Effects of Increases in the New Zealand Minimum Wage on Teenage Employment: An Empirical Examination using Survey and Administrative Data

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Abstract

This thesis presents empirical evidence of the effects of the 2001 minimum wage policy reform on teenage employment. This reform resulted in the largest increase in real minimum wages, which totalled 68% in March 2001 for 18-19-year-olds and 35% over the period March 2001/02. Although New Zealand empirical evidence exists on the effects of this policy reform on teenage employment, there have been no attempts to re-examine its impacts since administrative data have become available through Stats NZ's Integrated Data Infrastructure. Consequently, this thesis aims to fill this gap in the current New Zealand empirical evidence of the effects of minimum wage increases on teenage employment.

Existing New Zealand empirical evidence has relied on survey data to examine the effects of minimum wage increases on teenage employment. This thesis extends the current body of knowledge by utilising individual-level administrative data. This thesis also draws on survey data to compare the benefits and limitations of the administrative data available over the selected sample periods of the empirical analyses. Formal estimation of the effects of the 2001 minimum wage policy reform on teenage employment adopts several identification strategies, including difference-in-differences and regression discontinuity, common in the international empirical minimum wage literature but not in New Zealand.

The key value of the administrative data was revealed when measuring employment using administrative and survey data. When measured using administrative data, monthly variations in employment showed substantial seasonal fluctuation, which were not as observable when measuring employment at a quarterly-level using survey data. These monthly variations in employment were beneficial when formally estimating the effects of the 2001 minimum wage policy reform on teenage employment as they provided more variation in a key outcome variable. They also enabled formal validation of key assumptions to be undertaken, which would have been more challenging if relying on survey data. Although unique in the population examined, the final empirical analysis revealed effects on teenage employment from minimum wage increases which complemented some of the existing New Zealand empirical evidence.

There are several relevant research and policy implications drawn from the descriptive and formal empirical results presented in this thesis. For research, the key limitation of the administrative data was identified as the absence of information on of hours of work over the selected sample periods. This restrained the empirical examinations undertaken in this thesis from fully exploring the effects of minimum wage increases on a range of relevant outcomes, such as hourly wage or hours of work. For policy, the formal empirical results suggest that large minimum wage increases may not immediately impact teenage employment; however, employers respond through alternative channels of adjustment. By considering a wider range of potential responses by employers, more refined minimum wage policy could be formulated.

Disclaimer

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.

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Attestation of Authorship

I hereby declare that this submission is my own work and that, to the best of my knowledge and belief, it contains no material previously published or written by another person (except where explicitly defined in the acknowledgements), nor material which to a substantial extent has been submitted for the award of any other degree or diploma of a university or other institution of higher learning.

Signed: Date: 25/04/2022

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Chapter 1 Introduction and Outline

1.1 Introduction

Can we still assume that minimum wage increases adversely affect teenage employment? Although there has been a large body of empirical support for this hypothesis in the United States (US) (Bailey, DiNardo, & Stuart, 2020; Bazen & Le Gallo, 2009; Clemens & Wither, 2019; Currie & Fallick, 1996; Hoffman & Trace, 2009; Neumark, Salas, & Wascher, 2014; Neumark & Wascher, 1992, 1994, 2000; Wellington, 1991), the United Kingdom (UK) (Aitken, Dolton, & Riley, 2019; Fidrmuc & Tena, 2018; Machin, Rahman, & Manning, 2003), Europe (Dolton & Bondibene, 2012; Harasztosi & Lindner, 2019; Kabátek, 2021; Kreiner, Reck, & Skov, 2020; Laporšek, 2013; Martin, 2020; Pereira, 2003), and New Zealand (NZ) (Chapple, 1997; Hyslop, Maré, Stillman, & Timmins, 2012; Maloney, 1995, 1997; Pacheco, 2011), contrasting evidence has also been presented. For example, empirical studies from the US by (Card, 1992a, 1992b) revealed no adverse effects on teenage employment from increases in either federal or state-level minimum wages, while positive effects on employment were observed by Card and Krueger (1994) and Katz and Krueger (1992). In the UK, increases in the national minimum wage have been found to have had no adverse effects on the employment of low-wage workers (Stewart, 2004a, 2004b), and positive effects on the employment of low skilled workers (Dickens, Riley, & Wilkinson, 2014). Similarly, empirical evidence across Europe (Böckerman & Uusitalo, 2009; Garloff, 2019) and NZ (Hyslop & Stillman, 2007, 2021) has also revealed no adverse effects on employment from increases in the minimum wage.

The contrasting empirical evidence is not surprising when considered within the context of the theoretical debate. The traditional textbook prediction of the perfectly competitive labour market model (PCLM) aligns with the empirical evidence showing negative effects on employment from increases in the minimum wage. However, empirical examination of the PCLM has argued against its somewhat unrealistic assumptions, such as the homogeneity of workers (Bray, 2013; Zavodny, 1998) and full compliance with minimum wage legislation (Card, 1992a; Haugen, 2003; Hyslop & Stillman, 2007; Pacheco, 2007, 2009). Furthermore, alternative models such as dynamic monopsony have been noted as being more reflective of how firms and workers may behave in the labour market, and allows a range of predictions with respect to the effects on employment from increases in the minimum wage. Importantly, the dynamic monopsony model has found

empirical support, with minimum wage increases having been shown to have no adverse effects on employment, and in some instances, positive effects (Bachmann & Frings, 2017; Dickens, Machin, & Manning, 1999; Giuliano, 2013; Staiger, Spetz, & Phibbs, 2010). Therefore, although empirical evidence can be rationalised using alternative theoretical labour market models, the direction, magnitude and generalisability of the impacts measured may vary depending on the context under which the relationship is being examined (Cahuc, 2006).

Country specific research offers a sound example of where context matters. Factors such as the legislative framework governing minimum wages vary across countries. Consequently, effects on employment from minimum wage increases empirically estimated in one country would have relatively weak external validity when attempting to interpret those effects within the context of another country.[1] For example, minimum wages in the US are governed by both federal and state laws (U.S. Department of Labor, n.d.b), and include separate wage protection for youth workers under the age of 20 years.[2] These laws also regulate the minimum age for employment (Croucher & White, 2011) and maximum allowable hours, which vary by state (U.S. Department of Labor, 2021). In contrast, current minimum wages in NZ cover all workers above the age of 16 years, with a lower minimum wage payable to 16-19-year-olds depending on a range of criteria.[3] Coverage of the adult minimum wage starts at age 20. The nuances of these varying legislative frameworks illustrate why a minimum wage and employment relationship estimated within the US context may not necessarily be generalisable to the NZ context.

Along with legislative frameworks, other contextual factors, such as how high minimum wages are set, are also important to consider. Historically, NZ has had high minimum wages by international standards. When comparing the ratio of the minimum wage to average wage across 31 member countries of the Organisation for Economic Co-operation and Development (OECD) countries, NZ has consistently ranked as having one of the highest adult minimum wages. Over the period 2000-2020, the adult minimum wage in NZ was 48% of the average wage (OECD average was 46%) (The Organisation for Economic Co-operation and Development, n.d.).

[1] External validity "refers to the approximate validity with which we can infer that the presumed causal relationship can be generalized to and across alternate measures of the cause and effect and across different types of persons, settings, and times" (Cook & Campbell, 1979, p. 37).
[2] This is conditional for workers' first 90 consecutive calendar days of employment with any employer as long as their work does not displace other workers (U.S. Department of Labor, n.d.c).
[3] These criteria relate to duration of employment, participation in industry training and benefit receipt history. See Chapter 2 for a discussion of these criteria.

Along with relatively high minimum wages by international standards, there has also been substantial variation in teenage minimum wages in NZ which is a motivation for this research. 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. With relatively high minimum wages in NZ, along with periods of substantial variation in minimum wage rates for teenagers, NZ provides an excellent case study for empirically investigating the effects of minimum wage increases on teenage employment.

Further motivation for this thesis 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; 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; Pacheco, 2011; Pacheco & Maloney, 1999), with the exception being Hyslop et al. (2012) who used firm-level administrative data from the Linked Employer-Employee Data (LEED). Therefore, exploiting this lack of administrative empirical evidence as an opportunity, this thesis sets out to provide empirical evidence of the effects of minimum wage increases on teenage employment using individual-level administrative data.

There are several benefits of using the administrative data from the IDI. First, it offers population-level data, thus eliminating the issue of sampling error inherent in surveys such as such as 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 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. 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). Furthermore, the NZ minimum wage has re-emerged as an important topic of discussion among policy makers, partly due to the Labour-led government's commitment to raising the minimum wage to $20 per hour by 2021 (New Zealand Government, 2019). Therefore, within the context of this policy agenda, thoroughly understanding how teenage employment is impacted by minimum wage increases are vital to inform future policy decisions. Although the results of this thesis will not negate the necessity for policy makers to consider adverse outcomes on teenage employment, it may broaden the possible outcomes for policy makers to consider, and as such enable them to formulate policy decisions on a fuller evidence base.

The purpose of this thesis is to introduce individual-level administrative data to the empirical examination of the effects of minimum wage increases on teenage employment in NZ. Fundamentally, the key question this thesis answers is: What are the effects of the 2001 minimum wage reform on teenage employment? Utilising a sample constructed from individual-level administrative data in Stats NZ's IDI, this thesis will contribute to the current body of NZ empirical minimum wage evidence by achieving the following aims:

- present descriptive evidence comparing administrative and survey measures related to earnings and employment to identify the benefits and limitations;
- reproduce existing NZ minimum wage evidence using survey data;
- present empirical estimates of the effects of minimum wage increases on teenage employment using a difference-in-differences (DID) strategy in combination with individual-level administrative data not previously exploited in the existing NZ literature; and
- utilise a regression discontinuity (RD) strategy not formally adopted in the NZ empirical literature, in combination with individual-level administrative data.

1.2 Outline

This thesis consists of seven chapters. Chapter 2 presents the development of minimum wages in NZ. It summarises the history of minimum wage legislation, dating back to The Industrial Conciliation and Arbitration Act 1894. Exemptions to the Minimum Wage Act of 1983, the current legislative framework governing minimum wages, are noted, and reforms to this legislation since its introduction are described. As mentioned earlier, because this research is partly motivated by the substantial variation in the minimum wage, changes to the rates over the period 1985-2021 are described. The final part of Chapter 2 examines how recent reforms in the criteria governing a worker's eligibility for a particular minimum wage type (and rate) have become increasingly complex and have presented unique research implications.

Chapter 3 presents a concise literature review, commencing with a discussion of several theoretical labour market models. The benefits and limitations of these models are assessed, which will become an important foundation upon which to understand the effects of minimum wage increases on teenage employment estimated in this research. The second part of Chapter 3 presents an empirical literature review. Here, a brief history of the development of the US literature is presented, with the main focus being on the key exchanges between Card, Katz, and Krueger (1994), Card and Krueger (1994, 2000) and Neumark and Wascher (1994, 2000). Next, early NZ literature is summarised, followed by an in-depth and critical review of literature published in the new millennia. Chapter 3 concludes with a short summary of the key observations from both the theoretical and empirical literature, as well as a statement on the key opportunities identified whereby the use of administrative data in this research could extend the current literature from both a data and methodological perspective.

Chapter 4 presents information pertaining to the data, samples and measures utilised in this thesis. The key objective of this chapter is to provide detailed descriptive evidence comparing employment related outcomes and minimum wage groups, when measured using survey and administrative data, to identify the underlying benefits and limitations of the two data types. Chapter 4 commences with detailing all the data utilised within this thesis, including data from surveys such as the HLFS and HLFS-IS, and those from administrative sources contained within the IDI. Next, a comprehensive description of the processes for constructing the survey and administrative sample is presented. Providing these details, particularly of the administrative sample, was considered

necessary given this would be the first instance of this type of sample being used in the NZ empirical minimum wage research. Following the construction of the samples, key measures commonly examined in minimum wage literature, including employment, total weekly earnings and hourly wages, are defined and compared when measured using survey and administrative data. The final part of this chapter compares the characteristics of sub-minimum and minimum wage workers over the period 1999q2-2003q2 constructed using survey data, against equivalent groups constructed using the administrative sample. Chapter 4 concludes with a short description of the key observations with respect to the benefits and limitations of survey and administrative data within the context of conducting minimum wage research in NZ.

Chapter 5 empirically examines the effects of minimum wage increases on teenage employment. The empirical approach in this chapter was set in two steps. In the first step, existing survey-based evidence on the effects of minimum wage increases on teenage employment was reproduced in order to test an existing identification strategy in the NZ empirical literature, which could be utilised with administrative data. For this, the study by Hyslop and Stillman (2007) was selected as it focussed on the 2001 minimum wage reform which affected teenagers aged 16-17 and 18-19, and has resulted in the single largest increase in minimum wages to date (68% increase in real terms for 18-19-year-olds). The study by Hyslop and Stillman (2007) also utilised DID as an identification strategy, which is common in the international literature, and served as an attractive feature. The second step combined the identification strategy of the first step, with an administrative sample (constructed in Chapter 4). The sample characteristics are described, along with simple DID estimates, prior to formally examining the effects of the 2001 minimum wage reform on teenage employment, and assessing the key assumptions underpinning DID identification. Chapter 5 concludes by noting the policy and research implications taken from this empirical examination.

Chapter 6 estimates the effects of the 2001 minimum wage reform on employment using an alternative identification strategy in combination with an individual-level administrative sample. By exploiting an age discontinuity in the NZ minimum wage over the period of 2001-2008, the empirical analysis in this chapter examined the effects on workers' earnings and employment as they observed their 18th birthday using RD as the identification strategy. This feature of the NZ minimum wage has not yet been empirically examined using RD. Discontinuity in monthly earnings and employment was first

examined visually, followed by the generation of formal estimates. Given the somewhat surprising results, further examination was undertaken and required the addition of survey data from the HLFS-IS. The empirical results are then discussed with respect to theoretical labour market models and existing NZ empirical literature, with a range of explanations for the results considered. Chapter 6 concludes by noting the policy and research implications taken from this empirical examination.

Chapter 7 concludes this thesis by summarising the key findings and noting limitations to consider. Furthermore, policy implications are examined and direction for future research are offered.

Chapter 2 Development of Minimum Wage Legislation in New Zealand

This chapter provides a summary of the development of minimum wage legislation in NZ. Included are the history of wage legislation, exemptions to current minimum wages, as well as reforms and changes to minimum wage rates. The final section examines the complex nature of current minimum wage rules and the implications for empirical research.

2.1 The History of Minimum Wage Legislation

NZ has a long history of enforceable minimum wages dating back to the late 19th century. The Industrial Conciliation and Arbitration Act 1894 (ICAA) was the world's first system for governing labour relations. The ICAA 1894 also protected the majority of workers' income through pay-fixing arrangements which took place under an award system (Brosnan & Wilkinson, 1989).[4]

In 1940, the National Service Emergency Regulations (NSER) came into effect, enabling the government to direct workers into industries deemed essential for war-related activities (Brosnan & Wilkinson, 1989). Under this legislation, the government had the power to set a minimum wage rate to protect workers where no award system was in place or where work was precarious. The NSER was replaced by the Minimum Wage Act 1945 (MWA), which extended pay protection to all workers over the age of 21 (Brosnan & Wilkinson, 1989; Sutch, 1969).

Occupational awards and collective bargaining characterised wage setting arrangements during the 1980s. Although the MWA 1983 was introduced to govern national minimum wages, it was largely ineffective as most workers were covered by award wages which were set above the statutory minimum wage (Chapple, 1997). The award system was abolished following the introduction of the Labour Relations Act 1987 (LRA) and the Employment Contracts Act 1991 (ECA), making minimum wages set under the MWA 1983 the only legal wage floor (Hyslop & Stillman, 2007).

[4] Under the ICAA, only workers, who were also union members, were covered (Brosnan & Wilkinson, 1989).

To protect the real income of low-paid workers, section 5 of the MWA 1983 obligates the Minister responsible to review the minimum wage rate by 31 December each year (Ministry of Business Innovation & Employment, 2019a).[5] The Minister may make recommendations to the Governor-General regarding changes in minimum wage rates, and where approved, changes generally come into effect on the first day of April.

2.2 Exemptions and Deductions from Minimum Wages

2.2.1 Exemptions from paying minimum wages.

Minimum wages in NZ provide near-to-full coverage to all employees over the age of 16. However, section 8 of the MWA 1983 outlines exemptions when employing an individual with disability,[6] and section 9 details workers to whom the MWA 1983 does not apply.[7]

2.2.2 Allowable deductions from wages.

Section 7 of the MWA 1983 allows employers to make deductions for board, lodging or time lost. Where employees are provided with board or lodging, employers can deduct the cash value of the costs if fixed, or an amount of 15% for board and 5% for lodging if costs vary (Parliamentary Counsel Office, n.d.a).

2.3 A Timeline of Recent Minimum Wage Reforms

There have been a multitude of reforms to the MWA 1983. These are summarised in Figure 2.1. Wage protection for 16-19-year-olds was introduced in 1994 under the youth minimum wage and was set to 60% of the adult minimum wage.

The 2001 minimum wage reform introduced several changes: i) coverage of adult minimum wage was lowered to 18, and ii) the youth minimum wage was increased to 80% of the adult minimum wage over the period March 2001/02.

Wage protection was introduced in 2003 for workers obligated to undertake industry-related training as part of their employment. The training minimum wage was set at 60% of the adult minimum wage.

[5] At present, the Workplace Relations and Safety Minister is tasked with this responsibility (New Zealand Government, 2019).
[6] Labour Inspectors may issue exemption permits to people with disabilities that limits them from carrying out the requirements of their work. The employer must apply for an exemption permit (Ministry of Business Innovation & Employment, 2018a).
[7] These workers are generally covered under alternative legislative statutes (Parliamentary Counsel Office, n.d.a).

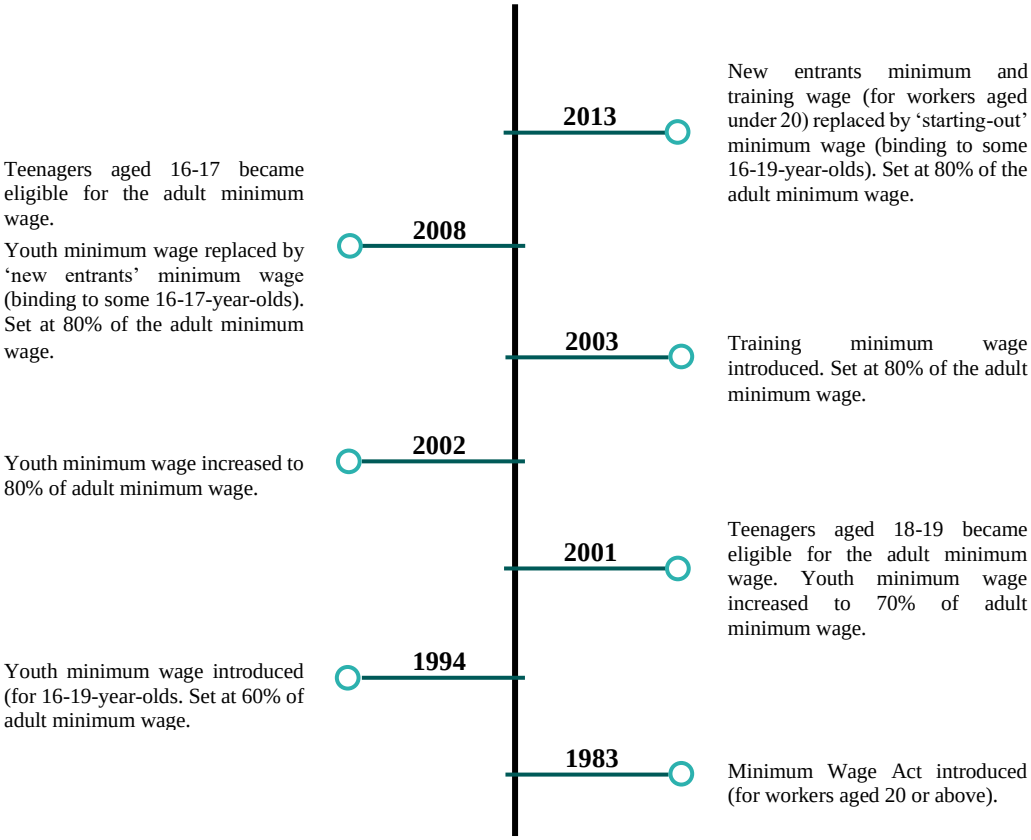

Figure 2.1: Timeline of reforms to the New Zealand minimum wage, 1983-present.
Information on minimum wage reforms are from the Ministry of Business Innovation & Employment (n.d.a, n.d.b). Author's compilation.

The youth minimum wage was replaced by the new entrant minimum wage in 2008. The intention of the new entrant minimum wage was to recognise that 16-17-year-olds often undertook similar work as older workers without the same rewards (New Zealand Government, 2008). This wage was also set at 80% of the adult minimum wage, and applied to workers who had completed less than 3 months or 200 hours of employment.

The new entrant minimum wage was replaced by the starting-out minimum wage in 2013. This reform was aimed at: i) creating demand for young people in a tight labour market, and ii) making the implementation of minimum wage rules simple for employers (New Zealand Government, 2012). Additional factors determining the legal minimum wage payable to 16-19-year-old workers accompanied the introduction of the starting-out minimum wage, including employer-specific employment duration, job tasks, benefit receipt history and duration. The complexity and implications of these minimum wage factors will be examined in detail in section 2.5.

2.4 Changes to the Minimum Wage Rates

Along with reforms to minimum wage legislation, the statutory rates have seen a multitude of changes over time. Table 2.1 presents nominal minimum wage rates and associated percentage changes, by age and minimum wage type from 1985-2021.

Table 2.1:

Minimum Wage Rates by Age and Minimum Wage Type, 1985-2021

	20+		18-19 years		16-17 years		Training[a]		New entrant[b]		Starting-out[a]	
Date	($)	%Δ	($)	%Δ	($)	%Δ	($)	%Δ	($)	%Δ	($)	%Δ
Pre-85	2.50											
Feb-85	2.80	12.00										
Sep-85	4.25	51.80										
Feb-87	5.25	23.50										
Feb-88	5.63	7.10										
May-89	5.88	4.40										
Sep-90	6.13	4.30										
Mar-94	6.13	0.00	3.68		3.68							
Mar-95	6.25	2.04	3.75	1.90	3.75	1.90						
Mar-96	6.38	2.00	3.83	2.13	3.83	2.13						
Mar-97	7.00	9.80	4.20	9.66	4.20	9.66						
Mar-00	7.55	7.86	4.55	8.33	4.55	8.33						
Mar-01	7.70	1.99	7.70	69.23	5.40	18.68						
Mar-02	8.00	3.90	8.00	3.90	6.40	18.52						
Mar-03	8.50	6.25	8.50	6.25	6.80	6.25	6.80					
Apr-04	9.00	5.88	9.00	5.88	7.20	5.88	7.20	5.88				
Mar-05	9.50	5.56	9.50	5.56	7.60	5.56	7.60	5.56				
Mar-06	10.25	7.89	10.25	7.89	8.20	7.89	8.20	7.89				
Apr-07	11.25	9.76	11.25	9.76	9.00	9.76	9.00	9.76				
Apr-08	12.00	6.67	12.00	6.67	12.00	33.33	9.60	6.67	9.60			
Apr-09	12.50	4.17	12.50	4.17	12.50	4.17	10.00	4.17	10.00	4.17		
Apr-10	12.75	2.00	12.75	2.00	12.75	2.00	10.20	2.00	10.20	2.00		
Apr-11	13.00	1.96	13.00	1.96	13.00	1.96	10.40	1.96	10.40	1.96		
Apr-12	13.50	3.85	13.50	3.85	13.50	3.85	10.80	3.85	10.80	3.85		
Apr-13	13.75	1.85	13.75	1.85	13.75	1.85	11.00	1.85	11.00	1.85		
May-13	13.75	0.00	13.75	0.00	13.75	0.00	11.00	0.00			11.00	
Apr-14	14.25	3.64	14.25	3.64	14.25	3.64	11.40	3.64			11.40	3.64
Apr-15	14.75	3.51	14.75	3.51	14.75	3.51	11.80	3.51			11.80	3.51
Apr-16	15.25	3.39	15.25	3.39	15.25	3.39	12.20	3.39			12.20	3.39
Apr-17	15.75	3.28	15.75	3.28	15.75	3.28	12.60	3.28			12.60	3.28
Apr-18	16.50	4.76	16.50	4.76	16.50	4.76	13.20	4.76			13.20	4.76
Apr-19	17.70	7.27	17.70	7.27	17.70	7.27	14.16	7.27			14.16	7.27
Apr-20	18.90	6.78	18.90	6.78	18.90	6.78	15.12	6.78			15.12	6.78
Apr-21	20.00	5.82	20.00	5.82	20.00	5.82	16.00	5.82			16.00	5.82

Note. Nominal minimum wage rates are from the Ministry of Business Innovation & Employment (n.d.a). Author's compilation.

[a] The training and starting-out minimum wages apply to a sub-group of 16-19-year-old workers if they meet certain criteria.

[b] The new entrant minimum wage applied to a sub-group of 16-17-year-old workers depending on employment history.

On average, the adult minimum wage rate has increased approximately 7% annually since February 1985. Over this period, there have been several large changes in adult minimum wages. The first such change saw the wage rate increase by approximately 52% in 1985, followed by a further 24% increase in 1987. Since then, large increases to minimum wages have generally impacted teenagers aged 16-19.

In 2001, coverage of the adult minimum wage was lowered to age 18. This single policy change saw the nominal wage rate for 18-19-year-old minimum wage workers increase by approximately 69%, which remains the largest single increase in nominal minimum wages since the introduction of the MWA 1983. As part of the same policy change, the nominal wage rate for 16-17-year-old minimum wage workers increased by around 37% in the period March 2001/02.

The introduction of the new entrant minimum wage in 2008 saw the wage rate for some 16-17-year-old minimum wage workers increase by 33%. Although the largest single increase for this age group, it was only binding to 16-17-year-old workers once they completed 3 months or 200 hours of employment. Since 2009, minimum wage increases have ranged between 2-7% for all age groups and wage types.

Changes in minimum wages were also examined in real terms to consider growth over time. Figure 2.2 depicts the changes in real minimum wage rates applicable to workers aged 18-19 and 20 or above in panel (a), and 16-17 and 18 or above in panel (b), resulting from the 2001 and 2008 minimum wage reforms, respectively. Changes in the real minimum wage rate for all ages and minimum wage types are included and summarised in Appendix A, Figure A1 and Figure A2, respectively.

Panel (a) of Figure 2.2 presents the real minimum wage rate for workers aged 18-19 and 20 or above over the period March 1984-March 2021. For workers aged 20 or above, two increases in real wages stand out: a 68% increase in March 1985 and a 20% increase in March 1987. Subsequent to these increases, the real value of the adult minimum wage diminished until March 1997, from which point it has increased consistently due to annual increases taking place from March 2000. Following the introduction of the youth minimum wage in March 1994, the real minimum wage rate for 18-19-year-olds only increased by approximately 8% up until March 2001, at which point the 2001 minimum wage reform resulted in an increase of approximately 68%.

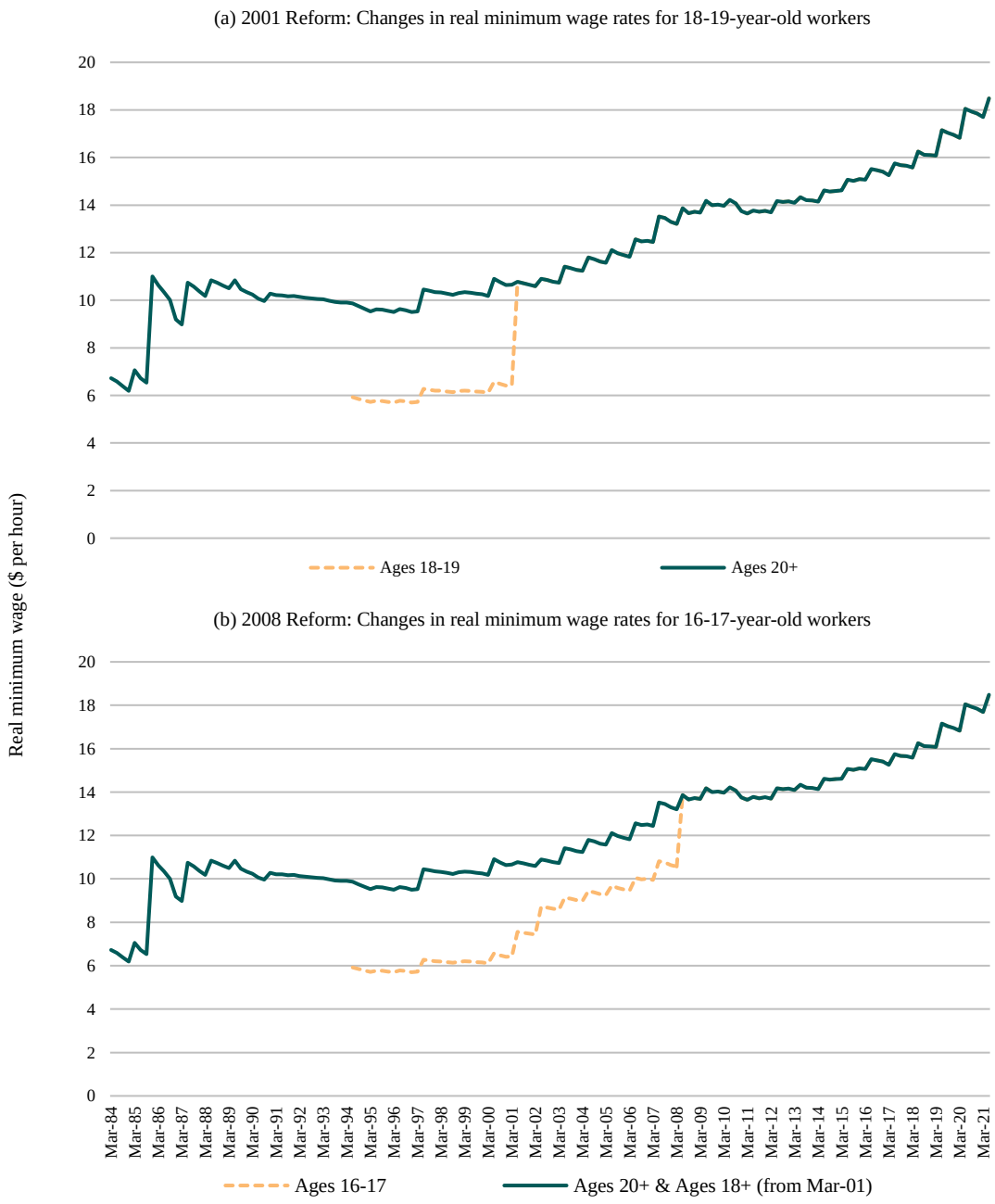

Figure 2.2: Changes in minimum wage rates from the 2001 and 2008 reforms.

Data on nominal minimum wages and CPI are from Ministry of Business Innovation & Employment (n.d.a) and Reserve Bank of New Zealand (2021), respectively. Nominal earnings were adjusted using the CPI (base month and year = March 2017). Author's compilation.

Panel (b) of Figure 2.2 presents the real minimum wage rate for workers aged 16-17 and 20 or above over the period March 1984-March 2021. As with 18-19-year-olds, 16-17-year-olds also saw little growth in the real youth minimum wage rate up until March 2001. The 2001 reform saw the real minimum wage rate for 16-17-year-olds workers increase by approximately 18% in March 2001, and a further 17% in March 2002. The single largest increase in real minimum wages for these workers occurred in April 2008, when the real minimum wage increased by approximately 31%. However, as noted earlier, this increase only impacted a sub-group of 16-17-year-old minimum wage workers depending on their previous employment experience.

Overall, changes in the real minimum wage rate presented in Figure 2.2 show how minimum wage growth for both groups of teenagers has far exceeded that of adults. At the lower end, the real minimum wage rate for teenagers aged 16-19 increased by 149% over the period March 1994-March 2021.[8] At the upper end, the real minimum wage rate for these teenagers increased by 212% over the same time period.[9] In comparison, changes in the real adult minimum wage rate only increased by 87%. These substantial differences act as a strong motivator for empirically examining the effects of minimum wage increases on teenage employment in NZ.

2.5 The Complex Nature of Minimum Wage Rules

As noted in section 2.3, rules governing eligibility for different minimum wage types have become increasingly complex. This section will examine the complexity of these rules in detail, as well as note their implications from a research perspective. The core of this examination will focus on the starting-out minimum wage, which is the current minimum wage payable to some 16-19-year-old minimum wage workers.

2.5.1 Increasing complexity of minimum wage rules.

Prior to the introduction of the new entrant minimum wage in March 2008, determining the applicable minimum wage was simply based on age. However, an age-determined youth minimum wage was considered discriminatory (New Zealand Parliament, 2007) and was replaced by the new entrant minimum wage in March 2008. The new entrant minimum wage type recognised that 16-17-year-olds may undertake similar work as older employees without the same rewards (New Zealand Government, 2008). Consequently, the coverage of the adult minimum wage was lowered to age 16 conditional on

[8] This value is based on taking the real values of the youth minimum wage and starting-out minimum wage.
[9] This value is based on taking the real values of the youth minimum wage and adult minimum wage.

employment experience. Specifically, 16-17-year-old minimum wage workers were entitled to the adult rate once they had completed 3 months or 200 hours of employment.

In response to a tight labour market and the apparent complexity of minimum wage rules, the starting-out minimum wage replaced the new entrant minimum wage in May 2013. The Labour Minister at the time, Kate Wilkinson, noted that the starting-out minimum wage aimed to: i) create demand for young people in a tight labour market, and ii) make implementing minimum wage rules simple for employers (New Zealand Government, 2012). This section will focus the discussion on the second aim of simplicity.

Applying the starting-out minimum wage requires different types of information depending on age. For 16-17-year-olds, information related to continuous employment duration per employer, job tasks and responsibilities, and industry training participation are required; in addition, social benefit receipt history by type and duration are required for 18-19-year-olds.[10] The complexity of determining what minimum wage is payable under the current legislative framework is presented in Figure 2.3.

It is difficult to reconcile the level of administration required to apply the starting-out minimum wage with the aim of creating a simple set of rules for employers. In fact, compared to the new entrant minimum wage which the starting-out minimum wage replaced, the current rules are more complex and require a greater level of personal and employment related information. Employers have reported similar perspectives in the National Survey of Employers (NSE).[11] In the 2015/16 iteration of the NSE, 35% of employers reported having employees aged 16-19. Of these employers, only 9% paid the starting-out minimum wage, citing reasons such as unclear criteria for applying the wage (6%) and administration costs outweighing potential savings (3%) (Ministry of Business Innovation & Employment, 2016).[12]

[10] Only a sub-set of social security benefits apply, and include: domestic purposes benefit, emergency benefit, independent youth benefit, invalid's benefit, jobseeker support, sickness benefit, sole parent support, supported living payment, unemployment benefit, widow's benefit, young parent payment and youth payment (Ministry of Business Innovation & Employment, n.d.b).
[11] Businesses are surveyed each year in the NSE, which collects information on workplace practices (Ministry of Business Innovation & Employment, n.d.c).
[12] Other reasons included it being unfair (50%), impacting their ability to attract quality employees (27%) or not being aware of the starting-out minimum wage (14%) (Ministry of Business Innovation & Employment, 2016).

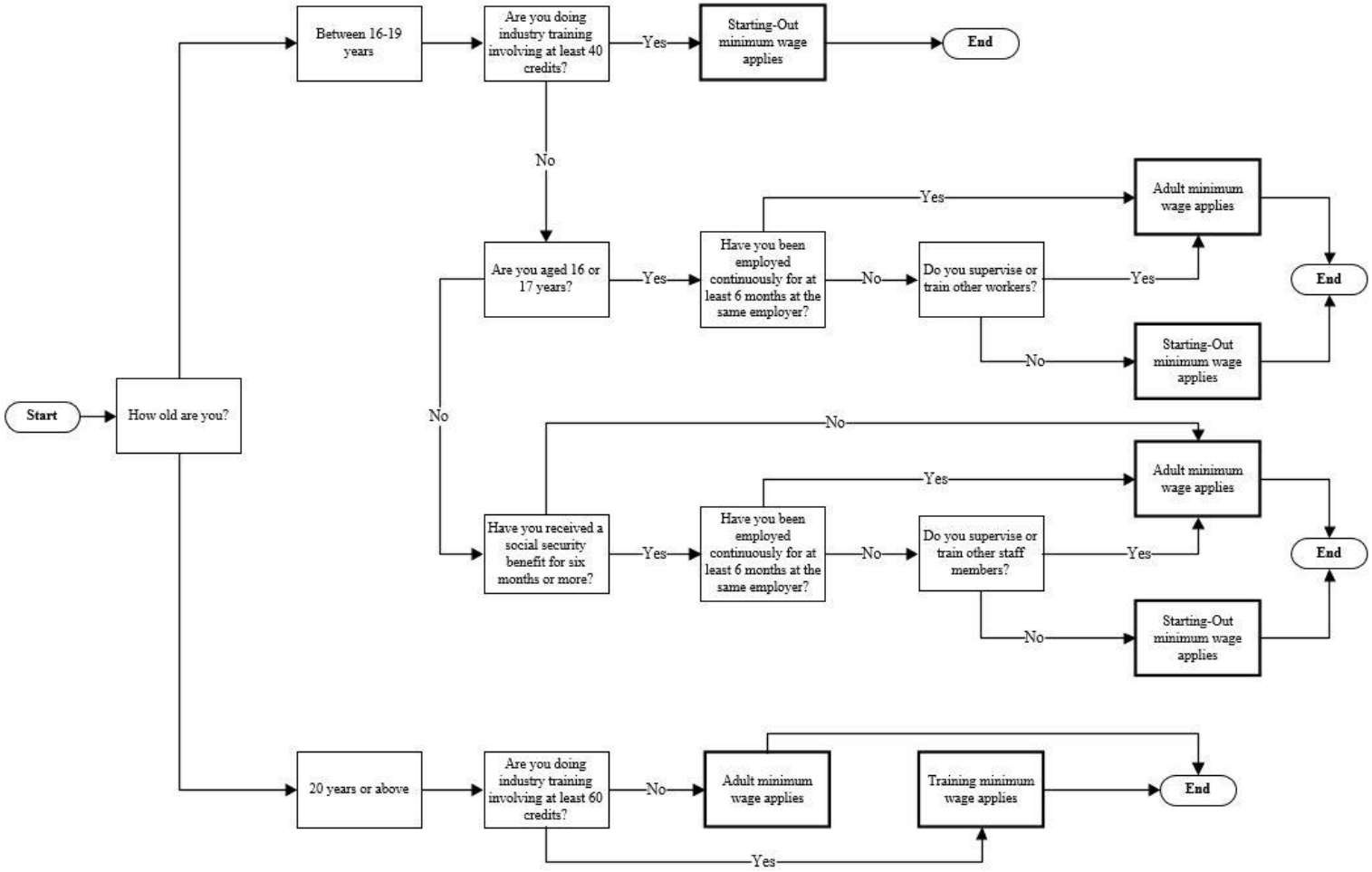

Figure 2.3. Determining the applicable minimum wage using criteria from the starting-out and adult minimum wages.
Minimum wage eligibility criteria are from the Ministry of Business Innovation & Employment (n.d.b). Author's compilation.

These complex rules also risk unintentional non-compliance, where employers may have received incomplete or inaccurate information from their new employees. [13] Unfortunately, finding descriptive evidence quantifying non-compliance from the Ministry of Business, Innovation and Employment (MBIE) is not feasible as changes to their reporting systems and collection processes for complaints, investigations and breaches of the minimum wage changed in 2012.[14] Consequently, whether the current rules have led to a higher incidence of minimum enquiries, complaints and investigations by the Labour Inspectorate is undetermined.

2.5.2 Research implications of current minimum wage rules.

The period covering the starting-out minimum wage presents several difficulties when empirically examining its effect on teenage employment. First, there has been little variation in the minimum wage rate, with annual increases averaging just over 4% in real terms. To date, the largest real increase in the starting-out minimum wage occurred in March 2020, totaling approximately 7%. In contrast, periods up to the introduction of the starting-out minimum wage included substantial increases for teenage minimum wage workers, totaling 68% in March 2001 for 18-19-year-olds; real minimum wages increased by 35% over the period March 2001/02 and 33% in March 2008 for 16-17-year-olds.

Next, the complexities inherent in the rules governing the starting-out minimum wage make identifying affected workers difficult. In contrast, identifying affected workers under age-based minimum wage policies is far simpler. This was the case up to the first quarter of 2008 when minimum wages were set as youth or adult rates.

Consequently, the period covering the youth and adult minimum wage, in particular surrounding the 2001 minimum wage reform, offers a beneficial setting from which to empirically examine the effects of minimum wage increases on teenage employment. This period offers substantial variation in teenage minimum wage rates and identifying affected minimum wage workers is relatively simple given the age-based policies.

[13] Although within a different context, Meyer, Wallace, and Sullivan (2009) and Marquis and Moore (2010) found that household survey respondents under-reported participation in social security programmes for reasons such as the stigma of programme participation or the sensitivity of income information.

[14] Data were collected via an Official Information Act request. However, the data were unreliable and thus are not reported here. Minimum wage enquires are only recorded through the MBIE's Service Centre Employment Line; no electronic enquiries are recorded (N. Summer, personal communication, March 1, 2019).

Chapter 3 Theoretical and Empirical Literature Review

3.1 Introduction

This thesis aims to empirically examine the effects of minimum wage increases on teenage employment in NZ. This chapter contributes to that aim by critically reviewing both theoretical labour market models and empirical evidence related to the employment and minimum wage relationship. To support this aim, several objectives are set:

- understand how several labour market models predict employment shifts from the imposition of statutory wage floors;
- identify the consensus among the existing empirical international literature on minimum wage impacts on employment;
- determine whether a consensus view of the effects of minimum wage increases on teenage employment exists in NZ; and
- identify opportunities where the use of administrative data could extend the NZ empirical literature on the effects of minimum wage increases on teenage employment, from both a data and methodological perspective.

This chapter is organised as follows. Section 3.2 reviews theoretical labour market models, with section 3.3 briefly summarising the development of the US empirical minimum wage literature. Section 3.4 summarises early NZ empirical literature pre-2000, before section 3.5 critically examines studies published post-2000. Section 3.6 concludes this chapter by noting key opportunities for this thesis to contribute to the NZ empirical literature.

3.2 Review of Theoretical Labour Market Models

This section provides an overview of the three widely used labour market models which have been used to help inform how minimum wage increases may impact employment. The limitations of these models are also noted.

3.2.1 The perfectly competitive labour market model.

The PCLM model is perhaps one of most widely used theoretical models when examining the effects of minimum wage increases on employment. Its history dates back to Marshall (1890), who argued that similar to the goods and services market, employment is determined by the forces of demand and supply. Based on this notion, the PCLM is

described as consisting of many firms who maximise their profits and workers are homogenous (Zavodny, 1998). The PCLM also assumes that workers must be paid more to induce them to supply additional labour, and that the value of the last unit of labour (i.e., marginal product of labour) declines as labour increases (Zavodny, 1998). These assumptions generate the upward sloping labour supply curve and upward downward labour demand curve, which, when combined, determine the market equilibrium wage rate.

Given that the labour demand curve reflects the declining value of labour (i.e., decreasing marginal product of labour [MPL]), employment decisions are dependent on whether the cost of additional labour exceeds the contribution that this labour makes to the marginal revenue product (Bray, 2013). Therefore, what is the predicted employment effect under the PCLM from the imposition of a minimum wage increase?

Figure 3.1 presents a visual representation of the PCLM. Before the introduction of a minimum wage, the market equilibrium is at point *A* (the intersection of the labour demand [L^d] and labour supply [L^s]curves). In equilibrium, firms pay the equilibrium wage rate of w^* and hire L^* units of labour.

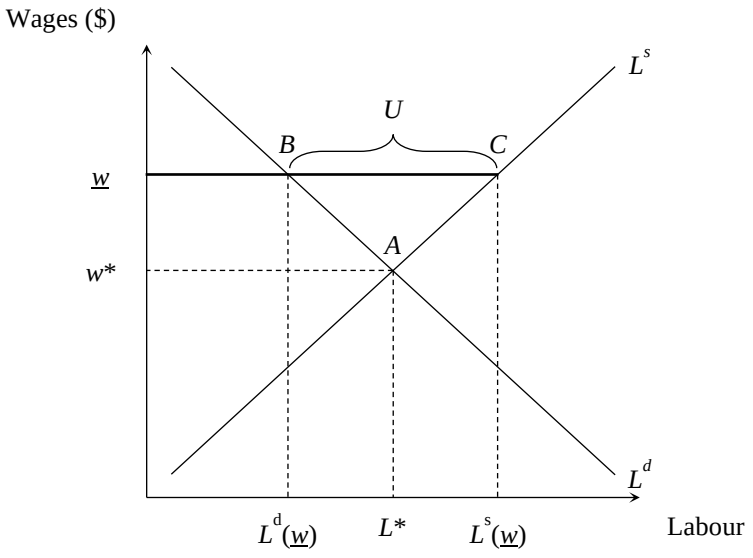

Figure 3.1: The minimum wage in a competitive labour market.
Adapted from Boeri and van Ours (2013). Author's compilation.

If a binding minimum wage ($\underline{w}$) were to be imposed above w^*, firms would be forced up the labour demand curve (moving from point A to point B). This response would negatively affect employment, depicted by the difference between $L^* - L^d(\underline{w})$. Furthermore, the imposition of $\underline{w}$ would also trigger a labour supply response, where some workers would be willing to supply additional units of labour, in combination with others entering the labour market. This response would move labour supply from point A to point C, depicted by the difference between $L^s(\underline{w}) - L^*$. Therefore, the difference between $L^s(\underline{w}) - L^d(\underline{w})$, or the potential surplus of labour at point U, represents the overall impact of the minimum wage increase, consisting of the disemployment effect ($L^* - L^d[\underline{w}]$) and the number of workers who would be willing to supply labour at $\underline{w}$ if jobs were available ($L^s(\underline{w}) - L^*$).[15] Such negative effects on employment have found wide support in the related empirical literature (Bazen & Le Gallo, 2009; Brown, Gilroy, & Kohen, 1982; Clemens & Wither, 2019; Kabátek, 2021; Kreiner et al., 2020; Neumark, 2001; Neumark & Wascher, 1992, 2000, 2002; Sabia, Burkhauser, & Hansen, 2012; Wolfson & Belman, 2019).

The magnitude of the effects on employment depends on the elasticity of labour demand. For example, if labour demand is relatively inelastic, there will be little change in the demand for labour following minimum wage increases, and the impact on employment would be small (Zavodny, 1998). Similarly, if labour demand is relatively elastic, the impact on employment from the minimum wage increase would be relatively more pronounced.

The assumptions underpinning the PCLM have often been thought of as over-simplifying the labour market (Zavodny, 1998). The following assumptions are examined here:

- homogeneity of workers;
- firms are able to accurately measure the MPL;
- full compliance; and
- full coverage.

The first assumption, that labour is homogenous, is clearly violated when examining any overall labour force. Labour is made up of individuals with different levels of human capital acquired from education, training and work experience, motivations and capacities

[15] As noted by Welch (1974) when a minimum wage exceeds the equilibrium wage, jobs are rationed. Therefore, not everyone who is willing to supply their labour at the minimum wage will find jobs.

(Bray, 2013). Consequently, different sub-markets of labour exist, such as teenagers or retail workers, each reflecting their own supply and demand. Furthermore, these various types of labour can be used by firms in different combinations to produce their goods and services. Consequently, when moving away from the assumption of homogenous labour, imposing a binding minimum wage may have no effect on total employment, as the demand for low-skilled labour may fall, whereas the demand for higher-skilled workers may rise (Zavodny, 1998).[16]

The next assumption relates to the ability of firms to accurately measure MPL. In order for this assumption to hold, firms would need to know the MPL of each employee in order to make instantaneous decisions regarding employment responses (Ehrenberg & Smith, 2009). However, this is unlikely as most firms typically operate on a medium-term financial plan and would therefore need to determine how to respond to additional wage costs over a longer time period, not instantaneously as proposed in the PCLM (Bray, 2013).

Full compliance is also assumed under the PCLM. Based on international and NZ empirical evidence, not all workers receive the statutory minimum wage (Card, 1992a, 1992b; Haugen, 2003; Haugen & Mellor, 1990; Hyslop & Stillman, 2007; Pacheco, 2009, 2011). Where a large proportion of employers did not comply with statutory minimum wage rates, predicted adverse effects on employment will not be observed; albeit partial effects are still possible where compliance is observed.

Along with full compliance, full coverage is also assumed under the PCLM. This assumption is challenged where employers are exempt from paying minimum wages, or where covered and uncovered industries may exist, giving rise to a two-sector model. In NZ, minimum wages provide near-to-full coverage to employees aged 16 or above.[17]

3.2.2 The monopsony model.

Another model, often presented as an alternative to the PCLM, is when the labour market has a monopsony structure. A monopsony consists of a labour market where there is only one buyer of labour, typically illustrated by the example of a small town with only one

[16] This assumes there are only two types of workers, and that the w^* is set above $\underline{w}$ for unskilled workers, but below $\underline{w}$ for skilled workers. An example of this effect was presented by Pereira (2003) who examined the impact of a large minimum wage increase in Portugal. The increase in minimum wage reduced employment for 18-19-year-olds while increasing employment for 20-25-year-olds.
[17] Although rare, exemptions to the NZ minimum wage include workers under the age of 16 and employees with disability (Ministry of Business Innovation & Employment, 2018a). The Parliamentary Counsel Office (n.d.a) details additional exemptions to the MWA 1983.

employer. Given the monopsonist is the single employer in the labour market, it faces an upward sloping labour supply curve, and must therefore pay higher wages to attract more workers (Neumark & Wascher, 2008). Several of the key differences between a monopsony and PCLM are summarised below:

- Unlike firms in the PCLM that are price takers, firms with monopsony power are able to set wages (Manning, 2003).
- The monopsonist pays workers a lower wage than any firm in the PCLM. Although the profit maximising wage (equilibrium wage) is at the point where the marginal cost of labour equals the marginal revenue product of labour (Robinson, 1933), due to their market power, monopsonists are able to pay workers less than their marginal product (Schmitt, 2015).
- In stark contrast to the predicted outcomes in the PCLM, the introduction of a minimum wage under a monopsony model can increase the level of employment, if not set too high (Robinson, 1933).[18]

Figure 3.2 illustrates the mechanics of the standard monopsony model (often referred to as the static monopsony model), as well as the effects on employment when introducing a minimum wage.

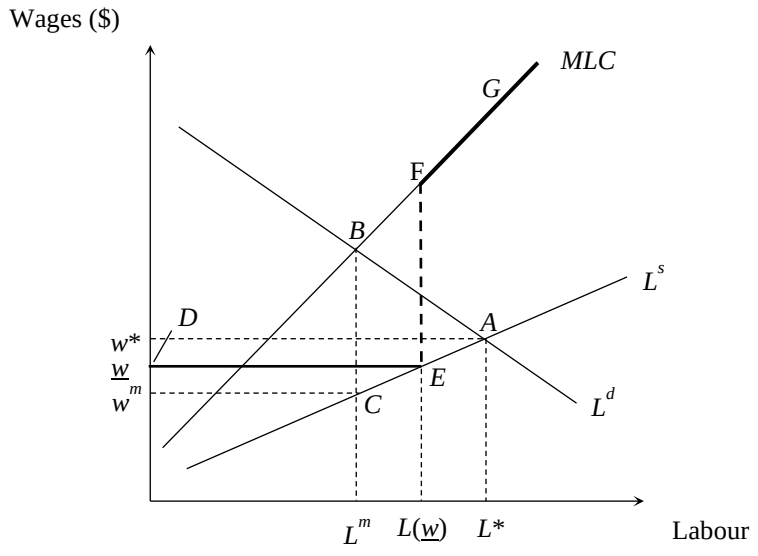

Figure 3.2: The minimum wage under a monopsony model.
Adapted from Boeri and van Ours (2013). Author's compilation.

[18] Recent empirical evidence by Azar, Huet-Vaughn, Marinescu, Taska, and von Wachter (2019) found support for the monopsony model. Examining a low-wage sector in the US, their findings revealed significant increases in the employment elasticity with respect to the minimum wage.

Equilibrium is achieved at point *B*, (which is where the marginal labour cost [*MLC*] curve and labour demand [L^d] intersect). The equilibrium employment level is therefore located at L^m, and the equilibrium wage at w^m. Note that the employment level and wage rate under a monopsonist are both lower than what they would be under the PCLM (L^* and w^*, respectively).

The introduction of minimum wages can affect employment under monopsony in several ways. Boal and Ransom (1997) described three employment determination regimes, including: i) non-binding, ii) supply-determined, and iii) demand-determined. Under a non-binding regime, a minimum wage ($\underline{w}$) set below w^m and would have no impact on employment. Under a supply-determined regime, $\underline{w}$ is set between w^m and w^*. In this instance, a new marginal cost curve is generated (segment *DEFG*). The horizontal segment *DE* shows that the marginal cost of additional workers is constant at $\underline{w}$, and if the monopsonist wished to hire more workers beyond $L(\underline{w})$, higher wages would need to be offered to attract additional workers. Therefore, under a supply-determined regime, minimum wage increases may positively impact employment. However, if $\underline{w}$ is set above w^*, the monopsonist's perceived supply curve is horizontal over the relevant area, with employment determined by L^d. Under demand-determined employment, higher minimum wages would likely decrease employment levels. The monopsony model has also found support in the empirical literature, with studies such as Dickens et al. (1999), who examined the impact of minimum wages set by British Wage Councils between 1975-1992, to be positive in many cases, with their estimates being robust across alternative minimum wage measures and specifications.[19]

3.2.3 The dynamic monopsony model.

The static monopsony model is limited by the underlying assumption of a market structure with a single buyer of labour. An alternative to the static monopsony is the dynamic monopsony, also referred to as the imperfect search model (Bray, 2013). Under a dynamic monopsony, there are many small firms who set wages. However, the key point of difference in this market structure, relative to the static monopsony, is that a firm's ability to retain or attract workers is also dependant on wages offered by other firms. This dependency occurs as workers are assumed to accept any wage higher than their current wage, while individuals who are not employed will accept any wage higher than their reservation wage (Burdett & Mortensen, 1989). In essence, the dynamic monopsony

[19] Additional empirical evidence of labour markets with monopsony power is provided by Manning (2006) and Staiger et al. (2010).

model holds that a firm's equilibrium employment is determined by the flow of workers (i.e., those that leave / join) and that the flow is a function of the wage offered by the firm (Ashenfelter, Farber, & Ransom, 2010).

