Statistics New Zealand
TATAURANGA AOTEAROA

Returning to Work from Injury: Longitudinal Evidence on Employment and Earnings (Update)

Sarah Crichton
Statistics New Zealand and
Department of Labour

Steven Stillman
Statistics New Zealand and
Motu Economic and Public Policy Research

Dean Hyslop
Statistics New Zealand and
The Treasury

July 2005

Acknowledgements

This work was undertaken while the authors were on secondment to Statistics New Zealand. We would like to thank Nairn MacGibbon, Andrew Hunter, Tas Papadopoulos, Neil Kelly, Christine Bycroft, and Philippa Graham for their valuable contribution. Any remaining errors remain the responsibility of the authors. Any views expressed are those of the authors and do not purport to represent those of Statistics New Zealand, the Department of Labour, the Treasury, or Motu Economic and Public Policy Research.

The tables in this paper contain information about groups of people so that the confidentiality of individuals is protected. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person or firm. The results are based in part on tax data supplied by Inland Revenue to Statistics New Zealand under the Tax Administration Act. This tax data must be used only for statistical purposes and no individual information is provided back to Inland Revenue for administrative or regulatory purposes. Careful consideration has been given to the privacy, security and confidentiality issues associated with using tax data in this project. A full discussion can be found in the *LEED Project Privacy Impact Assessment* paper available on the Statistics New Zealand website (www.stats.govt.nz).

Inland Revenue collects this data to support the efficient operation of the New Zealand taxation system, and its use as a base for the production of statistics places new and quite different demands on the data. Any discussion of data limitations or weaknesses is in the context of this latter use, and is not related to the ability of the data to support Inland Revenue's core operational requirements.

Abstract

New Zealand has a comprehensive accident insurance system that pays much of the direct cost of accidental injuries, and compensates workers 80 percent of their earnings for any time post-injury that they are unable to work. Statistics New Zealand's Linked Employer-Employee Database (LEED) contains monthly information on benefit receipts, earnings, and accident earnings-related compensation for all New Zealanders over a five-year period. 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 a matched 'control' group of non-injured individuals who have similar observed characteristics. We allow the magnitude of these effects to differ by key characteristics, including age, sex, industry, and prior earnings.

We find that injuries that result in more than two months of earnings-related compensation have negative effects on future labour market outcomes. For example, individuals who receive four months compensation have 2 percent lower employment rates and $140–$200 lower monthly incomes 18 months after compensation ends, compared with a matched sample of comparable non-injured workers. The magnitude of these effects increases with injury duration; individuals who receive 10–12 months of compensation have 10–15 percent lower employment rates, 3–4 percent higher benefit receipt rates, and $345–$540 lower monthly incomes. We also find evidence that longer-duration injuries have a greater impact on women, older workers, and workers with lower earnings or with less stable employment histories.

Keywords: Injury, Program Evaluation, Matching, Disability, New Zealand

1. Introduction

This paper examines the impact of injuries on labour market outcomes in New Zealand. In contrast to most developed countries, which have accident insurance systems that cover only workplace injuries, New Zealand has a comprehensive state-run accident insurance system that pays much of the direct cost of all accidental injuries and compensates workers 80 percent of their earnings for any time post-injury that they are unable to work. To the best of our knowledge, it is the only accident insurance system that does not differentiate based on where injuries occur. The system is administered by the Accident Compensation Corporation (ACC) and costs approximately 1.5 percent of GDP per annum.

Injuries can have large long-term effects on individuals and the community.[1] In addition, they can have substantial direct and indirect economic costs.[2] The analysis of the impact of injuries on individuals' labour market outcomes is generally not possible using survey data.[3] Statistics New Zealand's Linked Employer-Employee Database (LEED) contains monthly information on benefit receipts, employment earnings, and ACC earnings-related compensation for all New Zealanders over a five-year period from April 1999 to March 2004. It enables individuals to be followed longitudinally and to match them to their employers. Although LEED does not directly measure injuries, we can use receipt of ACC earnings-related compensation to proxy for injury, with the payment spell length a proxy for severity.

Our analysis begins with a description of the labour market outcomes for injured workers. We also compare the characteristics of these workers to those of a random sample of non-injured workers. As anticipated, injured workers are not representative of the working population, with evidence of differences across several dimensions: demographic, geographic location, and firm and industry characteristics. Taking inspiration from the programme evaluation literature, we use a matching methodology approach to construct 'control' groups of non-injured workers who have similar pre-injury observable characteristics as the injured population (Rubin 1979; Lalonde 1986).

---

1 Weil (1999) summarises the findings from a number of studies examining the long-term 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 percent in the five years following an injury.

2 Leigh, *et al* (1997) estimate that the total economic costs of occupational illnesses and injuries were approximately 3 percent of US GDP in 1992.

3 A number of factors affect the suitability of surveys for this task. First, general social surveys do not ask about injuries and many do not even ask about disabilities. Second, while surveys exist that do ask detailed questions about injuries, these typically only collect limited labour market information. Third, no standard survey instrument exists for measuring injury or disability and therefore these questions, even when asked, rarely measure something that is likely to be consistently reported across the population and over time (Weil 1999). Fourth, only a small percentage of individuals suffer relatively serious injuries, therefore sample populations are quite small in datasets that also have suitable labour market information.

We exploit three features of the LEED data for this purpose. First, LEED's comprehensive coverage allows us to use a non-parametric matching method, and to create control groups that share the same local labour market and are observed in the same time period as the injured population.[4] Second, LEED allows us to match workers within a firm, and therefore control 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 2004).

Past research on the impact of injuries on labour market outcomes has relied on administrative data from workers' compensation systems and has therefore 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). In addition, most studies have relied solely on data from the injured population to estimate the impact of injuries on labour market outcomes, either by comparing pre- and post-injury outcomes for the injured population (Berkowitz and Burton 1987), or by comparing workers with more- and less-serious injuries (Biddle 1998; Boden and Galizzi 1999; 2003). As discussed later in the paper, these approaches are likely to lead to biased estimates. Our analysis provides a twofold contribution to this existing literature. First, the institutional setting in New Zealand allows us to examine the impact of injuries regardless of where they occur. As a majority of injuries occur outside the workplace, this is a useful contribution.[5] Second, the comprehensive coverage of workers in LEED allows us to estimate the impact of injury by comparing outcomes for the injured with those of similar non-injured workers. Reville (1999) and Reville and Schoeni (2001) use a similar approach to the one taken in this paper, but are only able to examine workplace injuries.

Comparing changes in the observed outcomes for the injured population with changes in the control group of non-injured workers over the same time period, we find that injuries resulting in more than two months of earnings compensation have negative effects on future labour market outcomes. For example, individuals who are injured for four months have 2 percent lower employment rates, and $140–$200 lower monthly incomes, 18 months after compensation ends than do comparable non-injured workers. The magnitude of these effects increases with injury duration: individuals who have 10–12 month injury spells have 10–15 percent lower employment rates, 3–4 percent higher benefit receipt rates, and $345–$540 lower monthly incomes. We also find evidence that longer-duration injuries have a greater impact on women, older workers, and workers with lower earnings or with less stable employment histories.

---

4 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.
5 In New Zealand, 40 percent of claims involving earnings compensation stem from workplace injuries. Unfortunately the data does not allow us to distinguish work and non-work related injuries.

2. Background

The New Zealand accident insurance system

The New Zealand accident insurance system provides cover for all citizens, residents and temporary visitors.[6] 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.[7] ACC pays much of the direct medical and rehabilitative cost of injuries, and compensates workers 80 percent of their total earnings for any time post-injury that they are unable to work.[8] 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 per annum. 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, and 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 that lead to employment loss or earnings reductions for an individual; 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'.

