# RAISING THE MINIMUM WAGE

# Misguided Policy, Unintended Consequences

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# Contents

# Executive summary

Proposals to increase the minimum wage have re-emerged in provinces across the country. For instance, the Alberta government recently pledged to hike the provincial minimum wage from $10.20 to $15 per hour by 2018, already taking the first step with a $1 hike effective October 1, 2015. There has been a similar movement to raise the minimum wage to $15 in various jurisdictions in the United States.

Popular support for the minimum wage largely derives from the belief that it is a useful tool for boosting the wages of poor workers. However, the evidence paints a much different picture. For starters, the minimum wage does not effectively target workers in low-income households. In fact, 87.5% of Canadians earning minimum wage in 2012 lived in households above the Low Income Cut-Off (LICO), a widely used measure of relative poverty. Moreover, the vast majority of workers (83.4%) from households falling below the LICO threshold earned more than the minimum wage.

These counterintuitive results follow from the demographic composition of minimum wage earners. In 2014, 58.4% of those earning minimum wage were youths aged 15 to 24. Furthermore, 56.8% of all minimum wage earners were living with family, while 19.9% were married to a spouse who was also employed. Taken together, the data undercut the popular image of minimum wage earners being single breadwinners supporting a family. In fact, only 2.2% of those earning minimum wage were unmarried heads of household with at least one minor child.

The tenuous link between minimum wage earners and poor households makes the minimum wage a very crude method for targeting assistance to those who need it. At the same time, hiking the minimum wage can do considerable harm, most notably by decreasing employment opportunities among low-skilled workers—the very group the policy is designed to help. By making labour artificially more expensive, increasing the minimum wage may significantly reduce employment among teenagers and other groups of low-skilled workers. Besides reducing employment outright, hiking the minimum wage could lead to a reduction in hours and other benefits (such as on-the-job training) for those workers who keep their jobs.

There is an enormous body of empirical research examining the effects of the minimum wage. Canadian studies are considered of higher quality than US studies because (among other reasons) there is a wider variability in the provincial Canadian minimum-wage variable. The Canadian literature generally finds that a 10% increase in the minimum wage reduces employment among teens and young adults (ages 15 to 24) by 3% to 6%. By making it harder for low-skilled workers to obtain an entry-level position, the minimum wage may perversely hinder the development of human capital and harm the long-term career prospects of the very people it ostensibly helps. Indeed, Canadian researchers have found that hiking the minimum wage has no statistically significant impact on poverty and in some cases can increase it.

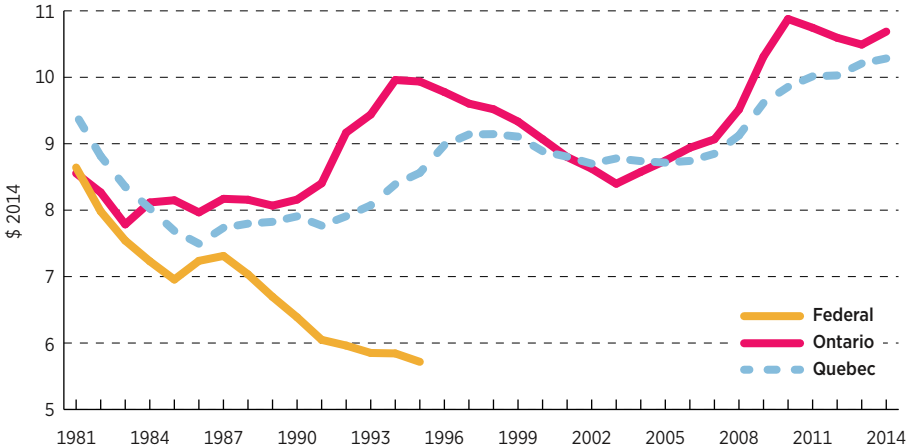
Up through the 1980s, research in the United States reached similar conclusions, though the impact of the minimum wage on youth employment was not as severe as reported in the Canadian studies. It is true that a wave of "new minimum wage research" emerged in the United States in the 1990s, challenging the original consensus. However, there have been dozens of US studies since then that endorse the original findings, some of which employ techniques similar to those of the revisionist studies. Furthermore, some leading researchers in the field actively dispute the validity of the "new" approaches.

Fortunately, rather than the dubious policy of increasing the minimum wage, there is a better option for helping workers from poor households, namely increasing the Working Income Tax Benefit (WITB). This Canadian program is a refundable tax credit that allows qualifying low-wage workers to keep more of their earnings, effectively subsidizing them to work more. Unlike traditional welfare programs, the WITB is specifically designed to minimize the disincentives that can occur when government assistance is removed at higher income levels. Because it targets the desired individuals more accurately and avoids price controls, the WITB is a much more sensible (although not flawless) approach to channeling resources to the aid of workers from low-income households.

# 1 Introduction

Proposals to increase the minimum wage have re-emerged in provinces across the country. For example, the Alberta government pledged to increase the province's minimum wage from $10.20 to $15.00 by 2018, and has already made good on the first leg of that promise—a $1 hike effective October 1, 2015. Likewise, the United States is rife with recent state and city initiatives to raise the minimum wage, such as Los Angeles' plan to move from $9 to $15 by 2020. More generally, there is a growing campaign from labour activists and some politicians to raise the US federal minimum wage to $15 (a more than 100% increase from its current level of $7.25). Figures 1a, 1b, and 1c display the minimum wage rate (adjusted for inflation) in the 10 provinces and federally over the period from 1981 to 2014. The general provincial trend is toward a higher wage rate (after adjusting for inflation) since the early 2000s.

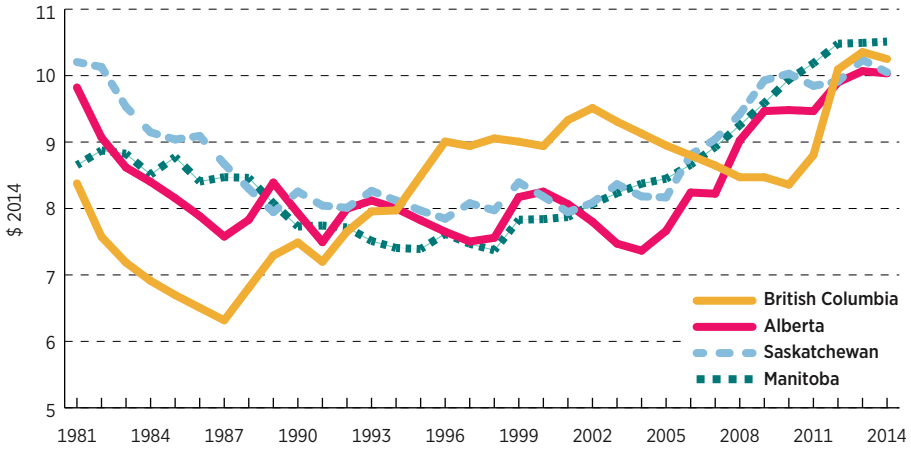
**Figure 1a: Minimum wage rates federally and in Central Canada (adjusted in 2014 dollars), 1981–2014**




Note: Up to 1995, the Canadian federal government set its own minimum-wage rate for employees working in federally regulated jurisdictions such as transportation and communications. Starting in 1996, the federal government deferred minimum-wage policy to the provinces, meaning that the prevailing minimum wage rate for any federally regulated industry equalled the prevailing provincial rate.

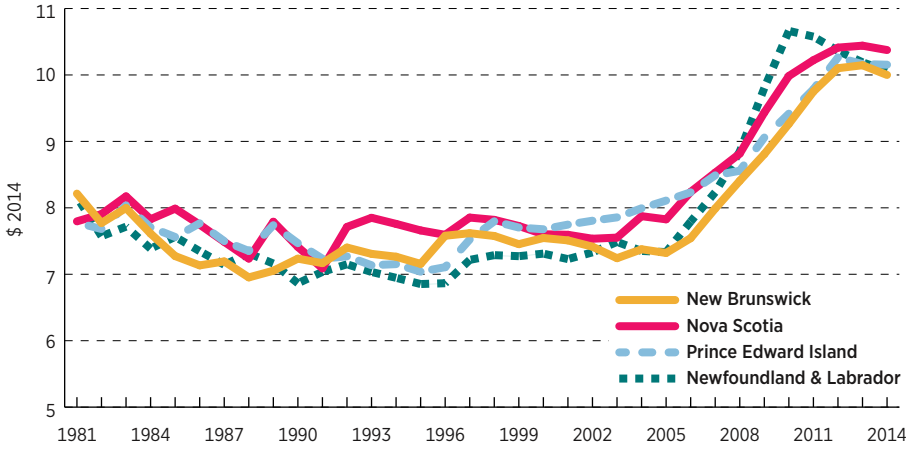
Source: Canada, Labour Progam, 2015.

**Figure 1b: Minimum wage rates in Western Canada (adjusted in 2014 dollars), 1981–2014**




Source: Canada, Labour Progam, 2015.

**Figure 1c: Minimum wage rates in Eastern Canada (adjusted in 2014 dollars), 1981–2014**




Source: Canada, Labour Progam, 2015.

Citizens and policy makers naturally turn to economists for help in assessing the likely results of such measures. After all, if low-income workers can be helped merely by legislative decree, what has taken so long? Are there any unintended consequences from what appears to be a "pro-labour" measure?

This study makes the case that there *are* significant harms from using the minimum wage as a policy device to help low-income workers. For example, it shows that the minimum wage is an ineffective tool for alleviating poverty since most minimum-wage workers belong to households *above* the low-income threshold. Put differently, the minimum wage does not effectively target the very group of people it intends to help.

The most hotly disputed adverse effect is the possible impact of the minimum wage on teen and young-adult employment. If increases in the minimum wage lead to large job losses among the very workers the public wants to help, then the minimum wage is clearly a dubious policy device. This study discusses the main results of the empirical literature. Although the issue is still open for debate among experts in the United States, there is a large literature—particularly pertaining to the Canadian experience—showing evidence that minimum wage hikes are associated with lower employment for teens and young adults.

The flaws of the minimum wage—ineffective targeting of low-income households and adverse employment effects—suggest that we should look to other policies to help low-income workers. Economists from across the political spectrum agree that the Canadian Working Income Tax Benefit (WITB), or what is called the Earned Income Tax Credit (EITC) in the United States, is much more efficient and better targeted. The study concludes by making the case for the WITB over the minimum wage.

# 2 Who Earns the Minimum Wage in Canada?

Public support for the minimum wage largely derives from the belief that it is a useful tool for boosting the wages of workers in low-income households to curb poverty. However, the actual data show a much different picture of the demographic composition of minimum wage earners. In this section, we will relay some of the key results about who actually earns the minimum wage in Canada, which cast considerable doubt upon the public perception of minimum wage recipients. The important takeaway is that minimum-wage legislation is not a good tool to target workers living in low-income households, primarily because most minimum-wage workers do not fall into this category.

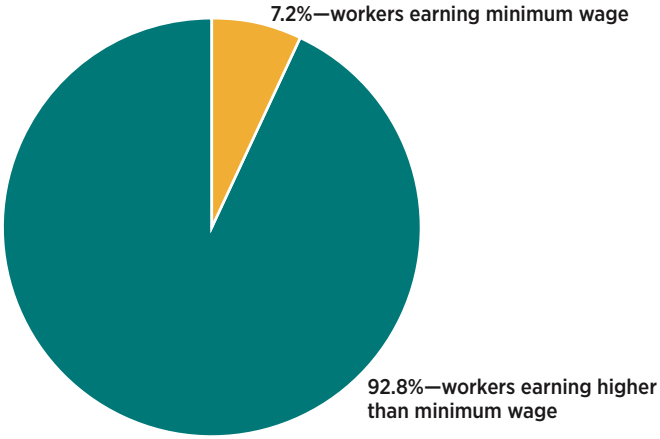
Looking at Canada as a whole, as of 2014 (the latest data available at the time of writing), about 1.1 million workers earned the minimum wage, which represented only 7.2% of total employees (**figure 2**). [1] The share of workers earning the minimum wage in a specific province can be considerably lower than this national figure, particularly in the cases of Alberta and Saskatchewan where the share in 2014 was 1.7% and 3.5%, respectively (see **Appendix A** for a detailed breakdown of minimum-wage data by province). Thus we see that regardless of what policy makers do with the minimum wage, it only affects a small segment of the Canadian work force.

It is also instructive to break down the 1.1 million minimum-wage earners by age. As **table 1** indicates, in 2014 36.4% of minimum-wage earners were teenagers. Furthermore, more than half—58.4%—were 24 years or younger. In contrast, only 21.8% of minimum-wage earners were between the ages of 25 and 44.

Moreover, the vast majority (84.9%) of young minimum wage earners in 2014 were a son, daughter, or other relative living with family. More generally, 56.9% of all minimum-wage earners in 2014 were living with family, while only 2.2% of minimum wage earners (24,000 workers total) were unmarried

[1] Demographic data for minimum-wage workers come from Statistics Canada's *Labour Force Survey* (LFS). For more information on LFS, see: <http://www23.statcan.gc.ca/imdb/p2SV.pl?Function=getSurvey&SDDS=3701>.

**Figure 2: Workers earning minimum wage as a share of total employees in Canada, 2014**




Source: Statistics Canada, 2015a.

**Table 1: Breakdown of the number of minimum wage earners in Canada by age, 2014**

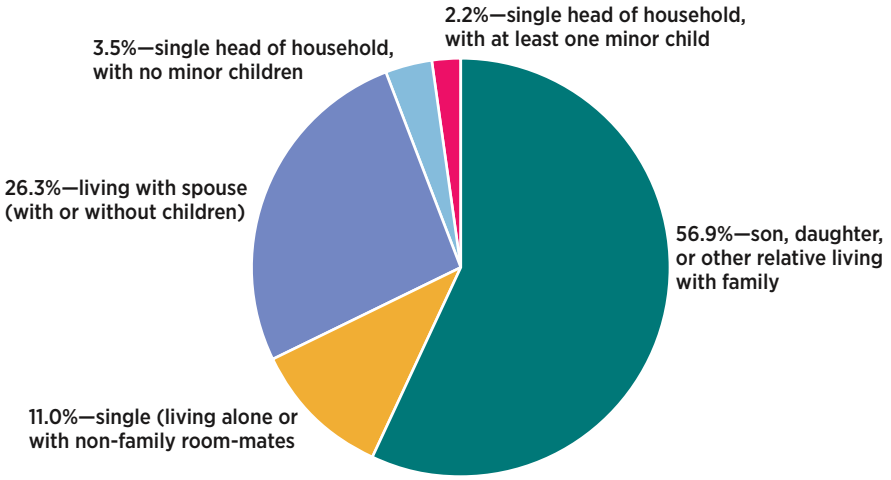
| Age bracket | Number of minimum wage earners (000s) | Share of total minimum wage earners (%) |
|---|---|---|
| 15 to 19 | 395.8 | 36.4% |
| 20 to 24 | 239.2 | 22.0% |
| 25 to 34 | 136.0 | 12.5% |
| 35 to 44 | 101.2 | 9.3% |
| 45 to 54 | 105.3 | 9.7% |
| 55 to 64 | 82.1 | 7.5% |

Source: Statistics Canada, 2015a.

heads of household with at least one minor child (figure 3). Additionally (though not directly shown in our summary charts), 19.9% of all minimum-wage earners were married to a spouse who was also employed. Of this group, 91.3% had spouses who either earned wages higher than the minimum wage or were self-employed. Taken together, these data undercut the popular image of minimum wage earners consisting of breadwinners supporting a family.

Consistent with the fact that minimum wage earners are predominately young, they also have much less education than the general work force. For example, in 2014 only 9.3% of the total employed had lower than a high-school diploma, while 70.1% had at least some postsecondary schooling. In contrast, among those workers earning the minimum wage, 26.2% had lower than a high-school diploma, while 45.9% had at least some postsecondary schooling (table 2).

### Figure 3: Family composition of employees earning minimum wage in Canada, 2014




Source: Statistics Canada, 2015a.

### Table 2: Educational attainment of workers in Canada, 2014

| Educational attainment | Share of total employed | Share of minimum wage earners |
|---|---|---|
| Less than high school diploma | 9.3% | 26.2% |
| High school diploma | 20.5% | 27.9% |
| At least some post-secondary | 70.1% | 45.9% |
| *Some post-secondary* | *7.1%* | *15.9%* |
| *Post-secondary diploma or certificate* | *36.0%* | *18.7%* |
| *University degree* | *27.0%* | *11.3%* |

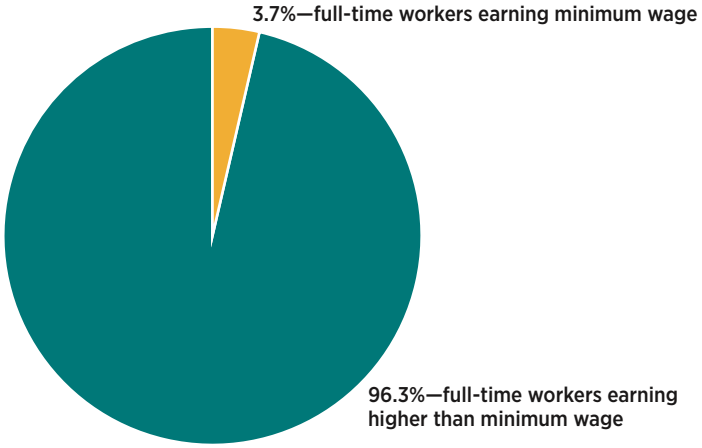
Source: Statistics Canada, 2015a.

Another revealing statistic is that, among minimum wage earners in 2014, more than half—54.1%—had only a high-school diploma or lower. In contrast, if we focus on those workers who have achieved a university degree or postsecondary diploma or certificate, then only 3.4% earn the minimum wage, while the remaining 96.6% earn more. These facts should put to rest the image of college graduates taking minimum-wage positions.