Another appealing feature of the dynamic monopsony model is that workers are assumed to have imperfect knowledge (Machin & Manning, 1992). Specifically, workers are uninformed of job opportunities available at all firms, with their knowledge only limited to the offers they receive. Consequently, in a situation where a firm offers lower wages when compared to those offered from other firms, over time, it will lose its employees (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2021; Zavodny, 1998). Arguably, workers having imperfect knowledge is more consistent with how the labour market operates when compared to workers having perfect knowledge under the assumption in the PCLM.

This type of market structure is more relatable with respect to examining the impacts of minimum wages on employment (Zavodny, 1998). For example, minimum wage employment is generally found within industries which are made up of many small firms (e.g., hospitality: cafes, bars, restaurants). Under a dynamic monopsony structure, the imposition of a minimum wage may increase employment.[20] For example, firms who were previously paying wages below the new minimum wage would be obligated to offer higher wages up to the new minimum requirements. This higher wage offer may attract individuals who have been unemployed if the new minimum wage is equal to their reservation wage. A recent study by Bachmann and Frings (2017) examined the degree of monopsony power across different low-wage industries. Different levels of monopsony power were observed across industries, suggesting that minimum wage increases may result in heterogeneous effects on employment, as proposed under a dynamic monopsony market structure. In industries where high monopsony power was observed (e.g., retail, hotels, restaurants), adverse effects on employment from minimum wage increases were mitigated; in other industries (e.g., other service & manufacturing of food products), this was not the case (Bachmann & Frings, 2017).

[20] Card and Krueger (1994) presented empirical evidence that higher minimum wage may have no impact, or in some cases positive impacts, on the employment of fast-food restaurants in New Jersey.

3.2.4 Rationalising contrasting theoretical labour market models.

The objective of this theoretical literature review was to understand how several labour market models predict employment shifts from the imposition of statutory wage floors. To achieve this objective, several labour market models commonly used to inform how employment may be affected by minimum wage increases were discussed. The review of these models revealed the contrasting predictions of the effects of minimum wage increases on employment, being negative in the PCLM, generally positive in the static monopsony, and mixed in the dynamic monopsony depending on the firm's wage offer in relation to other firms.

Furthermore, this review also uncovered limitations in the PCLM and the static monopsony. Several assumptions underlying the predictions of the PCLM are unrealistic when considering labour markets (e.g., homogeneity of workers, full compliance). Low-wage labour markets typically consist of more than one firm, unlike assumed under the static monopsony model. Notwithstanding these limitations, empirical support for all three labour market models were noted.

Consequently, how can these contrasting model predictions be rationalised with respect to the effects of increases of the minimum wage on employment? Perhaps a practical approach is to acknowledge that each model is attempting to explain a single dimension of a complex relationship where there are many variable factors (Bray, 2013). Furthermore, each of these models may only be applicable within specific contexts. As noted by Cahuc (2006):

> Everything depends on conditions at the outset. If the minimum wage is low, bordering on basic welfare, an increase in it attracts new workers whom firms have an interest in hiring. But if the minimum wage is high at the onset, every increase incentivizes firms to trim they payrolls. (pp. 45-46)

Based on these considerations, it appears that there is no single correct model predicting the effects on employment of minimum wage increases. Therefore, all models will be considered to understand the impacts of increases in minimum wages on teenage employment in NZ.

3.3 Brief History of Early Empirical Literature in the US

This section provides a brief history of the early empirical literature in the US with the intention being to identify the consensus of minimum wage impacts on employment among early empirical US literature.

3.3.1 What is the consensus?

The inconsistent impacts of minimum wage increases on employment have been observed for over a century. One of the earliest studies by Obenauer and von der Nienburg (1915), examined the effects of two minimum wage increases in 1913 and 1914 on the outcomes of women in Oregon. Their findings indicated that three groups of women displayed differential pre- and post-employment outcomes following minimum wage increases. In particular, the employment of female teenagers (i.e., those under 18 years) was positively affected, compared to the negative effects on the employment of experienced and inexperienced adult women.[21] These findings were rationalised by the tiered minimum wage rates, with a lower rate payable to teenage and adult women with no work experience. Consequently, in occupations where little skills were required, teenagers were preferred, at the expense of experienced, and more costly, adult women (Obenauer & von der Nienburg, 1915).

Several studies thereafter presented opposing views of minimum wage effects on employment. Based on the questionnaire responses of manufacturing executives from firms across several industries and geographies,[22] Lester (1946) concluded that minimum wage increases had little to no influence on a firm's employment. Peterson (1957) questioned the data quality of empirical studies between 1938-1950 which observed that minimum wage increases had no effects on employment, and particularly questioned whether adequate data were presented to isolate any effects. Moreover, Peterson (1957) argued that cross-sectional analysis of firms within the same industries would be a more appropriate method for testing the minimum wage-employment hypothesis. After examining the effects of the minimum wage increases from the Fair Labour Standards Act in 1938-1950 across three industries,[23] Peterson (1957) concluded that "employment changes will be inversely related to wage increases" (p. 430). In response, Lester (1960)

[21] Adult women, over the age of 18, were classified as experienced when they had completed more than 1 year of employment within a specific occupation (Obenauer & von der Nienburg, 1915).
[22] The industries included: furniture producers, metalworking, cotton clothing manufacturers, full-fashioned hosiery manufacturers, producers of shoes and leather, paint producers, chemical manufacturers and stove producers. The firms were restricted to Southern states, and included: Alabama, Arkansas, Florida, Georgia, Louisiana, North Carolina, Tennessee, Texas and Virginia (Lester, 1946).
[23] Minimum wages increased from $0.25 in 1938 to $0.75 in 1950 (U.S. Department of Labor, n.d.a). The three industries examined were: Southern Sawmill, men's cotton garment and seamless hosiery (Peterson, 1957).

re-examined the study by Peterson (1957) and concluded that due to Peterson's models having had low predictive power, he failed to accurately portray the effects of minimum wage increases on employment. The re-examination held that various effects were possible, including positive effects, following moderate minimum wage increases when accounting for a broader range of factors, such as technological advancements and employment regulations, which influence employment levels (Lester, 1960). Exchanges analogous to those of Lester (1946, 1960) and Peterson (1957, 1959, 1960) were symbolic of the debate on the minimum wage-employment relationship, and continued into the following decade (Gramlich, Flanagan, & Wachter, 1976; Kaitz, 1970; Ragan, 1977).

To help resolve the debate and bring to light 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. The commission was tasked with studying the social, political and economic implications of the federal minimum wage, which included the effects on employment and unemployment (Minimum Wage Study Commission, 1981). As part of this undertaking, Brown et al. (1982) surveyed time-series on cross-sectional empirical studies on the effects of minimum wage increases on employment and unemployment. Their conclusions, which were consistent with the predictions of the PCLM, held 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), with the effects for young adults (aged 20-24) being smaller (less than 1%). These conclusions became the general consensus concerning the direction and magnitude of minimum wage impacts on teenage and youth employment.

By the 1990s, new empirical evidence, which was published in the October 1992 issue of the *Industrial and Labor Relations Review* (ILRR), set the scene for the minimum wage debate to reignite. Of the four papers, three directly challenged the earlier consensus of youth disemployment effects.

One study by Card (1992a) examined the impact of the April 1990 increase in the federal minimum wage on several outcomes, including teenage employment. Performing a differences analyses using group and individual state-level data, Card (1992a) found no evidence that the federal minimum wage increase reduced teenage employment.

A second paper by Card (1992b) used the July 1988 increase in California's minimum wage to examine effects on the employment of two groups: i) teenagers, and ii) eating

and drinking establishments. Using data from the Current Population Survey (CPS), Card (1992b) adopted a DID approach and found that even with the increase in earnings (around 10%), the employment of teenagers also increased by 4%. For low-wage workers in eating and drinking establishments, no evidence of decreases in employment were found (Card, 1992b).

Adding fuel to the debate, Katz and Krueger (1992) compared the employment growth of fast-food restaurants in Texas following the increase in the federal minimum wage in April 1990 and April 1991. Data were collected from fast-food restaurants via self-administered questionnaires, the first sent in December 1990, and the second between July and August 1991.[24] Following a differences analysis, Katz and Krueger (1992) concluded that increases in the federal minimum wage had positive effects on the employment at fast-food restaurants most affected by the increase.

Evidence in support of the consensus was presented Neumark and Wascher (1992). Using national time-series data, state-by-year panels were used to re-evaluate existing evidence on how state minimum wage increases impacted the youth labour market. Neumark and Wascher (1992) found that there were significant disemployment effects for both teenagers (aged 16-19) and young adults (aged 16-24), with employment elasticities ranging from -0.1 to -0.2 and -0.15 to -0.2, respectively.

These contrasting findings initiated several exchanges between Card et al. (1994) and Neumark and Wascher (1994). The initial comment by Card et al. (1994) criticised several aspects of the empirical analyses by Neumark and Wascher (1992), including enrolment and minimum wage measures, and coverage rate adjustments. For school enrolment, Card et al. (1994) argued that the measure under-counted total enrolment as it excluded teenagers who were enrolled in school and employed concurrently. The inclusion of enrolment as part of the empirical estimation procedure was also questioned, given potential endogeneity with employment (Card et al., 1994). The minimum wage measure utilised by Neumark and Wascher (1992), was noted as having a negative association with the average wage of teenagers, when it should have been positive (Card et al., 1994). The use of the average adult wage in the state as the denominator in the minimum wage index was identified as driving the negative association between the minimum wage and

[24] The fast-food restaurants surveyed were Burger King, Kentucky Fried Chicken, and Wendy's.

average teenage wages.[25] Furthermore, the inclusion of the average adult wage in a state as the denominator to the index gave rise to further endogeneity concerns, as factors that cause adult wages to increase may also affect the growth of teenage employment (Card et al., 1994).

The validity of the coverage rate adjustment of the minimum wage index was another notable critique by Card et al. (1994). The coverage rate adjustment was based on a Bureau of Labor Statistics (BLS) estimate of non-supervisory workers in each state covered by the federal minimum wage. This coverage rate included all workers, which in itself was problematic, as the data in Neumark and Wascher (1992) study related to teenagers and young adults. The coverage rate also made no allowance for state minimum wage coverage, which Card et al. (1994) argued underestimated the coverage of the minimum wage.

Card et al. (1994) also re-examined the minimum wage effects on employment by using the data from Neumark and Wascher (1992), along with more precise state-level data.[26] The re-examination concluded there was no evidence of significant effects on employment, and that effects varied in direction and were close to zero (Card et al., 1994).

Neumark and Wascher (1994) replied to the comment by Card et al. (1994), and in doing so supported their earlier empirical paper. In response to their choice of measure for school enrolment, Neumark and Wascher (1994) argued it captured supply side shifts which may have impacted enrolment but not employment. Nonetheless, formally testing an alternative definition of enrolment re-affirmed their earlier findings of negative effects on the employment of teenagers and young adults (Neumark & Wascher, 1994).[27] To address the endogeneity concerns raised by Card et al. (1994), instruments were used to correct for any endogeneity-bias.[28] The results from utilising these instruments did not alter their original findings (Neumark & Wascher, 1994).

Responding to the issues raised regarding the minimum wage index, Neumark and Wascher (1994) maintained their earlier position, and critically addressed the alternative specification presented by Card et al. (1994). In particular, Neumark and Wascher (1994)

[25] Card et al. (1994) noted that states that had high or increasing adult wages, also had high or increasing teenage wages, which resulted in a negative correlation between the minimum wage index and average teenage wages.
[26] Employment-to-population ratios were taken from the BLS Geographic Profiles of Employment and Unemployment by Card et al. (1994) as opposed to being estimated from the May CPS data as per Neumark and Wascher (1992).
[27] The alternative definition counted individuals who reported their major activity as 'school'. This measurement of enrolment captured some instances where individuals were both enrolled in school and also employed (Neumark & Wascher, 1994).
[28] The instruments selected were school expenditures per pupil, pupil-teacher ratios and dummy variables for compulsory schooling ages (Neumark & Wascher, 1994).

stated that the use of a nominal minimum wage is unusual as both labour supply and labour demand are assumed to respond to changes in real wage rates. Furthermore, Neumark and Wascher (1994) also speculated that unlike a nominal minimum wage, a relative minimum wage variable can inherently offset part of the endogeneity bias.[29] The final critique aimed at the alternative specification presented by Card et al. (1994), was that it does not capture any of the employment-reducing effects described in standard production theory. In contrast, the relative minimum wage captures labour-to-labour substitution effects, where the relative cost of less-skilled labour becomes higher following minimum wage increases (Neumark & Wascher, 1994).

Another aspect of Neumark and Wascher (1992) which received critical attention from Card et al. (1994) was the coverage rate adjustment to the minimum wage index. Here, Neumark and Wascher (1994) acknowledged that they wanted to test the sensitivity of their findings using alternative measurements, although they were unable to do so due to data limitations. However, Neumark and Wascher (1994) did note that because their employment model included state and year effects, their coverage rate adjustment would only be problematic if the relative coverage rates of young workers and all workers changed both over time and across states. Nonetheless, a re-estimation of their employment model where the coverage rate was separated from the relative minimum wage rate still indicated negative effects on employment (Neumark & Wascher, 1994).

Further challenging the general consensus, Card and Krueger (1994) examined effects on the employment at fast-food restaurants in New Jersey (NJ) and Pennsylvania (PA) following a minimum wage increase in NJ.[30] Data were collected from 410 fast-food restaurants via phone interview between February-March 1992, and again in November-December 1992.[31] Based on the empirical results estimated using a DID analysis, Card and Krueger (1994) concluded that the increases in the NJ minimum wage had no negative effect on employment. In fact, evidence suggested employment increased in NJ relative to PA (Card & Krueger, 1994).

The start of the new millennia saw a new, and equally critical, exchange between Neumark and Wascher (2000) and Card and Krueger (2000). Like their earlier exchange,

[29] Neumark and Wascher (1994) argued that when a relative minimum wage variable is used, high labour demand might raise the average wage, thereby offsetting part of the endogeneity attributable to government wanting to increase the minimum wage in tight labour markets.
[30] New Jersey's state minimum wage increased to $5.05 per hour (up from $4.25 per hour) in April 1992; Pennsylvania's minimum wage remained unchanged (Card & Krueger, 1994).
[31] The sample included Burger King, KFC, Wendy's, and Roy Rogers chains (Card & Krueger, 1994).

factors concerning data featured prominently. Commenting on Card and Krueger (1994), Neumark and Wascher (2000) suggested the presence of severe measurement error in the data due to flaws in survey design.[32] To mitigate these data concerns, Neumark and Wascher (2000) constructed an administrative sample from payroll records of fast-food restaurants in NJ and PA, which "overlaps extensively with CK's sample" (p. 1390).[33] This was one of the first instances where administrative data were utilised in the empirical minimum wage literature. Based on a replication analysis of the DID estimation of Card and Krueger (1994), the NJ minimum wage increase decreased employment at fast-food restaurants in NJ relative to PA (Neumark & Wascher, 2000).

Given the contrasting employment outcomes from these analyses, Neumark and Wascher (2000) deemed it worthwhile to compare the estimates against those from an alternative data source, preferably one that stemmed from a standard data collection programme, such as the BLS series on establishment employment data.[34] Using these data, Neumark and Wascher (2000) found negative effects on employment at eating and drinking places, with the findings robust across several regression specifications.

Card and Krueger (2000) replied to Neumark and Wascher (2000) by undertaking several empirical analyses utilising establishment employment data from the BLS series.[35] Their first analysis adopted the same approach as Card and Krueger (1994) which confirmed their original findings that showed increases in the NJ minimum wage not impacting employment at fast-food restaurants. The next analysis examined long-run effects of the NJ minimum wage increase in 1994 using a sample covering the period 1991-1997. Here, their findings were also consistent with those from their earlier paper.

The BLS sample also enabled a new policy evaluation to be undertaken. In October 1996, the federal minimum wage increased, which was binding in PA but not NJ (Card & Krueger, 2000).[36] Empirically examining the employment effect of the PA minimum

[32] For example, Neumark and Wascher (2000) stated that employment, and the 'interval of employment', were not clearly defined in the questionnaire designed by Card et al. (1994), and was therefore open to interpretation by survey respondents.
[33] The administrative sample was considered advantageous as payroll records contained information on hours worked over clearly defined intervals, either weekly, bi-weekly or monthly (Neumark & Wascher, 2000).
[34] These data were collected as part of administering unemployment insurance programmes. These data were limited in that 'fast-food restaurants' were not identifiable in the BLS, though 'food and drinking places' were (Neumark & Wascher, 2000).
[35] Here, Card and Krueger (2000) adopted the ES-202 data, whereas Neumark and Wascher (2000) adopted both the BLS-790 data and the ES-202 data. The ES-202 is state and county level data only available via request to the BLS; BLS-790 is state data available on the internet (Neumark & Wascher, 2000).
[36] The federal minimum wage increased from $4.25 to $4.75 per hour. The NJ minimum wage was $5.05 per hour at that time (Card & Krueger, 2000).

wage increase, Card and Krueger (2000) found no evidence of employment loss in PA relative to NJ.

Card and Krueger (2000) also critically addressed the data central to the critique by Neumark and Wascher (2000). In particular, the sample was argued as non-representative due to the sampling method utilised and the timing of the data collection.[37] The sampling method were of particular interest and served as the focus of the critique. Payroll data were initially collected by Richard Berman of the Employment Policies Institute (EPI),[38] later supplemented with data collected by Neumark and Wascher (2000). Berman, through the EPI, partly collected data through industry contacts. Furthermore, data on all 23 restaurants in PA came from a single Burger King franchisee, and none from KFC, which was shown to be a slow-growing fast-food chain (Card & Krueger, 2000). Furthermore, Card and Krueger (2000) argued that letters sent to franchisees by Neumark and Wascher potentially induced certain response patterns, given their emphasise of working "in conjunction with the Employment Policies Institute, a restaurant-supported lobbying and research organization" (Neumark & Wascher, 2000, p. 1395). In comparing the payroll data of Neumark and Wascher (2000), Card and Krueger (1994) noted that they observed noticeable differences with respect to PA, especially where data collected by Berman were included. Taken collectively, these factors raised concerns about the data utilised by Neumark and Wascher (2000), even more so as it was central to their critique of Card and Krueger (1994).

Concluding their reply, Card and Krueger (2000) stated that "*the increase in New Jersey's minimum wage probably had no effect on total employment in New Jersey's fast-food industry, and possibly had a small positive effect.*" [italics in the original] (p. 1419).

3.4 Brief Review of New Zealand Empirical Literature Pre-2000

To begin the review of the NZ minimum wage literature, empirical studies published prior to the year 2000 will be summarised. This will provide initial insight into whether a consensus view of minimum wage impacts on teenage employment exists in NZ. Key details of the studies included in this review are summarised in Table 3.1.

[37] Data collection took place three years following the NJ minimum wage increase (Card & Krueger, 2000).
[38] Also referred to as the Berman sample.

Table 3.1:

Summary of Early NZ Empirical Literature Examining the Effects of Minimum Wage Increases on Employment

Study	Data[a]	Sample population	Method	Results
Maloney (1995)	HLFS 1985q4-1993q4.	Young adults (aged 20-24).	Time-series regression with the following dependent variables used in separate regressions: i) log of employment-to-population ratio of 15-19-year-olds relative to those aged 25 or above, and ii) log of employment-to-population ratio of 20-24-year-olds relative to those aged 25 or above. Right-hand side variables included the log of a price-adjusted minimum wage (current and 4-lags),[b] an annual time trend, quarterly dummies, prime-aged unemployment rate, the log of age-specific unemployment benefits, and the log of age-specific enrolment rates.	The employment elasticity for young adults was estimated at -0.35 (-0.57 when restricted to those with no qualifications). Positive spillovers observed for teenagers, with the teenage employment elasticity estimated at 0.69.
Chapple (1997)	HLFS 1985q4-1997q1; QES 1980-1997.	Young adults (aged 20-24); Low-wage industries.	Time-series regression with the employment-to-population ratio of 20-24-year-olds as the dependent variable. Right-hand side variables included the log of a price-adjusted minimum wage (current and 4-lags),[c] the log of price-adjusted average hourly earnings (4-lags),[d] the log of the price of inputs relative to outputs (4-lags), and the log of the employment-to-population ratio for the rest of the population.	The employment elasticities for young adults in the range of -0.18 to -0.34 were estimated, though deemed unreliable due to auto-correlations. Industry results were mixed. Across 29 industries examined there was a relatively even split between positive and negative employment elasticities. No correlation between low-wage industries and negative employment elasticities were observed.
Maloney (1997)	HLFS 1985q4-1996q2.	Young adults (aged 20-24).	Time series regression used, largely the same as in Maloney (1995). The following adjustments were made: i) the employment-to-population ratios used as dependent variable were not divided by the employment propensities of those aged 25 or above, ii) a three quarter lag was adopted, iii) no age-specific enrolment rates were included, and iv) to control for the introduction of the youth minimum wage in 1994, a dummy variable was added.	The employment elasticity for young adults was estimated at -0.38. Unlike in Maloney (1995), there was no evidence of spillovers observed for teenagers. Furthermore, the introduction of the youth minimum wage appeared to have had no impact on the employment of teenagers.
Pacheco and Maloney (1999)	HLFS 1986q2-2000q2.	Women with no qualifications (aged 15+, 20+ & 20-29); Females with qualifications acted as control group.	These regressions used the log of employment propensity[e] as the dependent variable. Right hand side variables included the log of a price adjusted minimum wage (current and 4-lags),[f] quarterly dummies, prime-aged male unemployment rate, and a linear time trend. A subsequent specification included the log of labour demand for qualified females.	The elasticities range between -0.71 and -0.72 for the labour demand of unqualified females in the 15+ and 20+ age groups. These were only statistically significant once the linear time trend was removed from the specification. Results were consistent when controlling for qualified females.

Note. Author's compilation.

[a] HLFS = Household Labour Force Survey; QES = Quarterly Employment Survey.

[b] The minimum wage variable was defined as the ratio of the minimum wage to the mean hourly earnings of all workers (Maloney, 1995).

[c] The minimum wage variable was adjusted using the Producer Price Index for Outputs (Chapple, 1997).

[d] The average hourly earnings variable was adjusted using the Producer Price Index for Outputs (Chapple, 1997).

[e] Employment propensity was defined as the percentage of the working-age population employed at the time of each survey (Pacheco & Maloney, 1999).

[f] The minimum wage variable was adjusted using the Producer Price Index for Outputs (Pacheco & Maloney, 1999).

The first significant study in NZ to empirically investigate the effects minimum wage increases on employment was by Maloney (1995). Using time-series regressions, Maloney (1995) examined whether increases in the adult minimum wage had differential effects on the employment of young adults relative to teenagers, who were exempt from minimum wage coverage over the period examined. Although findings suggested disemployment effects for young adults, and positive spillover effects for teenagers, the study was inhibited by a short time-series and minimal variation in the minimum wage.[39] Furthermore, there were concerns of endogeneity as enrolment was included as an explanatory variable, with both enrolment and employment being influenced by changes in the minimum wage. In a subsequent paper, Maloney (1997) utilised a longer time-series and excluded enrolment as an explanatory variable. Findings from the two papers were consistent for young adults. Overall, Maloney (1997) concluded that the findings were far from conclusive.

The next empirical study was by Chapple (1997) who examined the impact of minimum wage increases on changes to the relative employment of young adults, and employment changes across industries. The preferred specification could not reject the hypothesis that the minimum wage impact on youth employment was zero (Chapple, 1997). Alternative specification produced negative employment elasticities, however these were not considered robust due to auto-correlation (Chapple, 1997). Findings from the industry analysis were mixed between positive and negative employment changes. Furthermore, there was no correlation between low-wage industries and negative employment elasticities (Chapple, 1997).

The final empirical study was by Pacheco and Maloney (1999) who examined whether minimum wage increases reduced the employment of females of various age groups with no school or post-school qualifications. Females with both school and post-school qualifications were treated as the 'control' group in part of the analyses, as it was assumed that they generally faced higher wages in the labour market and would therefore not be directly affected by increases in the minimum wage. Negative employment elasticities were observed for unqualified females aged 15 or above, and 20 or over (Pacheco & Maloney, 1999). The results were consistent once adding a control for qualified females. Although providing valuable insight, the sample population was limited to a small sub-

[39] From 1985q4-1993q4, only one substantial increase in the nominal adult minimum wages occurred (approximately 24%).

group within the labour market and could therefore not be generalised across a wider population.

Although earlier NZ literature on the employment effects of minimum wage increases observed disemployment effects to some extent, there was arguably no consensus given the mixed empirical evidence. This was due to several reasons, being short time-series, non-robust findings from various specifications, and narrowly defined sample populations.

3.5 Review of New Zealand Empirical Literature Post-2000

To continue the review of the NZ minimum wage literature, empirical studies published following the year 2000 will be critically examined. In addition to the period of publication, the scope of this literature review includes: i) studies which examined labour market outcomes for teenagers, and/or ii) studies which utilised administrative data. The objective of this literature review is to: i) determine whether a consensus view of minimum wage impacts on teenage employment in NZ has been reached based on selected empirical evidence, and ii) identify opportunities where the use of administrative data could extend the NZ empirical literature on the effects of minimum wage increases on teenage employment, from both a data and methodological perspective.

3.5.1 Hyslop and Stillman (2007).

The impact of the 2001 minimum wage reform on a range of outcomes was empirically examined by Hyslop and Stillman (2007). Their paper exploited substantial variation in the real minimum wage rates for 16-17 and 18-19-year-olds, totalling 35% and 68%, respectively. Although a range of outcomes was examined (e.g., hours worked, earnings, inactivity) by Hyslop and Stillman (2007), for the purpose of this literature review, only employment will be considered.

Hyslop and Stillman (2007) utilised quarterly survey data from the HLFS covering the period 1997q1-2003q3. For the empirical analyses, a DID identification strategy was adopted to compare the average outcomes for two treatment groups, defined as 16-17 and 18-19-year-olds, against the average outcomes of the control group, defined as 20-25-year-olds, before and after the 2001 minimum wage reform. The pre- and post-treatment periods were defined as 1998q2-1999q3 and 2002q2-2003q3, respectively.

An initial examination of changes in employment before and after the 2001 minimum wage reform saw an increase in the percentages of employed 16-17 and 18-19-year-olds (Hyslop & Stillman, 2007). When compared to the changes for 20-25-year-olds, 16-17 and 18-19-year-olds both saw positive percentage point increases in employment, although not statistically significantly different from zero (Hyslop & Stillman, 2007).

Following the initial estimation of average treatment effects on employment, Hyslop and Stillman (2007) presented the following regression specification to control for a range of other factors which may have influenced the outcomes of interest:

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

where Y_{it} is the outcome of interest (i.e., employment dummy) for individual *i* at time *t*. On the right side of the equation, *age*16-17 and *age*18-19 are treatment group dummies, with *Post*-2001 being a post-reform period dummy. X_{itj} is a vector of *j* relevant covariates for individual *i* and time *t*. These controls included individual age dummies, quarter dummies, a range of demographic controls (e.g., gender, ethnicity, marital status) and the relative size of the population of each age group by year to control for supply side effects. Hyslop and Stillman (2007) also estimated a number of alternative specifications, the descriptions of which are presented in Table 3.2, along with a summary of the estimation results.

The empirical results revealed largely mixed estimations of the effect of the 2001 minimum wage reform on teenage employment across all specifications. In their preferred specification, which Hyslop and Stillman (2007) identified as specification (1.7), estimates on employment were negative across each post-reform year and across both treatment groups. For 16-17-year-old workers, there were negative effects on employment by 2003, albeit weakly significant (δ_{16-17} = -0.039, $p < 0.10$), suggesting that they were approximately 4 percentage points less likely to be employed at two years following the 2001 minimum wage reform, holding all other factors constant (Hyslop & Stillman, 2007). No evidence of statistically significant effects on employment for 18-19-year-olds were observed, or for 20-21-year-olds.

Table 3.2:

Definitions and Results of Regression Specifications Estimated by Hyslop and Stillman (2007)

Controls	Description	Specification (1.1)	(1.2)	(1.3)	(1.4)	(1.5)	(1.6)	(1.7)	Results
Covariates	Dummy variables for age, quarter, gender, ethnicity, marital status, NZ born, urbanicity and region of residence). A measure for the relative size of each age group in a particular year.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	16-17 = negative (insig.[a]); 18-19 = negative (insig.).
Spillover effects	Interaction term between age group dummy (*age20-21*) and post-treatment period (*Post-2001*).		Yes	Yes	Yes	Yes	Yes	Yes	16-17 = negative (insig.); 18-19 = negative (insig.).
Proxy responses	Dummy variable indicating a proxy response, plus interactions with each of the control variables.			Yes	Yes	Yes	Yes	Yes	16-17 = positive (insig.); 18-19 = negative (insig.).
Separate post-reform year impacts	Interaction term between the age group dummies and the individual year dummies (2001, 2002 & 2003) to allow the effects to vary by post-reform year				Yes	Yes	Yes	Yes	16-17 = mixed (weakly significant in 2001); 18-19 = mixed (insig.).
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	16-17 = mixed (weakly significant negative in 2003); 18-19 = Negative (significant in 2003).
Age-specific seasonal effects	Interaction terms between quarter dummies and individual age dummies.						Yes	Yes	16-17 = mixed (insig.); 18-19 = negative (insig.).
Age-specific business cycle effects	Interaction terms between prime-aged unemployment rate (aged 26-49) and individual age dummies.							Yes	16-17 = negative (weakly significant in 2003); 18-19 = negative (insig.).

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

[a] insig. = not statistically significantly different from zero.

Hyslop and Stillman (2007) noted that taken together, their results indicate a positive labour supply response to the 2001 minimum wage reform, partially accommodated by the demand side. Arguably, the more unexpected results were those pertaining to the employment of 18-19-year-olds given the magnitude of the increase in their real minimum wage. Hyslop and Stillman (2007) argued the weaker impact of the 2001 minimum wage reform on 18-19-year-olds was due to fewer 'affected' workers when compared to 16-17-year-olds.

One of the key assumptions underpinning identification using DID is the parallel trends assumptions. Parallel trends are vital as the control group provides a counterfactual of the trend the treatment group would have followed in the absence of the treatment. That is, if the 2001 minimum wage policy reform did not take place, the employment trend of 20-25-year-olds would provide the counterfactual of the employment trend for 16-17 and 18-19-year-olds.

Unfortunately, there is no way of definitively testing for parallel trends. However, visual inspections of the outcome variable over the pre-treatment observation window provides insight into whether the trends for the treatment and control groups moved in unison. Quarterly employment trends were visually examined over the period 1997q1-2003q3.[40] Although employment trends were summarised, they were not described within the context of parallel trends. Nonetheless, by examining the figure, strong seasonal shifts in teenage employment are observable over the pre- and post-treatment periods. Seasonal employment for 20-25-year-olds did not appear to move to the same extent as those of teenagers, and thus caution should be applied with respect to assuming parallel trends.

In their study, Hyslop and Stillman (2007) examined parallel trends between teenage and young adult employment in the years prior to the 2001 minimum wage policy reform. They concluded that, "Over the ten year period prior to the reforms, 1991–2001, we are not able to reject the hypothesis that teenage employment rates move one-for-one with those of young adults" (Hyslop & Stillman, 2007, p. 209). This was based on regressing teenage employment rates on that of 20-25-year-olds over a 40 month period; opposing regression results were observed when extending the period to 60 months. These opposing results were rationalised by Hyslop and Stillman (2007) as being influenced by the structural reforms of the 1980s, which provided more employment opportunities for

[40] See Hyslop and Stillman (2007), Figure 4, p. 216.

young people. Pacheco (2011) questioned whether it was valid to assume that the demand for teenage labour relative to the demand for young adult labour would not be differentially impacted by economic conditions post-2000, where teenagers appeared to benefit more than other age groups.

Hyslop and Stillman (2007) concluded their paper by noting that they observed weak evidence of lagged negative effects on the employment of 16-17-year-olds by 2003. However, by considering the critiques noted above, in combination with the inconsistent regression results across specifications, one could conclude that no clear evidence on the effects of minimum wage increases on teenage employment were presented.

3.5.2 Hyslop and Stillman (2021).

Following their initial paper on the 2001 minimum wage reform, Hyslop and Stillman (2021) also empirically examined the teenage employment impact from the introduction of the new entrant minimum wage in April 2008. The new entrant minimum wage increased the real wage of a subset of 16-17-year-old minimum wage workers by approximately 31%. Using this variation in the real minimum wage, Hyslop and Stillman (2021) examined its causal effect on a range of outcomes, including employment, hours worked, earnings and incomes of teenagers. As with their earlier paper, this literature review only considers employment.

For their main analysis, Hyslop and Stillman (2021) adopted a DID approach using quarterly survey data from the HLFS covering the period 1997-2010. They compared the average outcomes of 16-17-year-olds (i.e., the treatment group) against the average outcomes of the control group, defined as 20-21-year-olds before and after the 2008 minimum wage reform. The pre- and post-treatment periods were defined as 2006q1-2007q4 and 2008q2-2010q4, respectively. The outcomes of 18-19-year-olds were also included to ascertain whether any spillover effects could be observed, as these teenagers were not covered by the new entrant minimum wage.

An initial examination of changes in employment before and after the 2008 minimum wage reform saw a decrease in the percentages of employed 16-17-year-olds relative to 18-19 and 20-21-year-olds. When compared to the changes for 20-21-year-olds, the DID estimates indicated a decrease in the employment of 16-17-year-olds by 2.7 percentage points, significant at the 5% level (Hyslop & Stillman, 2021).

Following the initial estimation of average treatment effects on employment, Hyslop and Stillman (2021) presented the following regression specification to control for a range of other factors which may have influenced the outcomes of interest:

$$Y_{it} = \delta * (age16 - 17_{it} * Post - 2008) + X'_{itj}\beta + \varepsilon \quad (2)$$

where Y_{it} is the outcome of interest (i.e., employment dummy) for individual *i* at time *t*. On the right side of the equation, *age*16-17 is a treatment group dummy, with *Post*-2008 being a post-reform period dummy. X_{itj} is a vector of *j* relevant covariates for individual *i* and time *t*. These initially included individual age dummies, quarter dummies, and a pre-2003 dummy variable which was interacted with a 16-17-year-old age dummy.[41] As with their earlier paper, Hyslop and Stillman (2021) sequentially added additional control variables to specification (2), the descriptions of which are presented in Table 3.3, along with a summary of the estimation results.

The empirical results were generally consistent across specifications (2.1)-(2.4) where aggregate effects were examined, as well as specifications (2.5)-(2.8) where the effects were able to vary across individual post-reform years. Taking a closer look at specification (2.7), assumed to be the preferred specification,[42] the estimates on employment were negative for 16-17-year-olds in 2009 (δ_{16-17} = -0.037, $p < 0.05$) and 2010 (δ_{16-17} = -0.058, $p < 0.01$), suggesting that they were approximately 4 and 6 percentage points less likely to be employed in 2009 and 2010, respectively, following the introduction of the new entrant minimum wage, holding all other factors constant (Hyslop & Stillman, 2021). For the employment of 18-19-year-olds, immediate positive spillover effects were observed (δ_{18-19} = -0.037, $p < 0.05$).

As noted with Hyslop and Stillman (2007), a key assumption underpinning a DID analysis is the parallel trends assumptions. In this instance, Hyslop and Stillman (2021) did not detail whether they accepted parallel trends in the employment of 16-17 and 20-21-year-olds. Furthermore, no details of any examinations or checks used to validate the parallel trends assumption were reported.

[41] Hyslop and Stillman (2021) noted that this baseline regression effectively captured the change in outcomes for 16-17-year-olds after 2008 to their outcomes between 2003-2008, and to the same changes in outcomes for 18-19 and 20-21-year-olds. Extending the data back to 1997 enabled long-run age differences in employment to be estimated, without being contaminated by the effects from the 2001 minimum wage reform.

[42] Specification (2.7) was assumed to be the preferred specification as it was also used to examine other outcomes.

Table 3.3:

Definitions and Results of Regression Specifications Estimated by Hyslop and Stillman (2021)

Controls	Description	Specification (2.1)	(2.2)	(2.3)	(2.4)	(2.5)	(2.6)	(2.7)	(2.8)	(2.9)	Results
No covariates	Dummy variables for age and quarter. A pre-2003 dummy was interacted with a 16-17-year-old age dummy.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	16-17 = negative (significant at 1% level).
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	Yes	16-17 = negative (significant at 1% level).
Spillover effects	Interaction term between age group dummy (*age*18-19) and post-treatment period (*Post*-2008).			Yes	Yes	Yes	Yes	Yes	Yes	Yes	16-17 = negative (significant at 1% level); 18-19 = positive (insig.[a]).
Proxy responses	Dummy variable indicating a proxy response, plus interactions with each of the control variables.				Yes	Yes	Yes	Yes	Yes	Yes	16-17 = negative (significant at 1% level); 18-19 = positive (insig.).
Separate post-reform year impacts	Interaction term between the age groups dummies and the individual year dummies (2008, 2009 & 2010) to allow the effects to vary by post-reform year.					Yes	Yes	Yes	Yes	Yes	16-17 = mixed (significant negative in 2009 & 2010); 18-19 = mixed (weakly significant positive in 2008).
Announcement effects	Dummy for announcement effect which occurred in December 2007.						Yes	Yes	Yes	Yes	16-17 = mixed (significant negative in 2009 & 2010); 18-19 = mixed (weakly significant positive in 2008).
Age-specific seasonal effects	Interaction terms between quarter dummies and individual age dummies.							Yes	Yes	Yes	16-17 = mixed (significant negative in 2009 & 2010); 18-19 = mixed (weakly significant positive in 2008).
Age-specific business cycle effects (1)	Interaction terms between adult unemployment rate and individual age dummies.								Yes	Yes	16-17 = mixed (significant negative in 2009 & 2010); 18-19 = mixed (weakly significant positive in 2008).
Age-specific business cycle effects (2)	Age-specific linear trends to control for any age-specific secular (linear) trends in employment.									Yes	16-17 = mixed (weakly significant negative in 2010); 18-19 = mixed (significant positive in 2008).

Note. Details on regression specifications were taken from Hyslop and Stillman (2021). Author's compilation.

[a] insig. = not statistically significantly different from zero.

Hyslop and Stillman (2021) concluded their paper by stating that the 2008 minimum wage reform had no immediate impacts on the employment of 16-17-year-olds, whereas, negative impacts in the order of 4-6 percentage points in 2009 and 2010 were observed. Substitution in favour of 18-19-year-olds were identified as a potential explanation of these results. However, as noted by Hyslop and Stillman (2021), their regression results did not support such reasoning, as employment impacts for 18-19-year-olds occurred immediately following the introduction of the new entrant minimum wage, with negative effects for 16-17-year-olds taking place 1-2 years thereafter. Although these results provide evidence in favour of the negative effects on teenage employment, they have to be interpreted with some discretion given the absence of evidence validating the parallel trends assumption.

3.5.3 Pacheco (2011).

Previous empirical minimum wage studies in NZ examined effects on the employment of certain groups likely to be impacted (e.g., teenagers, unqualified females). However, as noted by Neumark and Wascher (2002), this approach can be flawed as it does not account for the probability that the minimum wage is binding to specific workers. To address this gap in the NZ minimum wage literature, Pacheco (2011) examined the impact of minimum wage increases on employment over the period 1986-2004, while concentrating the empirical analysis on workers directly affected by these increases. By imposing binding constraints, Pacheco (2011) was able to estimate employment impacts free from having an inflated denominator. This was valuable, as an inflated denominator may either result in underestimated impacts or imprecise estimates which are statistically insignificant not because there is no impact but because a portion of the group under examination were not affected (Belman & Wolfson, 2014).

Several specifications were presented by Pacheco (2011). This review will summarise the initial two specifications, and will then focus on the final specification where binding constraints were imposed on the employment regression.

Quarterly data from the HLFS over the period 1986-2004 were utilised, along with a sample consisting of 16-29-year-olds to estimate the following probit regression specification:

$$\Pr(E_{it}=1)=\delta+\sum_{k=16}^{29}\beta_{1k}A_{itk}T+\sum_{k=16}^{29}\beta_{2k}A_{itk}\ln rGDP_t+\sum_{j=1}^{46}\gamma_j X_{itj}+\sum_{k=0}^{4}\rho_k D_{i,t-k}+\sum_{k=0}^{4}\alpha_k D_{it}\ln RMW_{i,t-k}+\varepsilon_{it} \quad (3)$$

where the dependent variable is a dummy variable for employment status of individual *i* and time *t*. On the right side of the equation, variables included age-specific time trends ($A_{iyk}T$), a control for age-specific business cycle fluctuations ($A_{iyk}\ln rGDP_t$), controls for various demographic characteristics,[43] a dummy for capturing whether a minimum wage is applicable (D_{it}), which is then interacted with the minimum wage ($D_{it}\ln RMW_{i,t-k}$).

The regression estimates from specification (3) indicated negative and statistically significant impacts on the employment propensity for certain sub-groups within the sample, namely females, Māori and not being born in NZ. The one-off impact of the introduction of the youth minimum wage was estimated as positive for the employment propensity of 16-19-year-olds, increasing by 3.6 percentage points ($p < 0.01$). The regression results also indicated that a 10% increase in the minimum wage resulted in an immediate reduction in the probability of 16-29-year-olds being employed by 0.29 percentage points ($p < 0.01$). When considering the overall cumulative impact of the minimum wage, a 10% increase was estimated to increase the employment propensity of 16-29-year-olds by 0.32 percentage points ($p < 0.05$).

Next, Pacheco (2011) constructed binding constraints using data from the HLFS-IS over the period 1997q2-2004q2. Using these data with a sample consisting of 16-29-year-olds with valid wage information, Pacheco (2011) adopted the following probit regression specification to predict someone's probability of being affected by the minimum wage:

$$P_i = P(MW_i = 1 | X_i, \ln RMW_i, \ln RMW_i * Z_i, EMP_i = 1), \quad (4)$$

[43] These characteristics included: age dummies, gender, ethnicity, highest qualification, not being born in NZ, time spent in NZ, marital status, household structure (i.e., number of adults, number of children aged under 5, number of dependents aged between 5 and 18), region of residence, dummy legislated change in the minimum school leaving age, seasonal dummies and relative size of each age group in the working-age population (Pacheco, 2011).

where MW_i = 1 if the individual is deemed to be affected by increases in the minimum wage (i.e., classified as a minimum or sub-minimum wage worker). *Xi* is a vector which captures demographic characteristics of individual *i*, lnRMW_i is a measure of the minimum wage which was also interacted with a sub-set of variables (Z_i) under the vector *Xi*. This model returned β estimates for the employed HLFS-IS sample. These estimates were then utilised in the full sample of 16-29-year-olds over the years 1986-2004 to create a probability of the individual earning the minimum wage using the following specification:

$$\hat{P}(MW_{it} = 1) = \Phi(\hat{\beta}'X) \quad (5)$$

Specification (5) returned the probability from the standard cumulative normal distribution of the individual earning at or below the minimum wage (Pacheco, 2011).

The final specification updated specification (3) by adjusting the policy variables D_{it} and ln$RMW_{i,t-k}$ with the probability that individual *i* in time period *t* would find the minimum wage binding, giving $D_{i,t-k}\hat{P}_{it}$ and $\ln RMW_{i,t-k}\hat{P}_{it}$. When estimating the impacts of the introduction of the youth minimum wage accounting for binding constraints, the employment propensity of all 16-19-year-olds increased by 3.2 percentage points ($p < 0.01$). The regression results also indicated that for those aged 16-29, who found the minimum wage binding, a 10% increase in the minimum wage would result in an immediate reduction in the probability of being employed by 3.2 percentage points ($p < 0.01$). When examining the employment effect for all 16-29-year-olds (i.e., those who find the minimum wage binding and those who do not), a 10% increase in the minimum wage would decrease the employment propensity by 0.57 percentage points ($p < 0.01$).

When comparing the results from specification (5) to those from specification (3), the magnitude of the immediate reduction on employment propensity from higher minimum wages were larger. Furthermore, specification (3) estimated small positive impacts on the employment propensity of 16-29 year olds, whereas the results from specification (5) estimated small negative impacts. These results suggest that the binding constrains were an important addition to the NZ empirical minimum wage literature.

There are limitations to this study worth noting. First, in constructing binding constraints, Pacheco (2011) had to assume no sample selection bias, meaning "that non-workers and workers with the same X_i characteristics face the same probability of working for the minimum wage" (p. 597). However, as noted by Belman and Wolfson (2014), research in labour economics has demonstrated differences in the characteristics of those who are not employed when compared to those who are employed but are similar in other observable characteristics. Furthermore, Belman and Wolfson (2014) suggested that test results for sample selection bias would have been helpful in resolving this limitation. Given the assumption of no sample selection bias, the critique by Belman and Wolfson (2014) does raise some questions with respect the binding constraints used by Pacheco (2011), specifically with their construction.

Concerns with the identification of the probability measure P_i, and potential collinearity with other variables used in the final specification, were also raised by Belman and Wolfson (2014). The same demographic variables were used on both the minimum wage worker and employment specifications, and although Pacheco (2011) argued that the collinearity was minimised due to the method of constructing P_i, Belman and Wolfson (2014) held that since no variables from the wage equation, specification (4), were excluded from the employment specification with binding constraints, identification issues may be present.

Pacheco (2011) concluded this study by noting that the findings suggest negative and statistically significant effects on employment result from higher minimum wages when binding constraints are included. Furthermore, teenagers aged 16-19 appeared to be more adversely impacted when compared to young adults aged 20-29. Although this study made an important and original contribution to the NZ minimum wage literature, the limitations concerning identification suggest that the literature remained lacking in concrete empirical evidence on the effects of minimum wage increases on teenage employment.

3.5.4 Hyslop et al. (2012).

The first empirical examination on the effects of minimum wage increases on teenage employment using administrative data was undertaken by Hyslop et al. (2012). Using data from the Linked Employer-Employee Database (LEED), Hyslop et al. (2012) found firm-level changes in teenage employment from minimum wage increases over period April 1999-March 2007.

The sample was restricted to employees who had Pay-As-You-Earn tax deducted based on their records in the Employer Monthly Schedule (EMS) from the Inland Revenue (IRD).[44] LEED also provides Permanent Business Numbers (PBN), created by Stats NZ, which were adopted by Hyslop et al. (2012) as the definition of a firm. Teenagers were those aged 16-19.

Results from a preliminary descriptive analysis indicated the average teenage-employment share of 7.7% across all firms increased when comparing the averages of 1999-2000 and 2006/-2007, albeit by less than 1%. When observing teenage employment-share within the four main teenager-employing industries,[45] teenagers accounted for an average of 15.5% of total employment. These four industries also employed nearly 60% of all teenagers (Hyslop et al., 2012).

Employment changes for continuing firms with initially high teenage employment at the start of the sample period (defined as a teenage-employment share of 0.3 or greater) were also examined more formally.[46] The following OLS regression specification was estimated as the base model:

$$\Delta\lambda_i = \beta_0 + \beta_1 \cdot Hi_i + \beta_2 Main_i + \beta_3 Hi_i \cdot Main_i + u_i ; \quad i = 1, \dots, N. \qquad (6)$$

where $\Delta\lambda_i$ is the change in teenage-employment share between 2000-2001 and 2006-2007 for firm *i*, Hi_i is a dummy for whether the firm initially had a high share of teenage employment, $Main_i$ is a dummy for whether the firm was in one of the four main teenager-employing industries and $Hi_i \cdot Main_i$ is an interaction of these two variables (Hyslop et al., 2012). Extensions to the base model were made, the descriptions of which are presented in Table 3.4, along with a summary of the estimated changes in teenage-employment share.

[44] EMS data are explored in greater detail in Chapter 4. The sample excluded self-employed contractors due to the uncertain nature of selecting self-employed in the LEED (Hyslop et al., 2012).
[45] These industries were i) Retail Trade, ii) Accommodation, Cafes, and Restaurants, iii) Agriculture, Forestry and Fishing, and iv) Construction.
[46] Firm survival and entry effects were also examined but not included in this literature review.

Table 3.4:

Definitions and Results of Regression Specifications Estimated by Hyslop et al. (2012)

Extensions	Description	Δ teenage-employment share
Base model	Hi_i = dummy for whether teenage-employment share was 0.3 or greater in 2000/2001. $Main_i$ = dummy for whether industry was one of the four main teenager-employing industries.	Decrease of 20 percentage points in firms with initial high teenage-employment share. Approximate fall of 15 percentage points in firms with initial high teenage-employment share in one of the four main teenager-employing industries.
IV model	Hi_i = instrumented for first year analogues (i.e., whether teenage-employment share was 0.3 or greater in 1999/2000). $Main_i$ = as defined in base model.	Results nearly indistinguishable from base model.
Employment share of 20-24-year-olds	Hi_i = as defined in the IV model. $Main_i$ = as defined in the IV model. Control added for changes in the employment share of 20-24-year-olds.	Results nearly indistinguishable from IV model. Young adults appear to be substitutes for teenagers, with the young adult-employment share decreasing by approximately 11 percentage points.
Redefine high teenager-employment share (#1)	Hi_i = dummy for whether a firm's 2000/2001 teenage-employment share was 0.2 or greater. $Main_i$ = as defined in the IV model.	Approximate fall of 17 percentage points in firms with initial high teenage-employment share who are also in one of the four main teenager-employing industries.
Redefine high teenager-employment share (#2)	Hi_i = continuous 2000/2001 teenage-employment share variable. $Main_i$ = as defined in the IV model.	Decrease of approximately 36 percentage points in firms with initial high teenage-employment share. Approximate fall of 37 percentage points in firms with initial high teenage-employment share who are in main industries.

Note. Details on regression specifications were taken from Hyslop et al. (2012). Author's compilation.

The base specification, and the extensions estimates, revealed relatively consistent results. Overall, for firms which continued operations throughout the period 1999-2007, those with initial high teenage-employment shares reduced their teenage-employment shares relative to other firms (Hyslop et al., 2012). Interestingly, firms in the main teenager-employing industries actually increased their teenage-employment share between 2000-2001 and 2006-2007, relative to firms in other industries. However, when firms in the main teenager-employing industries were also classified as having initially high levels of teenage-employment shares, their teenage-employment share fell over the period examined.

Hyslop et al. (2012) concluded this study by noting that their empirical estimates, which showed that the cost of increasing teenage labour mainly impacts a sub-group of firms who employ high shares of teenagers, lend themselves well to the Hicks-Marshall production theory. This theory holds that demand for labour is more elastic when labour accounts for a larger share of costs (Hyslop et al., 2012). These empirical results may also explain why effects on employment examined across aggregate groups (e.g., teenagers, low-skilled) may be underestimated and why employing binding constraints, as in the case of Pacheco (2011), may be beneficial in some instances.

3.5.5 Maré and Hyslop (2021).

A recent addition to the NZ empirical literature by Maré and Hyslop (2021) presents a comprehensive descriptive analysis documenting the incidence of workers affected by minimum wage changes since the year 2000, as well as changes in wage distributions of selected age groups. The impact of minimum wage increases on employment were formally examined, along with an assessment of how effective minimum wages are as an income support policy. Although teenagers are not the primary target population, this study has been included in the literature review as teenagers feature in the empirical analyses. For the purpose of this literature review, a summary of the descriptive observations will be provided[47] followed by a close review of the estimation results with respect to the effects of minimum wage increases on employment.

Maré and Hyslop (2021) utilised annual survey data from the HLFS-IS covering the period 1997q2-2020q2 for the entire working-age population (aged 16 or above). These HLFS-IS data were matched with administrative tax-year data from the IRD to examine the incidence of minimum wages across individual and household income distributions. However, no administrative data were utilised in the empirical analyses examining the effects of minimum wage increases on employment.

Changes in population characteristics were first examined, and included changes to the incidence of workers affected by the minimum wage in the years 1997, 2008 and 2020. The descriptive statistics indicated a 5 percentage-point increase in the proportion of workers affected by the minimum wage (defined as employees paid wages at or below the minimum wage rate) between 1997 and 2020. Age group breakdowns also highlighted interesting trends. In 2008, 16-17-year-olds accounted for 3% of all employees, 22% of all minimum wage earners, with close to two-thirds (64%) of all 16-17-year-old workers being affected by the minimum wage (Maré & Hyslop, 2021). By 2020, 16-17-year-olds accounted for a smaller proportion of all employees (2%) and minimum wage earners (10%), with approximately 16% fewer 16-17-year-old workers being affected by the minimum wage (Maré & Hyslop, 2021). The comparable descriptive statistics for 18-19-year-olds were more consistent between 2008 and 2020, with these workers accounting for 4% and 3% of all employees, 13% and 14% of all minimum wage earners, and 36%

[47] The descriptive analyses were considered valuable to this literature review, as similar analyses feature in Chapter 4 and Chapter 6.

and 38% of all 18-19-year-olds being affected by the minimum wage, respectively (Maré & Hyslop, 2021).

Changes in wage distributions where then examined, categorised by age groups over three periods: i) 1997-2000, ii) 2008-2011, and iii) 2018-2020. For 16-17-year-olds, Maré and Hyslop (2021) observed that in later periods, wage distributions peaked around the prevailing minimum wage. In the period 1997-2000, only 4% of 16-17-year-olds were paid a wage at or below the youth minimum wage (Maré & Hyslop, 2021). By 2008-2011, the wage distribution for 16-17-year-olds peaked around the prevailing minimum wage, with 67% being paid a wage at or below the new entrant minimum wage (Maré & Hyslop, 2021). A similar wage distribution was observed for the period 2018-2020, where 57% of 16-17-year-olds received a wage at or below the prevailing minimum wage (Maré & Hyslop, 2021). For 18-19-year-olds, similar patterns were observed. In 2008-2011, 36% of 18-19-year-olds were paid wages at or below the prevailing minimum wage, increasing to 43% in 2018-2020 (Maré & Hyslop, 2021). These descriptive patterns suggest that compared to 1997-2000, minimum wage increases affected a rising proportion of teenage workers.

Using HLFS-IS data covering the period 1997q2-2020q2, Maré and Hyslop (2021) took a two-step approach in formally estimating the effects of minimum wage increases on employment. First, the authors examined an aggregate time series regression utilised by MBIE in their annual minimum wage review. Next, Maré and Hyslop (2021) trialled an alternative measure of the minimum wage using the same regression specification. The regression results from step one will be briefly summarised, followed by a close examination of the results from step two.

In examining the aggregate time series regression approach analogous to the one utilised by MBIE, Maré and Hyslop (2021) adopted the following base specification:

$$log(Y_t) = \alpha + \sum_{i=1}^{4} \beta_i log(Y_{t-i}) + \sum_{j=0}^{4} \gamma_j log\left(Kaitz_{t-j}\right) + \sum_{k=0}^{4} \theta_k log(D_{t-k}) + vt + \rho X_t + u_t \quad (7)$$

where Y is the labour market outcome of interest, *Kaitz* is the minimum wage index (defined as the ratio of the minimum wage to average wage), D controls for business cycle variations, X is a vector of other control variables (including age, gender, ethnicity and qualifications), t is a linear time trend for the employment rate of those aged 25 or above, and u is a random error.