---

6 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, the state-run system was reinstated. Crichton, *et al* (2004) discuss the accident insurance system in greater detail.

7 In a number of situations, such as intentional self-injury or injury suffered during criminal conduct, claimants may not receive certain entitlements. In other cases, ACC may pay the treatment costs but not other entitlement payments that may otherwise have been appropriate.

8 Compensation paid to wage and salary earners is capped (adjusted annually for inflation) – the maximum weekly amount paid in 2002/03 was $1,365 before tax – affecting approximately 5 percent of earnings compensation claims. Some employers elect to top-up an individual's compensation above the 80 percent 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 second to fifth weeks off work, the amount of compensation paid is based on the individual's average earnings in the four weeks prior to injury. Following the fifth week, compensation is based on the individual's average earnings in the 52 weeks prior to injury.

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 firms 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, firms may be more willing to accommodate injured workers, and injured workers who wish to change employers may find it easier to find a job with pay prospects equal to their old job.

3. Data Characteristics

The Linked Employer-Employee Database

This paper uses an experimental dataset under development at Statistics New Zealand, called the Linked Employer-Employee Database (LEED), which is based on monthly administrative data collected by Inland Revenue.[9] All employers in New Zealand are required to file an Employer Monthly Schedule (EMS) with Inland Revenue that lists all individuals employed at that firm in the month, the amount of income they received, and the amount of tax that was deducted at source.[10] Individuals and firms each have unique administrative identification numbers that can be used to track them longitudinally. In addition, two important ‘employee-employer’ relationships are identified in the database: receipt of working-age taxable benefits and receipt of ACC earnings related compensation.[11] LEED currently contains 60 months of linked employer-employee records, from April 1999 to March 2004.

9 Kelly (2003), and Carroll and Wood (2003) provide a detailed discussion of the LEED project and database.

10 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’ (PAYE) 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.

11 Welfare benefits and ACC compensation are taxed at source and thus reported to Inland Revenue. 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 individuals are not identified in the LEED dataset. ACC figures indicate that approximately 4 percent of all claims where earnings compensation is paid are associated with individuals who are employed by firms participating in the reimbursement programme.

Inland Revenue'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.[12] 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, which is likely to be a key characteristic in explaining an individual's exposure to workplace injuries and likelihood of risk-taking behaviors away from work. Information is not currently available on earnings from self-employment.[13]

The injured population

We classify individuals as 'on ACC' in any month they receive income from ACC. A series of consecutive months of receiving ACC income is referred to as an 'ACC spell'.[14] 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 select all individuals whose first observed ACC spell started during or after October 2000, and who were employees (this restriction drops the self-employed injured) and aged 15–69 years 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 a substantial 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 years worked in at least one month between October 2000 and March 2004. Of these, 120,000 started an ACC spell, and 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 percent had a single ACC spell, 13 percent had two ACC spells, and the remaining 3 percent had more than two spells over the period October 2000 to March 2004. Our analyses concentrate on the effects of the first ACC spell experienced by an individual during the study period.

---

12 Sex is actually derived from the title and names provided on the initial registration form. Industry information for firms is available via the Statistics New Zealand Business Frame/EMS link. Documentation available on the Statistics New Zealand website discusses the creation of these variables in more detail.

13 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.

14 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.[15] ACC spells are typically short: 68 percent of first spells last 1–2 months, 5 percent last longer than six months, and 8 percent of spells are right censored (and thus excluded from our analyses). Our main regression analysis examines outcomes six, 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 six-, 12-, and 18-month outcomes can be examined. Eighty-one percent of the population is observed at six months, 66 percent at 12 months, and 53 percent of the population is observed 18 months after the end of the first ACC spell.

Table 2 presents the characteristics of the injured and non-injured population in the month prior to injury. The 'non-injured' population is a 1 percent random sample of never-injured individuals in each month after September 2000, who were employed and aged 15–69 years (ie the same restrictions as in our injured population). The characteristics examined include: age, sex, region, employment and benefit status, average earnings and income in prior months, and the firm size and industry of an individual's main employer.[16] 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. Employment status, earnings and benefit receipts are measured in the six months prior to the month of injury or selection.

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 two years younger and more likely to be male (67 percent of the injured population is male compared with 50 percent 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. Those in the injured population are also much less likely to work at very large firms, or to be employed in finance/business/property services, other services, or education.17 As anticipated, industry is strongly correlated with the likelihood of being injured. Regional variation is likely to be 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.

---

15 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. Consequently, the duration of the ACC spell is an imperfect measure of the actual duration of the injury.

16 An individual's main employer 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 from any employer (besides ACC earnings compensation or welfare benefits) 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 refers to the total amount of income received from employers, welfare benefits, and ACC earnings compensation in a particular month.

17 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 of these two samples. Therefore 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.

Columns 3–9 present the results for the injured population stratified by the length of their first ACC spell. We examine spells shorter than four 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 with a first spell of more than 24 months in our sample, 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 a more difficult time recovering from injury. Individuals with longer-duration spells are also more likely to be female and to have higher average employment earnings in months prior to injury than are 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.

Labour market outcomes for the injured population

We now describe the various labour market outcomes for the injured population, in months relative to 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 average earnings, average benefit receipts, and average ACC compensation payments. The figures include the time during which an individual is injured and receiving ACC. These graphs are shown separately by the length of the first ACC spell. Note that, by construction, 100 percent 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 percent have a subsequent ACC spell, compared with 8 percent for those with a 7–9-month spell, and 11 percent 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 a new injury spell or a relapse associated with the original injury. However, as LEED does not distinguish between new injuries and recurrences 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 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 will see more of those individuals out of employment. Of those individuals with a first spell of one month, 82 percent and 71 percent, respectively, are employed 12 and 24 months prior to the start of the ACC spell. Of individuals with a 7–9-month first spell, employment rates prior to injury are slightly higher, with 84 percent and 76 percent employed in these months, respectively. For those with a first spell of one month, 81 percent are employed 12 months after the injury month, compared with 76 percent of individuals with a 7–9-month spell.

Most previous studies of the effect of injuries on labour market outcomes have compared changes in outcomes over time for injured workers either with expected changes, based on restricted regression models, or with the changes observed for workers with minor injuries (Berkowitz and Burton 1987; Biddle 1998; Boden and Galizzi 1999; 2003). The results in figures 2 and 3 can be used to make a similar comparison. For example, the results can be interpreted as showing either: i) injured workers who receive ACC for 7–9 months have an 8 percent lower employment rate 12 months after injury compared with 12 months before; or ii) injured workers who receive ACC for 7–9 months have a 7 percent greater drop in their employment rate from 12 months before injury to 12 months after, than do those receiving one month of ACC (an 8 percent versus 1 percent drop in employment).

However, both these interpretations are problematic. First, without knowing the decline in employment rates for similar non-injured workers during this time period, it is not possible to judge whether an 8 percent decline in the employment rate is a 'good' or 'bad' outcome. In an economy with significant job turnover, the employment rate for a fixed cohort of workers can fall 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 required to use them as a comparison group.

Calculating counterfactual outcomes for injured workers

Figure 3 displays the earnings of a hypothetical worker who experiences an injury at month 0 and remains on ACC for three 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, six 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 three months had they not missed work.

