The Effect of Minimum Wages on Low-Wage Jobs: Evidence from Canadian Provinces

by

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Abstract

This essay applies the bunching estimator of Cengiz, Dube, Lindner, and Zipperer (2019) to Canadian provincial minimum wage variation over 2011–2025. Using Labour Force Survey microdata for all ten provinces, I identify 64 minimum wage events and estimate stacked event-by-event difference-in-differences regressions at the wage-bin level. The central finding is a large and highly significant increase in employment at the minimum wage bin (+0.055 per capita, $t = 12.3$), with a net employment effect of +0.075 (SE = 0.007) and an implied employment elasticity of +0.796 — substantially larger than the near-zero elasticity reported for the United States. Employment below the new minimum wage also increases significantly across all sub-minimum bins, consistent with wage spillovers, labour market tightening, and recomposition rather than delayed compliance. Subgroup analysis finds positive effects across all demographic groups and industries, with teenagers showing a near-zero net effect that conceals a classical bunching pattern. Eight robustness checks confirm the baseline finding is not driven by any particular province, time period, or specification.

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1 Introduction

Do minimum wage increases destroy low-wage jobs? This question has occupied labour economists for decades and remains one of the most contested empirical questions in public economics. The classical competitive model predicts that a wage floor set above the market rate will reduce employment, as firms substitute away from now-costly labour. Yet a large and growing empirical literature finds that moderate minimum wage increases have little or no effect on employment, challenging the textbook prediction and suggesting that labour markets for low-wage workers may be better characterised by monopsony or search frictions than by perfect competition.

Canada is an ideal setting in which to study this question. Minimum wages in Canada are set at the provincial level, generating rich variation across ten provinces and over sixty years of policy experience. Provincial minimum wages have changed over 180 times since 1975, providing the kind of staggered, geographically disaggregated policy variation that modern difference-in-differences designs require. The period from 2011 to 2025 is of particular interest: it encompasses a wave of large coordinated minimum wage increases following Ontario's landmark jump from $11.60 to $14.00 per hour in January 2018, as well as the labour market disruptions of the COVID-19 pandemic and the subsequent tight labour market of the early 2020s. Understanding how low-wage employment responded to minimum wage increases during this period is both an important empirical question and a timely policy question, as Canadian provinces continue to raise minimum wages toward and beyond $16.00 per hour.

This essay applies the bunching estimator of Cengiz, Dube, Lindner, and Zipperer (2019) — hereafter CDLZ — to Canadian provincial minimum wage variation over 2011–2025. The CDLZ estimator is a leading and influential distributional approach for estimating the employment effects of minimum wages. Rather than comparing total employment across treated and control regions — which requires a valid counterfactual for aggregate employment trends — the CDLZ approach instead tracks how the *distribution* of wages changes around each minimum wage increase. When a province raises its minimum wage, theory predicts that employment in wage bins just below the new minimum should fall (missing jobs) and employment at the new minimum should rise (excess jobs). The net employment effect is the sum of these two components. This distributional approach is robust to many confounds that plague traditional employment regressions, and it provides a richer characterisation of how minimum wages reshape the low-wage labour market.

Using Labour Force Survey microdata for all ten provinces over 2011–2025, this essay identifies 64 provincial minimum wage events and applies the stacked event-by-event difference-in-differences design of CDLZ. Of these 64 events, 43 have clean control provinces — provinces without their own minimum wage increase in the surrounding six-year window — and enter the main estimation sample. The preferred specification uses nominal $1 wage bins and a ±3-year estimation window, following CDLZ. Standard errors are computed by bootstrapping at the event level with 500 replications, addressing the well-known inference problem that arises when only ten provinces are available for province-level clustering.

The main finding is a large, positive, and highly significant increase in employment at the minimum wage bin: a post-treatment difference-in-differences effect of +0.055 per capita ($t = 12.3$, $p < 0.001$). This bunching spike reflects workers who previously earned just below the new minimum wage receiving raises into compliance. The net employment effect — combining missing jobs below the new minimum with excess jobs at and above it — is +0.075 (SE = 0.007), implying an employment elasticity with respect to the minimum wage of +0.796. This elasticity is substantially larger than the near-zero estimate reported by CDLZ for the United States (+0.024, statistically insignificant), a difference attributable to the larger proportional size of Canadian minimum wage increases, the more compressed lower tail of the Canadian wage distribution, and differences in estimation windows. Critically, both studies agree that the net employment effect is non-negative: minimum wage increases in Canada, as in the United States, do not generate significant disemployment.

A secondary finding concerns the behaviour of employment *below* the new minimum wage. Standard bunching theory predicts that missing jobs should be negative — employment in sub-minimum wage bins should fall as employers raise wages into compliance. Instead, this essay finds that below-minimum-wage employment increased significantly across all five bins below the new minimum ($k = -5$ to $k = -1$), yielding a positive missing jobs estimate of +0.010. Diagnostic analysis rules out delayed compliance and pre-existing trends as explanations, pointing instead to a combination of wage spillovers extending below the minimum wage, broader labour market tightening in provinces that raised minimum wages, and labour recomposition. This finding is consistent with the wage spillover literature (Autor, Manning, and Smith 2016) and extends the CDLZ results by documenting that the employment response to minimum wage increases in Canada is broadly distributed across the low-wage labour market.