Another interesting comparison contrasts full-time and part-time employment among the general work force compared to minimum-wage earners. Specifically, looking at all employees in 2014, fully 81.6% had full-time jobs, while only the remaining 18.4% worked part time. Yet, when it comes to the minimum-wage earners, only 41.9% worked full time, while the remaining 58.1%—well over half—worked only part time. Similarly, we can point

out, that looking at all full-time workers in 2014, *a mere 3.7%* (about 456,000 workers in total) earned only the minimum wage, while the remaining 96.3% of full-time workers earned *more* than the minimum wage (figure 4). These statistics should provide the proper context for the notion of career workers depending on increases to the minimum wage.

**Figure 4: Full-time employees earning minimum wage and more in Canada, 2014**




Source: Statistics Canada, 2015a.

## Minimum wage and LICO

Finally, in what may be the most surprising statistic for the policy debate, in table 3 we relate the earnings data to "low-income cut-off" (LICO) household status, which is a generally used measure of relative poverty in Canada. [2] There are two crucial lessons to draw from table 3. First, the vast majority—87.5%—of minimum-wage earners in 2012 (the latest data available at the time of writing) lived in households *above* the LICO threshold. [3] Second, the vast majority—83.4%—of workers falling *below* the LICO threshold earned *more* than the minimum wage. These results may seem counterintuitive but consider that most minimum-wage earners are part of households with other income earners. Recall that 56.9% of minimum-wage earners live with family and another 19.9% have an employed spouse.

[2] The Low Income Cut-off threshold is calculated based on the percentage of income that a household spends on necessities relative to the average. Specifically, a household would be at the threshold if it is expected to spend 20 percentage points more of its household income on necessities than the average household.

[3] Data on the number of workers above or below the LICO threshold come from Statistics Canada's *Canadian Income Survey* (CIS). For more information on CIS, see: <http://www23.statcan.gc.ca/imdb/p2SV.pl?Function=getSurvey&SDDS=5200>.

**Table 3: The weak link between earning the minimum wage and living below the low income cut-off (LICO), 2012**

| Category | Number or share (%) |
|---|---|
| Total minimum wage earners | 925,000 |
| Total workers below LICO | 700,000 |
| Minimum wage earners below LICO | 116,000 |
| Share of minimum wage earners above LICO | 87.5% |
| Workers below LICO earning more than minimum wage | 584,000 |
| Share of workers below LICO earning more than minimum wage | 83.4% |

Note: The low income cut-off (LICO) data are calculated after tax and government transfers.

Source: Statistics Canada, 2015b

A report on the minimum wage commissioned by the Government of Ontario made similar observations on the demographics of Ontario's minimum-wage earners (Ontario, Minimum Wage Advisory Panel, 2014). The Minimum Wage Advisory Panel was established by the Ontario government to provide advice on the province's minimum-wage policy. The final report of the Panel pointed out that:

> The link between poverty and low wages is weak for a variety of reasons. Many poor families have no employed workers in the household or they work only a few hours, and many others work above the minimum wage. Many minimum wage workers are youth who live in non-poor families, or are persons in multiple earner families where the combined earnings takes them out of poverty. Moreover, minimum wage jobs are often taken as temporary stepping-stones to higher paying jobs. (Ontario, Minimum Wage Advisory Panel, 2014: 36) [4]

As the data presented in this section make clear, in Canada there is only a very tenuous link between minimum-wage earners and poor households (see Appendix A for a breakdown of some key data at the provincial level). In particular, citizens should be aware that raising the minimum

[4] Despite presenting evidence on the negative effects of increasing the minimum wage, the Minimum Wage Advisory Panel recommended annual increases to the minimum wage that are equal to the Ontario Consumer Price Index. The government has since enacted this recommendation.

wage—even if we only consider the people who still retain their jobs—is a very crude method for targeting assistance to poor workers. For one thing, there are many minimum-wage earners (such as teenagers) who come from affluent families, meaning that middle- and upper-income people would receive some of the benefit aimed at the poor. Moreover, in addition to this "spillover" effect (that is, conferring benefits on non-poor), raising the minimum wage would also *miss* many of the low-income workers, who already earn more than the minimum. [5]

[5] It is true that some relatively low-skilled workers might experience a pay rise if they are currently earning more than the minimum wage but are earning less than the new floor after a policy change. Nonetheless, the data in table 3 show just how big the disconnect is between the working poor and minimum-wage earners.

# 3 The Pros and Cons of Raising the Minimum Wage

In this section, we outline some of the major pros and cons of using the minimum wage as a policy tool to help low-wage workers. In the next section, we will review the empirical literature that seeks to quantify some of these key factors.

The obvious benefit of raising the minimum wage is that it increases the earnings of those workers previously earning *less* than the (new) minimum wage, and possibly the earnings of other low-wage workers who see a corresponding boost to their pay so their jobs remain relatively more attractive. However, in contrast to this *prima facie* benefit, there are several possible negative consequences. (After all, if there were *no* downsides, then policy makers could costlessly enact minimum wages of $50 or $100 per hour, showering benefits on the economy with no ill effects.)

### Loss of employment

The most obvious possible harm from raising the minimum wage is that it could reduce employment among low-skilled workers—the very demographic the policy is designed to help. This reduction in employment could manifest itself as a drop (relative to the counterfactual trend) in the actual number of employed workers, or in a reduction in hours given to minimum-wage workers who retain their jobs. These possible impacts on employment figures are the chief dispute in the empirical literature, which we will review in the next section.

### Loss of mobility

Related to the problem of workers being deprived of employment is the fact that minimum-wage jobs are often stepping stones to better paying jobs. Thus, it is incorrect to adopt a static framework and view the tradeoff as between higher wages from an increased minimum wage and possible job losses. In reality, the actual tradeoff involved is much more nuanced, because even in the *absence* of a minimum wage hike, *most* workers currently earning the minimum wage would soon enough enjoy increases in income, as they grow

older and develop work experience. Low-skilled workers who lose their jobs because of an increased minimum wage are jettisoned from this natural process of human capital development, missing out not just in the short term from low-paying jobs but also the higher-paying jobs they *might* have enjoyed down the road had they stayed employed.

The empirical literature shows that there is high worker mobility in the ranks of minimum wage earners. For example, Long (1999) looked at US data from the early 1990s and concluded that, of minimum wage earners who were still in hourly positions two years later, 67.8% had experienced an increase in real (inflation-adjusted) wages, with a mean pay increase of 40.8%. (Workers who went from minimum wage jobs into salaried positions or became self-employed saw their monthly earnings more than double, but they represented a small fraction of Long's sample.)

Canadian studies also show the mobility of workers and the typical transience of holding minimum-wage positions. For example, Battle (2003) showed that of Canadians who had held their position for at least five years, only 1% earned the minimum wage. Gunderson (2007) showed that, in Ontario, 46.4% of minimum-wage earners had been in their positions for less than a year. To repeat, the high mobility of the typical minimum-wage earner underscores a serious problem with raising the minimum wage: to the extent that it knocks some workers out of employment, they are denied the opportunity to develop their skills, while the benefit to the retained workers (in the form of a higher minimum wage) is something that most of them would have enjoyed anyway, over time.

### Increased prices for customers and effect on the poor

Another obvious potential problem with an increased minimum wage is that the affected firms will (at least partially) cope with higher wage costs by passing along price hikes to their own customers. For example, fast-food restaurants might accommodate higher wages for their minimum-wage workers by raising prices on burgers and fries.

The possibility of price increases is very relevant to the question of whether the minimum wage is an effective instrument in helping workers from low-income households. Remember that we documented in Section 2 that "workers earning the minimum wage" are *not* the same group as "poor people". Recall in particular the statistics we reported in table 3, which showed that only 12.5% of minimum-wage earners (in 2012) fell below the Low Income Cut-Off (LICO) threshold, while at the same time 83.4% of workers who fell below the LICO threshold earned *more* than the minimum wage. Therefore, even if we completely disregard the possibility of disemployment effects on low-wage workers, who might lose their jobs if the minimum wage is raised, we *still* cannot conclude that a minimum-wage hike will necessarily help "the poor". The reason is that a minimum wage hike may very well end

up disproportionately "taxing" poorer people (through higher retail prices) and distributing the dividends among people who are mostly living above the poverty line. To flesh out this line of argument (in a US context), MaCurdy and McIntyre (2001) (followed up by MaCurdy, 2015) assume for the sake of argument that there are *no* disemployment effects from a minimum wage hike. Even so, they argue that minimum wage hikes at either the US federal or state level would *still* be very poor mechanisms for aiding the poor.

The logic of MaCurdy and McIntyre is critical to understanding the flaws with the minimum wage as a policy device, so we will summarize their argument carefully. First, they point out that firms will respond to an increase in the minimum wage in one of three ways: (1) they will reduce employment (either in jobs or hours) among the affected workers; [6] (2) they will accept lower profits; (3) they will raise the prices that they charge their customers.

As already explained, for the sake of argument MaCurdy and McIntyre rule out possibility (1), and conservatively assume that the relevant workers all see a pay increase (to the new, higher minimum wage level) with no reduction in employment or hours worked. Yet, this necessarily means that the firms have a higher total wage bill, which must be paid *somehow*. Conceivably, a boost in morale or a reduction in labour turnover could mitigate the cost but, if so, the government would not need to force firms to engage in the policy—the Minister of Labour could simply send a fax to every firm's HR department, explaining how a pay hike would "pay for itself".

Thus, to the extent that the increase in the minimum wage actually causes firms to alter their behaviour, and if we assume away the possibility of a reduction in employment, the only options left are (2) a drop in profits for shareholders and (3) an increase in prices passed along to retail customers. Although in the short run a minimum wage hike might reduce profits (especially to the extent that it had been unanticipated), MaCurdy and McIntyre point out that capital is mobile and many sectors of the economy do not employ a large number of unskilled workers. It is also the case that the industries relying on minimum-wage workers tend to be very competitive, with little "mark up" in their pricing. Thus, it is unlikely that investors will absorb the higher wage bill accruing from an increase in the minimum wage.

This leaves option (3), increases in retail prices. Using US data, MaCurdy and McIntyre show that, if a hypothetical minimum wage hike (from $4.25 to $5.75 with their numbers) is "paid for" solely by passing along prices to consumers, then it represents a very unattractive method of helping poor workers. This is because the data presented by MaCurdy and McIntyre show

[6] Firms can also reduce other forms of compensation such as fringe benefits and on-the-job training to offset the new artificially higher (minimum) wage.

that minimum wage workers in the United States [7] are spread fairly evenly across families in all income quintiles and, furthermore, only about a quarter of the families in either of the lowest two quintiles would see an increase in income from the higher minimum wage. Thus the *benefits* of the hypothetical minimum wage hike—even if we rule out all disemployment effects—are spread across rich and poor alike and, at the same time, miss about three-quarters of the poor.

On the other hand, the *costs* of the hypothetical minimum wage hike act much as an increase in the sales tax, which is of course very regressive by its nature, because an increase in price of consumer goods (especially if we focus on those involving large numbers of minimum-wage workers) will have a disproportionate impact on poorer families. Putting these two ideas together, we realize just how poorly designed a minimum wage is, if the goal is for society to sacrifice some economic efficiency in order to increase the standard of living among poor families. As MaCurdy and McIntyre put it:

> It seems certain that there would be little public support for a national sales tax levied only on selective commodities and used to transfer income in nearly equal amounts to 1 out of every 4 wealthy families as well as to 1 in 4 poorer families. Yet, when one considers passing the costs of the minimum wage through prices, this is the effective outcome of a minimum wage increase. (MaCurdy and McIntyre, 2001: 32)

The logic and empirical evidence of the MaCurdy and McIntyre (2001) study show that the current arguments over the empirical employment impacts may be clouding the overall policy debate: even if it were true that hikes in the minimum wage had *zero* effect on the employment of unskilled workers, nonetheless the policy would be very ineffective at channeling assistance to low-income families. Yet such an outcome is precisely what the public has in mind when supporting minimum wage hikes.

## Effect of the minimum wage on the poor in Canada

There have been empirical studies using Canadian data that complement MaCurdy and McIntyre's work on the United States. For example, Campolieti, Gunderson, and Lee (2012) "estimate the effect of minimum wages on poverty for Canada using data from the Survey of Labour and Income Dynamics (SLID) for 1997 to 2007 and find that minimum wages do not have a statistically

[7] With Canadian data, Fortin and Lemieux (1998) use the 1993 SLID (Survey of Labor and Income Dynamics) to construct deciles of families according to their "income-to-need" ratios. As they report in table 6, minimum-wage workers were spread fairly evenly over the bottom five deciles, accounting for a total of 67.3% of minimum-wage workers. For example, the fifth decile had 12.2% of minimum-wage workers, while the bottom (first) decile had 12.5%.

significant effect on poverty". Similar to MaCurdy and McIntyre, they then run a simulation calibrated to the Canadian March 2008 Labour Force Survey (LFS) and "find that only about 30% of the net earnings gain from minimum wage increases goes to the poor while about 70% 'spill over' into the hands of the non-poor". This leads them to conclude that minimum wage hikes are "poorly targeted as an anti-poverty device and are at best an exceedingly blunt instrument for dealing with poverty" (2012: 287).

Other Canadian studies that complement MaCurdy and McIntyre include Sen, Rybczynski, and Van De Waal (2011), which found that increasing the minimum wage could actually increase poverty. Mascella, Teja, and Thompson (2009) found that the minimum wage in Ontario is poorly targeted to poor households. Furthermore, there have been direct empirical studies to verify the mechanisms at work (other than disemployment) resulting from minimum wage hikes. For example, Aaronson (2001) looked at restaurant data from Canada and the United States and found that minimum-wage hikes were generally accompanied by increased retail prices, concentrated in the quarter when the legislation is enacted.

Yet besides the implicit regressive tax-and-transfer aspect, there are other *independent* problems with use of the minimum wage as a means to help low-income workers. For example, employers might cut back on other job benefits besides explicit pay in order to reduce the extra wage expense. To see this, imagine a fast food restaurant where the employees initially could take long breaks during their shifts without the manager objecting and could eat their meals for free. After a minimum wage hike, even if the employees all retained their jobs, management might reduce break times and stop "comping" meals taken during a shift. In this type of example, it would be wrong to look at the gross increase in pay as a pure boon to the workers (who retained their jobs), because the quality of the job would have diminished in other dimensions. Empirical studies have documented various ways in which minimum wage hikes can reduce non-monetary job benefits. [8]

## Effect of the minimum wage on unemployment

Finally, let us discuss yet another major problem with reliance on the minimum wage as a means of helping poor workers, which involves the distinction between *employment* and *unemployment*. As we will discuss in our literature review in section 4, most empirical studies of the minimum wage

[8] For example, Neumark and Wascher (2001) found that employers offer less on-the-job training with a higher minimum wage, which matched the findings in earlier studies such as Grossberg and Sicilican (1999), Hashimoto (1982), and Leighton and Mincer (1981). (However, we note that Acemoglu and Pischke (2003) found no effect of minimum wage hikes on job training.) Marks (2011) uses cross-state variation in US minimum wages to find that employers (when free to do so with respect to non-discrimination laws) reduce employer-provided health benefits to low-skilled employees relative to high-skilled employees, following a hike in the minimum wage.

focus on its (possible) effect on the level of *employment*: how many people (in a particular group) have a job or, in some specifications, how many total worker-hours firms are buying.

There are valid reasons for the empirical focus on the level of employment, but we must remember that this concept is distinct from the number of people who are *unemployed*. As economists define the term, a person who is *unemployed* would like to work at the prevailing wage, has the skills to be interchangeable with workers currently earning that wage, [9] is actively seeking work, and yet still cannot find a job.

The distinction between the level of employment and the size of the unemployed labour force is significant in the minimum-wage debate because higher wages can attract more people into the labour pool seeking work. Depending on the relative elasticities of the supply and demand for unskilled labour, a hike in the minimum wage could simultaneously (a) have little measured effect on total employment while (b) greatly increasing unemployment among unskilled workers. Figure 5 illustrates this possibility.

**Figure 5: Hypothetical increase in the minimum wage that raises unemployment while having little effect on total employment**


With the numbers we have chosen for our illustrative example in figure 5, the minimum wage is raised from $11 to $15 per hour. By assumption, the demand curve for unskilled labour is very steep, while the supply curve is far more elastic, such that the reduction in total employment is quite modest—with these numbers, going from 1 million down to 0.999 million, a drop of a

[9] Some discussions of unemployment omit this condition but it is necessary in order to rule out absurdities such as a 17-year-old boy who is very short lamenting that he is "unemployed" because he cannot get hired as a centre for the Los Angeles Lakers—even though he would be perfectly happy to take that job at the prevailing industry wage rate.

mere 1,000 workers, or one one-thousandth of the original level of employment. [10] The typical empirical study described in our literature review in section 4 would assess this hypothetical situation and conclude that even this large increase in the minimum wage had a very modest effect on employment.

However, as figure 5 also indicates, even though the hypothetical minimum wage hike had little effect on total employment, it nonetheless causes a large increase in the number of *unemployed* workers in this sector. In the original equilibrium, to keep things simple we had assumed there were no unemployed workers. That is, 1 million workers wanted a job at $11 per hour, and firms wanted to hire 1 million workers at that wage rate. When the minimum wage is raised to $15 per hour, more people desire work at that higher rate and an additional 250,000 people—who had better things to do when the pay was only $11—enter the labour force, anxious to work for $15. In the new equilibrium, there are a total of 251,000 people who are officially unemployed.

The type of outcome depicted in figure 5 is very important for the policy debate over the minimum wage. If the higher wage attracts new entrants into the labour pool who have higher skills or are more productive than many people in the original pool, the end result might be that the newcomers *displace* the original unskilled workers. For example, at $15 many college students from middle- or upper-income families might take a part-time job at a restaurant, even though they wouldn't have done so when the minimum wage was only $11. If employers in the fast-food sector responded to the new arrangement by bringing in more automation, slightly reducing their total payrolls, and substituting towards more productive workers, many unskilled workers from poor families could lose their jobs (see Neumark and Wascher, 1995 for empirical US estimates of precisely this outcome). This outcome is *exactly the opposite* of what the public expects from a minimum wage hike, and moreover the empirical studies finding "little to no effect on employment" are entirely consistent with this type of result.

