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

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## *Note*

The outputs shown in this paper have been compiled using input data that is largely in its 'raw' form. Limitations exist in the ability of the raw data to support statistical outputs, some of which are outlined in this paper. Investigations suggest that the effects of these limitations may be reduced using appropriate methods that are being developed. However, these methods have not been fully incorporated into the outputs in this document. As such, results shown are to be regarded as illustrative of potential only. This document is released to inform interested parties of ongoing research and to encourage discussion of work in progress.

The tables in this paper contain information about groups of people so that the confidentiality of individuals is protected. All results presented in this paper have been rounded; counts to the nearest 100, percentages to the nearest one percent, and average dollar amounts to the nearest $10. All results presented in this paper are based on 100 or more individuals. 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 (IRD) 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 IRD 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 *Linked Employer-Employee Data Project: Privacy Impact Assessment* paper published on the Statistics New Zealand website at: www.stats.govt.nz.

The IRD 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 IRD's core operational requirements.

### *Abstract*

New Zealand has a comprehensive accident insurance system, administered by the Accident Compensation Corporation (ACC), which both pays the direct medical and rehabilitative costs of all accidental injuries and compensates all workers 80 percent of their earnings for any time post-injury that they are unable to work. This paper uses data from Statistics New Zealand's experimental Linked Employer-Employee Database (LEED) to examine the effect of suffering an injury on individual labour market outcomes. The experimental LEED database contains monthly information on ACC, benefit and earnings receipt for all New Zealanders over the three-year period from April 1999 to March 2002.

Using receipt of ACC earnings compensation as a proxy for being injured, we examine the effect of injuries on employment and benefit rates, earnings and benefit income. Individuals who are injured and receive earnings compensation are unlikely to be a random sample of the working population. For example, workers in particular industries and occupations are likely to be more exposed to hazardous situations. Thus, we estimate the effect of injury by comparing the observed changes in labour market outcomes for our injured population with a matched 'control' group of non-injured individuals who have similar observed characteristics as the injured population. We allow the magnitude of these effects to differ for individuals with different lengths of time receiving ACC compensation as a proxy for the severity of injury.

We find that injuries which result in more than three months of earnings compensation have negative effects on future labour market outcomes. For example, individuals who receive four months' compensation have 2-4 percent lower employment rates, 2 percent higher benefit receipt rates, and 6-8 percent lower monthly income six months after compensation ends than comparable non-injured workers. For individuals who receive seven to nine months of compensation, these negative effects are larger, with employment rates 8-10 percent lower, benefit rates 7 percent higher, and monthly income 13-17 percent lower. Furthermore, these negative effects remain at a similar magnitude 12 months after compensation ends.

# 1. Introduction

New Zealand has a comprehensive accident insurance system, administered by the Accident Compensation Corporation (ACC), which both pays the direct medical and rehabilitative costs of all accidental injuries and compensates all workers 80 percent of their earnings for any time post-injury that they are unable to work. This paper uses data from Statistics New Zealand's experimental Linked Employer-Employee Database (LEED) to examine the effect of suffering an injury on individual labour market outcomes. LEED contains monthly information on ACC, benefit and earnings receipt for all New Zealanders over the three-year period from April 1999 to March 2002. Using receipt of ACC earnings compensation as a proxy for being injured, we estimate the direct effect of injury on a variety of labour market outcomes, including employment and benefit rates, earnings, and total income, for individuals who are employed at the time of injury.

Injuries can have large long-term effects on individuals, potentially leading to a permanent loss of the ability to earn a living (Weil 1999). Most developed countries have disability insurance or benefits systems that help severely and/or permanently injured people maintain their pre-injury income or at least avoid becoming destitute. The great majority of these systems relate only to injuries that occur at the workplace or pay much lower levels of benefits to individuals injured elsewhere. To the best of our knowledge, New Zealand is one of the few countries with a comprehensive state-run accident insurance system that does not differentiate based on where the injury occurred.

This is important because it is not typically possible to estimate the effect of injuries on labour market outcomes using survey data.[1] Thus, past research on this topic has relied on administrative data from the United States workers compensation system (Berkowitz and Burton 1987; Biddle 1998; Boden and Galizzi 1999; 2003; Reville 1999; Reville and Schoeni 2001). These studies have only been able to examine the effect of workplace injuries on labour market outcomes. While this subset of injuries is quite relevant to policymakers as many governmental efforts to reduce injuries focus on the workplace, only a minority of injuries actually occur there. For example, in New Zealand only 40 percent of claims involving earnings compensation stem from workplace injuries. Furthermore, it is quite possible that workplace injuries have a lesser impact on individuals, as employers may have legal or moral obligations to help mitigate any negative effects. This study is the first to our knowledge that examines the impact of both work and non-work injuries on the future labour market outcomes of employed individuals.

The research cited above has mostly relied on data only for the population receiving workers compensation to estimate the effect of injuries on labour market outcomes. Individuals who become injured and receive earnings compensation are not likely to be a random sample of the working population. For example, workers in particular industries and occupations are likely to be more exposed to hazardous situations or might be greater risk-takers away from the office. Thus, in order to estimate the 'true' effect of injury on these individuals it is necessary to compare their outcomes to those achieved by a random sample of the non-injured population with similar exposure to injury. Previous studies, besides Reville (1999) and Reville and Schoeni (2001),

[1] First, general social surveys do not ask about injuries and many do not even ask about disabilities. Second, while surveys likely exist that do ask detailed questions about injuries, these typically collect 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 this is likely to be consistently reported across the population and over time (Weil 1999). Fourth, a small percentage of individuals suffer relatively serious injuries, and thus sample populations are quite small in datasets that also have the appropriate labour market information.

have been unable to do this and have instead relied on comparing pre- and post-injury outcomes for the injured population (Berkowitz and Burton 1987) or have compared workers with serious injuries to those with more minor injuries (Biddle 1998; Boden and Galizzi 1999; 2003).

The comprehensive nature of LEED allows us to be flexible in our parameterisation of the underlying relationship between observed characteristics and exposure to injury. We use a 'matching' method to create a control group from the non-injured population which, by design, has the same observable characteristics as the injured population (Rubin 1979). Because we are able to match most injured workers to comparable non-injured workers, we are able to examine the effects of both minor and major injuries on labour market outcomes and to create a control group which shares a common economic environment with the injured population.[2] Having longitudinal data from both the pre-injury and post-injury periods allows us to further ensure that our estimates are not contaminated by unobserved heterogeneity by using a 'difference-in-differences' matching estimator to compare the change in outcomes observed for our injured population to the change in outcomes observed over the same time period in the 'control' group (Heckman et al 1998; Smith and Todd 2004).

The remainder of the paper is organised as follows. In the next section, we provide some background information on the New Zealand accident insurance system. We describe the theoretical link between injuries and labour market outcomes, and our empirical strategy for estimating the direct effects of injury on labour market outcomes. Next, we introduce LEED and describe the characteristics of individuals who receive earnings compensation for injuries in our sample period. We then present our main results. Following, we then examine the robustness of these results to a variety of changes in our methodological approach and present a few additional results of interest. Our last section concludes.

# 2. The New Zealand Accident Insurance System

New Zealand has a comprehensive accident insurance system, administered by the ACC, which provides accident cover for all citizens, residents and temporary visitors. In return, individuals forsake the right to sue for personal injury, other than for exemplary damages. The scheme provides cover for all injuries no matter who is at fault. In a number of situations, such as suicide, intentional self-injury or injury suffered during criminal conduct resulting in a prison sentence, claimants may not receive certain entitlements.[3]

The ACC pays or contributes to the direct medical and rehabilitative costs of all injuries, and compensates all workers 80 percent of their total earnings[4] (including earnings from self-employment and multiple jobs) for any time post-injury that they are unable to work. The total cost of the ACC system was approximately $2 billion in the year to June 2003, which is covered by

[2] As discussed in Heckman et al (1998), having a treatment and control group that shares a common economic environment is a key requirement for getting correct estimates using a matching method.

[3] People who have committed suicide or self-injury will receive entitlements if the death or injury was a result of mental injury. In other cases, ACC may pay the treatment costs but not other entitlement payments that may otherwise have been appropriate. In the case of injury suffered during criminal conduct, ACC may apply to the District Court for disentitlement.

[4] Compensation paid to wage and salary earners is capped – the maximum weekly amount paid in 2002/03 was $1,365 before tax, so individuals earning more than $1,706 per week during that year received less than 80 percent of their pre-injury earnings.

levies paid by all employers and the self-employed, taxes paid by employees, petrol taxes and annual automobile relicensing fees and government appropriations.[5]

### *Type of Claims*

All claims registered by ACC are categorised according to the type of services received by the injured party. Typically, a single injury event results in a single claim, although in some cases more than one claim is made. There are two main categories of claims:

i) *Entitlement* claims are those which involve a payment to the injured party (or their family) either for income maintenance, independence allowance, social and vocational rehabilitation, or death benefits.

ii) *Treatment* claims are those where ACC directly pays a health professional for a treatment service (this includes medical and dental claims and those involving counselling), and do not involve any entitlement payments.

In 2000/01, there were nearly 1.5 million new registered claims, in total of which around 85,000 were ‘entitlement’ claims.

### *Coverage and Payments*

Once a claim is lodged, ACC decides whether or not to accept it (as set out in comprehensive regulations) and payments (including earnings compensation) are generally made within seven working days. In cases where cover is initially declined by ACC and then later accepted, payments may be delayed, and in some cases the injured party may receive earnings compensation for several months in a single payment. In some cases, individuals who have lodged claims with ACC may also apply for welfare benefits, including emergency, sickness or invalid’s benefit, and for supplementary benefits such as accommodation benefits. In cases where liability is accepted by ACC, the social welfare agency, the Ministry of Social Development (MSD), is reimbursed for the cost of these benefits.

ACC assists workers to return to their pre-injury job, and if that is not possible, to a new job which is appropriate to the individual’s skills and experience. ACC also provides rehabilitation and training in order to improve the likelihood that an injured worker can return to suitable employment. Employers are not legally required to hold an injured worker’s job open; however, they are obliged to follow employment contracts and regulations governing termination of workers’ contracts. In general, if claimants are assessed as being capable of working at least 35 hours per week, they are no longer entitled to receive earnings compensation.

### *Earnings Compensation*

As discussed, ACC compensates workers 80 percent of their pre-injury earnings if they are unable to work due to injury. The compensation paid to wage and salary earners is capped. For example, the maximum weekly amount paid in 2002/03 was $1,365 before tax, so individuals earning more than $1,706 per week during that year received less than 80 percent of their pre-injury earnings. ACC does not cover the first week of work missed following the injury (this can be up to seven days of work). However, if the injury is work related, employers are required to cover this week. Workers who are injured away from work can take sick leave, annual leave or leave without pay depending on their entitlements. During the second to fifth week off work, the amount of compensation paid is based on the individual’s average earnings in the four weeks prior to the first day they were unable to work due to injury. Following this five-week period, it is based on the individual’s average earnings in the 52 weeks prior to the first day they were unable to work.

[5] The Non-Earners Account is funded by government appropriation. New Zealand’s GDP was $130 billion in the year to March 2003.

In 2000/01, there were close to 50,000 new weekly compensation claims and just over 25,000 ongoing claims. The total cost associated with these claims was $149 million and $465 million, respectively[6]. These are the claims that are identifiable in the LEED data, and they are the focus of the analysis presented in this paper.

### *Private Market Period*

Until 1999, New Zealand's accident insurance system was entirely state-run, with some elements of experience rating for employers with higher levels of claims. As of 1 July 1999, employers were required to take out accident insurance cover with private insurers (the self-employed could elect to remain with ACC or move to a private insurer). This change was to lead to a complete privatisation of the accident insurance system, as far as workplace injuries were concerned, 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 (ACC) was immediately reinstated as the only insurer for workplace accidents, beginning 31 March 2000, with most private insurance contracts expiring on 30 June 2000. During this transition period, employers were allowed to cancel private insurance and return to ACC and all new businesses were covered directly by ACC.

Thus, for the one-year period from 1 July 1999 to 30 June 2000 (which we call the 'private market' period), the vast majority of new workplace claims are not identifiable in the LEED dataset. For example, in the 1999/00 year there were 32,000 new weekly compensation claims and only 3,000 involved workplace injuries to the non-self-employed. Since the focus of this paper in on measuring the effect of all injuries on labour market outcomes, we remove ACC spells started during the 'private market' period from the majority of our analyses. In section 6, we present some analysis for these ACC spells to help get a sense of whether the effects of injury differ for injuries occurring outside the workplace.

# 3. Examining the Effect of Injuries on Labour Market Outcomes

### *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 destroy the career of a furniture mover or ballet dancer and limit the effectiveness of any office worker who must sit at a desk for long periods of time. Even minor injuries can make work difficult if they affect a body part that is essential for a particular worker (for example, a paper cut for a concert pianist or stenographer). Even if an injury does not lead directly to a loss of productivity, time spent away from the workplace undergoing treatment or recuperating can lead to a reduction in human capital. For example, an individual may forget how to do specific work tasks or might miss out on a promotion opportunity. Injuries can also have strong non-physical effects on individuals that lead to employment loss or earnings reductions. For example 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] Source: ACC Injury Statistics 2004.

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 aggressive policies for assisting injured workers in their return to work. In particular, larger firms can often make sure that a position is made available that is appropriate for the injured worker. They are also more 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.

### *Estimating the Effect of Injuries on Labour Market Outcomes*

Figure 1 presents 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 indicates the actual earnings received by this worker in each month, while the dashed line indicates the earnings the worker would have received had they not been injured. The dashed line is a counterfactual state, which means we do not ever observe this outcome. 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 wage growth they would have experienced during the three months had they not missed work.

Thus, 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* had they not experienced 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 likely experienced a change in earnings and/or employment status had they not been injured (whether they would have experienced a gain or loss depends on a host of factors, including the strength of the economy and their relative performance at work). 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 they are not likely to be a random sample of the working population. For example, workers in particular industries and occupations are likely to be more exposed to hazardous situations. This same approach is used by Reville (1999) and Reville and Schoeni (2001) and draws its inspiration from the programme evaluation literature (Rubin 1979; Lalonde 1986).

### *Creating the Matched Non-Injured Comparison Group*

A comparison group of non-injured workers can be constructed by matching the population of injured workers to a random sample of the non-injured population based on the set of individual and firm level characteristics available in LEED. As discussed in more detail in the next section, a limited number of individual and firm characteristics are available, and thus we decided to use nonparametric case-control matching method, where each injured worker is matched to up to 10 non-injured workers with the same characteristics (some of these are allowed to have a limited range). Outcomes for the matched sample of non-injured workers, the 'control' group, can then be directly compared to the same outcomes for injured workers, with the outcomes for the 'control' group weighted by the inverse of the number of matches achieved for each injured individual.

We considered matching on subsets of the following variables: sex, age and geographic location (regional council) at the time of injury (proxied by the start of the ACC spell), employment status, earnings and welfare benefit receipt prior to injury, and the firm size and industry of the individual's main employer at time of injury (the employer at which they received the highest earnings in that month). We also considered matching injured individuals only to other workers at their main employer. 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. Section 5 describes the matching procedure and criteria in greater detail.

# 4. 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).[7] All employers in New Zealand file a monthly record with the tax authority, Inland Revenue (IRD), called an Employer Monthly Schedule (EMS), which lists all individuals employed at that firm in the last month, the amount of income they received, and the amount of tax that was deducted at source.[8] Two types of recipient are covered by the EMS: those who pay PAYE tax, who are employees; and those who pay withholding tax, who are a subset of self-employed individuals. Individuals and firms each have unique administrative identification numbers (IRD numbers) that can be used to track them longitudinally.[9] LEED currently contains 36 months of linked employer-employee records from April 1999 to March 2002.

IRD's administrative records contain some basic demographic information on individuals and firms that can be merged with each unique employer-employee record. This data includes sex, age and address details for employees, and industry information for employers.[10] 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 36-month period. In addition, two important 'employee-employer' relationships are identified in the database; we can identify all individuals receiving core welfare benefits, which includes unemployment, domestic purposes, sickness,

[7] Kelly (2003) and Carroll and Wood (2003) provide a detailed discussion of the LEED project and database.
[8] New Zealand has a relatively simple tax system and most tax on income from wages and salary is paid on a monthly 'pay-as-you-earn' basis, with few people needing to reconcile their taxes at the end of the year.
[9] Some issues do exist here. 2.4 percent of the monthly records have invalid or missing IRD numbers. 1.1 percent of records have IRD numbers that belong to business entities rather than individuals, and the remaining 1.3 percent are either missing or invalid. Missing numbers mainly arise from employees not supplying their IRD number to their employer, and in many cases numbers are eventually reported. Statistical matching procedures can be used to fill in some invalid and missing numbers. These procedures were implemented by the Statistics New Zealand LEED development team and resulted in 40 percent of the 1.3 percent of missing and invalid records being repaired. More information can be found on the Statistics New Zealand website: www.stats.govt.nz. All employee IRD numbers are anonymised in the data available to the research team. Employer IRD numbers are usually assigned uniquely to firms but can represent other administrative reporting units such as head offices or holding companies. The rules for transferring IRD numbers when firms change ownership are complex. While these issues are important, they have a limited effect on the analyses undertaken in this paper.
[10] Sex is actually derived from the title and names provided on the initial registration form. Date of birth is recorded on this initial form as well, but for privacy reasons we are only provided with the month and year of birth. Similarly, actual addresses for individuals were not provided to the researchers but are replaced with aggregated location variables. For example, in our analysis we control for location using the administrative level of regional council, of which there are 16 in New Zealand. There are also issues with the address records provided by IRD, and address information is not available before June 2001. Industry information for firms is available via the Statistics New Zealand Business Frame/EMS link, but is affected by the problem noted in the previous footnote; these can refer to head offices and holding companies. We use both the location and industry variables in our analysis and expect that these minor problems should have no quantitative effect on our results. Documentation available on the Statistics New Zealand website discusses these issues in much more detail and describes future plans for further cleaning of this data.

invalid and widows benefits, and all individuals receiving weekly earnings compensation from ACC.[11]

LEED has a number of limitations relevant to the research in this paper. As noted above, only a small amount of demographic information is available for individuals. Variables such as education and ethnicity are not available. More importantly, there is no information on occupation, which is likely to be a key variable in explaining an individual's exposure to workplace injuries and likelihood of risk-taking behaviour away from work. Information is not currently available on earnings from self-employment. Some individuals with self-employment income appear on the EMS of the firm which pays them, but many do not.[12] We have excluded these individuals from our analysis, since we have a limited understanding about the selection mechanism that leads some self-employed to file an EMS. We do provide limited information on the injured population who are self-employed, but exclude these individuals from our main analysis.

It is also important to note that LEED only records an individual's taxable earnings received in a particular calendar month, can include one-off payments such as bonuses or redundancy pay, and does not include income earned 'under the table' (ie undeclared to IRD). Because calendar months have uneven numbers of days, 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 be received for prior periods of eligibility.

### *The Injured Population*

Approximately 2.3 million New Zealanders worked during at least one month between April 1999 and March 2002. One hundred and fifty thousand of these individuals received earnings compensation from ACC.[13] We identify individuals as 'being on ACC' in any month they receive income from ACC. A series of consecutive months receiving ACC are referred to as an 'ACC spell'. The overall injured population is defined as all individuals who received any earnings compensation during the 36-month period. The number of claims reported by ACC is not directly comparable with the number of injured people identified in our analysis, for a variety of reasons.[14] For example, what looks to us to be a new ACC spell can, in fact, be the continuation of an older claim.

The timing of the first ACC spell experienced by an individual, and their employment status when that spell begins, are used to classify them as belonging to one of seven groups. ACC spells

[11] Welfare benefits and ACC compensation are taxed at source and thus recorded on an EMS for IRD. A unique IRD number identifies the social welfare agency (MSD) and ACC as the 'employer' for these payments and this is different from the IRD number used for their true employees. The same IRD number is used for all main benefits and thus we cannot distinguish between these. We also have similar records for student allowances and superannuation (state pension) payments but ignore these in the analyses that follow. 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 may not be 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.

[12] These records are separately identified in LEED and are referred to as 'withholding tax' as opposed to 'PAYE tax'. This type of tax reporting is mainly used by professionals working on temporary contracts. Other individuals with self-employment fill out a different form annually to figure out how much tax they owe. The Statistics New Zealand LEED development team is currently developing a methodology for integrating these annual income records with the core monthly LEED data.

[13] Some of the injured population do not have any earnings over the 36-month period because they are self-employed for the entire period.