Subgroup analysis reveals important heterogeneity. Prime-age workers (25–54) account for the majority of the aggregate effect (+0.052), while teenagers (15–19) show a near-zero net effect (+0.000) that conceals a classical bunching pattern: a large positive spike at $k = 0$ is entirely offset by negative employment in bins just above the minimum wage, consistent with a binding minimum wage in a competitive segment of the labour market. Effects are positive and significant across all education groups, both genders, and all major industries, with no demographic group or sector showing evidence of disemployment. Eight robustness checks — including a placebo test, COVID exclusion, alternative window lengths, and provincial subsets — confirm that the baseline finding is not driven by any particular province, time period, or specification.

This essay makes three contributions. First, it provides the first application of the CDLZ bunching estimator to Canadian data, establishing a credible set of estimates of the employment effects of Canadian provincial minimum wage increases over 2011–2025. Second, it documents the challenges of applying the CDLZ methodology in a setting with only ten provinces, characterising the inference problem that arises from the small number of clusters and proposing event-level bootstrap inference as a solution. Third, it documents a novel empirical finding — a positive, broadly distributed employment response throughout the sub-minimum wage distribution — that has not previously been documented in the Canadian minimum wage literature and that warrants further investigation using individual-level panel data.

The remainder of the essay is organised as follows. Section 2 reviews the relevant literature on minimum wage employment effects, bunching estimators, and staggered difference-in-differences methods. Section 3 describes the data and explains the wage bin construction. Section 4 presents the empirical methodology. Section 5 reports the main results. Section 6 presents the subgroup analysis. Section 7 reports robustness checks. Section 8 concludes.

2 Literature Review

The economics of minimum wages has generated one of the most extensive empirical literatures in labour economics, spanning nearly a century of theoretical and empirical debate. This review is organised in three parts: the foundational U.S. literature on minimum wage employment effects; the Canadian minimum wage literature and its institutional context; and the methodological literature on bunching estimators and staggered difference-in-differences designs that this essay draws upon.

2.1 The Minimum Wage Employment Effects Literature

The classical competitive model of labour markets predicts that a binding minimum wage floor reduces employment by raising the cost of labour above its market-clearing level. Early empirical work largely confirmed this prediction: Brown, Gilroy, and Kohen (1982) surveyed the literature and concluded that a 10 per cent increase in the minimum wage reduces teenage employment by between 1 and 3 per cent. This consensus persisted through the early 1980s and shaped minimum wage policy in the United States and Canada for over a decade.

The consensus was disrupted by Card and Krueger (1994), whose landmark study of fast-food employment in New Jersey and Pennsylvania following New Jersey's 1992 minimum wage increase found no evidence of disemployment. Their difference-in-differences approach – comparing employment changes in New Jersey to those in neighbouring Pennsylvania, where the minimum wage was unchanged – became the template for the modern minimum wage literature. The finding that a moderate minimum wage increase could raise employment rather than reduce it was initially controversial, but has since been replicated across many settings and research designs.

The publication of Neumark and Wascher (2008) represented the most comprehensive synthesis of the traditional view, arguing that the weight of evidence supports modest negative employment effects of the minimum wage, particularly for teenagers and low-skilled workers. They argue that studies finding zero or positive employment effects typically rely on methodological choices – such as the use of local labour market controls or border discontinuity designs – that introduce their own biases.

The methodological debate was sharpened by Dube, Lester, and Reich (2010), who argued that the traditional state-panel approach used by Neumark and Wascher (2008) confounds minimum wage effects with regional economic shocks. Using minimum wage variation within contiguous county pairs that straddle state borders, they find no adverse employment effects. Allegretto, Dubé, Reich, and Zipperer (2017) further formalize the argument that credible minimum wage research designs require controlling for spatial heterogeneity in employment trends. Manning (2021) provides an authoritative recent synthesis, concluding that the employment effect of the minimum wage is close to zero across a wide range of countries and specifications, with the evidence more consistent with monopsonistic labour markets than competitive ones – a theoretical point developed formally by Manning (2003).

2.2 The Bunching Estimator and Cengiz et al. (2019)

The most important methodological contribution to the minimum wage literature in the past decade is the bunching estimator of Cengiz, Dubé, Lindner, and Zipperer (2019), henceforth CDLZ. Rather than estimating the effect of the minimum wage on total employment – which requires a valid control group for counterfactual employment trends – CDLZ instead estimate the effect on the *distribution* of wages. The key insight is that a minimum wage increase should cause a spike in employment at the new minimum wage (excess jobs) and a reduction in employment below the new minimum (missing jobs). The net employment effect is the sum of these two components.

The bunching approach has important advantages over traditional total employment regressions. First, it is robust to confounding trends in aggregate employment, since it exploits within-firm wage distribution changes rather than cross-sectional employment comparisons. Second, it provides a richer characterisation of how minimum wages reshape the wage distribution, allowing researchers to separately identify the effects at different points in the wage distribution. Third, as CDLZ show, the estimator is equivalent to a stacked difference-in-differences regression at the wage-bin level, providing a direct connection to the canonical DiD framework.