Several iterations of specification (7) were estimated, including using an aggregate-level Kaitz index which assumed constant effects, an age-group-level Kaitz index to explore heterogeneous effects on employment, and a population sub-group specific Kaitz index across 18 mutually exclusive groups defined by Maré and Hyslop (2021). In the final specification using the sub-group specific Kaitz index interacted with main group indicators,[48] the estimates showed negative effects on the employment of 25-64-year-old European men with post-school level qualifications (elasticity = -0.38, $p < 0.01$). Employment of teens and young adults were found to be negatively affected by higher minimum wages, while the employment of those with low-qualifications were shown to be positively impacted (Maré & Hyslop, 2021).

Several explanations were noted to rationalise the somewhat surprising regression results. These included: i) there is no robust relationship between minimum wage levels and employment, or ii) the Kaitz index is too coarse to identify effects on employment across different population groups (Maré & Hyslop, 2021).

To assess the latter issue, Maré and Hyslop (2021) re-estimated the specifications using an alternative measure of the minimum wage. Here, the Kaitz index was replaced with a MW-bite measure, defined as "the fraction of workers last year who earned this year's minimum wage" (Maré & Hyslop, 2021, p. 34). All specifications, along with their descriptions and a summary of their results are presented in Table 3.5.

Specification (1) acted as the base specification using an aggregate level minimum wage-bite variable. This specification revealed no statistically significant effects of minimum wage increases on employment.

[48] Main groups were defined as teens, aged 20-24, aged 65 or above, females, those with low-qualifications, and non-Europeans (Maré & Hyslop, 2021).

Table 3.5:

Regressions, Descriptions and Results from Maré and Hyslop (2021)

Specification	Minimum wage-bite	Description	Estimated employment elasticity
Base	Aggregate	Controlled for age (using age-group dummies), gender, ethnicity, qualifications, linear time trends and a business cycle control variable (GDP growth).	No statistically significant effects on employment.
Non-linear (squared) bite effect	Aggregate	Allowed for a non-linear bite effect by including a squared-term of the minimum wage-bite.	Positive and statistically significant when the minimum wage-bite was lower than 0.1.
Age-group specific effects	Aggregate	Allowed for age-group specific minimum wage effects, by interacting the linear minimum wage-bite with age group dummies (aged 16-17, aged 18-19, aged 20-24, aged 25-64, aged 65 or above).	Positive and statistically significant effects for 25-64 year olds. Negative and statistically significant effects for 16-17-year-olds.
Non-linear (quadratic) bite effect with business cycle effects	Age-group	Allowed for a non-linear bite effect by including a quadratic-term of the minimum wage-bite. Also allowed the effect to vary over the business cycle, by interacting the minimum wage-bite with GDP growth. Replaced the aggregate minimum wage bite variable with an age-specific minimum wage bite variable.	Convex employment minimum wage-bite relationship observed. Positive and statistically significant when the minimum wage-bite was lower than 0.25. Negative and statistically significant when the minimum wage-bite was greater than 0.25.
Non-linear (quadratic) bite effect	Age-group	Comparable to preceding specification though excluding the interactions between the minimum wage-bite and GDP growth.	Consistent with the preceding results.
Non-linear (quadratic) bite effect with business cycle effects	Population sub-groups	Allowed for a non-linear bite effect by including a quadratic-term of the minimum wage-bite. Also allowed the effect to vary over the business cycle, by interacting the minimum wage-bite with GDP growth. Replaced the aggregate minimum wage bite variable with a sub-group specific minimum wage bite variable.	Convex employment minimum wage-bite relationship observed. Positive and statistically significant when the minimum wage-bite was lower than 0.33. Negative and statistically significant when the minimum wage-bite was greater than 0.33.
Main group effects	Population sub-groups	Allowed the effects to vary by main groups by interacting the minimum wage-bite with main groups (teens, aged 20-24, aged 65 or above, females, those with low-qualifications, non-Europeans).	Negative and statistically significant effect for 25-64-year-old European men with post-school qualifications. Negative and statistically significant effects across all age groups (teens, 20-24, age 65 or above). Positive and statistically significant effects for those with low qualifications.

Note. All results controlled for age (16-17, 18-19, 20-24, 25-64, 65+), gender, ethnicity (European, Māori, European-Māori, Pasifika, and Other), and qualifications (none, school, post-school, and degree qualifications), included linear time trends for the employment rate of those aged 25 and over, and also included GDP growth as a control variable for business cycle variation. Details on regression specifications were taken from Maré and Hyslop (2021).Author's compilation.

Specification (2) tested for non-linear effects between employment and the minimum wage-bite variable. Here, a squared term of the aggregate level minimum wage-bite variable was included. A non-linear relationship between employment and the minimum wage-bite variable was observed, with effects on employment being positive if the minimum wage bite was less than 0.1 (elasticity = 0.99, $p < 0.01$), and negative at higher values (Maré & Hyslop, 2021).

In specification (3), Maré and Hyslop (2021) interacted the age-group indicators with the aggregate linear minimum wage-bite variable. This specification estimated positive, but weakly significant, effects for 25-64-year-olds (elasticity = 0.19, $p < 0.10$), and negative effects for 16-17-year-olds (elasticity = -0.85, $p < 0.01$).

Specification (4) replaced the aggregate minimum wage-bite variable with an age-specific minimum wage-bite variable and allowed for a quadratic specification. This specification also allowed for the effect to vary over the business cycle by interacting the minimum wage-bite variable with GDP growth. The results highlighted a convex employment minimum wage-bite relationship. Where the minimum wage-bite was less than 0.25, the effects on employment were positive, and negative if higher (elasticity = 0.25, $p < 0.01$) (Maré & Hyslop, 2021).

Specification (5) repeated specification (4), though dropped the interaction between the minimum wage-bite variable and GDP growth. The results were consistent with those from the previous specification, with the effects on employment being positive where the bite was less than 0.28, and negative if higher (elasticity = 0.27, $p < 0.01$) (Maré & Hyslop, 2021).

Specification (6) replaced the aggregate minimum wage-bite variable with a population sub-group specific minimum wage-bite variable. The remainder of the specification was equivalent to specification (4). Here, the results also highlighted a convex employment minimum wage-bite relationship. Where the minimum wage-bite was less than 0.33, the effects on employment were positive, and negative if higher (elasticity = 0.33, $p < 0.01$) (Maré & Hyslop, 2021).

The final specification dropped the quadratic terms and allowed the effects to vary by interacting the minimum wage-bite with main groups. The main effect indicated that 10% increase in the Kaitz index was associated with a reduction in the employment of 25-64-year-old European men with post-school level qualifications, of 3.8% ($p < 0.01$) (Maré & Hyslop, 2021). Negative and statistically significant effects on employment were observed for all age groups (16-19, 20-24, 65 or above). An increase in the minimum wage-bite variable showed positive effects on the employment of those with low qualifications. These results were similar to those estimated using the sub-group specific Kaitz index, indicating that the minimum wage-bite variable made little difference to the somewhat surprising estimated effects on employment.

Further to the regression analyses, Maré and Hyslop (2021) undertook counterfactual exercises to demonstrate the nature and the magnitude implied by the regression results. The first counterfactual exercise utilised the Kaitz-based regression estimates to show the implied change in employment following an increase in the minimum wage from its actual level in a year, to the following year's level. The changes in employment were shown for the years 2018, 2019 and 2020. The second counterfactual exercise followed the same approach as the first, though it utilised the population sub-group specific minimum wage-bite variable.

As with the regression results, the counterfactual exercises revealed surprising results. For example, when examining employment change by qualification level, the net effect was shown to be near zero. However, this result was driven by the employment loss of individuals with post-school or degree level qualifications which was offset by the employment gain of those with no or school level qualifications. Maré and Hyslop (2021) noted that the fragility of the regression estimates would likely explain the variability of the results of the counterfactual exercises.

Maré and Hyslop (2021) provided comprehensive descriptive findings with respect to changes in the proportions of workers affected by minimum wages and wage distributions. However, their formal estimation of the effects of minimum wage increases on employment were non-robust across specifications and were surprising in terms of sub-groups adversely impacted by minimum wage increases, thus providing limited evidence to clarify the association between employment and the NZ minimum wage.

In addition to the reasons noted by Maré and Hyslop (2021) when explaining their surprising regressions results, another potential explanation rests in their choice of data. Specifically, with data from the HLFS-IS only being available annually, there may not have been enough variation in employment from which to estimate more robust effects. Within a 12-month period, there is plenty of variation in employment, especially for teenagers over summer holiday periods, which may get lost using an annual measure. This highlights an opportunity where administrative data could be of value. With IRD EMS data at a monthly-level, greater variation in employment levels could be exploited in empirical analyses. Nonetheless, Maré and Hyslop's (2021) choice of HLFS-IS data is understandable given the need for hourly wage information to construct the minimum wage-bite variable, which was not available in administrative data over the majority of the sample period defined by Maré and Hyslop (2021).

3.6 Conclusion

This literature review had several objectives. The first was to present several theoretical models of the labour market to help inform how employment may be affected by minimum wage increases. The next objective was to summarise a brief history of US empirical literature to identify whether a consensus exists. The final two objectives were focussed on post-2000 NZ empirical literature. These objectives set out to: i) determine whether a consensus view of the effects of minimum wage increases on teenage employment exists in NZ, and ii) identify opportunities where the use of administrative data could extend the NZ empirical literature from both a data and methodological perspective.

The theoretical literature review presented several theoretical labour market models to help inform how employment may be affected by minimum wage increases. The PCLM predicts minimum wage increases negatively affect employment due to the surplus of labour. Notwithstanding the differences in structures of the static and dynamic monopsony, these models indicate that a range of employment outcomes are possible depending on the level of the minimum wage in relation to the equilibrium wage, in the case of static monopsony, or the flow of workers between firms influenced by wage offers, in the case of a dynamic monopsony. Although each model has clear benefits and limitations, empirical evidence in support of all three models suggests that perhaps their predictions are context specific, and thus neither can be discounted.

A brief summary of the US empirical minimum wage literature revealed that although the consensus set by Brown et al. (1982) held that minimum wage increases negatively affect teenage employment, a series of empirical findings in the 1990s offered an alternative standpoint (Card, 1992a, 1992b; Card & Krueger, 1994; Katz & Krueger, 1992). In alignment with the PCLM predictions, Neumark and Wascher (1992) presented evidence of negative effects on the employment of teenagers and young adults, which led to an initial exchange between Card et al. (1994) and Neumark and Wascher (1994). A second exchange took place between these authors initiated by the empirical findings presented by Card and Krueger (1994), with data-related issues playing a pivotal role in the exchanges. Issues such as measurement error in self-collected survey data (Neumark & Wascher, 2000) and non-representative administrative samples stemming from sampling and data collection methods (Card & Krueger, 2000) were highlighted. Although this

exchange did not settle the debate on the effects of minimum wage increases on employment, it revived the interest in this field of empirical enquiry.

Reviewing a small selection of early NZ empirical literature yielded no clear consensus with respect to the effects of minimum wage increases on teenage employment. Although the early NZ literature observed disemployment effects to some extent, the empirical evidence was mixed, potentially due to short time-series, non-robust findings from various specifications and narrowly defined sample populations.

The first objective with respect to the post-2000 NZ empirical literature was to determine whether a consensus view of minimum wage impacts on teenage labour market outcomes have been reached. Based on the mixed empirical findings from the studies reviewed, there appears to be no definitive consensus. For example, the 2001 minimum wage reform was shown to have no immediate effects on the employment of 16-17 and 18-19-year-olds, but lagged negative effects for 16-17-year-olds by 2003 (Hyslop & Stillman, 2007). The introduction of the new entrant minimum wage had no immediate impacts on the employment of 16-17-year-olds, but decreased their employment in 2009 and 2010, while resulting in positive spillover effects on the employment of 18-19-year-olds (Hyslop & Stillman, 2021). Pacheco (2011) reported positive effects on the employment of teenagers from the introduction of the youth minimum wage in 1994, but negative cumulative effects from minimum wage increases. Finally, Maré and Hyslop (2021) reported negative effects on the employment 16-17-year-olds when using both measures of the minimum wage. However, their empirical results were non-robust due to surprising results showing positive effects for those with low qualifications and negative effects for European men with post-school qualifications (Maré & Hyslop, 2021).

The second objective with respect to the post-2000 NZ empirical literature was to identify opportunities where the use of administrative data could extend the current literature from both a data and methodological perspective. The following opportunities were noted. First, individual-level administrative data has not been utilised to empirically examine the effects minimum wage increases on teenage employment in NZ. The availability of these data, therefore, provides researchers with the opportunity to evaluate the benefit of administrative data by comparing measures such as employment derived from administrative data against those from survey data. Furthermore, having a largely untapped source of data also presents new opportunities for empirical endeavours, such

as reproducing survey-based evidence using administrative data, or adopting alternative identification strategies which would be challenging in the context survey data.

Chapter 4 Data Sources, Sample Construction and Wage Group Characteristics

This thesis aims to answer the following research question: What are the effects of the 2001 minimum wage reform on teenage employment? This chapter contributes to answering that question by introducing individual-level administrative data to the NZ minimum wage research setting. Data will cover the period 1997-2003 (survey) and 1999-2003 (administrative) to provide an adequate observation window to empirically assess the effects of minimum wage increases on teenage employment; more details on sample population and periods follow in section 4.3.

This chapter aims to understand:

- understand the benefits and limitations of administrative and survey data in relation to constructing a suitable sample for empirically examining the effects of minimum wage increases on teenage employment;
- understand the benefits and limitations of administrative and survey measures related to earnings and employment; and
- understand the boundaries of administrative data relative to survey data in terms of the types of empirical analyses that can be undertaken given any identified limitations.

This chapter is organised as follows: Data sources are described in section 4.1 (survey) and 4.2 (administrative) which feature throughout this thesis. Sample construction procedures are then detailed in section 4.3, before comparing survey and administrative measures of employment in section 4.4, weekly earnings in section 4.5 and hourly earnings in section 4.6. The characteristics of sub-minimum and minimum wage workers are examined in section 4.7, with the chapter concluding in section 4.8.

4.1 Survey data

4.1.1 The Household Labour Force Survey.

The HLFS is an ongoing quarterly survey administered by Stats NZ. It provides a comprehensive portrait of the NZ labour market dating back to 1986. One of the primary objectives of the HLFS is to measure the changes and characteristics of employment, unemployment and people not participating in the NZ labour force (Stats NZ, 2015a).

Data for the HLFS are collected by surveying a representative sample of NZ. This sample consists of approximately 15,000 households and 30,000 individuals each quarter (Stats NZ, 2015a). The target population for the HLFS is the working-age population of NZ, which is defined as "the non-institutionalised population 15 years and over, who usually live in New Zealand" (Stats NZ, 2015a, p. 11). The HLFS is a rotating panel survey, meaning that households are selected and contacted for a maximum of eight consecutive quarters before being replaced by new households.[49]

The HLFS collects a range of employment data pertaining to labour force status and hours worked, as well as information regarding the demographic, education, household and regional characteristics of survey participants.[50] Labour force status was used to construct an employment indicator which featured as the key measure throughout this thesis. The employment indicator represented an individual's employment state in each quarter of the relevant survey sample. This employment indicator took the value of '1' if the individual's labour force status was recorded as 'employed', and '0' if labour force status was recorded as 'unemployed' or 'not in the labour force'.

Although critical to the HLFS, labour force status presents a limitation. Specifically, Stats NZ has adopted a relatively broad definition of 'employed'. In essence, as long as a person is within the working-age population (i.e., aged 15-64) and has worked for at least one hour during the reference week,[51] they are classified as employed (Stats NZ, 2015a). In this context, work is defined as: i) working for pay as an employee, ii) working without pay in an operation owned by a relative or family member, or iii) being self-employed and working for profit. This broad definition of employment risks overestimating employment levels when using labour force status from the HLFS to measure employment.

Although there are several alternative survey data sources which provide household and labour market information, these sources present limitations which makes the HFLS preferrable for use in this thesis. The alternatives include the Census of Population and Dwellings,[52] the Household Economic Survey (HES) and the Survey of Family, Income and Employment (SoFIE). The benefit of the Census is that it collects full population-level data, however it is only administered every five years which makes it unsuitable for

[49] This means that every quarter, one-eighth of the households are rotated out of the survey (Stats NZ, 2015a).
[50] Demographic characteristics include information pertaining to gender, ethnicity and marital status. Information on household composition is collected, as are regional characteristics for region of residence (based on 12 local government regions from 1994).
[51] Definition of employed from Stats NZ (2015a).
[52] Referred to as 'Census' from here on.

use in this thesis. The HES collects information on incomes, expenditure and savings, along with personal and household demographic characteristics.[53] A key limitation of the HES is its small sample size, which was limited to 5,500 households up until June 2019, when it increased to 28,500 (Stats NZ, n.d.b). The SoFIE was an eight-wave annual household panel survey which ran from 2002-2010 and collected labour market information on approximately 22,200 adults in 11,500 households (Stats NZ, 2011). The key limitation of the SoFIE data when compared to the HLFS is the coverage period which excludes the 2001 minimum wage reform. Consequently, compared to the alternative sources of household and labour market information, the HLFS provides the most appropriate data in terms of sample size and frequency of data collection.

4.1.2 The Income Survey Supplement.

To address the absence of income and earnings information in the HLFS, Stats NZ introduced the HLFS-IS in June 1997. The HLFS-IS is administered annually in the June quarter and collects comprehensive information on household and personal earnings and incomes, including earnings from wages and salary employment and incomes from self-employment, government transfers and investments.

Information on weekly earnings from wage and salary employment, as well as weekly hours of work is of key interest to this thesis and were used to measure an individual's total weekly and hourly earnings from employment.[54] Within the context of this thesis, weekly earnings refer to the total actual weekly earnings in the main job. Similarly, hours of work refer to the total actual weekly hours worked in the main job. In both cases, these variables are derived by Stats NZ.

Actual reported earnings and hours, as opposed to usual reported earnings and hours (also available from the HLFS-IS), were selected for the upcoming empirical analysis for the following reasons. First, actual earnings and hours refer to the earnings the survey respondent reported as having received in a particular reference week and therefore reflects an objective value of earnings and hours, unlike their 'usual' earnings and hours which is open for interpretation (Stewart & Swaffield, 2002). Next, information on actual earnings permits comparison to the earnings information from the EMS, where employers record the actual gross earnings payable to an employee. To examine whether any implication should arise when using actual earnings and hours as opposed to usual

[53] The HES (income) is administered annually, with the HES (expenditure) and HES (savings) alternating in the two years in between the standalone HES (Income) year (Stats NZ, n.d.b).
[54] Definitions of total weekly and hourly earnings are provided in section 4.5 and 0, respectively.

earnings and hours, descriptive analyses were conducted. The first of these analyses plotted the means of these variables over the period 1999q2-2003q2 by age. In both cases, only negligible differences between actual and usual earnings and hours were observed.[55] Paired t-tests confirmed that the differences in the means of usual and actual hours worked per week were generally not statistically different from zero.[56]

One of the key limitations of surveys such as the HLFS-IS is the self-reported nature of the income data. Stats NZ relies on all respondents to accurately report income information. There are risks associated with survey respondents making errors due to several cognitive factors relating to income reporting (i.e., misunderstanding income concepts and terms, retrieval problems and sensitivity issues) or factors surrounding confidentiality of personal income information (Giusti & Little, 2011; Groves, 2001; Moore, Stinson, & Welniak, 2000). These risk factors could result in response error, defined as the "discrepancies between the objective truth concerning a respondent's income and his or her report about that income" (Moore et al., 2000, p. 335). Examples of response error include confusing before-tax and after-tax incomes, miscalculating weekly income from annual salaries or forgetting incomes from irregular sources (e.g., bonus payments). Consequently, these response errors may result in individual-level measurement error of reported incomes, earnings and/or hours, which may influence empirical results estimated with these data.

Although there are several alternative survey data sources which provide income and earnings information, these sources present limitations which makes the HFLS-IS preferrable for use in this thesis. The alternatives include the Census, the Labour Cost Index (LCI) which captures job-level earnings information, and the Quarterly Employment Survey (QES) which captures business-level information. The benefit of the Census is that it collects full population-level data, however it is only administered every five years which makes it unsuitable for use in this thesis. Both the LCI and QES are limited with respect to their samples. In the LCI, data are collected from a relatively small sample of 2,000 businesses (Stats NZ, 2020a), whereas the QES restricts its sample to businesses that are "economically significant in surveyed industries" [57] (Stats NZ, 2020b, p. 3). Consequently, compared to the alternative survey sources of earnings data, the

[55] See Appendix B, Figure B1 for the visual comparison of the differences between these measures.
[56] See Appendix B, Table B1 for the results from the paired t-tests on the differences in means between these measures.
[57] Criteria including turnover, number of employees and industry classification determine 'economically significant' status. The QES also does not collect data from businesses in the Agriculture and Fisheries industry, which is an industry with proportionately more minimum wage workers than other workers (Pacheco, 2009; Timmins, 2006).

HLFS-IS provides a more representative view of household and personal income levels in NZ. This is advantageous, as it enables comparisons across different demographic or income groups to be examined (Stats NZ, 2013b).

4.2 Administrative Data

4.2.1 Overview of the Integrated Data Infrastructure.

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 using a unique identifier (UID) constructed by Stats NZ, 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).

In December 2011, Stats NZ launched a prototype of the IDI to allow linkage of individual and business level data sets, and included information on education, labour market, benefit systems and movements in and out of NZ (Black, 2016). From 2013, the IDI was extended to include a broader range of data, including from the justice and social sectors (Black, 2016). At present, the IDI covers life events in education and training, benefits and social services, income and work, migration, health and population data (Stats NZ, 2018).

The IDI is made up of a central 'spine' and a series of 'nodes'. The target population of the spine are individuals who are, or have been, residents of NZ (Gibb, Bycroft, & Matheson-Dunning, 2016). Several data sources make up the spine, including information on births registered in NZ, all IRD numbers, and all visas granted to migrants[58] (Gibb et al., 2016). The combination of the three data sources ensure extensive coverage of the target population (Black, 2016).[59]

As data are collected for the IDI, several cleaning, validation and encryption processes are followed, before individual unique identifiers are created and data are linked. The data linking methodology follows either a deterministic or probabilistic linking approach, depending on what variables are available within each node.[60] Data within the IDI are

[58] Excludes visitor and transit visa holders.
[59] The IDI target population is defined as a resident population, which includes people born in NZ, permanent residents, people with resident/work/study visas, and those who live and work in NZ without requiring a formal visa (Black, 2016).
[60] Deterministic linking is conducted using common unique identifiers on both data sets, whereas probabilistic linking is conducted using key variables available within each data set such as name, gender or date of birth (Stats NZ, 2013a).

presented in a confidentialised form in accordance with the security and confidentially provisions of the Statistics Act 1975. Figure 4.1 presents an overview of the IDI.

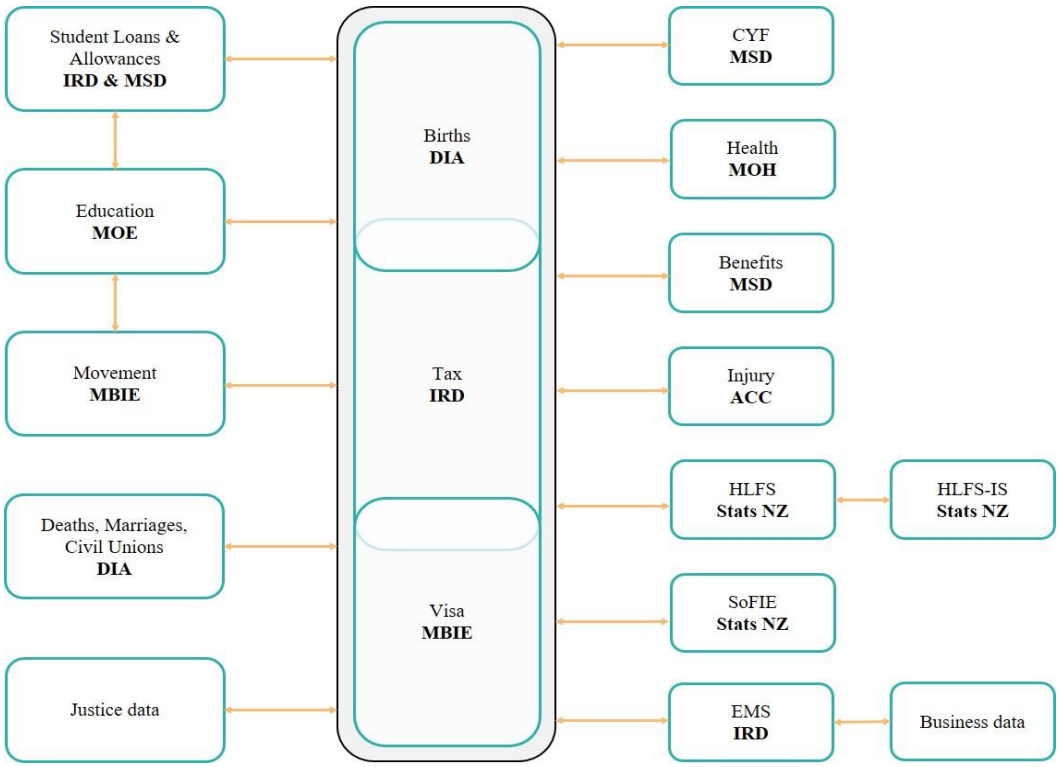

Figure 4.1: Structure of the IDI-spine with individual-level data since May 2015.
Adapted from Gibb et al. (2016). ACC: Accident Compensation Corporation; CYF: Child, Youth, and Family; DIA: Department of Internal Affairs; IRD: Inland Revenue; MBIE: Ministry of Business, Innovation & Enterprise; MOE: Ministry of Education; MOH: Ministry of Health; MSD: Ministry of Social Development; SoFIE: Survey of Family, Income, and Employment. Author's compilation.

4.2.1 Core IDI data: Personal details.

This thesis utilised personal details data which provides a 'best-guess' estimate of an individual's gender, month/year of birth and ethnicity. Stats NZ applies specific business rules across multiple data collections in the IDI to derive these personal details (Stats NZ, 2016). These data have the benefit of providing a single point of reference for key demographic characteristics. That is, researchers are not required to resolve any observable discrepancies in personal details across conflicting data sources.[61]

Personal details information was used in several aspects of this thesis. First, using the year of birth information, a sample population of 16-25-year-olds was constructed which features in multiple forthcoming empirical analyses. Moreover, the demographic

[61] These data are available to all IDI users with approved research projects (Stats NZ, 2016).

characteristics were used to examine sample characteristics and as control variables in regression specifications.

4.2.2 Geographic data: Addresses and border movements.

Information on addresses are collected from multiple data sources in the IDI to provide prioritised address histories for all UIDs where such information exists (Stats NZ, 2016). A set of business rules are utilised to limit the full address notification table to a 'best-guess' list of residential addresses (Stats NZ, 2016). Along with these addresses, information on meshblock, territorial authority and regional council are provided.

Address notification information was used to construct indicators describing which local government region individuals resided in during a reference period. These indicators were then utilised in describing sample characteristics and as control variables in empirical analyses.

Person overseas spell information is a summary of all border movements of individuals from the MBIE's immigration data since September 1997 (Stats NZ, 2016). The target population of the person overseas spell data are overseas visitors, NZ resident travellers and permanent and long-term migrants entering or leaving NZ (Stats NZ, 2015d).[62] These border movements were used to identify if, and for how long, individuals entered and/or departed from NZ. This was critical as these data enable NZ residents to be identified and retained when constructing the administrative sample.[63]

4.2.3 Income data: IRD EMS tax.

Administrative information on individual-level employment and income is available from the IRD EMS which employers are required to submit on a monthly basis. Coverage of the EMS data extends to all individuals who have received income where tax has been deducted at source (Black, 2016),[64] and covers the period April 1999-April 2019 (Stats NZ, 2019).[65]

There are several benefits to the EMS data. First, the EMS data have good coverage of the IDI target population formally engaged in the labour market. This makes the EMS particularly well suited to empirical analyses focussing on employment and income

[62] The target population excludes airline crew, military personnel and Antarctic personnel (Stats NZ, 2015d).
[63] See section 4.3.2.
[64] IRD data also include those who have joined KiwiSaver, applied for a student loan or child support, or applied for a Working for Families Tax Credit (Black, 2016).
[65] Administrative information on firm-level data linked with UIDs is available through the LEED. See Appendix B, Figure B2 for a diagram of the full IDI including LEED data.

related outcomes. Next, the EMS data are recorded at a monthly level. Therefore, changes in employment and income can be measured at a more frequent interval when compared to survey data (e.g., monthly versus quarterly for employment data & annually for income data). Furthermore, the EMS data are recorded for the purpose of income tax payments. With this purpose in mind, one could argue that earnings information from the EMS data are more objective, reliable and thus credible, when compared to self-reported incomes found in survey data, as legal penalties may be imposed where incomes are mis-reported.[66]

There are also some limitations to acknowledge. First, the EMS has relatively low coverage of young people (Black, 2016). This is understandable as young people, particularly teenagers, do not participate in the formal labour market at the same rate as adults.[67] Furthermore, some earnings may be classified as 'under the table' payments for informal labour activities (e.g., babysitting, yard work, dog walking) and would not be recorded in the EMS. Although this limitation is of little concern to this study with respect to minimum wage coverage (i.e., legislation only covers formal labour activities), there may be some implications for sample construction.[68]

Next, the coverage period of the EMS data (starting in April 1999) imposes some constraints. Particularly, with the impact of the 2001 minimum wage reform being the primary focus of this thesis, measuring variations in employment over a longer time period prior to the policy change would have been preferable. Although there is no manner in which the start of the coverage period could be extended, the EMS data are still of value as they provide a larger number of data points across a comparable sample period when compared to the HLFS. For example, between April 1999-March 2001, there are 23 periods of EMS data available, compared to 8 periods of employment information from the HLFS, and only 2 periods of earnings information from the HLFS-IS.

The final limitation of the EMS data, and perhaps the IDI in general, is the absence of hours of work information. Such information is key in establishing whether an individual's hourly earnings are at or above the relevant statutory minimum wage, as well as using hours of work as an alternative outcome to employment when empirically examining the effects of minimum wage increases on employment. Although work by

[66] When allowing for classical measurement error, Hyslop and Townsend (2020) found that earnings data reported in the EMS are more reliable when compared to survey reported incomes from the SoFIE, with reliability ratios of 90% and 85%, respectively.
[67] Based on the HLFS data over the period 1999q2-2003q3: 53% of 16-17-year-olds, 66% of 18-19-year-olds, and 73% of 20-25-year-olds participated in the labour market.
[68] See section 4.3.2 for a comprehensive overview of the construction of the administrative sample.

Fabling and Maré (2015) proposed a method to address the absence of hours in the IDI, several assumptions underpinning their method appear questionable, particularly in this research context.[69] For example, Fabling and Maré (2015) assumed that the statutory minimum wage is fully observed. However, Pacheco (2007, 2009) provides empirical evidence to the contrary. Furthermore, based on information from the MBIE's Labour Inspectorate, over the period July 2015-June 2018, on average, 15% of all confirmed breaches related to minimum wages.[70]

Although not of relevance for the time period under study in this thesis, but it is worth noting that from April 2019, a new method of collecting employment and earnings information was introduced by the IRD and required all employers to file payroll information every payday. The benefit of payday filing was to introduce hours worked information to the IDI (Stats NZ, 2019).[71]

The EMS data were used to construct an employment dummy, the key outcome variable throughout this thesis. The employment dummy represented an individual's employment state in each month of the administrative sample. This employment indicator took the value of '1' if the individual's income was sourced from wages or salary payments, and '0' otherwise.

Wage and salary earnings data were also utilised to examine the impact of minimum wage increases on monthly earnings in Chapter 6. In instances where multiple wage and salary records were present within the same month, the highest gross earnings sourced from wage or salary payments were selected and assumed to have been obtained from the individual's main job.[72]

To compare survey and administrative measures of weekly and hourly earnings, wage and salary data were also utilised and supplemented with hours of work data from the HLFS-IS. For a comparable measure of weekly earnings, monthly earnings were divided by the number of weeks in each corresponding month.[73] These comparisons are presented in sections 4.5-4.6.

[69] Their model suggests a method for deriving an approximation of individuals' monthly labour input (Fabling & Maré, 2015).
[70] Minimum wage breaches, as a percentage of all breaches, are provided in Appendix B, Table B2.
[71] The IRD gave employers 12 months to transition to a payday filing system (C. Ko, Stats NZ, personal communication, November 19, 2020).
[72] This assumption was also confirmed when examining the differences in actual gross weekly earnings from main, secondary and other jobs using the HLFS-IS. Across age groups, the actual gross weekly earnings from the main job constituted approximately 98%, on average, of the total actual weekly earnings from all paid employment.
[73] Using the start and end dates of each month, the number of weeks in a month were calculated as follows: $\frac{number\ of\ days\ in\ month}{7}$

4.2.4 Population data: Life events.

Several sets of life events data from the Department of Internal Affairs (DIA) were utilised in this thesis, including information on deaths, births and marriages. Coverage of all these life events date back to the 19th century and extends to all individuals who have had one of these events registered in NZ (Stats NZ, 2015e).[74]

There were several benefits of utilising these life events data. First, information on registered deaths were critical for constructing an administrative sample which excluded individuals who were deceased. Next, life events data relating to births and marriages enabled demographic control variables to be constructed and used as part of descriptive and empirical analyses.

4.2.5 Other data.

As part of testing multiple approaches for constructing administrative samples, several other sources of data were also accessed. These data included information on claims from the Accident Compensation Corporation (ACC), tertiary enrolment and attainment from the Ministry of Education (MOE), and hospital attendance and admissions from the Ministry of Health (MOH). These data were used to construct and apply activity indicators to identify individuals to retain in the sample. However, the samples using activity indicators were not selected for the empirical analysis for several reasons (to follow) – relevant details of these data are explained in due course when necessary.

4.3 Constructing the Samples

4.3.1 Constructing the survey sample.

With the key research question focussed on the teenage employment impact of the 2001 minimum wage reform, quarterly data from the HFLS were taken over the period 1997q1-2003q3. This sample period provides sufficient data for empirical analysis both prior and subsequent to the 2001 minimum wage reform. With teenagers and young adults being regarded as those most likely being impacted by minimum wage increases, the HLFS sample was restricted to individuals aged 16-25 within the sample period. No other adjustments were made to the HLFS sample at this point, resulting in a total of 125,418 observations.

[74] Births data are only used from 1920 (Stats NZ, 2015e).

For the HLFS-IS, annual data were taken over the period 1997q2-2003q2. The data consisted of the same sample population as the HLFS. Overall, the HLFS-IS sample consisted of 30,371 observations. Details on all the steps taken to construct the HLFS and HLFS-IS samples are presented in Appendix B, Table B3.

4.3.2 Constructing a population sample using administrative data.

Historically, the Census has been the primary source of information on the NZ estimated resident population (ERP), which provides an "estimate of all people who usually live in New Zealand at a given date" (Gibb et al., 2016, p. 8). The objective of constructing an ERP using administrative data is to generate an analogous sample to the Census ERP. As part of the core data in the IDI, Stats NZ provides ERP data which researchers can use to construct an ERP for a specified sample period.[75] However, the coverage of this ERP only starts in 2007 and thus does not cover the sample period of this thesis. The remainder of this section provides a detailed description of how the ERP was created for the years in the sample period of interest, which serves as the basis for all administrative analyses in this thesis.

There have been several methods developed for constructing an administrative ERP in the past few years (Gibb et al., 2016; Gibb & Shrosbree, 2014; McLeod, 2018; Stats NZ, 2017a). Closer examination of these methods revealed that they all follow a relatively similar approach, albeit with some distinct differences, such as the adopted definition of residency status. An overview of the general process for constructing an administrative ERP is presented in Figure 4.2.

[75] The Stats NZ ERP provides a snapshot of the population on reference date by identifying individuals who are classified as NZ residents. The Stats NZ ERP is constructed by identifying individuals with activity in administrative data sources (Stats NZ, 2016).

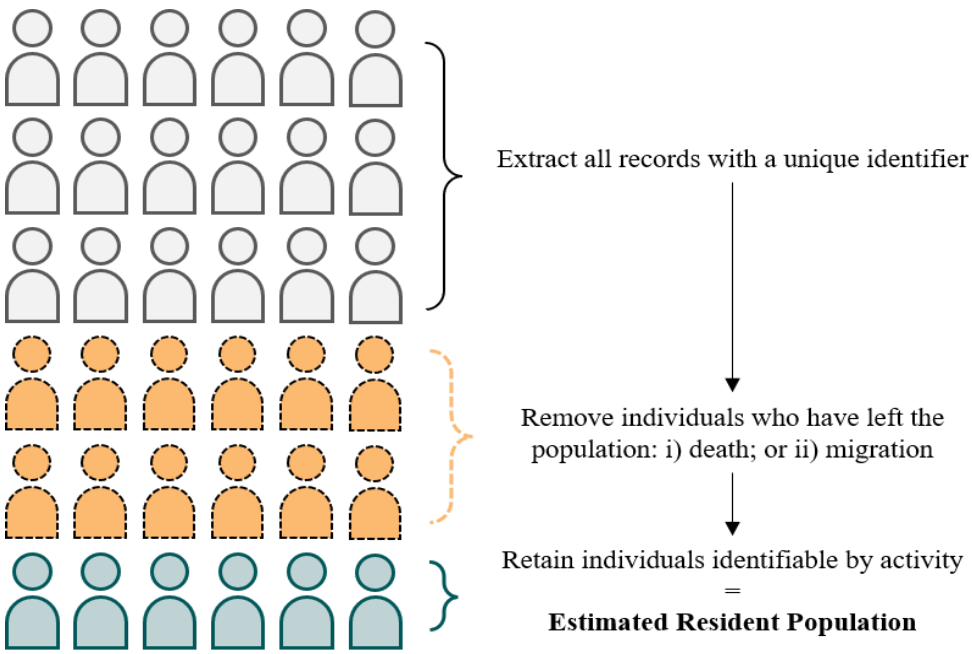

Figure 4.2: Constructing an estimated resident population using administrative data.
Adapted from Gibb et al. (2016) and Stats NZ (2017a). Author's compilation.

The base sample for this thesis consists of individuals aged 16-25 over the period April 1999-September 2003. Constructing the ERP commenced with extracting the UIDs of all individuals that would eventually comprise the sample spine.[76] The personal details table was deemed as the most suitable for this task as it includes a unique record for each individual who is part of the IDI-spine.[77] As the sample population was comprised of 16-25-year-olds, year of birth information, also available from the personal details table, was used that included only individuals who were part of the sample population age range.[78] No other restriction were imposed at this stage. Overall, a total of 10,173,186 unique records were extracted. Although this number is considerably larger than what would be expected for the NZ sample population, it also included individuals who had contact with government agencies but who did not form part of the ERP (e.g., overseas holiday-makers, business travellers and/or international students).

[76] The data were extracted using the October 2018 refresh of the IDI, as it was the most recent version available at the time when data collection commenced.
[77] This means a unique identifier is available for all individuals who have had contact with government and non-government agencies, and Stats NZ surveys from which IDI data are collected.
[78] Data for individuals born between 1973-1988 were extracted. This birth year range was selected to ensure that all individuals aged 16-25 during the sample period (April 1999-September 2003) were included. Although this birth year range resulted in individuals outside the sample age being included, they were removed before the sample was finalised.

Upon completion of the initial data extraction, individuals who had left the population were identified and removed. These individuals fell into two main categories of non-residency: i) death, or ii) migration.

Deceased individuals were identified using death records from the DIA. By using the month and year of death, individuals were identified and removed from the sample population for all periods following their death. Once deceased individuals were removed, the number of unique individuals remaining in the sample was 10,172,049.

Individuals who had left NZ due to permanent migration or short-term visits were also identified and removed from the sample using border movement information which is accessible in the person overseas spell table. This table provides information on all border movements in and out of NZ (i.e., when each travel spell starts/ends) from 1997. The key task underlying this identification process is defining 'usual residence in NZ'. There are no standard definitions of 'usual residence in NZ' as this concept is often self-defined and involves feelings of belonging, association and participation (McLeod, 2018). Consequently, in an attempt to introduce a standard definition of 'usual residence in NZ', Stats NZ (2017b) developed what is commonly referred to as the '12/16 rule' which uses the length of travel spells as a basis to determine usual residency status. The 12/16 rule stipulates, that if an individual spends a minimum of 12 months (of the 16 months from when the departure occurred) out of NZ, they are classified as a permanent departure, and considered a non-resident. Alternatively, if an individual arrives in NZ as an overseas visitor, and resides in the country for a minimum of 12 months (of the 16 months from when arrival occurred), they are classified as a resident.

Variations of the 12/16 rule have been tested in studies that have contributed methods for constructing an administrative ERP. For example, McLeod (2018) centres the 16 month period in the middle of the year, whereby an individual is considered to be a resident if they reside in NZ for at least 12 months of the 16 months from November the previous year to February the next year. The main advantage of this alternative definition is that the resident population can be defined in a more timely fashion, as less border movement information following the date of interest is required (McLeod, 2018).

This study adopts a calendar year version of the 12/16 rule which excluded individuals from the sample if they resided in NZ for less than 9 months of each calendar year.[79]

[79] Nine months per calendar year equates to 274 days.

Using the start and end date of each travel spell, the number of days out of NZ was calculated. The total number of days out of NZ was cumulative and could either be made up of single or multiple travel spells. Following the removal of non-residents, the number of unique individuals remaining in the sample was 990,165.

The key limitation of the person overseas spell information is that coverage only starts in September 1997. Thus, individuals who were born in NZ and permanently migrated prior to when coverage started were not identifiable using border movements alone. To address this limitation, Gibb et al. (2016) developed a process whereby a range of activity indicators were utilised to identify individuals who were present in NZ over a specific period relative to a reference date.

To test multiple methods of applying activity indicators, two definitions of activity were adopted. The first, followed the definition of Gibb et al. (2016), whereby individuals were retained in the sample if they had activity within administrative data sources in the 12 months prior to a reference date. The second, followed the definition of Stats NZ (2017a), whereby the period of activity was extended to 24 months prior to a reference date. Using linked data, the following data sets utilised by Gibb et al. (2016) and Stats NZ (2017a) were examined for activity records:

- **ACC claims:** provided date of accident information to determine ACC activity;
- **IRD EMS:** provided taxable income information to determine labour market and income activity;
- **MOE:** included data on tertiary enrolment and attainment to determine activity within the tertiary education sector; and
- **MOH:** included data attendance and hospital admissions to determine activity within the health sector.

One significant limitation of applying the methods of Gibb et al. (2016) and Stats NZ (2017a) in this study concerned the coverage period of several data sources. Specifically, the coverage periods of the following data did not align with the sample period of April 1999-Septemeber 2003:

- **MOH GP enrolment and attendance:** data available from 2002;
- **MOH pharmaceutical prescriptions:** data available from 2005;
- **MOH non-admission hospital visits:** data available from 2007; and
- **MOE secondary school enrolment:** data available from 2007.

In the case of two MOH data sets (i.e., pharmaceutical prescriptions & non-admission hospital visits) and the MOE secondary school enrolment data, the coverage periods start after the sample period end, making these data of no use in identifying activity in the IDI. In terms of GP enrolment and attendance data, coverage started within the sample period. However, these data were not considered suitable for identifying activity in the IDI as their use would result in sample selection issues (i.e., individuals treated differently across the sample period). Specifically, activity could only be identified from 2002, thus containing a disproportionate number of non-residents over the period 1999-2001.

MOE secondary school enrolment data were central to identifying the activity of teenagers, specifically 16-17-year-olds. Many of these individuals may still have been enrolled in secondary school and may not have participated in the formal labour market, and thus would likely only be identifiable in the secondary school data.[80] This limitation was noted by Black (2016) who pointed out that the IRD EMS data has low coverage of young people. Consequently, with no information available on secondary school enrolment covering the sample period, teenagers may likely be disproportionately removed from the sample due to having no (or little) activity in the IDI.

To evaluate the implications of having incomplete data to apply activity restrictions, employment levels derived from alternative samples were compared. Included were employment from the HLFS, a 12 month activity restricted sample, and a 24 month activity restricted sample. Table 4.1 presents mean employment by age for each of these samples. This brief examination uncovered the extent to which incomplete activity restriction data could impact the key outcome of this thesis.

When comparing mean employment levels for 12 month activity restrictions against the HLFS, it appears that differences are largest for 16 and 17-year-olds at 12 and 20 percentage points, respectively. For 18-19-year-olds, there was only a 5 percentage points difference in mean employment between the two samples. The smaller difference is not unexpected as some 18-year-olds would have already been enrolled in tertiary education and thus be identifiable in the MOE tertiary enrolment data. For the remaining ages, difference in mean employment ranged between 1-2 percentage points (over and under).

[80] For example, based on the HLFS data over the period 1999q2-2003q3: 67% of 16-17 studied and 53% participated in the labour market; 31% of 18-19-year-olds studied and 66% participated in the labour market; 15% of 20-25-year-olds studied and 73% participated in the labour market.

Table 4.1:
Mean Employment by Age using Activity Restriction and HLFS Samples

	Survey	Administrative activity restrictions	
Age	HLFS	12 months	24 months
16	.39	.51	.46
	(0.01)	(0.00)	(0.00)
17	.50	.59	.56
	(0.01)	(0.00)	(0.00)
18	.54	.59	.58
	(0.01)	(0.00)	(0.00)
19	.59	.61	.61
	(0.01)	(0.00)	(0.00)
20	.60	.63	.63
	(0.01)	(0.00)	(0.00)
21	.64	.65	.64
	(0.01)	(0.00)	(0.00)
22	.68	.67	.66
	(0.01)	(0.00)	(0.00)
23	.70	.68	.67
	(0.01)	(0.00)	(0.00)
24	.71	.69	.68
	(0.01)	(0.00)	(0.00)
25	.71	.70	.68
	(0.01)	(0.00)	(0.00)
Total observations	82,710	22,904,754	23,619,711
Total unique individuals[a]		727,863	734,841

Note. Survey data are from the HLFS and covers the period 1999q2-2003q3; Administrative sample covers the period April 1999-September 2003. Employment rates from the HLFS are weighted by the HLFS sampling weights. Standard errors are in parentheses. Author's compilation.
[a] This value is not available for the HLFS sample.

When using the 24 month activity indicator sample, the differences in mean employment of 16-17-year-olds were higher by approximately 7 percentage points when compared to those from the HLFS. There was also a 4 percentage points difference in mean employment of 18-year-olds between the two samples. For the remaining ages, the difference in mean employment ranged between 2-3 percentage points (over and under).

This brief examination highlighted the extent to which incomplete activity restriction data impacted employment, the key outcome of interest in this thesis. As expected, the employment of teenagers aged 16-17 was largely impacted. As noted, many 16-17-year-olds were likely to still be enrolled in secondary school and may not have participated in the formal labour market. Therefore, with little to no employment activity in the IRD EMS, and no secondary school enrolment data available, these teenagers were disproportionately removed from the activity restriction samples.

With 16-17-year-olds being one of the groups highly likely to be impacted by the 2001 minimum wage policy reform, it was imperative that an alternative approach to

constructing an ERP was considered. Consequently, an ERP which excluded activity indicators was constructed. Although this sample included noise (i.e., retaining some non-residents), the noise was expected to be consistent across the sample and would therefore not introduce any bias to a specific sub-group or period within the study. Therefore, this noise is expected to have little impact on the forthcoming regression analyses in Chapter 5 and Chapter 6. However, the inclusion of non-residents may impact the various descriptive analyses in this chapter as the addition of non-residents inflates the denominator. This can be seen in Table 4.2, where there are a larger remaining number of unique individuals in the sample with no activity indicators when compared to those with either 12 or 24 month activity indicators. Interestingly, even when activity indicators are applied, the remaining number of unique individuals is still much higher when compared to the mean ERP, highlighting the impact of the limited data coverage noted earlier. Thus, these alternative samples would still suffer from an inflated denominator.

Table 4.2:
Constructing Samples with and Without Activity Restrictions

Initial number of unique records extracted = 10,173,186	Activity indicator period		
	None	12 month	24 month
Number of records removed due to:			
Death	1,137	1,137	1,137
Non-residency status	9,181,884	9,167,556	9,168,987
Non-activity	-	164,796	161,265
Falling outside the sample population age (age < 16, age > 25)	84,924	97,605	92,493
Restricting to sample period (April 1999-September 2003)	15,249	14,226	14,460
Final number of unique individuals remaining[a]	889,989	727,863	734,841
Mean ERP from the 2013 Census[b]	533,102	533,102	533,102

Note. Totals may not add up correctly due to random rounding requirements by Stats NZ (2020c). Author's compilation.
[a] This represents the number of unique individuals aged 16-25 across the sample period April 1999-September 2003.
[b] This is the mean count of the usually resident population aged 16-25 over the period 1999q2-2003q3 taken from the estimates in the 2013 Census (Stats NZ, n.d.a).

To finalise the administrative sample, the population was restricted to 16-25-year-olds over the period April 1999-September 2003. The final sample consisted of 29,442,717 person-month observations, which were comprised of 889,989 unique individuals.

4.4 Comparing Employment Measures – Survey versus Administrative

Table 4.3 compares mean employment derived from the survey and administrative samples by age groups.[81] Across the sample population and various age groups, the comparison reveals that mean employment in the administrative sample was consistently lower than mean employment in the survey sample. For example, mean employment for

[81] To compare equivalent sample periods, the survey sample was restricted to the period 1999q2-2003q3.

16-17 and 18-19-year-olds was 5 and 6 percentage points lower in the administrative sample when compared to the survey sample. For those aged 20-25, the difference in mean employment was large. In the administrative sample mean employment was .54 compared to .67 in the survey sample.

Table 4.3:
Mean Employment by Age Group, Survey and Administrative Samples

Age group	Survey	Administrative
16-29	.60	.50
	(0.00)	(0.00)
16-17	.44	.39
	(0.00)	(0.00)
18-19	.56	.50
	(0.00)	(0.00)
20-25	.67	.54
	(0.00)	(0.00)
Observations	82,710	29,442,717

Note. Survey data are from the HLFS and covers the period 1999q2-2003q3; Administrative sample covers the period April 1999-September 2003. Observations are person-by-quarter for the survey sample, and person-by-month for the administrative sample. Survey employment means are weighted by the HLFS sampling weights generated by Stats NZ. Author's compilation.

There are a couple of potential explanations underlying these differences. First, there are differences in the definition of employment used in the survey and administrative data. As previously noted, a broad definition of employed is observed in the HLFS, meaning that even individuals who work without pay are classified as employed.[82] By including these individuals, mean employment in the survey sample is naturally higher when compared to mean employment in the administrative sample, which requires wage and salary tax records. Next, the differences in mean employment could be due to differences in samples. The HLFS includes proxy interviews which could result in measurement error.[83] On the other hand, the administrative sample include non-residents which deflates the mean employment by inflating the denominator.

4.5 Comparing Total Weekly Earnings

Figure 4.3 depicts the distribution of mean real total actual weekly earnings from the main job using survey data in panel (a) and administrative data in panel (b).[84] The total weekly earnings used here are analogous to the measures defined in section 4.1.2 for the survey data and section 4.2.3 for the administrative data. Nominal earnings were deflated using the Consumer Price Index (CPI) – base year/quarter 1999q2. In both panels, the

[82] Individuals who report as working without pay for a farm, business or practice owned or operated by a relative are classified as employed (Stats NZ, 2015a).
[83] Examining mean employment by excluding proxy interviews revealed potential measurement error. Using the same HLFS sample from Table 4.3, mean employment decreased by 3 and 2 percentage points for 18-19 and 20-25-year-olds, respectively.
[84] Referred to as 'total weekly earnings' from here on.

distributions were based on samples consisting of employed 16-25-year-olds, over the period 1999q2-2003q2. To adjust for outliers, total weekly earnings in each sample were censored below and above the 1st and 99th percentile, respectively. These adjustments are detailed in Appendix B, Table B4.

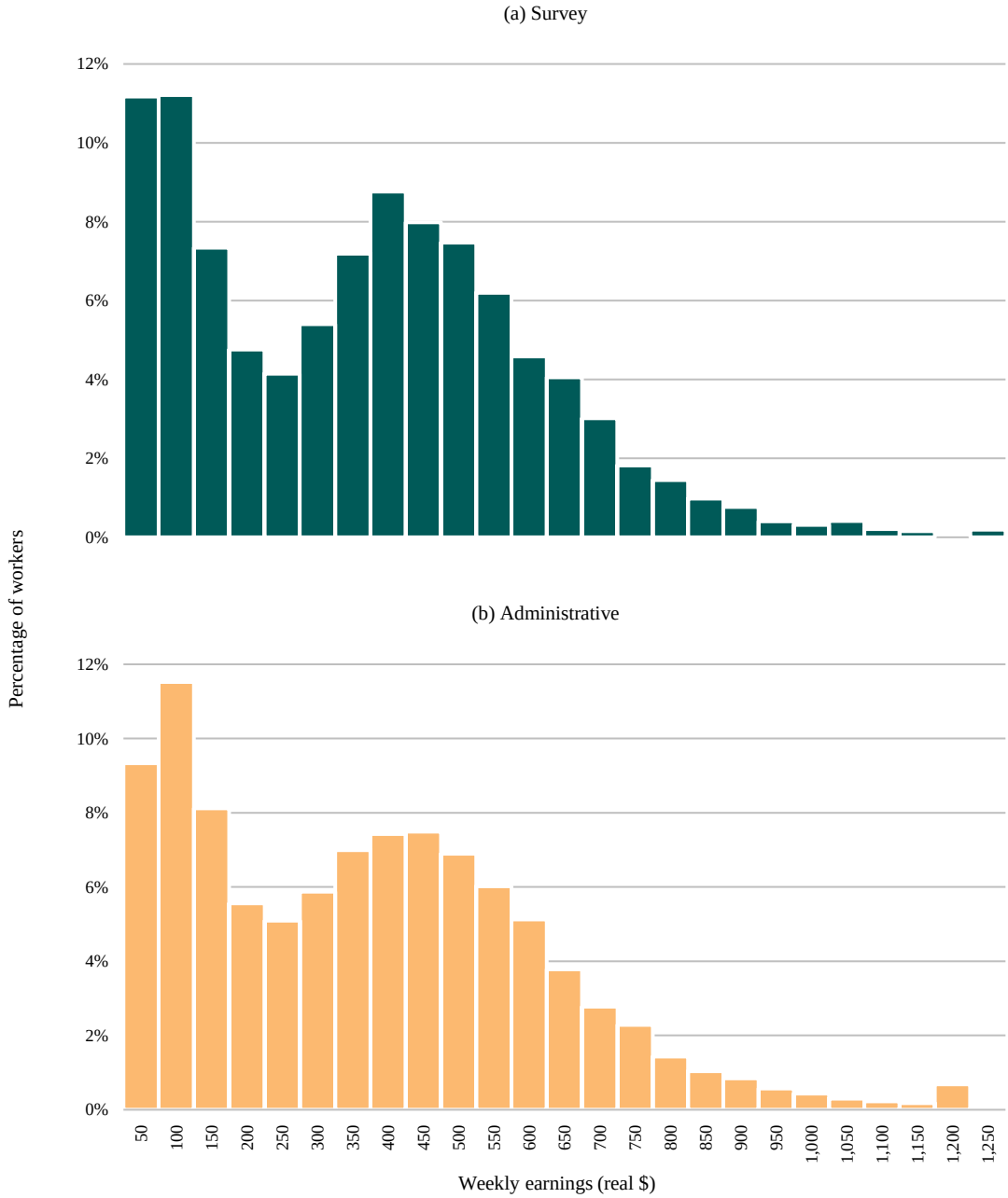

Figure 4.3: Mean real total weekly earnings using survey and administrative samples.
Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999-2003 (months of April-June). Earnings values were deflated by the CPI (base year/quarter 1999q2). Author's compilation.

