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Abstract: This paper empirically examines the impact of the 2001 New Zealand minimum wage reform on the employment of 16-17 and 18-19-year-olds using administrative data from Statistics New Zealand's Integrated Data Infrastructure. This reform increased the real minimum wage of 18-19-year-olds by 68%, and 16-17-year-olds by 35% in 2001 and 2002. The impact of the minimum wage reform on employment is estimated in two phases. First, existing New Zealand empirical evidence is reproduced using survey data from the Household Labour Force Survey to test and adopt an identification method which has examined the impact of this reform and is established in the international literature. Second, using this identification method in combination with administrative data, preliminary estimates highlight that the 2001 minimum wage reform had small and positive effects on the employment of teenagers. However, findings must be interpreted with caution due to concerns with a key identification assumption.[1]

Keywords: minimum wage; employment; teenage employment, administrative data; survey data, difference-in-differences

[1] Access to the data used in this study was provided by Stats NZ under conditions designed to give effect to the security & confidentiality provisions of the Statistics Act 1975. The results presented in this study are the work of the author, not Stats NZ or individual data suppliers. These results are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) which is carefully managed by Stats NZ. For more information about the IDI please visit https://www.stats.govt.nz/integrated-data/ The results are based in part on tax data supplied by Inland Revenue to Stats NZ under the Tax Administration Act 1994 for statistical purposes. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes and is not related to the data's ability to support Inland Revenue's core operational requirements.

1 Introduction

Do minimum wage policies adversely impact the very people they are designed to help? Although minimum wage policies are intended to protect the earnings of low-income workers, a large body of empirical evidence has supported the notion that minimum wage increases will reduce the employment of these workers. This is not to say that contradictory empirical evidence has not been presented. In fact, there has long been debate surrounding impacts of minimum wage increases on employment in the international literature (Gramlich, Flanagan, & Wachter, 1976; Lester, 1946, 1960; Obenauer & von der Nienburg, 1915; Peterson, 1957, 1960; Ragan, 1977). Even once negative employment effects became the general consensus (Brown, Gilroy, & Kohen, 1982), the debate continued with two distinct groups forming. The first group held that higher minimum wages had no negative effect of employment, and in some instances have positive effects (Card, 1992a, 1992b; Card, Katz, & Krueger, 1993, 1994; Card & Krueger, 1994, 2000), with the second group remaining firm on the disemployment effects hypothesis (Neumark & Wascher, 1992, 1994, 2000). Recent evidence would suggest that debate is ongoing and that no clear agreement on the direction, size and statistical significance of the effects of minimum wage increases on employment has been reached (Aitken, Dolton, & Riley, 2019; Bailey, DiNardo, & Stuart, 2020; Bazen & Le Gallo, 2009; Böckerman & Uusitalo, 2009; Brewer, Crossley, & Zilio, 2019; Cengiz, Dube, Lindner, & Zipperer, 2019; Clemens & Wither, 2019; Dickens, Riley, & Wilkinson, 2014; Garloff, 2019; Martin, 2020).

New Zealand (NZ) has a long history of enforceable minimum wages dating back to the late 19th century with The Industrial Conciliation and Arbitration Act 1894. Along with this rich history, statutory minimum wages in NZ provide near-to-full coverage to all employees in the labour market,[2] and have been relatively high by international standards.[3] One further characteristic of the NZ minimum wage which motivates this study is the substantial variation in statutory teenage rates. The single largest increase took place in March 2001, where coverage of the adult minimum wage was lowered to workers aged 18. This reform saw the statutory minimum wage rate applicable to 18-19-year-olds increase by approximately 68% in real terms. The same reform also resulted in a combined increase of 35% in the real minimum wage rate applicable to 16-17-year-olds over the period March 2001/02. Not only were these

[2] Exemptions include: i) employees under the age of 16 years, ii) retirees, iii) the self-employed, and iv) employees with disabilities (Ministry of Business Innovation & Employment, n.d.b, n.d.g).

[3] Over the period 2001-2008, NZ ranked as having the 4th highest minimum wage relative to median wage of adult full-time workers among OECD countries at 47% The Organisation for Economic Co-operation and Development (n.d.).

increases considerably larger than what has commonly been seen in NZ,[4] but by comparison, are also large by international standards. For example, in the UK, the minimum wage jumps by an average of 19% at age 22 (Dickens et al., 2014), whereas in the Netherlands, the minimum wage rate increases by 15-17% for each calendar year between the ages of 15 and 23 (Kabátek, 2021). Finally, Danish minimum wage workers faced a large increase of 40% in their statutory minimum wage rate upon their 18th birthday (Kreiner, Reck, & Skov, 2020). Consequently, given the magnitude of the increases in the real minimum wage rate introduced by the 2001 minimum wage reform presents a favourable setting upon which to empirically assess whether substantial variation in minimum wages impact teenage employment.

Further motivation for this study is the availability of administrative data accessible through Stats NZ's Integrated Data Infrastructure (IDI). Unlike the international literature, which has adopted the use of administrative data when examining the effects of minimum wage increases on employment (Böckerman & Uusitalo, 2009; Kabátek, 2021; Kreiner et al., 2020; Liu, Hyclak, & Regmi, 2016; Pereira, 2003; Thompson, 2009), there is a notable absence of NZ empirical evidence using administrative data. In fact, survey data from the Household Labour Force Survey (HLFS) has featured in the majority of the NZ empirical studies (Chapple, 1997; Hyslop & Stillman, 2007, 2021; Maloney, 1995, 1997; Maré & Hyslop, 2021; Pacheco, 2011), with the exception being Hyslop, Maré, Stillman, and Timmins (2012) who used firm-level administrative data from the Linked Employer-Employee Data (LEED).

The final motivation for this thesis is to provide additional empirical evidence on the effects of minimum wage increases on teenage employment as there appears to be no definitive consensus among the existing NZ studies. On one hand, negative effects on teenage employment from minimum wage increases have been observed (Hyslop & Stillman, 2021; Maré & Hyslop, 2021; Pacheco, 2011), while on the other hand there has been a lack of evidence indicating adverse effects on teenage employment (Hyslop & Stillman, 2007, 2021; Maloney, 1995, 1997).[5] This is important to consider, as policy makers generally formulate minimum wage policy decisions based on the view that adverse effects on teenage employment are associated with higher minimum wages either at the aggregate or sub-group level (Ministry of Business Innovation & Employment, 2019a).

[4] Increases in the real adult minimum wage have averaged at 7% between 1985-2021 (Ministry of Business Innovation & Employment, n.d.a).
[5] When extending the impact of minimum wage increases to youth employment, empirical evidence by Chapple (1997) and Maloney (1995, 1997) observed negative effects.

By exploiting administrative data from the IDI, this study aims to contribute to the NZ minimum wage evidence by empirically analysing the employment effects of the 2001 minimum wage reform using an administrative sample. A two-step approach will be taken. First, to test identification using difference-in-differences (DID) estimation, existing NZ empirical evidence on the employment effects of the 2001 minimum wage reform will be reproduced using a survey sample from the HLFS. Next, using the DID estimation method from the first step, the teenage employment effects of the 2001 minimum wage reform will be analysed using a sample constructed using administrative data from the IDI.

Overall, the causal estimates are small and positive for all age groups examined. Although these results are somewhat unique within the NZ context, a large body of international empirical evidence have found small positive effects (Dickens et al., 2014; Garloff, 2019; Giuliano, 2013; Turner & Demiralp, 2001) or no effects (Addison, Blackburn, & Cotti, 2013; Allegretto, Dube, Reich, & Zipperer, 2017; Stewart, 2004b; Totty, 2017) of higher minimum wage on employment. However, caution must be applied when interpreting these causal estimates for policy as there are concerns regarding the crucial parallel trends assumption. Nonetheless, administrative data was clearly beneficial within the context of empirically investigating the impact of minimum wage increases on teenage employment, given its size and ability to isolate strong seasonal fluctuations in employment trends.

This study is organised as follows: Section 2 briefly summarises the development of empirical minimum wage and employment research in the US, before delving into the current body of knowledge in NZ. Section 3 then revisits survey evidence from Hyslop and Stillman (2007) to trial and adopt an existing DID regression model. The construction of the individual-level administrative sample is then summarised in section 4, followed by the estimating the effects of higher minimum wage on teenage employment from the 2001 minimum wage reform. Section 4 concludes by conducting tests for parallel trends, while section 5 outlines the conclusion and implication of this study.

2 Literature Review

Empirical evidence on the direction, magnitude, and statistical significance of minimum wage on employment was mixed as early as the 1900s, with Obenauer and von der Nienburg (1915) finding that three groups of women displayed differential pre- and post-employment outcomes following minimum wage increases. Several studies thereafter presented opposing views of minimum wage effects on employment. For example, Lester (1946) concluded that minimum wage increases had little to no influence on a firm's employment, whereas Peterson (1957) questioned the data quality of empirical studies between 1938-1950 which observed that minimum wage increases had no effects on employment.

To help resolve the debate and shed light on issues surrounding the federal minimum wage since its introduction under the Fair Labor Standards Act 1938, the US Congress created the Minimum Wage Study Commission in 1977, who were tasked with studying the social, political, and economic implications of the federal minimum wage. As part of this undertaking, a survey of the empirical literature on minimum wages and employment concluded that "time-series studies typically find that a 10 percent increase in the minimum wage reduces teenage employment by one to three percent" (Brown et al., 1982, p. 524). At the time, this conclusion became the consensus concerning the direction and magnitude of minimum wage impacts on teenage employment.

The 1990s saw reignition of the minimum wage employment debate. In contrast to the conclusions of Brown et al. (1982), empirical studies by Card (1992a, 1992b) found no evidence of adverse effects on employment from minimum wage increases, with Katz and Krueger (1992) observing that increases in the federal minimum wage had positive effects on employment at fast-food restaurants who were most affected by the increase. In contrast to these findings, Neumark and Wascher (1992) found that state-level minimum wage increases adversely impacted the employment of teenagers and young adults. These contrasting findings initiated several exchanges between these authors, with concerns regarding estimation procedures, measures of the minimum wage, methods of data collection and data integrity being noted on both sides (Card et al., 1994; Card & Krueger, 1994, 2000; Neumark & Wascher, 1994, 2000).

As with the empirical literature from the US, findings from NZ studies on the impact of minimum wage increases on employment have been somewhat mixed. Amongst the earlier studies by Maloney (1995, 1997) and Chapple (1997), empirical evidence observed the employment of young adults being negatively affected by minimum wage increases, however, findings were less than conclusive. For example, Maloney (1995) detected direct negative effects on the employment of young adults, with positive indirect effects on the employment of teenager who were not covered by statutory minimum wages at the time. Although affects were detected, these could have been impacted by the short time series of data available.[6] In the case of Chapple (1997), empirical results indicated minimum wage increases having negative effects on the employment of young adults, however, these effects were concluded as being non-robust due to concerns of auto-correlation and omitted variable bias in the equations with significant elasticities.

More recent empirical studies have also delivered mixed empirical evidence on the impact of increases in the NZ minimum wage on employment. In examining the effect of the 2001 minimum wage policy reform on teenage employment, Hyslop and Stillman (2007) concluded that there were no immediate effects on the employment of 16-17 or 18-19-year-olds, with negative, but weakly significant lagged effects observed for 16-17-year-olds. Hyslop and Stillman (2021) examined the effect of introducing the new entrant minimum wage on teenage employment.[7] Again, no immediate effects on the employment of 16-17-year-olds were observed, with lagged negative effects occurring one and two years following the policy change; positive spillover effects on the employment of 18-19-year-olds were noted (Hyslop & Stillman, 2021). The effects of binding minimum wage increases on the employment of 16-29-year-olds were examined by Pacheco (2011). First, it was observed that the introduction of the youth minimum wage in 1994 had positive effects on teenage employment. However, the overall cumulative effects of minimum wage increases were shown to be negative on the employment of 16-29-year-olds. Among the existing NZ empirical evidence, Hyslop et al. (2012) conducted the only study examining the impact of minimum wage increases on employment using firm-level administrative data from the LEED. The authors concluded that firms in the main teenager-employing industries[8] who had high-levels of teenage employment, reduced their teenage-employment share following minimum wage increases over 1999-2007. Finally, Maré and Hyslop (2021) empirically examined the effect of minimum wage increases on employment by adopting an aggregate timeseries regression model utilised within the NZ minimum wage policy setting. The authors tested two minimum wage measures,[9] with both measures resulting in negative effects on the employment of 16-17-year-olds (Maré & Hyslop, 2021). However, the authors noted that the results were underwhelming with respect to delivering robust evidence of adverse effects from minimum wage increases.

