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A Literature Review on the Effects of Living Wage Policies

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Executive Summary

1. Background

This report has been prepared to inform Auckland Council about the potential impacts of introducing a living wage for the Auckland Council Group. This literature review concentrates on published empirical studies on the effects of living wage laws in other countries and minimum wage statutes in New Zealand.

The living wage is often defined as the wage necessary to provide a full-time, year-round worker and his or her family with the sufficient income to ensure an adequate standard of living and the ability to fully participate in society.

A living wage of \$18.40 has recently been proposed for all of New Zealand, with the possibility of a higher living wage of \$24.11 for Auckland due to its relatively higher living costs (Waldegrave and King, 2012). These living wage levels would be 33.8% and 75.3%, respectively, above the prevailing minimum wage of \$13.75 in this country.

Living wage campaigns are currently underway in other New Zealand cities. In April 2013, the first Auckland business, Tonzu, pledged to pay a Living Wage. In May, The Warehouse announced a career retailer wage benchmarked against the Living Wage. It will be available to all staff with 5,000 hours of service who have undertaken specific training. In June 2013, the Wellington City Council resolved, in principle, to become a Living Wage employer.

There is ample empirical evidence on the effects of living wage laws overseas that can inform this discussion about the possible consequences of adopting living wage provisions in this country. Most of this analysis comes from the experiences of numerous cities in the United States that have enacted living wage ordinances over the last two decades.

There are a number of common features of living wage laws around the world. Firstly, they have limited coverage. Unlike minimum wage laws that provide a universal wage floor, living wage ordinances cover specific groups of workers often within narrow geographic boundaries. Secondly, they set relatively high wage floors, often 30% or more above the national minimum wage. Thirdly, they are motivated by an explicit attempt to reduce poverty rates by setting a wage consistent with a minimum standard of living.

Not all living wage laws are the same. They vary primarily in terms of worker coverage. For example, the vast majority of living wage ordinances in the U.S. cover only city government contractors, or both contractors and employers receiving some form of business assistance. Very few cities have living wage provisions that cover their own employees, and attempts to extend living wage laws to all workers within a metropolitan area have thus far been unsuccessful. This is an important consideration in understanding how the overseas empirical evidence on living wage effects might be applicable in New Zealand. The London Living Wage, that contains the same voluntary elements of some New Zealand proposals, has not been subject to the same level of analysis as the U.S. laws.

2. Results

There is considerable evidence in the overseas literature that implementing a living wage directly increases the actual wages of targeted workers. Generally, only between 1% and 3% of workers in a city are covered by the living wage, and in some cities (especially London) this coverage rate can be much lower. There are numerous reasons why these higher wages have even relatively smaller impacts on employer costs. Paying a living wage can increase productivity, reduce worker turnover and absenteeism, and improve the quality of future job applicants. The net result is that the cost increases among living wage employers can be as low as 1% to 2% of total production costs. Furthermore, some evidence suggests that even these small cost increases can be absorbed through lower employer profits and higher product prices passed on to customers.

The key potential disadvantages of a living wage are that it could reduce either the employment levels or hours of work for affected employees. The empirical evidence on these effects is quite limited. However, one line of research in the U.S. by Neumark, Adams and co-authors has reported consistent evidence of small negative employment effects among low-wage workers in cities with business-assistance living wage laws. Similar results in cities with contractor-only living wage laws are negative, but smaller in magnitude and often statistically insignificant. Other studies have contested these findings on negative employment effects by citing recent evidence from the minimum wage literature. However, no published studies have thus far offered any direct empirical evidence to contradict this result of a small, negative employment effect from these broader living wage policies. Although there is no quantitative evidence on the potential losses in employment or hours of work from the London Living Wage, case studies suggest the possible loss in hours of work or employment for some affected workers.

There has been little systematic research in the overseas literature on the effects of living wage laws on employer location decisions. Literature from both the UK (Wills and Linneker, 2012) and the US (Fairris and Bujanda, 2007; Reich et al., 2005) shows some evidence of labour substitution effects. These studies showed that new hires (following the implementation of a living wage policy) tended to be better educated with higher wages in previous jobs. Whereas research in the UK found that new hires were more likely to be native-born, Fairris and Bujanda (2007) found that living wage workers in Los Angeles were more likely to be Latino and African American.

One of the most surprising results from the U.S. literature is that there is evidence that living wage laws have the intended effects in reducing poverty rates. This finding has often been ignored in surveys of this U.S. literature, and is striking because it runs counter to the consensus in the minimum wage literature of extremely small if any associated anti-poverty effects. Neumark and Adams found fairly consistent evidence that employer-assistance living wage policies lead to small reductions in overall city poverty rates. Clain (2008) using an entirely different data source confirmed this general result, by finding that county-level poverty rates declined “modestly” with the introduction of a living wage ordinance in the area.

3. Implications

What do these literature results mean for the possible adoption of a living wage provision in Auckland or elsewhere in New Zealand? Holzer (2008) reviewed the existing U.S. living wage literature at the time and concluded that the effects of living wage policies (either positive or negative) were likely at best to be “modest”. This same general conclusion holds with the updated literature. This is partly because such policies directly affect small proportions of the prevailing workforce. This doesn’t mean that such laws shouldn’t be enacted. Even small positive benefits for specific groups of low-wage workers and their families may be better than nothing.

To a large extent, the potential effects of a living wage law depend on the “type” of policy that is implemented. The U.S. literature suggests that broader-based ordinances that target both city contractors and employers receiving economic development assistance directly affect more workers and are more likely to have both positive effects (e.g., wage increases and poverty reductions) and negative effects (e.g., employment losses and employer dislocations). Unfortunately, the experiences in the U.S. may not be particularly relevant for the kind of living wage policies that are currently being proposed in New Zealand. A policy to pay the living wage to council employees and to lobby local employers to voluntarily pay this living wage in their organisations is much more closely aligned to the London Living Wage. Unfortunately, there has been almost no in-depth analysis to date on the consequences of this particular policy.

My own view, based on everything I know about this literature and other areas of economic analysis, is that the kind of living wage policies enacted in London and proposed for Auckland would have minimal effects. I would expect them to increase the actual wages paid to city employees, but that the increase in overall costs would be offset somewhat by a variety of factors. The employment effects would be largely negligible as would the overall effects on poverty in the city. There is the possibility, however, that in the long run labour substitution effects might occur. A large increase in the living wage would lead to more highly skilled and educated job applicants. This is a double-edged sword. On one hand, the hiring of more skilled workers would have positive productivity and costs effects. It may also raise the quality of services provided to ratepayers. On the other hand, this might mean fewer job opportunities for the most vulnerable workers in the community (i.e., those with lower skills, education and job experience).

Table 1: Summary of Key Arguments

Topic	For Living Wage	Against Living Wage	In sum
Poverty	“a necessary and important step in the reduction of poverty” (Living Wage Aotearoa) Neumark, Adams and their co-authors have consistently found empirical evidence of anti-poverty effects from US living wage laws. They found that a 30% wage increase under a broader living wage policy that applies to city government contractors and employers receiving some form of business assistance would be expected to reduce the poverty rate by 3.9%.	Not ‘target efficient’ – large proportions of minimum-wage workers don’t live in poor households, and large proportions of poor households don’t contain working members. In an evaluation of the London living wage, Lawton and Pennycook (2013: 36, 40) report that only one tenth of low earners live in poor households and argue that “increasing wages at the bottom of the labour market is a relatively inefficient way of tackling low-income across all households”.	Although inefficient, the living wage does have some effect on poverty.

Topic	For Living Wage	Against Living Wage	In sum
Employer costs / profitability	<p>Higher wages associated with living wages may have relatively small effects on employer costs (30% increase in wage = 1-2% increase in total costs for average firm) because of:</p> <ul style="list-style-type: none"> • small proportion of workers earning below the living wage; • labour costs make up a fraction of total production costs; • increased productivity (little evidence); • reduced worker turnover (strong evidence); • reduced absenteeism (little evidence) • more experienced and skilled pool of job applicants (strong evidence); • positive impact on employer reputation. 	<p>These costs are likely to be larger for firms that have higher proportions of low-wage workers, more labour-intensive production processes and experience smaller beneficial effects from increased productivity, lower worker turnover and absenteeism, and smaller improvements in the quality of job applicants.</p>	<p>The living wage has a relatively small cost impact on many firms, but would have a much larger impact on businesses with high proportions of low-wage workers.</p>
Jobs and hours of work	<p>When comparing employment levels before and after the enactment of living wage ordinances in the US, some (not all) researchers found that employment levels did not decline after employers started paying the mandated living wage (Brenner 2005; Reich et al.2005).</p>	<p>Neumark, Adams and their co-authors provide some empirical evidence of reductions in employment levels or aggregate hours of work. Loss of hours of work was experienced in some case studies from London.</p>	<p>Evidence is limited and inconsistent, but in balance points to some reduction in hours of employment and the opportunity for overtime.</p>
Employer location decisions	<p>What little research there is found no significant differences in the number of firms in cities with and without living wage ordinances. However, this doesn't necessarily indicate that living wages are unimportant for the location decisions of specific groups of firms.</p>	<p>Living wage laws could lead existing employers to leave a city or deter other employers from relocating to that same metropolitan area (little evidence).</p>	<p>There is insufficient evidence to be able to identify whether the living wage affects employer location decisions.</p>
Labour substitution	<p>There is strong evidence that paying a Living Wage results in a more experienced and skilled pool of job applicants, to the advantage of the employer.</p>	<p>Literature from both the UK (Wills and Linneker, 2012) and the US (Fairris and Bujanda, 2007; Reich et al., 2005) shows that new hires (following the implementation of a living wage policy) were better educated, had higher wages in previous jobs, and were more likely to be male. NZ Literature suggests an increased minimum wage may result in reduced employment for youth and Maori (Pacheco 2011),</p>	<p>The Living Wage may result in fewer job opportunities for the most disadvantaged workers in the community (e.g. young workers, low-skill workers).</p>

Topic	For Living Wage	Against Living Wage	In sum
Health and wellbeing	An association has been found between living wage employment and psychological wellbeing in London (Wills & Linneker 2012). 65% of surveyed workers in living wage workplaces reported experiencing some benefits in terms of their work, finances and family.	No association has been found between living wage employment and self-rated health in London. 35% of surveyed workers in living wage workplaces reported experiencing no benefits in terms of their work, finance and family. Relatively few living wage workers in Los Angeles and at San Francisco International Airport reported improvements in quality of life (Reich et al., 2003, Fairris et al., 2005: 82-3).	There is some association between Living Wage employment and improvements in work, finances and family life, quality of life and psychological wellbeing. However, studies in both the UK and the US have found that significant proportions of affected workers do not report these benefits.

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1.0 Introduction

The living wage is often defined as the wage necessary to provide a full-time, year-round worker and his or her family with sufficient income to ensure an adequate standard of living and the ability to fully participate in society. Living wages are based on costs of living in specific localities and therefore differ from place to place due to differences in the local costs of food, accommodation and other living expenses. The living wage generally falls between the minimum wage and the median wage in the areas in which it has been introduced.

Living wage laws have three common features. Firstly, they impose or promote a wage floor for specific groups of workers often in particular geographic areas (i.e., narrow coverage). Secondly, they set a wage substantially above existing statutory minimum wages (i.e., high relative values). Thirdly, living wages aim to lift workers and their families out of poverty (i.e., antipoverty rationale). More than 140 jurisdictions in the United States have adopted living wage ordinances since 1994. These living wage campaigns have spread to the United Kingdom, Canada, Australia, New Zealand and other countries. Most noteworthy, was the adoption of a living wage provision in London in 2001 following the introduction of a lower National Minimum Wage in the United Kingdom in 1999.

This purpose of this report is to summarise and critically review the existing literature on the estimated effects of both living wage laws overseas, and statutory minimum wages in New Zealand. This report has been commissioned by Auckland Council as part of their efforts to provide the Auckland Plan Committee with information about the potential impacts of introducing a living wage for the Auckland Council Group (Auckland Plan Committee, 2013).

