects on labour market outcomes for individuals.[1] In addition, they can inflict substantial costs on the overall economy.[2] Examining the causal impacts of injuries on labour market outcomes is often either not possible or problematic using survey data.[3] Instead, past research has relied on administrative data from workers compensation systems and thus has only been able to examine the impact of workplace injuries (Berkowitz and Burton 1987; Biddle 1998; Boden and Galizzi 1999; 2003; Reville 1999; Reville and Schoeni 2001).[4] In addition, with the exception of Reville (1999) and Reville and Schoeni (2001), these studies have relied solely on data from the injured population. Thus, they have been forced to use either pre-injury labour market outcomes for injured workers (Berkowitz and Burton 1987) or post-injury outcomes for workers with minor injuries (Biddle 1998; Boden and Galizzi 1999; 2003) to measure counterfactual outcomes for injured workers. However, pre-injury outcomes are not suitable for measuring this counterfactual if workers would have experienced a change in earnings and/or employment status had they not been injured, while workers with minor injuries will only provide a suitable comparison group if minor injuries have negligible impacts on labour market outcomes.

In this paper we exploit a unique accident insurance scheme, together with comprehensive administrative data on accident-related earnings compensation, to examine the impact of injuries on labour market outcomes. First, New Zealand has a state-run accident insurance system, commonly

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[1] Weil (2001) summarises the findings from a number of studies examining the long-run impacts of injuries and documents significant losses for injured workers. For example, Reville (1999) estimates that, for workers in California receiving permanent partial disability payments, earnings are reduced by about 40% in the 5 years following an injury.

[2] Leigh, et. al. (1997) estimates that the total economic costs of occupational illnesses and injuries were approximately 3% of US GDP in 1992.

[3] First, general social surveys do not ask about injuries and many do not even ask about disabilities. Second, surveys that do ask detailed questions about injuries, typically collect only limited labour market information. Third, no standard survey instrument exists for measuring injury or disability and thus these questions, even when asked, rarely measure something that is likely to be consistently reported across the population and over time (Weil 1999). Equally problematic, many commonly used questions on disability status are asked in a way that is unlikely to be independent of employment outcomes (for example, the US Census asks whether a respondent has a health condition limiting the kind or amount of work he or she can do). Fourth, only a small percentage of individuals suffer relatively serious injuries and thus sample populations are quite small in datasets that also have suitable labour market information.

[4] One notable exception is Krueger and Kruse (1995) which uses survey data collected by the authors to examine the impact of spinal cord injuries on long-term labour market outcomes using a similar approach to that taken in this paper. A number of other studies use survey data to describe labour market outcomes for disabled individuals, but the data used suffer from the problems discussed in the previous footnote, and these studies are typically unable to identify the causal impact of disability (eg. Haveman and Wolfe 1990; Yelin and Katz 1994). There is also an extensive literature that focuses on understanding the incentive effects of workers compensation insurance systems (eg. Meyer, Viscusi and Durbin 1995; Krueger 1990).

referred to as ACC, that pays the direct costs of all accidental injuries and compensates workers 80% of their earnings for any time post-injury that they are unable to work.[5] In contrast to most countries which have accident insurance systems that cover only workplace injuries, ACC does not differentiate based on where injuries occur. Second, Statistics New Zealand's (SNZ) Linked Employer-Employee Database (LEED) contains monthly information on earnings, welfare benefit income, and ACC earnings compensation for all New Zealanders from 1999-2004.[6] The structure of LEED also provides the ability to longitudinally follow all individuals and to match them to their employers.

Comparing the characteristics of injured and non-injured workers, we find that injured workers are not representative of the working population, with evidence of differences across several dimensions, including individual, firm and geographic characteristics. In order to control for such differences, following the program evaluation literature, we use a matching methodology approach to construct "control" groups of non-injured workers that have similar pre-injury observable characteristics as the injured population (Rubin 1979; Lalonde 1986). We exploit three features of LEED for this purpose. First, LEED's comprehensive coverage allows us to use a non-parametric matching method and create control groups that share both the same local labour market and the same time period as the injured population.[7] Second, LEED's structure allows us to match workers within firms, thus controlling for any firm-specific heterogeneity associated with workers' injuries and outcomes. Third, the longitudinal nature of LEED allows us to control for permanent unobserved individual heterogeneity by using a 'difference-in-differences' matching estimator that compares the change in injured versus non-injured group outcomes over a common time period (Heckman et. al. 1998; Smith and Todd 2005).

Our results indicate that injuries that result in more than 3 months of earnings compensation have negative effects on future labour market outcomes. For example, individuals who are injured for 4 months have 2% lower employment rates and 6-8% lower monthly incomes 18 months after compensation ends compared with 18 months prior to being injured than comparable non-injured

[5] The system costs approximately 1.5% of GDP per annum to run.
[6] While LEED does not directly measure injuries, we are able to use receipt of earnings compensation to proxy for injury, and the payment spell length to proxy for severity.
[7] Heckman, et. al. (1998) provide strong evidence that a common economic environment for the treatment and control groups is a key requirement for obtaining unbiased estimates using matching methods.

workers. The magnitude of these effects increase with injury duration: individuals who have 10-12 month injury spells have 10-15% lower employment rates, 3-4% higher benefit receipt rates, and 14-22% lower monthly incomes. We also find evidence that longer-duration injuries have larger impacts on women, older workers, and workers with lower earnings or with less stable employment histories.

This analysis provides a twofold contribution to the existing literature. First, the institutional setting in New Zealand allows us to examine the impact of injuries regardless of where they occur. This could have a large impact on the results because a majority of injuries occur outside the workplace and firms may be more accommodating to individuals who have been injured at work.[8] Second, LEED's comprehensive coverage allows us to estimate the impact of injuries by comparing outcomes for injured workers to those for a matched sample of non-injured workers, as opposed to relying solely on data from the injured population.[9]

2. Background

The New Zealand Accident Insurance System

The New Zealand accident insurance system (ACC) provides cover for all citizens, residents and temporary visitors.[10] In return, individuals forsake the right to sue for personal injury, other than for exemplary damages. The scheme provides cover for all injuries regardless of fault. ACC pays much of the direct medical and rehabilitative costs of injuries, and compensates workers 80% of their total earnings for any time post-injury that they are unable to work.[11] More than 1.5 million new claims are lodged each year. Of these, around 60,000 involve earnings compensation paid to individuals who have missed work because of their injury. This earnings compensation costs more than $600 million

[8] In New Zealand, 60% of claims involving earnings compensation stem from non-workplace injuries.

[9] While Reville (1999) and Reville and Schoeni (2001) use a similar estimation approach, these papers are only able to examine the impact of workplace injuries.

[10] The accident insurance system was privatised for workplace injuries from July 1999 to June 2000 and insurers were allowed to 'rate' employers and charge different levies based on their claims history. Following a change in government in the state-run system was reinstated. Crichton, et. al. (2004) discuss the accident insurance system in greater detail.

[11] Compensation paid to wage and salary earners is capped (adjusted annually for inflation) - the maximum weekly amount paid in 2002/03 was $1365 before tax – impacting approximately 5% of earnings compensation claims. Some employers elect to top-up an individual's compensation above the 80% reimbursement and/or above the total cap for work and in some cases non-work related injuries. ACC does not cover the first week of work missed following the injury. However, if the injury is work related, employers are required to cover this week. During the 2nd-5th week off work, the amount of compensation paid is based on the individual's average earnings in the 4 weeks prior to injury. Following the 5th week, compensation is based on the individual's average earnings in the 52 weeks prior to injury.

annually. The cost of the ACC system is covered by levies paid by employees and the self-employed, petrol and annual vehicle license levies and government appropriations.

Individuals are no longer entitled to receive earnings compensation if they are assessed by independent medical assessors as being capable of working at least 35 hours per week. ACC provides rehabilitation and training in order to assist workers to return to their pre-injury job or, if that is not possible, to a new job that is appropriate to the individual's skills and experience. Employers are not legally required to hold an injured worker's job open, however, they are required to follow employment contract provisions and regulations governing termination of workers' contracts.

Theoretical Link Between Injuries and Labour Market Outcomes

Injuries can affect labour market outcomes through a variety of pathways. Injuries may directly affect an individual's productivity by making work tasks difficult to perform. For example, a back injury can limit the effectiveness of an office worker who must sit at a desk for long periods of time. Even if an injury does not lead directly to a loss in productivity, time spent away from the workplace undergoing treatment or recuperating can lead to a reduction in general or firm-specific human capital. For example, an individual may miss out on a promotion opportunity. Injuries can also have strong psychological effects on individuals that lead to employment loss or earnings reductions; an employee may be uncomfortable returning to the same job or to the same employer, and a desire to change jobs may lead to a reduction in earnings through the acceptance of a less optimal job or because an individual has to 'start over'.

A variety of factors can mitigate the effect that injuries have on later outcomes. Rehabilitation can lessen or eliminate the direct effect injuries have on productivity. Some firms have active policies for assisting injured workers in their return to work. Firms may also vary in their ability and inclination to ensure that a position is made available that is appropriate for an injured worker. Some firm are also better able to bear the costs of providing improved access or other accommodations for disabled workers. Local labour market conditions can also influence the success of the return to work process. When the economy is strong, some firms may be more willing to accommodate injured

workers, and injured workers who wish to change employers may find it easier to locate a replacement job equal to their old job.

3. Data Characteristics

The Linked Employer-Employee Database

LEED is an experimental dataset under development at SNZ which is based on monthly administrative data collected by the Inland Revenue Department (IRD).[12] Each month, all employers in New Zealand are required to file an Employer Monthly Schedule with the IRD that lists all individuals employed at that firm, the amount of income they received, and the amount of tax that was deducted at source.[13] Individuals and firms each have unique administrative (tax) identification numbers that can be used to track them longitudinally. In addition, there are two important 'employee-employer' relationships identified in the database: receipt of working-age taxable benefits, and receipt of ACC earnings related compensation.[14] LEED currently contains 60 months of linked employer-employee records from April 1999 to March 2004.

IRD's administrative records contain some basic demographic information on individuals and firms. This data includes sex, age, and address details for employees and industry information for employers. This core data can also be used to create additional variables, such as the number of employees and the total payroll for all firms, the number of jobs held by each employee in a particular month and each individual's pattern of employment over the 60-month period. LEED has a number of limitations that impact on the research in this paper. Important demographic variables such as education and ethnicity are not available. More importantly, there is no information on occupation,

[12] Kelly (2003) and Carroll and Wood (2003) provide a detailed discussion of the LEED project and database.

[13] New Zealand has a relatively simple tax system and most tax on income from wages and salary is paid on a 'pay-as-you-earn' basis with only some groups of taxpayers needing to reconcile their taxes at the end of the year. LEED records an individual's taxable earnings received in each calendar month, this can include one-off payments such as bonuses or redundancy pay, and does not include undeclared income. Because calendar months have uneven numbers of days and pay periods are often weekly or fortnightly, earnings levels are affected by the timing of pay and the number of pay periods in a month. Furthermore, in months where individuals receive income from multiple employers (including from benefits or ACC) it is not possible to identify whether the two jobs are concurrent or whether the person has changed jobs during the month. Income received in a particular month can also reflect work undertaken in the past. Similarly, ACC and benefit payments can sometimes be received for prior periods of eligibility.

