ESSAYS IN LABOUR ECONOMICS: EVIDENCE FROM VOLUNTARY AND MANDATORY WAGE FLOORS IN THE UNITED STATES AND NEW ZEALAND

By

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ABSTRACT

This thesis expands the literature on minimum and living wages by investigating local minimum wage ordinances and voluntary living wage programs. This thesis is presented as three distinct papers; the first explores a county-wide minimum wage ordinance in New Mexico, USA, while papers 2 and 3 explore New Zealand's voluntary living wage program.

In the United States, local minimum wage ordinances are growing in popularity, and research is emerging on their effects. Setting minimum wages at the local level is politically easier than enacting Federal legislation, and local minimum wages may be better targeted to local economic conditions. In my first chapter, "Local Minimum Wage Laws and Labour Market Outcomes: Evidence from New Mexico," I use fixed effects and synthetic control analysis to uncover the effects of a local minimum wage law on the Albuquerque/Bernalillo region of New Mexico, with a focus on how provisions exempting tipped workers affect gains in earnings. My findings reveal that these provisions can lead to reductions in hourly wages for workers exempted from the minimum wage even when the labour market is not harmed overall. I find that the minimum wage ordinance did not reduce teen employment but that it served to increase the supply of teen labour leading to an increase in the teen unemployment rate.

The second and third papers in this thesis address the voluntary living wage program in New Zealand. In the first quantitative work on New Zealand's living wage, I utilize data from Statistics New Zealand's Integrated Data Infrastructure (IDI) to explore several facets of the living wage experience for employers and employees. In the second paper, "The New Zealand Living Wage: Earnings, Labour Costs and Turnover," I investigate the characteristics of New Zealand living wage firms and use fixed effects to examine the impact of living wage certification on employment, worker earnings and turnover. My results provide some evidence for increases in labour costs and worker earnings following certification but find that this change is driven by changes in small firms that employ few workers. I find no evidence of a reduction in turnover.

In my final chapter, "Who Benefits from Living Wage Certification?" I investigate the distribution of benefits from the living wage based on an employees' pre-treatment earnings, time of hire and whether or not they remained employed with the living wage firm. To do this, I utilize a worker-level panel dataset containing the full earnings history of all workers that were employed for a living wage or matched control firm between January 2014 and December 2015. I use fixed effects

models containing fixed effects for worker, firm and month to compare patterns of earnings growth for workers hired before certification (‘pre-hires’) with those hired after certification (‘joiners’) and those who left their living wage job but remained in the workforce (‘leavers’). I also estimate the impact of living wage employment on the earnings of low-income workers. I find that the financial benefit of the living wage accrues almost exclusively to workers hired after certification and to low income workers. In addition, my analysis on the worker-level panel suggests that overall earnings growth in living wage firms lagged that in control firms over the observation period. This result is driven by relative declines in earnings for living wage workers in large firms and is attributed to increases in the published living wage rate that lags behind wage growth in the relevant segments of the job market.

To my family, for believing this was a good idea.

And to my father, for trusting me.

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CHAPTER 1: INTRODUCTION

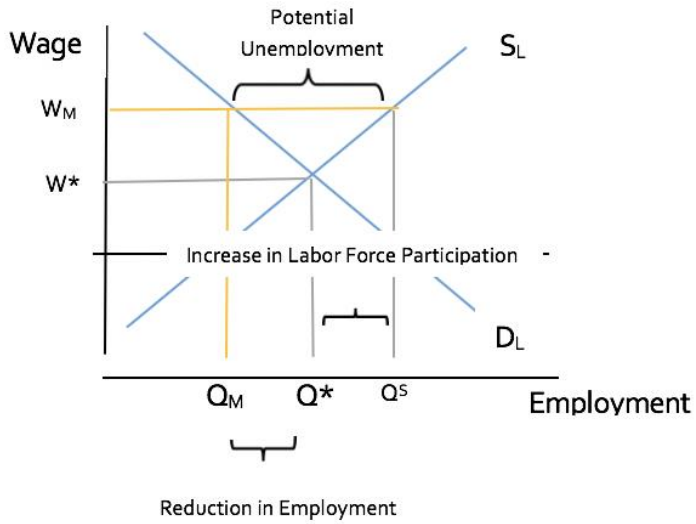
This thesis is composed of three papers relating to the effects of wage floors. The first paper examines the impact of a municipal minimum wage in the United States, while the second and third papers explore the effects of New Zealand's voluntary living wage. This section outlines the economic theory of wage floors to provide context for my work and concludes with an explanation of the motivation, structure and contribution of each paper.

What is the purpose of wage floors? Who benefits from them and who stands to lose? Although the debate over wage floors is a century old, the answers to these questions remain contentious. At the heart of the debate lies disagreement over the behaviour of labour markets. A widely held belief among economists is that the market for low-skill labour functions much like the market for other commodities. In this market, homogenous workers sell their labour to employers at a wage determined by the forces of supply and demand (Brown, Gilroy & Kohen, 1982). This traditional competitive view of the labour market predicts job losses following minimum wage hikes, and posits that increases in the minimum wage pits the interests of workers who benefit from higher wages against those who lose their jobs (Stigler, 1946). In the neo-classical view, minimum wages benefit few workers and reduce market efficiency. In contrast to this neoclassical view are monopsony theories which view the labour market as filled with frictions that tilt the balance of power in favour of employers (Manning, 2003). In monopsonistic labour markets, unequal bargaining power results in low market wages that maximize firm profits but hinder market efficiency. Under monopsony, efficiency can be improved with a well-chosen minimum wage.

Current research on the impact of minimum wages draws primarily from these competing theories. Neo-classical labour theory assumes that wages and employment levels are determined through forces of supply and demand. Both workers and employers are assumed to be price takers, as firms compete for labour and workers compete for jobs (Douglas, 1938). This model is typically depicted with a downward sloping labour demand curve where workers are paid wages equal to their marginal revenue product of labour (MRPL). Barring increases in efficiency, minimum wages will result in the layoff or reduction in hours of workers whose MRPL is below the minimum wage (Stigler, 1946). Firms may be able to offset some of these higher costs through automation or by employing more skilled workers (Card and Krueger, 1995). While the magnitude of job loss hinges on the elasticity of the demand curve, disemployment is inevitable. Workers who keep their jobs

will enjoy higher wages but there will be deadweight loss as employment falls below its equilibrium level.

Figure 1.1 | Stylized model of competitive labour markets



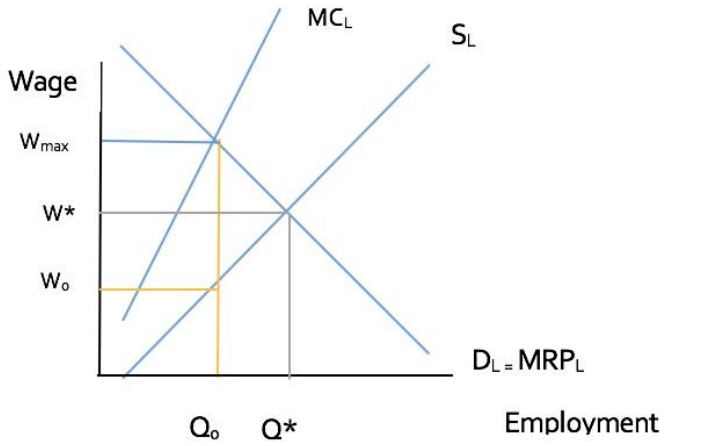
Source: Adapted from Brown, Gilroy, & Kohen (1982)

Figure 1.1 illustrates the neo-classical labour market prediction. In equilibrium, wages are W* and employment is Q*. The imposition of a minimum wage, W_M, causes a movement upwards and to the left on the labour demand curve. This demand side response occurs as employers respond to the increase in the wage floor by reducing the size of their workforces, either by laying-off workers or by reducing hiring following turnover (Brown, Gilroy, & Kohen, 1982). The reduction in employment can be measured by Q^*-Q_M. At the same time, there is a supply side response to higher wage floors: individuals are drawn into the workforce as the minimum wage surpasses their reservation wage. This is represented in Figure 2.2 as the increase from Q* to Q_S. These new workers compete with laid-off workers for a smaller number of jobs and unemployment rises. When possible, laid-off workers may move to uncovered sectors of the economy, where they depress wages (Stigler, 1946). The degree of unemployment will depend on the extent to which the new minimum wage succeeds in enticing new, would-be workers into the workforce as well as on how resilient these individuals are to an unsuccessful job search (Brown, 1999).

The monopsony model of the labour market provides an alternate explanation of labour markets in which firms capitalise on their position of power to negotiate lower wages with workers. The labour market response to increases in minimum wage under monopsony is presented in Figure

1.2. Firms with homogenous work forces face a marginal cost of labour (MCL) that lies above an upwardly sloping supply curve. In order to hire additional workers, employers must raise wages for all workers (Ashenfelter, Farber, & Ransom, 2010). Profit-maximising firms choose the number of workers to hire such that the marginal cost of the next worker is equal to that workers' marginal revenue product of labour. This occurs at a wage of W_0 and an employment level, Q_0 and results in wage, employment, and production levels below that seen in competitive equilibrium.

Figure 1.2 | Stylised model of monopsonistic labour markets



Source: Adapted from Brown, Gilroy, & Kohen (1982)

As illustrated in Figure 1.2, when the employer has monopsony power and is currently paying the profit maximizing wage, W_0, there exists a range of potential minimum wages over which employment, output and efficiency may increase. At any wage above W_0 and below W_{max}, a profit maximizing employer will benefit by increasing output. If a minimum wage is set within this range, employers may respond by increasing employment (Brown, 1999). At a minimum wage equal to the equilibrium wage W*, the employer would employ Q* workers, and at a minimum wage of W_{max}, employers would employ Q_0, the same quantity of labour utilized under monopsony. At minimum wages above W_{max}, the result will resemble that of a minimum wage under competition: workers whose marginal revenue product of labour is less than the minimum wage will experience job loss or a reduction in hours.

Modern economic literature produces only limited evidence that increases to the minimum wage cause job loss. The current state of minimum wage research centres around determining if modest increases in the minimum wage have no effects on employment or if the effect is very small. The

first position was made popular with a series of minimum wage work in the 1990s, best exemplified by Card and Krueger (1995). Their findings altered the current thinking on the economics of the minimum wage by providing evidence that minimum wages do not necessarily reduce employment. Since then, several influential papers have continued to show that modest minimum wages do not harm employment (for a review, see Schmitt, 2013). The second position, best summarised by Neumark and Wascher (2008), is that minimum wages have a small negative effect on employment with elasticities in the range of -0.1 to -0.2. These small elasticities imply that at current minimum wages, demand for low-wage (especially teen and food service) labour is inelastic; thus, it is somewhat resilient to increases in the minimum wage. This evidence of limited job loss has led many economists to the conclusion that low-wage labour markets are imperfectly competitive (Kaufman, 2010).

Questions have also arisen over the degree to which the wages of low-skilled workers reflect their true marginal product and over the extent to which we should allow wages to be determined solely by the market. There are a number of reasons to believe that there is an imbalance of power in the relationship between worker and employer. First, there is an imbalance of need. Employees need a job more than employers need any particular worker (Manning, 2003). Unlike other inputs in production, workers have needs that persist even when they do not have a buyer for their labour (Prasch, 2003). Workers rely on income received from the sale of their labour to support their livelihood. As such, workers do not really face a trade-off between labour and leisure, as the conventional wisdom holds. Instead, they face the less appealing trade-off between work and unemployment. This simple imbalance, coupled with incomplete information on jobs and wages, gives employers an advantage in wage negotiation. If unequal bargaining power results in wages below the marginal revenue product, workers are receiving less than they are 'worth.' This issue can become exacerbated by competition in product markets where firms compete on both product and price. Because labour demand is derived from demand for underlying products, downward pressure on prices for goods translates into downward pressure on wages (Greenwood, 2016). This problem where both prices and wages race to the bottom in competitive markets has been documented for over a century (Webb & Webb, 1897).

Concern over this imbalance in power, the downward pressure on wages and the cost of labour disputes in the nineteenth century gave rise to the concept of minimum wages. The earliest wage

floors were developed at the turn of the twentieth century in New Zealand, where Arbitration Courts were granted the power to resolve industrial disputes by determining fair wage rates (Hyman, 2002). These courts resolved settlements on a case by case basis until 1919 when they began setting wage floors more broadly (Williams, 1976). However, it was not until 1946 that New Zealand enacted a universal minimum wage (Hyman, 2002). In the United States, wage floors were encouraged but voluntary until states began passing minimum wage legislation in 1912 (Leonard, 2000). With the enactment of wage floors, both the government and the public hoped to relieve workers from downward pressure of wages by setting a minimum wage that would be incorporated into the cost structure of all businesses. The goal was not only to improve the lives of poor workers but also to bolster the economy by improving labour efficiency, reducing strikes and generating demand for the rising level of industrial output (Douglas, 1938).

Today, the arguments for the minimum wage are more likely to be expressed in the narrow terms of ameliorating poverty and reducing inequality (Sobel, 1999). Interestingly enough, there is ample evidence to suggest that the minimum wage is only marginally effective as a tool to reduce poverty and inequality (see Neumark & Wascher, 2008 for a review of the literature). Why then, is substantial research and policy effort expended on a policy that is only marginally successful? The answer to this lies in the underlying belief that minimum wages help improve efficiency by preventing the payment of very low wages. Minimum wages serve to limit the extent to which competitive pressures drive down wages to unacceptably low levels. This helps balance the bargaining power of employers and workers. However, minimum wages alone cannot eliminate poverty and inequality; they must be combined with social welfare systems such as transfers and tax credits. Minimum wages must be set in conjunction with tax and transfer policies in a way that encourages work while protecting vulnerable populations.

The debate over the effects of the minimum wage reflects a deeper divide in both economics and politics over the behaviour of markets and the appropriate role of government. Much of this debate is inherently political. Research is needed to provide thorough, timely and unbiased evaluations of programs and policies to help shape the dialog and guide decision making. Results of minimum wage research have been important influences in the debate over minimum wages and as policies, economic conditions and empirical strategies improve, it is important that research continues to evolve to provide the best estimates of current labour market behaviour and minimum wage

effects. In the current political environment, where momentum is growing to increase the minimum wage in both New Zealand and the United States, a steady stream of research on the results of these changes will help encourage policy making that is based on evidence rather than politics.

The three papers contained in this thesis explore the labour market effects of wage floors both mandated and voluntary. I have chosen to look at two wage floors with unusual provisions to investigate the ways in which policy nuance influences the impacts. The first paper in this thesis, "Local minimum wages: a case study from New Mexico," investigates the effects of rising municipal minimum wages on earnings and employment in a market with both a covered and uncovered sector. The second and third papers in this thesis address the voluntary living wage program in New Zealand in which employers are encouraged to pay higher wages in exchange for third-party certification as a 'Living Wage Employer'. In the third chapter, "The New Zealand living wage: Earnings, labour costs and turnover," I explore the history of the movement, the characteristics of certified employers and examine changes in wages, costs and turnover patterns using a firm-level dataset from New Zealand's Integrated Data Infrastructure (IDI). In Chapter four, "Who benefits from the living wage?" I use a different dataset generated in the IDI to explore changes in earnings from the perspective of workers to identify if higher wages associated with living wages allow firms to substitute skilled for unskilled labour in entry level jobs.

The first paper, "Local Minimum Wages and Labour Market Outcomes: Evidence from New Mexico" is motivated by the increasing popularity of municipal minimum wages in the United States. In 2012, the Albuquerque/Bernalillo area of New Mexico passed a municipal minimum wage ordinance that increased the most widely applicable minimum wage from $7.50 USD per hour to $8.50 USD per hour while including a sub-minimum wage provision for workers who customarily earn tips. Although the relationship between minimum wages and employment has been widely researched, little work has been done on minimum wages that offer substantial discounts for tipped workers. This is also the first study to look at the impact of the Albuquerque/Bernalillo minimum wage ordinance.

This paper uses both synthetic control and fixed effects methodologies to estimate the effect of the municipal minimum wage increase on wages, employment, unemployment and hours worked. Compared to existing literature, this study has the advantage of estimating changes in the intensive margin of employment by place of work rather than place of residence. It is also the first municipal

minimum wage paper to document wage reductions for workers exempt from the minimum wage. The findings indicate: (a) no evidence of employment reductions following the living wage ordinance; (b) a supply-side driven increase in unemployment among teenagers of 6.0%-7.9%; (c) some evidence of a reduction in hours worked for employees in the food service industry and (d) a decrease in earnings for tipped workers in the uncovered sector.

The second paper is titled "The New Zealand living wage: Earnings, labour costs and turnover." The New Zealand living wage was established in 2013 as a means to encourage a market-based wage that would allow a family of four with one-and-a-half incomes to earn enough money to meet their basic needs and participate modestly in the customary activities of New Zealand culture. In 2014, the grassroots organisation, Living Wage Aotearoa New Zealand, began offering certification for employers who agreed to comply with the higher wage floor, maintain employee hours/fringe benefits and provide workers with access to a union. The living wage movement in New Zealand is new and growing, yet there is currently no quantitative research on its effects. This paper provides the first quantitative assessment of the impact of the living wage on earnings, employment and turnover. To assess these impacts, I use high-quality, linked employer-employee microdata from New Zealand's Integrated Data Infrastructure (IDI).

This paper has two major goals. First, I seek to understand "who are living wage firms?" I begin with a thorough description of the characteristics of living wage enterprises using both publicly available information and data from the IDI. I investigate industry make-up, firm age, number of employees and firm location to describe the ways in which the makeup of living wage firms has evolved in the first years of certification. I find that the earliest employers to pursue certification were primarily religious, labour union or special interest groups, a finding that reflects the grassroots nature of the living wage movement. As time has progressed, the living wage has appealed to a growing number of small and market-based companies. Second, I ask "what effect does living wage certification have on these firms?" To answer this question, I assess labour costs, the prevalence of below-living wage earnings and turnover rates for living wage firms in comparison to a matched group of control firms. My results uncover a large degree of heterogeneity among firms but suggest that: (a) the living wage is successful at increasing earnings of employees. The proportion of workers earning less than the full-time living wage decreases by between 4.7 and 6.0 percent; (b) living wage firms may experience a short term increase in labour

costs following certification of approximately 4% but that channels of adjustment exist to offset a portion of these costs and (c) turnover does not seem to fall.

The final paper in this thesis, "Who Benefits from the Living Wage," investigates the distribution of earnings benefits from the living wage. To do this, I compare earnings increases for employees by time of hire to answer the question "Do existing employees or new hires derive the greater financial benefit from living wage certification?" I also estimate earnings gains for low-income workers and for workers that leave their living wage employer while remaining in the labour force. The empirical analysis in this paper consists of a series of fixed effects estimates on worker level data derived from the New Zealand IDI, with fixed effects for worker, firm and time. I use an unbalanced worker-level panel dataset that follows employees of living wage and control firms for four years as they move between jobs to compare patterns of earnings gains between living wage prehires and joiners.

My results reveal that not all workers benefit from living wage certification. In fact, the benefit of higher wages accrues almost exclusively to workers hired after certification and to workers who were low-paid in their previous jobs. This finding suggests that living wage firms do not hire skilled workers to fill jobs previously held by unskilled labourers. Additionally, workers hired before certification experience virtually no financial benefit from living wage certification. Overall, I find that earnings growth for living wage employees lags behind that of workers in control firms. This pattern of relative average wage loss may be explained by a variety of factors including wage compression, imperfect information, labour market frictions, or the fact that annual growth in the published living wage rate underestimated wage growth in the relevant segments of the labour market. Additional research will be needed to clarify the causes. Furthermore, while employees of medium-sized living wage organizations experienced earnings growth relative to employees of control firms, employees of large living wage firms experienced a decline in wages relative to both control employees and other living wage workers.

CHAPTER 2: LOCAL MINIMUM WAGES: A CASE STUDY FROM NEW MEXICO

2.1 ABSTRACT

This paper uses data from the Current Population Survey (CPS) and the American Community Survey (ACS) to explore the effect of the Albuquerque/Bernalillo minimum wage laws of 2013 on the teen and food-service labour markets. Results indicate that firms adapted to the higher minimum wage laws by both raising wages and increasing their reliance on the sub-minimum wage provision of the ordinances, which allows for the payment of very low wages to those who regularly earn tips at work. The minimum wage hike did not reduce employment or weekly hours for teens but may have served as an incentive for young workers to enter the labour market, as unemployment among 16-19-year-olds increased by roughly six percent following the minimum wage hike. Results suggest that sub-minimum wage provisions can offset potential gains to workers, an issue relevant to lawmakers when crafting minimum wage legislation.

2.2 INTRODUCTION

How does the minimum wage affect the employment opportunities of low skill workers? Economic theory predicts both demand and supply-side responses to increases in minimum wage. While economic theory suggests that rising minimum wages will increase the supply of low-skilled labour (Mincer, 1976), demand-side responses are more difficult to predict. Traditional, competitive models of the labour market indicate that employers will respond to higher minimum wages by reducing the quantity of labour demanded (Stigler, 1946). This may be seen as a reduction in hours or in a decline in the number of jobs. Although the traditional model has heavily influenced the debate over minimum wages, other models offer more nuanced predictions that may better fit the data (Card & Krueger, 1995). Dynamic monopsony and institutional models present the possibility that minimum wage increases may raise both wages and employment opportunities for low skilled workers (Manning, 2003; Kaufman, 2010). Both models posit that labour market frictions, incomplete information and unequal bargaining power result in wages less than the marginal revenue product of labour. In imperfectly competitive labour markets, the effect of minimum wages on employment is uncertain (Schmitt, 2013).

Empirically, there is a similar lack of consensus on the employment impact of minimum wages. On one hand, a substantial body of work finds employment loss consistent with competitive models of the labour market (for a review, see Neumark & Wascher, 2008), yet other researchers find little evidence that higher minimum wages lead to job loss (for a summary of this literature, see Schmitt, 2013). This lack of consensus has led to a shift in the minimum wage research paradigm. Rather than attempting to identify a single "minimum wage effect", researchers are now looking to understand the ways in which variations in policy or differences in economic forces may lead to varied outcomes following minimum wage increases (Neumark, 2017). One such policy variation lies in the treatment of tipped workers under minimum wage law. Federally, employees who earn tips are entitled to an hourly minimum wage of $2.13. States and municipalities may pass laws to raise this tipped minimum wage and many do. States such as California, Oregon and Washington mandate that employers pay tipped workers the full state minimum wage, while others (Washington D.C. and Illinois, among others) mandate tipped minimum wages that lie between the state minimum wage and the $2.13 federal rate (U.S. Department of Labor, 2019).

Sub-minimum wage provisions allow employers to pay lower wages to workers who customarily receive tips. These laws allow payment of a lower hourly wage but mandate that employers "make up the difference" if the wage plus tips is below the prevailing minimum wage. This would not harm workers who earned the tipped minimum wage before the minimum wage hike but if the minimum wage increase leads employers to switch to the tipped sub-minimum wage, these workers would effectively receive a pay cut. Because restaurant workers represent a large proportion of minimum wage earners, minimum wage laws that exempt tipped employees may have a different labour market effect than those that do not.

The purpose of this research is to further illuminate the impact of city- and county-wide minimum wage ordinances by studying the effect of the Albuquerque/Bernalillo minimum wage laws of 2012-2013. Minimum wages at the city and county level may have greater employment impacts than those at the state or federal level for two reasons. First, firms seeking to avoid paying higher minimum wages may choose to move across city boundaries. For most firms, this would be much easier than relocating to a nearby state. Second, firms that remain in the city with a higher minimum wage may adapt to the higher wage by raising prices (Hirsch, Kaufman, & Zelenska,

2015). In markets where customers frequently shop across city lines, this may put firms in high minimum wage cities at a competitive disadvantage (Schmitt & Rosnick, 2011).

To explore the impact of the Albuquerque/Bernalillo minimum wage ordinance, I utilize publicly available data from the Current Population Survey and the American Community Survey to estimate the impact of the Albuquerque/Bernalillo minimum wage ordinances on employment, unemployment, wages and hours worked. Minimum wage studies that utilize publicly available data must confront the challenge of identifying affected workers. Because of their high level of representation among minimum wage earners, teenagers and food service employees are the focus of many minimum wage studies and I use them here (Dube, Lester, & Reich, 2010; Dube & Zipperer, 2015; Giuliano, 2013). For teenagers, I explore the outcome variables of employment, unemployment and hours worked, while for food service workers, I explore only the intensive margin of employment: weekly hours. While much of the recent literature focuses on wages and employment, this approach is a bit myopic because it ignores supply side effects of labour market participation. Regardless of changes in the number of jobs available following minimum wage increases, policies that encourage labour force participation among low-skilled workers have the potential to influence lifetime attitudes toward work and income. Understanding the degree to which minimum wages increase unsuccessful job searches is an important part of understanding the effects of these policies.

Since Card and Krueger's (1994) landmark paper on the impact of minimum wages on employment in Pennsylvania and New Jersey fast food restaurants, the difference-in-differences and fixed effects models have been the identification strategy of choice in the minimum wage literature. Recent developments have raised issues surrounding the inability of these models to control for time-variant differences in patterns of growth between treated and control regions (Allegretto, Dube, Reich, & Zipperer, 2013; Neumark & Wascher, 2017). Numerous approaches have been developed to supplement or augment the traditional difference-in-differences methodology including the synthetic control estimator, which has been used in a number of recent minimum wage studies (Reich, Allegretto, & Godoy, 2017; Sabia, Burkhauser, & Hansen, 2012). This paper utilizes both fixed effects and synthetic control estimators and finds a large degree of similarity between the estimates.

My research makes two contributions to the current body of work on the minimum wage. First, I examine the impact of a minimum wage law that allows for the payment of sub-minimum wages to workers who normally and customarily receive tips at their jobs. Second, my work evaluates the effect on unemployment and hours worked—in addition to employment—to uncover more nuanced effects of minimum wage hikes.

2.3 LITERATURE REVIEW

This paper seeks to identify the labour market effects of the Albuquerque/Bernalillo minimum wage increases of 2012-2013. Like much of the work on the minimum wage, this paper explores the effects of a minimum wage hike on the earnings, employment, unemployment and hours worked of teenagers and food service workers. However, this work expands the literature by exploring the utilization of a sub-minimum wage provision of a municipal minimum wage. As municipal minimum wages are expanding in scope, it is becoming possible to explore ways in which details of these ordinances shape their labour market impacts. In this case, the Albuquerque/Bernalillo minimum wage ordinances allow employers to pay wages below the minimum to workers who customarily receive tips. Unlike student/apprentice or youth sub-minimum wage provisions designed to encourage employers to hire new labour market entrants, sub-minimum wages for tipped workers reduce the minimum wage burden on employers so long as wages *plus* tips equal the minimum wage. To date, no research has specifically looked at the effect of a tipped minimum wage provision on the effectiveness and impact of municipal minimum wage legislation.

However, few topics in economics have received as much research attention as the minimum wage. With roots stretching back to the early twentieth century (Obenauer & von der Nienburg, 1915), minimum wage research has evolved as changes in policies, developments in methodology and advancements in data have offered new opportunities for analysis. Early time-series work exploited changes in the Federal minimum wage to uncover the effect of minimum wage changes on employment and unemployment. Focusing largely on teens, the majority of time series studies found that minimum wage increases reduced job opportunities for young workers. Studies found both increases in unemployment (Adie, 1973) and decreases in employment (Wachter & Kim, 1979) following minimum wage hikes. Differences between youth by race were also noted, with some evidence emerging to suggest that the disemployment effects are strongest among non-white

youth (Ragan, 1977). Overall, elasticity estimates in the range of -0.1 to -0.3 were most common, suggesting that a 10 percent increase in the minimum wage reduces teenage employment by 1 to 3 percent (Brown, Gilroy, & Kohen, 1982).

Although not all time-series studies found disemployment effects following minimum wage increases (see Kaitz, 1970 and Wellington, 1991 as exceptions), the possibility that employment could be unaffected by changes in the minimum wage was not taken seriously until the advent of the "new minimum wage" literature in the early 1990s. Following the publication of influential work by David Card, Alan Krueger and others (see Katz & Krueger, 1992 and Machin and Manning, 1994), the consensus created by the time-series literature was broken, and economists became more receptive to the possibility that minimum wages could have negligible or even positive effects on employment. In perhaps the most influential of the minimum wage papers of the 1990s, David Card and Alan Krueger (1994) surveyed fast food restaurants in New Jersey and Pennsylvania following the 1992 increase in the New Jersey minimum wage. Using a difference-in-differences design, they found that employment in New Jersey fast food restaurants did not fall (and in some specifications, rose) after the minimum wage increase.

While the consensus achieved during the 1970s and 1980s had been broken, the issue remained contentious, and several studies continued to find evidence of employment loss following minimum wage hikes. One of the most cited studies was conducted by Neumark and Wascher (1992) who found statistically significant job losses consistent with earlier time series findings. Using Current Population Survey state level panel data from 1976 to 1989 to estimate a fixed effects model, Neumark and Wascher pioneered the state-level panel data methodology that became ubiquitous in the literature. Their results supported the estimates obtained in the earlier time series literature: a 10% increase in minimum wage leads to employment losses in the range of 1.7% to 3.3%.

Over the past two decades, minimum wage research has evolved in response to developments in both methodology and policy. On the methodological side, the popular two-way fixed effects model developed in the 1990s has been criticized for its inability to effectively control for differences between treated and control regions. Influential research by Addison, Blackburn, and Cotti (2009), Allegretto, Dube, and Reich (2011) and Dube, Lester, and Reich (2010) has revealed that by failing to account for spatial heterogeneity, the panel data approach commonly used in

minimum wage studies produces downwardly biased estimates. States enacting higher minimum wage laws typically have more polarized labour markets characterized by relatively slow growth in the low-skill labour market. Differences in rates of employment growth between treated and control states are often time variant, and when treated areas experience systematically lower levels of growth, the counterfactual created by untreated states will over-estimate the expected level of employment. As a result, minimum wage estimates are often incorrect and downward biased (Allegretto et al., 2013)

Researchers have employed a variety of approaches to control for spatial heterogeneity including the use of contiguous counties or commuting zones that span policy boundaries (Dube, Lester, & Reich, 2010; 2016), use of region-specific time trends (Allegretto, Dube, & Reich, 2011), triple difference methods (Clemens & Strain, 2017; 2018; Clemens & Wither, 2019) and synthetic control estimation (Allegretto, Godoy, & Reich, 2018; Jardim, et al., 2017). While many of these approaches yield estimates of employment elasticities that are indistinguishable from zero, notable estimates of negative employment effects remain. Namely, Jardim et al. (2018) find large negative employment effects following the implementation of Seattle's citywide minimum wage, and the research of Clemens and Wither (2019) reveals that minimum wage increases can reduce employment growth, even after differences in underlying employment trends are accounted for. Additionally, some concern remains that the use of region-specific linear time trends or region-specific time fixed effects eliminates both useful and confounding variation in the data, thus biasing results toward zero (Neumark, Salas, & Wascher, 2014).

On the policy side, recently passed state and municipal minimum wages have provided economists with the opportunity to study nuances of minimum wage impact. While for many years, variation in minimum wages was limited to the Federal level, an increasing number of states and municipalities are now passing minimum wage laws. City and county minimum wage laws are particularly interesting because they alter the labour market landscape of small geographic areas. If firms respond to these higher minimum wage laws by moving across city lines, the labour market impacts of the minimum wage may be amplified. Additionally, research shows that firms respond to higher minimum wages by raising prices (Hirsch, Kaufman, & Zelenska, 2015; Romich, Allard, Obara, Althauser, & Buszkiewicz, 2018), a fact that may put these firms at a competitive

disadvantage if consumers are easily able to shop in nearby cities with lower minimum wages and hence lower prices.

In 2003, San Francisco, California and Santa Fe, New Mexico became the first cities to enact citywide minimum wages. Between 2012 and 2015, thirty-three additional municipalities followed suit. Research on the effects of these laws is now emerging. The majority of the work has focused on Santa Fe (Hollis, 2015; Schmitt & Rosnick, 2011); San Francisco (Dube, Naidu, & Reich, 2007); San Jose (Allegretto & Reich, 2018) and Seattle (Allegretto, Godoy, & Reich, 2018; Reich, Allegretto, & Godoy, 2017; Jardim, et al., 2017). Like other recent minimum wage literature, studies of municipal minimum wages pay attention to both identifying affected employees and establishing a valid counterfactual. While some studies employ the standard two-way fixed effects model (Hollis, 2015; Yelowitz, 2005), others have employed more sophisticated methods such as synthetic control and factor models to critically evaluate the suitability of the control group (Allegretto, Godoy, & Reich, 2018; Jardim et al., 2017).

In a thorough but highly criticized study of the Seattle minimum wage, Jardim et al. (2017) utilize workers compensation data to investigate changes in earnings, employment and hours worked for all low paid jobs at single site establishments in Seattle. Using synthetic control and interactive fixed effects models, they find that the Seattle minimum wage raised hourly wages for workers while dramatically reducing their employment opportunities. Disemployment effects were larger at higher levels of the minimum wage, suggesting non-linearity in the labour market response. The authors find that the first increase in the Seattle minimum wage—from $9.47 to $11.00 per hour—did not reduce employment. However, the second increase—from $11.00 to $13.00 per hour—resulted in substantial disemployment. Finding employment elasticities as large as -2.6, the authors conclude that the magnitude of employment loss from the second minimum wage increase was significant enough to reduce payroll expenses for low-wage labour.

This study has been criticized for producing improbable results and for being impossible to replicate due to the confidentiality of the dataset used. Namely, the findings suggest that the minimum wage increase led to disemployment effects much larger than the range typically found in the literature, while also identifying effects of the minimum wage at earnings levels well beyond those typically impacted by the minimum wage (Zipperer & Schmitt, 2017). By restricting themselves to single-site employers, they excluded the experience of as much as 48 percent of the

Seattle low-wage workforce (West, 2017). Because minimum wage increases may affect small and large employers differently, their results may be negatively biased.

Reich, Allegretto, and Godoy (2017) examined the effect of the Seattle minimum wage on food services workers using the Quarterly Census of Employment and Wages (QCEW), a commonly used and widely available dataset. Using the synthetic control methodology, they found that the Seattle minimum wage increased earnings for workers in restaurants (elasticity 0.098) but that the earnings increase was much higher for workers in limited services restaurants (elasticity 0.229). The Seattle minimum wage ordinance introduced a tip-credit provision in 2015 that set the minimum wage for tipped workers at $10.00. Before this, tipped workers were entitled to the citywide minimum wage of $9.47 per hour, so although the minimum wage ordinance allowed a tip-credit, tipped employees earning minimum wage received a mandated raise. The employment elasticities identified in the study were statistically indistinguishable from zero in all specifications. In limited services restaurants, the coefficient was negative but imprecisely estimated.

Using an event study and synthetic control estimates, Allegretto et al. (2018) investigate the effect of citywide minimum wage laws on earnings and employment for food services workers in six cities. Overall, they find minimum wage elasticities ranging between 0.13 and 0.25 suggesting that a 10 percent increase in the minimum wage increases earnings in the restaurant sector by 1.2 to 2.5 percent. These findings are misleading and perhaps understated however, because two of the six cities researched allowed for deeply reduced minimum wages for tipped employees. In contrast to the experience in Seattle, where the minimum wage law created a tipped wage provision by legislating smaller wage increases for tipped workers, in Chicago and Washington DC, the minimum wage legislation did not require raises for tipped employees. This situation—mirrored in Albuquerque/Bernalillo—may have been responsible for the statistically insignificant increases in wages for food services workers in these cities. The authors did not investigate this in detail. Unlike the results in Jardim et al. (2017), this study finds no statistically significant changes in employment. No work has yet focused on the Albuquerque/Bernalillo minimum wage ordinances, nor have any studies explored the ways in which a deeply discounted tipped minimum wage impacts earnings gains for low paid workers that rely on tips.

2.4 POLICY AND CONTEXT

The United States enacted its first federal minimum wage law in 1938 with the passage of the Fair Labour Standards Act. Because the nominal value of the US minimum wage is not indexed to inflation, the real value of the minimum wage declines in the years between successive rate increases. Public support for raising the minimum wage is high among Americans but strong ideological divides have prevented Congress from acting to increase the Federal minimum wage (Elwell, 2014). At the State and local level however, minimum wage increases have not met similar roadblocks. Through both legislative action and direct ballot measures, 29 States and 34 municipalities now have minimum wages that are set above the federal level (National Employment Law Project, 2016). While States have been setting more stringent minimum wages for half a century, local minimum wage laws are relatively new and are growing in popularity. Table 2.2 summarizes the current local minimum wage laws in the United States. In 2003, San Francisco, CA and Santa Fe, NM were the first cities to enact local minimum wage ordinances. Nine years later, Albuquerque, NM and San Jose, CA followed suit. Since then, 31 other cities and counties have enacted local minimum wages and eight more municipalities are currently evaluating minimum wage proposals (UC Berkeley Labor Center, 2015).