[14] For example, during the period from 1 July 2000 to 30 June 2001, we identify in LEED 42,500 individuals with new spells receiving ACC and 48,700 individuals continuing older ACC spells, compared with 50,291 and 26,567 new and on-going claims reported by ACC over this period.

already under way in April 1999 are grouped as 'left censored'. Further classifications are based on:

i) whether the spell begins in the 'public market' or 'private market' period

ii) whether the individual is 'employed' (ie is recorded on an EMS paying tax at source) in the month prior to starting their first ACC spell or otherwise 'self-employed'

iii) whether or not the spell begins at least seven months after April 1999 (ie after October 1999) and ends at least six months before March 2002 (ie by September 2001).

The first two criteria follow from previous discussion; the third criterion is important because our main analysis uses an individual's experience in the seven months prior to starting an ACC spell to match them to an appropriate individual from the non-injured population and measures outcomes at least six months after their first ACC spell ends.

Table 1 presents the distribution of number of ACC spells experienced by individuals in each of these groups, as well as the distribution of the length of first ACC spells and earnings compensation received while on ACC. For the reasons noted above, we concentrate on the results for 'employed' workers who begin their first ACC spell in the 'public market' period and finish that spell prior to October 2001 (in column 5). In the sample period, 87 percent of these individuals have only one ACC spell. We spent some time examining the gaps between spells for individuals with multiple ACC spells and decided not to fill in short gaps (for example, one- or two-month gaps) between spells.[15] While we present some data on individuals with multiple ACC spells, our main analysis concentrates on the effects of the first ACC spell experienced by an individual and censors the record for that individual at the onset of a second spell. Thus, any negative impact of injury on earnings or employment in between spells of receiving ACC compensation is included in the outcomes of the injured population.

ACC spells are typically short. With the exception of the 'left censored' group, 75 percent of spells last for one or two months. Spells in the 'left censored' group are much longer (31 percent last for the entire 36 months), which is not surprising given that many of these individuals may have already been receiving ACC for a long period of time prior to the sample period. Spells are longer in the private period. This occurs because the observation window is longer for individuals beginning ACC earlier in the sample period. Because our data has no direct measure of the severity of an individual's injury, we use the duration in months of the ACC spell as a proxy for severity.[16]

The distribution of ACC compensation received gives us a feel for the wage distribution of each group (recall that ACC payments are determined as 80 percent of the pre-injury wage up to a capped amount). This is particularly useful for the 'self-employed' and 'left censored' groups since we have no measure of prior earnings for them. Importantly, many ACC payments are for partial months and thus average payments are lower for shorter duration spells (as the length of the ACC spell increases, the ratio of possible partial months to completed months declines). This likely explains the higher payments found for the 'left-censored' spells at all points in the

[15] Filling in short gaps had a limited effect on the distribution of spells as the majority of spells were separated by three or more months, and we disliked the somewhat arbitrary nature of deciding what a 'short' gap was.

[16] Because both earnings before injury and total ACC compensation are observable, we are also able to create a variable measuring the number of weeks total compensation covers. This measure is only accurate if earnings in the prior month are available (remember, we do not know when different earnings spells start or end). Future work will investigate a severity measure based on this information.

distribution. The self-employed have higher mean ACC receipt (and thus likely earnings) than wage and salary employees, and the distribution of payments is more skewed as well. Compensation is slightly higher in the private market period, which may be related to the longer average duration of spells found in this period.

Table 2a examines the characteristics of the injured and non-injured populations. Column 1 presents the results for all individuals receiving ACC at any point in the sample period. The characteristics examined include age, sex, region, employment and benefit status in prior months, earnings and income in prior months, and the firm size and industry of an individual's main employer.[17] Individual characteristics, such as age and region, are measured in the reference month, which is the first month of ACC receipt. An individual's main employer is the employer from whom they received the highest earnings in the month *prior* to the reference month; however, the characteristics of the main employer (ie firm size and industry) are measured in the reference month. Column 3 presents the same characteristics for all individuals who began their first ACC spell during the public market period, received earnings in the month prior to the start of this spell (ie they are unlikely to be solely self-employed), and finished this spell prior to October 2001. This population is the focus of our analysis, and throughout the rest of the paper we refer to it as our 'main analysis' sample.

The main analysis sample makes up 25 percent of the injured population, and is younger, more likely to be employed and has higher income prior to being injured than the overall injured population. These differences are likely to be due to the exclusion of individuals who are solely self-employed from the main analysis sample. A more relevant comparison is of the main analysis sample and the non-injured population. Column 2 presents sample estimates for the non-injured population over the same periods, where the non-injured sample is constructed by taking a 1 percent sample of never-injured individuals in each sample month who are employed in the *prior* month (ie the same restriction as in our main analysis sample). The sample month is treated as the reference month for calculating individual and employer characteristics.

Comparing columns 2 and 3, we can see that the injured population is younger and much more likely to be male (68 percent of the injured population are male compared to 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); to be employed in agriculture/ fishing/forestry, manufacturing, transport/storage/communications, and construction; and is much less likely to work at very large firms and be employed in finance/business/property services, other services, and education.

Columns 4 and 5 present the same characteristics for individuals who started their first ACC spell during the private market period (after October 1999) and meet the other main sample criteria (employed in the month prior to starting the spell and end the spell prior to October 2001) and the appropriate sample of the non-injured population. As discussed in the previous section, most workplace injuries are not included in the ACC system during the private market period. The main findings in the public market period are true here, with the injured population more likely to be younger, male, have lower earnings and income and work in particular industries. At least based on the characteristics available in LEED, individuals suffering non-workplace injuries appear to be fairly representative of the overall injured population.

[17] 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. Note that this definition of employment is different to that used in official surveys, such as the Household Labour Force Survey.

The univariate comparisons examined in Table 2a are possibly misleading. For example, the large gender difference between the injured and non-injured populations might occur because of the large difference in industrial composition in these two samples. In Table 2b, we present the results from a linear probability regression model for the outcome ‘whether an individual is injured or not’, using the individual and employer characteristics presented in Table 2a as the independent variables. We compare individuals in the main analysis sample with the appropriate non-injured population over the same period. Regression coefficients are presented in column 1 and standard errors in column 2 of Table 2b.[18]

Women are found to be 8 percent less likely to be injured, controlling for other characteristics, which is less than half the observed difference. As expected, industry is strongly correlated with the likelihood of being injured, with workers in agriculture/fishing/forestry and manufacturing 7 percent more likely to be injured than those in retail trade, workers in construction 13 percent more likely, those in other services 5 percent less likely, and those in education 6 percent less likely to be injured than those in retail trade. Individuals living in Auckland or Wellington are 3–4 percent less likely to be injured than others. Individuals who have spent more time in receipt of welfare benefits and less time in employment are also more likely to be injured. These differences in location and employment status are likely to be due to regional differences in occupational composition within industries, with safer and more stable office jobs more likely to be located in the Auckland and Wellington regions.

Table 3 presents the same characteristics as presented in Table 2a for the main analysis sample stratified by the length of an individual’s first ACC spell. We examine spells shorter than five months on their own and then, because of declining sample size, we group together five and six months, and seven to nine months in this analysis. As there are fewer than 200 individuals in our sample with a first spell of 10 months or more, we do not include them in our main analyses. 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 first spells also appear to have higher average earnings (in months employed) prior to injury than those with shorter first spells; those with first spells of two to three months had an average earnings prior to injury of $2,400, while those with first spells of four to six months had an average earnings prior to injury of $2,500.

### *Event Study of the Effect of Injury on Labour Market Outcomes*

Figures 2 and 3 show the average labour market outcomes over time for the main analysis sample. This can be thought of as an ‘event study’ of the effect of being injured on labour market outcomes. The timeline on these figures is relative to the first month of ACC receipt, which we refer to as the reference month, labelled ‘post0’. The solid vertical line corresponds to the month prior to the reference month, labelled ‘prior1’. The first month after the reference month is ‘post1’. The months after the reference month include the time during which an individual is still injured and receiving ACC. These graphs are shown separately by the length of the first ACC spell. To reduce the noise on the graphs, we only present cells that have at least 100 observations.

Figure 2 presents information on the proportion of the injured population receiving income from employment, welfare benefits, and ACC earnings compensation. Figure 3 presents similar information on average earnings, average benefit receipt, and average ACC compensation. Table 4 summarises the main information displayed in these figures, focusing on outcomes 20, 14 and

[18] 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.

8 months prior to, and 6, 12 and 18 months after the start of the first ACC spell.[19] Note that, by definition, 100 percent of the main analysis sample is employed in the month prior to the reference month.

Individuals can receive ACC compensation, welfare benefits and earnings in the same month. For example, most individuals who received ACC compensation in a single month also received earnings in that month. Spells of one month include injuries that resulted 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. Of those with a single month first spell, the average earnings compensation received is close to $1,000 compared to average pre-injury earnings close to $2,500. This corresponds to an average period of compensation of two weeks. Of those whose first spell on ACC lasted four months, approximately 40 percent received income from employment in the second and third month of injury, and this accounts for 25 percent of total income in these months. This could reflect a variety of situations, including employers topping up ACC compensation, a partial return to work with reduced hours or reduced duties, and/or a very short period back at work followed by a further period of incapacity.[20]

The results presented here are based on the duration of the first spell on ACC. As first spell length increases, the likelihood of having a subsequent ACC spell also increases. For those with a first spell of one to three months, 3 percent received compensation in a subsequent month, compared to 4 percent for those with a four- to six-month spell, and 8 percent for those with a seven- to nine-month first 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 an ACC spell. Similarly, average earnings, which includes those with no earnings, rises in the months leading up to the start of the ACC spell and then falls in the months following the ACC spell. Of those individuals with a first spell of one month, 81 percent and 87 percent are employed 14 and 8 months prior to the start of the ACC spell. Of those individuals with a first spell between seven and nine months long, employment rates prior to injury are slightly lower, with 78 percent and 84 percent employed in these months. For those with a first spell of one month, 83 percent are employed 12 months after the reference month, while only 75 percent of individuals with a first spell between seven and nine months long are employed at this point.

Most previous studies of the effect of injuries on labour market outcomes have compared changes in outcome 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). This is precisely what we have done in Table 4. For example, the above comparison can be interpreted as either:

i) injured workers who receive ACC for seven to nine months have 3 percent lower employment rates 12 months after injury than 14 months before

ii) injured workers who receive ACC for seven to nine months have a 5 percent greater decline in employment rates from 14 months before injury to 12 months after than

[19] We generally count backwards from the month prior to the start of the ACC spell because ACC payments typically lag the start of the injury (and time away from work) by two weeks. Thus, these are symmetric 7-, 13-, and 19-month windows from 'prior1'.

[20] Some employers elect to provide additional compensation to injured workers. For example, some employers 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.

those receiving one month of ACC (a 3 percent decline versus a 2 percent increase in employment rate).

Unfortunately, both of these comparisons have serious problems of interpretation. First, without knowing whether employment rates are increasing or decreasing for similar non-injured workers during this time period, we have no way to judge whether a 3 percent decline in employment rates is a ‘good’ or ‘bad’ outcome. For example, if this occurred during a deep recession, we might interpret a 3 percent decline as a ‘good’ outcome. Second, without knowing whether individuals with minor injuries are unaffected by the injury or not, we have no way to judge whether they are 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. Thus, we turn to our main results, which compare the labour market outcomes of our analysis sample to those achieved in a ‘control’ group derived from the non-injured population.

# 5. Main Results

### *Characteristics of the Matched Samples*

Two main match criteria are used to produce the main results presented in this report. Other match criteria are also investigated and the robustness of the results to these alternative match criteria are presented in Section 6. We match injured workers to a sample of eligible never injured workers in the same reference month (ie the month the injured worker starts their first ACC spell). As our definition of the main analysis sample requires that individuals are employed in the month prior to the reference month, we apply this same requirement to the eligible non-injured population. The advantage of this approach is that calendar effects are minimised, as the same actual months are observed for both the injured and matched non-injured populations.

First, we construct an ‘individual’ match in which injured workers are matched to non-injured workers with the same sex, age (within two years), location (defined by 12 regional council areas), number of months employed in months two to seven prior to the reference month, number of employees (seven standard groups), industry (14 one-digit ANZSIC groups) of their main employer, and within 20 percent of the same average earnings (for the months employed) in months two through seven prior to ACC receipt. Using these criteria, 89 percent of the injured individuals are successfully matched to at least one non-injured individual.

Second, we construct a ‘firm’ match in which injured workers are matched to non-injured workers with the same main employer, location (defined by 12 regional council areas), and within 20 percent of the same average earnings (for the months employed) in months two to seven prior to ACC receipt. These criteria are quite similar to those used in Reville (1999) and Reville and Schoeni (2001). Using these criteria, 77 percent of the injured individuals are successfully matched to at least one non-injured individual.

Overall, 71 percent of the main analysis sample match on both the firm and individual criteria, 18 percent match only on the individual criteria, 6 percent match only on the firm criteria, and the remaining 5 percent do not match using either criterion. By using two quite different match criteria, we aim to provide evidence of the robustness of our results to the choice of matching algorithm. In many cases, more than one potential match is identified. We randomly select a maximum of 10 matches for each injured worker. Using the individual match, 11 percent of injured workers have no match, 7 percent have a single match, 22 percent have between two and nine matches, and the remaining 60 percent have 10 or more matches. Of those where at least one match was identified, the average number of matches obtained was 8.0. Using the firm match, 22 percent of injured workers have no match, 12 percent have only one match, 37 percent have between two and nine matches, and the remaining 40 percent have 10 or more matches. Of those where at least one match was identified, the average number of matches obtained was 6.8. The matching was done separately for each reference month, and with replacement, so that one non-injured sample member may be matched to more than one injured sample member. In the case of the firm match, 84 percent of the total non-injured sample is associated with only one injured sample member, 13 percent with two, 2 percent with three and less than 1 percent are associated with four or more injured sample members. So while the average number of matches for each injured worker is 8.0, taking into account that the same non-injured worker can be matched to more than one injured worker, the average number of unique matches is 6.6. For the individual match, 83 percent of the total non-injured sample is associated with one injured sample member, 14 percent with two, 3 percent with three and less than 1 percent with four or more injured sample members, and the average number of unique matches is 5.7.

The first three columns of Table 5 compare the characteristics of the main analysis sample with the subsets of this sample that are matched using these two criteria. The injured workers for whom a suitable match does not exist in the individual match are more likely to work at large firms, but are otherwise quite similar to the overall main analysis population. For the firm match, the unmatched individuals are more likely to be employed in firms employing less than 10 workers, and to be employed in all six prior months. The last two columns display the same information for the tighter match criteria discussed in detail in Section 6. Tightening the match criteria resulted in a large drop in support, and in matched populations that are progressively different from the main analysis sample. For this reason, our main results focus on the results from the two match criteria described above.

### *Simple Matching Estimates*

The labour market outcomes of the injured workers can now be directly compared to those of the matched comparison group of non-injured workers, where outcomes for the non-injured comparison 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 rates, average monthly income from earnings and benefit, and average monthly income from earnings and benefit conditional on receipt (ie removing all zero values) at successive months before and after injury. These results are presented in Figures A.4 through A.9 with the outcomes stratified by length of the first ACC spell, for both the firm and individual matched populations.[21] The vertical line in all the graphs denotes the month immediately prior to first ACC receipt and is labelled ‘prior1’. The first month after the first ACC spell ends is labelled ‘postfirst1’.

These results are summarised in Figures 4 through 9, which present the same information but with outcomes measured as the percentage difference between the injured population and the appropriate control group. The percentage difference for each outcome is calculated as $(\overline{Y}_{injured} - \overline{Y}_{control}) / \overline{Y}_{control}$, where $\overline{Y}$ is the average outcome for the particular group (ie proportions for employment and benefit receipt). Only outcomes for individuals with ACC spells of length 1, 4,

[21] Once spells of different durations are combined in a single analysis it seems more appropriate to measure the ‘after’ outcome relative to the end of the first ACC spell, as opposed to the start of that spell. We generally count backwards from the month prior to the start of the ACC spell because ACC payments typically lag the start of the injury (and time away from work) by two weeks. Outcomes for individuals who have second spells on ACC are censored at the start of the second spell.

5–6, and 7–9 months are shown in these graphs to increase the readability. The regression results in the next section parsimoniously examine outcomes for all durations. Note that due to our design of the matched sample, the employment rates are identical at time 'prior1', but this is not necessarily true for other outcomes.

Overall, these results suggest that the outcomes of injured workers who receive a relatively short period of earnings compensation (such as a spell of one month) are worse both prior and post injury than those of a comparable group of non-injured. For example, using the individual match comparison group, the employment rates 6 and 12 months post injury are 2 percent lower for injured workers with a first spell length of one month than those of the comparable non-injured group. Relative employment rates 18 months prior to injury were also 2 percent lower for these workers. Similarly, relative earnings 6 and 12 months post injury are 2 percent and 3 percent lower for these workers, and earnings 18 months prior to injury are also 3 percent lower.

Smaller differences are found when using the firm match, with the relative employment rates 6 and 12 months post injury 1 percent lower for injured workers with a first spell of one month compared with rates 1 percent lower 18 months prior to injury. Similarly, relative earnings 6 and 12 months post injury are 1 percent and 2 percent lower for injured workers compared with 3 percent lower at 18 months prior to injury. This pattern is also found for relative benefit rates and for the outcomes of injured workers who received earnings compensation for two to three months.

On the other hand, the outcomes of injured workers who receive earnings compensation for *four months* are worse six to 12 months post injury than those of a comparable group of the non-injured, while the outcomes six to 18 months prior to injury are very similar for injured and non-injured. Based on the individual match, the employment rates at 6 and 12 months post injury are 4 percent and 5 percent lower, and average earnings are 7 percent and 6 percent lower than that of a comparable group of non-injured. Similarly, using the firm match, employment rates 6 and 12 months post injury are 2 percent lower and earnings 5 percent and 3 percent lower for injured workers.

The outcomes of injured workers who receive earnings compensation for *five to six months* are considerably worse six to 12 months post injury than those of a comparable group of non-injured, and again, employment and earnings six to 18 months prior to injury are very similar for injured and non-injured. Based on the individual match, the employment rates at 6 and 12 months post injury are 7 percent and 4 percent lower, and average earnings are 13 percent and 11 percent lower than that of a comparable group of non-injured. Based on the firm match, employment rates 6 and 12 months post injury are 8 percent and 3 percent lower, and earnings 11 percent and 10 percent lower for injured workers.

The outcomes of injured workers who receive earnings compensation for *seven to nine months* are also considerably worse six to 11 months post injury than those of a comparable group of non-injured. Based on the individual match, employment rates six and 11 months post injury are 11 percent and 13 percent lower, and average earnings 16 percent and 10 percent lower than for the non-injured. However, employment rates 12 to 18 months prior to injury are 3-5 percent lower, and earnings 5-8 percent lower for injured workers. Similarly, using the firm match, employment rates six and 11 months post injury are 12 percent and 13 percent lower, and earnings 17 percent and 12 percent lower for injured workers. However, employment rates 12 to 18 months prior to injury are again 3–7 percent lower, and earnings are 0–5 percent lower for injured workers.

The individual and firm level results are fairly consistent; however, one noteworthy difference is that the results based on the individual match show a greater impact of injury on employment rates than is evident in those based on the firm match. Interestingly, the results for average earnings were similar, using both match criteria. Overall, there is clear evidence that the outcomes of injured workers who receive earnings compensation over a period of five to nine months are considerably worse post injury than those of a comparable group of the non-injured. 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, there is also evidence that the outcomes for these individuals differ from the control group prior to injury. 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. Regression analysis allows us to control for variables not included in our matching algorithm and allows us to directly test various hypotheses concerning the effect of injury on labour market outcomes. Adding additional 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 by this addition to the analysis.

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

$$Y = \delta_{Injury} * [injury] + \sum_{i=2}^{6} \delta_{Duration(i)} * [injury * duration(i)] + X'\beta + \mu \qquad (1)$$

where $Y$ is the outcome of interest, *injury* is a dummy variable equal to 1 if the observation comes from the injured main analysis sample and equal to 0 if it comes from the matched control group; *duration(i)* is a dummy variable that equals 1 if the individual's ACC spell (or the spell of the person they are matched to) is of length *i,* where *i*=2, *i*=3, *i*=4 if the spell is two, three or four months, *i*=5 if the spell is five to six months, and *i*=6 if the spell length is seven to nine months; $X$ is a vector of variables to control for other factors influencing the outcome; and $\mu$ is an error term to capture unobserved effects. All models include in $X$ the *duration(i)* dummy variables, allowing there to be systematic differences between individuals with different-length ACC spells. Our focus is on the coefficients $\delta_{Injury}$ , which represents the *main* effect of one month ACC spells on outcomes and $\delta_{Duration(i)}$ , which represents the *additional* effect on top of the main effect of longer-duration injuries.