[6] Data were available from 1985:4-1993:4 (Maloney, 1995).
[7] In 2008, the new entrant minimum wage replaced the youth minimum wage.
[8] These industries were i) Retail Trade, ii) Accommodation, Cafes, and Restaurants, iii) Agriculture, Forestry and Fishing, and iv) Construction (Hyslop et al., 2012).
[9] The two measures were i) Kaitz index, and ii) A measure of the minimum wage 'bite' (Maré & Hyslop, 2021).

This summary of current NZ empirical evidence indicates that there is no definitive consensus about the impact of minimum wage increases on the employment of teenagers. Furthermore, unlike the international literature which has moved toward estimation using administrative data (Böckerman & Uusitalo, 2009; Kabátek, 2021; Kreiner et al., 2020; Liu et al., 2016; Pereira, 2003; Thompson, 2009), NZ studies have relied on survey data from the HLFS. The aim of this study, and key contribution to the NZ literature, was to address this lack of administrative evidence by using an individual-level administrative sample to generate causal estimates of the 2001 minimum wage reform on teenage employment.

3 Reproducing the Effects on Employment Using Survey Data

Step one of this study re-examined existing NZ empirical evidence on the effects of minimum wage increases on employment. The purpose of this re-examination was to test an existing empirical model estimated using survey data, for use in later empirical analysis with a sample constructed from administrative data.

The empirical study by Hyslop and Stillman (2007) was selected for re-examination for several reasons. First, it empirically examined the impact of the 2001 minimum wage reform on teenage employment. This was an attractive feature of their study given the magnitude of the increases in the real minimum wage rates for 16-17 and 18-19-year-olds (35% over 2001/02 & 68%, respectively) as part of the reform. Next, it adopted a DID identification strategy, which has featured prominently in the international literature (Aitken et al., 2019; Card, 1992b; Card & Krueger, 1994; Dube, Lester, & Reich, 2010; Stewart, 2004a). Finally, within the context of the NZ empirical minimum wage literature, their study was influential in progressing the body of knowledge by presenting alternative conclusions with respect to the minimum wage and teenage employment relationship.

Using the replication framework presented by Reed (2017), the re-examination of the study by Hyslop and Stillman (2007) was defined as a reproduction, which is "the act of attempting to duplicate the findings from an original study" (p. 4). The reproduction analysis focussed on the teenage employment effects of the 2001 minimum wage policy reform. In attempting to successfully reproduce those results, this study followed as closely as feasible the methods documented in the study by Hyslop and Stillman (2007), including data sources, sample selection and regression specifications.[10]

3.1 Data and sample characteristics

Data were taken from the HLFS and restricted to a sample population of 16-25-year-olds over the period 2007q1-2003q3. Table 1 presents a selection of characteristics from the sample. Following the approach of Hyslop and Stillman (2007), the characteristics of two distinct sub-groups were included: i) the full sample, and ii) a wage and salary worker sample.

Table 1: Sample Characteristics from the Reproduction Analysis
Sample Characteristics from the Reproduction Analysis

	Hyslop and Stillman (2007)[a]		Reproduction Samples	
	Full	Workers	Full	Workers
Age	20.38	20.83	20.39	20.84
	(0.03)	(0.03)	(0.01)	(0.01)
Female	.49	.47	.50	.48
	(0.00)	(0.00)	(0.00)	(0.00)
Married	.18	.20	.18	.20
	(0.00)	(0.01)	(0.00)	(0.00)
NZ born	.84	.88	.84	.88
	(0.01)	(0.00)	(0.00)	(0.00)
Pākehā	.67	.76	.65	.73
	(0.01)	(0.001)	(0.00)	(0.00)
Māori	.15	.12	.18	.15
	(0.00)	(0.00)	(0.00)	(0.00)
Pacific Islander	.07	.06	.07	.06
	(0.00)	(0.00)	(0.00)	(0.00)
Asian	.06	.03	.06	.03
	(0.00)	(0.00)	(0.00)	(0.00)
Wage and salary worker	.58	1	.60	1
	(0.00)		(0.00)	
Hours worked last week	30.50	30.50	32.83	32.83
	(0.20)	(0.20)	(0.07)	(0.07)
No. proxies	43,485	25,151	43,485	25,152
Observations	125,486	70,993	125,487	70,992

Note. Standard errors are in parentheses. All summary statistics are weighted by the HLFS sampling weights. Author's compilation.
[a] Results are taken from Table 2, columns (1)-(2) in Hyslop and Stillman (2007, p. 208).

For the purpose of comparison, the corresponding sample characteristics reported by Hyslop and Stillman (2007) were also included in Table 1. Compared to the original sample, the characteristics from the reproduced sample were largely consistent. The characteristics of the full sample indicated an even gender mix, with approximately one-fifth of the sample being

[10] The authors also kindly provided guidance and several code files to assist in this process.

married and a large majority born in NZ. In terms of ethnicity, 65% reported being Pākehā, 18% Māori, with Pacific Islander and Asian accounting for relatively smaller proportions of the sample.[11] Approximately 60% were employed as wage and salary workers, spending around 32 hours working each week. Overall, the sample consisted of 125,487 observations and included 35% proxy responses.

For workers, there were proportionately fewer females, with one-fifth of the sample being married, and the majority born in NZ. A large proportion of workers reported being Pākehā (73%), followed by Māori (15%). Overall, the wage and salary worker sample consisted of 70,992 observations and included 35% proxy responses.

3.2 Regression results and evaluation

The following base specification, as outlined by Hyslop and Stillman (2007), was adopted:

$$Y_{it} = \delta_{16-17} * (age16-17_{it} * Post-2001) + \delta_{18-19} * (age18-19_{it} * Post-2001) + X'_{itj}\beta + u \quad (1)$$

where Y_{it} is employment (defined as a dummy = 1 if employed; 0 otherwise), *age*16-17$_{it}$ and *age*18-19$_{it}$ are dummy variables for the respective treatment groups, *Post*-2001 is a dummy variable for the post-reform period, X_{itj} is a vector of *j* relevant demographic controls for individual *i* at time *t*, and *u* is an error term to capture unobserved effects. The primary focus is on $\delta_{16\text{-}17}$ and $\delta_{18\text{-}19}$, which is the effect of the 2001 minimum wage reform on teenage employment, controlling for other factors. Overall, seven regression specifications were estimated,[12] with the results from the final two specifications reported here.[13]

Regression results for specifications (6)-(7) are available in Table 2. The corresponding regression results from the original study are also included, which will subsequently be used to evaluate the reproduction results.

[11] Non-prioritised ethnicity dummies were also created by way of robustness checks. In these instances, the proportion of Pakeha increased to 70%, with Māori decreasing to 14%. The distribution of the remaining ethnicities remained unchanged. Regression results from specifications (6) and (7) in Table 2 were robust to the use of non-prioritised ethnicity dummies.
[12] The definitions of all regression specifications are presented in Appendix A, Table A1.
[13] The regression results for specifications (1)-(5) are available in Appendix A, Table A2.

Table 2: Evaluation of Regression Results from the Reproduction Analysis

Evaluation of Regression Results from the Reproduction Analysis

	Hyslop and Stillman (2007)[a]		Reproduction	
	(6)	(7)	(6)	(7)
Age 16-17 * Post-2001				
2001	0.006	-0.001	-0.003	-0.010
	(0.013)	(0.018)	(0.018)	(0.025)
			[-0.037, 0.031]	[-0.060, 0.039]
2002	0.006	-0.007	0.008	-0.002
	(0.013)	(0.022)	(0.016)	(0.027)
			[-0.024, 0.040]	[-0.054, 0.050]
2003	-0.023	-0.039*	-0.018	-0.028
	(0.015)	(0.022)	(0.018)	(0.032)
			[-0.053, 0.017]	[-0.091, 0.035]
Age 18-19 * Post-2001				
2001	-0.005	-0.005	-0.012	-0.008
	(0.013)	(0.015)	(0.017)	(0.024)
			[-0.050, 0.021]	[-0.054, 0.039]
2002	-0.001	-0.002	-0.011	-0.006
	(0.010)	(0.017)	(0.016)	(0.026)
			[-0.043, 0.020]	[-0.056, 0.044]
2003	-0.021	-0.021	-0.025	-0.018
	(0.013)	(0.020)	(0.018)	(0.032)
			[0.061, 0.010]	[-0.080, 0.044]
Age 20-21 * Post-2001				
2001	0.030**	0.019	0.022	0.015
	(0.013)	(0.016)	(0.017)	(0.024)
			[-0.011, 0.055]	[-0.032, 0.061]
2002	0.009	-0.009	-0.002	-0.009
	(0.011)	(0.020)	(0.016)	(0.026)
			[-0.034, 0.031]	[-0.059, 0.041]
2003	-0.012	-0.034	-0.013	-0.023
	(0.015)	(0.026)	(0.018)	(0.031)
			[-0.048, 0.022]	[-0.084, 0.038]
R^2	.11	.11	.12	.12
Observations	125,422	125,422	125,487	125,487

Note. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5% and 1% level, respectively. All specifications are estimated by OLS. Huber-White robust standard errors are in parentheses. 95% confidence intervals are provided in square brackets. The covariates include: dummy variables for individual–age and quarter, gender, marital status, ethnicity, NZ born, urbanicity and region of residence, and the relative size of the population of each age group (16-17, 18-19, 20-21 and 22-25) in a particular year. Author's compilation.

[a] Results taken from Table 4, columns (6)-(7) in Hyslop and Stillman (2007, p. 222)

Specification (6) added controls for age-specific seasonal variation in employment by interacting quarterly and age-specific dummy variables. Overall, the regression estimates suggested small, though mixed, effects on the employment of 16-17-year-olds from the minimum wage increases across all three post-reform years. None of the regression estimates for this age group were statistically significantly different from zero. For 18-19-year-olds, the regression estimates indicated negative impacts on their employment in the range of 1-3 percentage points across the three post-reform years. Again, none of these regression estimates were statistically significant. Finally, for 20-21-year-olds, the regression estimates were also mixed, both in direction and magnitude. The estimated impact immediately following the 2001 minimum wage reform appeared to reflect positive spillover effects on the employment of this age group, though estimates were not statistically significantly different from zero.

Specification (7) added controls to allow for age-specific responses to the business cycle by interacting the prime-age unemployment rate with age-specific dummy variables. Even when controlling for age-specific responses to the business cycle, the regression results from this specification were remarkably similar with those from specification (6) for each of the three age groups and post-reform years. Again, none of the regression estimates were statistically significantly different from zero, and thus no evidence of the minimum wage increases impacting the employment of teenagers were observed.

To evaluate the reproduction results, the guide presented by LeBel, Vanpaemel, Cheung, and Campbell (2019) was utilised.[14] As part of their guide, LeBel et al. (2019) highlighted three key areas to consider when evaluating the outputs of individual studies, including: i) whether any signal was observed,[15] ii) the consistency of the replication effect size (ES) relative to the original study, and iii) the relative precision of the replication ES compared to the original study. Once these areas are evaluated, the outcome of the replication study can be classified as consistent or inconsistent with the empirical results of the original study.[16] To apply the framework, the reproduced empirical results from specifications (6)-(7) were evaluated against those reported by Hyslop and Stillman (2007) for the same specifications.

For specification (6), Hyslop and Stillman (2007) only reported a statistically significant point estimate for 20-21-year-olds in 2001 ($\delta_{20\text{-}21} = 0.030$, $p < 0.05$).; all other point estimates were not statistically different from zero. Starting with the result for 20-21-year-olds in 2001, the reproduced regression model did not yield a statistically significant point estimate ($\delta_{20\text{-}21} = 0.022$, $p > 0.10$). However, the 95% confidence interval associated with the reproduced point estimate for 20-21-year-olds in 2001 included zero as well as the point estimate from the original study, 95% CI [-0.011, 0.055]. Based on these observations, although no signal was

[14] The concept of replication was not clearly defined by LeBel et al. (2019); however, their guidelines appear to support any study looking to reproduce existing empirical evidence.
[15] LeBel et al. (2019) presented Pearson's *r* correlations as measures of 'signal'. In this evaluation, statistically significant point estimates were used to determine whether a 'signal' was observed.
[16] See Appendix A, Table A3 for the criteria used to classify the reproduction results.

detected, the effect size was consistent with the original one. Moving on to the remaining point estimates, like Hyslop and Stillman (2007), the reproduced regression model did not yield any statistically significant results. In each instance, the 95% confidence interval included zero, as well as the comparable point estimates from the original study. Consequently, these results could also be classified as consistent with those from the original study.