The potential for workers to be paid wages that are considered to be “unacceptable” from an ethical or social perspective can be traced back to the work of Adam Smith (1776). Altman (2012) and Wills and Linneker (2012), for example, trace the history of modern Living Wage campaigns back to similar movements in the 19th and 20th centuries. Statutory minimum wages were also originally motivated as anti-poverty policies, intended to directly raise the incomes of the poorest households through a comprehensive wage floor. Although this current literature review does not directly examine overseas studies on the effects of minimum wages, the close connections between minimum wage and living wage research will be referred to throughout this report.

Because the vast majority of published studies have focussed on the possible consequences of living wage laws in the United States, we begin with this area of the literature. These studies are summarised and critiqued in Section 3 of this report. We then consider the analysis on the London Living Wage in Section 4. Although there has been no research to date on the possible effects of a living wages in New Zealand, the analysis of recent and substantial increases in minimum wages in this country might be particularly relevant. This literature is summarised, critiqued and extended to the living wage discussion in Section 5.

Some issues relating to the effects of a living wage have not been discussed in this review. Important drivers of living wage campaigns include reducing poverty and income inequality as well as broader principles of promoting social justice. A further objective of some living wage campaigners and local authorities that have adopted the living wage provisions is to ensure that there is a ‘market’ for contractors who pay higher wages and choose to compete

on quality or ethical standards rather than low costs of services achievable through low wages. The authors of this review acknowledge that living wage policies have diverse objectives and potential benefits, but have only been able to comment on the effectiveness of the policy in achieving those benefits and objectives for which evidence is available.

For example, a primary objective of living wage policies is to reduce (income) inequality. We can assume that living wage policies have some effect on income inequality within individual companies – an increase in the lowest wages without an increase in the wages of those at the top of the pay-scale resulting in a reduced gap between the lowest and the highest wages. Information was unfortunately not available on the effects of living wage policies on inequality more broadly measured (e.g., the effects on a city's Gini coefficient).

A second issue is that the living wage is clearly described by campaigners and employers a 'step in the right direction' or a 'piece of the puzzle' rather than a complete solution to problems of inequality and poverty. While it is acknowledged by the authors of this report that the living wage may form part of a suite of measures to address these issues, this review assesses the effects of the living wage itself rather than in combination with other measures.

Third, information about how living wage policies are implemented (e.g., phased in over a number of years) may have been useful, but was not readily available. The only available evidence concerned the initial exemption of certain types of employers (e.g., small businesses and not-for-profits) from living wage policies in some US cities (see section 5.1).

Finally, it would also be useful to understand the likely effects of living wage policies on specific groups such as women, Māori and Pacific Peoples among whom low wage work is particularly prevalent. This evidence is not available because the living wage has not been implemented in New Zealand, but one can assume that women, Māori and Pacific Peoples will receive a disproportionate share of any impacts from a living wage policy due to their overrepresentation among low-wage earners (e.g., labour substitution effects). The international literature on the living wage shows that new hires (following the implementation of a living wage policy) are better educated, had higher wages in previous jobs, and are more likely to be male (Fairris and Bujanda, 2007; Reich et al., 2005). In terms of the implications for ethnic minorities, whereas Wills and Linneker's (2012) research in the UK found that new hires were more likely to be native-born, Fairris and Bujanda (2007) found that living wage workers in Los Angeles were more likely to be Latino and African American. Literature in New Zealand suggests that Māori may be particularly vulnerable to decreases in their employment rates and usual total weekly hours with increases in the minimum wage (Pacheco, 2011). It is unknown whether similar effects could result from a living wage policy.

One of the overriding concerns in any literature review on overseas studies relates to the possible 'external validity' of these findings. In other words, can the conclusions derived from US and UK research be applied directly to the New Zealand context? This is not an easy question to answer. Populations, economic systems and policy environments vary along a number of dimensions. The advantage of the US literature is the extensive analysis of living wage policies enacted across more than 140 jurisdictions since 1996. Many of these communities are similar in population size to Auckland, but are different in a number of other

respects. The disadvantage of the US literature is that it relates almost entirely to a compulsory living wage provision that affects local government contractors and sometimes other employers receiving economic development assistance. Only rarely do these living wage provisions apply directly to municipal workers. The advantage of the UK experiences is that they relate to a living wage policy that is more similar in design to the one being contemplated for New Zealand. These living wage provisions would directly affect city employees and contractors, with the potential voluntary uptake by other employers in the area. The disadvantage of the UK literature is the lack of extensive research on the possible effects of the London Living Wage. This is a quandary. The country with the most extensive history and analysis on living wage effects is probably less relevant to New Zealand. The country with less experience and little research on living wage effects is probably more relevant to New Zealand because of a similarity in the type of living wage policy being proposed.

It is clear from inadequacies of some of the evidence presented in this review that the implementation and effectiveness of living wage policies have not been adequately monitored and evaluated, particularly in the UK. It is part of the responsibility of any public authority, in introducing evidence-based policies, to put in place evaluative systems for testing whether the policy has worked. If the Auckland Council Group implements a living wage policy, mechanisms should be put in place to monitor and assess the effects of that policy.

2.0 North American Studies on Living Wage Policies

Baltimore was the first U.S. city to adopt a living wage law in 1994. Since that time, more than 140 jurisdictions in the U.S. (cities and other geographic regions) have implemented living wage ordinances. Early studies used existing local data, survey results and behavioural assumptions to forecast the likely effects of a particular living wage law (e.g., see Pollin and Luce, 1998). These ‘ex-ante policy simulations’ were the only type of analysis possible in these early years given the absence of any historical evidence on the experiences of other cities under living wage laws. Even now, such policy simulations are often done each time a city considers the adoption of a living wage (e.g., see Neumark et al., 2012b, in the case of New York City). This is because each city has a set of unique characteristics and circumstances that mean its outcomes may differ from those of other cities that have previously implemented living wage laws.

However, we’ve now accumulated quite a bit of empirical evidence on the effects of living wage laws across the U.S. cities have operated under these policies for nearly two decades. These findings on wages, employment and poverty from these ‘before-and-after studies’ are summarised below. The results from a broad spectrum of twenty four of the most widely cited published reports in the U.S. are analysed in this section. Brief summaries of these studies can be found in the Appendix to this report.

There are a number of things that need to be mentioned at the outset before we analyse this empirical literature. Firstly, the context for the widespread adoption of living wage laws in the U.S. is important. There were substantial declines in effective federal and state minimum wages since the late 1960s. For example, Pollin (2005, pp. 3-4) noted that between 1968 and 2001 the federal minimum wage fell by 37% relative to the average cost of living and by 80% relative to average productivity growth. Living wages have often been promoted as an alternative policy for reversing this substantial decline in the general wage floor in the U.S. In addition, there has been a considerable increase in wage inequality over this same period. Living wages may be seen as a way of arresting the on-going deterioration in the wages of the lowest paid workers in the U.S.

Secondly, living wage policies are not homogeneous. Some cities impose wage floors on businesses that have contracts with the city (sometimes including non-profits). These are referred to as “contractor-only” living wage ordinances. Other municipalities also impose wage floors on firms that receive business assistance or economic development grants or tax breaks from the city. These are called “employer-assistance” living wage ordinances. Finally, a small number of cities also require that their own employees are paid a living wage. These differences in living wage laws are important because the empirical findings often vary across types of living wage policies.¹

Thirdly, although these policies are referred to as living wages, there is uncertainty over the appropriate level of the wage floor that would be necessary to lift all targeted individuals out

¹ The only jurisdiction that passed a city-wide minimum wage covering all workers had this law overturned by a state Supreme Court (e.g., see the analysis by Pollin et al., 2002, for New Orleans). Santa Monica had a proposed local, general minimum wage defeated in a referendum in 2001 (e.g., see Sander and Williams, 2005).

of poverty (e.g., see Anker, 2006). This is because wages are paid to individuals, but poverty is measured at a family or household level. In the U.S., the living wage is often motivated as the wage that would be required for a single full-time, year-round worker to lift a family consisting of two adults and two children above the official poverty line (sometimes adjusted for higher costs of living in that city). No matter what living wage is adopted or what poverty threshold is set, there will always be “target inefficiency” in the living wage as a policy for eliminating poverty in society. This is because not all directly affected workers necessarily live initially in poor households, wage increases may be insufficient to lift some households above the poverty line (e.g., those in exceptionally large families), and only a small fraction of poor households will directly benefit from most living wage laws (e.g., see Troikka et al, 2005).

Finally, the most important features of living wage provisions are that they have limited coverage rates, but set relatively high wage floors. For example, it is estimated that approximately 1% of workers are directly affected by the contractor-only living wage laws in the U.S., and that even with the addition on employer-assistance provisions these coverage rates range between at most 2% to 3% of workers in a city (e.g., see Neumark and Adams, 2003a and Neumark et al., 2012a). Typically, living wages are set between 30% and 60% above the prevailing minimum wage.

2.1 Evidence on Wage Effects and Employer Costs

The first questions to ask are: (i) Do living wage laws increase the actual wages of targeted workers?; (ii) To what extent are these higher wages passed on to employers in higher production costs, and ultimately born by firms in lower profits or customers in higher prices?

Answers to these questions come from a number of data sources, none of which are ideal. David Neumark, Scott Adams and co-authors used individual data on workers from the Current Population Survey (CPS) in a series of seven published studies (Neumark and Adams, 2003a, 2003b; Neumark, 2004; Adams and Neumark, 2005a and 2005b; Neumark et al., 2012a; and Neumark et al., 2012b). They compared outcomes for workers in cities with and without a living wage law. We’ll refer to these as the CPS studies from this point forward.² The idea was fairly simple. Using data from 1996 onwards, the authors compared wages, employment outcomes and family incomes of workers who were “potentially” affected by living wage ordinances. Similar workers in cities without a living wage policy at the time of the surveys were effectively the “control group” in this analysis. To isolate the possible effects on wages, these CPS studies largely concentrated on workers in the bottom decile of the wage distribution (i.e., workers receiving the lowest 10% of wages). This first set of studies used data from 1996 through 2000, which contained relatively few observations from living wage cities. Later studies were updated with data from later periods (1996-2002 and 1996-2009), which provided many more observations on living wage cities.

These CPS studies have been the subject to some criticism in the literature (e.g., see Brenner, 2005). The CPS records the place of residence, not employment. It can’t identify workers directly affected by living wage laws (e.g., those employed by city contractors or in

² The CPS is similar in design to the Household Labour Force Survey (HLFS) in New Zealand. Both are regular, national population surveys of labour force status, demographic characteristics and income sources and amounts. The CPS has a larger sample size and is conducted monthly, while the HLFS is conducted quarterly.

firms receiving employer assistance). The main alternative has been to conduct what we'll call "case studies". These include both ex-ante policy simulations on proposed living wage laws (e.g., Pollin et al., 2002 in New Orleans), and ex-post studies on specific living wage ordinances in San Francisco (Reich et al., 2005 with on airport workers; and Howes, 2005 on homecare workers), Boston (Brenner, 2005), Los Angeles (Pollin, 2005; Sander and Williams 2005; and Fairris and Bujanda 2007) and Detroit (Reynolds and Vortkamp, 2005).

There is fairly consistent evidence in the literature that living wage laws have an overall impact in raising the wages of targeted workers. This is consistent with similar evidence from general minimum wage studies. For example, the first CPS studies found wage elasticities in the range of 0.05 to 0.07 for workers in the lowest wage decile at least one year after the implementation of living wage policies. This implies that that a 30% increase in the living wage could be expected to increase the wages of lowest-paid 10% of workers by between 1.5% and 2.1%. The magnitude of this effect is surprisingly large given that living wage laws in U.S. cities are estimated to cover between 1% and 3% of workers. The slightly higher estimated wage elasticities suggest that there may be some indirect, spillover effects from this policy. In other words, these living wage provisions may be extended to uncovered workers in this bottom decile. It should be noted, however, that these wage elasticities are not estimated with much precision. In fact, later CPS studies with updated data estimate lower wage elasticities of around 0.04 which are not significantly different from zero. One consistent finding from these CPS studies is that the wage elasticities are generally larger and statistically significant for living wage laws that include employer assistance provisions. The latest study by Neumark et al. (2012) estimates a wage elasticity of around 0.07 for living wage laws containing employer assistance provisions.