To keep the results tractable, in this section we focus on three outcomes: employment, benefit receipt, and total income measured at six and 12 months after finishing the first ACC spell.[22] All injured individuals who do not have a second ACC spell in this six-month period and who received ACC for less than 10 months are included in the regression analysis.[23] All the results are based on ordinary least squares (OLS) regressions. We have used OLS for our two binary

[22] We considered using log income, but as monthly incomes are less skewed than annual measures and no standard method exists for converting zero incomes to logs, we decided to stay with a linear measure of income.

[23] As in the graphical analysis, we censor an individual's post first ACC spell period when they have a subsequent ACC spell. We cannot tell in our data whether this spell is related to the first spell (ie a relapse) or is independent, and thus it is not clear how to incorporate this information into our analysis. In Section six, we present results where individuals with multiple ACC spells are completely excluded from the analysis. We drop individuals with a spell length greater than nine months because the extremely small sample size of this group leads to decidedly non-robust estimates.

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). In addition, the key outcomes variable, the employment rate, is not close to 0 or 1, which gives us confidence that the OLS results will be very comparable to those obtained under a Logit or Probit model. Further work will investigate a non-linear specification.

Table 6 presents regression estimates of the effect of injury on an individual's labour market outcomes six months after they stop receiving ACC compensation. The first panel examines the likelihood of being employed. In the first two columns, no variables besides our proxy measure of injury severity (ie duration of the first ACC spell) are included in the regression (remember, this variable is defined for the controls via their injured match) to allow for correlation between employment rates and exposure to injury. In the first column, we use the individual match control group, while in the second, we use the firm match control group. The results in these columns are essentially equivalent to the results in Figure 4 at six months after the first ACC spell, except that Figure 4 shows the percentage difference between the average employment rate of the injured population and the control group, while the regression parameters represent the absolute difference in the employment rates of these groups.

In columns 3 and 4, we repeat the regressions in columns one and two, adding a comprehensive set of covariates to the model (all measured in the reference month). We control for age (five-year age groups); sex; region (12 regional council areas, and missing); the number of months in which an individual is employed during the two and seven 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 two and seven months prior to injury; their average benefit receipt when receiving benefits in these months; the number of employees at their main place of employment (seven categories); the industry of this employer (11 industries, and missing); and when during the sample period the injury occurred (five quarterly groups). As discussed above, including additional control variables should improve our estimates by further controlling for differences in the injured and non-injured population by regression-adjusting our matched sample.

Injuries that result in one to three months of ACC compensation appear to have a small negative effect on employment six months following the spell, with the likelihood of being employed six months later 1–2 percent lower for injured compared to non-injured. Longer-duration injuries are found to have larger effects on employment, with a four month ACC spell reducing the likelihood of being employed by 2–4 percent, a five- to six-month spell reducing it by 6–7 percent, and a seven- to nine-month spell reducing it by 9–11 percent. The choice of control group has no systematic effect on the results, with smaller effects in the range found for four month spells and the larger effects for five- to six- and seven- to nine-month spells when the firm match is used instead of the individual match. Adding covariates to these models also has no qualitative effects on the results.

As expected, these results are quite similar to those found in the graphical analysis. One interpretation is that even injuries associated with short periods of compensation result in worse outcomes for the injured population relative to the non-injured population. However, it is possible that because we are unable to match on important characteristics such as occupation, our control groups are still 'different' in the sense that they have better labour market outcome than our injured groups. The graphical analysis provided some evidence of this possibility, with observable differences in earnings and employment found in the 12 to 18 months prior to injury, and differences in benefit receipt found at 18 months prior to injury for the injured population versus the control groups.

Heckman et al (1998), and Smith and Todd (2004) both demonstrate that this is a fairly common finding in the programme evaluation literature. These papers suggest that comparing *changes* in outcomes from the pre-treatment to post-treatment period in the treatment versus control group (where injury is the treatment), instead of the outcome *level* in the post-treatment period in the treatment versus control group, 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 heterogeneity in both the injured and non-injured population by differencing out all fixed unobservables that are correlated with both the outcome (for example, employment status) and the likelihood of being treated (for example, injured).

Thus, in columns 5 through 8, we repeat the regressions in columns 1 through 4, but examine the effect injuries have on the likelihood of *change* in employment status six months after leaving ACC compared with seven months prior to being injured. Examining the regressions that include covariates, our results remain essentially unchanged; an injury associated with a period of one to three months of compensation reduces employment by 1–2 percent, four-month spells reduce employment by 2–4 percent, five- to six-month spells by 5-7 percent, and seven- to nine-month spells by 8–10 percent. An important caveat to these results is that the observed differences between the injured and control groups in the pre-injury period are larger the further we move away from the time of injury (partially because of our design of the matched sample), which suggests that 'better' estimates would be found by looking at changes from further back in time. In the next table, we examine estimates where outcomes are differenced from 12 months prior to injury, but future work with a longer time span of data will be able to look at this issue in greater detail.

The second panel repeats these regressions, instead examining the likelihood of receiving benefits six months after leaving ACC (or the likelihood of receiving benefits six months after compared with seven months prior to injury). Again, in general, the choice of control group does not have a systematic effect on our estimates, nor does adding comprehensive covariates or changing to 'difference-in-differences' matching. For the reasons discussed above, the 'difference-in-differences' estimator that also includes covariates is our preferred specification, and thus we focus our discussion on the results from these models. Consistent with our finding on employment, longer-duration injuries are found to increase the likelihood of receiving benefits, with one-month spells increasing the likelihood by 1 percent, four-month spells by 2 percent, five- to six-month spells by 6 percent, and six- to nine-month spells by 7 percent. These are large percentage increases, as the average rate of benefit receipt is only 7 percent for the non-injured population.

The third panel again repeats these regressions, examining total income for all individuals. We view total income as measuring the overall effect of injuries on individuals. This outcome incorporates three possible channels through which injuries affect individuals: a reduced likelihood of employment; a decline in earnings for individuals remaining employed (or less earnings growth than that experienced by the non-injured population); and an increased likelihood of receiving welfare benefits (designed in part to offset the other two effects).

A wider range of estimates is found across our eight specifications for this outcome, but we consistently 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 $150–$195 (6–8 percent) decline in total income, having a five- to six-month injury spell leads to a $210–$235 (8–

9 percent) decline, and having a seven- to nine-month injury spell leads to a $345–$460 (13–17 percent) decline. These declines are greater in magnitude than the reduction in employment rates found for these groups, indicating that individuals with longer-duration ACC spells who remained employed suffered post-injury earnings loses. As can be seen in Figure A6, most of this decline in earnings occurred because these individuals had less earnings *growth* than the non-injured population (much like our hypothetical worker in Figure 1).

Table 7 presents the same regression models as in Table 6, but measures labour market outcomes 12 months after an individual stops receiving ACC compensation (the difference-in-differences regressions examine the change from 13 months prior to being injured to 12 months after leaving ACC). This approximately halves the sample size of our injured population, as only individuals injured in the first 10 months of the 'public market' period are observed 12 months after finishing their ACC spell (assuming the spell is short). The results at 12 months are quite similar to those found at six months for all three labour market outcomes. Importantly, there is no evidence that the negative effects of longer-duration injuries on employment, benefit receipt and total income decline in magnitude over time. Being away from work with an injury for four or more months appears to reduce an individual's likelihood of being employed, and their total income, and increase their likelihood of receiving government benefits over the 12-month period post injury. Moreover, as discussed above, the magnitude of these effects is quite substantial.

# 6. Additional Results

### *Robustness of Main Results*

There are several decisions made in defining our main sample that could influence our results. For example, we decided to include individuals with multiple ACC spells in our main analysis, censoring their information when they have a second spell. It is possible that individuals who have multiple injury events in our sample are systematically different from the general population either because of their personal characteristics (for example, they are careless) or because of unobserved characteristics of their job, and that these differences are correlated with both the likelihood of becoming injured and labour market outcomes. To examine the importance of this decision, we re-estimate our main models (the difference-in-differences specification including covariates), excluding individuals with multiple ACC spells during the sample period.

Table 8 presents the results from this exercise. The first four columns present results for six months after the first ACC spell ends minus seven months before injury, and the next four for 12 months after minus 13 months before. The first two columns in each of these sets replicate the appropriate main results from Tables 6 and 7 so these can be directly compared to the new results. Dropping individuals with multiple ACC spells has no qualitative effect on the results for any of the three labour market outcomes. In fact, the estimated coefficients show little variation from corresponding results in Tables 6 and 7.

As discussed in Section 5, we chose matching criteria for our main analysis that resulted in fairly high match rates for our injured population. By using two very different match criteria, we have tried to provide evidence of the robustness of our results to the choice of matching algorithm. Here, we further examine the importance of match criteria by presenting results when two tighter matching criteria are used. We use both a tighter version of our individual match, where each control is required to have, in addition to the previous criteria, the same industry at the three-digit ANZSIC level; the same number of months receiving benefit in the two to seven months prior to the reference month; and within 10 percent of the same average earnings in these months as the injured individual; and a tighter version of our firm match, where each individual is required to have, in addition to the previous criteria, the same number of months in employment and receiving benefit in the six months prior to the reference period, and the same sex and age (within five years) as the injured individual.

Using these tighter match criteria, the match rate falls to 65 percent for our individual match and 48 percent for our firm match. Tightening the individual match criteria leads to a matched population that is much more likely to have continuous employment and no benefit receipt in the six months prior to injury, while tightening the firm match criteria leads to a matched population that is much more likely to have continuous employment and no benefit receipt in the six months prior to injury, to be employed in larger firms, and to have higher average earnings prior to injury.

Table 9 presents the results using these tighter match criteria. The layout of this table is identical to Table 8, with the appropriate main results replicated from Tables 5 and 6 present in columns 1, 2, 5 and 6. Perhaps surprisingly, given the large change in the sample support, our main results are unaffected by the change to the tighter match criteria. Spells on ACC greater than three months again have large negative effects on labour market outcomes. It is important to point out that while these tighter match criteria have the advantage that our matched samples are more likely to have similar probabilities of becoming injured as our injured population, the effective sample size is greatly reduced, which lowers the precision of our estimates and restricts the population to whom the results can be applied.

### *Private Market Period*

As discussed in Section 3, during the private market period, we are only able to observe injuries for individuals that occur outside the workplace. Labour market outcomes may differ depending on where an injury occurs. For example, as hypothesised earlier, workplace injuries may have a lesser effect on labour market outcomes than those that occur away from the workplace. By doing a parallel analysis for injuries that occurred during the private market period, we are able to examine whether this is, in fact, the case. Unfortunately, the short length of the private market period leads to a limited sample of injured individuals and, because of the timing of our sample window, we observe few of these individuals 12 months prior to becoming injured. Thus, we only examine the six month difference-in-difference outcomes for individuals injured during the private market period.

Table 10 presents the main results for the private market period. The first two columns replicate the corresponding results from the public market period for direct comparison. Interestingly, little systematic difference is found between the results in these two periods for any of the three labour market outcomes. This evidence suggests that individuals with workplace injuries have similar outcomes in the short run to individuals who are injured away from work. However, recall that overall, 60 percent of injuries occur away from the workplace, and thus we are comparing the results from a sample with a majority of non-workplace injuries to those from a sample with only non-workplace injuries. This type of comparison has limited power to detect differences in outcome, especially given the small number of serious injuries that occur.

# 7. Conclusions

In this paper, we examine the effect of injuries on a variety of labour market outcomes. As New Zealand has a comprehensive accident insurance system, we have been able to examine the effect of both work and non-work injuries on these outcomes. Using receipt of earnings compensation as a proxy for being injured, we find that injuries which result in more than three months of earnings compensation have negative effects on future labour market outcomes. For example, individuals who receive four months' compensation have 2–4 percent lower employment rates, 2 percent higher benefit receipt rates, and $150–$195 (6–8 percent) lower monthly incomes six months after being injured than comparable non-injured workers. For individuals who receive seven to nine months of compensation, these negative effects are even larger, with employment rates 8–10 percent lower, benefit rates 7 percent higher, and monthly income $345–$460 (13–17 percent) lower. We also find that these negative effects remain at a similar magnitude 12 months after compensation ends and are robust to a variety of different estimation strategies.

Our results also show that injuries associated with very short periods of compensation have small negative impacts on outcomes. We are not sure what to make of this finding, as it is surprising to us that minor injuries have longer-term impacts on individuals. Without a larger window of data, it is difficult for us to examine this issue in greater detail (ie by changing our matching algorithm to use more pre-injury information). If these results are viewed as biased estimates, it is possible that a similar-sized bias affects our estimates of the impact of longer-duration injuries, although there is less evidence that the injured and control groups differ prior to injury for longer-duration spells. One possible solution is to subtract the impacts of the short duration ACC spells from the results for the longer spells (this is in the spirit of a triple-difference estimator, which uses the difference in changes in outcome for individuals with minor injuries and their controls as an additional control for unobserved heterogeneity in the injured population). This results in 1-2 percent, 1 percent, and $80–85 reduction in our estimates of the impacts of major injuries on employment rates, benefit receipt, and total income, respectively. Even with these lower-bound estimates, longer-duration ACC spells are found to have major impacts on future labour market outcomes.

Much of the analysis in this paper was only possible because of the comprehensive data collected in LEED. The ability to observe the earnings history of all the individuals who receive earnings compensation and all those who do not was essential for this paper. As noted previously, merely having information on the injured population is not enough to get appropriate estimates of the true effect of injury on labour market outcomes. Importantly, the comprehensive nature of LEED allowed us to experiment with different matching criteria, including matching injured individuals to other workers at the same firm and matching them to other workers with very similar employment histories. Because of our ability to test whether the choice of matching criteria affected our main results, we feel a much greater level of confidence can be placed in the quality of our estimates.

We feel it is important to point out that this paper uses participation in the accident insurance system merely as a proxy for injury in order to measure the effects of injuries on labour market outcomes. We do not seek to evaluate the ACC programme. A proper evaluation of the ACC requires an in-depth look at the monetary and social benefits of the system, its costs, and its overall efficiency in providing a unique service to New Zealanders. This paper merely points out that serious injuries have subsequent effects on people that result in worse labour market outcomes up to 12 months later.

The extension of the LEED database and programme over the coming year will allow improvements to be made to this analysis. For example, having more months of data will allow us to both examine the effect of longer-duration spells on outcomes and examine outcomes further away from the time of injury, and to better control for pre-injury heterogeneity in the injured population by conditioning on outcomes that occur years prior to injury (as opposed to our current use of outcomes 6 and 12 months prior). It will also be possible to examine whether the impacts of injury vary depending on the observed characteristics of the individual, such as age, gender and earnings prior to injury. In addition, we will be able to incorporate the self-employed into our analysis. These individuals are in a unique situation as far as control over their work environment is concerned. It will be quite informative to observe whether they have similar changes in outcomes to the employed sample. This paper would also greatly benefit from data from the ACC administrative system being merged into LEED. This would allow us to examine the importance of injury type, severity and treatment in interpreting our results.

The following figures and tables appear in the downloadable Excel files accompanying this PDF document (except for Figure 1, which appears at the end of this document).

## List of Figures

## List of Tables

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Figure 1

## Hypothetical Earnings Loss of Injured Workers

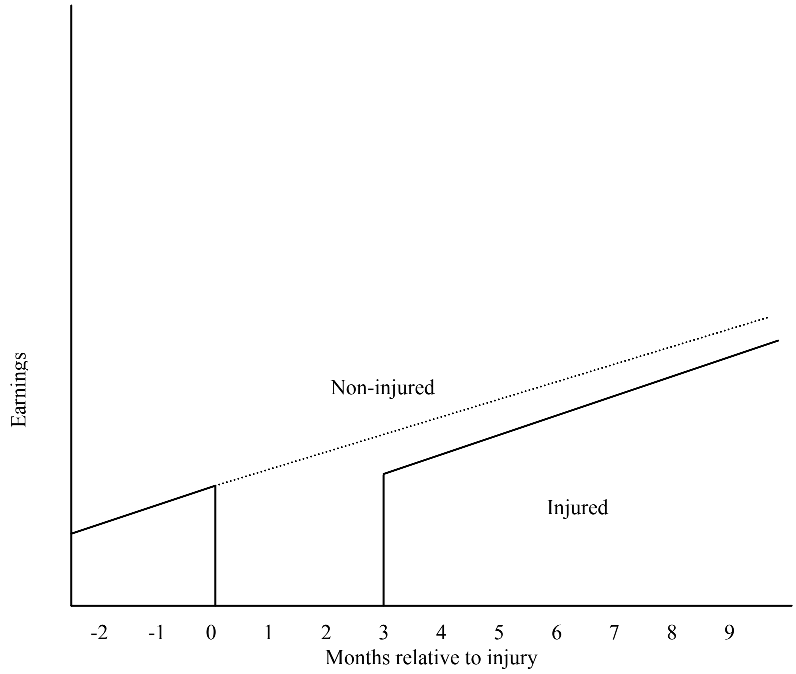
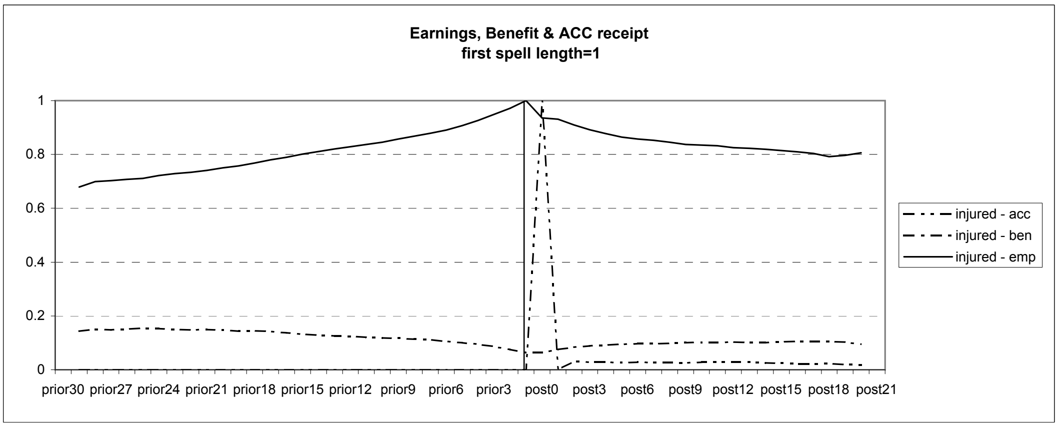
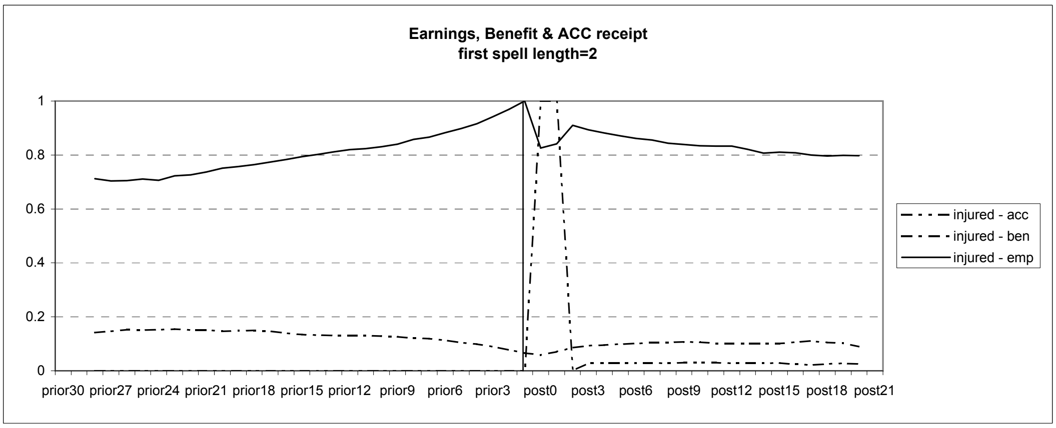


