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Youth Minimum Wage Reform and the Labour Market [*]

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Youth Minimum Wage Reform and the Labour Market

Abstract

In this paper, we analyse the effects of a large reform in the minimum wages affecting youth workers in New Zealand in 2001. Prior to this reform, a youth minimum wage, set at 60% of the adult minimum, applied to 16-19 year-olds. The reform had two components. First, it lowered the eligible age for the adult minimum wage from 20 to 18 years, and resulted in a 69 percent increase in the minimum wage for 18 and 19 year-olds. Second, the reform raised the youth minimum wage in two annual steps from 60% to 80% of the adult minimum, and resulted in a 41 percent increase in the minimum wage for 16 and 17 year-olds over a two-year period. We use data from the New Zealand Household Labour Force Survey (HLFS) to estimate the impact of these changes on a variety of outcomes including employment, hours worked, income, and educational status. We compare the average outcomes of 16-17 and 18-19 year-olds, before and after the policy reform, to those of 20-25 year-olds, who were unaffected by the reform. The results provide no evidence of any adverse effect of the minimum wage changes on youth employment or hours worked. In fact, we find surprisingly little impact of the minimum wage change for 18-19 year-olds, while the results suggest that 16-17 year-olds' hours worked increased by 10-15 percent relative to the other age groups following the minimum wage increase. We also find that labour earnings and incomes increased for both affected groups relative to other young adults, and find some evidence that study rates decreased and inactivity increased for teenagers relative to young adults.

Keywords: minimum wage, New Zealand, natural experiment, difference-in-differences

JEL CODE: J38, J22, J23, J24

I Introduction

Until recently, it had been widely believed by economists that the imposition of a binding wage floor, e.g. minimum wage, would reduce the employment of younger and less-skilled workers. Both simple theoretical models of competitive labour markets and time-series evidence on the relationship between minimum wages and youth employment supported this consensus (Brown, Gilroy and Kohen 1982). However, recent empirical research relying on quasi-experimental evaluations of cross-sectional and longitudinal data, in particular Card (1992) and Card and Krueger (1994), has failed to find negative employment effects for young or low-wage workers in the United States. The inherent complications with quasi-experimental evaluations have led some researchers to question the results in these papers (Burkhauser, Couch and Wittenburg 2000; Neumark and Wascher 2000).[1] Still, it seems safe to say that the consensus has been broken and that the empirical evidence indicates, in certain situations, an increase in the minimum wage may not reduce employment.

In this paper, we analyse the effects of a large reform in the minimum wages affecting teenage workers that occurred in New Zealand in 2001. Prior to this change there was an adult minimum wage that applied to workers over the age of 20, and a youth minimum wage, set at 60% of the adult minimum, that applied to 16-19 year-old workers. The 2001 reform involved two components: first, it lowered the eligibility age for the adult minimum wage to 18, resulting in a 69 percent increase in the minimum wage for 18 and 19 year-olds; and second, the youth minimum wage was raised in two annual steps to 80% of the adult minimum, resulting in a 41 percent increase in the minimum wage for 16 and 17 year-olds over this two year period.[2]

These large and focused changes provide an ideal opportunity for studying the effects of minimum wage policy on the youth labour market. Using data from the New Zealand Household Labour Force Survey (HLFS) for the period 1997—2002, we examine changes in the labour market experiences of the two groups of teenagers that are directly affected by the reform and compare these with the changes experienced by young adults, aged between 20 and 25. We focus primarily on the

[1] See Card and Krueger (2000) for a reply to Neumark and Wascher's comment on their prior paper.
[2] To emphasize the magnitude of these changes, note that the *large* minimum wage increases analysed by Card (1992) and by Card and Krueger (1994) were 27 percent and 19 percent increases, respectively.

impact of the policy reform on employment and the hours worked by teenage workers, but also examine its impact on a variety of related outcomes: namely educational status, unemployment, inactivity (defined as neither employed nor studying), benefit receipt, labour earnings, and total income.

Our initial analysis uses cross-sectional data to estimate simple difference-in-differences models that compare the average employment and hours worked for both 16-17 and 18-19 year-olds relative to those of 20-25 year-olds before and after the policy reform. We then extend this approach using regression analysis to control for observable characteristics of the various age groups that may differ. These analyses provide no evidence of any adverse effects of the minimum wage changes on youth employment or hours worked. In fact, we find surprisingly little effect of the minimum wage change for 18-19 year-olds on these outcomes and the results suggest that 16-17 year-olds' employment rate did not change while, contrary to standard economic predictions, their hours worked increased by 10-15 percent relative to the other age groups following the minimum wage increase. However, we do find some evidence of adverse effects for both age groups in the form of a decline in educational enrollment and an increase in inactivity. Not surprisingly, given the absence of any adverse employment or hours worked effects, we find a significant increase in labour earnings for both affected groups relative to young adults and also an increase in total income for 16-17 year-olds.

The sampling frame of the HLFS consists of rolling 8-quarter panels that enables individuals to be linked across multiple surveys if they remain in the same sample dwelling. Although our analysis concentrates on the cross-sectional data, the use of this longitudinal sample enables us to control for individual heterogeneity in the outcomes of interest.[3] Using this data we examine, using fixed effects models, the effects of the policy changes on individuals observed in two consecutive years. The evidence from this analysis is largely consistent with our findings from the cross-sectional analysis, although relatively small sample sizes means these estimates are relatively imprecise.

[3] The disadvantage in using the longitudinal sample is that high levels of sample attrition lead to a large decrease in sample size and a possible reduction in sample representativeness.

II Background

New Zealand's Minimum Wage Legislation

A statutory minimum wage was codified into law in New Zealand in 1983 by the passage of the Minimum Wage Act 1983. This act set a binding wage floor for all workers 20 years or older with the only exemptions being for workers who undertake a set level of training and for disabled workers who are employed in approved sheltered workshops.[4] This legislation decreed that the minimum wage rate must be reviewed each year by December 31st. Any changes in the minimum are then typically implemented in the following March. In the 1980s, legal floors were also set on market wages in all industries covered by union collective bargaining awards and these were typically set higher than the minimum wage. This system was abolished in 1991 under the Employment Contracts Act, and the national minimum wage became the only legal wage floor. In March 1994, a youth minimum wage, set at 60% of the adult minimum, was introduced for 16-19 year-olds.

A general election was held in November 1999 and won by the center-left (a coalition government of the Labour Party and the Alliance). The Alliance campaigned to improve the labour market outcomes of youth workers, and viewed an increase in the youth minimum wage as critical for such an improvement. Following the 1999 annual review of the minimum wage, the intention to review the youth minimum wage was announced. In March 2000, a preliminary decision was made to lower the eligibility age for the adult minimum wage from 20 to 18, and to increase the youth minimum wage from 60% to 80% of the adult minimum, with both changes taking effect in July 2000. The intent and details of these proposed changes were announced in a speech by government ministers on 4 April, and concerned individuals were given 10 days to comment on the proposal. A decision was subsequently made to postpone these changes until the next minimum wage review was implemented in March 2001, and to increase the youth minimum in two annual steps from 60% to 70% of the adult minimum in the first, and 70% to 80% in the second. This final decision was formally announced on 14 December 2000.

[4] If an employee is provided with board and lodging, a deduction of 15 percent for board and 5 percent for lodging can be made against the wage.

Table 1 summarises the changes to the statutory minimum wages that have occurred since 1990. The adult rate was held constant from 1990 through to 1995, increased 2 percent in 1995 and 1996, followed by a large (10 percent) increase in 1997, was then held constant for 3 years, and has been increased each year since 2000 (by 8, 2, 4 and 6 percent respectively). The youth minimum rate tracked the adult minimum from its introduction in 1994 until 2001. Lowering the age eligibility for the adult minimum from 20 to 18 in March 2001 had the effect of increasing the minimum wage for 18 and 19 year-olds by 69 percent. Coupled with the 4 percent increase in the adult minimum wage in March 2002, the minimum wage for this group rose by 76 percent over a two-year period, compared to the 6 percent increase in the adult minimum wage. Together with the adult minimum wage changes, the increases in the youth minimum wage from 60% to 70% of the adult minimum in March 2001, and further to 80% in March 2002, each had the effect of increasing the minimum wage for 16 and 17 year-olds by 19 percent, and by 41 percent in combination.

Figure 1 describes the trends in the real minimum wages (in June 1999 dollar values) that applied to 16-17 and 18-19 year-olds, and to adult (20 years and over) workers over the period of our analysis, 1997—2002. Although the statutory minimum wage remained constant at $7 from March 1997 until March 2000, inflation was low or negative, which meant there was relatively little erosion in the real value of the minimum wages. The 8 percent increase in 2000 provided a combined catch-up and real increase in the minimum wages over those prevailing in the late 1990s. The large changes observed in figure 1 correspond to the shift of 18-19 year-olds from the youth to adult minimum wages in March 2001, and the two-step increase in the youth minimum for 16-17 year-olds from 60% to 70% of the adult minimum in March 2001, and to 80% in March 2002.

Relevant Minimum Wage Research

Limited prior research has examined the relationship between minimum wages and labour market outcomes in New Zealand. Maloney (1995), Chapple (1997), and Pacheco and Maloney (1999) each examine the time-series evidence on the relationship between minimum wage changes during the 1980s and early 1990s and employment outcomes for youth and low-skilled workers. Although there

is some evidence that these prior minimum wage increases had negative employment effects, the findings, in general, are not robust to different model specifications. Summing up this research, Chapple (p. 47) concludes, "overall consideration of the employment impact of minimum wage rates suggests that conclusions regarding significant negative employment effects from real minimum wage increases are strikingly non-robust."

Interestingly, Portugal undertook a similar reform of the youth minimum wage in 1987.[5] Pereira (2002) and Portugal and Cardoso (2002) examine the impact these reforms on youth labour market outcomes using firm level data. Pereira (2002) estimated negative and statistically significant effects of the increase in minimum wages on the employment of 18-19 year-olds, with an implied elasticity in the range –0.2 to –0.4. She also found evidence of a positive spillover effect on the employment of 20-25 year-olds from this reform. In contrast, Portugal and Cardoso (2002) estimate a significant positive impact of the minimum wage change on the employment of the affected teenage groups.[6] Portugal and Cardoso decompose the effect of these changes into "separation" and "accession" effects, and conclude that there is a reduction in worker separations from firms that outweighs the impact of a reduction in accessions to firms.