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. As illustrated in this example, an individual's pre-injury labour market outcomes are not suitable for measuring this because the worker would have been likely to experience 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 were 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 with the same 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 (ie 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 injured workers to all non-injured workers in the month prior to injury. This approach minimizes the calendar effects, as the same actual months are observed for both the injured and matched non-injured populations. Two match criteria are used in the analysis in this paper. The main distinction between these two matches is that the first uses primarily individual-level information for matching, while the second matches workers within the same firm. All matching is done with replacement, so a non-injured worker can be matched to more than one injured worker.

First, we construct an ‘individual’ match in which injured workers are matched to non-injured workers with the same: sex, age (+/– two years), location (12 regional council areas), number of months employed in the seven months prior to injury, the number of employees (seven standard groups) and industry (14 one-digit ANZSIC groups) of their main employer. The match includes being within 20 percent of the average earnings in the 2–7 months of employment prior to injury.[18] Second, we construct a ‘firm’ match in which injured workers are matched to non-injured workers with the same main employer, location, and within 20 percent of the average earnings in the 2–7 months of employment prior to injury.

In many cases more than one potential match is identified and we randomly select a maximum of 10 matches for each injured worker.[19] Overall, 72 percent of the injured population matches on both the firm and individual criteria, 19 percent match only on the individual criteria, 5 percent match only on the firm criteria and the remaining 4 percent do not match using either set of criteria. By using two quite different sets of match criteria, we aim to provide evidence of the robustness of our results to the choice of matching algorithm. Our ‘firm’ match is quite similar to the match criteria used in Reville (1999) and Reville and Schoeni (2001), allowing for some comparability of our results.

18 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.

19 Using the individual match, 8 percent of injured workers have no match, 6 percent have one match, 25 percent have 2–9 matches, and the remaining 61 percent have at least 10 matches. Of those with at least one match, the average number of matches obtained is 8.0. Using the firm match, 23 percent of injured workers have no match, 11 percent have only 1 match, 28 percent have 2–9 matches, and the remaining 38 percent have at least 10 matches. Of those with at least one match, the average number of matches obtained is 6.7.

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 match does not exist in the individual match are less likely to be employed in all seven 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 fewer than 10 workers, and, as in the individual match, less likely to be employed in all seven 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 control groups of non-injured workers. The outcomes of the non-injured control groups are weighted to reflect the number of matches available for each injured worker (ie the weight is equal to the inverse of the number of matches). We examine employment and benefit receipt rates (Figure 4), and average monthly earnings and benefit receipts (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 whom 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 in our regression analysis. In contrast to figures 1 and 2, these figures exclude the period of the ACC spell. 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 for injured workers who receive earnings compensation over a period of more than three months are considerably worse, post-injury, than those for the non-injured control group. For example, employment rates for individuals receiving 4–6 months of ACC are 5 percent lower, and average earnings are 8 percent and 9 percent lower at six and 12 months post-injury, respectively, than those of the non-injured group. The outcomes for injured workers who receive ACC for 10–12 months are much worse: employment rates six and 12 months post-injury are 14 percent and 16 percent lower, respectively, and average earnings 23 percent and 24 percent lower, respectively, than those for 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 percent lower and earnings 2 percent lower for injured workers receiving 4–6 months of ACC, and 3 percent (employment rate) and 5 percent (earnings) 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 six and 12 months post-injury are 2 percent lower for injured workers with a one-month ACC spell than for those of the comparable non-injured group, while the relative employment rate 18 months prior to injury is 1 percent lower. Similarly, relative earnings six and 12 months post-injury are 3 percent and 4 percent, respectively, lower for these workers, while relative earnings 18 months prior to injury are 2 percent 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:

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

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 with whom 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 1 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.[20] 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.[21] All the results are based on ordinary least squares (OLS) regressions. We have used OLS for our two binary outcomes, in preference to non-linear alternatives such as logit or probit models, because of the relative ease of obtaining estimated probability effects associated with the various factors of interest, which is the natural scale to use for interpreting the results (at least in terms of the magnitudes of the estimated effects).[22]

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 six months after the ACC spell ended, and include no variables besides our proxy measure of injury severity (ie 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.[23]

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

---

20 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.

21 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 extremely small sample size of this group leads to extremely imprecise estimates.

22 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.

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

24 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 percent for our individual match and 48 percent for our firm match), but have little qualitative effect 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 experience injury, 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 (2004) demonstrate that this is a fairly common finding in the programme evaluation literature. These papers suggest that comparing *changes* in outcomes between the pre-treatment and post-treatment period, 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 differentiating individual fixed effects which are correlated with both the outcome (eg employment status) and the likelihood of being treated.

This procedure assumes that differences between the treatment and control group are time-invariant, therefore, 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 require 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. Given that we have at least 18 months of pre-injury data on all individuals, we chose to control for outcomes at this point.[25]

In columns 5–8 of table 4 we repeat the regressions in columns 1–4, but examine the effect injuries have on the likelihood of a *change* in employment status six 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 six months after compensation ends. The impact of longer-duration injuries is also dampened, but we still find strong evidence of large effects on employment, with a four-month ACC spell reducing the likelihood of being employed by 2 percent, a six-month spell by 4–6 percent, 8–9 month spells by 9–10 percent, and 13–24 month spells by 12–15 percent compared with the change in employment experienced by the control group.

---

25 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 five months in duration have a more limited impact on outcomes.

It is possible that longer-duration injuries merely have short-run impacts on employment. To examine this, we repeat the difference-in-differences with covariate regressions for the change in employment status 12 months (in columns 9 and 10) and 18 months (in columns 11 and 12) after leaving ACC, compared with 18 months prior to injury. Here, we find no evidence that the negative effects of longer-duration injuries on employment decline in magnitude over time.[26]

Table 5 presents regression estimates of the effect of injury on changes in employment, benefit receipt, and total income six months (columns 1–6) and 18 months (columns 7–12) 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 six months after leaving ACC compared with 18 months prior to injury. For example, four-month spells increase the likelihood by 1 percent, six-month spells by 3–4 percent, and longer spells by 5 percent. These are large relative increases as the average rate of benefit receipt is only 7 percent for the non-injured population. Overall, a 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 declines over time.

Changes in total income measure the overall effect of injuries on individuals by incorporating three possible channels through which injuries affect them: 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 four months of ACC compensation leads to a $115–$160 decline in total income, a six-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 six months post-injury compared with 18 months prior to 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 different impacts on individuals with particular characteristics. For example, Boden and Galizzi (2003) find that women are less likely to return to work than are men with similar duration injuries, while Biddle, *et al* (2003) find that older workers suffer larger wage losses following injury than do 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 therefore 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.

26 This contrasts with most job displacement studies which find that labour market impact decreases significantly over time.

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, 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 represents 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 different 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 (or 8–24) month ACC spell reduces male employment by 4 percent (or 11 percent), but reduces female employment by 7 percent (or 14 percent). Pronounced differences are also seen for individuals with different employment histories. For example, having a 5–7 (or 8–24) month ACC spell reduces employment by 5 percent (or 12 percent) for individuals who worked in 5–6 months of the six months prior to injury. This increases to a 10 percent (or 16 percent) reduction for individuals who worked in 3–4 months, and to 14 percent (33 percent) for individuals who worked in 1–2 months in the six 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 percent, 30–49 year olds by 12 percent, and 50–69 year olds by 15 percent. It also reduces employment of individuals in the highest quartile (approximately) of earnings prior to injury by 10 percent, third quartile earnings by 11 percent, second quartile by 12 percent, and lowest quartile by 18 percent. Individuals working in accommodation/restaurants, finance/business/property, health/community services, and other services have employment reduced by 14–20 percent; and individuals working in agriculture/fishing/forestry, manufacturing, transport/storage, and construction by 9–12 percent after an 8–24-month ACC spell.[27]

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

---

27 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 who are employed at different size firms (unreported).