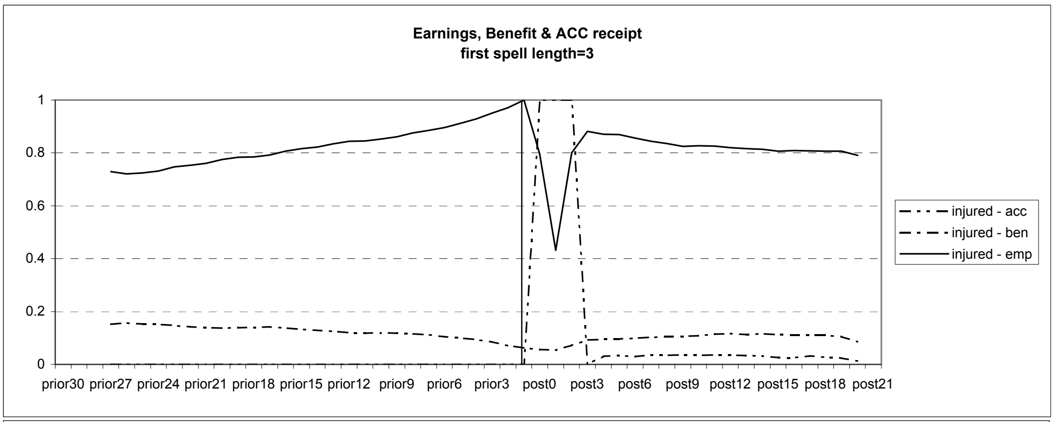
Figure 2

**Proportion of Injured Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post the first month received ACC**

**By length of first ACC spell**



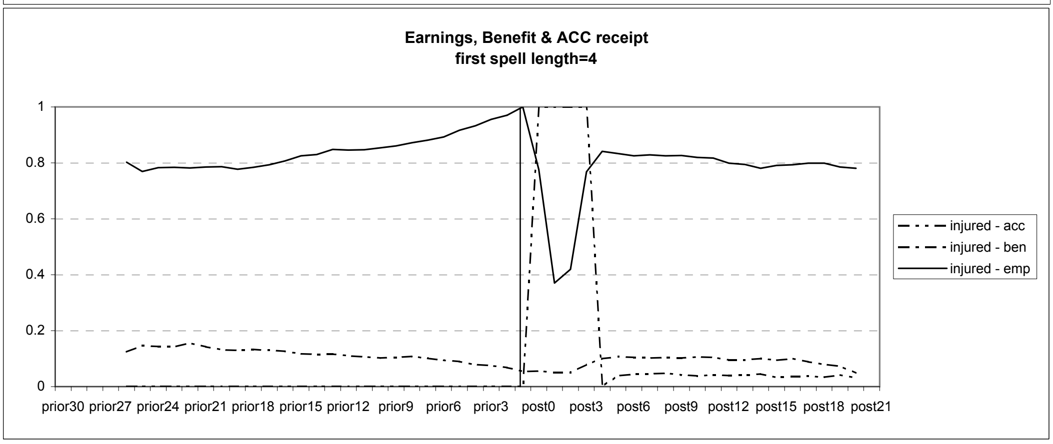



Figure 2
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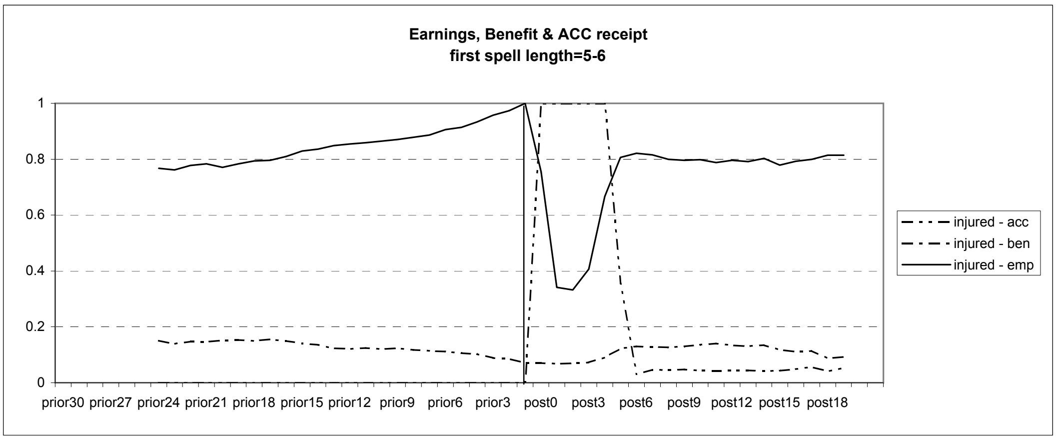
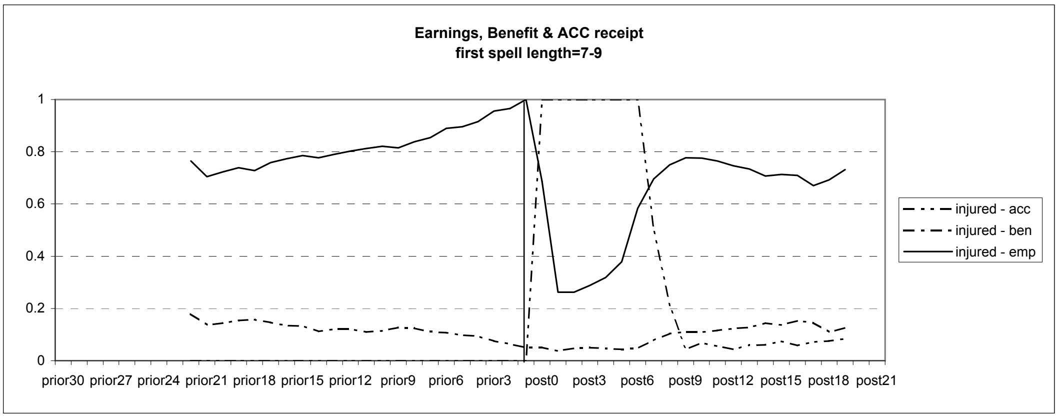
**Proportion of Injured Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post the first month received ACC**

**By length of first ACC spell**



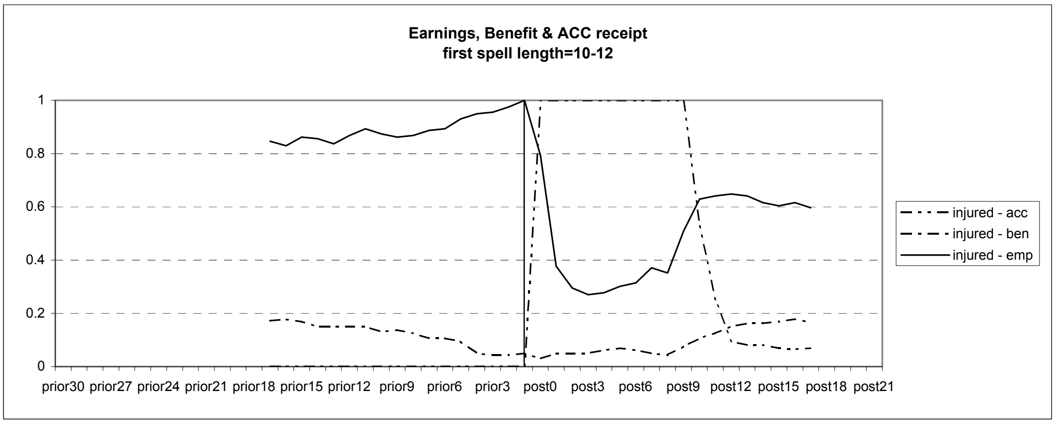
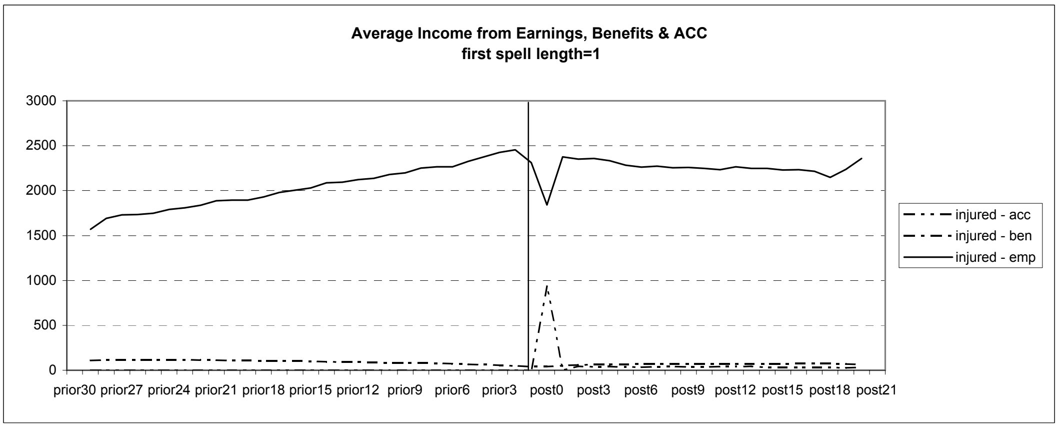
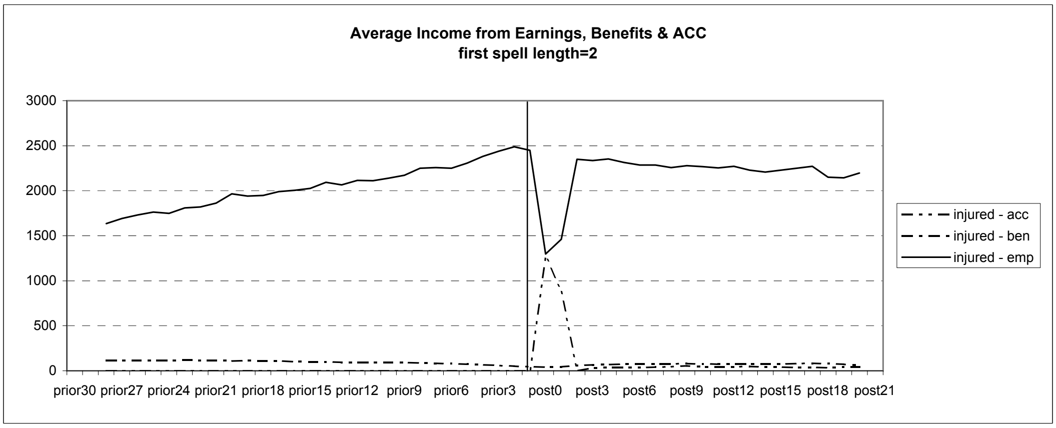



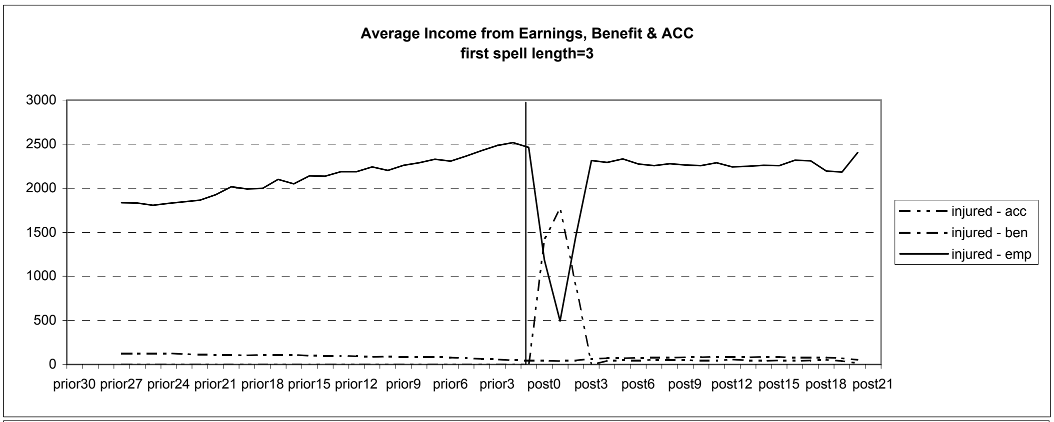
Figure 3

**Proportion of Injured Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post the first month received ACC**

**By length of first ACC spell**



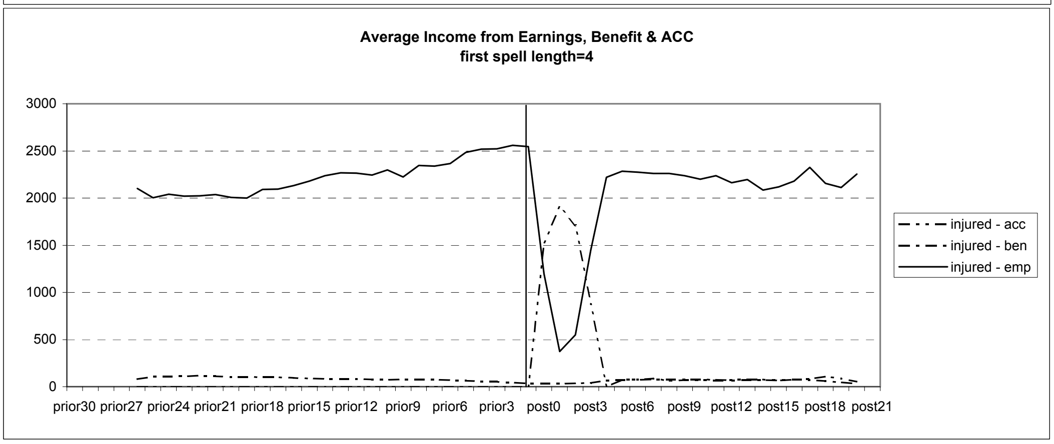
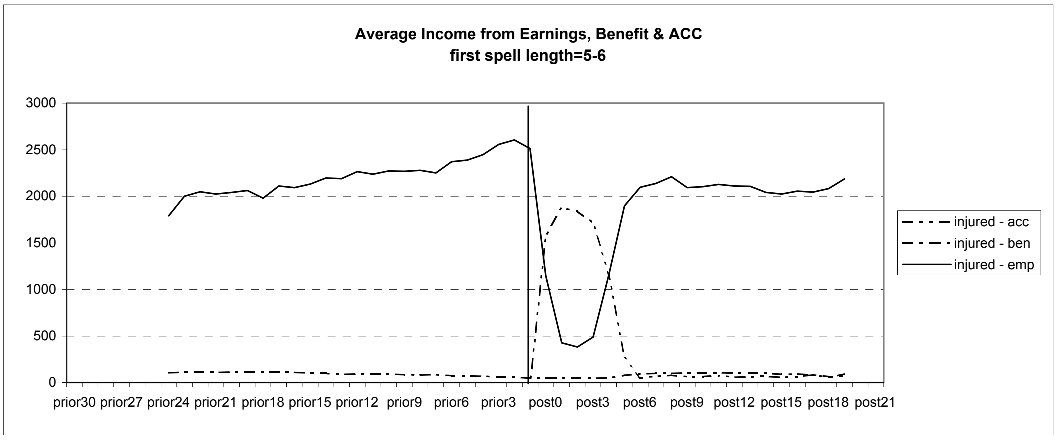
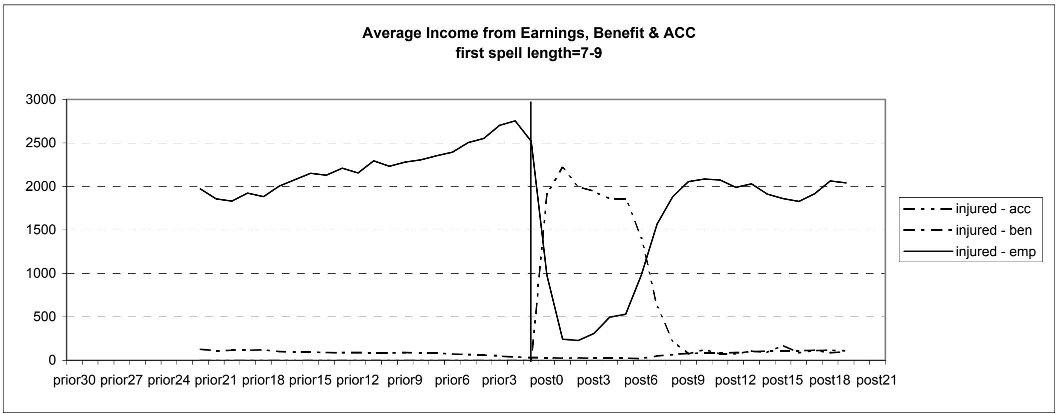



Figure 3
*continued*

**Proportion of Injured Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post the first month received ACC**

**By length of first ACC spell**



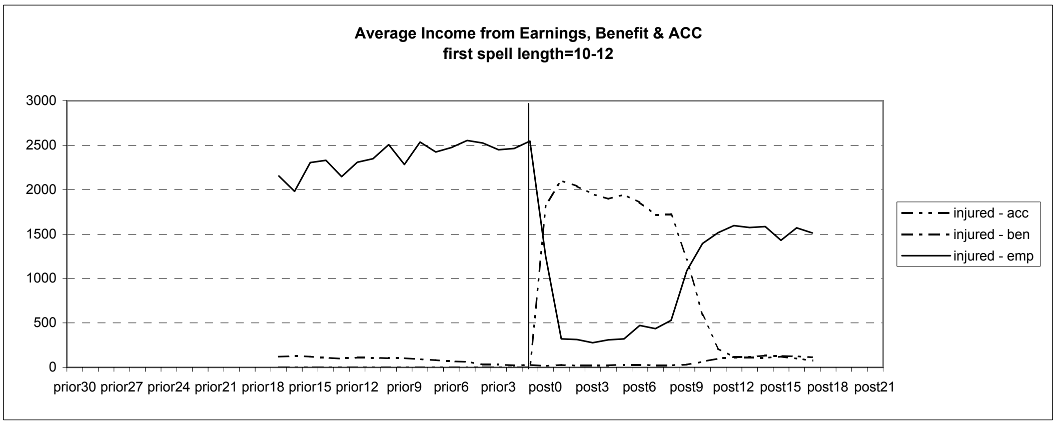
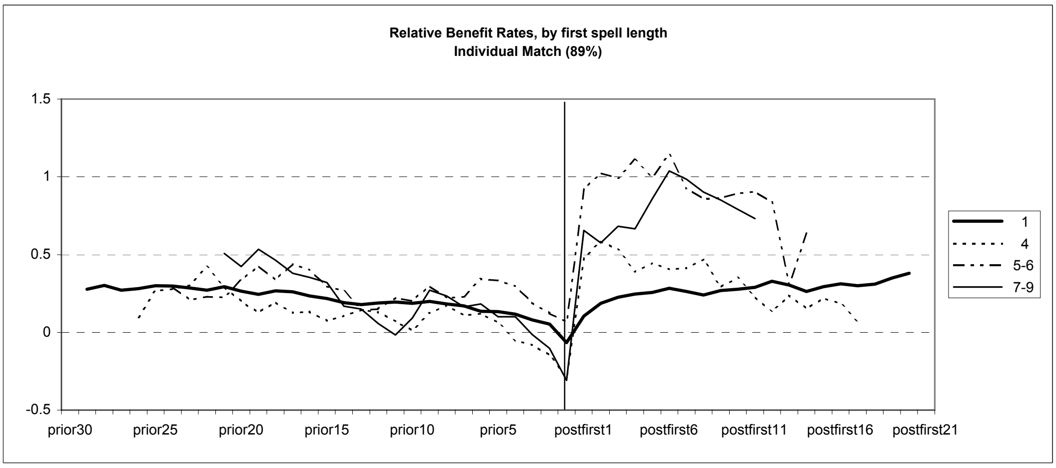



Figure 4

**Percentage Difference in the Proportion of Injured and Non-injured Employed and Receiving Benefits**
**Months prior and post the first ACC spell**

**By length of first ACC spell**



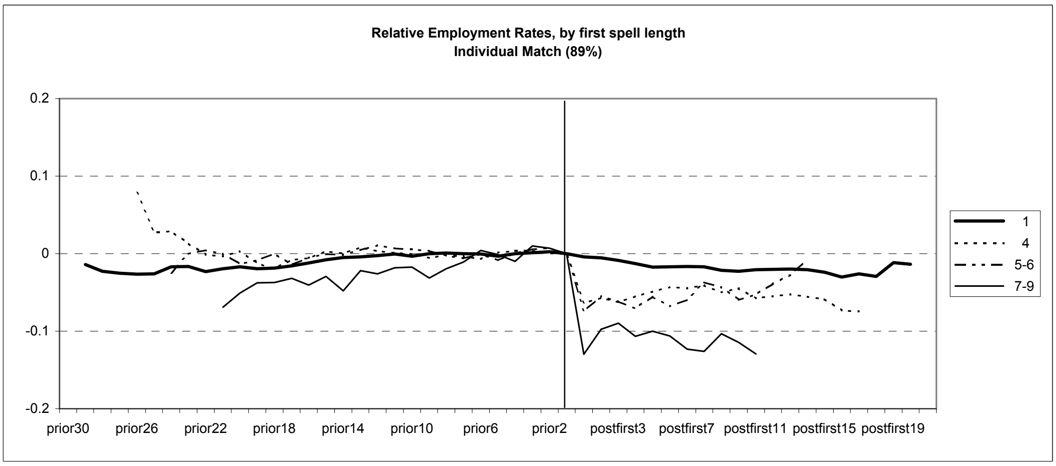
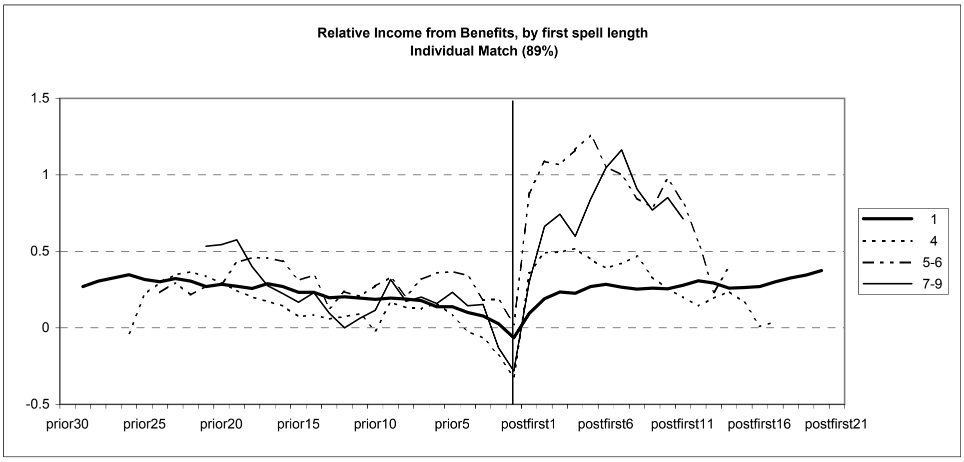