The city of Albuquerque and surrounding Bernalillo County were among the first municipalities to set minimum wage levels above the state or Federal level. In 2006, both municipalities enacted minimum wage laws that brought the minimum wage from $5.15 to $7.50 over a period of three years (Table 2.3**)**. However, because neither ordinance was indexed for inflation, the nominal value of the minimum wage remained unchanged for four years. During that time, both the state and Federal minimum wage levels increased so businesses in Albuquerque/Bernalillo faced the same wage floor as the remainder of New Mexico.

In 2012-2013, both municipalities amended their minimum wage ordinances to raise minimum wages above the state and Federal levels. In November 2012 the City of Albuquerque passed a ballot measure to raise the minimum wage for most employees working within city limits. The 13.3% increase from $7.50 to $8.50 went into effect on January 1, 2013 and is indexed for inflation

thereafter. The city also amended its minimum wage ordinance to raise pay for tipped employees.[1] Under the new ordinance, tipped employees would be paid a minimum of $3.83 per hour in 2013, and 60% of the non-tipped minimum thereafter (City of Albuquerque, 2012).

Table 2.1 Municipal minimum wage laws in the United States

Municipal minimum wage laws in the United States		
Year Passed	**City and State**	**Minimum Wage**
2003	Santa Fe, NM	$10.84
	San Francisco, CA	$12.25
2012	Albuquerque, NM	$8.75
	San Jose, CA	$10.30
2013	Bernalillo County, NM	$8.65
	Washington, DC	$11.50 (by 2016)
	Montgomery County, MD	$11.50 (by 2017)
	Prince George's County, MD	$11.50 (by 2017)
	SeaTac, WA	$15.24
2014	Las Cruces, NM	$10.10 (by 2019)
	Santa Fe County, NM	$10.84
	Mountain View, CA	$10.30
	Sunnyvale, CA	$10.30
	San Diego, CA	$11.50 (by 2017)
	Oakland, CA	$12.25
	Berkeley, CA	$12.53 (by 2016)
	Richmond, CA	$13.00 (by 2018)
	Louisville, KY	$9.00 (by 2017)
	Chicago, IL	$13.00 (by 2019)
	San Francisco, CA	$15.00 (by 2018)
	Seattle, WA	$15.00 (by 2018-2021)
2015	Emeryville, CA	$15.00 (by 2018)
	Los Angeles, CA	$15.00 (by 2020)
	Portland, ME	$10.68 (by 2017)
	Kansas City, MO	$13.00 (by 2020)
	Birmingham, AL	$10.10 (by 2017)
	St. Louis, MO	$11.00 (by 2018)
	Palo Alto, CA	$11.00 (by 2016)
	Johnson County, IA	$10.10 (by 2017)
	Los Angeles County, CA	$15.00 (by 2020-2021))
	Mountain View, CA	$15.00 (by 2018)
	Sacramento, CA	$12.50 (by 2020)
	Lexington, KY	$10.10 (by 2018)
	Tacoma, WA	$12.00 (by 2018)
	Bangor, ME	$9.75 (by 2019)

[1] The New Mexico minimum wage ordinance does not specify a tipped minimum wage. Employees who customarily receive tips are covered under Federal minimum wage law, which has been set at $2.13 per hour since 1996. In the case that an employee's cash wages plus tips fall short of the minimum wage, employers are supposed to make up the difference so that the employee receives a wage that is at least equal to the highest minimum wage to which they are entitled.

Source: National Employment Law Project, 2016

Bernalillo County's minimum wage increased in July of 2013. The amendment to the 2006 ordinance raised the minimum wage to $8.50 per hour and indexed the nominal value to the Consumer Price Index. No changes were made to the tipped minimum wage rate at the county level (Bernalillo County, 2013). For firms that provide at least $2,500 per year in health care or childcare benefits to workers, both ordinances allow a $1.00 per hour reduction in minimum wage. The Bernalillo County minimum wage covers all workers living in unincorporated areas of Bernalillo County. In 2010, Bernalillo County had 662,564 residents, with 545,852 (82%) living in Albuquerque. Of the remainder, 9,565 (1.4%) lived in other incorporated cities within the county (Mid-Region Council of Governments, n.d.). Employees within these incorporated areas would have been eligible for either the state or Federal minimum wage.

Table 2.2 | Relevant minimum wages by jurisdiction

Relevant minimum wages by jurisdiction								
	USA		New Mexico		Albuquerque		Bernalillo	
Year	Regular	Tipped	Regular	Tipped	Regular	Tipped	Regular	Tipped
2008	6.55	2.13	6.50	2.13	7.15	2.13	6.55	2.13
2009	**7.25**	2.13	**7.50**	2.13	**7.50**	2.13	**7.25**	2.13
2010	7.25	2.13	7.50	2.13	7.50	2.13	7.50	2.13
2011	7.25	2.13	7.50	2.13	7.50	2.13	7.50	2.13
2012	7.25	2.13	7.50	2.13	7.50	2.13	7.50	2.13
2013	7.25	2.13	7.50	2.13	**8.50**	3.83	**8.00**	2.13
2014	7.25	2.13	7.50	2.13	**8.60**	5.16	**8.50**	2.13
2015	7.25	2.13	7.50	2.13	**8.75**	5.25	**8.50**	2.13
2016	7.25	2.13	7.50	2.13	**8.75**	5.25	**8.65**	2.13
2017	7.25	2.13	7.50	2.13	**8.80**	5.28	**8.70**	2.13
2018	7.25	2.13	7.50	2.13	**8.95**	5.37	**8.85**	2.13
2019	7.25	2.13	7.50	2.13	**9.20**	5.52	**9.05**	2.13

Source: Bernalillo County, 2013; City of Albuquerque, 2012; The tipped minimum wage applies to workers who regularly and customarily receives tips in the course of their jobs; if total hourly earnings are less than the regular minimum wage for tipped employees, employers are to make up the difference.

Although the nominal values of the minimum wages in the two municipalities differ, businesses in both areas were subjected to increases in the minimum wage at roughly equal intervals.[2] The data I use do not provide fine enough geographic delineation to allow me to differentiate

[2] Albuquerque minimum wage increases began on January 1 of each year. The first Bernalillo County increase became effective on July 1, 2013.

Albuquerque city from the remainder of Bernalillo County but this should not cause serious issues because the majority of Bernalillo residents live in the city of Albuquerque. Thus, while 98.6% of Bernalillo County residents were eligible for a higher minimum wage after 2012, 82% were eligible for the slightly higher Albuquerque minimum wage.

Each minimum wage law covers different groups of workers and depending on circumstance and location, a given worker living in New Mexico may be covered by the Federal, state or local ordinance.[3] The Albuquerque City and Bernalillo County ordinances have the most extensive coverage of the minimum wage laws, while the New Mexico State minimum wage law has the lowest coverage.

While all three laws exempt Federal employees and registered apprentices, the New Mexico law also exempts domestic employees, certain categories of agricultural workers and most teenagers (New Mexico State, 2009). The United States Federal minimum wage law covers employees not covered by state or local ordinances. Details of the different wage laws and their exclusions are available in Table 2.4. In general, an employee is to be paid a wage that is at least equal to the *highest* minimum wage applicable to their location and position. For example, a teenager in a retail job in 2013 may have been entitled to a minimum wage of $7.25, $7.50, $8.00 or $8.50 an hour, depending on place of work. A server in a restaurant may have similarly been entitled to an hourly cash wage of $2.13 or $3.83, depending on the location of the restaurant.

2.5 DATA

I use two data sets: the Current Population Survey (CPS) and the American Community Survey (ACS). The two national surveys provide complementary information on employment, unemployment, hours worked and wages. Although both surveys are nationally representative and designed to produce reliable annual estimates of labour force statistics for the state and sub-state level, the sampling error is large enough that the estimates generated using the two surveys may differ.

[3] The city of Santa Fe also has a citywide minimum wage that is indexed to inflation. The Santa Fe rate increases on January 1 each year and ranged between $9.50 per hour in 2009 to $10.84 per hour in 2015.

Table 2.3 | Minimum wage coverage by jurisdiction

Jurisdiction	Employer Coverage	Employer Exemptions	Worker Coverage	Worker Exemptions
Albuquerque	Any employer required to have a business license or business registration from the City of Albuquerque. Any agency that hires and has discression over wages on behalf of an employer. The City of Albuquerque	The United States, the State of New Mexico or any political subdivision of the State.	Full time, part-time, seasonal or temporary workers who work for at least two hours in a given week.	Interns working for academic credit or work-study employees. Volunteers. Salespeople paid by piecework, on a flat rate schedule or who receive commission. Registered apprentices. $1.00 per hour reduction in MW for workers receiving more than $2,500 in annual health insurance.
Bernalillo	Any employer required to register as a business with the county that has control over wages, hours or working conditions of employees. Includes Bernalillo county.	The United States, the State of New Mexico or any political subdivision of the State.	Full time, part-time, seasonal or temporary workers who work for at least two hours in a given week.	Any person employed by parents, spouses or siblings. Babysitters working in the employer's home on a casual basis. Any employee under age 16. Tipped employees (those who 'customarily and regularly receive tips or gratuities' shall be paid the sub-minimum wage of $2.13). $1.00 per hour reduction in MW for workers receiving more than $2,500 in annual health insurance.
New Mexico	Any individual, partnership, association, corporation, business trust, legal representative or any organized group of persons employing one or more employees at any one time, acting directly or indirectly in the interest of an employer in relation to an employee, but shall not include the United States Government.	The United States government	All employees unless explicitly excluded.	Domestic servants in a private home. Executives, managers, supervisors. Volunteers. Salespeople paid by piecework, on a flat rate schedule or who receive commission. Any employee aged 18 or younger who has not graduated from secondary school. Seasonal employees who work for employers that have received exemptions. Any employee receiving board. Most agricultural employees.

Source: City of Albuquerque, 2012; Bernalillo County, 2013; New Mexico State, 2009

The Current Population Survey

The Current Population Survey (CPS) is a monthly household survey administered by the United States Census Bureau for the Bureau of Labour Statistics. Each month, professional interviewers use computer assisted telephone interviews to survey roughly 60,000 households (United States Census Bureau, n.d.). Each household is surveyed for a total of eight months in a 4-8-4 sampling design whereby each house is surveyed monthly for four months, given an eight-month break and then surveyed again for a final four months. Information on employment status and demographic characteristics is collected from respondents each month that they are interviewed but information on wages and hours worked is collected only in the fourth and eighth month of interviews (National Bureau of Economic Research, 2016).

The Current Population Survey offers several advantages. First, the CPS is the source of data used to compile the official United States labour force statistics and thus is best suited to provide measures of labour market outcomes that are consistent with official definitions. Second, the CPS explicitly asks respondents to report their hourly wage. This is useful for estimating the proportion of the population that is likely to be affected by changes in the minimum wage and for estimating the impact of minimum wage changes on income. Third, the use of CPS data in the analysis of minimum wage impacts is well established.

The CPS is designed to provide annual employment status measures for the entire population and for different socioeconomic groups at the Federal, state and sub-state level[4] (United States Census Bureau, n.d.). The smallest geographic unit for which data is collected in the CPS is the county, although not all counties are identified in the sample. In New Mexico, data is available for four counties: Bernalillo, Dona Ana, San Juan and Santa Fe. Data for respondents living outside of these counties is grouped together. The use of county as the geographical unit of measure is convenient for this project because it allows for the easy identification of treated individuals.

The main drawback of the CPS data is that respondents are not surveyed about their place of work. This is particularly problematic for estimating the impact of a minimum wage ordinance on hours worked and on wages. If increases in minimum wages cause employers to reduce hours, the impact will be seen among those who work in the affected jurisdiction, regardless of their place of

[4] For sub-state areas with populations of 65,000 or greater.

residence. For this reason, I use the American Community Survey to estimate the impact on the intensive margin of employment.

The American Community Survey

The American Community Survey (ACS) is an annual survey administered by the U.S. Census Bureau that is designed to replace the long form of the United States decennial census. Each year, the Census Bureau collects data on a wide range of topics including demographics, education, employment and commute times. The main advantages of the ACS are its relatively large sample size and its inclusion of a variable for place of work. The Census Bureau samples roughly three million households for the ACS each year, making it the largest household survey conducted in the United States (United States Census Bureau, 2014). The large sample allows for better analysis of impact at the local level and for small subgroups of the population. This is particularly advantageous for minimum wage studies which tend to focus on groups with high proportions of minimum wage earners such as teens.

The main drawbacks of the ACS data lie in the reference period, the absence of a wage variable and the inconsistent in-state geographic boundaries. The ACS reference period for earnings questions is the 12-month period immediately preceding the survey (United States Census Bureau, 2014). Respondents are asked to report their earnings over the previous twelve months and researchers must then estimate the hourly wage using reported earnings, weeks worked and usual weekly hours. This results in a very noisy wage variable (for a thorough discussion, see Allegretto, Dube, Reich, & Zipperer, 2013). This issue is compounded by the effective time period captured in the annual earnings data for a given year. Individuals surveyed, for example, in January 2013 were asked to reference the time period from February 2012 to January 2013, while individuals surveyed in December 2013 were asked to reference the time period from January 2013 to December 2013. This means that the 2013 earnings data will actually contain information spanning the 23 months between February 2012 and December 2013 (United States Census Bureau, 2014).

Another problem with the American Community Survey is that the in-state geographic boundaries are not consistent throughout the time period covered in this study. The finest level of geographic delineation in the ACS is the Public Use Microdata Area (PUMA). There are 18 residential PUMAs and 35 place of work PUMAs in New Mexico but the Census Bureau made substantial changes to the boundaries of the Public Use Microdata Areas following the 2010 census. The new

boundaries were incorporated into the ACS data in 2012. For this reason, 2009-2011 PUMAs are not directly comparable to 2012–2014 PUMAs.

This presents a problem for delineating consistent treatment and control groups and for statistical analysis using fixed effects. In 2009-2011, the Albuquerque PUMA contained only Bernalillo County. However, when the boundaries were redrawn after the 2010 census, neighbouring Valencia County was added to the Albuquerque PUMA. Therefore, I am unable to define the treatment group with as much precision in the 2012-2014 period. Because Valencia county is small relative to Bernalillo County, this should not pose too much of a problem but it will bias my results toward zero.[5] The change in PUMA boundaries also impacts my ability to use geographical area fixed effects. Instead of using all 35 place of work PUMAs in the fixed effects analysis, I am limited to six regions that have consistent boundaries throughout the duration of study. These 'consistent PUMAs' are much larger and may therefore mask some intra-state economic diversity.

2.6 SAMPLE SELECTION

I construct two samples, one using the Current Population Survey (CPS) and one using the American Community Survey (ACS). I use the CPS sample to analyse the impact of minimum wage increases on employment and unemployment and the ACS to assess the impact on hours worked.

I begin by taking the full sample from each data set and dropping members of the armed forces and those who live in institutionalized group quarters to yield the civilian non-institutional sample.[6] Then, to eliminate workers who are not covered by the minimum wage laws in Albuquerque/Bernalillo, I drop employees of state or Federal governments, unpaid workers in family businesses, the self-employed and those working fewer than three hours per week. For both samples, I construct dummy treatment variables equal to 1 if the individual lived (or worked, in the case of the ACS) in Bernalillo County, 0 otherwise.

Demographic data for treatment and control groups for the CPS sample is available in Table 2.4. In comparison to the remainder of New Mexico, Bernalillo County is relatively affluent.

[5] The population of Valencia county at the 2010 census was 76,559 compared to 662,654 in Bernalillo.
[6] The CPS is designed to survey the civilian non-institutional population but military personnel may be included in the sample for a variety of reasons.

Employment and labour force participation are higher in Bernalillo and unemployment is lower. Similarly, Bernalillo County is home to a larger proportion of college graduates and fewer workers are employed in low-wage occupations. The proportion of both Hispanic and Native American residents is lower in Bernalillo than in the remainder of New Mexico, although there is a larger proportion of black residents. There are also slightly more non-citizen immigrants in Bernalillo than in the remainder of New Mexico.

Table 2.4 | Demographic characteristics in Bernalillo and the remainder of New Mexico

Sample characteristics		
	Albuquerque	Other NM
Employed	0.506	0.447
	(0.50)	*(0.50)*
Unemployed	0.084	0.094
	(0.29)	*(0.28)*
In labour force	0.552	0.494
	(0.50)	*(0.50)*
Age	44.2	46.5
	(19.5)	*(19.0)*
Male	0.473	0.478
	(0.50)	*(0.50)*
Married	0.451	0.490
	(0.50)	*(0.50)*
Black	0.038	0.017
	(0.13)	*(0.19)*
Hispanic	0.431	0.439
	(0.50)	*(0.50)*
Native	0.051	0.131
	(0.34)	*(0.22)*
Non-citizen	0.084	0.071
	(0.26)	*(0.28)*
Less than high school education	0.134	0.179
	(0.38)	*(0.34)*
High school education only	0.252	0.300
	(0.46)	*(0.43)*
College Graduate	0.278	0.167
	(0.37)	*(0.45)*
Family size	2.619	2.850
	(1.76)	*(1.56)*
Employed in low-wage occupations	0.097	0.100
	(0.30)	*(0.30)*
Observations	19906	35976

Note: Data from the Current Population Survey; standard deviation in parenthesis

2.7 METHODOLOGY AND RESULTS

To uncover the relationship between the minimum wage increase and labour market outcomes in Bernalillo/Albuquerque, I begin by identifying the impact of the minimum wage hike on hourly wages and the distribution of wages for low wage workers. This analysis is included in Section 3.7.1. I then investigate the degree to which the higher minimum wage resulted in changes in employment, unemployment and hours worked for teens and food service workers in affected jurisdictions. A description of my methodology and an overview of the results is presented in Section 3.7.2.

Although difference-in-differences models estimated with OLS have long been the accepted method for estimating minimum wage effects, recent work has highlighted the importance of confirming the parallel trends assumption and/or controlling for differences in pre-treatment trends between treatment and control groups (Allegretto et al., 2013). To account for the possibility of differing pre-treatment trends, this paper uses both OLS and Synthetic Control models to estimate the minimum wage effects on employment outcomes. The estimates generated using the two methods are remarkably similar.

Wages

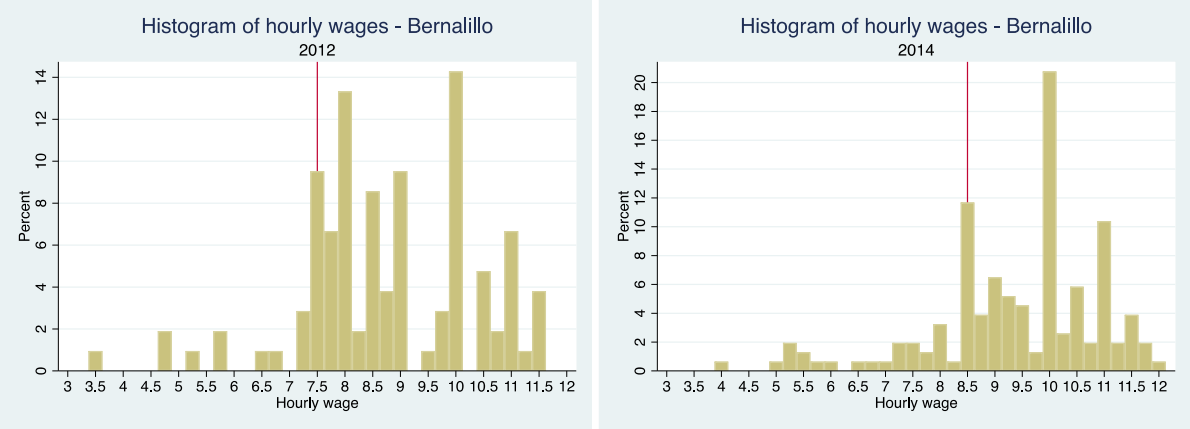
The Bernalillo/Albuquerque minimum wage ordinance was successful at shifting the wage distribution of low wage workers, although evidence suggests that employers of tipped employees responded to the higher minimum wage by shifting employees onto the lower tipped minimum wage. Histograms of wages for workers reporting hourly earnings of $12 or less are presented in Figure 2.1.[7] In the left-hand panel, the pre-treatment distribution of low-wage earnings reveals a binding minimum wage of $7.50 per hour with additional spikes at $8.00, $8.50, $9.00 and $10.00 per hour. The right-hand panel shows that post-ordinance, the low-wage distribution has shifted rightward with a spike at the new minimum wage of $8.50. From these histograms, it is apparent that the ordinance placed a binding constraint on employers and that firms responded to the ordinance by increasing wages for minimum wage workers. However, the distribution of earnings

[7] Histograms of hourly wages for the remainder of New Mexico are available in the Appendix. They are not affected by the ordinance.

does not completely clear out at all values below $8.50. This suggests issues with non-compliance or the use of the sub-minimum wage for tipped workers.

The Albuquerque/Bernalillo minimum wage laws are unusual among municipal minimum wages in the fact that they allow for a significantly reduced sub-minimum wage for tipped employees. Both San Francisco and San Jose ordinances require employers to pay tipped workers at the same minimum wage rate as non-tipped workers, while in Seattle, the minimum wage law created a tipped wage provision by limiting the raise required for tipped workers. This contrasts sharply to the requirements of the Albuquerque/Bernalillo ordinances that specify deeply discounted minimum wage rates for workers who customarily receive tips on the job. The tipped minimum wage in Albuquerque increased from $2.13 in 2012 to $3.38 in 2013 and was set at 60% of the regular minimum wage thereafter. In Bernalillo, tipped workers were entitled only to the Federal tipped minimum wage of $2.13.

Figure 2.1 | Histogram of hourly wages for low wage workers



Note: Data from CPS. Distribution of hourly earnings for workers reporting wages of less than $12 per hour. Vertical lines represent the prevailing minimum wage. The Bernalillo minimum wage was implemented in July 2013 so the higher minimum applied for roughly half of the year. The 2012 histogram shows the pre-treatment distribution and 2014 represents the post-treatment distribution.

My findings reveal that the Albuquerque/Bernalillo minimum wage ordinance had mixed success at raising wages for low paid workers. While non-tipped employees received an increase in wages, many tipped employees experienced a reduction in their hourly rate of pay. The tipped minimum wage offered a potentially unintended channel of adjustment for employers of workers who ordinarily earned tips. Table 2.5 shows minimum wage and sub-minimum wage earnings in Bernalillo County and the remainder of New Mexico from 2011 to 2015. Two patterns emerge in

this table. First, except for a small increase in 2013, the proportion of Bernalillo workers receiving minimum wage remains relatively unchanged throughout the observation window. Second, the number of workers earning less than minimum wage jumps dramatically in 2013 and remains high in the following years.

To identify which groups are experiencing an increase in sub-minimum wage earnings, I compare changes in wages by demographic and occupational groups (Table 2.6). I first separate hourly wages into three bins: below $7.50, between $7.50 and $8.49 and $8.50 or higher. For each group of workers, I then calculate the pre- and post- treatment proportion of earners with wages in each category as well as the percentage point change associated with the minimum wage increase. The results show that nearly all groups of workers experienced a decline in the prevalence of wages between $7.50 and $8.49 per hour. The most dramatic shift was seen among teens, where the proportion of earners with wages in this bin declined by 22.6 percentage points. As a group, teens were more likely to earn wages above $8.50 per hour following the ordinance than any other sub-group. Before the ordinance, only 29.2% of teens earned $8.50 or more per hour. After the passage of the ordinance, this number increased 30.1 percentage points to 59.2%.

Table 2.5 | Minimum and below minimum wage earners (2011-2015)

Minimum wage and below minimum wage earnings, 2011-2015										
	Bernalillo County					Other NM				
	2011	2012	2013	2014	2015	2011	2012	2013	2014	2015
Minimum Wage	$7.50	$7.50	$8.00 / $8.50**	$8.50 / $8.60	$8.65 / $8.75	$7.50	$7.50	$7.50	$7.50	$7.50
observations	*372*	*370*	*382*	*459*	*729*	*600*	*580*	*538*	*771*	*1,324*
Percent earning:										
Below MW*	4.3%	3.8%	12.8%	11.3%	11.7%	5.7%	6.6%	5.0%	4.7%	4.0%
MW	2.3%	2.7%	5.5%	3.2%	2.4%	3.8%	4.9%	3.9%	4.3%	3.0%
Below next year's MW	4.3%	13.2%	17.9%	13.2%	11.7%	5.7%	6.6%	5.0%	4.7%	4.0%

*Note: Data from CPS. *Includes tipped workers and those excluded from coverage under state law. **Minimum wages for Bernalillo and Albuquerque, respectively. Percentages reference the Albuquerque ordinance.*

Not all groups experienced this earnings benefit following the minimum wage increase. The increase in sub-minimum wage earnings was concentrated among food-service workers and workers who report earning tips. The presence of the sub-minimum wage provision for tipped employees allowed employers to respond to the minimum wage hike by *reducing* wages of workers who typically receive tips. This is evident in Table 2.6: the proportion of food service

workers earning between $7.50 and $8.49 decreased by 19.4 percentage points. This change is evidence of employers changing the hourly wage of minimum-wage workers in compliance with the law. However, while some employers complied by increasing wages to the new minimum wage, others complied by reducing wages to the lower tipped minimum or by placing new hires on the lower tipped wage. The impact on wages for minimum wage workers in the food service industry was divided: the percentage receiving pay cuts roughly equalled the number receiving pay raises. Following the ordinance, the proportion earning sub-minimum wages increased by 9.5 percentage points, while the proportion earning minimum wage or above increased by 9.9 percentage points. Among workers who specifically state that they receive tips, the increase in sub-minimum wage earnings is more pronounced. Among this group, the proportion earning $7.50-8.49 did not change following the ordinance but the prevalence of sub-minimum wage earnings increased by 5.9 percentage points. At the same time, the proportion of tipped workers earning at least $8.50 per hour declined by 6.1 percentage points; this change is consistent with increased reliance on the sub-minimum wage provision of the law.

Table 2.6 | Pre- and post-treatment comparisons of worker earnings by group

Changes in earnings by wage bracket, occupation and demographic									
	<7.50			7.50 - 8.49			>=8.50		
	Pre	Post	Change	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All workers	4.8%	5.4%	0.6%	8.5%	5.1%	-3.4%	86.7%	89.5%	2.8%
Teens (16-18)	17.8%	10.3%	-7.5%	53.1%	30.5%	-22.6%	29.2%	59.2%	30.1%
Young adults (20-24)	10.4%	8.4%	-2.1%	20.7%	13.7%	-7.0%	68.8%	77.9%	9.1%
Female	5.7%	4.6%	-1.1%	9.5%	6.0%	-3.5%	84.8%	89.4%	4.6%
Less than HS education	4.3%	1.7%	-2.6%	15.9%	4.1%	-11.8%	79.8%	94.2%	14.4%
Non-citizens	4.3%	11.3%	7.0%	19.9%	2.9%	-17.0%	75.8%	85.8%	10.0%
Food service	26.9%	36.4%	9.5%	23.0%	3.7%	-19.4%	50.1%	60.0%	9.9%
Those earning tips	11.7%	17.5%	5.9%	6.1%	6.4%	0.2%	82.2%	76.1%	-6.1%

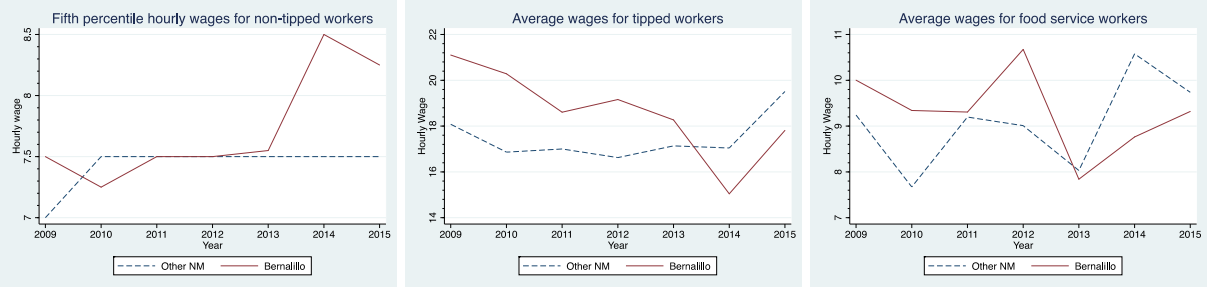
Note: Data from CPS. Author's calculations. Percentage point change presented in columns (3), (6) and (9).

Further evidence in support of this hypothesis is presented in Figure 2.2. The left-hand panel of Figure 2.2 shows changes in the fifth percentile hourly wage for workers who do not report earning tips. The upward shift in earnings for this group in 2014 shows that the minimum wage was successful at raising wages for non-tipped workers. For both treated and control group workers, the fifth percentile wage tracks nicely with the statutory minimum wage. Average wages for tipped

and food service workers are plotted in the centre and right-hand panels of Figure 2.2.[8] Although most tipped and food service workers earn wages above the statutory minimum, a decline in average wages is apparent for both groups following the minimum wage hike, a result that reflects a decline in the hourly wages of the lowest paid food service and tipped workers.

The growth in prevalence of sub-minimum wage earnings could have several explanations outside of an expanded reliance on the tipped provision of the law. First, employers could fail to comply with the law either out of defiance or ignorance. If this were the case, non-compliance would be evident in the histograms by the persistence of wage bunching at the old minimum wage level of $7.50; however, this is not evident. Another possible explanation for the increase in sub-minimum wage earnings is an expansion in health care offerings by employers. Under the Bernalillo/Albuquerque ordinances, employers spending more than $2,500 per year on an employee's health insurance may pay $1.00 per hour less than the current minimum wage rate. The data does not allow me to test specifically for this but the absence of spikes in the histograms at $7.50 per hour suggest that this practice is not widespread.

Figure 2.2 | Pre- and post-ordinance plots of wages



Note: Data from CPS.

The Bernalillo/Albuquerque minimum wage ordinance was successful at raising wages for non-tipped workers. Overall, teens and young adults were more likely to earn wages at or above $8.50 per hour, as were adults with less than a high school education. However, whether by design or oversight, the sub-minimum wage provision of the ordinance that allows employers to pay reduced hourly wages to tipped employees served to offset the benefit of the higher minimum wage for food service and tipped workers.

[8] Due to sample size restrictions, I cannot plot the 5th percentile of earnings for either tipped or food service workers.

Employment, Unemployment and Hours Worked

To assess the impact of minimum wages, I begin with a standard fixed effects model that is common in the literature. Using data organized as a region (county or PUMA)/year panel, I employ the following fixed effects model:

$$(1)\ Y_{it} = \beta_0 + \gamma_1 R_i + \gamma_2 T_t + \beta_1(\mathrm{MW}_{it}) + \delta_3 X_{it} + \varepsilon_i$$

Here, γ_1 and γ_2 represent region and time fixed effects. X_{it} is a vector of demographic covariates including race, gender, educational attainment, union membership and marital status. Variable MW_{it} is a dummy treatment variable equalling 1 if worker *i* was subject to the ordinance at time *t* and β_1 is our coefficient of interest. For employment and unemployment, workers are considered subject to the ordinance if they lived in Bernalillo County, while for hours worked, individuals are considered to be subject to the ordinance if they work in Bernalillo County. Although race and gender are fixed attributes for individuals, the percentage of individuals of a given race or gender within a county will change over time. The coefficients on these covariates allow me to control for race and gender through changes in the relative racial and gender compositions of the different counties.

To supplement the fixed effects estimates, I employ the synthetic control methodology developed by Abadie, Diamond and Hainmueller (2010). The synthetic control estimator is an extension of the difference-in-difference estimator that accommodates differing pre-treatment trends between treated and control units. Like difference-in-differences models, the synthetic control model estimates the treatment effect by comparing pre- and post-treatment differences in treated and control regions. However, unlike the difference-in-differences model, in which all control units are weighted equally, the synthetic control model assigns weights to each potential control region such that the pre-treatment difference between the treatment and control regions are minimized. The trajectory of the outcome variable is then monitored post-treatment to determine the effect of treatment. This allows for easy visual identification of a treatment effect by establishing a common pre-treatment trend in the variable of interest.

In creating the synthetic control group, I include as possible donors all counties in the 20 U.S. states that did not experience a minimum wage increase between 2009 and 2015 and whose

populations were large enough for independent inclusion in the CPS or ACS.[9] In total, I have 98 potential donor regions in the CPS and 338 in the ACS. Each county in the donor pool is assigned a weight between 0 and 1. These weights are chosen to minimize the difference between the treatment and control group in the pre-treatment period with respect to key variables. I chose to minimize the yearly pre-treatment differences of the dependent variable as well as mean values of key demographic variables such as race, education, union membership and the prevalence of low-wage work in the county. Inference is performed by analysing adjusted p-values as outlined in Abadie et al. (2010).

I implement the synthetic control estimator using the synth_runner package for Stata created by Galiani and Quistorff (2017). The estimates generated are based on a summative factor model of the form:

$$(2)\ Y_{it} = \sum_{t=1}^{T} \alpha_{it} D_{it} + Y_{it}^{N}$$

In this model, Y_{it} is our outcome variable of interest (employment, unemployment or hours worked), α_{it} is a time-varying treatment effect and D_{it} is a dummy variable indicating that unit *i* is treated in time *t*. The variable Y_{it}^{N} is the estimated level of the outcome variable as estimated by post-treatment level in the synthetic control group.

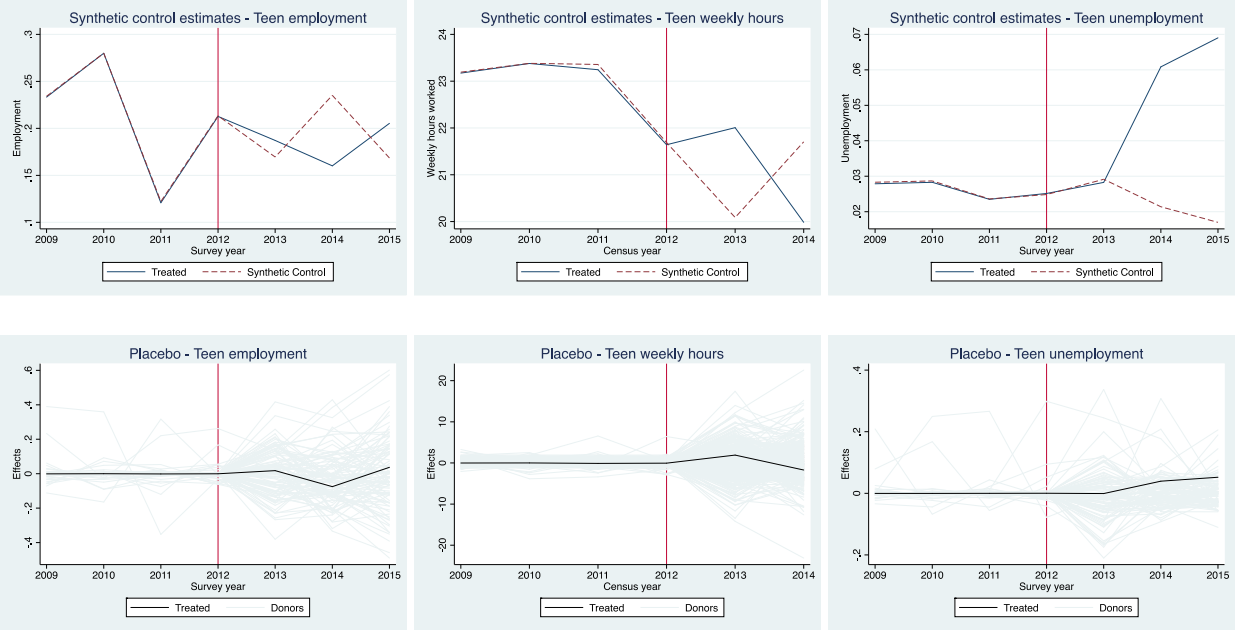
Do employment opportunities decline following a binding minimum wage increase? Traditional economic theory would predict that an ordinance requiring wage increases both increases the supply and reduces the demand for labour. My results provide partial support for this hypothesis. While suggesting that the number of jobs available to teenagers remained unchanged following the ordinance, my findings reveal an increase in the relative supply of teenage labour and a corresponding increase in the teenage unemployment rate; I find no change in the weekly number of hours worked by teens. I do, however, detect a decline in the intensive margin of employment for food service workers, suggesting that the higher minimum wage led employers to improve

[9] Synthetic control states include Alabama; Georgia; Idaho; Iowa; Kansas; Louisiana; Maine; Mississippi; New Hampshire; North Carolina; North Dakota; Oklahoma; Pennsylvania; South Carolina; Tennessee; Texas; Utah; Virginia and Wisconsin.

efficiency with staffing even as they increased their reliance on the tipped provision of the minimum wage ordinance.