[14] Welfare benefits and ACC compensation are taxed at source and thus reported to IRD. Unique IRD numbers identify the social welfare agency and ACC as the 'employer' for these payments. ACC has an employer reimbursement programme whereby employers continue to pay employees while they are off work due to injury, and are later reimbursed by ACC. These injured individual's are not identified in the LEED dataset. ACC figures indicate that approximately 4% of all claims where earnings compensation is paid are associated with individuals that are employed by firm participating in the re-imbursement programme.

which is likely to be a key characteristic in explaining an individual's exposure to workplace injuries and their likelihood of risk-taking behaviour away from work. Information is also not currently available on earnings from self-employment.[15]

The Injured Population

We classify individuals as 'on ACC' in any month they receive income from ACC. A series of consecutive months receiving ACC are referred to as an 'ACC spell'.[16] Although the injury may have occurred in the period prior to ACC payment receipt, we refer to the first month of ACC receipt as the "injury" month. We focus our analysis on all individuals whose first observed ACC spell started after September 2000, and who were employees (this restriction drops the injured self-employed) and aged 15–69 in the month prior to injury. We exclude individuals who received ACC in the period prior to October 2000 for two reasons: first, this provides an 18-month period prior to injury to control for differences between the injured and non-injured populations; and, second, workplace injury claims are not identifiable in LEED during the one-year period from July 1999 to June 2000 when the ACC scheme was privatised.

Approximately 2.25 million individuals aged 15–69 worked in at least 1 month between October 2000 and March 2004. Of these, 120,000 started an ACC spell; we refer to this group of individuals as the 'injured population'. Table 1 presents the distribution of the number and length of ACC spells experienced by the injured population. Of this population, 84% had a single ACC spell, 13% have 2 ACC spells, and the remaining 3% have more than 2 spells over the period October 2000 to March 2004. Our analyses focuses on the effects of the first ACC spell experienced by an individual during the study period.

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[15] Most self-employed individuals fill out a different form to calculate their tax liability - this information is not currently available within LEED. Some income from self-employment does appear on the monthly returns from employers, and is separately identified in LEED. We have excluded this income from our analysis.

[16] We spent some time examining the gaps between spells for individuals with multiple ACC spells and decided not to fill in short gaps between spells. Filling in short gaps has a limited effect on the distribution of spells, as the majority of spells are separated by three or more months, and we disliked the somewhat arbitrary nature of deciding what is a 'short' gap, especially without having any information on whether a new ACC spell is recurrence of an old injury..

Because our data has no direct measure of the severity of an individual's injury, we use the duration of the ACC spell as a proxy for severity.[17] ACC spells are typically short: 68% of first spells last 1–2 months, 5% last longer than 6 months, and 8% of spells are right censored (and thus excluded from our analyses). Our main regression analysis examines outcomes 6, 12, and 18 months after the end of the first ACC spell. This leads to a progressive decrease in the number of ACC spells (particularly, longer-duration spells) for which 6, 12, and 18 months outcomes can be examined. 81% are observed at 6 months, 66% at 12 months, and 53% of the population are observed 18 months after the end of the first ACC spell.

Table 2 presents the characteristics of the injured population in the month prior to injury, and a comparison 1% random sample of the never-injured individuals in each month after September 2000 who were employed and aged 15-69 (i.e. satisfies the same restrictions as in our injured population): we refer to this latter sample as the "non-injured" population. The characteristics examined include age, sex, region, employment and benefit status, average earnings and income in prior months, firm size and industry.[18] Individual and firm characteristics are measured in the month prior to injury for the injured population and in the selection month for the non-injured population, while employment status, earnings and benefit receipt are measured in the 6 months prior to this month.

Column 1 presents the results for the non-injured population and column 2 for the injured population. Comparing columns 1 and 2, we can see that the injured population is 2 years younger and more likely to be male (67% of the injured population is male compared to 50% of the non-injured population), to live outside the Auckland and Wellington regions, to have spent more time in receipt of welfare benefits prior to injury, to have lower average earnings (and income), and to be employed in agriculture/fishing/forestry/mining, manufacturing, and construction, and less likely to work at very large firms, and to be employed in finance/business/property services, other services, and

[17] It is important to note that spells of one month include injuries that result in only a single day or a few days of compensation, while many two-month spells cover only a week or two, over two consecutive calendar months, and thus the duration of the ACC spell is an imperfect measure of the actual duration of the injury.

[18] Firm size and industry are measured for each individual's main employer, which is the employer from whom they received the highest earnings in the month prior to the reference month. In the remainder of this paper, employment refers to receiving any income for any employer in a particular month, benefit receipt refers to receiving any income from welfare benefits in a particular month, earnings refers to the amount of income received from all employers (besides ACC earnings compensation or welfare benefits) in a particular month, and income the total amount of income received from employers, welfare benefits, and ACC earnings compensation in a particular month.

education.[19] As anticipated, industry is strongly correlated with the likelihood of being injured. Regional variation is likely capturing differences in occupational composition across regions, with safer white collar jobs more likely to be located in the Auckland and Wellington regions. Similarly, workers with lower earnings and less time in employment prior to injury are also less likely to work in white collar jobs.

Columns 3–9 present the results for the injured population stratified by the length of their first ACC spell. We examine spells shorter than 4 months on their own and then for conciseness, group 4–6, 7–9, 10–12 and 13–24 months together in this table. As there are fewer than 500 individuals in our sample with a first spell of more than 24 months, we do not include them in this table or in any of the remaining analyses in this paper. In general, there appears to be little systematic difference in the characteristics of individuals with different length spells. One exception is age, with spells of longer duration having a progressively higher mean age. This may occur because older individuals have more difficulty recovering from injury. Individuals with longer-duration spells are also more likely to be female and to have higher average earnings in months employed prior to injury than those with shorter ACC spells, however, these differences are not large and the earnings difference may be caused by the increased age of individuals with longer-duration injuries.

Event Study of the Effect on Injuries on Labour Market Outcomes

We next describe the longitudinal patterns of alternative labour market outcomes before and after an injury occurs. For this purpose, figures 1 and 2 present results of an "event" study of various labour market outcomes before and after the injury month, which is normalized to 0. Figure 1 presents the proportions of the injured population employed, receiving welfare benefits, and receiving ACC earning compensation in the months before, during and after an ACC spell. Analogously, figure 2 presents the unconditional average earnings, average benefit receipt, and average ACC compensation. The figures include the time during which an individual is injured and receiving ACC. These graphs

[19] These univariate comparisons are potentially misleading. For example, the large sex difference between the injured and non-injured population might occur because of the large difference in the industrial composition in these two samples. Thus, we also estimate a linear probability regression model for the outcome 'whether an individual is injured or not', using the individual and employer characteristics as independent variables. These multivariate results tell the same basic story as the univariate comparisons.

are shown separately by the length of the first ACC spell. By construction, 100% of the sample is employed in the month prior to injury.

As the length of the first ACC spell increases, the likelihood of having a subsequent ACC spell also increases. For those with a first spell of 1–3 months, roughly 3% have a subsequent ACC spell, compared to 8% for those with a 7–9 month spell, and 11% for those with a 13–24 month spell. Having a long-duration ACC spell appears to be associated with an increased likelihood of future spells. This may reflect either new injury spells or a relapses associated with the original injury. However, as the LEED does not distinguish between new injuries and reoccurrences of the initial injury, and we observe relatively few individuals with multiple ACC spells, our main analysis focuses on the first observed spell and censors outcomes at the onset of a second spell.

There is a distinctive pattern of increasing employment in the months leading up to the start of the ACC spell and decreasing employment rates in the months following the end of the spell. A similar pattern is seen for average earnings unconditional on employment. This occurs because we are examining a fixed cohort of individuals who are employed in the month prior to injury, and as we move away from that month (in either direction), general job churning sees more of those individuals out of employment. Of those individuals with a first spell of 1 month, 82% and 71% are employed 12 and 24 months prior to the start of the ACC spell. Of those individuals with a 7–9 month first spell, employment rates prior to injury are slightly higher, with 84% and 76% employed in these months, respectively. For those with a first spell of 1 month, 81% are employed 12 months after the injury month, compared to 76% of individuals with a 7–9 month spell who are employed.

Most previous studies of the effect of injuries on labour market outcomes have compared changes in outcomes over time for injured workers either to expected changes based on restricted regression models, or the changes observed for workers with minor injuries (Berkowitz and Burton 1987; Biddle 1998; Boden and Galizzi 1999; 2003). The results in figures 1 and 2 can be used to perform a similar comparison. For example, these results can be interpreted as showing either: i) injured workers who receive ACC for 7–9 months have 8% lower employment rates 12 months after injury than 12 months before; or ii) injured workers who receive ACC for 7–9 months have a 7% greater decline in

employment rates from 12 months before injury to 12 months after than those receiving 1 month of ACC (a 8% versus 1% decline in employment rate).

However, both these interpretations are problematic. First, without knowing the decline in the employment rate for similar non-injured workers during this time period it is not possible to judge whether an 8% decline is a 'good' or 'bad' outcome. In an economy with significant job turnover, the employment rate for a fixed cohort of workers can decline substantially over time. Second, without knowing whether individuals with minor injuries are actually unaffected by the injury, it is not possible to judge whether they form an appropriate comparison group. Furthermore, as the population with minor injuries is much smaller than the non-injured population, greater parametric assumptions are typically required to use them as a comparison group.

Calculating Counterfactual Outcomes for Injured Workers

In order to calculate the effect of injury on labour market outcomes we need to construct an estimate of what an injured individual's labour market outcomes *would have been* in the absence of the injury. Figure 3 displays the hypothetical earnings of a worker who experiences an injury at month 0 and remains on ACC for 3 months, returning to work at month 3. The solid line represents the actual earnings received by this worker in each month, while the dashed line represents the counterfactual earnings that the worker would have received had they not been injured. The difference between the dashed line and the solid line at any point in time (say at month 9, 6 months after returning to work) represents the effect of injury on a worker's earnings. In this particular example, even though the individual returns to work at their pre-injury pay and experiences earnings growth in the following months, the individual still suffers an earnings loss because of the loss of the wage growth they would have experienced during the 3 months had they not missed work.

As illustrated in this example, an individual's pre-injury labour market outcomes are not suitable for measuring this because the worker would have likely experienced a change in earnings and/or employment status had they not been injured. Our approach to estimating these unobserved outcomes is to match our injured population to a random sample of individuals designed to have similar observed characteristics in the period before these individuals where injured. It is necessary to match

injured individuals to other similar workers because, as discussed above, they are not a random sample of the working population.

As a limited number of individual and firm characteristics are available in LEED, we decided to use a nonparametric case-control matching method, where each injured worker is matched to non-injured workers with the same characteristics. Outcomes for the matched sample of non-injured workers, the 'control' group, can then be directly compared to the outcomes for injured workers. We considered matching on subsets of the following variables: sex, age and geographic location at the time of injury, employment status, earnings and benefit receipt prior to injury, and the firm size and industry of the individual's main employer at time of injury. We also considered matching injured individuals exclusively to their co-workers. We investigated several comparison groups based on different combinations of characteristics, examining the trade-off between sample support (i.e. the percentage of the population of injured workers that can be matched to any member of the control group) and the precision of the match.

Characteristics of the Matched Samples

We match each injured worker to individuals from the population of non-injured workers in the month prior to their injury. This approach minimizes calendar effects as the same months are observed for both the injured and matched non-injured populations. Two match criteria are used in the analysis. The main distinction between these two criteria is that the first matches workers based primarily on individual-level information, while the second matches workers within the same firm. In the 'individual' match, injured workers are matched to non-injured workers with the same sex, age (+/- 2 years), location (12 regional council areas), number of months employed in the 7 months prior to injury, number of employees (7 standard groups) and industry (14 1-digit ANZSIC groups) of their main employer, and within 20% of the average earnings in the months employed in months 2–7 prior to injury.[20] In the 'firm' match, injured workers are matched to non-injured workers with the same

[20] For some individuals, injury is likely to have occurred prior to the first month of ACC receipt (the month we refer to as the "injury" month), and affected their earnings in this month. For this reason, we exclude this month's earnings for matching purposes.

main employer, location, and within 20% of the average earnings in the months employed in months 2–7 prior to injury.