Figure 5

**Percentage Difference in Average Income from Employment and Benefits for Injured and Non-injured Months prior and post the first ACC spell**

**By length of first ACC spell**



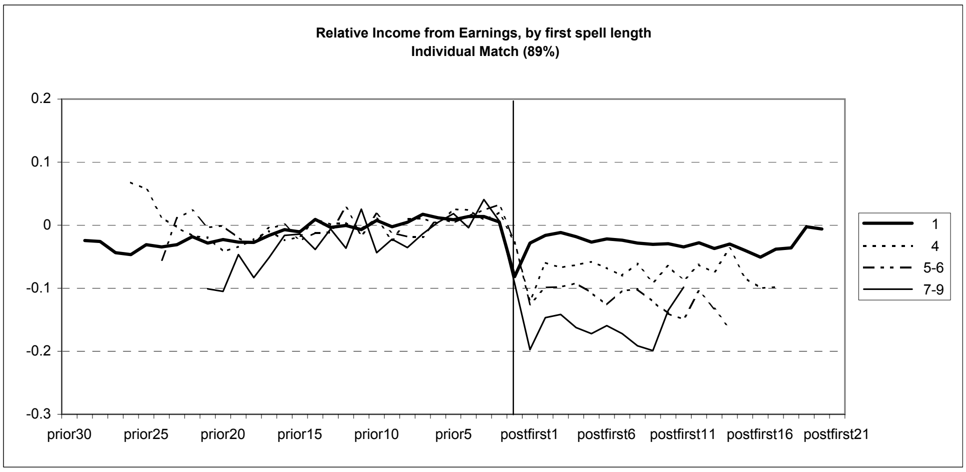
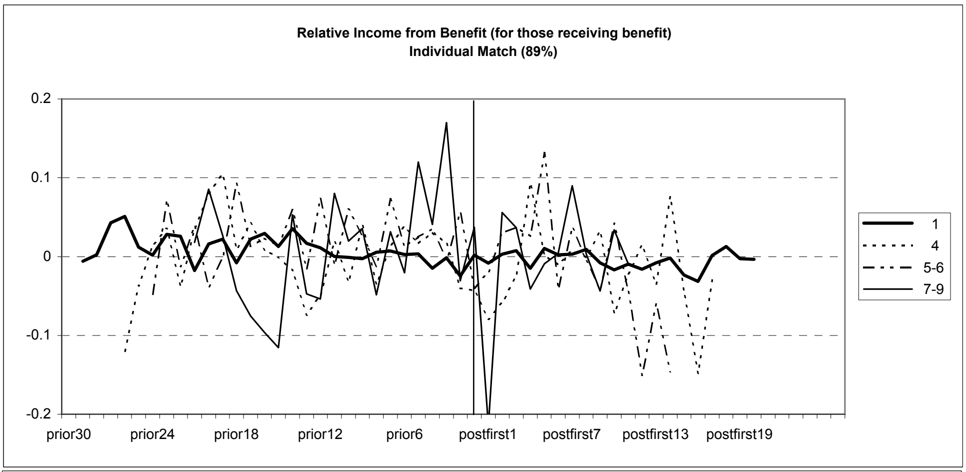



Figure 6

**Percentage Difference in Average Income from Employment and Benefits for Injured and Non-injured Months prior and post first ACC spell**

**By length of first ACC spell**



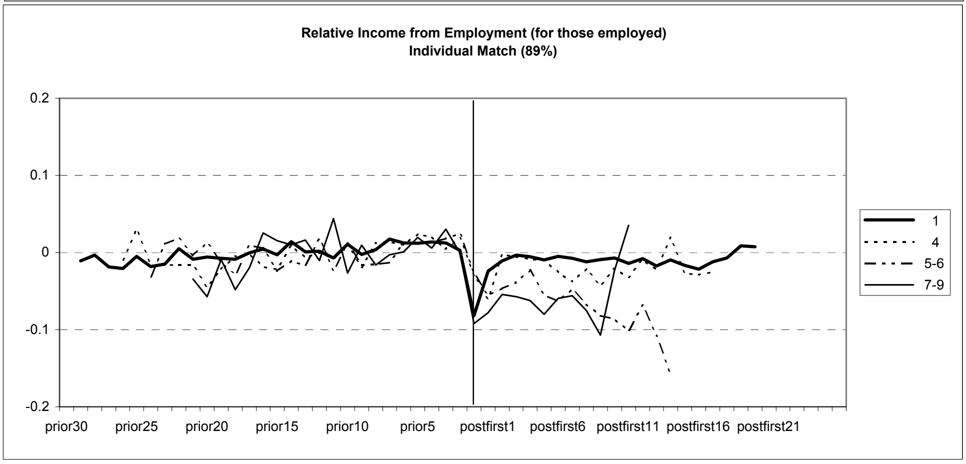
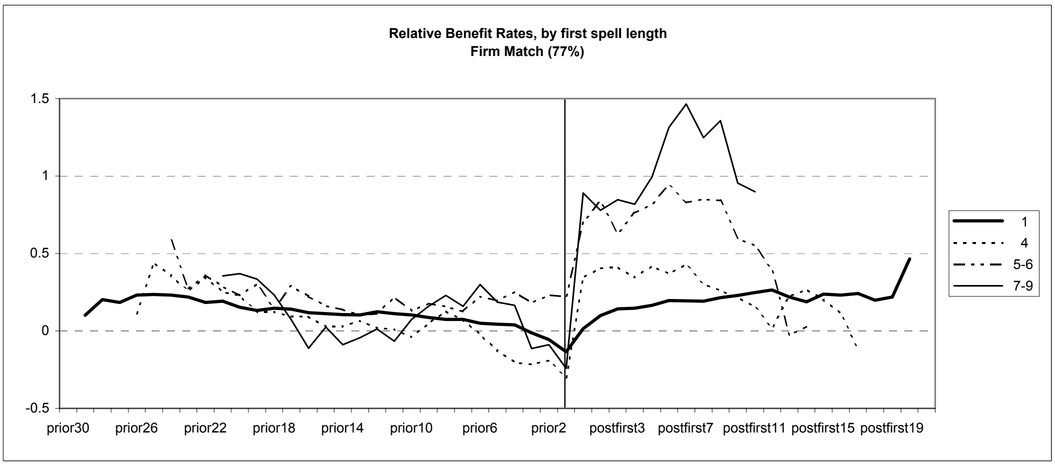



Figure 7

**Percentage Difference in the Proportion of Injured and Non-injured Employed and Receiving Benefits**
**Months prior and post the first ACC spell**

**By length of first ACC spell**



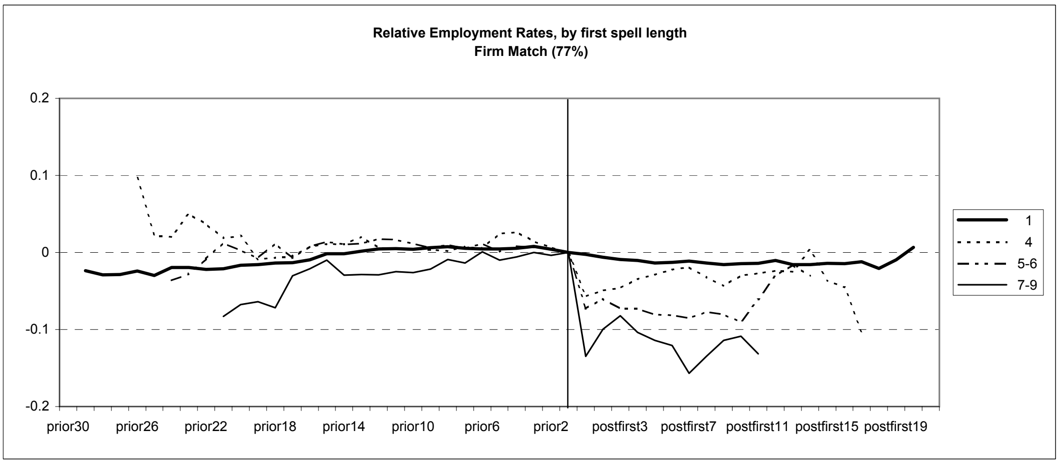
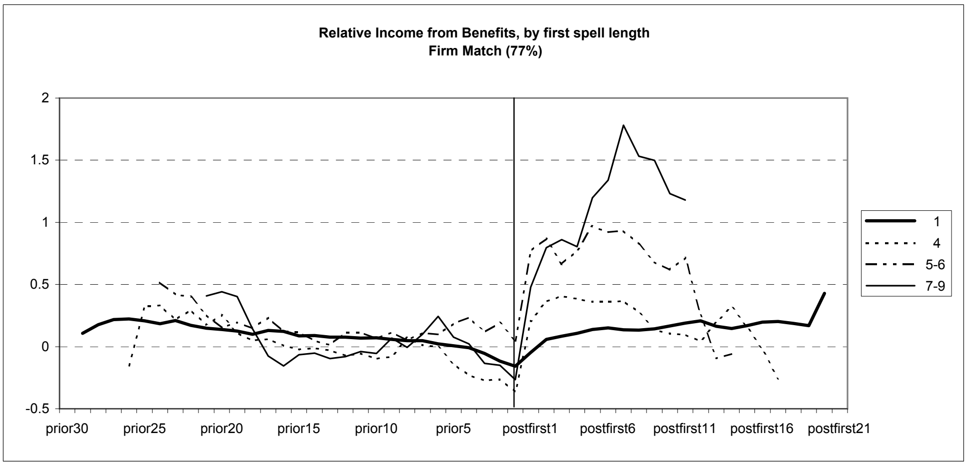



Figure 8

**Percentage Difference in Average Income from Employment and Benefits for Injured and Non-injured Months prior and post the first ACC spell**

**By length of first ACC spell**



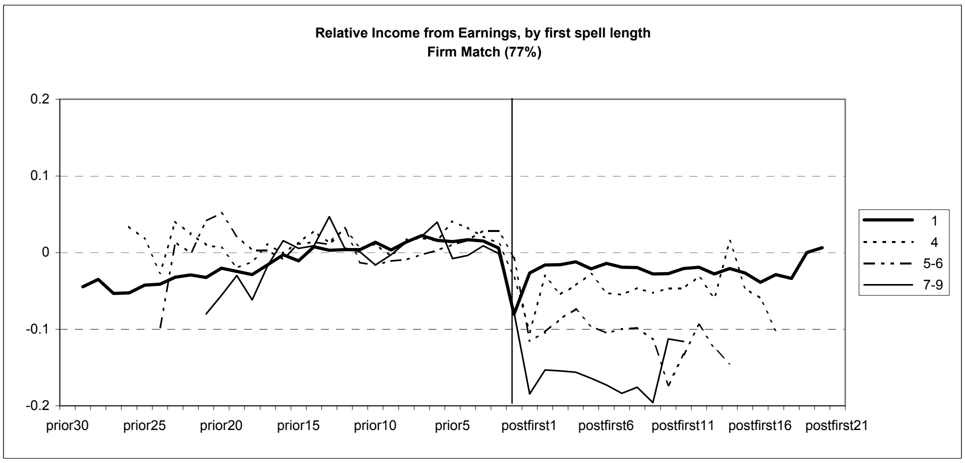
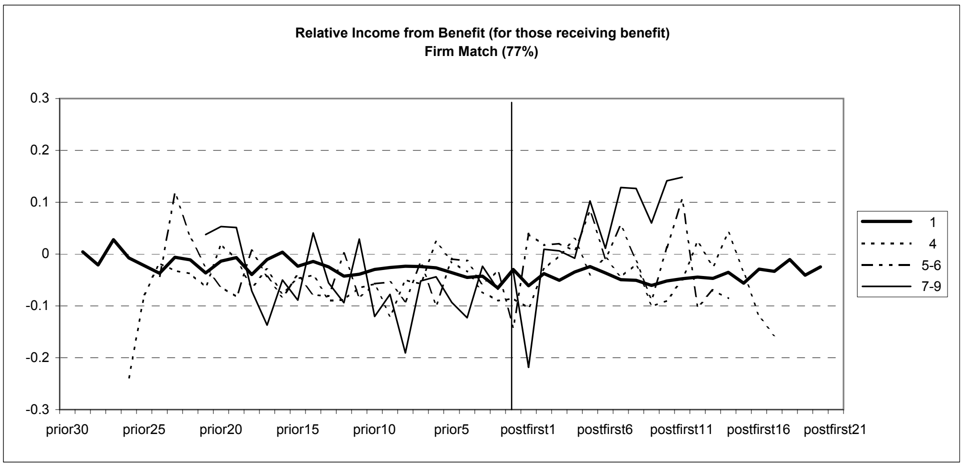



Figure 9

**Percentage Difference in Average Income from Employment and Benefits for Injured and Non-injured Months prior and post first ACC spell**

**By length of first ACC spell**



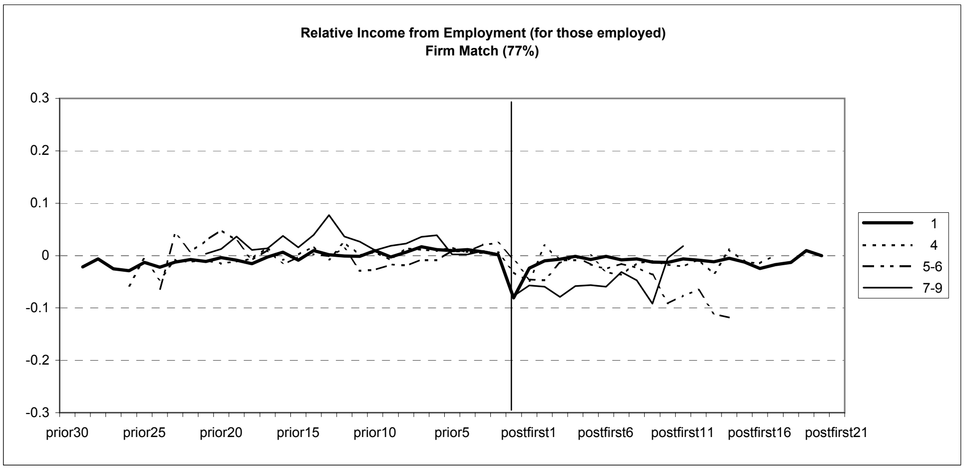
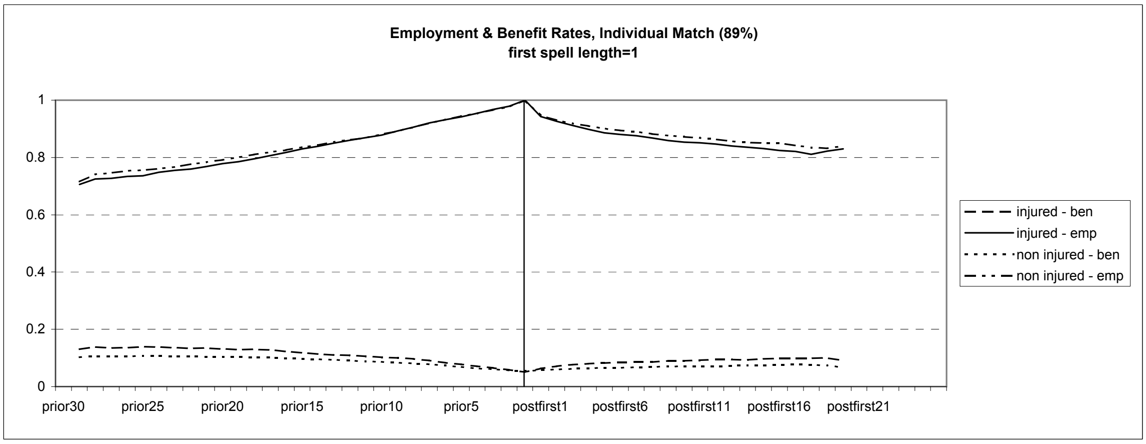
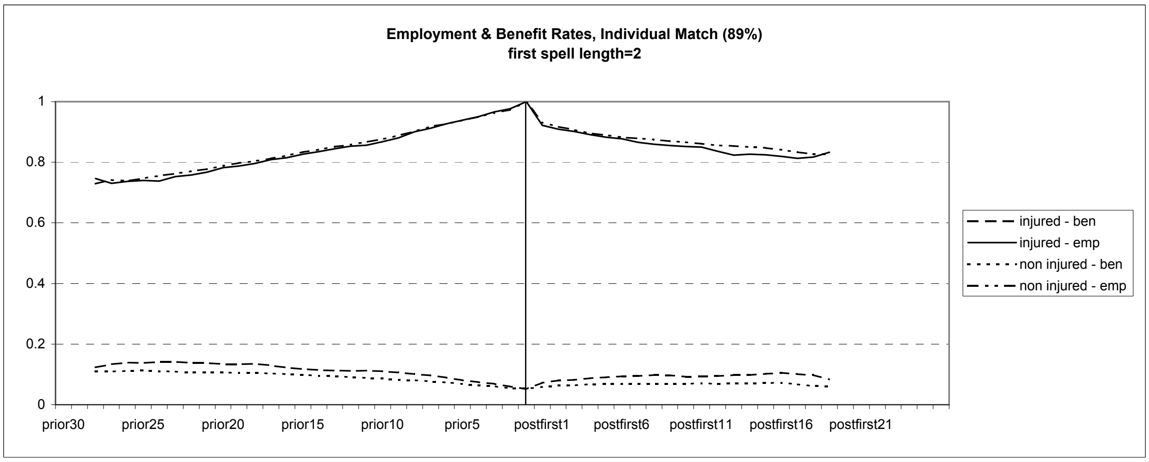