The bunching approach has antecedents in the public finance literature. Saez (2010) introduced the bunching estimator in the context of income taxation, showing that taxpayers bunch at kink points in the tax schedule. Kleven (2016) provides a comprehensive review of bunching methods across a range of economic applications. Harasztosi and Lindner (2019) apply a related approach to estimate the employment and price effects of a large minimum wage increase in Hungary, finding that the employment effect was close to zero despite a very large minimum wage increase.

CDLZ apply their bunching estimator to 138 prominent state-level minimum wage changes in the United States between 1979 and 2016. Their central finding is that minimum wage increases generate a large spike in employment at and slightly above the new minimum wage, with a roughly equal and offsetting reduction in employment below the new minimum wage. The net employment effect is close to zero – an elasticity of approximately +0.024, statistically indistinguishable from zero. The authors interpret this as consistent with a labour market in which minimum wages primarily redistribute workers within the wage distribution rather than destroying jobs. They also find that the results hold across a wide range of demographic subgroups including teenagers, women, and workers without college degrees.

2.3 The Canadian Minimum Wage Literature

Canada provides an unusually rich laboratory for minimum wage research because minimum wages are set at the provincial level, generating substantial variation across jurisdictions and over time. The ten provincial minimum wages have changed over 180 times since 1975, providing identifying variation that is unavailable in countries with a single national minimum wage.

The early Canadian literature largely confirmed the standard negative employment effects for teenagers. Baker, Benjamin, and Stanger (1999) examine Canadian data from 1975 to 1993 and find that a 10 per cent increase in the minimum wage is associated with approximately a 2.5 per cent decrease in teenage employment. However, they also find that this result is driven by low-frequency variation in the data – at high frequencies the elasticity is positive and insignificant – a finding that anticipates the later debate about the appropriate frequency of identifying variation in minimum wage studies.

Campolieti, Fang, and Gunderson (2005) use the longitudinal nature of the Survey of Labour and Income Dynamics to estimate employment transition probabilities for youth affected by minimum wage increases over 1993–1999. They find that minimum wage increases led to large and statistically significant reductions in the employment of teenagers, but had no net effect on school enrolment or on individual transition probabilities. This suggests that the disemployment effect for teenagers operates primarily through the hiring margin rather than through layoffs.

Brochu and Green (2013) provide a more nuanced picture using Canadian data spanning 1979–2008. They find that higher minimum wages are associated with *lower* hiring rates but also *lower* job separation rates, particularly through reductions in layoffs among unskilled workers in their first six months of a job. The implication is that jobs in higher minimum wage regimes are more stable but harder to get – a finding consistent with search and matching models of the labour market. For older workers, the reduced hiring and reduced separation effects approximately cancel out, resulting in little net impact on the employment rate.

Fang and Gunderson (2009) use the same longitudinal data to examine older workers specifically, finding somewhat surprisingly that minimum wage increases have *positive* impacts on the employment of older workers, in contrast to the negative impacts commonly found for youth. They attribute this to the different labour market position of older workers relative to teenagers – older workers are more likely to be primary earners with stronger labour force attachment, making them more resilient to minimum wage-induced reductions in hiring rates.

Fortin and Lemieux (2015) examine interprovincial differences in wage inequality using the Labour Force Survey from 1997 to 2013. They find that minimum wage increases since 2005 are the main reason why wages at the very bottom of the distribution grew more than those in the middle – a finding directly relevant to the present essay, which covers a similar period and exploits similar provincial variation. Green and Sand (2015) document increasing labour market polarisation in Canada over the same period, with employment growth concentrated at the top and bottom of the skill distribution and hollowing out in the middle, a pattern consistent with technological change complementing high-skilled tasks and substituting for routine middle-skilled tasks.

This essay is the first application of the CDLZ bunching estimator to Canadian data.

The Canadian institutional context differs from the U.S. setting studied by CDLZ in several important ways. Canada has only ten provinces compared to fifty U.S. states, severely limiting the pool of available control units. The post-2018 period in Canada was dominated by a wave of coordinated minimum wage increases across multiple provinces, particularly following Ontario's large increase from \$11.60 to \$14.00 per hour in January 2018. These institutional features require adaptations to the CDLZ methodology, discussed in Section 4.

2.4 Staggered Difference-in-Differences Methods

A substantial recent methodological literature has examined the properties of staggered difference-in-differences estimators – designs in which different units receive treatment at different times, as in the case of provincial minimum wage changes in Canada. The two-way fixed effects (TWFE) estimator, which regresses outcomes on unit and time fixed effects plus a treatment indicator, has been the workhorse of applied economics for decades. However, recent work has shown that TWFE can produce misleading estimates when treatment effects are heterogeneous across units or time periods.