All matching is done with replacement, thus a non-injured worker can be matched to more than one injured worker. In many cases more than one potential match is identified and we randomly select a maximum of ten matches for each injured worker.[21] Overall, 72% of the injured population matches on both the firm and individual criteria, 19% match only on the individual criteria, 5% match only on the firm criteria and the remaining 4% do not match using either criterion. By using these two quite distinct match criteria, we aim to provide evidence of the robustness of our results to the choice of matching algorithm. Our 'firm' match is facilitated by the feature of the LEED data that enables workers to be linked to their firms and is quite similar to the match criteria used in Reville (1999) and Reville and Schoeni (2001) allowing for some comparability of our results.

Table 3 compares the characteristics of the injured population with the subsets of the population that are matched using these two criteria (dropping individuals with a completed first ACC spell more than 24 months in duration). The injured workers for whom a suitable individual match does not exist are less likely to be employed in all 7 months prior to injury, but are otherwise quite similar to the overall injured population. For the firm match, unmatched individuals are more likely to be employed in firms employing less than 10 workers, and, as in the individuals match, less likely to be employed in all 7 months prior to injury. Both our matched subsets appear to be representative of the injured population over a broad set of characteristics.

4. Main Results

Simple Matching Estimates

We now compare the labour market outcomes of the injured workers with those of the matched comparison groups of non-injured workers. The outcomes of the non-injured comparison groups are weighted to reflect the number of matches available for each injured worker (i.e. the weight is equal to

[21] Using the individual match, 8% of injured workers have no match, 6% have only 1 match, 25% have between 2-9 matches, and the remaining 61% have at least 10 matches. Of those with at least 1 match, the average number of matches obtained is 8.0. Using the firm match, 23% of injured workers have no match, 11% have only 1 match, 28% have between 2-9 matches, and the remaining 38% have at least 10 matches. Of those with at least 1 match, the average number of matches obtained is 6.7.

the inverse of the number of matches). We examine employment and benefit rates (figure 4), and average monthly earnings and benefit receipt (figure 5) at successive months before and after injury with the outcomes stratified by length of the first ACC spell (or the spell of the person to which they are matched). These figures present the results from the individual match. The results from the firm match are very similar and are presented in appendix figures 1 and 2. Results using both match criteria are presented below in our regression analysis. In contrast to figures 1 and 2, these figures exclude the period of the ACC spell. Thus, month -1 is the month immediately prior to injury, month 1 is the first month after the ACC spell ends, while the vertical line (at month 0) indicates the injury period. Note that due to the match criteria, all workers are employed in month -1, however, the other outcomes can differ in this month.

Overall, there is clear evidence that the outcomes of injured workers who receive earnings compensation over a period of more than 3 months are considerably worse post-injury than those of the non-injured comparison group. For example, employment rates for individuals receiving 4–6 months of ACC are 5% lower and average earning are 8% and 9% lower at 6 and 12 months post-injury than those of the non-injured group. The outcomes of injured workers who receive ACC for 10–12 months are much worse: employment rates 6 and 12 months post-injury are 14% and 16% lower and average earning 23% and 24% lower than those of the non-injured group. While there are differences between injured and non-injured workers prior to injury (which suggests there are likely to be differences in post-injury outcomes as well), these differences are much smaller than the post-injury differences. For example, employment rates 18 months prior to injury are 1% lower and earnings 2% lower for injured workers receiving 4–6 months of ACC and 3% and 5% lower for those receiving ACC for 10–12 months.

The results for workers with shorter periods of earnings compensation are less clear. While there is evidence that outcomes for these individuals are worse than those for the control group after injury, similar size differences exist prior to injury as well. For example, employment rates 6 and 12 months post-injury are 2% lower for injured workers with a 1-month ACC spell than those of the comparable non-injured group, while relative employment rates 18 months prior to injury are 1% lower. Similarly, relative earnings 6 and 12 months post injury are 3% and 4% lower for these workers,

while relative earnings 18 months prior to injury are 2% lower. There are a variety of ways to interpret these findings for short-duration ACC spells and we discuss this in greater detail after presenting our regression results.

Regression Estimates

The graphical analysis in the previous section gives a broad overview of our main findings. We now present results from our regression analysis, which allows us to control for variables not included in our matching algorithm and to test various hypotheses concerning the effect of injury on labour market outcomes. Adding control variables increases the precision of our estimates by allowing these variables to be correlated with labour market outcomes. The inclusion of variables not already included in our matching algorithm, such as prior benefit receipt, further controls for the possibility that these characteristics are correlated with the likelihood of becoming injured. In order to use this additional 'regression matching' we need to make parametric assumptions about the relationship between these variables and the likelihood of being injured, but with the advantage that sample support is not reduced.

Ignoring individual and time subscripts, the basic regression specification we use is

$$Y = \alpha + \sum_i \delta_{Duration(i)} * [injury * duration(i)] + X'\beta + \mu \qquad (1)$$

where Y is the outcome of interest, *injury* is a dummy variable equal to 1 if the individual has an ACC spell and equal to 0 if they are drawn from the matched control group, α is the model intercept, *duration(l)* is a dummy variable that equals 1 if the individual's ACC spell (or the spell of the person to which they are matched) is of length l, where l=1, 2, 3, 4, 5, 6, 7, 8–9, 10–12, and 13–24 months, X is a vector of variables to control for other factors influencing the outcome, and μ is an error term to capture unobserved effects. All models include the *duration(l)* dummy variables (minus one to avoid collinearity) in the vector of control variables, X, allowing there to be systematic differences between individuals with different length ACC spells. We focus on the coefficients $\delta_{Duration(l)}$, which represent the effects of injury spells of duration i on outcome Y.

To keep the results tractable, we focus on three outcomes: employment, benefit receipt, and total income.[22] All injured individuals who do not have a second ACC spell in the observation period of a particular regression and who received ACC for less than 25 months are included in the analysis.[23] All the results are based on ordinary least squares (OLS) regressions. As all of our control variables are discrete, an OLS regression with a binary outcome variable is equivalent to a non-parametric comparison of mean outcomes in each cell defined by the full set of control variables.[24]

Table 4 presents regression estimates of the effect of injury on employment rates from a variety of specifications. In columns 1 and 2, we examine employment rates 6 months after the ACC spell ended and include no variables besides our proxy measure of injury severity (i.e. duration of the first ACC spell) in the regression (remember, this variable is defined for the controls via their match to the injured population). In column 1, we use the individual match control group, while in column 2 we use the firm match control group. The results in column 1 are essentially equivalent to those in figure 4. In columns 3 and 4, we repeat the regressions in columns 1 and 2 adding a comprehensive set of covariates to the model to allow for correlation between employment rates and exposure to injury.[25]

Injuries that result in 1–2 months of earnings compensation appears to have a small negative effect on employment 6 months following the spell: the employment rate is 1–2% lower for injured than the non-injured group. Longer-duration injuries are found to have larger effects on employment, with a 4-month ACC spell reducing the likelihood of being employed by 3–4%, a 6-month spell reducing it by 6–7%, 8–9 month spells reducing it by 10–11% and 13–24 month spells reducing it by 15–17%. The choice of control group has no systematic effect on the results and there is little overall

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[22] We considered using log income, but as no standard method exists for converting zero incomes to logs, we decided to stay with a linear measure. We do discuss the magnitude of the effects on income levels relative to the income levels in the non-injured population.

[23] As in the graphical analysis, we censor an individual's post first ACC spell period when they have a subsequent ACC spell. We have also re-estimated these regressions excluding individuals with multiple ACC spells, and this has no qualitative effect on the results. We drop individuals with a spell length greater than 24 months because the small sample size of this group leads to imprecise estimates.

[24] It is important to note that because our sample covers the entire population, standard errors presented in this paper do not have their typical interpretation as a measure of sampling variation. It is more useful to interpret them in a Bayesian framework as representing the parameter variability if 'new' populations are examined.

[25] We control for age (5-year age groups), sex, region (12 regional councils and missing) in the reference month, the number of month in which an individual is employed during the 2-7 months prior to injury, their average earnings when working in these months (5 categories), the number of months in which an individual receives benefits during the 2-7 months prior to injury, their average benefit receipt when receiving benefits in these months (5 categories), the number of employees at their main place of employment (5 categories), the industry of this employer (11 industries and missing), and when during the sample period the injury occurred (12 quarters).

difference in the estimates.[26] Similarly, adding covariates to these models has little impact on the results.

These results are very similar to those found in the graphical analysis. One interpretation is that even injuries associated with short periods of earnings compensation result in worse outcomes for the injured population relative to the non-injured population. However, it is possible there are important unobserved characteristics, such as occupation and/or individual propensity to miss work because of minor injuries, that lead to persistent differences between the injured population and the matched control groups. This is consistent with the graphical evidence in figures 4 and 5 which shows that employment, benefit receipt and earnings prior to injury differ for the control groups relative to the injured population.

Heckman, et. al. (1998) and Smith and Todd (2005) demonstrate that this is a fairly common finding in the program evaluation literature. These papers suggest that comparing *changes* in outcomes between the pre-treatment and post-treatment periods in the treatment versus control group (where injury is the treatment), instead of the outcome differences in the post-treatment period, can greatly reduce this problem. They refer to this as the 'difference-in-differences' matching estimator, and demonstrate that this approach allows one to further control for unobserved differences between the injured and non-injured populations by differencing out individual fixed effects which are correlated with both the outcome (e.g. employment status) and the likelihood of being treated.

This procedure assumes that differences between the treatment and control group are time-invariant, thus, if the differences are actually time-varying, this approach will not completely control for unobserved heterogeneity between the groups. Although the differences in the pre-injury period apparent in figures 4 and 5 appear to increase as one looks further back in time from the injury month, it is not clear how much of this change is an artifact of the matching criteria that requires the same employment and earnings profiles in the months prior to injury. We believe that a longer time-series of data is needed to model whether the observed pre-injury differences are indeed time-varying.

[26] We further examine the importance of match criteria by re-estimating our main results using two tighter matching criteria. We use both a tighter version of our individual match, where earnings and benefit histories are more comprehensively matched, and of our firm match, where individual characteristics are used to match people within firms. These tighter match criteria result in large declines in sample support (67% for our individual match and 48% for our firm match), but have little qualitative effect on the results.

Given that we have at least 18 months of pre-injury data on all individuals, we choose to control for pre-treatment differences in outcomes at this point.[27]

Thus, in columns 5–8 we repeat the regressions in columns 1–4, but examine the effect injuries have on the likelihood of a *change* in employment status 6 months after leaving ACC compared with 18 months prior to being injured. Again, the choice of control group and the addition of covariates have no qualitative effects on the results. Examining the regressions that include covariates, we now find that injuries associated with 1–2 months of compensation have almost no impact on employment 6 month after compensation ends. The impact of longer-duration injuries are also dampened, but we still find strong evidence of large effects on employment, with a 4-month ACC spell reducing the likelihood of being employed by 2%, a 6-month spell by 4–6%, 8–9 month spells by 9–10% and 13–24 month spells by 12–15% compared to the change in employment experienced by the control group.