Changes in teenage labour market outcomes can be seen in Figure 2.4. The top panel shows how employment, unemployment and hours worked evolved in Bernalillo compared to that in its synthetic control, while the bottom plot presents placebo results. To create the placebo, the synthetic control method is applied individually to all counties in the donor pool. Each line on the placebo graph represents the trajectory of the dependent variable in a donor county relative to its unique synthetic control group. An examination of the placebo plots reveals the magnitude of change in Bernalillo relative to that in counties that experienced no policy intervention.

Figure 2.3 | Synthetic control plots of labour market outcomes | Teens



Note: Top panel illustrates employment, unemployment and hours worked in Bernalillo compared to its synthetic control group. Bottom panel contains placebo plots.

Contrary to the predictions of traditional economic theory, the number of jobs for teenage workers did not decline following the minimum wage increase. Figure 2.4 shows that teenage employment levels in Bernalillo fluctuate closely around those of its synthetic control in the post-treatment period and placebo plots suggest that Bernalillo's post-treatment difference from its synthetic control is small relative to that of other donor counties. The exact magnitude of the changes and their corresponding p-values can be found in Table 2.7. The synthetic control method produced a

mixture of positive and negative coefficients, although none are statistically significant. Fixed effects methods also fail to produce statistically significant estimates of changes in employment. While the coefficient is negative, it is imprecisely estimated. Taken together, these estimates lie in line with many recent studies that find that minimum wage increases do not necessarily lead to job losses.

Table 2.7 | Estimates of the minimum wage effect on teenagers

Estimates of the effect on teens			
Fixed effects estimates			
	Employment	Unemployment	Hours
	(1)	(2)	(3)
Treatment Effect	-0.062	0.079***	-1.282
	(0.09)	(0.03)	(6.51)
Constant	-0.149	0.308**	304.491**
	(0.26)	(0.11)	(115.79)
Data Source	CPS	CPS	ACS
Observations	35	35	42
R-squared	0.675	0.769	0.583
Synthetic control estimates			
	Employment	Unemployment	Hours
	(1)	(2)	(3)
Treatment effect (2013)	0.018	-0.001	1.92
P-value	(0.60)	(0.39)	(0.67)
Treatment effect (2014)	-0.075	0.039**	-1.72
P-value	(0.47)	(0.03)	(0.70)
Treatment effect (2015)	0.037	0.052**	-
P-value	(0.56)	(0.02)	-
Data Source	CPS	CPS	ACS
Donor regions	98	98	338

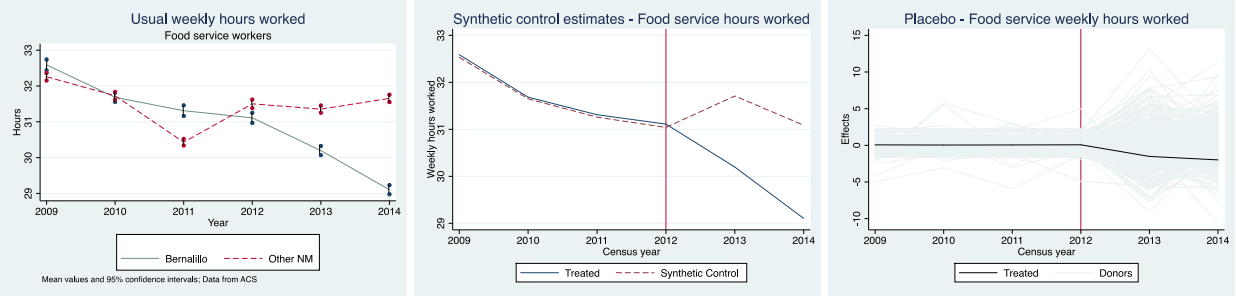
*Note: Robust standard errors in parenthesis below fixed effects estimates; Adjusted P-values in parenthesis below synthetic control estimates. *** p<0.01, ** p<0.05, * p<0*

Employers facing higher labour costs may choose to reduce employment by decreasing hours rather than by cutting the total number of workers employed. My results suggest that this did not happen. Synthetic control and placebo plots of weekly hours worked by teens are presented in the centre column of Table 2.4. These plots reveal fluctuations in the intensive margin of employment but no apparent decline. The coefficients reveal that hours worked by teens in Bernalillo increased relative to the control group by 1.92 hours per week in 2013 but fell in 2014 by 1.72 hours. Neither

estimate is statistically significant. The OLS estimate for hours worked is negative but imprecisely estimated.

Job losses and reductions in hours represent demand-side shifts in the labour market following increases in the minimum wage. Changes in unemployment may result from changes in either supply or demand. I find that the minimum wage ordinance was successful at raising the labour force participation rate of teenagers. Without a corresponding increase in available jobs, many of these labour force entrants faced unsuccessful job searches. This is evidenced by the increase in unemployment identified by both synthetic control and regression models. Synthetic control plots reveal a large increase in teenage unemployment in Bernalillo, the magnitude of which can be seen in both the upper and lower panels of Figure 2.4. Synthetic control estimates of changes in teenage unemployment reveal a statistically significant increase in teenage unemployment of 3.9 percentage points in 2014 and 5.2 percentage points in 2015. These results are supported by the OLS estimates suggesting that the minimum wage increased teenage unemployment by approximately 7.9 percent.

Figure 2.4 | Impact of minimum wage on hours worked in the food service industry



Note: Left-hand panel shows hours worked for food service workers in Bernalillo in comparison to the remainder of New Mexico. Centre panel shows hours worked for food service workers in Bernalillo against that of its synthetic control. Right-hand panel contains placebo plots of hours worked in food service.

Instead of raising wages for food service workers, employers responded by expanding their use of the tipped provision of the minimum wage law. Although employers were able to use the lower tipped wage to offset some of the costs of the higher minimum wage, many employers also responded by reducing hours for food service workers. Faced with a perceived increase in wages, employers sought efficiencies in scheduling that likely placed some employees in the compromised situation of having both lower wages and fewer hours. The reduction in hours worked is apparent in Figure 2.5. The left-hand panel plots weekly hours worked by food service

employees in Bernalillo relative to the remainder of New Mexico. The centre and right-hand panels show the synthetic control and placebo plots. OLS estimates of the effect on hours worked suggest a negative but imprecisely estimated result, while synthetic control estimates place the reduction in weekly hours at 1.51 in 2013 and 1.98 in 2014 (Table 2.8). These estimates are significant at the 10% and 5% level respectively.

Table 2.8 | Estimates of the minimum wage effect on food service workers

Estimates of the effect on food service workers		
Fixed effects estimates		
	Employment	Hours
	(4)	(2)
Treatment Effect	-0.003	-1.056
	(0.09)	(2.49)
Constant	1.374	27.517
	(1.42)	(38.87)
Data Source	CPS	ACS
Observations	35	42
R-squared	0.728	0.576
Synthetic control estimates		
	Employment	Hours
	(4)	(5)
Treatment effect (2013)	0.071	-1.51*
P-value	(0.06)	(0.08)
Treatment effect (2014)	0.029	-1.98**
P-value	(0.11)	(0.04)
Treatment effect (2015)	0.046	-
P-value	(0.11)	-
Data Source	CPS	ACS
Donor regions	95	340

*Note: Robust standard errors in parenthesis below fixed effects estimates; Adjusted P-values in parenthesis below synthetic control estimates. *** p<0.01, ** p<0.05, * p<0*

On average, minimum wage workers in food service establishments were made worse off by the Bernalillo minimum wage. My estimates suggest that roughly half of minimum wage workers in the food service industry received raises from $7.50 to $8.50 per hour, while the other half was shifted to sub-minimum wages. My methodology does not allow me to discern which of these workers also experienced a reduction in the intensive margin of employment. However, for a worker who received a raise to the new minimum but a reduction in hours, the weekly net financial loss would be $15. For those who received both a pay cut and a reduction in hours, the financial

loss would be much larger, up to $128 per week.[10] Unless restaurant patrons simultaneously became more generous tippers, this decline would be borne directly by workers.

2.8 DISCUSSION/CONCLUSION

Overall, my analysis shows the Bernalillo minimum wage ordinance had mixed effects on the labour market. While I find no evidence of a direct loss of jobs following the ordinance, the labour market responses were not uniformly positive. Teens were the largest beneficiary of wage increases with almost 30 percent of the teen workforce receiving a raise to the new minimum wage rate. However, the higher minimum wage served to draw teenagers into the labour force at a time when the number of jobs was not growing. As a result, teen unemployment increased by approximately 5 percentage points as many of these labour-market-hopefuls were unable to find jobs.

In the food service sector, the labour market response was less positive. Evidence suggests that employers adapted to the minimum wage increase by relying on two avenues of adjustment: the tipped provision of the minimum wage law and the intensive margin of employment as measured by weekly hours worked. The proportion of workers in the food service industry earning less than the new minimum wage grew by 9.5 percentage points in the two years following the minimum wage increase. At the same time, weekly hours worked in the food service industry declined. A serious limitation in my methodology is that I cannot observe wages and hours worked for particular employees. It is impossible for me to discern whether workers experienced wage cuts directly or if employers started new hires at the lower tipped minimum wage. I am similarly unable to tell where the weekly reduction in hours occurred. It is possible that the number of hours worked in food service establishments was not changed *overall*. Without looking at food service employment, I cannot tell what factors contributed to the reduction in average weekly hours. The reduction could have resulted, as neo-classical theory predicts, from an attempt at cost containment on the part of employers. On the other hand, the reduction in hours could be the result of an increase in overall employee numbers accompanied by a shift toward part-time workers. More research in this area would be needed before firm conclusions could be drawn.

[10] Based on a reduction in wage from $7.50 to $3.60 per hour and reduction in hours from 31 to 29 hours per week.

CHAPTER 3: THE NEW ZEALAND LIVING WAGE: EARNINGS, LABOUR COSTS AND TURNOVER

Disclaimer:
The results in this thesis are not official statistics, they have been created for research purposes from the Integrated Data Infrastructure (IDI) managed by Statistics New Zealand.

The opinions, findings, recommendations and conclusions expressed in this thesis are those of the author not Statistics NZ or Victoria University.

Access to the anonymised data used in this study was provided by Statistics NZ in accordance with security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person, household, business or organisation and the results in this thesis have been confidentialised to protect these groups from identification.

Careful consideration has been given to the privacy, security and confidentiality issues associated with using administrative and survey data in the IDI. Further detail can be found in the Privacy impact assessment for the Integrated Data Infrastructure available from www.stats.govt.nz.

The results are based in part on tax data supplied by Inland Revenue to Statistics NZ under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information may be published or disclosed in any other form, or provided to Inland Revenue for administrative or regulatory purposes.

Any person who has had access to the unit-record data has certified that they have been shown, have read, and have understood section 81 of the Tax Administration Act 1994, which relates to secrecy. 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.

3.1 ABSTRACT

This paper explores the voluntary living wage program in New Zealand. Using data from Statistics New Zealand's Integrated Data Infrastructure (IDI), I investigate the characteristics of living wage firms, the impact of certification on employee earnings, and the effect of the living wage on firm turnover. My findings suggest that the living wage is gaining popularity among small firms and for-profit entities in New Zealand and that relative to a matched group of control firms, the living wage is effective at raising earnings of low-income workers. I find evidence that average labour costs increase in living wage enterprises following certification but find no evidence that firms cut jobs in response to these higher costs. Additionally, I find that living wage firms are no more likely to go out of business than are their un-certified counterparts. Additionally, I find no evidence of declining turnover in certified organizations.

3.2 INTRODUCTION

Despite minimum wages, tax credits and other targeted welfare transfer systems, poverty remains a problem for working households in developed countries. Within New Zealand and much of the OECD, market wages for low-skill work are often insufficient to provide an adequate standard of living. Within New Zealand, half of poor individuals under 65 years of age live in households whose primary source of income is wages (Perry, 2017). This inability of low-skilled workers to support themselves and their families is a problem for which no simple policy solution exists.

In the United States, Canada, the United Kingdom and New Zealand, living wage campaigns have developed as a way to define and encourage a market-based wage that when coupled with existing systems of welfare transfer provide an adequate but modest livelihood for a family with two children. The idea that the market wage should support a family sits in contrast to the traditional approach of compensating workers for the completion of a particular set of tasks. Instead of focusing on the dollar value generated by the employee's effort, the living wage places the focus on the minimum earnings necessary to support people in the society in which they live. In other words, a living wage is one that provides sufficient income to keep a family out of poverty.

While living wages are mandatory for some employers in the US, the living wage systems in Canada, the UK and New Zealand are voluntary. In voluntary living wage programs, employers who choose to pay the living wage can pursue certification through a local certifying body. To

date there is little research on the effects of voluntary living wage ordinances. Specifically, we do not yet know if voluntary living wage policies are successful at raising earnings of low-wage workers or if paying higher wages can improve business outcomes for firms. The research that exists is best thought of as exploratory because it relies on qualitative surveys (Brown, Newman, & Blair, 2014; Stansfield, 2017) or quantitative analysis on small samples of firms without significance testing (Wills & Linneker, 2012). There exists a small body of literature on mandatory living wages in the United States. The results of this work point to many possible outcomes for living wage firms including a reduction in turnover (Reich, Hall, & Jacobs, 2005; Howes, 2005), improved recruitment (Fairris, 2005) and the ability to hire better trained or qualified workers for low-skill jobs (Fairris & Bujanda, 2008). However, living wages are mandatory in the US and it is unclear if the same outcomes would result from voluntary programs.

This paper focuses on exploring several facets of the living wage experience for employers and workers in New Zealand. I begin with a careful exploration of the characteristics of New Zealand living wage firms and discuss possible motivations for pursuing living wage certification. I look at industry composition, size, age and employment levels and estimate the degree to which the make-up of living wage firms is evolving over time. Second, I investigate whether living wage certification leads to changes in average labour costs, employment levels or employee earnings. Lastly, I look at patterns of job turnover before and after certification to see if lower turnover rates are associated with the living wage.

Traditional economic wisdom suggests that higher labour costs will negatively impact both employment levels and overall firm competitiveness. However, the voluntary nature of the New Zealand living wage suggests that firms become certified in part because they can afford to pay higher wages and in part because they derive some utility from doing so. My estimates reveal that New Zealand's voluntary living wage program is successful at raising monthly earnings of low paid workers by approximately 17% but that firms are able to offset wage increases to these workers through wage compression, making overall increases in total labour costs difficult to detect. I find no evidence of disemployment in certified living wage firms. In fact, the point estimates for firm head-count are consistently positive, but heterogeneity makes it difficult to estimate the coefficients with precision. Contrary to qualitative research results indicating a

reduction in turnover in some living wage firms, I am unable to find evidence of a reduction in turnover using any of my metrics.

To uncover the effects of the living wage on earnings, labour costs and turnover, I rely on the two-way fixed effects methodology that is standard in the labour economics literature. By including fixed effects for both firm and month, I am able to control for stable differences between firms as well as for changes over time that have similar effects across firms. By adding linear and quadratic time trends to some specifications, I am able to control for differences in trends between treated and control firms. By applying this methodology to administrative and payroll data sourced from the IDI, I have the best chance to accurately detect and measure changes in employee earnings, labour costs and turnover following living wage certification. As such, this work provides a starting point to understand the effects of the living wage on household poverty and benefit receipt. The key disadvantage of this method lies in the fact that individuals cannot be linked into households within the IDI. Therefore, I cannot estimate changes in means-tested benefits or the prevalence of poverty among living wage workers. To understand these important links, additional work will be needed.

3.3 The New Zealand Living Wage and Poverty

Within New Zealand, the idea that a wage should be sufficient to meet household needs is not new. In 1894, Parliament enacted the Industrial Conciliation and Arbitration Act (ICA). The goal of the ICA was to eliminate costly labour strikes by providing a forum for labour and management to discuss and adjudicate labour disputes (Chelliah & Mukhi, 2004). Under the act, labour disputes were handled on a case-by-case basis, and Court awards were valid for three years. Union strike action was prohibited during the time when a case was pending before the Court or was covered by a standing settlement. The effect of this legislation was to change the way wages were determined. Rather than being driven by market forces, the wage for low-skill labour was fixed by the Arbitration Courts (Williams, 1976). For many years, the Court set wages by referencing pay provided by relatively high-paying employers. However, as the number of arbitration settlements grew and inflation eroded the value of the wages established through arbitration, it became more difficult to identify the appropriate ‘fair wage’ for a specific type of labour. This led to a shift away from fair market wages toward ‘family’ or ‘living wages’ based on estimates of need (Hyman,

2002). This paved the way for a long period of mandated living wages for low skilled workers in New Zealand.

While today's living wage is gender neutral, the language of the Industrial Conciliation and Arbitration Act focused on encouraging wages sufficient to support a male breadwinner and his dependent family. In 1936, the family wage was officially codified into law with the establishment of specific wage rates that would provide a base level of purchasing power for workers. For males, the rate was designed to support the worker, his wife, and three dependent children; a lower rate was specified for females (Hyman, 2002). Arbitration courts were to award this rate to all cases under their jurisdiction. Criticisms of the Breadwinner model mirror the criticisms of today's minimum and living wages. Employers protested high mandated wage floors, and diversity in household composition meant that the 'family wage' wage was rarely properly sized for a given worker's financial responsibilities. Single employed mothers with children were vastly under compensated on a needs-based level. This Breadwinner model of living wages was phased out throughout the 1940s and 1950s as minimum wages coupled with targeted assistance replaced the family wage (Ministry of Social Development, 2018).

Have we come full circle to resurrect a flawed model? The simple answer is 'no'. The modern living wage movement shares some similarities with the original breadwinner model - namely the idea that wages from work should provide an adequate standard of living for a household. Instead of being mandated, the modern living wage is promoted as a voluntary way for employers to improve financial outcomes for workers. Unlike low-skilled work of the early 20th century, many of today's low-paid workers do not produce items for which the marginal product of labour is readily observable. This fact, coupled with imperfect information and the relatively low bargaining power of low-skill workers may mean that many workers have earnings below their marginal product. For employers who are uncertain of a fair starting wage, the published living wage serves as an ethical starting point (Brown, Newman, & Blair, 2014). By focusing on a household's ability to cover basic expenses and allow modest participation in customary social activities, the living wage movement aims to provide a benchmark for a socially responsible wage.

The modern incarnation of the family wage began in 2012, when the Living Wage Campaign commissioned Peter King and Charles Waldegrave of the Family Centre Social Policy Research

Unit to investigate and determine a single hourly rate that would serve as the New Zealand Living Wage. King and Waldegrave based their analysis on the following definition of a living wage:

> *A living wage is the income necessary to provide workers and their families with the basic necessities of life. A living wage will enable workers to live with dignity and to participate as active citizens in society. (King & Waldegrave, 2012)*

This definition of the living wage contains two components. First, a living wage should provide for basic necessities such as nutritious food, adequate housing, clothing, heat, transportation, and childcare. Second, it should provide enough income that people are able to participate actively in the social and cultural activities of society such as taking modest vacations, making school donations, being financially prepared for emergencies and saving for retirement.

There is a high degree of similarity between the definition of the living wage and the concept of poverty. There are two widely used definitions of poverty, and the living wage definition encompasses both. Absolute poverty refers to the inability of an individual to obtain basic goods necessary for survival such as food, shelter and clothing, while relative poverty addresses the resources necessary to allow for participation in the broader society (UNESCO, 2017). Relative poverty measures were popularized by British sociologist Peter Townsend, whose definition of poverty shares many similarities with the New Zealand definition of the living wage:

> *Individuals, families and groups in the population can be said to be in poverty when they lack the resources to obtain the type of diet, participate in the activities and have the living conditions and amenities which are customary, or at least widely encouraged, or approved, in the societies to which they belong. (Townsend, 1979)*

Comparing the definition of the living wage to Townsend's definition of poverty, it is apparent that each focuses on having adequate resources to partake in society. As such, one goal of the living wage is to reduce the prevalence of relative poverty.

Earned income is only one factor contributing to the resources available to a household. While increasing wages for low skilled workers is critical, alone this would be insufficient to erase poverty (St. John & So, 2017). By focusing only on a prescribed level of earned income, proponents of the living wage are oversimplifying the relationship between earned income and poverty. A household's level of savings or debt, access to family or social support and responsibility for care of other family members can influence whether a given level of income is sufficient (Perry, 2017). Because of the complex relationship between income level and income

adequacy, research shows only limited overlap between measures of low income and measures of material deprivation (Carter & Gunasekara, 2012). Thus, while the living wage seeks to increase earned income, this is but one of the many factors that influence a household's ability to participate in the customary activities of society. Comprehensive efforts at poverty reduction must also focus on ways to build workers capacity through education and sustainable economic growth, policies to encourage labour force participation, development of a strong social safety net as well as policies to increase current levels of earned income.

As was true of the original Breadwinner model, the living wage is insufficient to provide an adequate standard of living for many households. The living wage has limited potential as a poverty reduction tool specifically for families, sole parents and those living in high-rent areas such as Auckland. For these households, a living wage pay check will not be sufficient to cover the costs of a socially integrated lifestyle (Perry, 2019). Families and sole parents face high marginal tax rates resulting from the decline in benefits that occur as income increases (Boston & Chapple, 2014). In fact, while these households will experience an increase in net earnings if their incomes rise, much of the financial benefit will be reaped by the government. As such, the living wage acts as tax on labour that is paid by the employer. Despite this apparent shortcoming, there may be a social benefit to this 'silent tax' that will reduce the burden placed on the public safety net by low wage employment.

Similarly, the poverty reduction potential of the living wage is limited by its voluntary nature. However, raising the minimum wage to the level of the living wage would place New Zealand's minimum wage at the highest level within the OECD (Galt & Palmer, 2013), a move that would likely have negative effects on the labour market. Although much of the minimum wage research has found little job loss following modest minimum wage increases, the magnitude of this increase would lie outside the realm of what is usually studied. This is of particular concern because unemployment dramatically increases the likelihood of poverty among low income families (Whiteford & Adema, 2007). In New Zealand, poverty rates decline sharply when even one adult holds full time employment (Perry, 2019). This decline is seen across a range of poverty measures. Because of the negative relationship between work and poverty, policymakers should strive to encourage and maintain employment, and care should be taken when considering large increases

to the minimum wage. Allowing employers to self-select their living wage status provides an incentive to raise wages while not forcing wage increases across the board.

The living wage is a useful tool that will increase earnings for a segment of low-income workers, while simultaneously reducing the financial burden placed on government services by those in low wage employment. However, living wages are only one piece in the complex puzzle of poverty alleviation. Care should be taken to remember that living wage does *not* represent the true required cost of social participation for most New Zealand households and that many of the more vulnerable households have needs exceeding the income provided by the living wage. That said, all household types will be made better off when an earner is raised from the minimum wage to the living wage (Galt & Palmer, 2013) and this should be considered consistent with the goals of the movement, even it is an imperfect poverty reduction tool.

3.4 CHOOSING A TARGET HOUSEHOLD

When seeking to establish a single living wage rate for New Zealand, King and Waldegrave began by selecting a target household type that the wage should support. They selected a family size of two adults with two dependent children as their target household for the living wage. The reason for this is twofold. First, there is a precedent for using this family type when estimating living wages;[11] second, it is the minimum family size that allows a population to replace itself (King & Waldegrave,2012; King & Waldegrave, 2014). This choice of family type has been criticised for its lack of applicability in New Zealand, as it represents only a small proportion of low earners (Galt & Palmer, 2013). Relatively speaking, couples with two children make up only a small proportion of New Zealand households. The majority of New Zealand households (57%) are composed of one or two usual residents and only 10.35% of households in New Zealand are composed of a couple residing with two children (Statistics New Zealand, 2014). Thus, the living wage is not designed to fit the experience of the majority of New Zealanders. That said, choosing to set the living wage at a level sufficient to provide for a family with children acknowledges the importance of providing for children, even if the majority of households do not contain them.

[11] In the US, living wages are generally based on a family size of two adults and two children (AFL-CIO, 2000); in Canada, living wage rates are based on needs of a couple with two children, ages four and seven (Living Wage Canada).

Poverty, as measured by income levels and experiences of deprivation, is higher for children than for the New Zealand population as a whole and the income-tested poverty rate for children has roughly doubled since the 1980s (Perry, 2017).

In Canada, where the living wage is also defined for a family of two adults and two children, proponents of the living wage argue that by providing modestly for families with children, the living wage supports people in all phases of life. The living wage would "also support a family throughout the life cycle so that young adults are not discouraged from having children and older workers can have some extra income as they age" (Living Wage Canada). So, while the living wage would offer the opportunity of subsistence and modest social participation to a target family of two adults and two children, other household types may also fare better on a living wage income and this is best seen as a secondary goal of the movement rather than as a shortcoming.

The second decision that King and Waldegrave made about the target household is that each adult would be required to work. They envisioned one adult working full time and the other working half time for a total of 1.5 full time incomes, or 60 hours of work per week. King and Waldegrave chose this figure because they felt that it best reflected the working habits of their target household. They report that the 2012 Household Labour Force Survey found that 68.5% of two adult/two child households rely on two incomes (King & Waldegrave, 2012). While true, this figure is somewhat misleading and may understate the working hours of two parent households. The 2013 Census data indicate that 40.3% of partnered mothers with children are employed full time and 27.9% are employed part time (Statistics New Zealand, 2014). This suggests that 1.75 full time incomes might have been a better reflection of the labour force activities of New Zealand families. However, this criticism is also short-sighted. As families work more hours, the number of hours of required childcare increases and the effect on the final hourly wage is unclear. Additionally, the 1.5 income requirement already disadvantages single parent households so this might be a reasonable compromise.

Finally, instead of establishing regional living wage rates, King and Waldegrave established a single rate for the entire country. This is different than the approach taken in the United States, the United Kingdom or Canada and represents the largest shortcoming in the New Zealand living wage measure. The most significant cost that varies by region is that of housing (King & Waldegrave, 2012). Regional variations in the cost of housing are substantial; the mean cost of rent in Auckland

is roughly $200 per week greater than that in Southland, and the national average rent lies right in the middle (Statistics New Zealand, 2013). This means that the annual income required to provide a living wage could vary by over $10,000 based on place of residence. This is important to our target household for three main reasons. First, housing costs are rising faster than incomes: the proportion of total income consumed by housing costs for the lowest quintile of earners has increased from 29% to 54% since the 1990s (Perry, 2017). Second, rising housing costs are particularly problematic for low- and middle-income households whose ability to purchase basic necessities such as food, clothing and transportation can be forced to take a back seat to rent (Perry, 2017). Third, a household earning the living wage will have income above the cut-off required to receive the Accommodation Supplement and will thus need to shoulder the entire cost of housing (King & Waldegrave, 2012). Taken together, these three factors show that accounting for cost of living is particularly important in the calculation of the living wage. Households living in above-average cost rental markets will need to compensate for their high rent by cutting costs in other areas and this is not reflected in the calculation of the living wage.

3.5 Determining The Hourly Rate

After deciding on the target-household for the living wage, King and Waldegrave estimated the necessary level of net household income. They then accounted for taxes, tax credits, accommodation supplements and child care subsidies to back-out the level of gross household income that was necessary to yield an after-tax living wage income. Identifying the required level of household net income was done by conducting focus groups, investigating household expenditures for lower-decile households and estimating the actual cost of a core basket of goods including food, rent and childcare.

Table 3.1 outlines the expenditure categories and corresponding weekly dollar allowances that went into the final calculation of the living wage in 2012, as well as the updated values used in 2018.[12] Between 2012 and 2017, the living wage was adjusted annually to keep pace with overall increases in wages but not for changes in the cost of living. In 2018, the living wage was reviewed for the first time to account for changes in cost of living, availability of new data and the

[12] The living wage underwent its first major five-yearly review in 2018. The updated methodology is discussed at the end of this section.

introduction of the Labour-led coalition government's Family Package of tax credits and supplements.

To determine the cost of living estimates, researchers at the Family Centre Social Policy Research Unit (FCSPRU) used a mixture of needs-based totals and actual expenditures made by low income households. Most line-item costs were estimated using the actual average weekly expenditures of households with two adults and two children in income deciles 1-5 as estimated by the Household Expenditure Survey (HES). Estimates of actual cost are used wherever available data permits. In 2012, actual cost estimates were obtained for food, rent and childcare. In 2018, this list was expanded to include household energy, health, communication and education.

Table 3.1| Weekly expenditure categories in the hourly living wage estimate

Expenditure Categories from the Household Economic Survey for a Couple with Two Dependent Children	Weekly Expenditure Figures	
	2012	2018
Food	226*	212*
Clothing and footwear	18	48
Rental housing	275*	332*
Household energy	46	72*
Household contents and services	33	39
Health	14	23*
Transport	121	132
Communication	29	31*
Recreation and culture	78	92
Education	37	45*
Miscellaneous goods and services	64	72
Other expenditures	66	70
Childcare**	31*	-
Weekly total	1038	1,169
Annual total	53,976	60,784
Total gross income necessary	57,432	64,059
Hourly rate (60 hours per week)	$18.41	$20.53
* Based on estimates of actual costs. Other cost estimates drawn from the average weekly expenditures of decile 1-5 income earners in the Household Expenditure Survey.		
Food costs from the Otago nutrition survey; rent estimates from the MBIE rent bond database; energy estimates from the fuel poverty study; health costs estimated from Ministry of Health and Pharmac data; transportation cost estimates from Ministry of Transport; communication costs based on actual costs of cellular and broadband coverage; education and childcare costs from NZCER and ECE surveys.		
** Costs of childcare and education are combined in the 2018 estimates. The cost breakdown is $29.80 for childcare and $15 for primary school expenses.		

Source: King & Waldegrave, 2012; Waldegrave, King, & Urbanova, 2018

Data for actual costs of necessary items are drawn from a number of sources, which are briefly described below.[13]

- Weekly food cost estimates are obtained from the University of Otago's *Food Cost Survey*, which estimates the cost of purchasing a nutritionally complete market basket of food for individuals of different ages and genders.[14]
- Rent estimates are derived from the Tenancy Bond Database maintained by the Ministry of Business, Innovation and Employment. This database maintains records of tenant bonds lodged with private landlords at the national, regional, territorial authority and suburb level (Ministry of Business, Innovation & Employment, n.d.). The rent figure used in the calculation of the living wage is the highest rent amount in the lowest quartile of national rents for three-bedroom houses, the minimum house size that allows a family of four to live without overcrowding (King & Waldegrave, 2012).
- The weekly childcare allowance is based on 30 hours per week at the actual cost of subsidized childcare. Low income families receive 20 hours of free childcare per week, the remaining 10 hours would be paid by the family at the subsidized rate (King & Waldegrave, 2012; Waldegrave, King, & Urbanova, 2018). In 2018, the costs of childcare and education have been combined into a single education category.
- Household energy cost estimates are derived from estimates of the actual number of kilowatt hours of energy needed to heat a three-bedroom house throughout the year, purchased at current market rates.[15] The living wage dollar estimate is a weighted-average of the estimated cost for four regions: Auckland, Wellington, Canterbury and Otago (Waldegrave, King, & Urbanova, 2018).

[13] For a full discussion of the determination of needs-based cost estimates, see Waldegrave, King & Urbanova (2018).
[14] For more information on the food cost survey, see http://www.otago.ac.nz/humannutrition/research/food-cost-survey/otago057919.html
[15] The living wage estimate for necessary energy expenditures of $72 per week is 15% greater than the actual expenditure on household energy reported in the HES of $62.30. This difference is likely due to a combination of factors including fuel poverty and low standards of heating and insulation in rental accommodation. For a full discussion of fuel poverty in New Zealand, see Howden-Chapman, et al. (2012).

- Health costs are calculated using Ministry of Health and PHARMAC data to calculate the actual cost of doctors' visits and prescription medications based on average national actual usage (Waldegrave, King, & Urbanova, 2018).
- Communication estimates include broadband internet coverage for the household and basic cellular plans for the two adults. The estimates were generated as the average cost of basic plans from New Zealand's major providers (Waldegrave, King, & Urbanova, 2018).
- Education costs are calculated as the median level of weekly expenditure on primary school students from the New Zealand Council for Education Research (NZCER) survey in 2007. These values were then updated for inflation based on changes in reported spending on the HES (Waldegrave, King, & Urbanova, 2018). The weekly estimate for spending on a primary school child is estimated to be $15 in 2018 which is added to an estimated $29.80 per week in childcare costs to come up with the $45 total estimate for education.

The researchers then used the cost of living estimates to calculate the necessary level of after-tax income to support a family of four. In 2018, that amount was calculated at $60,784, an increase of 12% over the 2012 value of $53,976. To determine the annual gross income equivalent, the researchers accounted for taxes and welfare transfers such as the accommodation supplement and tax credits. In 2012, this resulted in a necessary annual gross income of $57,432, the equivalent of $18.41 per hour. Between 2012 and 2017, the hourly value of the living wage was adjusted annually to reflect market movements in wages. During this time the living wage rate increased from $18.40 to $20.20 per hour, an average increase of 1.8% per year.

In 2017, the Labour-led coalition government enacted the Families Package, a system of tax credits and subsidies designed to alleviate child poverty. This package includes a subsidy for winter energy usage, a tax credit for the birth of new children and changes to the accommodation supplement (Ministry of Social Development, 2017). The effect of this package on the requisite gross level of household income underscores the importance of government transfer payments on the take-home pay of households. The enactment of the Families Package served to significantly offset the upward pressure on the hourly living wage. Without the introduction of the Families Package, the living wage for 2018 would have been $22.45 per hour, which is nearly $2 per hour higher than the calculated rate of $20.50 (Waldegrave, King, & Urbanova, 2018). Furthermore, without the Families Package, the increase in the living wage would have been the largest single

increase since its inception, revealing that increases in cost of living for low-income families have been rising faster than the general growth in wages.

3.6 THE CERTIFICATION PROCESS

Living Wage Aotearoa began certifying living wage employers in 2014. Organisations interested in pursuing living wage certification must meet three criteria:

1. All direct employees and regular contractors must be paid the current living wage rate.
2. Hours or benefits must not be reduced by employers in an effort to offset higher wage costs.
3. Employees must be provided with access to a union.

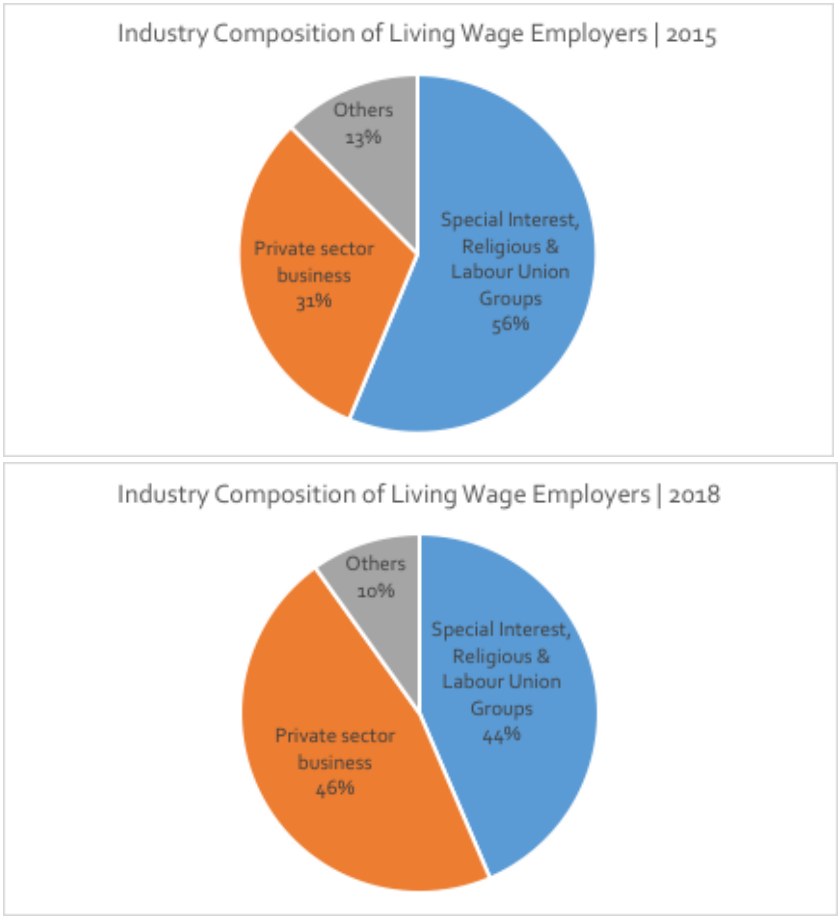
Firms wishing to become living wage certified must pay the current living wage to all direct employees as well as to regularly scheduled contractors whose work is central to the operation of the business. A contractor must be paid the living wage if they perform a regular service for the organisation that would need to be performed by an employee in the absence of the contractor. Cleaners are the most cited example but covered contractors also include gardeners or landscapers as well as contractors hired to dispose of confidential documents. Firms are prohibited from reducing hours of employment or fringe benefits at the *time of certification.* This prevents firms from cutting employee hours or eliminating paid time off in order to avoid increasing labour costs. This provision does not limit the firms' ability to restructure or re-negotiate labour contracts based on evolving business needs and legal requirements. In situations where organisations have existing contracts with workers who are paid less than the living wage, these firms may apply for certification as long as they agree to re-negotiate the contracts at the living wage rate when the contracts expire.