These findings of positive wage elasticities are reinforced by the various case studies cited earlier. There is widespread evidence that directly targeted workers received substantial increases in hourly earnings following the implementation of living wage ordinances. Possible issues here are non-compliance or exemptions from these living wage provisions. This is an issue with national, statutory minimum wages. There is no evidence in the literature of non-compliance with living wage laws. This is not surprising given that these policies are more tightly targeted at specific employers and groups of workers. It seems reasonable to conclude that the wage elasticity from living wage laws will be, at a minimum, approximately equal to the coverage rate. In other words, virtually all establishments that are obligated to pay a higher living wage can be expected to comply. In fact, there is some evidence from both the CPS and case studies that at least some uncovered workers will also receive an increase in wages in cities that enact living wage laws.

The next issue involves the extent to which these higher wages translate into higher labour costs for employers. This issue is not directly addressed in the CPS studies, but has been explored in various case studies. The key finding is that the higher wages associated with living wage laws may have relatively small effects on employer costs for a number of reasons. Even though the percentage increase in wages can be substantial for covered workers, not all employees in an organisation are covered (i.e., initially paid less than the living wage). Furthermore, labour costs make up only a fraction of the total costs of production. For example, suppose a firm faces a 30% increase in wages under a living wage ordinance. However, only 25% of its workforce experiences this full increase in wages and

labour costs make up 50% of its total production costs. The one-off increase in the firm's overall costs will be 3.75% ($0.3 \times 0.25 \times 0.5$).

In addition, even modest impacts on employer costs could be further offset by a variety of indirect benefits from living wage laws. One of the most commonly discussed issues in the literature is the possibility that living wages could lead to higher productivity in the workplace. In some cases, previous studies have simply assumed or hypothesised that living wages would lead to these productivity improvements (e.g., Pollin et al., 2002; Pollin, 2005; and Altman, 2012). In other cases, employer surveys cited qualitative evidence of improvements in worker effort or general workplace productivity (e.g., Brenner, 2005; Pollin, 2005; Reich et al, 2005). However, there is no quantitative evidence to date linking living wage laws to actual, measured improvements in labour productivity. This is primarily due to the facts that we rarely observe actual output per unit of labour, and would need to isolate the effects of living wage laws from all other factors that could lead to variations in measured worker productivity across firms or cities.³

There is substantial evidence of indirect productivity benefits from living wage laws through reduced worker turnover (e.g., see Reich et al., 2005; and Howes, 2005). These effects would further reduce the impact of higher wages on firm costs. Less clear is the possibility that living wages might also reduce absenteeism (e.g., see Brenner, 2005 for limited evidence of the opposite effect in Boston). Even more importantly is the considerable evidence on what can be called "labour substitution effects". Once employers raise wages, they face a more experienced and skilled pool of job applicants. Firms will become more productive over time because of this compositional change in their workforces. For example, Fairris and Bujanda (2007) find that employers paying a living wage were able to hire more workers with previous training and those paid higher wages in previous jobs. Similar results have been found throughout the literature.

The net result of all of these factors is a consensus in the literature that even large increases in living wages will have relatively small effects on the overall costs of production for affected employers. Increases in living wages of 30% or more will have net effects of somewhere between 1% and 2% of total costs for the average firm. Of course, it's possible for these costs to be larger for specific firms that have higher proportions of low-wage workers, more labour-intensive production processes and experience smaller beneficial effects from increased productivity, lower worker turnover and absenteeism, and smaller improvements in the quality of job applicants.

Finally, even modest-to-large increases in production costs from living wage laws may be absorbed by the firm in lower profits and by customers or clients in higher prices. Case studies provide some survey evidence from living wage employers that they receive lower profits and pass on their higher production costs to customers in the form of higher prices (e.g., see Brenner, 2005). The ability of employers to absorb higher living wages in lower profits suggests that imperfect competition or barriers to entry exist that lead firms to initially

³ There is little dispute that higher wages would lead to higher productivity in theory. Any increase in the wages that firms face would lead them up to move up their labour demand curves. However, this issue relates more to a possible outward shift in these labour demand functions because of an increase in worker effort or technological or efficiency improvements brought about by higher wages. In practice, even with rarely available measures of output per hour, it is difficult to distinguish between these two sources of productivity improvements stemming from higher wages.

receive excess profits prior to the enactment of a living wage. The ability of contractors to pass on higher costs to the city in the form of higher contract costs also suggests that these living wage laws generally raise costs for all firms that bid on local government contracts. These issues have not been fully explored in the literature.

2.2 Evidence on Employment Effects and Employer Dislocation

The second set of questions relate to potential indirect effects: (i) Do living wage laws reduce the employment or hours of work of targeted workers? (ii) To what extent do these higher wages lead to the relocation of firms outside of living wage cities or deter employers from moving into such cities?

Answers to the first question come almost exclusively from the CPS studies referred to earlier. The authors used cities without living wage ordinances as a kind of control group. They estimated whether the employment rates of individuals who were expected to face wages in the lowest decile were relatively lower in living wage cities. There are two fundamental issues with these studies. Firstly, there is no direct information in the CPS data over whether or not a particular worker would face a higher wage in the event of the passage of a living wage law. The authors can't directly distinguish between covered and uncovered workers. They infer coverage by estimating potential wages and focusing on the individuals with the lowest 10% of expected wages. Secondly, the methodological approach (quasi-experimental) assumes that the implementation of living wage laws was exogenous. Cities that never adopted such laws, or earlier years for cities that did enact such laws, are assumed to form the appropriate counterfactual. In other words, cities with living wage laws could be assumed to have the same employment rates as cities without such ordinances. Despite these caveats, these CPS studies provide by far the best evidence that we have on the possible employment effects of living wages. These potential employment effects are critical to our understanding of the overall effects of living wage policies. Although these laws may be effective in raising the wages of targeted workers, their positive effects in raising income and reducing poverty could be at least partially offset by reductions in employment levels or aggregate hours of work. Like minimum wages, living wages guarantee wages, but not necessarily jobs.

The earliest CPS studies, using data from 1996 through 2000, found evidence of a negative employment elasticity of around -0.13 with a lag of at least one year. This suggests that at least one year after the implementation of a living wage policy, a 30% increase in the living wage could be expected to decrease the employment of workers facing the lowest 10% of wages in the area by approximately 4%. This employment effect was found to be statistically significant only for employer-assistance living wage laws (possibly because of the much lower coverage rates of contractor-only living wage ordinances). There was no evidence of statistically significant detrimental effects on aggregate hours of work.

The latest CPS studies, using data from 1996 through 2009, confirmed previous evidence of negative employment elasticities from living-wage laws. Although the wage elasticities weakened in later studies, the employment elasticities increased in magnitude and statistical significance. The preferred specification by the authors produces an estimated employment elasticity of -0.17. This estimated effect is no longer statistically significant only for employer-assistance living wage laws. This point estimate suggests approximately a 5% loss in

employment for individuals facing the lowest 10% of wages from a 30% increase in the living wage in the average city.

The case study literature does not provide any direct quantitative evidence on the possible employment effects from living wage laws. Partly this is due to the fact that these case studies have no obvious control groups for their analysis. They do, however, often report the results from surveys of affected firms in terms of comparing employment levels before and after the enactment of living wage ordinances. The results here are mixed. Reich et al. (2005) found no clear evidence of lower employment levels of affected firms at the San Francisco airport. Brenner (2005) reported, in his small survey of 51 Boston contractors, that employment levels did not decline after they started paying the mandated living wage. However, Reynolds and Vortkamp (2005) cited some evidence in their survey of affected employers in Detroit that layoffs did result from the implementation of the living wage in this city. In the case of the proposed universal living wage in Santa Monica that was never implemented, Sander and Williams (2005) found that some employers (especially in amusement parks, restaurants and hotels) would have likely reduced their employment levels.

Part of the negative employment effects that might result from a living wage policy could be related to employer location decisions. Bartik (2004) cited his own earlier work that found that even small differences in cost structures could have large impacts on employer location decisions. He hypothesised that living wage laws could have substantial effects on existing employers in leaving a city or deterring other employers from relocating to that same metropolitan area. It would be expected that these employer location effects could be relatively less important for firms that are tied to particular cities through service contracts. This employer relocation issue has been raised in numerous studies (e.g., Pollin et al., 2002; Pollin, 2005; and Sander and Williams, 2005). There has been little research to date estimating the effects of living wage laws on employer relocation outcomes. One exception was a study by Lester (2011) who used a national panel dataset on firms to compare cities with and without living wage ordinances. He found that there were no significant differences in the *number* of establishments across these cities. However, this finding of no impact on the overall number of employers in an area doesn't necessarily indicate that living wages are unimportant for either the location decisions of specific groups of firms or the overall level of employment for low-wage workers.

2.3 Evidence on Income and Poverty Effects

The final question to ask relates to the ultimate motivation for living wage policies: Do these laws increase family incomes and reduce poverty incidence? It is helpful here to briefly review the general findings in this area from the minimum wage literature. The overwhelming consensus is that statutory minimum wages have, at best, minimal effects in reducing income inequality and lowering poverty rates. The literature suggests that minimum wages are not very "target efficient" as an anti-poverty policy. This is because large proportions of minimum-wage workers don't live in poor households (e.g., teenagers and young adults), and large proportions of poor households don't contain working members (e.g., households entirely dependent on social welfare benefits). These facts, combined with possible losses in employment and hours of work for the most vulnerable workers, mean that increases in minimum wages have little or no effect in reducing poverty rates. This has been a fairly

consistent finding in the empirical literature (e.g., see the review of this overseas evidence in Maloney and Pacheco, 2012).

Given this background, it is particularly surprising that the CPS studies referred to above have consistently found empirical evidence of significant anti-poverty effects from living wage laws. Neumark, Adams and their co-authors used the same quasi-experimental approach as they had in finding evidence of positive wage and negative employment effects from living wage laws. Cities with these ordinances are compared to cities without these provisions as a kind of control group. They estimate whether the overall poverty rate is relatively lower in cities with living wage laws. As before, this assumes that the cities that never adopted such laws are an appropriate counterfactual. In other words, cities with living wage laws are assumed to have the same poverty rates as cities without such ordinances. They have the distinct advantage, however, in being able to observe the earlier poverty rates in cities prior to the enactment of these living wage laws.

The earliest CPS studies, using data from 1996 through 2000, found evidence of a negative and statistically significant poverty effect for cities with employer-assistance living wage laws of around -0.0345 with a lag of at least one year. This effect was smaller and statistically insignificant for contractor-only living wage ordinances. This suggests that at least one year after the implementation of this broader policy, a 30% increase in the living wage could be expected to decrease poverty incidence by 1.04 percentage points through higher earnings alone. To put this effect in context, the average city poverty rate was 18.61% in this sample. Thus, a 30% increase in the employer-assistance living wage would be expected to reduce the poverty rate by 5.6%.

The latest CPS studies, using data from 1996 through 2009, largely confirmed previous evidence of negative poverty elasticities from living-wage laws. However, this estimated poverty elasticity was somewhat smaller in magnitude and more variable depending on the specification used. It was again found to be statistically significant for only employer-assistance living wage laws. The preferred specification by the authors produced an estimated poverty effect of -0.024 with a lag of at least one year. This suggests that a 30% increase in the employer-assistance living wage would be expected to decrease the poverty rate by 0.72 percentage points. This suggests that a 30% increase in this broader living wage could be expected to reduce the poverty rate by 3.9%.

Using an entirely different data source, Clain (2008) found confirmatory evidence of an anti-poverty effect from living wage ordinances. The author used data on the county-level poverty rates from 1994 through 2003. Although state minimum wages had no measurable impact on county poverty rates, a negative and significant effect was found for counties that had enacted a living wage law.

Numerous case studies on living wage laws in the U.S. provide no comparable estimates for the impact of living wages on poverty rates. Generally, they provide indirect support for this poverty-reducing effect by citing the general increase in wages affected workers in living wage cities combined with the assumption that there would be minimal if any loss in employment and hours of work.

Adams and Neumark (2005a) realised that their estimated poverty-reducing effects of living wages was out of step with the minimum wage literature that showed little evidence of

similar effects on poverty. In particular, they sought to reconcile these findings with the employment-reducing effects from the same policies. They suggested that living wage laws are particularly effective at raising family incomes for those just slightly below the poverty line. They provide evidence that workers experiencing a positive gain in earnings from these laws may not be those in the lowest wage decile. Correspondingly, workers most likely to experience a loss in employment from living wage ordinances may be the most vulnerable and live in the poorest households. Although employer-assistance living wage laws may reduce the poverty rate, these policies may do little to help the very poorest households in society.