It is possible that longer-duration injuries merely have short-run impacts on employment. To examine this, we repeat the difference-in-differences with covariates regressions for the change in employment status 12 (in columns 9 and 10) and 18 (in columns 11 and 12) months after leaving ACC compared with 18 months prior to injury. We find no evidence that the negative effects of longer-duration injuries on employment decline in magnitude over time.[28]

Table 5 presents regression estimates of the effect of injury on changes in employment, benefit receipt, and total income 6 (columns 1–6) and 18 (columns 7–12) months after leaving ACC compared with 18 months prior to injury. All covariates are included in each of these regressions. For comparison, the results for employment rates in columns 1, 2, 7 and 8 are replicated from table 5. Turning to columns 3 and 4, consistent with our finding on employment, longer-duration injuries are found to increase the likelihood of receiving benefits 6 months after leaving ACC compared with 18 months prior to injury. For example, 4-month spells increase the likelihood by 1%, 6-month spells by 3–4%, and longer spells by 5%. These are large relative increases as the average rate of benefit

[27] Beyond this point, injuries that occur early in our sample period would be dropped from the analysis and we would be unable to examine post-injury outcomes for long-duration injuries. As the graphical analysis does provide some evidence that differences may increase as one looks further back in time, we have re-estimated our main results controlling for outcomes 36 months prior to injury. This did not impact qualitatively on our results for longer-duration injuries, but did provide some evidence that injuries shorter than 5 months in duration may have a more limited impact on outcomes.

[28] This contrasts with most job displacement studies which find that labour market impacts decrease significantly over time.

receipt is only 7% for the non-injured population. Overall, one-half to two-thirds of the decline in employment is accompanied by a move on to benefits. Examining columns 9 and 10, there is also no evidence that the impact of longer-duration injuries on benefit receipt decline over time.

Changes in total income measure the overall effect of injuries on individuals by incorporating three possible channels through which injuries affect individuals: a reduced likelihood of employment, a decline in earnings for individuals who remain employed (or less earnings growth than that experienced by the non-injured population), and an increased likelihood of receiving benefits (designed in part to offset the other two effects). Examining columns 5 and 6, we find that longer-duration ACC spells have a large negative effect on total income. Having an injury that results in 4 months of ACC compensation leads to a $115–$160 decline in total income, a 6-month spell a $195–$220 decline, an 8–9 month spell a $420–$455 decline, and a 13–24 month spell a $440–$625 decline 6 months post-injury compared with 18 months prior-injury. Again, we find no evidence that the impact of injuries on total income declines over time (columns 11 and 12).

Previous research has found that injuries have differential impacts on individuals with particular characteristics. For example, Boden and Galizzi (2003) find that women are less likely to return to work than men with similar duration injuries, while Biddle, et. al. (2003) find that older workers suffer larger wage losses following injury than younger workers. There are a variety of reasons why the impact of injury may vary for individuals with different characteristics; women and older workers may have higher reservation wages and thus their employment status may be more sensitive to wage loses, individuals employed in certain industries may have less flexible employers or find it more difficult to return to work after an injury, low wage workers or those with limited labour force attachment may be more vulnerable to jobs 'disappearing' while they are injured.

Table 6 presents regression estimates of the effect of injury on changes in employment 12 months after leaving ACC compared with 18 months prior to injury for different groups of individuals. We stratify our results by sex, age, industry, and prior earnings, benefit receipt, and employment (and in unreported results by firm size and prior benefit amount). Only the individual match control group is used in these regressions to insure that injured workers are matched to non-injured workers with the same stratification characteristics. Each row of coefficients represent the results from a separate

regression run for the identified group. To keep the results tractable, we group injuries into four duration lengths based on the results in table 4 and 5: 1–2, 3–4, 5–7, and 8–24 month spells. This allows us to examine whether injuries of different severities have differential impacts on certain groups of individuals.

We find no systematic differences in the impact of short-duration (1–2 and 3–4 month) injuries on different groups of workers. However, the impact of longer-duration injuries does appear to vary across groups. For example, having a 5–7 (8–24) month ACC spell reduces male employment by 4% (11%), but female employment by 7% (14%). Pronounced differences are also seen for individuals with different employment histories: for example, having a 5–7 (8–24) month ACC spell reduces employment by 5% (12%) for individuals who worked in 5–6 months, by 10% (16%) for individuals who worked in 3–4 months, and by 14% (33%) for individuals who worked in 1–2 months in the 6 months prior to injury.

For other groups, differences are only apparent for 8–24 month spells, but again are quite large. For example, having an 8–24 month ACC spell reduces employment of 15–29 year-olds by 10%, 30–49 year-olds by 12%, and 50–69 year-olds by 15%; of individuals in the highest quartile of earnings prior to injury by 10%, third quartile by 11%, second quartile by 12%, and lowest quartile by 18%; of individuals working in accommodations/restaurants, finance/business/property, health/community services, and other services by 14–20%, and individuals working in agriculture/fishing/forestry, manufacturing, transport/storage, and construction by 9–12%.[29]

Table 7 presents regression estimates of the effect of injury on changes in total income 12 months after leaving ACC compared with 18 months prior to injury for different groups of individuals. The structure of this table and of the estimated regression models is identical to that in table 6. Individuals with different characteristics have large differences in average incomes, thus, our measure of change in total income can be a misleading indicator of the relative impact that injuries have had on individuals. For this reason, we also present the average income of the non-injured population 12

[29] We find no systematic differences in the impact of longer duration injuries on individuals with different benefit receipt histories, average benefit receipt amounts (unreported), or whom are employed at different size firms (unreported).

months post-injury for each stratification group. The impact of injuries on changes in total income can be judged against these figures for different groups to assess their relative impact.

The results in this table are less consistent, but, in general, tell a similar story to those in table 6. Longer-duration injury spells are found to have larger impacts on total income for women, older workers, workers with lower earnings or with less consistent employment histories. For example, having an 8–24 month ACC spell reduces male total income by 14%, but female total income by 19%; of 15–29 year-olds by 22%, 30–49 year-olds by 11%, and 50–69 year-olds by 18%; of individuals in the highest quartile of earnings prior to injury by 9%, third quartile by 16%, second quartile by 25%, and lowest quartile by 15%; of individuals who worked in 5–6 months by 14%, individuals who worked in 3–4 months by 33%, and individuals who worked in 1–2 months in the 6 months prior to injury by 59%. Longer-duration injuries do not appear to have a differential impact on the total income of workers in different industries.

Discussion

We find that injuries that result in more than 3 months of earnings compensation have negative effects on future labour market outcomes and that for longer-duration injuries the magnitude of these effects are quite substantial. For example, individuals who receive 4 months compensation have 2% lower employment rates and $140–$200 lower monthly incomes 18 months after compensation ends, compared with 18 months prior to being injured, than comparable non-injured workers. Individuals who receive 10–12 months of compensation have a 10–15% decline in employment rate, 3–4% increase in benefit receipt rate, and $345–$540 decline in monthly income. Individuals who receive 13–24 months of compensation have a 16–17% decline in employment rate, 7–9% increase in benefit receipt rate, and $605–$610 decline in monthly income. These are large impacts relative to the average outcomes for non-injured workers; employment rates are 20% lower, benefit rates 90–120% higher, and total income 25% lower.[30]

[30] Reville (1999) and Reville and Schoeni (2001) examine the impact of workplace injuries that result in permanent partial disability on Californians using a similar methodology to that employed in this paper. They find that the earning of injured workers are 25% of the earnings of uninjured workers 4-5 years after injury and that a large share of this loss is due to a decline in employment among the injured workers. While it is not possible to directly compare these results to ours for New

We find no evidence that the magnitude of these impacts decline over time (at least in the first 18 months after leaving ACC) suggesting that injuries have long-run effects on individual labour market prospects. It is somewhat surprising that we do not observe any decline in impacts over time. As noted previously, there is suggestive evidence that differences between the injured population and the control group in the pre-injury period appear to increase as one looks further back in time from the injury month. If the differences are time-varying in this manner, and are not an artifact of the matching criteria that requires the same employment and earnings profiles in the months prior to injury, our estimates of the longer-term impacts of injuries will be overstated. A longer data window would enable us to examine observed differences many years prior to injury, and to better assess and deal with unobserved differences between the injured and non-injured populations. Despite this caveat, the estimated magnitude of the impact of longer duration injuries is substantially larger than the possible confounding effects, thus providing clear evidence that longer-duration injuries have substantial and long-term effects on individual's labour market outcomes.[31]

We also find evidence that longer-duration injuries have larger impacts on women, older workers, and workers with lower earnings or with less stable employment histories. Our findings for female and older workers match closely with those in the literature (Boden and Galizzi 2003; Biddle et. al. 2003). As discussed in those papers, these results may be indicative of discrimination against these workers or may occur because women and older workers have higher reservation wages and thus their employment status is more sensitive to wage losses.[32] Because we have limited information on worker and firm characteristics, and no information on injury characteristics in our data, it is not possible to examine this in greater detail. Individuals who are on the margins of the labour force appear to be quite vulnerable to outside shocks such as injury. We find by far the strongest impact of long-duration injuries on workers with the least attachment to the labour force prior to injury, with

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Zealand because of institutional differences, it is worth noting that our findings for longer-duration injuries tell a very similar story.

[31] It is worth noting that while individuals who have left ACC have been assessed by an independent medical assessor as being capable of working at least 35 hours per week, it is possible that they still suffer reduced productivity because of the initial injury. Unfortunately, without additional detailed information on the injury itself, especially whether it is an acute or chronic injury, it is not possible to judge through which pathways long-term effects on labour market outcomes are occurring.

[32] Age or sex discrimination might occur if employers consider injuries to be a signal about overall worker quality and are more sensitive to signals about groups of workers towards which they already have prejudicial beliefs.

their employment rates declining by 33% and their total income by 58% after injury (relative to a 12% decline in employment and 15% decline in total income for individuals with the most attachment to the labour force prior to injury).

Our results in table 5 and unreported results examining earnings conditional on employment suggest that most of the impact on total income is caused by the large reduction in the employment of injured workers. In other unreported results, we find that a smaller proportion of injured workers are employed with their pre-injury employer after the end of their injury spell than the proportion of the non-injured control group employed with the same employer after an identical amount of time.[33] Job change may help injured workers avoid some of the negative consequences of time away from work (for example, loss of firm specific human capital or missed promotion opportunities). We feel it is beyond this paper to examine these pathways in more detail, but getting a better understanding of how some workers avoid the negative impact of longer-duration injuries (or use an injury as a spur to achieve positive outcomes) is clearly a fruitful area for future research.

5. Conclusions

New Zealand has a comprehensive accident insurance system that covers both work and non-work injuries. Statistics New Zealand's Linked Employer-Employee Database contains monthly information on benefit receipt, earnings, and ACC earnings compensation for all New Zealanders over a five-year period. This institutional system and data source allow us to use a 'program evaluation' approach to examine the impact of injuries on employment, benefit receipt, and total income. Using time receiving earnings compensation as a proxy for injury severity, we compare the observed changes in outcomes for the injured population with a matched control group of non-injured workers.

We find that injuries that result in more than 3 months of earnings compensation have negative effects on future labour market outcomes and that for longer-duration injuries the magnitude of these effects are quite substantial. For example, individuals who receive 4 months compensation have 2% lower employment rates and 6–8% lower monthly incomes 18 months after compensation ends

[33] For example, among workers who have 10-12 month ACC spells, 41% are employed with the same employer pre- and immediately post-injury, 32% are with a different employer immediately post-injury, and 27% are not employed immediately post-injury. This compares to 70%, 20% and 10%, respectively, for the same point-in-time comparison in the control group.

compared with 18 months prior to being injured than comparable non-injured workers. The magnitude of these effects increase with injury duration; individuals who receive 10–12 months of compensation have 10–15% lower employment rates, 3–4% higher benefit receipt rates, and 14–22% lower monthly incomes.