Once a firm meets all three criteria, it is invited to apply for certification. The fee for application varies by firm size/type and ranges from $100 per year for a small (fewer than five employees) charity or community organisation to $2,500 per year for a private or government sector firm with more than 500 employees (Living Wage Aotearoa New Zealand, n.d.). The application process takes five weeks to complete on average. Certification is good for one year, and certified living wage firms are able to use the Living Wage Employer Mark as part of their promotional materials (Living Wage Aotearoa New Zealand, n.d.).

3.7 New Zealand Living Wage Firms: Information from Public Sources[16]

Since Living Wage Aotearoa began certifying firms in 2014, living wage employers have grown in both number and scope. In the first two years of certification, 48 organisations became living wage certified and the industry composition of these employers reflected the grassroots nature of the living wage movement. According to publicly available data, 56% of employers certified in 2015 were special interest groups, labour unions or religious groups and only 31% were private sector business (Figure 3.1).

Figure 3.1 | Industry composition of living wage employers, 2015 - 2018



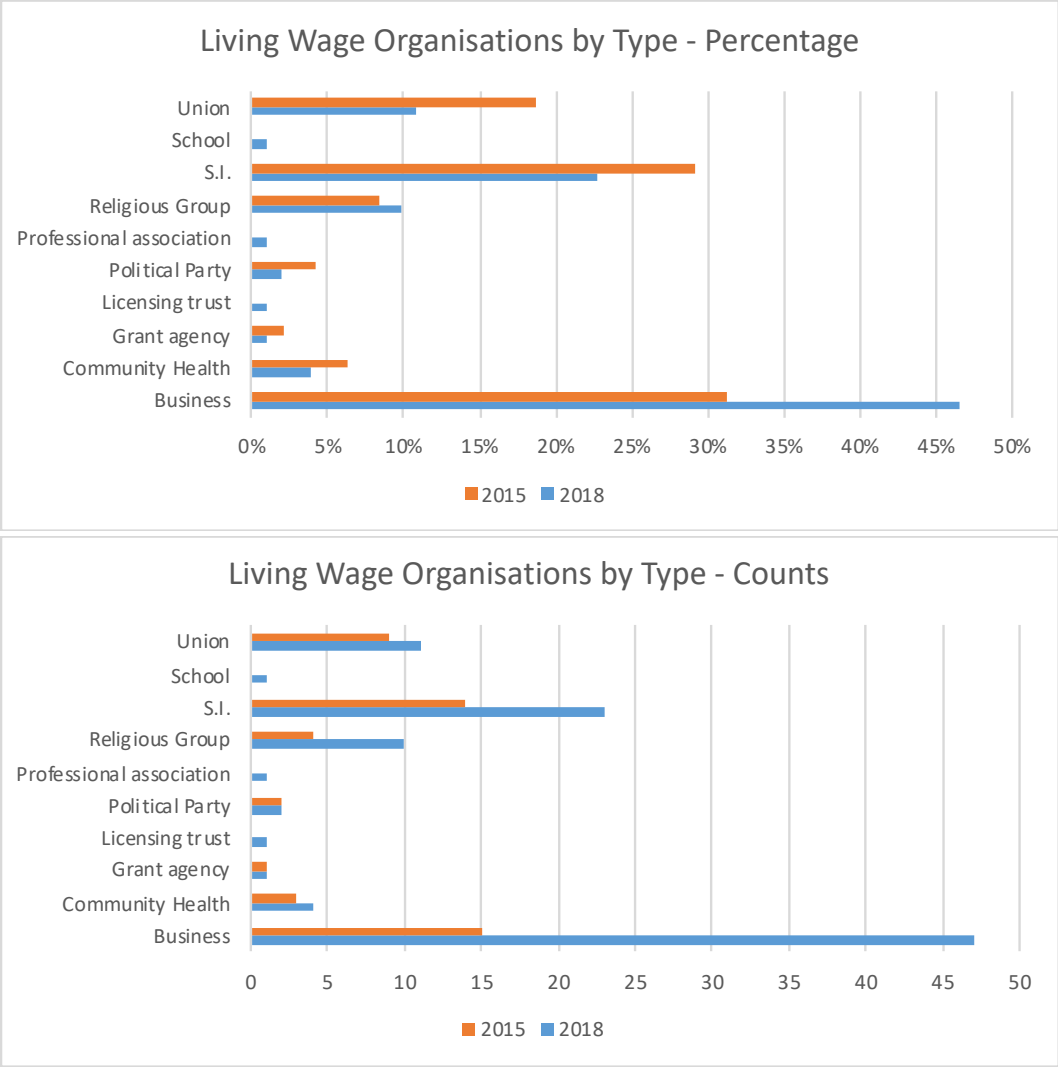
Source: Author's calculations; publicly available data

By 2018, the living wage movement had expanded and diversified. As of July 2018, the number of certified employers had grown to 102 and much of that growth was fuelled by adoption of the

[16] Information in this section is derived using publicly available information and personal conversations with employees and owners of living wage firms.

living wage by private sector firms. Between 2015 and 2018, the number of private sector living wage firms grew from 15 to 47, making private businesses the largest single category of living wage employer (Figure 3.2).

Figure 3.2 | Living wage organisations by type



Source: Author's calculations; S.I. stands for 'special interest group'; information from publicly available sources.

Certified living wage businesses come from a wide array of industries and the vast majority are successful firms. Perhaps surprisingly, a majority of these firms operate in traditionally low paying or slow growing industries, suggesting that there are profitable niches within typically competitive sectors. Of the 49 private sector businesses that are currently certified living wage employers, 13% are food service establishments and 8% are retail firms. Together, these two industry groups have

the highest proportion of below living wage employees in New Zealand: nationwide, only 10% of employees in accommodation and food service and 32% of retail workers earn more than the living wage (Galt & Palmer, 2013). Similarly, 35% of living wage firms are manufacturers, an industrial classification that experienced a reduction in total number of jobs between 1989 and 2012 (Galt & Palmer, 2013). The adoption of the living wage by firms in these tight industries reflects the fact that there are market niches within broadly defined industry categories that remain profitable even when the broader industry is stagnant or highly competitive. For profitable firms in these industries, prevailing market wages may be a poor reflection of the economic value generated by workers. Therefore, the living wage may offer an opportunity for socially minded employers to share profit with employees.

A large percentage of living wage firms also come from industries that employ relatively low numbers of low-wage workers. The number of workers earning more than the living wage is relatively high in both the professional, scientific and technical services industry and in the construction industry, with at least half of workers earning above the living wage (56% and 50% respectively) (Galt & Palmer, 2013). Together, these industries make up 25% of certified living wage businesses in 2018, up from 13% in 2015. While relatively high-paying firms will have smaller proportions of low-wage workers and will experience smaller direct cost increases associated with the living wage, these firms may still experience increases in operational costs through increases in contract costs. Firms who contract for services such as cleaning, janitorial, landscaping or document destruction will need to ensure that all contract employees are receiving the living wage as part of the certification requirements. The impact of increases in these costs, while potentially significant, cannot be measured with my data.

The majority of living wage employers are successful entities and most choose to renew their certification after the first year. Of the 48 firms that were certified living wage employers at the end of 2015, 46 of these firms are still operational.[17] Of these 46 firms, 40 have chosen to remain certified as living wage employers. The six that have not renewed their certification have done so for a variety of reasons, although at least two of these organisations continue to pay living wage rates to all employees. Personal conversations with three of these organisations revealed that,

[17] One firm has ceased operations. Another firm underwent a merger to form a new organisation that is living wage certified.

although not still certified, the organisations still abide by the rules and principles of certification and employees at most of these organisations continue to view their employers as living wage firms despite the lack of formal certification. A fourth firm continues to believe in the principles of the living wage movement but has found it difficult to pass higher costs along to customers, an important reminder that customer support is crucial for the success of market-based living wage firms.

3.8 WHY PAY THE LIVING WAGE?

Why would organisations voluntarily pay above market wages for low skilled labour? For the majority of living wage employers, this seemingly complex question has a surprisingly simple answer: Values and Strategy. Employers who pursue living wage certification primarily do so because it is consistent with their organization's mission or market position. Strategies based on values of equity and sustainability find support among a wide variety of New Zealand stakeholders. The New Zealand living wage has gained traction because it receives support from a variety of key groups including unions, non-profit organizations, religious groups, entrepreneurs, consumers and corporations. Members of these groups who embrace Living Wage Aotearoa do so because it reflects their values.

For founding groups of the living wage movement such as unions, special interest, and faith-based groups, paying the living wage and promoting its strengths is part of their organizational objective. Unions such as the New Zealand Public Service Association, NZEI Te Riu Roa, and FIRST Union make promoting and paying the living wage part of their platform to increase pay equity, reduce inequality and improve the living standards of New Zealanders. NZEI Te Riu Roa, the largest NZ education union, summarises this interplay between values and strategy with their slogan "living wage for learning" (NZEI Te Riu Roa, n.d.). The members of NZEI embrace the living wage because raising the earnings of low pay workers help promote learning by supporting educators and learners. By ensuring that typically low-paid staff such as instructional aides are sufficiently paid, NZEI hopes to ensure that these important positions provide sustainable wages; by encouraging the living wage in the broader community, NZEI hopes that more children will grow up in families that have the resources to nourish their minds and bodies (NZEI Te Riu Roa, n.d.). These groups pay the living wage because it is a core component of their organizations mission.

Market-based firms that chose living wage certification typically do so as part of a strategic commitment to corporate social responsibility (CSR). While there is no single accepted definition of corporate social responsibility, it is based on the belief that firms have a responsibility to uphold and promote the values of society. This responsibility includes a duty of care to environmental or social causes in addition to the firms economic and legal responsibilities (Davis, 1973). While some scholars of CSR emphasise that socially responsible actions incur costs for which the firm cannot expect to receive a return (Walton, 1967), others believe that CSR can form the core of a competitive corporate growth strategy (Porter & Kramer, 2011). For-profit companies choosing living wage certification do so because they believe that firms have a responsibility to contribute to the quality of life of New Zealanders.

Of the for-profit firms holding living wage certification, many are small entities with values-driven strategies based on sustainability. While some of these firms operate in business niches that will reward them financially for their efforts, others will find ways to absorb the costs of higher wages through reductions in profits or increases in prices. For these firms, paying the living wage is a manifestation of their mission. One company exemplifying this idea is the beauty bar company Ethique, whose natural products are packaged completely without the use of plastic. Their business strategy is based on creating products that benefit the environment, the workers and the company. Produced without animal testing and using living wage labour, Ethique's products have been developed for a market niche that is based on the principle of turning good corporate citizenship into a viable business (Ethique, n.d.). Their customers chose them for both their products and their ethos. Another example of a company whose corporate strategy is based on the ideas of sustainability and corporate social responsibility is Good Fortune Coffee Company. As a certified living wage and fair-trade coffee roaster, Good Fortune has positioned itself as an organisation dedicated to the care for all the workers whose labour helps produce their coffee. From the farmers who grow the beans, to the roasters in Petone, Good Fortune aims to provide fair wages for all workers in their supply chain (Good Fortune Coffee Company, n.d.). For firms that embrace a values-driven corporate strategy, the living wage simply represents responsible business practice.

Large corporations pursuing living wage certification share a commitment to socially responsible ideals such as pay equity and reducing inequality but they are less likely to have launched their business on a platform of social responsibility. Examples of large corporate entities that have

announced their support for the living wage include Auckland-based Vector Energy, Westpac Bank and Countdown Market. Each of these companies shares a commitment to corporate social responsibility. Vector Energy, the first large living wage corporation in New Zealand, expresses this as an affirmation of their commitment to pay equity and to reducing income inequality in New Zealand (Vector, 2017). Australia-based Westpac Bank embraces the living wage as part of its larger strategy to support sustainability which also includes initiatives such as carbon-neutrality and the funding of loans for sustainability projects (Westpac, 2019). Countdown Market, one of New Zealand's largest food and grocery suppliers, agreed to pursue living wage certification this year after long negotiations with FIRST Union. On the subject of the decision, the company's general manager of operations, Brett Ashley, said "We're proud to be a good employer and ensuring our team can continue to grow their earning ability is a key part of this . . . while also balancing the realities of keeping and creating jobs, and keeping food prices affordable for New Zealanders" (Fyfe, 2019). While the definition of the living wage positions it as a tool to reduce poverty, certified employers often express its value in terms of 'equity,' 'equality' or 'sustainability' rather than poverty.

Values-based strategic management is viable today in New Zealand because it has sufficient consumer support. However, asymmetric information makes it difficult for consumers to individually judge the business practices of firms. Because ethical practices are easy to claim but difficult to prove, a number of third-party certification systems have arisen to help evaluate and certify corporate behaviour. Recent research has highlighted the fact that consumers increasingly search for ethically produced options when selecting among products (Freestone & McGoldrick, 2008) and growing evidence indicates that consumers are willing to pay a premium for products they believe to be ethically produced (Nilsen, 2015). Third party certification serves as an important signal to consumers driven to shop in line with their conscience and the inclusion of a reputable third-party certification may allow firms to charge higher prices while simultaneously increasing sales (Hainmueller, Hiscox, & Sequeira, 2015). For consumers concerned with the personal and social cost of low wage employment, the living wage brandmark confirms that the employer is committed to ethical pay for low skilled workers. For firms concerned with promoting strong wages, choosing certification may help them pass some of the costs on to consumers in the form of higher prices.

3.9 LITERATURE REVIEW

During the 1990s and early 2000s, when the majority of living wage ordinances were enacted in the United States, the living wage received a fair amount of research attention. However, in the last decade American municipalities have shifted away from enacting living wage laws. Instead, the policy focus for local governments has been on city and county minimum wages; thus, research attention has shifted accordingly. As a result, there has been very little research on the living wage since 2005. This is unfortunate because the living wage movements in both the UK and New Zealand have been expanding and the availability of Linked Employer Employee Data (LEED) has been improving. In New Zealand, where the living wage is relatively new and increasing in popularity, the use of the Integrated Data Infrastructure (IDI) to investigate changes in earnings, turnover and labour-labour substitution is both relevant and timely.

The main goal of living wage programs is to increase earnings and reduce poverty among low wage workers (Adams & Neumark, 2004). In the United States, two veins of literature have developed to evaluate the degree to which the living wage has met this goal. The first vein, exemplified by Adams and Neumark (2003) uses large, publicly available datasets to estimate the impact on wages, employment and poverty levels for low-wage workers in cities with living wage ordinances, while the second vein (see Brenner, 2005; Fairris, 2005 and Reich et al., 2005) uses survey data to investigate the micro-economic responses of firms and workers who were directly affected by the living wage. Outside the United States, research on the living wage has been limited. I discuss the existing research here.

Adams and Neumark (2003, 2004, 2005) and Neumark et al. (2012) investigate the policy success of American living wage ordinances (LWOs) using the Current Population Survey, a large publicly available dataset. In the United States, living wage ordinances are mandated programs affecting firms operating within the jurisdictions of the cities or counties that enact them. By 2003, when much of the research on living wages was being conducted, more than 115 local governments had passed some sort of living wage ordinance (Brenner, 2005). LWOs require firms that have contracts with the city (‘contractor only laws’) or firms that otherwise receive financial assistance from local government or lease government land (‘business assistance laws’) to pay all workers at least the legislated living wage. Living wages vary by jurisdiction, but lie between the minimum wage and the median market wage for the area.

In the US, living wages—like minimum wages—are involuntary wage floors that act like a tax on low-wage labour and may therefore result in a loss of jobs. The Adams and Neumark papers attempt to estimate net benefit/loss of living wage laws on wages, employment and poverty for low-wage workers and low-income families. During the early 2000s, they published a series of papers using fixed-effects regression models to compare cities that passed living wage ordinances to those that did not. Each of their papers followed the same methodology but expanded on previous efforts to utilize newly available data and respond to criticisms. They focused on the lowest decile of earners as well as on earners between the 10th and 50th percentile to try to uncover nuances of impact. Their results suggest that living wage laws increase wages for a subset of low-wage earners while also resulting in job loss. These findings are robust across all of their studies, as is the finding that the net effect of living wage laws is to reduce poverty for a subset of low-income families (Neumark, Thompson, & Koyle, 2012). However, their findings suggest that reductions in poverty do not accrue to the most vulnerable workers; rather the households most likely to be lifted out of poverty are those containing workers above the 10th percentile of earners (Adams & Neumark, 2003).

Therefore, from a policy perspective, living wages are an effective—albeit rather blunt—tool for addressing urban poverty (Holzer, 2008). Given the fact that public mandates to raise wage floors present a trade-off between earnings and employment, such tools must be used judiciously. The findings that the living wage has a net-benefit needs to be reconciled with the authors' previous and subsequent findings that other wage floors, namely the minimum wage, do more harm than good (Neumark, Schweitzer, & Wascher, 2004). More effort needs to be expended to understand why some wage floors have net benefits while others result in net losses.

The Current Population Survey (CPS), used by Adams, Neumark and others, is a good data source for investigating the net-effects of living wage policy but it is not well suited to providing information on micro-level responses by firms or workers. Several studies in the early 2000s used survey data to explore firm responses and employee outcomes following living wage certification. While informative, there are a number of problems with data quality or research design that limit the generalizability of these studies. Nevertheless, a few recurring themes emerge from this research that are useful indicators of likely outcomes from living wage policies. First, nearly all studies find that turnover falls in affected firms following the passage of a living wage ordinance

(see Fairris, 2005 and Reich et al., 2005). Second, some authors have found that firms are able to recruit more qualified or better educated workers to fill vacancies after a living wage ordinance (see Fairris & Bujanda, 2008; Reich et al., 2005; Wills & Linneker, 2012). However, despite these two benefits, there is some evidence of disemployment following living wage ordinances as firms either reduce hours (Wills & Linneker, 2012) or cut over-time work (Fairris, 2005) in an attempt to control costs. This paper estimates the effect of the living wage on the earnings, turnover and employment in certified firms. The issue of substitution toward more qualified or better educated workers is addressed in Chapter 4 of this thesis.

Intuitively, wages and turnover are likely to be inversely related. However, research has uncovered a more complicated relationship between wages and turnover than one might expect. While a substantial body of literature has found that turnover is lower in firms with higher relative wages (see Burgess, Lane, & Stevens, 2000; Dale-Olsen, 2006 and Khatri, Fern, & Pudhwar, 2001), it is also clear that pay is unlikely to be the strongest determinant of voluntary employee turnover. Instead, a number of non-pay job attributes such as opportunity for promotion, work group cohesion and perceived alternative employment opportunities may be more important than pay in determining voluntary turnover (Griffeth, Hom, & Gaertner, 2000).

The living wage is specifically designed to affect worker pay and because of this, we might expect to see a decline in turnover. However, the living wage might reduce turnover through a secondary channel as well. A relatively high level of pay may reduce the value of perceived alternative employment opportunities for workers. When workers believe that they face strong employment prospects, they are more likely to search for other jobs, express turnover intent and leave their current position (Griffeth, Hom, & Gaertner, 2000). If the living wage is successful at raising the income of affected workers or if it is successful at changing the perception of relative wages for these workers, turnover intention may fall.

It is therefore not surprising that many inquiries into the living wage have focused on the impact of higher wages on turnover. Brenner (2005), Reich et al. (2005), Howes (2005), Fairris (2005) and Wills and Linneker (2012) have all investigated the relationship between living wages and employee turnover. With the exception of Brenner (2005), each study found lower turnover associated with the living wage. However, each of these studies use different definitions of turnover and small sample sizes limit the ability of many studies to control for key factors such as

demographics, worker tenure, firm size or industry. None of the studies employ a control group of comparable unaffected firms. Additionally, because turnover can be driven by forces relating to firm strategy or job-worker fit, a clear conceptualization of turnover is necessary to understand the specific mechanisms by which the living wage impacts turnover. For these reasons, existing research on turnover in living wage firms is best thought of as exploratory. However, there is a robust literature on turnover outside of the living wage. Pioneered by Davis and Haltiwanger (1999) and expanded upon by Burgess, Lane and Stevens (2000), the literature on turnover focuses on the flow of both jobs and workers. Separating turnover into firm-driven and employee-driven components is important for understanding the impact of living wage on turnover. To my knowledge, no studies have attempted to apply the established definitions of turnover to the living wage question.

The reduction in turnover points to the possibility that firms are able to offset some of the higher labour costs through improvements in productivity. This does not imply that the living wage 'pays for itself;' instead there is evidence that employers may reduce employee hours (Wills & Linneker, 2012) or opportunities for overtime (Fairris, 2005). Given the fact that wage floors disincentivize the use of low-skill labour, it is not surprising that studies in the United States find disemployment following living wage ordinances. However, the fact that employee hours fell in London where the living wage is voluntary is more curious. The fact that London living wage employers select into the program does not prevent them from seeking ways to ameliorate the higher costs (Wills & Linneker, 2012).

Research on the New Zealand living wage is slim, with only two qualitative papers released to date. In 2014, Brown, Newman and Blair conducted qualitative interviews with four living wage employers and five living wage employees to gather information on the experiences of living wage firms and workers. In particular, the authors wanted to uncover the motivation behind employers' decisions to pay the living wage and to identify specific benefits accruing to employees who earn the living wage. Responses were collected and analysed using thematic analysis.[18] Although the

[18] Thematic analysis is a method of analysing qualitative data to uncover recurrent central themes. For more information, visit https://www.psych.auckland.ac.nz/en/about/our-research/research-groups/thematic-analysis/about-thematic-analysis.html

sample size was small, an effort was made to ensure that a variety of industries and backgrounds were represented.

Interviews with employers revealed that the decision to pay the living wage was motivated primarily by ethics but that employers also believed in a business case for the living wage. Employers believed that paying the living wage was the morally right thing to do and expressed their motivation in terms of investing both in people and in society. Choosing to pay the living wage was both an investment in the well-being of employees and a statement of social justice. Firms expressed the idea that that the employer/employee relationship was that of reciprocal investment: firms wanted employees that were willing to invest in the workplace and employers believed that paying the living wage would encourage that. Some firms reported greater efficiency or commitment among their workers following the implementation of the living wage. One employer noted that the increase in costs had been less than expected and attributed the difference to gains in efficiency.

For workers, earning the living wage increased their motivation at work and improved job satisfaction. The additional income associated with earning a living wage was credited with reducing life-stress and allowing workers to participate in social and cultural activities. Employees reported feeling not only more valued and accountable but also more pressured. Having the extra income was credited with taking pressure off in other areas of life so workers were happy to work harder and invest more of themselves at work. Although the increase in income was modest, employees agree that it made a difference in family finances by allowing people to make small contributions to savings, pay for unexpected expenses or help others. One worker who moved from minimum wage to the living wage said that the increase in income made the difference between living and ‘just existing’.

More recently, John Stansfield (2017) has conducted interviews with living wage employers and staff. In addition to reporting on perceptions of the living wage brand and on changes experienced in the lives of workers, the research asked employers to reflect on ways that living wage certification has impacted their organisation. One emerging theme was that living wage certification altered how firms approached remuneration. One employer noted that increased effort went into making rosters in an attempt to improve efficiency and reduce downtime. Another employer noted difficulties with maintaining and adjusting internal wage hierarchies following the

adoption of the living wage. A third employer noted that, as a certified living wage employer, they do not need to worry about finding a fair level of pay; by signing up for the living wage, they knew that they were paying generous, competitive wages.

Table 3.2 | List of living wage employers included in this study

Certified Living Wage Employers			
Employer	Dates Certified	Employer	Dates Certified
Auckland Women's Centre Incorporated	Feb 14 - Feb 16	Greenpeace	Jun 15 - May 16
FIRST Union	Feb 14 - Jan 15	Methodist City Action	Jun 15 - May 16
Friendship House	Feb 14 - Feb 16	Tommy & James Ltd	Jun 15 - May 16
NZEI	Feb 14 - Feb 16	Tuaropaki Trust	Jun 15 - May 16
Opticmix Limited	Feb 14 - Feb 16	Waikato Environment Centre	Jun 15 - May 16
Oxfam	Feb 14 - Feb 16	Yellow Vested Management Ltd	Jun 15 - May 16
PSA	Feb 14 - Feb 16	Auckland Unitarian Church	Aug 15 - Jul 16
SFWU	Feb 14 -Feb 16	Heathrose Research Ltd	Aug 15 - Jul 16
Teu	Feb 14 - Jan 15	Peace Movement Aotearoa	Aug 15 - Jul 16
Tonzu-Chalmers Organices Ltd	Feb 14 - Jan 16	WE'AR Righteous Ltd	Aug 15 - Jul 16
Vaka Tautua Ltd	Feb 14 - Jan 15	Community Networks Aotearoa	Sep 15 - Aug 16
Waitakere Union Health Centre	Feb 14 - Aug 16	La Boca Loca Ltd	Sep 15 - Aug 16
NZCTU	Feb 14 - Mar 16	Shield New Zealand Security Agent	Sep 15 - Aug 16
Young Workers Resource Centre	Jun 14 - May 16	Unite Union	Sep 15 - Aug 16
EcoMatters Environment Trust	Jun 14 - May 16	Common Unity Project Aotearoa	Oct 15 - Sep 16
Miguard Security Ltd	Jun 14 - May 16	Connecting Communities Wairarapa	Oct 15 - Sep 16
Anglican Diocese of Wellington	Mar 15 - Feb 16	Presland & Co	Oct 15 - Sep 16
NZMWU	Mar 15 - Feb 16	Newtown Union Health Service Ltd	Nov 15 - Oct 16
JR McKenzie	Apr 15 - Mar 16	Pivotal Thames	Nov 15 - Oct 16
Angel Food Ltd	May 15 - Apr 16	The Fresh Desk	Nov 15 - Oct 16
North Shore Women's Centre	May 15 - Apr 16	E tu Incorporated	Dec 15 - Nov 16
NZ Labour Party	Mar 15 - Feb 16	Hutt Union	Dec 15 - Nov 16
ChangeMaker Refugee Forum Incorporated	Jun 15 - May 16	Unreal Films Ltd	Dec 15 - Nov 16
Green Party Aotearoa NZ	Jun 15 - May 16	Women's Health Action Trust	Dec 15 - Nov 16

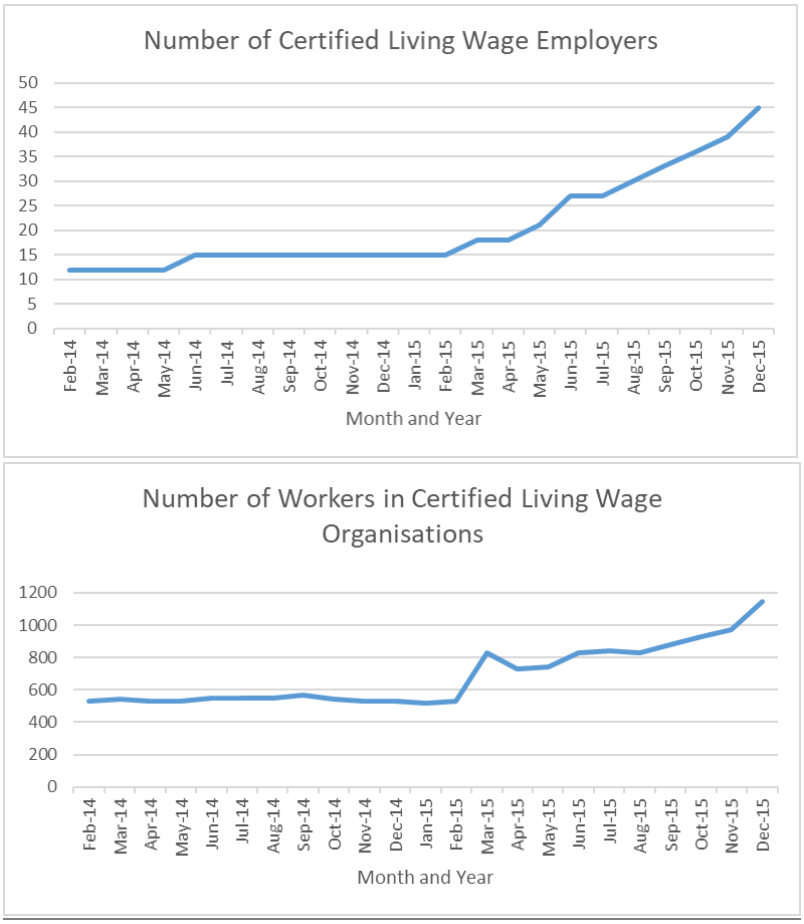
Source: Living Wage Aotearoa

This research provides the only insight into the New Zealand living wage experience. Its primary contribution is that it provides a qualitative look into the motivations and experiences of those affected by the living wage. The small sample size and lack of descriptive data in both studies makes it impossible to assess the degree to which the perspectives presented are generalizable to other living wage firms. It provides no concrete data on costs or turnover. Nonetheless, it gives us some perspective into the reasons that firms choose living wage certification as well as an insight into the experiences of the affected workers.

3.10 The Data: New Zealand's Integrated Data Infrastructure

My analysis utilizes the Integrated Data Infrastructure (IDI) maintained by Statistics New Zealand. The IDI is a broad-based, linked research database containing both administrative and survey data. The linked nature of the data allows researchers to access information on individuals and firms that has been collected by a variety of New Zealand agencies. When linked, data on individuals compiled by the Inland Revenue Department and data on firms from the Business Registry provide a rich source of employer-employee information with monthly records of wages and employment for all New Zealand firms and workers. This rich, longitudinal data set is compiled by Statistics New Zealand for use in creating official statistics and supporting approved research.

Figure 3.3 | Number of certified employers and workers

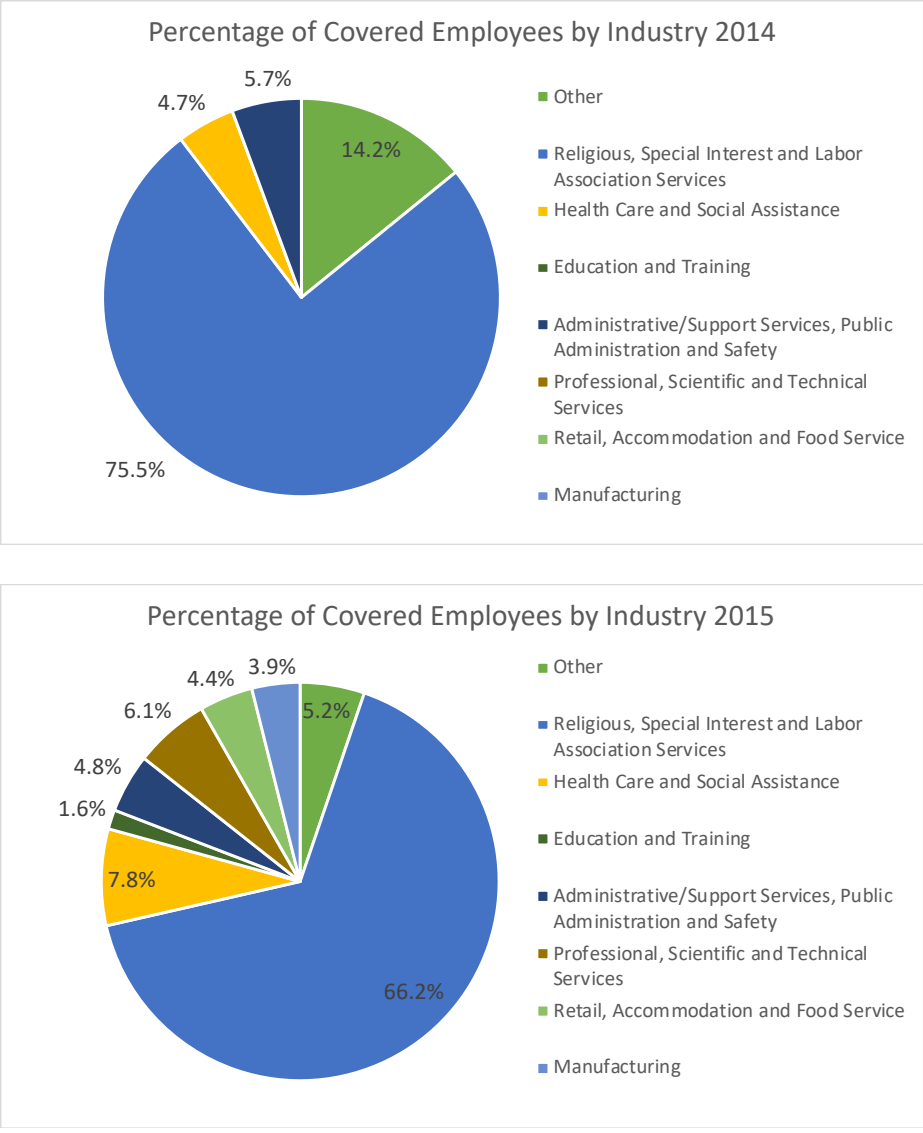


Source: Author's calculations; values rounded as required to preserve confidentiality; data from the IDI

I was aided in my work by Living Wage Aotearoa which provided certification dates for all firms that were certified before December 2015, as well as by Statistics New Zealand which was able to

identify and mark 45 of the 48[19] living wage firms in the data. Through an identifying 'living wage' marker, I was able to link data on employers to the earnings and demographic data of employees. This linking allowed me to utilize firm information such as industry, location and employee count along with employee wage and location information to investigate changes in earnings and turnover resulting from the living wage.

Figure 3.4 | Living wage employees by industry, 2014-2015



Source: Author's calculations; data from the IDI; counts rounded as required to protect confidentiality

[19] To protect privacy and confidentiality, Statistics New Zealand requires that all firm counts be randomly rounded to base 3. To comply with these requirements, all counts derived from data in the IDI will be rounded.

The IDI has advantages and disadvantages for investigating the effects of living wage certification. Among its main advantages is the fact that it offers longitudinal data on all employers and employees dating back to 1999. This is the first study to utilize panel data and a matched control group to look at firm and employee-level outcomes associated with the living wage. The key drawback of the IDI is that it lacks information on hourly wages or hours worked. The IDI contains information on gross monthly employee earnings, which reflects both wages and hours but it is difficult to disentangle the relative effects of these two forces. However, given that the purpose of the living wage is to increase earnings for low-wage workers and low-income families, this is not necessarily inappropriate.

3.11 LIVING WAGE EMPLOYERS: DATA FROM THE IDI[20]

Certification of living wage firms began in 2014. Since then, the number of certified firms has increased from 15 to 102, and the range of industries represented has expanded. This study looks at firms that were first certified between January 2014 and December 2015. During this time period, there was growth in both the number of certified organisations and the number of covered employees. In the first month of certification, 12 firms employing 530 workers were established as living wage employers. By the end of 2015, there were 48 firms that had registered as certified living wage employers and the number of covered employees had grown to 1,148 (Figure 3.3). The living wage firms that were certified at the time of this study are listed in Table 3.2.

When certification began in 2014, a majority of certified employers were from special interest, labour association or religious groups. This industry composition reflects the grassroots nature of the living wage movement. In the second year of certification, the industry make-up of firms became more diverse, with a growing number of firms coming from other industries. In 2015, the number of religious, community or labour union organisations registered as living wage employers doubled from 9 to 18 but the proportion of living wage employees working for such organisations fell from 76% to 66% (Figure 3.4). This indicates support for the living wage among a growing number of market-based firms, many of which come from traditionally low paying sectors such as

[20] In this section, I discuss data on Living Wage firms that was obtained through the IDI. The numbers will differ from that discussed earlier due to Statistics New Zealand's confidentiality requirements and to potential differences in industry assignment.

retail, manufacturing and hospitality (Table 3.3). Additionally, as time has progressed, newly certified firms also tend to be smaller. While the average number of workers in firms certified in 2014 was 37, firms certified in 2015 had, on average, 21 workers. Nearly half of living wage firms have 10 or fewer workers and 20% of living wage organisations employ fewer than three workers. This last fact is important as it illustrates that smaller businesses *are* pursuing certification, despite concerns that the living wage would be cost-prohibitive for smaller firms.