The results in this table are less consistent, but, in general, tell a similar story to that in Table 7. 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 percent, but female total income by 19 percent. It reduces the total income of 15–29 year olds by 22 percent, 30–49 year olds by 11 percent, and 50–69 year olds by 18 percent. For individuals in the highest quartile (approximately) of earnings prior to injury, total income is reduced by 9 percent, for those in the third quartile by 16 percent, the second quartile by 25 percent, and the lowest quartile by 15 percent. For individuals who worked in 5–6 months of the six months prior to injury, total income reduces by 14 percent, individuals who worked in 3–4 months reduce by 33 percent, and for those who worked in 1–2 months, total income is down by 59 percent. Longer-duration injuries do not appear to have a different impact on the total income of workers in different industries.

Discussion

We find that injuries that result in more than two 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 four months compensation have 2 percent 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 percent decline in employment rate, a 3–4 percent increase in benefit receipt rate, and a $345–$540 decline in monthly income. Individuals who receive 13–24 months of compensation have a 16–17 percent decline in employment rate, a 7–9 percent increase in benefit receipt rate, and a $605–$610 decline in monthly income. These are very large impacts relative to the average outcomes for non-injured workers; employment rates are 20 percent lower, benefit receipt rates 90–120 percent higher, and total income 25 percent lower.[28]

We find no evidence that the magnitude of these impacts declines over time (at least in the first 18 months after leaving ACC) suggesting that injuries have long-term 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 differences are time-varying in this manner, and are not an artifact of the matching criteria that require 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 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 these possible confounding effects, which provides clear evidence that longer-

28 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 earnings of injured workers are 25 percent 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 those for New Zealand, because of institutional differences, it is worth noting that our findings for longer-duration injuries tell a very similar story.

duration injuries have substantial and long-term effects on individual's labour market outcomes.

We also find evidence that longer-duration injuries have greater impact 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 therefore their employment status is more sensitive to wage losses.[29] Because we have limited information on individual 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 that by far the strongest impact of long-duration injuries is on workers with the least attachment to the labour force prior to injury, with their employment rates declining by 33 percent and their total income by 58 percent after injury (compared with a 12 percent fall in employment and a 15 percent decline in total income for individuals with the greatest attachment to the labour force prior to injury).

Our results in Table 6, 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 the proportion of injured workers who were working for their pre-injury employer at specific times after the spell ended, was lower than for the non-injured control group and decreased with injury duration.[30] Job change may help injured workers avoid some of the negative consequences of injury (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 (LEED) contains monthly information on benefit receipts, earnings, and accident earnings-related compensation for all New Zealanders over a five-year period. This institutional system and data source allow us to use a 'programme 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 individuals who have similar observed characteristics.

29 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.
30 For example, among workers who have 10–12-month ACC spells, 41 percent are employed with the same employer pre- and immediately post-injury, 32 percent are with a different employer immediately post-injury, and 27 percent are not employed immediately post-injury. This compares with 70 percent, 20 percent and 10 percent, respectively, for the same point-in-time comparison in the control group.

We find that injuries that result in more than two months of earnings compensation have negative effects on future labour market outcomes and that for longer-duration injuries the magnitude of these effects is quite substantial. For example, individuals who receive four months compensation have 2 percent lower employment rates (or 2–3 percent relative to the control group's employment rate) and $140–$200 (6–8 percent) lower monthly incomes, 18 months after compensation ends than comparable non-injured workers. Individuals who receive 10–12 months of compensation have a 10–15 percent lower employment rate (12–17 percent), 3–4 percent higher benefit receipt rates (42–49 percent), and $345–$540 (14–22 percent) lower monthly incomes.

We find no evidence that the magnitude of these impacts declines over the first 18 months after leaving ACC. This conclusion is subject to the caveat that the estimated longer-term 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-term effects on an individual's labour market outcomes that may even be permanent in nature.

We also find evidence that longer-duration injuries have greater impact 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. Using other data sources to better understand why injuries have greater impact on workers with certain characteristics would be a fruitful area for future research. This type of detailed analysis might also suggest potential policies for reducing the negative impact of injuries for all workers.

References

Berkowitz M and Burton Jr J F (1987). *Permanent Disability Benefits in Workers' Compensation*, W E Upjohn Institute for Employment Research, Kalamazoo, MI.

Biddle J, Boden L I and Reville R T (2003). "Older workers face more serious consequences from workplace injuries", *Health and Income Security*, 5 (December), 1–4.

Biddle J (1998). "Estimation and Analysis of Long Term Wage Losses and Wage Replacement Rates of Washington State Workers' Compensation Claimants", draft (August).

Boden L I and Galizzi M (2003). "Income losses of women and men injured at work", *Journal of Human Resources,* 38 (Summer): no. 3, 722–757.

Boden L I and Galizzi M (1999). "Economic consequences of workplace injuries: lost earnings and benefit adequacy", *American Journal of Industrial Medicine,* 36: no. 5, 487–503.

Carroll N and Wood J (2003). *Preliminary Research into Sustainable Employment Measures Using the Linked Employer-Employee Database (LEED),* Statistics New Zealand, Wellington.

Crichton S, Stillman S, and Hyslop D (2004). *Returning to Work from Injury: Longitudinal Evidence on Employment and Earnings*, Statistics New Zealand, Wellington.

Heckman J J, Ichimura H and Todd P (1998). "Matching as an econometric evaluation estimator" *Review of Economic Studies,* 65 (April): no. 2, 261–294.

Kelly N (2003). *Prototype Outputs Using Linked Employer-Employee Data,* Statistics New Zealand, Wellington.

Lalonde R (1986). "Evaluating the econometric evaluations of training programs", *American Economic Review,* 76, 604–620.

Leigh J P, Markowitz S B, Fahs M, Shin C, Landrigan P J (1997). "Occupational injury and illness in the United States. Estimates of costs, morbidity, and mortality", *Archives of Internal Medicine*, 157: no. 14, 1557–1568.

Reville R T (1999). "The Impact of a Disabling Workplace Injury on Earnings and Labor Force Participation" in *The Creation and Analysis of Employer-Employee Matched Data,* eds J Haltiwanger and J Lane, Elsevier Science, North-Holland, New York.

Reville R T and Schoeni R F (2001). "Disability from injuries at work: the effects on earnings and employment", *RAND Labor and Population Program Working Paper,* 01–08 (May).

Rubin D (1979). "Using multivariate matched sampling and regression adjustments to control bias in observational studies", *Journal of the American Statistical Association,* 74, 318–329.

Smith J and Todd P (2004). "Does matching overcome Lalonde's critique of nonexperimental estimators", *Journal of Econometrics*, forthcoming.

Weil D (1999). "Valuing the Economic Consequences of Work Injury and Illness: A Comparison of Methods and Findings", draft (October).