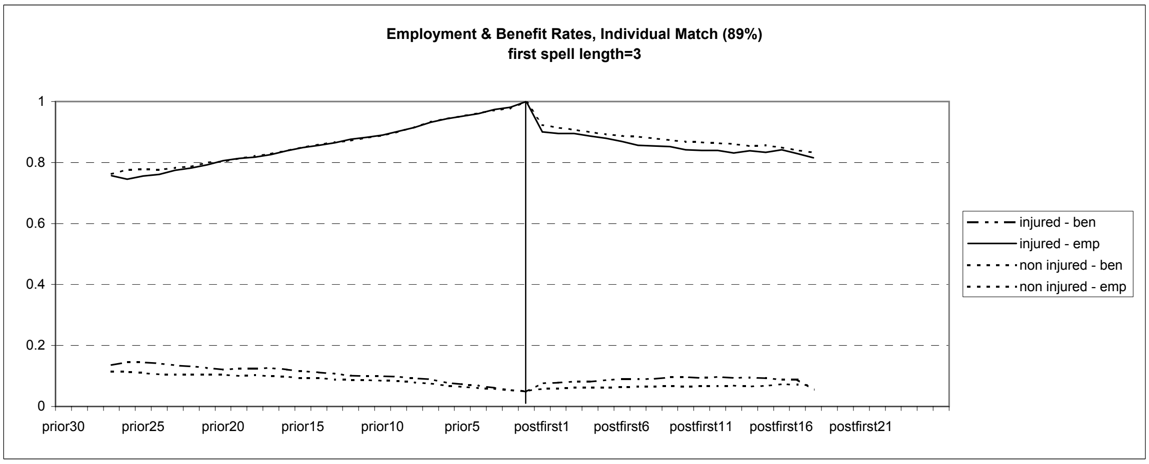
Figure A.4

**Proportion of Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post first ACC spell**

**By length of first ACC spell**



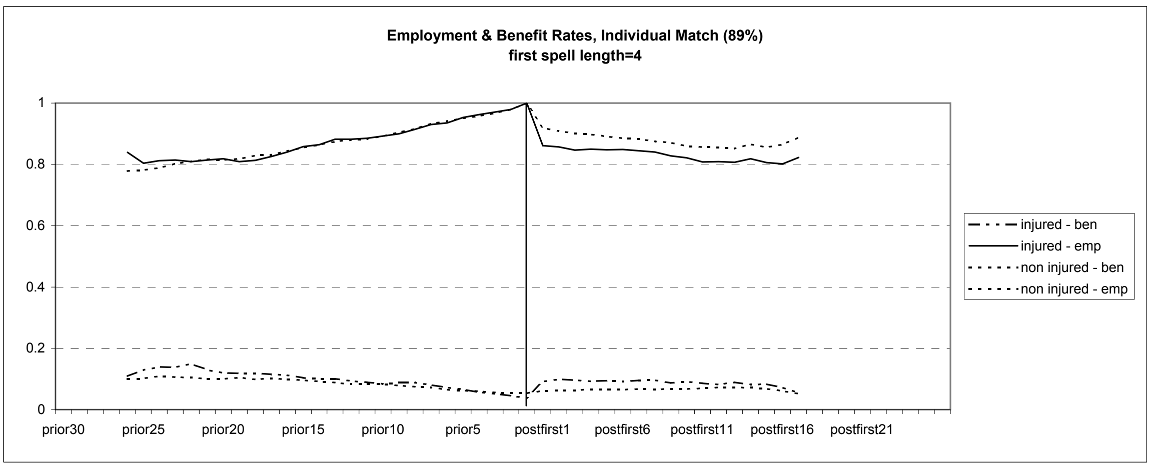



Figure A.4
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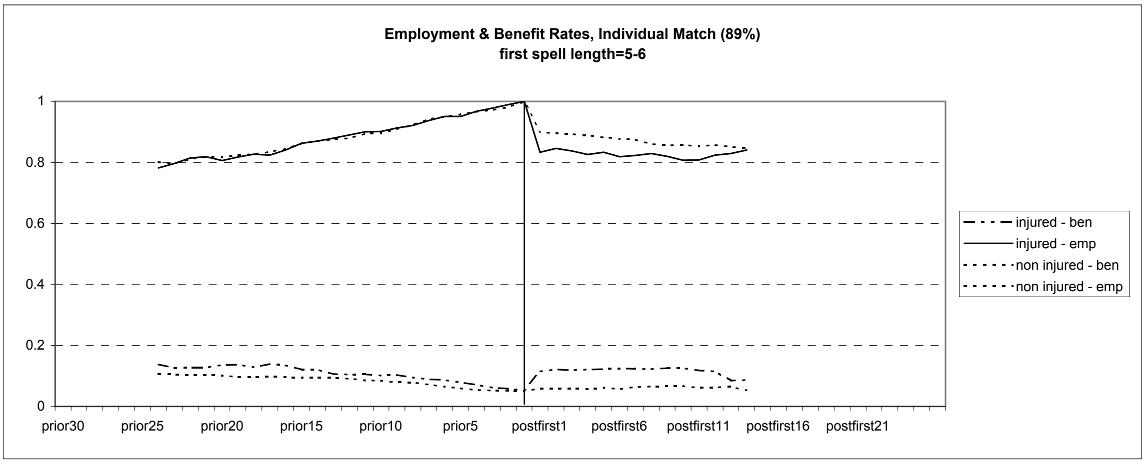
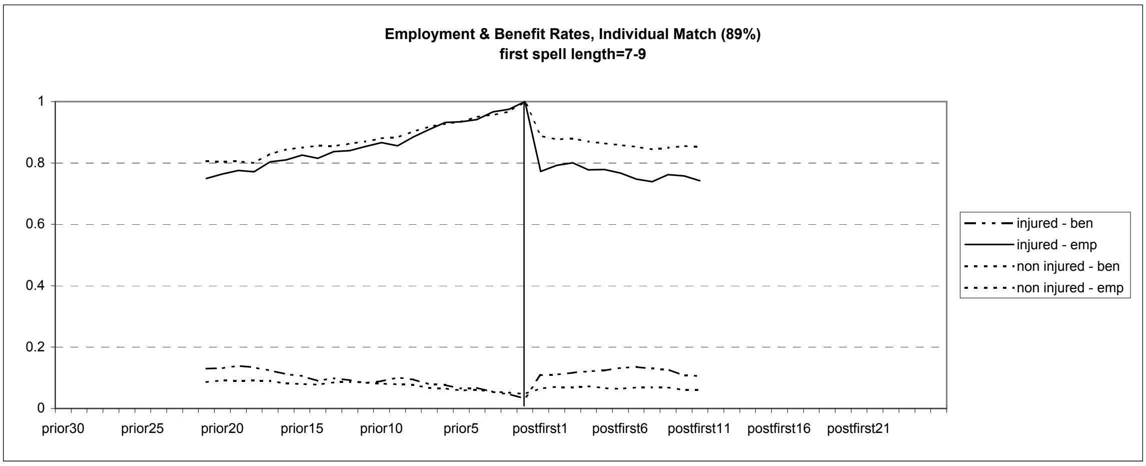
**Proportion of Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post first ACC spell**

**By length of first ACC spell**



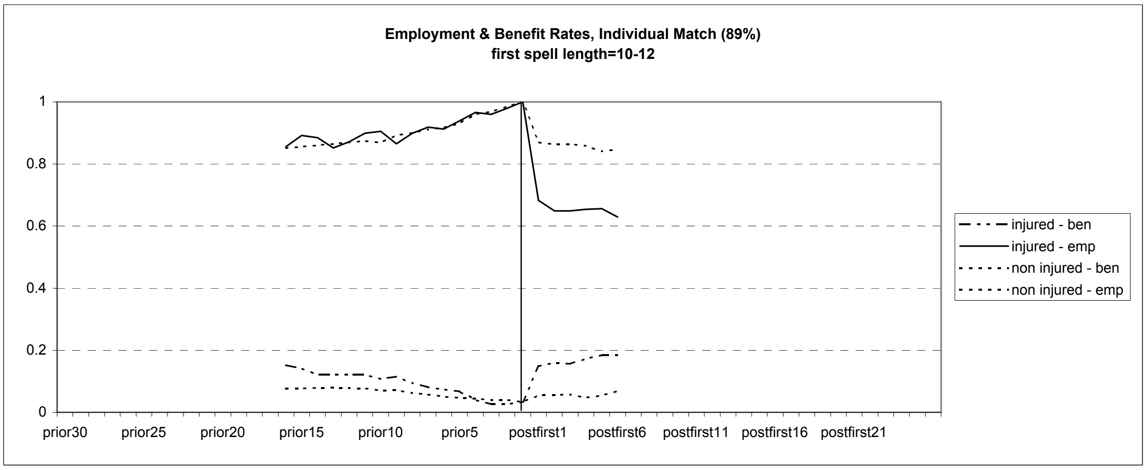
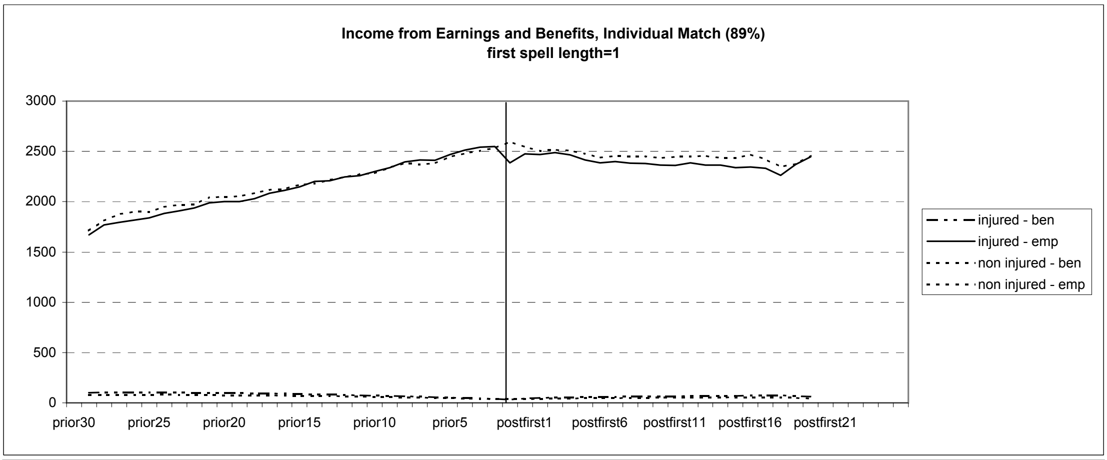
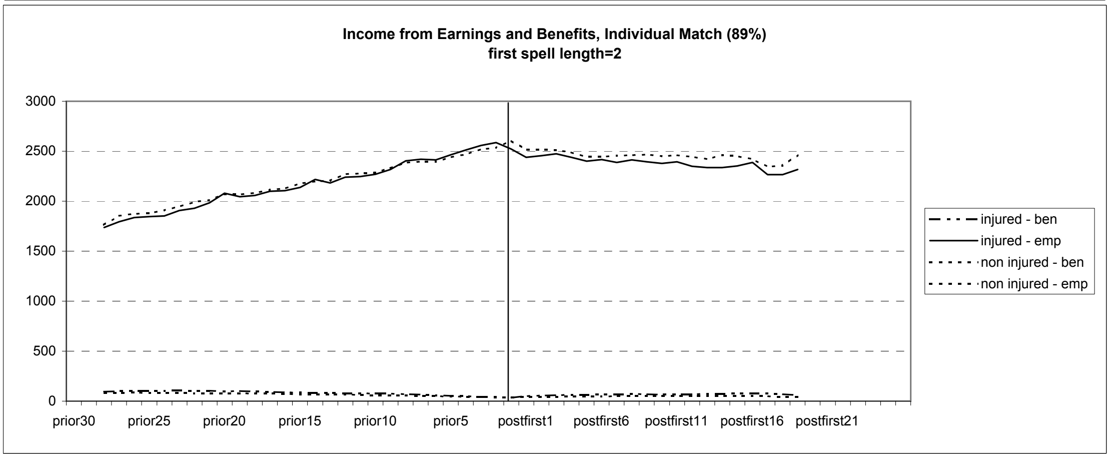
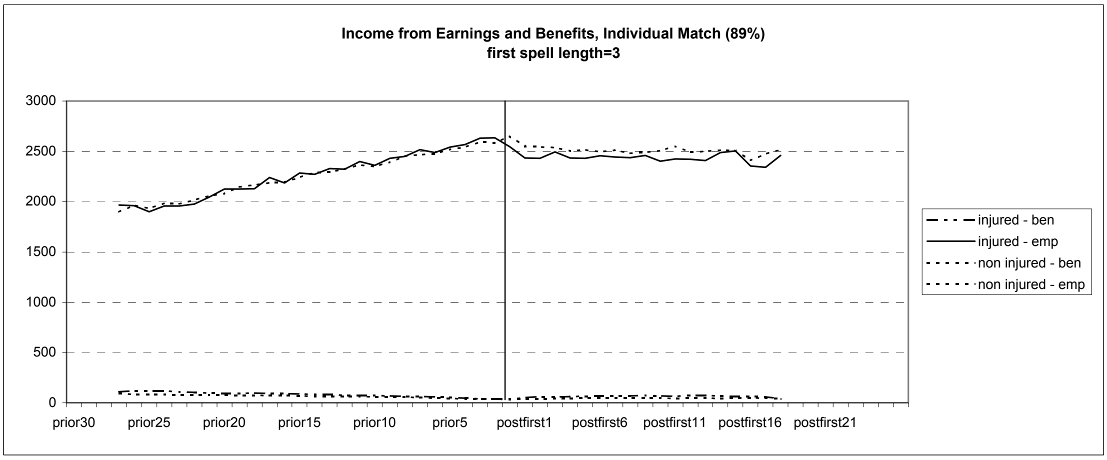



Figure A.5

**Average Income from Employment and Benefits**
**Months prior and post first ACC spell**
**By length of first ACC spell**



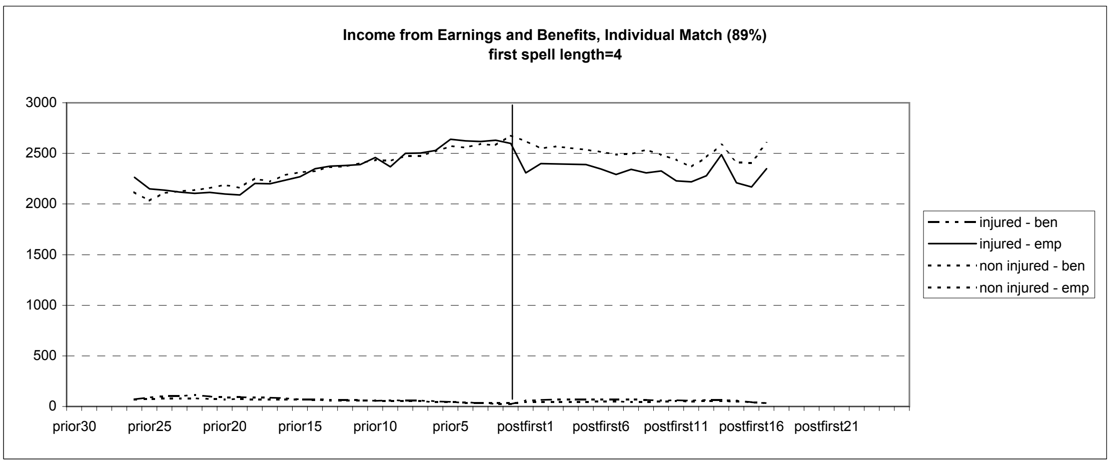



Figure A.5
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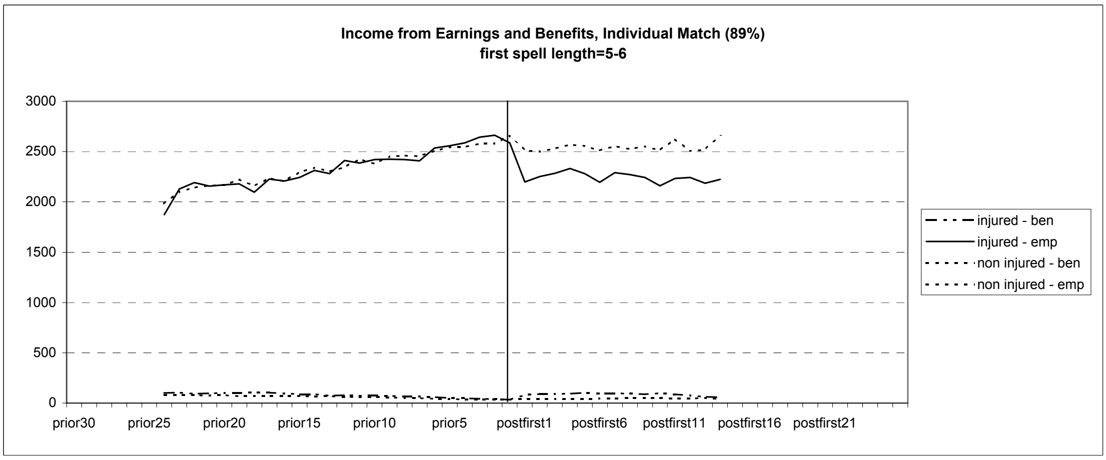
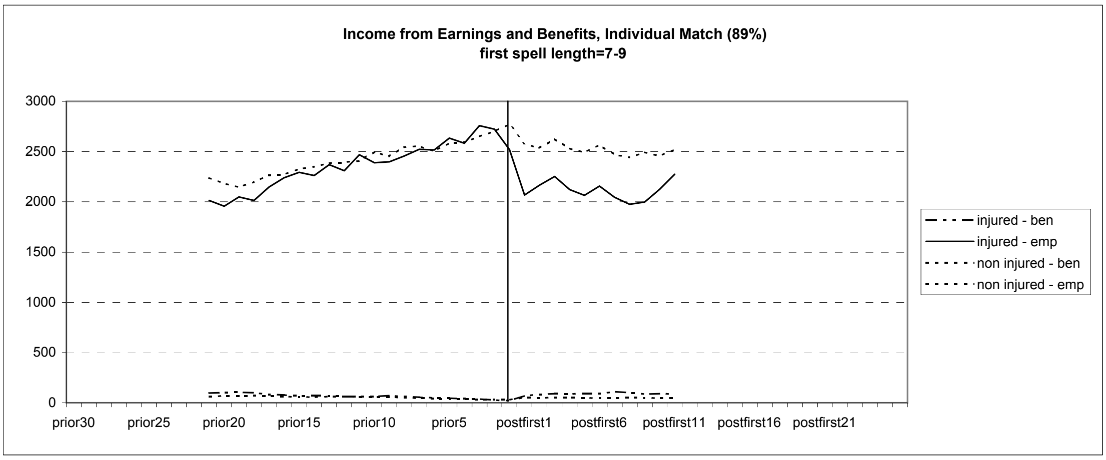
**Average Income from Employment and Benefits**
**Months prior and post first ACC spell**

**By length of first ACC spell**



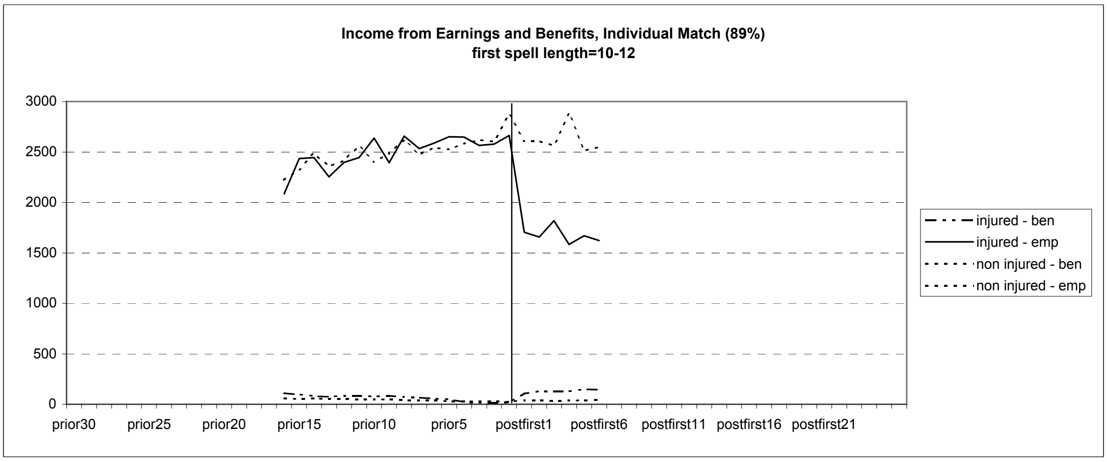
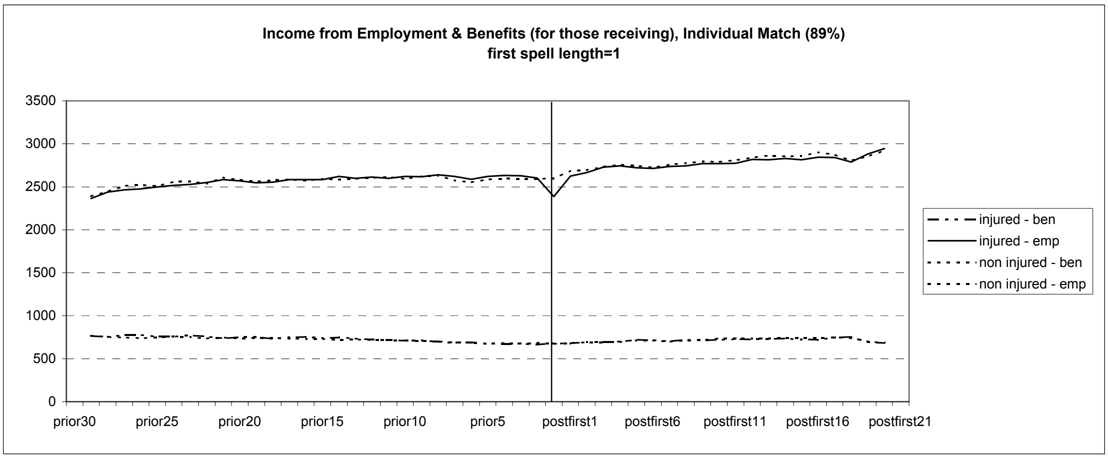
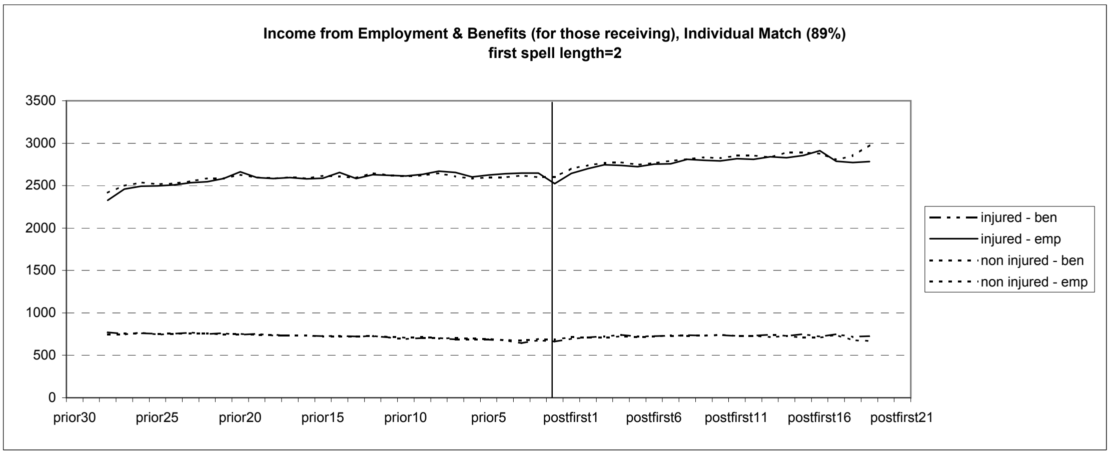