The general distribution of total weekly earnings appears similar across both sample populations. Of note are the initial peaks at the lower ends of the distributions, where 22% and 21% of the survey and administrative samples, respectively, recorded total earnings

of up to $100 per week. However, the proportion of the administrative sample with total weekly earnings of up to $50 per week is approximately 2 percentage points lower when compared to the survey sample. There also appears to be a bimodal characteristic to the distributions of total weekly earnings in both samples, with a secondary peak forming at around $400 per week. However, this secondary peak appears more pronounced in the survey sample with approximately 9% of total earnings falling in the range of $350-$400 per week compared to 7% in the administrative sample.

4.6 Comparing Hourly Wages

4.6.1 Measuring hourly wages of low-wage workers using survey data.

Using survey data from the HLFS-IS, Figure 4.4 presents the distributions of four alternative definitions of hourly wages for a sample of employed 16-25-year-olds over the period 1999q2-2003q2. The two series represent the pre-reform (1999) and post-reform (2003) real hourly wage distributions, respectively. These graphs were restricted to hourly wages of up to $10 per hour to focus the descriptive analysis on low-wage workers, which are the primary sub-group of interest in this chapter.[85] Nominal wages were deflated using the CPI – base year/quarter 1999q2. In all cases, hourly wages were censored below and above the 1st and 99th percentile, respectively, to adjust for outliers. These adjustments are detailed in Appendix B, Table B5.

The purpose of examining the distributions of hourly wage measures was to assess whether any selection bias would result in selecting a particular measure for further descriptive analysis. Panel (a) represents the distribution of hourly wages derived by Stats NZ and used in the analysis of minimum wage workers by Timmins (2006). Panel (b) represents the distribution of regular usual hourly earnings as defined in Pacheco (2009). Panel (c) represents the distribution of actual hourly earnings from the main wage and salary jobs derived in this study. Panel (d) represents the distribution of usual hourly earnings from the main wage and salary job derived in this study. Full definitions of all these measures are presented in Appendix B, Table B6.

[85] On average, wages of up to $10 per hour covered approximately 54% of the hourly wage distribution for the full sample of 16-25-year-old workers.

There were some similarities in the distributions of hourly wages across all four measures. For example, the distribution of each measure peaked at wages of up to $9 per hour in the pre-reform period. This was also the case in the post-reform period for the distributions in panels (a) and (c).

The hourly wage distributions highlighted several differences which largely occurred at the lower and upper ends of the distributions. Starting with differences at the lower end, there were no instances of workers earning wages of up to $2 per hour in panels (b) and (d) in 1999 and 2003. Conversely, in panels (a) and (c), a small proportion of workers earned wages of up to $1 per hour, being 2% in 1999 and 6% in 2003. A small number of individuals in panels (a) and (c) earned wages between $1 and up to $2 per hours, however these values had to be suppressed as they were too low for release given confidentiality rules from Stats NZ (2020c).

Changes in hourly wages also differed at the upper end of the distributions. Between 1999 and 2003, there were 1 percentage point fewer workers earnings wages between $8 and $9 per hour in panels (a) and (c), compared to 6 percentage points fewer in panels (b) and (d). Furthermore, fewer workers earned wages between $9 and $10 per hour in panels (a) and (c), compared to more workers in panels (b) and (d).

The observed differences in hourly wage distributions can likely be explained by the opposing definitions of ‘actual’ and ‘usual’. As noted in section 4.1.2, actual earnings and hours refer to an objective measure of earnings received or hours worked in a particular reference week. Conversely, although usual has the benefit of presenting a better average of an individual’s earnings and hours, these measures are open to interpretation. Thus, an individual’s actual and usual hourly wages may differ if they did not receive their typical earnings, or work their typical hour, in the reference week. Consequently, due to the objectivity of actual earnings and hours worked, and the comparability of actual earnings to those from the EMS, actual hourly wages from panel (c) will be used in the remainder of the descriptive analysis.

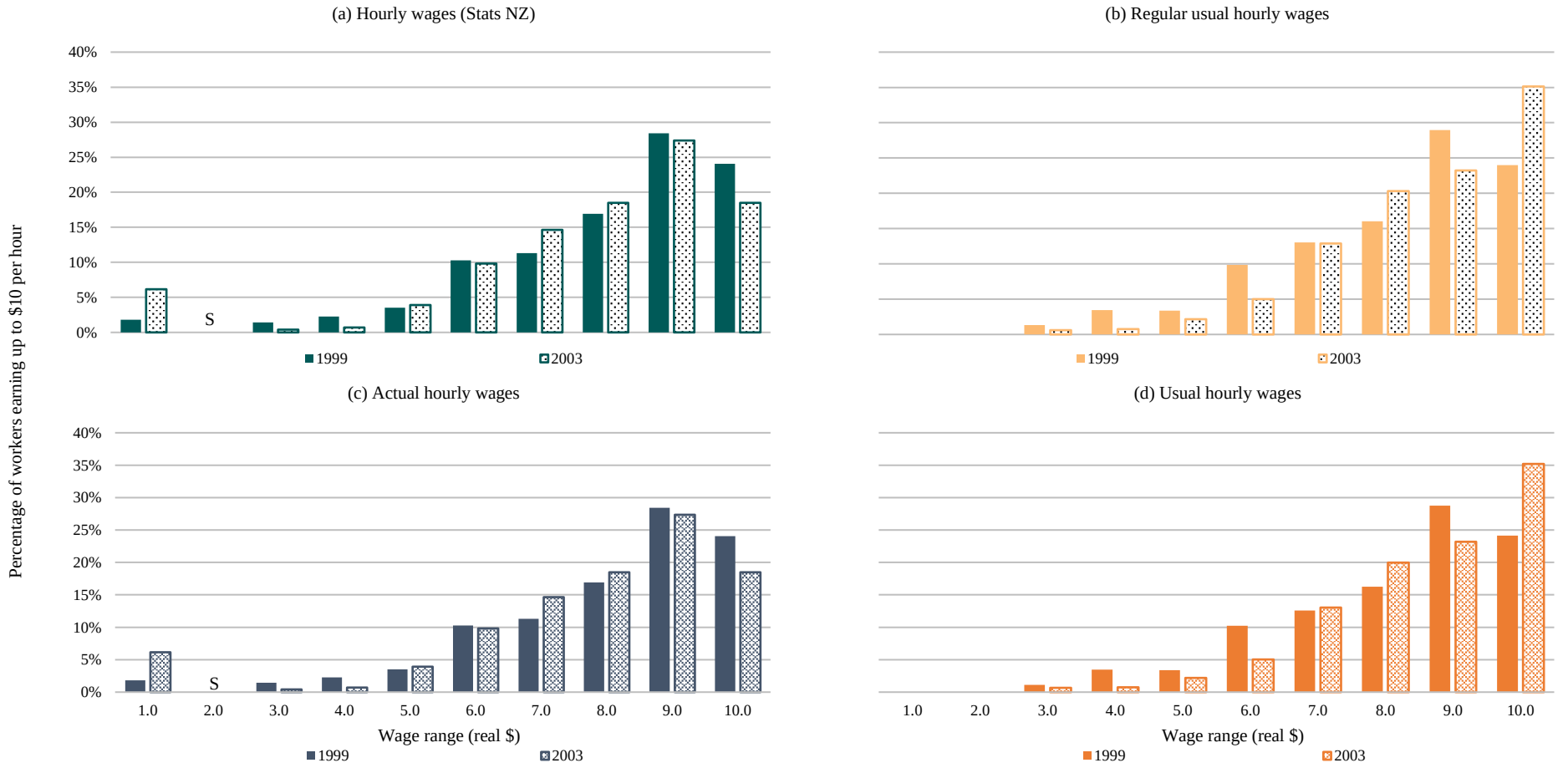

Figure 4.4: Distributions of hourly wages using alternative survey data definitions.
Survey data are from the HLFS-IS covering the period 1999q2-2003q2. Earnings values were deflated by the CPI (base year/quarter 1999q2). 'S' = value suppressed due to suppression criteria outlined in Stats NZ (2020c). Measures are defined as follows: (a) Hourly wages are actual hourly earnings from 1st wage and salary job; (b) Regular usual hourly wages are usual hourly earnings excluding overtime earnings and hours; (c) Actual hourly earnings are from main wage and salary job and include actual overtime hours and earnings, and (d) Usual hourly earnings are from main wage and salary job and include usual overtime hours and earnings. Author's compilation.

4.6.2 Addressing the absence of hours worked information in the EMS.

The absence of hours worked information is a key limitation of the EMS up to April 2019. This limitation becomes especially problematic when wanting to compare hourly wages between survey and administrative samples, as would be the case when using hourly wages to identify sub-minimum and minimum wage workers and conducting descriptive comparisons of these population sub-groups (upcoming in section 4.7). Consequently, to address this limitation, the administrative sample required hours worked data to be imputed from an alternative source. With no direct linking of individual records possible between the HLFS-IS and administrative data prior to June 2007 (Stats NZ, 2015b), the next best alternative was to impute hours worked information using sub-groups from the HLFS-IS sample population.

The annual availability of hours worked information in the HLFS-IS was important to consider before imputing the data to the administrative sample. Therefore, to ensure comparability between the survey and administrative samples, administrative records from April, May and June were retained as these months directly compare to when HLFS-IS interviews are conducted.

Another important consideration was to ensure that the imputed hours of work from the HLFS-IS would closely resemble an individual's likely hours of work. The earlier examination of mean employment in section 4.4 revealed variations in mean employment across age groups. With that in mind, this imputation exercise assumed that variations were also present in hours worked, and that these variations existed across age groups, gender and ethnicity. To explore this further, Figure 4.5 plots mean hours worked across these demographic characteristics.

Mean hours worked is plotted by individual age from 16-19 (panel [a]) and 20-25 years (panel [b]). As would be expected, mean hours of work generally increased with age in panels (a) and (b). For teenagers in panel (a), there was an overall increase in mean hours worked over the sample period, albeit only a marginal increase for 19-year-old workers. For young adults in panel (b), there were small decreases in mean hours worked over the sample period for each age, except 25-year-olds, where hours increased marginally. In panel (c), mean hours worked is plotted by gender. Overall, mean hours worked was higher for males. Over the sample period, there was a marginal increase in mean hours worked for males compared with a small decrease for females. In panel (d), mean hours worked are plotted by ethnicity. As was observed with mean hours worked in panels (a)

and (b), clear differences are also observable by ethnicity. Over the sample period, hours appeared to converge to just over 30 hours worked per week by 2003

Based on the variations observed in Figure 4.5 the imputation of hours into the administrative sample utilised the mean values of actual hours worked per week in the main job[86] at an age group, gender, ethnicity and year level. For example, the mean hours worked of 16-17-year-old Pākehā males in 1999q2 (from the HLFS-IS) was imputed into the administrative sample for all 16-17-year-old Pākehā males for the months April-June 1999.[87] The same approach was used for all other combinations of the demographic groups and years.[88]

Figure 4.6 presents the mean hours worked by age group and year using both the survey and administrative samples. Mean hours worked from the administrative sample was generated using the imputed hours worked from the HLFS-IS. For all age groups, trends in the mean hours worked were similar between the survey and administrative samples. However, by age group, mean hours worked from the administrative sample was higher on average for each age group when compared to those from the survey sample. This could be explained by differences in the demographic composition across the two samples given how hours of work were imputed from the HLFS-IS into the administrative sample.

In panel (a), mean hours worked is plotted for 16-17-year-olds. From 2001, mean hours worked from the administrative sample was relatively higher than that from the survey sample. Across both samples, mean hours worked increased over the sample period. In panel (b), mean hours worked is plotted for 18-19-year-olds. Over 1999 and 2000, mean hours worked from the administrative sample was relatively higher than that from the survey sample. Interestingly, mean hours over the sample period from the administrative sample remained relatively flat, compared to a steady increase from the survey sample. In panel (c), mean hours worked is plotted for 20-25-year-olds. Across the full sample period, mean hours worked from the administrative sample was relatively higher than that from the survey sample. Across both samples, mean hours worked displayed a decreasing trend between 1999-2002.

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[86] Referred to as 'hours worked' from here on.
[87] Age group means (i.e., 16-17, 18-19, 20-25) were selected over individual ages given the small counts of records once disaggregated by gender, ethnicity and year.
[88] In total, 12% of records in the administrative sample had no ethnicity specified. In these cases, the mean actual hours worked per week in main job at an age group, gender, and year level was imputed.

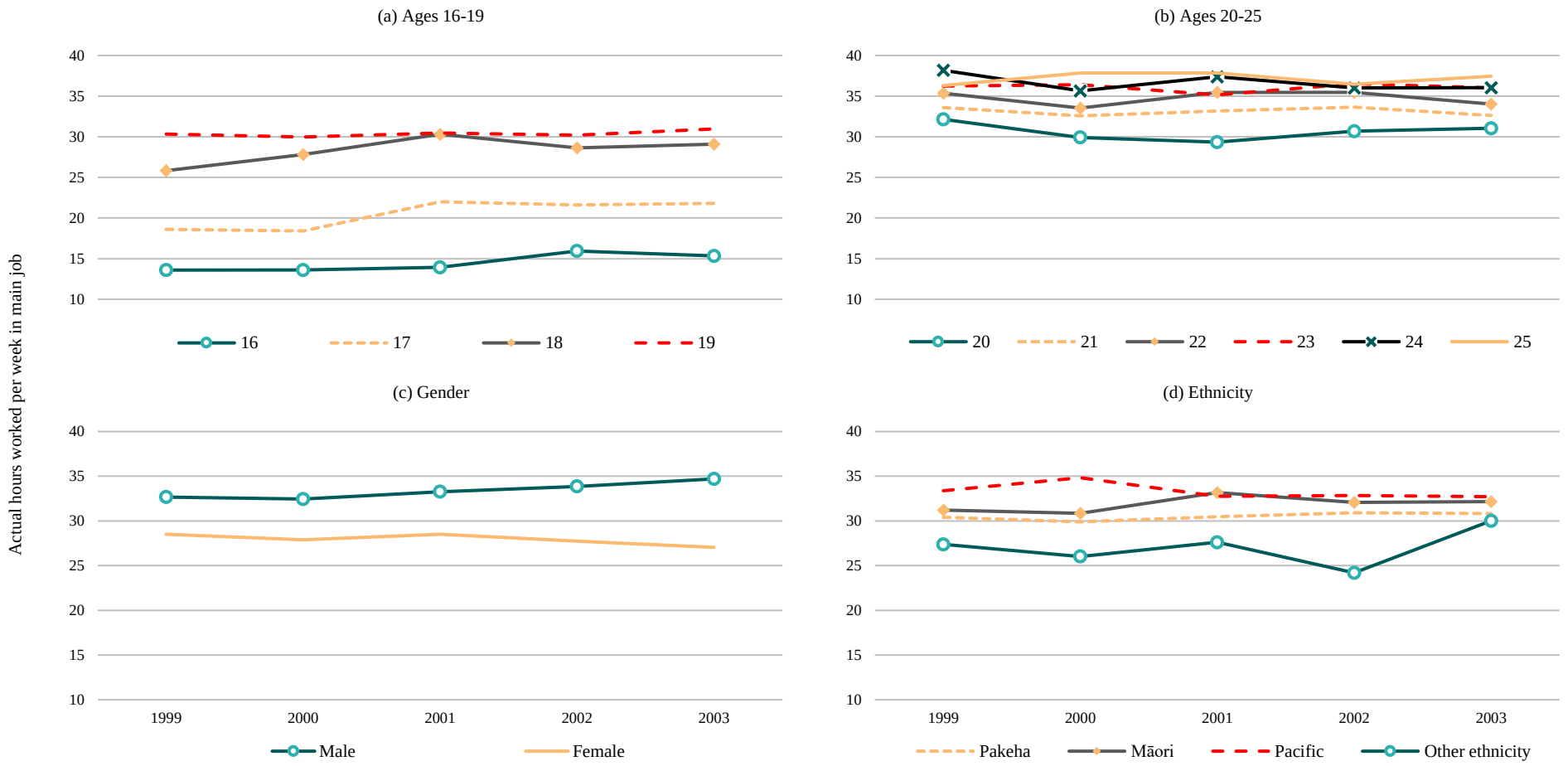


Figure 4.5: Mean actual hours worked in main job by age, gender and ethnicity.
Data are from the HLFS-IS covering the period 1999q2-2003q2. Values on the y-axis start at 10 hours per week to clearly highlight the variation in the actual hours worked over the sample period by each sub-group. Ethnicity dummy variables were constructed by adopting the prioritised ethnicity approach by Meehan, Pacheco, and Pushon (2019). Other ethnicity consists of the following categories: Asian, Other and Not specified. Author's compilation.

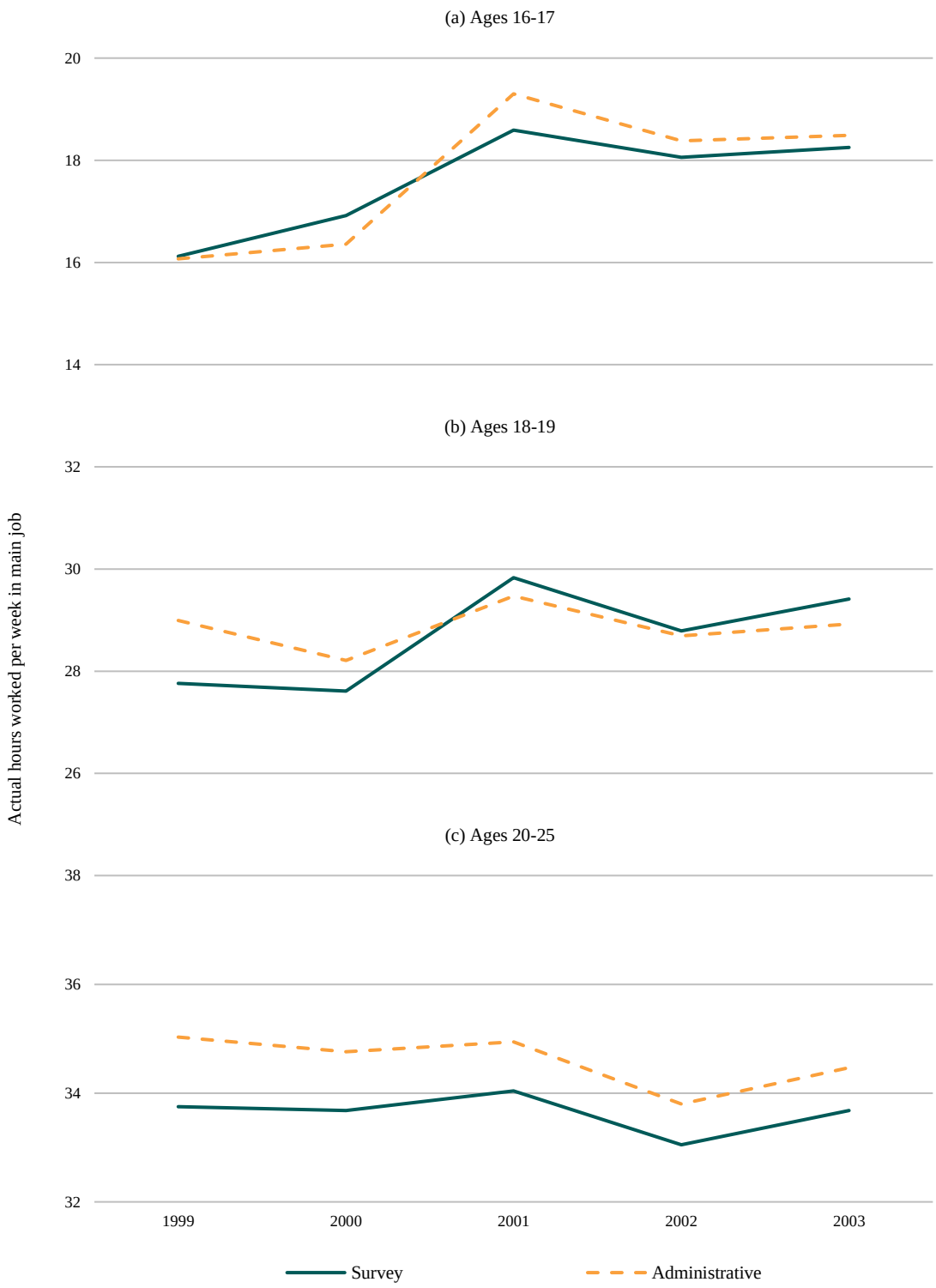


Figure 4.6: Mean actual hours worked per week in main job by age group, survey and administrative samples.
Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999-2003 (months of April-June). Values on the y-axis start at different levels to clearly highlight the variation in the actual hours worked over the sample period by each sub-group. Author's compilation.

Although these differences in hours worked appear relatively small across age groups, they could have measurable impacts when estimating hourly wages. More specifically, given the generally similar distributions of total weekly earnings discussed in section 4.5, the differences in hours worked observed in Figure 4.6 could make measurable impacts when deriving hourly wages. The extent of this impact will be explored next.

4.6.3 Comparing hourly wages of low-wage workers using survey and administrative data.

Figure 4.7 presents the distribution of hourly wages of employed 16-25-year-olds earning up to $10 per hour over the period 1999q2-2003q2. The survey measure is presented in panel (a), with the administrative measure in panel (b).

The two series represent the pre-reform (1999) and post-reform (2003) real hourly wage distributions, respectively. Nominal wages were deflated using the CPI – base year/quarter 1999q2. In all cases, hourly wages were censored between the 1st and 99th percentile to adjust for outliers. These adjustments are detailed in Appendix B, Table B5.

The survey measure is the same as the actual hourly earnings measures from Figure 4.4 panel (c). The administrative measure utilised actual weekly earnings from the main job (as defined in section 4.2.3) and this was divided by actual hours imputed from the HLFS-IS.[89]

Figure 4.7 reveals two distinct distributions in hourly wages using the survey and administrative samples. In panel (a), the distribution of hourly wages is skewed to the left, peaking at $9.00 per hour in both 1999 and 2003. In terms of changes when comparing 1999 and 2003, there were fewer workers earning low wages of around $3 to $4 per hour, as well as $9 to $10 per hour. Positive shifts took place where workers earned between $5 to $8 per hour between 1999 and 2003.

[89] Definitions of these measures are available in Appendix B, Table B6.

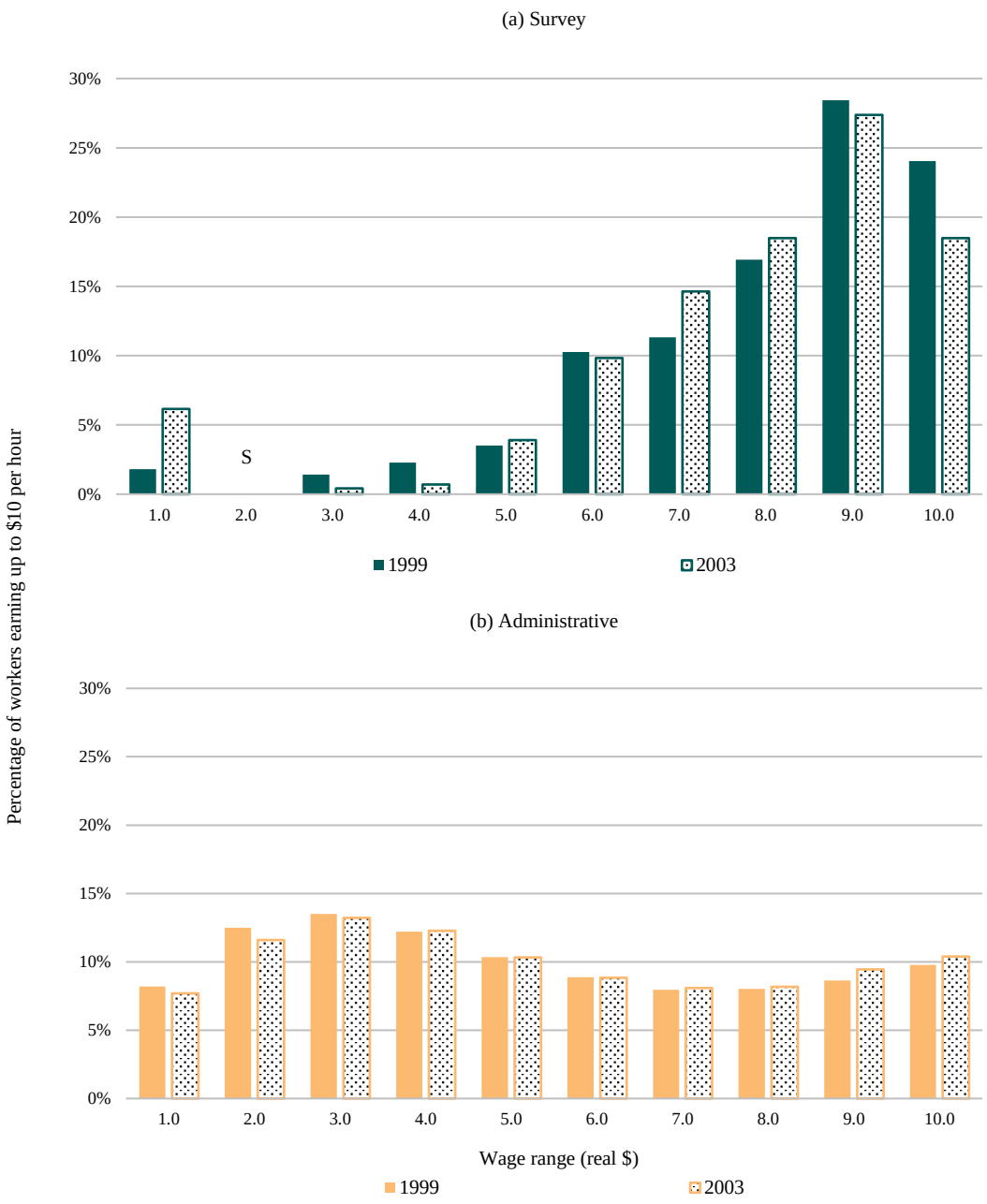


Figure 4.7: Distributions of hourly wages using survey and administrative measures.
Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999-2003 (months of April-June). Earnings values were deflated by the CPI (base year/quarter 1999q2). Author's compilation.

Similar characteristics in the distribution of hourly wages in panel (b) are not observed. First, the peak of the distributions in both the pre- and post-reform periods occur towards the lower end of the distribution at $3.00 per hour. Relative to panel (a), the distribution of hourly wages in panel (b) appears more uniform. For example, using wages of up to or below $5 per hour as a basis of comparison, an average of 10% of the sample in panel (a) was earning corresponding wages across both periods, compared to an average of 56% in panel (b). Furthermore, the positive shift in wages observed in panel (a) from the 2001

minimum wage reform were not observed to the same extent in panel (b). Although there were decreases and increases at the lower and upper end of the distribution, approximately 61% of workers in both 1999 and 2003 were earnings hourly wages in the range of $4 to $8 per hour in panel (b). The likely explanation for the contrasting hourly wage distributions in Figure 4.7 relates to potential measurement error of self-reported earnings and hours data (Moore et al., 2000; Pedace & Bates, 2000) from the HLFS-IS which were imputed into the administrative sample.

4.7 Characteristics of Sub-minimum and Minimum Wage Workers

4.7.1 Construction of wage groups.

The first issue considered was how to define different wage groups, with several approaches having been adopted in the empirical literature. The simplest approach would be to define a minimum wage worker as someone receiving an hourly wage equal to the statutory minimum wage for their age at the time of the survey (Long, 1999; Smith & Vavrichek, 1992). However, this approach would risk introducing significant measurement error into the analysis as salaried workers rarely know their hourly rate and must estimate it using earnings and hours information (Maloney & Pacheco, 2012).

A more common approach is to set a band around the minimum wage to account for any potential measurement error in reported earnings or hours. For example, Haugen and Mellor (1990) defined minimum wage workers as those earning an hourly wage equal to or less than the statutory minimum wage. An alternative approach by Bernstein and Schmitt (2000) defined minimum wage workers as those earnings an hourly wage equal to the statutory minimum wage and up to one dollar above that amount. These approaches are similar to those used in the NZ literature examining minimum wage worker characteristics. For example, Timmins (2006) defined minimum wage workers as those earning hourly wage rates between the current minimum wage rate and next year's minimum wage rate. Similarly, Pacheco (2007) classified minimum wage workers based on whether their regular usual hourly earnings were more than or equal to the current minimum wage but less than 10% above the minimum.

An alternative definition of wage groups was presented by Maloney and Pacheco (2012), where bands were created on either side of the current and previous age relevant minimum wages. These bands were fixed in real dollar amounts and set at intervals of 20-cents, 50-cents and 100-cents. The benefit of this approach was in recognising the timing of legislated minimum wage increases against the timing of the HLFS-IS data. Specifically,

minimum wages generally increase in March or April, with HLFS-IS interviews being conducted over the months of April to June. Consequently, some individuals may be reporting earnings that are yet to be adjusted for the rise in the minimum wage. For the purpose of the descriptive analysis on wage group characteristics, wage groups were generated using the definitions utilised by Maloney and Pacheco (2012). Table 4.4 outlines these definitions.

Table 4.4:
Definitions of Wage Groups

Wage group	Definition
Sub-minimum	Actual hourly wages < (previous age relevant minimum wage – wage band)
Minimum	Actual hourly wages > (previous age relevant minimum wage – wage band) and Actual hourly wages < (current age relevant minimum wage + wage band)
Other	Actual hourly wages > (current age relevant minimum wage + wage band)

Note. Adapted from Maloney and Pacheco (2012). Wage bands were set at 20-cents, 50-cents, and 100-cents and fixed in real dollar amounts (deflated by the CPI; base year/quarter 1999q2). Author's compilation.

Based on these definitions, wage groups were constructed using the actual hourly earnings measures presented section 4.6.3. Both survey and administrative samples were restricted to employed individuals over the period 1999q2-2003q2. Figure 4.8 presents the relative sizes of sub-minimum and minimum wage worker groups using the 50-cents wage band, which will be the focus of the remaining descriptive analyses in this chapter.

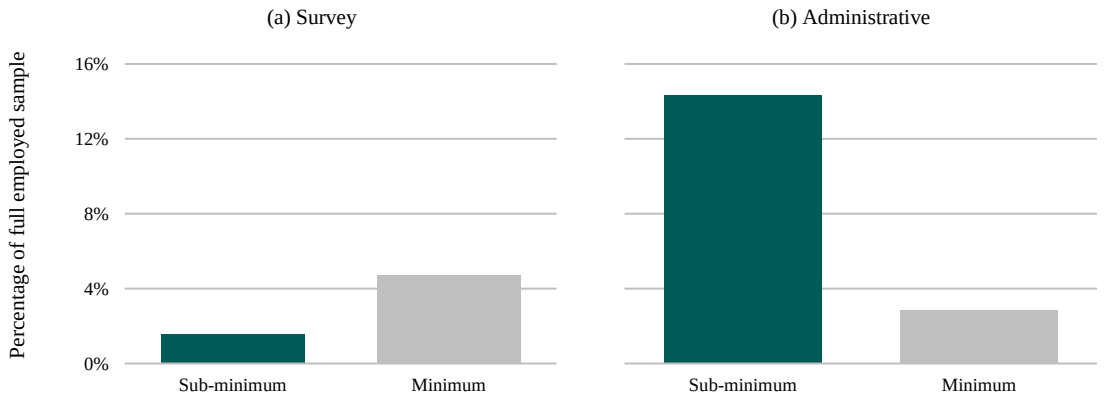


Figure 4.8: Percentage of sub-minimum and minimum wage workers.
Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999-2003 (months of April-June). Percentages are based on using the 50-cents wage band. Author's compilation.

Starting with wage groups in panel (a), of all workers, approximately 2% were classified as sub-minimum wage workers, and 5% as minimum wage workers. In panel (b), approximately 14% of all workers were classified as sub-minimum wage workers and 3% as minimum wage workers.

The relative size of the sub-minimum and minimum wage groups also changed over time. Figure 4.9 presents the percentages of sub-minimum and minimum wage workers using the survey and administrative samples over the sample period by year.

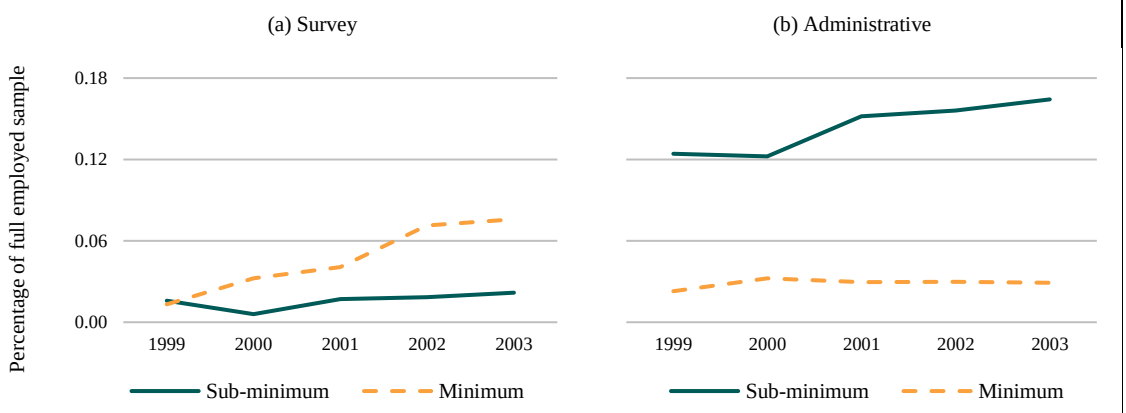


Figure 4.9: Percentage of sub-minimum and minimum wage workers by year.
Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999 2003 (months of April-June). Percentages are based on using the 50-cents wage band. Author's compilation.

In panel (a), there were proportionately more minimum wage workers than sub-minimum wage workers from 2000-2003. Over the sample period, the proportion of sub-minimum wage workers remained relatively steady at around 2%; the proportion of minimum wage workers grew steadily from 3% in 1999 to 8% in 2003.

In panel (b), there were proportionately more sub-minimum wage workers than minimum wage workers over the full sample period. The proportion of sub-minimum wage workers grew from 12% in 1999 to 16% in 2003. No substantial variation was observed on average for minimum wage workers, remaining steady at 3%.

4.7.2 Characteristics of sub-minimum and minimum wage workers.

Table 4.5 compares the demographic, regional and employment characteristics of sub-minimum and minimum wage workers over the period 1999q2-2003q2 using both the survey and administrative samples.[90]

Of the sub-minimum wage workers in the survey sample, 49% were aged 20-25 and consisted of marginally fewer females. Approximately two-thirds were Pākehā, with 20% classified as Māori and 4% Pacific. Using local government region indicators, over half

[90] These characteristics were also examined using 20-cents and 100-cents wage bands and are available in Appendix B. Characteristics from the survey sample are in Table B7 (20-cents) and Table B8 (100-cents), and those from the administrative sample are in Table B9 (20-cents) and Table B10 (100-cents). Although the relative size of each wage group differed depending on the wage band utilised, the qualitative results were consistent across all three wage bands.

of all sub-minimum wage workers were spread across Auckland, Wellington and Canterbury. Sub-minimum wage workers from the administrative sample differed in age composition (fewer 18-19-year-olds) and gender (larger proportion of females). However, ethnic and regional characteristics across both samples were similar.

Notable differences between the two sub-minimum wage worker populations were observed based on their employment characteristics. In the administrative sample, most sub-minimum wage workers were working full-time hours (62%) as opposed to a minority in the survey sample (47%). Sub-minimum wage workers from the administrative sample worked approximately 2.23 hour per week more on average, when compared to the survey sample. Next, real actual hourly wages as a proportion of the age relevant real minimum wage for sub-minimum wage workers in the administrative sample (0.44) was substantially lower than in the survey sample (0.67). Similarly, real actual weekly earnings for sub-minimum wage workers from the administrative sample were approximately $40 per week lower when compared to their survey counterparts.

Minimum wage workers from the survey sample were relatively younger when compared to sub-minimum wage workers. Overall, each age group accounted for approximately a third of minimum wage workers, with the largest segment being 20-25-year-olds at 37%. Minimum wage workers from the survey sample were largely Pākehā (68%) and Māori (17%), with those of Pacific and other ethnicities accounting for 6% and 9%, respectively. Regionally, the majority of minimum wage workers resided in either Auckland (22%), Waikato (13%) or Canterbury (19%). Compared to the survey sample, minimum wage workers from the administrative sample differed in age composition, with fewer 18-19-year-olds (23% & 34%, respectively) and more 20-25-year-olds (44% & 37%, respectively). The remaining demographic characteristics of minimum wage workers between the two samples were largely similar, with only a few small differences in regional composition.

Table 4.5:

Comparing Wage Group Characteristics using Survey and Administrative Samples

	Survey		Administrative	
Characteristics	Sub-minimum	Minimum	Sub-minimum	Minimum
Percentage of full sample	1.72	5.23	14.35	2.87
Demographic				
Age	19.92	19.14	19.89	19.44
	(0.16)	(0.08)	(0.00)	(0.01)
16-17	.18	.30	.22	.33
	(0.02)	(0.01)	(0.00)	(0.00)
18-19	.32	.34	.24	.23
	(0.03)	(0.02)	(0.00)	(0.00)
20-25	.50	.37	.54	.44
	(0.03)	(0.02)	(0.00)	(0.00)
Female	.49	.50	.54	.51
	(0.03)	(0.02)	(0.00)	(0.00)
Pākehā	.67	.68	.68	.67
	(0.03)	(0.02)	(0.00)	(0.00)
Māori	.20	.17	.17	.18
	(0.02)	(0.01)	(0.00)	(0.00)
Pacific	.04	.06	.06	.07
	(0.01)	(0.01)	(0.00)	(0.00)
Other	.09	.09	.09	.08
	(0.02)	(0.01)	(0.00)	(0.00)
NZ Born	.86	.86	.89	.90
	(0.02)	(0.01)	(0.00)	(0.00)
Regional				
Northland	.03	.03	.02	.03
	(0.01)	(0.00)	(0.00)	(0.00)
Auckland	.23	.22	.25	.24
	(0.03)	(0.02)	(0.00)	(0.00)
Waikato	.10	.13	.10	.11
	(0.02)	(0.01)	(0.00)	(0.00)
Bay of Plenty	.06	.07	.07	.08
	(0.01)	(0.01)	(0.00)	(0.00)
Gisborne/Hawkes Bay	.06	.06	.05	.06
	(0.01)	(0.01)	(0.00)	(0.00)
Taranaki	.04	.03	.03	.03
	(0.01)	(0.00)	(0.00)	(0.00)
Manawatu-Wanganui	.06	.05	.06	.06
	(0.01)	(0.01)	(0.00)	(0.00)
Wellington	.12	.09	.14	.12
	(0.02)	(0.01)	(0.00)	(0.00)
Nelson-Tasman-Marlborough-West Coast	.04	.03	.03	.04
	(0.01)	(0.00)	(0.00)	(0.00)
Canterbury	.16	.19	.14	.14
	(0.02)	(0.01)	(0.00)	(0.00)
Otago	.06	.07	.08	.06
	(0.01)	(0.01)	(0.00)	(0.00)
Southland	.04	.03	.02	.03
	(0.01)	(0.00)	(0.00)	(0.00)

(continues)

Characteristics	Survey		Administrative	
	Sub-minimum	Minimum	Sub-minimum	Minimum
Employment				
Full-time	.47	.43	.62	.54
	(0.03)	(0.02)	(0.00)	(0.00)
Actual total weekly hours	27.31	24.92	29.54	27.89
	(1.08)	(0.56)	(0.01)	(0.02)
Hourly earnings as a proportion of the age relevant minimum wage	0.67	1.00	0.44	0.97
	(0.01)	(0.00)	(0.00)	(0.00)
Real actual weekly earnings from main job ($)[a]	130.36	178.14	89.41	180.59
	(6.24)	(4.24)	(0.06)	(0.17)
Data				
Proxy	.06	.04	n/a	n/a
	(0.01)	(0.01)	n/a	n/a
Imputed	.18	.14	n/a	n/a
	(0.02)	(0.01)	n/a	n/a
Observations	381	1,158	1,174,950	234,747

Note. Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999 2003 (months of April-June). Earnings values are deflated by the CPI (base year/quarter 1999q2). Survey means were weighted using HLFS-IS sampling weights. Ethnicity dummy variables were constructed by adopting the prioritised ethnicity approach by Meehan et al. (2019). Standard errors are in parentheses. Author's compilation.

[a] Real actual weekly earnings from the main job include overtime earnings from the main job.

As was observed with sub-minimum wage workers, there were notable differences in employment characteristics between the two minimum wage worker populations. For instance, in the administrative sample, most minimum wage workers were working full-time hours (54%) as opposed to a minority in the survey sample (43%). Minimum wage workers from the administrative sample also worked more hours per week on average, at 27.89 per week compared to 24.92 per week in the survey sample. Of note, real earnings were similar between the two groups of minimum wage workers, with those from the administrative sample earning $180.59 per week on average, compared to $178.14 per week on average in the survey sample.

4.7.1 Key observations from wage group analysis.

The descriptive analysis in section 4.7.2 revealed several observations worth noting. First, although the sample selection limitations of the administrative sample were noted in section 4.3.2, demographic characteristics relating to gender, ethnicity and region were largely similar between the survey and administrative samples and wage groups.

Next, characteristics where hourly wages were used as inputs showed incongruency. This was initially noted in the construction and comparisons of wage groups across the two samples in section 4.7.1. The size of the sub-minimum wage worker population could likely be explained by the number of workers with earnings on the margin of a sub-minimum wage or minimum wage worker classification. Here, with weekly earnings largely comparable across the two samples, and imputed weekly hours slightly higher

across all age groups, as examined in Figure 4.6, an individual may be classified as a sub-minimum wage worker in the administrative sample and a minimum wage worker in the survey sample.

Unfortunately, delving further into the reasons behind such contrasting wage group classifications is not possible as there is no ability to directly link individual records between the two samples over the sample period of this study. The impact of imputed hours worked was also observed with employment characteristics relating to full-time status, actual total weekly hours, hourly earnings (as a proportion of the age relevant minimum wage) and weekly earnings – particularly for sub-minimum wage workers.

Based on the above observations, the descriptive characteristics from the administrative sample must be interpreted with some caution. In this descriptive analysis, survey results are arguably more informative in understanding the characteristics of sub-minimum and minimum wage workers over the sample period of this study. Nonetheless, the descriptive statistics from the administrative sample are enlightening as they provide evidence of the necessity for an alternative source of hours worked information where administrative data are utilised.

4.8 Conclusion

There were several objectives set for this chapter. The first objective was to understand the strengths and limitations of the available administrative and survey data when constructing a suitable sample for empirically examining the effects of minimum wage increases on teenage employment. The next objective was to understand the benefits and limitations of administrative and survey measures related to earnings and employment. The final objective was to understand the boundaries of administrative data relative to survey data with respect to the types of empirical analyses that can be undertaken given the identified limitations.

A comprehensive description of all administrative data sources utilised in this section was presented in section 4.2. Here, the primary administrative data limitation was noted as being the absence of hours of work information. Such data would be beneficial for exploring how individual hourly wages compare to minimum wages, and for empirically examining the impact of minimum wage increases on alternative outcomes such as hours of work. Limitations were also uncovered when the main administrative sample was constructed in section 4.3.2. These related to coverage periods of data sources required to

construct an administrative sample using activity restrictions. Coverage of secondary school enrolment data from the MOH only starts in 2007, meaning 16-17, and some 18-year-olds, would be disproportionately removed from the sample if activity indicators were utilised. Consequently, activity restrictions were not utilised.

This chapter undertook a comprehensive descriptive analysis which compared a range of measures constructed from survey and administrative data. Employment was compared in section 4.4. The benefit of the administrative data was observable in utilising a narrower definition of employment based on wage and salary tax records. In contrast, the comparable survey measure from the HLFS uses a broad definition of employment which could inflate observable employment levels. The limitation of the administrative employment measure related to the construction of the administrative sample, being that the inclusion of non-residents inflated the denominator and thus resulted in potentially measuring lower levels of employment.

Hourly wages were compared in 4.6. The limitation of the administrative measure is that it was based on imputed hours of work data from the HLFS-IS. This resulted in the hours of work measure being consistently higher in the administrative sample due to differences in the demographic composition of the two samples. Consequently, hourly wages measured using the administrative measure were consistently lower when compared against the survey measure. The implication of this became evident in section 4.7 where characteristics of sub-minimum and minimum wage workers were compared. When using the administrative data, a larger proportion of sub-minimum wage workers was observed when compared to the survey sample.

Missing hours of work information in the administrative data set a clear boundary for understanding the types of empirical analyses that can be undertaken. Specifically, imputation of hours of work information from the HLFS-IS into the administrative sample likely resulted in measurement error which creates bias in any empirical estimations. Unfortunately, this limits the types of descriptive analyses that can be undertaken and limits the range of outcomes that can be empirically assessed. However, should the need for hours of work data arise (e.g., where no effects on employment of minimum wage increases are observed), observations from the descriptive analyses in this chapter suggests that a standalone and comparable survey sample could be used. Such a sample could support empirical analyses by providing supplementary insights.

Chapter 5 Effects of Minimum Wage Increases on Teenage Employment: Survey Versus Administrative Data

This thesis aims to empirically examine the effects of minimum wage increases on teenage employment in NZ. This chapter contributes to that aim by presenting empirical estimates of the effects of the 2001 minimum wage reform on teenage employment. To support this aim, several objectives are set:

- reproduce existing NZ minimum wage research using survey data; and
- present empirical estimates of the effects of the 2001 minimum wage reform on teenage employment using DID in combination with individual-level administrative data not previously exploited in the existing NZ literature.

Taking a two-step approach, the identification strategy utilised in this chapter is confirmed by reproducing existing survey-based evidence using individual-level administrative data. The second step empirically estimates the impacts of minimum wage increases and found no evidence of adverse impacts on teenage employment. However, findings must be interpreted with some caution due to concerns with a key identification assumption.

This chapter is organised as follows: Section 5.1 revisits survey data used by Hyslop and Stillman (2007) to trial and adopt an existing DID regression model. Administrative data are introduced to the empirical analysis in section 5.2. This section examines sample characteristics and outcome trends and presents simple DID employment estimates as well as causal estimates of the impact of the 2001 minimum wage impact on employment. Section 5.2 concludes by conducting tests for parallel trends. In the final section of this chapter, the empirical findings are related to existing minimum wage literature, followed by a discussion on policy and research implications.

5.1 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 using survey data. 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 (35% over 2001/02) and 18-19- (68%) year-olds 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 at the time, their paper was influential in progressing the body of knowledge by presenting alternative conclusions with respect to the minimum wage and employment relationships.

In order to adopt the identification strategy from Hyslop and Stillman (2007) in step two, their empirical results need to be replicated. Although replication of empirical studies can be considered as a valuable post-publication quality check, or even beneficial in the pre-publication peer review process, evidence of replications in economics is sparse (Mueller-Langer, Fecher, Harhoff, & Wagner, 2019). Part of the explanation could lie in the lack of interest to pursue replication, or that major errors in published empirical studies are rare (Hamermesh, 2007), or because of potential publication bias (De Long & Lang, 1992).

Additionally, there is no consensus on the concept of replication and what it entails. For example, McCullough (2009) and Vinod (2009) only considered studies as replications if the same regression specifications and data sets were utilised (i.e., no alterations were made from the original study). In contrast, Pesaran (2003) and Hamermesh (2007) considered replication studies to also include those which adjusted the research design of the original study as part of the replication process. In an attempt to bring consistency in how replication studies are labelled and what they entail, Clemens (2017) introduced a standardised framework classifying studies as either replication (i.e., verification or reproduction) or robustness (i.e., reanalysis or extension).[91] Similarly, Reed (2017) defined six kinds of replication studies based on their choice of the measurement, analysis, data, and population utilised. Within the context of this study, replication is defined as a reproduction, which is "the act of attempting to duplicate the findings from an original study" (Reed, 2017, p. 4).

This reproduction analysis only focussed on the effects of the 2001 minimum wage policy reform on teenage employment reported by Hyslop and Stillman (2007). In attempting to

[91] This standardised framework offers criteria to classify studies as a type of replication or robustness study depending on the sample, population and specification utilised relative to the original study.

successfully reproduce those results, this study followed as closely as feasible the methods documented in their paper, including data sources, sample selection and regression specifications.[92]

5.1.1 Data and sample characteristics.

Data were taken from the HLFS and restricted to a sample population of 16-25-year-olds over the period 1997q1-2003q3. Table 5.1 presents a selection of characteristics from the sample. Following the approach of Hyslop and Stillman (2007), the characteristics for two distinct sub-groups were included: i) the full sample, and ii) a wage and salary worker sample.

For the purpose of comparison, the corresponding sample characteristics reported by Hyslop and Stillman (2007) were also included in Table 5.1. Compared to the original sample, the characteristics from the reproduced sample were largely consistent. The small differences in characteristics, particularly those relating to the distribution of ethnicity, could be due to alternative approaches in defining variables.[93]

The characteristics of the full sample indicated an even gender mix, with approximately one-fifth of the sample being 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.[94] 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.

[92] The authors also kindly provided some guidance and several code files to assist in this process.
[93] For example, no information was reported by Hyslop and Stillman (2007) regarding whether ethnicity dummies were based on prioritised ethnicity. With respondents able to report multiple ethnicities in the HLFS, this study adopted a prioritisation framework following the approach by Meehan et al. (2019).
[94] Non-prioritised ethnicity dummies were also created by way of robustness checks. In these instances, the proportion of Pākehā increased to 70%, with Māori decreasing to 14%. The distribution of the remaining ethnicities remained unchanged. Regression results from specifications (8).6) and (8).7) in Table 5.5 were robust to the use of non-prioritised ethnicity dummies.

Table 5.1:
Sample Characteristics from the Reproduction Analysis

	Hyslop and Stillman (2007)[a]		Replicated 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).

5.1.2 Regression results.

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

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

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. Table 5.2 outlines how each of the regression specifications in the original study was defined. 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.

Table 5.2:
Definitions of Regressions Estimated by Hyslop and Stillman (2007)

Controls	Description	Specification						
		(8.1)	(8.2)	(8.3)	(8.4)	(8.5)	(8.6)	(8.7)
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). Results from specifications (8.1)-(8.5) are presented in Table 5.3, that of specifications (8.6)-(8.7) in Table 5.5. Author's compilation.

The regression results for specifications (8.1)-(8.5) are presented in Table 5.3. Overall, the empirical results from these specifications were generally consistent with those reported by Hyslop and Stillman (2007).

The results from specification (8.1), which controlled for a range of covariates, showed evidence of small negative impacts on the employment of 16-17 and 18-19-year-olds following the 2001 minimum wage policy change, although not statistically significant.

The addition of a control for spillover effects in specification (8.2) made little difference on the point estimates for teenagers. Both 16-17 and 18-19-year-olds appeared to be less likely to be employed following the minimum wage policy change, although the point estimates were not statistically different from zero. For 20-21-year-olds, the results were indicative of positive spillover effects, though again not statistically significant.

The addition of controls for proxy responses in specification (8.3) resulted in point estimates which were smaller for 16-17 and 18-19-year-olds, and marginally larger for 20-21-year-olds; none were statistically significantly different from zero.

Table 5.3:

Estimated Employment Impacts from the Reproduction Analysis

	Specification				
	(8.1)	(8.2)	(8.3)	(8.4)	(8.5)
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.

Separate year effects were controlled for in specification (8.4), which allowed the impact of the minimum wage policy change to vary across the three post-reform years.[95] Overall, the regression results were mixed for all age groups. Positive spillover effects were observed for 20-21-year-olds in 2001 ($\delta_{20\text{-}21} = 0.031$, $p < 0.05$). These results indicated that in 2001, 20-21-year-olds were 3.1 percentage-points more likely to be employed immediately following the minimum wage policy change, holding all other factors constant. However, as stated by Hyslop and Stillman (2007), these results must be interpreted with caution given the absence of direct disemployment effects for 18-19-year-olds in the same year.

The results from specification (8.5), which controlled for announcement effects were also mixed for all three age groups, with most point estimates statistically insignificant. Evidence of weak disemployment effects for 18-19-year-olds was observed for 2003 ($\delta_{18\text{-}19} = -0.032$, $p < 0.10$). These results indicated that by 2003, 18-19-year-olds were 3.2 percentage points less likely to be employed following the minimum wage policy change, holding all other factors constant.

The regression results for specifications (8.6) and (8.7) are closely examined in the next section where the reproduction results are evaluated against those from the original study. Overall, both specifications indicated negative effects on employment across all three age groups. However, all results were statistically insignificant and thus provided no evidence of adverse effects on employment from the minimum wage policy change.

5.1.3 Evaluation of reproduction results.

Specifications (8.6) and (8.7) were selected as input to this evaluation,[96] with the regression results from both the original and reproduction analyses presented in Table 5.5. Overall, the regression results from the reproduction analysis found no evidence of the minimum wage policy change affecting employment. These results were comparable to those reported by Hyslop and Stillman (2007), and thus the reproduced empirical models were deemed as delivering results consistent with those of the original study.

[95] This is an important addition to the regression model. First, the policy change phased in two moderate minimum wage increases for 16-17-year-olds (March 2001 & March 2002), and one substantial increase in March 2001 for 18-19-year-olds. Therefore, there could likely have been immediate and/or delayed effects on employment for 16-17-year-olds, and immediate effects for 18-19-year-olds.

[96] These two specifications were selected as they were presented as preferred specifications by Hyslop and Stillman (2007). Their reasoning for presenting two sets of results was that business-cycle effects are important in the employment outcome, but not necessarily in other outcomes. Thus, presenting two specifications provides a trade-off between tailoring specific specifications to individual outcomes compared to imposing common specifications on each (Hyslop & Stillman, 2007).

Key to undertaking the evaluation of the reproduction results is to clearly define how they were evaluated against those from the original study. As noted earlier, studies which re-examine existing empirical evidence can be defined in multiple ways, which creates ambiguity regarding how the outputs of such studies should be evaluated. That is, how does one determine the success or failure of a reproduction study?

Recent work by LeBel, Vanpaemel, Cheung, and Campbell (2019) presented a brief guide for evaluating replication studies.[97] 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,[98] 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. There are several nuances inherent to classifying empirical results as consistent or inconsistent, which are summarised in Table 5.4.