In this section, we have shown several mechanisms through which a higher minimum wage could backfire in its intended goal of providing economic support to low-wage workers. [11] In the next section, we will summarize the extensive empirical literature about the specific possible harm of disemployment effects from higher minimum wages.

[10] The elasticity implied by this relatively small reduction in employment is for illustrative purposes only. In reality, the Canadian empirical evidence suggests that a 10% increase in the minimum wage would lead to a 3% to 6% drop in youth employment.

[11] For a thorough review of the various issues and literature from Canadian researchers, see Campoliete and Gunderson, 2010.

# 4 Key Studies on the Effects of the Minimum Wage

There is a voluminous literature that assesses empirically the possible effects of the minimum wage on employment. A full review of the literature lies outside the scope of the present paper; interested readers should consult the 184-page review in Neumark and Wascher (2007) as well as Godin and Veldhuis (2009) for the Canadian literature. For our purposes, we will provide a chronological, narrative review of key papers in the literature, so that the reader can understand the evolution of professional economists' views on the subject. We start with the Canadian literature and then move on to the United States, addressing claims that the so-called "new minimum-wage research" has overturned the traditional consensus.

Before diving into our review, we should alert the reader to an important caveat: The minimum wage literature typically concentrates on overall *employment*—how many workers of certain characteristics have a job, period!, and how the number of employed workers might fall because of a minimum-wage hike. Our review below will reflect this focus on employment itself. However, some researchers argue that another important dimension along which employers might react to an increased minimum wage is to reduce the number of total hours employees can work. Such a reaction would avoid the distasteful elements of outright layoffs, while still allowing the employer to reduce the increase in the total wage bill. Couch and Wittenburg (2001), for example, look at US data and conclude that hours worked by teenagers fall more in response to a minimum wage hike than the more conventional approaches (which look at aggregate employment) would suggest. With this caveat in mind, we now proceed to our review of the empirical literature on the effect minimum wage legislation has on employment.

## Empirical studies of Canadian minimum wage laws

Besides the Canadian research that we already summarized in section 3, there have been many studies of the possible disemployment effect of the minimum wage. The Fraser Institute has published several recent reviews of

this Canadian literature. For example, Godin and Veldhuis (2009) reviewed 14 studies that assessed the impact of Canadian minimum wage hikes on the employment of 15- to 24-year-olds. Godin and Veldhuis summarize in this way:

> The Canadian studies span almost 30 years and can be organized into two groups: (1) 12 studies that examine the impact of increasing the minimum wage on large groups of people who typically earn the minimum wage (i.e., teens and young adults); and, (2) three studies that examine the employment effects on workers who are most directly affected, that is, workers earning a wage that falls in between the old minimum wage and the new minimum wage after a policy change. The results are striking. The first group of studies report employment effects ranging from –0.3 to –0.6, meaning that a 10% increase in the minimum wage will likely decrease employment among those who typically earn minimum wage by 3% to 6%. The second group of studies report employment effects ranging from about –0.45 to –2.0, meaning that a 10% increase in the minimum wage will decrease employment among this smaller, more directly affected group of workers by 4.5% to 20%. (2009: 4)

Veldhuis and Karabegović, 2011 and Lammam, 2014 are Fraser Institute publications that update the review of the latest literature, and assess the impact of various Canadian minimum-wage as well as "living wage" policy proposals. The literature was also recently reviewed in the report by the Ontario government's Minimum Wage Advisory Panel (Ontario, Minimum Wage Advisory Panel, 2014). The results of all three literature reviews are consistent with the 2009 survey.

For example, Sen, Rybczynski, and Van De Waal (2011) looked at the variations in minimum wages among Canadian provinces from 1981 through 2004. They concluded that a 10% increase in the minimum wage was associated with a 3%–5% drop in teen employment, and furthermore that such a minimum-wage hike was associated with a 4%–6% increase in the number of families living below Low-Income Cut-Off (LICO) thresholds. (Both results were statistically significant.) They conclude that "a higher minimum wage may paradoxically result in a significant negative shock to household income among low-income families" (2011: 36).

At this point, we should note two important takeaways: (1) The Canadian empirical results are consistent with the traditional US findings (which we summarize in the next subsection) and in fact exhibit *stronger* disemployment effects among teens and young adults (those aged 15 to 24). Specifically, the original (i.e. during the 1980s) US consensus was that a 10% increase in the minimum wage would reduce employment by 1% to 3%, whereas in Canada

the literature suggests a reduction of 3% to 6%. (2) In light of the Canadian data, it is even more difficult to accept the narrative of the new minimum-wage research that the traditional consensus has been overturned.

For completeness, we note that there are critics of the orthodox Canadian findings. For example, Brennan and Stanford (2014) look at wage and employment data from all provinces from 1983 to 2012, and running various regressions of employment variables against GDP, lagged GDP, and the minimum wage, find "almost no evidence of any connection whatsoever between higher minimum wages and employment levels in Canada" (2014: 5). They conclude that minimum wage hikes in Canada have had no clear effect either way on items such as teenage employment, and that macroeconomic conditions have much more predictive value. However, their method involves a somewhat simplistic approach [12] and presents a weak challenge to the orthodox findings, relative to the sophisticated new minimum-wage research in the United States, which we analyze below.

## The traditional US consensus

Flowing out of work they had done for a government commission report in 1981, Brown, Gilroy, and Kohen (1982) reviewed dozens of time-series and cross-sectional analyses of postwar US employment and wage data. They concluded that the time-series analyses of teenagers typically found that a 10% hike in the minimum wage would result in a 1%–3% reduction in employment, while cross-sectional studies had a wider range in results but were still centered on this finding. They also found that the minimum wage has negative impacts on the employment of young adults (20–24 years), though not so severely as with teenagers. Finally, they found that results were much more mixed in studies that looked at particular low-wage industries; some (such as manufacturing and agriculture) were consistently associated with employment declines because of a minimum-wage increase, while others were not. Economists from across the political spectrum had no reason to doubt this

[12] In particular, Brennan and Stanford (2014) run dozens of regressions, each of which only contains three independent variables (GDP, lagged GDP, and the minimum wage) in order to explain various dependent variables such as employment and the unemployment rate among 15- to 24-year-olds. Each regression is confined to a particular province's data over the period from 1983 to 2012, which reduces the variability in the minimum wage variable. The authors' conclusion derives from the fact that there are few examples where the coefficient on the minimum wage variable is statistically significant. Yet, by the same token, there are three examples in their output table where none of the independent variables has a statistically significant coefficient when explaining this variable. Would Brennan and Stanford conclude that GDP has "no connection whatsoever" to the unemployment rate among 15- to 24-year-olds in these three provinces?

apparent consensus in the empirical literature. After all, it seemed to be confirming that employers buy fewer labour hours when government officials artificially raise the price without corresponding increases in productivity.

## The new case study approach—the US consensus begins to unravel

The traditional consensus had been built on regressions involving large amounts of data, often covering more than a decade of observations from the entire United States. A typical regression would have employment status as the dependent variable, with the independent variables including a measure of the minimum wage, but also variables reflecting the state of the national economy, as well as personal characteristics such as age, sex, race, and educational status. With this framework, regressions would typically find statistically significant negative coefficients on the minimum wage variable, meaning that it seemed to independently harm employment status, however defined (Brown, Gilroy, and Kohen, 1982).

However, Amerian economists began using a new approach in this literature in the 1990s, starting a strand of the literature often referred to as the new minimum-wage research. Originally, it began as a "case study" approach, because it focused on individual episodes of a minimum wage change, to see whether an affected group was harmed *compared to a specified baseline* or "control" group.

Katz and Krueger (1992) analyzed the impact of the 1991 increase in the US federal minimum wage on low-wage workers in Texas. Specifically, they conducted a longitudinal survey of Burger King™, Wendy's™, and KFC™ restaurants in Texas, [13] administering the survey first in December 1990 and then again in July and August of 1991. Contrary to the textbook theory, they found that "employment increased more in those firms likely to have been most affected by the 1991 minimum wage increase than in other firms" (1992: 6).

Card (1992) assessed the impact of California's state-specific increase in the minimum wage in July 1988 from $3.35 to $4.25. Using Current Population Survey (CPS) data, and using regions with similar labour-market characteristics (but in states such as Florida and Texas with no minimum wage hike) as his control group, Card concluded that the California increase "raised the earnings of low-wage workers by 5–10%. Contrary to conventional predictions, however, there was no decline in teenage employment, or any relative loss of jobs in retail trade" (1992: 38).

[13] They did not include McDonald's™ restaurants because none of the locations responded to Katz and Krueger's pre-test survey.

The most famous of the original wave of "case study" or "natural experiment" papers is Card and Krueger (1994), which assessed the impact of the 1992 increase in New Jersey's minimum wage from $4.25 to $5.05. The authors surveyed 410 fast food restaurants in New Jersey and (adjacent) Pennsylvania before and after the hike. They concluded:

> Relative to stores in Pennsylvania, fast food restaurants in New Jersey increased employment by 13 percent. We also compare employment growth at stores in New Jersey that were initially paying high wages (and were unaffected by the new law) to employment changes at lower-wage stores. *Stores that were unaffected by the minimum wage had the same employment growth as stores in Pennsylvania, while stores that had to increase their wages increased their employment.* [14]

It is the last aspect in particular—where Card and Krueger found that high-wage stores in New Jersey and Pennsylvania exhibited similar employment growth—that strengthens the argument that the hike in minimum wages *caused* the (relative) increase in employment among low-wage firms in New Jersey. If, for example, one tries to explain away the main result by saying that New Jersey may have experienced a regional boom in demand that just so happened to coincide with the minimum wage hike, then one might expect *all* stores in New Jersey to experience higher employment growth than their peers in Pennsylvania, including the ones that originally had wages higher than the (new) New Jersey minimum. But to repeat, Card and Krueger found that the relative increase in employment was pronounced among precisely those New Jersey stores upon which the new minimum-wage hike was binding.

Although the new wave of research produced several key studies that challenged the traditional findings, the reader should *not* conclude that *all* "case study" or "natural experiment" approaches disagree with the orthodox textbook treatment. For example, there are several recent papers that use "control group" techniques applied to particular episodes and find that minimum wage hikes *do* hurt employment opportunities among teenagers. Here we will summarize three of them.

Singell and Terborg (2007) use voter referendums in Oregon and Washington State on the minimum wage to conduct "natural experiments". They conclude that "the minimum wage generates consistently negative employment effects for eating and drinking workers where the minimum is shown to be relatively binding, but not for hotel and lodging workers where the minimum is less binding". They also find that "[r]egressions using

[14] This quotation is taken from Card and Krueger, 1993: "Abstract" (emphasis added), the NBER Working Paper version of their celebrated paper, Card and Krueger, 1994.

job-specific want-ad data from Portland and Seattle newspapers also indicate a reduction in hiring solicitation relating to the extent that the minimum wage binds" (2007: 40).

Hoffman and Trace (2009) turn the famous Card-Krueger (1994) result on its head, [15] by analyzing employment trends between Pennsylvania and New Jersey when the federal government raised the national minimum wage and thus *differentially* raised it in Pennsylvania. Specifically, between 1996 and 1997 the federal minimum was raised (in two steps) from $4.25 to $5.15. This corresponded to an increase of 90¢ in Pennsylvania (which always adopted the federal level) but only an effective increase of 10¢ in adjacent New Jersey (which, recall, had already pre-emptively raised its state minimum to $5.05 back in 1992). Hoffman and Trace thus analyzed whether this much larger effective rise in the minimum wage in Pennsylvania affected relevant employment growth compared to New Jersey. Using variations of a "difference-in-difference" approach on individual worker CPS data (excluding fast food stores) in 1995 and then 1998 (before and after the federal hike), they conclude:

> Our NJ–PA comparisons show that the 1996 and 1997 minimum wage increases had a negative effect on employment rates in PA for groups most likely to be affected by the increase. While the effects are not always large and are not always statistically significant, they are always in the same direction and they are often stronger for more narrowly defined groups that are arguably more likely to be affected … Interestingly, PA had slightly higher employment growth over this time period for groups unlikely to have been affected. (Hoffman and Trace, 2009: 116)

Most recently, a working paper by Clemens and Wither (2014) looks at the differential impacts of the 2009 federal minimum-wage increase on states with and without their own minimum wages, with a particular focus on at-risk workers.[16] Here is the background. In July 2009, the US federal minimum wage was increased from $6.55 to $7.25. At the time, this caused roughly half the states to experience a full increase of 70¢, while the other states (which had previously enacted their own, higher minimum wages) experienced increases (on average) of only 10¢.

[15] There have also been papers that challenge the actual Card-Krueger (1994) study directly. For example, Neumark and Wascher (2000), among other criticisms, explain that they could not locate some of the claimed business establishments and that there were problems with the statistical programming Card and Krueger used.

[16] Clemens (2015) followed up Clemens and Wither (2014), using a similar methodology but with different data (the *Current Population Survey* rather than the *Survey of Income and Program Participation*). The results from Clemens (2015) reinforces Clemens and Wither (2014).

In this setting, Clemens and Wither construct two different groups of workers within each state, which could be considered "treatment" and "control" groups. Specifically, they used 12 months of baseline data (from August 2008 through July 2009) to construct groups of low-skilled workers who were in the affected wage range and, for comparison, low-skilled workers who originally were earning wages slightly above the new, higher wage that would prevail in July 2009. Thus, Clemens and Wither had two separate sources of control variation, namely that different states experienced different (effective) increases in the minimum wage, and different groups of low-skilled workers in each state would find the increase either binding or not. Clemens and Wither conclude:

> [W]e find that minimum wage increases significantly reduced the employment of low-skilled workers. By the second year following the $7.25 minimum's implementation [at the federal level], we estimate that targeted workers' employment rates had fallen by 6 percentage points (8%) more in "bound" states than in "unbound" states … In addition to reducing employment, we find a 2 percentage point (12%) increase in the likelihood that targeted individuals work without pay. (Clemens and Wither, 2014: 3–4)

As our selective survey has demonstrated, the new "case study" approach that emerged in the 1990s began to cast serious doubt on the original US consensus. More and more economists became open to the possibility that (at least modest) increases in the minimum wage did *not* necessarily lead to reductions in employment among the relevant groups of workers.

However, even though some of the most famous studies in this genre showed even positive employment effects, this result was *not* unanimous. There are several studies, including very recent ones, using the "case study" approach that validate the original, traditional assessments. Furthermore, critics have pointed out that the "case study" literature often focuses on short-run effects, when in reality the full (and negative) impact of an increased minimum wage might not manifest itself immediately. For example, Baker, Benjamin, and Stanger (1999) studied Canadian data from 1975 to 1993, and concluded that a 10% increase in the minimum wage was associated with a 2.5% decline in teen employment. Perhaps more important, they showed that if they analyzed the same data at "high frequencies", then the apparent effect of the minimum wage vanished. Their conclusion was that the "new minimum wage research" was erroneously missing the actual impact on teen employment because it did not place adequate weight on long-term impacts.

## Generalizing the "case study" US approach

As discussed above, one of the key limitations of the case study or "natural experiment" study design is that it represents, after all, only *one episode* of a particular policy change. Subsequent papers in the "new minimum wage research" genre overcame this limitation by *generalizing* the approach.

A classic paper in this vein is Dube, Lester, and Reich (2010). [17] Rather than focusing on employment and wage data from the entire nation, instead they construct a sample of *counties* that straddle a state border using restaurant data from 1990 to 2006. Using this smaller sample, Dube, Lester, and Reich first reproduce the traditional results, showing that if we only correct for "national" (that is, across all counties in the sample) time-varying trends and county "fixed effects" (which do not vary over time), as well as controlling for total county employment and county population, then it appears that a given county's minimum wage has a negative effect on employment—an elasticity of –0.211, significant at the 5% level when no total private employment control is used, or –0.176 at the 10% level with such a control—in accordance with the traditional findings.

Next, Dube, Lester, and Reich begin introducing more control variables, which allow for heterogeneity in regional, time-varying trends. Specifically, allowing for Census division (each of which includes several states) trends, state-specific linear time-varying trends, and finally Metropolitan Statistical Area (MSA) time-varying trends, the apparent impact of a county's own minimum wage shrinks (in absolute value) more and more. Thus Dube, Lester, and Reich argue that the traditional specifications, which found strong negative effects, were biased because of regional trends in employment that were not adequately controlled for.

Finally, in their preferred specification, Dube, Lester, and Reich match counties with *contiguous counties across the state border*, and allow for county-pair-specific time-varying trends. In this approach, because some counties in various pairs will be in states with different minimum wages, the impact of the minimum wage can be identified, while sweeping away any local economic trends that (presumably) would be unlikely to affect only one county but not its adjacent peer. In this preferred specification, Dube, Lester, and Reich find that employment elasticity is slightly positive (+0.016) though not statistically significant. However, they construct a confidence interval and rule out negative elasticities greater (in absolute value) than –0.147 with 90% confidence.

[17] To show that this is indeed a stand-out contribution in this genre, in Appendix B we will quote Krugman (2015) who singles out Dube, Lester, and Reich (2010) when explaining matter-of-factly to a lay audience that the latest research shows no ill employment effects from minimum wage hikes.

It is understandable why many economists find such results compelling. However, setting aside the elegance of the study design, we point out two facts: First, the "preferred specification" (involving matched contiguous county pairs) of Dube, Lester, and Reich found a slightly positive employment effect, but the result was not statistically significant. Furthermore, by saying that they could rule out employment effects more negative than 1.47%, Dube, Lester, and Reich have hardly overturned the traditional consensus. Recall that Brown, Gilroy, and Kohen (1982) summarized the research at that time by saying that disemployment effects generally fell between 1% and 3% (for a 10% hike). In this light, Dube, Lester, and Reich (2010) does not seem to be such an earth-shattering critique, after all.