The evaluation of the reproduced regression results from specification (7) were largely the same as specification (6). That is, Hyslop and Stillman (2007) only reported one statistically significant point estimate for 16-17-year-olds in 2003 ($\delta_{16\text{-}17} = -0.039$, $p < 0.01$). The comparable point estimate from the reproduced regression model was not statistically significant; however, the 95% confidence interval included zero as well as the point estimate from the original study ($\delta_{16\text{-}17} = -0.028$, $p > 0.10$, 95% CI [-0.091, 0.035]). The remaining point estimates were all similar in magnitude and the same direction, with the 95% confidence intervals including zero as well as the point estimates from the original study. Based on these considerations, the reproduced results from specification (7) were also classified as consistent with those from the original study.

Overall, the original study observed no immediate effects on teenage employment following the minimum wage policy change, with negative, but weakly significant lagged effects observed for 16-17-year-olds. The reproduced empirical model generated results which supported these findings. Consequently, the empirical specifications trialled in this reproduction analysis were deemed suitable for application in the next step of this study.

4 Examining the Effects on Employment Using Administrative Data

Existing NZ empirical studies have heavily relied on survey data from the HLFS to investigate the impact of higher minimum wages on employment (Chapple, 1997; Hyslop & Stillman, 2007, 2021; Maloney, 1995, 1997; Pacheco, 2011). Step two of this paper draws on data from Stats NZ's IDI to empirically examine the effects of minimum wage increases on teenage employment using individual-level administrative data in combination with the regression specification from step one.

4.1 Data and sample characteristics.

Data were taken from several sources in the IDI to construct an administrative sample population. There are several benefits of using the administrative data from the IDI. First, it offers population-level data, thus providing a larger sample to utilise for empirical examination when compared to survey data from the HLFS. Next, self-reported survey data may suffer from non-response or attrition of survey respondents. This is not the case with administrative records, as data are based on official records from participating government and non-government agencies, and include longitudinal data on all individuals (e.g., full earnings and employment history from tax records). Finally, administrative data in the IDI offers earnings and employment data at a monthly-level, as opposed to quarterly (for employment in the HLFS) or annually (for earnings or wages in the HLFS-Income Survey [HLFS-IS]). This allows for additional variation of outcome variables to be exploited in empirical analyses, which is of particular benefit when assessing teenage employment given the strong seasonal nature of their labour market participation.

The IDI is a research database made up of a collection of data sets from various government and non-government agencies and Stats NZ surveys. The data sets are linked at the individual-level, and when combined with the longitudinal nature of the IDI, enable researchers to undertake policy evaluations and examine the economic and social transitions of people in NZ (Black, 2016).

The administrative sample population consisted of individuals aged 16-25 years over the period April 1999-September 2003. The sample was also purged of individuals who have left the population due to death or migration. Deceased individuals were identified using life events data. Based on month and year of death, deceased individuals were removed from the sample for all periods following their death. Individuals who have left NZ due to permanent migration or short-term visits were also identified and removed from the sample using border movement information and by adopting a calendar year version of the '12/16 rule' (Gibb, Bycroft, & Matheson-Dunning, 2016; Stats NZ, 2017b). By calculating the total number of days out of NZ using the start and end date of travel spells, individuals were removed from the sample if they were present in NZ for less than 9-months within each calendar year.

The key limitation of the border movement information is that coverage only starts in September 1997. Thus, individuals who were born in NZ and permanently migrated prior to this date were not identifiable using border movements alone. To address this limitation, individuals could be identified using activity in administrative data sources over a period of 12-months (Gibb et al., 2016) or 24-months ((Stats NZ, 2017b)) prior to a reference period. However, due to coverage period misalignment of multiple data sources, particularly secondary school enrolment data, the administrative sample was constructed without using activity indicators. Although the resulting sample included noise (i.e., retaining some non-residents), the noise would be consistent across the sample and would therefore not introduce bias to any specific age group or period within the study.[17]

Mean values of employment and individual characteristics of the administrative sample are presented in Table 3. Overall, 50% were employed with a mean age of approximately 20 and a half years. The sample was evenly split by gender and largely made up of individuals born in NZ, and of Pākehā ethnicity. Māori made up just under one-fifth of the sample, with the remaining ethnicities accounting for 17%. The final sample consisted of 29,442,717 person-month observations, which were comprised of 889,989 unique individuals.

Table 3:
Characteristics of 16-25-year-olds Using an Administrative Sample

Characteristics	Mean
Employment	
Employed	.50
	(0.00)
Individual	
Age	20.44
	(0.00)
Female	.50
	(0.00)
Married	.04
	(0.00)
NZ born	.82
	(0.00)
Pākehā	.53
	(0.00)
Māori	.18
	(0.00)
Pacific Islander	.07
	(0.00)
Asian	.06
	(0.00)
MELAA	.02
	(0.00)
Other	.02
	(0.00)
Ethnicity not specified	.12
	(0.00)

[17] The steps in constructing the administrative sample are outlined in Appendix B, Table B1, with Table B2 summarising the definitions and data sources of all variables utilised in these empirical analyses.

Data	
Total observations	29,442,717
Total unique individuals	889,989

Note. The sample covers the period April 1999-September 2003. Standard errors are in parentheses. Author's compilation.

4.2 Review of employment trends.

Figure 1 depicts mean employment for 16-17, 18-19 and 20-25-year-olds over the period April 1999-September 2003. The two solid vertical lines represent the months where minimum wages were increased as part of the 2001 minimum wage reform for 18-19-year-olds (in April 2001) and 16-17-year-olds (in April 2001 & April 2002). At the first vertical line, coverage of the adult minimum wage was lowered to 18-19-year-olds, increasing this real minimum wage rate by 68%. The youth minimum wage, applicable to 16-17-year-olds increased to 70% and 80% of the adult minimum wage in April 2001 and April 2002, respectively.

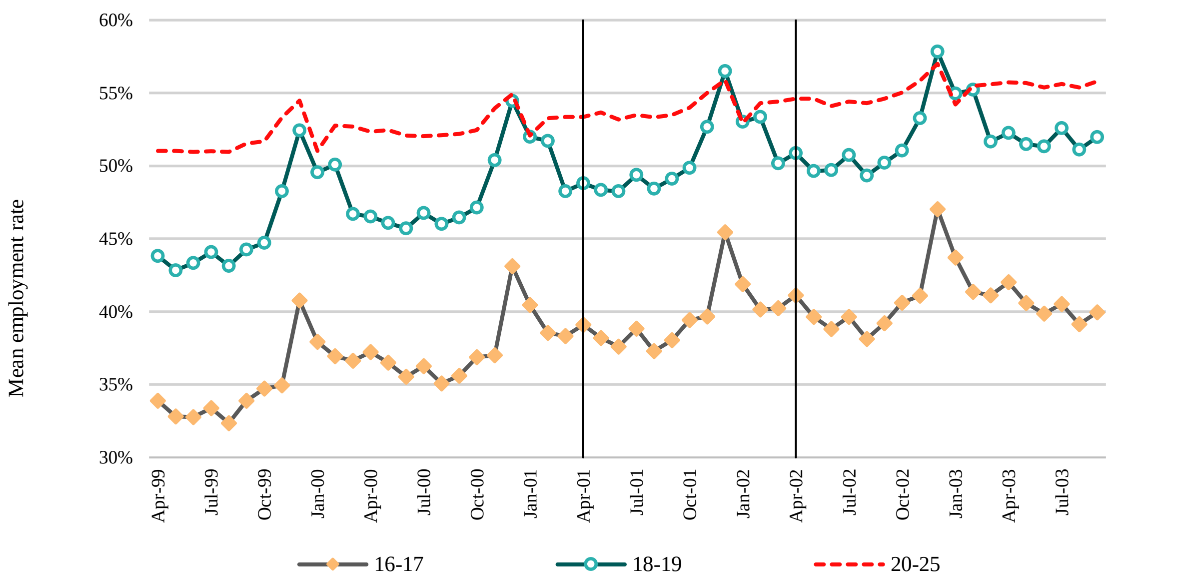


Figure 1: Employment rate by age groups.
The sample covers the period April 1999-September 2003. Values on the y-axis start at 30% to clearly highlight the variation in the employment rates over time. Author's compilation.

Focussing on the two groups of teenagers targeted by the 2001 minimum wage policy reform, mean employment increased over the sample period by approximately 6 percentage points for 16-17-year-olds, and 8 percentage points for 18-19-year-olds. Additionally, mean employment of teenagers saw a sharp rise over summer holiday periods where their participation in the labour market increases. For 16-17-year-olds, mean employment increased by an average of 6 percentage-points from November-December; 18-19-year-olds saw an average increase of 7 percentage points in their mean employment from October-December.

Employment growth appeared to slow for both groups of teenagers when considering periods of policy implementation. Prior to the 2001 minimum wage policy reform, mean employment increased by 4 percentage points for both 16-17 and 18-19-year-olds. During the first period following the policy implementation (April 2001-March 2002), mean employment for both groups of teenagers grew by only 1 percentage point. The 2001 minimum wage policy reform also introduced a second increase in the statutory minimum wage rate for 16-17-year-olds in April 2002. Mean employment of 16-17-year-olds fell by 1 percentage point between April 2002-September 2003. These initial observations suggest that there may have been some impact on teenage employment from the 2001 minimum wage policy reform, supporting further empirical investigation.

4.3 Simple difference-in-differences' estimates on employment.

The analysis continues with a simple DID estimation of the employment impact of the 2001 minimum wage policy reform. Table 4 presents the average employment outcomes over the pre-treatment (April 1999-September 2000) and post-treatment (April 2001-September 2003) periods for 16-17, 18-19 and 20-25-year-olds.[18] The difference between the pre- and post-treatment periods is also presented, along with differences in employment outcomes for 16-17 and 18-19-year-olds when compared to 20-25-year-olds. The shaded cells represent the simple DID estimates of the impact of the 2001 minimum wage reform on the employment of 16-17 and 18-19-year-olds.

Table 4:
Simple DID Estimates of Changes in Teenage Employment

	Age group			Difference (from 20-25-year-olds)	
	16-17	18-19	20-25	16-17	18-19
Pre-treatment period	0.359	0.469	0.526	-0.167	-0.058
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	[2,008,167]	[2,028,756]	[5,975,418]	[7,983,582]	[8,004,174]
Post-treatment period	0.413	0.526	0.558	-0.145	-0.032
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	[2,006,448]	[1,980,798]	[5,743,575]	[7,750,023]	[7,724,373]
Difference	0.054	0.057	0.031	**0.022*** **	**0.026*** **
	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)
	[4,014615]	[4,009,554]	[11,718,993]	[15,733,605]	[15,728,547]

[18] The dates for the post-treatment period accounts for the second stage of wage increases for 16-17-year-olds. The pre-treatment period was selected to ensure a balanced seasonal sample and excluded the date of the announcement of the upcoming minimum wage reforms (December 2000).

Note. The pre- and post-treatment periods are April 1999-September 2000 and April 2001-September 2003, respectively. Standard errors in parenthesis. Number of observations in square brackets. Numbers in bold are the simple DID estimate average treatment effects. Three stars indicate that these DID estimates are significantly different from zero at the 1% level. Author's compilation.

Positive employment changes were observed for all three age groups when comparing mean employment in the pre- and post-treatment periods. When comparing the differences in the employment outcomes of both treatment groups against those of 20-25-year-olds, the DID estimate indicates positive impacts for these teenagers. For 16-17-year-olds, the DID estimate shows a positive change in their mean employment of 2.2 percentage points ($p < 0.01$). Similarly, a positive change of 2.6 percentage points ($p < 0.01$) in the mean employment of 18-19-year-olds is also observed.