Elsewhere, Abowd, Kramarz, Lemieux and Margolis (2000) examine the impact of minimum wages on youth employment in both France and the US using household survey data. By comparing youth workers whose wages lie between the current and next year's minimum wage, with those workers whose wages are marginally above this level, they find that subsequent employment probabilities of the former group are lower and conclude, for both countries, that real minimum wage changes are typically associated with significant employment effects in line with competitive labour market theory. Currie & Fallick (1996) use data from the US National Longitudinal Survey of Youth

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[5] Prior to 1987, the minimum wage applicable to 18 and 19 year-old workers was set at 75% of the minimum wage applying to adults (aged 20+), and the minimum wage for 15-17 year-olds was set at 50%. The reform in January 1987 raised the minimum wage for 18-19 year-olds to that of the adult minimum, generating a 49.3 percent increase in the nominal minimum wage for these workers, and a 33.9 percent increase in their minimum wage relative to that of adult workers. For 17 year-olds, the minimum was raised to 75% of the adult minimum wage. The minimum wage for 16-17 year-olds was raised from 50% to 75% of the adult minimum in 1988.

[6] These differences may be due to firm-selection and weighting issues. In particular, Pereira uses a "balanced" sample of firms that existed before and after the reforms, while Portugal and Cardoso used an "unbalanced" sample that includes firms that may exist either before and/or after the reforms. Furthermore, Portugal and Cardoso's results are weighted by firm employment size, whereas Pereira's results are unweighted.

to examine the relationship between changes in the minimum wage and youth employment, and find that youth workers who are affected by a minimum wage increase are about 3% less likely to be employed in the following year. Neumark and Wascher (1995) examine spillover effects of minimum wages on teenage school enrollments using state level data, and find negative effects on enrollments and positive (i.e. increasing) effects on inactivity rates.

III Data Description

The data we use in all analyses in this paper comes from the New Zealand Household Labour Force Survey (HLFS). In this section we begin with a brief discussion of the HLFS, outline the selection criteria we use to obtain our analysis data samples, and describe the characteristics of these samples.

The Household Labour Force Survey

The HLFS is an ongoing quarterly survey which began in 1985, and is designed to produce a comprehensive range of statistics relating to the employed, the unemployed and those not in the labour force who comprise New Zealand's working-age population. The current target population for the survey is the civilian non-institutionalised usually resident New Zealand population aged 15 and over. Each quarter, a representative sample of approximately 15,000 households and 30,000 individuals are surveyed and have a statutory obligation to respond to the survey.

Although the HLFS is essentially a cross-sectional survey, each household dwelling in the sample frame is surveyed for eight consecutive quarters with one-eighth of the sample rotating out each quarter in typical years.[7] Longitudinal attrition is high due to individuals and families changing dwellings. For example, Kuzmicich and Wigbout (2001) report that, of the respondents who were initially surveyed in March 1997, only 43 percent provided data in all eight quarters.

Personal interviews are used to collect the responses to a household questionnaire and to an individual questionnaire for each working-age person in the household in the initial quarter, and then

[7] The sampling frame for the HLFS is updated every five years following the New Zealand Census. When this occurs more complicated rotation rules are used to speed-up the transition to the new frame; this reduces the

telephone interviews are used in the subsequent quarters. These questionnaires collect information on labour-force status, hours worked, and educational status,[8] together with basic demographic information, of individuals and households, but do not collect any wage or non-categorical income information. However, since 1997, the June quarter HLFS has included an extensive supplemental questionnaire known as the New Zealand Income Survey (HLFS-IS), which collects information on pre-tax income from self-employment, wages and salaries, government transfers, and other sources for the purpose of producing a comprehensive range of income statistics.

The Samples

For the analysis in this paper we use data from the 1997-2002 June quarter HLFS surveys and HLFS-IS supplements, for all 16-25 year-olds.[9] Our main analyses use the cross-sectional data, without linking individuals across quarters; however, we also present some analysis using longitudinally linked data on individuals. Although the longitudinal data is a subset of the available cross-sectional data, Statistics New Zealand provides separate *cross-sectional* and *longitudinal* data extracts.[10] For this reason we also refer to them separately. In this section we provide a brief discussion of the samples used in the subsequent analyses; the data appendix provides further details of the selection criteria applied to obtain these samples.

The HLFS-IS questions can be answered by proxy interview and missing responses are usually imputed. Although observations with imputed responses are flagged, there is no flag for which questions have been imputed. Because of this, we drop all observations from our analysis where any data has been proxied or imputed. Our final cross-sectional sample has 22,835 observations, with an

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number of quarters that many dwellings are in the frame.

[8] The information collected in the HLFS does not allow us to accurately identify individuals who are studying if they have finished secondary school and are employed. This makes it difficult to meaningfully compare study rates across groups with different employment rates.

[9] Currently we only have access to the June quarter HLFS data. We intend to use data from the non-June quarters of the 1997-2002 HLFS, to more closely examine the timing of labour market adjustment to minimum wage changes, once these surveys are made available to us. Fifteen year-olds are not covered by minimum wage legislation and thus we choose to exclude them from our current analysis. Although they may provide a suitable comparison group, the minimum schooling leaving age of 16 implies that all employed 15 year-olds should be students working part-time.

[10] Currently we only have access to the 1997/98 – 2000/01 longitudinally linked HLFS data.

average of 380 observations in each age-year cell.[11] The summary statistics and regression results for the cross-sectional sample are adjusted using sampling weights created by Statistics New Zealand to increase the representativeness of the quarterly samples in the face of non-random survey non-response and individual attrition, while the reported longitudinal analyses are unweighted.

Table 2 presents summary statistics for key demographic characteristics and all outcome variables for our analysis sample. The first two columns pertain to the cross-sectional sample, while columns (3) and (4) pertain to the longitudinal sample. The first column describes the characteristics of the full cross-sectional sample. On average, during 1997—2002, 57 percent of 16-25 year-olds are employed as wage or salary workers, 35 percent are studying, 9 percent are unemployed, 16 percent are inactive and 16 percent receive a non-student benefit.[12] The average real wage of the wage and salary workers is $10.82 and they work around 30 hours per week.

Our analysis of employment, hours worked, hourly wages, and weekly labour earnings focuses on wage and salary workers, as minimum wage laws do not apply to the self-employed.[13] The second column pertains to the 12,544 wage or salary workers in our data. Compared to the full sample, these workers are, on average, older, more likely to be male, married, have European ethnicity (and less likely to be Maori, Pacific Islander or Asian), less likely to be studying or receiving benefit income, and have higher total incomes.

As households are typically sampled in the HLFS for eight quarters, they will usually respond to two June HLFS surveys, including Income Supplements. Thus, matched samples can be created across two adjacent years containing rough half the dwellings in the sample in either cross-section.[14] For our secondary analysis, we pool data for all 16-25 year-olds (in the second year) from the 1997-98—2000-01 matched HLFS-IS. In each sample, many individuals cannot be matched because they

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[11] In our regression analysis using the cross-sectional HLFS data, 117 observations are dropped from models of benefits receipt, 3 observations are dropped from models of hours worked, and 152 observations are dropped from models of labour earnings because of missing data or zero values on these outcomes. An additional 38 observations are dropped from all models with covariates because of missing marital status or place of birth.

[12] Note that these states are not all mutually exclusive – e.g. students may also be working. Labour market inactivity is defined as neither working nor studying.

[13] We often refer to wage and salary workers simply as 'workers' throughout the paper. When the distinction is not obvious, we do use the wordier phrase.

[14] However, a sampling frame update occurred during 1999 and, as a result, only one-quarter of each cross-section could be matched across the 1998-99 and 1999-2000 linked samples.

leave the sample dwelling in the year between the two surveys. In addition to this attrition, a further group of individuals are present in both surveys but have data proxied or imputed in one or both years. Overall, our longitudinal analysis sample contains 4,601 paired observations on 2,301 individuals.

The third column in table 2 presents summary statistics for this matched sample. The summary statistics on all time-varying variables are averages across the two years of data. Because the sample is based on age in the second year, individuals in this matched sample are younger. They are also less likely to be married or have European ethnicity, than the full sample in column (1). Given the age group, this probably reflects teenagers living at home with their parents and married couples, both of whom are more likely to remain at the same address and hence are less likely to attrit. The younger age of this sample is probably also a large explanatory factor of why individuals in the matched sample are more likely to be studying, less likely to be working, work fewer hours, and have lower wages, earnings and incomes than in the overall sample.

The fourth column pertains to the 926 individuals who work in both years in the sample. As for the cross-sectional comparison between workers and the overall sample (columns (1) and (2)), individuals in the linked sample who work in both years are older, more likely to be married, have European ethnicity (and less likely to be Maori, Pacific Islander or Asian), less likely to be studying or receiving benefit income, and have substantially higher total incomes. Although the linked samples show some differences from the cross-sectional samples, we conclude that the characteristics of the two are broadly similar.

IV Descriptive Analysis

The approach we adopt in our analysis of the impact of changes in the minimum wage for youth workers is to compare the average outcomes of the age groups affected by the changes, namely 16-17 year-olds and 18-19 year-olds, to those of young adults (aged 20-25) before and after the policy reform in March 2001. Two crucial identifying assumptions underlie this approach: first, that young adults will not be affected by changes to the youth minimum wage; and, second, that any secular trends in the labour market outcomes for 16-25 year-olds are common across these various age

groups. The first assumption requires that there are no spillover effects of the changes in the youth minimum wage on the outcomes of those workers not directly affected by the change. This assumption would not be satisfied if, for example, employers bound by the higher minimum wage on youth workers respond by employing more experienced and productive older workers in preference to youth workers. Such a response would lead to the direct effect of the minimum wage increase on youth employment being overstated: the measured impact would consist of the direct effect on youth workers plus the indirect effect on young adult workers. Alternatively, in order to maintain a relative wage differences, a minimum wage increase for youth workers may cause wages for adult workers to increase, and result in negative employment spillovers for these workers. The second assumption requires, for instance, that there are no differential age-specific trends. For example, our estimates could be confounded if demand for teenagers depends more on overall economic conditions than demand for young adults. Underlying trends in outcomes that differ by age group would also violate the second assumption.