Figure 1

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

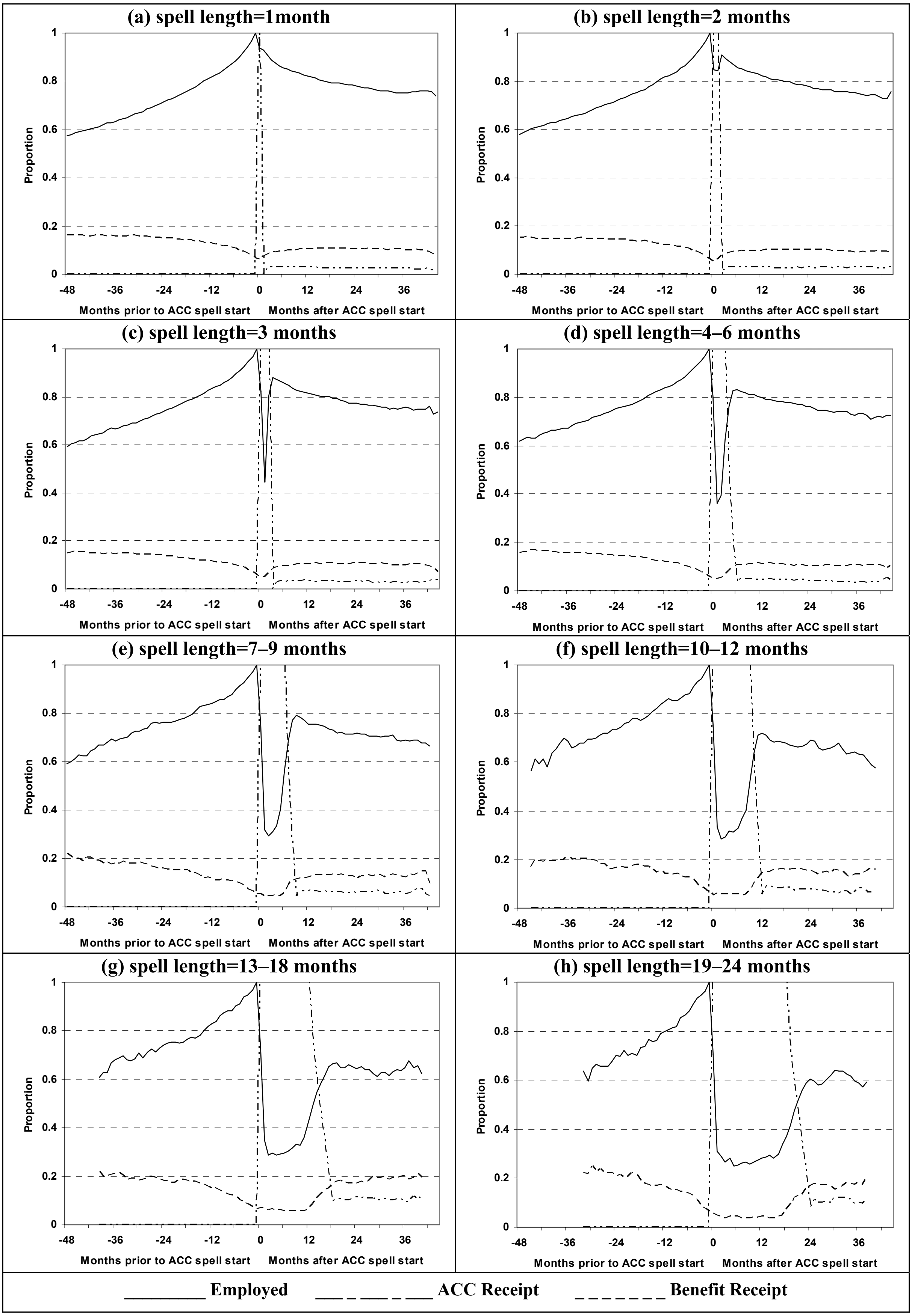
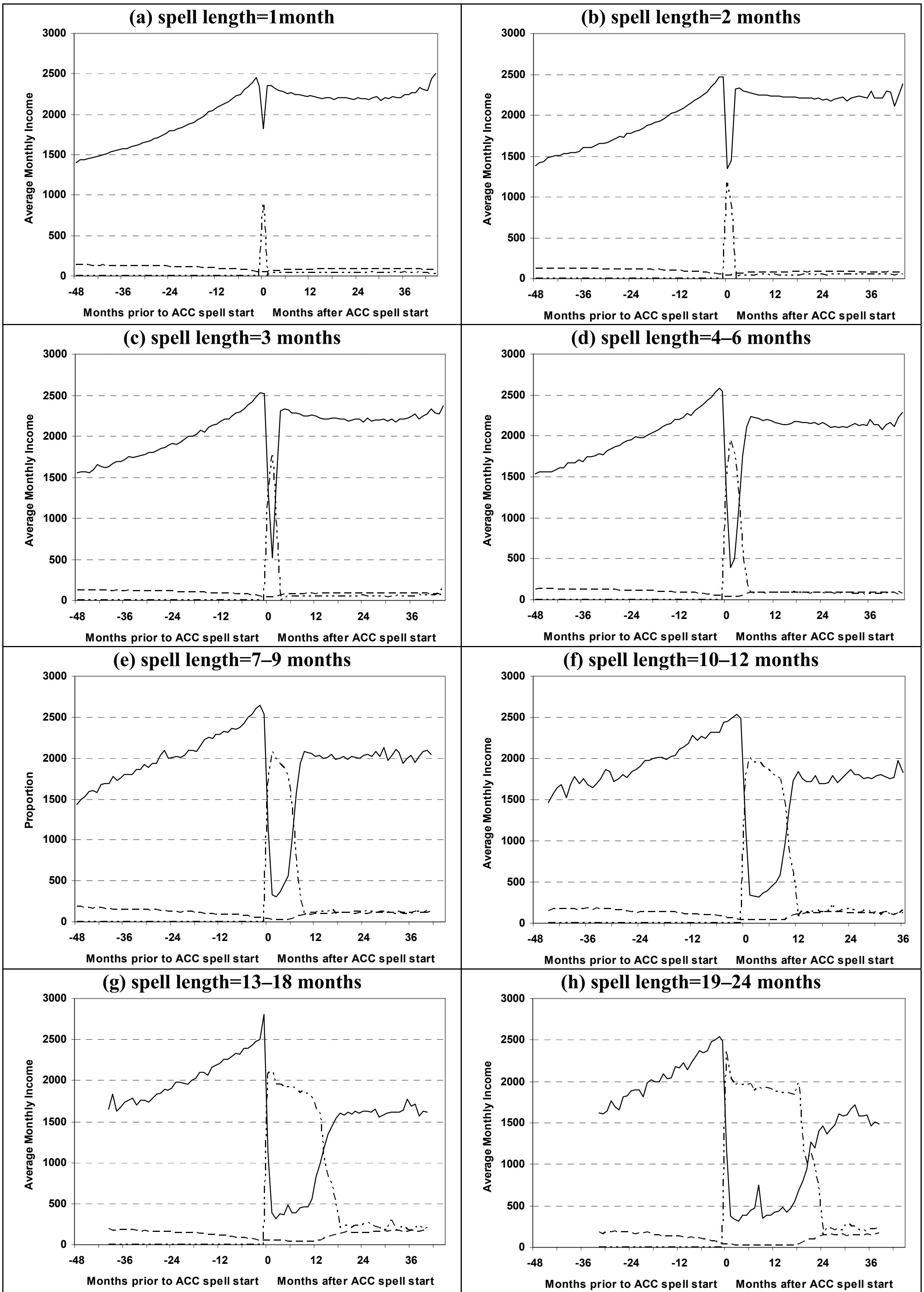