4.4 Difference-in-differences regressions results.

Although the simple DID analysis estimated positive effects on the employment of both treatment groups following the 2001 minimum wage reform, more sophisticated empirical analysis is required to account for other factors which may have influenced teenage employment over the sample period. To that end, the following base specification was adopted to estimate the effects of the 2001 minimum wage reform on teenage employment:

$$Y_{it} = \delta_{16-17} * (age16 - 17_{it} * Post - 2001) + \delta_{18-19} * (age18 - 19_{it} * Post - 2001) + X'_{itj}\beta + u \quad (8)$$

where Y_{it} is employment (defined as a dummy = 1 if employed; 0 otherwise) measured at a monthly-level as opposed to a quarterly level in the reproduction analysis. $age16\text{-}17_{it}$ and $age18\text{-}19_{it}$ are dummy variables for the respective treatment groups, with *Post*-2001 being a dummy variable for the post-reform period, X_{itj} is a vector of j relevant demographic controls for individual i at time t, and u is an error term to capture unobserved effects. Again, the primary focus is on $\delta_{16\text{-}17}$ and $\delta_{18\text{-}19}$, which is the effect of the 2001 minimum wage reform on teenage employment, controlling for other factors.

Although specification (8) was largely the same as specification (1) used in the survey reproduction analysis, there are differences to note with respect to definitions of variables under the vector X_{itj} (e.g., married & ethnicity dummies).[19] Furthermore, as in the reproduction analysis, several iterations of specification (8) were estimated. However, a key difference

[19] In the administrative sample, the married dummy captured instances of legal marriages registered in NZ only (as defined by the DIA), while in the HLFS survey sample, the married dummy captured instances of couples being legally married or living together as married (Stats NZ, 2015c, p. 24). In specification (8), an additional ethnicity dummy was included for 'Middle Eastern, Latin American and African' (MELAA). In the survey reproduction analysis, MELAA would likely have been captured under 'Other ethnicities'.

between the specifications estimated here, and those estimated in the survey reproduction analysis, is that no proxy controls were added as individuals are not identifiable in the IDI.[20] In all cases, the differences with respect to variable definitions and inclusion of controls under X_{itj} reflect the transition of estimating the same regression specification using survey and administrative data. Although every effort was made to be consistent in all facets of the estimation procedure, differences are inevitable due to the nature and purpose of household survey data and sources of administrative data.[21]

The regression results from all specifications are presented in Table 5. The base specification controlled for various individual characteristics and the relative size of each age group in a particular year. The regression results pointed toward small, positive and statistically significant effects on the employment of 18-19-year-olds ($\delta_{18\text{-}19} = 0.008$, $p < 0.01$). Holding all other factors constant, 18-19-year-olds were 0.8 percentage points more likely to be employed following the minimum wage policy change. No evidence of minimum wage impacts for 16-17-year-olds were observed.

Specification (9) controlled for potential spillover effects on the employment of 20-21-year-olds. Although the regression results highlighted negative and statistically significant impacts on their employment ($\delta_{20\text{-}21} = -0.003$, $p < 0.05$), the magnitude of these effects was negligible. The results for both groups of teenagers remained consistent with those from the base specification - that is, there was no statistically significant effect on the employment of 16-17-year-olds, and positive effects on the employment of 18-19-year-olds ($\delta_{18\text{-}19} = 0.007$, $p < 0.01$).

[20] No data were available concerning whether the records were attained from the individual to whom the records were assigned (i.e., the UID), or from an alternative agent representing the individual.

[21] The purpose of the HLFS is to produce official labour market statistics (Stats NZ, 2015c), whereas administrative data are collected for the "purposes of registration, transaction and record keeping, and administrative data are often associated with the delivery of a service" Woollard (2014, p. 49).

Table 5:

Estimated Effects on Employment from the 2001 Minimum Wage Reform Using an Administrative Sample

	(8)	(9)	(10)	(11)	(12)	(13)
Age 16-17 * Post-2001	0.001	-0.001				
	(0.001)	(0.001)				
	[-0.002, 0.003]	[-0.003, 0.002]				
2001			-0.002*	-0.004*	0.011***	0.012***
			(0.001)	(0.002)	(0.002)	(0.002)
			[-0.005, 0.000]	[-0.000, 0.007]	[0.007, 0.015]	[0.007, 0.016]
2002			0.004**	0.006***	0.014***	0.014***
			(0.002)	(0.002)	(0.002)	(0.003)
			[0.001, 0.007]	[0.003, 0.010]	[0.010, 0.018]	[0.009, 0.020]
2003			-0.005***	0.001	0.014***	0.015***
			(0.002)	(0.002)	(0.002)	(0.002)
			[-0.008, -0.001]	[-0.003, 0.005]	[0.010, 0.019]	[0.010, 0.018]
Age 18-19 * Post-2001	0.008***	0.007***				
	(0.001)	(0.001)				
	[0.006, 0.011]	[0.004, 0.010]				
2001			0.008***	0.008***	0.017***	0.014***
			(0.002)	(0.002)	(0.002)	(0.002)
			[0.005, 0.011]	[0.004, 0.011]	[0.013, 0.020]	[0.010, 0.018]
2002			0.010***	0.008***	0.017***	0.013***
			(0.002)	(0.002)	(0.002)	(0.003)
			[0.007, 0.013]	[0.004, 0.012]	[0.013, 0.021]	[0.008, 0.019]
2003			-0.001	0.003	0.022***	0.017***
			(0.002)	(0.002)	(0.002)	(0.003)
			[-0.005, 0.003]	[-0.001, 0.007]	[0.018, 0.026]	[0.011, 0.024]
Age 20-21 * Post-2001		-0.003**				
		(0.001)				
		[-0.006, -0.000]				
2001			-0.004**	-0.000	0.006***	0.006***
			(0.001)	(0.002)	(0.002)	(0.002)
			[-0.006, -0.001]	[-0.004, 0.004]	[0.002, 0.010]	[0.002, 0.010]
2002			-0.000	0.001	0.008***	0.007**
			(0.002)	(0.002)	(0.002)	(0.003)
			[-0.004, 0.003]	[-0.003, 0.005]	[0.003, 0.012]	[0.002, 0.012]
2003			-0.008***	-0.003	0.009***	0.008**
			(0.002)	(0.002)	(0.002)	(0.003)
			[-0.011, -0.004]	[-0.007, 0.001]	[0.005, 0.013]	[0.002, 0.014]
R2	.20	.20	.20	.20	.20	.20

Note. The sample covers the period April 1999-September 2003. All specifications are estimated by OLS on 29,442,717 observations. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5% and 1% level, respectively. Huber-White robust standard errors are presented in parenthesis. These are clustered by UID, allowing for correlation within UID, but independence between UIDs. 95% confidence intervals are provided in square brackets. The covariates include dummy variables for individual–age and quarter, gender, marital status, ethnicity, NZ born, urbanicity and region of residence, and the relative size of the population of each age group (16-17, 18-19, 20-21 and 22-25) in a particular year. Author's compilation.

Specification (10) allowed the impact of the policy change to vary over the individual post-reform years. Regression results were mixed across the age groups and post-reform years. For the employment of 16-17-year-olds, there were immediate small, negative and weakly significant effects in 2001 ($\delta_{16\text{-}17} = -0.002$, $p < 0.10$), positive effects in 2002 ($\delta_{16\text{-}17} = 0.004$, $p < 0.05$), and negative effects in 2003 ($\delta_{16\text{-}17} = -0.005$, $p < 0.01$). Notwithstanding the changing direction of these impacts, their magnitudes were small. The regression results were more consistent for 18-19-year-olds when compared with those from earlier specifications. When holding all other factors constant, 18-19-year-olds were 0.8 percentage points ($p < 0.01$) more likely to be employed in 2001, and 1 percentage point more likely to be employed in 2002 ($p < 0.01$), following the minimum wage policy change. Negative effects on the employment of 20-21-year-olds from minimum wage increases were estimated. These impacts occurred immediately in 2001 ($\delta_{20\text{-}21} = -0.004$, $p < 0.05$), and again in 2003 ($\delta_{20\text{-}21} = -0.008$, $p < 0.01$).

Specification (11) controlled for announcement effects of the minimum wage reform prior to implementation, as well as announcement effects of annual increases occurring post-implementation. The regression results were once again mixed for 16-17-year-olds. When holding all other factors constant, 16-17-year-olds were 0.4 percentage points ($p < 0.10$) less likely to be employed immediately following the minimum wage policy change, and by 2002, were 0.6 percentage points more likely to be employed ($p < 0.01$). Positive effects on employment were again estimated for 18-19-year-olds in both 2001 ($\delta_{18\text{-}19} = 0.008$, $p < 0.01$) and 2002 ($\delta_{18\text{-}19} = 0.008$, $p < 0.01$). In this specification, there were no statistically significant effects on employment estimated for 20-21-year-olds.

Specification (12) controlled for age-specific seasonal effects. Overall, the results from this specification indicated small, positive and statistically significant effects on the employment of all three age groups. For example, following the minimum wage reform, 16-17-year-olds were 1.1 to 1.4 percentage points more likely to be employed ($p < 0.01$ for all three estimates), holding all other factors constant. The effects on employment estimated for 18-19-year-olds were larger than those for 16-17-year-olds, as well as larger than those estimated in previous specifications. Specifically, effects on employment were estimated to be in the range of 1.7 to 2.2 percentage points over the three post-reform years ($p < 0.01$ for all three estimates). The magnitude of the effects on employment for 20-21-year-olds were comparatively smaller, with estimates ranging from 0.6 to 0.9 percentage points ($p < 0.01$ for all three estimates).

Specification (13) controlled for age-specific business cycle effects. Here, the regression results were consistent with those from specification (12), with small, positive and statistically significant effects on employment observed across all age groups. For 16-17 and 20-21-year-olds, the estimated effects on their employment from this specification were near identical to those estimated in the previous specification. For 18-19-year-olds, the estimated effects on their employment were marginally smaller than those from specification (12), albeit consistent in direction and level of statistical significance. The estimates indicated that following the minimum wage change, 18-19-year-olds were 1.3 to 1.7 percentage points more likely to be employed ($p < 0.01$ for all three estimates), holding all other factors constant.

Overall, regression estimates from all specifications indicated that the 2001 minimum wage reform had small impacts on the employment of both treatment groups. For 16-17-year-olds, these effects were somewhat mixed, with small negative and positive effects observed, depending on the specification adopted. For 18-19-year-olds, the empirical evidence was consistent and showed small positive effects on employment across all specifications. However, pending the examination of the key identification assumption underpinning DID estimation, being parallel trends, the empirical results should still be interpreted with some caution.

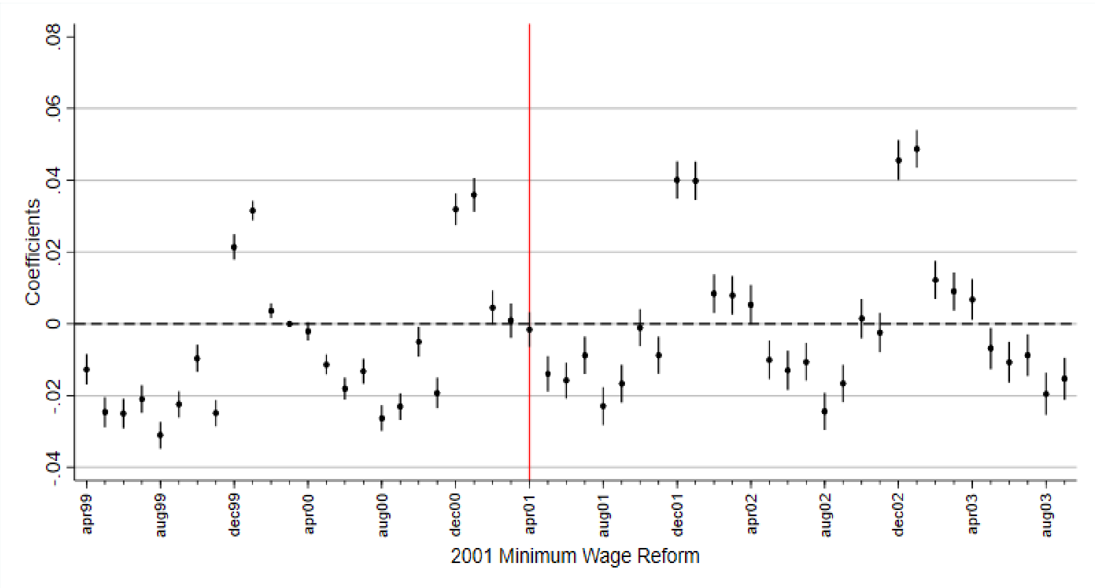
4.5 Examining the parallel trends assumption.