Table 5.4:
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.

To evaluate the outcome of the reproduction analysis, the approach by LeBel et al. (2019) was adopted given the clear framework it presented for determining the consistency of the reproduction results against those from the original study. To apply the framework, the reproduced empirical results from specifications (8.6) and (8.7) were evaluated

[97] 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.
[98] 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.

against those reported by Hyslop and Stillman (2007) for the same specifications, presented in Table 5.5.

Table 5.5:
Evaluation of Regression Results from the Reproduction Analysis

	Hyslop and Stillman (2007)[a]		Reproduction	
	(6)	(7)	(8.6)	(8.7)
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).

For specification (8.6), Hyslop and Stillman (2007) only reported a statistically significant point estimate for 20-21-year-olds in 2001; 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.

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. Based on these observations, although no signal was 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 (8.7) were largely the same as specification (8.6). That is, Hyslop and Stillman (2007) only reported one statistically significant point estimate for 16-17-year-olds in 2003. 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. 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 (8.7) were also classified as consistent with those from the original study.

Overall, the original study observed no evidence of immediate adverse effects on teenage employment following the minimum wage policy change (depending on specification), with some weak evidence of negative outcomes by 2003. The reproduced empirical model generated results which supported these findings. This evaluation was reinforced after applying the framework presented by LeBel et al. (2019), which concluded that the empirical results from specifications (8.6) and (8.7) were consistent with those reported by Hyslop and Stillman (2007). Consequently, the empirical specifications trialled in this reproduction analysis were deemed suitable for application in the next step of this study.

5.2 Examining the Effects on Employment Using Administrative Data

Although the effects of minimum wage increases on employment have been previously examined (Chapple, 1997; Hyslop & Stillman, 2007, 2021; Maloney, 1995, 1997; Pacheco, 2011; Pacheco & Maloney, 1999), unlike the international literature which has moved towards administrative data (Böckerman & Uusitalo, 2009; Kabátek, 2021; Kreiner et al., 2019; Liu, Hyclak, & Regmi, 2016; Pereira, 2003; Thompson, 2009) NZ studies have largely relied on survey data from the HLFS.

However, administrative data that have become available through Stats NZ's IDI provides an opportunity to empirically examine the effects of minimum wage increases on employment from an alternative standpoint. Having already established an empirical model in step one of this study, step two contributes to the NZ empirical literature by utilising a population sample constructed from individual-level administrative data to empirically examine the impact of the 2001 minimum wage policy reform on the employment of 16-17 and 18-19-year-olds.

5.2.1 Data and sample characteristics.

Data were taken from several sources in the IDI to construct an administrative sample population. The sample population was 16-25-year-olds over the period April 1999-September 2003.[99] The final sample consisted of 29,442,717 person-month observations, which were comprised of 889,989 unique individuals.

Data sources included a range of data that forms part of the core data in the IDI,[100] as well as the DIA and IRD. The full end-to-end process for constructing the administrative sample population is detailed in Chapter 4.

To measure employment, monthly wage and salary records were taken from the IRD EMS. Demographic characteristics, such as gender and ethnicity, were measured using the personal details information from the IDI. Other demographics such as marital status and country of birth were measured using data from DIA marriages and birth records, respectively. Characteristics pertaining to region were measured using address notification information from the IDI, with urbanicity measured using post-codes sourced from NZ Post. Definitions, data sources and limitations of all variables utilised in these empirical analyses are presented in Appendix C, Table C1.

Mean values of employment and individual characteristics of the administrative sample are presented in Table 5.6. 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%.

[99] This sample period was shorter than the period from the survey reproduction analysis; however, this period was restricted due to the availability of IRD EMS data.
[100] These data are available to all IDI users with approved research projects Stats NZ (2016).

Table 5.6:
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)
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.

5.2.2 Review of employment trends.

To begin the analysis, employment trends by age group were visually examined over the sample period. This examination was able to provide a valuable starting point in gathering insight into whether the 2001 minimum wage policy reform impacted the employment of teenagers.

Figure 5.1 depicts mean employment for 16-17, 18-19 and 20-25-year-olds over the period April 1999-September 2003. The two 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 their real minimum wage by 68%. The youth minimum wage increased to 70% and 80% of the adult minimum wage in April 2001 and April 2002, respectively.

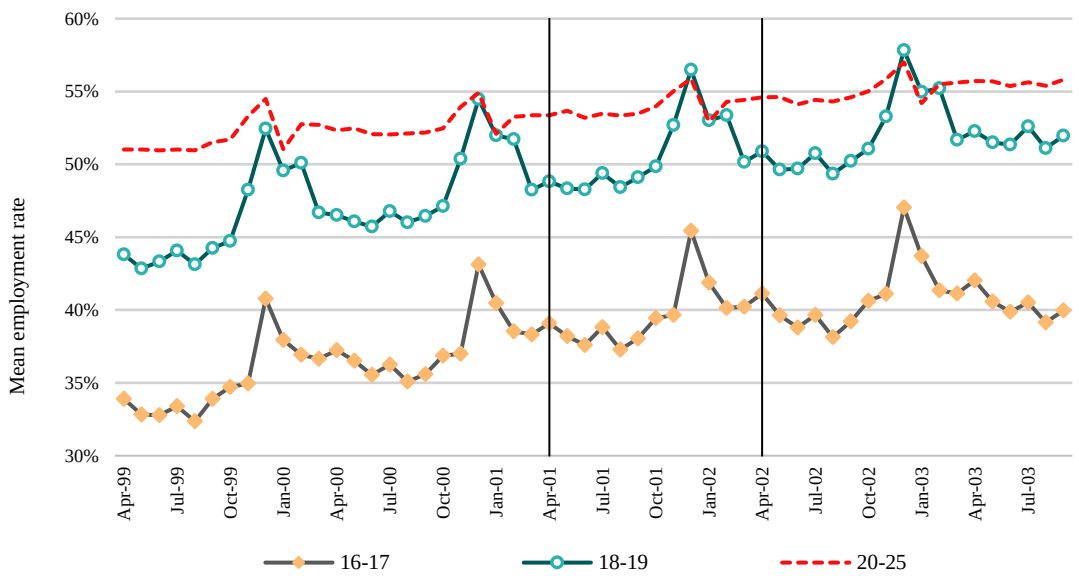


Figure 5.1: Employment rate by age groups.
The sample covers the period April 1999-September 2003. Values on the y-axis start at 30% to clearly highlight the variation in the employment rates over time. Author's compilation.

The employment rate of 16-17-year-olds increased by approximately 6 percentage points between April 1999-September 2003. Their employment was seasonal, with clear increases occurring in November and December (averaging around 6 percentage points). To visually assess the impact of minimum wage increases, employment trends were examined around the times when policy changes occurred. In the period prior to the first phase of the policy change (i.e., April 1999-March 2001), the employment rate of 16-17-year-olds increased from 34% to 38%. In the period between April 2001-March 2002 (following the first phase of the policy change), the employment rate of 16-17-year-olds increased by approximately 1 percentage point. In the final period, April 2002-September 2003 (following the second phase of the policy change), the employment rate of 16-17-year-olds decreased from 41% to 40%. These trends suggest that the rate of their employment growth slowed following the periods of minimum wage increases, relative to the changes in their employment prior to 2001.

The employment rate for 18-19-year-olds also saw a moderate growth over the sample period, increasing from 44% in April 1999 to 52% in September 2003. As with 16-17-year-olds, seasonal employment trends were observed, with increases averaging around 7 percentage points occurring annually between October and December. In the period

prior to the first phase of the policy change, the employment rate of 18-19-year-olds increased from 44% to 48%. In the period between April 2001-March 2002, the employment rate of 18-19-year-olds increased by 1 percentage point. As with the younger group of teenagers, the trends observed here suggest that the rate of employment growth slowed following the period when the initial minimum wage increase occurred, relative to the changes in their employment prior to 2001.

The employment rate of 20-25-year-olds increased by approximately 5 percentage points between April 1999-September 2003. Seasonal employment trends were also observed for this age group, with increases averaging around 4 percentage points occurring annually between October and December. These seasonal effects were expected, as they were likely driven by individuals still enrolled in tertiary education, and thus depicting the effect of holiday employment. With the minimum wage policy changes directly targeting teenagers, the employment of 20-25-year-olds should not have been impacted. This was confirmed by their employment rate remaining relatively stable at 53% over the periods following the minimum wage policy changes.

The visual examination of employment trends highlighted that the rate of teenage employment growth slowed following the periods of minimum wage increases. However, strong seasonal employment for both 16-17 and 18-19-year-olds generally increased employment between the individual phases of the minimum wage policy reform. Consequently, these dynamics could make it difficult to isolate the effects on employment of the minimum wage increases.

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

The analysis continues with a simple DID estimation to initially examine the impact of the 2001 minimum wage reform on teenage employment. Table 5.7 presents the average employment outcomes over the pre-treatment (April 1999-September 2000) and post-treatment period (April 2001-September 2003) for 16-17, 18-19 and 20-25-year-olds.[101] 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.

[101] 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).

Table 5.7:
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]

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. This simple DID analysis estimated positive effects on the employment of both treatment groups following the 2001 minimum wage reform. Although insightful, more comprehensive empirical analysis is required to account for other factors which may have impacted their employment over the sample period.

5.2.4 Difference-in-differences regressions results.

The following base specification was adopted to estimate the effects of the 2001 minimum wage reform on teenage employment:

$$Y_{it} = \delta_{16-17} * (age16-17_{it} * Post-2001) + \delta_{18-19} * (age18-19_{it} * Post-2001) + X'_{itj}\beta + u \quad (9)$$

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 (9) was largely the same as specification (8) used in the reproduction analysis in section 5.1, there are differences to note with respect to definitions of variables under the vector X_{itj} (e.g., married & ethnicity dummies).[102] Furthermore, as in the reproduction analysis, several iterations of specification (9) were estimated. However, a key difference between the specifications estimated here, and those estimated in the survey reproduction analysis (outlined in Table 5.2), is that no proxy controls were added due to individuals not being identifiable in the IDI.[103] 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 every facet of the estimation procedure, differences are inevitable due to the nature and purpose of household survey data and sources of administrative data.[104]

The regression results from all specifications are presented in Table 5.8. Based on the results from specification (9.6), the fullest specified regression, the 2001 minimum wage policy reform had small, positive, and statistically significant impacts on the employment of 16-17 and 18-19-year-olds, as well as negligible positive effects on the employment of 20-21-year-olds. These regression results for 16-17 and 18-19-year-olds were also largely consistent with the simple DID estimated average treatment effects presented in Table 5.7.

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, $p < 0.01$). That is, 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.

[102] 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 (9), 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'.
[103] 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.
[104] 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 5.8:

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

	(9.1)	(9.2)	(9.3)	(9.4)	(9.5)	(9.6)
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 (9.2) 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 ($\delta_{18\text{-}19} = 0.007$, $p < 0.01$).

Specification (9.3) 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. That is, 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 (9.4) controlled for announcement effects of the minimum wage reform prior to implementation, as well as announcement effects for 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 (9.5) 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 (9.6) controlled for age-specific business cycle effects. Here, the regression results were consistent with those from specification (9.5), 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 (9.5), 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 there was no adverse impact from the 2001 minimum wage reform 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. The evidence for 20-21-year-olds was mixed, with no empirical support for the spillover effects hypothesis. These effects were somewhat confusing since no negative effects on the of 18-19-year-olds were observed, thus negating the substitution of 18-19-year-olds for 20-21-year-olds as a possible explanation.

Further to these observations, the empirical results should still be interpreted with some caution, as the key assumption of parallel trends, underpinning identification using DID, is yet to be tested. Consequently, before drawing any conclusions regarding the relevance and implications of these results for policy, several tests examining the parallel trends assumption are undertaken in the next section.

5.2.5 Examining the parallel trends assumption.

The credibility of the causal estimates presented in Table 5.8 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. In this section, visual inspections of parallel trends were undertaken, followed by formally examining parallel trends by way of regression analyses. As a final check, parallel trends for alternative treatment and control groups were examined to test whether any computational errors were influencing the formal regression results.

Starting with visual inspections, Figure 5.2 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.

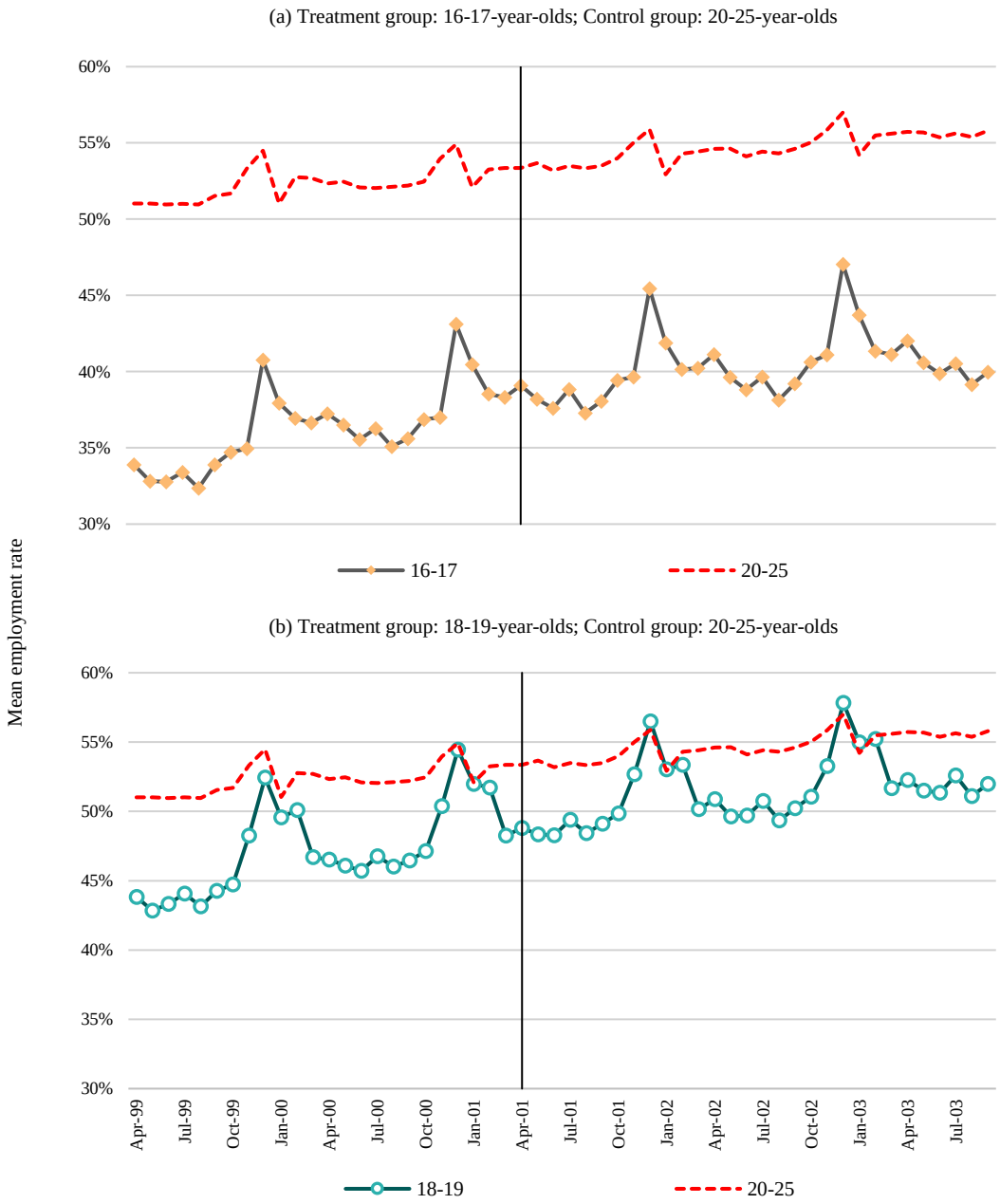


Figure 5.2: Pre-treatment employment levels for 16-17, 18-19 and 20-25-year-olds.
The sample covers the period April 1999-September 2003. The pre- and post-treatment periods are April 1999-September 2000 and April 2001-September 2003, respectively. Values on the y-axis start at 30% to clearly highlight the variation in the pre-treatment employment levels. Author's compilation.

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.

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.

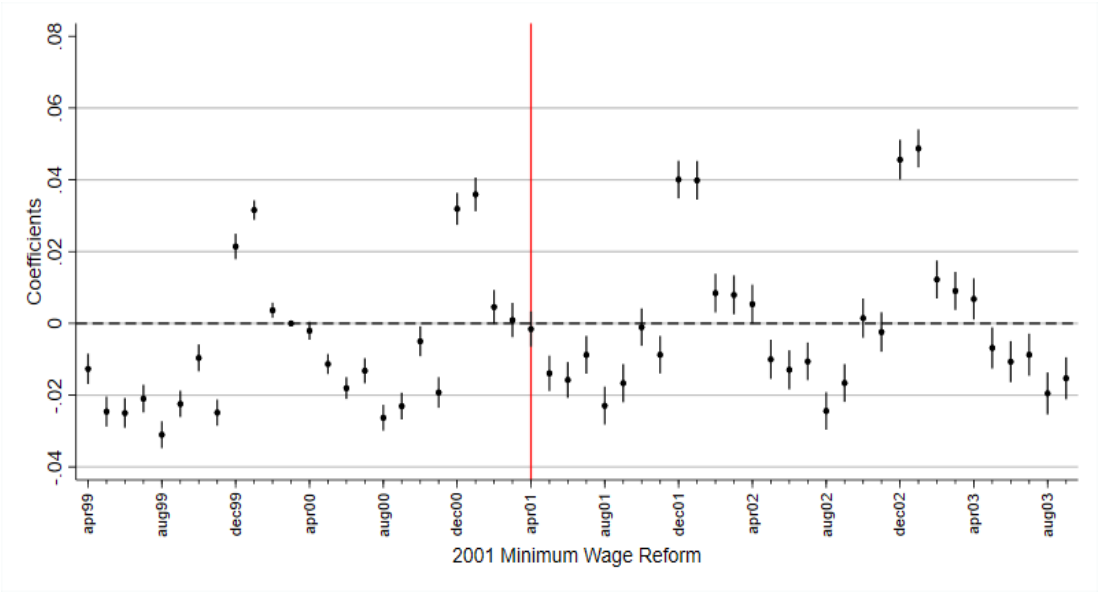
When considering differences in the employment trends and seasonality of 18-19 and 20-25-year-olds, no constant difference in employment was observed. Consequently, this visual inspection yielded no evidence in support of parallel trends.

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 (9.6).[105] 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 5.3. To accept the null hypothesis of parallel trends in employment, the coefficients in the pre-treatment observation 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.

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 (Figure 5.3, 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.

[105] Parallel trends were also tested using alternative regression specifications for both sets of treatment and control groups. These plots are presented in Figure C1 for 16-17-year-olds and 20-25-year-olds, and Figure C2 for 18-19 and 20-25-year-olds. In all cases, the null hypothesis of parallel trends was rejected.

(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

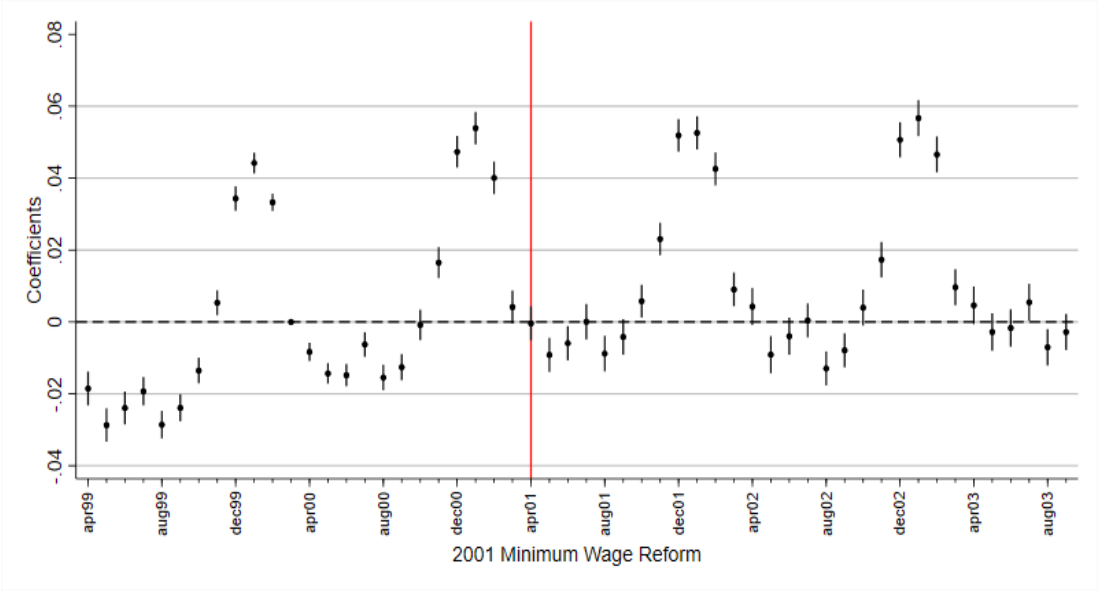


Figure 5.3: Formally testing for parallel trends in employment.
The sample covers the period April 1999-September 2003. All specifications are estimated by OLS on 23,462,325 observations for 16-17 and 20-25-year-olds, and on 23,442,792 observations for 18-19 and 20-25-year-olds. Huber-White robust standard errors included. These were clustered by UID, allowing for correlation within UID, but independence between UIDs. Author's compilation.

In panel (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.[106] 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

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

observed in the visual examination (Figure 5.3, panel [b]). Consequently, 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.

The first variation selected a group of individuals who were treated in the 2001 minimum wage policy reform. This sample was made up of 17-year-olds, where half of the sample were randomly assigned to a treatment group, with the remainder acting as the control group. As in the formal testing, the coefficients on the interaction between the treatment group and months were plotted, along with their associated 95% confidence intervals, presented in Appendix C, Figure C3. Prior to March 2000, the 95% confidence intervals associated with the coefficients included the null value, zero, and were thus not statistically significantly different from zero. These results suggest that there were constant differences in employment of the treatment and control groups. Consequently, the employment trend of the control group provided a suitable counterfactual for the treatment group in the absence of the minimum wage policy reform. Based on these considerations, the null hypothesis of parallel trends was accepted.

The second variation selected a group of individuals who were not treated in the 2001 minimum wage policy reform. This sample was made up of 24-year-olds, who were assigned to the treatment group, and 25-year-olds who were assigned to the control group. By all accounts, these individuals are similar in labour market characteristics and as such would be expected to have similar employment outcomes. Again, the coefficients on the interaction between the treatment group and months were plotted, along with their associated 95% confidence intervals, presented in Appendix C, Figure C4. Prior to March 2000, the 95% confidence intervals associated with the coefficients included the null value, zero, and were thus not statistically significantly different from zero. These results suggest that there were constant differences in the employment of 24 and 25-year-olds. Consequently, the employment trend of 25-year-olds provided a suitable counterfactual

for 24-year-olds in the absence of the minimum wage policy reform. Based on these considerations, 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 of these 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.

5.3 Conclusion and Implications

There were two key objectives set for this chapter. The first objective was to reproduce existing NZ minimum wage evidence using survey data. The next objective was to present empirical estimates of the effects of the 2001 minimum wage reform on teenage employment using DID in combination with individual-level administrative data not previously exploited in the existing NZ literature.

Although the effects of minimum wage increases on employment have been examined (Chapple, 1997; Hyslop & Stillman, 2007, 2021; Maloney, 1995, 1997; Pacheco, 2011; Pacheco & Maloney, 1999), 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 largely relied on survey data from the HLFS. The aim of this chapter, 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. To achieve this aim, 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. These results were evaluated and deemed as being consistent with those reported in the original study. This 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 consistent empirical results relative to the original study, was that step two of the analysis commenced with confidence in the interpretation of the adopted regression specifications.

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 5.7, 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 in this analysis, 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.

5.3.1 Policy implications.

With the causal estimates from the regression models potentially biased, caution should be applied when interpreting these findings for the purpose of policy. However, several observations stand out. First, under the assumption that these effects were not over-estimated, there was 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 employment, or groups of workers such as teenagers, women, or Māori, where aggregate impacts on employment are negligible (Ministry of Business Innovation & Employment, 2019a). Consequently, the findings from this study may require policy makers to broaden the scope of the potential teenage employment outcomes considered as part of their policy decisions.

5.3.2 Research implications.

The use of administrative data in this application resulted in several observations. First, the analyses under section 5.2 clearly highlight the value of administrative data in empirical research on the effects of minimum wage on employment. Particularly, the level of aggregation stands out as a key benefit.

The administrative sample was set at a monthly-level as opposed to a quarterly-level in the case of the survey sample from the HLFS. This lower aggregation was advantageous when examining the employment trends of 16-17 and 18-19-year-olds as shown in Figure 5.1, where strong and distinct seasonal fluctuations were observable for each treatment group. The benefit of the administrative data was particularly 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 as shown in Figure 5.2 and Figure 5.3, which supported the conclusions from the hypothesis testing of parallel trends.

5.3.3 Implications for further research.

One of the contributions of this chapter is to address the lack of administrative evidence on minimum wages and employment in the NZ empirical literature. Overall, this chapter makes that contribution by exploiting an individual-level administrative sample to estimate the impact of the 2001 minimum wage reform on teenage employment. However, true effects on employment remain uncertain due to concerns relating to the validity of the parallel trends assumption critical to the identification strategy. Consequently, to generate empirical results that could provide a basis for policy decisions, further research on the effects of minimum wage increases on teenage employment may consider an alternative identification strategy. To this end, the next chapter in this thesis will provide additional empirical evidence by adopting RD as an alternative quasi-experimental identification strategy to estimate the causal impact of the 2001 minimum wage reform on teenage employment.

Chapter 6 Effects of a Minimum Wage Discontinuity on Employment at Age 18

This thesis aims to empirically examine the effects of minimum wage increases on teenage employment. To support this aim, the following objective is set:

- Utilise a RD strategy not previously employed in the NZ literature, in combination with individual-level administrative data.

Arguably, RD has not been adopted in previous NZ empirical minimum wage studies, as administrative data, which facilitates the method through the provision of large samples, was not readily available at the time of those studies. As noted by Cunningham (2021), RD requires a great deal of data around a discontinuity, which in itself implies that data sets used in RD studies are very large and typically constructed from administrative sources such as birth records. In contrast, survey data from the HLFS which features strongly in existing NZ empirical minimum wage literature, is made up of a small representative NZ sample population, which by design is unable to offer the same quantity of observations around a discontinuity that an administrative sample could. Fortunately, utilising RD to empirically examine the impacts of minimum wage increases on teenage employment is now possible with the use of individual-level administrative data in the IDI.

Exploiting a large discontinuity in the minimum wage at age 18 between 2001-2008, the main empirical results from this chapter reveal that there were no statistically significant effects on the employment of 18-year-olds NZ born workers. Supplementary descriptive analysis identifies potential explanations for these results.

This chapter is organised as follows: Section 6.1 details the NZ minimum wage policy background motivating this study, with section 6.2 describing the research methodology adopted in this chapter along with potential threats to the identification strategy. Section 6.3 summarises the administrative data sources and characteristics of the main analysis sample, with visual inspections and formal estimation of the impact of a minimum wage age discontinuity at 18 on earnings and employment undertaken in sections 6.4 and 6.5. Section 6.6 introduces a survey sample to examine potential explanations for the main empirical results, with section 6.7 discussing a range of explanations of the results, and exploring the results in relation to existing empirical evidence and theoretical models of the labour market. Section 6.8 concludes this chapter.

6.1 NZ Minimum Wage Policy Background

NZ provides an excellent case study for empirically investigating the impact of higher minimum wages on labour market outcomes such as earnings and employment given its long history of enforceable minimum wages and periods of substantial variation in minimum wage rates. Prior to the minimum wage reform in 2001, both 16-17 and 18-19-year-olds were covered by the youth minimum wage which was set at 60% of the adult minimum wage rate; minimum wage workers aged 20 or above were covered by the adult minimum wage. The 2001 minimum wage reform lowered coverage of the adult minimum wage to 18 years; 16-17-year-olds were still covered by the youth minimum wage set at 70% and 80% of the adult minimum wage in March 2001 and March 2002, respectively. These statutory minimum wages remained in place until April 2008, when coverage of the adult minimum wage was lowered to some 16-17-year-olds.[107] According to Minister of Labour Margaret Wilson, lowering the age threshold for the adult minimum wage to 18 years was to "ensure that low-paid young people receive wages that are fair" (New Zealand Government, 2000, para. 7) and brought "the age of eligibility for adult rates into line with other policies for youth, particularly the community wage and New Zealand's obligations under the United Nations Convention on the Rights of the Child" (New Zealand Government, 2000, para. 8).[108]

The changes to statutory minimum wages resulting from the 2001 minimum wage reform, which were in effect until March 2008, are examined in this chapter. In terms of variation in minimum wage rates, 16-17-year-old minimum wage workers saw an increase totalling approximately 37% in hourly wages over a two-year period, with their statutory wage rates increasing from $4.55 to $5.40 in March 2001, and then to $6.40 in March 2002. For 18-19-year-old minimum wage workers, moving from the youth to adult minimum wage increased their statutory minimum wage rate by 69% (from $4.55 to $7.70 in March 2001). Figure 6.1 plots the mean youth and adult statutory minimum wage rate by age over the period 2001-2008. As can be observed, over the period there was a clear discontinuity in statutory minimum wage rate entitlements, averaging 27%, once a minimum wage worker turned 18 years old.

[107] Changes to statutory minimum wages are detailed in Chapter 2.

[108] At the time, the Community Wage was an income support payment for job seekers in return for participating in community based work with the aim of supporting community or environmental focussed projects (New Zealand Government, 1998).

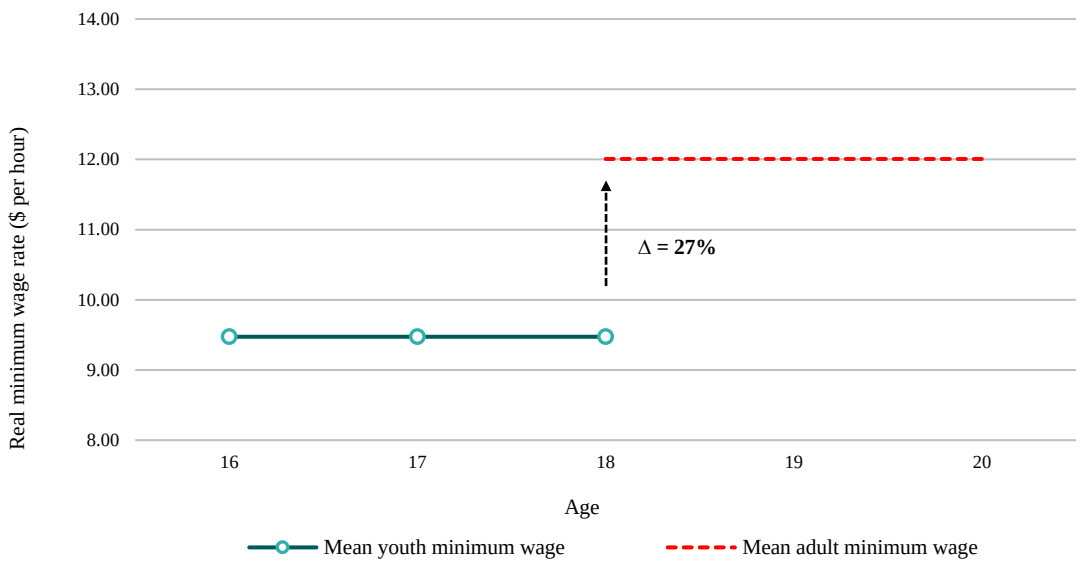


Figure 6.1: Mean minimum wages entitlements by age in years, 2001q2-2008q1.
Data on nominal minimum wages and the CPI are from Ministry of Business Innovation & Employment (n.d.a) and the Reserve Bank of New Zealand (2021). Nominal minimum wages adjusted using the CPI (base month/year = March 2017). Values on the y-axis start at $8.00 per hour to clearly highlight the jump in the minimum wage payable at age 18. Author's compilation.

This discontinuity in the minimum wage at age 18 offers a unique opportunity to examine the causal effect of a higher minimum wage on the labour market outcome for 18-year-olds. Utilising this age discontinuity has yet to feature in the NZ minimum wage empirical literature, which has largely focussed on the overall impact of minimum wage policy reforms (Hyslop & Stillman, 2007, 2021; Pacheco, 2011). This analysis will therefore exploit the age discontinuity in minimum wages to empirically examine the impact of a higher minimum wages on the earnings and employment outcomes of 18-year-old workers in NZ.

In the next section, an overview of the research methodology will be presented, outlining how this discontinuity in minimum wages will be utilised in forthcoming empirical analyses.

6.2 Research Methodology

6.2.1 Overview of identification strategy.

To empirically examine the impact of a discontinuity in the minimum wage at age 18, this empirical analysis adopted a RD approach. RD is a quasi-experimental method used to estimate programme impacts where treatment is determined by whether the observed

value of an assignment variable exceeds a designated threshold or cutoff.[109] In its first application by Thistlethwaite and Campbell (1960), RD was used to examine the impact of merit awards on future academic outcomes. With treatment being based on an observed test score, Thistlethwaite and Campbell (1960) noted that individuals just below the cutoff (those who were not treated/did not receive a Certificate of Merit) were good comparisons to those just above the cutoff (those who were treated/did receive a Certificate of Merit). The sharp discontinuity in the relationship between the observed test score and future academic outcomes (e.g., seeking a MD or PhD degree) was taken as the causal effect of the treatment.

Nonetheless, despite its long history, RD only gained popularity in economics during the 1990s (Angrist & Lavy, 1999; van der Klaauw, 2002), with its estimation procedures being formalised (Hahn, Todd, & Van der Klaauw, 2001) and further developed (Imbens & Kalyanaraman, 2012). More recently, to provide a summary of best practice, and dispel any misconceptions of implementing RD design, 'user guides' have been prepared for those new to RD (Bloom, 2012; Lee & Lemieux, 2010).

RD has also been applied in empirical minimum wage studies to examine the causal effects of minimum wage rules that create discontinuities at specific age thresholds on labour market outcomes such as earnings and employment. For example, Dayioglu-Tayfur, Kucukbayrak, and Tumen (2020) examined the employment and education impact of age-specific minimum wage rates for 16-year-old workers in Turkey. Their empirical results revealed lower employment for young males. Dickens et al. (2014) studied the impact of a large increase, of approximately 20%, in the UK national minimum wage at age 22 and found positive effects across several labour market outcomes, whereas Fidrmuc and Tena (2018) examined both UK minimum wage discontinuities at age 18 (a 17% increase) and age 22 and observed negative effects on employment at age 18 and anticipation effects for male workers aged 21. Kabátek (2021) showed how minimum wages in the Netherlands, which increased in a step-wise structure as a function of calendar age,[110] increased job separation and had heterogenous effects on workers and across sectors of employment. Finally, Kreiner et al. (2020) observed how a large discontinuity in the Danish minimum wage at age 18, of approximately 40%, had negative impacts on employment.

[109] The assignment variable is also referred to in the literature as the 'forcing' variable or 'running' variable (Lee & Lemieux, 2010).
[110] From age 15, the applicable minimum wage increased by between 15-17% each year until age 23 (Kabátek, 2021).

There are also advantages to using the RD approach for causal analysis when compared to other estimation procedures such as DID. First, in RD design, the economy is examined under the conditions of market equilibrium and thus if there is an effect on the outcome, it represents an immediate effect. In fact, Lee and Lemieux (2010) noted that if the effect is not immediate, it will not generate a discontinuity in the outcome. In contrast, the DID approach must leverage time periods following the treatment as the economy is arguably transitioning to a new equilibrium. An implication of this is that if the effects diffuse gradually over time, it may be difficult to separate the treatment effect from the time effect due to other factors varying over the same time period (Lee, 2016).

An example of this time effect from the NZ empirical literature is the study by Hyslop and Stillman (2007) who adopted a DID approach. They observed no immediate effects on teenage employment but some weakly significant negative effects occurring two years following the 2001 minimum wage reform. Within the context of the NZ legislative landscape, finding no immediate effects on employment could arguably be the result of human rights and labour market protections in place for workers. That is, if the employment of a 16-19-year-old was terminated because of the higher statutory wage entitlements introduced by the 2001 minimum wage reform, such an action would likely be deemed as a breach of sections 21-22 of the Human Rights Act 1993.[111] However, this does not mean that employers are unable to respond to increases in the minimum wage. For example, as long as employers act in good faith and workers are suitably consulted, employers are able to reduce hours of work as a means to offset higher wage entitlements (Ministry of Business Innovation & Employment, 2019c).[112] In their study, Hyslop and Stillman (2007) found empirical evidence consistent with employers reducing the hours worked of 18-19-year-olds immediately following the 2001 minimum wage reform, albeit these impacts were not statistically significantly different from zero.

[111] Section 21(1)(i) of the Human Rights Act 1993 declares age to be a prohibited ground for discrimination, with section 22(1)(c) outlining human rights' breaches with respect to employment, including termination.
[112] The grounds for employers to adjust hours of work are examined in section 6.7.3.

6.2.2 Potential threats to identification.

This section describes a couple of factors commonly discussed as potential threats to RD identification. These are:

- manipulation of the assignment variable, a standard threat discussed in all RD empirical studies; and
- using age as the assignment variable, a more specific threat when examining age-based discontinuities.

Starting with the assignment variable, a standard threat discussed in all RD empirical studies is whether individuals can manipulate it. If individuals had control over it, they could manipulate their assignment in order to gain treatment. McCrary (2008) describes two types of manipulation i) partial, and ii) complete. Partial manipulation is where the assignment is under the individual's control but also has some idiosyncratic element, whereas complete manipulation is where the individual has complete control over the assignment variable (McCrary, 2008). Although there may be instances where age, the assignment variable in this study, is misreported for wage purposes, such behaviour would likely be limited. This is due to employers being legally required to record age for workers under 20 years as part of keeping accurate wage and time records.[113] Consequently, if employees intentionally misreport their age, their employment may be subject to disciplinary procedures. To formally test for manipulation of age as the assignment variable, the procedure of inspecting the continuity of baseline covariates presented by Lee and Lemieux (2010) was adopted.[114]

The second potential threat concerns instances where age is used as the assignment variable. In these instances, treatment cannot be considered as being truly random since everyone eventually observes the age where treatment occurs. Within the context of this study, all 17-year-olds would turn 18 and thus become eligible for the adult minimum wage. Dickens et al. (2014) noted that under the circumstances of age-based minimum wages, some individuals may alter their behaviour in anticipation of receiving treatment. On one hand, individuals may increase their labour supply prior to turning 18 years in order to account for any time it may take to find a job or build up relevant skills (Blundell, Francesconi, & van der Klaauw, 2011). Alternatively, firms may terminate minimum wage jobs in anticipation of having to pay teenagers higher wages upon their 18th

[113] This requirement is outlined in section 130 of the Employment Relations Act 2000 (Parliamentary Counsel Office, n.d.b).
[114] Visual evidence from the formal examination is presented in Appendix D, Figure D7, with the associated estimation results in Table D5. In all cases, no evidence of manipulation of age was identified.

birthday. In examining the impact of a large increase in the UK national minimum wage at age 22, Dickens et al. (2014) noted that labour supply adjustments resulting from anticipation was not an issue amongst workers in their sample. This study adopted the approach by Kreiner et al. (2020) and controlled for anticipation in the regression specifications outlined in section 6.5. In addition, flows in and out of employment were also examined for anticipatory behaviours.[115]

6.3 Data Sample and Characteristics

This section provides a description of the data sample, including the data sources, construction of the sample and definitions of key outcome variables. Summary statistics of individual and employment characteristics are also presented.

6.3.1 Data sample

The main sample population consisted of individuals born in NZ and who turned 18 over the period January 2003-December 2007. Individuals born in NZ were preferred as a sample population as this would likely reduce the presence of non-residents within the sample, as was the case in the descriptive and empirical analyses in Chapter 4 and Chapter 5, respectively. This sample period was selected as it covered the period where a discontinuity in statutory minimum wage rates at age 18 were present. This main sample population consisted of individual-year age cohorts for each year from 2003-2007. Each birth cohort was constructed using several data sources from the IDI and generally followed the same procedure outlined in Chapter 4, section 4.3.2. The key difference in the construction of this sample was that the removal of deceased and non-residents was based on a 24 month observation window on either side of individuals' 18th birthday, as opposed to the '12/16 rule' utilised in Chapter 4. Here, the aim was to restrict the sample to a population who were NZ residents for the full duration of the observation periods relevant to each cohort. In comparison, the aim in Chapter 4 was to construct a full resident-population including NZ born and non-NZ born. To examine the impact of a higher minimum wage at age 18 on alternative sample populations, the regression analyses were repeated using a combined NZ and non-NZ born population, in addition to a NZ born resident population constructed using the same procedures outlined in Chapter 4, section 4.3.2.[116]

[115] See Appendix D, Figure D8.
[116] Descriptive statistics for these samples are presented in Appendix D, Table D7, with estimation results in Table D8. The empirical results were consistent with those from the main empirical analysis.

The main sample population was constructed as follows. First, using year of birth information from the personal details table in the IDI and birth records in the life events data from the DIA, unique identifiers of all individuals born in NZ between 1985-1989 were extracted. Year of birth information was also utilised to assign individuals to relevant individual-year age cohorts. For example, an individual born during the calendar year of 1985 was assigned to the '2003 18-year-olds cohort' – the year in which they would observe their 18th birthday. Upon completion of the initial extraction, individuals who had left the population due to death or migration were removed.

Deceased individuals were identified using death records in the life events data from the DIA. More specifically, by using the year and month of death, individuals were identified and removed from their individual-year age cohorts if the date of their death occurred within a period starting 24-months prior to the month of their 18th birthday and ending 24-months following the month of their 18th birthday.

Individuals who had left NZ due to permanent migration or short-term visits were also identified and removed from each individual-year age cohort. Here, using border movement data from the overseas spell table (i.e., dates of arrival/departure), non-residents were identified using the '12/16 rule' as defined in Chapter 4, section 4.3.2. The key difference in the construction of this sample is that individuals were removed if they were classified as non-resident within a period starting 24-months prior to the month of their 18th birthday and ending 24-months following the month of their 18th birthday. For the reasons outlined in Chapter 4, section 4.3.2, no activity restrictions were imposed on the individual-year age cohorts due to the coverage period limitations of several data sources used by Gibb et al. (2016) and Stats NZ (2017b),[117] and the subsequent disproportionate removal of 16-17 and some 18-year-olds from the sample population.

The removal of deceased individuals and non-residents was the final step in constructing each individual-year age cohort. The steps in constructing each birth cohort are outlined in Appendix D, Table D1, with the number of unique individuals removed at each step.

Forthcoming analyses require three key measures. The first measure concerns age. To derive age, month and year of birth information from the personal details table was combined. The next two key measures relate to the outcomes of interest, being earnings and employment. To measure these outcomes, individual-level wage and salary records

[117] Specifically, MOH data coverage starts between 2002-2007, with secondary school enrolment data starting in 2007. These data are required to impose activity restrictions on each individual-year age cohort sample.

from the IRD EMS were merged by UIDs available in both the constructed sample and IRD EMS records.

Earnings were defined as real gross monthly wage or salary payments from a main job.[118] Several adjustments to earnings were made. To account for outliers, earnings data were censored between the 1st and 99th percentile of wage and salary records (excluding missing values). Next, where missing wage or salary records were observed in a reference month, a value of zero was imputed. This adjustment was required in order to retain these values in the analysis (e.g., examining the earnings impact of the minimum wage age discontinuity for all individuals, including non-employed). Appendix D, Table D2 provides details of all adjustments to monthly earnings.

Employment was defined as a dummy variable, with '1' equalling 'employed' if a wage and salary record was observed in the reference month, and '0' otherwise.[119] No adjustments to the employment measure were required.

To finalise the main analysis sample, the individual-year age cohorts were appended into a single sample. This generated a final sample population consisting of 225,612 unique individual and 11,055,036 person-month observations.

As noted in previous chapters, the key limitation of the IDI is the absence of hours worked information. In this analysis, hours of work information would be beneficial to assess the proportion of workers affected by the minimum wage age discontinuity as this could be one of the factors influencing the employment response. Additionally, hours of work information could also be utilised as an alternative outcome variable, as employers may respond to the minimum wage hike by adjusting working hours. Consequently, data from the HLFS-IS will be utilised in this chapter to provide supplementary evidence with respect to the impacts of the minimum wage discontinuity at age 18. Details on the sample construction, variable definitions and descriptive analysis are in section 6.6.

6.3.2 Sample characteristics.

Mean values of individual and employment characteristics over the period 2001-2009 are presented in Table 6.1. Standard errors are provided in parentheses.

[118] For the remainder of this section, real gross monthly wage or salary payments will be referred to as monthly earnings.
[119] An alternative definition of employment was also used as a robustness measure. This alternative employment was also defined as a dummy variable, but only equalled '1' (employed) where a wage or salary record was greater than or equal to $50 in the reference month, and '0' (not employed) otherwise.

Table 6.1:

Cohort, Individual and Employment Characteristics of NZ Born Sample

Characteristics	Mean (Standard Errors)
Cohort	
2003	.19
	(0.00)
2004	.19
	(0.00)
2005	.20
	(0.00)
2006	.21
	(0.00)
2007	.21
	(0.00)
Individual[a]	
Age	17.55
	(0.00)
Female	.48
	(0.00)
Pākehā	.59
	(0.00)
Māori	.24
	(0.00)
Pacific	.09
	(0.00)
Asian	.03
	(0.00)
Other	.03
	(0.00)
Ethnicity not specified	.02
	(0.00)
Employment	
Employed	.54
	(0.00)
Monthly earnings (real $)[b]	1,249.41
	(0.38)
Weekly earnings (real $)[c]	287.38
	(0.09)
Data	
Total unique individuals	225,612
Total observations	11,055,036

Note. The sample covers the period January 2001-December 2009. Standard errors are in parentheses. Author's compilation.

[a] Ethnicities were prioritised using the framework from Meehan et al. (2019).

[b] Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).

[c] Weekly earnings were derived by dividing monthly earnings by the number of weeks in the corresponding month.

Overall, the sample consisted of a relatively even split of cohorts, with each cohort accounting for approximately 19% to 21% of the sample. In terms of individual characteristics, the mean age of the sample was approximately 17 and a half years, with

marginally fewer females to males. The sample also consisted predominantly of Pākehā and Māori, which when combined accounted for 83% of the sample. Over half of the sample were employed, earning approximately $1,249 per month (or $287 per week). Definitions and data sources of the variables utilised in these empirical analyses are presented in Appendix D, Table D3.

6.4 Visual Inspection of Earnings and Employment Discontinuities

This section presents visual evidence to inspect for potential discontinuities in earnings and employment using the main analysis sample. In each case, visual inspections were also undertaken on the individual-year age cohorts from 2003-2007. Plots are included in Appendix D, Figure D1.

6.4.1 Effect of a minimum wage discontinuity on earnings.

Figure 6.2 plots average earnings of all individuals by age, in months relative to workers' 18th birthday. Age, the assignment variable, was divided into 49 bins, consisting of 24 months leading up to workers' 18th birthday, the month workers observed their 18th birthday (i.e., highlighted by the vertical line), and 24 months following workers' 18th birthday. The average value of real monthly earnings is plotted against the mid-point of each bin. The black lines represent fitted polynomials.

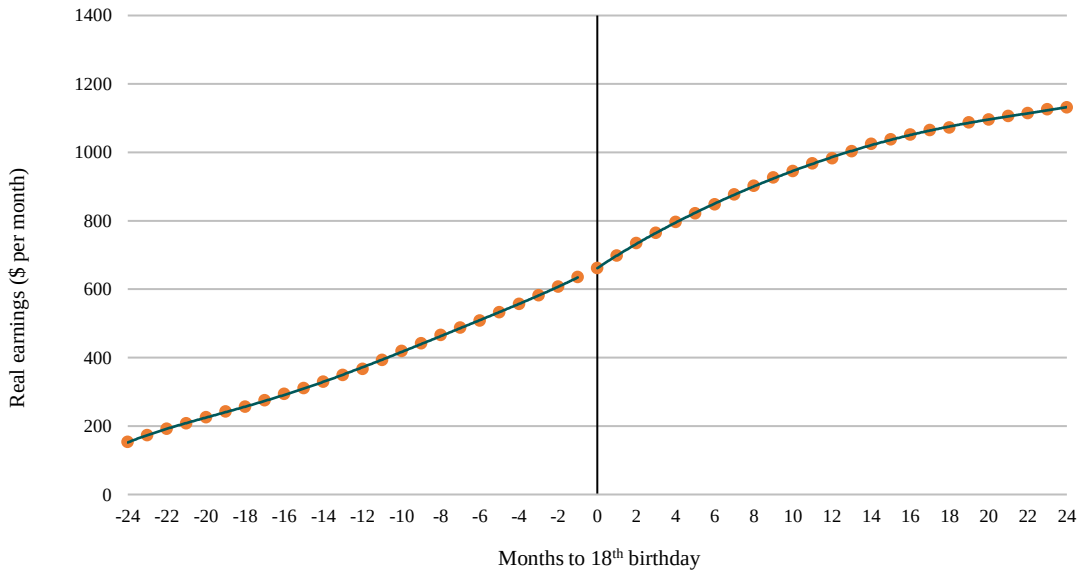


Figure 6.2: Monthly wage and salary earnings around workers' 18th birthday.
The sample covers the period January 2001-December 2009. Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021). Values on the y-axis start at $200 per month to clearly highlight any variation in the real average earnings over the months relative to workers' 18th birthday. Author's compilation.

Examining the relationship between earnings and age is important as any impact may be a driving mechanism by which effects on employment may be observed (Dickens et al., 2014). That is, if the trends in average monthly earnings indicate no discontinuity, it may signal that employment adjustments were made to offset the higher wage costs. Such employment adjustments could be in the form of terminating jobs, or alternatively be combined with other responses such as reducing hours of work. In examining Figure 6.2, a steady increase in average earnings is visible in all months leading up to workers' 18th birthday. More notably, there is no visual evidence of discontinuity in average monthly earnings.[120] Such results imply an earnings elasticity of -1, which is substantial and would require a full offset of the wage increases associated with the minimum wage age discontinuity. Consequently, discontinuity in average employment is anticipated as this would support the results observed in Figure 6.2.

6.4.2 Effect of a minimum wage discontinuity on employment.

Figure 6.3 plots the employment rate by age, in months. Here, age was also divided into 49 bins, consisting of 24-months leading up to workers' 18th birthday, the month workers observed their 18th birthday, and 24-months following workers' 18th birthday. The average value of the employment rate is plotted against the mid-point of each bin. As with Figure 6.2, the black lines represent fitted polynomials.

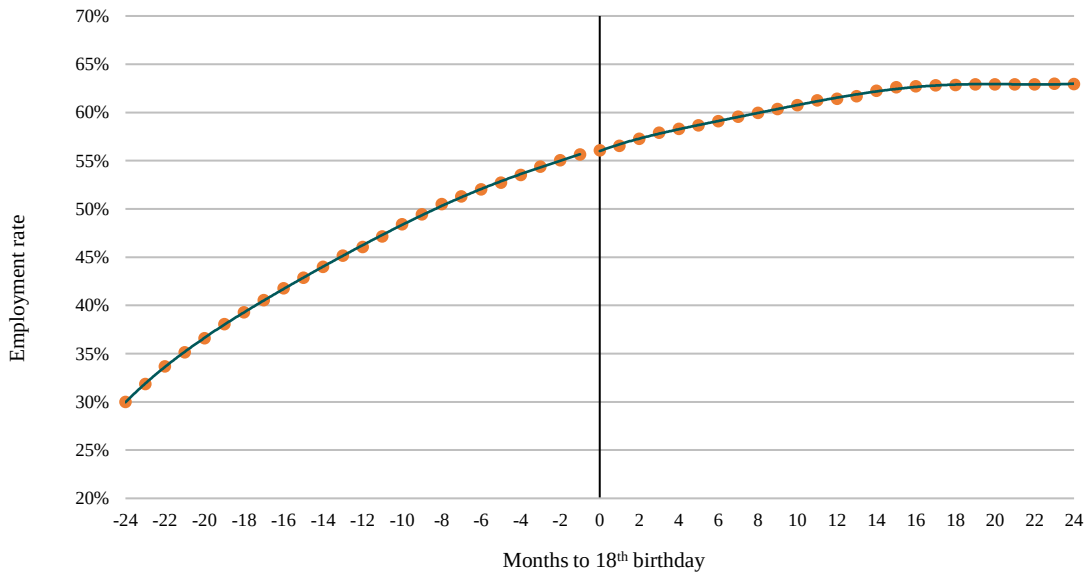


Figure 6.3: Employment around workers' 18th birthday.
The sample covers the period January 2001-December 2009. Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthday. Author's compilation.

[120] The same examination was undertaken using a sample restricted to employed individuals. The examination of the restricted sample resulted in the same qualitative observations. See Appendix D, Figure D2, panel (a).

Starting at 24 months prior to workers' 18th birthday, the average employment rate increased steadily across the whole observation window. From 16 months following workers' 18th birthday, the average employment rate settled at approximately 63%. Crucially, Figure 6.3 provides no evidence of discontinuity in average employment.[121] In fact, examining the employment trends implies an employment elasticity of zero. Although these results are not entirely surprising within the context of the broad legal protections for employees in NZ,[122] they do pose a challenge to the interpretation of Figure 6.2, which shows no discontinuity in average monthly earnings. There are two possible explanations for these results. First, the minimum wage may not have been binding. Alternatively, employers may have responded to higher minimum wages by only adjusting hours of work. Unfortunately, neither of these explanations can be formally examined within the administrative sample due to the absence of hours of work data. Therefore, survey data from the HLFS-IS will be utilised to gather supplementary evidence to interpret these results. However, before stepping into the survey evidence, the effects of the minimum wage discontinuity at age 18 on earnings and employment will be formally examined.

6.5 Empirical Estimation

This section presents the key empirical results from the formal estimation of the impact of the minimum wage discontinuity at age 18 on monthly earnings and employment. The formal estimation uncovered no evidence of effects on earnings or employment using the preferred specification; the main empirical results were confirmed using alternative specifications.

To empirically examine the impact of the minimum wage discontinuity at age 18 on monthly earnings and employment, the main analysis sample constructed in section 6.3.1 was utilised in combination with the RD approach summarised in section 6.2.1. The following regression specification, adopted from Kreiner et al. (2020), was estimated using a sample of 11,055,036 observations over the period 2001-2009:

$$E[y_{it}] = \psi \times 1\{a_{it} \geq 18\} + \sum_{d=0}^{D} \alpha_d \, a_{it}^{d} + \rho \times 1\{a_{it} = 18\} \qquad (10)$$

[121] The same examination was undertaken using an alternative measure of employment where individuals were classified as employed if their wage or salary record was greater than or equal to $50 in the reference month. The alternative examination resulted in the same qualitative observations. See Appendix D, Figure D2, panel (b).