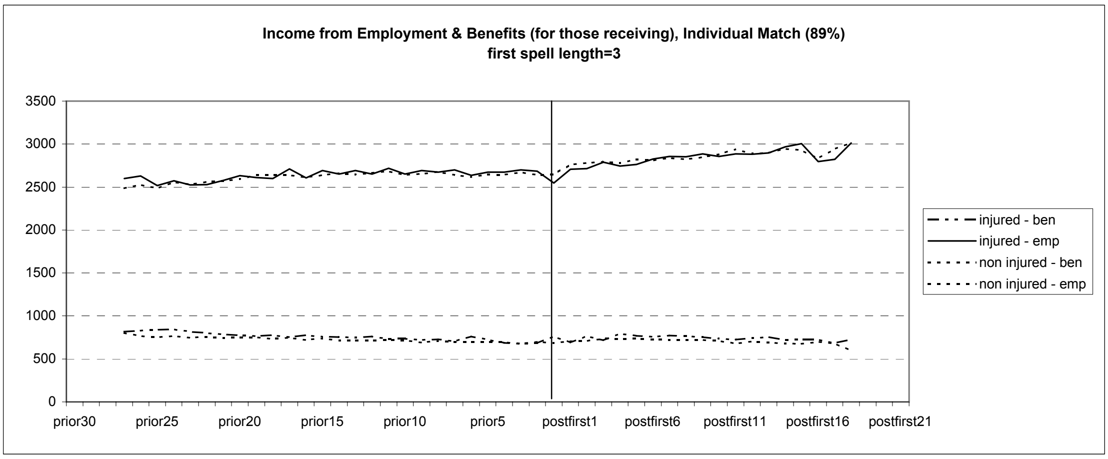
Figure A.6

**Average Income from Employment and Benefits, for those receiving income from that source**
**Months prior and post first ACC spell**

**By length of first ACC spell**



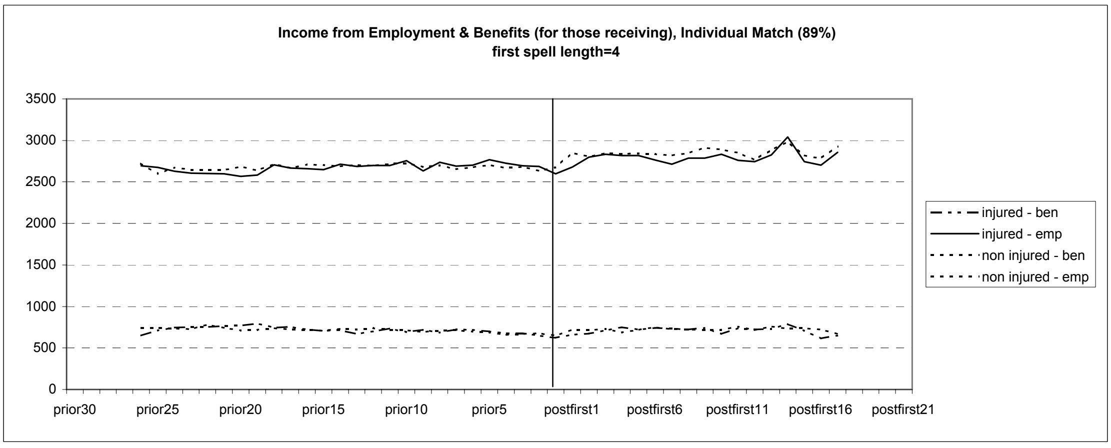



Figure A.6
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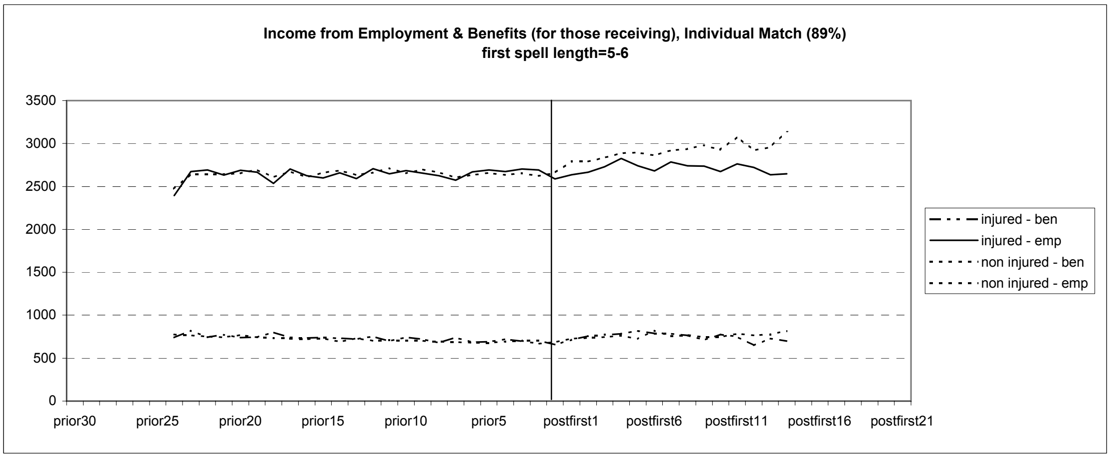
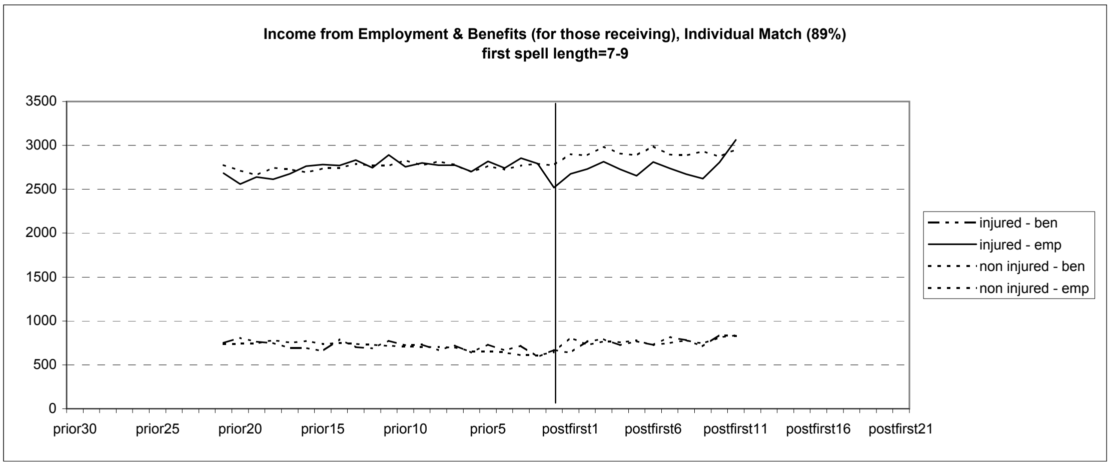
**Average Income from Employment and Benefits, for those receiving income from that source**
**Months prior and post first ACC spell**
**By length of first ACC spell**



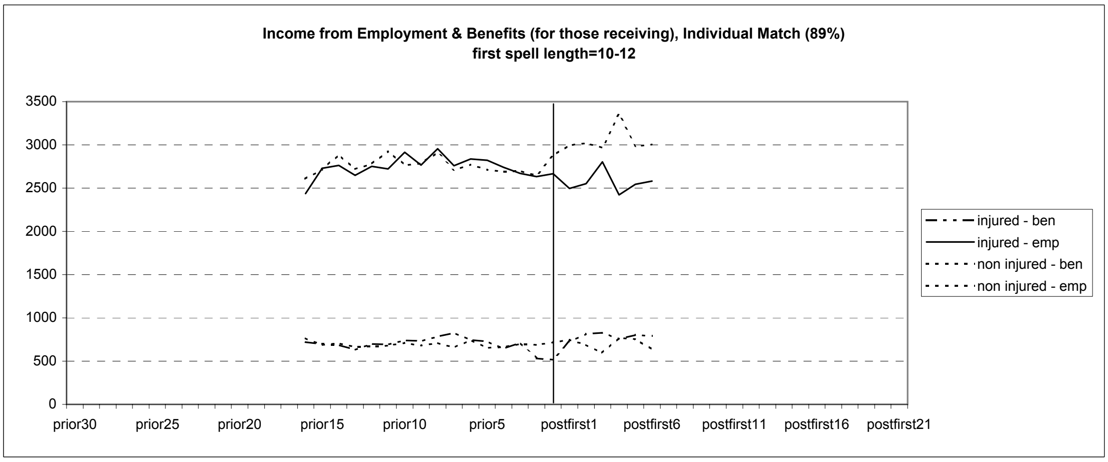
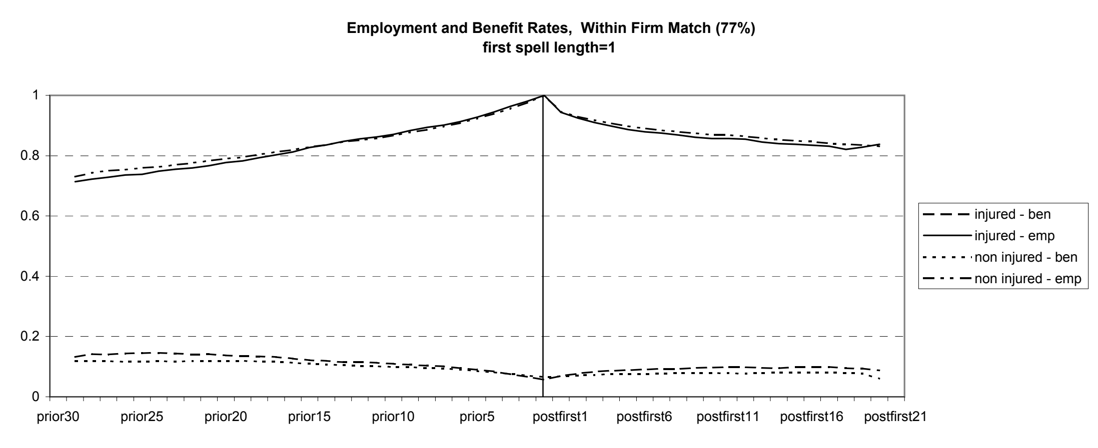
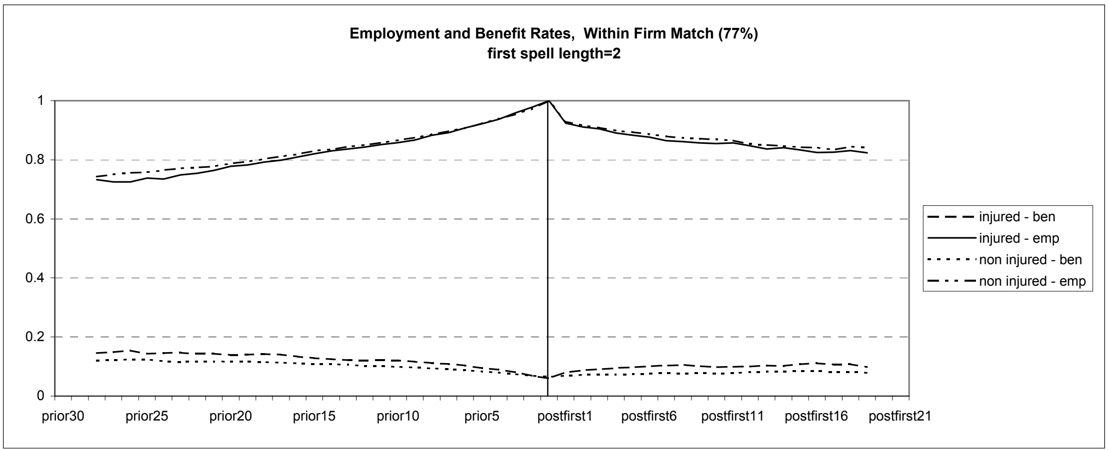



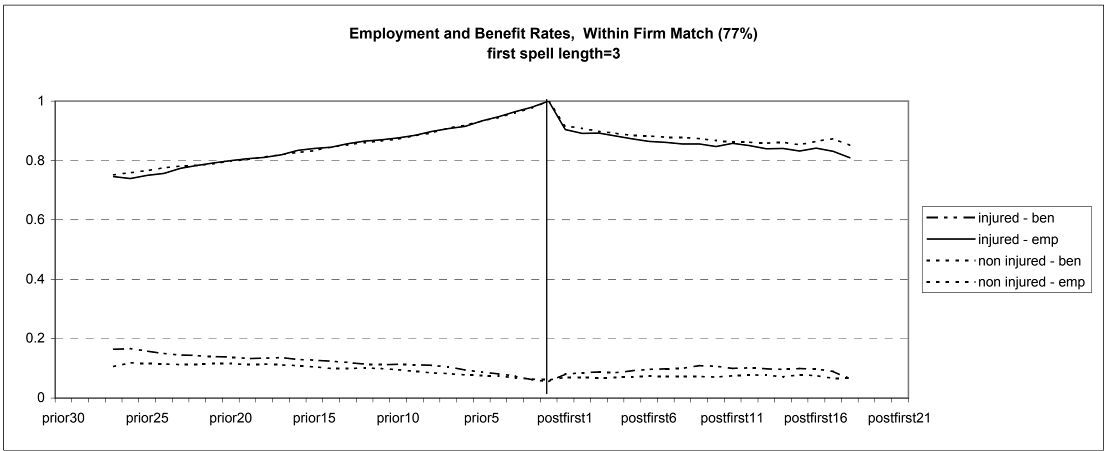
Figure A.7

**Proportion of Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post first ACC spell**

**By length of first ACC spell**



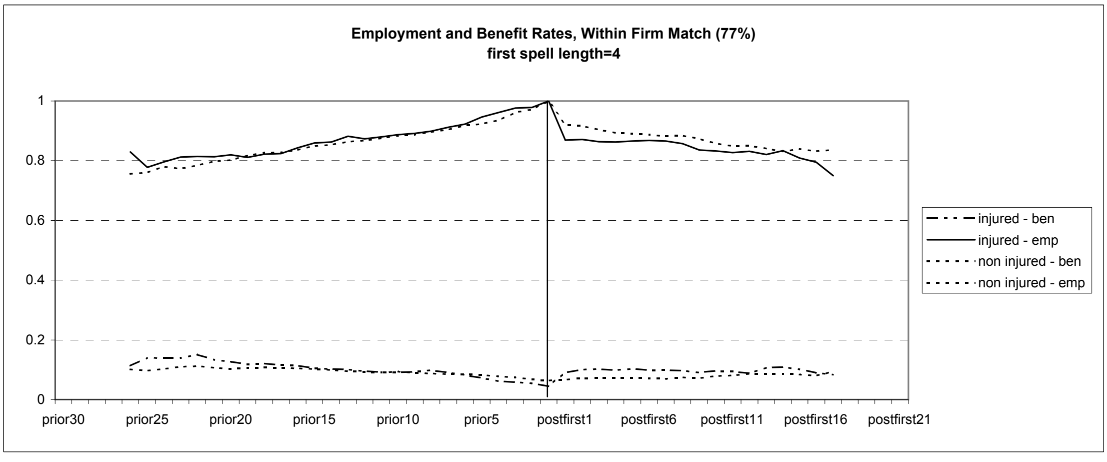
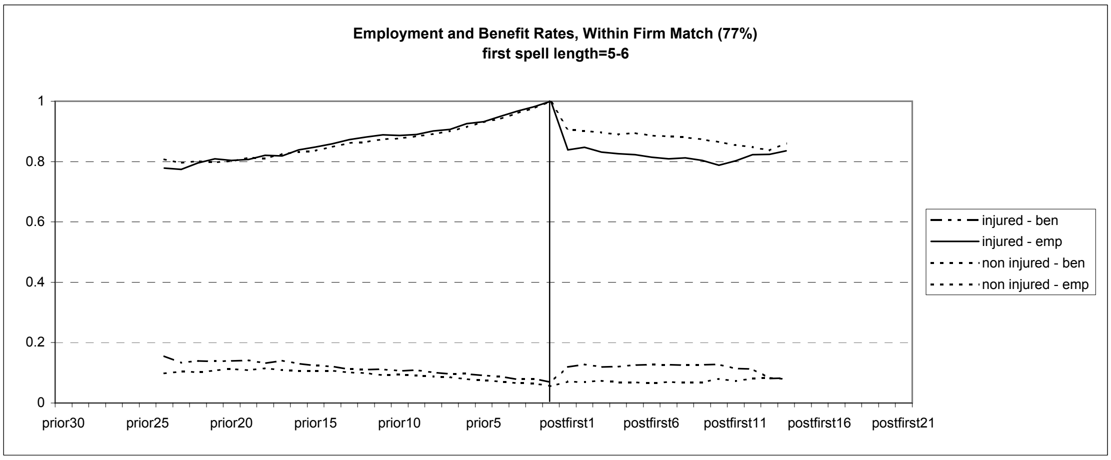
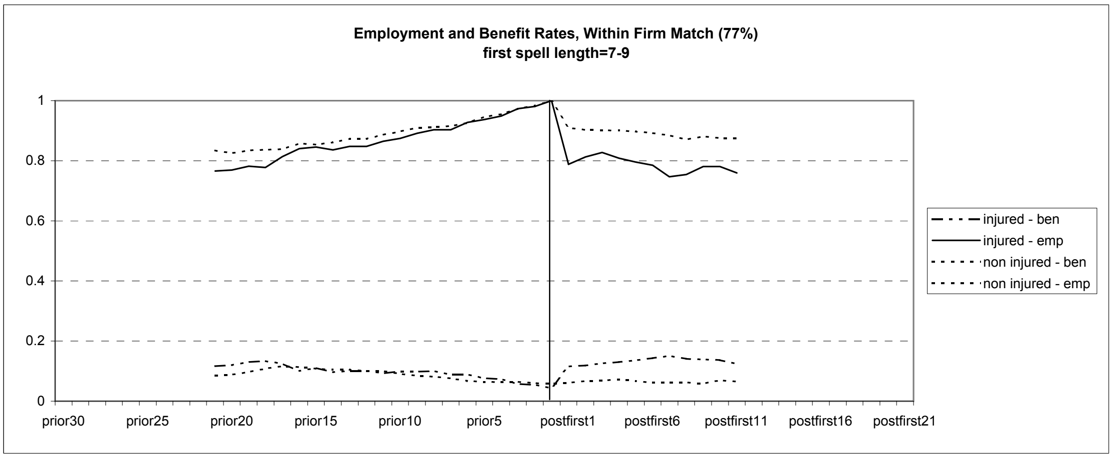



Figure A.7
*continued*

**Proportion of Workers Employed, Receiving Benefits and Receiving ACC Earnings Compensation**
**Months prior and post first ACC spell**