We find no evidence that the magnitude of these impacts decline over the first 18 months after leaving ACC. This conclusion is subject to the caveat that the estimated impacts of longer term injuries may be overstated if the differences between the injured and control groups' outcomes vary with time from the injury event. Despite this, our results strongly suggest that injuries have long-run effects on individual's labour market outcomes that may even be permanent in nature.

We also find evidence that longer-duration injuries have larger impacts on women, older workers and workers with lower earnings or with less stable employment histories. The information available in LEED does not allow us to further evaluate the reasons for these differences. It would be a fruitful area for future research to use other data sources to better understand why injuries have greater impacts on workers with certain characteristics. This type of detailed analysis might also suggest potential policies for reducing the negative impacts of injuries for all workers.

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Figure 1

Employment, Benefit, and ACC Receipt Rates for the Injured Population

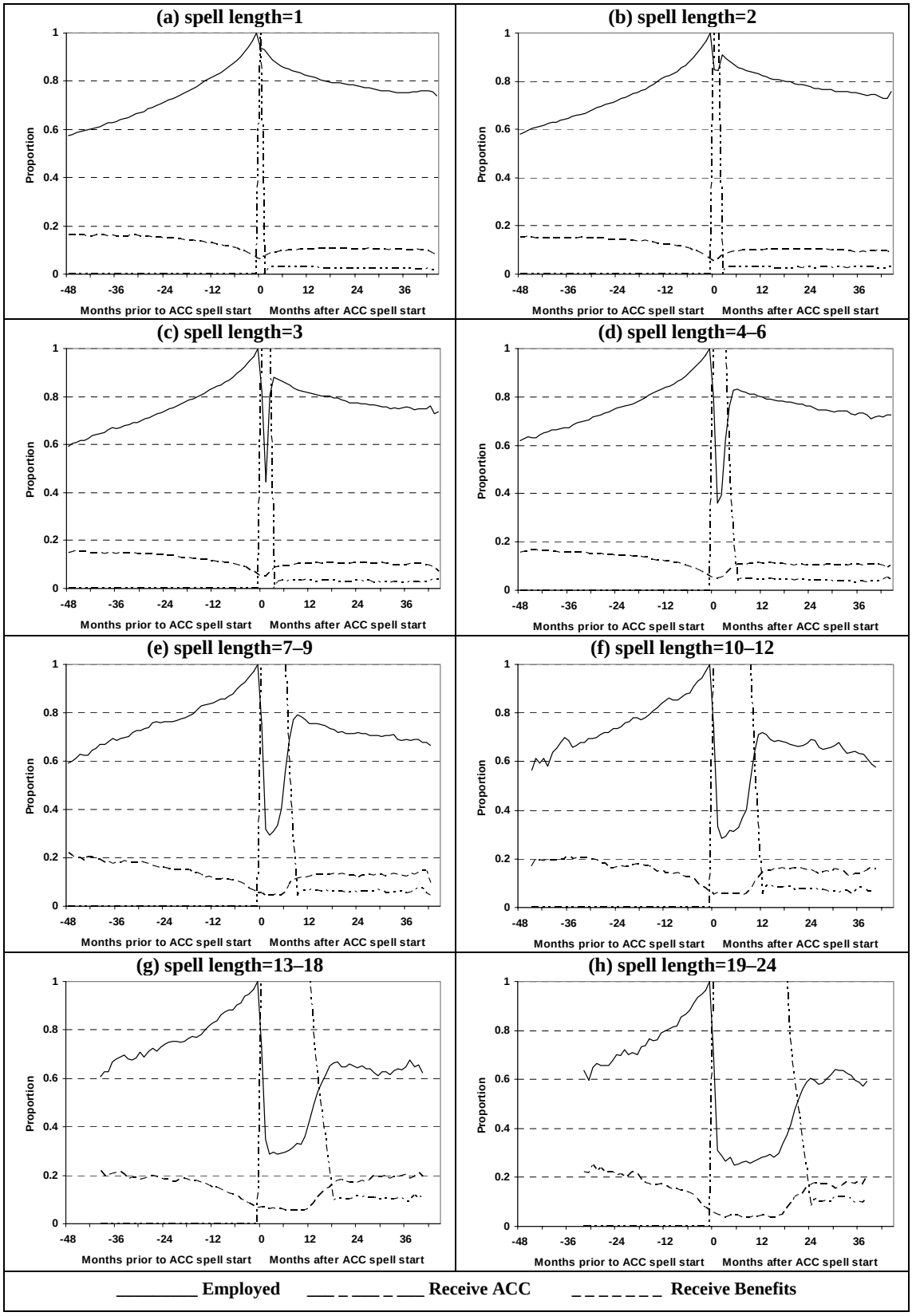


Figure 2

Earnings, Benefit, and ACC Receipt for the Injured Population

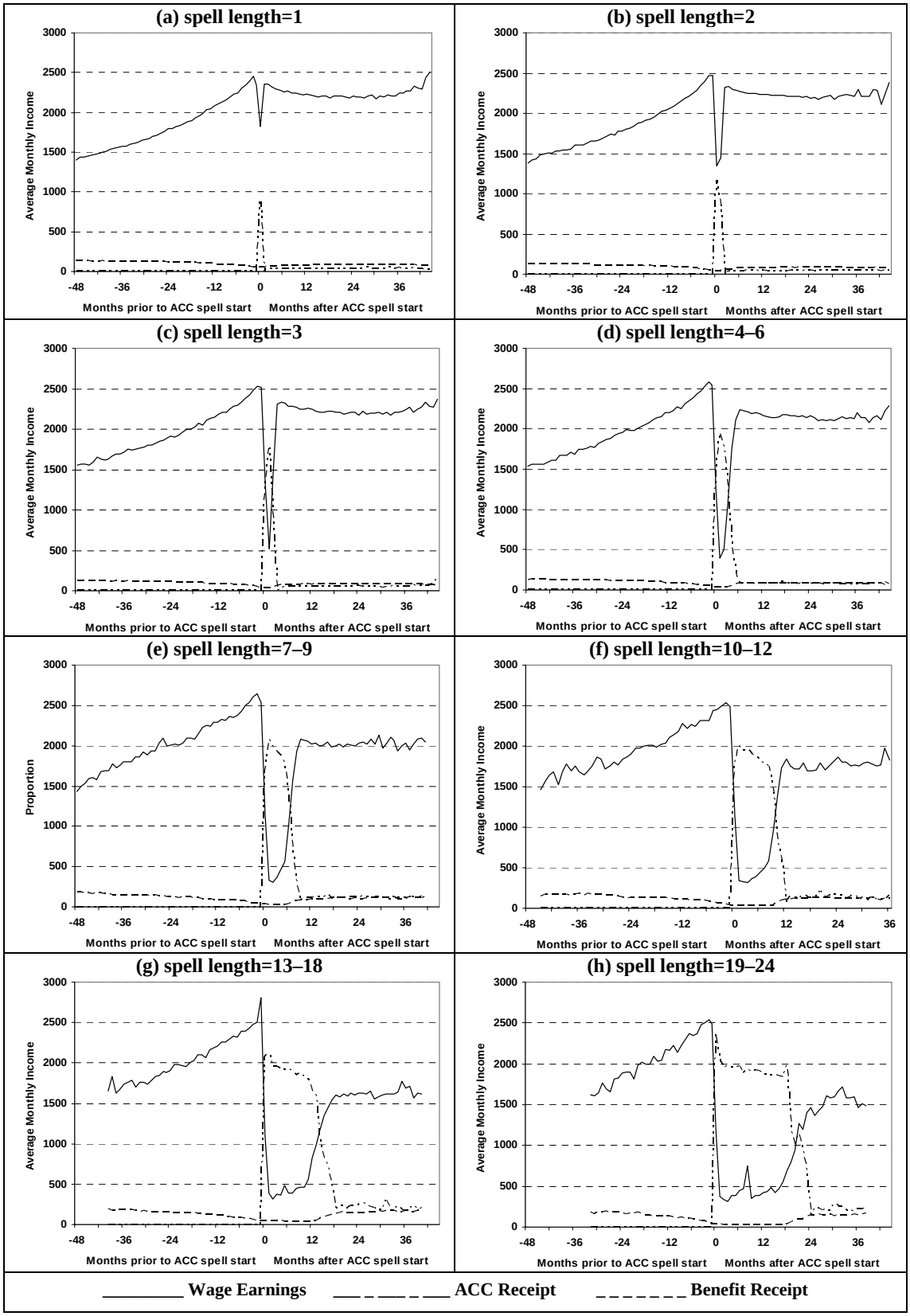


Figure 3

Hypothetical Earnings Loss of An Injured Worker

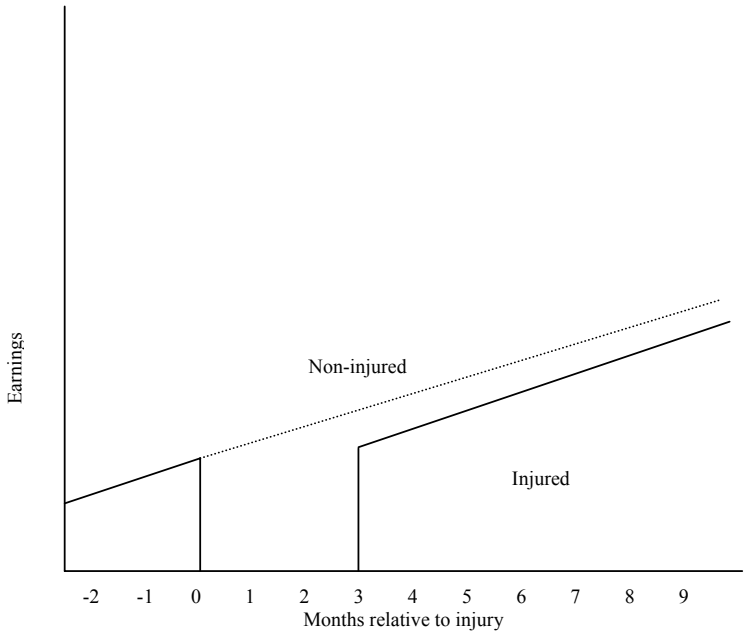


Figure 4

Matched Comparison of Employment and Benefit Receipt Rates for the Injured and Non-Injured Population by Length of First ACC Spell (Individual Match)