2.4 Social effects of living wages policies

There is very little if any evidence on the social effects of living wage policies. The results that are reported are often difficult to interpret because of small sample sizes and the lack of control groups. The best example of a study in this area is Reich et al. (2005). They looked at the impacts of living wage policies at the San Francisco airport. They compared 83 workers who were directly affected by the living wage with 92 workers who were *not* covered by the living wage. Firstly, this survey ignored the possibility that some individuals may have lost their jobs because of the living wage (displaced workers weren't included in the sample frame). Secondly, even though the authors did survey non-living-wage workers, it wasn't clear that these individuals were a valid control group (i.e., comparable in other respects to the living-wage workers). Finally, the authors concluded that relatively few living-wage and non-living-wage workers reported any improvements in "quality of life" around the time of the implementation of the living wage ordinance. However, living-wage workers were less likely to report *deteriorations* over the last two years in "time spent with family", "vacation time", "housing situation" and "health". The authors concluded that the living wage arrested the downward trends in quality of life, even though these differences were generally *not* statistically significant.

In an evaluation of the Los Angeles living wage (Fairris et al., 2005: 82-3), at least 36% of affected workers reported improvements in their lives as a result of the living wage, from less stress to being able to buy a car to being better able to face a financial emergency. Yet most affected workers do not report dramatic quality of life changes since receiving the raise. This was not surprising to Fairris et al. as, at the time of the research, other costs, like housing and healthcare, had been increasing at a faster rate than the living wage.

- Six per cent of affected workers attributed improvements in their housing situation to the living wage ordinance.
- Two per cent of affected workers said they were better able to support their families with the extra income from the living wage.
- Three per cent of workers said that being paid the living wage reduced their stress.
- Almost a quarter of workers said they spent more money on entertainment after the raise while 11 per cent said they spent less. The higher the raise worker the received, the more likely they were to report spending more on entertainment

2.5 Who are the living wage workers?

The Los Angeles Alliance for a New Economy conducted a survey in 2002 of 320 randomly selected workers who benefited from the Los Angeles living wage ordinance (Fairris et al. 2005). This survey showed that:

- 96% were age 20 and older; 58% were 35 and older
- 86% worked full time
- 71% had only a high school degree or less
- On average, workers had been in the workforce nearly 20 years
- 29% were African American
- 57% were female

Brenner and Luce (2005) surveyed 97 low-wage workers employed in the industries most affected by Boston's living wage policy and found that:

- Workers were predominantly adult, full-time workers
- People of colour were overrepresented; 40% of covered workers were African American
- Women constituted 79% of covered workers
- The average age of covered workers in Boston was 32, with 95% age 20 or older
- Over half (54%) were from households with incomes too low to afford a basic needs budget.
- The average covered worker worked 43 hour per week.

Although we don't have really accurate data on the profiles of living wage workers (age, gender, ethnicity, etc.), these studies suggest they are more likely to be older and primary earners in the household. It is the opinion of the authors that this is the best explanation for the finding that living wages may reduce poverty (direct impacts mainly on older, primary earners), while minimum wages have very weak anti-poverty effects (direct impacts mostly on younger, secondary earners).

2.6 Labour substitution

Fairris and Bujanda (2007) conducted surveys of both employers and workers that were affected by the living wage in Los Angeles. Their final dataset covered 231 workers. They distinguished between "stayers" and "joiners" with the idea that workers hired after the implementation of the living wage (joiners) might be different to workers hired before this ordinance was enacted (stayers). The authors found that joiners were more likely to be male, and either Latino or African American. They also found that joiners were slightly younger and better educated, and had more prior training and received higher wages in previous jobs compared to stayers. No control groups were used, so it was impossible to compare living-wage and non-living-wage workers in this study. No attempts were made to survey any displaced workers. Most of the differences between stayers and joiners were not statistically significant. This study showed that even though employers did not raise or alter their hiring standards, a better applicant pool (perhaps attracted by the higher wages) drives labour substitution effects. Reich et al. (2005) also found some evidence of labour substitution – new hires were better educated and included more males.

3.0 United Kingdom Studies on Living Wage Effects

It is much easier to summarise the literature on living wage policies in the United Kingdom. Unlike the U.S. where the first living wage law was adopted in 1994 and where more than 140 jurisdictions have now implemented living wage ordinances, only the London living wage has received any attention from researchers.⁴ Only six published studies were found on the London Living Wage (LLW). These are summarised and critiqued below. None of these studies are comparable to the CPS analysis discussed in the previous section. This may be partly due to the fact that only a single metropolitan area in the U.K. has adopted a living wage law and relatively little time has elapsed in order to perform the sort of quantitative analysis done by Neumark, Adams and co-authors. There would be inevitable issues about comparing wages, employment and poverty outcomes between London and other cities over the same period. The non-living wage cities in the U.K. may be a poorer control group relative to the non-living wage cities in the U.S.

The first study on the London Living Wage was work done by the Greater London Authority (GLA Economics, 2009). This report summarised the U.S. literature discussed in the previous section, and concluded in the Executive Summary that these studies found that "... Living wage provisions are not generally associated with job losses or worker displacements." This would not be my summary of this literature. Again, the CPS studies found evidence of negative employment effects. It would be fair to say, however, that this evidence is relatively inconsistent and thin (i.e., it hasn't been replicated outside this single line of research). In fact, no research has been conducted to date on the possibility that living wages have resulted in actual job losses or worker displacements (layoffs). This study also claimed that this U.S. literature found "... some evidence of productivity increases." This is equally dubious. This evidence comes from a few case study surveys of employer opinions without reference to a control group of firms or quantitative data on actual changes in productivity levels. It would be fairer to say that this literature provides some evidence of both negative employment and positive productivity effects, but the evidence on both is limited and somewhat inconclusive. Given the fact that living wage laws are generally promoted as anti-poverty policies, it was surprising that this study by GLA Economics failed to mention the rather surprising findings of the significant effects of living wage ordinances in reducing overall poverty rates.

This GLA Economics study reported the results of a survey of firms that had implemented the London Living Wage. Consistent with similar employer surveys in the U.S., they reported evidence of positive effects on reputation, recruitment and retention, worker morale, productivity, etc. They also reported some evidence of associated negative effects in having to re-negotiate contracts, increasing wage and production costs, and declining profits or share prices. No attempts were made to verify these possible outcomes by examining the actual outcomes experienced by these firms, or to measure the net effects of these positive and negative effects. Furthermore, no attempts were made to examine any changes in the composition of employment among these firms that paid the LLW.

⁴ There are references to other U.K. living wage campaigns in various studies but no mention of whether or not these other policies have been implemented outside of London or what effects these ordinances might have in these other cities.

A case study on the effects of the introduction of a living wage among cleaners at Queen Mary, University of London in 2008 was conducted by Wills and her co-authors (2009). They carried out both on-line surveys and face-to-face interviews with affected cleaners and other relevant parties at the University. They found evidence of a general increase in job satisfaction, improved productivity and service delivery. This was associated with little increase in employer costs. There are two important points to mention with regard to this particular case study. Firstly, two fundamental changes occurred simultaneously in this situation. Not only were wages and benefits increased as part of this living wage campaign, but the employees of a contracted firm were offered employment at the same time with the University. This made it difficult to attribute any reported effects specifically to the living wage. In other words, some or all of the reported positive effects could have occurred solely because this cleaning service was moved “in-house”. Secondly, and more intriguing, is the possibility that these changes resulted in a reduction in either employment levels or hours of work for cleaning staff. This issue is not explored directly in this study. However, Wills reported that there was a considerable loss in the number of cleaning staff as the service was moved in-house (due to immigration and other issues for existing employees). Despite the substantial increase in pay and benefits, only minimal increases in overall employment costs were reported between 2006/07 (pre-living wage) and 2008/09 (post-living wage). This suggests at least the potential for a decrease in aggregate hours of employment for this same cleaning service.

Morelli and Seaman (2010) produced a discussion paper on the potential effects of the London Living Wage in reducing income inequality in the U.K. The authors explored the theoretical underpinnings of the LLW policy. They suggested that if the relevant low-wage labour market is characterised by pervasive “monopsony effects” then the implementation of a living wage may not have the negative employment effects that would be anticipated in a more competitive labour market. This claim is similar to the one made by Card and Krueger (1995) in their explanation for the absence of any evidence of a negative employment effect in raising some statutory minimum wages in the U.S. The key is that employers of low-wage workers must have some “market power” in the hiring process. Because they individually face upward-sloping supply curves, they tend to have an incentive (and the ability) to restrict employment and lower the wages that they pay. As a result, increases in wage floors (either through minimum or living wages) could result in increases, rather than reductions in employment. What is not discussed in this report is the possibility that a sufficiently high wage floor could still lead to a reduction in employment even in a monopsony setting. This is particularly relevant when examining the large increases in living wages over prevailing minimum or market wages. In the end, no empirical evidence was produced in this study to support the hypothesis of pervasive monopsony forces in the U.K. The authors used individual level data from the Quarterly Labour Force Survey between 2005 and 2008 to estimate what would likely happen to labour costs if a particular living wage was adopted, assuming away any potential negative employment effects. The authors found that the main beneficiaries would be youth and women (particularly single parents), and that this increase in the wage floor could reduce overall income inequality. In particular, living wages would have a relatively small impact on the overall labour costs in the public sector.

A second study by GLA Economics (2012) was produced at the time of the announced increase in the London Living Wage to £8.55. This report surveyed the rationale, methods and calculations behind the setting of the LLW. This wage floor was set at 38% above the

National Minimum Wage of £6.19 at the time. It also discussed the success of getting private employers to sign up to this Living Wage Accord. Unlike the mandated living wages in U.S. cities, the London campaign is based largely on the voluntary compliance of employers in paying this stipulated wage. Complying employers are listed in this publication in the same way that other campaigns often publically identify accredited organisations (e.g., the “Fair Trade Mark”).

One of the issues not directly discussed in this report, is the relatively low coverage rate for the London living wage. Data from the Annual Survey of Hours and Earnings showed that that 18.7% of workers in London at the time were paid less than this £8.55 living wage (GLA Economics, 2012, p.25). Earlier in the same document it was stated that an estimated 11,500 workers had directly benefited from the London living wage (p.5). If aggregate employment in London was 4,000,000 at the time, and 18.7% of these workers were paid less than this living wage this implies that only 11,500 of approximately 748,000 low-wage workers benefit from this living wage. This is a “coverage rate” is around 1.5% for low-wage workers and less than 0.3% for all workers. These are probably overestimates of the true coverage rates because the estimated 11,500 direct beneficiaries of the LLW is a cumulative figure compiled over a number of years. The true coverage rates are probably much lower. Thus, the LLW at this point directly benefits probably less than one of out of every 100 workers being paid less than this living wage. This coverage rate under this voluntary arrangement is most likely substantially below the coverage rates of the mandatory ordinances in many U.S. cities.

Another British study examined the costs and benefits of the London Living Wage (Wills and Linneker, 2012). The authors traced the roots of the LLW campaign back to similar campaigns in the British coalfields in the 19th century. Wills and Linneker attempted to compare the outcomes for 17 employers between those that did and did not comply with the LLW. They experienced data collection issues and ended up with only seven employers that provided data both before and after their compliance with the LLW. These included employers almost exclusively from the cleaning industry. They attempted to match LLW employers with similar firms that didn't pay this living wage. Consistent with previous U.S. studies, the authors found evidence of relatively lower worker turnover rates among LLW firms. However, because of low reported costs in recruitment, they concluded that this reduction in turnover had a minimal impact of cost savings for LLW employers. Also consistent with previous U.S. studies, there was evidence of labour substitution effects for employers paying the higher living wage. These LLW firms reported being able to hire more native-born, educated and disciplined workers, and those with higher wages in previous jobs. Although no direct information was apparently solicited from firms on changes in employment that might have resulted from paying a higher wage, the authors did report results from their interviews with LLW firms that are consistent with lower employment levels (e.g., getting existing staff to work harder, more closely monitoring/inspecting work on job sites, etc.). Also consistent with the U.S. literature, Wills and Linneker concluded that as a result of the aforementioned responses to the LLW only a portion of the increase in wages was passed through in higher contract costs.