We begin this section with a discussion of changes in the wage distributions of the 16-17, 18-19 and 20-25 year-olds age groups, before and after the policy reforms. This provides a description of how the youth minimum wage changes affected the teenage wage distributions, together with a sense of the size of the group of teenage workers affected by the minimum wage changes. Following this, we briefly describe the trends, over the sample period, in the outcomes of interest for the three age groups.

The Wage Distributions

Figure 2 presents kernel density estimates of the distributions of log real actual hourly wages at each worker's main job, separately for 16-17, 18-19, and 20-25 year-olds both before and after the youth minimum wage reforms.[15] For 18-19 and 20-25 year-olds, we combine the two years before (1999-

[15] Each kernel density is evaluated at the same 250 evaluation points, using an Epanechnikov kernel with a halfwidth of .0175 units (.03 units for single year densities), and each observation is weighted by its sampling weight and by the corresponding hours worked at the main job. The scale on each graph has been converted to real units but is a log-scale.

2000), and the two years after (2001-2002) the change in the minimum wage affecting 18-19 year-olds. For the 16-17 year-olds we also combine the two years before the reform (1999-2000), but estimate wage densities separately for 2001 and 2002, as the reform affected this age group in two annual steps. If the minimum wage is binding on youth workers, we would expect to see changes in the lower-end of the wage distributions for 16-17 and 18-19 year-olds after the reform.

The top-left graph presents the kernel densities for the 16-17 year-olds in the three periods. The three vertical lines on this figure indicate, from left to right, the youth minimum wage rate in 2000, 2001 and 2002. The solid line describes the wage distribution in 1999 and 2000, while the dashed and dotted lines show the 2001 and 2002 distributions respectively. Comparing the pre-reform and the 2001-year distributions, there is clearly a large reduction in the mass at the lower end of the wage distribution following the minimum wage reform. This reduction occurs both in the range between the 2000 and 2001 minimum wages, and in the range between the 2001 and 2002 minimum wages.[16] Comparing the 2001 and 2002 distributions indicates a further reduction in the mass around the 2001 minimum wage level. A large increase in the mass at wages just above the 2002 minimum wage is also apparent.

The top-right graph presents the kernel densities for 18-19 year-olds before and after the reform. The two vertical lines on this figure show the 2000 and 2001 minimum wage rates affecting this group. The solid line again represents the 1999 and 2000 wage density, while the dashed line represents the density for 2001 and 2002 wages. In comparison to the densities for 16-17 year-olds described above, there are two points to note. First, the fraction of the 18-19 year-old pre-reform distribution that lies to the left of the 2001 minimum wage is somewhat smaller than the fraction of the 16-17 year-old pre-reform distribution that lies below the 2002 minimum. That is, the affected group of 16-17 year-old workers is relatively larger than that of 18-19 year-olds. Second, the reduction in the density in the affected range for 18-19 year-olds is relatively less than for 16-17 year-olds. Nonetheless, there appears to be a small reduction in mass below the 2001 minimum wage in

[16] Note that, although employers knew in 2001 that the youth minimum was to rise from 70% to 80% in 2002 of the adult minimum, they didn't know what the 2002 adult minimum would be.

the post-reform distribution of 18-19 year-olds.[17]

The bottom graph in figure 2 presents the kernel densities for 20-25 year-olds' wages. Again, the vertical lines on this figure show the 2000 and 2001 minimum wage rates. Reassuring for our analysis, these graphs suggest that, over the four year period around the youth minimum wage reforms, the wage distribution for 20-25 year-olds was quite stable, particularly in the lower end of the distribution. This provides some confidence that the changes in the lower-end of the wage distribution for 16-17 and 18-19 year-olds were driven by the youth minimum wage reform. This reform appears to have shifted the lower-end of the wage distribution to the right for both 16-17 and 18-19 year-olds relative to older adults. Also, the shift in the wage distribution is larger for 16-17 year-olds than for 18-19 year-olds,[18] partly because less of the 18-19 year-olds' wage distribution was below the post-reform minimum wage.

We next describe the fractions of workers reporting wages below the current minimum wage, below the next year's minimum, and exactly equal to the current minimum, for these three age groups of workers over the sample period. Figure 3 describes the annual trends in three statistics. In each graph, we use dashed, solid and dotted lines to represent the 16-17, 18-19 and 20-25 year-olds' respectively, and mark relevant minimum wage levels using vertical lines.

First, the top-left graph shows the fraction of wage or salary workers whose current main-job wage is below the current minimum wage. Assuming that no workers are excluded from the minimum wage coverage, and that there is accurate reporting of wages, this statistic measures the degree of non-compliance with the statutory minimum wage.[19] The fractions of workers reporting below minimum wages lie between 2 and 8 percent in the pre-reform period. The fraction is generally

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[17] A more noticeable change is the large increase in density just above the 2001 minimum wage, which is due partly to the fall in density below the minimum.

[18] For example, the post-reform average wage for 16-17 year-olds is 6 percent higher than the pre-reform average, while the increase for 18-19 year-olds is only 1 percent.

[19] Recall that the legislation allows worker disability and training exemptions from the minimum wage, as well as a room and board allowance to be deducted from wages by their employer. In addition, individuals may report their wage in one of three ways: (1) their gross pay in their last paycheck together with the hours worked in that pay period; (2) their hourly wage which is hours weighted if they worked at more than one wage in the last week; or (3) their salary and their hours worked in the last week. Although we expect minimum wage exemptions to have relatively little effect on the incidence of workers paid sub-minimum wages, measurement error in reported wages, either directly or through reported earnings or hours, is likely to be common and create a significant number of sub-minimum wage observations.

highest for 16-17 year-olds and lowest for 18-19 year-olds, and tends to be declining over the pre-reform period.[20] Following the minimum wage reform, the fractions of 16-17 and 18-19 year-olds workers paid below the minimum wage increased substantially, while the fraction of young adults was largely unaffected. In particular, the fraction of 18-19 year-olds affected increased from 2 percent in 2000 to 13 percent in 2001, before dropping to 11 percent in 2002; while the fractions for 16-17 year-olds increased from 3 percent in 2000 to 9 percent in 2001 and 10 percent in 2002. Assuming that both the extent of exemptions from the statutory minimum and the structure of measurement error in reported wages was reasonably stable over this period, these changes reflect a significant increase in non-compliance with the statutory minimum wage.[21]

Second, the top-right graph in figure 3 shows the fraction of workers whose current wage is below the next year's (nominal) minimum wage. This provides a measure of the degree to which current workers may be affected by next year's minimum wage.[22] Not surprisingly, given the constant nominal minimum wage and low inflation between 1997 and 1999, the fraction of workers paid below next year's minimum wage is only marginally different from the fraction paid below the current minimum. However, there is a noticeable difference between the comparable fractions below the current and next-year minimum wages for 16-17 and 18-19 year-olds following the youth minimum wage changes. Eighteen percent of 18-19 year-old workers report wages in 2000 and 2001 below next year's minimum. This compares to 13 and 11 percent of workers who are paid below the current minimum wage in 2001 and 2002 and, ignoring measurement error issues, suggests there is perhaps 30-40 percent compliance rate for the affected 18-19 year-olds. Similarly, 14 and 22 percent of 16-17 year-old workers report wages in 2000 and 2001, respectively, that are below the following year's minimum. Compared to the 9 and 10 percent of workers who are paid below the minimum wage in

[20] Given the nominal minimum wage was constant between 1997 and 1999, the declining trends are not too surprising. However, there is no apparent effect of the 10 percent increase in minimum wages in 2000 of the fractions paid below minimum.

[21] Such non-compliance may include employers who were unaware of the change, those who responded slowly (e.g., some June quarter interviews would occur fairly soon after the change came into effect in March), as well as those who willingly refused to comply.

[22] This measure is obviously subject to the same exemption and measurement error conditions discussed above. In addition, it also ignores any secular changes to the wage distribution that may affect this statistic although, as discussed above for the 20-25 year-olds, there is little to suggest that this factor is important over the period.

2001 and 2002, this implies a compliance rate for affected 16-17 year-olds of 35-50 percent. Also, although the 2003 data is not yet available, it is interesting to note that by 2002, 24 percent of 16-17 year-old workers, and 26 percent of 18-19 year-old workers, report wages below the current (2003) minimum wage level, which implies that the changes to the youth minimum wage in conjunction with the increasing adult minimum wage now affects a large fraction of teenage workers.

Third, the bottom graph in figure 3 shows the fraction of workers who are paid exactly the minimum wage in each year. This provides another measure of the extent to which the minimum wage binds. Prior to the youth minimum wage reform, a small (less than 1 percent) and declining fraction of workers in each age group reported earning exactly the minimum wage, which further suggests that these minimum wages were essentially non-binding. After the reform, in 2001, 1 percent of 16-17 year-old workers and 3 percent of 18-19 year-old workers reported exactly the minimum wage, and these fractions increased to 6 and 10 percent respectively in 2002.[23]

Outcome Trends over the Period

We now turn our attention to describing the trends over the sample period in a variety of outcomes of interest to this analysis for the three age groups used above.[24] The two main outcomes that we focus on are being employed, capturing the extensive margin of labor market adjustment, and the number of hours worked conditional on being employed capturing the intensive margin. Figure 4 graphs the fraction of each age group that is employed as a wage or salary worker, and the average actual weekly hours worked by wage or salary workers from 1997-2002. Simple models for the impact of the minimum wage changes would expect to see any change occur between June 2000 and June 2001 for 18-19 year-olds, or between the 2000-2001 and 2001-2002 years for 16-17 year-olds.