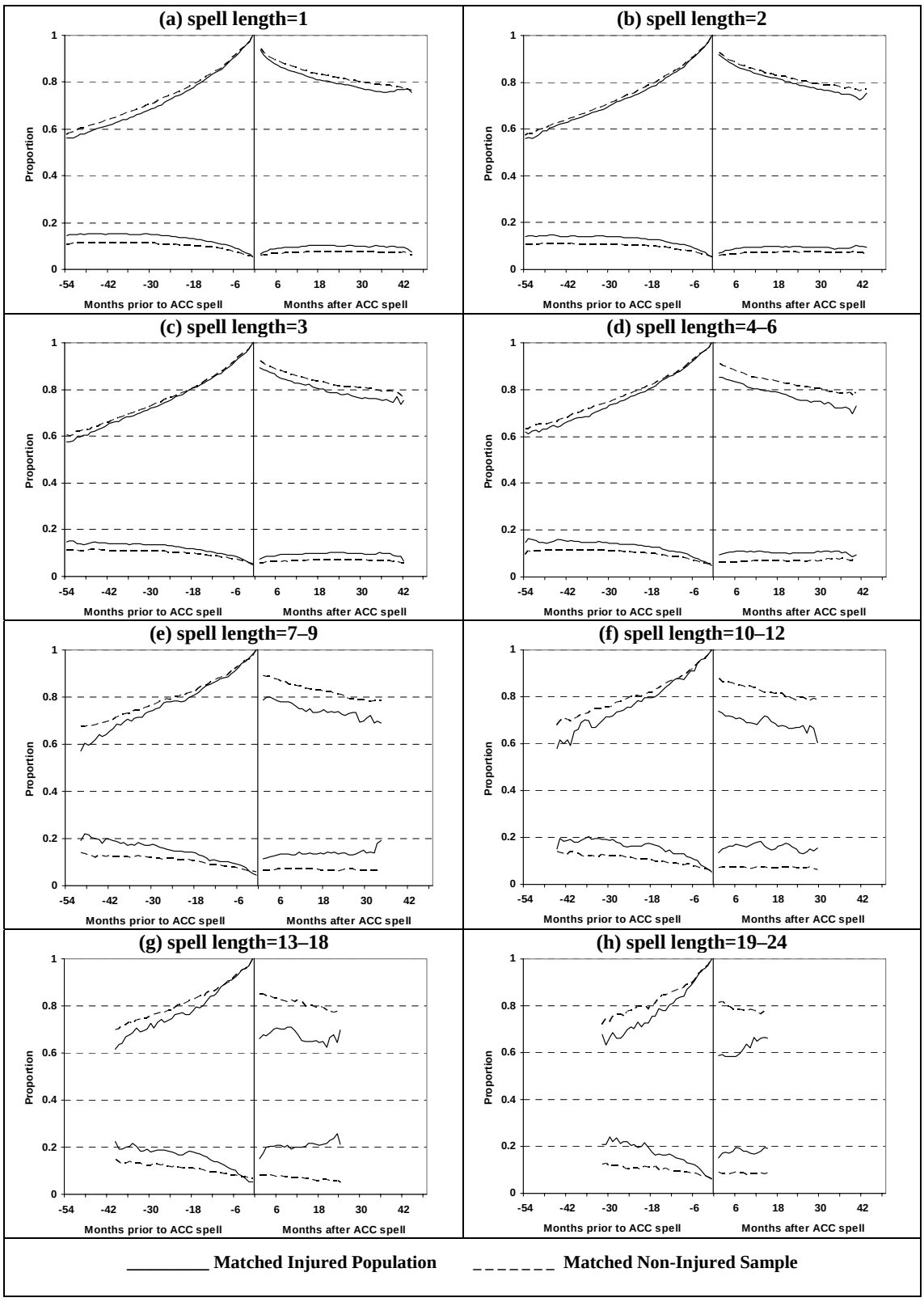


Figure 5

Matched Comparison of Wage Earnings and Benefit Receipt for the Injured and Non-Injured Population by Length of First ACC Spell (Individual Match)

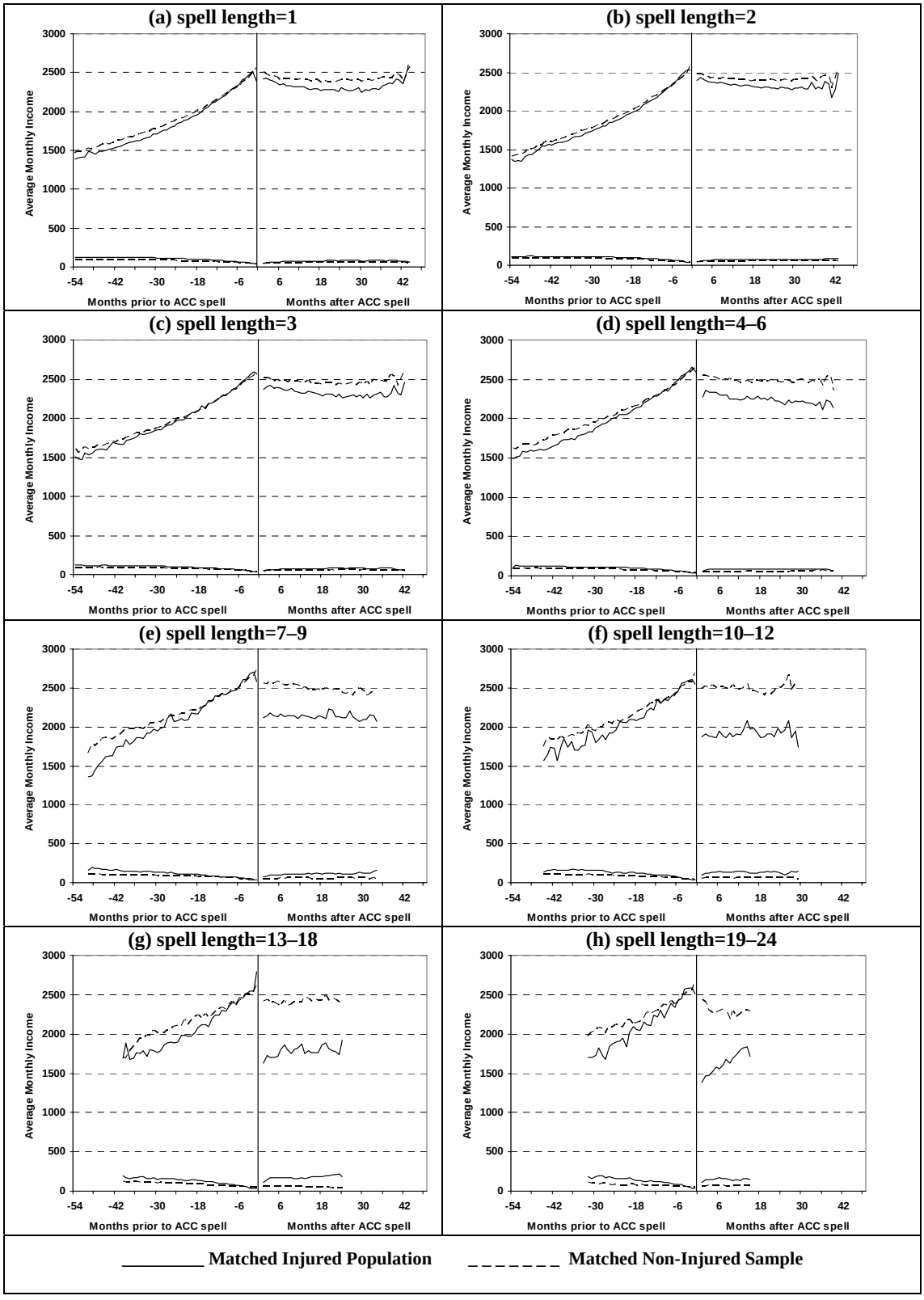


Table 1

Detailed Information on ACC Recipients for Different Injured Groups

	All Spells Starting After September 2000	Spells Finishing At Least 18 Months Before March 2004
	Percentage	
Number of Spells Receiving ACC Compensation		
1	84	77
2	13	18
3+	3	5
Number of Months Receiving ACC Compensation During First Spell		
1	44	50
2	25	27
3	10	11
4–6	9	9
7–9	2	2
10–12	1	1
13–24	1	1
25–41	0	0
Right Censored	8	0
Number of Spells Ending		
At Least 6 Months Before March 2004	81	100
At Least 12 Months Before March 2004	66	100
At Least 18 Months Before March 2004	53	100
Summary Statistics		
Number of Individuals	119,980	62,970

Notes: Spell lengths are based on uncensored spells only.

Table 2

Characteristics of the Injured and Non-Injured Population

Sample Characteristics	Non-Injured	Injured	Injured by Number of Months Receiving ACC Compensation						
			1	2	3	4–6	7–9	10–12	13–24
Mean									
Age	37.4	35.4	34.5	34.5	36.0	37.6	38.9	39.1	40.3
Employment and Benefit Status									
% Months Employed Prior to Injury	80	78	77	77	79	80	81	81	81
% Months on Benefit Prior to Injury	9	13	13	13	12	13	14	16	16
% Months Not Observed Prior to Injury	16	16	16	16	15	13	12	11	11
Income									
Earnings Prior to Injury	2,840	2,347	2,305	2,316	2,403	2,447	2,532	2,468	2,497
Benefit Income Prior to Injury	682	662	658	655	660	661	699	680	688
Total Income Prior to Injury	2,555	2,072	2,036	2,039	2,141	2,188	2,271	2,229	2,266
Percentage									
Sex									
Female	50	33	33	32	33	34	32	35	39
Region									
Northland	3	4	4	4	3	4	4	3	4
Auckland	32	23	23	23	23	23	23	21	24
Waikato	9	11	11	11	11	11	12	13	10
Bay of Plenty	6	7	7	7	7	7	8	7	9
Hawkes Bay / Gisborne	5	6	6	6	6	7	6	7	5
Taranaki	3	3	3	3	3	3	4	3	3
Manawatu / Wanganui	6	6	6	6	6	7	6	6	6
Wellington	12	9	9	9	8	9	9	11	10
West Coast / Tasman / Nelson / Marlb	4	5	5	5	5	5	5	6	5
Canterbury	13	16	16	16	16	15	14	14	13
Otago	5	6	6	6	6	6	6	6	6
Southland	3	3	3	4	4	3	4	2	3
Missing	1	0	0	0	0	0	0	0	1
Firm Size									
0–4 Employees	10	12	10	12	13	13	13	13	12
5–9 Employees	9	11	10	12	12	12	12	14	12
10–29 Employees	16	20	20	20	20	19	21	18	21
30–49 Employees	6	9	9	9	9	8	9	9	10
50–99 Employees	8	10	11	10	10	10	9	9	9
100–999 Employees	23	23	25	23	22	22	22	22	22
1000+ Employees	27	15	15	14	15	15	15	16	14
Industry									
Agriculture, Fishing, Forestry, Mining	5	10	10	10	11	11	11	12	8
Manufacturing	14	22	22	22	21	21	22	20	20
Transport, Storage, Com, Elect, Gas	6	7	7	7	7	7	8	6	9
Construction	5	11	11	11	11	11	12	11	11
Wholesale Trade	6	5	5	5	5	5	5	5	6
Retail Trade	13	12	13	13	12	11	12	10	11
Accommodation, Restaurants	5	5	6	5	5	4	5	5	6
Finance, Business, Property	16	10	10	9	9	10	7	9	9
Other Services	10	5	5	5	5	5	5	6	6
Education	4	1	1	1	1	2	1	1	1
Health, Community Services	10	10	10	9	10	11	10	12	12
Missing	6	2	2	2	2	2	2	3	2
Summary Statistics									
Percentage of Injured Population			48	27	11	10	2	1	1
Number of Individuals	582,510	119,980	52,640	29,590	12,340	10,730	2,620	1,130	1,290

Notes: Mean benefits and earnings are conditional on benefit receipt and employment. Age, region and employer characteristics are measured in the reference month. All values are in March quarter 2004 dollars.