This same study also reports the results of surveys with 416 workers in living-wage and non-living-wage firms. Surprisingly, those working for LLW employers did not report a higher average expectation of longer tenure with their current firms. The authors reported a

significant difference in indicators of psychological health between workers in LLW and non-LLW firms, but concluded that the better psychological health of the higher paid workers could not be linked in a causal sense to the living wage. Although the majority of workers in LLW firms reported receiving benefits from policy, it was noteworthy that 35% of these workers reported no benefits from operating under a living wage. The authors also found evidence that both overtime hours and end-of-year bonuses were reduced in firms after they began to pay the LLW. Finally, calculations were made over the possible cost savings to the government if all low-wage workers in London were paid the existing LLW. They estimated that increased taxes and reduced transfer payments from this extension of the living wage to 580,000 low-wage workers would save the government £823 million per year. This is a highly contentious claim, given that they have no estimates for the possible reductions in employment or hours of work among firms currently paying the LLW, and no way of knowing what these effects might be if this policy was scaled-up and made compulsory among all employers in London. An ex-ante policy simulation of this order would require a much more extensive and deeper analysis.

One final study by Lawton and Pennycook sought to conduct "... the first full economic analysis of living wages in the UK" (p.9, 2013). It falls well short of this stated goal. Instead, it estimated the magnitude of the possible employment losses from a "universal extension" of the living wage throughout the country. The authors estimated that this universal provision of a living wage would result in a loss of 160,000 jobs or 4% of the number of workers who would likely experience an increase in earnings under the living wage. There is no direct information in this report on the basis for this estimated employment effect. Similar employment estimates were *not* produced for the current London Living Wage, although the authors suggested that they would likely be negligible given its voluntary nature.

The authors go on to estimate the fiscal implications of this same universal adoption of the living wage. They concluded that there would be a substantial benefit to the government in reduced transfer payments and increased tax revenue. Strangely, the authors base these calculations on an assumption that there would be no loss in employment from this universal living wage immediately after estimating that it would likely result in a loss of 160,000 jobs. The study concluded with a set of recommendations that are difficult to reconcile from the evidence presented in this report. Firstly, the living wage should *not* be made compulsory or universal because of the potential negative employment effects. Secondly, the current voluntary living wage with a low coverage rate can be justified because of an absence of significant negative employment effects and clear fiscal benefits for the government, despite the fact that neither of these claims is directly tested in this analysis.

4.0 New Zealand Studies on Minimum Wage Effects

There are a number of key differences between minimum wages and living wages, but because both policies are essentially “wage floors” this domestic minimum wage literature may have some important implications for understanding the potential effects of living wages in this country. The biggest differences between minimum and living wage policies is that the former have more-or-less complete coverage over all low-wage workers over a wide geographic area, where the latter have limited coverage over a specified set of low-wage workers in a narrow geographic area. Living wages are also often set well above the minimum wage, reflecting the fact that they are based on living costs. But there are other important differences between these policies that have ramifications for their potential economic effects. Because living wages are often targeted primarily (if not exclusively) at local government contractors, they may be less “price sensitive.” In other words, employers who hold contracts with the city may be able to pass on higher living wage costs through to the city and ultimately the tax or rate payers. Because living wages may cover only those employees of a firm that are working on a public contract, employers may be able to reallocate workers within the organisation. They may be able to shift more highly skilled and educated employees over to public contracts. Because of incomplete coverage, displaced workers or employees who lose their jobs in covered firms may simply end up working in uncovered firms. This reallocation of displaced workers to the uncovered sector could, in theory, actually result in lower wages than those received prior to the enactment of the living wage. This can’t happen under minimum wage laws with universal coverage. Finally, because firms can opt out of receiving economic development assistance, they can avoid having to pay these higher wages under business-assistance living wage laws. The same is obviously true when cities like London introduce a voluntary living wage accord. Firms find it much more difficult to opt out of having to pay statutory minimum wages.

There have been numerous studies of the effects of the statutory minimum wages in New Zealand since 1995. Any earlier research on the impacts of the minimum wage would have been extremely difficult given the complications of the centralised Awards System for setting sectorial minimum wages. This Awards System was abolished in the 1991 Employment Contracts Act. This gave the statutory minimum wage a much more prominent role as an effective, universal wage floor in New Zealand. Until April 1994 teenagers were specifically exempt from minimum wage protection. Maloney (1995) used this situation to isolate the employment effects of minimum wages on young adults aged between 20 and 24, and estimated an employment elasticity of -0.35 for this group. He also estimated a positive employment elasticity for teenagers of roughly the same magnitude. This suggested that covered young adults suffered a loss in employment, while uncovered teenagers indirectly benefited from an increase in the adult minimum wage. This provided some evidence of a labour substitution effect from experiences with this exclusive adult minimum wage between 1986 and 1993.

Hyslop and Stillman (2007) showed evidence of a possible positive effect from the introduction of the teenage minimum wage in New Zealand on the hours of work of 16 to 19 year-olds. These results parallel the results of Card and Krueger in the U.S. However, these results are somewhat mixed. Hyslop and Stillman estimated negative employment elasticities for teenagers, but this effect is more than offset by an increase in hours of work

following substantial increases in teenage minimum wages beginning in 2000. They also find unusual, and difficult to explain, positive impacts of teenage minimum wages on labour supply, unemployment and economic inactivity.

Two final studies are of particular relevance for understanding the potential impacts of introducing a living wage for the Auckland Council Group. The first study is by Maloney and Pacheco (2012). The authors used individual-level data from the Income Supplements to the Household Labour Force Survey over the 1997 to 2008 period to estimate the possible anti-poverty effects of the substantial increases in both the adult and teenage minimum wages beginning in 2000. They found evidence of very small reductions in the New Zealand poverty rate from these minimum wage increases. They estimated that, ignoring any loss in employment or hours of work as result of an increase in the wage floor, a 10% increase in the minimum wage would reduce the proportion of households living below 50% of adjusted median income by 0.1 percentage points. Even this small poverty reduction would disappear completely if a 10% increases in these minimum wage lead to a 3% reduction in hours of work.

The study by Hyslop et al. (2012) may be particularly relevant for the debate over the adoption of a living wage policy by the Auckland Council Group. They used data from the Linked Employer-Employee Database (LEED) from April 1999 through March 2007 to estimate the extent to which employers may have reduced their share of teenage workers as a result of the substantial increases in the teenage minimum wages since 2000. They also estimated whether or not these higher teenage minimum wages might have reduced the survival rate of firms that initially used high proportions of teenage labour. Most importantly, the firm was the unit of analysis for their study unlike the individual worker in the previous minimum wage studies above and the earlier CPS studies in the U.S. The authors found that teenage employment tends to highly concentrated in particular industries, and in certain firms within these industries. Firms that had high levels of teenage employment in 2000/2001 reduced their share of teenage workers by 15 to 20 percentage points relative to other firms by 2006/2007. They were also less relatively likely to survive over this period (by 5% in the main teenage-employing industries and by 10-20% in other industries).

The authors concluded that the costs of increasing the teenage minimum wages were mainly born by a small subset of firms that employed large numbers of teenage workers. Even though the cost increases associated with these higher minimum wages were small for the average firm (and potentially easily absorbed), they were substantial for a small group of firms. One particularly surprising result was that Hyslop et al. found evidence that firms that entered the main teenage-employing industries tended to have relatively *higher* proportions of teenage workers compared to continuing firms in that industry. They suggested that newly entering firms are able to adapt their technology more readily than continuing firms in being able to use a higher proportion of teenage workers more efficiently. This is an interesting result and interpretation. However, it may be that this is also a normal growth process for new firms entering these industries. It may be part of the maturation process for firms, in that they might start out with relatively high proportions of teenagers as they initially expand. One way to test this proposition would be to look similar outcomes for firms in these industries over a period when minimum wages were not increasing.

5.0 Living Wage Employers

5.1 Type of employer

The Greater London Authority (GLA) report on the 2012 living wage in London lists accredited living wage employers, employers in the process of accreditation and employers that are committed to paying the living wage but not seeking accreditation (Greater London Authority, 2012). Of the 196 employers listed, the majority are in the private sector (92 or 47%) and the third sector (67 or 34%). There were 19 public sector living wage employers (10%), 12 higher education living wage employers (6%) and 4 education living wage employers (2%). Most of the private sector employers listed by the GLA are legal (22 or 24% of private sector living wage employers) or finance (35 or 38%) companies.

Lawton and Pennycook (2013: 8) give the following characterisation of London living wage employers:

Despite recent progress, the number of accredited living wage employers – primarily high-profile financial and legal firms and public sector bodies – remains small. Aside from a handful of notable exceptions such as the cosmetics retailer, Lush, and eight London hotels under the management of Intercontinental Hotel Group (IHG), relatively few companies in sectors with a large proportion of low-paid workers have become living wage employers.

Many of the high-profile financial and legal firms described by Lawton and Pennynook are large, multinational businesses such as KPMG and JP Morgan.

In the United States, living wage employers are predominantly businesses with municipal or state government contracts or businesses that receive some form of support from municipal or state government. Contractors are likely to work in areas such as cleaning, waste management, janitorial services and landscape maintenance. Businesses receiving support may include real estate developers that receive public subsidies (and their subcontractors). The table below shows industries affected by the Los Angeles living wage.

Table 2: Los Angeles Living Wage Affected Firms and Jobs by Industry Groups

Industry Group	% of Affected Firms	% of Affected Jobs
Airline Services	3%	30%
Janitorial	13%	12%
Landscape Maintenance	10%	2%
Miscellaneous	23%	8%
Retail and Food Service	23%	10%
Security and Parking	6%	31%
Social Service	23%	8%

Source: Living Wage Employer Survey, weighted by firm and by number of jobs where mandatory raises were given. N = 82 Margin of error ranges from $\pm 3\%$ to $\pm 11\%$ (Fairris et al., 2005: 21)

Some living wage ordinances exempt non-profit and small businesses. For example, non-profit and small businesses were initially exempt from the San Francisco living wage and then paid a lower rate until 2006. Other living wage policies specifically target big businesses. For example, the Washington D.C. City Council is currently considering a 'Large Retailer Accountability Act', which would require corporations operating stores of 75,000 square feet or greater to pay employees a living wage of \$11.75 per hour.

The tendency for living wage employers to be large businesses is confirmed in the Los Angeles case study. Over a third of living wage affected establishments in L.A. County have more than 100 employees, compared to only 2 per cent for establishments in similar industries in the County. Less than half of all living wage affected establishments have fewer than 20 employees, compared to over 80 per cent for establishments in similar industries (Fairris et al., 2005: 23).

5.2 Level of living wage

Living wage rates are based on the cost of living and therefore differ between countries and cities.

The New Zealand living wage of \$18.40 is 34% higher than the national minimum wage of \$13.75 and 88% of the median hourly earnings of \$20.86.⁵

In 2012 the UK National Living Wage (NLW) was set at £7.45 (NZ\$14.02) and the London Living Wage (LLW) at £8.55 (NZ\$16.10). This compares to a National Minimum Wage (NMW) in the United Kingdom of £6.19. In 2005 the London living wage was 33% above the NMW of £5.05. Since then the differential has risen in absolute terms from £1.65 to £2.36. The 2012 LLW is 38% above the NMW (Greater London Authority, 2012) and 54% of the median hourly earnings of those who work in London.⁶

In Canada, First Call and Victoria's Community Council have calculated a 2013 living wage for Metro Vancouver of \$19.62 (NZ\$23.57) per hour (Ivanova and Klein, 2013). This is 83% above the state minimum wage in British Columbia of \$10.25 and 81% of the average hourly wage (\$24.26) in British Columbia.

A number of cities in the United States have living wage ordinances that stipulate that a wage higher than the federal minimum wage (FMW) of \$7.25 has to be paid by all employers with municipal contracts. The table below shows the great variety in living wage rates across the United States.