The employment and hours-worked graphs in figure 4 display the importance of both age effects (young adults are more likely to work, and work more hours, than teenagers), and also business cycle

[23] We suspect that part of the large fraction of 18-19 year-olds reporting the minimum wage in 2002 is due to the minimum wage being exactly $8.00, rather than a binding minimum wage effect per se. That is, there is a strong tendency for reported (and probably actual) wages to be at round nominal values.

[24] Each outcome is measured for the seven days prior to the HLFS interview.

(and other) effects over time. Employment levels declined from 1997 until 1999 (2000 for 17-18 year-olds), before increasing over the later part of the period. The minimum wage changes that occurred after 2001 do not appear to have had a substantial effect on employment rates of teenage workers relative to young adults, although the trends for the different groups do vary somewhat. Employment rates of 18-19 year-olds picked up later than for 16-17 and 20-24 year-olds, and continued to increase from 2001 to 2002, while the employment of the other groups stabilized or fell a little. A similar story is observed for hours worked. Average hours worked for both 18-19 and 20-25 year-olds remain fairly stable over the sample period, while hours worked for 16-17 year-olds are fairly constant or weakly increasing throughout the sample period, except for a noticeable increase in 2002. These two graphs suggest that, except perhaps hours worked by 16-17 year-olds in 2002, there is little evidence that the minimum wage increases affected the employment or hours worked by teenage workers.

In figure 5 we examine the patterns for a variety of alternative outcomes that may be affected by the youth minimum wage changes: namely the fractions of individuals studying, unemployed, inactive (neither working nor studying), and receiving non-student benefits. Over the sample period, study rates fell slightly for 16-17 year-olds, and rose for 18-19 and 20-24 year-olds; while the other outcomes exhibit a much less stable time pattern. As with the employment and hours worked in figure 4, there is little, if any, evidence of a systematic effect of the youth minimum wage changes on the outcomes of the affected age groups.

Figure 6 graphs the average actual weekly earnings for wage and salary workers, and the average actual weekly income for all individuals in each age group over the same sample period. Changes in average weekly earnings conditional on employment reflect the combination of changes in hours worked and changes in wage rates over time. This outcome allows us to evaluate the effect of the minimum wage reform on the average wage and salary worker. On the other hand, as changes in average weekly income also factor in the possible displacement effects that an increase in the minimum wage may have (i.e. some workers lose their jobs and receive either lower or zero income), it provides a better overall measure of the welfare effects of the minimum wage reform. Both of these graphs show similar patterns. The average earnings and incomes of 20-25 year-olds were roughly

constant over the sample period, while 18-19 year-olds' earnings increased until 2001 before falling in 2002, and their incomes increased slightly. Both the average earnings and incomes of 16-17 year-olds show a distinct increase after 2000 that is consistent with, and suggestive of, the increase in minimum wages of this group in 2001 and 2002.

The descriptive evidence presented here is suggestive of, at best, weak effects of the large increases in the minimum wages for young workers. However, it is difficult to capture the complex relationship between age, year, and minimum wage effects on labor market outcomes in a graphical manner alone. We next formalise the analysis to control for these and other factors, and quantify the impact of the youth minimum wage policy changes on these outcomes.

V Analysis of the Impact of Youth Minimum Wage Changes

Difference-in-Differences Estimates

Table 3 summarises the levels and changes in the employment rates and hours worked conditional on employment before and after the 2001 youth minimum wage policy changes for 16-17, 18-19, and 20-25 year-olds. We present the data by age group in columns (1) – (3), and the differences between the 16-17 and the 20-25 year-olds' outcomes, and between the 18-19 and the 20-25 year-olds' outcomes, in columns (4) and (5) respectively. Panel A pertains to the employment rate, while panel B pertains to hours worked. Within each panel, rows 1 and 2 show the average outcomes in the period before (1997-2000) and after (2001-02) the policy change respectively, and row 3 presents the change in these outcomes.[25] The numbers in bold at the bottom right of each panel are the simple difference-in-differences estimates of the impact of the minimum wage changes on the respective outcomes.

Panel A shows that the fraction of both 18-19 and 20-25 year-olds employed as wage or salary workers increased by about 1 percentage point after the policy reform. Comparing these changes, the simple difference-in-differences estimate of the effect of the minimum wage increase for 18-19 year-

[25] Note, in this exercise we combine the two years after the initial policy change for 16-17 year-olds as well as 18-19 and 20-25 year-olds.

olds in wage or salary employment is a 1 percentage point decrease (due to rounding), which is not statistically significantly different from zero. Meanwhile, the fraction of 16-17 year-olds employed increased by 3%, resulting in the difference-in-differences estimate of the impact of 1 percent, which again is statistically insignificant.

Likewise, panel B shows that hours worked increased by 1.19 and 0.58 hours for 16-17 and 18-19 year-olds respectively, and decreased 0.65 hours for 20-25 year-olds. The resulting difference-in-differences estimates of the policy change are 1.84 hours for 16-17 year-olds, and 1.23 hours for 18-19 year-olds, and these estimates are statistically significant at the 5% and 15% levels respectively. Each of these represent relatively large increases in hours – e.g., from a base of 16.5 hours per week, the 1.8 hours increase for 16-17 year-olds represents an 11 percent increase.

Regression Analysis of Employment and Hours Worked

We next extend the simple difference-in-differences analysis of the impact of the youth minimum wage changes using regression methods to control for other sources of variation that may confound the estimates of interest. Ignoring individual and time subscripts, the basic regression specification we use has the form

$$Y = d * (age18\text{-}19 * Post\text{-}2001) + X'b + u \quad (1)$$

where Y is the outcome of interest, "*age18-19*Post-2001*" is an indicator variable which equals 1 for 18-19 year-olds after the 2001 reforms and 0 otherwise, X is a vector of variables to control for other factors influencing the outcome, u is an error term to capture unobserved effects. Our main coefficient of interest is thus d, which represents the effect of the minimum wage reform on 18-19 year-olds controlling for other factors. All regressions include age-specific dummy variables to capture systematic age differences in the outcomes, and year dummy variables to capture time-varying effects due to aggregate business cycle and other trends. The age effects are constrained to be the same across years, and the time effects are constrained to be the same across age groups. Also, because the analysis essentially compares group averages of outcome variables to determine the effect of the policy reform, we present Huber-White robust standard errors that allow for arbitrary

correlation in individual error terms within age-year cells.

Table 4 contains the estimated policy effects on employment, in panel A, and hours worked, in panel B, based on a variety of specifications. The first three specifications focus on the effect of the minimum wage reform on 18-19 year-olds. For these models, we drop 16-17 year-olds from the analysis and use data for only those aged 18-25. In column (1), we use data just from 2000 and 2001, and only control for age and year effects. In panel A, the estimated effect on employment (-0.01) is the same as the difference-in-differences estimate in table 3 while, in panel B, the estimated effect on hours worked (0.7) is about one-half the magnitude of the estimate in table 3 (1.2 hours). As with the difference-in-differences estimates, neither estimate in column (1) is statistically significant.

In column (2), we include all sample years for 18-25 year-olds, and assume that any minimum wage effect on outcomes is constant across the post-reform period (2001 and 2002). Compared to column (1), this specification increases the precision by including more years of data but assumes the overall age pattern of each outcome is constant from 1997-2002. This model is the difference-in-differences specification in table 3, except that it also controls for age and year effects. The results for the estimated policy effect on employment are about the same as in column (1), while the effect on hours worked nearly doubles to 1.2 hours worked; and both results closely replicate those in table 3. Again, neither estimate is significant at conventional levels.

As discussed above, if the policy reform has a direct effect on 18-19 year-olds it may also have an indirect effect on the outcomes of young adults. To examine this possibility, the specification in column (3) allows the policy change to have spillover effects on 20-21 year-olds, by including a variable, *age20-21*Post-2001,* that is equal to 1 for 20-21 year-olds after 2001 and 0 otherwise.[26] To the extent that a spillover effect exists, the coefficient on the "*age18-19*Post-2001*" variable will represent the <u>direct</u> effect of the minimum wage change on the affected age-group, while the coefficient on the "*age20-21*Post-2001*" will represent the <u>indirect</u> effect on the unaffected age-group. The results for employment show little change on the effect for 18-19 year-olds, and find a statistically insignificant 1 percent increase in employment for 20-21 year-olds in the post-reform

[26] The results are comparable using a single-year spillover for 20 year-olds.

period, relative to that of 22-25 year-olds. However, we find that allowing for a spillover effect results in a statistically significant negative effect of the minimum wage change on 20-21 year-olds hours worked (coefficient = -1.3 hours per week, p-value=0.06).

The specifications in columns (4) – (6) of table 4 also include 16-17 year-olds. The first of these specifications is the same as that in column (3), except with the 16-17 year-olds included with relevant age effects and a single, post-2001, minimum wage effect for this group. The results for the 18-19 and 20-21 year-olds are essentially unchanged from column (3). The estimated effects of the minimum wage change on 16-17 year-olds are both positive, but statistically insignificant; 16-17 year-olds' employment is estimated to have risen 1.5 percent, and their hours by 1.3 hours per week following the minimum wage changes.

The models so far assume the effects of the minimum wage reforms in 2001 (and 2002 for 16-17 year-olds) on outcomes are constant in the post-reform period. The next specification, in column (5) of table 4, relaxes this restriction and allows the policy changes to differ across the two post-reform years. This may be particularly important for 16-17 year-olds, given that the reform changed the minimum wage for this group in two annual steps. In addition, as some June quarter HLFS interviews occur close to the March implementation dates of the policy change, the effects of the policy reform may be missed in the 2001 data. In column (5), the coefficients on the post-2001 interaction terms represent the change in outcomes for the age groups of interest between the pre-reform years and 2001 relative to the same change for 22-25 year-olds; while the coefficient on the post-2002 interaction terms provide the marginal effects in 2002.

The major impact of this change is on the estimated hours worked effects for 16-17 year-olds; the estimated effect of the reforms on their 2001 hours are now small and insignificant, while the marginal effect on 16-17 year-old workers hours in 2002 is now 2.1 hours per week (p-value=0.07). The combined effect of these two coefficients imply that 16-17 year-old workers worked 2.3 hours per week more in 2002 because of the minimum wage increases, a 14 percent increase on the pre-reform average hours worked. The other results in this specification are in line with the previous specifications.