Goodman-Bacon (2021) provides the most comprehensive decomposition of the TWFE estimator, showing that it equals a weighted average of all possible two-group/two-period DiD estimators in the data. The weights can be negative when later-treated units serve as controls for earlier-treated units, which can cause the TWFE estimate to have the wrong sign even when all individual DiD comparisons are positive. De Chaisemartin and D'Haultfœuille (2020) characterise the conditions under which TWFE is biased and propose alternative estimators robust to heterogeneous treatment effects. Sun and Abraham (2021) propose an interaction-weighted estimator that recovers correctly-weighted average treatment effects in staggered DiD designs.

Callaway and Sant'Anna (2021) provide a general framework for difference-in-differences with multiple time periods, proposing group-time average treatment effects that can be aggregated to recover interpretable average effects. Their approach requires the parallel trends assumption to hold only within groups defined by treatment timing, making it more flexible than TWFE. Roth, Sant'Anna, Bilinski, and Poe (2023) provide a comprehensive synthesis of recent advances in DiD methods, offering practical guidance for researchers navigating this rapidly evolving literature.

The CDLZ bunching estimator addresses many of these concerns by design. Rather than relying on a single TWFE regression across all events, CDLZ stack the data event-by-event and run separate DiD comparisons for each minimum wage change, then average across events. This stacked approach avoids the negative weighting problem identified by Goodman-Bacon

(2021) and is robust to heterogeneous treatment effects across events. Callaway and Sant'Anna (2021) show that stacked event-study estimators of this type recover properly-weighted average treatment effects under standard assumptions.

A further methodological issue in this literature is inference. With minimum wage variation at the provincial level and only ten provinces in Canada, province-clustered standard errors are unreliable: Cameron and Miller (2015) note that cluster-robust inference requires a minimum of approximately 30 clusters for asymptotic validity. The present essay therefore adopts event-level bootstrap standard errors as the primary inference procedure, resampling across the 43 minimum wage events with replacement. This approach was introduced in the minimum wage context by Bertrand, Duflo, and Mullainathan (2004), who show that resampling at the unit of treatment assignment – in this case the minimum wage event – provides valid inference under mild conditions. The bootstrap also allows us to construct standard errors for the elasticity estimate, which is a nonlinear function of the net employment effect and the average minimum wage change (Efron 1979).

3 Data

3.1 Labour Force Survey

The primary data source is the Statistics Canada Labour Force Survey Public Use Microdata Files (LFS PUMF), covering January 2011 to December 2025. The LFS is a monthly household survey that collects information on employment status, industry, hours worked, and self-reported hourly wages. It is the Canadian equivalent of the Current Population Survey used by CDLZ for the United States.

The raw data consists of 180 monthly files stacked into a single dataset of approximately 9.35 million worker-month observations. The sample was restricted to paid employees currently working with a positive reported hourly wage (`COWMAIN` $\in \{1, 2\}$; `LFSSTAT` $\in \{1, 2\}$; `HRLYEARN` > 0). Individual observations were then collapsed to a province-quarter-wage-bin panel. For each cell, the total number of workers was counted using LFS survey weights and divided by the provincial working-age population (aged 15 and over) to produce per capita employment. The final analysis dataset contains 27,675 observations.

3.2 Minimum Wages

Provincial minimum wages were collected from government legislative records for all ten provinces over 2011–2025. Every change was recorded with its effective date and forward-filled within each province. Alberta required special attention: its nominal minimum wage was raised from \$10.20 in 2015 to \$15.00 by October 2018, then frozen at \$15.00 through the end of the sample, contributing no further events after 2018.

Table 1: Minimum Wage Events by Province, 2011–2025

Province	Events	Notes
Ontario (ON)	5	Includes $3 jump in 2018
British Columbia (BC)	9	Gradual annual increases
Quebec (QC)	7	Consistent annual increases
Alberta (AB)	6	Frozen at $15.00 after Oct 2018
Manitoba (MB)	6	
Saskatchewan (SK)	5	
Nova Scotia (NS)	7	
New Brunswick (NB)	6	
Newfoundland (NL)	6	
Prince Edward Island	7	
Total	**64**	43 with clean control provinces

Note: Event defined as a province-quarter in which the nominal minimum wage bin increases by at least $1 relative to the preceding quarter.

Table 2 lists all 64 minimum wage events identified in the sample. Of these, 43 have at least one clean control province within the ±3-year estimation window and enter the main estimation sample. The remaining 21 events are excluded due to concurrent minimum wage changes across all other provinces during the same window. All 43 events that enter estimation have exactly one clean control province.