⁵ http://www.stats.govt.nz/browse_for_stats/income-and-work/Income/NZIncomeSurvey_HOTPJun12qtr.aspx

⁶ http://www.ons.gov.uk/ons/dcp171766_299377.pdf

Table 3: Living Wage Rates in the US

City	Living Wage Requirement (May 2013)*		% greater than federal minimum wage (\$7.25)	% of median wage (\$16.71)
	US\$	NZ\$		
Minneapolis, Minnesota	\$14.41	\$17.87	99	86
Burlington, Vermont	\$13.94	\$17.29	92	83
Santa Monica, California	\$13.82	\$17.14	91	83
Montgomery, Maryland	\$13.65	\$16.93	88	82
Boston, Massachusetts	\$13.49	\$16.73	86	81
Hartford, Connecticut	\$13.30	\$16.49	83	80
Ann Arbor, Michigan	\$12.17	\$15.09	68	73
Madison, Wisconsin	\$12.19	\$15.12	68	73
Durham, North Carolina	\$11.91	\$14.77	64	71
Oakland, California	\$11.70	\$14.51	61	70
Chicago, Illinois	\$11.53	\$14.30	59	69
Denver, Colorado	\$11.08	\$13.74	53	66
San Antonio, Texas	\$11.08	\$13.74	53	66
Rochester, New York	\$10.77	\$13.35	49	64
Baltimore, Maryland	\$10.73	\$13.31	48	64
Los Angeles, California	\$10.70	\$13.27	48	64
San Francisco, California	\$10.24	\$12.70	41	61
San Jose, California	\$10	\$12.40	38	60
Tucson, Arizona	\$9.84	\$12.20	36	59

Note: In many cases different living wage rates are stipulated depending on whether the employer provides health benefits as well. All of the living wage rates in this table are 'with health benefits' rates.

The Fair Wages for New Yorkers Act, passed by the New York City Council in June 2012, stipulates that a living wage of \$10 per hour base wage plus an additional supplement of \$1.50 per hour (which may in the form of either health benefits or supplemental wages) has to be paid by (a) employers that directly receive financial assistance; (b) tenants operating on property developed with financial assistance; and (d) on-site service contractors such as staffing agencies or food service contractors operating on property developed with financial assistance.

A number of states and cities have higher minimum wages than the federal minimum wage. As of May 2013, the state minimum wage is \$9.19 in Washington, \$8.60 in Vermont, \$8.25 in Nevada, Illinois and Connecticut, and \$8 in California and Massachusetts. The minimum wages in San Francisco and Santa Fe are significantly higher than the federal and state

minimum wages at \$10.24 and \$10.51 respectively.⁷ Albuquerque has a minimum wage of \$8.50.

The Living Wage Law in the state of Maryland (effective as of October 1, 2007) requires certain contractors and subcontractors to pay minimum wage rates to employees working under certain state services contracts. The Living Wage Law currently requires the payment of the living wage of either \$12.49 per hour, or \$9.39 per hour effective depending upon the jurisdiction where the services are performed. The State living wage does not apply to county and municipal contracts although some local governments such as Montgomery County and Baltimore City (see above) have their own living wage requirements.

5.3 Maintaining wage differentials

Indirect wage increases are those received by workers not covered by the living wage but still affected by it. Vertical wage increases occur in firms covered by the living wage policy when workers earning at or above the mandated wage receive increases in order to maintain some or all of the wage differentials within the firm. There is limited evidence on how living wage employers maintain wage differentials through vertical wage increases.

In an evaluation of the Los Angeles living wage, Fairris et al. (2005: 45) found that, in addition to providing the mandated raise in wages, thirty-nine per cent of affected firms said they gave non-mandated raises to additional workers in order to maintain wage differentials on city contracts. On average, these workers received a non-mandated raise about half the size of the average mandated pay increase. It appears that the vertical wage push mostly affected other low wage workers. On average, the non-mandated raise was given to workers who earned up to 12% more than the living wage.

Firms provided several reasons for giving vertical non-mandated raises. Many firms said they did so to maintain fairness in the wage structure, while others cited employee complaints, particularly among supervisors making only slightly more than the workers they oversee. Other firms said the vertical raise made it easier to recruit supervisors.

In an analysis of the introduction of a living wage at San Francisco International Airport, Reich et al. (2003) found that vertical indirect wage increases were relatively small, and that most of the indirect wage increases were across, rather than within firms.

In a review of the London living wage, Wills and Linneker (2012) describe the difficulties of a grounds service contractor that failed to factor in the real costs of maintaining wage differentials above the living wage. This was costing the contractor money that had to be taken out of their margins. In addition to costing the business, the reduction in wage differentials that resulted from the introduction of the living wage was aggravating staff. As one respondent explained: *“People who were on a slightly higher wage felt that they should have been given a step up with the living wage. They should have gone through the whole spectrum of staff. We have not been rewarded and why should we take on more responsibility if we are not getting paid more?”*

⁷ <http://sfgsa.org/index.aspx?page=411>; <http://www.santafenm.gov/index.aspx?NID=84>

5.4 Rationale for becoming living wage employers

Living wage campaigners frame their debates in terms of justice and decency as well as addressing poverty and inequality.

Living wage ordinances have been passed in a number of US cities in response to campaigns, but there is little information available on the reasoning behind the decision to pay a living wage. There is some information available on individual US employers that pay higher wages.

Costco Wholesale, a large format retailer in the US, pays its employees significantly higher rates than similar employers like Wal-Mart. Higher rates of pay for part of Costco's commitment to 'take care of our employees' in their Code of Ethics. As well as being framed in terms of ethics, Costco's higher rates of pay are explained in terms of 'good business sense'. According to Costco CEO Craig Jelinek,

An important reason for the success of Costco's business model is the attraction and retention of great employees. Instead of minimizing wages, we know it's a lot more profitable in the long term to minimize employee turnover and maximize employee productivity, commitment and loyalty. We support efforts to increase the federal minimum wage.⁸

At San Francisco International Airport (SFO) a living wage was introduced as part of a Quality Standards Program (QSP), which was designed to improve safety and security as well as improve the conditions of the SFO labour market. Reich et al. (2003) suggest that low pay at SFO was seen as problematic in itself and as a contributor to high turnover, lower service quality and low security standards.

There is a little more evidence on the motivations of UK employers. In their research on the London living wage, Wills and Linneker (2012: 22) found that all the employers and 'clients' (those who sub-contract living wage employers) they spoke to had made the decision to move towards the living wage on ethical and/or reputational grounds before the issue of costs was explored.

In New Zealand, The Warehouse has explained its decision to pay higher wages in business terms - to improve the public perception of retailing, attract more talented staff, increase team management, lower team turnover, improve sales and raise productivity.

⁸ <http://www.politicususa.com/costco-proves-republicans-wrong-paying-living-wage-watching-profits-soar.html>

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Appendix A North American Studies on Living Wage Policies

Year, Author(s) and Publication Location	Sample and/or Focus of Study	Summary and Analysis of Findings (in Chronological Order)
2002 Pollin, Brenner and Luce <i>Journal of Economic Issues</i>	Case study: New Orleans LWO (February 2002). Survey data from 444 New Orleans firms in 1999.	Broad proposed LWO that would raise the local MW to \$6.15 (19.4% above the federal level). Overturned by state Supreme Court in Sept 2002. The survey includes 444 firms employing 23.4% of workers in New Orleans. Estimated that 26.3% of workers earned less than \$6.15, but a ¼ of those earned less than federal MW. Assume 'ripple effects' (i.e., wage increases to some above \$6.15). Conclude that New Orleans firms would be able to absorb cost increases of LWO through a combination of price increases and productivity improvements. Acknowledge possibility of firm relocation. No new empirical findings to back up these claims, and limited literature support.
2003a Neumark and Adams <i>Industrial Relations</i>	1996-2000 monthly CPS rotation group data. LW laws in 36 US metro and other areas	Limitations in using CPS data are: (i) that we can't directly distinguish between 'covered' and 'uncovered' workers; (ii) small number of observations on LW laws. They estimate wage elasticities for those in bottom decile of 0.05-0.07 (large relative to estimated coverage rates of 1-2%). Negative employment elasticity of -0.14 for bottom decile. Although statistically weak or inconsistent disemployment effects, authors point to strong associations between situations where the wage effects are positive and employment effects are negative. These estimated effects only appear in LW laws that include employer assistance provisions (possibly due to broader coverage).
2003b Neumark and Adams <i>Journal of Human Resources</i>	1996-2000 monthly CPS rotation group data. LW laws in 36 US metro and other areas	They estimate wage elasticity for those in bottom decile of 0.07. This estimated effect is larger (0.11) for those expected to be in the 'covered sector'. The time lag is particularly important. It takes at least one year for these positive wage effects to show up. They estimate a negative employment elasticity of -0.13 for the bottom decile (using predicted wages). Some evidence of a substitution effect higher in the wagger distribution. No evidence of a statistically significant impact on hours of work. Authors find evidence that LW laws reduce poverty through labour market earnings alone. This also requires at least a one year lag, with an estimated elasticity of -0.19. The estimated effects of LWs on income poverty of smaller and marginally significant. The increase in earnings from higher LWs may be partially offset by a drop in social welfare benefits or income support.

2004 Bartik <i>Urban Affairs Review</i>	No new data. Largely a summary of predictions and qualitative studies from Baltimore, Detroit and LA and cites other research from Bartik.	Mixed results. 'Moderate' LWs most likely have positive effects on local government workers and their contractors. 'Excessive' LWs or LWs applied to other workers may have negative side effects. Dependent on state of the local economy. May reduce economic growth in weak local economies. No new empirical analysis. Largely a summary of work by Neumark and Adams. Concern about incompatibility of findings, especially large employment losses and poverty reductions. Author's own work suggests that business location decisions may be sensitive to even small cost differences. Could have even more detrimental effects on employment over the long run.
2004 Neumark <i>Industrial and Labor Relations Review</i>	1996-2000 monthly CPS rotation groups data. LW laws in 36 US metro and other areas	LW laws covering contractors have significant positive effects on unionised local government workers earning below the median wage in the metro area. Estimated elasticity around 0.18, implying that a LW 30% above the MW will increase workers' wages by slightly less than 6%. No measured effects for non-union or non-governmental workers, or for LW laws covering economic development assistance. Suggests that this evidence is consistent with the hypothesis that LW laws exist to protect unionised city workers from low-wage competition by potential contractors. LW laws reflect the rent-seeking behaviour of lower-wage unionised city workers.
2005a Adams and Neumark <i>Economic Development Quarterly</i>	1996-2002 monthly CPS rotation groups data.	The authors update their earlier analyses with data from January 1996 through December 2002. Estimated wage elasticity has fallen to 0.04 and is now insignificant. It's 0.07 and significant only for business-assistance LW laws. Address a number of criticisms raised by Brenner et al. (2002). The biggest is that the wage effect is insignificant in a quantile regression. The authors appropriately note that this asking a completely different question. The issue at hand is whether LW laws increase the absolute wages of the poorest workers, not relative wages. Negative employment elasticity is now about -0.12. Estimated effect is again larger (and only significant) for business-assistance LW laws. Estimated poverty elasticity remains at -0.19 (see Neumark and Scott, 2003, <i>JHR</i>). LW laws don't reduce the depth of poverty. Helps families both below and above the poverty line. Loss in employment may still fall largely on low-skill, low-wage workers.
2005b Adams and Neumark <i>Journal of Urban Economics</i>	1996-2002 monthly CPS rotation group data. Restrict analysis to cities with successful and failed campaigns	Use cities with failed LW campaigns as a control group for estimating the effects of LWOs in other cities. Maybe this can explain their earlier results of a wage elasticity of around 0.07 when coverage is around 0.02 (would expect a maximum wage elasticity of one). Confirms earlier results of the authors about the magnitude of the positive wage elasticity and negative employment elasticities for the bottom decile of workers. It's the laws themselves that matter, and not the campaigns.
2005 Reich, Hall	Case study: San Francisco Airport LWO 1999	This LWO at San Francisco International Airport (SFO) directly affected nearly 1/3 rd of the 30,000 employees. An unusual aspect is that it raised educational standards for new hires and mandated more training for current staff. Since this covered all workers at the SFO, this is more closely related to the minimum wage studies than other LWO situations. Claim that recent studies on the state MW in California suggests no loss in employment likely