The above specifications all assume that any change in outcomes between the pre- and post-

reform period for the affected age groups relative to the unaffected groups are due to the changes in minimum wage. It is possible that the demographics of the sample changed over the period in such a way as to confound our estimates of the minimum wage effects. The final specification in table 4 adds in controls for observable demographic characteristics of the sample: dummy variables for gender, ethnicity, marital status, New Zealand born, and region of residence. The results from this specification are similar to the previous results. Of note, the employment coefficient on 20-21 year-olds' post-2001 variable is now 2 percent and statistically significant at the 10% level, and the hours worked coefficients for this age group in 2001 and 2002, while both significant at the 10% level, are opposite signed and have roughly equal magnitudes.

To summarise the findings from table 4, first, and somewhat surprisingly given the size of the increase in the minimum wage for 18-19 year-olds, we find no evidence of any significant direct effects of this increase on either their employment or hours worked. We consistently find a small reduction in employment rates for this group, together with a small increase in hours worked conditional on employment, and neither effect is statistically significant. Second, our results do show significant effects on the hours worked for 20-21 year-old workers following this minimum wage change. However, given the absence of any direct effects on 18-19 year-olds, it is doubtful whether this finding can be attributed to the youth minimum wage change. Finally, and in contrast to simple competitive model predictions of the effect of minimum wage increases, we find no evidence of adverse employment effects for 16-17 year-olds. In fact, the estimates of the employment rate effects are positive, though small and statistically insignificant, while we estimate quite large and statistically significant increases in hours worked following the second increase in the minimum wage for 16-17 year-olds in 2002; this increase is on the order of 15 percent. However, that we find no evidence of a comparable change following the 2001 increase for this age group, does draw into question the interpretation of this finding as being attributable to the change in minimum wages.[27]

[27] It is possible that the group of workers affected by the initial increase was too small to detect a response: recall that the fraction of 16-17 year-old workers in 2000 with wages below the 2001 minimum was 13 percent, compared with 22 percent of 2001 workers with wages below the 2002 minimum wage. A similar argument may apply to 18-19 year-olds, although the fraction of workers reporting wages below next year's minimum was 18 percent in both 2000 and 2001.

Other Outcomes

Although attention generally focuses on the impact of minimum wage changes on employment, several other outcomes are of interest, particularly in the case of youth minimum wages. A sizeable change in the minimum wage for young workers, by making work relatively more attractive, may affect the labour-supply decisions of non-participants. For example, for those youth still in school, it may affect their decision of when to leave school and enter the labour market. We now examine the effects of the youth minimum wage changes on other outcomes.

Table 5 presents regression results for six other outcomes: studying, unemployment, inactivity, receiving benefit income, the logarithm of actual weekly earnings for wage or salary workers, and the logarithm of actual weekly income.[28] Each of these regressions adopts the full set of controls used in the specification reported in column (6) of table 4. The results for studying in column (1) show a statistically significant 4 percent falls in the fraction of 16-17 year-olds (p-value<0.01), and the fraction of 18-19 year-olds (p-value=0.1), studying after the minimum wage increases in 2001. The results in column (2) indicate the unemployment rate for 16-17 year-olds increased 2 percent in 2001 before falling the same 2 percent in 2002. Given the fluidity between unemployed and not-in-the-labour-force states for many youth, Inactivity (i.e. neither working nor studying) may provide a more reliable outcome measure than unemployment for these age groups. In column (3), we find there was a significant increase (2.5 percent) in the fraction of 16-17 year-olds who are inactive after 2001. These results also indicate a large (6 percent) increase in inactivity for 18-19 year-olds in 2001, which was followed by a 4 percent fall in inactivity in 2002. These results are broadly consistent with those for unemployment in column (2), but with somewhat better precision and increased statistical significance. The results in column (4) for the receipt of benefits show neither large nor statistically significant effects for any of the age-group and year interaction variables.

Columns (5) and (6) of table 5 present the regression results for changes in log weekly earnings

[28] To handle non-positive incomes, we have censored incomes at the 1st percentile of the positive all age income distribution, corresponding to $28. This affects 5,865 individuals with zero or negative earnings (from self-employment).

and log weekly income. Given that the minimum wage increases appear to have increased the average wages of teenage workers, and that there are no adverse employment effects, it is not surprising that we find an increase in earnings for these groups. The earnings of 16-17 year-olds increased about 15 percent after the 2001 reforms, and a further (marginally significant) 9 percent in 2002, while earnings of 18-19 year-olds increased about 10 percent after 2001. As earnings are only available for those who are employed, we also examine the impact on individuals' total weekly income, which will be available for the non-employed and arguably provides a better measure of the welfare effects of the minimum wage change. Interestingly, the results for this specification in column (6) only show significant changes for 16-17 year-olds: their incomes increased 5 percent in 2001, and a further 10 percent in 2002.

Longitudinal Analysis of the 2001 Minimum Wage Changes

We now turn attention to the analysis of the impact of the youth minimum wage changes on the set of outcomes using the longitudinal data from the linked HLFS for the 1997-98 – 2000-01 pairs of years. As we do not yet have access to the 2001-02 data, this analysis is necessarily restricted to the impact of the minimum wage changes that occurred in March 2001 on the outcomes in June of 2001. In the analyses presented we control for the full set of age, year, and other time-varying demographic effects, and also allow for unrestricted individual fixed effects.

Table 6 contains estimates of the effects for the discrete outcomes of interest. For each outcome we present estimates derived from both a fixed effect linear probability model in panel A, and a conditional logit model in panel B. By and large, due to relatively small sample sizes, there is little evidence of any significant effects from these specifications. The only statistically significant effect in both specifications is that of the 20-21 year-olds in the studying equation: both specifications imply that, conditional on other factors, the study rate for this group increased in 2001. These estimates compare to the insignificant effect estimated using the cross sectional data, presented in table 5. In the linear probability specification for studying, we estimate a statistically significant increase for 16-17 year-olds in 2001, which is in contrast to the significant negative effect estimated using the cross

sectional data (table 5) and the negative but insignificant effect estimated in the conditional logit specification. We also find a marginally significant increase in benefit receipt for 20-21 year-olds in 2001 in the conditional logit model.

Table 7 presents estimates of the impact of the 2001 minimum wage changes on the three continuous outcomes that we analyse, estimated using linear fixed effects models. For hours worked and log earnings, we restrict the sample to the 926 individuals who work in each year.[29] For log income, we use all 2,301 individuals in the sample. The only statistically significant effect in this table is a large positive increase in earnings for 16-17 year-olds in 2001, which is consistent with the cross sectional finding in table 5. However, although the other coefficients are not statistically significant, their signs are generally consistent with the cross-sectional estimates presented above.[30]

VI Concluding Comments

In this paper, we examine the impact of large increases in minimum wages for teenage workers that occurred in New Zealand in the early 2000s, and find no evidence of adverse effects on youth employment. In fact, we find little effect of the 70 percent increase in the minimum wage impacting 18-19 year-olds on a wide variety of outcomes. Even though 15-20 percent of these workers were affected by this increase, we find little discernible impact on the wage distribution, and no effect on their employment rates or hours worked. Our secondary analyses do suggest that this change may have reduced study rates and increased inactivity for this group, and increased the earnings of those working, however the evidence is not particularly robust.

On the other hand, we find more systematic effects for 16-17 year-old workers. The minimum wage reform appears to have had a greater effect on raising the wages of these workers. We find that the average hours worked increased by 10-15 percent, earnings for workers by 15-25 percent, and total income by about 15 percent following the reform. We also find evidence of a drop in study rates and an increase in the unemployment and inactivity rates for 16-17 year-olds. These results are

[29] Hours is missing for one individual, so the hours worked regression is based on the remaining 925 individuals.
[30] The only exceptions are the age 18-19 and 20-21 coefficients in the log income equation, but these effects are small and imprecisely estimated using the cross sectional data.

suggestive of some strong labour supply responses by this age group, together with relatively benign demand responses by employers.

One potentially important caveat to the analysis is the large increase in the fraction of teenage workers reporting below minimum wages since the reforms, which is suggestive of employer non-compliance. For example, in 2002 over 10 percent of teenage workers reported sub-minimum wages, in contrast to 2000 when only 2-3 percent did. Furthermore, in 2002 one-quarter of teenage workers reported wages below the current (2003) minimum wage levels. This implies that the minimum wage reforms, in conjunction with the recent increases in the adult minimum wage, should now have a significant impact on the youth labour market. Assuming employers comply with the statutory minimum wages, we should expect to see significant increases in wages in the lower tail of the wage distributions for teenage workers. To the extent that this occurs, there should be additional scope to measure an impact of these changes on youth employment and related outcomes.

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Data Appendix

Cross-sectional Sample selection

We begin with a sample of observations from each June HLFS whose age is between 16 and 25; this consists of 26,645 observations. The selection criteria we apply are as follows (with number of observations dropped in parentheses):

		Observations
1.	Base sample	26,645
2.	Drop if individual did not respond to both the HLFS and HLFS-IS (436)	26,207
3.	Drop if individual had any imputed responses (3,374)	22,835

Longitudinal Sample Selection

We begin with a sample of observations from the linked June HLFS whose age is between 16 and 25 in the second year; this consists of 7,762 observations on 4,885 individuals. (Note that, except for 2000-01, the linked extracts provided by Statistics New Zealand included only observations on individuals observed in the two years.) The selection criteria we apply are as follows (with number of observations dropped in parentheses):

		Individuals	Observations
1.	Base sample	4,885	7,762
2.	Drop if the individual is not observed in both years (2,008)	2,877	5,754
3.	Drop if individual did not respond to both the HLFS and HLFS-IS in each year (326)	2,714	5,428
4.	Drop if individual had any imputed responses in either year (806)	2,311	4,622
5.	Drop if the individual's age fell or increased by more than 2 years between HLFS years (2)	2,310	4,620
6.	Drop if the individual's reported gender changed (18)	2,301	4,602

Table 1: Summary of Minimum Wage Changes

Effective Date	Nominal Minimum Wage Affecting		
	Adults	**18-19 Year-Olds**	**16-17 Year-Olds**
17 September 1990	6.13	---	---
31 March 1994[a]	---	3.68	3.68
22 March 1995	6.25 (2.0)	3.75 (1.9)	3.75 (1.9)
18 March 1996	6.38 (2.0)	3.83 (1.9)	3.83 (1.9)
1 March 1997	7.00 (9.8)	4.20 (9.7)	4.20 (9.7)
6 March 2000	7.55 (7.9)	4.55 (8.3)	4.55 (8.3)
5 March 2001[b]	7.70 (2.0)	7.70 (69.2)	5.40 (18.7)
18 March 2002[c]	8.00 (3.9)	8.00 (3.9)	6.40 (18.5)
24 March 2003	8.50 (6.3)	8.50 (6.3)	6.80 (6.3)
Increase: (2000 — 2002)	6.0%	75.8%	40.7%

Notes: Percentage increase reported in parentheses.