[122] Section 21(1)(i) of the Human Rights Act 1993 declares age to be a prohibited ground of discrimination, with section 22(1)(c) outlining breaches of human rights' with respect to employment, including termination.

where y_{it} are the outcomes of interest (as defined in section 6.3.1). The main effect of interest is ψ which captures the change in $E[y_{it}]$ when workers observe their 18th birthday. The second term on the right, *a*, is a polynomial in age of degree *D*. The results reported in this section used $D = 5$.[123] Along with estimating ψ, percentage changes in ψ were calculated using equation (11):

$$\Delta = \frac{\psi}{\sum_{d=0}^{D} \alpha_d \alpha_{18}^d + \frac{\psi}{2}} \tag{11}$$

where the denominator is evaluated using the mid-point method (where *a* is exactly equal to 18 years).

Following the approach by Kreiner et al. (2019), several iterations of specification (10) were sequentially estimated. These iterations, along with their definitions, are summarised in Table 6.2.

Table 6.2:
Definitions of Estimated Regression Specifications

		Results are in Table 6.3, column:				
Controls	Description	(1)	(2)	(3)	(4)	(5)
Base	Equivalent to the description of specification (3)	Yes	Yes	Yes	Yes	
Month fixed effects	Adds month dummies to control for any month specific shocks.		Yes	Yes	Yes	
Birth (quarter) cohort fixed effects[a]	Adds birth (quarter) cohort fixed effects to control for any shocks specific to a particular birth cohort.			Yes	Yes	
Anticipation & inertia controls	Adds month dummies for the two-months prior, and subsequent to, the month of workers' 18th birthdays to control for anticipation or inertia effects.				Yes	
Non-parametric estimation	Estimates the impact of the age discontinuity across the one month prior, and subsequent to, workers' 18th birthday.					Yes

Note. Details on regression specifications from Kreiner et al. (2020). Author's compilation.
[a] Note that Kreiner et al. (2020) controlled for birth (month) cohort fixed effects. However, in these estimations, birth (quarter) cohort fixed effects were included to correct for collinearity when using birth (month) cohort fixed effects.

The main empirical results are depicted in Figure 6.4, which shows estimates of average earnings per month in panel (a) and employment rate in panel (b) by age, for 24 months before and after workers' 18th birthday. The solid black lines are fitted polynomials of age in $D = 5$. In both panels, the estimated impact of the minimum wage discontinuity at age 18 appears to have had no visible impact on the outcomes of workers in the main analysis sample. In fact, the calculated percentage changes in the average monthly earnings and employment rate at age 18 were close to zero (0.02% & -0.01%,

[123] Lower-order polynomials were tested using specification (10) with the estimation results available in Appendix D, Table D4 and plots in Figure D3-Figure D6. The estimation results and implied elasticities (with respect to employment) when using lower-order polynomials remained consistent with the main empirical results.

respectively). These results were produced using specification (10) and are the preferred results of this formal estimation. Full regression results are presented in Table 6.3.

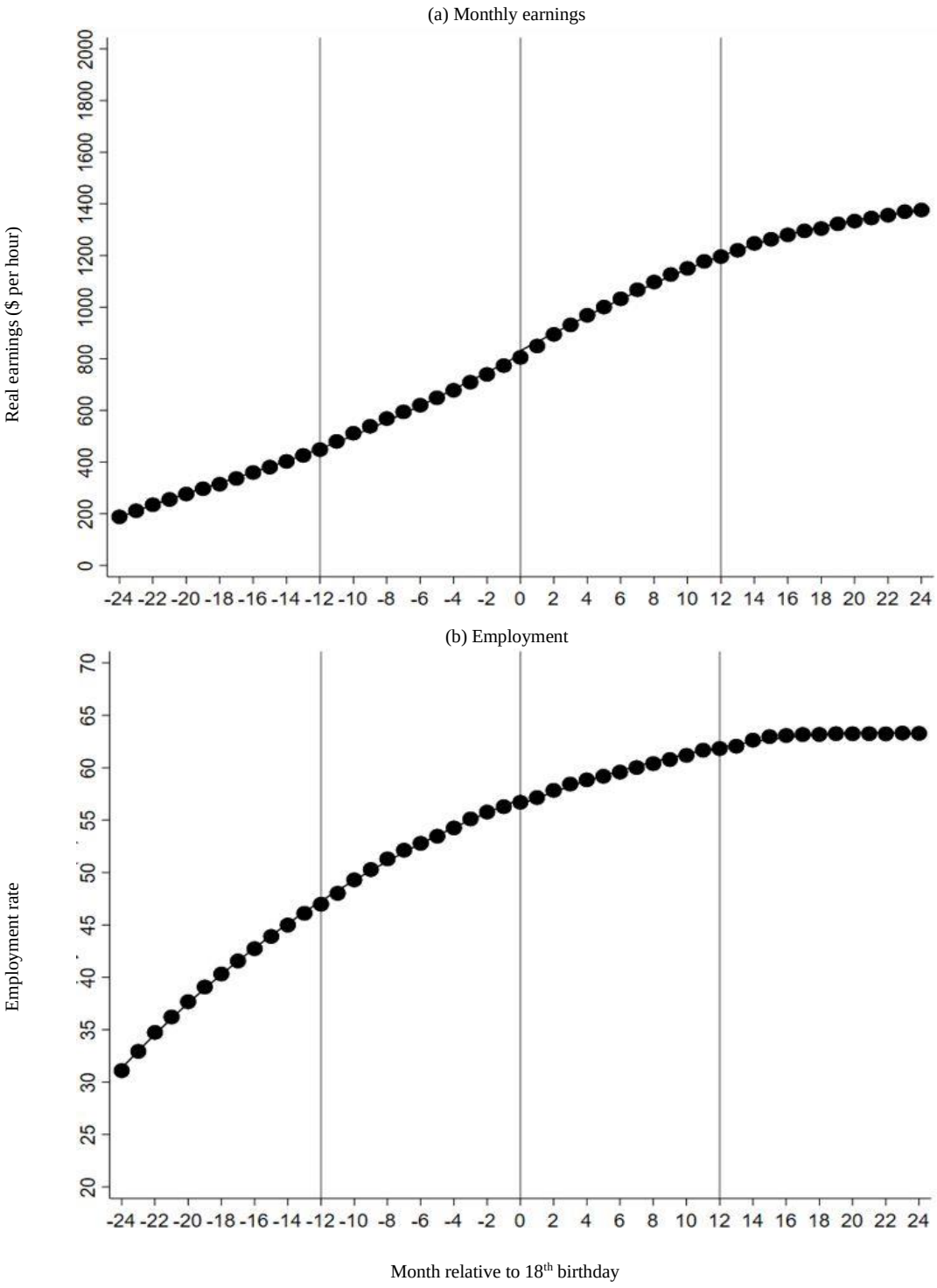


Figure 6.4: Estimated impact of a minimum wage discontinuity at age 18 on monthly earnings and employment.

The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS on 11,055,036 observations. Author's compilation.

[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).

[b] Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthdays.

The preferred set of estimates in column (1) indicate a positive impact on monthly earnings. However, the increase in monthly earnings was small, estimated at approximately $16 per month and was not statistically significantly different from zero ($\psi = 16.07$, $p > 0.10$). Furthermore, using equation (11), the calculated percentage change in monthly earnings was close to zero ($\Delta = 0.02$). In terms of employment, the estimates were small, negative and not statistically significantly different from zero ($\psi = -0.50$, $p > 0.10$). Similar to monthly earnings, the calculated percentage change was also close to zero ($\Delta = -0.01\%$). To obtain a simple estimate of employment elasticity with respect to a change in the statutory minimum wage, the percentage change in employment was divided by the average increase in the real statutory minimum wage over the sample period (27%). This resulted in an implied employment elasticity of zero.[124]

Table 6.3:

Regression Estimates of the Impact of a Minimum Wage Discontinuity at Age 18 on Monthly Earnings and Employment

	(1)	(2)	(3)	(4)	(5)
Monthly earnings[a]					
Coefficient[b]	16.07	9.74	9.78*	19.03***	75.86***
	(13.57)	(6.18)	(5.00)	(7.32)	(19.32)
	[-10.54, 42.68]	[-3.43, 22.92]	[-0.03, 19.59]	[4.68, 33.38]	[37.60, 114.12]
Percent change	0.02	0.01	0.01	0.02***	0.09***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
	[-0.01, 0.05]	[-0.00, 0.03]	[-0.00, 0.02]	[0.01, 0.04]	[0.05, 0.14]
Employment					
Coefficient	-0.50	-0.69***	-0.70***	-0.88***	0.87
	(0.40)	(0.17)	(0.15)	(0.23)	(0.55)
	[-1.28, 0.29]	[-1.03, -0.36]	[-0.99, -0.40]	[-1.33, -0.42]	[-0.22, 1.95]
Percent change	-0.01	-0.01***	-0.01***	-0.02***	0.02
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
	[-0.02, 0.01]	[-0.18, -0.01]	[-0.02, -0.01]	[-0.02, -0.008]	[-0.00, 0.03]
Implied elasticity[c]	-0.00	-0.00	-0.00	-0.00	0.00
Observations	11,055,036	11,055,036	11,055,036	11,055,036	451,227

Note. Sample covers the period January 2001-December 2009. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5%, and 1% level, respectively. Standard errors (in parenthesis) are clustered by (monthly) birth cohort. 95% confidence intervals are provided in square brackets. Column 1 is the baseline specification; see specification (10) . Subsequent columns add month (column 2) and birth-quarter cohort (column 3) fixed effects. Column 4 adds dummy variables from two months before to two months after the workers' 18th birthdays to remove these months from the estimation of the age polynomial and discontinuity. Column (5) only uses the month before and after the workers' 18th birthdays to produce a non-parametric estimate. Author's compilation.

[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).

[b] This is the coefficient of interest which measures the effect of the discontinuity ψ in specification (10).

[c] Implied elasticity was calculated by dividing the percentage change in employment by the average percentage change in the real statutory minimum wage discontinuity between 2001q2 – 2008q1 (27%).

[124] Specification (10) was also estimated by individual-year age cohort. The results were consistent with those reported in Table 6.3, column (1) – see Appendix D, Table D6 for full estimation results. Alternative measures of monthly earnings and employment were tested using specification (10). For monthly earnings, the percentage change was marginally higher ($\Delta = 0.03\%$) and was statistically significant at the 5% level. For employment, the results were consistent with in Table 6.3. Full estimation results are presented in Appendix D, Table D9.

The results from column (2) added month fixed effects to specification (10). Overall, the estimated impacts on monthly earnings were small, positive, and not statistically significantly different from zero ($\psi = 9.74$, $p > 0.10$). In terms of employment, the estimated negative effects were marginally larger and were statistically significant ($\psi = -0.69$, $p > 0.01$). Nonetheless, the calculated percentage change ($\Delta = -0.01\%$) and the implied elasticity (-0.00), were consistent with those reported in column (1).

The results from column (3) added birth (quarterly) cohort fixed effects. The regression results for both monthly earnings and employment were almost indistinguishable from the estimations in column (2), indicating that cohort-specific shocks had no bearing on the estimates. The percentage changes for monthly earnings and employment were calculated at 0.01% and -0.01%, respectively, with an implied employment elasticity close to zero.

Anticipation of receiving higher minimum wages was highlighted as a potential threat to the identification strategy in section 6.2.2. Therefore, to examine the possibility of anticipation and inertia effects, column (4) added dummy variables for the two months before and after workers' 18th birthday. In this case, ψ also included individuals who lost their jobs in the months surrounding their 18th birthday, as opposed to just the month of their 18th birthday. The addition of these controls revealed positive and statistically significant effects on earnings ($\psi = 19.03$, $p < 0.01$), with the percentage change in average earnings calculated at 0.02%. For employment, the estimate was slightly larger than preceding estimates and was statistically significant ($\psi = 19.03$, $p < 0.01$). However, when calculating the percentage change in employment, and subsequently the implied employment elasticity, the results were close to zero and thus consistent with those reported in columns (1)-(3). Consequently, although it appears that there may have been some anticipation or inertia induced behaviours, the impact of these on the estimates were minimal.[125]

The final column in Table 6.3 adopts a non-parametric specification to examine the impact of the minimum wage discontinuity at age 18 on monthly earnings and employment. With respect to earnings, the estimates in column (5) were the largest when compared to those from the preceding specifications. Here, the non-parametric estimates indicated positive and statistically significant impacts on average monthly earnings ($\psi =$

[125] Flows in and out of employment were also examined for anticipatory behaviours, and are presented in Appendix D, Figure D8. Some evidence of anticipatory behaviour was observed, both in terms of entry and exit rates, with marginal increases visible one month before workers' 18th birthday. The net effect of these increases was close to zero.

75.86, $p < 0.01$). Using these results, it was calculated that following the month when workers turned 18, average monthly earnings increased by approximately 0.10% when compared to the month prior to turning 18. Moving on to employment, the results were largely consistent with those from the previous specifications with respect to size. That is, the non-parametric estimates indicated a positive percentage change in employment of approximately 0.02%, with an implied elasticity close to zero.

Overall, the key themes that emerged from the empirical estimation is that both monthly earnings and employment remained virtually unchanged upon workers' 18th birthday. These empirical results are consistent with those from the visual inspections in 6.4, and thus also present the same implications. Specifically, in order for there to be no discontinuity in average monthly earnings or employment, minimum wages may not have been binding or alternative channels of adjustment, such as hours of work, would have been utilised.

In terms of alternative channels, if adjustment were made to hours of work to offset the wage increases associated with the minimum wage age discontinuity, it would have enabled employers to retain their workers. This would explain why no discontinuity in average employment is observed in Figure 6.3. Furthermore, employers were able to legally adjust working hours as long as they acted in good faith and workers were suitably consulted (Ministry of Business Innovation & Employment, 2019c). Similar responses have been seen in the empirical literature. For example, having observed that the introduction of a statutory minimum wage in Germany in 2015 had little impact on employment, Bruttel, Baumann, and Dütsch (2018) explored the wider impacts of the minimum wage and found a reduction in hours worked by employees of highly affected firms. Also examining the impacts of the German minimum wage, Caliendo, Fedorets, Preuss, Schröder, and Wittbrodt (2017) observed a reduction in contractual hours. Chen (2019) examined how establishments in the US adjusted their production on various margins in response to higher minimum wages and a reduction in working hours of 0.7 percent was observed. Finding similar effect sizes, McGuinness and Redmon (2018) observed that working hours reduced by 0.5 hours per week in response to increases in the Irish minimum wage, and in 2008, Stewart and Swaffield examined the impact of the introduction of the UK national minimum, with their empirical results revealing a reduction in working hours by 1-2 hours per week. Finally, in NZ, Pacheco (2011) examined the effects of minimum wage increases on working hours and observed a

reduction for both 16-17 and 18-19-year-olds of approximately 1.9 and 1.7 hours per week, respectively.

Unfortunately, formally estimating the impact of the minimum wage age discontinuity on hours of work was not feasible due to the absence of hours of work data in the administrative sample. However, supplementary descriptive evidence can be attained by turning to survey data from the HLFS-IS. These survey data can be used to examine the likely size of the group of affected workers based on their wage distribution, as well as to examine how earnings, wages and working hours changed by age. This descriptive evidence will inform whether hours of work adjustments were likely utilised to offset the wage increases associated with the minimum wage age discontinuity.

6.6 Descriptive Analysis Using the HLFS-IS

This section discusses the survey data from the HLFS-IS that was utilised to examine teenage wage distribution as well as to evaluate the impact of the minimum wage discontinuity at age 18 on earnings, wages, and hours of work. The underlying objective of this descriptive analysis is to assess the likelihood of hours of work adjustments in response to the higher minimum wages payable at age 18. The first step of this descriptive analysis will examine teenage wage distribution, with the second step examining the impact of the minimum wage discontinuity at age 18 on earnings, wages, and hours of work.

6.6.1 Teenage wage distribution.

The purpose behind examining teenage wage distribution is to gauge the size of teenage workers likely affected by the minimum wage discontinuity at age 18. Following a similar approach to Card (1992b), the sample of teenagers were grouped according to their wage groups: i) paid below the age relevant minimum wage, ii) paid the minimum wage, iii) paid above the current minimum wage but below the next minimum wage, and iv) paid above the minimum wage.

The descriptive analysis utilised data from the HLFS-IS, which includes information on household and personal earnings and incomes.[126] For the purpose of examining the wage distribution of teenagers, HFLS-IS data from 2007 were utilised.[127] The sample was restricted to individuals born in NZ to ensure comparability to the main analysis sample

[126] A comprehensive overview of the HLFS-IS, including its strengths and limitations was presented in Chapter 4.
[127] The selection of this particular year was arbitrary. To assess whether any differences could be observed across alternative years, teenage wage distributions were also examined for 2003 and 2005. See Appendix D, Figure D9.

using administrative data, and also to employed teenagers aged 16-18. Overall, the survey sample consisted of 624 observations.

The key measure required for this examination was hourly wages. The HLFS-IS includes several measures of hourly wages, including measures representing actual or usual hourly wages, measures inclusive or exclusive of overtime earnings, and measures for main and secondary jobs. For the purpose of this examination, information on actual total hourly earnings from the first wage and salary job were taken. This measure of hourly wages was constructed by Stats NZ using self-reported responses from survey participants and includes wages earned from overtime hours.

There are a number of considerations to note regarding this measure. First, weekly earnings and hours were self-reported in nature. This method of collecting earnings data risks reporting error due to cognitive factors (Giusti & Little, 2011; Groves, 2001) or confidentiality concerns (Moore et al., 2000) which could result in measurement error. Next, the measure of hourly earnings includes overtime hours and thus may vary when compared to workers' contractual hourly wages. This hourly wage measure was still considered appropriate as it provided an objective value and was open to less interpretation when compared to other measures such as usual wages (Stewart & Swaffield, 2002).

As was the case with the administrative sample, adjustments to these measures were made. To adjust for outliers in hourly wages and hours worked per week, values were censored between the 1st and 99th percentile. Overall, the number of adjustments were small, and could not be reported due to suppression requirements outlined by Stats NZ (2020c).

The sample of teenagers were divided into four groups based on their level of reported hourly wages relative to the statutory youth and adult minimum wage rate in 2007. Group 1 consisted of workers who earned hourly wages less than their applicable statutory minimum wage (youth minimum wage of $9.00 per hour for 16-17-year-olds; adult minimum wage of $11.25 per hour for 18-year-olds). Group 2 consisted of workers who earned hourly wages equal to their applicable statutory minimum wage. Group 3 consisted of 16-17-year-old workers who earned hourly wages greater than the youth minimum wage but less than the adult minimum wage. Group 4 consisted of 16-18-year-

old workers earning wages greater than the adult minimum wage. Figure 6.5 presents wage groups 1-3 by age.[128]

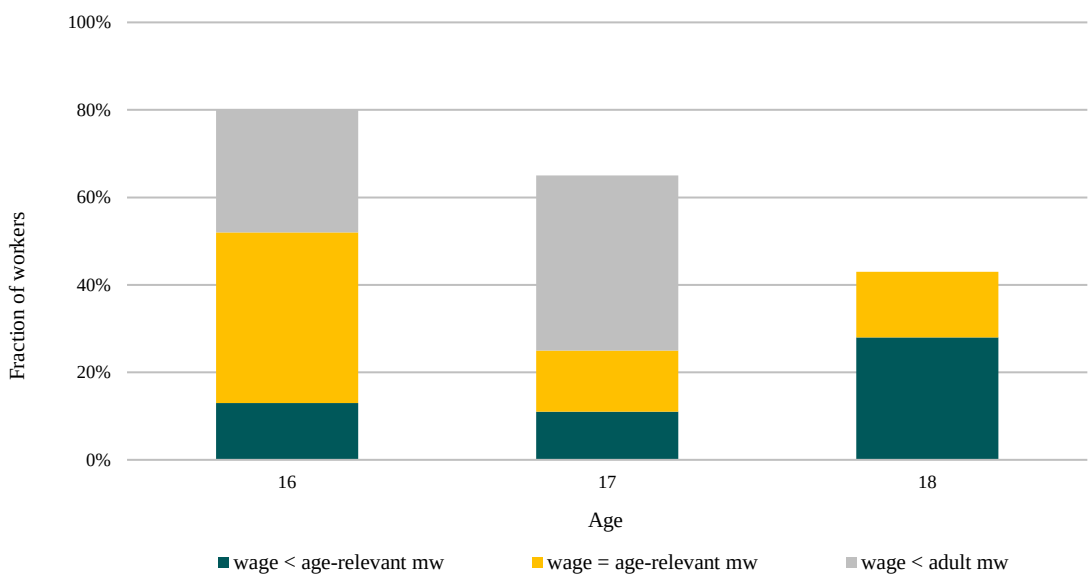


Figure 6.5: Fraction of teenage workers earning hourly wages less than, equal to or more than the statutory minimum wage.
Survey data are from HLFS-IS 2007q2. The sample was restricted to 16-18-year-olds, totalling 624 observations. Author's compilation.

For 16-year-old workers, over half reported as earning wages at least equal to the statutory youth minimum wage rate. Approximately 28% reported wage rates between the statutory youth and adult minimum wage.

Within the context of this study, 17-year-old minimum wage workers were in line for wage increases up to the statutory adult minimum wage rate upon their 18th birthday. In Figure 6.5, 65% of these workers reported earning wages below the statutory adult minimum wage rate. This indicates that nearly two-thirds of 17-year-old workers would become eligible for wage increases by their next birthday.

Examining the distribution of wages at age 18 highlights two insights. First, it appears there may either have been a delay or possible non-compliance in eligible 17-year-old workers receiving applicable wage increases, as 28% of these workers reported wages below the statutory adult minimum wage rate. Next, approximately 72% of 18-year-old workers reported hourly wages above the statutory adult minimum wage rate, indicating that many eligible 17-year-old workers received wage increases as required by law.

[128] Group 4 were not included in Figure 6.5 as these workers were not impacted by increases in the youth minimum wage.

Based on this descriptive evidence, it appears that the increase in minimum wage at age 18 was largely binding, albeit not perfectly. The next section will evaluate the impact of the minimum wage discontinuity on earnings, wages, and hours worked.

6.6.2 Effect of a minimum wage discontinuity on hourly wages and hours of work using survey data.

This section presents an evaluation of the impact of the minimum wage discontinuity at age 18 on weekly earnings, hourly wages, and hours of work using data from the HLFS-IS. The evaluation uncovered suggestive evidence of adjustments to the hours of work following increases in the minimum wage at age 18.

Data were taken from the HLFS-IS to construct individual-year age cohorts amongst respondents who were aged 18 at the time of the survey for the years 2003-2007. Identifying 18-year-olds was limited in this way as the data did not include any information on birth year or month.[129] The next challenge was to construct an observation window comparable to the 49 month period utilised in the main administrative sample. Unfortunately, given the rotating design of the HLFS-IS, survey respondents have a maximum of two periods of observation. Therefore, in order to construct a comparable survey sample, data were taken for the two years on either side of the individual-year age cohorts. For example, for the '2003 18-year-olds' cohort', data from the 2001q2 HLFS-IS were taken and restricted to 16-year-olds, with data from the 2002q2 HLFS-IS were taken and restricted to 17-year-olds. Similarly, data from the 2004q2 and 2005q2 HLFS-IS surveys were taken and restricted to 19 and 20-year-olds, respectively. A similar approach was taken for the remaining cohorts of 2004-2007.

The sample was then restricted to individuals born in NZ to ensure comparability to the main analysis sample using administrative data. Overall, the main survey sample, which was an appended sample consisting of each individual-year age cohort, consisted of 9,582 observations. The steps in constructing each birth cohort are outlined in Appendix D, Table D10, with the number of unique individuals removed at each step.

This descriptive evaluation required several key measures. First, hourly wages were measured using actual total hourly earnings from first wage and salary job – the same as the measure used in examining teenage wage distribution (section 6.6.2). Next, to

[129] These data were part of a custom data file created for earlier research by Pacheco (2007, 2009, 2011) and Pacheco and Maloney (1999) and did not include month or year of birth information.

measure weekly hours worked, information on actual total hours worked per week for a first wage and salary job was utilised.[130] These derived measures were constructed by Stats NZ using self-reported responses from survey participants. Weekly earnings were also examined as part of this descriptive evaluation, and information on actual total weekly earnings was selected as the measure. All three measures were self-reported in nature and may thus have been subject to the same limitations noted in section 6.6.1 regarding measurement error. As was the case with the administrative sample, adjustments to these measures were made. To adjust for outliers in hourly wages and hours worked per week, values were censored between the 1st and 99th percentile. See Appendix D, Table D11 for details on all adjustments made to these measures.

The evaluation in this section on the impact of the minimum wage discontinuity at age 18 on earnings, wages, and hours worked was undertaken by comparing observed outcomes against predicted outcomes. Specifically, the selected outcome variable (i.e., real earnings per week, real wages per hour or hours worked per week) was regressed against the assignment variable, defined as years relative to workers' 18th birthday. The assignment variable was divided into five equal bins representing years relative to age 18, with age 18 being coded as zero. The following simple linear regression specification was estimated:

$$Y_i = a + \beta(Z_i - Z_0) + \varepsilon \qquad (12)$$

where Y_i is the relevant outcome variable for individual *I*, and Z_i is observed age in years minus years from age 18 – the assignment variable. The estimation of specification (12) was restricted to where Z_i were equal to -2 and -1. The β estimates from this regression was then used to predict fitted values for the remaining bin intervals of the assignment variable (i.e., where Z_i = 0, 1, 2).[131]

Figure 6.6 plots the average estimated, predicted, and observed values for each outcome variable against the assignment variable. The cutoff is highlighted by the vertical line. The trend line depicts the predicted trend in the outcome variable over the assignment variable.[132]

[130] This measure of hours worked per week was inclusive of overtime hours and hours on paid leave.
[131] Note that this 'prediction' was a simple linear extrapolation.
[132] All estimated, predicted and observed values for each outcome variable are also included for reference in tabular form. See Appendix D, Table D12.

Panel (a) of Figure 6.6 presents the change in average real earnings per week over the years relative to workers' 18th birthday. With weekly earnings being a function of hourly wages and hours worked per week, any differences between the predicted and observed values may indicate adjustments to either of the other two variables. The estimated real average earnings at bins intervals -2 ($173) and -1 ($285) were generally comparable to the corresponding observed average earnings per week ($172 & $278, respectively). At the cutoff point, predicted real average earnings per week were lower than the observed average earnings ($397 & $435, respectively). From bin intervals 1 and 2, predicted real average earnings were higher when compared to average observed earnings for the corresponding bins. For example, by bin interval 2, predicted average earnings were approximately $69 per week higher when compared to observed average earnings. This differences in average observed weekly earnings were lower than average predicted earnings could be indicative of a downward adjustment to hourly wages or hours worked per week.

Panel (b) of Figure 6.6 presents the change in the average real wages per hour over the years relative to workers' 18th birthday. Having already examined the teenage wage distribution in section 6.6.1 and concluded that the minimum wage increase associated with the age discontinuity appeared binding, this evaluation will add an additional layer of insight into changes in hourly wages over the years relative to age 18. The estimated real hourly wages at bins intervals -2 ($10.66) and -1 ($12.02) were comparable to the corresponding observed average hourly wages ($10.56 & $11.85, respectively). Overall, predicted average hourly wages across all bin interval were nearly indistinguishable from observed average hourly wages. In fact, the largest difference between these two measures occurred at bin zero (i.e., age 18), with observed wages being 18 cents per hour higher than predicted wages. When considering these results in combination with those from panel (a), logic would dictate that downward adjustments in hours worked per week would likely explain the lower observed real weekly earnings relative to what was predicted.

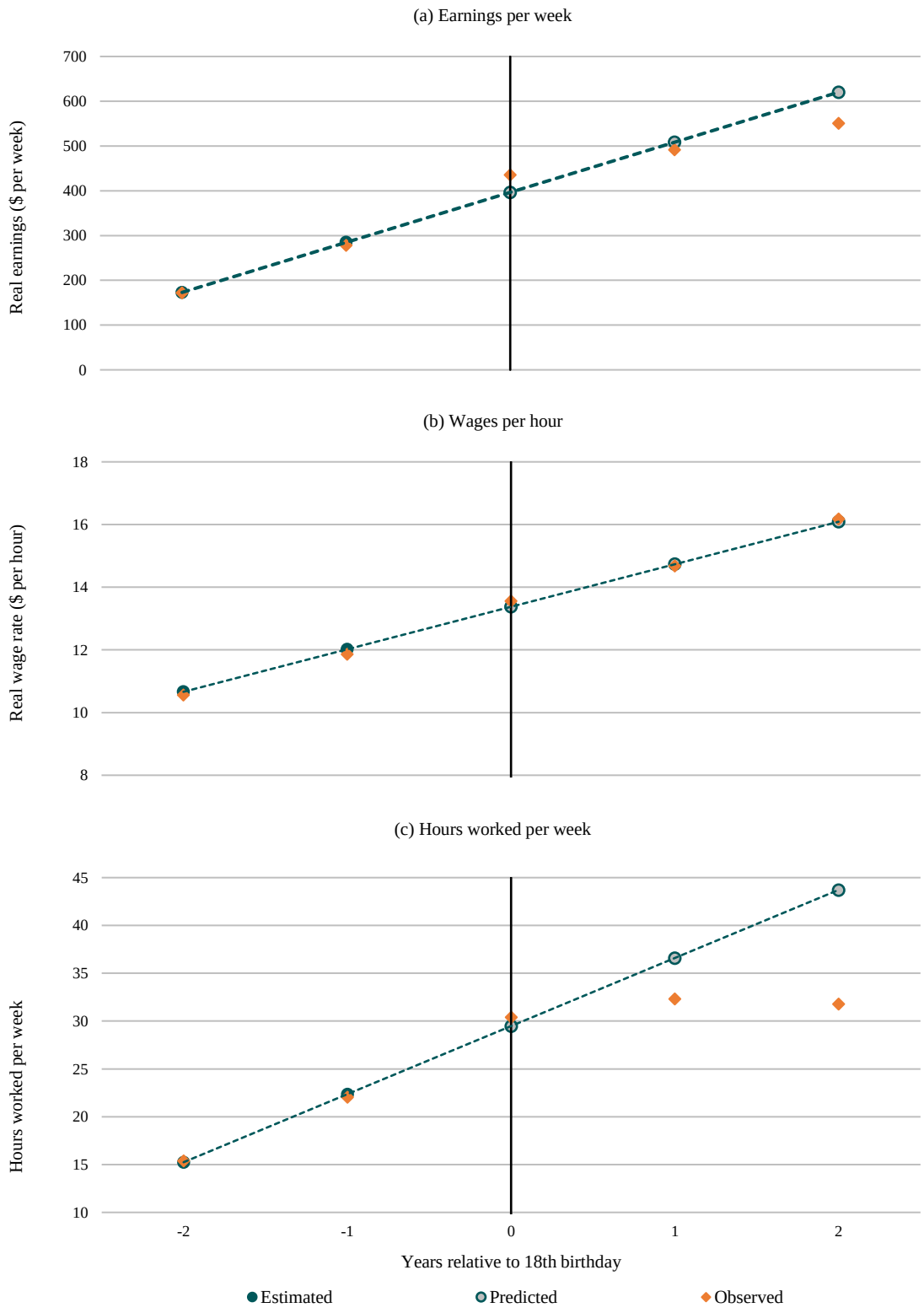


Figure 6.6: Predicted and observed values of weekly earnings, hourly wages, and hour worked using survey data.
The survey data are from HLFS-IS covering the period 2001q2-2009q2. The sample was restricted to employed. Nominal earnings and wages were deflated by the CPI (base year/quarter = 20017q1). Data on the CPI are from the Reserve Bank of New Zealand (2021). Author's compilation.

Panel (c) of Figure 6.6 presents the change in average hours worked per week over the years relative to workers' 18th birthday. Overall, there appears to be a positive association between changes in average hours worked per week and years to/from workers' 18th birthday. Such an association would be expected as workers would likely have increased their hours as age increased due to full-time employment or increased flexibility while engaging in tertiary study. The estimated average hours worked per week at bin intervals -2 (22.36) and -1 (15.25) were comparable to the corresponding observed average hourly wages (15.38 & 22.05, respectively). Similarly, at bin zero (i.e., age 18), predicted and observed average hours worked per week were close, with observed weekly hours being approximately 1 hour higher. However, at bin intervals 1 and 2, there was a clear divergence between predicted and observed average hours worked per week. At one year from workers' 18th birthday, predicted average hours were approximately 37 hours per week, compared to an observed average of 32 hours per week (approximately 12% lower). At two years from workers' 18th birthday, the difference between predicted and observed average hours worked per week had more than doubled; predicted average hours were approximately 44 hours per week, whereas the observed average hours were approximately 32 hours per week, a difference of around 27%.

The underlying objective of this descriptive analysis was to assess the likelihood of hours of work adjustments in response to the higher minimum wages payable at age 18. The descriptive analyses have provided clear descriptive evidence that adjustment to hours of work were likely a channel through which employers attempted to offset the wage increases associated with the minimum wage age discontinuity. However, it is important to note that these results are suggestive in nature and do not directly attribute the reduction in observed working hours to the minimum wage discontinuity at age 18. Nonetheless, these results provide complementary evidence to the main empirical results and thus provide a foundation upon which they can be rationalised.

6.7 Discussion of Results

This chapter empirically examined the effect of a large discontinuity (approximately 27%) in the real statutory minimum wage at age 18 over the period 2001q2-2008q1 on workers' earnings and employment. Both the visual evidence from section 6.4 and the main empirical estimation results in section 6.5 revealed consistent results. First, there appears to have been no immediate impact on monthly earnings from the minimum wage discontinuity at age 18, indicating that the higher statutory minimum wage rates payable at age 18 were fully offset. Crucially, the results also indicated that immediate

adjustments were not made to employment, with the implied employment elasticity calculated at close to zero. Combined, these results pointed toward alternative channels of adjustment, where descriptive evidence using the HLFS-IS showed that hours of work was a likely channel through which employers responded to the minimum wage discontinuity at age 18.

This section will use these main empirical results and compare them to the current NZ empirical literature on the effects of minimum wage increases on teenage employment effects, consider how they relate to theoretical labour market models reviewed in section 3.2, and then discuss a range of potential explanations for them.

6.7.1 Comparison to existing NZ minimum wage literature.

The main empirical findings presented in Table 6.3 are consistent with other NZ empirical evidence regarding the effects of minimum wage increases on teenage earnings and employment.

In terms of earnings, the main empirical results from this chapter observed small positive effects, though not statistically significant, on the average monthly earnings of workers at age 18 from the minimum wage discontinuity over the period 2001-2009. In examining the impact of the 2001 minimum wage reform, the empirical analysis by Hyslop and Stillman (2007) showed that weekly earnings for 18-19-year-olds were unaffected. Their estimates were mixed in direction, and none were statistically significantly different from zero. In their recent addition to the empirical literature, Hyslop and Stillman (2021) showed that the introduction of the new entrant minimum wage in 2008 had no impacts on the weekly earnings of 18-19-year-olds. These results would likely have been anticipated as 18-19-year-olds were not covered by the new entrant minimum wage.

In terms of employment, the main empirical results from this chapter observed small negative, but not statistically significant effects, on the employment of workers at age 18. Similar to these results, the estimates from Hyslop and Stillman (2007) revealed no immediate adverse effects on teenage employment from the 2001 minimum wage reform. Given that the RD approach utilised in this study examined the effects on employment under the condition of market equilibrium, the main findings with respect to employment complements those reported by Hyslop and Stillman (2007). In contrast to the main employment results from this chapter, Pacheco (2011) observed negative effects on employment from binding minimum wages for 18-19-year-olds; however, results were

mixed across different sub-groups of these teenagers (e.g., no effect for 18-19-year-old females; positive effects for 18-19-year-olds with no qualification).

6.7.2 How these findings relate to theoretical labour market models.

The theoretical models summarised in Chapter 3 generally predict different outcomes related to the effects of minimum wage increases on employment. The adverse outcomes predicted from the PCLM do not appear relevant in the context of this study, given the main empirical findings indicated no employment loss resulting from the large minimum wage discontinuity at age 18. Consequently, it appears that these findings are more consistent with the predictions of firms having some monopsony power, where minimum wage increases can have no effect on employment.

Under a monopsony structure, employment would have been able to remain steady in several ways. First, where firms were required to increase the wages of their affected workers, fewer of these workers may have left to take alternative jobs. Alternatively, if workers still left their firms, the reallocation of workers from low-wage high turnover firms to higher wage lower turnover firms could have reduced or levelled out overall turnover (Dube, 2019). Furthermore, if firms were able to offset the higher wage costs through alternative channels (e.g., price adjustments of goods and services), labour demand may not have been the primary factor determining the level of employment (Dube, 2019) and thus even with minimum wage increases, employment could have remained unchanged.

Price adjustments in response to minimum wage increases have found empirical support in several industries, including restaurants (Aaronson, 2001; Allegretto & Reich, 2018) and retail (Leung, 2021; Renkin, Montialoux, & Siegenthaler, 2020). Both of these industries have been identified as high teenage-employing industries (Hyslop et al., 2012). Thus, assuming NZ firms in these two industries are able to respond to minimum wage hikes through price adjustments, the main empirical findings with respect to employment are in line with outcomes predicted under a monopsony setting.

6.7.3 Alternative explanations to the main empirical findings.

In an attempt to gather supplementary evidence to rationalise the main empirical results, the descriptive analysis using the HLFS-IS uncovered the possibility that hours of work may have been a likely channel through which employers responded to the minimum wage discontinuity at age 18. Overall, the descriptive evidence indicated that reductions

to the observed hours of work, averaged between 12-27% over the two years subsequent to turning 18, relative to what was predicted. Although this evidence was merely suggestive, it did support the notion that employers adjusted hours of work in response to higher minimum wages as opposed to terminating employment. Existing empirical evidence has also revealed how working hours are negatively impacted by minimum wage increases (Bruttel et al., 2018; Caliendo et al., 2017; Chen, 2019; Mastracci & Persky, 2008; McGuinness & Redmon, 2018; Pacheco, 2011; Stewart & Swaffield, 2008), suggesting that this explanation for the main empirical results has some merit.

This does however raise an important consideration: how do employers reduce hours of work in NZ given the broad legislative protection covering employees?[133]In NZ, all employees must have a written employment agreement which outlines a range of terms and conditions, including an agreement or indication of the number of hours the employee will work – applicable to all permanent or fixed-term workers with full-or-part-time status (Ministry of Business Innovation & Employment, 2021, n.d.e). Furthermore, if employers want to change the terms and conditions of an employment agreement, the proposed change must be agreed to by the employee in order to form a new contract (Ministry of Business Innovation & Employment, 2019c). If a clause in the employment agreement reserves the right for an employer to change hours, the employer has to act fairly and reasonably before they do. An employee who considers themselves disadvantaged by the change in hours can discuss their concerns with their employer in order to resolve the issue, and has the right to access Employment Mediation Services provided by MBIE if the issue remains unresolved (Ministry of Business Innovation & Employment, 2018b). In some instances, such as genuine financial difficulty, a reduction in hours may be presented as an alternative to redundancy; however, in these instances employers must follow the usual process for organisational change, and allow employees time to consider and respond to the proposal (Ministry of Business Innovation & Employment, 2019c).

In relation to the main empirical findings from this study, the minimum wage discontinuity at age 18 may have placed a heavy financial burden on some employers, particularly those with a high proportion of teenage-employment in industries such as those in the Accommodation, Cafes, and Restaurants or Retail trade. These employers may have proposed a reduction in hours of work to affected employees in order to offset the increased wage bill. This seems like a plausible explanation for the main empirical

[133] For example, Minimum Wage Act 1983, Health and Safety at Work Act 2015, and the Employment Relations Act 2000 (Ministry of Business Innovation & Employment, n.d.d) are several of many statutes offering legislative protection for NZ workers.

results, as affected workers would have likely been engaged in both employment and formal study (e.g., finishing secondary or commencing tertiary study), and thus a reduction in working hours would not have disadvantaged them to a large extent. An alternative explanation is that employers made no immediate adjustment to working hours, but once an employee departed the business, the employer hired a replacement with fewer hours stipulated in the employment agreement.

The preceding explanations assume that all employers abided by the legislative requirements. In practice, this may not necessarily have been the case. An employer may have presented an ultimatum to an employee, with one option being to keep their job with reduced hours, and the other being for their employment to end. It could also be argued that at age 18, employees may not fully be aware of their employment rights and thus may have accepted reduced hours without being consulted appropriately.

Up to this point, the discussion has been driven by the empirical and descriptive evidence generated in this study and the existing literature. However, a qualitative perspective may shed some insight into explanations underlying the main empirical results. In NZ, businesses are surveyed each year in the NSE, which collects information on workplace practices (Ministry of Business Innovation & Employment, n.d.c). Each iteration varies in the type of information it collects, though questions on minimum wages are always included. Unfortunately, the NSE only commenced in 2011 and thus does not cover the main analysis period. However, drawing insights from the NSE could provide complementary evidence to the main empirical findings.

As part of the NSE, employers are asked to detail how they respond to minimum wage increases. The most recent NSE of 2018/19 indicated that out of all employers, over half (56%) made no adjustments to their business practice, while 8% reduced hours, and 13% did not replace staff who departed the business (Ministry of Business Innovation & Employment, 2019b). Using the NSE 2011/12, along with interviews covering 53 employers in four industries across NZ, Houghton (2012) found that employers had a combination of responses to large minimum wage increases. For example, if minimum wage increases were $1 per hour or above,[134] employers reported that they would reduce hours of work, change hiring practices (substituting youth for more productive adults), or not fill vacancies. Some employers also reported price adjustments of goods and services

[134] Over the period 2001-2008, the average difference between the youth and adult minimum wages was $2.09.

as common practice (14%), especially in manufacturing, accommodation and hospitality, and retail (Houghton, 2012).

These qualitative insights support the main empirical findings as they demonstrate that many employers make no immediate adjustment to employment; instead, they respond to large minimum wage increases by utilising multiple channels, including hours of work. Any employment related adjustments are time dependant (i.e., not filling vacancies, substituting youth for adults) and thus would not be captured under a RD analysis.

6.8 Conclusion and Implications

The primary objective of this chapter was to utilise an identification strategy not previously adopted in the NZ literature while utilising individual-level administrative data. In response to the objective, this chapter set out to empirically estimate the effect of an increase in the NZ minimum wage on the employment of workers following their 18th birthday. The main empirical analysis exploited a large discontinuity (approximately 27%) in the real statutory minimum wage at age 18 over the period 2001q2-2008q1. Using an individual-level administrative sample, RD estimates revealed no immediate impacts on average monthly earnings or employment from the sharp minimum wage increase at age 18, suggesting the increases in the wage bill were fully offset through alternative channels of adjustment. These estimates were robust across multiple specifications.

Although the main empirical results were consistent with existing empirical evidence (Hyslop & Stillman, 2007, 2021), they did compel further investigation to understand through which channels employers responded to increases in the statutory minimum wage at age 18. Using data from the HLFS-IS, descriptive evidence was gathered which supported the notion that reductions in working hours could explain the main empirical results. Furthermore, the NSE provided complementary qualitative evidence demonstrating that employers adjust hours in response to large minimum wage increases, along with making adjustments in other channels (e.g., price adjustments of goods and services; not filling vacancies). From a legislative perspective, the absence of an employment discontinuity is consistent with broad legal protections for employees, including the Human Rights Act 1993 which prohibits the termination of employment based on increases to an age-specific minimum wage. Furthermore, alternative employment legislation enables employers to reduce hours of work as long as they act in good faith and workers are suitably consulted. Although a reduction in working hours as an explanation for the main empirical results could not be formally examined, based on

an array of descriptive, empirical and qualitative evidence, in addition to having legal provisions, it appears likely to be a channel through which employers responded to the minimum wage discontinuity at age 18.

6.8.1 Policy implications.

The main empirical findings from this thesis indicated that there were no immediate effects on average monthly earnings or employment from the minimum wage discontinuity at age 18. This is noteworthy given the average size of the increase in the real statutory minimum wage over the sample period was 27%. Although existing NZ empirical evidence supports these results (Hyslop & Stillman, 2007, 2021), the main empirical findings highlighted a key consideration for policy makers.

It appears that the annual minimum wage review process is generally developed with the view that increases in the statutory minimum wage have adverse impacts on aggregate employment, or particular groups of workers such as teenagers, women, or Māori, where aggregate impacts on employment are negligible (Ministry of Business Innovation & Employment, 2019a). Although such a view is consistent with NZ empirical evidence (Hyslop et al., 2012; Hyslop & Stillman, 2021; Pacheco, 2011), the main empirical findings in this chapter may serve as a basis for policy makers to acknowledge that immediate negative effects on employment from minimum wage increases may not necessarily occur. Based on this alternative empirical evidence, policy makers could formulate minimum wage policy decisions by assessing the consequences of increases in the minimum wage in terms of alternative outcomes such as reduced working hours for affected workers.

6.8.2 Research implications.

The use of administrative data in this application led to several observations. The key benefit was the availability of historical individual-level historical administrative data to construct a sample incorporating 49 months of earnings and employment information. Birth data from the DIA was beneficial in restricting the main analysis sample to individuals born in NZ, and border movements data was vital for removing non-residents. The large-scale of the administrative sample also enabled the use of RD analysis, which has not previously been adopted in the existing NZ empirical minimum wage literature; instead, the literature has largely relied on survey data.

The key limitation of the administrative sample was consistent with that identified in Chapter 4, namely the absence of hours of work information. If hours of work data were available from an administrative source, working hours and hourly wages could have been formally examined to: i) explain the main empirical results, and ii) use as additional outcome variables in specification (10).

Future research would need to resolve the absence of hours of work data to overcome the limitations noted above. This could be achieved by empirically examining the effects of minimum wage increases on teenage employment from April 2019 when administrative hours of work data became available through payday filing records. The benefit of this approach would be having access to administrative hours of work information which would arguably be prone to less measurement error, relative to survey data, given it is recorded for tax purposes. Alternatively, effects of minimum wage increases on teenage employment could be empirically examined from 2007, at which stage individual-level records can be linked between an administrative sample and the HLFS or HLFS-IS using unique identifiers. Either approach would require the adoption of an alternative identification strategy, as the discontinuity in the statutory minimum wage exploited in this chapter was only present over the period of 2001-2008.

Chapter 7 Conclusion

The empirical evidence on the impact of an increase in minimum wages on teenage employment is mixed. There is a large body of empirical support indicating that increases in minimum wages adversely affect teenage employment both in the international (Laporšek, 2013; Machin et al., 2003; Neumark & Wascher, 1992, 1994, 2000) and NZ (Chapple, 1997; Hyslop et al., 2012; Maloney, 1995, 1997; Pacheco, 2011) literature. There has also been a growing body of empirical evidence indicating that increases in minimum wages may have no impact, or in some cases, positive impacts on teenage employment (Böckerman & Uusitalo, 2009; Card, 1992a, 1992b; Card & Krueger, 1994; Garloff, 2019; Hyslop & Stillman, 2007, 2021; Katz & Krueger, 1992).

An important consideration influencing these contrasting empirical findings relates to the context under which they were estimated. Factors such as country-specific legislative frameworks governing minimum wages (and the extent to which the minimum threshold is binding and enforced) and employment protection may vary across countries and could help shed some insight into the mechanisms underpinning different estimated employment effects of increases in minimum wages. Along with country-specific differences, factors such as data quality and availability and identification strategies are also likely to influence differences in the impacts of minimum wages increases on employment.

NZ has a long history of enforceable minimum wages dating back to the 19th century, with current legislation providing near-to-full coverage for all workers over the age of 16 years. Strong labour market protections are also in place for workers that provide limited grounds for employment to be terminated due to increases in statutory minimum wages.

NZ also stands out as having high minimum wages by international standards across developed economies and has experienced periods of substantial variation in teenage minimum wages. In particular, the 2001 minimum wage reform saw coverage of the adult minimum wage lowered to age 18, increasing the wage rate for 18-19-year-old minimum wage workers by approximately 68% in real terms in March 2001. As part of the same reform, a 35% increase in the real youth minimum wage rate, applicable to 16-17-year-olds, also occurred over the period March 2001/02.

This thesis was motivated by the availability of individual-level administrative data from Stats NZ's IDI. To date, existing NZ empirical studies on minimum wages have largely

used estimates based on survey data from the HLFS (Chapple, 1997; Hyslop & Stillman, 2007, 2021; Pacheco, 2011; Pacheco & Maloney, 1999). Consequently, the findings from this thesis contribute unique empirical evidence to the current body of knowledge on teenage employment effects of increases in the NZ minimum wage.

The purpose of this thesis is to introduce individual-level administrative data to the empirical examination of the effects of minimum wage increases on teenage employment in NZ. Fundamentally, the key question this thesis answers is: What are the effects of the 2001 minimum wage reform on teenage employment? The following section presents a concise summary of key findings, limitations, and directions for future research.

7.1 Summary of Key Findings

The descriptive analysis in Chapter 4 set out to understand the benefits and limitations of administrative and survey data within the context of empirically examining the teenage employment effects of increases in the NZ minimum wage. Focussing on administrative data, two main benefits related to the employment measure were identified: i) its ability to impose a narrow and objective definition of employment based on wage and salary tax records, and ii) the availability of the data at a monthly-level, which is a higher frequency relative to quarterly or annual survey alternatives examined in this thesis. The narrow definition of employment meant that only individuals with wage or salary records were classified as employed in a reference month, as opposed to the broader definition of employment in the HLFS which not only includes individuals who worked for pay, but also those who worked without pay. This broader definition of employment in the HLFS potentially overestimates employment levels. Having administrative employment information available at a monthly level enabled additional variation in outcome variables to be exploited in empirical analyses. This was particularly beneficial when assessing teenage employment effects given the strong seasonal nature of their labour market participation. The key limitation of the administrative data was the absence of hours of work information, meaning descriptive analyses of hourly wage distributions and wage group comparisons were severely hindered. However, this limitation did set a clear boundary in terms of what outcomes of interest were feasible for the regression analyses conducted in Chapter 5 and Chapter 6, with outcomes limited to monthly earnings and employment.

The empirical analysis in Chapter 5 set out to formally estimate the teenage employment effects of increases in the minimum wage using individual-level administrative data. A

two-step approach was adopted, with the first step reproducing the employment results from the study by Hyslop and Stillman (2007) using survey data from the HLFS; the reproduced results were consistent with those from the original study. The second step combined the regression specifications from step one with an individual-level administrative sample to empirically examine the teenage employment effects of the 2001 minimum wage reform. The main empirical findings indicated that increases in the minimum wage resulted in small, positive, and statistically significant effects on the average employment of 16-17 and 18-19-year-olds across all individual post-reform years. However, no evidence in support of parallel trends was observed. Consequently, the estimated teenage employment effects of the 2001 minimum wage reform remained uncertain. Notwithstanding the main empirical findings, a key benefit of the individual-level administrative data in this application emerged. In particular, the monthly-level of the employment data was advantageous in uncovering substantial seasonal fluctuations in teenage employment, which was beneficial for assessing the parallel trends assumption.

The empirical analysis in Chapter 6 utilised RD as the identification strategy to estimate the effect of a large discontinuity in the statutory minimum wage at age 18 on monthly earnings and employment. Utilising an individual-level administrative sample, RD estimates revealed no immediate impact on average monthly earnings or employment from the sharp minimum wage increase at age 18. The results suggested that increases in the wage bill were offset by alternative channels of adjustment, with hours of work being identified as a likely channel. Unfortunately, no administrative data on working hours were available to formally examine the likelihood of hours of work adjustments. Consequently, survey data from the HLFS-IS were utilised to generate descriptive evidence, which revealed that reductions in working hours could in fact explain the main empirical results. This descriptive evidence was also complemented by qualitative evidence from the NSE, which revealed that NZ employers utilised several channels of adjustments, including a reduction of working hours, to offset large increases in minimum wages.

As noted, the purpose of this thesis was to introduce individual-level administrative data into the empirical examination of the effects of minimum wage increases on teenage employment in NZ. The descriptive and empirical analyses conducted throughout this thesis contributed to the current body of knowledge on the teenage employment effects from increases in NZ minimum wages as follows:

- presented a comprehensive review of descriptive evidence comparing administrative and survey measures related to earnings and employment;
- reproduced existing NZ minimum wage empirical evidence using survey data. This contribution is of unique value as studies which reproduce existing empirical evidence in economics are rare, both internationally and in NZ;
- presented DID employment estimates on increases in the youth and adult minimum wage using individual-level administrative data. These data have not previously been exploited in the existing NZ empirical minimum wage literature; and
- presented RD estimates on monthly earnings and employment using individual-level administrative data. As an identification strategy, RD has not formally been adopted in the existing NZ empirical minimum wage literature.

7.2 Limitations

There were two key limitations to the empirical analyses in this thesis: i) the absence of an administrative source of hours of work information, and ii) the restricted coverage period of several data sources required for imposing activity restrictions when constructing the administrative sample. In both instances, the limitations were related to data provided to Stats NZ from participating agencies in the IDI.

7.2.1 Hours of work limitations.

There were multiple instances where an administrative source of hours of work information would have been beneficial. Unfortunately, over the sample periods examined in this thesis, no such data were available.

The descriptive analyses in Chapter 4 were hindered by the imputation of hours of work information from the HLFS-IS into the administrative sample. This was particularly evident when comparing the characteristics of sub-minimum and minimum wage workers using both survey and administrative samples. The administrative sample showed the sub-minimum wage worker group was approximately 12 percentage points larger than the comparable group in the survey sample. Additionally, the minimum wage worker group in the administrative sample was around half the size of the comparable group in the survey sample. Consequently, it was challenging to compare the characteristics of these wage groups. Administrative data on hours of work may have facilitated a comparison with more meaningful insights regarding the benefits and limitations of both survey and administrative data within the context of minimum wage research in NZ.

In Chapter 6, the main empirical results indicated that there were no immediate employment effects from the large discontinuity in the statutory minimum wage at age 18. However, given that the empirical results for monthly earnings indicated that increases in the statutory minimum wage were fully offset, thus leaving average monthly earnings unaffected, the likely channel of adjustment was hours of work. Consequently, administrative hours of work data would have been beneficial to: i) derive a measure of hourly wages and estimate the impact of the minimum wage discontinuity on hourly wages and working hours, and ii) present a more formal basis for explaining the main empirical results. Although Fabling and Maré (2015) presented a method for addressing the absence of hours of work information in the IDI, several assumptions, such as full compliance with statutory minimum wage rates, made their method less viable for application within the context of this thesis.