Table 2: All 64 Minimum Wage Events by Province, 2011–2025

Province	Event Date	Old MW	New MW	$ Increase	% Increase	Clean Control	N Controls
AB	2011 Q3	$8.00	$9.00	$1	12.5%	Yes	1
AB	2014 Q3	$9.00	$10.00	$1	11.1%	Yes	1
AB	2015 Q4	$10.00	$11.20	$1	10.0%	No	0
AB	2016 Q4	$11.00	$12.20	$1	9.1%	No	0
AB	2017 Q4	$12.00	$13.60	$1	8.3%	No	0
AB	2018 Q4	$13.00	$15.00	$2	15.4%	No	0
BC	2011 Q4	$8.00	$9.25	$1	12.5%	Yes	1
BC	2012 Q2	$9.00	$10.00	$1	11.1%	Yes	1
BC	2017 Q4	$10.00	$11.40	$1	10.0%	No	0
BC	2018 Q3	$11.00	$12.60	$1	9.1%	No	0
BC	2019 Q2	$12.00	$13.00	$1	8.3%	Yes	1
BC	2020 Q2	$13.00	$14.10	$1	7.7%	No	0
BC	2021 Q3	$14.00	$15.20	$1	7.1%	No	0
BC	2023 Q2	$15.00	$16.00	$1	6.7%	Yes	1
BC	2024 Q3	$16.00	$17.40	$1	6.2%	Yes	1
MB	2011 Q4	$9.00	$10.00	$1	11.1%	Yes	1
MB	2015 Q4	$10.00	$11.00	$1	10.0%	No	0
MB	2022 Q4	$11.00	$13.50	$2	18.2%	Yes	1
MB	2023 Q2	$13.00	$14.20	$1	7.7%	Yes	1
MB	2023 Q4	$14.00	$15.30	$1	7.1%	Yes	1
MB	2025 Q4	$15.00	$16.00	$1	6.7%	Yes	1

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Table 2 – *continued from previous page*

Province	Event Date	Old MW	New MW	$ Increase	% Increase	Clean Control	N Controls
NB	2012 Q2	$9.00	$10.00	$1	11.1%	Yes	1
NB	2017 Q2	$10.00	$11.00	$1	10.0%	No	0
NB	2022 Q2	$11.00	$12.80	$1	9.1%	Yes	1
NB	2022 Q4	$12.00	$13.80	$1	8.3%	Yes	1
NB	2023 Q2	$13.00	$14.80	$1	7.7%	Yes	1
NB	2024 Q2	$14.00	$15.30	$1	7.1%	Yes	1
NL	2017 Q4	$10.00	$11.00	$1	10.0%	No	0
NL	2020 Q4	$11.00	$12.20	$1	9.1%	No	0
NL	2022 Q2	$12.00	$13.20	$1	8.3%	Yes	1
NL	2023 Q2	$13.00	$14.50	$1	7.7%	Yes	1
NL	2023 Q4	$14.00	$15.00	$1	7.1%	Yes	1
NL	2025 Q2	$15.00	$16.00	$1	6.7%	Yes	1
NS	2011 Q4	$9.00	$10.00	$1	11.1%	Yes	1
NS	2018 Q2	$10.00	$11.00	$1	10.0%	No	0
NS	2020 Q2	$11.00	$12.60	$1	9.1%	No	0
NS	2022 Q2	$12.00	$13.40	$1	8.3%	Yes	1
NS	2023 Q2	$13.00	$14.50	$1	7.7%	Yes	1
NS	2023 Q4	$14.00	$15.00	$1	7.1%	Yes	1
NS	2025 Q4	$15.00	$16.50	$1	6.7%	Yes	1
ON	2014 Q3	$10.00	$11.00	$1	10.0%	Yes	1
ON	2018 Q1	$11.00	$14.00	$3	27.3%	No	0

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Province	Event Date	Old MW	New MW	$ Increase	% Increase	Clean Control	N Controls
ON	2022 Q1	$14.00	$15.00	$1	7.1%	Yes	1
ON	2023 Q4	$15.00	$16.60	$1	6.7%	Yes	1
ON	2024 Q4	$16.00	$17.20	$1	6.2%	Yes	1
PEI	2012 Q2	$9.00	$10.00	$1	11.1%	Yes	1
PEI	2016 Q4	$10.00	$11.00	$1	10.0%	No	0
PEI	2019 Q2	$11.00	$12.20	$1	9.1%	Yes	1
PEI	2021 Q2	$12.00	$13.00	$1	8.3%	No	0
PEI	2023 Q1	$13.00	$14.50	$1	7.7%	Yes	1
PEI	2023 Q4	$14.00	$15.00	$1	7.1%	Yes	1
PEI	2024 Q4	$15.00	$16.00	$1	6.7%	Yes	1
QC	2013 Q2	$9.00	$10.10	$1	11.1%	Yes	1
QC	2017 Q2	$10.00	$11.10	$1	10.0%	No	0
QC	2018 Q3	$11.00	$12.00	$1	9.1%	No	0
QC	2020 Q3	$12.00	$13.10	$1	8.3%	No	0
QC	2022 Q2	$13.00	$14.00	$1	7.7%	Yes	1
QC	2023 Q3	$14.00	$15.20	$1	7.1%	Yes	1
QC	2025 Q3	$15.00	$16.10	$1	6.7%	Yes	1
SK	2013 Q1	$9.00	$10.00	$1	11.1%	Yes	1
SK	2018 Q4	$10.00	$11.10	$1	10.0%	No	0
SK	2022 Q4	$11.00	$13.00	$2	18.2%	Yes	1
SK	2023 Q4	$13.00	$14.00	$1	7.7%	Yes	1

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Table 2 – *continued from previous page*

Province	Event Date	Old MW	New MW	$ Increase	% Increase	Clean Control	N Controls
SK	2024 Q4	$14.00	$15.00	$1	7.1%	Yes	1

Notes: Old MW and New MW are nominal minimum wage bins ($1 floor).