## More recent US studies

In line with the new wave of research, Allegretto, Dube, and Reich (2011) continue with the claim that the traditional findings were plagued by insufficient controls for regional economic trends. However, they reach this conclusion *not* through a "matching" approach, but instead starting with the traditional panel-data approach, and adding more specific, localized controls. Specifically, they use CPS data on teenagers from 1990 to 2009, and first find (using traditional specifications with only state- and year-fixed effects) the familiar result of a negative employment effect, namely of –0.118 (significant at 5%). However, when they allow for Census division-specific time effects, as well as a state-specific linear time trend, then the coefficient on the minimum wage variable becomes +0.047 (though not statistically significant). This leads them to conclude:

> Our analysis finds that heterogeneity in employment patterns and selectivity among states constitute significant concerns for conventional minimum wage studies. Although adding division and state trend controls does not constitute a panacea, they provide important controls that mitigate the bias from unobserved heterogeneities that may be correlated with minimum wage changes. Since estimates in previous national-level studies insufficiently address this issue, they do not provide a credible guide for public policy. Interpretations of the quality and nature of the evidence in the existing minimum wage literature, such as those in Neumark and Wascher … must be revised substantially. Put simply, our findings indicate that minimum wage increases—in the range that have been implemented in the United States—do not reduce employment among teens. (Allegretto, Dube, and Reich 2011: 238)

For those following the public debate among economists, it is conclusions such as these that lead proponents of minimum-wage hikes to claim that the old consensus has been overturned. They believe that not only are their new results more accurate, but by reproducing the original consensus findings, economists in this vein think that they have isolated its methodological weakness, and thus feel confident in rejecting the 100+ studies that apparently show a negative employment effect.

To be sure, economists subscribing to the traditional view have hit back in the academic journals. For example, Neumark and Salas (2013) offer several objections to Allegretto, Dube, and Reich (2011), such as showing that by changing the time periods under scrutiny, or by changing the functional form of the state-specific time trend (by allowing it to be a higher order than linear), the minimum wage once again appears to harm employment. This leads Neumark and Salas to comment that Allegretto, Dube, and Reich's "claim that underlying trends that vary by state generate spurious evidence of negative minimum wage effects on teen employment is clearly not true. Rather, only with a very specific form of controlling for this spatial heterogeneity" (2013: 14) do the negative effects of minimum wage laws seem to evaporate.

Other recent critics of the new research have emphasized the long-run dynamic impacts of minimum-wage hikes. For example, Meer and West (2013) argue that it may be more appropriate for researchers to look at the minimum wage's impact on the *growth* of employment, rather than the *level* of employment. Meer and West conduct a simulation showing that the practice of including state-specific time trends in a regression can make it appear as if the minimum wage has little effect on the *level* of employment, even if (by construction) the true effect of the minimum wage is to reduce *new* hires by expanding establishments. In a similar fashion, Sorkin (2015) uses a dynamic industry model of labour demand to argue that the apparent findings of little employment effect are merely *short-run* estimates, while the true long-run elasticity of the minimum wage is significantly negative.

The single best review upholding the traditional view is Neumark and Wascher (2007), which thoroughly surveys more than 100 studies published since the onset of the new minimum-wage research, including results from outside the United States. For our purposes, we will simply quote from their concluding section:

> Our lengthy review of the new minimum wage research documents the wide range of estimates of the effects of the minimum wage on employment, especially when compared to the review of the earlier literature by Brown et al. (1982). For example, few of the studies in the Brown et al. survey were outside of the consensus range of –0.1 to –0.3 for the elasticity of teenage employment with respect to the minimum wage. In contrast, even limiting the sample of studies to

> those focused on the effects of the minimum wage of teenagers in the United States, the range of studies comprising the new minimum wage research extends from near –1 to above zero. This wider range for the United States undoubtedly reflects both the new sources of variation used to identify minimum wage effects—notably the greater state-level variation in minimum wages—and the new approaches and methods used to estimate these effects. And, the range would be considerably wider if we were to include estimates for narrower subsets of workers and industries or estimates from other countries.
>
> Nonetheless, the oft-stated assertion that the new minimum wage research fails to support the conclusion that the minimum wage reduces the employment of low-skilled workers is clearly incorrect. Indeed, in our view, the preponderance of the evidence points to disemployment effects. For example, the studies surveyed in this monograph correspond to 102 entries in our summary tables. Of these, nearly two-thirds give a relatively consistent (although by no means always statistically significant) indication of negative employment effects of minimum wages, while only eight give a relatively consistent indication of positive employment effects. In addition, we have highlighted in the tables 33 studies (or entries) that we regard as providing the most credible evidence, and 28 (85 percent) of these point to negative employment effects. Moreover, when researchers focus on the least-skilled groups most likely to be adversely affected by minimum wages, the evidence for disemployment effects seems especially strong. In contrast, we see very few—if any—cases where a study provides convincing evidence of positive employment effects of minimum wages, especially among the studies that focus on broader groups for which the competitive model predicts disemployment effects. (Neumark and Wascher, 2007: 163–164)

Thus, although the original consensus has been *challenged*—in some cases, elegantly so—it is flatly incorrect to assert (as some economists have done) that the debate has been settled. There have been many dozens of studies confirming the original consensus, all published in the wake of the new minimum wage research. Only by focusing on very particular studies showing modest or even positive effects, and ignoring the volumes of studies finding negative effects, can one be fooled into thinking the textbook results have been definitively overturned.

In Appendix B, we summarize two very recent and technical contributions to the literature, in the form of "refereeing" specific arguments between leaders from both camps. This discussion will give the reader a taste of the cutting-edge disputes, and shed light on how it could be that professional economists reach such diametrically opposed conclusions from the same

underlying data. However, the lesson for our purposes is that the disemployment effects from minimum wage hikes in the United States are still an open empirical question, even though some pro-minimum wage economists have led the public to believe that the old findings were decisively debunked.

## Conclusions from the review of the empirical literature

By the early 1980s, a consensus had emerged among economists that US data had convincingly supported the textbook arguments showing that minimum wage hikes led to disemployment, particularly among teenagers. Specifically, various studies had generally found that a 10% increase in the minimum wage was associated with a 1% to 3% reduction in employment among teenagers. These results relied on large time series analyses, with controls added for obvious factors such as recessions, favourable climates in certain states, and so forth. To this day, and including many countries besides the United States, econometric analyses adopting such an approach tend to find statistically significant disemployment effects. Many economists find this outcome to be quite reasonable: it shows that employers tend to buy fewer hours of unskilled labour when legislators make it artificially more expensive.

However, there is a different strand in the minimum wage literature, which began in the early 1990s. Rather than running regressions on large amounts of data covering huge regions and many years of experience, the new strand used a "case study" approach. At first, such as in the famous Card-Krueger (1994) study, researchers looked at individual case studies, comparing a region hit by a minimum wage hike with other regions that were considered to be comparable control groups. Although some case study approaches did indeed find the traditional disemployment effects, there were several popular examples that apparently violated the standard findings. Many economists disputed the new findings, pointing out potential drawbacks to the study designs, which included (of course) the fact that a case study was simply *one episode* which, in principle, might be driven by any number of outside factors.

The case-study approach was then generalized into a method that apparently enjoyed the best of both worlds by combining the broader sample size of the time-series approach with methods for constructing control groups. A classic example is Dube, Lester, and Reich (2010). Papers in this genre seemed to demonstrate that the one-off result in Card-Krueger was true *in general*. They explained away the previous consensus as being due to the non-random distribution of federal minimum-wage hikes and states that imposed their own, higher minimum wages. The researchers in this new

minimum-wage literature argued that regional trends in the economy—which had nothing to do with the minimum wage—were hampering employment and that this coincidentally tended to occur in areas of the country with higher minimum wages.

Although many economists—especially including those who tend to favour government intervention in other areas—have concluded that the new approach in minimum-wage research is clearly superior to the traditional approach, this is still an open empirical dispute. In particular, the leaders in this camp have pushed back strongly on some of the pillars of the new approach. For example, Neumark, Salas, and Wascher (2014b) summarize key arguments that undercut the construction of control groups (see Appendix B for more details) and show how the findings of "little effect" are often driven by very particular choices of data and functional forms. More generally, the exhaustive survey of the literature in Neumark and Wascher (2007) shows that several dozen high-quality studies, published since the onset of the new wave of research, are consistent with the traditional consensus.

When it comes to Canadian data, the traditional results are even more pronounced: studies regularly find that an increase in the minimum wage causes significant disemployment effects, especially among teenagers. Although there are detractors from this view, in general the Canadian results make it hard to accept that the traditional US consensus was the result of a geographical coincidence. Furthermore, many economists consider the Canadian evidence to be stronger, because of substantial variation in the minimum wage across provinces and over time. This stands in contrast to the older US time series studies where the minimum wage would typically only change infrequently and at the federal level. [18]

If professional economists publishing in peer-reviewed journals currently cannot agree on the interpretation of the decades of employment and wage data drawn from several countries, then policy makers can hardly be expected to wade into the literature to draw their own firm conclusions. However, the following four propositions should be uncontroversial:

1 The "law of demand" states that an increase in price will lead (other things equal) to a reduction in the quantity desired for purchase. There are several theoretical reasons that this result might not apply (or might apply but very weakly in size) to unskilled labour markets but we should not pretend that we are a rudderless ship adrift at sea. Absent compelling evidence to the

[18] For example, Baker, Benjamin, and Stanger (1999: 319, fn 4) explain the superiority of their Canadian data set compared to the US experience, citing not only the richer variability in the minimum wage levels across provinces, but also the fact that the minimum wage coverage is more nearly universal in Canada.

contrary, it makes perfect *sense* to suppose that an increase—especially a large one—in the minimum wage will lead employers to reduce the amount of labour-hours they wish to purchase.

**2** In the 1980s, the consensus among economists was that the empirical research had confirmed the textbook theory: minimum-wage hikes were associated with lower employment of unskilled workers. Since then, many dozens of studies have followed the traditional design, using the most recent data, and they generally (though not universally) agree with the original consensus: increases in the minimum wage are associated with lower employment of unskilled workers.

**3** Since the 1990s, a new approach emerged in the minimum-wage literature, relying on matching "treatment" groups (which experienced a minimum wage hike) with "control" groups that were thought to be more representative of the baseline than the entire sample. The general (though not universal) conclusion from this new approach is that minimum wage hikes are *not* associated with serious disemployment effects.

**4** Many economists, including some of the most published in this field, dispute the validity of the particular methods used in this new genre of papers. Yet, even if we accept the findings of the new literature at face value, their authors typically conclude that *modest* hikes in the minimum wage have no serious disemployment effects. There is quite simply no empirical rationale for extrapolating from these papers the idea that *large* increases in the minimum wage—including the larger policy moves being seriously proposed in both Canadian provinces and the United States—will have no deleterious effect.

Policymakers must be very cautious when considering large minimum wage hikes. They certainly should *not* believe that the latest research gives a green light to large policy shifts; economists saying otherwise are exaggerating what the literature actually contains.

# 5 A Better Way to Help Workers from Low-Income Households—the Working Income Tax Benefit

In section 3, we outlined several possible mechanisms through which an increase in the minimum wage might perversely harm low-income workers, the very group the public intends to help. In Section 4, we surveyed the empirical literature to quantify some of these possible harms, and found that the Canadian research presented strong evidence that the minimum wage had reduced employment among 15- to 24-year-olds. However, many people still support the minimum wage, presumably because they believe that the benefit it confers—namely, higher wages—on certain workers outweighs the possible harms. This raises the question: Is there a better option?

Indeed there is. Most economists agree that the Working Income Tax Benefit (WITB)—and its American analog, the Earned Income Tax Credit (EITC)—represent a mechanism preferable to the minimum wage, if the goal is to boost the income of poor workers. [17] To demonstrate that there really is broad support for a mechanism of this sort among economists across the political spectrum, we will quote liberally from a *New York Times* column by Professor Christina Romer of the University of California at Berkeley, who was the Chair of the Council of Economic Advisors for the newly-elected President Obama in January 2009 (up through September 2010). In her article (2013), Romer explains:

> Raising the minimum wage, as President Obama proposed in his State of the Union address, tends to be more popular with the general public than with economists.

[17] In Canada, individuals and organizations across the political spectrum support the WITB. For example, the program was implemented by a federal government led by the Conservative Party, and former New Democrat Party leader and Chair of the Broadbent Institute, Ed Broadbent, called for an expansion of WITB in a submission to a Parliamentary committee (Broadbent, 2013).

> I don't believe that's because economists care less about the plight of the poor … Rather, economic analysis raises questions about whether a higher minimum wage will achieve better outcomes for the economy and reduce poverty.
>
> … Though a desire to help the poor is largely a moral issue, economics can help us think about how successful a higher minimum wage would be at reducing poverty.
>
> … Some evidence suggests that employment doesn't fall much because the higher minimum wage lowers labor turnover, which raises productivity and labor demand. But it's possible that productivity also rises because the higher minimum attracts more efficient workers to the labor pool. If these new workers are typically more affluent—perhaps middle-income spouses or retirees—and *end up taking some jobs held by poorer workers, a higher minimum could harm the truly disadvantaged*.
>
> Another reason that employment may not fall is that businesses pass along some of the cost of a higher minimum wage to consumers through higher prices. Often, the customers paying those prices—including some of the diners at McDonald's and the shoppers at Walmart—have very low family incomes. Thus *this price effect may harm the very people whom a minimum wage is supposed to help*.
>
> It's precisely because the redistributive effects of a minimum wage are complicated that *most economists prefer other ways to help low-income families. For example, the current tax system already subsidizes work by the poor via an earned-income tax credit …*
>
> … If a higher minimum wage were the only anti-poverty initiative available, I would support it …
>
> But we could do so much better if we were willing to spend some money. *A more generous earned-income tax credit would provide more support for the working poor and would be pro-business at the same time.* (Romer, 2013; emphasis added)

As the quotation from Christina Romer indicates, most economists in the United States consider an expansion of the Earned Income Tax Credit (EITC) to be preferable to an increase in the minimum wage, as a way of helping poor workers. In Canada, the analogous policy would be an expansion of the Working Income Tax Benefit (WITB). We now provide a brief explanation of these programs.

## Working Income Tax Benefit (Canada) and the Earned Income Tax Credit (United States)

A fundamental problem with government subsidies to low-income individuals is that they can give rise to a "welfare wall", in which poor workers face extraordinarily high marginal income-tax rates over certain ranges, effectively taking away their incentive to work. [18] For example, in Canada a person who originally has no income may be receiving explicit monetary benefits in the form of welfare payments, as well as in-kind benefits such as housing assistance. Suppose the total value of these explicit and implicit subsidies amounts to $5,000 in the case of this person. Now, if the amount of these benefits decreases dollar-for-dollar (or even more, when the two are combined) with increased income, then the person has no financial incentive at all to work, at least for modest ranges of income. He effectively faces a marginal income-tax rate of 100% or more, meaning that his potential standard of living may not improve as he boosts his income from $0 to (say) $5,000. Thus, a poorly designed welfare program can make it less likely that people will escape from poverty, as they lose the short-term rewards for taking an initial job (which probably does not pay very well) in order to develop work experience and skills. [19]

The structure of the Earned Income Tax Credit (EITC), introduced in the United States in 1975, and the Working Income Tax Benefit (WITB), unveiled in 2007 in Canada, was explicitly designed to overcome such problems. [20] They both featured a refundable tax credit tied to low ranges of income, but where the benefit was only gradually phased out in order to maintain the incentive for extra work. They also allowed for different permutations for singles, couples, parents with varying numbers of children, and workers with disabilities. (Note that some of these features only apply to the WITB or the EITC, not necessarily to both of them.)

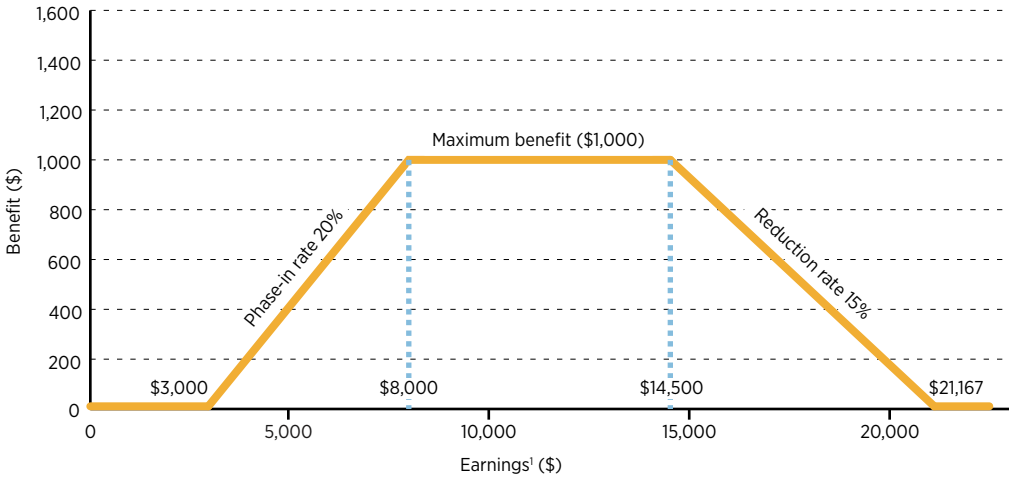
---

[18] The term "welfare wall" is often used by policy analysts and appeared in the Government of Canada's The Budget Plan 2007, in which the Working Income Tax Benefit was introduced (Canada, Department of Finance, 2007: 78).