7.2.2 Data coverage limitations.

The second limitation relates to the misalignment of coverage periods for several data sources required to generate activity indicators to identify NZ residents within the IDI. The implication of not having all data sets covering the sample periods of this thesis was that non-residents were included in the administrative sample. Although less of a concern in the empirical examinations conducted in Chapter 5 and Chapter 6 due to the noise not being expected to introduce bias to a specific sub-group or period within the study, it did result in an inflated denominator which impacted the descriptive analyses in Chapter 4. In particular, the inflated denominator made it challenging to draw valuable insights from descriptive comparisons of sample populations (e.g., wage groups) or outcome measures using survey and administrative data.

7.3 Policy Implications

Policy recommendations as part of the annual minimum wage review process are generally developed with the view that increases in the statutory minimum wage have adverse impacts on aggregate employment, or particular groups of workers such as teenagers, women, or Māori, where aggregate impacts on employment are negligible (Ministry of Business Innovation & Employment, 2019a). These disemployment effects are largely represented as restraints in job growth resulting from proposed increases in minimum wages. Although reductions in hours of work are acknowledged as a potential outcome of increasing minimum wages, no estimates are presented quantifying the impact. Consequently, hours of work do not appear to be formally considered as part of policy formulation.

The main empirical findings estimated in this thesis, based on large minimum wage increases implemented as part of the 2001 minimum reform, have presented evidence highlighting the need to consider the effects of minimum wage increases on hours of work. The RD estimates presented in Chapter 6 showed that there were no direct effects on average monthly earnings or employment from the discontinuity in the minimum wage at age 18. Complementing these findings was suggestive evidence which indicated that employers likely responded to the minimum wage increases by adjusting hours of work.

Although formally assessing the direct effects of minimum wage increases on employment should continue as part of the annual minimum wage review process, the main empirical findings from this thesis suggest further modelling should also be undertaken to understand impacts on hours of work. This additional modelling would enable minimum wage policies to be formulated by considering a wider range of potential responses by employers, as well as provide a richer evidence base for policy makers to evaluate.

7.4 Future Research

There are a number of opportunities for future empirical research on the NZ minimum wage beyond the scope of this thesis. The first opportunity could be to re-examine the impacts on employment from recent increases in minimum wages. Such research would be motivated by minimum wage policies continuing to be an important topic of discussion, due to the Labour-led government's previous commitment to raising minimum wages to $20 per hour by 2021. The benefit of examining minimum wage impacts associated with this policy would also eliminate both limitations acknowledged in section 7.2. First, access to administrative data on hours of work is available in the IDI starting April 2019. Consequently, a broader spectrum of outcomes could be empirically examined, including hourly wages and working hours. Next, there is the appropriate data required to generate an administrative ERP.

The next research opportunity could be to extend the range of outcomes examined in this thesis to include enrolment in tertiary education. Several factors would motivate this research. First, there is a lack of policy discussion on alternative outcomes, such as tertiary enrolment, that form part of the annual minimum wage review in NZ. Next, there is a small body of empirical NZ literature which has examined how increases in minimum wages impact educational enrolment (Hyslop & Stillman, 2007, 2021; Pacheco & Cruickshank, 2007). However, these studies have examined impacts on educational

enrolment in general, with no studies having specifically focussed on the impacts on tertiary enrolment. Coverage of administrative tertiary enrolment data in the IDI extends back to 1994. This provides future research the option of restricting empirical analysis to the sample periods examined in this thesis, which would enable it to exploit the substantial variation in statutory teenage minimum wage rates surrounding the 2001 minimum wage reform period, or to examine longitudinal impacts over the period 1999-2021. Finally, the existing NZ empirical literature on educational enrolment impacts from increases in the minimum wage has made estimations using a range of survey data. Consequently, the final motivation for this research opportunity would be to contribute empirical findings to the current body of NZ knowledge based on individual-level administrative data.

Appendix A.

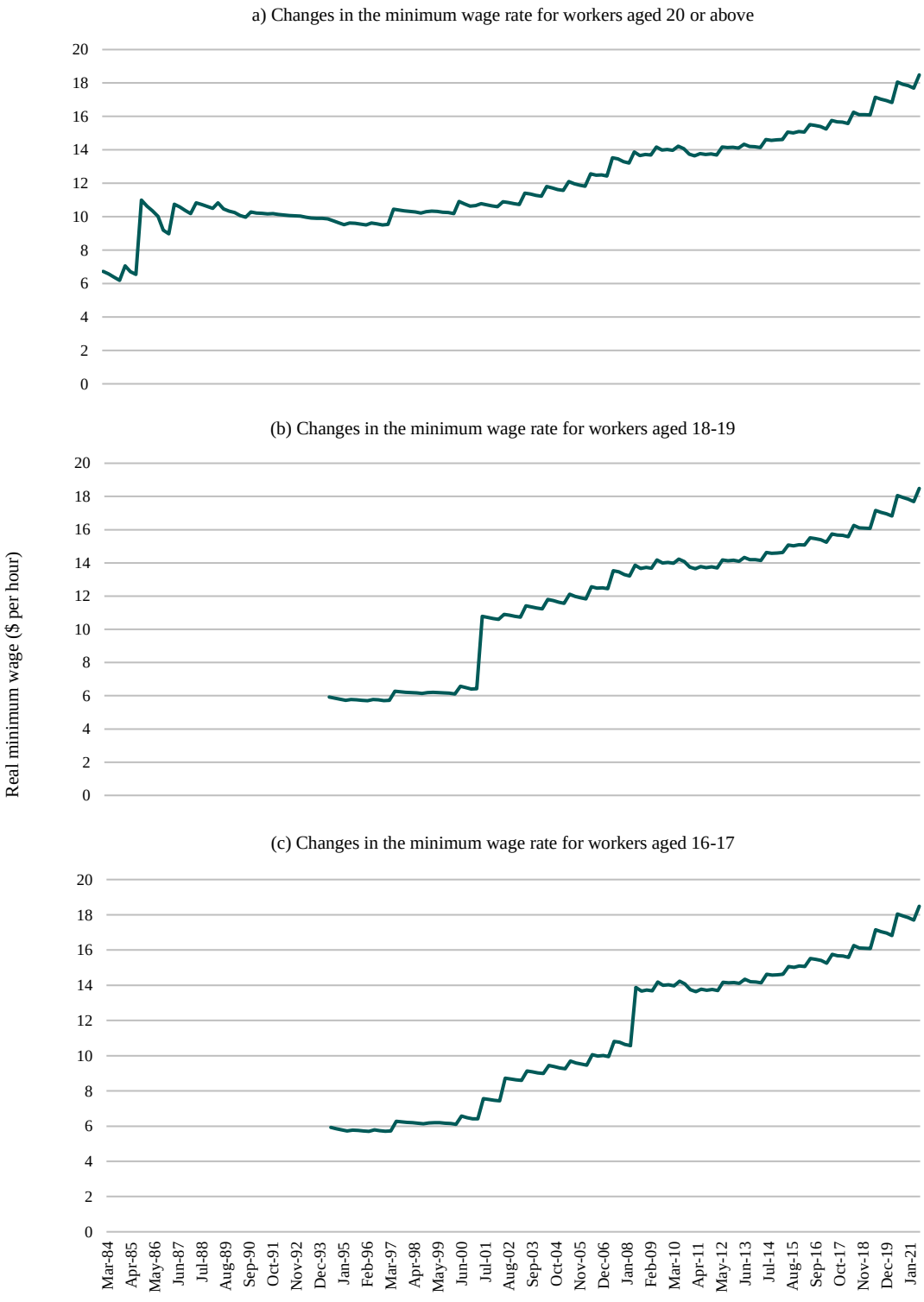


Figure A1: Changes to real minimum wage rate by age.
Data on nominal minimum wages and CPI are from Ministry of Business Innovation & Employment (n.d.a) and Reserve Bank of New Zealand (2021), respectively. Nominal earnings were adjusted using the CPI (base month and year = March 2017). Author's compilation.

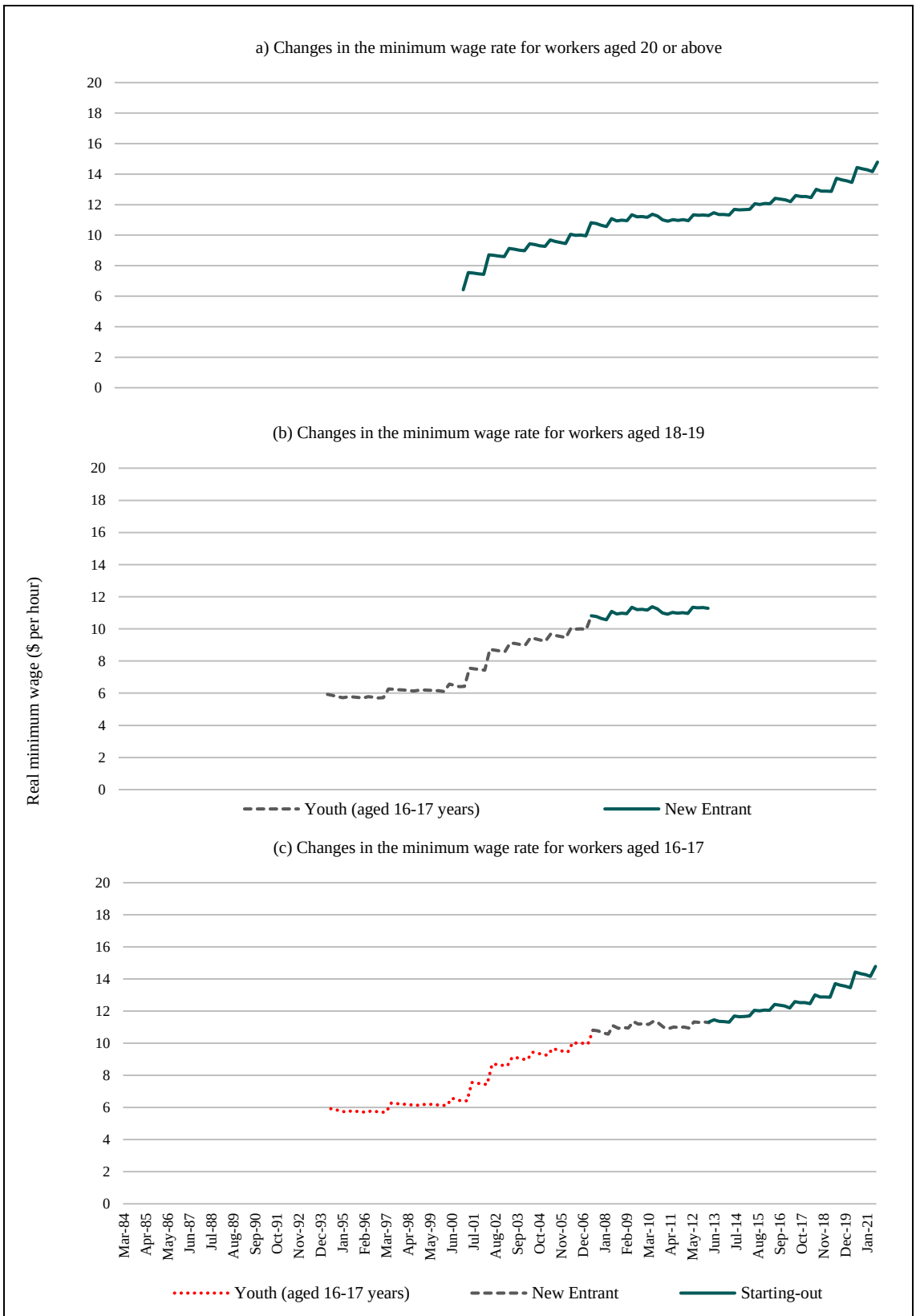


Figure A2: Changes to real minimum wage rate by types.
Data on nominal minimum wages and CPI are from Ministry of Business Innovation & Employment (n.d.a) and Reserve Bank of New Zealand (2021), respectively. Nominal earnings were adjusted using the CPI (base month and year = March 2017). Author's compilation.

Appendix B.

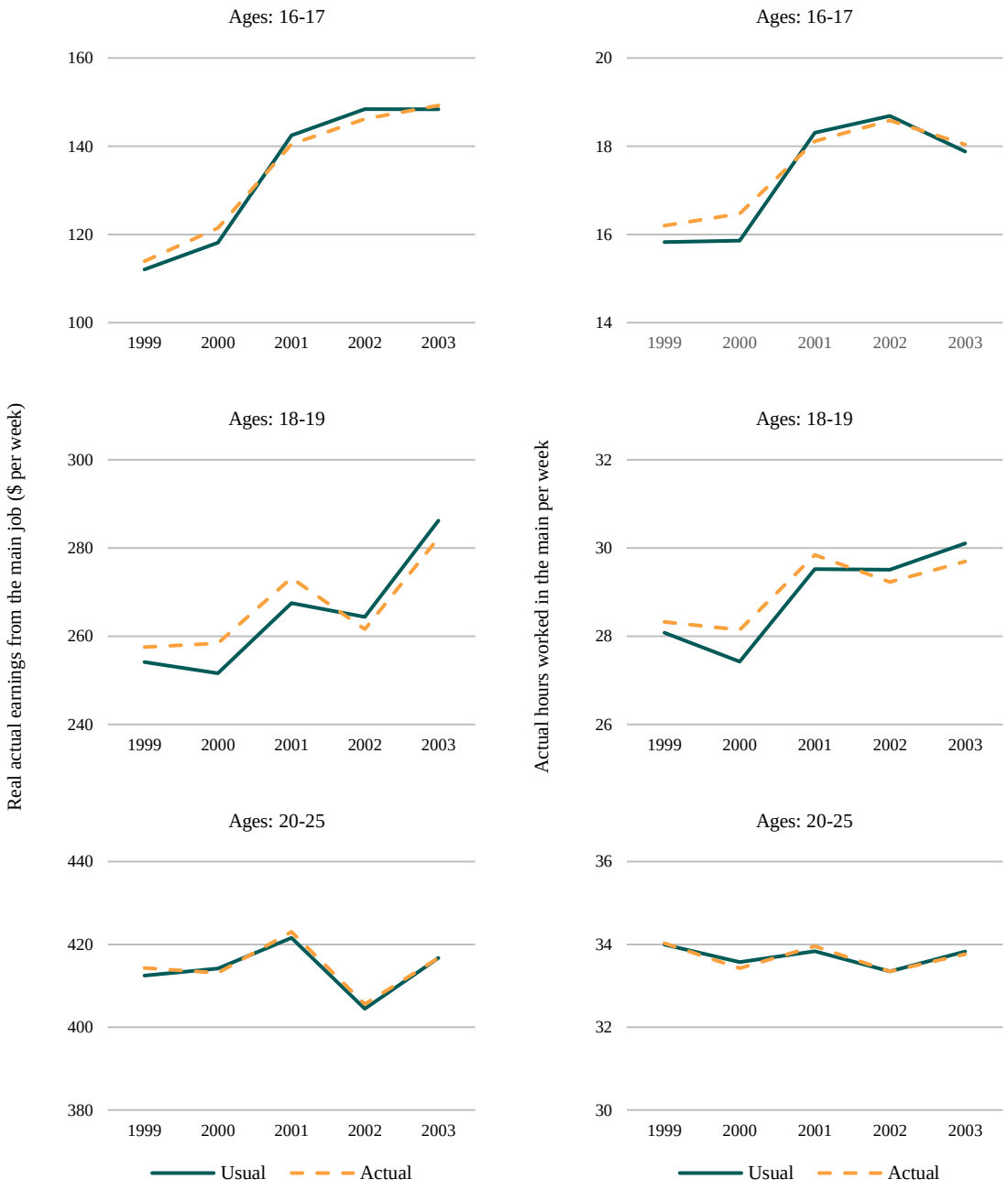


Figure B1: Mean actual and usual earnings and hours from main job by age group and year.
Data are from the HLFS-IS covering the period 1999q2-2003q2. Means are weighted by the HLFS-IS sampling weights generated by Stats NZ. Values on the y-axis start at different levels to clearly highlight the variation in actual and usual earnings hours over the sample period by each sub-group. Earnings values are deflated by the CPI (base year/quarter 1999q2). Author's compilation.

Table B1:

T-tests on Mean Usual and Actual Earnings and Hours from Main Job by Age Group

		Real weekly earnings		Hours worked	
Age	Year	Usual	Actual	Usual	Actual
16-17	1999	112.06	113.93	15.83	16.20*
	2000	118.14	121.44**	15.86	16.47***
	2001	142.44	140.48	18.31	18.11
	2002	148.41	146.17	18.69	18.58
	2003	148.40	149.24	17.88	18.04
18-19	1999	254.11	257.54***	28.08	28.33
	2000	251.60	258.40***	27.42	28.14***
	2001	267.53	273.21**	29.52	29.84
	2002	264.35	261.60	29.51	29.23
	2003	286.20	282.26	30.10	29.69
20-25	1999	412.49	414.32*	33.99	34.03
	2000	414.13	413.10	33.57	33.42
	2001	421.58	423.02	33.83	33.96
	2002	404.45	405.56	33.35	33.35
	2003	416.69	416.71	33.83	33.76

Note. Data are from the HLFS-IS covering the period 1999q2-2003q2. Earnings values were deflated by the CPI (base year/quarter 1999q2). * 10%, ** 5%, *** 1% significance level difference in the means of usual and actual: i) real weekly earnings, and ii) weekly hours worked. Author's compilation.

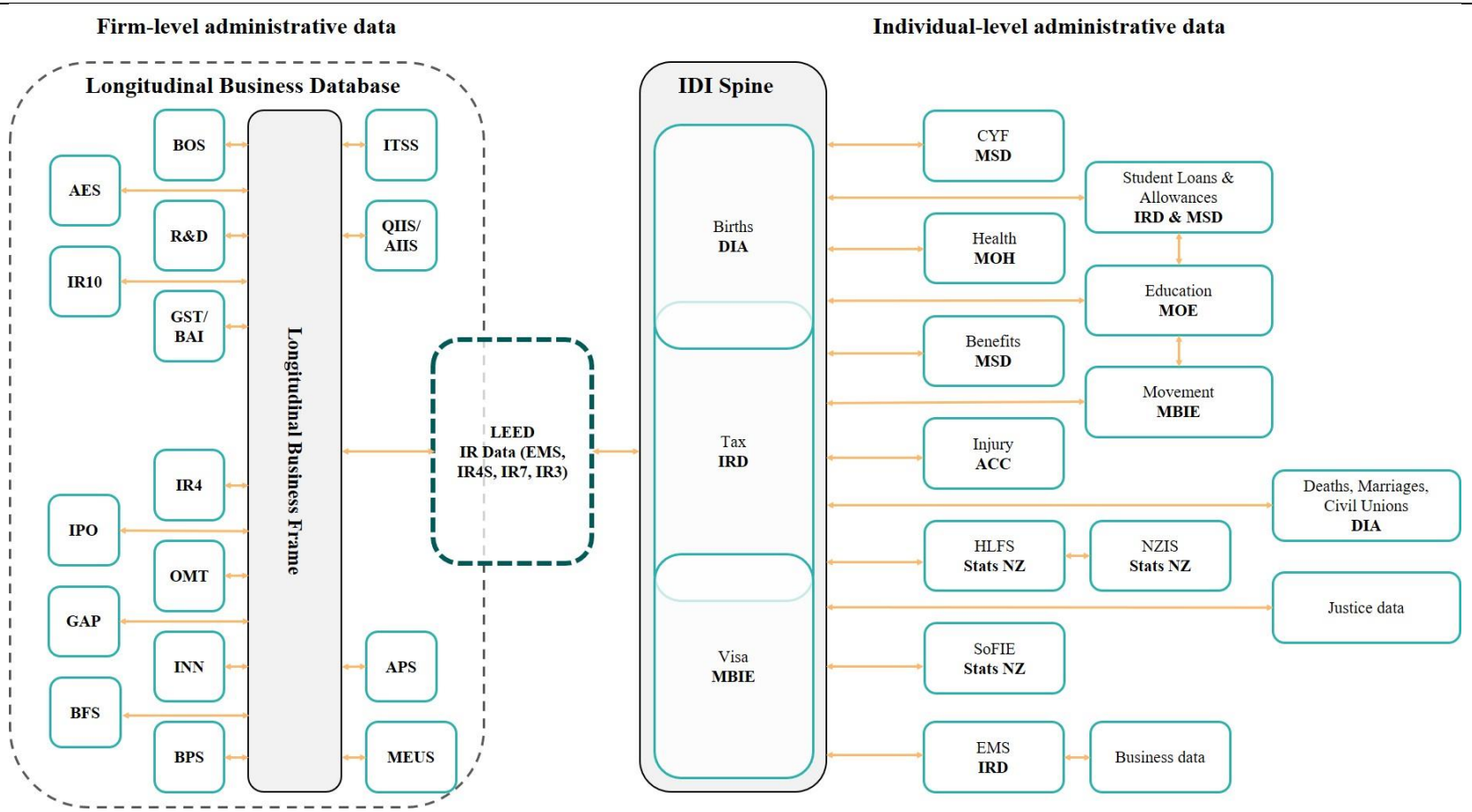


Figure B2: Structure of the IDI with firm- and individual-level administrative data.

Adapted from Fabling and Sanderson (2016). ACC: Accident Compensation Corporation; AES: Annual Enterprise Survey; APS: Agricultural Production Survey; BFS: The Business Finance Survey; BOS: Business Operations Survey; BPS: The Business Practice Survey; CYF: Child, Youth, and Family; DIA: Department of Internal Affairs; EMS: Employer Monthly Schedule; GAP: Government Assistance Programme; GST/BAI: Goods & Services Tax/ Business Activity Indicator; IR3: Individual Tax Return; IR4: Company Income Tax Return; IR4S: Company Shareholder Details; IR7: Income Tax Return for Partnerships; IR10: Tax-filed accounts information; IR45: IRD: Inland Revenue; INN: The Innovation Survey; IPO: Intellectual Property Office; ITS: International Trade in Services and Royalties Survey; MBIE: Ministry of Business, Innovation & Enterprise; MEUS: The Manufacturing Energy Use Survey; MOE: Ministry of Education; MOH: Ministry of Health; MSD: Ministry of Social Development; OMT: Merchandise Trade Survey; R&D: Research & Development & Innovation; SoFIE: Survey of Family, Income, and Employment. QIIS/AIIS: Quarterly/Annual International Investment Surveys; Author's compilation.

Table B2:

Minimum Wage Breaches as a Percentage of Total Breaches

	2015[a]	2016	2017	2018[b]
Total completed investigations[c]	335	867	635	385
Total breaches identified	222	596	469	248
Total breaches (as a % of all completed investigations)	66%	69%	74%	64%
Minimum wages breaches identified	32	99	81	25
Minimum wages (as a % of all breaches)	14%	17%	17%	10%

Note. Data from the Labour Inspectorate, MBIE. Data were provided through an Official Information Act request. Author's compilation.
[a, b] Figures for 2015 are for July-December; Figures for 2018 are for January-June; Other figures are full calendar year totals.
[c] Data on the total investigations pertaining to minimum wage were not provided.

Table B3:
Construction of the Survey Samples

(a) Base HLFS sample	
Initial number of person-quarter observations extracted (1997q1-2003q3)	1,036,773
Removed due to: Age < 15 years or age > 25 years	911,355
Total number of person-quarter observations remaining	125,418
(b) Base HLFS-IS sample	
Initial number of person-year observations extracted (1997q2-2003q2)	205,995
Removed due to: Age < 15 years or age > 25 years	174,624
Total number of person-quarter observations remaining	31,371

Note. Data are from HLFS-IS. Author's compilation.

Table B4:

Adjustments to Measures of Weekly Earnings Using Survey and Administrative Data

(a) Adjustments to HLFS-IS weekly earnings

Adjusted due to:	Number adjusted
Actual weekly earnings are missing and adjusted to mean earnings	S
Actual weekly earnings < 1st percentile of age group based earnings distribution	117
Actual weekly earnings > 99th percentile of age group based earnings distribution	0

(b) Adjustments to administrative weekly earnings

Adjusted due to:	Number adjusted
Actual weekly earnings are missing and adjusted to mean earnings	0
Actual weekly earnings < 1st percentile of age group based earnings distribution	40,305
Actual weekly earnings > 99th percentile of age group based earnings distribution	40,617

Note. Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999-2003 (months of April-June). Earnings values are deflated by the CPI (base year/quarter 1999q2). Real actual weekly earnings are from main job. 'S' = value suppressed due to suppression criteria outlined in Stats NZ (2020c). Author's compilation.

Table B5:

Adjustments to Measures of Hourly Wages Using Survey and Administrative Data

(a) Adjustments to HLFS-IS hourly wages

Adjusted due to:	Number adjusted
Derived hourly wage is missing and adjusted to mean hourly wage	S
Derived hourly wage < 1st percentile of age group based hourly wage distribution	0
Derived hourly wage > 99th percentile of age group based hourly wage distribution	117
Regular usual hourly wage is missing and adjusted to mean hourly wage	687
Regular usual hourly wage < 1st percentile of age group based hourly wage distribution	105
Regular usual hourly wage > 99th percentile of age group based hourly wage distribution	111
Actual hourly wage is missing and adjusted to mean hourly wage	S
Actual hourly wage < 1st percentile of age group based hourly wage distribution	0
Actual hourly wage > 99th percentile of age group based hourly wage distribution	117
Usual hourly wage is missing and adjusted to mean hourly wage	528
Usual hourly wage < 1st percentile of age group based hourly wage distribution	105
Usual hourly wage > 99th percentile of age group based hourly wage distribution	111

(b) Adjustments to administrative hourly wages

Adjusted due to:	Number adjusted
Actual hourly wage is missing and adjusted to mean hourly wage	0
Actual hourly wage < 1st percentile of age group based hourly wage distribution	40,530
Actual hourly wage > 99th percentile of age group based hourly wage distribution	40,635

Note. Survey data are from the HLFS-IS covering the period 1999q2-2003q2; Administrative sample covers the period 1999-2003 (months of April-June). Actual hourly wages are from main job. 'S' = value suppressed due to suppression criteria outlined in Stats NZ (2020c). Author's compilation.

Table B6:

Defining Alternative Measures of Hourly Wage Using the HLFS-IS

Measure	Definition	Source	Figure
Hourly wage SNZ	Actual hourly earnings from 1st wage and salary job[a]	Timmins (2006)	Figure 4.4
Regular usual hourly wage	$\frac{\text{(Usual total weekly earnings – usual overtime weekly earnings)}}{\text{(Usual total weekly hours – usual overtime weekly hours)}}$	Pacheco (2009)	Figure 4.4
Actual hourly wage from main wage and salary job (for survey and administrative measure)	$\frac{\text{Actual total weekly earnings}}{\text{Actual total weekly hours worked}}$	Author	Figure 4.4 Figure 4.7
Usual hourly wage from main wage and salary job	$\frac{\text{Usual total weekly earnings}}{\text{Usual total weekly hours worked}}$	Author	Figure 4.4

Note. Author's compilation.
[a] Source: Stats NZ (2015b).

Table B7:

Comparing Wage Group Characteristics Using a 20-cents Wage Band and Survey Data

Characteristics	Full sample	Sub-minimum	Minimum	Other
Percentage of full sample	100	2.06	2.86	95.08
Demographic				
Age	20.36	19.83	18.84	20.41
	(0.02)	(0.15)	(0.11)	(0.02)
16-17	.22	.21	.35	.22
	(0.00)	(0.02)	(0.02)	(0.00)
18-19	.20	.31	.32	.19
	(0.00)	(0.02)	(0.02)	(0.00)
20-25	.59	.48	.33	.59
	(0.00)	(0.03)	(0.02)	(0.00)
Female	.50	.47	.52	.50
	(0.00)	(0.03)	(0.02)	(0.00)
Pākehā	.63	.67	.72	.63
	(0.00)	(0.03)	(0.02)	(0.00)
Māori	.18	.21	.13	.18
	(0.00)	(0.02)	(0.01)	(0.00)
Pacific	.07	.04	.05	.08
	(0.00)	(0.01)	(0.01)	(0.00)
Other	.11	.08	.10	.11
	(0.00)	(0.02)	(0.01)	(0.00)
NZ Born	.83	.86	.86	.83
	(0.00)	(0.02)	(0.02)	(0.00)
Regional				
Northland	.03	.03	.03	.03
	(0.01)	(0.01)	(0.00)	(0.00)
Auckland	.23	.20	.34	.34
	(0.03)	(0.02)	(0.00)	(0.00)
Waikato	.09	.13	.10	.10
	(0.02)	(0.02)	(0.00)	(0.00)
Bay of Plenty	.06	.08	.05	.05
	(0.01)	(0.01)	(0.00)	(0.00)
Gisborne/Hawkes Bay	.06	.06	.04	.04
	(0.01)	(0.01)	(0.00	(0.00)
Taranaki	.05	.03	.02	.02
	(0.01)	(0.01)	(0.00)	(0.00)
Manawatu-Wanganui	.06	.04	.06	.06
	(0.01)	(0.01)	(0.00)	(0.00)
Wellington	.11	.10	.11	.11
	(0.02)	(0.01)	(0.00)	(0.00)
Nelson-Tasman-Marlborough-West Coast	.04	.04	.03	.03
	(0.01)	(0.01)	(0.00)	(0.00)
Canterbury	.17	.20	.13	.13
	(0.02)	(0.02)	(0.00)	(0.00)
Otago	.06	.07	.07	.07
	(0.01)	(0.01)	(0.00)	(0.00)
Southland	.03	.04	.02	.02
	(0.01)	(0.01)	(0.00)	(0.00)

(continues)

Characteristics	Full sample	Sub-minimum	Minimum	Other
Employment				
Full-time	.36	.48	.39	.36
	(0.00)	(0.03)	(0.02)	(0.00)
Actual total weekly hours	17.69	27.69	23.82	17.33
	(0.15)	(1.03)	(0.76)	(0.16)
Hourly earnings as a proportion of the age relevant minimum wage	0.93	0.71	0.98	0.93
	(0.01)	(0.01)	(0.00)	(0.01)
Real actual weekly earnings from main job ($)[a]	206.34	140.22	165.05	208.76
	(2.26)	(6.38)	(5.59)	(2.35)
Data				
Proxy	.03	.07	.05	.03
	(0.00)	(0.01)	(0.01)	(0.00)
Imputed	.13	.19	.14	.13
	(0.00)	(0.02)	(0.02)	(0.00)
Observations	22,128	456	633	21,039

Note. Survey data are from the HLFS-IS covering the period 1999q2-2003q2. Earnings values are deflated by the CPI (base year/quarter 1999q2). Means were weighted using HLFS-IS sampling weights. Ethnicity dummy variables were constructed by adopting the prioritised ethnicity approach by Meehan et al. (2019). Standard errors are in parentheses. Author's compilation.

[a] Real actual weekly earnings from main job include overtime earnings from main job.

Table B8:

Comparing Wage Group Characteristics Using a 100-cents Wage Band and Survey Data

Characteristics	Full sample	Sub-minimum	Minimum	Other
Percentage of full sample	100	1.25	8.77	89.99
Demographic				
Age	20.36	20.21	19.26	20.46
	(0.02)	(0.20)	(0.07)	(0.02)
16-17	.22	.17	.29	.21
	(0.00)	(0.02)	(0.01)	(0.00)
18-19	.20	.28	.30	.19
	(0.00)	(0.03)	(0.01)	(0.00)
20-25	.59	.56	.40	.60
	(0.00)	(0.03)	(0.01)	(0.00)
Female	.50	.50	.52	.50
	(0.00)	(0.04)	(0.01)	(0.00)
Pākehā	.63	.69	.69	.62
	(0.00)	(0.03)	(0.01)	(0.00)
Māori	.18	.19	.17	.18
	(0.00)	(0.03)	(0.01)	(0.00)
Pacific	.07	.03	.06	.08
	(0.00)	(0.01)	(0.01)	(0.00)
Other	.11	.09	.08	.11
	(0.00)	(0.02)	(0.01)	(0.00)
NZ Born	.83	.85	.87	.83
	(0.00)	(0.03)	(0.01)	(0.00)
Regional				
Northland	.03	.04	.02	.03
	(0.00)	(0.01)	(0.00)	(0.00)
Auckland	.34	.23	.22	.35
	(0.00)	(0.04)	(0.01)	(0.00)
Waikato	.10	.09	.12	.10
	(0.00)	(0.02)	(0.01)	(0.00)
Bay of Plenty	.05	.05	.08	.05
	(0.00)	(0.01)	(0.01)	(0.00)
Gisborne/Hawkes Bay	.04	.06	.06	.04
	(0.00)	(0.02)	(0.01)	(0.00)
Taranaki	.02	.05	.03	.02
	(0.00)	(0.01)	(0.00)	(0.00)
Manawatu-Wanganui	.06	.06	.05	.06
	(0.00)	(0.01)	(0.01)	(0.00)
Wellington	.11	.10	.09	.12
	(0.00)	(0.02)	(0.01)	(0.00)
Nelson-Tasman-Marlborough-West Coast	.03	.04	.04	.03
	(0.00)	(0.01)	(0.00)	(0.00)
Canterbury	.13	.17	.18	.13
	(0.00)	(0.02)	(0.01)	(0.00)
Otago	.07	.07	.08	.07
	(0.00)	(0.01)	(0.01)	(0.00)
Southland	.02	.04	.03	.02
	(0.00)	(0.01)	(0.00)	(0.00)

(continues)

Characteristics	Full sample	Sub-minimum	Minimum	Other
Employment				
Full-time	.36	.47	.43	.35
	(0.00)	(0.04)	(0.01)	(0.00)
Actual total weekly hours	17.69	27.69	25.01	16.92
	(0.15)	(1.29)	(0.42)	(0.16)
Hourly earnings as a proportion of the age relevant minimum wage	0.93	0.61	1.04	0.93
	(0.01)	(0.02)	(0.00)	(0.01)
Real actual weekly earnings from main job ($)[a]	206.34	121.12	183.27	209.47
	(2.26)	(7.33)	(3.30)	(2.47)
Data				
Proxy	.03	.08	.03	.03
	(0.00)	(0.02)	(0.00)	(0.00)
Imputed	.13	.16	.14	.13
	(0.00)	(0.03)	(0.01)	(0.00)
Observations	22,128	276	1,941	19,914

Note. Survey data are from the HLFS-IS covering the period 1999q2-2003q2. Earnings values are deflated by the CPI (base year/quarter 1999q2). Means were weighted using HLFS-IS sampling weights. Ethnicity dummy variables were constructed by adopting the prioritised ethnicity approach by Meehan et al. (2019). Standard errors are in parentheses. Author's compilation.

[a] Real actual weekly earnings from main job include overtime earnings from main job.

Table B9:

Comparing Wage Group Characteristics Using a 20-cents Wage Band and Administrative Data

Characteristics	Full sample	Sub-minimum	Minimum	Other
Percentage of full sample	100	15.00	1.58	83.42
Demographic				
Age	20.45	19.87	19.35	20.57
	(0.00)	(0.00)	(0.01)	(0.00)
16-17	.20	.23	.36	.20
	(0.00)	(0.00)	(0.00)	(0.00)
18-19	.20	.24	.21	.20
	(0.00)	(0.00)	(0.00)	(0.00)
20-25	.59	.53	.43	.61
	(0.00)	(0.00)	(0.00)	(0.00)
Female	.50	.54	.52	.49
	(0.00)	(0.00)	(0.00)	(0.00)
Pākehā	.53	.68	.67	.50
	(0.00)	(0.00)	(0.00)	(0.00)
Māori	.18	.17	.18	.18
	(0.00)	(0.00)	(0.00)	(0.00)
Pacific	.07	.06	.07	.07
	(0.00)	(0.00)	(0.00)	(0.00)
Other	.10	.09	.08	.10
	(0.00)	(0.00)	(0.00)	(0.00)
NZ Born	.82	.89	.90	.81
	(0.00)	(0.00)	(0.00)	(0.00)
Regional				
Northland	.03	.02	.03	.03
	(0.00)	(0.00)	(0.00)	(0.00)
Auckland	.27	.25	.24	.28
	(0.00)	(0.00)	(0.00)	(0.00)
Waikato	.11	.10	.11	.11
	(0.00)	(0.00)	(0.00)	(0.00)
Bay of Plenty	.07	.07	.08	.07
	(0.00)	(0.00)	(0.00)	(0.00)
Gisborne/Hawkes Bay	.05	.05	.06	.05
	(0.00)	(0.00)	(0.00)	(0.00)
Taranaki	.03	.03	.03	.03
	(0.00)	(0.00)	(0.00)	(0.00)
Manawatu-Wanganui	.06	.06	.06	.06
	(0.00)	(0.00)	(0.00)	(0.00)
Wellington	.12	.14	.13	.12
	(0.00)	(0.00)	(0.00)	(0.00)
Nelson-Tasman-Marlborough-West Coast	.03	.03	.04	.03
	(0.00)	(0.00)	(0.00)	(0.00)
Canterbury	.13	.14	.14	.13
	(0.00)	(0.00)	(0.00)	(0.00)
Otago	.06	.08	.06	.06
	(0.00)	(0.00)	(0.00)	(0.00)
Southland	.02	.02	.03	.02
	(0.00)	(0.00)	(0.00)	(0.00)

(continues)

Characteristics	Full sample	Sub-minimum	Minimum	Other
Employment				
Full-time	.66	.62	.52	.68
	(0.00)	(0.00)	(0.00)	(0.00)
Actual total weekly hours	30.00	29.46	27.53	30.14
	(0.00)	(0.01)	(0.02)	(0.00)
Hourly earnings as a proportion of the age relevant minimum wage	1.61	0.46	0.96	2.16
	(0.00)	(0.00)	(0.00)	(0.00)
Real actual weekly earnings from main job ($)[a]	345.59	92.57	178.37	468.39
	(0.12)	(0.06)	(0.23)	(0.12)
Data				
Observations	8,188,191	1,228,026	129,603	6,830,562

Note. Administrative sample covers the period 1999-2003 (months of April-June). Earnings values are deflated by the CPI (base year/quarter 1999q2). Ethnicity dummy variables were constructed by adopting the prioritised ethnicity approach by Meehan et al. (2019). Standard errors are in parentheses. Author's compilation.

[a] Real actual weekly earnings from main job include overtime earnings from main job.

Table B10:

Comparing Wage Group Characteristics Using a 100-cents Wage Band and Administrative Data

Characteristics	Full sample	Sub-minimum	Minimum	Other
Percentage of full sample	100	13.24	5.05	81.71
Demographic				
Age	20.45	19.94	19.51	20.59
	(0.00)	(0.00)	(0.00)	(0.00)
16-17	0.20	0.21	0.31	0.20
	(0.00)	(0.00)	(0.00)	(0.00)
18-19	0.20	0.24	0.23	0.20
	(0.00)	(0.00)	(0.00)	(0.00)
20-25	0.59	0.55	0.45	0.61
	(0.00)	(0.00)	(0.00)	(0.00)
Female	0.50	0.54	0.51	0.49
	(0.00)	(0.00)	(0.00)	(0.00)
Pākehā	0.53	0.68	0.67	0.50
	(0.00)	(0.00)	(0.00)	(0.00)
Māori	0.18	0.17	0.18	0.18
	(0.00)	(0.00)	(0.00)	(0.00)
Pacific	0.07	0.06	0.07	0.07
	(0.00)	(0.00)	(0.00)	(0.00)
Other	0.10	0.09	0.08	0.10
	(0.00)	(0.00)	(0.00)	(0.00)
NZ Born	0.82	0.89	0.90	0.81
	(0.00)	(0.00)	(0.00)	(0.00)
Regional				
Northland	0.03	0.02	0.03	0.03
	(0.00)	(0.00)	(0.00)	(0.00)
Auckland	0.27	0.25	0.24	0.28
	(0.00)	(0.00)	(0.00)	(0.00)
Waikato	0.11	0.10	0.11	0.11
	(0.00)	(0.00)	(0.00)	(0.00)
Bay of Plenty	0.07	0.07	0.08	0.07
	(0.00)	(0.00)	(0.00)	(0.00)
Gisborne/Hawkes Bay	0.05	0.05	0.06	0.05
	(0.00)	(0.00)	(0.00)	(0.00)
Taranaki	0.03	0.03	0.03	0.03
	(0.00)	(0.00)	(0.00)	(0.00)
Manawatu-Wanganui	0.06	0.06	0.06	0.06
	(0.00)	(0.00)	(0.00)	(0.00)
Wellington	0.12	0.14	0.13	0.12
	(0.00)	(0.00)	(0.00)	(0.00)
Nelson-Tasman-Marlborough-West Coast	0.03	0.03	0.04	0.03
	(0.00)	(0.00)	(0.00)	(0.00)
Canterbury	0.13	0.14	0.14	0.13
	(0.00)	(0.00)	(0.00)	(0.00)
Otago	0.06	0.08	0.06	0.06
	(0.00)	(0.00)	(0.00)	(0.00)
Southland	0.02	0.02	0.03	0.02
	(0.00)	(0.00)	(0.00)	(0.00)

(continues)

Characteristics	Full sample	Sub-minimum	Minimum	Other
Employment				
Full-time	0.66	0.63	0.56	0.68
	(0.00)	(0.00)	(0.00)	(0.00)
Actual total weekly hours	30.00	29.71	28.16	30.16
	(0.00)	(0.01)	(0.01)	(0.00)
Hourly earnings as a proportion of the age relevant minimum wage	1.61	0.41	0.97	2.22
	(0.00)	(0.00)	(0.00)	(0.00)
Real actual weekly earnings from main job ($)[a]	345.59	84.34	182.75	482.19
	(0.12)	(0.05)	(0.13)	(0.12)
Data				
Observations	8,188,191	1,084,173	413,712	6,690,306

Note. Administrative sample covers the period 1999-2003 (months of April-June). Earnings values are deflated by the CPI (base year/quarter 1999q2). Ethnicity dummy variables were constructed by adopting the prioritised ethnicity approach by Meehan et al. (2019). Standard errors are in parentheses. Author's compilation.

[a] Real actual weekly earnings from main job include overtime earnings from main job.

Appendix C.

Table C1:

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]

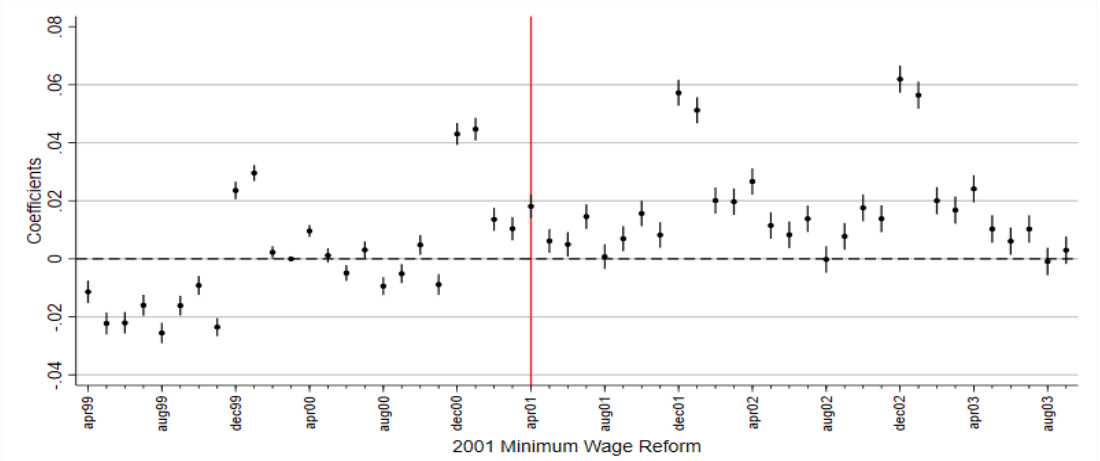
Note. Author's compilation.

[a] Ethnicities were prioritised using the framework from Meehan et al. (2019).

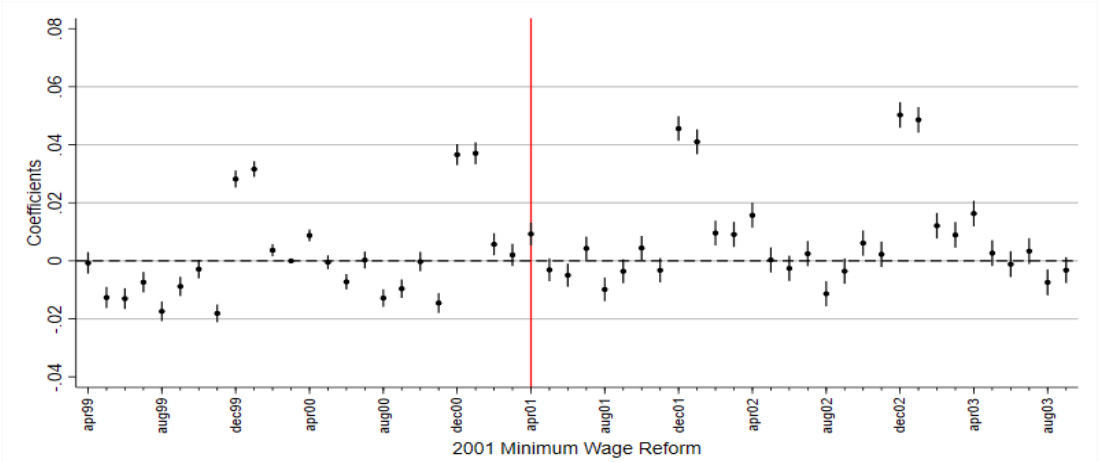
[b] Data were sourced from New Zealand Post (n.d.).

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

(a) Specification: Base[a]



(b) Specification: Demographics[b]



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

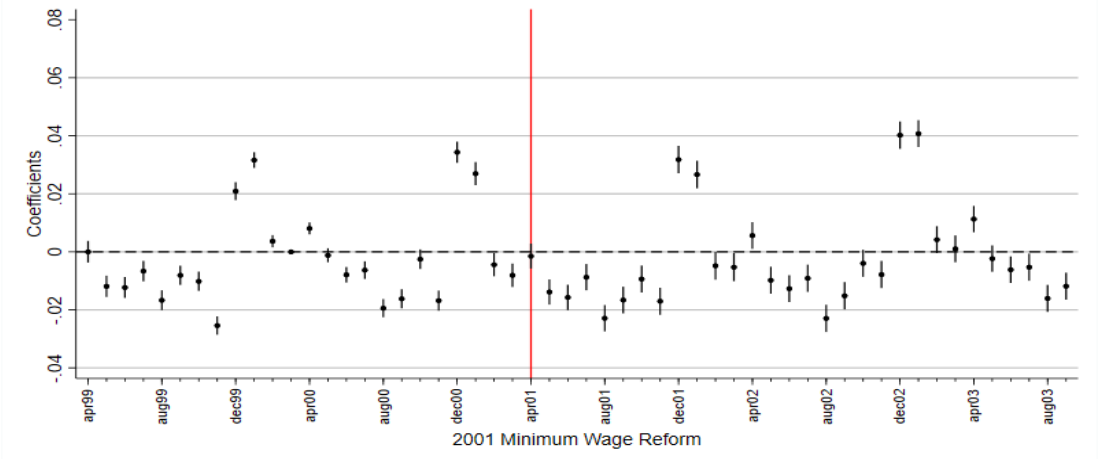
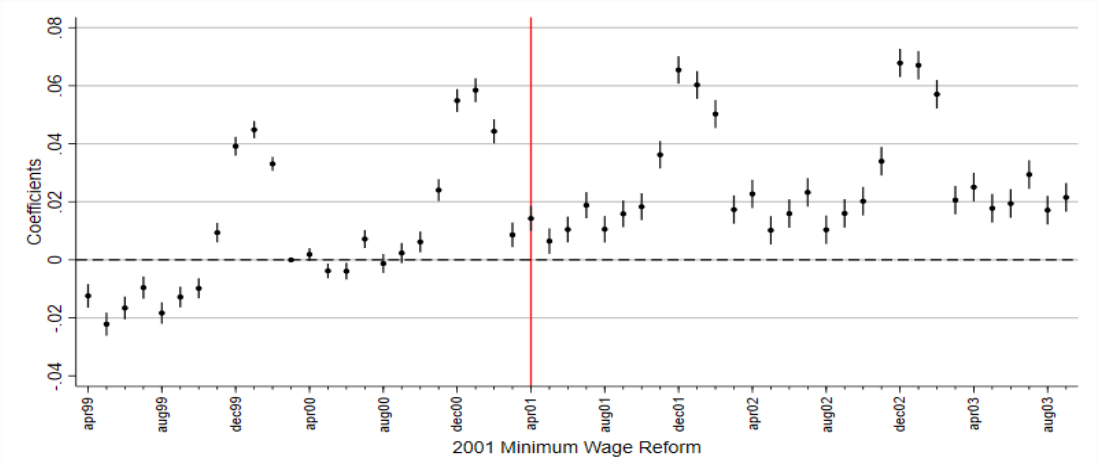
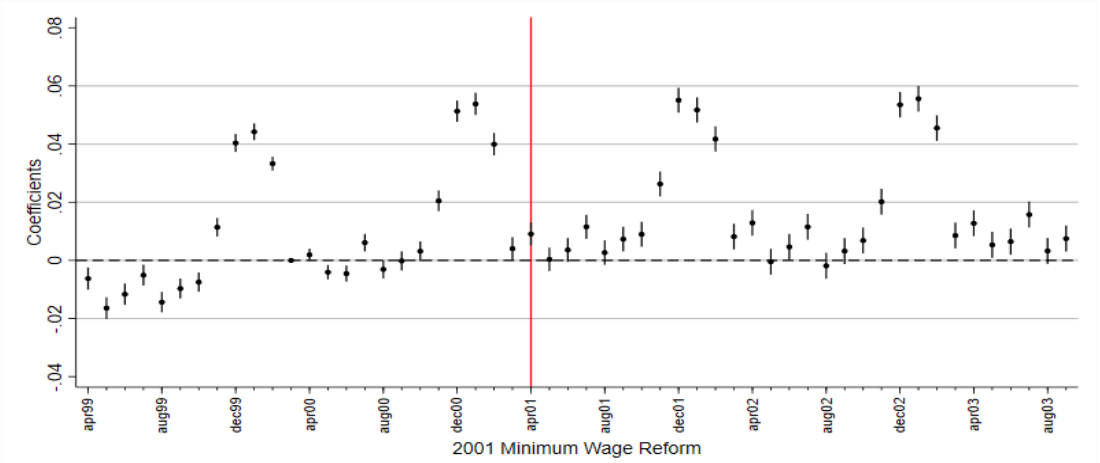


Figure C1: Testing for parallel trends in employment. 16-17 & 20-25-year-olds.
The sample covers the period April 1999-September 2003. All specifications were estimated by OLS on 23,462,325 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation 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]

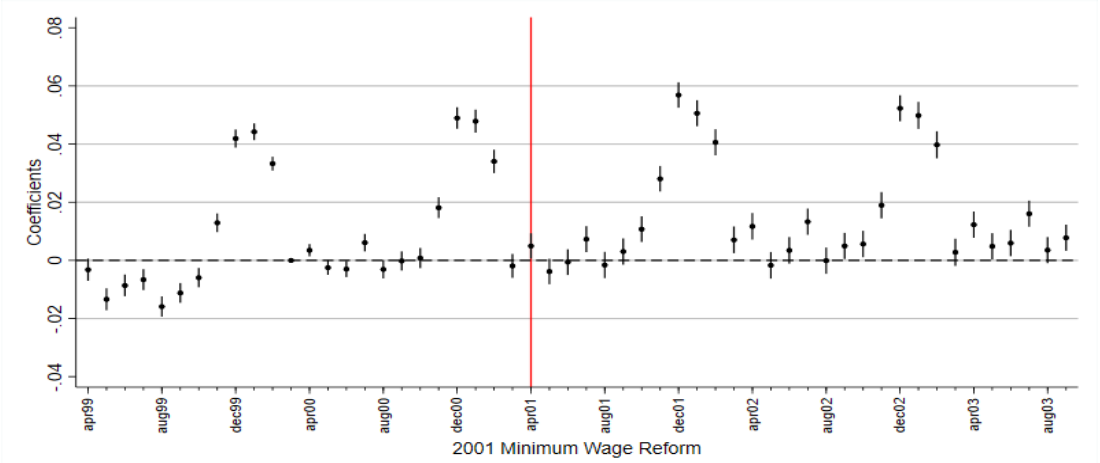
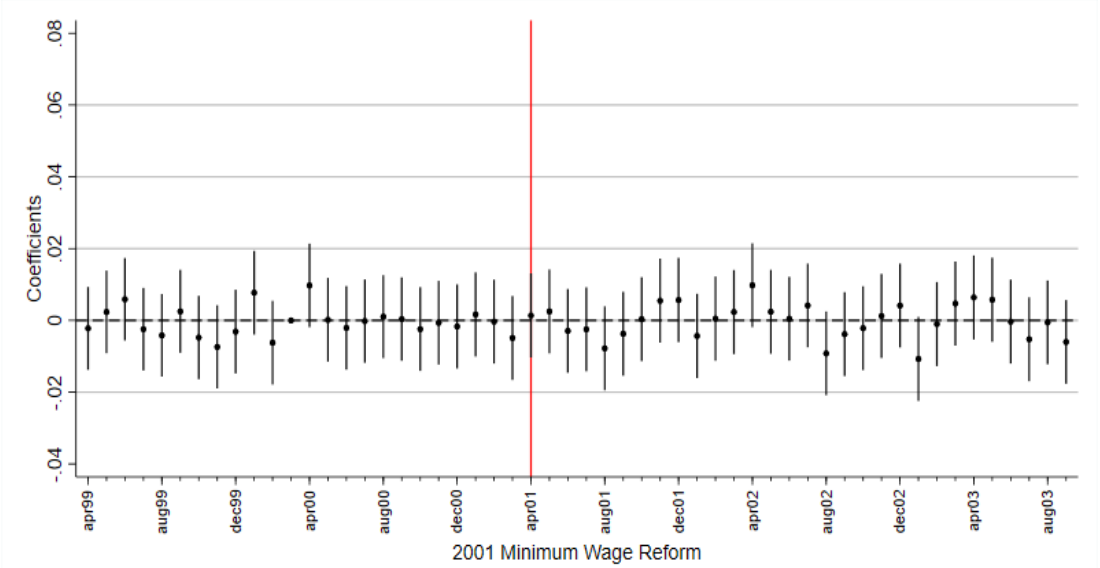


Figure C2: Testing for parallel trends in employment. 18-19 & 20-25-year-olds.
The sample covers the period April 1999-September 2003. All specifications were estimated by OLS on 23,442,792 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation 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]

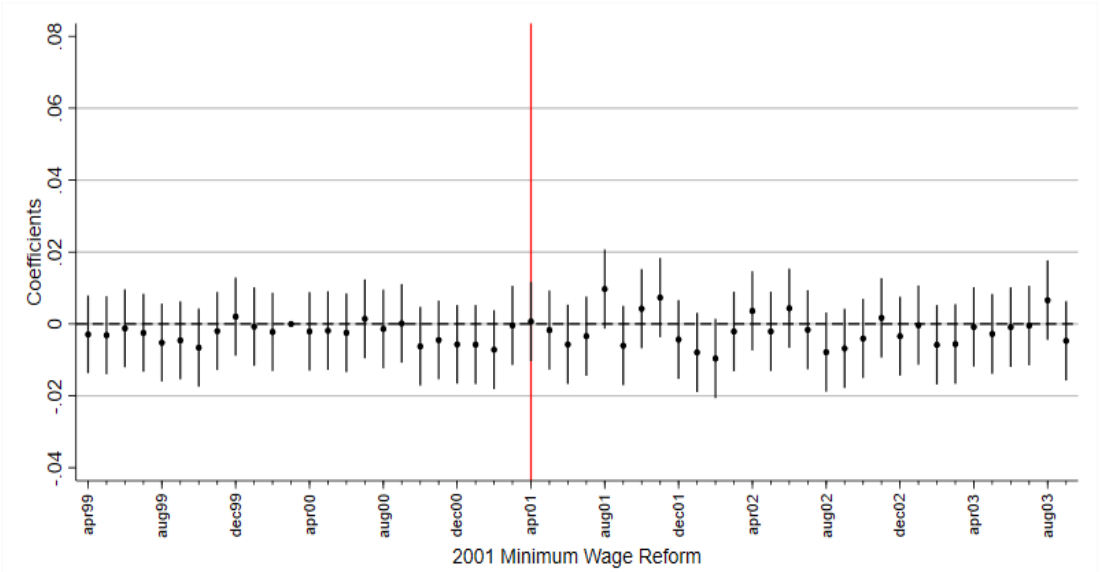
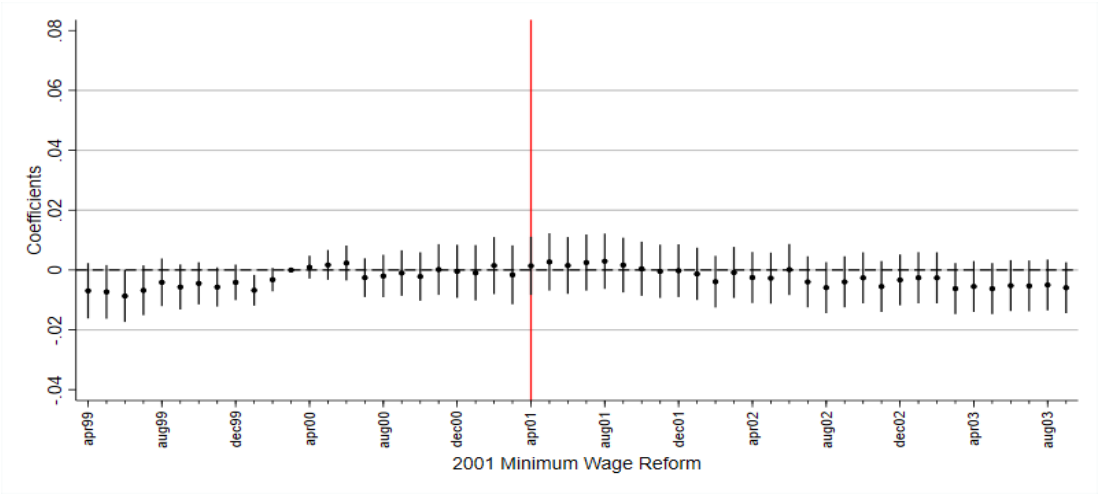
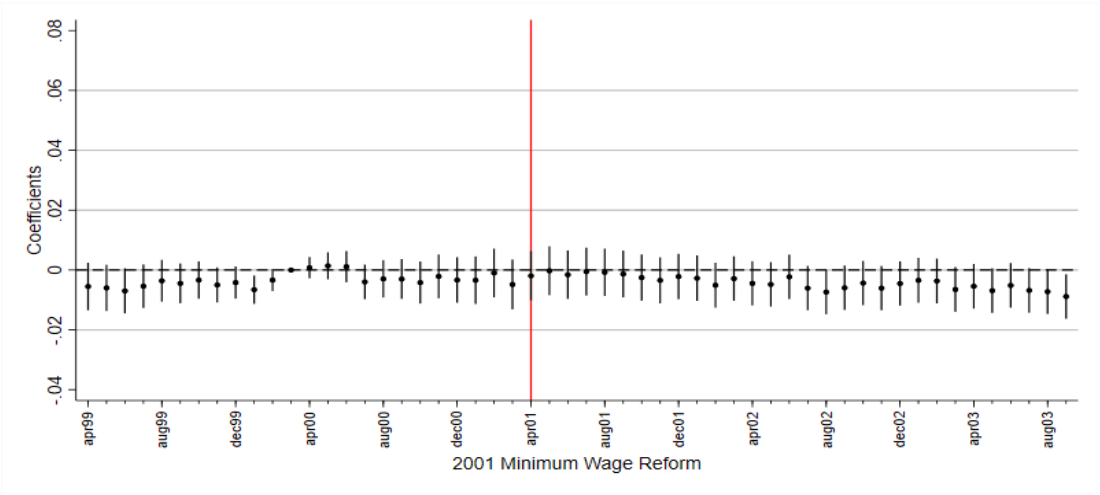


Figure C3: Testing for parallel trends in employment, 17-year-olds.
The sample covers the period April 1999-September 2003. All specifications were estimated by OLS on 2,994,276 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation within 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]

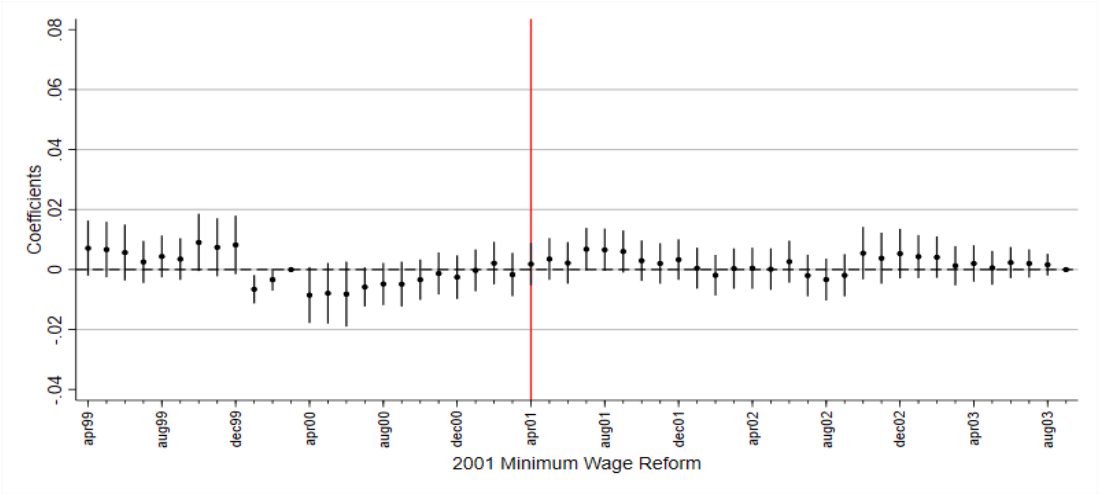


Figure C4: Testing for parallel trends in employment, 24 and 25-year-olds.
The sample covers the period April 1999-September 2003. All specifications are estimated by OLS on 5,685,342 observations. Huber-White robust standard errors were included. These were clustered by UID, allowing for correlation 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).