Figure 2

Earnings, Benefit Receipts and ACC Receipts for the Injured Population



_________ Wage Earnings   ___ _ ___ _ ___ ACC Receipts   _ _ _ _ _ _ _ Benefit Receipts

Figure 3

Hypothetical Earnings Loss of An Injured Worker

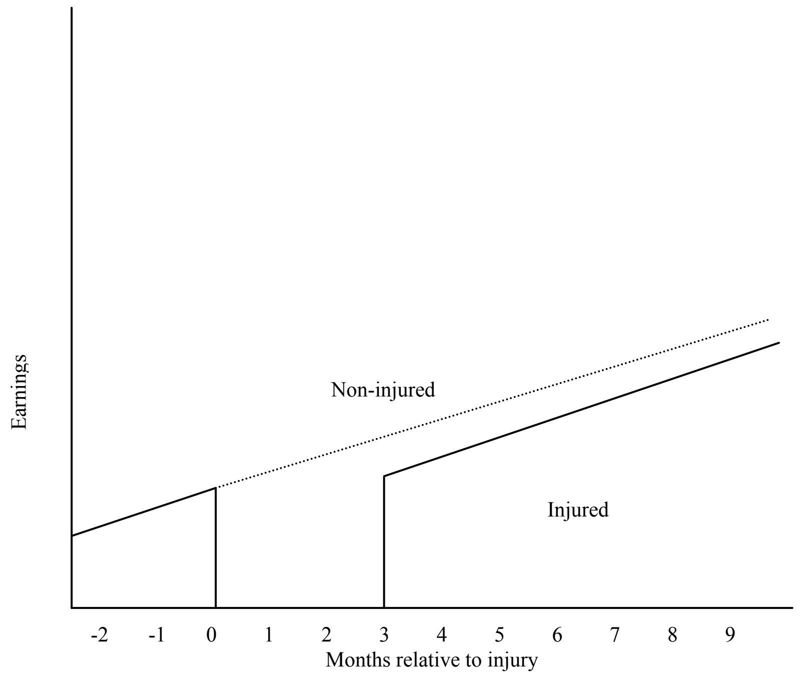


Figure 4

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

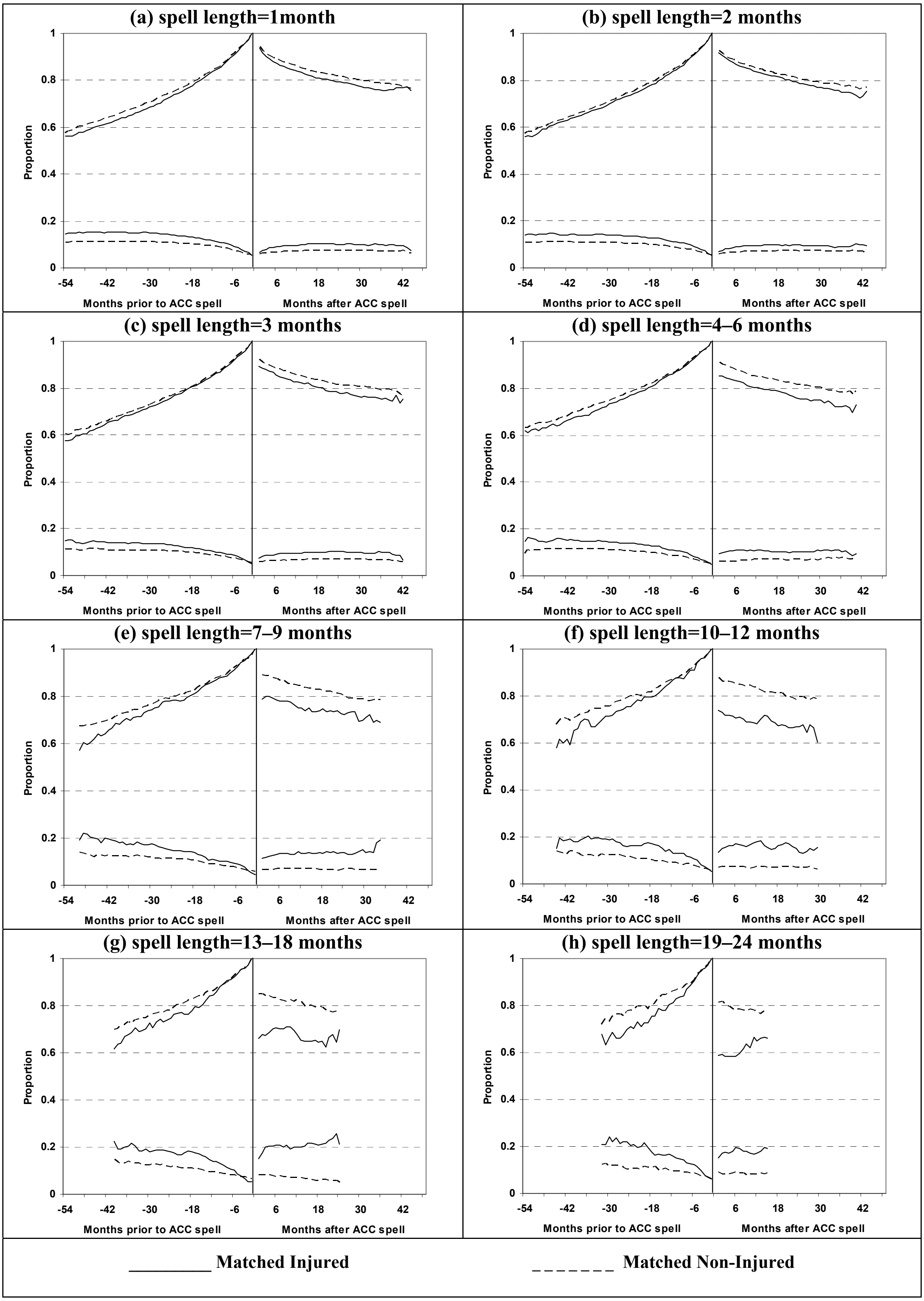


Figure 5

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

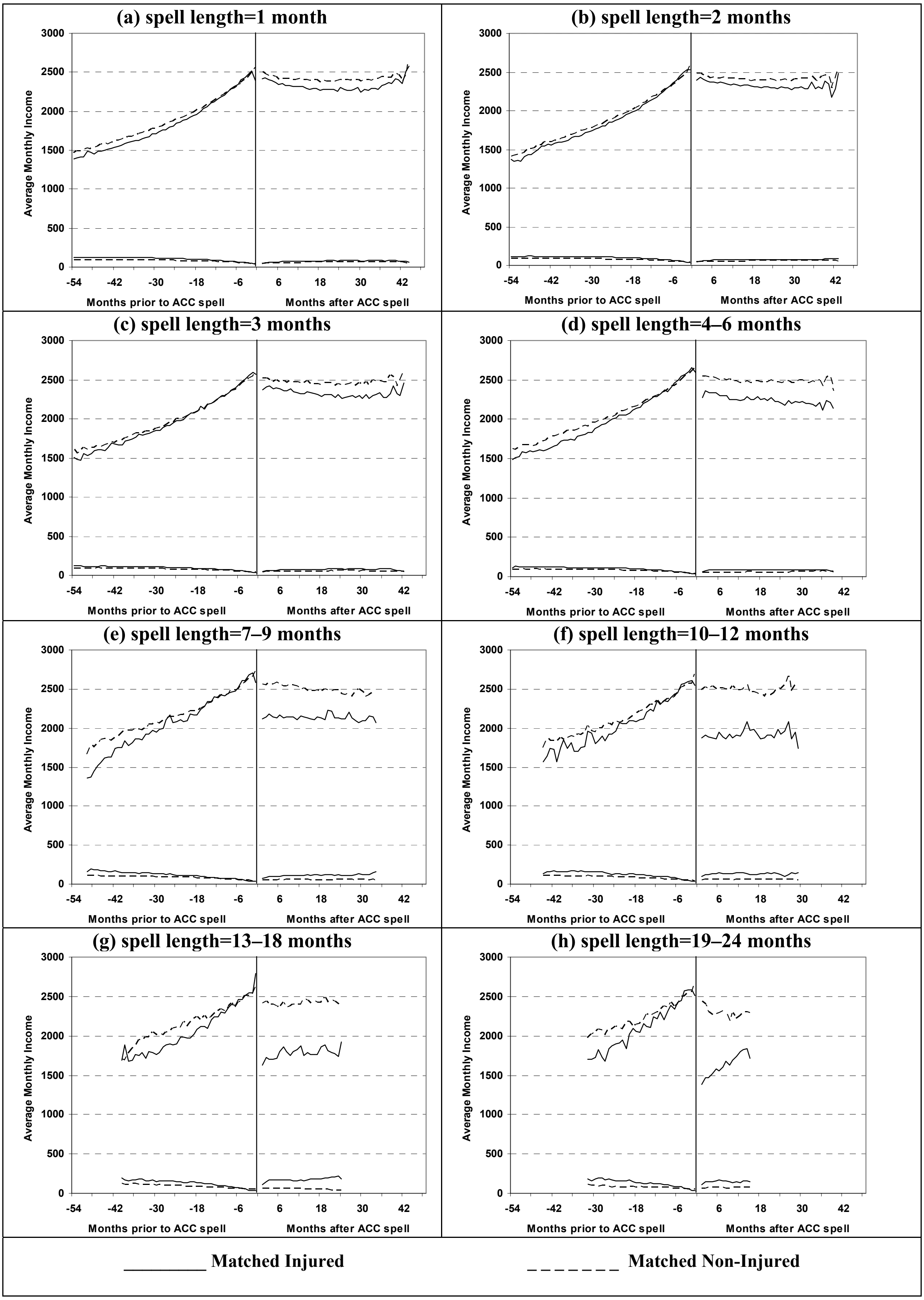


Table 1

ACC Recipients

	All Spells Starting After September 2000	Spells Finishing at Least 18 Months Before March 2004
	Percentage of total	
Number of spells receiving ACC compensation		
1	84	77
2	13	18
3+	3	5
Number of months receiving ACC 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
Total number of individuals	119,980	62,970

Note: Spell lengths are based on uncensored spells only.

Table 2

Characteristics of the Injured and Non-Injured Populations

Sample Characteristics	Non-Injured	Total Injured	Number of Months Receiving ACC Compensation: 1	2	3	4–6	7–9	10–12	13–24
Mean									
Age (years)	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 (no. of employees)									
0–4	10	12	10	12	13	13	13	13	12
5–9	9	11	10	12	12	12	12	14	12
10–29	16	20	20	20	20	19	21	18	21
30–49	6	9	9	9	9	8	9	9	10
50–99	8	10	11	10	10	10	9	9	9
100–999	23	23	25	23	22	22	22	22	22
1000+	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, Comms., 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	na	100	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

Note: 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 (months)			
1	48	48	48
2	27	27	26
3	11	11	11
4–6	10	10	10
7–9	2	2	2
10–12	1	1	1
13–24	1	1	1
Age (years)			
15–29	39	39	36
30–39	24	25	25
40–49	20	20	21
50–59	13	13	14
60–69	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 (no. of employees)			
0–4	11	11	3
5–9	11	11	8
10–29	20	20	19
30–99	19	18	22
100+	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 0 months in the 6 months prior to injury	1	1	1
Employed 1–5 months in the 6 months prior to injury	18	14	15
Employed in all 6 months prior to injury	81	85	84
Prior benefit receipt (excluding month before injury)			
Received no benefits in the 6 months prior to injury	86	88	87
Received benefits 1–5 months in the 6 months prior to injury	9	8	8
Received benefits in all 6 months prior to injury	5	4	5
	Mean		
Prior income (excluding month before injury) ($)			