Table 3

Characteristics of the Matched Injured Population

Sample Characteristics	Injured Population	Individual Match	Firm Match
	Percentage		
Duration of ACC Spell			
1 Month	48	48	48
2 Months	27	27	26
3 Months	11	11	11
4–6 Months	10	10	10
7–9 Months	2	2	2
10–12 Months	1	1	1
13–24 Months	1	1	1
Age			
Between 15 and 29	39	39	36
Age Between 30 and 39	24	25	25
Age Between 40 and 49	20	20	21
Age Between 50 and 59	13	13	14
Age Between 60 and 69	4	3	4
Sex			
Female	33	33	35
Region			
Northland / Auckland	27	28	29
Waikato / Bay of Plenty / Hawkes Bay / Gisborne	24	24	23
Taranaki / Manawatu / Wanganui / Wellington	18	18	18
West Coast / Tasman / Nelson / Marlborough / Canterbury	21	21	22
Otago / Southland	9	9	9
Firm Size			
0–4 Employees	11	11	3
5–9 Employees	11	11	8
10–29 Employees	20	20	19
30–99 Employees	19	18	22
100+ Employees	39	40	48
Industry			
Agriculture, Fishing, Forestry, Mining	10	10	7
Manufacturing	22	23	24
Transport, Storage, Communications, Electricity, Gas	7	7	7
Construction	11	11	9
Wholesale/Retail Trade, Accommodation, Restaurants	23	23	23
Finance, Business, Property	10	9	10
Education and Other Services	6	6	6
Health and Community Services	10	10	11
Prior Employment Status (excluding month before injury)			
Employed Zero Months Prior to Injury	1	1	1
Employed in 1–5 Months Prior to Injury	18	14	15
Employed in All Months Prior to Injury	81	85	84
Prior Benefit Receipt (excluding month before injury)			
No Benefits in 6 Months Prior to Injury	86	88	87
Received Benefits in 1–5 Month Prior to Injury	9	8	8
Received Benefits in All Months Prior to Injury	5	4	5
	Mean		
Prior Income (excluding month before injury)			
Earnings in 6 Months Prior to Injury	2,494	2,552	2,594
Benefit Income in 6 Months Prior to Injury	653	640	653
Summary Statistics			
Percentage of Injured Population		92	77
Number of Individuals	110,330	100,960	85,390

Notes: Mean benefits and earnings are conditional on benefit receipt and employment. Age, region and employer characteristics are measured in the reference month. All values are in March quarter 2004 dollars.

Table 4

Regression Estimates of the Effect of Injuries on Employment After First ACC Spell

	6 Months After		6 Months After		6 Months After Vs. 18 Months Before		6 Months After Vs. 18 Months Before		12 Months After Vs. 18 Months Before		18 Months After Vs. 18 Months Before	
	Indv Match No Cov	Firm Match No Cov	Indv Match Covariates	Firm Match Covariates	Indv Match No Cov	Firm Match No Cov	Indv Match Covariates	Firm Match Covariates	Indv Match Covariates	Firm Match Covariates	Indv Match Covariates	Firm Match Covariates
Effect of ACC Spell of:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1 Month Duration	-0.017	-0.013	-0.017	-0.012	-0.003	-0.002	-0.004	-0.006	-0.007	-0.008	-0.013	-0.012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
2 Month Duration	-0.011	-0.013	-0.010	-0.011	-0.001	-0.003	-0.002	-0.008	-0.007	-0.013	-0.008	-0.014
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
3 Month Duration	-0.028	-0.019	-0.028	-0.019	-0.030	-0.023	-0.030	-0.025	-0.033	-0.034	-0.034	-0.028
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
4 Month Duration	-0.036	-0.030	-0.036	-0.033	-0.023	-0.022	-0.022	-0.020	-0.026	-0.020	-0.016	-0.023
	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)
5 Month Duration	-0.060	-0.063	-0.060	-0.064	-0.041	-0.040	-0.042	-0.039	-0.038	-0.034	-0.042	-0.029
	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)	(0.009)	(0.010)	(0.011)
6 Month Duration	-0.069	-0.060	-0.067	-0.064	-0.062	-0.043	-0.062	-0.036	-0.072	-0.055	-0.095	-0.065
	(0.006)	(0.007)	(0.006)	(0.007)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.012)	(0.013)	(0.015)
7 Month Duration	-0.059	-0.073	-0.059	-0.075	-0.041	-0.061	-0.040	-0.058	-0.058	-0.070	-0.061	-0.065
	(0.008)	(0.009)	(0.007)	(0.009)	(0.011)	(0.013)	(0.011)	(0.012)	(0.013)	(0.015)	(0.016)	(0.019)
8–9 Month Duration	-0.109	-0.097	-0.109	-0.098	-0.102	-0.088	-0.103	-0.087	-0.096	-0.085	-0.060	-0.069
	(0.007)	(0.008)	(0.007)	(0.008)	(0.010)	(0.012)	(0.010)	(0.011)	(0.012)	(0.014)	(0.015)	(0.018)
10–12 Month Duration	-0.132	-0.119	-0.129	-0.117	-0.095	-0.089	-0.096	-0.090	-0.134	-0.127	-0.100	-0.145
	(0.008)	(0.010)	(0.008)	(0.009)	(0.012)	(0.014)	(0.012)	(0.013)	(0.014)	(0.016)	(0.018)	(0.021)
13–24 Month Duration	-0.149	-0.171	-0.147	-0.171	-0.113	-0.151	-0.116	-0.152	-0.135	-0.134	-0.162	-0.166
	(0.008)	(0.009)	(0.008)	(0.009)	(0.011)	(0.013)	(0.011)	(0.013)	(0.015)	(0.017)	(0.020)	(0.023)
Summary Statistics												
R-Squared	0.005	0.005	0.081	0.085	0.004	0.003	0.053	0.055	0.055	0.056	0.052	0.054
Observations	730,720	529,520	730,720	529,520	730,720	529,520	730,720	529,520	562,070	409,720	426,080	311,910

Notes: The displayed coefficients indicate the marginal effect of a particular duration ACC spell on the outcome in each panel. Standard errors are in parentheses.

Table 5

Regression Estimates of the Effect of Injuries on Outcomes After First ACC Spell

	6 Months After Versus 18 Months Before						18 Months After Versus 18 Months Before					
	Employment		Benefit Receipt		Total Income		Employment		Benefit Receipt		Total Income	
	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match
Effect of ACC Spell of:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1 Month Duration	-0.004	-0.006	-0.006	-0.001	-37.2	-35.0	-0.013	-0.012	0.002	0.003	-73.1	-69.1
	(0.002)	(0.002)	(0.001)	(0.001)	(6.6)	(7.6)	(0.002)	(0.002)	(0.001)	(0.002)	(9.2)	(10.7)
2 Month Duration	-0.002	-0.008	-0.001	0.001	-41.9	-39.5	-0.008	-0.014	0.001	0.002	-88.7	-89.1
	(0.002)	(0.002)	(0.001)	(0.002)	(8.8)	(10.4)	(0.003)	(0.003)	(0.002)	(0.002)	(12.6)	(14.8)
3 Month Duration	-0.030	-0.025	0.009	0.014	-71.8	-78.0	-0.034	-0.028	0.011	0.014	-161.6	-143.8
	(0.003)	(0.004)	(0.002)	(0.003)	(13.7)	(16.2)	(0.005)	(0.005)	(0.003)	(0.004)	(19.9)	(23.4)
4 Month Duration	-0.022	-0.020	0.008	0.009	-162.1	-116.4	-0.016	-0.023	-0.002	0.004	-206.3	-142.2
	(0.005)	(0.006)	(0.003)	(0.004)	(20.4)	(24.0)	(0.007)	(0.008)	(0.005)	(0.006)	(30.0)	(35.1)
5 Month Duration	-0.042	-0.039	0.031	0.024	-135.2	-148.6	-0.042	-0.029	0.036	0.022	-208.8	-210.3
	(0.007)	(0.008)	(0.005)	(0.006)	(28.1)	(32.7)	(0.010)	(0.011)	(0.007)	(0.008)	(41.8)	(48.2)
6 Month Duration	-0.062	-0.036	0.040	0.033	-222.4	-193.5	-0.095	-0.065	0.039	0.037	-278.0	-235.8
	(0.009)	(0.010)	(0.006)	(0.007)	(36.5)	(42.5)	(0.013)	(0.015)	(0.009)	(0.010)	(54.7)	(63.7)
7 Month Duration	-0.040	-0.058	0.007	0.015	-205.4	-212.5	-0.061	-0.065	0.018	0.010	-226.4	-248.4
	(0.011)	(0.012)	(0.008)	(0.009)	(45.7)	(53.6)	(0.016)	(0.019)	(0.011)	(0.013)	(69.3)	(81.4)
8–9 Month Duration	-0.103	-0.087	0.048	0.049	-455.1	-417.3	-0.060	-0.069	0.071	0.072	-219.4	-224.6
	(0.010)	(0.011)	(0.007)	(0.008)	(42.2)	(48.9)	(0.015)	(0.018)	(0.011)	(0.012)	(66.1)	(76.4)
10–12 Month Duration	-0.096	-0.090	0.022	0.053	-366.0	-383.5	-0.100	-0.145	0.035	0.034	-345.0	-539.1
	(0.012)	(0.013)	(0.008)	(0.010)	(48.8)	(57.6)	(0.018)	(0.021)	(0.012)	(0.015)	(76.6)	(89.5)
13–24 Month Duration	-0.116	-0.152	0.051	0.050	-441.8	-624.9	-0.162	-0.166	0.086	0.073	-604.4	-610.6
	(0.011)	(0.013)	(0.008)	(0.009)	(46.7)	(55.5)	(0.020)	(0.023)	(0.014)	(0.016)	(86.5)	(100.1)
Summary Statistics												
R-Squared	0.053	0.055	0.031	0.033	0.018	0.017	0.052	0.054	0.049	0.050	0.026	0.024
Observations	730,720	529,520	730,720	529,520	730,720	529,520	426,080	311,910	426,080	311,910	426,080	311,910

Notes: The displayed coefficients indicate the marginal effect of a particular duration ACC spell on the outcome in each panel. Standard errors are in parentheses. Total income is in March quarter 2004 values. All regressions include a full-set of covariates as described in the paper

Table 6

Regression Estimates of the Effect of Injuries on Employment 12 Months After Versus 18 Months Before First ACC Spell Stratified by Characteristics