**By length of first ACC spell**



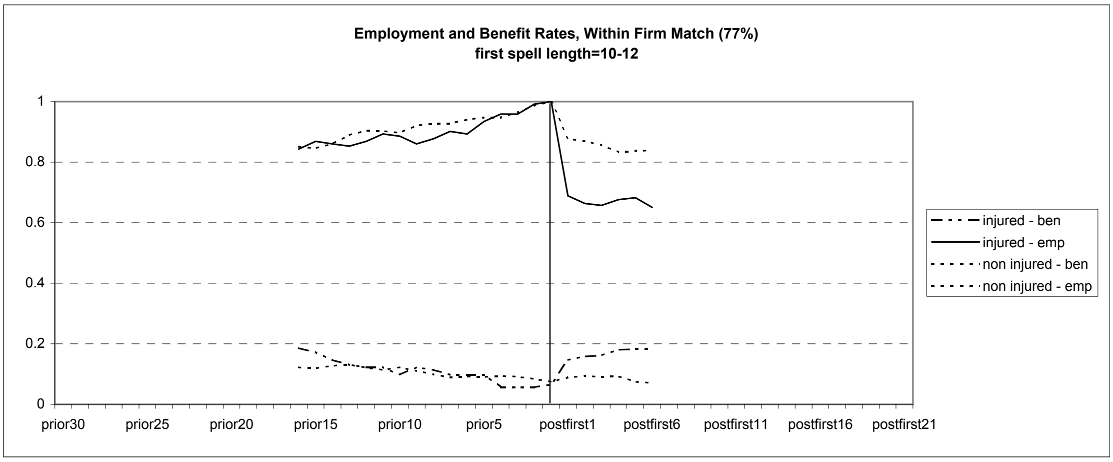
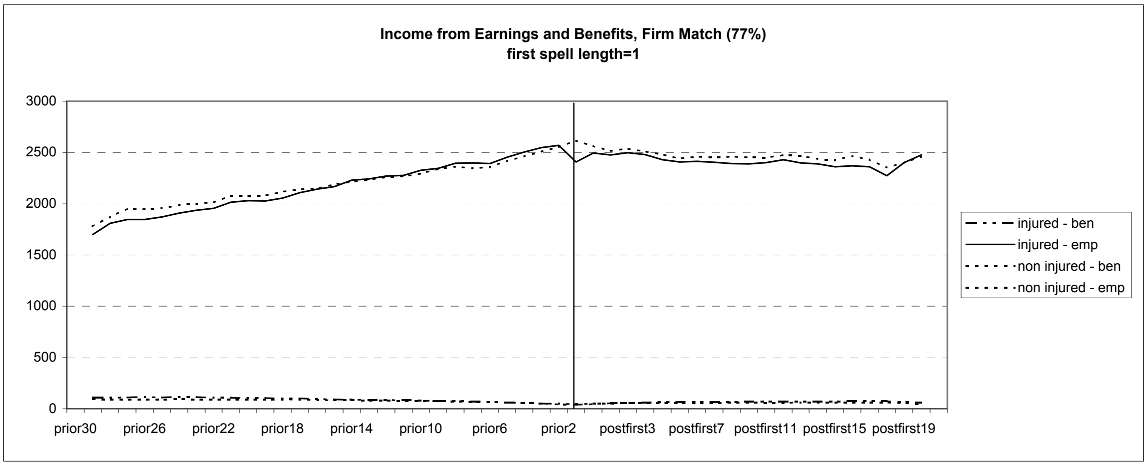
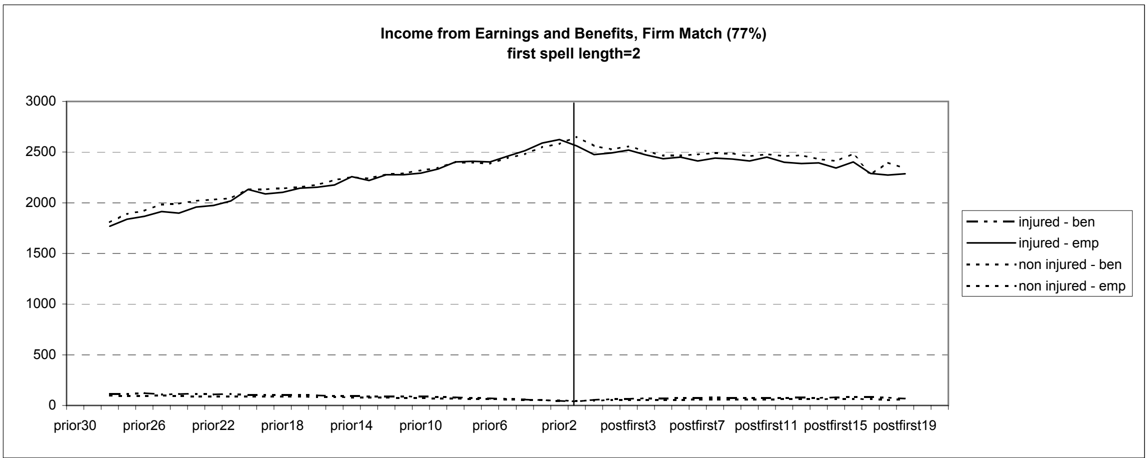



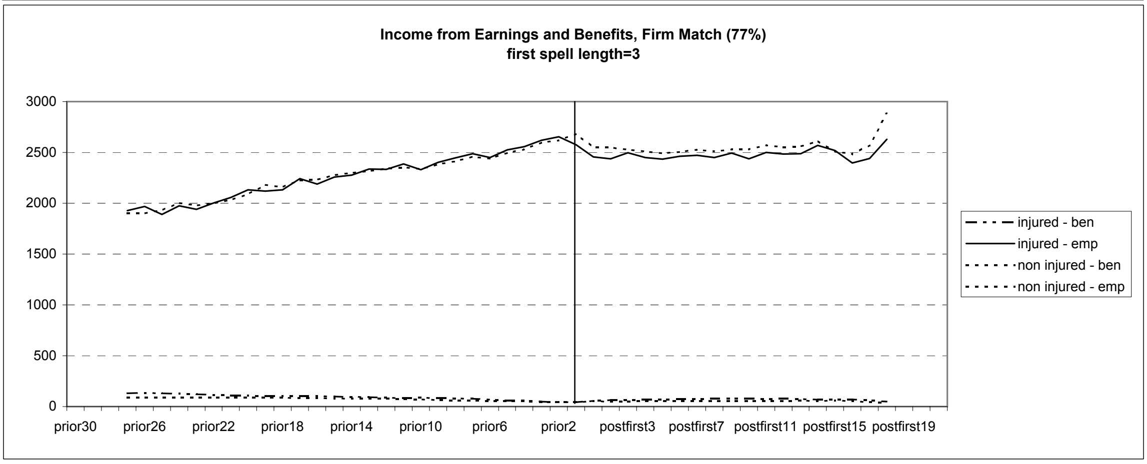
Figure A.8

**Average Income from Employment and Benefits**
**Months prior and post first ACC spell**

**By length of first ACC spell**



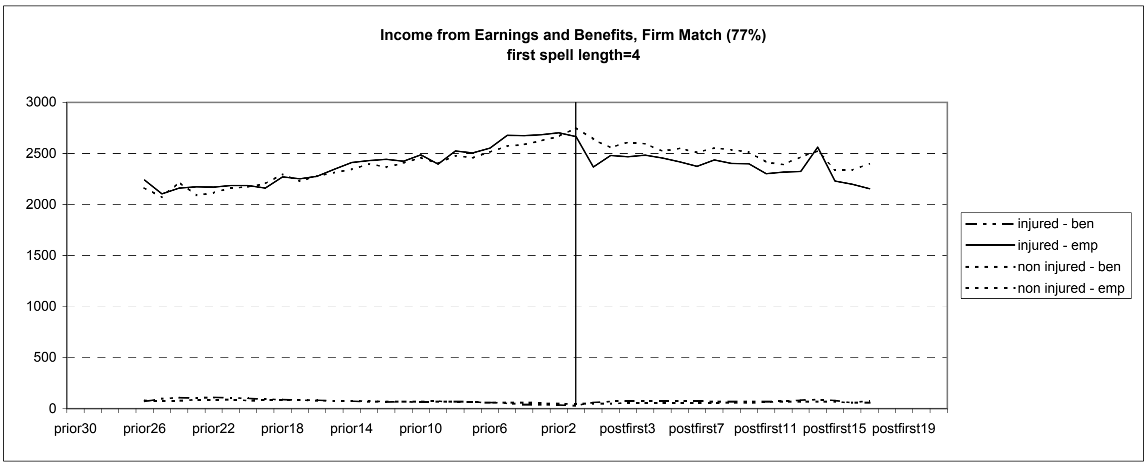
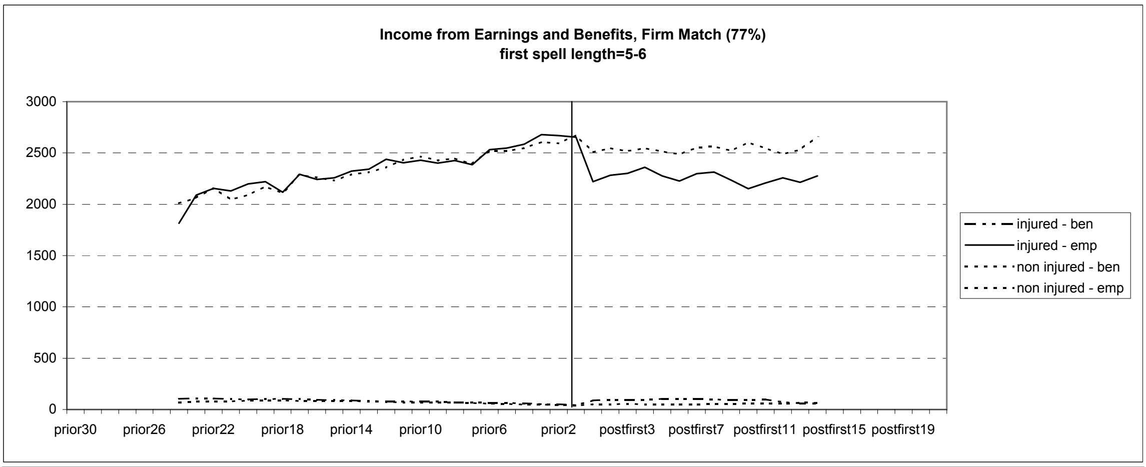
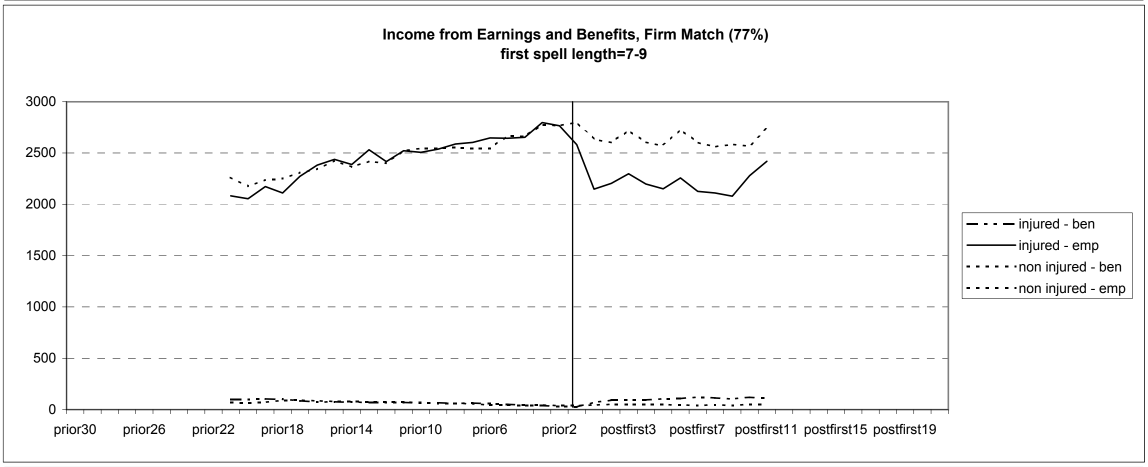



Figure A.8
*continued*

**Average Income from Employment and Benefits**
**Months prior and post first ACC spell**

**By length of first ACC spell**



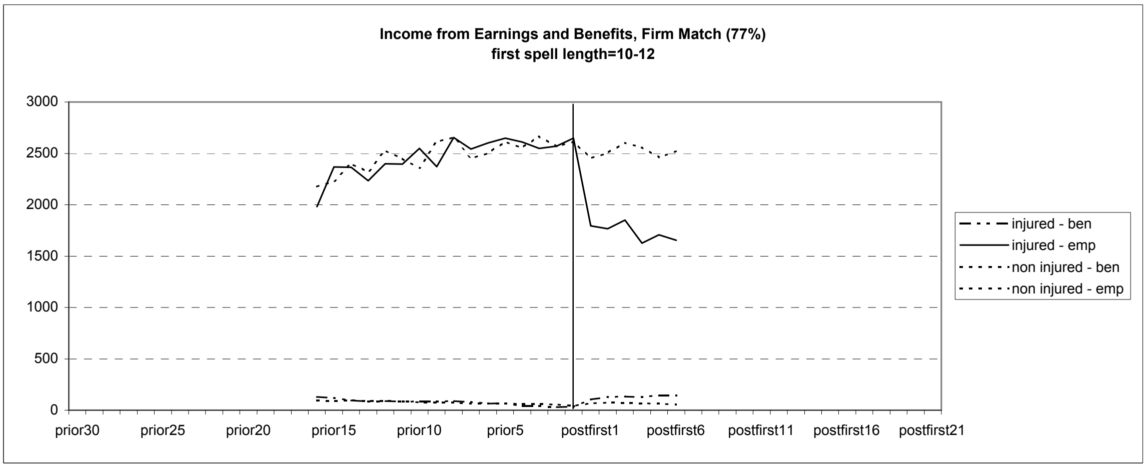
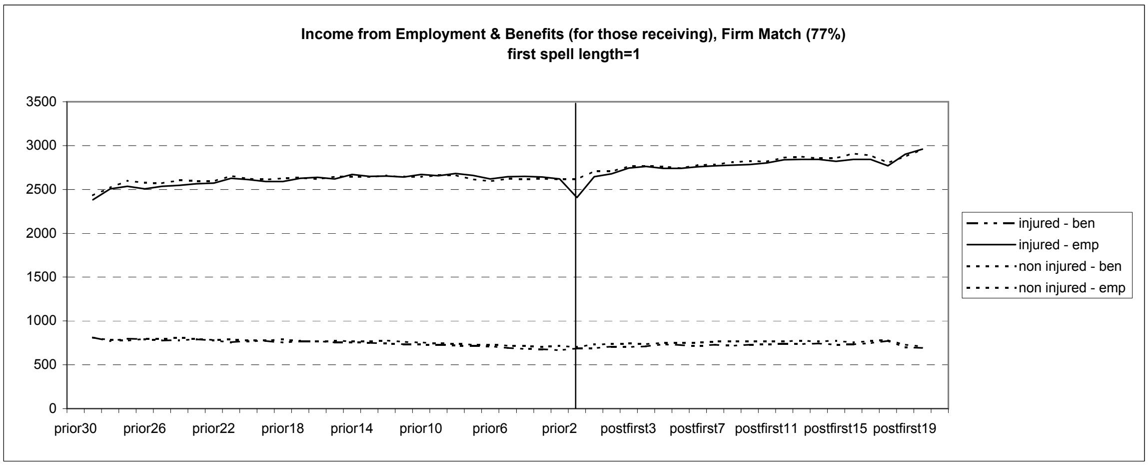
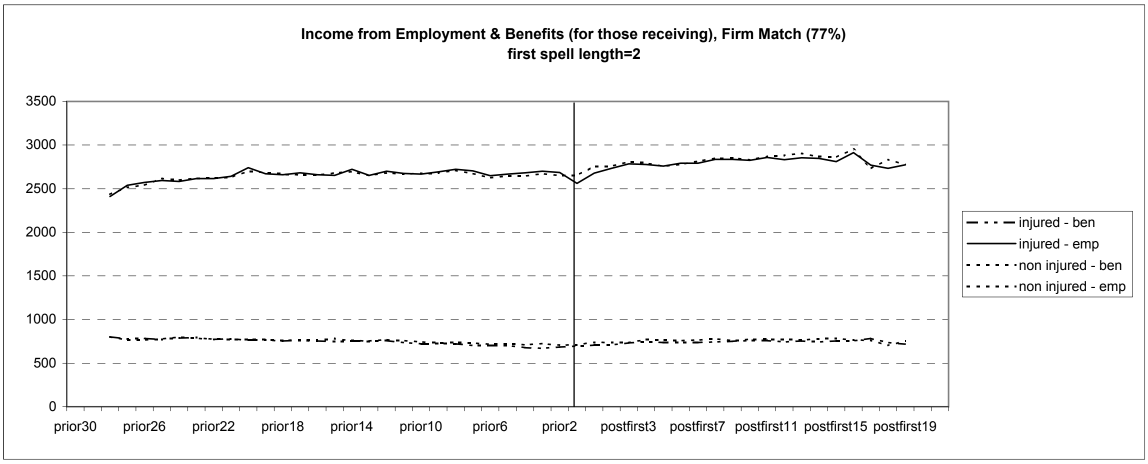



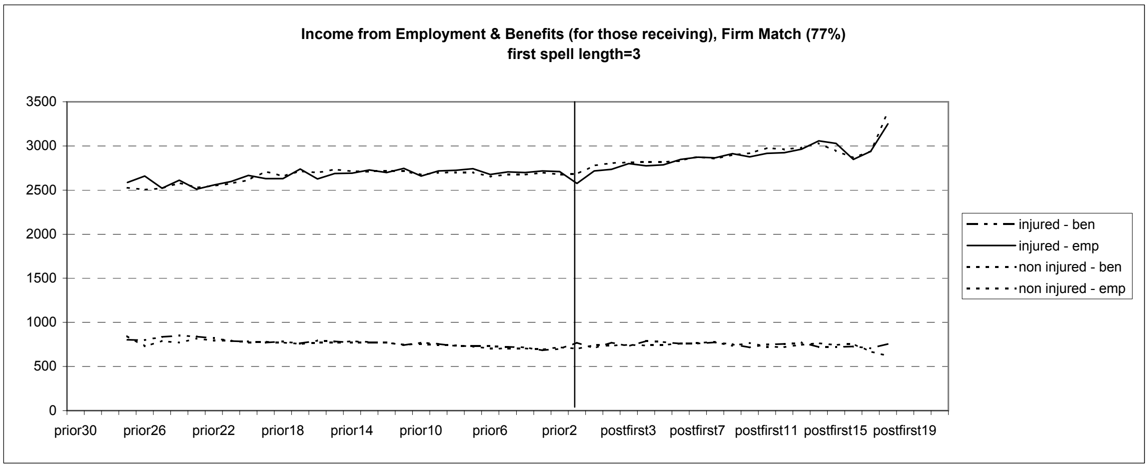
Figure A.9

**Average Income from Employment and Benefits, for those receiving income from that source**
**Months prior and post first ACC spell**

**By length of first ACC spell**



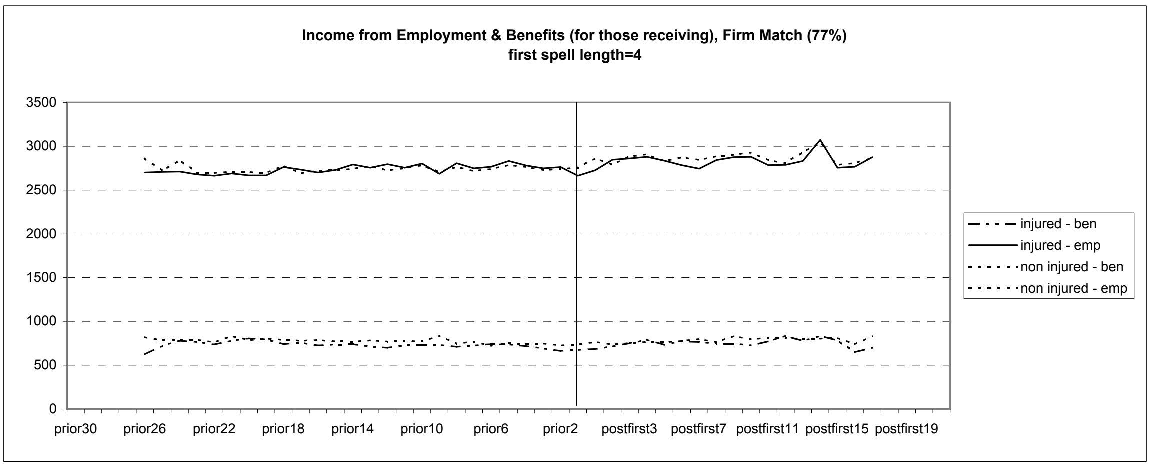



Figure A.9
*continued*

**Average Income from Employment and Benefits, for those receiving income from that source**
**Months prior and post first ACC spell**
**By length of first ACC spell**