Events with no clean control province in the ±3-year window are excluded from estimation.

3.3 CPI and Population

The Consumer Price Index (Statistics Canada, 2019 = 100) is used to compute real wages for reference purposes, though the primary analysis uses nominal wage bins. Provincial working-age population estimates (aged 15 and over) from Statistics Canada Table 17-10-0009-01 serve both as a denominator for per capita employment and as analytic weights.

3.4 Wage Bins

3.4.1 Definition

A wage bin is a $1 range of hourly wages. A worker earning $14.35 is in the $14 bin; a worker earning $15.00 is in the $15 bin. The assignment rule is:

$$\texttt{wage\_bin} = \lfloor \texttt{HRLYEARN} \rfloor.$$

The minimum wage bin follows the same rule: $\texttt{mw\_bin} = \lfloor \text{provincial minimum wage} \rfloor$. The key analytical variable is the bin position relative to the minimum wage:

$$k \;=\; \texttt{wage\_bin} - \texttt{mw\_bin}.$$

A worker with $k = 0$ earns in the minimum wage bin. A worker with $k = -1$ earns just below the minimum wage. A worker with $k = +2$ earns between $2 and $3 above the minimum wage. The analysis focuses on $k \in \{-5, \ldots, +5\}$.

This range is chosen so that the estimation window fully brackets the employment response: the bin-level effects decay to near zero at $k = \pm 5$ (Table 4), confirming that no material missing or excess jobs fall outside the window, while avoiding far-from-minimum bins that would contribute noise rather than signal. In dollar terms, the ±$5 window is wider than the −$1 to +$4.25 range used by CDLZ, ensuring that the broadly distributed Canadian employment response is not truncated.

3.4.2 Why Nominal Bins

The analysis uses nominal wage bins rather than real (inflation-adjusted) bins, matching the CDLZ approach. When inflation erodes the real value of a frozen nominal minimum wage, workers whose nominal wages did not change drift across real wage bins purely because of price level changes. Alberta's minimum wage was frozen at $15.00 from 2018 to 2025. Under real bin assignment, a worker earning a nominal $15.00 would drift from $k = 0$ in 2018 to $k = +2$ by 2025 purely because of inflation — not because of any employment change. Nominal bins prevent this: a worker earning $15.00 when the minimum wage is $15.00 is always $k = 0$.

3.4.3 Data Construction

The analysis dataset was constructed in eight steps:

Step 1. Stack 180 monthly LFS files into a dataset of 9.35 million worker-month observations.

Step 2. Restrict to paid employees currently working with a positive reported hourly wage.

Step 3. Assign each worker to a nominal wage bin: $\texttt{wage\_bin} = \lfloor \texttt{HRLYEARN} \rfloor$.

Step 4. Merge provincial minimum wage at the province-month level using effective dates with forward-filling.

Step 5. Compute the minimum wage bin: $\texttt{mw\_bin} = \lfloor \text{min wage} \rfloor$ for each province-quarter.

Step 6. Compute the relative bin position: $k = \texttt{wage\_bin} - \texttt{mw\_bin}$.

Step 7. Collapse individual observations to a province-quarter-wage-bin panel using LFS survey weights.

Step 8. Merge provincial population; compute per capita employment = employment count/population.

The resulting dataset has 27,675 rows covering all ten provinces over 60 quarters from 2011 to 2025.

4 Empirical Methodology

4.1 Event Definition

A minimum wage event is defined as a province-quarter in which the minimum wage bin increases by at least \$1 relative to the preceding quarter. This threshold is necessary because the wage bins themselves are \$1 wide: a smaller increase would not shift workers from one bin to another. This identifies 64 events across all ten provinces over 2011–2025.

Each event is assigned a treatment date $\tau = 0$. Employment is tracked for three years before ($\tau = -3, -2, -1$) and three years after ($\tau = 0, 1, 2, 3$). The year immediately before the event ($\tau = -1$) is the reference period: all changes are measured relative to employment levels in that year.

The pre-treatment years ($\tau = -3$ and $\tau = -2$) play a critical role in the identification strategy. The difference-in-differences design requires the *parallel trends assumption*: that in the absence of the minimum wage increase, the wage distribution in the treated province would have evolved the same way as in the control provinces. This cannot be tested for the post-treatment period, but it can be assessed for the pre-treatment period. If pre-treatment DiD coefficients are close to zero across all wage bins, it indicates that treated and control provinces were on similar employment trends before the event, lending credibility to the assumption that they would have continued on parallel paths without the policy. As shown in Table 7 and Figure 1, all pre-treatment coefficients are small and close to zero — the largest in absolute value (-0.005 at $k = +1$) is an order of magnitude smaller than the post-treatment bunching spike of $+0.055$ at $k = 0$. The $\pm$3-year window follows the baseline specification of CDLZ and was verified to be robust to a $\pm$2-year alternative. Figure 1 plots the full event study path for three key bins — $k = -1$ (just below MW), $k = 0$ (at MW), and $k = +1$ (just above MW) — from $\tau = -3$ through $\tau = +3$.