The credibility of the causal estimates presented in Table 5 rely on the key identifying assumption of parallel trends. This means that parallel trends assume that the untreated units (20-25-year-olds) provide an appropriate counterfactual of the employment trend that the treated units (16-17 and 18-19-year-olds) would have followed if they were never treated (McKenzie, 2020). In the context of this study, it would need to be assumed that if the 2001 minimum wage policy reform did not take place, the employment trend of 20-25-year-olds would have provided a valid counterfactual of the trend that the employment of 16-17 and 18-19-year-olds would have followed.

To examine parallel trends, visual inspections of parallel trends were undertaken and discussed in Appendix C, Figure C1. In summary, the visual inspection of parallel trends yielded no evidence in support of the assumption. Factors such as life stage likely influence both labour market participation and human capital acquisition, thereby explaining difference in employment pre-treatment employment levels and trends and levels. Formal estimation of parallel trends is presented here, followed by estimating parallel trends for alternative treatment and control groups.

Formal testing of parallel trends estimated a regression where the treatment group dummy was interacted with a month factor variable that included a category for each month in the sample period (base category = March 2000). The regression results presented in this section include all of the control variables from specification (13).[22] Regressions were estimated for each treatment group separately and the null hypotheses H_0: parallel trends against H_1: no parallel trends was tested. The coefficients and 95% confidence intervals on the interaction between treatment group and months are depicted in Figure 2. To accept the null hypothesis of parallel trends in employment, the coefficients in the pre-treatment observation window prior to the base category (i.e., March 2000) should be statistically insignificant, indicating that there is a constant difference in the employment of the treatment and control groups.

(a) Treatment group: 16-17-year-olds; Control group: 20-25-year-olds



(b) Treatment group: 18-19-year-olds; Control group: 20-25-year-olds

[22] Parallel trends were also tested using alternative regression specifications for both sets of treatment and control groups. These plots are presented in Appendix C, Figure C2 for 16-17-year-olds and 20-25-year-olds, and Figure C3 for 18-19 and 20-25-year-olds. In all cases, the null hypothesis of parallel trends was rejected.

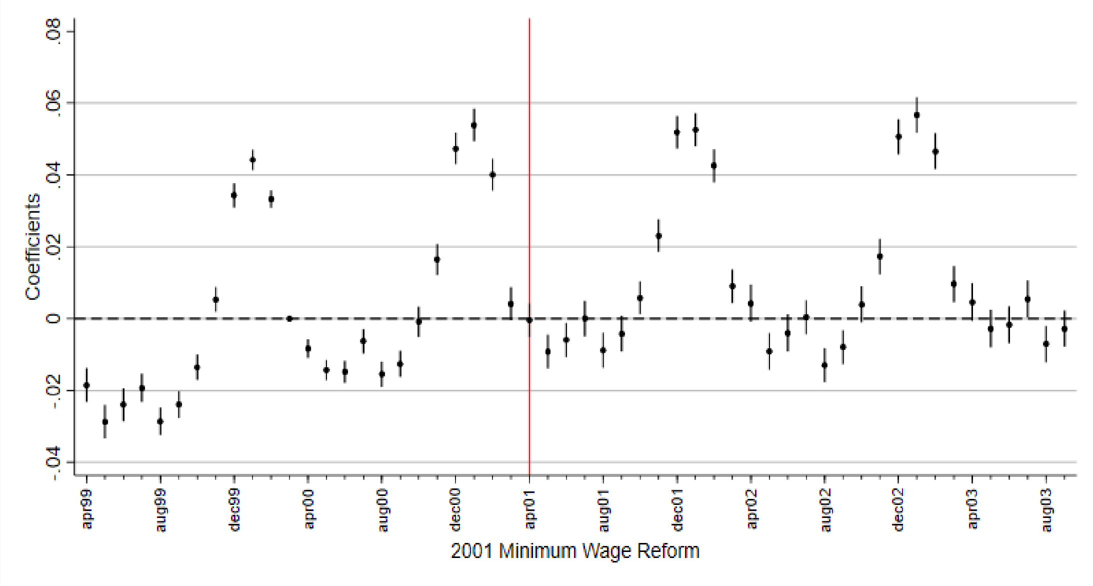


Figure 2: Formally testing for parallel trends in employment.
The sample covers the period April 1999-September 2003. All specifications are estimated by OLS on 23,462,325 observations for 16-17 and 20-25-year-olds, and on 23,442,792 observations for 18-19 and 20-25-year-olds. Huber-White robust standard errors included. These were clustered by UID, allowing for correlation within UID, but independence between UIDs. Author's compilation.

In panel (a), 16-17-year-olds were the treatment group and 20-25-year-olds the control group; 18-19-year-olds were removed from the sample. As can be observed, prior to March 2000, none of the 95% confidence intervals associated with the coefficients included the null value, zero, and were thus statistically significant. These results suggest that there were no constant differences in employment between 16-17 and 20-25-year-olds. The evidence in panel (a) re-affirms the fact there were strong seasonal fluctuations in teenage employment as observed in the visual examination (Appendix C, Figure C1, panel [a]). Consequently, the employment trend of 20-25-year-olds did not provide a suitable counterfactual for 16-17-year-olds in the absence of the minimum wage policy reform. Based on these considerations, the null hypothesis of parallel trends in the employment of 16-17 and 20-25-year-olds was rejected.

In panel (b), 18-19-year-olds were the treatment group and 20-25-year-olds the control group; 16-17-year-olds were removed from the sample. Prior to March 2000, the 95% confidence intervals associated with the coefficients did not include the null value, zero, and were thus statistically significant.[23] These results suggest that there were no constant differences in employment between 18-19 and 20-25-year-olds. The evidence in panel (b) re-affirms the fact there were strong seasonal fluctuations in teenage employment as observed in the visual examination (Appendix C, Figure C1, panel [b]). Consequently,

[23] February 2000 was the only marginal exception (coefficient = -0.001, 95% CIs [-0.002, 0.000]).

the employment trend of 20-25-year-olds did not provide a suitable counterfactual for 18-19-year-olds in the absence of the minimum wage policy reform. Based on these considerations, the null hypothesis of parallel trends in the employment of 18-19 and 20-25-year-olds was rejected.

With no evidence of parallel trends observed following the formal examination, further testing was conducted to ensure no computation errors were influencing the regression results. Here, the formal tests were repeated using alternative treatment and control groups where one would expect the presence of parallel trends in employment.[24] The first variation selected 17-year-olds, where half of the sample were randomly assigned to a treatment group, with the remainder acting as the control group. The second variation selected 24-year-olds as the treatment groups, with 25-year-olds as the control group. In both instances, results suggested that there were constant differences in employment of the treatment and control groups. Consequently, the null hypothesis of parallel trends was accepted.

The assumption for parallel trends was tested visually by plotting the employment trends of the treatment and control groups, and formally through regression analyses, which were verified through tests to check for computational errors. All tests failed to find support for assuming parallel trends in employment for either combination of the treatment (i.e., 16-17 or 18-19-year-olds) and the control group (i.e., 20-25-year-olds). As noted by Kahn-Lang and Lang (2019), along with pre-trend testing, there is a need for logical reasoning when assuming parallel trends. In this regard, it is difficult to find logical support for parallel trends in employment for either 16-17 or 18-19-year-olds when compared to 20-25-year-olds. As noted, factors such as life stage differentially impact labour market participation and acquisition of human capital. These impacts could be argued as ubiquitous and would therefore persistently and differentially impact the outcome trends of the treatment and control groups.

5 Conclusion and Implications

This study exploited substantial variations in real minimum wages of 16-17 and 18-19-year-olds (33% over March 2001-2002 & 68% in March 2001, respectively) to empirically investigate how increases in these wage rates impact teenage employment. Although the impact of minimum wage increases on teenage employment have been

[24] See Appendix c, Figure C4 and Figure C5.

empirically examined in NZ, existing studies have delivered mixed evidence and thus no clear relationship has been established (Chapple, 1997; Hyslop & Stillman, 2007, 2021; Maloney, 1995, 1997; Maré & Hyslop, 2021; Pacheco, 2011).

Furthermore, existing NZ studies have primarily used survey data from the HLFS. This study contributes to the NZ literature by exploiting administrative data from Stats NZ's IDI to provide causal estimates of the impact of the 2001 minimum wage reform on teenage employment using an individual-level administrative sample.

In undertaking this study, a two-step approach was taken. Step one utilised a survey sample from the HLFS and reproduced existing empirical findings from Hyslop and Stillman (2007) using the DID regression models specified in their paper. The empirical results, which did not yield any evidence of minimum wage increases impacting teenager employment, were deemed consistent with those from the original study when evaluated using the framework of LeBel et al. (2019). The reproduction analysis makes a unique contribution to the NZ empirical minimum wage literature, as studies which re-examine existing empirical evidence in economics are sparse. The additional benefit of having reproduced empirical results which were consistent relative to those from the original study, was that it provided a suitable regression specification to use in step two of the study.

Step two combined the regression specifications with individual-level linked administrative data from Stats NZ's IDI to empirically examine the effects of the 2001 minimum wage reform on teenage employment. Generally consistent with the simple DID employment estimates in Table 4, findings from the regression models revealed that overall, the 2001 minimum wage policy reform had small positive impacts on the employment of teenagers, as well as negligible spillover effects on the employment of 20-21-year-olds. However, when testing the validity of the key assumption underlying the DID identification strategy, no evidence in support of parallel trends was found. Consequently, the effects of the 2001 minimum wage reform on teenage employment using administrative data remain uncertain as the estimates presented here may be biased. Nonetheless, several contributions of this empirical analysis, from both policy and research perspective, are worth considering.

Although no concrete policy recommendations can be made from this study given the potential bias in the empirical results, the following is worth noting. By assuming that the effects were not over-estimated, this study showed no evidence of statistically significant adverse effects on teenage employment. This is critical from a policy perspective in NZ, as policy recommendations are generally developed with the view that increases in the statutory minimum wage have adverse impacts on aggregate or sub-group level employment (Ministry of Business Innovation & Employment, 2019a). Consequently, the findings from this study may motivate policy makers to reassess their initial underlying assumption concerning the employment impacts of minimum wage increases and allow a broader scope of empirical evidence to inform policy discussions.

From a research perspective, the level of aggregation of administrative data stood out as a key benefit. The lower aggregation was advantageous when examining the employment trends of 16-17 and 18-19-year-olds as shown in Figure 1, where strong and distinct seasonal fluctuations were observable for each treatment group. The value of this was also notable with pre-trend testing for parallel trends. Once again, when compared to 20-25-year-olds, clear seasonal fluctuation in the employment of both treatment groups were observable in Figure 2, which supported the conclusions from the hypothesis testing of parallel trends.

One of the contributions made by this study was to address the lack of administrative evidence on minimum wages and employment in the NZ empirical literature. However, given the true effects on employment remain uncertain due no observable evidence for parallel trends, future research may look toward alternative identification strategies to generate empirical results more suitable for policy discussions and recommendations.

Appendix A

Table A1: Definitions of Regressions Estimated by Hyslop and Stillman (2007)
Definitions of Regressions Estimated by Hyslop and Stillman (2007)

Controls	Description	Specification (1)	(2)	(3)	(4)	(5)	(6)	(7)
Covariates	Dummy variables for age, quarter, demographic characteristics (gender, ethnicity, marital status, NZ born, urbanicity, region of residence). A measure for the relative size of each age group in a particular year.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spillover effects	Interaction term between age group dummy (*age*20-21) and post-treatment period (*Post*-2001). 20-21-year-olds were covered by the adult minimum wage, and were thus not affected by the 2001 minimum wage reform.		Yes	Yes	Yes	Yes	Yes	Yes
Proxy responses	Dummy variable indicating a proxy response, plus interactions with each of the control variables.			Yes	Yes	Yes	Yes	Yes
Separate post-reform year impacts	Interaction term between the age groups dummies and the individual year dummies (following each of the 2001, 2002 and 2003 changes).				Yes	Yes	Yes	Yes
Announcement effects	Dummies for the initial policy announcement and main announcements occurring in 2000, 2001, 2002. These dummies were also interacted with age groups dummies.					Yes	Yes	Yes
Age-specific seasonal effects	Interaction terms between quarter dummies and individual age dummies.						Yes	Yes
Age-specific business cycle effects	Interaction terms between prime-aged unemployment rate (aged 26-49) and individual age dummies.							Yes

Note. Details on regression specifications from Hyslop and Stillman (2007). Author's compilation.