[19] Randolph (2014) makes a similar point regarding the seemingly generous system of benefits available to low-income households in the state of Illinois. One of its conclusions is that a single parent faces "a tremendous disincentive to seek work that pays more, essentially trapping single parents between the minimum wage and $12 per hour. It is unlikely that persons in this situation would be able to triple their incomes in order to recover lost benefits from the cliff. The system subsequently discourages any natural effort on the part of the parent to seek a better paying job or to advance her situation, contrary to what market forces would incentivize if left untampered" (2014: 13).

[20] For a more detailed discussion of the EITC and WITB, see Clemens, Veldhuis, and Murphy, 2013: 4–9, 31–34.

**Figure 6: Entitlement to Working Income Tax Benefit, single parents and couples (2007)**




Note: Phase-out based on net family income.
Source: Figure based on Chart 3.2, in: Canada, Department of Finance (2007). *The Budget Plan 2007*. Cat. No. F1-23/2007-3E. <http://www.budget.gc.ca/2007/pdf/bp2007e.pdf>, page 80.

To understand how the WITB works, it will be instructive to study the specifics of the 2007 WITB, as it pertained to single parents and couples. Figure 6 outlines its mechanics. The new WITB allowed low-income Canadian single parents and couples in 2007 to reduce their income tax liability (and even receive the balance directly from the government, if the WITB benefit was more than what they otherwise owed). Specifically, once income reached $3,000, the Canadian government would begin "paying" (in the sense of a tax credit) the worker $20 for every additional $100 that the worker earned in market income. Thus the value of the WITB was not a flat benefit amount, but would actually be higher, the more a worker earned. In this way, the WITB encouraged low-wage workers to earn higher incomes.

However, the WITB benefit reached its maximum at $1,000. In other words, once the worker's income hit $8,000, the Canadian government would no longer offer additional "payments" (in the form of a tax credit) when the worker earned even more income. Yet, in order to prevent millionaires from enjoying the maximum $1,000 subsidy as well, the WITB eventually began phasing out, specifically at an income of $14,500. Once the worker had reached this threshold, the Canadian government would reduce the total WITB benefit by $15 for every additional $100 in market income. The WITB benefit eventually reached $0 by the time the worker earns $21,167 in market income. [21]

[21] Note that (with rounding) $21,167 – $14,500 = $6,667, and $6,667 × 15% = $1,000, the maximum WITB benefit in 2007.

To be sure, the WITB in Canada and the EITC in the United States are not perfect. They both still penalize additional work effort, over and above conventional income and payroll taxes, in the relevant phase-out range. For example, as figure 6 suggests, the relevant Canadian workers in 2007 effectively faced an additional 15% marginal tax on their labour income, for incomes in the range of $14,500 to $21,167. This implicit surtax on lower-income workers is at odds with the spirit of the program, and it could be mitigated by having a lower phase-out rate (such as 10% or 5%). However, the lower the phase-out rate, the higher the income at which a worker receives some WITB benefit, meaning that it no longer targets "the poor" as effectively.

Another flaw with the use of the WITB (and the EITC in the United States) is that it could contribute to the growth of government spending by increasing the fraction of the citizenry that pays very little in net taxes. This was the focus of Clemens, Veldhuis, and Murphy (2013), who reported (among other statistics) that considering all taxes—not just income—across federal, province, and local levels, the lowest quintile of income earners in Canada paid only 2% of total tax receipts in 2012. Such a scenario can make it difficult to limit the growth of inefficient (from a social standpoint) government spending, when a large group of voters enjoys the benefits but bears very little of the cost.

Notwithstanding these inevitable tradeoffs in the precise structure of the WITB, most economists consider it a very effective program for the purpose of using government policy to encourage labour supply while boosting incomes among the poor. There are numerous studies documenting the effectiveness of the EITC in the United States in benefiting its recipients. [22] For example, in a 2015 literature review, Nichols and Rothstein conclude:

> Researchers have documented beneficial effects on poverty, on consumption, on health, and on children's academic outcomes. The magnitude of these effects is large: Millions of families are brought above the poverty line, and estimates of the effects on children indicate that this may have extremely important effects on the intergenerational transmission of poverty as well. Taking all of the evidence together, the EITC appears to benefit recipients—and especially their children—substantially. (Nichols and Rothstein, 2015: 5)

[22] There are several papers that evaluate the efficacy of the EITC in its goals of aiding poor families while boosting the incentive to work. A recent summary is Marr, Huang, Sherman, and DeBot (2015). For other examples, see: Holt, 2011; Annie E. Casey Foundation, 2008; Eissa and Hoynes, 1998 2005, 2008. A summary of Eissa and Hoynes, 2005 is available at <http://www.nber.org/digest/aug06/w11729.html>.

To be sure, the EITC (and WITB in Canada) does spill over into effectively subsidizing businesses that employ eligible workers. However, the WITB, in contrast to an increased minimum wage, allows policy makers to be much more precise when targeting particular groups—in this case, workers from low-income households—for assistance.

Since their inceptions (in 1975 and 2007, respectively), the American EITC and Canadian WITB have been modified in several ways, with adjustments made to the benefit amounts, eligibility requirements, threshold levels, and so forth. (The WITB maximum benefit was most recently increased in 2009.) The relevance for this particular study is that policy makers who wish to tailor government policy to assist the working poor would be far more effective if they expanded these programs rather than raising the minimum wage. This is a point on which a broad spectrum of economists agree.

# 6 Conclusion

This study has cast considerable doubt upon the use of the minimum wage as a policy device for aiding workers from low-income households. In section 2, we used the most recent Canadian data to show that "the working poor" are not the same group as "minimum wage earners". In particular, only 12.5% of minimum wage earners (in 2012) fell below the Low Income Cut-Off (LICO) threshold, while at the same time a whopping 83.4% of workers who fell below the LICO threshold earned more than the minimum wage. When tied with other consequences of minimum wage hikes (such as the possibility of bringing in new workers who "crowd out" the original workers), these facts show that the minimum wage is at best a very blunt instrument for helping the working poor.

Furthermore, potentially one of the worst consequences of increases in the minimum wage are the possible disemployment effects among the very workers the public wants to help. In section 4, we reviewed the enormous empirical literature and found that the bulk of it confirmed textbook logic: by making labour artificially more expensive, increases in the minimum wage reduced employment among teenagers and other groups of low-skilled and low-productivity workers. The results from Canadian data were even stronger than for the United States.

It is true that a wave of new minimum-wage research emerged in the United States in the 1990s, which challenged the original consensus. However, experts in the field are still hotly disputing these findings. At best, outsiders can only say that economists no longer hold a consensus on the employment effects of minimum wage hikes. Even so, policy makers should be aware that even the "revisionist" studies conclude only that *modest* increases in the minimum wage have little ill effect on employment. Some of the more aggressive minimum and "living" wage proposals in Canada and the United States fall well outside the ranges studied in the empirical literature, meaning that we have no basis on which to predict that they will have benign impacts on employment.

Finally, we showed policy makers an alternative measure, namely enhancing the Working Income Tax Benefit (WITB), which is supported by economists across the political spectrum. This program allows workers from

low-income households to keep more of their earnings, effectively subsidizing them to work more. Unlike traditional welfare programs, the WITB is specifically designed to minimize the disincentive effects that can occur when government assistance is removed at higher income levels. By more accurately targeting the desired individuals and avoiding price controls, the WITB is a much more sensible approach to channeling resources to the aid of the working poor.

# Appendix A: Demographics of Minimum Wage Earners in Canada and the Provinces



**Notes**: The sum of persons with a working or non-working spouse does not add up to the total number of persons with spouses since certain spouses may have been outside the target group. This happens when one spouse is a member of the armed forces or is in an institution (and there may be other reasons). The *Labour Force Survey* only collects information on the spouse not out of scope. • The question concerning educational status was not asked of persons 65 and over. For this reason, the sum of the totals beased on educational status for "non-family persons" and "son, daughter or other relative living with the family" is not exactly equal to the total number of presons in those two categories. • A low-income household is defined as one that is below the Low Income Cut-Off (after taxes and government transfers). The low-income data come from the *Canadian Income Survey* (CIS). The latest year of data available at the time of writing was 2012. • "F" in the tables indicates that data is not available.

**Sources**: Statistics Canada, special request from Income Statistics Division using data from the *Labour Force Survey*; received February 20, 2015. • Statistics Canada, special request from Income Statistics Division using data from the *Canadian Income Survey*; received February 20, 2015.

## Table A1: Demographics of Minimum Wage Earners in Canada, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 15,076.9 | 100.0 | 1,087.8 | 100.0 | 7.2 |
| **Age** | | | | | |
| *15 to 19* | 818.5 | 5.4 | 395.8 | 36.4 | 48.4 |
| *20 to 24* | 1,590.3 | 10.5 | 239.2 | 22.0 | 15.0 |
| *25 to 34* | 3,430.7 | 22.8 | 136.0 | 12.5 | 4.0 |
| *35 to 44* | 3,252.9 | 21.6 | 101.2 | 9.3 | 3.1 |
| *45 to 54* | 3,430.9 | 22.8 | 105.3 | 9.7 | 3.1 |
| *55 to 64* | 2,165.7 | 14.4 | 82.1 | 7.5 | 3.8 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 1,406.5 | 9.3 | 284.9 | 26.2 | 20.3 |
| *Highschool diploma* | 3,097.8 | 20.5 | 303.2 | 27.9 | 9.8 |
| *At least some post-secondary* | 10,572.6 | 70.1 | 499.8 | 45.9 | 4.7 |
| Some post-secondary | 1,066.9 | 7.1 | 173.4 | 15.9 | 16.3 |
| Post-secondary diploma or certificate | 5,432.8 | 36.0 | 203.3 | 18.7 | 3.7 |
| University degree | 4,073.0 | 27.0 | 123.1 | 11.3 | 3.0 |
| **Job status** | | | | | |
| *Full-time* | 12,298.5 | 81.6 | 455.6 | 41.9 | 3.7 |
| *Part-time* | 2,778.4 | 18.4 | 632.2 | 58.1 | 22.8 |
| **Household status** | | | | | |
| *Member of a couple* | 8,685.1 | 57.6 | 286.1 | 26.3 | 3.3 |
| Spouse not employed | 1,639.3 | 10.9 | 68.4 | 6.3 | 4.2 |
| *Youngest child is less than 18 years* | 655.1 | 4.3 | 24.1 | 2.2 | 3.7 |
| *No children or youngest child 18 or older* | 984.2 | 6.5 | 44.4 | 4.1 | 4.5 |
| Spouse employed | 7,012.3 | 46.5 | 216.1 | 19.9 | 3.1 |
| *Spouse making minimum wage or less* | 180.4 | 1.2 | 18.7 | 1.7 | 10.4 |
| *Spouse making greater than minimum wage* | 5,856.3 | 38.8 | 161.7 | 14.9 | 2.8 |
| *Spouse self-employed* | 975.6 | 6.5 | 35.7 | 3.3 | 3.7 |
| *Head of household, no spouse* | 1,075.3 | 7.1 | 62.1 | 5.7 | 5.8 |
| Youngest child is less than 18 years | 499.3 | 3.3 | 24.0 | 2.2 | 4.8 |
| No children or youngest child 18 or older | 576.0 | 3.8 | 38.1 | 3.5 | 6.6 |
| *Son, daughter, or other relative living with family* | 2,841.9 | 18.8 | 618.4 | 56.8 | 21.8 |
| 15–19 in school | 449.0 | 3.0 | 245.7 | 22.6 | 54.7 |
| 15–19 not in school | 311.2 | 2.1 | 126.8 | 11.7 | 40.7 |
| 20–24 in school | 268.6 | 1.8 | 65.7 | 6.0 | 24.5 |
| 20–24 not in school | 672.1 | 4.5 | 100.6 | 9.2 | 15.0 |
| 25 or over in school | 65.1 | 0.4 | 5.3 | 0.5 | 8.1 |
| 25 or over not in school | 1,076.0 | 7.1 | 74.4 | 6.8 | 6.9 |
| *Single* | 2,456.0 | 16.3 | 120.0 | 11.0 | 4.9 |
| Living alone | 1,654.9 | 11.0 | 62.2 | 5.7 | 3.8 |
| *15–24* | 124.0 | 0.8 | 13.2 | 1.2 | 10.6 |
| *25–54* | 1,126.2 | 7.5 | 30.2 | 2.8 | 2.7 |
| *55 and over* | 404.6 | 2.7 | 18.7 | 1.7 | 4.6 |
| Living with non-relatives | 801.2 | 5.3 | 57.8 | 5.3 | 7.2 |
| *15–24* | 234.4 | 1.6 | 30.3 | 2.8 | 12.9 |
| *25–54* | 515.6 | 3.4 | 24.8 | 2.3 | 4.8 |
| *55 and over* | 51.3 | 0.3 | 2.7 | 0.2 | 5.3 |
| *Member of a low-income household (2012)* | 700.0 | 4.9 | 116.0 | 12.5 | 16.6 |

## Table A2: Demographics of Minimum Wage Earners in British Columbia, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 1,882.5 | 100.0 | 110.4 | 100.0 | 5.9 |
| **Age** | | | | | |
| *15 to 19* | 103.0 | 5.5 | 37.0 | 33.5 | 35.9 |
| *20 to 24* | 202.2 | 10.7 | 22.3 | 20.2 | 11.0 |
| *25 to 34* | 431.8 | 22.9 | 14.8 | 13.4 | 3.4 |
| *35 to 44* | 403.6 | 21.4 | 11.1 | 10.1 | 2.8 |
| *45 to 54* | 416.8 | 22.1 | 13.2 | 12.0 | 3.2 |
| *55 to 64* | 278.7 | 14.8 | 8.7 | 7.9 | 3.1 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 139.4 | 7.4 | 28.0 | 25.4 | 20.1 |
| *Highschool diploma* | 430.2 | 22.9 | 31.7 | 28.7 | 7.4 |
| *At least some post-secondary* | 1,312.9 | 69.7 | 50.7 | 45.9 | 3.9 |
| Some post-secondary | 167.1 | 8.9 | 17.7 | 16.0 | 10.6 |
| Post-secondary diploma or certificate | 634.6 | 33.7 | 19.1 | 17.3 | 3.0 |
| University degree | 511.2 | 27.2 | 13.9 | 12.6 | 2.7 |
| **Job status** | | | | | |
| *Full-time* | 1,497.1 | 79.5 | 47.9 | 43.4 | 3.2 |
| *Part-time* | 385.4 | 20.5 | 62.5 | 56.6 | 16.2 |
| **Household status** | | | | | |
| *Member of a couple* | 1,041.4 | 55.3 | 27.8 | 25.2 | 2.7 |
| Spouse not employed | 210.2 | 11.2 | 6.6 | 6.0 | 3.1 |
| *Youngest child is less than 18 years* | 81.5 | 4.3 | 2.1 | 1.9 | 2.6 |
| *No children or youngest child 18 or older* | 128.7 | 6.8 | 4.5 | 4.1 | 3.5 |
| Spouse employed | 827.7 | 44.0 | 21.0 | 19.0 | 2.5 |
| *Spouse making minimum wage or less* | 17.0 | 0.9 | F | F | F |
| *Spouse making greater than minimum wage* | 678.7 | 36.1 | 15.8 | 14.3 | 2.3 |
| *Spouse self-employed* | 132.0 | 7.0 | 4.0 | 3.6 | 3.0 |
| *Head of household, no spouse* | 134.7 | 7.2 | 7.8 | 7.1 | 5.8 |
| Youngest child is less than 18 years | 58.3 | 3.1 | 2.3 | 2.1 | 3.9 |
| No children or youngest child 18 or older | 76.5 | 4.1 | 5.6 | 5.1 | 7.3 |
| *Son, daughter, or other relative living with family* | 362.1 | 19.2 | 57.8 | 52.4 | 16.0 |
| 15–19 in school | 49.9 | 2.7 | 20.9 | 18.9 | 41.9 |
| 15–19 not in school | 43.2 | 2.3 | 13.1 | 11.9 | 30.3 |
| 20–24 in school | 30.8 | 1.6 | 5.3 | 4.8 | 17.2 |
| 20–24 not in school | 80.8 | 4.3 | 7.9 | 7.2 | 9.8 |
| 25 or over in school | 10.1 | 0.5 | F | F | F |
| 25 or over not in school | 147.2 | 7.8 | 9.4 | 8.5 | 6.4 |
| *Single* | 341.0 | 18.1 | 16.6 | 15.0 | 4.9 |
| Living alone | 216.2 | 11.5 | 7.6 | 6.9 | 3.5 |
| *15–24* | 17.6 | 0.9 | 1.9 | 1.7 | 10.8 |
| *25–54* | 147.5 | 7.8 | 3.5 | 3.2 | 2.4 |
| *55 and over* | 51.2 | 2.7 | 2.1 | 1.9 | 4.1 |
| Living with non-relatives | 124.8 | 6.6 | 9.1 | 8.2 | 7.3 |
| *15–24* | 38.1 | 2.0 | 4.8 | 4.3 | 12.6 |
| *25–54* | 79.3 | 4.2 | 3.9 | 3.5 | 4.9 |
| *55 and over* | 7.3 | 0.4 | F | F | F |
| *Member of a low-income household (for 2012)* | 99.0 | 5.6 | F | 11.2 | F |