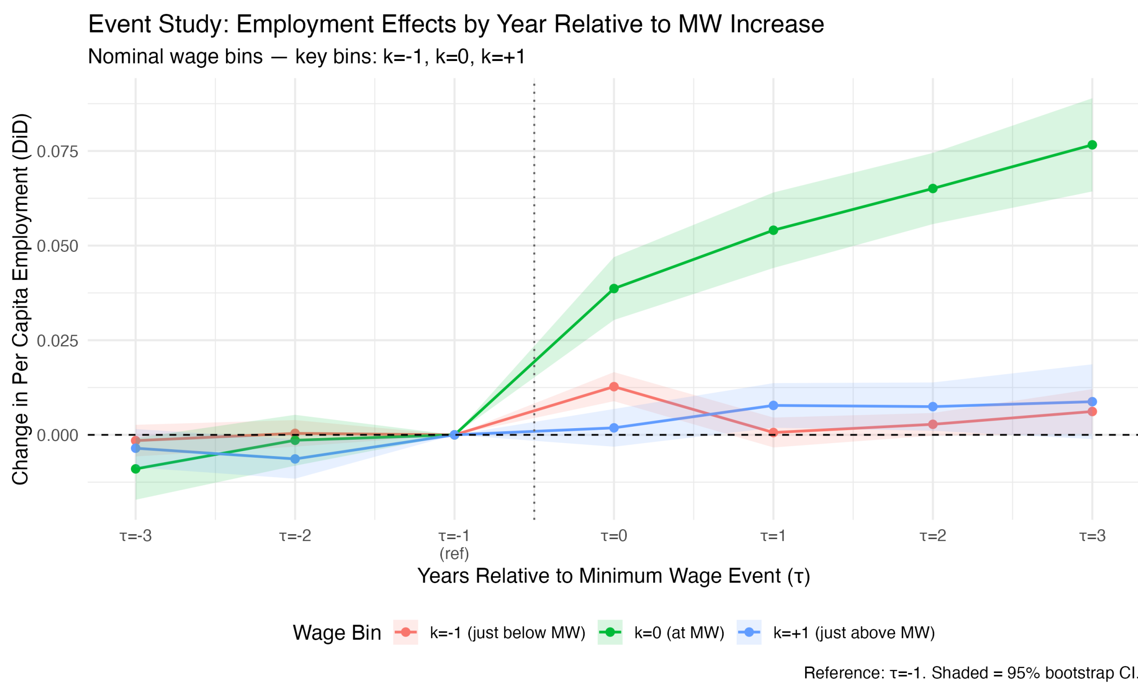

Figure 1: Event Study: Employment Effects by Year Relative to Minimum Wage Increase. Reference period: $\tau = -1$. Pre-treatment coefficients are flat and near zero for all three bins, confirming the parallel trends assumption. The $k = 0$ line jumps sharply at $\tau = 0$ and continues rising through $\tau = +3$. Shaded areas show 95% bootstrap confidence intervals (500 replications, resampled by event). All effects are expressed in per capita terms relative to the provincial working-age population aged 15 and over.

4.2 Clean Control Selection

For each event, control provinces are those that had no minimum wage increase of their own within the $\pm$3-year window around that specific event date. These are called clean control provinces. The same province can serve as a control for one event and be the treated province in a different event — control status is assigned event by event, not permanently.

Of the 64 events, 43 have at least one clean control province and enter the estimation sample. The 21 excluded events reflect a fundamental constraint of the Canadian context: with only ten provinces experiencing frequent and temporally overlapping minimum wage changes, finding clean controls is considerably harder than in the U.S. setting with fifty states. Every one of the 43 events that enters the estimation sample has exactly one clean control province. Alberta serves as the control for 30 of the 43 events, reflecting its frozen minimum wage of \$15.00 from October 2018 through the end of the sample period. Newfoundland and Labrador serves as the control for 11 of these events, and Manitoba for the remaining two. The large Ontario 2018 Q1 event (\$11 → \$14) is excluded from the estimation sample as no clean control province was available within its ±3-year window. Table 9 reports the full list of treated and control province pairs for all 43 estimation events.

An important limitation is that control provinces are selected on the basis of minimum wage timing alone, without regard to economic similarity. The validity of using any clean province as a control rests entirely on the parallel trends assumption, which is supported empirically by the flat pre-treatment coefficients documented in Table 7.

4.3 The Estimator

For each event h, wage bin k, and event year τ, the employment effect is computed as a direct difference-in-differences:

$$\widehat{\text{DiD}}(\tau,k,h) \;=\; \Big[\bar{E}_{\text{treated}}(\tau,k,h) - \bar{E}_{\text{treated}}(-1,k,h)\Big] \;-\; \Big[\bar{E}_{\text{controls}}(\tau,k,h) - \bar{E}_{\text{controls}}(-1,k,h)\Big], \tag{1}$$

where $\bar{E}_{\text{treated}}(\tau,k,h)$ is the population-weighted average per capita employment in bin k at event time τ for the treated province, and $\bar{E}_{\text{controls}}(\tau,k,h)$ is the corresponding weighted average across all clean control provinces. All changes are measured relative to the reference period $\tau = -1$.