Table 3.3 | Living wage firms by industry and size, 2014-2015

Employee Count by Industry DIvision, December 2014			
Industry Division	***Underlying # of Firms (RR3)***	***Number of Employees****	***Percentage of Employees*** **
Other	3	75	14.2%
Religious, Special Interest and Labor Association Services	9	400	75.5%
Health Care and Social Assistance	3	25	4.7%
Education and Training	0	0	0.0%
Administrative/Support Services, Public Administration and Safety	3	30	5.7%
Professional, Scientific and Technical Services	0	0	0.0%
Retail, Accommodation and Food Service	0	0	0.0%
Manufacturing	0	0	0.0%
TOTAL	**18**	**530**	**100%**
*Graduated random rounding, Industries with fewer than 3 underlying firms are reported as having no participating firms in 2014. Employees of these industries are included in "other"			
** Based on graduated random rounded counts			

Employee Count by Industry DIvision, December 2015			
Industry Division	***Underlying # of Firms (RR3)***	***Number of Employees****	***Percentage of Employees*** **
Other	3	60	5.2%
Religious, Special Interest and Labor Association Services	18	760	66.2%
Health Care and Social Assistance	6	90	7.8%
Education and Training	3	18	1.6%
Administrative/Support Services, Public Administration and Safety	6	55	4.8%
Professional, Scientific and Technical Services	3	70	6.1%
Retail, Accommodation and Food Service	3	50	4.4%
Manufacturing	3	45	3.9%
TOTAL	**45**	**1148**	**100%**
*Graduated random rounding. Industries with fewer than 3 underlying firms are included in "other"			
** Based on graduated random rounded counts			

Source: Author's calculations; data from the IDI; counts rounded as required to protect confidentiality

Statistics on the age, size and persistence of living wage firms can be found in Table 3.4. The majority of living wage firms are well established entities. Two-thirds have been in business for at

least five years, and nearly half have been in business since the IDI was created in 1999. Despite this, the living wage also appeals to start-up ventures. Six living wage firms (13%) had been operational for less than three months at the time of certification and three of these firms first appeared in the IDI in the same month as they became certified. Additionally, living wage firms have similar success rates to the control firms in my sample: 87% of living wage firms and 89% of control firms remained in business through the last month of the study, although this is an imperfect measure of persistence because not all firms appear in the data every month.

Table 3.4 | Age and size characteristics of living wage firms

Age and Size Characteristics of Living Wage Firms		
Category	Percent	# of firms
fewer than 3	20.0%	9
4-10	26.7%	12
11-25	20.0%	9
26-50	13.3%	6
50-100	6.7%	3
more than 100	13.3%	6
First appearance in data:		
Category	Percent	# of firms
before 1999	46.7%	21
1999-2004	6.7%	3
2004-2009	20.0%	9
2009-2014	13.3%	6
after 2014	13.3%	6
Firm age at certification*:		
Category	Percent	# of firms
3 months or less	13%	6
3-18 months	7%	3
18 months - 5 years	13%	6
5-14 years	20%	9
more than 14 years	47%	21
Total number of fims: 45; Based on rounded counts		
*firms in existance before 1999 could be older than 14 years.		

Source: Author's calculations; numbers rounded where required to protect confidentiality

3.12 SAMPLE SELECTION

My sample consists of 36 living wage firms and 573 matched control firms. To be included in my sample, firms needed to have data in the IDI for at least 27 months stretching from 14 months pre-certification to 12 months post-certification. Additionally, I only include firms that employed workers whose monthly earnings were below the full-time living wage rate in the 10 months before likely compliance. The process and rationale for my sample selection are detailed below.

Although data in the IDI dates to 1999, most firms in my sample are newer than this, with roughly one-third being less than five years old. Additionally, because there are differences in size, average pay, and racial composition between young and old firms within my sample, analysis based on an unbalanced panel is likely to be biased. My goal in selecting my sample is to create a balanced panel of treated and control firms that utilizes data from as many living wage firms as possible while maintaining a sufficient pre- and post- certification study period to capture the effect of living wage certification on employee earnings and turnover.

Of the 45 living wage firms identified in the data, I have excluded nine firms that have insufficient data.[21] Six of these have noticeably short histories, having gone into business within three months of becoming certified. The short pre-certification history of these firms makes it difficult to include them in my analysis because there is little data with which to estimate the pre-certification levels of earnings or turnover. The other three have large gaps in the data during which they have no employees with earnings. I have chosen a sample period of 14 months pre-certification through 12 months post-certification to include as many firms with sufficient pre-treatment histories as possible. This allows me to use data from 36 living wage firms that come from all industries except for cleaning and security. The exclusion of firms from this historically low-paying industry group is troubling because any benefit associated with the living wage might be expected to be more pronounced for firms that have to raise wages significantly to come into compliance.

Additionally, I exclude three firms that do not have any directly employed workers earning less than the full-time living wage equivalent. Not all living wage firms need to raise wages of directly employed workers in order to come into compliance with certification requirements. For these firms, the impact of certification would be seen in increased contract costs for services such as cleaning, janitorial, landscaping or document destruction. While firms are required to pay the living wage to workers employed on such contracts, I do not have data on either the cost of these contracts or on the employment histories of these workers. Therefore, I focus on the impact of living wage certification on firms for which the living wage is likely to place a binding constraint on the wages of direct employees.

[21] Based on rounded counts as required to preserve confidentiality.

I define a potentially affected worker as someone who earned, on average, less than the full-time living wage equivalent in the year before certification.[22] Using this definition, three of the 36 living wage firms with sufficient pre- and post-certification data would not have been directly affected by the wage requirements of certification. These firms were from the relatively high paying sectors of professional or technical services and labour unions, which are two industry groups that are well represented in the remaining sample.

My final sample therefore consists of the 33 living wage firms that have both sufficient data and directly employed workers who are potentially affected by the living wage. To establish a counterfactual, I create a control group by manually matching each living wage firm to one or more control firms. My matching procedure is designed to construct a control group that operated within the same customer and labour market environment as the living wage firms. To do this, I began by making exact matches on the basis of industry and region. This, for instance, would ensure that environmental groups in the Waikato are only matched to other environmental groups in the Waikato. Then, I looked at firm size on the date of certification. It is likely that a firm with 100 employees faces different economic and labour market conditions than a firm with 10 employees. For this variable, I matched within callipers because firm size varies over time and requiring an exact match was too restrictive. Lastly, to avoid comparing start-ups with long established firms that are likely to have different strategies and constraints, I matched on firm age within three categories. The result of this matching procedure is that a five-year old cosmetics company based in Auckland with 10 employees would not be matched with a national brand. Instead, it would be matched to other Auckland based cosmetics companies with 5-15 employees that had been in business since 2014. A full description of callipers used, as well as t-tests for the differences in means between living wage and control firms with respect to key variables can be found in the appendix.

This matching procedure allows control firms to act as matches for more than one treated firm. Where this occurred, I include the matched firm in the analysis *k* times, where *k* is equal to the number of treated firm matches for that control firm. This method of matching yields between 1 and 92 matches for each living wage firm. I then weight each control firm by 1/n where n is the

[22] Monthly full-time living wage equivalent = (40 hours per week * hourly living wage rate * 52 weeks)/12 months.

number of matches for a given living wage firm. Weights are used in the calculation of summary statistics but are not used in regression analysis.[23]

Table 3.5 | Table of descriptive statistics

Table of Descriptive Statistics					
All Workers			Potentially Affected Workers		
	LW	Control		LW	Control
Monthly Earnings	4138	4056	Monthly Earnings	2012	1936
(Standard deviation)	(1731)	(1958)	*(Standard deviation)*	(1166)	(904)
% Earning less than LW (40 hrs/wk)	0.4345	0.4579	% Earning less than LW (40 hrs/wk)	0.8490	0.8727
	(0.25)	(0.30)		(0.25)	(0.22)
% Earning less than LW (30 hrs/wk)	0.3083	0.3381	% Earning less than LW (30 hrs/wk)	0.6142	0.6670
	(0.22)	(0.28)		(0.33)	(0.32)
Firm age in months	172	185	Firm age in months	170	185
	(59)	(51)		(60)	(51)
Number of employees	34.9	46.3	Number of employees	36.4	47.2
	(42.8)	(49.7)		(43.6)	(50.0)
Number of potentially affected workers	7.7	8.7	Number of potentially affected workers	8.2	9.0
	(11.3)	(7.9)		(11.4)	(7.8)
Employee tenure	76	83	Employee tenure	66	74
	(32)	(36)		(41)	(36)
Employee age	45.3	44.8	Employee age	44.2	43.6
	(7.5)	(8.9)		(11.6)	(10.6)
% Female	0.6927	0.6774	% Female	0.7814	0.6892
	(0.26)	(0.28)		(0.26)	(0.31)
% European*	0.8401	0.8237	% European*	0.8324	0.8376
	(0.17)	(0.22)		(0.23)	(0.25)
% Maori	0.1764	0.1845	% Maori	0.1871	0.1886
	(0.16)	(0.17)		(0.26)	(0.23)
% Pacific Islander	0.0975	0.0994	% Pacific Islander	0.1156	0.0897
	(0.18)	(0.17)		(0.21)	(0.20)
% with University**	0.1441	0.1408	% with University**	0.1312	0.1387
	(0.13)	(0.13)		(0.19)	(0.18)
% with High School	0.1055	0.1173	% with High School	0.1324	0.1172
	(0.13)	(0.13)		(0.20)	(0.18)
Firms	33	606	Firms	33	606
Workers	2400	17800	Workers	440	3700
Statistics computed first by firm, then averaged across treatment status.					
Values for firm age, employee count and number of potentially affected workers vary slightly because not all firms have potentially affected workers in every period.					
*Employees can belong to more than one ethnicity					
** Highest qualification					

Source: Author's calculations

I tested the suitability of this control group by confirming the presence of parallel trends. To test for parallel trends, I used three common methodologies. First, for all outcome variables, I created visual plots to observe the patterns of change and did not notice any obvious violations of the parallel trends assumption. I also included pre-treatment dummy variables in regression

[23] I ran all my regressions both with and without these weights and found that weighting did not affect the results.

specifications. Statistically significant coefficients on the pre-treatment dummy variables would reveal that living wage and control firms differed before certification and would suggest a violation of the parallel trends assumption. These coefficients were statistically insignificant. Finally, I included linear and quadratic time trends in my regressions to control for the possibility of differing trends between living wage and control firms. The results were robust to the inclusion of these time trends, a finding that confirms that the two groups did not have different pre-treatment trends in employment, earnings or turnover. When the inclusion of firm-specific linear time trends alters the regression coefficients, this is evidence that the firms have different patterns of underlying growth in the dependent variables (Allegretto, Dube, & Reich, 2011).

Pre and post comparisons of employee earnings and turnover are based on the living wage firms date of likely compliance with living wage requirements. Because living wage certification requires that firms raise wages where necessary *before* applying for certification and because the certification process takes between one and two months, I use two months before certification as the date of likely compliance with the living wage. I then assign a certification date to each control firm based on that of their matched living wage employer. Both the certification date and two months prior are indicated on all plots but regressions and estimates of treatment effects are based on the date of likely compliance, which is two months prior to certification.[24]

In total, my sample contains 33 living wage firms and 606 control firms. Descriptive statistics for both the full sample and a sub-sample of potentially affected workers can be found in Table 3.5. The left-hand panel contains statistics for the full-sample of workers, while the right-hand panel contains statistics for the sub-sample of employees who were likely to experience an increase in wages associated with living wage certification. The data in Table 3.5 suggest that the firms are well matched on most observable measures related to employee characteristics and earnings while the sub-sample of potentially affected workers differs from the full sample primarily with respect to earnings.

[24] I repeated my regression analysis using 1-month prior to certification date as the date of likely compliance. This does not substantially change the results.

3.13 METHODOLOGY

Using a firm-level panel dataset with monthly observations, I estimate three fixed-effects regression equations to identify the effect of the living wage on labour costs, employee earnings and turnover for both the full sample of workers and the sub-sample of potentially affected workers. To control for differences in firm-level trends in earnings and turnover, I add firm-specific linear and quadratic time trends to some specifications.

I begin by analysing the impact of impact of living wage certification on employee earnings, firm labour costs and employment. To do this, I look at three firm-level dependent variables: log average monthly earnings, the proportion of the workforce earning less than the 30 hour per week full-time equivalent living wage and number of employees. I then move to an analysis of the living wage's impact on turnover. To estimate the effect of living wage certification on employee turnover, I use three dependent variables: worker flow rate, job reallocation rate and churning flow rate. Taken together, these variables help provide a picture of overall turnover while disentangling turnover driven by changes in firm size from turnover driven by voluntary quits or terminations. Here, I follow the definitions of turnover popularized by Davis and Haltiwanger (1992) and Burgess et al. (2001). These definitions have become standard in the turnover literature and are useful at detecting changes in overall turnover and explaining the forces that drive it (Dale-Olsen, 2016). A detailed description of the variables used is presented in Section 3.9.1.

Explanation of Variables Used in the Calculation of Turnover

There are many forces that influence workers' decisions to move between jobs or between periods of employment and unemployment. Broadly speaking, these forces can be divided into two groups: the first relates to the availability of jobs in the economy, while the second relates to the forces that drive workers to change jobs or enter/exit the workforce. The first group influences the number and type of available jobs and includes factors such as economic conditions, industry health, the competitive landscape, regulations and changes in firm costs. The second group includes forces that influence satisfaction with the employer-worker match such as pay, potential for advancement and perception of alternative employment opportunities as well as personal or family factors that induce workers to join or leave the labour force.

By increasing wages for low-skilled work, the living wage has the potential to influence both firm costs and employee satisfaction. As such, certified living wage firms may experience changes in

both types of turnover. To capture changes in turnover stemming from these different causes, I use three complementary measures of turnover. The first measures turnover associated with the creation or destruction of jobs, while the second measures total turnover and the third captures changes associated with match re-evaluation. The variables are explained in detail below.

Job flow, JF_{it}, measures the creation or destruction of jobs in firm *i*, in month *t*, by comparing employment at time *t* with employment at time *t-1*. *Job Flow* is calculated as $JF_{it} = E_{it} - E_{it-1}$ where E_{it} refers to the number of workers at firm *i* in month *t*. This measure represents the month-to-month change in employment necessary to accommodate the firms level of expansion or contraction. Job creation occurs when job flow is positive, $JF_{it} > 0$; job destruction occurs when job flow is negative $JF_{it} < 0$. *Job reallocation* is the total number of jobs created or destroyed within a month and is calculated as the absolute value of job flow, $JR_{it} = |JF_{it}|$. Job reallocation is designed to capture turnover resulting from firm-instigated changes in staffing levels. However, in a supply constrained labour market, job reallocation may inadvertently capture reduction in staff sizes resulting from failed attempts at recruitment.

Worker flow measures the total movement of workers into and out of an establishment for any reason and is calculated as the sum of hires (H_{it}) and separations (S_{it}): $WF_{it} = H_{it} + S_{it}$. Worker flow is always greater than or equal to job flow as it incorporates both the turnover necessary to meet a firms' staffing needs as well as the movement of workers between jobs that occurs when employees and employers seek to improve the quality of their matches. The movement of workers in excess of that required to maintain current desired staffing levels is called *churning flow,* CF_{it}. Churning flow is calculated as the difference between Worker Flow and Job Reallocation: $CF_{it} = WF_{it} - JR_{it}$ and can be conceptualized as a 'job match re-evaluation' instigated by either employer or employee (Burgess, Lane, & Stevens, 2000).

All of these measures are most useful when converted into a monthly rate. To calculate monthly rates, I use the methodology of Davis and Haltiwanger (1992) and divide by the two-month average employment level within the firm, N_{it}, where $N_{it} = .5(E_{it} + E_{it-1})$. This yields turnover rates that are symmetric about zero and that lie in the interval of [-2,2]. These turnover rates can be summarized as:

Job Reallocation Rate (JRR)

$$JRR_{it} = \frac{|E_{it} - E_{it-1}|}{.5(E_{it} + E_{it-1})}$$

Worker Flow Rate (WFR)

$$WFR_{it} = \frac{H_{it} + S_{it}}{.5(E_{it} + E_{it-1})}$$

Churning Flow Rate (CFR)

$$CFR_{it} = \frac{WF_{it} - JR_{it}}{.5(E_{it} + E_{it-1})}$$

Each of these measures sheds light on a different cause of turnover. Changes in the worker flow rate represent the overall level of turnover for a firm in a given month, while the job reallocation rate is the portion of turnover necessary for a firm to achieve its desired size and level of staffing. Job reallocation rates will increase as firms create or destroy jobs so should be interpreted in conjunction with estimates on changes in firm size. The churning flow rate is the percentage of workers each month who move in and out of the organisation for reasons beyond staffing needs. Those churners may leave due to either their own volition or following the decision of their employer. The purpose of churning flows is generally believed to be the improvement of the fit between worker and job.

When applied to the sub-sample of potentially affected workers in my sample, turnover measures do not effectively capture the hiring of these employees. This stems from the fact that potentially hired workers were by definition employed both before and after certification. This dataset does not contain earnings information on workers before they joined the living wage and control firms so my analysis of turnover for potentially affected workers is limited to an analysis of separations or separations plus rehires. Therefore, I restrict my discussion of turnover on potentially affected workers to an analysis of worker flow rate.

Models

I use a firm-month panel dataset and fixed effects regression estimates to examine the effect of the living wage. The focus is primarily on the coefficient for a dummy variable indicating living wage certification. The specifications differ in the number and type of covariates included and are

designed to test the robustness of any measured effect to the inclusion of control variables, as well as to estimate changes in the impact of the living wage over time. Additionally, because there is a high-level of heterogeneity in average earnings across firms, I attempt to separate the living wage effect from firm-level differences in wage growth by including both linear and quadratic time trends to each model. I compare the results of my fixed effects models with both visual plots and simple difference-in-difference analysis and find a high degree of consistency in my estimates across specifications. I discuss the models below and follow with a discussion of the results in Section 10.

I begin with a simple regression model with fixed effects for both firm and time:

$$(1) \quad Y_{it} = \beta_1 lwe_{it} + \alpha_i + \lambda_t + \varepsilon_{it}$$

Here, Y_{it} is the dependent variable of interest for firm *i* in month *t.* The treatment effect of living wage certification is estimated by β_1 and lwe_{it} is a dummy variable equal to 1 if firm *i* is a living wage employer at time *t.*[25] Firm fixed effects are captured by α_i, and λ_t represent a full set of 27 dummies for each calendar month. The tern, ε_{it}, is an idiosyncratic error term. The time fixed effect, λ_t captures time-specific shocks that are common to all firms. To control for firm-level heterogeneity in patterns of wage growth, I add linear and quadratic firm-specific time trends to some interactions.

My next model controls for observable factors including firm size as well as for demographic, employment and educational characteristics of employees.

$$(2) \quad Y_{it} = \beta_1 lwe_{it} + \delta_1 X'_{it} + \alpha_i + \lambda_t + \varepsilon_{it}$$

The vector of demographic and educational controls is represented by X_{it} and contains variables representing the proportion of the workforce that matches certain demographic and educational characteristics and accounts for gaps in employment. I control for the average number of months that an employee is absent from his or her job throughout job tenure with the employer, as well as for gender and race. Additionally, I include two controls to indicate the average level of maximum educational attainment in the organisation. This specification allows me to control for factors about

[25] I treat a firm as being certified two months prior to its actual certification date as compliance with wages is required before a firm can apply for certification.

employees that are likely to affect earnings. Again, I include linear and quadratic firm-specific time trends as controls in some iterations of the model.

My final specification seeks to identify changes in the living wage effect over time. Firms operate in dynamic environments; hence, it is possible that the effect of the living wage could vary over time. To detect these variations, I include time/treatment interaction dummies:

$$(3) \quad Y_{it} = \gamma_1 T_{-1} + \gamma_2 T_0 + \gamma_3 T_1 + \gamma_4 T_2 + \gamma_5 T_3 + \delta_1 X_{it} + \alpha_i + \lambda_t + \varepsilon_{it}$$

In this specification, I include five time/living wage interaction variables. I do not include a separate living wage treatment variable so there is no omitted group; each coefficient can be interpreted as the treatment effect in that time period.[26] The time variables are delineated as follows: $T_{-1}$is equal to 1 for all living wage firms in the six months prior to the time of likely compliance, while T_0 equals 1 for living wage firms in the two months prior to certification (months -2 to 0) during which time the firms are more likely to have raised wages but are not yet certified. The post-certification period is captured by three 4-month intervals represented by the variables T_1 - T_3. In the regression models, the coefficients on the time variables capture differences between living wage and control firms at different periods of time, allowing for the living wage to have a dynamic effect on earnings and turnover. By adding a pre-treatment time variable, I am able to investigate changes that occur in living wage firms in the months leading up to certification. The absence of statistically significant pre-treatment coefficients helps support the common trend assumption. The regression results are presented in Section 3.14. I include firm specific linear and quadratic time trends to some estimations and the results of all models are presented together.

3.14 RESULTS

Employee Earnings and Firm Labour Costs

I begin with an analysis of changes in average gross monthly earnings by firm in the full sample and in the sub-sample of potentially affected workers. The measure of average monthly employee

[26] This yields mathematically identical results to those generated using an overall treatment group but I use this approach because the interpretation is more intuitive when a pre-treatment dummy is included. When a general treatment variable is included in this model, the interpretation of the pre-treatment interaction coefficient is different from the other time/treatment interactions.

earnings for all workers is my best measure of changes in total firm labour costs following living wage certification. However, because earnings increases for individual workers are concentrated among low-paid employees, this variable will not give us much information about the increase in earnings for individual workers. A comparison of changes in unconditional mean monthly salaries shows that employee earnings increased in both treatment and control firms over the study period but that the increase is larger in living wage and control firms (Table 3.6). Following certification, the average monthly employee earnings increased in living wage firms by 5.3%, compared to 1.5% in control firms.

This general pattern can be seen in Figure 3.5, which plots the natural log of monthly gross earnings over time. The dependent variable is average employee earnings by firm, which is then averaged by treatment status. On the x-axis, there are two vertical lines indicating both the month of certification (month 0) and the time of likely compliance (month -2). The left-hand panel of Figure 3.5 plots the raw data and reveals substantial month-to-month variation in employee average earnings and does not suggest a clear change in earnings at the time of certification. In the right-hand panel, I apply a 3-month moving average smoother to the earnings data. Plots of the smoothed data indicate that average earnings in living wage firms rose relative to that in control firms beginning roughly two months before certification but that this increase may have been short lived. By the eighth month following certification, the earnings in living wage firms return to the level of that in control firms, suggesting that firms may be able to contain the extra costs through increases in efficiency or through wage compression.

Table 3.6 | Before and after comparisons | Earnings | All workers

Before and After Comparisons of Earnings in Living Wage & Control Firms All Workers						
	Living Wage			Control		
	Pre	Post	% Change	Pre	Post	% Change
Monthly earnings	$3,926	$4,133	5.3%	$4,007	$4,067	1.5%
(Standard deviation)	1,843	1,764		1,978	1,979	
% earning less than LW (30 hrs/wk)	0.348	0.311	-10.7%	0.344	0.340	-1.2%
	0.23	0.24		0.29	0.29	
Firm size (number of employees)	32.3	35.3	9.2%	46.0	46.9	1.9%
	(39.5)	(44.8)		(49.9)	(49.7)	
Number of firms	36	36		573	573	
Number of workers	1700	2100		11700	13400	

Source: Author's calculations, firm counts rounded to protect confidentiality.

Figure 3.5 | Log earnings | All workers

Source: Author's calculations; values rounded as required to protect confidentiality.

Regression results for log average employee earnings are presented in Table 3.7. The results in panel 1 show a statistically significant increase in labour costs in the range of 5.5% to 8.9%. However, this effect becomes smaller and less statistically significant with the addition of firm-specific time trends. Linear firm-specific time trends are added in panel 2, while panel 3 contains both linear and quadratic time trends. Because the New Zealand living wage is not randomly assigned, there are likely to be important but unobservable factors influencing the process of self-selection. The firm fixed-effect captures these changes if they are time invariant but other factors such as firm-specific trends in wage growth are captured neither by the firm nor time fixed effect. To understand the impact of the living wage on worker earnings and employee labour costs, I want to see if the adoption of the living wage lead firms to change their behaviour *away from their pre-existing trajectory*. For this reason, I prefer the estimates provided in the final panel as it contains the most robust set of controls. Panel 3 of Table 3.7 reveals that living wage certification may be responsible for a 4.4%-8.5% increase in overall labour costs but that substantial heterogeneity between firms makes cost increases difficult to measure precisely. Importantly, in all specifications, the pre-treatment difference between employee earnings in living wage and control firms is close to 0 and statistically insignificant. This finding helps validate the common trends assumption and to reinforce the appropriateness of the control group.

The effect of living wage certification on earnings for the sub-group of potentially affected workers is more pronounced. Unconditional mean monthly earnings rise for potentially affected workers in both living wage and control firms but the increase is greater in living wage firms (Table 3.8).

A simple difference in difference estimation of average earnings suggests an earnings advantage for potentially affected workers in living wage firms of $182 per month. Plots of log earnings for potentially affected workers (Figure 3.6) reveal less month-to-month volatility and a more noticeable treatment effect than that for the full sample. Over a two-year period, we expect to see incomes of low-paid employees increase at a faster rate than that of the overall workforce as younger workers 'move up the ladder' in their careers. Additional research in this area should focus on differentiating between workers who are temporarily low-income and those who are persistently earning low-income wages.

Table 3.7 | Fixed effects estimates | Log average employee earnings | All workers

Fixed-effects estimates of the effect of living wage certification on log monthly gross earnings All Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross
LW Certification	0.0548**	0.0894***		0.0553	0.0951**		0.0439	0.0862*	
	(0.02)	(0.03)		(0.04)	(0.04)		(0.05)	(0.05)	
LW * 6 months pre			0.0282			0.0239			0.00635
			(0.04)			(0.03)			(0.04)
LW * Transition period			0.108**			0.113*			0.0995
			(0.05)			(0.07)			(0.08)
LW * Months 1-4			0.105**			0.108			0.104
			(0.04)			(0.08)			(0.09)
LW * Months 5-8			0.123***			0.129			0.145
			(0.04)			(0.11)			(0.10)
LW * Months 9-12			0.0785*			0.0906			0.139
			(0.05)			(0.15)			(0.11)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	7.903***	8.090***	8.091***	7.794***	7.621***	7.671***	-6.988	-321.3**	-277.5*
	(0.02)	(0.09)	(0.09)	(0.29)	(0.28)	(0.28)	(133.30)	(128.80)	(158.40)
Observations	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300
R-squared	0.866	0.874	0.874	0.888	0.896	0.896	0.897	0.905	0.905

*Note: Earnings averaged first by firm, then by treatment. Standard error adjusted for clusters by firm; *** p<0.01, ** p<0.05, * p<0.1; Each time period is coded as a separate treatment variable. There is no omitted group.*

Table 3.8 | Before and after comparisons | Earnings | Potentially affected workers

Before and After Comparisons of Earnings in Living Wage & Control Firms Potentially Affected Workers						
	Living Wage			Control		
	Pre	Post	% Change	Pre	Post	% Change
Monthly earnings	$ 1,651	$ 2,215	34.2%	$ 1,719	$ 2,101	22.2%
(Standard deviation)	837	1,324		769	970	
% earning less than LW (30 hrs/wk)	0.706	0.564	-20.1%	0.723	0.624	-13.6%
	(0.30)	(0.34)		(0.30)	(0.32)	
Firm size (number of employees)	34.3	36.1	5.0%	47.1	47.7	1.3%
	(40.5)	(45.4)		(50.4)	(49.9)	
Number of firms	36	36		573	573	
Number of workers	440	430		4100	4000	

Source: Author's calculations; firm count rounded where required to protect confidentiality.

Figure 3.6 | Log earnings | Potentially affected workers

Source: Author's calculations; values rounded as required to protect confidentiality.

Regression results (Table 3.9) indicate that the treatment effect for potentially affected workers ranges between 7.4% and 8% and is only weakly significant. However, with firm-specific time trends, the result is more pronounced suggesting an increase in monthly earnings for low-paid workers of between 14.5% and 18.9%. The difference between these estimates suggests that low-paid workers in living wage firms experienced slower rates of earnings growth in the year leading up to certification but that earnings increased more rapidly in living wage firms following certification. The reason for this lower pre-treatment growth in earnings may be related to a postponement in scheduled wage raises in firms that were gearing up for wage certification. However, columns 3, 6 and 9 show that pre-treatment differences in earnings for low wage workers were not different in the two groups, suggesting any pre-treatment changes in wage policy were not significant enough to threaten the parallel trends assumption. In addition, the increase in

earnings for low-paid workers appears to be more persistent than the overall increase in labour costs. This suggests that firms are able to control overall wage costs through improved efficiency and/or wage compression but that the gain to low-wage workers persists.

Table 3.9 | Fixed effects estimates | Log average earnings | Potentially affected workers

Fixed-effects estimates of the effect of living wage certification on log monthly gross earnings Potentially Affected Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross	lrgross
LW Certification	0.0797*	0.0744		0.145***	0.152***		0.177**	0.189***	
	(0.04)	(0.05)		(0.05)	(0.05)		(0.07)	(0.07)	
LW * 6 months pre			-0.075			-0.0215			-0.00638
			(0.09)			(0.08)			(0.08)
LW * Transition period			0.023			0.111			0.142
			(0.09)			(0.09)			(0.09)
LW * Months 1-4			0.0765			0.190*			0.216**
			(0.07)			(0.10)			(0.10)
LW * Months 5-8			0.022			0.16			0.148
			(0.08)			(0.16)			(0.14)
LW * Months 9-12			0.0129			0.185			0.124
			(0.09)			(0.18)			(0.17)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	7.214***	7.428***	7.424***	7.138***	6.851***	6.832***	-914.4***	-1,131***	-1,169***
	(0.05)	(0.12)	(0.12)	(0.51)	(0.54)	(0.54)	(235.40)	(226.30)	(278.40)
Observations	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500
R-squared	0.734	0.743	0.743	0.802	0.806	0.806	0.829	0.833	0.833

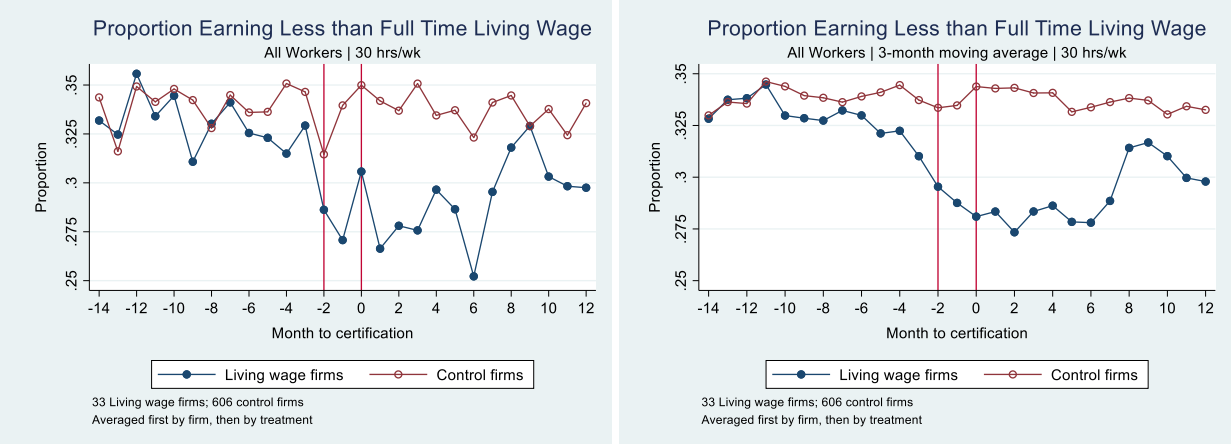
*Note: Earnings averaged first by firm, then by treatment. Standard error adjusted for clusters by firm; *** p<0.01, ** p<0.05, * p<0.1; Each time period is coded as a separate treatment variable. There is no omitted group.*

The living wage is calculated to provide an income necessary to cover the costs of a modest but sufficient New Zealand lifestyle. For this reason, the proportion of the workforce earning less than the full-time living wage equivalent is a complementary measure of the effect on wages.[27] By measuring the proportion of the workforce with sub-living wage earnings, I am able to estimate the effectiveness of the living wage movement at reaching its goal. A comparison of the full sample of workers in both living wage and control firms (Table 3.6) indicates that the proportion of the

[27] In most of this analysis, I define full-time work as 30 hours per week. As a robustness check, I repeated my analysis with a 40- hour week definition of full-time work. Those results are presented in the Appendix.

workforce with sub-living wage earnings fell by 10.7% in living wage firms but only declined by 1.2% in control firms. Plots of sub-living wage earnings over time (Figure 3.7) reveal a substantial decline in the prevalence of below living wage earnings in living wage establishments during the first eight months of certification. In contrast, the control plots show noise but no shift in the level of sub-living wage earnings.

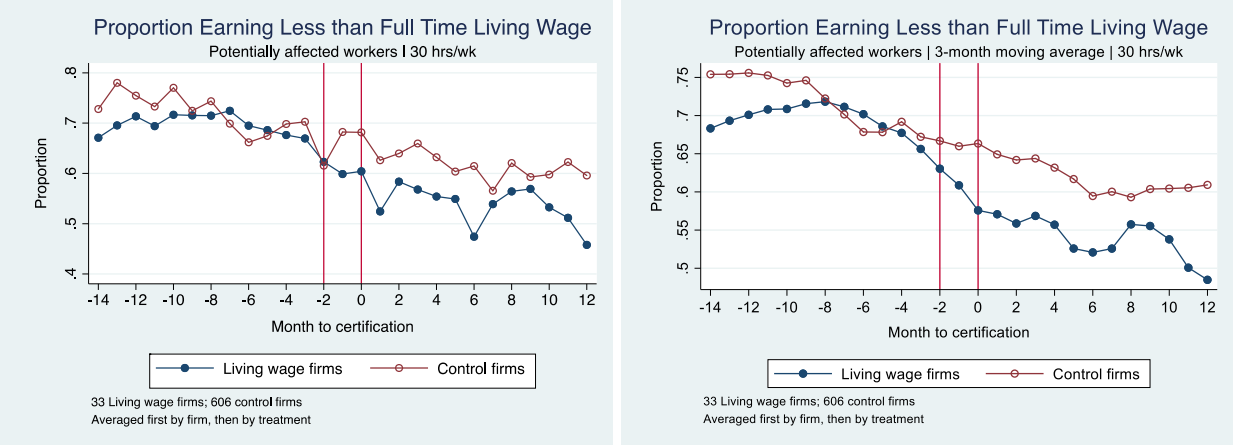
Figure 3.7 | Sub-LW earnings | All workers



Source: Author's calculations; values rounded as required to protect confidentiality.

Regression estimates (Table 3.10) indicate that the prevalence of below living wage earnings declined by between 4.0% and 7.3% in the full sample and that the largest decrease occurred during the transition period and in the first eight months of certification. The inclusion of firm-specific linear and quadratic time trends does little to change the significance or magnitude of the point estimates.

Figure 3.8 | Sub-LW earnings | Potentially affected workers



Source: Author's calculations; firm counts rounded where necessary to preserve confidentiality.

Among potentially affected workers, the pattern is similar, although low-paid workers in control groups also experience a decline in sub-living wage earnings throughout the treatment period. A comparison of unconditional mean values (Table 3.8) between treated and control employees suggests that the percentage of workers earning less than the full-time living wage fell by 20% in living wage firms, compared to 13.6% in control firms. Plots showing the proportion of workers with earnings below the full-time living wage (Figure 3.8) reveal declines in treated firms that outpace that in control firms. Regression results suggest a statistically significant decline in the prevalence of below living wage earnings of between 4.7% and 6%, (Table 3.11) with the largest changes occurring in the first four months of certification. Again, the absence of pre-treatment differences supports the presence of common pre-treatment trends.