Average earnings in 6 months prior to injury	2,494	2,552	2,594
Average benefit income in 6 months prior to injury	653	640	653
Summary statistics			
Percentage of injured population	100	92	77
Number of individuals	110,330	100,960	85,390

Note: 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 ACC spell ends				6 months after vs. 18 months before				12 months after vs. 18 months before		18 months after vs. 18 months before	
Effect of an ACC Spell	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
of length:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1 month	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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

Note: 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 vs. 18 months before						18 months after versus 18 months before					
	Employment		Benefit Receipt		Total Income		Employment		Benefit Receipt		Total Income	
Effect of an ACC Spell	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match	Indv Match	Firm Match
of length:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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 months	-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

Note: 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, by Length of First ACC Spell, by Characteristics

	Duration of ACC spell				
Characteristic	1–2 Months	3–4 Months	5–7 Months	8–24 Months	R-squared Observations
Total	-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 years	-0.002	-0.032	-0.056	-0.098	0.056
	(0.003)	(0.006)	(0.012)	(0.018)	217,530
Age: 30–49 years	-0.010	-0.031	-0.052	-0.117	0.039
	(0.002)	(0.004)	(0.007)	(0.010)	259,680
Age: 50–69 years	-0.011	-0.026	-0.048	-0.145	0.041
	(0.003)	(0.007)	(0.011)	(0.015)	84,860
Agriculture	-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	-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
Some benefits in 6mths prior	-0.005	-0.066	-0.055	-0.106	0.025
	(0.006)	(0.015)	(0.026)	(0.034)	47,900
Employed 1–2 of 6 mths prior	-0.017	-0.045	-0.139	-0.328	0.044
	(0.016)	(0.042)	(0.073)	(0.116)	7,210
Employed 3–4 of 6 mths prior	-0.018	-0.010	-0.098	-0.163	0.028
	(0.010)	(0.023)	(0.038)	(0.053)	22,240
Employed 5–6 of 6 mths prior	-0.006	-0.032	-0.047	-0.118	0.034
	(0.001)	(0.003)	(0.006)	(0.008)	527,980

Note: 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 represents 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