and Jacobs		from LWO. Directly increased the wages (and health benefits) of low-wage workers, with some spillover to uncovered firms (horizontal ripple effects). May have arrested deterioration in quality of life for these workers (small sample of workers). Substantial decreases in worker turnover, productivity improvement based on employer survey. Overall cost were 0.7% of airline revenue or \$1.42 per passenger. No evidence (although limited) on a negative impact on economic activity at SFO or employment. Some evidence of labour-labour substitution (more male, better educated hired).
2005 Brenner	Case study: Boston, Massachusetts. 2001 survey on city contractors	Criticise studies by Neumark and Adams. CPS data can't identify place of employment, too few covered workers in resulting samples, and conflicts with other work showing larger effects for LWO targeting contractors not business assistance. LWO passed in 1998 exempted small contractors and non-profits. Surveyed all covered firms. 51% response rate (approximately 40% of all covered contracts). Just under 1/4 of firms had to raise wages under the LWO. Their DID analysis uses affected firms as the treatment, and unaffected firms as the control. One concern is that the survey was conducted 3 years after the LWO. Includes a very small sample of at most 51 firms. No evidence of a reduction in employment for affected contractors. Evidence of a switch to full-time workers. Counterintuitive finding on turnover and absenteeism (fell for unaffected, but not affected firms). Report some improvement in employee effort and morale among affected firms, but no controls for this analysis. Evidence that 38% firms reduced profits and only 8% increased prices, but again no controls.
2005 Freeman	Survey piece on LW campaigns	Discusses the origins of Living Wage campaigns. Points out differences in trade unions and Non-Worker Organisations (NWOs) that are behind many of the local LWOs in the US. This is an advantage in that public see NWOs as more objective and independent than a self-interested trade union. This is why Harvard students are painted as heroes. They're occupying buildings on behalf of a completely separate groups of low-wage workers. LW campaigns often depend on the formation of coalitions across numerous community groups. Local LW campaigns attract more support than broader national campaigns for worker benefits. They can be tailored to local circumstances (e.g., how large a wage increase? Which employers will be targeted?). But there's a price to be paid here too. Targeting small groups means less impact. Also LWs mean more taxes and less benefits, reducing net wage increases by up to 1/2. (p.25 ...while local living wage campaigns are a potentially efficient way to link higher pay to local market realities, they cover too few workers to have as large an impact on poverty as other anti-poverty policy tools.
2005 Howes	Case study: Homecare workers in San Francisco	Following the LWO (which nearly doubled the wage for these homecare workers between 1998 and 2000), turnover fell by 57% for new providers and 31% for all providers. Important to note that many of these providers are taking care of own-family members. Logit analysis suggests that this decline in turnover is related to both the increase in wages and the provision of health care insurance under the LWO.
2005 Pollin	Uses largely case study data from Los Angeles, New Orleans and Santa Monica	Motivates the need for a LW with a decline in the purchasing power of the MW and the rise in average labour productivity in the US since 1968. Admits that the appropriate level of the LW is not obvious even at a conceptual level. Explores several approaches. Costs generally range from at most 1-2% of firm revenue (higher in Santa Monica among hotels and restaurants). Do LWOs result in job losses, firm relocation and excessive strains on municipal budgets? Suggests that competition will stop contractors from passing higher costs on to cities. Some will be absorbed by normal productivity improvements. Simply doubts other negative effects will exist. No direct empirical evidence is offered. Expects costs to widely shared by firms, governments, taxpayers and consumers.

<i>Quarterly</i>		
2005 Reynolds and Vortkamp <i>Economic Development</i> <i>Quarterly</i>	Case study: Detroit LWO Employer survey of non-profits	Conducted a phone survey of non-profit employers following the 1998 enactment of a LWO in Detroit. 96 large non-profits identified with city contracts. Survey had a response rate of 2/3 rd s. Most said effects would be minor or minimal. A few did suggest major effects that could include layoffs or a reduction in services. Key is that there is no control group. No quantitative assessment of potential job losses, but conclude that there is no evidence of ... appreciable job losses. Conclude that if there are negative employment effects, they are more likely to be found for (more vulnerable) non-profits. Firstly, this effect wasn't really shown. Secondly, non-profits may be more likely to protect the jobs of their workers in the face of LWOs.
2005 Sander and Williams <i>Economic Development</i> <i>Quarterly</i>	Case study of Santa Monica LWO Employer survey	Proposal in 2001 to more than double (\$12.25) the federal and (nearly \$6.75) state MWs was defeated in a referendum. Conclude that this proposal was 'deeply flawed'. Key: this is a (large) neighbourhood in LA. Creates particular problems when applied to a specific region within a metro area. Proponents felt area restaurants and hotels were distinctive, earning monopoly rents and wouldn't relocate as a result. Employers could seek exemptions if they not be viable paying the LW (e.g., pier amusement park). Conclude that hotels wouldn't relocate (poor understanding of the issue of 'sunk costs'), restaurants would be particularly vulnerable, high wage costs and low profit margins, large retailers would very likely relocate. Hypothesise that this LWO would have increased poverty. Low wage workers would receive few benefits but bear the brunt of job losses. Poor target efficiency. 86% of the benefits of a local EITC would reach poor households, where the benefits of this local LWO would reach only 7% of poor households.
2005 Troikka, Yelowitz and Neveu <i>Economic Development</i> <i>Quarterly</i>	Data from 1999 Survey of Program Participation to simulate LWOs	Simulations assume the median LW in 1999. Assuming no loss in employment from the LW, the authors find that average marginal tax rate for those affected by the living wage is 30.2%. Conclude that the LW is not well targeted at the poor. Estimated that 75% of those directly affected by the LW do not live in poor household, with 40% living at least twice the poverty line. For those living in poverty, additional earnings from the LWOs largely disappear through lower benefits and higher taxes.
2006 Anker	Uses data from 2000 in 12 low, medium and high income countries to	Key difference between poverty lines and living wages. Poverty lines are measured at the household level, while living wages are applied at the individual worker level. Households can be poor and all workers in it could be receiving a living wage. Households can be non-poor and contain workers not receiving a living wage. This lack of one-to-one correspondence reduces the target efficiency of LWOs. A series of assumptions are used

<i>International Labour Review</i>	compute 'comparable' living wages	to build a basis for a living wage (e.g., size of a representative household, number of full-time equivalent workers, minimum food budget, ratio of other minimum expenditures relative to the food budget). Results suggest that living wages increase with economic development (i.e., wealthier countries should have much higher living wages.)
2007 Fairris and Bujanda <i>Southern Economic Journal</i>	Case study: Los Angeles LWO Using worker-firm matched data	Use LWO in Los Angeles to estimate the labour-labour substitution effect using matched employee-employer data. 1997 law covers employers with contracts, business assistance and who lease city land. Estimated to affect about 8,000 workers (2% of workers in the city). Set roughly 70% above state MW. Survey of 82 LW employers (response rate of 84%). 320 workers surveyed (response rate 81%). Final dataset has 44 employers and 231 workers. Find significant evidence of labour substitution. After the LWO, workers who joiners are much more likely to be male or Latino/black, have prior training and higher previous wages. Several issues with this analysis, small possibility non-representative samples, no information on workers who leave affected employers. Even though employers don't raise/alter hiring standard, better applicant pool drives labour substitution effects. Firms are able to hire more productive workers.
2008 Clain <i>Journal of Labor Research</i>	County and City Database 1994-2003	Unit of observation is the poverty rate in a county in a particular year. Finds that LWOs have a negative and significant effect on poverty, while state MWs have negative and insignificant effects. However, concludes that the magnitude of the poverty-reducing effects of the LW are modest (p.213).
2008 Holzer Brookings Institute Working Paper	Review and synthesis of the LW literature	This is a review of the existing empirical literature on the various effects of LWOs in the US. Finds the criticisms of Adams and Neumark less than compelling, but does find a number of concerns: sample sizes and representativeness of cities remains. Concern about the endogeneity of adopted LWOs. Are other cities valid controls? Overall, the effects (either positive or negative) are likely to be 'modest'. So few workers are directly affected by such laws. Doesn't conclude that such laws should not be passed. Even small positive benefits for some low-wage workers may be better than nothing. Might be part of a more general public awareness campaign on growing wage inequality.
2011 Lester <i>Economic Development Quarterly</i>	National Establishment Time Series Data 1990-2005	Uses panel data on establishments between 1990 and 2005. Propensity scoring is used to match living and non-living wage cities. No evidence of reduced employment growth for firms in LW cities. No evidence of harm to the 'business climate' in terms of firms avoiding location or relocating from LW cities. No information on wages in NETS. Estimates the impact of LWOs on the aggregate employment and number of establishments in a city. (Note: this raises comparability issues: Neumark's work focuses on employment loss for bottom decile of workers. Employment effects may be difficult to detect at an aggregate level or at the level of all government contractors.) Doesn't really 'contradict the disemployment effects of Adams and Neumark (2005)' (p.252). Findings on the 'number of establishments' doesn't say anything about firms relocating or not locating to LW cities.

<p>2012</p> <p>Altman</p> <p><i>Forum for Social Economics</i></p>	<p>Survey of Conceptual LW Effects</p>	<p>Argues that LWOs could efficiency improvements and technological change that could offset any distortionary affects. Links LW campaigns to earlier motivations for living wages (John Ryan 1906 and 1935). Hypothesises that LWOs could increase material wellbeing through net efficient gains to society. Provides no empirical evidence for such effects.</p>
<p>2012</p> <p>Neumark, Thompson and Koyle</p> <p><i>IZA Journal of Labor Policy</i></p>	<p>Updated monthly CPS rotation group data 1996-2009</p>	<p>Update of earlier work with CPS data was complicated by the change in the definition of metropolitan areas in 2005. Updated results suggest that LWO have a positive (but insignificant) effect on wages and a negative (and significant) effect on employment of workers in the bottom decile. For business-assistance LWOs, the wage elasticity (0.067) is almost identical to the previous finding (0.07), the same is true with the employment elasticity of around -0.17. Overall, the wage effect has weakened with the updated results, and the employment effect is stronger. The latter is no longer confined to business-assistance LWOs. Based on more observations of LWOs and larger sample (double the cities and double the time period). The estimated effects on poverty are similar, although somewhat more variable and sometimes insignificant. Again, if they exist, they appear to be relegated to business-assistance LWOs. Preferred wage elasticities are now 0.037 for contractor-only LWOs and 0.051 for employer-assistance LWOs. 50% increase in LW leads to 2.4% point reduction in employment for contractor-only LWO and 2.8% point reduction in employment for business-assistance LWO (all associated with bottom decile of workers). Poverty estimates more variable and less certain. 50% increase in LW leads to 1.2% point reduction in poverty (full sample, not just the bottom decile) for business-assistance LWOs.</p>
<p>2012</p> <p>Neumark, Thompson, Brindisi, Koyle and Reck</p> <p>NBER Working Paper</p>	<p>Combination of prior findings, simulations and location-specific data to evaluate a proposed LWO for New York City</p>	<p>This study is particularly relevant to any city that is considering a proposed LWO (e.g., Auckland). There is an existing literature on the possible effects of such laws. Some of this involves ex ante simulations (e.g., Pollin and Luce 1998), some of this involves empirical estimates of these effects. This specific case involves a 2010 proposal to extend a contractor-only LWO to a business-assistance LWO. Ex ante studies depend crucially on the employment effects of LWOs. Sometimes based on recent MW research showing an absence of any employment losses. This assumption is simply extended to these ex ante studies (e.g., see the work of Pollin). The alternative is to use the empirical findings from past longitudinal studies. Review the analysis and findings in Neumark, Thompson and Koyle, 2012). Use data from American Community Survey (ACS) to get data on the NYC workforce over the period 2006-2008. Used to identify workers whose wages might be directly affected by the LWO extensions. Next they estimated how many live in poor households, and estimate the loss in transfers that might accompany these higher earnings. Allow for some loss in employment. Find that the effects on poverty would be mixed. 'Extreme' poverty would worsen slightly, but 'near' poverty would improve. The overall impact is a very small improvement in the poverty rate. These are further offset by a reduction in transfer payments. Overall, family earnings would increase by \$1.6 million, offset by \$4.6 million reduction in EITC. This is a net increase of \$206 per year for 34,000 affected workers.</p>