Table 3.10 | Fixed effects estimates | Sub-LW earnings | All workers

Fixed-effects estimates of the effect of living wage certification on sub Living Wage Earnings 30 hour per week threshold All Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30
LW Certification	-0.0396*	-0.0560**		-0.0510*	-0.0728**		-0.0493*	-0.0720**	
	(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)	
LW * 6 months pre			-0.000226			0.019			0.0162
			(0.02)			(0.02)			(0.02)
LW * Transition period			-0.0628**			-0.0351			-0.0412
			(0.03)			(0.04)			(0.04)
LW * Months 1-4			-0.0659**			-0.0277			-0.0342
			(0.03)			(0.04)			(0.05)
LW * Months 5-8			-0.0622**			-0.0159			-0.0215
			(0.03)			(0.05)			(0.05)
LW * Months 9-12			-0.0344			0.0186			0.0157
			(0.03)			(0.06)			(0.06)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	0.462***	0.310***	0.310***	0.632***	0.645***	0.654***	46.52	231.9***	258.1***
	(0.01)	(0.05)	(0.05)	(0.18)	(0.18)	(0.19)	(86.06)	(79.87)	(93.64)
Observations	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300
R-squared	0.775	0.786	0.786	0.811	0.821	0.821	0.827	0.837	0.837

*Note: Full-time status defined as 30 hours of work per week. Standard error adjusted for clusters by firm; *** p<0.01, ** p<0.05, * p<0.1; Each time period is coded as a separate treatment variable. There is no omitted group.*

Table 3.11 | Fixed effects estimates | Sub- LW earnings | Potentially affected workers

Fixed-effects estimates of the effect of living wage certification on sub Living Wage earnings 30 Hour per week threshold Potentially Affected Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30	lwunder30
LW Certification	-0.0589**	-0.0600*		-0.0477**	-0.0511***		-0.0541**	-0.0576**	
	(0.03)	(0.03)		(0.02)	(0.02)		(0.02)	(0.02)	
LW * 6 months pre			0.0139			0.0219			0.00365
			(0.03)			(0.04)			(0.05)
LW * Transition period			-0.0255			-0.0102			-0.028
			(0.03)			(0.05)			(0.06)
LW * Months 1-4			-0.0648*			-0.0468			-0.0512
			(0.04)			(0.07)			(0.08)
LW * Months 5-8			-0.0627			-0.0376			-0.0145
			(0.04)			(0.09)			(0.09)
LW * Months 9-12			-0.0516			-0.0238			0.0355
			(0.05)			(0.12)			(0.11)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	0.725***	0.577***	0.578***	0.985***	1.012***	1.027***	502.2***	601.8***	663.8***
	(0.03)	(0.06)	(0.06)	(0.35)	(0.37)	(0.37)	(140.40)	(140.20)	(160.50)
Observations	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500
R-squared	0.594	0.6	0.6	0.664	0.666	0.666	0.695	0.697	0.697

*Note: Full-time status defined as 30 hours of work per week. Standard error adjusted for clusters by firm; *** p<0.01, ** p<0.05, * p<0.1; Each time period is coded as a separate treatment variable. There is no omitted group.*

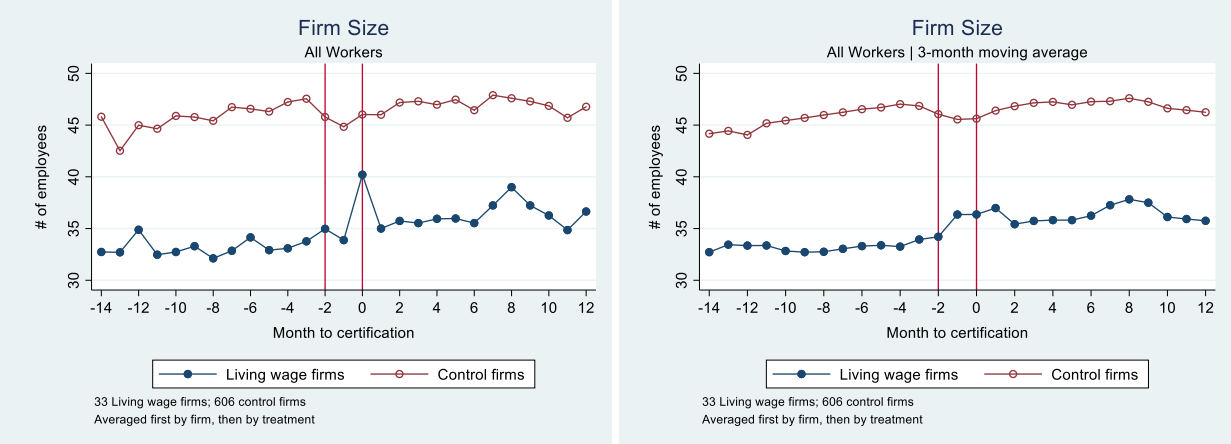
Living wage certification lead to an increase in monthly earnings for low-paid workers of between 14.5 and 18.9 log percent (Table 3.9) and reduced the proportion of the workforce earning less than the full-time living wage by between 4.7% and 6.0% (Table 3.11). The fact that changes in overall labour costs are hard to detect suggests that firms manage rising wages by compressing the existing wage structure, an assumption that is supported by findings of qualitative research with New Zealand living wage employers (Stansfield, 2017). Additionally, some of the wage effects appear to be short lived, a pattern that may indicate that firms undergo a period of adjustment where they shift schedules and employment relationships to mitigate the costs of the higher wages. This area deserves further research attention. Although the terms of living wage certification do not permit adjusting hours or benefits at the time of certification, it does not prevent firms from re-assessing their staffing needs over time and adjusting hours as needed. Qualitative research on living wage employers find that firms adapt to higher wages by paying increased attention to

staffing rosters to avoid downtime (Stansfield, 2017). This may represent improved efficiency with labour scheduling that serves to minimize cost increases but which may also limit earnings gains.

Employment & Disemployment

I find no evidence of disemployment among living wage firms following certification. On average, living wage firms added employees over the observation period, with staff sizes growing by 9.2% on average. Over the same period, growth in control firms was a smaller 1.9% (Table 3.6). Plots of firm size over time (Figure 3.9) show a general pattern of slow growth for both sets of firms, although the gap between groups falls post-treatment, suggesting that living wage firms are growing more quickly than control firms. The regression results (Table 3.12) produce consistent but imprecisely measured estimates of growth in employment of roughly two workers. The addition of firm-specific linear and quadratic time trends to the specifications improves the precision of the estimates but does not change the significance or magnitude of the results. There is no pre-treatment effect in any specification. There is no evidence that New Zealand living wage firms respond to higher labour costs by reducing the size of their work force. In fact, I find weak evidence that these firms have experienced stronger growth than their un-certified counterparts.

Figure 3.9 | Firm Size | All workers



Source: Author's calculations; firm counts rounded where required to protect confidentiality.

This result contrasts with findings from other studies on both voluntary (Wills & Linneker, 2012) and mandated living wage programs (Neumark, Thompson, & Koyle, 2012). One possible explanation for the absence of disemployment is that living wage firms are able to improve efficiency through small cost-cutting measures and adjustments to internal pay hierarchies to make the higher costs of living wage employment sustainable. Another possibility that is worthy of

future research is that firms can pass small cost increases on to customers through slight price increases. Perhaps the living wage brandmark is successful at attracting customers in otherwise saturated markets. This would be consistent with findings from other research on brandmarks such as 'Fare Trade' (Hainmueller, Hiscox, & Sequeira, 2015).

Table 3.12 | Fixed effects estimates | Firm size | All workers

Fixed Effects Estimates of the Effect of Living Wage Certification on Firm Size									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	orgemp	orgemp	orgemp	orgemp	orgemp	orgemp	orgemp	orgemp	orgemp
LW Certification	2.483	2.446		2.35	2.247		2.364	2.221	
	(2.14)	(2.15)		(1.67)	(1.67)		(1.79)	(1.80)	
LW * 6 months pre			2.806			0.741			1.155
			(2.28)			(1.85)			(1.86)
LW * Transition period			1.645			-1.024			-1.158
			(1.92)			(2.57)			(2.49)
LW * Months 1-4			2.953			-0.405			-1.441
			(2.34)			(2.72)			(3.03)
LW * Months 5-8			2.076			-1.832			-3.717
			(2.56)			(3.73)			(4.86)
LW * Months 9-12			-0.205			-1.4			-0.862
			(0.51)			(1.06)			(0.62)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	12.16***	12.74***	12.73***	-8.959	-6.96	-8.938	-19,520***	-20,147***	-22,801***
	-0.749	-1.28	-1.28	-6.891	-6.852	-6.62	-3,238	-3,167	-4,937
Observations	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300
R-squared	0.967	0.968	0.968	0.979	0.98	0.98	0.981	0.982	0.982

*Note: Firm size determined on a headcount basis. Standard error adjusted for clusters by firm; *** p<0.01, ** p<0.05, * p<0.1; Each time period is coded as a separate treatment variable. There is no omitted group.*

Employee Turnover & Separation

Despite qualitative reports of reductions in turnover and improvements in retention (Brown, Newman, & Blair, 2014; Stansfield, 2017), I can find no quantitative evidence that living wage certification reduces average turnover. However, there is substantial heterogeneity in turnover across firms and it remains likely that impacts on turnover vary widely. Turnover in living wage firms was lower than that in control firms both before and after certification but I do not detect changes in patterns of turnover following certification (Table 3.13). Turnover fell by all measures in both groups of firms throughout the observation window with the exception of churning flow

rate, which increased by one tenth of a percentage point in living wage firms. This increase was not statistically significant.

Table 3.13 | Before and after comparisons | Turnover | All workers

Before and After Comparisons of Turnover in Living Wage & Control Firms All Workers						
	Living Wage			Control		
	Pre	Post	% Change	Pre	Post	% Change
Worker flow rate	0.152	0.148	-2.2%	0.168	0.159	-5.8%
(Standard deviation)	(0.22)	(0.24)		(0.25)	(0.25)	
Job reallocation rate	0.099	0.094	-5.0%	0.095	0.091	-3.9%
	(0.19)	(0.21)		(0.18)	(0.19)	
Churning flow rate	0.053	0.054	3.1%	0.074	0.068	-8.1%
	0.2349	0.2445		(0.29)	(0.29)	
Firm size (number of employees)	32.3	35.3	9.2%	46.0	46.9	1.9%
	(39.5)	(44.8)		(49.9)	(49.7)	
Number of firms	36	36		573	573	
Number of workers	1700	2100		11700	13400	

Source: Author's calculations; firm count rounded where required to protect confidentiality.

Overall turnover in the full sample is best represented by worker flow rate. Plots of worker flow rate for all employees are presented in Figure 3.10. Visually, total turnover becomes more variable in living wage firms following certification but the overall level does not change. This is confirmed by fixed effects estimates that produce coefficients remarkably close to zero and statistically insignificant in all specifications (Table 3.14). Adjustments to specifications by including firm-specific time trends increases the point estimates somewhat but does not alter the precision. It does not appear that living wage certification leads to declines in overall turnover.

Measures of the job reallocation and churning rates yield similar results. Visually, there is no change in job reallocation (Figure 3.11) or churning flow (Figure 3.12) at the time of certification and regression results show that changes in both measures are quite small and statistically insignificant. Regression results for job reallocation are presented in Table 3.14. Changes in the job reallocation rate would indicate that firms have created or destroyed jobs through changing the size of their workforces. Because job reallocation is defined as an absolute value, an increase in job reallocation could indicate either creation or destruction of jobs. The first panel of Table 3.14 suggests that living wage certification resulted in no change to job reallocation. However, the second two panels (columns 3-6) present coefficients in the range of 2.1% to 3.0%. Although

statistically insignificant, when coupled with the estimates on employment levels presented earlier, these point toward the possibility of increased employment and growth in firm size.

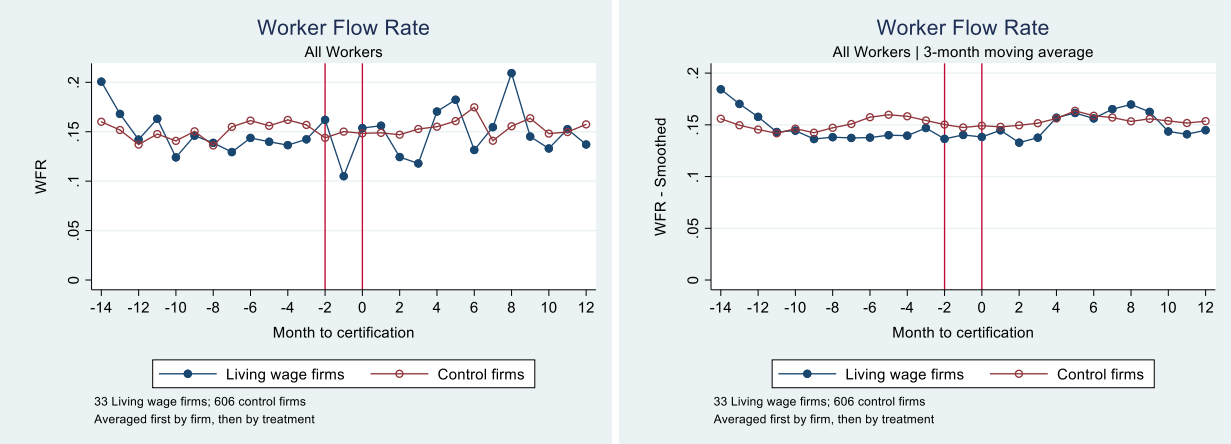
Table 3.14 | Fixed effects estimates | All measures of turnover | All workers

Fixed Effects Estimates of the Effect of Living Wage Certification on Turnover Rates All Workers						
	1		2		3	
	(1)	(2)	(3)	(4)	(5)	(6)
Worker flow rate	-0.000003	-0.00639	0.00826	-0.00183	0.00786	-0.00199
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
R-squared	0.388	0.396	0.421	0.43	0.457	0.467
Job reallocation rate	-0.00015	-0.0015	0.0258	0.0213	0.0299	0.0244
	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)
R-squared	0.355	0.359	0.385	0.391	0.418	0.426
Churning flow rate	0.000147	-0.00489	-0.0176	-0.0231	-0.0221	-0.0263
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
R-squared	0.234	0.241	0.286	0.291	0.328	0.332
Time fixed effects	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y
Firm-specific linear time trend			Y	Y	Y	Y
Firm-specific quadratic time trend					Y	Y
Observations	17300	17300	17300	17300	17300	17300

*Source: Author's calculations. Standard error adjusted for clusters by firm; *** p<0.01, ** p<0.05, * p<0.1; Regression results for different treatment periods show no effect and are presented in the Appendix.*

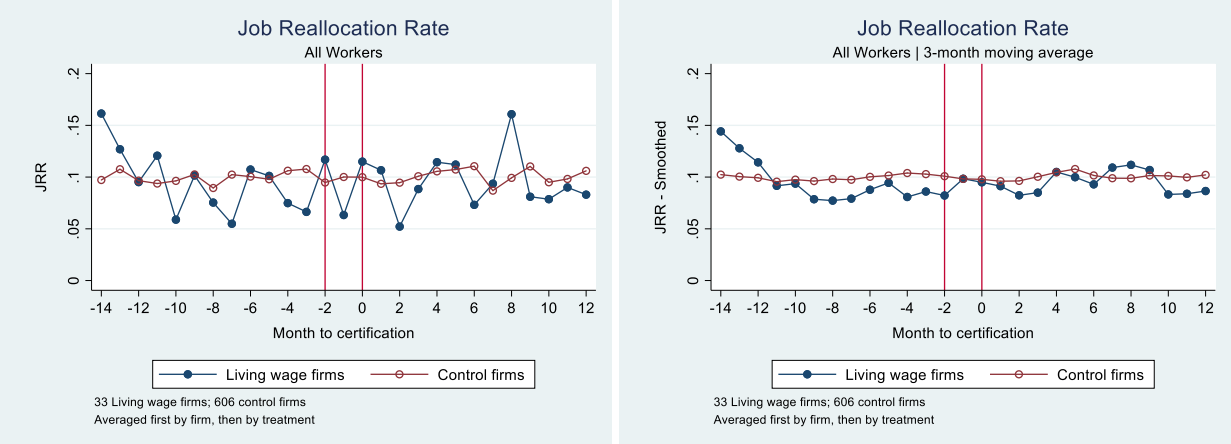
Overall turnover (worker flow) is defined as the sum of job reallocation and churning flow. Where job reallocation measures turnover resulting from the creation or destruction of jobs, churning flow measures turnover resulting from the process of job-fit re-evaluation by both employers and employees. As seen with job reallocation, changes in patterns of churning flow are neither visible in plots by treatment, nor are they detectable using fixed effects estimation. However, the coefficients for churning flow presented in Table 3.14 are overwhelmingly negative, a result that lends weak support to the findings of reduced turnover produced in other living wage studies. A follow-up study with a larger sample of currently certified firms could explore this issue further.

Figure 3.10 | Worker flow rate | All workers



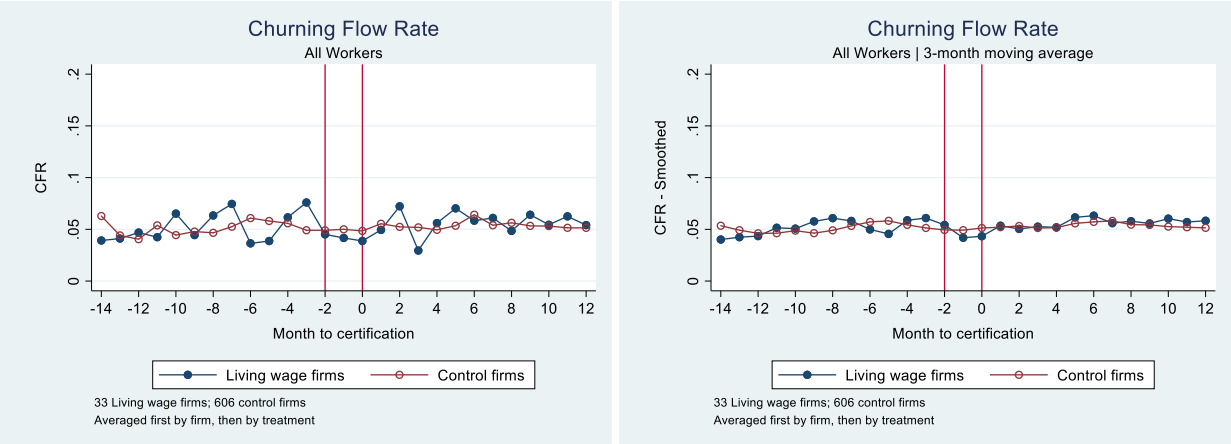
Notes: Author's calculation; firm counts rounded as required to protect confidentiality.

Figure 3.11 | Job reallocation rate | All workers



Notes: Author's calculation; firm counts rounded as required to protect confidentiality.

Figure 3.12 | Churning flow rate | All workers



Notes: Author's calculation; firm counts rounded as required to protect confidentiality.

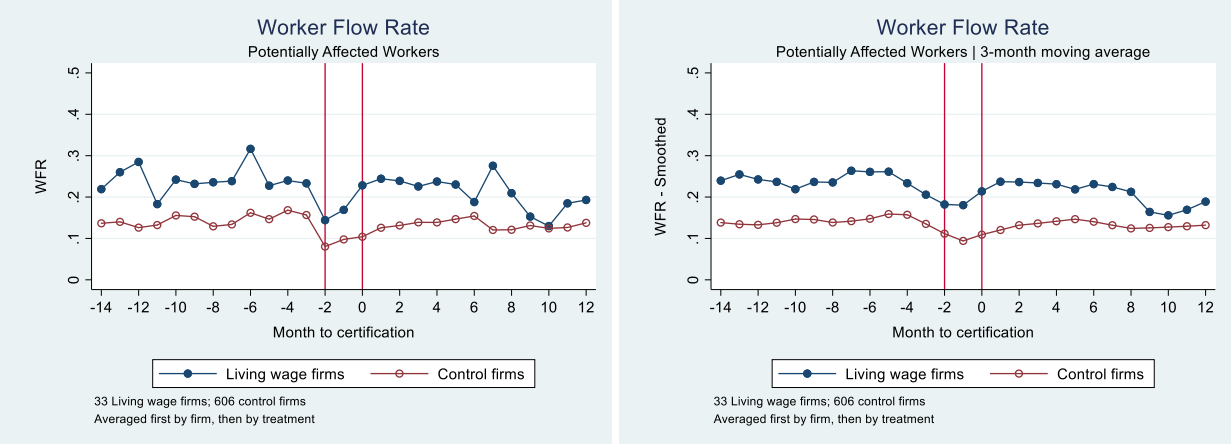
The sub-sample of potentially affected workers is defined based on their average pre-treatment earnings in the living wage and control firms. As such, this group of employees was employed both before and after certification and my estimation of changes in turnover is limited to estimates of their separations and rehires. Simple comparisons of changes in average turnover rates suggests that turnover among low-paid workers fell in living wage firms relative to control firms (Table 3.15) and this change is visible in plots of the worker flow rate for potentially affected workers (Figure 3.13). However, fixed effects estimation suggests that these changes are not statistically significant (Table 3.16). In fact, the coefficients estimating the effect of living wage certification on worker flow rates of potentially affected workers are very close to zero and are just as likely to be positive as negative.

Table 3.15 | Before and after comparisons | Turnover | Potentially affected workers

Before and After Comparisons of Turnover in Living Wage & Control Firms Potentially Affected Workers						
	Living Wage			Control		
	Pre	Post	% Change	Pre	Post	% Change
Worker flow rate	0.245	0.196	-20.2%	0.292	0.288	-1.4%
(Standard deviation)	(0.40)	(0.38)		(0.40)	(0.42)	
Number of firms	36	36		573	573	
Number of workers	440	430		4100	4000	

Source: Author's calculations; firm count rounded where required to protect confidentiality.

Figure 3.13 | Worker flow rate | Potentially affected workers



Notes: Author's calculation; firm counts rounded as required to protect confidentiality.

Table 3.16 | Fixed effects estimates | Worker flow rate | Potentially affected workers

Fixed Effects Estimation of the Effect of Living Wage Certification on Separation Rates Potentially Affected Workers									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	wfr	wfr	wfr	wfr	wfr	wfr	wfr	wfr	wfr
LW Certification	-0.0171	-0.0145		0.00133	0.000493		-0.00949	-0.0102	
	(0.02)	(0.02)		(0.03)	(0.03)		(0.04)	(0.04)	
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	0.135***	0.129**	0.129**	-0.0252	-0.516	-0.429	526.7***	598.4***	560.0***
	(0.03)	(0.05)	(0.05)	(0.35)	(0.38)	(0.38)	(179.50)	(173.00)	(204.10)
Observations	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500
R-squared	0.46	0.471	0.471	0.51	0.517	0.518	0.549	0.556	0.556

*Source: Author's calculations. Standard error adjusted for clusters by firm; *** p<0.01, ** p<0.05, * p<0.1; Regression results for different treatment periods show no effect and are presented in the Appendix.*

3.15 CONCLUSION AND DISCUSSION

The New Zealand living wage movement strives to encourage employers to pay wages sufficient to support households with dignity. International research into the effects of the living wage has found evidence that the living wage can increase wages (Adams & Neumark, 2003), reduce poverty (Neumark, Thompson, & Koyle, 2012) and improve retention (Howes, 2005) but that living wages—like other wage floors—can result in disemployment, even when the higher wages are voluntary (Wills & Linneker, 2012). In New Zealand, there is growing support for the living wage despite concerns that it is poorly targeted for reducing poverty (Boston & Chapple, 2014; Perry, 2019).

This paper provides the first quantitative analysis of a voluntary living wage program using high quality, linked employer-employee data. My findings reveal a great deal of heterogeneity in firm response to the voluntary wage floor, making it difficult to draw strong conclusions about the impact of living wage certification on labour costs, turnover and disemployment. However, my results clearly indicate that monthly earnings increase for low-income workers following living wage certification and that the proportion of the workforce earning less than the full-time living wage equivalent also declines. This suggests that the living wage is successful at raising earnings for a band of low-income earners.

I find no evidence of disemployment following living wage certification, indicating that firms are able to absorb the costs of higher entry-level wages without reducing the overall number of workers in the firm. In fact, there is weak evidence that living wage firms are growing more rapidly than their matched control firms. This is critical, because job loss would be antithetical to the mission of the living wage. Surprisingly, I find no evidence that turnover declines when firms become certified. This finding lies in direct contrast to qualitative reports of improved retention in New Zealand after living wage certification (Brown, Newman, & Blair, 2014) but does not deny the possibility that some firms experience turnover reduction; it may be just that the effect is too small to be detected using firm level averages. It also does not imply that there is no business case for the living wage. Firms could benefit from the living wage in the form of increased sales or improved reputation, which are two areas for further research.

3.16 APPENDIX | ADDITIONAL ANALYSIS & ROBUSTNESS CHECKS

Appendix exhibit 3.16.1 | Description of matching criteria

Variables:

1. Industry (ANZSIC 06) – exact match
2. Region (RA) – exact match
3. Size (employees) – within callipers. Each firm is matched to similarly sized firms based on the following formulas.
4. N=firm size at date of certification
    - Micro (1-10): .5N to 2N
    - Small (11-30): +/- .33N
    - Medium (31-80): +/- .25N
    - Large (>80): +/- .2N
5. Age (time in data) – each living wage firm matched to a control firm based on its age as measured by its first appearance in the data. Three categories used:
    - Pre 1999
    - 1999-2014
    - Post 2014

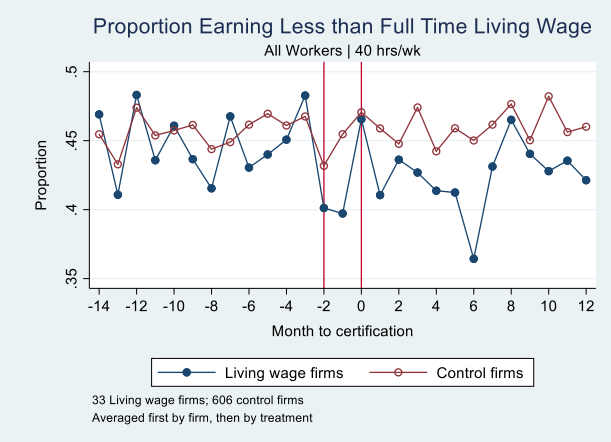
Appendix exhibit 3.16.2 | T-tests for differences in means between living wage and control firms

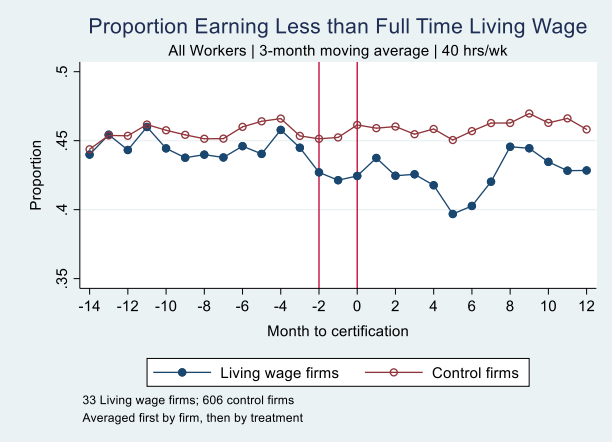
T-Test of Difference in Means		
All Workers	**All workers**	**Potentially affected workers**
	T-statistic	**T-statistic**
Monthly earnings	0.004	0.187
Log monthly earnings	0.307	-0.403
% Earnings less than LW (40 hrs/wk)	-0.238	-0.442
% Earnings less than LW (30 hrs/wk)	-0.329	-0.708
Firm age in months	-1.364	-1.499
Number of employees	-1.630	-1.546
Number of potentially affected employees	-0.614	-0.503
Employee tenure	-1.348	-1.269
Employee age	-0.009	0.008
% Female	-0.033	1.390
% European*	0.684	0.004
% Maori	-0.369	-0.088
% Pacific	-0.205	0.555
% with University**	0.845	0.468
% with High School	-0.631	0.354

Source: Author's calculations.
Underlying statistics computed first by firm, then by treatment status; two-tailed test of differences in means; sample size: 33 living wage firms, 573 control firms

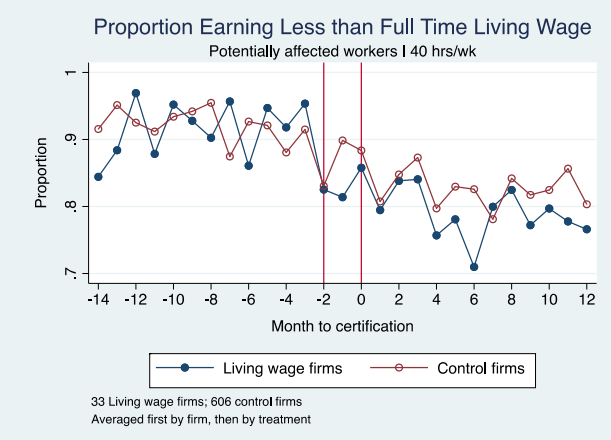
Appendix exhibit 3.16.3 | Log average gross monthly earnings, smoothed

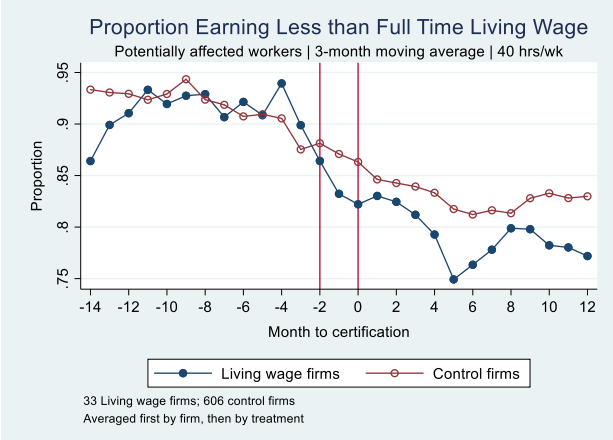
Appendix exhibit 3.16.4 | Proportion of the full sample earning less than the full-time living wage equivalent, based on a 40-hour workweek





Appendix exhibit 3.16.5 | Proportion of potentially affected workers earning less than the full-time living wage equivalent, based on a 40-hour workweek





Appendix exhibit 3.16.6 | Fixed effects estimation of the effects of living wage certification on the prevalence of sub living wage earnings – 40 hour per week threshold – All workers

Fixed-effects estimates of the effect of living wage certification on sub Living Wage Earnings 40 hour per week threshold All Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder
LW Certification	-0.0302*	-0.0438**		-0.0333	-0.0507**		-0.0323	-0.0502**	
	(0.02)	(0.02)		(0.02)	(0.03)		(0.02)	(0.03)	
LW * 6 months pre			0.000597			-0.00934			-0.00522
			(0.02)			(0.02)			(0.02)
LW * Transition period			-0.0417			-0.0627			-0.0599
			(0.03)			(0.05)			(0.05)
LW * Months 1-4			-0.0421*			-0.0676			-0.068
			(0.02)			(0.05)			(0.05)
LW * Months 5-8			-0.0528**			-0.0869			-0.0952
			(0.03)			(0.06)			(0.06)
LW * Months 9-12			-0.0369			-0.0779			-0.0953
			(0.03)			(0.07)			(0.08)
Number of employees		0.00575***	0.00576***		0.00663***	0.00663***		0.00662***	0.00662***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Gaps in employment		0.00218***	0.00217***		0.00250***	0.00250***		0.00271***	0.00271***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
% Female		0.111***	0.111***		0.0804**	0.0804**		0.104***	0.104***
		(0.04)	(0.04)		(0.03)	(0.03)		(0.03)	(0.03)
% European*		-0.014	-0.014		0.00922	0.00909		-0.00552	-0.0058
		(0.04)	(0.04)		(0.05)	(0.05)		(0.05)	(0.05)
% Maori		0.0363	0.0363		0.00697	0.00691		-0.0249	-0.0252
		(0.04)	(0.04)		(0.04)	(0.04)		(0.05)	(0.05)
% Pacific Islander		0.0324	0.0325		0.0496	0.05		0.0682	0.0687
		(0.05)	(0.05)		(0.06)	(0.06)		(0.08)	(0.08)
% with university **		-0.0487	-0.0488		-0.0381	-0.0381		-0.0507	-0.0505
		(0.04)	(0.04)		(0.04)	(0.04)		(0.04)	(0.04)
% with high school**		0.0631*	0.0631*		0.0710**	0.0709**		0.0396	0.0394
		(0.04)	(0.04)		(0.03)	(0.03)		(0.04)	(0.04)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	0.577***	0.418***	0.418***	0.315*	0.290*	0.276	-6.855	141.4**	106.5
	(0.02)	(0.05)	(0.05)	(0.17)	(0.17)	(0.17)	(78.49)	(70.64)	(83.07)
Observations	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300
R-squared	0.782	0.79	0.79	0.813	0.819	0.819	0.825	0.831	0.831
Standard errors adjusted for clusters by firm									
*** p<0.01, ** p<0.05, * p<0.1									
* Workers can claim more than one ethnicity									
** Maximum qualification									
Each time period is coded as a separate treatment variable. There is no omitted group.									

Appendix exhibit 3.16.7 | Fixed effects estimation of the effects of living wage certification on the prevalence of sub-living wage earnings – 40 hour per week threshold – Potentially affected workers

Fixed-effects estimates of the effect of living wage certification on sub Living Wage earnings 40 Hour per week threshold Potentially Affected Workers									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder	lwunder
LW Certification	-0.0325	-0.0269		-0.0630**	-0.0600**		-0.0686**	-0.0655**	
	(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)	
LW * 6 months pre			0.0267			-0.036			-0.0471
			(0.02)			(0.04)			(0.05)
LW * Transition period			0.0000974			-0.115			-0.124
			(0.03)			(0.08)			(0.09)
LW * Months 1-4			-0.0139			-0.163*			-0.152
			(0.03)			(0.09)			(0.09)
LW * Months 5-8			-0.0209			-0.207*			-0.17
			(0.03)			(0.12)			(0.12)
LW * Months 9-12			-0.0144			-0.233			-0.177
			(0.03)			(0.15)			(0.14)
Number of employees		-0.00221***	-0.00221***		-0.00139*	-0.00140*		-0.00140*	-0.00142*
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Gaps in employment		0.00135***	0.00135***		0.00114***	0.00114***		0.00131***	0.00132***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
% Female		0.0162	0.0162		-0.0045	-0.00397		0.00178	0.00245
		(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)
% European*		0.0121	0.0119		-0.029	-0.0294		-0.00984	-0.0101
		(0.05)	(0.05)		(0.04)	(0.04)		(0.04)	(0.04)
% Maori		-0.00941	-0.00927		-0.00911	-0.0091		-0.00513	-0.00546
		(0.03)	(0.03)		(0.04)	(0.04)		(0.04)	(0.04)
% Pacific Islander		0.0185	0.0187		-0.00858	-0.00884		0.0239	0.0235
		(0.03)	(0.03)		(0.04)	(0.04)		(0.04)	(0.04)
% with university **		-0.00212	-0.00223		-0.015	-0.015		0.0144	0.0143
		(0.03)	(0.03)		(0.04)	(0.04)		(0.03)	(0.03)
% with high school**		-0.0335	-0.0333		0.00564	0.00528		0.00431	0.00363
		(0.03)	(0.03)		(0.02)	(0.02)		(0.03)	(0.03)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trent							Y	Y	Y
Constant	0.912***	0.914***	0.915***	1.090***	1.153***	1.113***	811.2***	813.2***	833.0***
	(0.03)	(0.05)	(0.05)	(0.27)	(0.27)	(0.28)	(94.76)	(92.12)	(128.70)
Observations	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500	16,500
R-squared	0.352	0.354	0.354	0.428	0.429	0.429	0.465	0.465	0.465
Standard errors adjusted for clusters by firm									
*** p<0.01, ** p<0.05, * p<0.1									
* Workers can claim more than one ethnicity									
** Maximum qualification									
Each time period is coded as a separate treatment variable. There is no omitted group.									

Appendix exhibit 3.16.8| Complete table of regression output | Worker flow rate | All workers

Fixed Effects Estimates of the Effect of Living Wage Certificaition on Worker Flow Rate All Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	wfr	wfr	wfr	wfr	wfr	wfr	wfr	wfr	wfr
LW Certification	-0.000003	-0.00639		0.00826	-0.00183		0.00786	-0.00199	
	(0.01)	(0.01)		(0.02)	(0.02)		(0.02)	(0.02)	
LW * 6 months pre			-0.0162			-0.00509			0.00986
			(0.01)			(0.02)			(0.03)
LW * Transition period			-0.0236			-0.00777			0.000613
			(0.02)			(0.04)			(0.05)
LW * Months 1-4			-0.0182			0.00769			0.00635
			(0.01)			(0.06)			(0.06)
LW * Months 5-8			0.00601			0.0389			0.0182
			(0.03)			(0.07)			(0.07)
LW * Months 9-12			-0.0246			0.0198			-0.0255
			(0.02)			(0.09)			(0.09)
Number of employees		0.00230**	0.00229**		0.00351***	0.00350***		0.00366***	0.00365***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Gaps in employment		0.00347***	0.00348***		0.00410***	0.00411***		0.00478***	0.00479***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
% Female		-0.00125	-0.00117		0.011	0.0112		0.0226	0.0226
		(0.03)	(0.03)		(0.05)	(0.05)		(0.05)	(0.05)
% European*		-0.113**	-0.112**		-0.115*	-0.115*		-0.108	-0.108
		(0.05)	(0.05)		(0.06)	(0.06)		(0.08)	(0.08)
% Maori		-0.00517	-0.00531		-0.0364	-0.0367		-0.0173	-0.0175
		(0.04)	(0.04)		(0.06)	(0.06)		(0.07)	(0.07)
% Pacific Islander		-0.00404	-0.00466		0.0442	0.043		0.0337	0.0331
		(0.04)	(0.04)		(0.07)	(0.07)		(0.08)	(0.08)
% with university **		-0.0411	-0.0415		-0.0785*	-0.0793*		-0.0997*	-0.1000*
		(0.04)	(0.04)		(0.05)	(0.05)		(0.06)	(0.06)
% with high school**		-0.0489	-0.0492		-0.0901	-0.0906		-0.106	-0.106
		(0.04)	(0.04)		(0.06)	(0.06)		(0.07)	(0.07)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	0.167***	0.226***	0.226***	0.0124	0.126	0.122	-196	-145.4	-173.1
	(0.02)	(0.05)	(0.05)	(0.25)	(0.26)	(0.27)	(126.40)	(122.40)	(148.00)
Observations	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300
R-squared	0.388	0.396	0.396	0.421	0.43	0.431	0.457	0.467	0.467
Standard errors adjusted for clusters by firm									
*** p<0.01, ** p<0.05, * p<0.1									
* Workers can claim more than one ethnicity									
** Maximum qualification									
Each time period is coded as a separate treatment variable. There is no omitted group.									