[a] Youth minimum wage introduced for 16-19 year olds at 60 percent of the adult rate.

[b] Adult minimum wage applied to 18-19 year olds; minimum wage for 16-17 year olds raised to 70 percent of the adult minimum wage. Announced 14 December 2000.

[c] Minimum wage for 16-17 year olds raised to 80 percent of the adult minimum wage. Announced 14 December 2000.

Table 2: HLFS Sample Characteristics

	Cross-Sectional Sample		Longitudinal Sample	
	Full Sample (1)	Workers (2)	Linked Sample (3)	Work both Years (4)
Age	20.37	20.87	19.00	20.11
	(0.02)	(0.03)	(0.06)	(0.10)
Female	0.50	0.49	0.50	0.50
	(0.004)	(0.005)	(0.01)	(0.02)
Married	0.18	0.21	0.13	0.19
	(0.003)	(0.004)	(0.01)	(0.01)
Pakeha / European	0.69	0.77	0.66	0.75
	(0.003)	(0.004)	(0.01)	(0.01)
Maori	0.15	0.12	0.16	0.12
	(0.002)	(0.003)	(0.01)	(0.01)
Pacific Islander	0.07	0.06	0.09	0.06
	(0.002)	(0.002)	(0.01)	(0.01)
Asian	0.06	0.03	0.08	0.06
	(0.002)	(0.002)	(0.01)	(0.01)
Wage or Salary Worker	0.57	1	0.52	1
	(0.004)		(0.01)	
Currently Studying	0.35	0.16	0.48	0.23
	(0.004)	(0.004)	(0.01)	(0.01)
Unemployed	0.09	0	0.04	0
	(0.002)		(0.003)	
Inactive (Not Working or Studying)	0.16	0	0.14	0
	(0.003)		(0.01)	
Received Non-Student Benefits	0.16	0.06	0.14	0.04
	(0.003)	(0.002)	(0.01)	(0.01)
Total Hours Worked[(a)] Last Week	30.42	30.42	25.74	28.78
	(0.17)	(0.17)	(0.41)	(0.48)
Hourly Wage	10.82	10.82	9.50	10.08
	(0.06)	(0.06)	(0.12)	(0.12)
Labour Earnings[(a)] Last Week	357.99	357.99	272.35	321.98
	(2.65)	(2.65)	(5.67)	(7.20)
Total Income Last Week	252.56	372.44	186.17	331.46
	(2.07)	(2.69)	(4.24)	(7.02)
Observations	22,835	12,544	2,301	926

Notes: Estimated standard errors in parentheses. The cross-sectional summary statistics are weighted by the HLFS sampling weights; the longitudinal summary statistics are unweighted. The summary statistics for the longitudinal samples are averages across the two years of the panel. All hourly wages, and weekly earnings and incomes are in constant (June 1999) dollar values, adjusted using the CPI.

[(a)] Hours worked and labour earnings are conditional on working.

Table 3: Difference-in-Differences' Estimates of Changes in Employment and Hours Worked

	Age group			Difference (from 20–25)	
	16-17	**18-19**	**20-25**	**16-17**	**18-19**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
(A) Wage or Salary Employment					
1. 1997-2000	0.41	0.52	0.64	-0.23	-0.12
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[3,553]	[2,977]	[8,618]		
2. 2001-02	0.44	0.53	0.65	-0.22	-0.13
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	[1,806]	[1,535]	[4,346]		
3. Difference	0.03	0.01	0.01	**0.01**	**-0.01**
(2001-02 – 1997-2000)	(0.02)	(0.02)	(0.01)	**(0.02)**	**(0.02)**
(B) Actual Total Hours Worked Last Week (Conditional on Employment)					
1. 1997-2000	16.58	27.65	34.59	-18.01	-6.95
	(0.46)	(0.47)	(0.25)	(0.52)	(0.53)
	[1,398]	[1,500]	[5,338]		
2. 2001-02	17.77	28.22	33.94	-16.17	-5.72
	(0.58)	(0.64)	(0.33)	(0.67)	(0.72)
	[779]	[783]	[2,746]		
3. Difference	1.19	0.58	-0.65	**1.84**	**1.23**
(2001-02 – 1997-2000)	(0.74)	(0.80)	(0.42)	**(0.85)**	**(0.90)**

Notes: Estimated standard errors in parentheses; number of observations in square brackets.

Table 4: Regression Estimates of Changes in Employment and Hours Worked

	2000 vs 2001 (1)	All Years (2)	20-21 Spillover (3)	Add 16-17 (4)	Two Year Effects (5)	Covariates (6)
(A) Wage or Salary Employment						
age16-17				0.015	0.012	0.005
*Post-2001				(0.01)	(0.01)	(0.01)
age16-17					0.007	0.011
*Post-2002					(0.01)	(0.01)
age18-19	-0.011	-0.013	-0.009	-0.009	-0.029	-0.027
*Post-2001	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
age18-19					0.038	0.037
*Post-2002					(0.03)	(0.03)
age20-21			0.011	0.012	0.013	0.020*
*Post-2001			(0.01)	(0.01)	(0.01)	(0.01)
age20-21					-0.004	-0.003
*Post-2002					(0.02)	(0.01)
Observations	5,679	17,476	17,476	22,835	22,835	22,797
R-squared	0.03	0.02	0.02	0.04	0.04	0.10
(B) Actual Total Hours Worked Last Week (Conditional on Employment)						
age16-17				1.264	0.184	0.186
*Post-2001				(0.78)	(1.06)	(1.04)
age16-17					2.102*	2.513**
*Post-2002					(1.17)	(1.19)
age18-19	0.683	1.176	0.767	0.769	0.496	0.489
*Post-2001	(0.56)	(0.77)	(0.83)	(0.84)	(1.20)	(1.06)
age18-19					0.532	0.756
*Post-2002					(1.21)	(1.05)
age20-21			-1.255*	-1.248*	-1.946**	-1.642*
*Post-2001			(0.65)	(0.64)	(0.88)	(0.86)
age20-21					1.379	1.768*
*Post-2002					(0.96)	(0.97)
Observations	3,353	10,365	10,365	12,541	12,541	12,524
R-squared	0.05	0.05	0.05	0.17	0.17	0.21

Notes: Coefficients followed by one, two, and three stars are significantly different from zero at the 10, 5, and 1 percent level, respectively. Huber-White robust standard errors, which allow for arbitrary correlation in individual error terms within age-year cells, are in parentheses. All specifications are estimated using OLS and include single age-specific and year-specific effects. The covariates included in column (6) are dummy variables for gender, marital status, ethnicity, New Zealand born, and region of residence.

Table 5: Regression Estimates of Changes in Other Outcomes

	Studying (1)	Unemployed (2)	Inactive (3)	Received Benefits (4)	Log Labour Earnings (5)	Log Total Income (6)
age16-17	-0.038***	0.019*	0.025***	0.013	0.150***	0.046*
*Post-2001	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)
age16-17	-0.005	-0.022*	-0.012	0.014	0.085	0.092**
*Post-2002	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)	(0.04)
age18-19	-0.042*	0.025	0.062***	0.005	0.094*	0.005
*Post-2001	(0.03)	(0.02)	(0.01)	(0.01)	(0.05)	(0.05)
age18-19	0.036	-0.006	-0.044***	0.016	-0.037	0.087
*Post-2002	(0.02)	(0.02)	(0.02)	(0.01)	(0.06)	(0.05)
age20-21	0.002	-0.002	0.005	0.006	-0.075	0.015
*Post-2001	(0.01)	(0.01)	(0.01)	(0.01)	(0.07)	(0.04)
age20-21	-0.013	0.001	-0.021*	-0.010	0.117	0.009
*Post-2002	(0.01)	(0.02)	(0.01)	(0.01)	(0.07)	(0.04)
Observations	22,797	22,797	22,797	22,681	12,375	22,797
R-squared	0.34	0.02	0.08	0.11	0.39	0.35

Notes: Coefficients followed by one, two, and three stars are significantly different from zero at the 10, 5, and 1 percent level, respectively. Huber-White robust standard errors, which allow for arbitrary correlation in individual error terms within age-year cells, are in parentheses. All specifications include single age-specific and year-specific effects and other covariates (as specified in Table 4, column (6)), and are estimated using OLS.