## Table A3: Demographics of Minimum Wage Earners in Alberta, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 1,890.7 | 100.0 | 32.5 | 100.0 | 1.7 |
| **Age** | | | | | |
| *15 to 19* | 105.5 | 5.6 | 10.3 | 31.7 | 9.8 |
| *20 to 24* | 205.2 | 10.9 | 5.5 | 16.9 | 2.7 |
| *25 to 34* | 498.9 | 26.4 | 6.0 | 18.5 | 1.2 |
| *35 to 44* | 415.2 | 22.0 | 4.0 | 12.3 | 1.0 |
| *45 to 54* | 376.3 | 19.9 | 2.5 | 7.7 | 0.7 |
| *55 to 64* | 246.1 | 13.0 | 2.8 | 8.6 | 1.1 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 197.6 | 10.5 | 11.3 | 34.8 | 5.7 |
| *Highschool diploma* | 456.3 | 24.1 | 9.0 | 27.7 | 2.0 |
| *At least some post-secondary* | 1,236.8 | 65.4 | 12.2 | 37.5 | 1.0 |
| Some post-secondary | 118.7 | 6.3 | 3.0 | 9.2 | 2.5 |
| Post-secondary diploma or certificate | 633.8 | 33.5 | 5.4 | 16.6 | 0.9 |
| University degree | 484.3 | 25.6 | 3.8 | 11.7 | 0.8 |
| **Job status** | | | | | |
| *Full-time* | 1,594.1 | 84.3 | 15.4 | 47.4 | 1.0 |
| *Part-time* | 296.6 | 15.7 | 17.2 | 52.9 | 5.8 |
| **Household status** | | | | | |
| *Member of a couple* | 1,042.9 | 55.2 | 10.8 | 33.2 | 1.0 |
| Spouse not employed | 195.2 | 10.3 | 2.2 | 6.8 | 1.1 |
| *Youngest child is less than 18 years* | 97.4 | 5.2 | F | F | F |
| *No children or youngest child 18 or older* | 97.8 | 5.2 | F | F | F |
| Spouse employed | 843.6 | 44.6 | 8.5 | 26.2 | 1.0 |
| *Spouse making minimum wage or less* | 6.5 | 0.3 | F | F | F |
| *Spouse making greater than minimum wage* | 709.0 | 37.5 | 6.1 | 18.8 | 0.9 |
| *Spouse self-employed* | 128.2 | 6.8 | 2.0 | 6.2 | 1.6 |
| *Head of household, no spouse* | 134.0 | 7.1 | F | F | F |
| Youngest child is less than 18 years | 56.7 | 3.0 | F | F | F |
| No children or youngest child 18 or older | 77.3 | 4.1 | F | F | F |
| *Son, daughter, or other relative living with family* | 342.3 | 18.1 | 16.2 | 49.8 | 4.7 |
| 15–19 in school | 52.9 | 2.8 | 7.5 | 23.1 | 14.2 |
| 15–19 not in school | 42.2 | 2.2 | 2.4 | 7.4 | 5.7 |
| 20–24 in school | 23.0 | 1.2 | F | F | F |
| 20–24 not in school | 78.3 | 4.1 | 2.5 | 7.7 | 3.2 |
| 25 or over in school | 7.3 | 0.4 | F | F | F |
| 25 or over not in school | 138.7 | 7.3 | 2.3 | 7.1 | 1.7 |
| *Single* | 367.9 | 19.5 | 4.1 | 12.6 | 1.1 |
| Living alone | 195.0 | 10.3 | 1.7 | 5.2 | 0.9 |
| *15–24* | 14.8 | 0.8 | F | F | F |
| *25–54* | 137.5 | 7.3 | F | F | F |
| *55 and over* | 42.7 | 2.3 | F | F | F |
| Living with non-relatives | 172.9 | 9.1 | 2.4 | 7.4 | 1.4 |
| *15–24* | 50.5 | 2.7 | F | F | F |
| *25–54* | 115.0 | 6.1 | F | F | F |
| *55 and over* | 7.3 | 0.4 | F | F | F |
| *Member of a low-income household (for 2012)* | 70.0 | 3.9 | F | F | F |

## Table A4: Demographics of Minimum Wage Earners in Saskatchewan, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 460.2 | 100.0 | 15.9 | 100.0 | 3.5 |
| **Age** | | | | | |
| *15 to 19* | 29.7 | 6.5 | 6.0 | 37.7 | 20.2 |
| *20 to 24* | 52.2 | 11.3 | 3.3 | 20.8 | 6.3 |
| *25 to 34* | 114.0 | 24.8 | 2.2 | 13.8 | 1.9 |
| *35 to 44* | 92.1 | 20.0 | 1.3 | 8.2 | 1.4 |
| *45 to 54* | 94.2 | 20.5 | 1.2 | 7.5 | 1.3 |
| *55 to 64* | 64.5 | 14.0 | 1.4 | 8.8 | 2.2 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 49.6 | 10.8 | 5.6 | 35.2 | 11.3 |
| *Highschool diploma* | 116.5 | 25.3 | 4.0 | 25.2 | 3.4 |
| *At least some post-secondary* | 294.1 | 63.9 | 6.2 | 39.0 | 2.1 |
| Some post-secondary | 33.9 | 7.4 | 1.8 | 11.3 | 5.3 |
| Post-secondary diploma or certificate | 150.4 | 32.7 | 2.2 | 13.8 | 1.5 |
| University degree | 109.7 | 23.8 | 2.2 | 13.8 | 2.0 |
| **Job status** | | | | | |
| *Full-time* | 382.6 | 83.1 | 6.2 | 39.0 | 1.6 |
| *Part-time* | 77.7 | 16.9 | 9.7 | 61.0 | 12.5 |
| **Household status** | | | | | |
| *Member of a couple* | 266.8 | 58.0 | 4.5 | 28.3 | 1.7 |
| Spouse not employed | 45.1 | 9.8 | 0.9 | 5.7 | 2.0 |
| *Youngest child is less than 18 years* | 17.5 | 3.8 | F | F | F |
| *No children or youngest child 18 or older* | 27.6 | 6.0 | 0.7 | 4.4 | 2.5 |
| Spouse employed | 221.0 | 48.0 | 3.5 | 22.0 | 1.6 |
| *Spouse making minimum wage or less* | 2.5 | 0.5 | F | F | F |
| *Spouse making greater than minimum wage* | 182.8 | 39.7 | 2.4 | 15.1 | 1.3 |
| *Spouse self-employed* | 35.7 | 7.8 | 1.0 | 6.3 | 2.8 |
| *Head of household, no spouse* | 32.0 | 7.0 | 0.8 | 5.0 | 2.5 |
| Youngest child is less than 18 years | 16.4 | 3.6 | F | F | F |
| No children or youngest child 18 or older | 15.6 | 3.4 | 0.5 | 3.1 | 3.2 |
| *Son, daughter, or other relative living with family* | 73.3 | 15.9 | 8.3 | 52.2 | 11.3 |
| 15–19 in school | 15.9 | 3.5 | 3.9 | 24.5 | 24.5 |
| 15–19 not in school | 11.2 | 2.4 | 1.7 | 10.7 | 15.2 |
| 20–24 in school | 4.8 | 1.0 | 0.9 | 5.7 | 18.8 |
| 20–24 not in school | 17.9 | 3.9 | 1.0 | 6.3 | 5.6 |
| 25 or over in school | 0.9 | 0.2 | F | F | F |
| 25 or over not in school | 22.6 | 4.9 | 0.7 | 4.4 | 3.1 |
| *Single* | 87.8 | 19.1 | 2.4 | 15.1 | 2.7 |
| Living alone | 53.4 | 11.6 | 1.1 | 6.9 | 2.1 |
| *15–24* | 5.4 | 1.2 | F | F | F |
| *25–54* | 34.0 | 7.4 | 0.5 | 3.1 | 1.5 |
| *55 and over* | 14.0 | 3.0 | F | F | F |
| Living with non-relatives | 34.4 | 7.5 | 1.3 | 8.2 | 3.8 |
| *15–24* | 12.8 | 2.8 | 0.6 | 3.8 | 4.7 |
| *25–54* | 20.4 | 4.4 | 0.5 | 3.1 | 2.5 |
| *55 and over* | 1.2 | 0.3 | F | F | F |
| *Member of a low-income household (for 2012)* | 14.0 | 3.2 | F | 5.3 | F |

## Table A5: Demographics of Minimum Wage Earners in Manitoba, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 543.4 | 100.0 | 27.5 | 100.0 | 5.1 |
| **Age** | | | | | |
| *15 to 19* | 33.2 | 6.1 | 9.6 | 34.9 | 28.9 |
| *20 to 24* | 61.7 | 11.4 | 5.5 | 20.0 | 8.9 |
| *25 to 34* | 121.1 | 22.3 | 3.7 | 13.5 | 3.1 |
| *35 to 44* | 113.3 | 20.9 | 2.6 | 9.5 | 2.3 |
| *45 to 54* | 120.9 | 22.2 | 3.2 | 11.6 | 2.6 |
| *55 to 64* | 79.1 | 14.6 | 2.1 | 7.6 | 2.7 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 63.0 | 11.6 | 7.4 | 26.9 | 11.7 |
| *Highschool diploma* | 132.5 | 24.4 | 8.5 | 30.9 | 6.4 |
| *At least some post-secondary* | 347.9 | 64.0 | 11.6 | 42.2 | 3.3 |
| Some post-secondary | 52.6 | 9.7 | 4.0 | 14.5 | 7.6 |
| Post-secondary diploma or certificate | 170.3 | 31.3 | 4.3 | 15.6 | 2.5 |
| University degree | 125.0 | 23.0 | 3.2 | 11.6 | 2.6 |
| **Job status** | | | | | |
| *Full-time* | 441.3 | 81.2 | 11.9 | 43.3 | 2.7 |
| *Part-time* | 102.2 | 18.8 | 15.6 | 56.7 | 15.3 |
| **Household status** | | | | | |
| *Member of a couple* | 313.2 | 57.6 | 7.6 | 27.6 | 2.4 |
| Spouse not employed | 53.6 | 9.9 | 1.6 | 5.8 | 3.0 |
| *Youngest child is less than 18 years* | 21.5 | 4.0 | 0.7 | 2.5 | 3.3 |
| *No children or youngest child 18 or older* | 32.1 | 5.9 | 1.0 | 3.6 | 3.1 |
| Spouse employed | 258.3 | 47.5 | 5.9 | 21.5 | 2.3 |
| *Spouse making minimum wage or less* | 5.3 | 1.0 | 0.7 | 2.5 | 13.2 |
| *Spouse making greater than minimum wage* | 224.2 | 41.3 | 4.6 | 16.7 | 2.1 |
| *Spouse self-employed* | 28.9 | 5.3 | 0.6 | 2.2 | 2.1 |
| *Head of household, no spouse* | 36.4 | 6.7 | 1.8 | 6.5 | 4.9 |
| Youngest child is less than 18 years | 16.7 | 3.1 | 0.6 | 2.2 | 3.6 |
| No children or youngest child 18 or older | 19.7 | 3.6 | 1.2 | 4.4 | 6.1 |
| *Son, daughter, or other relative living with family* | 107.4 | 19.8 | 15.0 | 54.5 | 14.0 |
| 15–19 in school | 16.0 | 2.9 | 5.8 | 21.1 | 36.3 |
| 15–19 not in school | 15.1 | 2.8 | 3.3 | 12.0 | 21.9 |
| 20–24 in school | 9.5 | 1.7 | 1.4 | 5.1 | 14.7 |
| 20–24 not in school | 27.2 | 5.0 | 2.3 | 8.4 | 8.5 |
| 25 or over in school | 2.4 | 0.4 | F | F | F |
| 25 or over not in school | 37.2 | 6.8 | 2.0 | 7.3 | 5.4 |
| *Single* | 85.8 | 15.8 | 3.1 | 11.3 | 3.6 |
| Living alone | 59.6 | 11.0 | 1.7 | 6.2 | 2.9 |
| *15–24* | 4.4 | 0.8 | F | F | F |
| *25–54* | 39.2 | 7.2 | 0.9 | 3.3 | 2.3 |
| *55 and over* | 16.0 | 2.9 | 0.5 | 1.8 | 3.1 |
| Living with non-relatives | 26.2 | 4.8 | 1.4 | 5.1 | 5.3 |
| *15–24* | 8.6 | 1.6 | 0.7 | 2.5 | 8.1 |
| *25–54* | 16.4 | 3.0 | 0.6 | 2.2 | 3.7 |
| *55 and over* | 1.2 | 0.2 | F | F | F |
| *Member of a low-income household (for 2012)* | 28.0 | 5.3 | F | 14.1 | F |

## Table A6: Demographics of Minimum Wage Earners in Ontario, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 5,822.4 | 100.0 | 632.9 | 100.0 | 10.9 |
| **Age** | | | | | |
| *15 to 19* | 305.5 | 5.2 | 239.1 | 37.8 | 78.3 |
| *20 to 24* | 608.7 | 10.5 | 142.3 | 22.5 | 23.4 |
| *25 to 34* | 1,292.9 | 22.2 | 77.9 | 12.3 | 6.0 |
| *35 to 44* | 1,259.3 | 21.6 | 58.2 | 9.2 | 4.6 |
| *45 to 54* | 1,358.3 | 23.3 | 57.1 | 9.0 | 4.2 |
| *55 to 64* | 832.6 | 14.3 | 43.9 | 6.9 | 5.3 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 490.8 | 8.4 | 168.0 | 26.5 | 34.2 |
| *Highschool diploma* | 1,237.3 | 21.3 | 190.8 | 30.1 | 15.4 |
| *At least some post-secondary* | 4,094.4 | 70.3 | 274.1 | 43.3 | 6.7 |
| Some post-secondary | 409.8 | 7.0 | 97.1 | 15.3 | 23.7 |
| Post-secondary diploma or certificate | 1,939.3 | 33.3 | 102.3 | 16.2 | 5.3 |
| University degree | 1,745.3 | 30.0 | 74.7 | 11.8 | 4.3 |
| **Job status** | | | | | |
| *Full-time* | 4,745.7 | 81.5 | 260.8 | 41.2 | 5.5 |
| *Part-time* | 1,076.7 | 18.5 | 372.1 | 58.8 | 34.6 |
| **Household status** | | | | | |
| *Member of a couple* | 3,345.3 | 57.5 | 157.4 | 24.9 | 4.7 |
| Spouse not employed | 635.9 | 10.9 | 36.4 | 5.8 | 5.7 |
| *Youngest child is less than 18 years* | 268.2 | 4.6 | 15.3 | 2.4 | 5.7 |
| *No children or youngest child 18 or older* | 367.8 | 6.3 | 21.1 | 3.3 | 5.7 |
| Spouse employed | 2,697.1 | 46.3 | 120.2 | 19.0 | 4.5 |
| *Spouse making minimum wage or less* | 100.6 | 1.7 | 11.5 | 1.8 | 11.4 |
| *Spouse making greater than minimum wage* | 2,226.5 | 38.2 | 89.1 | 14.1 | 4.0 |
| *Spouse self-employed* | 370.0 | 6.4 | 19.5 | 3.1 | 5.3 |
| *Head of household, no spouse* | 425.0 | 7.3 | 34.8 | 5.5 | 8.2 |
| Youngest child is less than 18 years | 183.2 | 3.1 | 13.2 | 2.1 | 7.2 |
| No children or youngest child 18 or older | 241.7 | 4.2 | 21.6 | 3.4 | 8.9 |
| *Son, daughter, or other relative living with family* | 1,241.8 | 21.3 | 378.3 | 59.8 | 30.5 |
| 15–19 in school | 176.2 | 3.0 | 149.7 | 23.7 | 85.0 |
| 15–19 not in school | 112.1 | 1.9 | 75.8 | 12.0 | 67.6 |
| 20–24 in school | 112.8 | 1.9 | 38.6 | 6.1 | 34.2 |
| 20–24 not in school | 301.0 | 5.2 | 65.4 | 10.3 | 21.7 |
| 25 or over in school | 27.4 | 0.5 | 2.4 | 0.4 | 8.8 |
| 25 or over not in school | 512.6 | 8.8 | 46.4 | 7.3 | 9.1 |
| *Single* | 801.2 | 13.8 | 61.6 | 9.7 | 7.7 |
| Living alone | 546.1 | 9.4 | 30.5 | 4.8 | 5.6 |
| *15–24* | 33.3 | 0.6 | 6.3 | 1.0 | 18.9 |
| *25–54* | 369.6 | 6.3 | 15.1 | 2.4 | 4.1 |
| *55 and over* | 143.2 | 2.5 | 9.0 | 1.4 | 6.3 |
| Living with non-relatives | 255.0 | 4.4 | 31.2 | 4.9 | 12.2 |
| *15–24* | 64.8 | 1.1 | 17.0 | 2.7 | 26.2 |
| *25–54* | 172.5 | 3.0 | 13.4 | 2.1 | 7.8 |
| *55 and over* | 17.8 | 0.3 | F | F | F |
| *Member of a low-income household (for 2012)* | 289.0 | 5.3 | 82.0 | 15.4 | 28.4 |