Appendix D.

Table D1:

Construction of Individual-Year Age Cohorts using Administrative Data

	Individual-year cohorts					
	2003	2004	2005	2006	2007	2003-2007
Period covered in individual-year cohort	2001-2005	2002-2006	2003-2007	2004-2008	2005-2009	2001-2009
Total unique records extracted	731,379	725,517	735,912	745,695	724,284	
Removed:						
Non-NZ	678,720	671,910	679,803	687,699	665,232	
Death	927	999	978	1,020	1,008	
Non-residency status	8,508	8,931	9,828	10,722	10,890	
Total unique individuals	43,224	43,677	45,303	46,254	47,154	225,612
Total observations	2,118,024	2,140,125	2,219,847	2,266,446	2,310,594	11,055,036

Note. Data extracted from IDI using the October 2020 refresh. Author's compilation.

Table D2:

Adjustments to Administrative Monthly Earnings

	Individual-year cohorts				
	2003	2004	2005	2006	2007
Adjustments					
Replace missing records	1,009,344	994,050	1,022,328	1,040,988	1,090,053
Adjust to 1st Percentile	7,518	7,908	8,124	8,331	8,436
Adjust to 99th Percentile	4,377	4,494	4,554	4,677	4,587
Observations	2,118,024	2,140,125	2,219,847	2,266,446	2,310,594

Note. Adjustments were made to wage and salary records from the IRD EMS data merged with the administrative sample. Author's compilation.

Table D3:

Variable Definitions and Sources Utilised in Regression Discontinuity Analysis

Variable	Definition	Source
Monthly earnings	Real gross monthly wage or salary payments from main job	IRD: EMS
Weekly earnings	Monthly earnings / number of weeks in the corresponding month.	Generated
Employed	Dummy variable = 1 if earning wages or salary; 0 otherwise.	IRD: EMS
Age in years	birth date – return period / 365.25.	Generated
Female	Dummy variable = 1 if female; 0 otherwise.	Personal details
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, Other and Not specified.	Personal details

Note. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021). Author's compilation.

[a] Ethnicities were prioritised using the framework from Meehan et al. (2019).

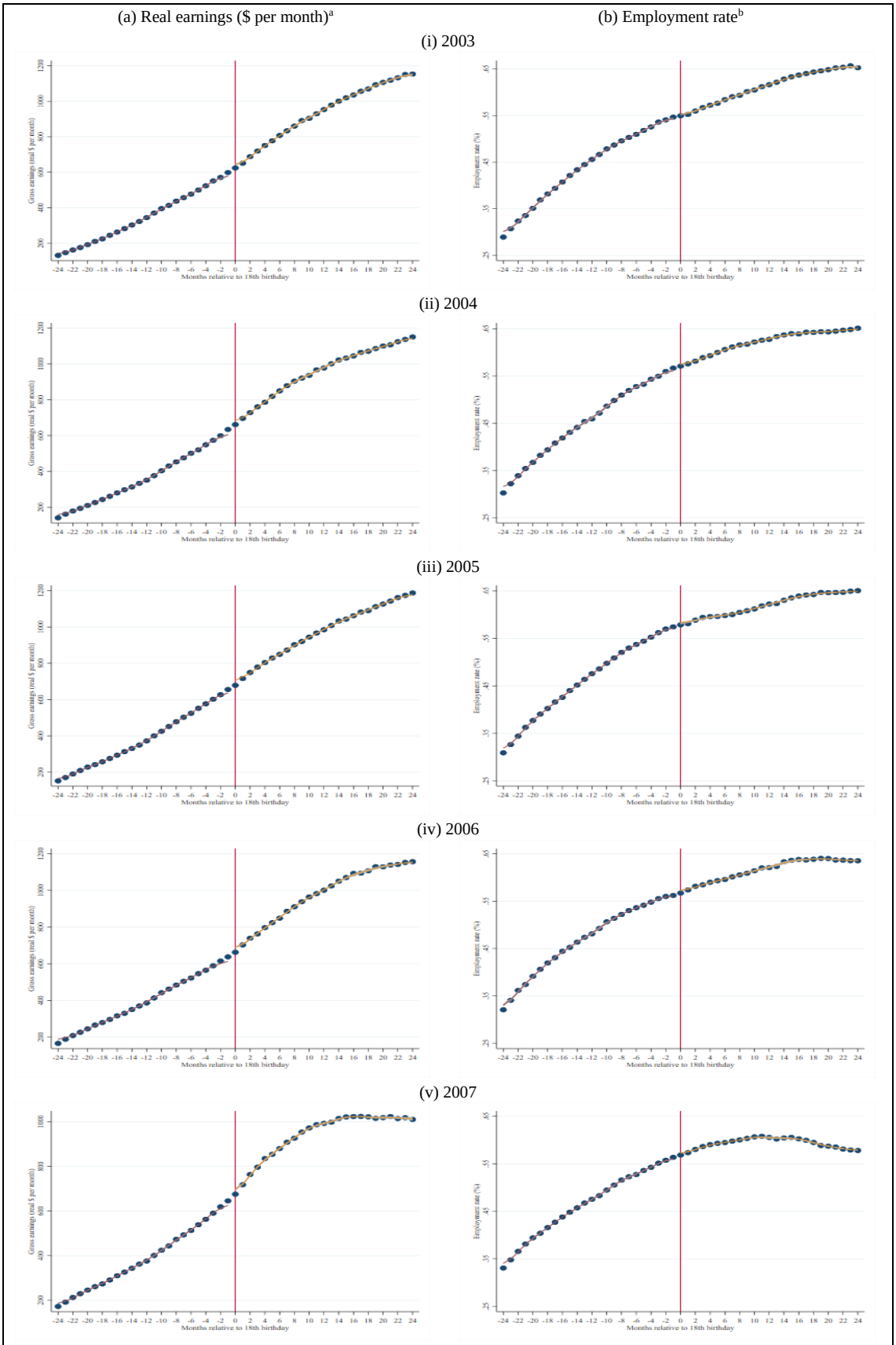


Figure D1: Real earnings and employment rate by individual-year age cohort.
The sample period covers January 2001-December 2009. Author's compilation.
[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021). Values on the y-axis start at $200 per month to clearly highlight any variation in the real average earnings over the months relative to workers' 18th birthday.
[b] Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthday.

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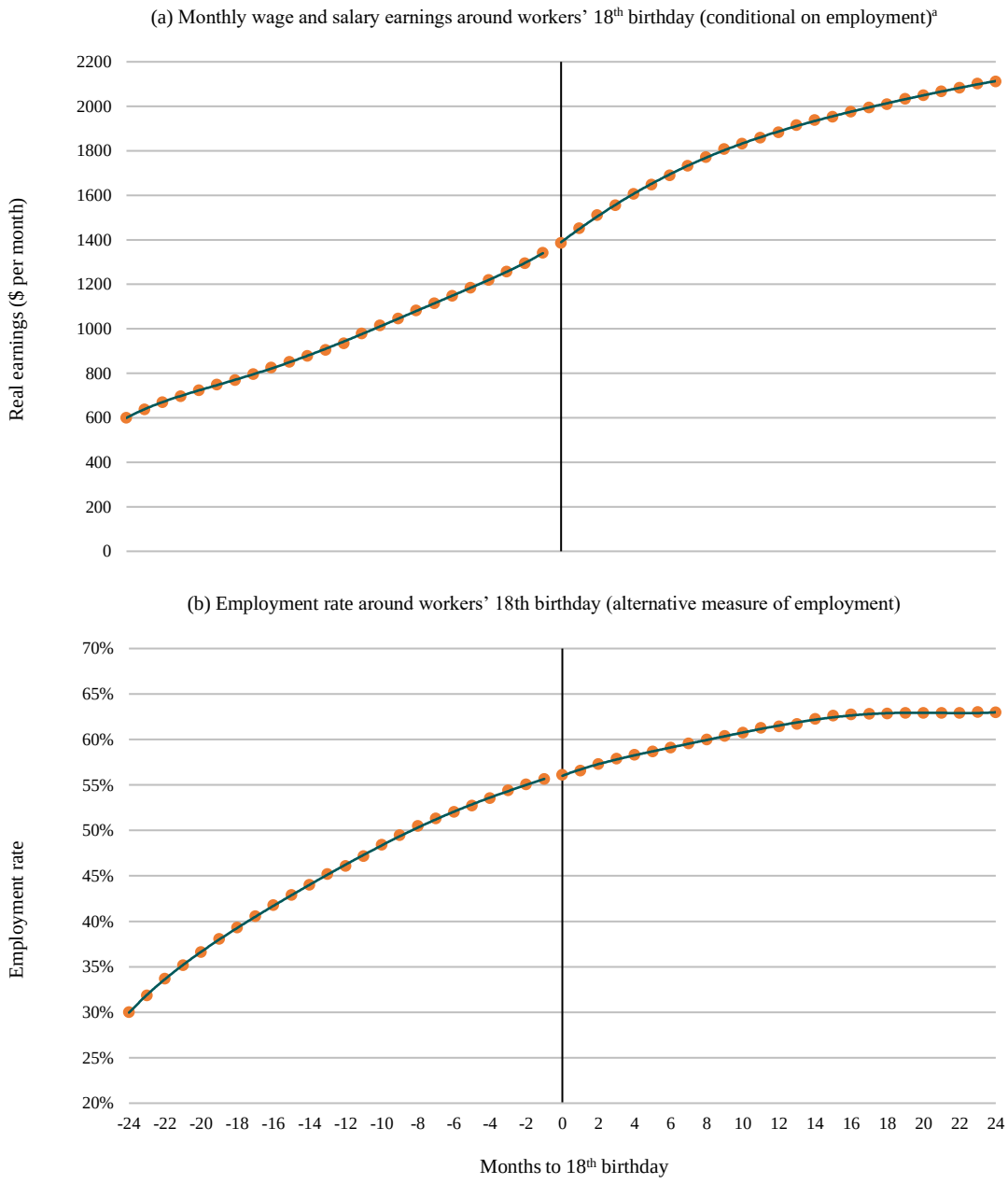

Figure D2: Alternative measures of monthly earnings and employment rates around workers' 18th birthday.

The sample period covers January 2001-December 2009. Author's compilation.

[a] The sample was restricted to individuals who were classified as employed in the reference month. Nominal earnings were adjusted using the CPI (base month and year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021). Values on the y-axis start at $200 per month to clearly highlight any variation in the real average earnings from employment over the months relative to workers' 18th birthday.

[b] Individuals were classified as employed if they had a wage or salary record greater than or equal to $50 in the reference month. Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthday.

Table D4:

Regression Estimates from Testing Lower-Order Polynomials

	Polynomial in age of degree, D =			
	1	2	3	4
Monthly earnings[a]				
Coefficient[b]	122.13***	122.13***	36.75***	36.75***
	(7.76)	(7.76)	(10.57)	(10.52)
	[106.92, 137.34]	[106.92, 137.34]	[16.02, 57.47]	[16.12, 57.37]
Percent change	0.15***	0.15***	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.13, 0.17]	[0.13, 0.17]	[0.02, 0.07]	[0.02, 0.07]
Employment				
Coefficient	-1.12***	-1.12***	-0.50	-0.50
	(0.23)	(0.23)	(0.31)	(0.31)
	[-1.57, -0.67]	[-1.57, -0.67]	[-1.11, 0.11]	[-1.11, 0.11]
Percent change	-0.02***	-0.02***	-0.01	-0.01
	(0.00)	(0.00)	(0.01)	(0.01)
	[-0.03, -0.01]	[-0.03, -0.01]	[-0.02, 0.00]	[-0.02, 0.00]
Implied elasticity[c]	-0.00	-0.00	-0.00	-0.00

Note. The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS on 11,055,036 observations. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5%, and 1% level, respectively. Standard errors (in parenthesis) are clustered by (monthly) birth cohort. 95% confidence intervals are provided in square brackets. Author's compilation.

[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).

[b] This is the coefficient of interest which measures the effect of the discontinuity ψ in specification (10).

[c] The implied elasticity is calculated by dividing the percentage change in employment by the average percentage change in the real statutory minimum wage discontinuity between 2001q2 – 2008q1 (27%).

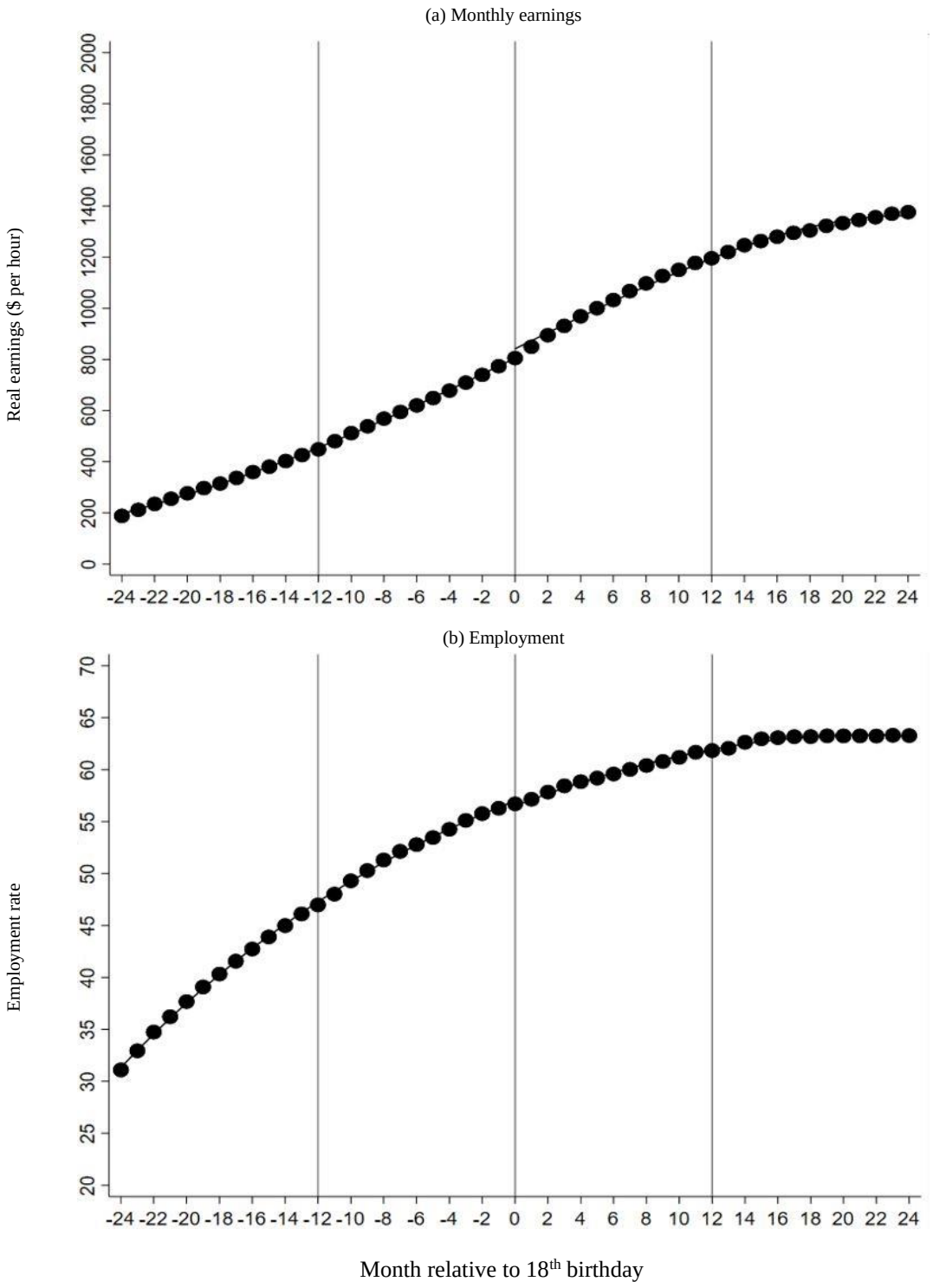

Figure D3: Testing lower-order polynomial (in age of degree 4).
The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS on 11,055,036 observations. Author's compilation.
[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).
[b] Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthday.

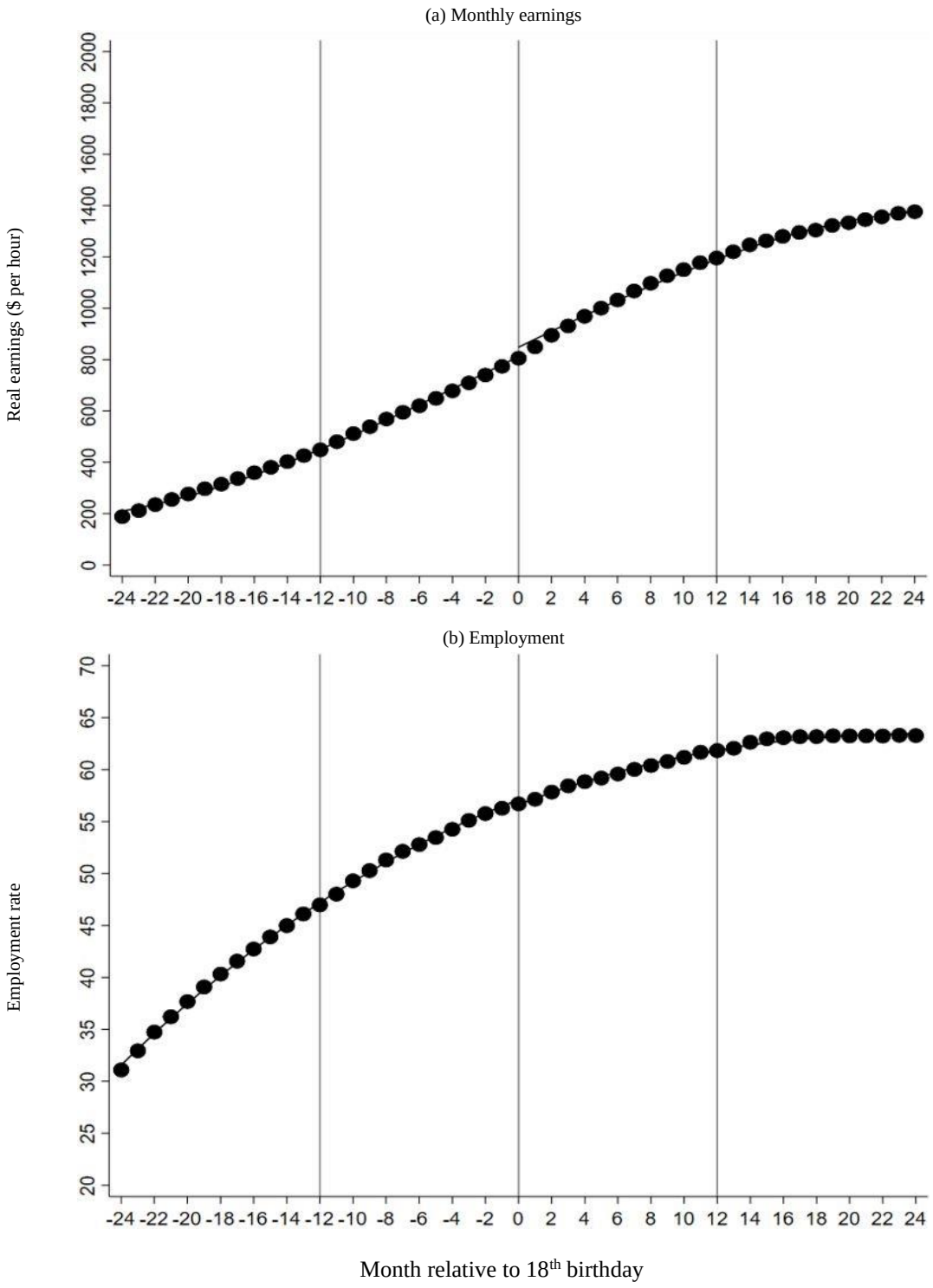

Figure D4: Testing lower-order polynomial (in age of degree 3).
The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS on 11,055,036 observations. Author's compilation.
[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).
[b] Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthday.

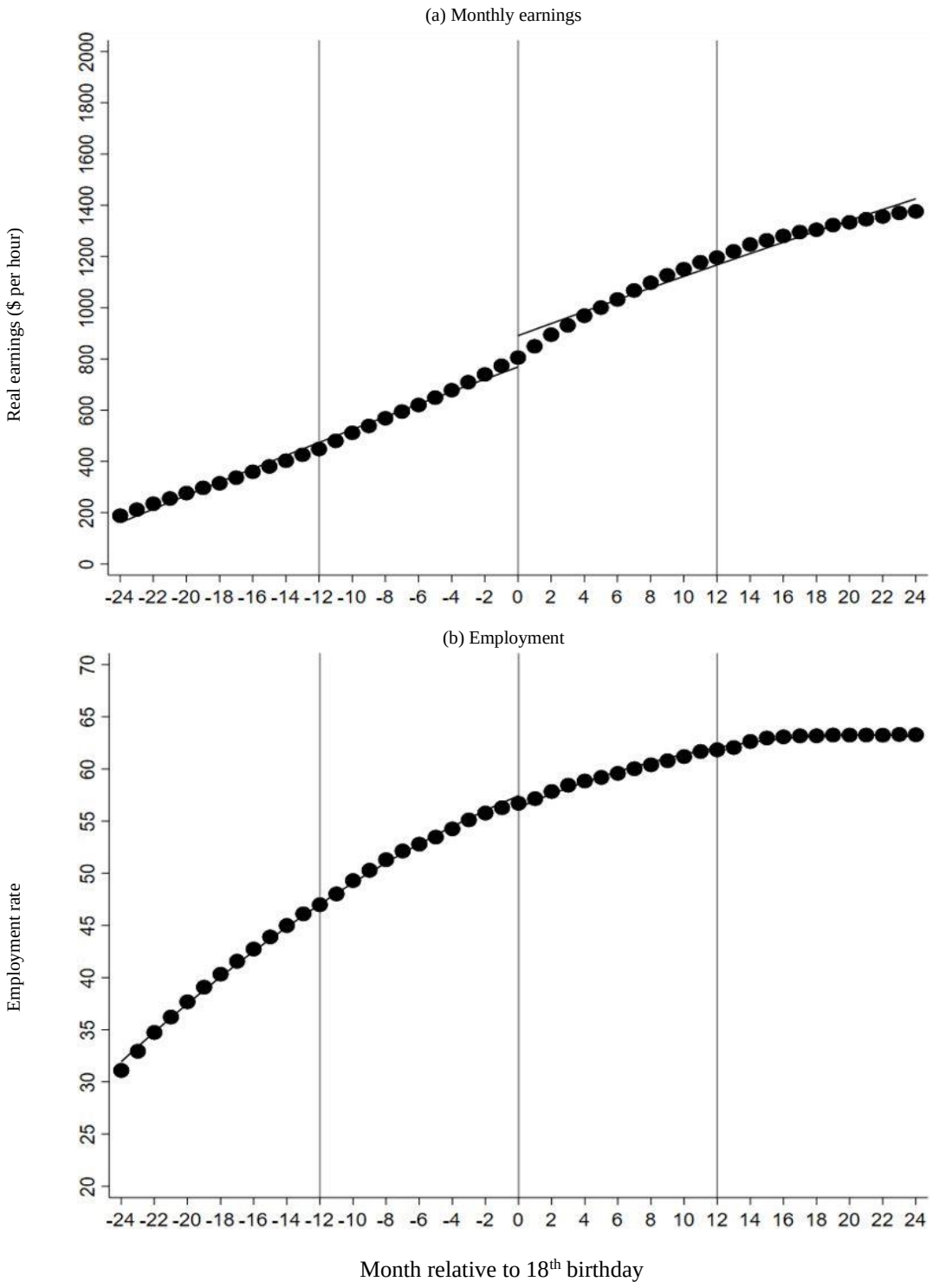

Figure D5: Testing lower-order polynomial (in age of degree 2).
The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS on 11,055,036 observations. Author's compilation.
[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).
[b] Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthday.

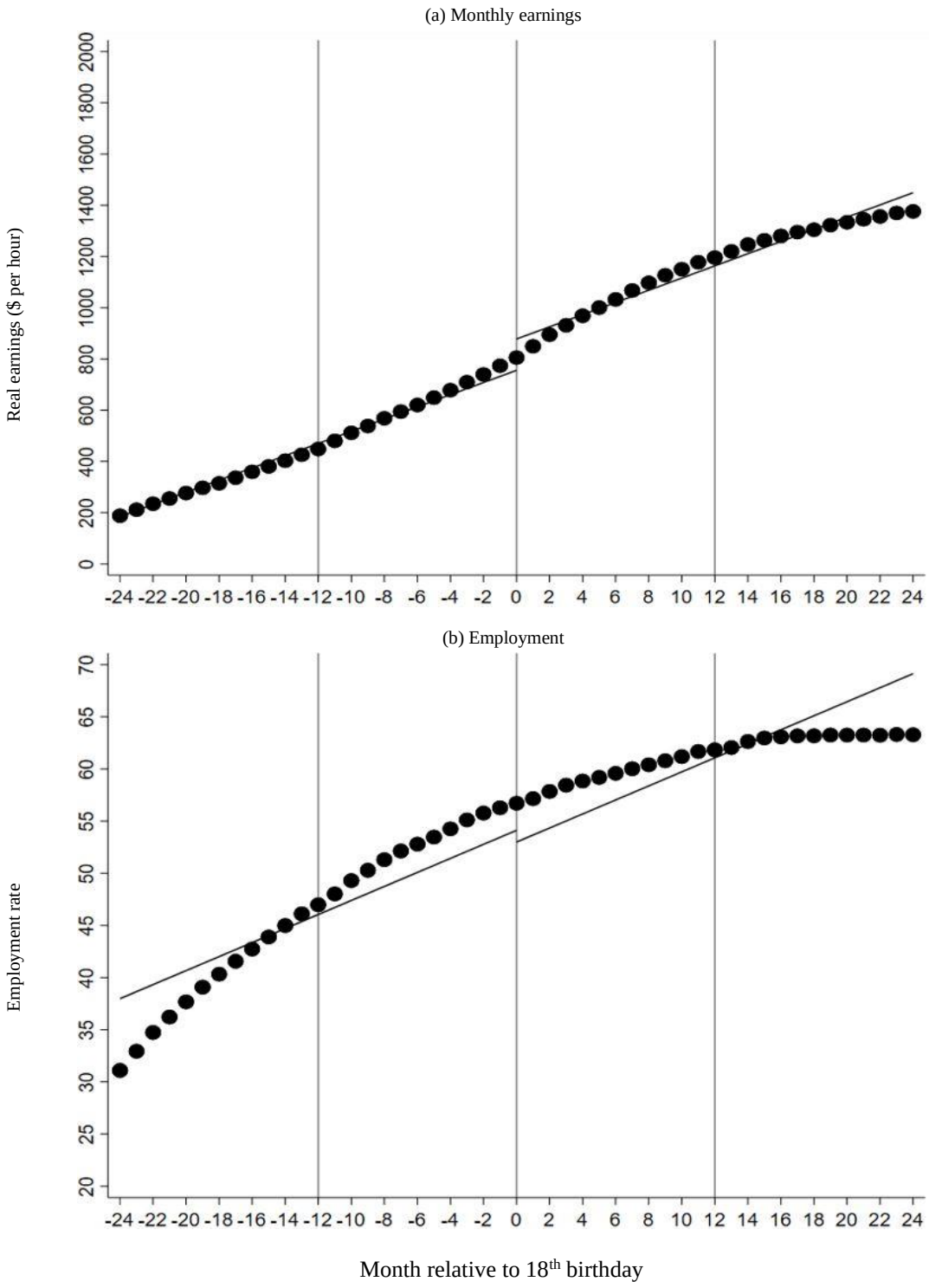

Figure D6: Testing lower-order polynomial (in age of degree 1).
The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS on 11,055,036 observations. Author's compilation.
[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).
[b] Values on the y-axis start at 20% to clearly highlight any variation in the employment rates over the months relative to workers' 18th birthday.

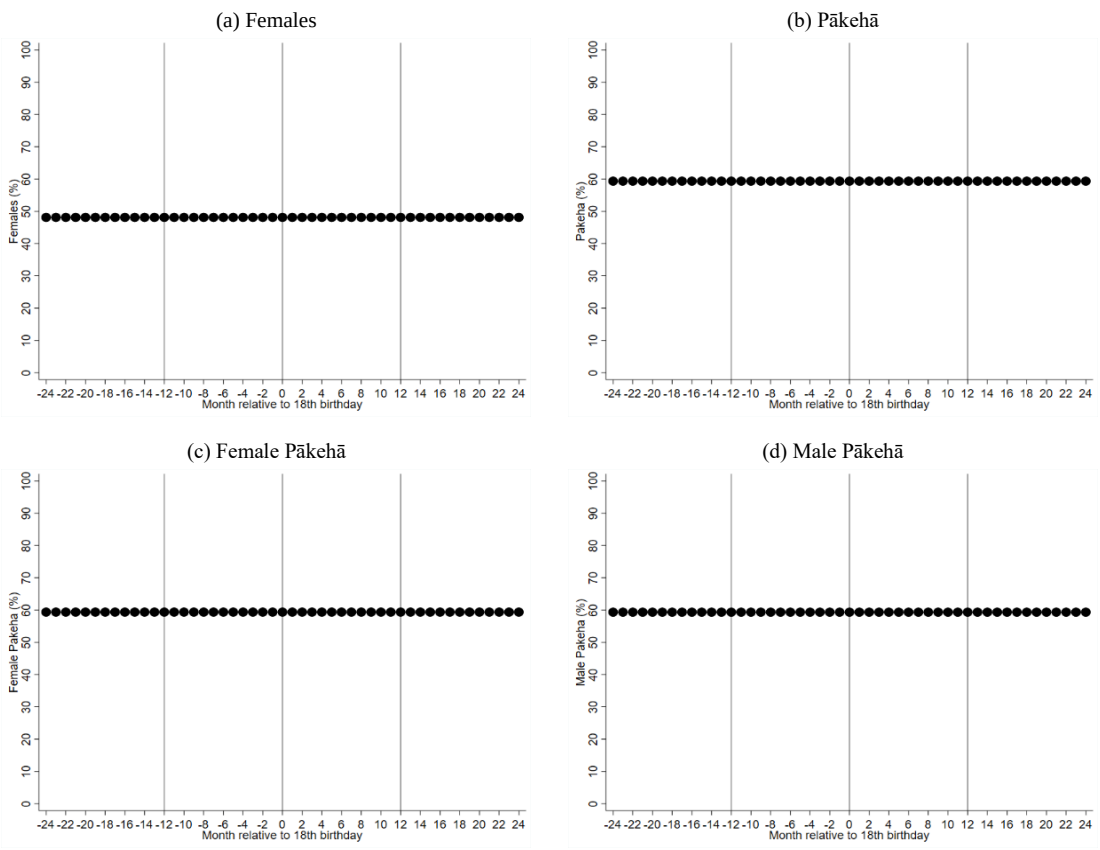

Figure D7: Visually inspecting the continuity of age across baseline covariates.
The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS on 11,055,036 observations in panels (a) and (b), 5,320,269 observations in panel (c), and 5,734,665 observations in panel (d). Author's compilation.

Table D5:

Estimation Results from Inspecting the Continuity of Age Across Baseline Covariates

	Samples		
	(1)	(2)	(3)
	Combined	Female	Male
Female			
Coefficient[a]	-0.00		
	(0.10)		
	[-0.19, 0.19]		
Pākehā			
Coefficient	0.00	0.00	0.00
	(0.23)	(0.27)	(0.23)
	[-0.45, 0.45]	[-0.52, 0.52]	[-0.44, 0.44]
Observations	11,055,036	5,320,269	5,734,665

Note. The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5%, and 1% level, respectively. Standard errors (in parenthesis) are clustered by (monthly) birth cohort. 95% confidence intervals are provided in square brackets. Author's compilation.

[a] This is the coefficient of interest which measures the effect of the discontinuity ψ in specification (10).

Table D6:

Regression Estimates of the Impact of a Minimum Wage Discontinuity at Age 18 on Monthly Earnings and Employment by Individual-Year Age Cohorts

	Individual-year cohorts				
	2003	2004	2005	2006	2007
Monthly earnings[a]					
Coefficient[b]	10.66	13.17	11.63	13.09	30.89
	(29.916)	(29.21)	(28.25)	(29.50)	(29.45)
	[-48.09, 69.40]	[-44.20, 70.55]	[-43.85, 67.11]	[-44.85, 71.04]	[-26.94, 88.73]
Percent change	0.01	0.02	0.01	0.02	0.04
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
	[-0.06, 0.09]	[-0.05, 0.09]	[-0.05, 0.08]	[-0.05, 0.09]	[-0.03, 0.10]
Employment					
Coefficient	-0.64	-0.80	-0.50	-0.25	-0.32
	(0.90)	(0.83)	(0.79)	(0.86)	(0.81)
	[-2.41, 1.18]	[-2.43, 0.83]	[-2.04, 1.04]	[-1.94, 1.45]	[-1.91, 1.28]
Percent change	-0.01	-0.01	-0.01	-0.00	-0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
	[-0.04, 0.02]	[-0.04, 0.2]	[-0.04, 0.02]	[-0.03, 0.03]	[-0.03, 0.02]
Implied elasticity[c]	-0.00	-0.00	-0.00	-0.00	-0.00
	2,118,024	2,140,125	2,219,847	2,266,446	2,310,594

Note. The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5%, and 1% level, respectively. Standard errors (in parenthesis) are clustered by (monthly) birth cohort. 95% confidence intervals are provided in square brackets. Author's compilation.

[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021).

[b] This is the coefficient of interest which measures the effect of the discontinuity ψ in specification (10).

[c] The implied elasticity is calculated by dividing the percentage change in employment by the average percentage change in the real statutory minimum wage discontinuity between 2001q2-2008q1 (27%).

Table D7:

Descriptive Characteristics of Main and Alternative Samples

Characteristics	Mean (Standard Errors) NZ born	NZ/non-NZ born	ERP 12/16 rule
Cohort			
2003	.19	.19	.19
	(0.00)	(0.00)	(0.00)
2004	.19	.19	.19
	(0.00)	(0.00)	(0.00)
2005	.20	.20	.20
	(0.00)	(0.00)	(0.00)
2006	.21	.21	.21
	(0.00)	(0.00)	(0.00)
2007	.21	.21	.21
	(0.00)	(0.00)	(0.00)
Individual[a]			
Age	17.55	17.55	17.55
	(0.00)	(0.00)	(0.00)
Female	.48	.49	.49
	(0.00)	(0.00)	(0.00)
Pākehā	.59	.46	.59
	(0.00)	(0.00)	(0.00)
Māori	.24	.17	.24
	(0.00)	(0.00)	(0.00)
Pacific	.09	.07	.09
	(0.00)	(0.00)	(0.00)
Asian	.03	.10	.03
	(0.00)	(0.00)	(0.00)
Other	.03	.04	.03
	(0.00)	(0.00)	(0.00)
Ethnicity not specified	.02	.16	.02
	(0.00)	(0.00)	(0.00)
NZ born	1	0.69	1
	(0.00)	(0.00)	(0.00)
Employment			
Employed	.54	.41	.52
	(0.00)	(0.00)	(0.00)
Monthly earnings (real $)[b]	1,249.41	1,491.03	1,512.14
	(0.38)	(0.44)	(0.45)
Weekly earnings (real $)[c]	287.38	342.96	347.82
	(0.09)	(0.10)	(0.10)
Data			
Total unique individuals	225,612	328,305	255,075
Total observations	11,055,036	16,086,993	12,498,675

Note. The sample covers the period January 2001-December 2009. Standard errors are in parentheses. The NZ born sample is the main analysis sample used in this chapter, the NZ/non-NZ born sample extends the main sample by including individuals not born in NZ, and the ERP 12/16 sample follows the sample construction procedure outlined in Chapter 4. Author's compilation.

[a] Ethnicities were prioritised using the framework from Meehan et al. (2019).

[b] Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021)

[c] Weekly earnings were derived by dividing monthly earnings by the number of weeks in the corresponding month.

Table D8:

Robustness Checks using Alternative Sample Populations

	Alternative sample populations	
	(1)	(2)
	Combined NZ and non-NZ born	Constructed using the '12/16 rule'
Monthly earnings[a]		
Coefficient[b]	13.00	16.73
	(10.26)	(13.34)
	[-7.13, 33.13]	[-9.44, 42.89]
Percent change[c]	0.02	0.02
	(0.02)	(0.02)
	[-0.01, 0.05]	[-0.01, 0.05]
Employment		
Coefficient	-0.37	-0.47
	(0.33)	(0.39)
	[-1.01, 0.27]	[-1.24, 0.29]
Percent change	-0.01	-0.01
	(0.01)	(0.01)
	[-0.02, 0.01]	[-0.02, 0.01]
Implied elasticity[d]	-0.00	
Observations	16,086,993	12,498,675

Note. The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5%, and 1% level, respectively. Standard errors (in parenthesis) are clustered by (monthly) birth cohort. 95% confidence intervals are provided in square brackets. Author's compilation.

[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021)

[b] This is the coefficient of interest which measures the effect of the discontinuity ψ in specification (10).

[c] This is the percent change in the outcome variable, calculated using the mid-point method.

[d] The implied elasticity is calculated by dividing the percentage change in employment by the average percentage change in the real statutory minimum wage discontinuity between 2001q2-2008q1 (27%).

Table D9:

Robustness Analysis using Alternative Measures of Monthly Earnings and Employment

Monthly earnings (conditional on employment)[a]	
Coefficient[b]	37.34**
	(16.74)
	[4.53, 10.16]
Percent change	0.03**
	(0.01)
	[0.00, 0.05]
Observations	5,927,910
Employment	
Coefficient	-0.38
	(0.41)
	[-1.19, 0.42]
Percent change	-0.01
	(0.01)
	[-0.02, 0.01]
Implied elasticity[c]	-0.00
Observations	11,055,036

Note. The sample covers the period January 2001-December 2009. Specification (10) was estimated using OLS. Coefficients followed by one, two, and three stars are significantly different from zero at the 10%, 5%, and 1% level, respectively. Standard errors (in parenthesis) are clustered by (monthly) birth cohort. 95% confidence intervals are provided in square brackets. Author's compilation.

[a] Monthly earnings are from wage or salary payments. Nominal earnings were adjusted using the CPI (base month/year = March 2017). Data on the CPI are from the Reserve Bank of New Zealand (2021)

[b] This is the coefficient of interest which measures the effect of the discontinuity ψ in specification (10).

[c] The implied elasticity is calculated by dividing the percentage change in employment by the average percentage change in the real statutory minimum wage discontinuity between 2001q2 – 2008q1 (27%).

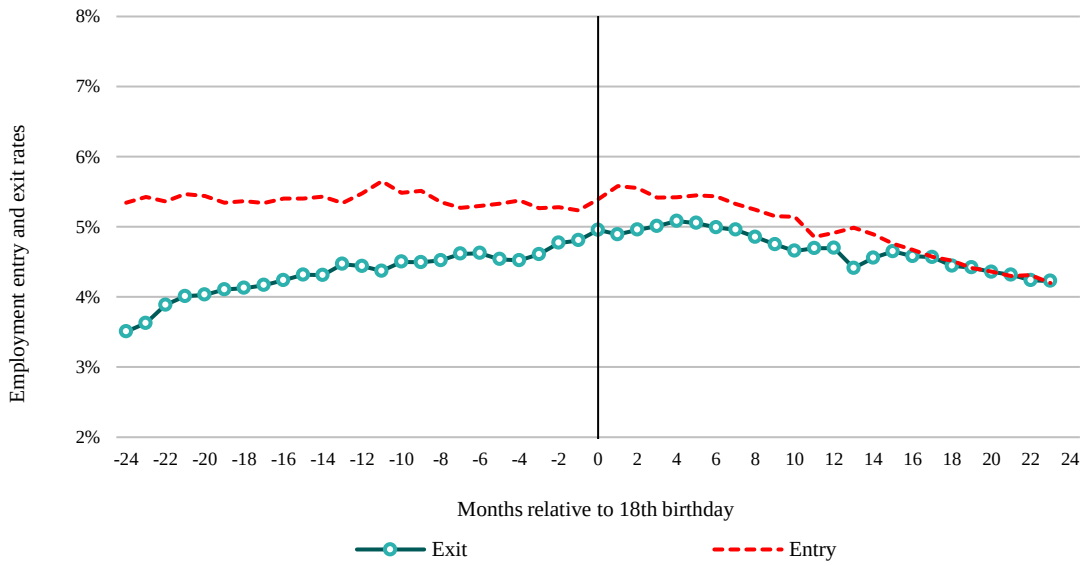

Figure D8: Employment entry and exit rates around workers' 18th birthday.
The sample covers the period January 2001-December 2009. Author's compilation.

Figure D8 depicts the flow in and out of employment around workers' 18th birthday. Following the approach of Kreiner et al. (2020), entry into employment was calculated as the fraction of workers not employed in the current month who become employed in the subsequent month. Exit from employment was calculated as the fraction of workers employed in the current month who are no longer employed in the subsequent month.

As can be observed, there appears to be some anticipatory behaviour both in terms of entry and exit rates, with marginal increases visible one month before workers' 18th birthday; albeit the net effect of these increase is close to zero, consistent with the main empirical results from Table 6.3, column (1). An increase in the entry rate continues the month subsequent to workers' 18th birthday. However, the differences between entry and exit remain small and eventually converge.

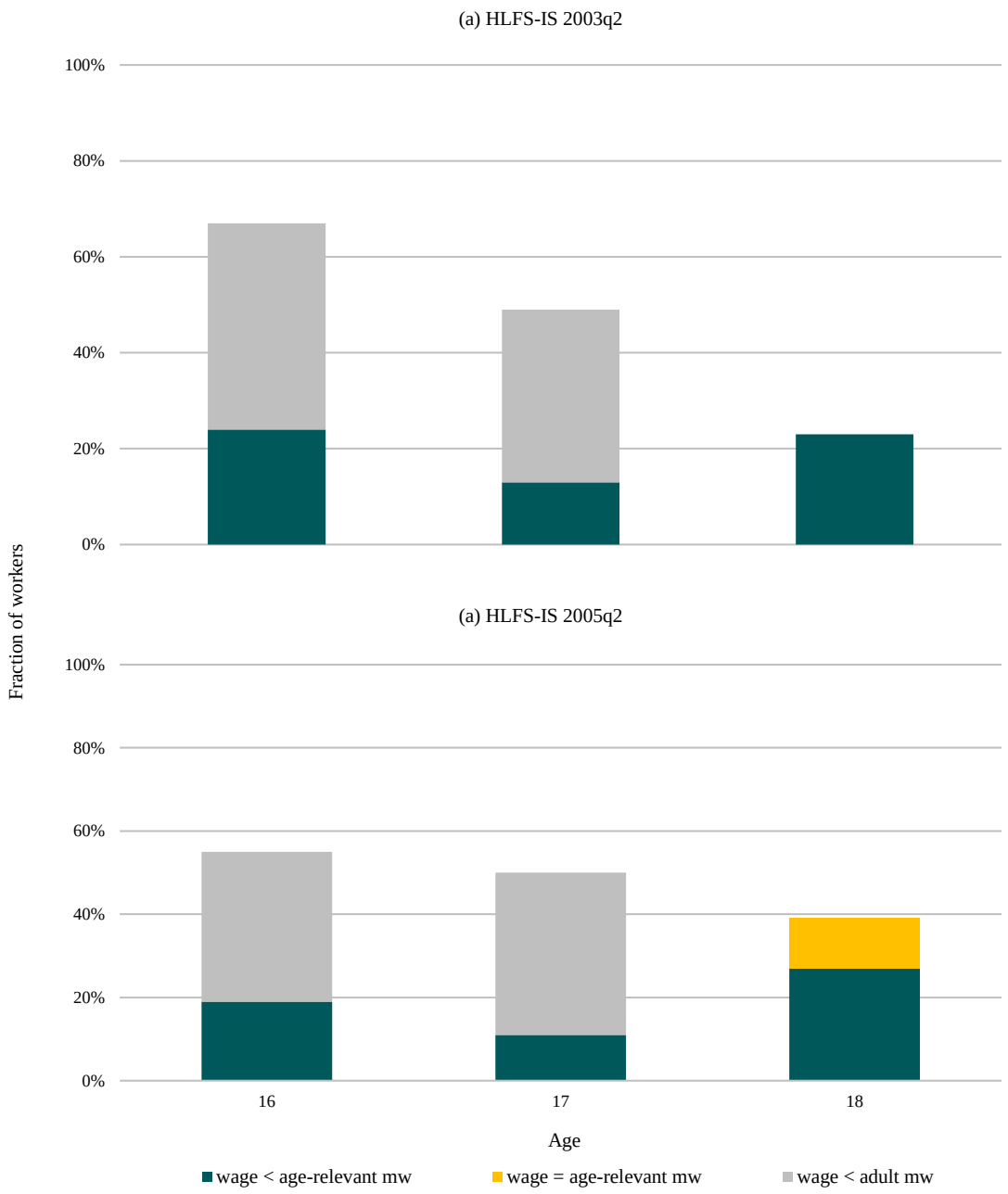

Figure D9: Fraction of teenagers earning less, equal to or more than the statutory minimum wage.
Survey data are from HLFS-IS. Samples were restricted to 16-18-year-olds. HLFS-IS 2003q2 = 666 observations. HLFS-IS 2005q2 = 618 observations Author's compilation.

Table D10:

Construction of Individual-Year Age Cohorts using Survey Data

	Cohort					
	2003	2004	2005	2006	2007	2003-2007
Constructed from HLFS-IS surveys	2001-2005	2002-2006	2003-2007	2004-2008	2005-2009	2001-2009
Initial size of individual-year cohort	2,457	2,256	2,322	2,286	2,268	-
Restricted to NZ born	438	375	357	417	423	-
Final number of individual-month observations	2,016	1,884	1,965	1,872	1,845	9,582

Note. Survey data are from HLFS-IS covering the period 2001q2-2009q2. Author's compilation.

Table D11:

Weekly Earnings, Wage, and Hours Adjustments

	Cohort				
	2003	2004	2005	2006	2007
Weekly earnings					
Replace missing records	S	S	S	S	S
Censor to 1st percentile	S	S	S	S	S
Censor to 99th percentile	21	S	21	S	S
Hours worked					
Replace missing records	S	S	S	S	S
Censor to 60 hours per week	S	S	S	S	S
Censor to 1st percentile	S	S	S	S	S
Censor to 99th percentile	S	S	21	S	S
Hourly wages					
Replace missing records	S	S	S	S	S
Censor to 1st percentile	S	S	S	S	S
Censor to 99th percentile	21	21	21	S	S
Observations	2,016	1,884	1,965	1,872	1,845

Note. Survey data are from HLFS-IS covering the period 2001q2-2009q2. 'S' = value suppressed due to suppression criteria outlined in Stats NZ (2020c). Author's compilation.

Table D12:

Estimated, Predicted, and Observed Means of Weekly Earnings, Hourly Wages, and Hours Worked per Week

	Years relative to workers' 18th birthday				
	-2	-1	0	1	2
Earnings per week (real $)					
Estimated	173.00	284.77			
Predicted			396.54	508.31	620.08
Observed	171.75	278.04	435.3	491.61	550.81
Difference (Obs. – Est./Pred.)	-1.25	-6.73	38.76	-16.7	-69.28
Wages per hour (real $)					
Estimated	10.66	12.02			
Predicted			13.38	14.73	16.09
Observed	10.56	11.85	13.55	14.67	16.17
Difference (Obs. – Est./Pred.)	-0.11	-0.17	0.18	-0.06	0.08
Hours per week					
Estimated	15.25	22.36			
Predicted			29.48	36.6	43.71
Observed	15.38	22.05	30.4	32.33	31.79
Difference (Obs. – Est./Pred.)	0.14	-0.31	0.92	-4.26	-11.93
Observations	1,008	1,092	1,074	1,029	1,065

Note. Survey data are from HLFS-IS covering the period 2001q2-2009q2. Nominal earnings and wages were deflated by the CPI (base year/quarter = 20017q1). Data on the CPI are from the Reserve Bank of New Zealand (2021). Author's compilation.

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