Table 6: Longitudinal Estimates of Minimum Wage Effects – Discrete Outcomes

	Worked	Studying	Unemployed	Inactive	Received Benefits
	(1)	(2)	(3)	(4)	(5)
			(A) Linear Probability Fixed Effects Models		
age16-17	-0.014	0.029**	-0.026	0.021	0.026
*Post-2001	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)
age18-19	0.016	-0.003	-0.0001	-0.021	-0.051
*Post-2001	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
age20-21	-0.025	0.111**	0.007	-0.016	-0.036
*Post-2001	(0.04)	(0.05)	(0.02)	(0.03)	(0.05)
Individuals	2,301	2,301	2,301	2,301	2,301
R-squared:					
Within	0.04	0.13	0.01	0.02	0.03
Incl FE	0.78	0.83	0.60	0.77	0.81
			(B) Conditional Logit Models		
age16-17	-0.690	-0.307	1.845	0.373	-0.343
*Post-2001	(0.58)	(1.25)	(1.50)	(0.85)	(1.25)
age18-19	-0.634	1.316	1.669	-0.210	-0.644
*Post-2001	(0.62)	(0.88)	(1.53)	(0.81)	(1.03)
age20-21	-0.896	1.987**	2.530	-0.124	-2.528*
*Post-2001	(0.72)	(0.81)	(2.10)	(0.83)	(1.31)
Individuals[(a)]	522	441	127	268	206

Notes: Coefficients followed by one, two, and three stars are significantly different from zero at the 10, 5, and 1 percent level, respectively. Huber-White robust standard errors, which allow for arbitrary correlation in individual error terms within age-year cells, are in parentheses for the linear probability models. Traditional standard errors are presented for the conditional logit model. All specifications include single age and year effects and other covariates (as specified in Table 4, column (6), except New Zealand born).

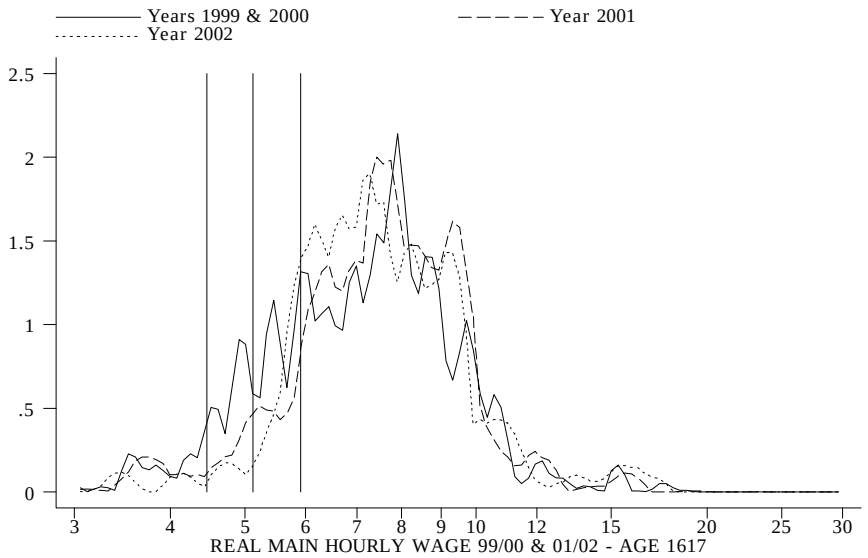
[(a)] Number of individuals whose outcome changes and that contribute to the conditional likelihood.

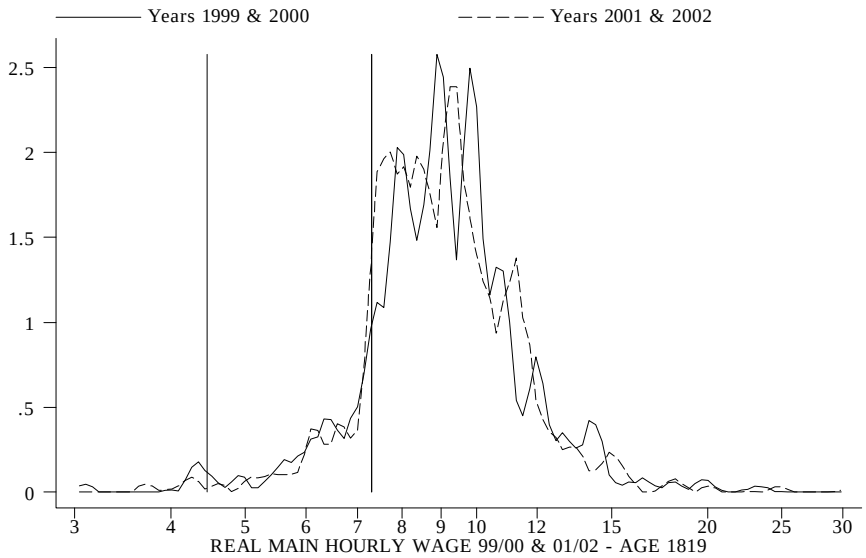
Table 7: Longitudinal Estimates of Minimum Wage Effects – Continuous Outcomes

	Hours Worked (1)	Log Labour Earnings (2)	Log Total Income (3)
age16-17	1.336	0.217***	0.023
*Post-2001	(1.37)	(0.08)	(0.09)
age18-19	0.148	0.120	-0.075
*Post-2001	(1.76)	(0.10)	(0.13)
age20-21	-2.449	-0.157	-0.157
*Post-2001	(3.52)	(0.10)	(0.11)
Individuals	925	926	2,301
R-squared:			
Within	0.07	0.19	0.12
Incl FE	0.82	0.90	0.87

Notes: Coefficients followed by one, two, and three stars are significantly different from zero at the 10, 5, and 1 percent level, respectively. Huber-White robust standard errors, which allow for arbitrary correlation in individual error terms within age-year cells, are in parentheses. All specifications include single age and year effects and other covariates (as specified in Table 4, column (6)), and are estimated using linear fixed effects models.