## Table A7: Demographics of Minimum Wage Earners in Quebec, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 3,501.9 | 100.0 | 210.2 | 100.0 | 6.0 |
| **Age** | | | | | |
| *15 to 19* | 191.8 | 5.5 | 75.9 | 36.1 | 39.6 |
| *20 to 24* | 367.0 | 10.5 | 48.5 | 23.1 | 13.2 |
| *25 to 34* | 773.9 | 22.1 | 23.5 | 11.2 | 3.0 |
| *35 to 44* | 763.3 | 21.8 | 17.7 | 8.4 | 2.3 |
| *45 to 54* | 821.5 | 23.5 | 21.5 | 10.2 | 2.6 |
| *55 to 64* | 505.4 | 14.4 | 16.8 | 8.0 | 3.3 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 366.8 | 10.5 | 48.6 | 23.1 | 13.2 |
| *Highschool diploma* | 525.0 | 15.0 | 42.7 | 20.3 | 8.1 |
| *At least some post-secondary* | 2,610.0 | 74.5 | 118.8 | 56.5 | 4.6 |
| Some post-secondary | 221.9 | 6.3 | 43.4 | 20.6 | 19.6 |
| Post-secondary diploma or certificate | 1,520.2 | 43.4 | 56.1 | 26.7 | 3.7 |
| University degree | 867.9 | 24.8 | 19.3 | 9.2 | 2.2 |
| **Job status** | | | | | |
| *Full-time* | 2,818.2 | 80.5 | 84.6 | 40.2 | 3.0 |
| *Part-time* | 683.7 | 19.5 | 125.6 | 59.8 | 18.4 |
| **Household status** | | | | | |
| *Member of a couple* | 2,064.0 | 58.9 | 58.4 | 27.8 | 2.8 |
| Spouse not employed | 367.6 | 10.5 | 14.9 | 7.1 | 4.1 |
| *Youngest child is less than 18 years* | 128.4 | 3.7 | 3.3 | 1.6 | 2.6 |
| *No children or youngest child 18 or older* | 239.2 | 6.8 | 11.6 | 5.5 | 4.8 |
| Spouse employed | 1,690.8 | 48.3 | 43.4 | 20.6 | 2.6 |
| *Spouse making minimum wage or less* | 36.4 | 1.0 | 3.7 | 1.8 | 10.2 |
| *Spouse making greater than minimum wage* | 1,426.8 | 40.7 | 32.7 | 15.6 | 2.3 |
| *Spouse self-employed* | 227.6 | 6.5 | 7.0 | 3.3 | 3.1 |
| *Head of household, no spouse* | 249.0 | 7.1 | 11.7 | 5.6 | 4.7 |
| Youngest child is less than 18 years | 132.2 | 3.8 | 5.1 | 2.4 | 3.9 |
| No children or youngest child 18 or older | 116.8 | 3.3 | 6.6 | 3.1 | 5.7 |
| *Son, daughter, or other relative living with family* | 566.7 | 16.2 | 116.0 | 55.2 | 20.5 |
| 15–19 in school | 112.3 | 3.2 | 47.8 | 22.7 | 42.6 |
| 15–19 not in school | 67.6 | 1.9 | 24.2 | 11.5 | 35.8 |
| 20–24 in school | 78.6 | 2.2 | 16.4 | 7.8 | 20.9 |
| 20–24 not in school | 129.9 | 3.7 | 16.3 | 7.8 | 12.5 |
| 25 or over in school | 14.7 | 0.4 | F | F | F |
| 25 or over not in school | 163.6 | 4.7 | 10.3 | 4.9 | 6.3 |
| *Single* | 620.9 | 17.7 | 24.2 | 11.5 | 3.9 |
| Living alone | 486.4 | 13.9 | 15.4 | 7.3 | 3.2 |
| *15–24* | 40.6 | 1.2 | 3.0 | 1.4 | 7.4 |
| *25–54* | 335.4 | 9.6 | 7.4 | 3.5 | 2.2 |
| *55 and over* | 110.4 | 3.2 | 5.0 | 2.4 | 4.5 |
| Living with non-relatives | 134.5 | 3.8 | 8.8 | 4.2 | 6.5 |
| *15–24* | 42.2 | 1.2 | 4.1 | 2.0 | 9.7 |
| *25–54* | 79.8 | 2.3 | 3.8 | 1.8 | 4.8 |
| *55 and over* | 12.6 | 0.4 | F | F | F |
| *Member of a low-income household (for 2012)* | 173.0 | 5.1 | F | 6.2 | F |

## Table A8: Demographics of Minimum Wage Earners in New Brunswick, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 310.5 | 100.0 | 20.9 | 100.0 | 6.7 |
| **Age** | | | | | |
| *15 to 19* | 15.8 | 5.1 | 7.2 | 34.4 | 45.6 |
| *20 to 24* | 28.8 | 9.3 | 3.9 | 18.7 | 13.5 |
| *25 to 34* | 60.4 | 19.5 | 2.3 | 11.0 | 3.8 |
| *35 to 44* | 67.5 | 21.7 | 1.9 | 9.1 | 2.8 |
| *45 to 54* | 77.3 | 24.9 | 2.6 | 12.4 | 3.4 |
| *55 to 64* | 52.2 | 16.8 | 2.1 | 10.0 | 4.0 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 32.2 | 10.4 | 6.6 | 31.6 | 20.5 |
| *Highschool diploma* | 74.2 | 23.9 | 5.9 | 28.2 | 8.0 |
| *At least some post-secondary* | 204.1 | 65.7 | 8.4 | 40.2 | 4.1 |
| Some post-secondary | 18.0 | 5.8 | 1.7 | 8.1 | 9.4 |
| Post-secondary diploma or certificate | 120.4 | 38.8 | 5.2 | 24.9 | 4.3 |
| University degree | 65.7 | 21.2 | 1.5 | 7.2 | 2.3 |
| **Job status** | | | | | |
| *Full-time* | 264.0 | 85.0 | 9.6 | 45.9 | 3.6 |
| *Part-time* | 46.5 | 15.0 | 11.3 | 54.1 | 24.3 |
| **Household status** | | | | | |
| *Member of a couple* | 197.8 | 63.7 | 7.1 | 34.0 | 3.6 |
| Spouse not employed | 40.8 | 13.1 | 1.9 | 9.1 | 4.7 |
| *Youngest child is less than 18 years* | 11.6 | 3.7 | 0.5 | 2.4 | 4.3 |
| *No children or youngest child 18 or older* | 29.2 | 9.4 | 1.5 | 7.2 | 5.1 |
| Spouse employed | 155.6 | 50.1 | 5.0 | 23.9 | 3.2 |
| *Spouse making minimum wage or less* | 4.3 | 1.4 | F | F | F |
| *Spouse making greater than minimum wage* | 133.2 | 42.9 | 4.0 | 19.1 | 3.0 |
| *Spouse self-employed* | 18.1 | 5.8 | 0.7 | 3.3 | 3.9 |
| *Head of household, no spouse* | 20.8 | 6.7 | 1.0 | 4.8 | 4.8 |
| Youngest child is less than 18 years | 11.6 | 3.7 | 0.5 | 2.4 | 4.3 |
| No children or youngest child 18 or older | 9.2 | 3.0 | 0.5 | 2.4 | 5.4 |
| *Son, daughter, or other relative living with family* | 47.2 | 15.2 | 10.1 | 48.3 | 21.4 |
| 15–19 in school | 8.0 | 2.6 | 4.2 | 20.1 | 52.5 |
| 15–19 not in school | 6.3 | 2.0 | 2.4 | 11.5 | 38.1 |
| 20–24 in school | 2.2 | 0.7 | 0.5 | 2.4 | 22.7 |
| 20–24 not in school | 12.5 | 4.0 | 1.9 | 9.1 | 15.2 |
| 25 or over in school | 0.5 | 0.2 | F | F | F |
| 25 or over not in school | 17.8 | 5.7 | 1.1 | 5.3 | 6.2 |
| *Single* | 44.5 | 14.3 | 2.7 | 12.9 | 6.1 |
| Living alone | 33.3 | 10.7 | 1.6 | 7.7 | 4.8 |
| *15–24* | 2.8 | 0.9 | F | F | F |
| *25–54* | 21.5 | 6.9 | 0.5 | 2.4 | 2.3 |
| *55 and over* | 9.0 | 2.9 | 0.6 | 2.9 | 6.7 |
| Living with non-relatives | 11.2 | 3.6 | 1.1 | 5.3 | 9.8 |
| *15–24* | 3.4 | 1.1 | 0.6 | 2.9 | 17.6 |
| *25–54* | 6.9 | 2.2 | F | F | F |
| *55 and over* | 0.8 | 0.3 | F | F | F |
| *Member of a low-income household (for 2012)* | 9.0 | 2.8 | F | 10.6 | F |

## Table A9: Demographics of Minimum Wage Earners in Nova Scotia, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 386.8 | 100.0 | 22.2 | 100.0 | 5.7 |
| **Age** | | | | | |
| *15 to 19* | 20.3 | 5.2 | 5.6 | 25.2 | 27.6 |
| *20 to 24* | 39.2 | 10.1 | 4.7 | 21.2 | 12.0 |
| *25 to 34* | 81.0 | 20.9 | 3.7 | 16.7 | 4.6 |
| *35 to 44* | 78.5 | 20.3 | 2.8 | 12.6 | 3.6 |
| *45 to 54* | 94.9 | 24.5 | 2.2 | 9.9 | 2.3 |
| *55 to 64* | 63.2 | 16.3 | 2.7 | 12.2 | 4.3 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 37.0 | 9.6 | 4.9 | 22.1 | 13.2 |
| *Highschool diploma* | 76.8 | 19.9 | 6.1 | 27.5 | 7.9 |
| *At least some post-secondary* | 273.1 | 70.6 | 11.2 | 50.5 | 4.1 |
| Some post-secondary | 27.6 | 7.1 | 2.7 | 12.2 | 9.8 |
| Post-secondary diploma or certificate | 143.9 | 37.2 | 5.2 | 23.4 | 3.6 |
| University degree | 101.5 | 26.2 | 3.2 | 14.4 | 3.2 |
| **Job status** | | | | | |
| *Full-time* | 318.7 | 82.4 | 11.1 | 50.0 | 3.5 |
| *Part-time* | 68.1 | 17.6 | 11.0 | 49.5 | 16.2 |
| **Household status** | | | | | |
| *Member of a couple* | 231.3 | 59.8 | 7.4 | 33.3 | 3.2 |
| Spouse not employed | 48.2 | 12.5 | 2.1 | 9.5 | 4.4 |
| *Youngest child is less than 18 years* | 14.7 | 3.8 | F | F | F |
| *No children or youngest child 18 or older* | 33.5 | 8.7 | 1.7 | 7.7 | 5.1 |
| Spouse employed | 178.6 | 46.2 | 5.2 | 23.4 | 2.9 |
| *Spouse making minimum wage or less* | 4.5 | 1.2 | 0.6 | 2.7 | 13.3 |
| *Spouse making greater than minimum wage* | 152.0 | 39.3 | 3.9 | 17.6 | 2.6 |
| *Spouse self-employed* | 22.1 | 5.7 | 0.7 | 3.2 | 3.2 |
| *Head of household, no spouse* | 26.6 | 6.9 | 1.7 | 7.7 | 6.4 |
| Youngest child is less than 18 years | 14.7 | 3.8 | 1.0 | 4.5 | 6.8 |
| No children or youngest child 18 or older | 11.9 | 3.1 | 0.8 | 3.6 | 6.7 |
| *Son, daughter, or other relative living with family* | 58.2 | 15.0 | 9.5 | 42.8 | 16.3 |
| 15–19 in school | 10.8 | 2.8 | 3.2 | 14.4 | 29.6 |
| 15–19 not in school | 7.7 | 2.0 | 2.0 | 9.0 | 26.0 |
| 20–24 in school | 4.0 | 1.0 | 0.7 | 3.2 | 17.5 |
| 20–24 not in school | 14.7 | 3.8 | 2.1 | 9.5 | 14.3 |
| 25 or over in school | 1.0 | 0.3 | F | F | F |
| 25 or over not in school | 20.1 | 5.2 | 1.3 | 5.9 | 6.5 |
| *Single* | 70.4 | 18.2 | 3.5 | 15.8 | 5.0 |
| Living alone | 41.9 | 10.8 | 1.8 | 8.1 | 4.3 |
| *15–24* | 3.2 | 0.8 | F | F | F |
| *25–54* | 27.0 | 7.0 | 1.0 | 4.5 | 3.7 |
| *55 and over* | 11.8 | 3.1 | F | F | F |
| Living with non-relatives | 28.5 | 7.4 | 1.7 | 7.7 | 6.0 |
| *15–24* | 9.7 | 2.5 | 0.9 | 4.1 | 9.3 |
| *25–54* | 17.1 | 4.4 | 0.7 | 3.2 | 4.1 |
| *55 and over* | 1.7 | 0.4 | F | F | F |
| *Member of a low-income household (for 2012)* | 14.0 | 3.8 | F | 9.9 | F |

## Table A10: Demographics of Minimum Wage Earners in Prince Edward Island, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 62.8 | 100.0 | 3.8 | 100.0 | 6.1 |
| **Age** | | | | | |
| *15 to 19* | 4.2 | 6.7 | 1.3 | 34.2 | 31.0 |
| *20 to 24* | 6.4 | 10.2 | 0.9 | 23.7 | 14.1 |
| *25 to 34* | 11.8 | 18.8 | 0.3 | 7.9 | 2.5 |
| *35 to 44* | 12.7 | 20.2 | 0.2 | 5.3 | 1.6 |
| *45 to 54* | 15.2 | 24.2 | 0.4 | 10.5 | 2.6 |
| *55 to 64* | 10.4 | 16.6 | 0.4 | 10.5 | 3.8 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 7.7 | 12.3 | 1.1 | 28.9 | 14.3 |
| *Highschool diploma* | 13.2 | 21.0 | 1.2 | 31.6 | 9.1 |
| *At least some post-secondary* | 41.9 | 66.7 | 1.5 | 39.5 | 3.6 |
| Some post-secondary | 4.8 | 7.6 | 0.5 | 13.2 | 10.4 |
| Post-secondary diploma or certificate | 22.6 | 36.0 | 0.7 | 18.4 | 3.1 |
| University degree | 14.5 | 23.1 | 0.4 | 10.5 | 2.8 |
| **Job status** | | | | | |
| *Full-time* | 52.5 | 83.6 | 1.8 | 47.4 | 3.4 |
| *Part-time* | 10.3 | 16.4 | 2.0 | 52.6 | 19.4 |
| **Household status** | | | | | |
| *Member of a couple* | 40.0 | 63.7 | 1.1 | 28.9 | 2.8 |
| Spouse not employed | 7.4 | 11.8 | 0.3 | 7.9 | 4.1 |
| *Youngest child is less than 18 years* | 2.3 | 3.7 | 0.2 | 5.3 | 8.7 |
| *No children or youngest child 18 or older* | 5.1 | 8.1 | 0.2 | 5.3 | 3.9 |
| Spouse employed | 32.5 | 51.8 | 0.8 | 21.1 | 2.5 |
| *Spouse making minimum wage or less* | 0.7 | 1.1 | F | F | F |
| *Spouse making greater than minimum wage* | 27.4 | 43.6 | 0.6 | 15.8 | 2.2 |
| *Spouse self-employed* | 4.4 | 7.0 | F | F | F |
| *Head of household, no spouse* | 4.0 | 6.4 | 0.2 | 5.3 | 5.0 |
| Youngest child is less than 18 years | 2.5 | 4.0 | F | F | F |
| No children or youngest child 18 or older | 1.5 | 2.4 | F | F | F |
| *Son, daughter, or other relative living with family* | 10.2 | 16.2 | 1.8 | 47.4 | 17.6 |
| 15–19 in school | 2.0 | 3.2 | 0.8 | 21.1 | 40.0 |
| 15–19 not in school | 1.8 | 2.9 | 0.4 | 10.5 | 22.2 |
| 20–24 in school | 0.8 | 1.3 | 0.2 | 5.3 | 25.0 |
| 20–24 not in school | 2.4 | 3.8 | 0.3 | 7.9 | 12.5 |
| 25 or over in school | 0.2 | 0.3 | F | F | F |
| 25 or over not in school | 3.0 | 4.8 | F | F | F |
| *Single* | 8.6 | 13.7 | 0.6 | 15.8 | 7.0 |
| Living alone | 5.9 | 9.4 | 0.3 | 7.9 | 5.1 |
| *15–24* | 0.5 | 0.8 | F | F | F |
| *25–54* | 3.6 | 5.7 | F | F | F |
| *55 and over* | 1.8 | 2.9 | F | F | F |
| Living with non-relatives | 2.7 | 4.3 | 0.3 | 7.9 | 11.1 |
| *15–24* | 1.1 | 1.8 | 0.2 | 5.3 | 18.2 |
| *25–54* | 1.4 | 2.2 | F | F | F |
| *55 and over* | 0.2 | 0.3 | F | F | F |
| *Member of a low-income household (for 2012)* | F | 1.6 | F | F | F |

## Table A11: Demographics of Minimum Wage Earners in Newfoundland & Labrador, 2014

| | Total employees | | Minimum wage | | |
|---|---|---|---|---|---|
| | Total (000s) | Share (%) | Total (000s) | Share (%) | Incidence (%) |
| **Total** | 215.6 | 100.0 | 11.6 | 100.0 | 5.4 |
| **Age** | | | | | |
| *15 to 19* | 9.5 | 4.4 | 3.8 | 32.8 | 40.0 |
| *20 to 24* | 19.1 | 8.9 | 2.3 | 19.8 | 12.0 |
| *25 to 34* | 45.0 | 20.9 | 1.5 | 12.9 | 3.3 |
| *35 to 44* | 47.4 | 22.0 | 1.3 | 11.2 | 2.7 |
| *45 to 54* | 55.6 | 25.8 | 1.5 | 12.9 | 2.7 |
| *55 to 64* | 33.5 | 15.5 | 1.1 | 9.5 | 3.3 |
| **Education attainment** | | | | | |
| *Less than a highschool diploma* | 22.3 | 10.3 | 3.2 | 27.6 | 14.3 |
| *Highschool diploma* | 35.8 | 16.6 | 3.4 | 29.3 | 9.5 |
| *At least some post-secondary* | 157.5 | 73.1 | 5.0 | 43.1 | 3.2 |
| Some post-secondary | 12.5 | 5.8 | 1.4 | 12.1 | 11.2 |
| Post-secondary diploma or certificate | 97.3 | 45.1 | 2.8 | 24.1 | 2.9 |
| University degree | 47.7 | 22.1 | 0.8 | 6.9 | 1.7 |
| **Job status** | | | | | |
| *Full-time* | 184.2 | 85.4 | 6.3 | 54.3 | 3.4 |
| *Part-time* | 31.4 | 14.6 | 5.4 | 46.6 | 17.2 |
| **Household status** | | | | | |
| *Member of a couple* | 142.4 | 66.0 | 4.1 | 35.3 | 2.9 |
| Spouse not employed | 35.2 | 16.3 | 1.4 | 12.1 | 4.0 |
| *Youngest child is less than 18 years* | 12.0 | 5.6 | F | F | F |
| *No children or youngest child 18 or older* | 23.2 | 10.8 | 1.0 | 8.6 | 4.3 |
| Spouse employed | 106.9 | 49.6 | 2.7 | 23.3 | 2.5 |
| *Spouse making minimum wage or less* | 2.5 | 1.2 | F | F | F |
| *Spouse making greater than minimum wage* | 95.7 | 44.4 | 2.4 | 20.7 | 2.5 |
| *Spouse self-employed* | 8.7 | 4.0 | F | F | F |
| *Head of household, no spouse* | 12.8 | 5.9 | 0.8 | 6.9 | 6.3 |
| Youngest child is less than 18 years | 7.0 | 3.2 | F | F | F |
| No children or youngest child 18 or older | 5.8 | 2.7 | 0.5 | 4.3 | 8.6 |
| *Son, daughter, or other relative living with family* | 32.5 | 15.1 | 5.5 | 47.4 | 16.9 |
| 15–19 in school | 4.9 | 2.3 | 2.0 | 17.2 | 40.8 |
| 15–19 not in school | 4.0 | 1.9 | 1.5 | 12.9 | 37.5 |
| 20–24 in school | 2.3 | 1.1 | F | F | F |
| 20–24 not in school | 7.5 | 3.5 | 0.9 | 7.8 | 12.0 |
| 25 or over in school | 0.6 | 0.3 | F | F | F |
| 25 or over not in school | 13.2 | 6.1 | 0.7 | 6.0 | 5.3 |
| *Single* | 27.8 | 12.9 | 1.2 | 10.3 | 4.3 |
| Living alone | 17.0 | 7.9 | 0.6 | 5.2 | 3.5 |
| *15–24* | 1.4 | 0.6 | F | F | F |
| *25–54* | 10.9 | 5.1 | F | F | F |
| *55 and over* | 4.6 | 2.1 | F | F | F |
| Living with non-relatives | 10.9 | 5.1 | 0.6 | 5.2 | 5.5 |
| *15–24* | 3.1 | 1.4 | F | F | F |
| *25–54* | 6.7 | 3.1 | F | F | F |
| *55 and over* | 1.1 | 0.5 | F | F | F |
| *Member of a low-income household (for 2012)* | F | 1.6 | F | F | F |

# Appendix B: The Experts Argue over Disemployment Effects in the United States

As the review in the main text indicates, there are two schools of thought or camps in the scholarly literature on the minimum wage, with one group of economists finding significant disemployment effects, while the rival group finds modest or even positive impacts. A full assessment of the dispute between leaders in the two camps lies outside the scope of the present study. However, to convey to the reader the flavor of the arguments—and to show how it's possible that professional economists can still disagree so strongly on what decades of detailed evidence tell us about the minimum wage—in this appendix we review the back-and-forth in journal articles on two key issues.