Appendix exhibit 3.16.9 | Complete table of regression output | Job reallocation rate | All workers

Fixed Effects Estimates of the Effect of Living Wage Certification on Job Reallocation Rate All Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	jrr	jrr	jrr	jrr	jrr	jrr	jrr	jrr	jrr
LW Certification	-0.00015	-0.0015		0.0258	0.0213		0.0299	0.0244	
	(0.01)	(0.01)		(0.02)	(0.02)		(0.03)	(0.02)	
LW * 6 months pre			-0.0277			-0.0201			0.00422
			(0.02)			(0.02)			(0.02)
LW * Transition period			-0.0141			-0.0044			0.0154
			(0.01)			(0.03)			(0.04)
LW * Months 1-4			-0.0179			-0.00106			0.00668
			(0.01)			(0.05)			(0.05)
LW * Months 5-8			-0.000304			0.019			0.000582
			(0.02)			(0.06)			(0.06)
LW * Months 9-12			-0.0296***			-0.00545			-0.0633
			(0.01)			(0.08)			(0.08)
Number of employees		0.000279	0.000266		0.0014	0.00139		0.00175	0.00173
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Gaps in employment		0.00262**	0.00262**		0.00335***	0.00336***		0.00387***	0.00387***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
% Female		0.00509	0.00508		0.0134	0.0137		0.0202	0.0203
		(0.03)	(0.03)		(0.04)	(0.04)		(0.05)	(0.05)
% European*		-0.0577	-0.0575		-0.0572	-0.0569		-0.055	-0.0544
		(0.04)	(0.04)		(0.05)	(0.05)		(0.07)	(0.07)
% Maori		-0.00584	-0.00593		-0.0199	-0.0204		-0.00593	-0.00646
		(0.04)	(0.04)		(0.05)	(0.05)		(0.06)	(0.06)
% Pacific Islander		0.0498	0.0493		0.107	0.106		0.112	0.111
		(0.04)	(0.04)		(0.07)	(0.07)		(0.08)	(0.08)
% with university **		0.00604	0.00574		-0.0404	-0.0411		-0.0504	-0.0507
		(0.03)	(0.03)		(0.04)	(0.04)		(0.05)	(0.05)
% with high school**		-0.0175	-0.018		-0.0402	-0.0408		-0.0443	-0.0443
		(0.03)	(0.03)		(0.05)	(0.05)		(0.07)	(0.07)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	0.118***	0.141***	0.140***	0.253	0.291	0.267	-154.7	-125	-195.1
	(0.02)	(0.04)	(0.04)	(0.24)	(0.25)	(0.25)	(107.00)	(102.90)	(119.50)
Observations	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300
R-squared	0.355	0.359	0.36	0.385	0.391	0.391	0.418	0.426	0.426
Standard errors adjusted for clusters by firm									
*** p<0.01, ** p<0.05, * p<0.1									
* Workers can claim more than one ethnicity									
** Maximum qualification									
Each time period is coded as a separate treatment variable. There is no omitted group.									

Appendix exhibit 3.16.10 | Complete table of regression output | Churning flow rate | All workers

Fixed Effects Estimates of the Effect of Living Wage Certification on Churning Flow Rates All Workers									
	1			2			3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	cfr	cfr	cfr	cfr	cfr	cfr	cfr	cfr	cfr
LW Certification	0.000147	-0.00489		-0.0176	-0.0231		-0.0221	-0.0263	
	(0.01)	(0.01)		(0.02)	(0.02)		(0.02)	(0.02)	
LW * 6 months pre			0.0115			0.015			0.00564
			(0.01)			(0.01)			(0.02)
LW * Transition period			-0.00943			-0.00337			-0.0148
			(0.01)			(0.02)			(0.03)
LW * Months 1-4			-0.000361			0.00876			-0.000334
			(0.01)			(0.03)			(0.03)
LW * Months 5-8			0.00632			0.0199			0.0176
			(0.01)			(0.04)			(0.04)
LW * Months 9-12			0.00503			0.0252			0.0378
			(0.01)			(0.05)			(0.05)
Number of employees		0.00202***	0.00203***		0.00211**	0.00211**		0.00191*	0.00192*
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Gaps in employment		0.000850**	0.000853**		0.000743	0.000745		0.000914*	0.000916*
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
% Female		-0.00634	-0.00625		-0.00243	-0.0025		0.00241	0.00228
		(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)
% European*		-0.0550**	-0.0550**		-0.0577*	-0.0577*		-0.0534	-0.0533
		(0.02)	(0.02)		(0.03)	(0.03)		(0.04)	(0.04)
% Maori		0.000671	0.000619		-0.0165	-0.0164		-0.0114	-0.011
		(0.02)	(0.02)		(0.02)	(0.02)		(0.03)	(0.03)
% Pacific Islander		-0.0538*	-0.0540*		-0.0625	-0.0627		-0.078	-0.0781
		(0.03)	(0.03)		(0.05)	(0.05)		(0.05)	(0.05)
% with university **		-0.0472**	-0.0472**		-0.0381	-0.0382		-0.0493	-0.0493
		(0.02)	(0.02)		(0.03)	(0.03)		(0.04)	(0.04)
% with high school**		-0.0314	-0.0312		-0.0499	-0.0499		-0.0622	-0.0621
		(0.03)	(0.03)		(0.03)	(0.03)		(0.04)	(0.04)
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm specific linear time trend				Y	Y	Y	Y	Y	Y
Firm specific quadratic time trend							Y	Y	Y
Constant	0.0485***	0.0851***	0.0855***	-0.240*	-0.165	-0.145	-41.33	-20.39	21.94
	(0.01)	(0.03)	(0.03)	(0.14)	(0.14)	(0.14)	(77.05)	(75.87)	(88.57)
Observations	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300	17,300
R-squared	0.234	0.241	0.241	0.286	0.291	0.291	0.328	0.332	0.332
Standard errors adjusted for clusters by firm									
*** p<0.01, ** p<0.05, * p<0.1									
* Workers can claim more than one ethnicity									
** Maximum qualification									
Each time period is coded as a separate treatment variable. There is no omitted group.									

CHAPTER 4: WHO BENEFITS FROM THE LIVING WAGE?

Disclaimer:
The results in this thesis are not official statistics, they have been created for research purposes from the Integrated Data Infrastructure (IDI) managed by Statistics New Zealand.

The opinions, findings, recommendations and conclusions expressed in this thesis are those of the author not Statistics NZ or Victoria University.

Access to the anonymised data used in this study was provided by Statistics NZ in accordance with security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person, household, business or organisation and the results in this thesis have been confidentialised to protect these groups from identification.

Careful consideration has been given to the privacy, security and confidentiality issues associated with using administrative and survey data in the IDI. Further detail can be found in the Privacy impact assessment for the Integrated Data Infrastructure available from www.stats.govt.nz.

The results are based in part on tax data supplied by Inland Revenue to Statistics NZ under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information may be published or disclosed in any other form, or provided to Inland Revenue for administrative or regulatory purposes.

Any person who has had access to the unit-record data has certified that they have been shown, have read, and have understood section 81 of the Tax Administration Act 1994, which relates to secrecy. 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.

4.1 ABSTRACT

In this paper, I investigate changes in earnings for living wage employees using a worker-level panel dataset following living wage and control employees as they move between jobs. This expanded dataset allows me to explore the distribution of benefits from living wage certification. To do this, I compare earnings gains between workers hired before certification ('pre-hires'), those hired after certification ('joiners') and those that eventually leave their living wage job and seek new employment ('leavers'). I find that while both treated and control workers experience earnings growth, this growth is slower in living wage firms than in control firms, a finding that may have resulted from annual increases in the living wage rate that were below industry averages and/or from the concentration of wage gains in small firms that employ relatively few workers.[28] Workers hired before certification have virtually no earnings advantage over workers in matched control firms; rather the entire earnings benefit of living wage certification accrues to workers hired after certification. Regardless of when a worker is hired, workers who left their living wage jobs were those who received the smallest benefit from the living wage, reinforcing the idea that low earnings are an important driver of employee quits.

4.2 INTRODUCTION

The goal of this paper is to determine which groups of employees derive the largest benefit from living wage certification and to elaborate on the patterns that I uncovered in my earlier work. I differentiate between workers by time of hire, not household structure. This focus reflects a desire to understand the degree to which living wage certification offers employers a hiring advantage in the workforce but will provide little insight into the ways in which the living wage impacts *household* earnings. While the latter is important when assessing the impact of the living wage on poverty, the former will let us know whether the benefit of the living wage accrues to low-wage workers or to more skilled workers drawn by higher wages to fill entry level jobs. The application of this work to our understanding of poverty is therefore limited in that I cannot discern single adults from sole parents; I similarly cannot identify the number of earners in a household nor can I see their dependents. Instead, this paper seeks to answer a much simpler, but necessary question:

[28] This finding appears to contradict the findings from my earlier analyses. For a reconciliation of these differences, see Appendix II.

what is the impact of living wage certification on the gross income of workers, and is there evidence that newly hired workers are more skilled or better qualified than the workers they replace? If employers substitute toward workers with more education or experience, the earnings increase generated by the living wage will be diffused and low-income workers will not benefit.

This concern has been validated by earlier research (Fairris & Bujanda, 2008) and the success of the living wage movement hinges, in part, on whether workers hired into living wage firms would have been able to command higher wages in alternate employment. My results suggest that they could not. In fact, I find that wage gains are concentrated among workers hired after certification, evidence that living wage employment represents a real gain to a band of low-wage earners. While telling us that these higher wages are not lost on workers with more skill, it does not tell us how these extra earnings are divided between workers and the government. I cannot see how much of the windfall is absorbed by government and how much accrues to the employee. Existing work by the Treasury shows that a movement from the minimum wage to the living wage will improve the take-home earnings of workers in all household types (Galt & Palmer, 2013). However, because of steep abatement rates in means-tested benefits, households with children will receive a much smaller increase in household pay than will childless households. Because of this, the groups that stand to benefit most from the living wage are not the groups that the movement had originally hoped to help (Perry, 2019). The presence of this wedge between gross earnings and take-home income severely limits the poverty reduction capabilities of the living wage (Boston & Chapple, 2014). Therefore, the living wage must be conceptualised, not as a cure for in-work poverty but as a step toward promoting income adequacy by encouraging employers to pay higher wages when their circumstances allow.

This paper presents two key findings. First is that the benefit of the living wage accrues almost exclusively to workers hired after certification. Second, I find that overall earnings gains for living wage workers lag behind those in control firms. I attribute this surprising finding to annual updates of the living wage rate that underestimate earnings growth in relevant industries and attempts by firms to contain costs by compressing wages. These changes are made possible by imperfect information and labour market frictions that make it difficult for employees to know their economic value and disincentivises changing jobs. To motivate my work, I rely on the theory of monopsonistic labour markets as discussed by Brofenbrenner (1956) and popularized by Alan

Manning (2003). This theory can help explain how firms that chose living wage certification as a means to provide workers with an above-market wage can end up inadvertently offering wages that fail to keep up with the competition.

Monopsony power accrues to employers when frictions and imperfect information are present (Burdett & Mortensen, 1998). Labour market frictions represent the actual and perceived costs to employees of changing jobs, and such frictions reduce workers' propensity to seek alternate employment. In addition to frictions, workers have incomplete information about external job opportunities and their own earning potential. The result of these forces is that employers possess a degree of market power when hiring workers, setting wages and negotiating pay increases. In a competitive labour market, wages are set such that each employee receives compensation that equals the marginal revenue generated for the firm by the employees' work. In monopsony, this marginal revenue product of labour is unknown or disregarded and employers rely on rules or industry benchmarks to determine levels of pay (Brofenbrenner, 1956). Qualitative research on the New Zealand living wage indicates that one outcome of living wage certification is a reduced reliance on established industry benchmarks for wages and raises. Instead, firms report relying on the published living wage rate as an indication of competitive market wages (Stansfield, 2017). In a labour market characterised by imperfect information on earnings and outside job opportunities, workers may similarly rely on external signals of fairness.

In labour markets characterised by imperfect information on wages and job opportunities, living wage certification may act as a signal in the market about a firm's relative level of pay. However, my research suggests that this signal may, in fact, be unreliable. My findings indicate that earnings for living wage employees lagged behind those of control employees. Between 2013 and 2017, average monthly earnings in control firms increased by 3.39% per year, on average, compared to 2.79% per year in living wage firms. Over the same time period, the published living wage rate increased by an average of 1.8% per year. Additionally, while some groups of workers do gain financially following the implementation of the living wage, the benefit accrues nearly exclusively to employees hired after certification. Workers employed for living wage firms before certification received virtually no increase in their monthly gross earnings.

4.3 DATA

My data comes from the Integrated Data Infrastructure (IDI) created and maintained by Statistics New Zealand. The IDI contains administrative and survey data on firms and individuals that have been collected by a variety of government agencies. Information from different agencies can be linked by individual or firm identification number, allowing researchers to compile rich datasets using information from a number of sources. The dataset used in this study links worker earnings information from Inland Revenue with business data from the Business Registry. Educational information is provided by the Ministry of Education and demographic information is compiled by Statistics New Zealand. I was assisted in my research by Living Wage Aotearoa who provided names and certification dates for the 48 living wage firms certified before December 2015. Staff at Statistics New Zealand was then able to identify and flag 45 of these firms in the IDI. Through an identifying 'living wage' marker, I was able to link data on employers to the earnings, demographic and education data of workers.

4.4 SAMPLE AND VARIABLES

This paper uses an unbalanced worker-level panel dataset containing monthly earnings information for the 15,900 employees that worked for a living wage or matched control firm at any time between January 2014 and December 2015.[29] In total, my sample contains the full earnings history from all jobs on 2,100 living wage and 13,800 control employees for 49 months spanning 24 months pre-treatment to 24 months post-treatment.

A worker is defined as treated when they become employed for a certified living wage employer. For workers employed in a living wage firm before certification ('pre-hires'), treatment occurs when the firm becomes certified. For workers hired by a living wage firm after certification ('joiners'), treatment occurs in the month that they join the living wage firm. Control firms are assigned the same date of certification as their matched living wage firm and control employees are designated as 'pre-hires' or 'joiners' using the same criteria applied to living wage employees. All living wage firms in my sample remain certified throughout the observation period so workers remain treated until they change jobs. In Table 4.1, I present the raw counts of workers by treatment

[29]For information about the selection and matching of living wage and control firms, see the second paper in this thesis.

category, separated by time of hire and whether they left their job with the living wage/control firm and took other employment. Workers are classified as pre-hires or joiners based on time of hire and workers from either category that leave their job are designated as 'leavers'. Table 4.2 presents descriptive statistics on earnings, gender and education based on weighted counts. For this table, I weight observations in the control group by 1/k, where k is the number of control matches for each living wage firm. Applying these weights produces descriptive statistics that are more representative of the sample; weights are not used in the estimation of regression models.[30]

Figure 4.1: Definitions and explanation of terms

Terminology and definitions	
Pre-Hire	Worker hired before the firm (or its matched living wage firm) was certified.
Joiner*	Worker hired after certification.
Stayer*	A worker hired before certification who does not leave the firm for other employment. Stayers remain in their living wage/control job for the remainder of their time in the data.
Leaver*	Worker who leaves the treated or control firm for other employment. May be hired pre- or post-certification.
Potentially affected worker	Employees who earned less than the full-time equivalent living wage, based on a 40-hour work-week, on average, in the year before treatment.
Living wage employee	An employee who worked for a certified living wage firm at some point between January 2014 and December 2015. These workers are identified with a living wage dummy throughout my sample regardless of their place of employment in any given month.
Control employee	A worker who was employed for a matched control firm at some point during the 2014-2015 calendar year. These workers are identified with a control worker dummy throughout the analysis regardless of their place of employment in a particular month.
Log monthly earnings	Natural log of total monthly earnings from all jobs.

*Note: Variables marked with * were coined by Farris and Bujanda (2008)*

In my sample, 35% of living wage workers and 38% of control employees earn less than the full-time living wage equivalent in any given month. However, a much larger proportion earned less than the full-time living wage equivalent—on average—in the year before certification, a figure that can be seen in the fourth line of Table 4.1. Waldegrave, King, and Urbanova (2018) estimate

[30] The inclusion of weights does not impact the regression estimates. I report only the results of unweighted regressions in this paper.

that roughly one-third of New Zealand workers earn less than the hourly living wage rate, an estimate that is broadly consistent with the earnings data for workers in my sample. Also, most workers in my sample (73%) were hired before certification, with 27% being newly hired in the last two years of observation. Comparisons of descriptive statistics for living wage and control firms reveal similarities between living wage and control employees. In general, workers hired before certification tend to be better paid, more highly educated and more experienced than new hires, differences consistent with both the replacement of retired workers with younger and less experienced employees and with higher turnover among workers earlier in their careers.

Table 4.1 | Worker counts by treatment status and group

Sample Counts		
	Living Wage	Control
Total Workers	2100	13800
% of sample	*100%*	*100%*
Pre-Hires	1600	10000
% of sample	*76%*	*72%*
Joiners	500	3800
% of sample	*24%*	*28%*
Potentially Affected Workers	830	8800
% of sample	*40%*	*64%*
Leavers	1100	9000
% of sample	*52%*	*65%*

Raw, unweighted counts; rounded as required to protect confidentiality; leavers are workers that left their living wage / control job and found other employment. They may have been hired before or after certification; potentially affected workers are those who earned less than the full time living wage equivalent in the year before treatment, regardless of when they were hired and whether they stayed in their job.

Source: Author's calculations

4.5 METHODOLOGY

The purpose of this paper is to investigate the distribution of benefits from the living wage. To do this, I estimate changes in earnings for living wage workers based on time of hire and whether they eventually left their job. Throughout the analysis, I stratify the sample by time of hire (pre-hires

and joiners) and then use interaction terms to isolate the effect on potentially affected workers and leavers.[31]

To investigate changes in total monthly earnings following living wage certification, I use five models that each contain fixed effects for firm, time and worker. The first model identifies the overall treatment effect, models two & three test for differences in treatment for potentially affected workers and leavers. The fourth model examines pre-treatment differences between treated and control firms and the last model estimates differences in treatment effect by firm size. For the full sample and the sub-sample of prehires, I run each model twice, once with firm-specific linear time trends and once without.[32] The inclusion of linear time-trends separates the treatment effect from differences in existing trends in earnings growth between living wage and control firms; this helps separate the effect of living wage certification from differences in firm-level trajectories in earnings and employment.[33] When looking at the impact on joiners, who by definition change employers at the time of certification, the inclusion of firm-specific time trends does not help identify the treatment effect. Therefore, I exclude firm-specific time trends in my analysis of joiners.

My first model includes a single treatment variable and fixed effects for worker, firm and time. This model can be represented as:

(1) $Y_{it} = \alpha + \beta_1 LWE_{ift} + \gamma Size_{ft} + \lambda_i + \lambda_f + \lambda_t + e_{ift}$

In this model, Y_{it} is the variable of interest-log gross monthly earnings for worker *i* from all jobs in month *t*. Next, LWE_{it} is the treatment variable that equals one if worker *i* is employed in a certified living wage firm, *f*, in month *t*. $Size_{ft}$ controls for firm size as this varies over time and is not captured by the firm fixed-effect. Individual, firm and calendar month fixed effects are

[31] I chose this approach because of sample size restrictions.
[32] I experimented with the inclusion of firm-specific quadratic time trends but they did not alter the results. For simplicity, I present only results using firm-specific linear time trends.
[33] I experimented with three additional specifications. The first is a simple ols regression on earnings for treated firms without the inclusion of a control firm. The second utilizes calendar month/year * treatment dummies, a specification that allows for a non-linear time effect for treated firms. The last includes treatment-specific rather than firm-specific time trends. The results of these regressions are presented with the robustness checks at the end of this chapter and do not alter the primary findings of the paper.

captured by λ_i, λ_f and λ_t, respectively and e_{it} is an idiosyncratic error term.[34] Standard errors are adjusted for cluster by firm. The treatment effect is captured by β_1 and represents the log percent change in average earnings for living wage workers following treatment.

Table 4.2 | Table of descriptive statistics

Descriptive Statistics by Treatment Status and Employee Type								
	All		Prehires		Joiners		Leavers	
	LW	Control	LW	Control	LW	Control	LW	Control
% Earning less than LW	0.353	0.383	0.285	0.360	0.411	0.346	0.401	0.395
standard deviation	*(0.48)*	*(0.49)*	*(0.45)*	*(0.48)*	*(0.49)*	*(0.48)*	*(0.49)*	*(0.49)*
Gross monthly earnings	4400	4300	4800	4500	3700	4200	3800	4100
	(2700)	*(2800)*	*(2800)*	*(2900)*	*(2500)*	*(2900)*	*(2500)*	*(2800)*
Maximum Qualification	2.34	2.47	2.43	2.46	2.18	2.49	2.19	2.41
	(1.42)	*(1.45)*	*(1.45)*	*(1.42)*	*(1.31)*	*(1.47)*	*(1.35)*	*(1.40)*
Experience at Hire (Months)	133	121	141	128	111	109	124	111
	(57)	*(60)*	*(53)*	*(56)*	*(61)*	*(65)*	*(60)*	*(61)*
Age	45.7	42.3	47.9	44.5	40.3	38.2	42.8	38.6
	(13.5)	*(14.2)*	*(12.7)*	*(14.0)*	*(13.8)*	*(13.1)*	*(13.9)*	*(13.4)*
% Female	0.558	0.593	0.565	0.635	0.572	0.549	0.548	0.573
	(0.50)	*(0.49)*	*(0.50)*	*(0.48)*	*(0.49)*	*(0.50)*	*(0.50)*	*(0.49)*
% European	0.720	0.743	0.759	0.761	0.615	0.706	0.639	0.725
	(0.45)	*(0.44)*	*(0.43)*	*(0.43)*	*(0.49)*	*(0.46)*	*(0.48)*	*(0.45)*
% Maori	0.201	0.173	0.172	0.159	0.261	0.187	0.260	0.192
	(0.40)	*(0.38)*	*(0.38)*	*(0.37)*	*(0.44)*	*(0.39)*	*(0.44)*	*(0.39)*
% Pacific	0.109	0.076	0.087	0.082	0.165	0.075	0.139	0.081
	(0.31)	*(0.27)*	*(0.28)*	*(0.27)*	*(0.37)*	*(0.26)*	*(0.35)*	*(0.27)*

Weighted averages. Workers can report more than one ethnicity; frequency weights used to account for differences in number of living wage matches; standard deviation in parenthesis; Maximum qualification: 1=High School, 2=Graduate certificate or diploma, 3=Bachelors degree, 4=Postgraduate certificate, diploma or honours, 5=Advanced degree. Rounding applied as required to protect confidentiality.

Source: Author's calculations.

My goal in this paper is to uncover the differences in living wage impact for workers based on their time of hire and by their eventual propensity to quit. To identify differences in effects between groups, I add two interaction terms to the model. Model (2) adds an interaction term, $LWE_{ift} * PAW_i$, that identifies potentially affected living wage employees. Workers are defined as potentially affected if they earned less than the full-time living wage equivalent in the year before certification.[35] Model (3) seeks to estimate the difference in living wage treatment for those workers who eventually leave their jobs and find other employment. The variable L_i identifies

[34] Fixed effects are included for each of the living wage and control firms; all other firms are grouped together under a single fixed effect.

[35] For this indicator, I define full time as 40 hours of work per week.

these leavers and the interaction term $LWE_{ift} * L_i$ identifies leavers during the time that they were employed for a living wage firm[36]. These specifications are:

(2) $Y_{it} = \beta_1 LWE_{ift} + \beta_2(LWE_{ift} * PAW_i) + \gamma Size_{ft} + \lambda_i + \lambda_f + \lambda_t + e_{it}$

(3) $Y_{it} = \beta_1 LWE_{itf} + \beta_2(LWE_{ift} * L_i) + \gamma Size_{ft} + \lambda_i + \lambda_f + \lambda_t + e_{it}$

The effect on the group identified by the interaction term can be calculated as $\beta_1 + \beta_2$, while β_1 represents the effect on the omitted group. When the interaction term identifies potentially affected workers, the omitted group includes workers who earned more than the living wage in the year leading up to certification. Similarly, when leavers are identified by the interaction term, stayers are the omitted group. Standard errors are clustered at the firm level.

My fourth specification tests for dynamic treatment effects by including time-specific treatment variables. By including treatment variables for both the pre- and post-treatment periods, I am able to test for both pre-treatment differences between groups and investigate treatment dynamics. An absence of differences in earnings between living wage and control employees before treatment lends support to the common trend assumption. I do not include a separate treatment variable (LWE_{ift}) so there is no excluded time group.

(4) $Y_{it} = \beta_1 T_{-2}^{LW} + \beta_2 T_{-1}^{LW} + \beta_3 T_0^{LW} + \beta_4 T_1^{LW} + \beta_5 T_2^{LW} + \gamma Size_{ft} + \lambda_i + \lambda_f + \lambda_t + e_{ift}$

In this specification, the pre-treatment variables T_{-1}^{LW} and T_{-2}^{LW} are equal to one for living wage employees in the first and second pre-treatment years respectively. During this time, workers employed in living wage firms (prehires) are not yet treated and should not experience changes in earnings relative to their own histories or the earnings of control workers. The presence of common trends is less important for joiners. Treatment happens at the firm level so while common pre-treatment trends in outcome variables for living wage and control *firms* are important, there is no reason to assume that the workers hired into these firms share common trends *before* they are hired. Therefore, among the full sample and the sub-sample of joiners, I do not report the coefficient on the pre-treatment variables. The variable T_0^{LW} is equal to one for treated living wage employees in the first three months of treatment. This transition period is useful at capturing the

[36] $L_i = 0$ for workers that either stayed employed with their living wage/control firm or left the workforce.

transition into living wage status for prehires[37] as well as the period of temporarily low earnings that characterises the first few months of earnings for joiners. Variables T_1^{LW} and T_2^{LW} are dummy variables equal to one for treated living wage employees in the first and second year after they become subject to the living wage, respectively. Coefficients on these variables will reveal any differences in the effect of the living wage effect over time.

The final specification seeks to identify differences in treatment effect across firms of different sizes. I include three treatment dummy variables, one each for small (10 or fewer employees), medium (11-50 employees) and large (50+ employees) firms.

$$(5)\ Y_{it} = \beta_1 S_{it}^{sm} + \beta_2 S_{it}^{med} + \beta_3 S_{it}^{lg} + \gamma Size_{ft} + \lambda_i + \lambda_f + \lambda_t + e_{it}$$

A worker that is treated in month *t* will have a dummy variable equal to 1 for either S_{it}^{sm}, S_{it}^{med} or S_{it}^{lg}. Control employees will have dummy variables equal to 0 for all size categories. Variable β_1 measures the treatment effect on employees of small firms, β_2 captures the treatment effect on employees in mid-size firms and β_3 estimates the treatment effect on employees of large firms relative to both treated living wage employees in firms of other sizes, untreated living wage employees and control workers.

4.6 RESULTS

For workers hired before certification, there does not appear to be a financial benefit to employment in a living wage enterprise.[38] In fact, when earnings are analysed at the worker level, it becomes apparent that for the majority of workers hired by living wage firms prior to certification, growth in monthly earnings does not keep up with that that of control workers.

Plots of earnings over time for prehires in living wage and control firms illustrate the differences in earnings growth rates for living wage and control employees over the sample period (Figure 4.1). The left-hand panel plots unweighted earnings, while the right-hand panel shows earnings

[37] Living Wage Aotearoa New Zealand requires firms to become compliant with the wage requirements of the living wage *before* applying for certification. The certification process takes five weeks on average so it is likely that firms become compliant with the living wage one-two months before their date of certification.

[38] For a discussion of the differences between this dataset and the dataset used in the last chapter,as well as information on the full sample of workers, see Appendix II.

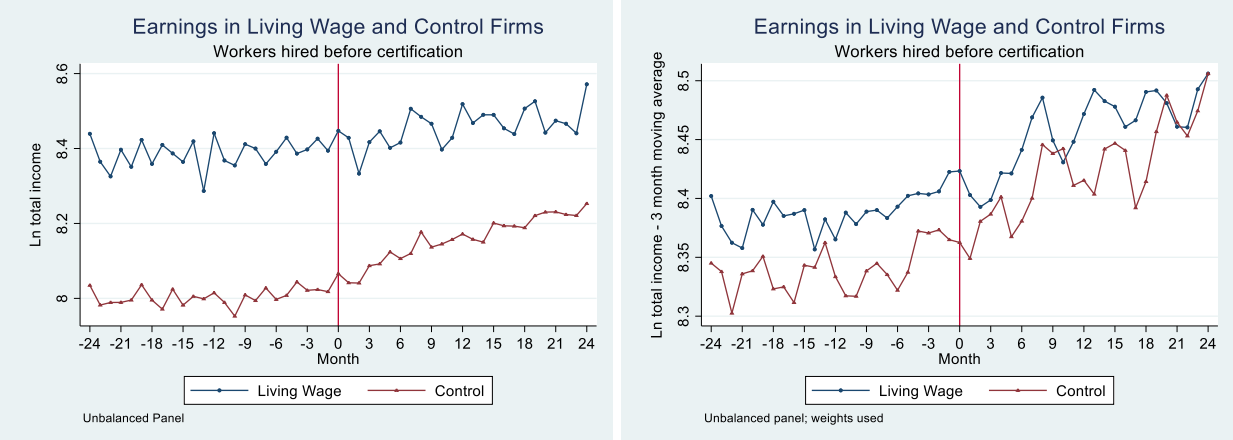
with frequency weights applied. In both plots, living wage workers have an earnings advantage before certification. This advantage persists but narrows following certification, a result that is confirmed by regression results. A comparison of unconditional mean earnings of prehires in living wage and control firms provides context. Estimates of the average level of earnings among living wage and control workers presented in Table 4.3 reveal that earnings for living wage prehires increased from $4,689 per month in 2013 to $5,188 per month in 2017. This represents an overall increase of 10.6%, or an annualized increase of 2.6%. For prehires in control firms, earnings rose from $4,368 per month in 2013 to $4,964 per month in 2017, an overall increase of 13.6% or 3.25% per year, suggesting that on average, earnings grew faster for control employees.

Table 4.3 | Comparison of earnings growth 2013 to 2017

Growth in Earnings by Subsample in Living Wage and Control Firms						
	All Employees		Prehires		Joiners	
	LW	Control	LW	Control	LW	Control
2013 Average monthly earnings	4223	4183	4689	4368	3786	4380
(standard deviation)	*(2698)*	*(2921)*	*(2723)*	*(2876)*	*(2489)*	*(2944)*
2017 Average monthly earnings	4714	4781	5188	4964	4284	4679
	(2901)	*(3052)*	*(2878)*	*(3058)*	*(2638)*	*(2874)*
Total increase (2013-2017)	11.63%	14.29%	10.64%	13.64%	13.16%	6.82%
Annualized increase	2.79%	3.39%	2.56%	3.25%	3.14%	1.66%
Weights used; counts rounded as required to protect confidentiality						

Source: Author's calculations

Figure 4.2 | Log earnings by month | Prehires



Source: Author's calculations

Table 4.4 | Fixed effects estimates | Log earnings | Pre-hires

Fixed Effects Estimtates of the Effect of Living Wage Certification on Log Total Monthly Earnings from all Jobs Pre-hires										
	(1)		(2)		(3)		(4)		(5)	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat	-0.110***	0.0073	-0.107***	0.00941	-0.105***	0.0278*				
	(0.04)	(0.01)	(0.03)	(0.01)	(0.04)	(0.01)				
Treat * potentially affected			-0.0102	-0.00707						
			(0.03)	(0.02)						
Treat * leaver					-0.0203	-0.0649*				
					(0.04)	(0.04)				
Treat * 1st observation year							0.154	0.0518		
							(0.17)	(0.19)		
Treat * 2nd observation year							0.0966	0.0742		
							(0.17)	(0.18)		
Treat * transitional period							0.0436	0.0637		
							(0.17)	(0.17)		
Treat * 1st year post							0.0409	0.099		
							(0.18)	(0.18)		
Treat * 2nd year post							-0.0398	0.0913		
							(0.18)	(0.17)		
Treat * small									-0.0505	0.0367
									(0.06)	(0.06)
Treat * medium									-0.0397	0.0471**
									(0.03)	(0.02)
Treat * large									-0.142***	-0.0377
									(0.05)	(0.02)
Firm size	4.17e-05*	3.64e-05*	4.17e-05*	3.64e-05*	4.17e-05*	3.64e-05*	4.14e-05*	3.64e-05*	4.17e-05*	4.17e-05*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	7.794***	6.194***	7.794***	6.194***	7.794***	6.194***	7.787***	6.192***	7.793***	6.851***
	(0.07)	(0.18)	(0.07)	(0.17)	(0.07)	(0.18)	(0.07)	(0.18)	(0.07)	(0.26)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Worker fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Matched control group	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-specific linear time trends		Y		Y		Y		Y		Y
Observations	432,500	432,500	432,500	432,500	432,500	432,500	432,500	432,500	432,500	432,500
R-squared	0.702	0.707	0.702	0.707	0.702	0.707	0.703	0.707	0.703	0.707

*Note: Standard errors in parenthesis (adjusted for cluster by firm); *** p<0.01, ** p<0.05, * p<0.1; Firm size: Small (0-10 employees) Medium (11-50 employees) Large (50+); First and second observational year refer to pre-treatment time periods.*

Regression estimates confirm the significance of the differences noted in the graph and in the comparison of unconditional means. Estimates of the living wage effect on earnings for the prehires are shown in Table 4.4. Each of the five panels represents one of the regression specifications discussed in the methodology section and the results in the right-hand column of each panel include firm-specific linear time trends. The first result to note is the difference that the inclusion of firm-specific linear time trends makes to the coefficients. In all specifications, the addition of firm-specific time trends alters the results. In panel 1, for instance, without time trends,

the regression estimates indicate that living wage prehires suffered a 10.0% relative reduction in earnings following certification. However, when firm-specific linear time trends are included, the coefficient becomes positive, statistically insignificant and close to zero. These differences do not reflect a problem in the identification strategy; rather they illuminate one of the key findings related to the living wages' impact on earnings: relative to a matched group of control workers, earnings for prehires in living wage firms decline over the observation period. However, in comparison to the overall pattern of earnings growth for individual living wage firms, there is virtually no change in earnings for workers hired before certification. I do not attribute this to maliciousness on the part of living wage employers but rather to the reliance on the published annual increase in the living wage which has understated overall earnings growth in the relevant sub-section of the job market. Relative to the 1.8% annualized benchmark, living wage firms are more than competitive with pay but basing increases in the living wage on national average movements in wages has understated growth in relevant sectors of the job market.

To identify the effects on workers who earned less than the full-time living wage equivalent, I add an interaction term to the basic specification. These results are presented in panel 2. The pattern for the main treatment effect in this specification mirrors that from the first specification. When firm-specific linear time trends are excluded, earnings for prehires to living wage firms show a 10.5 log percent decline relative to the earnings of control firm pre-hires but when linear time trends are added the coefficient falls to zero and becomes statistically insignificant. The coefficient on the interaction term in both specifications is close to zero and statistically insignificant. Taken together, this indicates that the treatment effect for potentially affected workers hired before certification is zero. Low-paid workers hired before living wage certification do not experience higher earnings after their employer becomes a certified living wage firm. However, this zero treatment effect occurs in an employment landscape of negative relative earnings growth. The certification process requires that employers raise wages of workers before applying for certification, so the absence of change is not driven by non-compliance. Additionally, firms applying for certification must agree not to cut employee hours in order to offset the costs of certification. However, informal interviews with living wage employers and feedback from seminar participants suggest that living wage employees may display income targeting behaviour when their employers become certified. By reducing the number of hours worked, employees can meet their income goals while spending more time engaged in other pursuits such as leisure or

time with family. The absence of a clear treatment effect among previously low-paid workers may reflect such behaviour and is an area for future research.

The only specification in which living wage pre-hires have an earnings advantage is with the inclusion of time trends in panel 3, where I separate workers by their eventual propensity to quit. Here the interaction term is $LWE_{it} * L_i$, which captures the treatment effect for leavers, the group of workers who eventually quit and find other employment. The omitted group—stayers—includes those workers hired before certification who remain employed for the living wage firm throughout the entire period of observation. When time trends are excluded, the coefficients suggest the same pattern seen with potentially affected workers, a 10.5% loss in earnings with no difference for the group identified with the interaction. However, when firm-specific linear time trends are added, we see that earnings increased for stayers by a weakly significant 2.8%, while falling for leavers by 3.7%, implying that workers who switch jobs following living wage certification were those for whom the living wage provided the least benefit.