Table A2: Estimated Employment Impacts from the Reproduction Analysis

Estimated Employment Impacts from the Reproduction Analysis

	Specification				
	(1)	(2)	(3)	(4)	(5)
Age 16-17 * Post-2001	-0.006	-0.005	-0.001		
	(0.009)	(0.010)	(0.010)		
	[-0.023, 0.011]	[-0.025, 0.015]	[-0.021, 0.020]		
2001				0.011	-0.003
				(0.014)	(0.017)
				[-0.017, 0.039]	[-0.037, 0.030]
2002				0.003	0.007
				(0.013)	(0.016)
				[-0.022, 0.028]	[-0.024, 0.039]
2003				-0.020	-0.024
				(0.016)	(0.017)
				[-0.052, 0.012]	[-0.058, 0.009]
Age 18-19 * Post-2001	-0.013	-0.012	-0.007		
	(0.008)	(0.010)	(0.010)		
	[-0.030, 0.003]	[-0.031, 0.007]	[-0.027, 0.012]		
2001				0.005	-0.012
				(0.013)	(0.016)
				[-0.022, 0.030]	[-0.043, 0.020]
2002				-0.006	-0.012
				(0.013)	(0.015)
				[-0.031, 0.019]	[-0.043, 0.018]
2003				-0.024	-0.032*
				(0.016)	(0.017)
				[-0.056, 0.008]	[-0.070, 0.002]
Age 20-21 * Post-2001		0.002	0.005		
		(0.010)	(0.010)		
		[-0.018, 0.022]	[-0.015, 0.025]		
2001				0.031**	0.016
				(0.013)	(0.016)
				[0.005, 0.057]	[-0.016, 0.047]
2002				-0.001	-0.008
				(0.013)	(0.016)
				[-0.026, 0.025]	[-0.039, 0.023]
2003				-0.021	-0.028
				(0.016)	(0.018)
				[-0.052, 0.010]	[-0.062, 0.006]
R2	.12	.12	.12	.12	.12

Note. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5% and 1% level, respectively. All specifications are estimated by OLS on 125,487 observations. Huber-White robust standard errors are presented in parentheses. 95% confidence intervals are provided in square brackets. The covariates include dummy variables for individual–age and quarter, gender, marital status, ethnicity, NZ born, urbanicity and region of residence, and the relative size of the population of each age group (16-17, 18-19, 20-21 and 22-25) in a particular year. Author's compilation.

Table A3: Guidelines Adopted for Evaluating the Reproduction Results

Guidelines Adopted for Evaluating the Reproduction Results

Was a signal detected in the reproduction?	Did the ES 95% CI include zero?	Did the 95% CI include the ES from the original study?	Outcome
Where a signal was detected in the original study:			
Yes	No	Yes	Signal – consistent
Yes	No	No	Signal – inconsistent[a]
No	Yes	Yes	No signal – consistent
No	Yes	No	No signal – inconsistent
Where a signal was not detected in the original study:			
No	Yes	Yes	No signal – consistent
Yes	No	Yes	Signal – consistent
Yes	No	No	Signal – inconsistent[b]

Note. Adapted from LeBel et al. (2019). Author’s compilation.
[a] This outcome can also be classified as larger, smaller, or opposite depending on the size and direction of the point estimate relative to the comparable point estimate in the original.
[b] This outcome can also be classified as positive or negative depending on the direction of the point estimate.

Appendix B.

Table B1: Construction of administrative sample

Construction of administrative sample

Total unique records extracted	10,173,186
Removed:	
Death	1,137
Non-residency status	9,181,884
Falling outside the sample population age	84,924
Restricting to sample period (April 1999 – September 2003)	15,249
Total unique individuals[a]	889,992
Total observations	29,442,717

Note. Totals may not add up correctly due to random rounding requirements from Stats NZ (2020c) compilation.
[a] This represents the number of unique individuals aged 16-25 years across the sample period of April 1999-September 2003.

Table B2: Variable Definitions and Sources Utilised in Difference-in-Differences Analysis

Variable Definitions and Sources Utilised in Difference-in-Differences Analysis

Variable	Definition	Source
Employed	Dummy variable = 1 if earning wages or salary; 0 otherwise.	IRD: EMS
Age	Dummy variable = 1 if age equals the particular year; 0 otherwise. For example, age16 = 1 if aged 16; 0 otherwise. Dummies generated for ages 16-25 years. Base category = 16.	Personal details
Age group	Dummy variable = 1 if age falls within age group range; 0 otherwise. For example, age16-17 = 1 if aged 16 or 17; 0 otherwise. Dummies generated for age groups 16-17, 18-19 and 20-21.	Personal details
Female	Dummy variable = 1 if female; 0 otherwise.	Personal details
Married	Dummy variable = 1 if married; 0 otherwise.	DIA: Marriages
NZ born	Dummy variable = 1 if born in NZ; 0 otherwise	DIA: Birth
Ethnicity[a]	Dummy variable = 1 if ethnicity equals a relevant category; 0 otherwise. For example, Pākehā = 1 if ethnicity is recorded as Pākehā; 0 otherwise. Dummies generated for Pākehā, Māori, Pacific Islander, Asian, MELAA, Other and Not specified. Base category = Pākehā.	Personal details
Region	Dummy variable = 1 if region equals a relevant category; 0 otherwise. For example, Auckland = 1 if region is recorded as Northland; 0 otherwise. Dummies generated for Northland, Auckland, Waikato, Bay of Plenty, Gisborne/Hawkes Bay, Taranaki, Manawatu/Wanganui, Wellington, Nelson/Tasman/Marlborough/West Coast, Canterbury, Otago and Southland. Base category = Auckland.	Address notification
Urbanicity	Dummy variable = 1 if residing in rural district; 0 otherwise.	NZ Post[a]
Supply-side control	The relative size of the population of each age group in a particular year.	Generated[c]
Post-reform period	Dummy variable = 1 for all for all months from April 2001 to September 2003.	Generated
Post-reform period by individual years	Dummy variable = 1 for all months from April 2001 to March 2002; 0 otherwise. Dummy variable = 1 for all months from April 2002 to March 2003; 0 otherwise. Dummy variable = 1 for all months from April 2003 to September 2003; 0 otherwise.	Generated
Initial announcement	Dummy variable = 1 for all months from April 2000 to December 2000; 0 otherwise. Initial announcement dummy also interacted with age group dummies.	Generated
Annual announcements	Dummy variable = 1 for all months from January 2001 to March 2001. Dummy variable = 1 for all months from January 2002 to March 2002. Dummy variable = 1 for all months from January 2003 to March 2003. Annual announcement dummies also interacted with age group dummies.	Generated
Quarterly dummies	Dummy variable = 1 if quarter equals a relevant category; 0 otherwise. For example, Q1 = 1 if quarter is recorded as quarter one; 0 otherwise. Base category = Q1. Quarterly dummies also interacted with age dummies. Base categories = age25-Q2, age25-Q3 and age25-Q4.	Generated
Prime aged unemployment rate	Unemployment rate for 26-49-year-olds. Unemployment rate also interacted with age dummies. Base category = age25-urate.	Generated[d]

Note. Author's compilation.

[a] Ethnicities were prioritised using the framework from Meehan, Pacheco, and Pushon (2019).

[b] Data were sourced from New Zealand Post (n.d.).

[c, d] Generated using code provided by Hyslop and Stillman (2007).

Appendix C.

Starting with visual inspections, Figure C1 presents the employment trends of 16-17 and 20-25-year-olds in panel (a), as well as 18-19 and 20-25-year-olds in panel (b) across both the pre- and post-treatment observation windows. In panel (a), the employment trends within the sample indicate that mean employment for 16-17-year-olds grew marginally more when compared with 20-25-year-olds. However, over the pre-treatment observation window, mean employment for 16-17-year-olds increased by 4 percentage points between April 1999-March 2001, compared to a 2 percentage point increase for 20-25-year-olds. Employment for 16-17-year-olds also exhibits strong seasonal fluctuations, with mean employment over the pre-treatment observation window increasing an average of 6 percentage points between November and December each year. Although seasonality in the employment of 20-25-year-olds is observable, the annual seasonal surge started earlier and only increases by an average of 2 percentage points between October and December each year.

A notable difference in the employment of 16-17 and 20-25-year-olds was their initial levels of mean employment – approximately a 17 percentage points difference. Although parallel trends do not require the levels of an outcome to be the same (Yannelis, 2014), recent work by Kahn-Lang and Lang (2019) stated that parallel trends would be more plausible if the treatment and control groups had similar levels to begin with. In instances where levels are different, researchers must explain why such differences exist, and why the underlying mechanism does not also impact trends in the outcome (Kahn-Lang & Lang, 2019). Arguably, the difference in the initial employment levels is driven by life stage. That is, 16-17-year-olds are primarily engaged in full-time secondary education, and thus fewer are participating in the formal labour market. Furthermore, 16-17-year-olds have also acquired relatively less human capital when compared with 20-25-year-olds. For 16-17-year-olds, human capital acquisition has largely been through their education and, in some instances, part-time employment or on-the-job training. In contrast, 20-25-year-olds have generally completed all formal education and have supplemented their human capital through part- and full-time employment and on-the-job-training. In this context, life stage could also be considered ubiquitous and would have persistently impacted differences in the employment trends of these two groups.

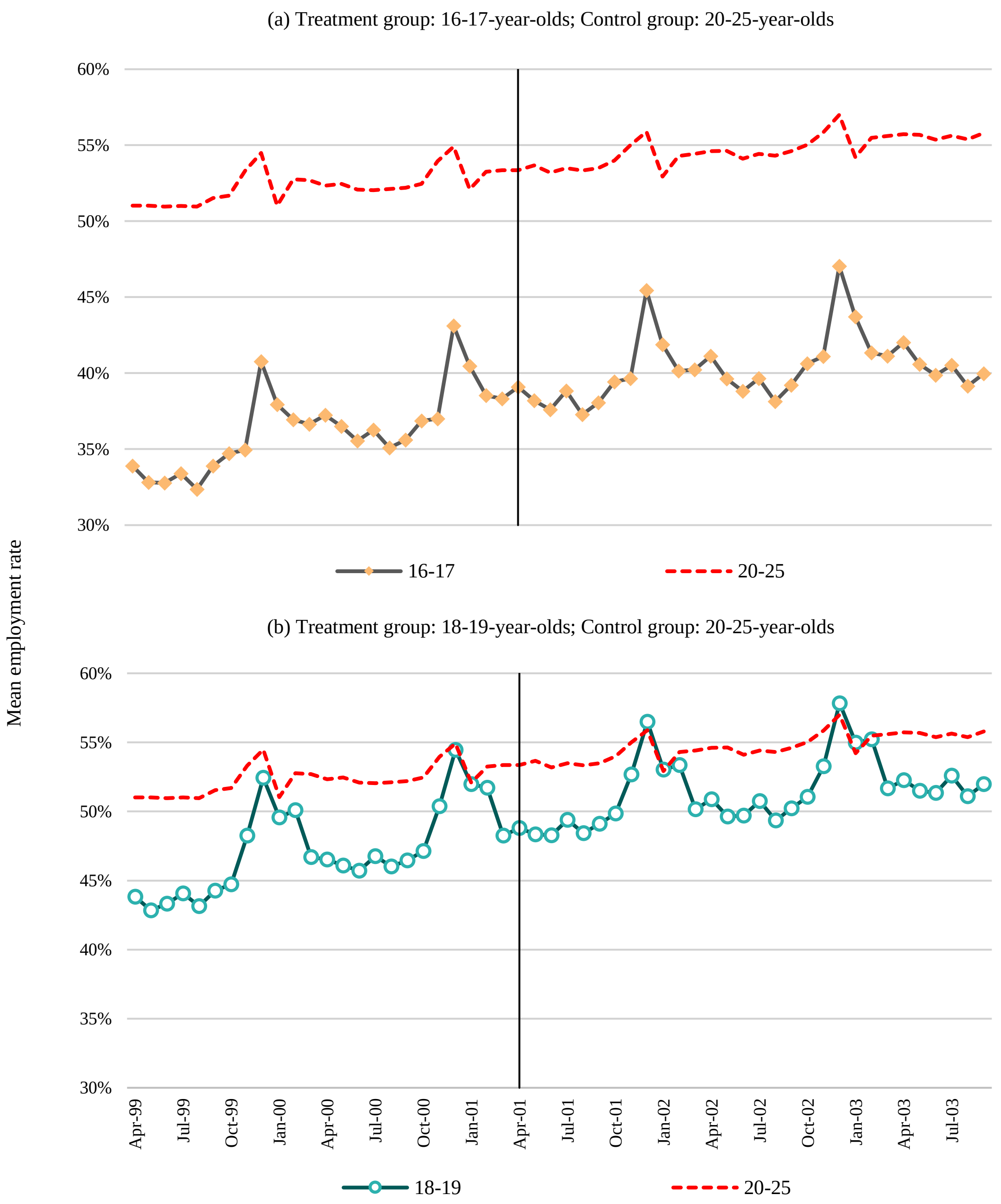


Figure C1: Pre-treatment employment levels for 16-17, 18-19 and 20-25-year-olds.
The sample covers the period April 1999-September 2003. The pre- and post-treatment periods are April 1999-September 2000 and April 2001-September 2003, respectively. Values on the y-axis start at 30% to clearly highlight the variation in the pre-treatment employment levels. Author's compilation.