Before summarizing the particular lines of argument, let us repeat the overall theme of the empirical debate centered on the U.S. experience: The traditional time series analyses, including both federal and state-specific variations in minimum wages, agreed with the introductory textbook logic, and tended to find statistically significant disemployment effects, especially if we focused on (generally) unskilled groups such as teenagers.

However, in the 1990s a new wave of minimum-wage research emerged, which found little evidence of significant disemployment effects, at least from modest minimum wage hikes. The new conclusions did *not* come from the use of newly available data; the original "orthodox" results still apply up to the most recent time series studies. Rather, the "new" results come from a methodological difference: through various methods, they select a "control group" to serve as a baseline against which to compare employment in the "treatment group"—namely where the minimum wage is raised. [1]

Superficially, one could hardly object to the superiority of the latter method. After all, in many scientific applications, it is only natural to carefully select a control group, in order to assess the effect of a "treatment", be it an experimental drug, a new exercise regimen, or (in economic policy) a

[1] It is tempting to say that the traditional findings rely on time series analyses while the newer wave relies on panel data, but the actual situation is more nuanced. It is true that the orthodox consensus described in Brown, Gilroy, and Kohen (1982) refers to time series (and to a lesser extent, cross-sectional) studies. However, Neumark and Wascher (1992) rectified that shortcoming by analyzing state-level panel data from 1973 to 1989, finding that a 10% increase in the minimum wage reduces employment among teenagers by 1% to 2%. Therefore, those economists who today argue that the minimum wage is benign are suggesting that even standard panel data approaches miss important omitted variables, and that researchers must construct more adequate "control groups".

new method of distributing educational vouchers. *A priori*, then, we would expect studies with explicit "matching" methods in their designs to be more compelling than those studies relying on the traditional approach (Kuehn, 2014). This is why economists such as Paul Krugman now summarize the history of the debate in this fashion:

> Until the Card-Krueger study, most economists, myself included, assumed that raising the minimum wage would have a clear negative effect on employment. But they found, if anything, a positive effect. Their result has since been confirmed using data from many episodes. There's just no evidence that raising the minimum wage costs jobs, at least when the starting point is as low as it is in modern America. (Krugman, 2015)

As our literature review in the main text has made clear, there are actually *mountains* of evidence that "raising the minimum wage costs jobs". The reason Krugman declares, "There's just no evidence", is that he believes the new methodological innovation—of matching treatment with control groups, rather than running regressions on huge samples of panel data—has demonstrated decisively that the old approach was producing spurious results.

There are two main avenues by which to challenge such an argument. First, we can point out that some "matching" studies do indeed show disemployment effects. For example, in our review above we mentioned Hoffman and Trace (2009), Singell and Terborg (2007), and Clemens and Wither (2014) as showing disemployment effects, even though these three studies all fall under the "matching" or "control group" or "natural experiment" design method.

In addition, another main avenue of criticism against the new wave of findings is to question whether these "control groups" are actually better counterfactuals than the bulk of the sample data in the traditional studies. It is here, on this battleground, that we will relay the back-and-forth on two key disputes between leaders of the respective camps.

### Expert Dispute 1: The use of a "synthetic control group" method to allegedly validate traditional studies

In Dube, Lester, and Reich (2010)—the paper to which Krugman provided a hyperlink in his column for the *New York Times* when writing that the original Card-Krueger results had "since been confirmed using data from many episodes" (2015)—the key claim is that their method of contiguous-county-pair matching provided a better control group than the traditional approaches. In their words:

> Our results indicate that the negative employment effects in national-level studies reflect spatial heterogeneity and improper construction of control groups. We find that in the traditional fixed-effects

> specification, employment levels and trends are negative prior to the minimum wage increase. In contrast, the levels and trends are close to 0 for our local specification, which provides evidence that contiguous counties are valid controls. (Dube, Lester, and Reich, 2010: 945–946)

In other words, the argument is that geographically closer regions provide better control groups to test the effect of minimum wage hikes. (Recall that the original Card-Krueger study looked at adjacent counties that straddled the Pennsylvania-New Jersey border. [2]) However, in Neumark, Salas, and Wascher (2014a), the authors challenge Dube, Lester, and Reich by arguing that this belief is itself a mere assumption that can be *tested*. Relying on techniques from the "synthetic control group" literature, Neumark, Salas, and Wascher (2014a) purport to show that, when constructing a control group for a US state, the optimal weights placed on states within the same Census division are often not higher than the weights placed on randomly selected states. In other words, if one particular US state raises its minimum wage then, when researchers construct a baseline counterfactual of employment, they might as well include the experience of all *other* US states, rather than paying more attention to what happened to the states in the immediate vicinity. If this particular argument from Neumark, Salas, and Wascher (2014a) is sound, then it destroys the entire intellectual foundation of the new minimum-wage research, and helps to restore the prestige of the orthodox body of empirical work.

Naturally, the leaders in the new wave of minimum-wage researche did not take such an assault lying down. For example, in Allegretto, Dube, Reich, and Zipperer, 2013, the authors claim that, using the very "synthetic control group" approach favoured by Neumark, Salas, and Wascher, US states within the same Census division receive weights (when contributing to an optimal control group) that are "2.8 to 4.1 times as large" (2013: 66) as weights placed on US states outside the Census division. [3] Allegretto, Dube, Reich, and Zipperer take this result as obvious evidence that geographically closer states tend to serve as better control groups for gauging the impact of a minimum

[2] From the pioneering Card-Krueger (1994) article: "We believe that a control group of fast-food stores in eastern Pennsylvania forms a natural basis for comparison with the experiences of restaurants in New Jersey. Wage variation across stores in New Jersey, however, allows us to compare the experiences of high-wage and low-wage stores within New Jersey and to test the validity of the Pennsylvania control group" (2014: 773).

[3] In case the reader wonders how Allegretto, Dube, Reich, and Zipperer, 2013 could be *responding* to Neumark, Salas, and Wascher, 2014a, it appears that Allegretto et al. were using both a working paper and a forthcoming (accepted journal article) version of Neumark, Salas, and Wascher's arguments, which were actually published formally in the following calendar year.

wage hike in a particular state, thus validating the new approach and casting doubt upon the traditional time-series approach (in which all regions contribute equally to the regression coefficients).

As decisive as this counterattack from Allegretto et al. (2013) seems, it is not the final word. For in Neumark, Salas, and Wascher (2014b), the authors reply that Allegretto et al. used a fallacious method when computing the proper weights with the synthetic control group approach. Specifically, they claim that "ADRZ's [Allegretto, Dube, Reich, and Zipperer, 2013] conclusion is based on a flawed calculation that weights states in a manner that mechanically tends to produce a high ratio of the weight they compute on same division versus non-same-division states" (2014b: 3). To demonstrate this, Neumark, Salas, and Wascher go through a simulated example where it seems apparent that the weighting method used by Allegretto, Dube, Reich, and Zipperer will artificially generate higher weights for nearby states than for distant ones.

At this stage in the back-and-forth, Neumark et al. stand by the claim they made in their (2014a) paper: generally speaking, the synthetic control approach shows *empirically* that US states within the same Census division should not necessarily be considered a better control group than US states from other Census divisions. If this is correct, then it undercuts the whole thesis of the new minimum wage literature, which is that the traditional results were systematically biased because of "spatial heterogeneities in employment trends".

### Expert Dispute 2: The use of a "placebo effect" method to allegedly invalidate traditional studies

Another fascinating line of attack in the empirical debate concerns the "placebo effect" of neighbouring minimum wage hikes. Recall the overall theme: the leaders in the new minimum wage research claim that states that raise their minimum wages above the federal level are *not* randomly dispersed across the United States and in fact happen to be located such that they overlap with regional employment trends in a way that biases the traditional regression coefficients downward. Their hypothesis is that the geographical distribution of state policies makes it *appear* that a state raising its minimum wage causes it to experience slower employment growth in the relevant category (such as the fast food industry or among teenagers), but actually this is mere correlation and not causation. In order to illustrate their hypothesis, these researchers design a scenario in which economists can agree there must be *no* (strong) causal relationship, and yet the traditional time series regression approach will report a strong negative disemployment effect. This is the analog of a "placebo" (such as a sugar pill) having similar effects in a clinical medical trial, which shows that the actual drug treatment must not be efficacious after all.

Specifically, Dube, Lester, and Reich (2010) construct a "placebo group" of US counties that are located on state borders and that were *also* located

in states that did *not* impose minimum wages higher than the federal level. Then, they matched them to contiguous counties that were in adjacent states. (Note that these *matched* counties—outside of the placebo group—could be located in states that raised their minimum wage higher than the federal level.) Then Dube, Lester, and Reich ran a regression comparable to the traditional approach (including controls for national time effects and county-specific fixed effects), with employment as the dependent variable, on the sample of counties in their placebo group. However, Dube, Lester, and Reich added the twist that instead of using the minimum wage applicable in the placebo group (which by construction was always the federal level), they plugged in as one of the independent variables the minimum wage prevailing in the *matched county* group (which could be higher than the federal level, if the matched county were in such a state and depending on the time period).

With this approach, Dube, Lester, and Reich found a large negative weight on the minimum-wage variable. [4] They interpreted this result to reinforce their hypothesis that the traditional panel data approach was biasing the coefficients downward on a given state's minimum wage. For example, the traditional approach might show that a minimum wage hike in New Jersey was associated with lower employment in New Jersey, and (of course) traditional economists would have assumed the association was causal. But if the same traditional approach shows that a minimum wage hike in New Jersey was associated with lower employment *in Pennsylvania* (which always matched the federal level), then surely there must be an "omitted variable" at work, because it makes little sense to assume New Jersey's policies could have such a strong effect on Pennsylvania employment. Perhaps there was some regional trend affecting the economies of New Jersey and Pennsylvania at those times when New Jersey legislators happened to raise the state's minimum wage.

This "placebo" approach is very clever, and would seem decisive to those readers who already were predisposed to support the conclusions of Dube and his colleagues. However, once again there is a legitimate response from the opposing camp. Specifically, Neumark, Salinas, and Wascher (2014a) argue that the placebo approach overlooks something quite important: *the federal minimum wage itself varied during the period in question*. Therefore, at least some of the time, when a matched county's minimum wage rose, it *was not* simply because of that state exceeding the federal level, but it could also be because the federal level itself was raised. Therefore, Neumark et al. point out, we would *expect* the regression to pick up this effect, and register a negative effect from the neighbouring county's minimum wage hike.

[4] Specifically, the coefficient in the placebo sample was –0.123, though it was not statistically significant, and it was much lower (in absolute value) than the minimum wage coefficient when the proper (non-placebo) regression was run on the matched counties, namely –0.208.

To return to our illustrative example: if we regress a Pennsylvania county's employment against the minimum wage in an adjacent county in New Jersey, then we should expect to find a negative relationship if the federal government occasionally raises the minimum wage in a way that is binding in both New Jersey and Pennsylvania. In this case, there is indeed an "omitted variable"—but it is the federal minimum wage that (by construction) is binding on the county in the placebo group, yet which was not included in the regression by Dube, Lester, and Reich (2010).

To drive home this point, Neumark, Salas, and Wascher (2014a) then perform the same placebo experiment, except this time they remove any of the minimum wage changes due to *federal* hikes, so that the only variation in minimum wages occur from unilateral policy changes by legislators in the states adjacent to our placebo counties. With this restricted placebo sample, Neumark, Salas, and Wascher find a much smaller (in absolute terms) and statistically insignificant coefficient on the adjacent-county minimum wage variable. [5] They conclude that the "placebo" group constructed by Dube, Lester, and Reich (2010) was actually a *treatment* group, because the counties had indeed experienced minimum wage hikes every time the federal government raised the level.

As should be expected at this point, Allegretto, Dube, Reich, and Zipperer (2013) reject this response. After summarizing the original placebo argument from the 2010 paper, and the critique from Neumark, Salas, and Wascher (which they abbreviate "NSW"), they write:

> NSW's critique suggests that they misunderstand this entire exercise. They claim that our placebo sample is "contaminated" because minimum wages are changing. They are changing, however, in exactly the same way in all counties in the placebo sample, since they all pay the federal minimum and are fully correlated with time effects. In other words, there is *zero* cross-sectional variation in minimum wages in the sample. NSW's argument about "contamination" misunderstands the basic sources of statistical variation used in a fixed effects model. In this sample, replacing the actual (common) minimum wage with a fictitious one (from the neighbor) should not produce a negative result. Yet it does, suggesting that the canonical specification is biased due to spatial heterogeneity.

[5] Specifically, they restrict the analysis from the third quarter of 1998 through the second quarter of 2006, when the federal minimum wage was unchanged. When using the actual minimum wage in a given county as the independent variable, they found employment effects of –0.247, significant at the 1% level. (In other words, a 10% increase in the minimum wage leads to a 2.47% reduction in employment.) In contrast, when using the minimum wage of the contiguous county (i.e., the placebo), they found employment effects of –0.107, which was not statistically significant.

> As a solution to a non-problem, NSW then get rid of 80 percent of the sample by cutting out many of the years, and then by imposing an arbitrary restriction on cross-border minimum wage variation. Once they get rid of the 80 percent of data using arbitrary criteria, they discover that the placebo estimate becomes close to zero. This "solution" does not shed any light on the validity of the placebo exercise, because there was no problem with the exercise in the first place. (Allegretto, Dube, Reich, and Zipperer, 2013: 76; italics in original.)

In other words, Allegretto and her colleagues are claiming that, because all of the counties in their original placebo group have a minimum wage always equal to the federal level (by construction), then in their regression the national time period fixed effects variable should have captured all the impact of this common factor. This leads to such striking language above as accusing Neumark, Salas, and Wascher of "misunderstand[ing] this entire exercise" and also misunderstanding "the basic sources of statistical variation used in a fixed effects model". Again, at this point in the debate, someone who originally had been a supporter of Allegretto et al. would probably think that these researchers clearly had the upper hand, for it seems that Neumark et al. do not know even know the basics of regression analysis.

Yet Neumark, Salas, and Wascher (2014b) have a response. They admit that, in the placebo group setup, the minimum wages *in the placebo group counties* would be "perfectly collinear" with the national time period fixed effects, and hence should not be an independent source of variation in placebo employment. However, Dube et al. did not run the regression against the *actual* minimum wages in the placebo counties, but instead ran it against the minimum wages prevailing *in their matched counties* across a state border. Sometimes when the federal minimum wage was raised, this would raise the relevant matched minimum wage, but sometimes it would not; specifically, if the matched county in question were in a state with a minimum wage higher than even the new federal level. (In this case, economists would say the federal increase was not "binding".) Thus the minimum wages used in Dube, Lester, and Reich's placebo regression are *not* perfectly collinear with the period fixed effects. As Neumark, Salas, and Wascher explain it: "[F]ederal minimum variation is not swept out by the period fixed effects, and therefore the cross-border minimum wage variation will be correlated with the actual state minimum wage variation" (2014b: 18). They therefore stand by their original critique of the placebo experiment, claiming that it is picking up the actual causal effect of an omitted variable—namely, the federal minimum wage—operating on the counties in the placebo group. This is why (they claim) the apparent placebo effect significantly diminishes when administered over a period with a constant federal minimum wage.

In this appendix, we have reviewed in some depth two particular lines of argument in the broader empirical debate over the possible disemployment effects of minimum wage increases. As our sketch of the arguments reveals, there are no obvious conclusions to be drawn from the various studies. Even professional economists familiar with econometric analysis would have difficulty refereeing the disputes, as they are fairly technical and often assume the reader is familiar with the relevant body of literature.

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