Panel 4 tests for dynamic treatment effects through the inclusion of 12-month time-specific treatment variable for each 12-month period in the sample, as well as a 3-month time-specific treatment variable to identify change in treatment occurring during the transition period to living wage. The absence of statistically significant coefficients during the pre-treatment periods lend support to the common trend assumption and indicate that relative changes in the rate of growth between treated and control firms occurs in the two years after certification. In the sample of prehires, the coefficients on all time variables are insignificant, suggesting that the treatment effect does not vary over time. The absence of differences in earnings between living wage and control firms in the pre-treatment period lends support to the common trend assumption.

Estimates of differences in treatment effects for firms of different sizes are presented in panel 5. When time trends are excluded, there is strong evidence of a 14.2 log percent decline in earnings for employees of the largest living wage firms. Large living wage firms make up a small minority of firms but employ most workers so this coefficient tells us that the relative decline in earnings affects a majority of workers. When firm-specific linear time trends are included, the coefficients for small and medium firms become negative and only the coefficient for medium firms is statistically significant, indicating that relative to the overall pattern in earnings growth within their firms, employees of mid-size living wage employers received a 4.7% increase in earnings after

living wage certification. The patterns in column 5 explain part of the discrepancy between the findings of this paper and those in my earlier work that focused on firm-level changes in average monthly earnings. In that paper, all firms received equal weighting regardless of size. In this paper, which focuses on worker-level analyses, large firms receive more weight because they impact the experience of a larger number of workers.

Table 4.5 | Fixed effects estimates | Log earnings | Joiners

Fixed Effects Estimtates of the Effect of Living Wage Certification on Log Total Monthly Earnings from all Jobs Joiners					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	log_e	log_e	log_e	log_e	log_e
Treat	0.352**	0.149	0.587***		
	(0.15)	(0.15)	(0.18)		
Treat * potentially affected		0.501***			
		(0.07)			
Treat * leaver			-0.425***		
			(0.11)		
Treat * 1st observation year				-0.0361***	
				(0.01)	
Treat * 2nd observation year				-0.113***	
				(0.01)	
Treat * transitional period				0.178	
				(0.17)	
Treat * 1st year post				0.407***	
				(0.14)	
Treat * 2nd year post				0.325**	
				(0.15)	
Treat * small					0.0945
					(0.21)
Treat * medium					0.433***
					(0.17)
Treat * large					0.315*
					(0.16)
Firm size	0.0000109	0.0000105	0.0000111	0.00000954	0.0000109
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	7.504***	7.504***	7.503***	7.506***	7.504***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Firm fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
Worker fixed effects	Y	Y	Y	Y	Y
Matched control group	Y	Y	Y	Y	Y
Firm-specific linear time trends					
Observations	129,800	129,800	129,800	129,800	129,800
R-squared	0.633	0.634	0.633	0.634	0.633

*Note: Standard errors in parenthesis (adjusted for cluster by firm); *** p<0.01, ** p<0.05, * p<0.1; Firm size: Small (0-10 employees) Medium (11-50 employees) Large (50+); First and second observational year refer to pre-treatment time periods.*

Unlike pre-hires, joiners experience a clear and significant boost in monthly earnings following their move into a living wage organisation. Joiners as a group receive a wage boost but the greatest impact can be found among potentially affected joiners and joiners who stay in their jobs. The magnitude of the coefficients suggest that joiners receive both a pay raise and an increase in hours in their living wage jobs.

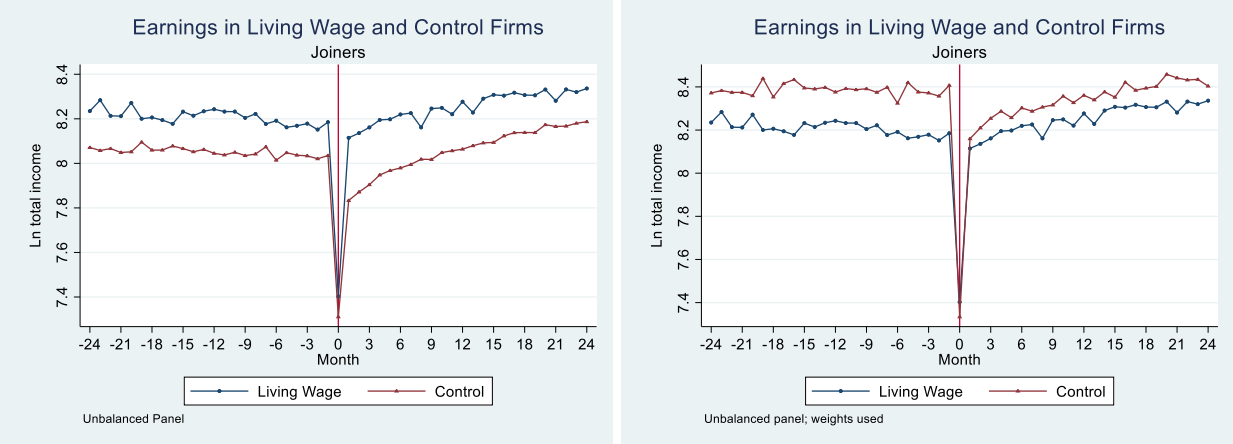
Regression estimates for joiners are presented in Table 4.5. Because joiners, by definition, change jobs at certification, the inclusion of firm specific time trends will not help identify the treatment effect. Before joining living wage firms, the 500 living wage joiners in my sample had earnings histories that included employment spells with over 30,000 different employers. As such, I present regression results for joiners without firm-specific time trends (Table 4.5).[39] The first column presents the results of my basic specification and shows a statistically significant 35.2 log percent increase in earnings for joiners to living wage firms. We can interpret this as the conditional post-treatment increase in earnings for living wage joiners relative to their average earnings over the observation period.

Specifications (2) and (3) add interaction terms to separate the treatment effects for potentially affected workers and for those who leave their employment with living wage firms. The results in these columns reveal two key findings of this paper. First, the earnings benefit of living wage certification accrues primarily to joiners who had low earnings in the year before certification. Previously low-paid workers experienced a 50.1 log percentage increase in monthly earnings, a substantial boost. This boost in earnings for low-income workers is consistent with the goal of the living wage movement and supports the movements claims that the living wage raises earnings of low-wage workers. However, the benefit of higher earnings was concentrated among low-earning workers hired after certification. Second, among those hired after certification, workers who received the greatest gains in pay were those who stayed with their living wage employers. The treatment effect for those who stayed in their living wage jobs was 59 log percent, whereas those workers who left living wage employment for work elsewhere had a much lower treatment effect of 16 percent.

[39] I experimented with the inclusion of treatment specific time trends and time trends in which all ‘outside’ firms were coded as a single firm. These results are in Appendix I.

However, these high estimates need to be interpreted with caution because much of the gain seems to be driven by a short-term loss in earnings experienced by joiners to control firms. Figure 4.2 plots average log monthly earnings over time for living wage and control joiners. The left-hand panel shows unweighted averages, while the averages in the right-hand plot are weighted. A comparison of unconditional mean changes in earnings for joiners suggest much smaller relative increases. For living wage joiners, earnings increased from $3,786 in 2013 to $4,284 in 2017, a total increase of 13.2%, or 3.14% per year. Joiners to control firms had higher absolute earnings in both 2013 ($4,380) and 2017 ($4,679), for a total increase of 6.8% overall or 1.7% per year. However, because of the prolonged drop in post treatment earnings for joiners, their post-treatment average monthly earnings are actually lower than their pre-treatment earnings.

Figure 4.3 | Log earnings by month | Joiners



Source: Author's calculations

The model presented in column (4) tests for endogeneity by examining pre-treatment differences between living wage and control joiners. However, since treatment occurs at the firm level, the presence of pre-treatment differences between treated and control workers *before* joining the sample firms is informative but less concerning. Because these coefficients represent separate treatment effects rather than differences from an omitted group, they can be interpreted as the conditional mean difference in pre-treatment earnings relative to *both* the control group and each worker's individual average earnings. The coefficients in column (4) reveal a pattern of lower pre-treatment earnings and higher post-treatment earnings for living wage joiners, confirming the changes in unconditional mean earnings discussed above. The final specification in column (5) shows treatment effects for firms of different sizes. The coefficient for firms of all sizes are positive

but statistically significant only for medium firms (43.3 log percent) and large firms (31.2 log percent), suggesting that workers in mid-sized firms received the greatest financial benefit from living wage certification.

4.7 DISCUSSION

This paper uses an unbalanced, worker-level dataset containing the full earnings history of employees who worked for living wage or matched control firms at any time between January 2014 and 2016. This research expands the understanding of the living wage experience in New Zealand. The work in this paper is the first quantitative research assessing earnings gains and labour-labour substitution in New Zealand living wage firms.

The New Zealand living wage is designed to help raise the incomes for low-wage workers and their families; this paper finds that the living wage is achieving some success in that regard. Not all workers benefit equally from the living wage, however. Employees hired before certification experience no increase in gross monthly earnings on average, while workers hired after certification receive substantial boosts in monthly earnings. It is likely that these differences are made possible by incomplete information and frictions in the labour market and are exaggerated by income targeting on the part of living wage prehires. Imperfect information on prevailing wages led living wage employers to rely on increases in the annual measure of the living wage, a measure that underestimated earnings increases in the relevant segments of the labour market. At the same time, labour market frictions may mean smaller raises are required to keep existing employees than to recruit new ones. However, the data indicate that there may be limits to this market power, as my results show that the workers who experienced the smallest gain (or greatest loss) following living wage certification were those that left their jobs.

Appendix table 4.8.1 | Additional regression results | Pre-hires

Log Total Monthly Earnings from all Jobs															
Fixed Effects Estimates of the Effect of Living Wage Certification on the Log Monthly Earnings of Prehires															
Robustness Checks															
specification	(1)			(2)			(3)			(4)			(5)		
column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
VARIABLES	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e
Treat	-0.0174	-0.0177	-0.00819	-0.0164	-0.0183	-0.0046	-0.00488	-0.00601	0.00293						
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)						
Treat * potentially affected				-0.00346	0.00211	-0.0132									
				(0.03)	(0.03)	(0.03)									
Treat * leaver							-0.0472	-0.044	-0.0409						
							(0.04)	(0.04)	(0.05)						
Treat * 1st observation year										0.00593	0.102	0.11			
										(0.19)	(0.17)	(0.17)			
Treat * 2nd observation year										-0.0513	0.0445	0.104			
										(0.20)	(0.18)	(0.18)			
Treat * transitional period										-0.0693	0.0263	0.0836			
										(0.19)	(0.18)	(0.17)			
Treat * 1st year post										-0.0464	0.0485	0.109			
										(0.20)	(0.19)	(0.18)			
Treat * 2nd year post										-0.0666	0.0267	0.0755			
										(0.19)	(0.18)	(0.18)			
Treat * small													0.0506	0.0503	0.0367
													(0.05)	(0.05)	(0.06)
Treat * medium													0.0348*	0.0387*	0.0471**
													(0.02)	(0.02)	(0.02)
Treat * large													-0.0465**	-0.0487**	-0.0377
													(0.02)	(0.02)	(0.02)
Firm size	4.33E-05	4.19e-05*	4.17e-05*	4.33E-05	4.19e-05*	4.17e-05*	4.33E-05	4.19e-05*	4.17e-05*	4.27E-05	4.18e-05*	4.17e-05*	4.32E-05	4.19e-05*	4.17e-05*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	8.360***	7.756***	6.865***	8.360***	7.757***	6.866***	8.357***	7.755***	6.867***	8.327***	7.759***	6.856***	8.355***	7.759***	6.851***
	(0.10)	(0.10)	(0.27)	(0.10)	(0.10)	(0.27)	(0.11)	(0.10)	(0.27)	(0.09)	(0.10)	(0.26)	(0.10)	(0.10)	(0.26)
Matched control group		Y	Y		Y	Y		Y	Y		Y	Y		Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Worker fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Treatment * calendar date dummies		Y			Y			Y			Y			Y	
Treatment-specific linear time trend			Y			Y			Y			Y			Y
Observations	67,900	432,500	432,500	67,900	432,500	432,500	67,900	432,500	432,500	67,900	432,500	432,500	67,900	432,500	432,500
R-squared	0.741	0.703	0.703	0.741	0.703	0.703	0.741	0.703	0.703	0.741	0.703	0.703	0.741	0.703	0.703
Standard errors in parenthesis (Adjusted for cluster in firm)															
*** p<0.01, ** p<0.05, * p<0.1															
Firm size: Small (0-10 employees) Medium (11-50 employees) Large (50+)															
First and second observational year refer to pre-treatment time periods															

Log Total Monthly Earnings from all Jobs															
Fixed Effects Estimates of the Effect of Living Wage Certification on the Log Monthly Earnings of Joiners															
Robustness Checks															
specification	(1)			(2)			(3)			(4)			(5)		
column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
VARIABLES	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e	log_e
Treat	0.605**	0.380***	0.394***	0.442*	0.179	0.194	0.932***	0.661***	0.676***						
	(0.27)	(0.14)	(0.14)	(0.26)	(0.15)	(0.15)	(0.28)	(0.17)	(0.17)						
Treat * potentially affected				0.457***	0.483***	0.486***									
				(0.08)	(0.07)	(0.07)									
Treat * leaver							-0.543***	-0.505***	-0.492***						
							(0.11)	(0.11)	(0.11)						
Treat * 1st observation year										-0.0221	-0.0376***	-0.0369***			
										(0.02)	(0.01)	(0.01)			
Treat * 2nd observation year										-0.117***	-0.115***	-0.114***			
										(0.04)	(0.01)	(0.01)			
Treat * transitional period										0.383	0.148	0.184			
										(0.26)	(0.16)	(0.16)			
Treat * 1st year post										0.671**	0.422***	0.451***			
										(0.27)	(0.13)	(0.13)			
Treat * 2nd year post										0.676**	0.433***	0.428***			
										(0.29)	(0.14)	(0.14)			
Treat * small													0.284	0.0606	0.101
													(0.31)	(0.21)	(0.21)
Treat * medium													0.643**	0.419***	0.447***
													(0.27)	(0.16)	(0.16)
Treat * large													0.585**	0.362**	0.369**
													(0.28)	(0.15)	(0.15)
Firm size	-1.47E-05	1.08E-05	1.08E-05	-1.68E-05	1.04E-05	1.04E-05	-1.30E-05	1.09E-05	1.10E-05	-1.57E-05	9.38E-06	9.48E-06	-1.44E-05	1.08E-05	1.08E-05
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	7.541***	7.528***	5.527***	7.549***	7.526***	5.523***	7.528***	7.526***	5.551***	7.543***	7.532***	5.538***	7.541***	7.528***	5.524***
	(0.14)	(0.06)	(0.22)	(0.13)	(0.06)	(0.22)	(0.15)	(0.06)	(0.22)	(0.14)	(0.06)	(0.22)	(0.14)	(0.06)	(0.22)
Matched control group		Y	Y		Y	Y		Y	Y		Y	Y		Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Worker fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Treatment * calendar date dummies		Y			Y			Y			Y			Y	
Treatment-specific linear time trend			Y			Y			Y			Y			Y
Observations	17,300	129,800	129,800	17,300	129,800	129,800	17,300	129,800	129,800	17,300	129,800	129,800	17,300	129,800	129,800
R-squared	0.658	0.633	0.633	0.663	0.634	0.634	0.664	0.634	0.634	0.662	0.634	0.634	0.659	0.633	0.633
Standard errors in parenthesis (Adjusted for cluster in firm)															
*** p<0.01, ** p<0.05, * p<0.1															
Firm size: Small (0-10 employees) Medium (11-50 employees) Large (50+)															
First and second observational year refer to pre-treatment time periods															

4.9 Appendix II | Reconciling differences in estimates of Employee Earnings

In my first paper on the living wage, I use a balanced firm-month panel dataset to investigate changes in earnings following living wage certification. Using average monthly earnings by firm as my dependent variable and the time of likely compliance with the living wage as my independent variable, I find weak evidence of earnings increases for all workers and fairly strong evidence of earnings increases for the sub-sample of low-paid employees.

Moving into my second paper, I want to continue my investigation into changes in earnings for living wage employees using an unbalanced worker-level panel dataset that follows my sample of treated and control employees as they move jobs. This expanded dataset allows for the investigation of earnings dynamics for and allows me to explore changes in hiring patterns in living wage firms follow certification.

The first step in my analysis with the new data set was to attempt to replicate the findings of my first paper. However, regression estimations from the worker-level dataset are substantially different from those produced from firm-level data. This appendix seeks to identify and document the source of this difference.

For this appendix, I focus on a single outcome variable, log monthly earnings, for the entire sample of workers and I complete a series of regression analyses to confirm the comparability of the two datasets. First, I replicate my original findings by applying the methodology of my original work to the new dataset. Finding similar results, I work backwards to identify the source of the discrepancy. This process leads me to form three conclusions that shape the direction for my third chapter. First, living wage earnings increases are concentrated in smaller living wage firms; this is a group that represents a majority of firms but which employs a minority of workers. Second, living wage certification primarily benefits workers who were hired after certification. Workers hired before certification do not have an earnings advantage over control employees, on average. Lastly, because earnings increases are concentrated in small firms, the majority of living wage employees do not benefit from living wage status. The second and third of these new findings are explored in detail in Chapter 3 of my thesis.

The rest of this appendix is organized as follows: in the Data section, I discuss the creation of the two datasets and highlight their similarities and differences. The regression models used are described in the Models section, while the results are outlined and discussed in the Comparison section. The Discussion section concludes. Full regression results for all specifications are presented at the end.

Data

In this appendix, I am comparing results of regression analyses using two datasets that I have created as part of my doctoral thesis. Both datasets are drawn from Statistics New Zealand's Integrated Data Infrastructure and were created by linking variables across the Inland Revenue employer monthly survey, Business Registry, Ministry of Education qualification and Personal Details tables. This section explains the datasets and highlights their similarities and differences.

For the first living wage chapter of my thesis, I use a firm-level dataset to capture changes in average labour costs, prevalence of below-living wage earnings and turnover patterns within living wage and control firms. This dataset contains the monthly payroll amounts for all living wage employers as well as for a matched group of control firms and covers a 27-month period spanning 14 months prior to certification through 12 months post-certification. Control groups were matched with replacement on the basis of industry, region, size and age; where a firm was matched with more than one living wage firm, it appears in the dataset once for each match. In total, there are 33 living wage employers, and 554 matched control firms. Control firms are assigned the same date of certification as their matched control firm and multiple inclusions of control firms with multiple matches allows firms to be matched to each control firm based on their date of certification. For each firm, the average monthly employee earnings is calculated at the firm level and includes only earnings from employment in the sample firm; in the event that an employee has multiple jobs, only the earnings in the sample firm are included. Workers who are employed in more than one sample firm appear in the data for each firm in the months for which they were employed.

The third chapter of my thesis is designed to look at differences in earnings gains between groups of employees, based on time of hire and retention. To do this, I take four years of earnings history for all workers who were employed in any of my sample firms at any point between January 2014 and December 2015. This gives me the full earnings history of 2,100 living wage workers and

13,700 control employees. The dataset is organised as an unbalanced worker/month panel in which monthly income is calculated as the total earnings from all jobs per month.

Both datasets can be used to estimate earnings and to estimate the proportion of workers earning less than the full-time living wage equivalent. However, the estimates produced using the two datasets may differ for a handful of reasons. First, and perhaps most importantly, is the fact that the firm-level dataset reports change at the firm level. Each living wage firm is given equal weight in the analysis and the overall results are reflective of the experiences at the level of the firm. In the worker level dataset, I am estimating the effects on the average employee and large firms or firms employing more low-paid workers will naturally receive greater weight. Second, the time period covered by the two datasets is different. The firm level dataset covers 27 months and captures 12 months post-treatment while the worker level dataset covers 49 months and contains data on two years post treatment. Lastly, the firm-level dataset only contains employment records for those firms, while the worker level dataset focuses on the worker and includes data from all jobs, whether previous, concurrent or post.

Models

For my firm-level regression analysis in Chapter 2, I employed three fixed effects specifications. I modify them here for use on a worker-level panel and add two additional specifications to estimate differences in treatment effect by firm size. Descriptions of the original regression specifications are as follows:

(1) $Y_{it} = \beta_1 L_{it} + \sum_{i=1}^{638} \theta_i + \sum_{t=1}^{27} \theta_t + e_{it}$

This is the simplest of my specifications. L_{it} is equal to one if firm i is compliant with the living wage in month *t*. The treatment effect is captured by β_1. I include firm fixed effects and fixed effects for calendar year/month.

(2) $Y_{it} = \beta_1 L_{it} + \gamma X_{it} + \sum_{i=1}^{638} \theta_i + \sum_{t=1}^{27} \theta_t + e_{it}$

In the second specification, I add a vector of firm-level control variables to the model, represented by X_{it}. I control for firm size, average length of employment gaps and for the proportion of the workforce that is female, European, Maori or Pacific Islander. I also include two controls for maximum educational qualification.

To account for the possibility that the living wage treatment effect varies over time, I add four treatment-time variables to my third specification. D_0 is equal to one for living wage firms in the two months prior to their official living wage certification. During this time, it is likely that firms are compliant with the requirements of the living wage despite not yet being officially certified. D_1 is a dummy variable indicating that a firm is in the first six months of its living wage certification, while D_2 represents the second six months following certification. To test for endogeneity, I include D_{-1} a dummy variable identifying living wage firms in the six months prior to certification.

(3) $Y_{it} = \beta_1 D_0 * L_{it} + \beta_2 D_1 * L_{it} + \beta_3 D_2 * L_{it} + \beta_3 D_{-1} * L_{it} + \gamma X_{it} + \sum_{i=1}^{638} \theta_i + \sum_{t=1}^{27} \theta_t + e_{it}$

The methodology that I employ for firm-level analysis compares average levels of earnings and turnover by firm, applying equal weights to each firm regardless of size. However, to see if firms of different sizes respond differently to the living wage, my final two specifications add additional variables to account for firm size. In specification 4, I interact the treatment variable with a dummy for 'large' which is equal to one if the firm has more than 50 employees in the observation month. The effect of living wage certification for small and medium enterprises is captured by β_1, while the impact on large firms is captured by $\beta_1 + \beta_2$.

(4) $Y_{it} = \beta_1 L_{it} + \beta_2 Lg_{it} * L_{it} + \gamma X_{it} + \sum_{i=1}^{638} \theta_i + \sum_{t=1}^{27} \theta_t + e_{it}$

In the last specification, I include three dummy variables for treated firms of different sizes. S_1 is a dummy indicating that a firm is both treated and employs 10 or fewer employees. S_2 interacts treatment status with a dummy equalling one in firms with between 11 and 49 employees, while S_3 identifies treated firms with more than 50 employees. Treated firms fall into one of the three categories in each month so there is no omitted group. The living wage effect on firms in each size category is measured by the corresponding beta coefficient.

(5) $Y_{it} = \beta_1 S_{it}^{sm} + \beta_2 S_{it}^{med} + \beta_3 S_{it}^{lg} + \gamma X_{it} + \sum_{i=1}^{638} \theta_i + \sum_{t=1}^{27} \theta_t + e_{it}$

My preferred specification are contained in columns 3 and 5, and I present those specifications for comparison here. These columns account for heterogeneity in treatment effect by time and firm size and reveal the most nuanced picture of the living wage on firm average labour costs.

To modify these specifications for use with worker level data with firm and time fixed-effects, I alter the subscripts *i* and *t*. In worker-level data, the subscript *i* denotes individual workers, while *t* indicates the month to living wage *treatment*. For workers hired before living wage certification, the time of treatment is equal to the date of certification. However, for workers hired after certification, treatment occurs in the month that the worker is first employed with the living wage firm. To accommodate the inclusion of worker fixed effects, all time-invariant covariates must be dropped. Therefore, the vector X_{it} becomes a single covariate for number of employees in the firm.

Appendix table 4.9.1 | Comparison of firm-level regression results in the two datasets

Comparison of Regression Results Using Different Datasets						
Log-Monthly Earnings						
Panel	A			B		
Dataset and Analysis Level	Ch 2 | Firm-Level			Ch 3 | Firm-Level		
Specification	(2)	(3)	(5)	(2)	(3)	(5)
Treatment	0.0894***			0.0473		
	(0.03)			(0.03)		
Treat * transitional months		0.108**			0.0730*	
		(0.05)			(0.04)	
Treat * first six months post		0.122***			0.0721*	
		(0.04)			(0.04)	
Treat * second six months post		0.0825*			0.0516	
		(0.05)			(0.05)	
Treat * six months pre		0.0282			0.0335	
		(0.04)			(0.04)	
Treat * small			0.114**			0.106**
			(0.05)			(0.05)
Treat * medium			0.0459**			0.0395
			(0.02)			(0.03)
Treat * large			0.147			-0.0364
			(0.10)			(0.05)
Time fixed effects	x	x	x	x	x	x
Firm fixed effects	x	x	x	x	x	x
Worker fixed effects						
Includes earnings from all jobs				x	x	x
Observations	17,300	17,300	17,300	14,700	14,700	14,700
R-squared	0.874	0.874	0.874	0.863	0.863	0.863
Number of firms	639	639	639	588	588	588
Standard Errors Adjusted for Clusters at Firm Level						
*** p<0.01, ** p<0.05, * p<0.1						
Size categories; 1-10 employees, 11-50 employees, 50+ employees						

Note: Author's calculations. Data from the IDI.

Comparisons

I start with the regression results for the full firm-level sample as presented in Chapter 3. I then run the same regressions on the worker-level dataset (collapsed at the firm level and using only data from sample firms) and get similar results. Panel A contains the regression results for specifications 2, 3, and 5 for the whole sample of workers as presented in Chapter 3, while panel B presents the results of the same regression specifications using the worker-level dataset from Chapter 4. The coefficients represent the change in average firm employee earnings following compliance with the living wage and is a good estimate of changes in firm labour costs following certification. The full table is presented at the end of this appendix and contains coefficients on all covariates.[40]

Earnings in second jobs are included in panel B if the worker held two jobs in a single month and one of those jobs was in a living wage firm or control firm. With the addition of second jobs, the coefficients decline in magnitude and become less statistically significant but the time trend interaction variables still show a positive and statistically significant treatment effect in both the transitional period and the first six months of certification. The effect in small firms also remains positive and statistically significant. The change in the coefficient for large treated firms is potentially troubling but since neither coefficient is statistically significant, the conclusion remains that living wage certification does not affect average earnings in large firms.

My next step in assessing comparability of the two datasets is to analyse the dataset from Chapter 3 using a *worker-level analysis*, with and without worker fixed effects. My results so far have suggested that the living wage impact is stronger in small firms, with large firms experiencing no increase in average earnings following certification. By analysing the data at the worker-level, firms with more employees will naturally receive a larger weight, and because these firms are less likely to experience a change in earnings, these results should be smaller and less statistically significant. Panel C of the regression table shows that this is the case. When using worker-level data and firm/time fixed effects, there is no overall treatment effect of living wage certification on

[40] The number of observations between panel A and panel B differ because I duplicated the records of control firms with multiple living wage matches in the Chapter 3 analysis. There are 587 unique firms but some are included more than once, resulting in the addition of approximately 90 employee records per month. When doing this analysis, I re-created panel B using the duplicated firm records and it did not alter the results.

log monthly earnings. The only coefficient that remains statistically significant is the effect of treatment on small firms, which reinforces the finding that living wage certification impacts workers in small firms. Unfortunately, only 10% of my sample works in firms with fewer than 10 employees, suggesting that the impact of the living wage is isolated among very few employees, despite having an impact on firm salary expenditures in general.

Appendix table 4.9.2 | Comparison of worker-level regression results in the two datasets

Comparisons of Regression Results Using Different Datasets									
Log-Monthly Earnings									
Panel	C			D			E		
Dataset and Analysis Level	Ch 2 | Worker-Level			Ch 2 | Worker-Level (prehires)			Ch 3 | Worker-Level (prehires)		
Specification	(2)	(3)	(5)	(2)	(3)	(5)	(2)	(3)	(5)
Treatment	0.0615			-0.0444*			-0.0503**		
	(0.05)			(0.02)			(0.02)		
Treat * transitional months		0.0498			-0.0760*			-0.0762***	
		(0.05)			(0.04)			(0.03)	
Treat * first six months post		0.0799			-0.0490*			-0.0659***	
		(0.05)			(0.03)			(0.03)	
Treat * second six months post		0.032			-0.0614**			-0.0728*	
		(0.05)			(0.03)			(0.04)	
Treat * six months pre		-0.0123			-0.0287*			-0.0352**	
		(0.02)			(0.02)			(0.01)	
Treat * small			0.185***			0.0431			0.00962
			(0.07)			(0.06)			(0.07)
Treat * medium			0.0401			-0.0138			-0.021
			(0.03)			(0.01)			(0.02)
Treat * large			0.0643			-0.0690*			-0.0701***
			(0.07)			(0.04)			(0.03)
Time fixed effects	x	x	x	x	x	x	x	x	x
Firm fixed effects	x	x	x	x	x	x	x	x	x
Worker fixed effects				x	x	x	x	x	x
Includes earnings from all jobs							x	x	x
Observations	232,800	232,800	232,800	232,800	232,800	232,800	176,700	176,700	176,700
R-squared	0.35	0.35	0.35	0.874	0.874	0.874	0.798	0.798	0.798
Number of firms	639	639	639	639	639	639	588	588	588
Standard Errors Adjusted for Clusters at Firm Level									
*** p<0.01, ** p<0.05, * p<0.1									
Size categories; 1-10 employees, 11-50 employees, 50+ employees									

Note: Author's calculations. Data from the IDI.

In panel D, I add worker fixed effects to the regressions presented in panel C and I compare this to the estimates for workers hired before certification in Panel E. Panels D and E reveal an interesting nuance of living wage impact: workers hired before living wage certification are at a slight earnings disadvantage compared to their counterparts in control firms although they experience an increase in gross monthly earnings over the time period studied, a result that becomes one of the main focuses of Chapter 4.

Additionally, I compare regression results from the two datasets after the inclusion of worker fixed effects. This impacts the results in two ways. First, it controls for all time-invariant worker characteristics; second, it changes the group of workers for whom the average treatment effect is estimated. Because fixed effects work by comparing changes in individuals' earnings before and after treatment to their average observed earnings over the study period, the only workers that contribute to the estimates produced in panel D are those for whom there are both 'before' and 'after' observations of earnings; they are workers hired before certification who remain employed in sample firms after certification.

Discussion

In this appendix, I present a series of regression specifications to establish the equivalence of two datasets. My analysis reveals that although the datasets differ in their treatment of second jobs and treatment of control firms with multiple living wage matches, they produce equivalent results in comparable regression specifications. Additionally, this analysis pointed out an interesting area of discrepancy in the earnings effect of the living wage: the living wage has not affected all employees equally. Regression results for workers hired before certification reveal that this group of workers actually experiences a decline in earnings relative to the matched group of control employees, suggesting that the bulk of the living wage benefit accrues to new hires, a finding that is explored in more detail in the third chapter of this thesis

CHAPTER 5: SUMMARY AND CONCLUSION

This thesis is composed of three papers devoted to the exploration of the effects of wage floors. While the debate over wage floors is more than a century old, changes in the political economy and developments in methodology create fertile ground for research and discussion. Continual changes in government, economics and public sentiment shape the context of the minimum wage debate. At its core however, the debate over wage floors remains centred on issues of market efficiency, earnings equity and welfare of vulnerable workers. The papers in this thesis provide insight into the effect that two unique wage floors have on the earnings and employment of workers and the ways in which employers respond to the mandate.

In Chapter 2, I examine the effect of a municipal minimum wage law in the Albuquerque/Bernalillo region of New Mexico. Like much of the literature, this work focuses on the behaviour of the low wage labour market by estimating the effect of a minimum wage increase on teens and food service workers. As predicted by monopsony labour market theories, a small increase in the minimum wage does not necessarily reduce the number of jobs available to these workers. I find that neither employment nor hours declined for teens after the enactment of the ordinance but that food service workers experienced a reduction in wages as firms moved to take advantage of a sub-minimum wage provision in the law. While the ordinance did not reduce teenage employment, a rise in labour force participation for teens led to increases in unemployment driven by excess supply.

This paper breaks new ground in minimum wage research by investigating the ways in which firms adjust to minimum wage increases when the law exempts a large section of the low-wage workforce from coverage. Under the Albuquerque/Bernalillo minimum wage ordinance, employers in New Mexico were required to pay tipped workers a mere $3.60 USD per hour. My results show that prior to the ordinance, many employers did not rely on the sub-minimum wage provision of the law. Rather, the majority of tipped and food service workers earned at least the regular minimum wage of $7.50 per hour. After the ordinance, the prevalence of sub-minimum wage earnings grew among these workers as employers who previously paid the regular minimum began paying sub-minimum wages. While perhaps not surprising on the surface, these results are curious in that these employers *could* have paid sub-minimum wages to tipped workers before the local ordinance but chose not to. This implies either that the ordinance alerted employers to the presence of the tipped minimum wage or that rising overall labour costs led firms to reduce

expenses by cutting wages for tipped staff. This finding, consistent with competitive models of labour markets, suggests that higher wage floors in a covered sector can put downward pressure on wages in an uncovered sector. From a policy perspective, this work offers useful insights into the consequences of exempting groups of low-income employees from coverage under minimum wage law. If the goal of minimum wage laws is to improve equity and the welfare of workers, care must be paid to ensure that vulnerable populations are protected.

Chapters 3 and 4 of this thesis focus not on mandated wage floors but on New Zealand's voluntary living wage program. Originally launched by union, religions and faith-based groups, the living wage movement has gained traction among market-based firms of all sizes. While unexpected, this growth among for-profit entities receives support in the marketplace from consumers concerned with the social footprint of their purchasing choices. Despite its growth, the living wage is a controversial program. The language of its mission implies that it is intended as a tool to limit or reduce poverty among the working poor. To this end, it is criticised for being poorly targeted and for being incompatible with the existing New Zealand system of taxes and transfers. While valid, these criticisms overlook the fact that increased earnings for low-wage workers are likely to improve welfare regardless of a workers poverty status, household structure, marginal tax rate or family obligations. My work seeks to assess the degree to which the living wage is successful at increasing earnings for low-paid workers and whether living wage organisations are able to derive a benefit from this through reductions in turnover. Additionally, I explore how gains in earnings vary based on when an employee was hired into a firm. Workers that join certified living wage firms after certification receive a greater benefit than do workers hired before certification. This implies that living wage employers continue to hire low-skilled workers for entry level jobs rather than by recruiting more skilled employees for these positions.

My third chapter shows that the living wage is successful at raising earnings for workers while having only a small effect on the total wage bill for employers. At the same time, I find no evidence of disemployment at living wage firms. Thus, while gross monthly earnings increase for low-paid workers, firms are able to offset much of these increases through wage compression. My fourth chapter explores this issue further and reveals that the earnings gains accrue almost exclusively to workers hired after certification. These new hires receive a substantial pay increase relative to workers hired into comparable firms. However, workers hired before certification do not receive

a similar benefit. Employees hired before living wage certification experience smaller wage gains than do employees of control firms. I am not convinced that this relative decline in wages results exclusively from the desire to mitigate cost increases. Rather, I hypothesise that in a labour market characterised by imperfect competition, well-intentioned employers have based the value of pay raises on increases in the published living wage rate that underestimated wage gains in the relevant labour markets. While existing qualitative evidence supports this theory, further research is needed.

While earnings for newly-hired workers may rise, this might not make workers more likely to stay in their jobs. I find no statistically significant evidence of reduced turnover in living wage firms. This unexpected finding contradicts existing international work on the living wage but does not rule out the possibility that living wage certification helps employers retain staff. Rather, there is a great deal of turnover within all firms in my sample and measures of turnover are noisy. Most of my regression coefficients show that the effect on turnover is negative, but firm-level heterogeneity and high levels of variability in turnover make it difficult to measure changes with precision. Follow up work with a larger sample and a longer time frame is needed to determine the effect on turnover.

The most significant shortcoming of my living wage research is that I was unable to identify one of the groups most affected by the program: contract staff and cleaners. Many living wage firms directly employ few effected workers. Instead, their low-paid staff is hired through contract arrangements for services such as cleaning. For this reason, I have underestimated the number of impacted workers and have biased my results toward zero. Having access to data on these workers' wages, hours and turnover would give a more full and nuanced picture of the effect of the living wage. However, my work serves as a good starting point to understand the composition and motivation of living wage firms and establishes the fact that the living wage program is successful at raising incomes for previously low-paid workers. Although the program has limited ability to reach vulnerable populations such as sole parents, proponents of the movement emphasise that goals for the program include promoting equity and improving welfare for low-paid workers in all household types. And in these efforts, my research suggests that the program has been relatively successful.

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