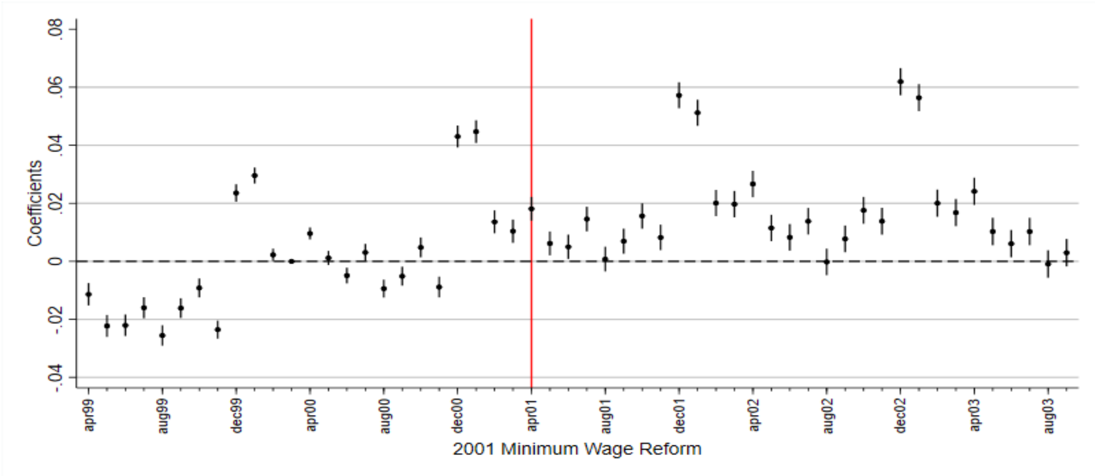
Supporting visual evidence of parallel trends would have required a constant difference in the employment of 16-17 and 20-25-year-olds over the pre-treatment observation window. When considering the difference in the employment trends, seasonality of employment and life stage of these groups, no visual evidence in support of parallel trends was found.

In panel (b), the employment trends over the sample indicate that mean employment for 18-19-year-olds grew more when compared with 20-25-year-olds. Likewise, over the pre-treatment observation window, mean employment for 18-19-year-olds increased by 4 percentage points between April 1999-March 2001, compared to a 2 percentage point increase for 20-25-year-olds. As with younger teenagers, the employment of 18-19-year-olds also appeared to exhibit strong seasonal fluctuations. Over the pre-treatment observation window, mean employment of 18-19-year-olds increased by 7 percentage points on average between October and December each year, compared to an average of 2 percentage points for 20-25-year-olds during the same months.

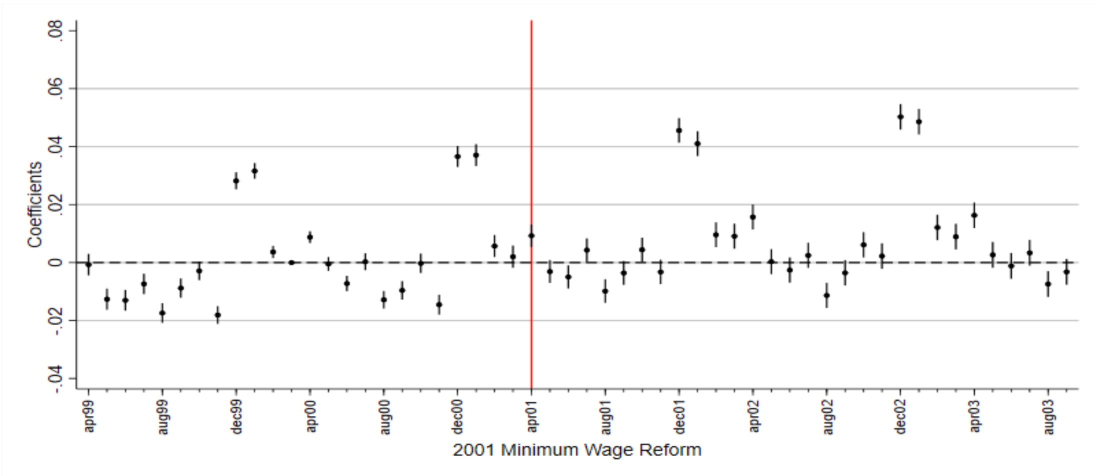
Employment trends in panel (b) also highlight the differences in the initial levels of employment for 18-19 and 20-25-year-olds – approximately a 7 percentage points difference. To a large extent, life stage is also a relevant explanation for these differences in initial employment levels. That is, some 18-year-olds are still enrolled in full-time secondary education, with many 18-19-year-olds commencing tertiary study and accumulating human capital through part-time employment or on-the-job training. In comparison, 20-25-year-olds have generally completed all formal education and are engaged in full-time employment, thus acquiring comparatively more human capital through employment and on-the-job-training.

When considering differences in the employment trends and seasonality of 18-19 and 20-25-year-olds, no constant difference in employment was observed. Consequently, this visual inspection yielded no evidence in support of parallel trends.

(a) Specification: Base[a]



(b) Specification: Demographics[b]



(c) Specification: Supply-side and spillovers[c]

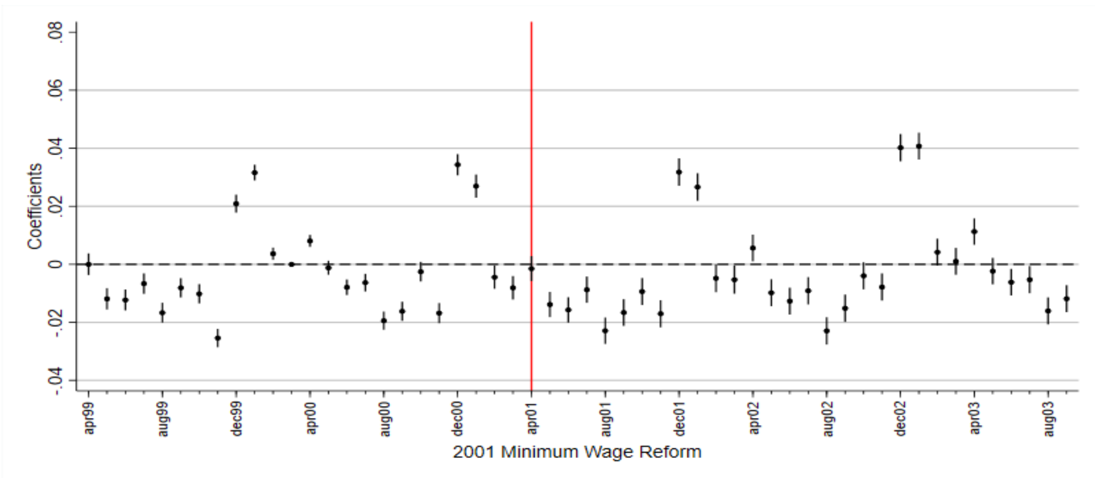


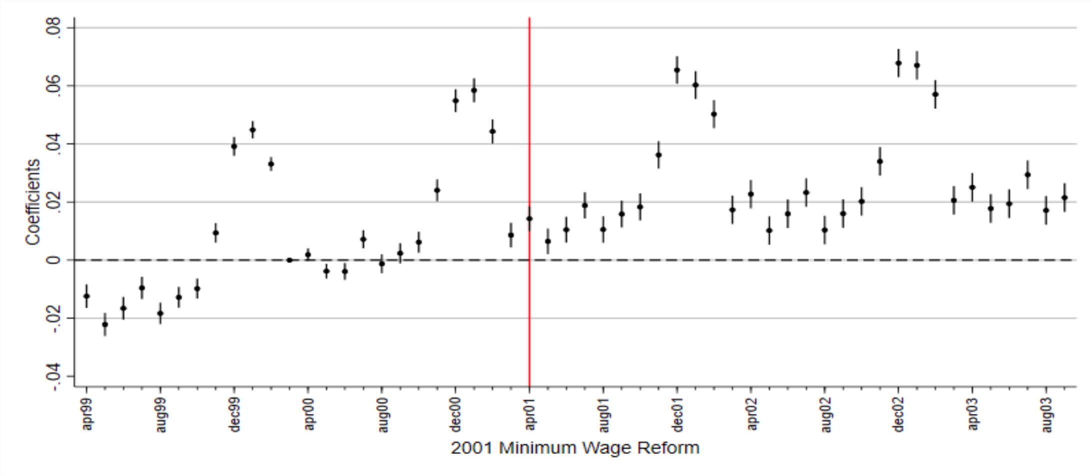
Figure C2: Testing for parallel trends in employment, 16-17 & 20-25-year-olds.
The sample covers the period April 1999-September 2003. All specifications were estimated by OLS on 23,462,325 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation within UID, but independence between UIDs. Author's compilation.

[a] Interaction between treatment group indicator and month indicator.

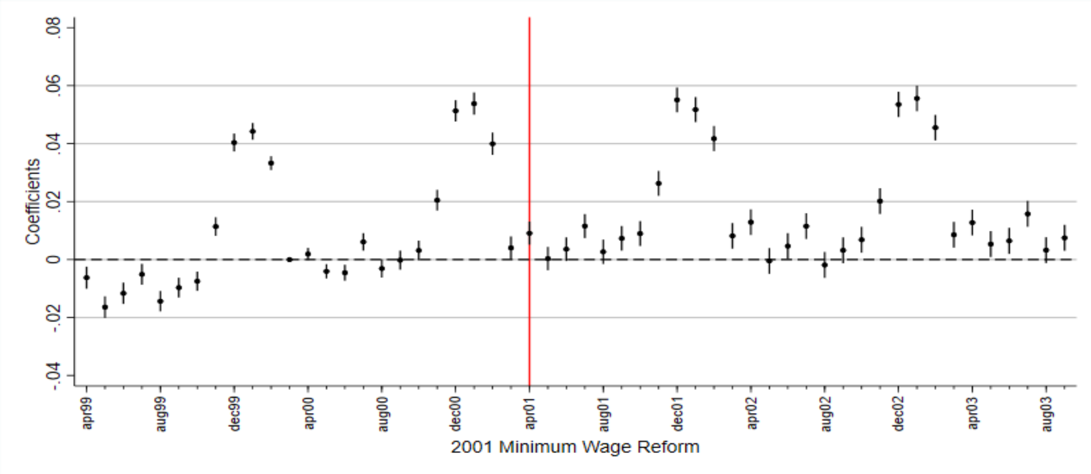
[b] Controls were included for age, quarter, demographic characteristics (gender, ethnicity, marital status, NZ born), urbanicity and region of residence.

[c] Controls were included for supply side effects (relative size of each age group in a particular year) and spillover effects (interaction between age group indicator for 20-21-year-olds and a post-treatment period indicator).