Appendix B United Kingdom Studies on Living Wage Policies

Year, Author(s) and Publication Location	Sample and/or Focus of Study	Summary and Analysis of Findings (in Chronological Order)
2009 Wills (with Kakpo and Begum)	Online Survey of Cleaners at Queen Mary, University of London	This paper reports the results from an online survey of 292 cleaners at Queen Mary, University of London, followed by face-to-face interviews with 73 cleaning staff plus others. The authors report that becoming a living wage employer (and bringing cleaners in house) ... improved job quality, productivity and service delivery, with very little increase in costs. (p.2) Interesting that only 55% of cleaners were 'retained' when the service was shifted from a contractor to in-house service. The survey was conducted at the end of 2008. 170 cleaners were on the payroll for the contractor, and 90 were transferred in Jan 2008, with 77 remained in May (others left with invalid work permits or false names). 73 were surveyed. <i>Q: What does this say about the change in employment levels?</i> If the 2008/09 budget is only slightly higher than the 2006/07 budget, doesn't this suggest that aggregate hours of employment may have fallen? Could the University be using fewer cleaning hours and be getting better outcomes as a result of higher wages and better management? This issue is never addressed in this study. The biggest concern about this study is that two fundamental changes were occurring simultaneously: moving service in-house and implementing a living wage. This makes it impossible to distinguish the source of any associated effects.
2009 London Economics Working Paper	Greater London Authority	GLA commissioned this work from the consulting firm London Economics. Conclude that literature review (mostly from the US) finds that Living wage provisions are not generally associated with job losses or worker displacement. This is <u>not</u> my conclusion, especially the latter. Report employer benefits (based on survey): reputation, recruitment and retention, worker morale and motivation, productivity, quality of service, absenteeism, ability to attract higher quality workers. Transitional costs reported in moving to LW (re-negotiation of contracts, maintaining pay equivalence, increased labour costs, impact on profits). No attempt to look at employment effects or impact on poverty and family incomes.
2010 Morelli and Seaman Working Paper	Use the UK Quarterly Labour Force Survey and the British Household Panel Survey	Examines the 'theoretical underpinnings' of the LW movement. Authors assume that the labour market is characterised by monopsony, but offer no empirical support for this assumption. This will clearly lead to a possibility of no loss in employment from a LW, but in the end this is contentious and the issue ultimately empirical. Estimates relatively small impacts on the public sector wage bill from paying a LW. It would tend to disproportionately help youth and women. Limited analysis of poverty effects. No attempt to look at employment effects.

<p>2012</p> <p>Wills and Linneker</p> <p>Working Paper</p>	<p>Case study of 17 employers and 416 workers: Comparison between LW and non-LW cases.</p>	<p>Trace roots of the LW campaigns in the UK to the coalfields in the 1870s. Discusses at the time of the implementation of the National MW in 1998. Like the US, concern that the national MW was too low to lift working households out of poverty. London LW Campaign launched in 2001. Set for government contractors, but also extended to other employers. Claimed to have a direct impact on over 100 employers and 10,000 workers since 2001. Different from the US is the attempt at more centralisation and coordination of the level of LWs outside of London. Empirical analysis based on at most 17 workplaces (mostly cleaning firms) with limited data from some. Lots of data issues, including poor response rate from non-living wage employers (no figure given) Found relatively lower turnover in LW firms compared to non-LW firms, but small cost savings because of low recruitment costs. Found small 'pass through' effects in terms of higher contract costs (p.16). Lots of reports on specific interview results, but little light shed on how reported cost savings were achieved. The ones that were mentioned could lead to disemployment (working harder, closer monitoring of labour, 'sharper inspections'). Those born in the EU more likely to paid a LW and higher education. Are other migrants relegated to non-LW jobs? (small sample $n = 416$). Those in LW jobs didn't report longer expectations of future tenure with their firms. Those in LW jobs had higher previous earnings, suggesting possible positive selection on observable and unobservable characteristics. Better psychological health for LW workers (holding other factors constant), but authors admit that any causal connection can't be inferred (p.29). Interesting that 35% of workers in LW workplaces reported no benefits from the LW. Some reports of cuts in overtime hours and bonuses after LW implemented (p.30). Benefits were not universal in LW workplaces (p.32). No attempt to estimate any employment effects from the LW. Yet, authors admit (p.36) that some firms managed higher wages through Reductions in head count and/or hours of work. In fact, only workers in LW workplaces were interviews. Very limited information on any impacts on poverty incidence and family income. Analysis doesn't live up to title. No overall assessment of costs and benefits of the LW, yet a conclusion that the net benefits are positive. Just a note: claim that 10,000 workers directly affected by the LW in London since 2001, but there are currently 580,000 workers in London being paid less than the LW. Thus, a conservative 'coverage rate would be 1.7%!</p>
<p>2012</p> <p>GLA Economics</p> <p>Working Paper</p>	<p>Greater London Authority</p>	<p>Announcement by the mayor (Boris Johnson) of £8.55 LLW for 2012. Claim that there are 3,400 workers benefiting from the LLW through contracts, 200 employers paying the LW. The purpose of this paper is to provide alternative measures for the construction of the LLW (Basic Living Costs vs. the Income Distribution Approach). The average of these two figures is taken as the LLW. Data from the Annual Survey of Hours and Earnings finds that 11.4% of full-time workers and 46.2% of part-time workers earn less than the LW (18.7% of total workers). If aggregate employment is around 4,000,000, this means that there are nearly 750,000 earning less than the LLW. With an estimated number of 11,550 who directly benefit this is about 1.5% of workers earning less than the LLW, or about 0.3% of all workers. No estimates of either the positive or negative effects of the LLW in this report.</p>
<p>2013</p> <p>Lawton and Pennycook</p> <p>Working Paper</p>	<p>No original empirical estimates of the effects of the living wage, but various simulation results from a universal extension of the living wage</p>	<p>This study seems to straddle the line between openly advocating for living wage policies vs. objectively examining the possible effects of such a policy. I disagree with the authors' summary of the U.S. evidence. In particular, living wages in the U.S are not ... almost exclusively linked to public sector jobs and public sector procurement (p.6). Empirical evidence (both positive and negative) is largely relegated to broader living wage ordinances that cover employers receiving economic development assistance of one form or another. I also disagree with the stated distinction between statutory minimum wages and living wages (e.g., p.10). Potential job losses are often considered in setting minimum wages, but this issue is not something that can necessarily be ignored in the living wage discussion. Chapter 3 purports to conduct ... a full economic analysis of the living wage in the UK. What it does is describe the extent and location of widespread low-paid employment in the UK (earning less than two-thirds of median hourly earnings). They conclude that raising 4 million low-paid UK workers to the living wage would reduce employment by 160,000. That's a 4% loss in aggregate employment. There is no discussion in this report on the basis for this calculation. Not sure what elasticity if being assumed or the source of this estimated value. If it's 0.3, this</p>

		<p>implies slightly more than a 13% increase in the average pay for these affected groups. This may or may not be accurate, but it says nothing about the employment effects of the current living wage. The authors go on to estimate the impact of the wage bill of employers. They're found to be small as consistently found in the literature, except in areas like bars & restaurants and retailing. Chapter 5 looks at the possible fiscal benefits of a universal living wage. This is a strange analysis because it ignores the impact of the job losses already mentioned earlier in the same report. It paints a best case scenario of the reduced transfers and increased tax revenue. But this also weakens the anti-poverty effects of the living wage. It also ignores the price effects of this policy on consumer welfare.</p>
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Appendix C New Zealand Studies on Minimum Wage Policies

Year, Author(s) and Publication Location	Sample and/or Focus of Study	Summary and Analysis of Findings (in Chronological Order)
1995 Maloney <i>New Zealand Economic Papers</i>	Aggregated quarterly HLFS data 1986-1993. Comparing young adults to teenagers.	Used unique feature of the NZ MWs to estimate effects on employment and unemployment. Teens are the 'control group' (exempt from MW protection until March 1994). Young adults are the 'treatment group' (most likely age group directly affected by the adult MW. First study to econometrically isolate this effect in NZ. Estimate of MW elasticity of employment -0.35 for young adults and -0.57 for young adults w/o qualifications. Found evidence of a labour substitution effect. The MW elasticity of +0.69 for (uncovered) tends. Allowed for lagged effects over 6 quarters. Criticisms of this study (e.g., Chapple 1997) involved concerns over alternative specifications that resulted in smaller and sometimes insignificant employment effect.
2007 Hyslop and Stillman <i>Labour Economics</i>	Disaggregated Annual HLFS-IS data 1997-2003.	Focus on the extension of the MW to teens (note different lower MWs for 16 and 17 year-olds compared to 18 and 19 year-olds). Estimate positive effects on hours of work for younger teens, and positive employment effects on 18 and 19 year-olds. But these results are sensitive to specifications used. These findings parallel those of Card and Krueger for the US (2000). But also find a decline in educational enrolments and a relatively larger increase in labour supply (an unusual, difficult-to-explain result). Thus, they find that the MW increased teen unemployment and economic inactivity. Does show evidence of a clear spike in the estimated wage distribution around these MWs. Also evidence of considerable exemptions or non-compliance. Some estimates of negative MW employment elasticities by 2003 for 16-17 year-olds of -0.2 and 18-19 year-olds of -0.04. Swamped by hours effects. Uncomfortable with emphasis on the supply-side effects.
2009 McLaughlin <i>British Journal of Industrial Relations</i>	Surveys of 52 union officials, senior civil servants and IR academics.	Emphasis on the possible productivity effects of the MW in NZ and Denmark. Claim that firms are 'shocked' into adopting better production processes. Concern that these effects are sometimes constrained by institutional barriers (e.g., no increase in training in the UK in response to the 1999 NMW). Claim the NMW in the UK wasn't set 'high enough', and employers to fearful of 'poaching'. Effectively, employers are caught in a low-wage/low-training equilibrium. MWs shock them to adopt a high-wage/high-training equilibrium. Expresses some hope that increased government funding in the early and mid-2000s will increase training under a voluntary arrangement. Concern that employers are resistant. No quantitative evidence on levels of training and impacts on wages and employment.
2011 Pacheco	Disaggregated quarterly HLFS data 1986-2004 and HLFS-IS data 1997-	No single demographic group finds the MW 'binding' for everyone. Estimated the probability that any individual will face a binding MW (being paid at or below the legislated MW). This variable is then interacted with the level of the MW to restrict its affect to only those individuals likely to be directly affected. These probabilities of binding are estimated with the 1997-2004 IS data for 16 to 29 year-olds. Find evidence of substantial dis-employment effects for teens and somewhat smaller effects for young adults if they are likely to be directly

<i>Economic Record</i>	2004	affected by the MW. There is a methodological issue of the appropriateness of using a predicted variable as a covariate in this analysis.
2012 Hyslop, Maré, Stillman and Timmins <i>Labour</i>	Disaggregated LEED data 1999-2007	This is particularly relevant for the LW debate. Uses the firm as unit of observation. There are both industries (and firms within these industries) that contain large proportions of teens. Is there a heterogeneous firm response over the period of large increases in teen MWs (depending on the proportion of teens employed)? Small estimated employment effects. Firms with high initial levels of teen employment reduced shares of teen employment. Moreover, there was a reduction in the probability that these firms survived. This is useful in the LW context, because this relates to firms with high% of covered workers and large increases in the wage floor (like the LW). However, entering firms tended employ more teens maybe because of the adoption of different technology. There are both industries (and firms within these industries) that contain large proportions of teens. (p.450) Mention that this could be more a function of the normal differences between entrants and incumbents, rather than the direct effect of entry on teen employment.
2012 Maloney and Pacheco <i>Review of Income and Wealth</i>	Disaggregated HLFS-IS data 1997-2008	Like previous study, took advantage of the substantial increase in the teenage MW since 2000. Emphasis here is on the antipoverty effects. There are two reasons to suspect weak antipoverty effects: (i) many low-wage workers don't live in poor households (especially teens); (ii) many poor households don't contain any workers. Find very weak antipoverty effects. A 10% increase in the MW at most reduces the poverty rate (households with equivalised income below 50% of the median) by 0.1% point. This ignores any loss in employment. Consistent with overseas literature (particularly the US and Australia). With an average 3% loss in hours of work, there is almost no reduction in the poverty rate. The conclusion is that the MW is not a very well-targeted antipoverty programme.