Figure 1: Real Minimum Wage by Age Group





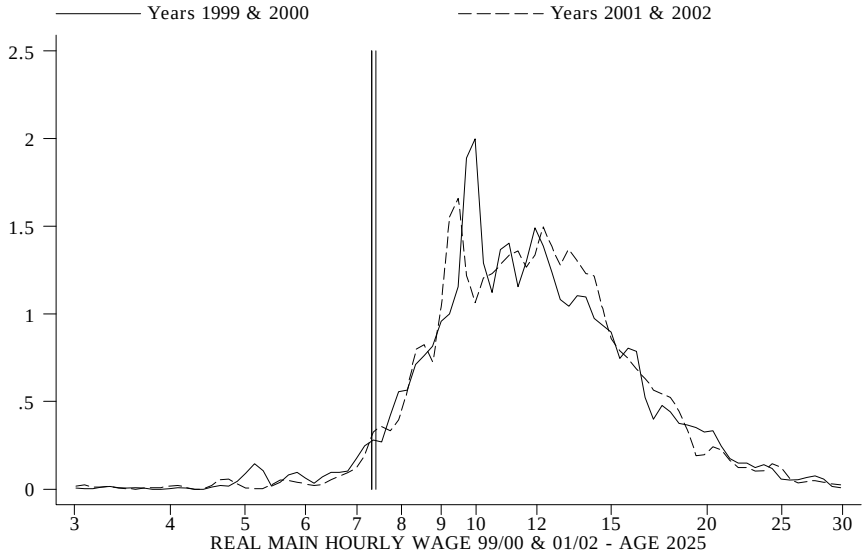


Figure 2: Hours Weighted Hourly Wage Kernel Densities

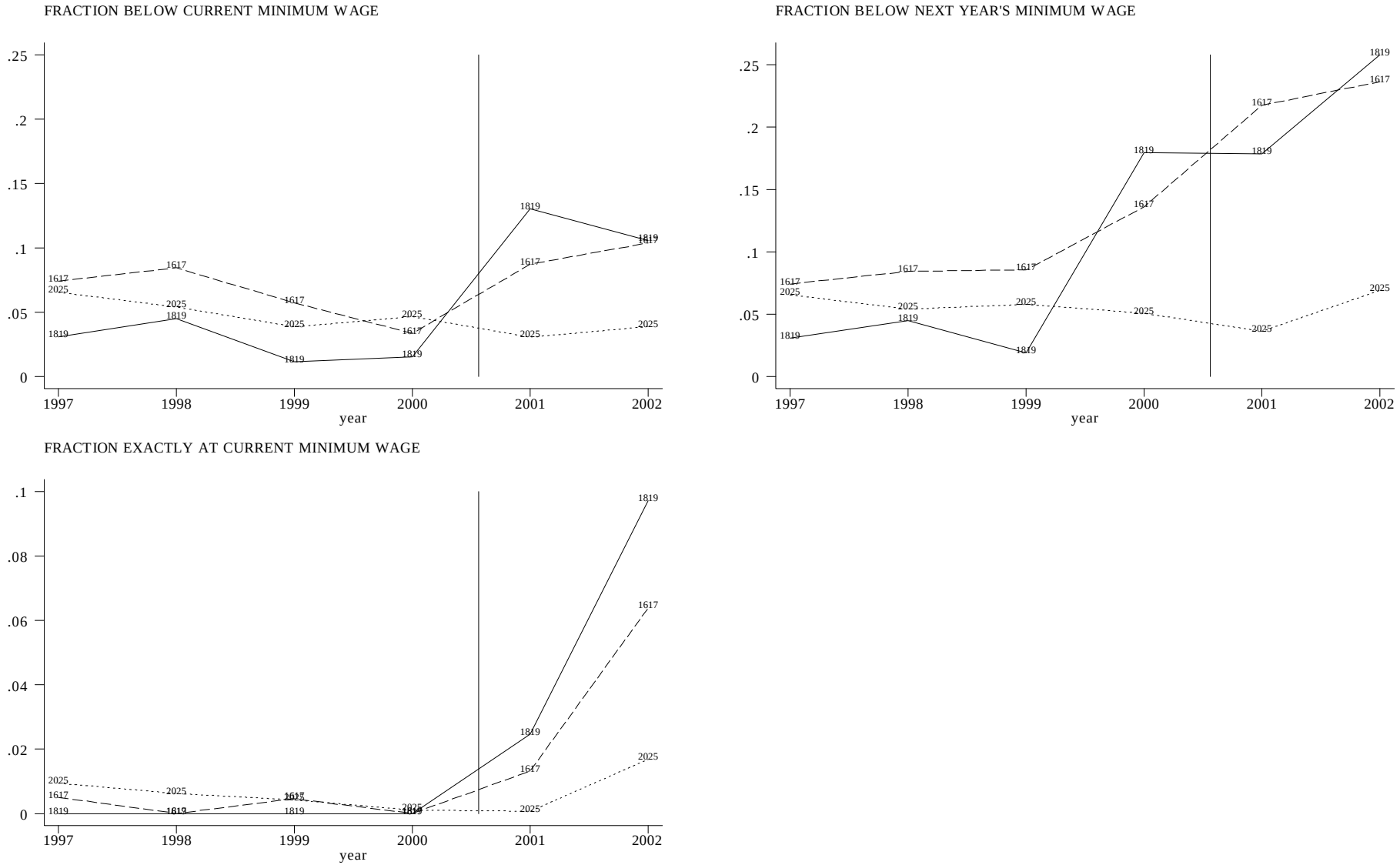


Figure 3: Fractions At and Below the Minimum Wage

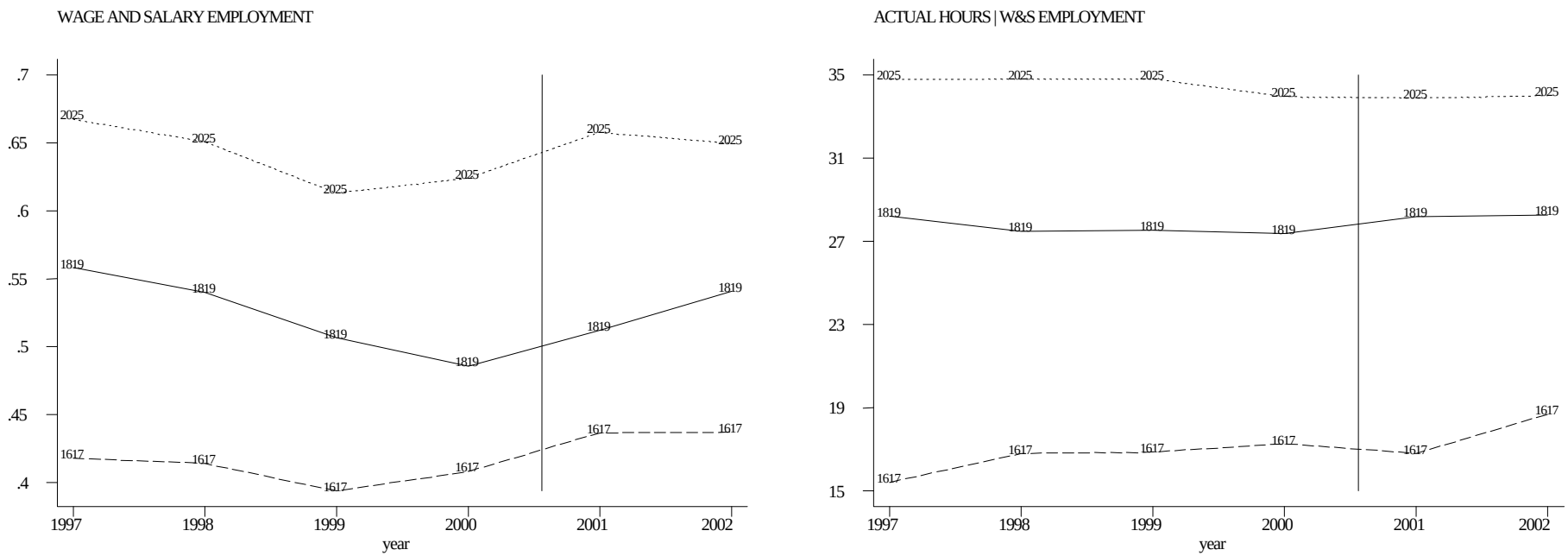


Figure 4: Wage and Salary Employment and Hours Worked

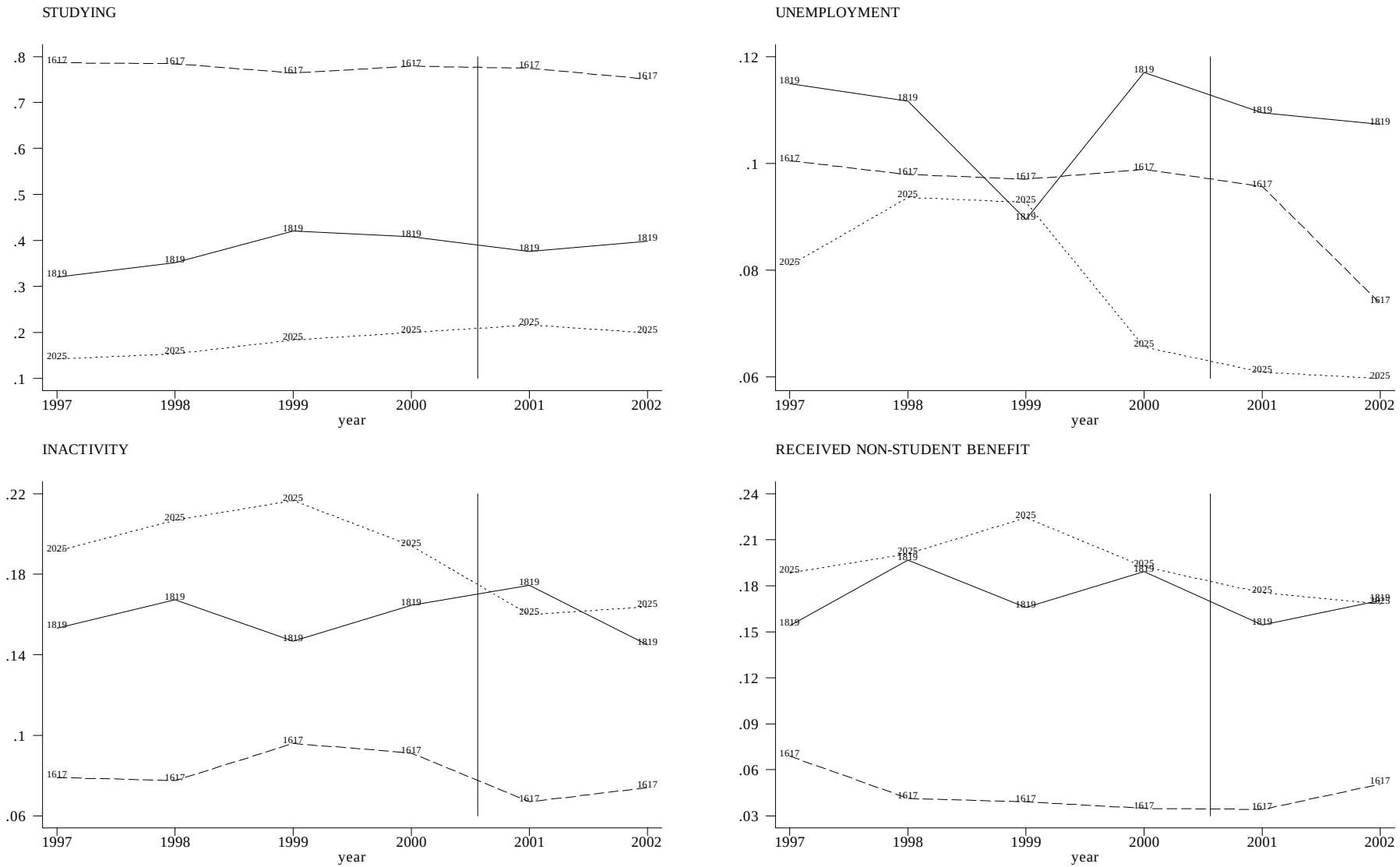


Figure 5: Studying, Unemployment, Inactivity, and Benefit Receipt

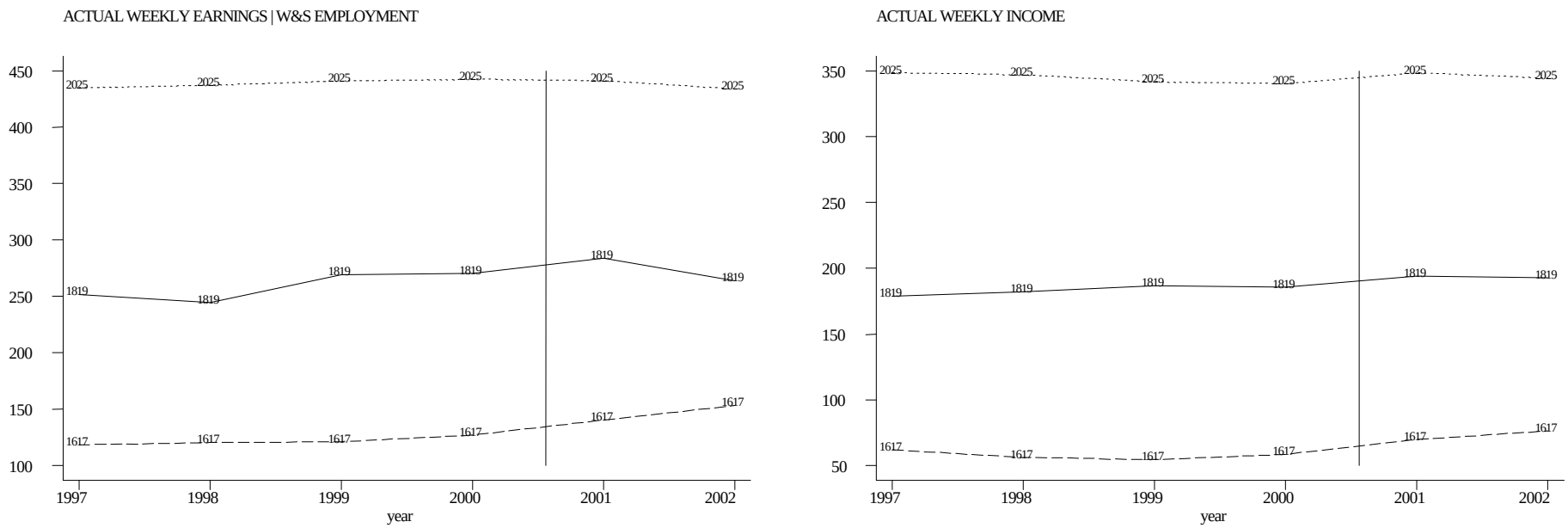


Figure 6: Weekly Labour Earnings and Total Income