(a) Specification: Base[a]



(b) Specification: Demographics[b]



(c) Specification: Supply-side and spillovers[c]

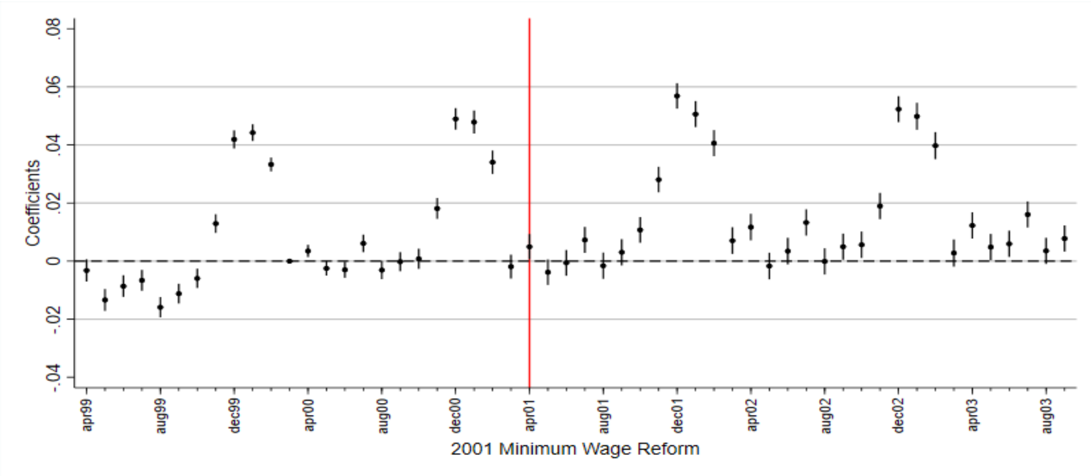


Figure C3: Testing for parallel trends in employment, 18-19 & 20-25-year-olds.

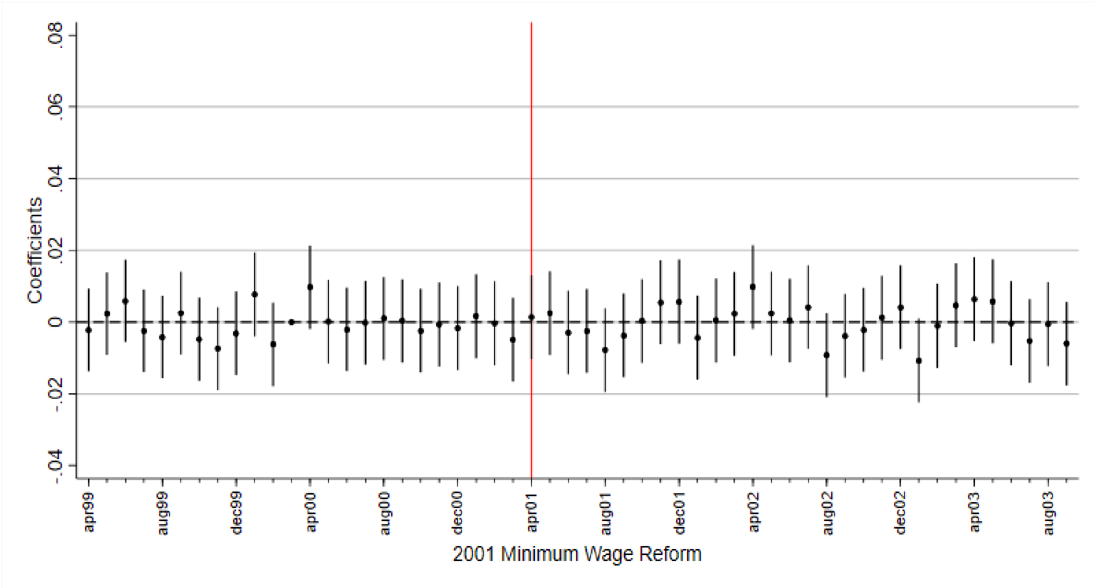
The sample covers the period April 1999-September 2003. All specifications were estimated by OLS on 23,442,792 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation within UID, but independence between UIDs. Author's compilation.

[a] Interaction between treatment group indicator and month indicator.

[b] Controls were included for age, quarter, demographic characteristics (gender, ethnicity, marital status, NZ born), urbanicity and region of residence.

[c] Controls were included for supply side effects (relative size of each age group in a particular year) and spillover effects (interaction between age group indicator for 20-21-year-olds and a post-treatment period indicator).

(a) Specification: Base[a]



(b) Specification: Demographics[b]

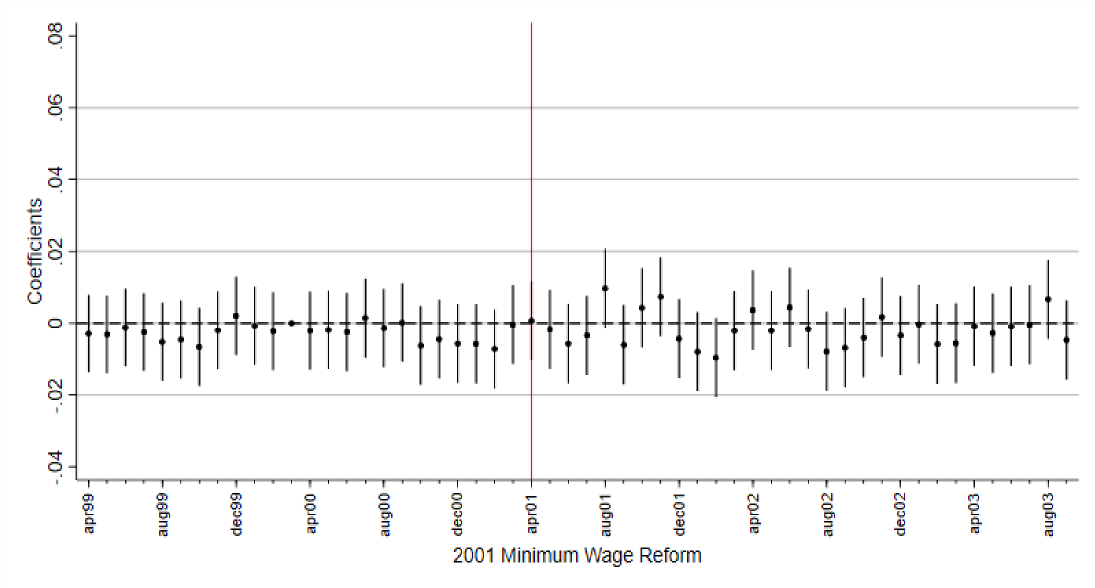


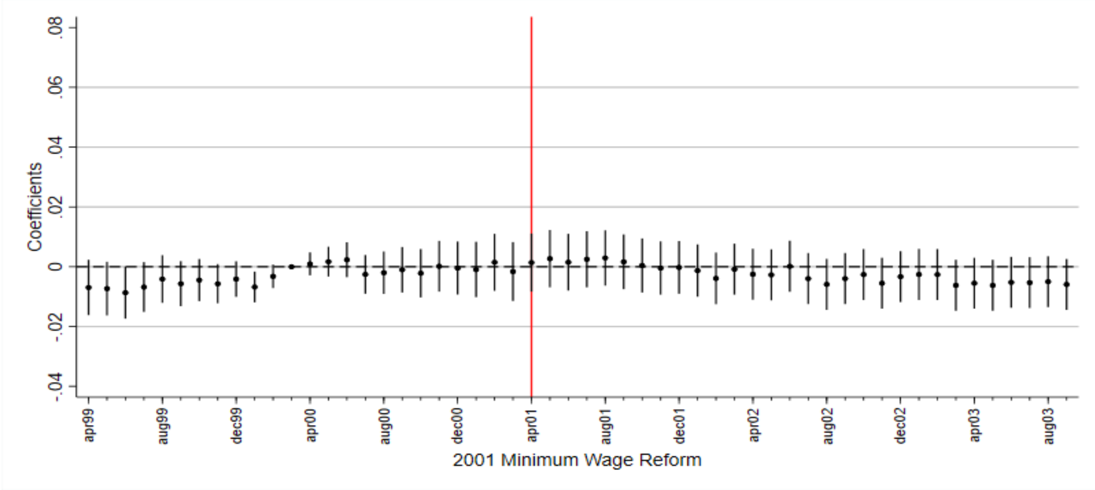
Figure C4: Testing for parallel trends in employment, 17-year-olds.

The sample covers the period April 1999-September 2003. All specifications were estimated by OLS on 2,994,276 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation within UID, but independence between UIDs. Author's compilation.

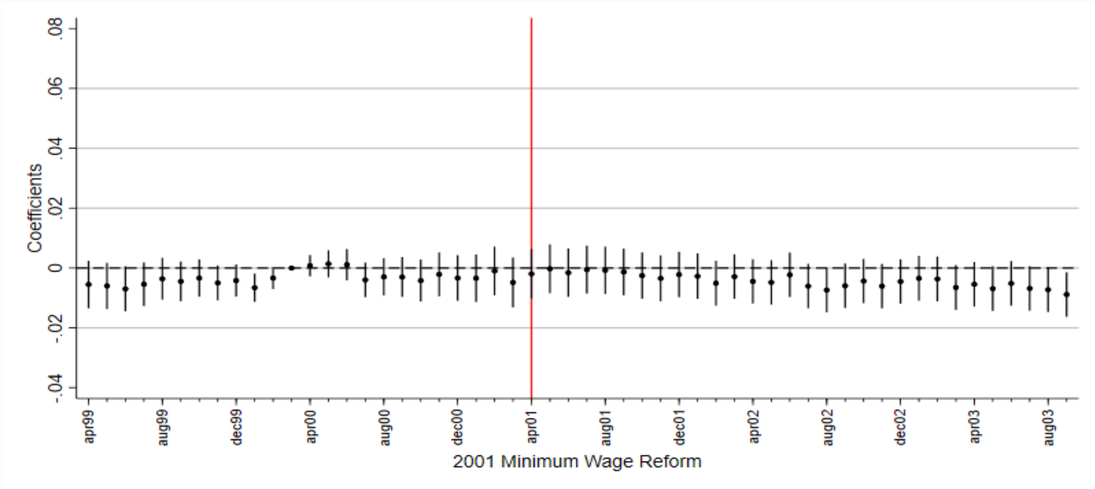
[a] Interaction between treatment group indicator and month indicator.

[b] Controls were included for quarter, demographic characteristics (gender, ethnicity, marital status, NZ born), urbanicity and region of residence.

(a) Specification: Base[a]



(b) Specification: Demographics[b]



(c) Specification: Supply-side and spillovers[c]

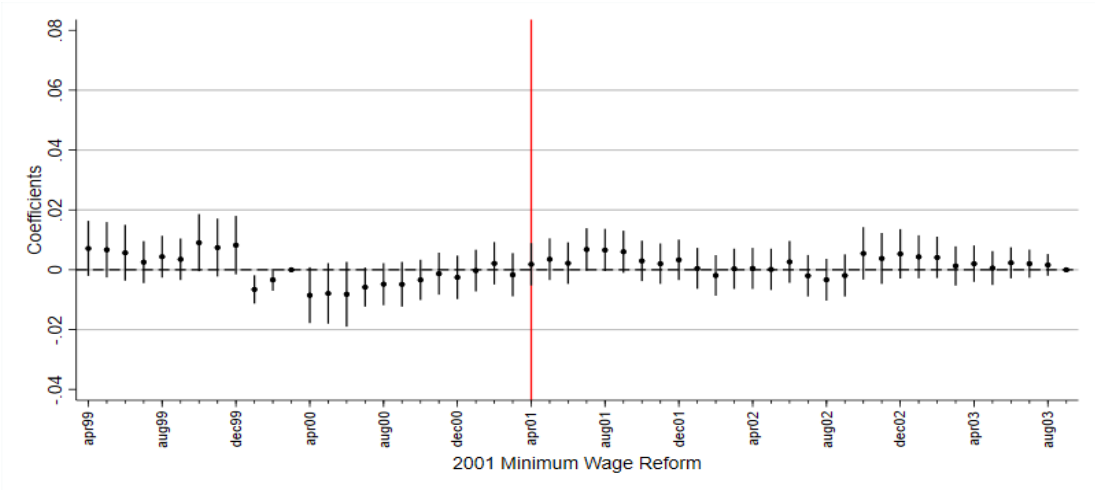


Figure C5: Testing for parallel trends in employment, 24 and 25-year-olds.

The sample covers the period April 1999-September 2003. All specifications are estimated by OLS on 5,685,342 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation within UID, but independence between UIDs. Author's compilation.

[a] Interaction between treatment group indicator and month indicator.

[b] Controls were included for age, quarter, demographic characteristics (gender, ethnicity, marital status, NZ born), urbanicity and region of residence.

[c] Controls were included for supply side effects (relative size of each age group in a particular year).

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