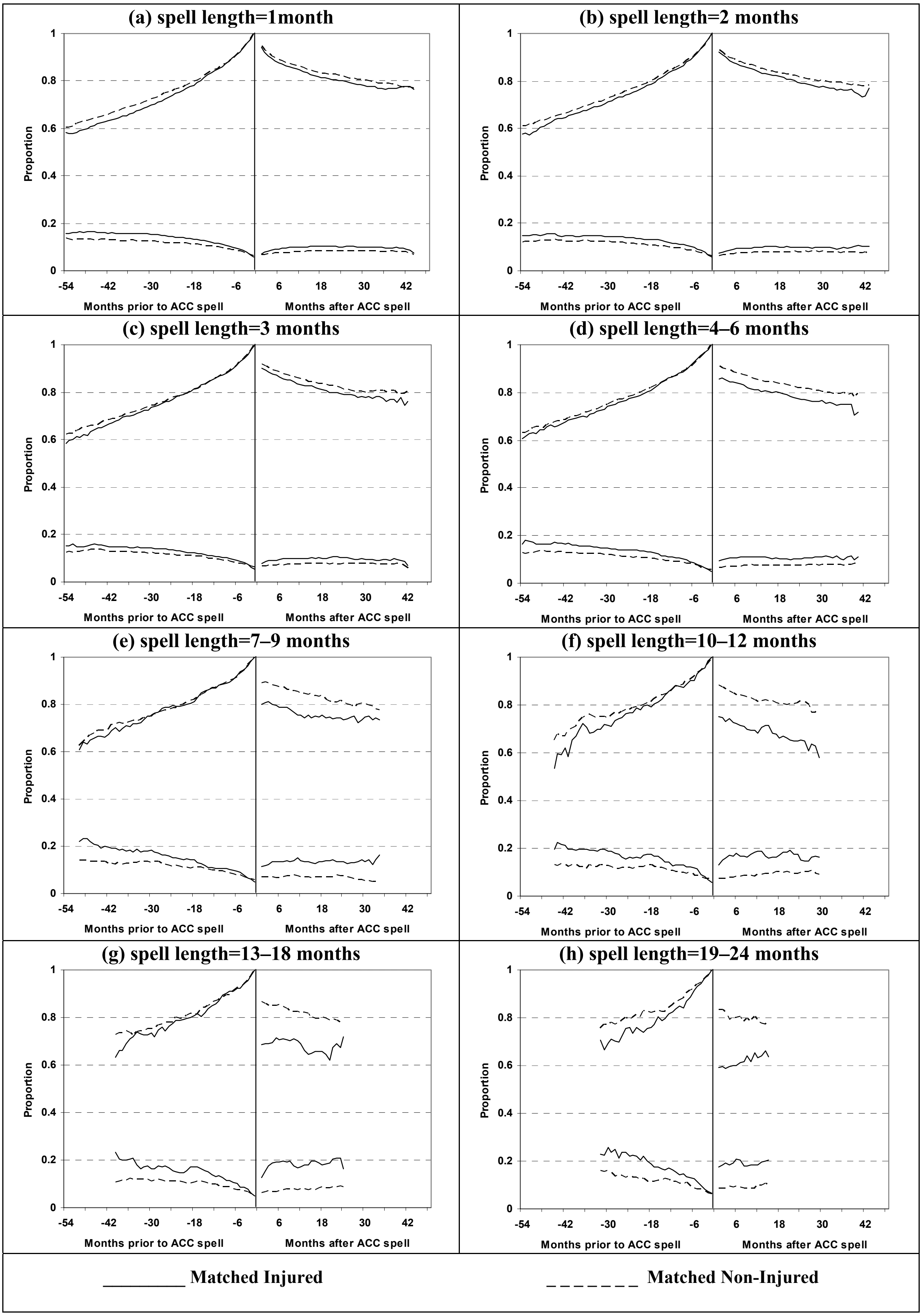
Regression Estimates of the Effect of Injuries on Total Income 12 Months After, versus 18 Months Before, by First ACC Spell, by Characteristics

Characteristic	Average Income ($)	Duration of ACC spell 1–2 months	3–4 months	5–7 months	8–24 months	R-squared Observations
Total	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	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	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 6mths prior	2,565	-67.9	-171.4	-214.3	-414.8	0.022
		(6.9)	(15.0)	(26.8)	(36.9)	514,160
Some benefits in 6mths prior	1,780	-19.1	-121.0	-130.6	-177.2	0.036
		(15.8)	(36.1)	(62.7)	(82.7)	47,900
Employed 1–2 of 6mths prior	1,427	-68.5	-222.0	-158.8	-846.0	0.053
		(40.6)	(104.3)	(181.4)	(287.0)	7,200
Employed 3–4 of 6mths prior	1,657	-100.8	-80.3	-11.0	-550.2	0.024
		(33.0)	(78.1)	(129.1)	(178.7)	22,240
Employed 5–6 of 6mths prior	2,580	-60.9	-166.1	-217.6	-373.0	0.021
		(6.7)	(14.4)	(25.7)	(35.1)	527,980

Note: The displayed coefficients indicate the marginal effect of a particular duration ACC spell. 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 represents the results from a separate regression run for the identified group. All regressions include a full set of covariates, as described in the paper.

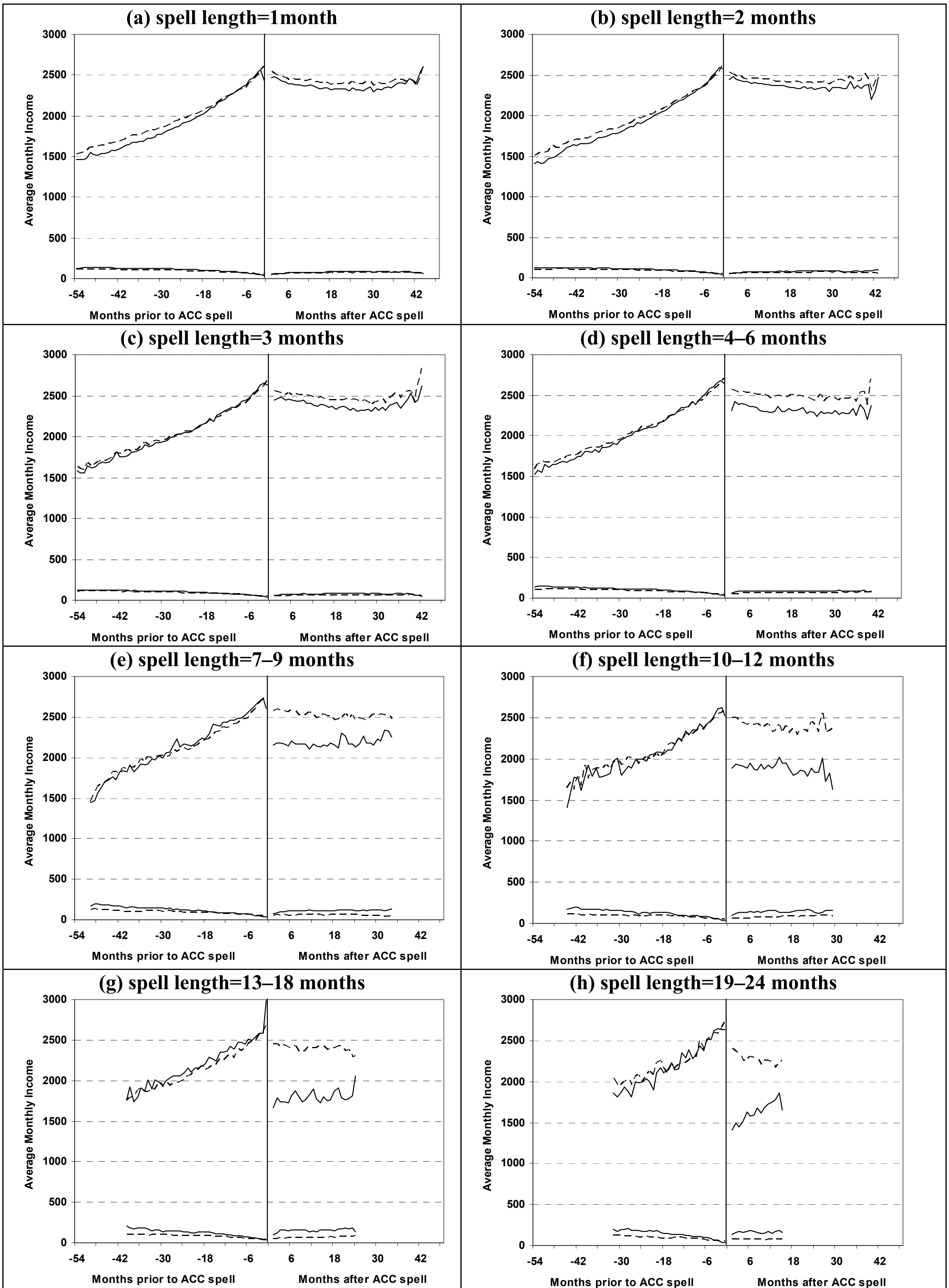
Appendix: Figure 1

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



Appendix: Figure 2

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



________ **Matched Injured** _ _ _ _ _ _ _ **Matched Non-Injured**