Duration on ACC	1–2 Months	3–4 Months	5–7 Months	8–24 Months	R-Squared
	(1)	(2)	(3)	(4)	Observations
Overall	-0.007	-0.031	-0.052	-0.118	0.054
	(0.001)	(0.003)	(0.006)	(0.008)	562,070
Male	-0.006	-0.030	-0.044	-0.107	0.059
	(0.002)	(0.004)	(0.007)	(0.010)	380,180
Female	-0.009	-0.032	-0.068	-0.140	0.047
	(0.003)	(0.006)	(0.010)	(0.014)	181,890
Age 15–29	-0.002	-0.032	-0.056	-0.098	0.056
	(0.003)	(0.006)	(0.012)	(0.018)	217,530
Age 30–49	-0.010	-0.031	-0.052	-0.117	0.039
	(0.002)	(0.004)	(0.007)	(0.010)	259,680
Age 50–69	-0.011	-0.026	-0.048	-0.145	0.041
	(0.003)	(0.007)	(0.011)	(0.015)	84,860
Agriculture, et. al.	-0.004	-0.042	-0.034	-0.109	0.052
	(0.006)	(0.011)	(0.020)	(0.031)	48,780
Manufacturing	-0.005	-0.032	-0.063	-0.104	0.064
	(0.003)	(0.006)	(0.011)	(0.015)	139,580
Transport, et. al.	-0.016	-0.020	-0.087	-0.115	0.053
	(0.005)	(0.011)	(0.018)	(0.024)	35,660
Construction	0.004	-0.031	-0.016	-0.087	0.054
	(0.005)	(0.010)	(0.018)	(0.023)	61,350
Wholesale Trade	-0.016	-0.029	-0.041	-0.099	0.064
	(0.006)	(0.014)	(0.025)	(0.035)	28,720
Retail Trade	-0.003	-0.034	-0.004	-0.105	0.080
	(0.004)	(0.009)	(0.017)	(0.023)	74,440
Accommodation, Restaurants	-0.009	0.003	-0.097	-0.195	0.047
	(0.008)	(0.019)	(0.034)	(0.044)	26,950
Finance, Business, Property	-0.009	-0.047	-0.066	-0.136	0.053
	(0.005)	(0.011)	(0.019)	(0.030)	53,820
Other Services	-0.018	-0.052	-0.183	-0.158	0.058
	(0.008)	(0.017)	(0.030)	(0.039)	20,170
Education	-0.025	-0.047	-0.108	-0.112	0.065
	(0.014)	(0.030)	(0.049)	(0.096)	5,920
Health, Community Services	-0.007	0.007	-0.026	-0.155	0.043
	(0.004)	(0.009)	(0.016)	(0.020)	59,780
6mth Prior Earnings < 1750	-0.006	-0.023	-0.067	-0.178	0.049
	(0.004)	(0.009)	(0.015)	(0.022)	127,570
6mth Prior Earnings 1750–2500	-0.004	-0.016	-0.056	-0.118	0.037
	(0.003)	(0.007)	(0.012)	(0.016)	140,520
6mth Prior Earnings 2500–3250	-0.005	-0.044	-0.026	-0.106	0.031
	(0.003)	(0.006)	(0.010)	(0.013)	140,850
6mth Prior Earnings >= 3250	-0.013	-0.039	-0.060	-0.095	0.021
	(0.002)	(0.005)	(0.008)	(0.011)	148,490
No Benefits in 6mth Prior	-0.007	-0.027	-0.052	-0.121	0.056
	(0.001)	(0.003)	(0.006)	(0.008)	514,160
Benefits in 6mth Prior	-0.005	-0.066	-0.055	-0.106	0.025
	(0.006)	(0.015)	(0.026)	(0.034)	47,900
Employed in 1–2 of Prior 6mth	-0.017	-0.045	-0.139	-0.328	0.044
	(0.016)	(0.042)	(0.073)	(0.116)	7,210
Employed in 3–4 of Prior 6mth	-0.018	-0.010	-0.098	-0.163	0.028
	(0.010)	(0.023)	(0.038)	(0.053)	22,240
Employed in 5–6 of Prior 6mth	-0.006	-0.032	-0.047	-0.118	0.034
	(0.001)	(0.003)	(0.006)	(0.008)	527,980

Notes: The displayed coefficients indicate the marginal effect of a particular duration ACC spell on the outcome in each panel. Standard errors are in parentheses. Each row of coefficients represent the results from a separate regression run for the identified group. All regressions include a full-set of covariates as described in the paper.

Table 7

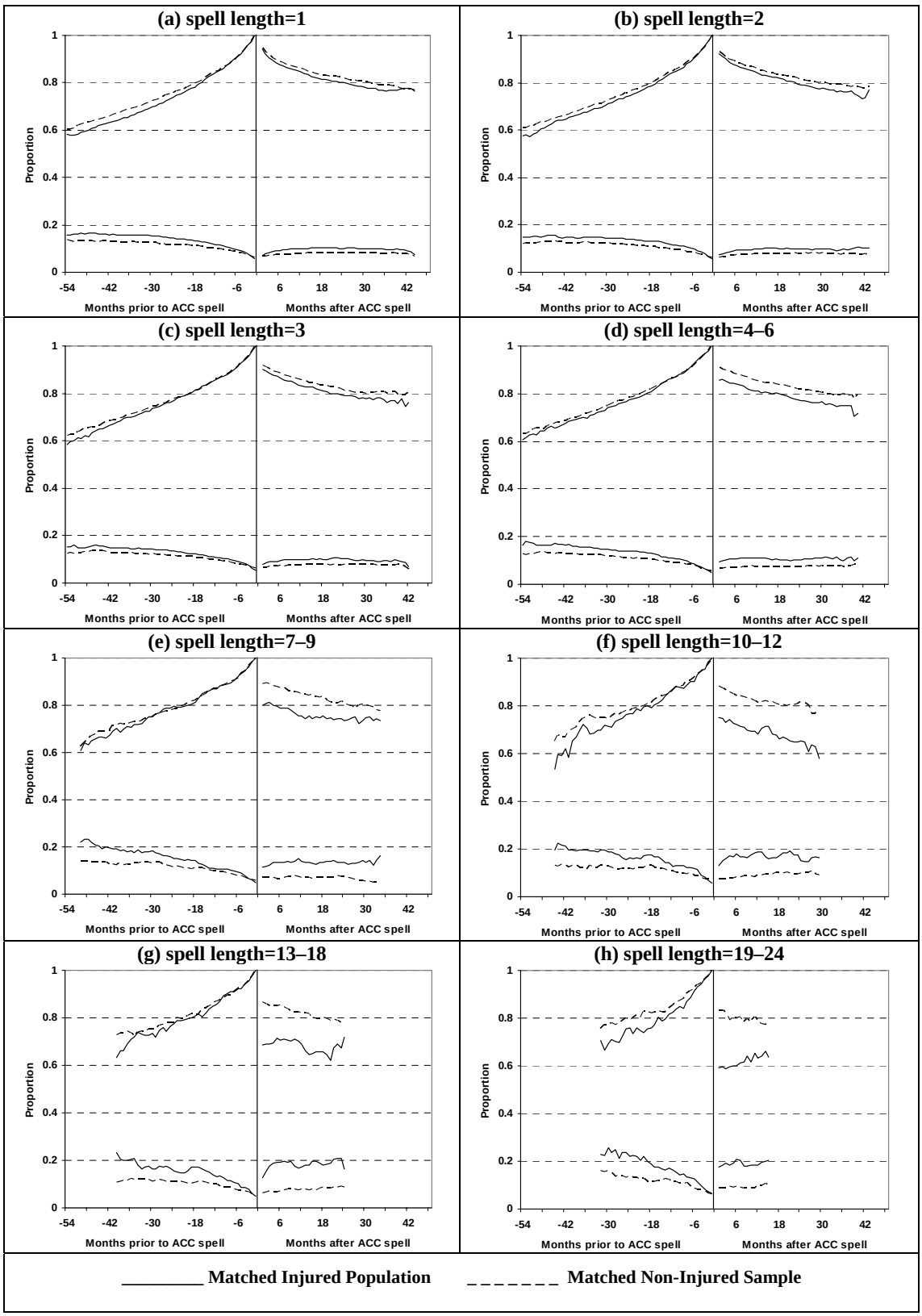
Regression Estimates of the Effect of Injuries on Total Income 12 Months After Versus 18 Months Before First ACC Spell Stratified by Characteristics

Duration on ACC	Avg. Income	1–2 Months	3–4 Months	5–7 Months	8–24 Months	R-Squared
	(1)	(2)	(3)	(4)	(5)	Observations
Overall	2,480	-62.1	-163.8	-204.7	-381.7	0.023
		(6.4)	(14.0)	(24.9)	(34.1)	562,070
Male	2,719	-79.8	-198.1	-175.3	-380.8	0.022
		(8.5)	(18.6)	(33.2)	(45.8)	380,180
Female	1,994	-25.1	-93.9	-262.6	-381.1	0.024
		(8.8)	(19.3)	(33.8)	(45.8)	181,890
Age 15–29	2,012	-35.4	-138.8	-206.5	-441.2	0.027
		(7.8)	(18.4)	(36.3)	(54.3)	217,530
Age 30–49	2,845	-94.9	-200.4	-239.1	-327.2	0.009
		(10.5)	(22.4)	(38.5)	(50.5)	259,680
Age 50–69	2,598	-40.0	-121.9	-127.8	-458.0	0.013
		(19.7)	(39.0)	(63.3)	(87.0)	84,860
Agriculture, et. al.	2,100	-13.4	-89.9	-38.1	-366.5	0.029
		(20.2)	(41.8)	(72.6)	(113.4)	48,780
Manufacturing	2,760	-64.6	-149.1	-213.2	-406.8	0.031
		(12.2)	(27.1)	(47.9)	(65.2)	139,580
Transport, et. al.	3,236	-42.5	-298.4	-385.0	-498.4	0.019
		(33.1)	(73.2)	(118.3)	(158.7)	35,660
Construction	2,713	-47.0	-195.9	-84.8	-259.7	0.025
		(22.9)	(48.3)	(88.0)	(112.3)	61,350
Wholesale Trade	2,837	-113.4	-174.1	-430.0	-608.7	0.029
		(31.6)	(71.4)	(132.7)	(183.3)	28,720
Retail Trade	1,988	-19.3	-84.1	-123.9	-538.6	0.039
		(12.6)	(28.3)	(52.5)	(71.7)	74,440
Accommodation, Restaurants	1,578	-14.5	-7.2	-70.7	-152.2	0.041
		(18.2)	(44.6)	(80.4)	(104.1)	26,940
Finance, Business, Property	2,763	-185.0	-337.4	-343.4	19.7	0.020
		(27.4)	(61.3)	(108.9)	(167.7)	53,820
Other Services	2,429	-135.5	-274.6	-407.1	-550.5	0.032
		(31.2)	(66.3)	(117.4)	(156.9)	20,170
Education	2,389	-95.2	-230.1	-1039.9	-189.3	0.071
		(52.8)	(109.9)	(181.6)	(356.2)	5,920
Health, Community Services	2,237	-26.4	-81.8	-136.9	-436.6	0.025
		(16.3)	(33.8)	(61.9)	(77.0)	59,780
6mth Prior Earnings < 1750	1,355	5.1	-49.5	-111.5	-207.7	0.066
		(7.9)	(18.2)	(32.9)	(46.3)	127,570
6mth Prior Earnings 1750–2500	2,092	-47.1	-114.2	-187.8	-529.3	0.038
		(10.3)	(22.9)	(40.4)	(54.3)	140,520
6mth Prior Earnings 2500–3250	2,738	-46.5	-194.2	-149.2	-436.7	0.026
		(11.8)	(25.2)	(43.8)	(58.6)	140,840
6mth Prior Earnings >= 3250	4,014	-181.0	-287.7	-364.4	-343.6	0.012
		(19.0)	(39.4)	(70.0)	(97.8)	148,490
No Benefits in 6mth Prior	2,565	-67.9	-171.4	-214.3	-414.8	0.022
		(6.9)	(15.0)	(26.8)	(36.9)	514,160
Benefits in 6mth Prior	1,780	-19.1	-121.0	-130.6	-177.2	0.036
		(15.8)	(36.1)	(62.7)	(82.7)	47,900
Employed in 1–2 of Prior 6mth	1,427	-68.5	-222.0	-158.8	-846.0	0.053
		(40.6)	(104.3)	(181.4)	(287.0)	7,200
Employed in 3–4 of Prior 6mth	1,657	-100.8	-80.3	-11.0	-550.2	0.024
		(33.0)	(78.1)	(129.1)	(178.7)	22,240
Employed in 5–6 of Prior 6mth	2,580	-60.9	-166.1	-217.6	-373.0	0.021
		(6.7)	(14.4)	(25.7)	(35.1)	527,980

Notes: The displayed coefficients indicate the marginal effect of a particular duration ACC spell on the outcome in each panel. Standard errors are in parentheses. Total income is in March quarter 2004 values. The first column displays the average income for the non-injured population 12 months after the reference month. Each row of coefficients represent the results from a separate regression run for the identified group. All regressions include a full-set of covariates as described in the paper.

Appendix Figure 1

Matched Comparison of Employment and Benefit Receipt Rates for the Injured and Non-Injured Population by Length of First ACC Spell (Firm Match)



Appendix Figure 2

Matched Comparison of Wage Earnings and Benefit Receipt for the Injured and Non-Injured Population by Length of First ACC Spell (Firm Match)