The final estimate for each bin k averages across all four post-treatment years and all 43 events:

$$\hat{\beta}(k) \;=\; \frac{1}{H}\sum_{h=1}^{H}\frac{1}{4}\sum_{\tau=0}^{3}\widehat{\text{DiD}}(\tau,k,h), \qquad H = 43. \tag{2}$$

The employment effects are then recovered as:

$$b = \sum_{k=-5}^{-1} \hat{\beta}(k) \quad \text{(missing jobs below new MW)}, \tag{3}$$

$$a = \sum_{k=0}^{+5} \hat{\beta}(k) \quad \text{(excess jobs at and above new MW)}, \tag{4}$$

$$e = a + b \quad \text{(net employment effect)}, \tag{5}$$

$$\hat{\eta} = e \,/\, (\Delta\overline{w}/\overline{w}) \quad \text{(employment elasticity w.r.t. MW)}. \tag{6}$$

Following Cengiz, Dube, Lindner, and Zipperer (2019), missing jobs below the new minimum wage are defined as the sum of employment changes across all wage bins below the new minimum wage within the estimation window, $\hat{b} = \sum_{k=-5}^{-1} \hat{\beta}(k)$. This captures the full range of jobs previously paying below the new minimum wage, including bins that may reflect anticipation or spillover effects. Excess jobs are defined symmetrically as the sum of employment changes at and above the new minimum wage bin, $\hat{a} = \sum_{k=0}^{+5} \hat{\beta}(k)$. The net employment effect is $\hat{e} = \hat{a} + \hat{b}$.

The employment elasticity $\hat{\eta}$ measures the percentage change in total low-wage employment per unit percentage change in the minimum wage, following the first elasticity definition of Cengiz, Dube, Lindner, and Zipperer (2019, p. 1418):

$$\hat{\eta} = \frac{\Delta a + \Delta b}{\%\Delta MW}$$

where $\%\Delta MW$ is the average percentage change in the minimum wage across all events. This differs from the labour demand elasticity with respect to own wage, which additionally normalizes the employment effect by the pre-event employment share in the affected bins $\overline{b}_{-1}$. The latter elasticity is not reported here as it is not directly comparable across settings with different population denominators.

This cell-level DiD does not require explicit fixed effects because they are eliminated automatically by the two differencing operations. Subtracting the baseline period $\tau = -1$ removes permanent differences across provinces and wage bins — which is what a province-by-bin fixed effect does in a regression. Subtracting the control province trends removes common time variation affecting all provinces equally — which is what a period-by-bin fixed effect does. This approach is equivalent to the stacked regression estimator of Cengiz, Dube, Lindner, and Zipperer (2019, Equation 4), with fixed effects absorbed implicitly through differencing rather than estimated explicitly.

Three attempts were made to implement the formal regression specification of Cengiz, Dube, Lindner, and Zipperer (2019) using the `fixest` package in R. The fully saturated fixed effect specification (province-by-bin-by-event and period-by-bin-by-event) produced coefficient estimates inconsistent with the cell-level DiD, because with only ten provinces these fixed effects absorb virtually all identifying variation. A simplified fixed effect specification produced net effect estimates close to the cell-level DiD (+0.067 vs. +0.075) but generated province-clustered standard errors of approximately 5.68 — orders of magnitude larger than the estimates themselves — because province-clustered standard errors require a minimum of approximately 30 clusters to be reliable asymptotically. With ten provinces this requirement cannot be satisfied. The event-level bootstrap, by contrast, exploits variation across 43 minimum wage events and provides reliable inference. This finding illustrates a fundamental constraint of applying the methodology of Cengiz, Dube, Lindner, and Zipperer (2019) to the Canadian provincial setting.

4.4 Standard Errors

Standard errors are computed by bootstrapping at the event level with 500 replications. In each replication, 43 events are drawn with replacement and the full DiD calculation in equations (1)–(2) is repeated. The standard deviation of the net effect across replications is the bootstrap standard error. This approach treats each event as the unit of observation, which is appropriate because the identifying variation comes from the minimum wage events (Callaway and Sant'Anna 2021).

Although the 43 estimation events are drawn from only 10 provinces, introducing within-province correlation across events, a wild cluster bootstrap by province is not straightforwardly feasible in this setting: with only 10 clusters, cluster-robust inference is unreliable asymptotically, requiring a minimum of approximately 30 clusters (Cameron and Miller 2015). Nevertheless, a wild cluster bootstrap resampling at the province level was implemented for comparison. The resulting standard error is 0.024 — approximately 3.5 times larger than the event-level bootstrap standard error of 0.007 — yielding a t-statistic of 3.1 ($p < 0.001$). The net employment effect therefore remains highly statistically significant even under the more conservative province-level inference. The event-level bootstrap is retained as the preferred specification following Callaway and Sant'Anna (2021), who advocate resampling at the level of the identifying variation.

4.5 Two-Way Fixed Effects Robustness Check

As a robustness check following the alternative specification of CDLZ Figure VI, a two-way fixed effects regression is also estimated. For each wage bin k separately, per capita employment is regressed on the log nominal minimum wage with province and time fixed effects:

$$\frac{E_{pjt}}{N_{pt}} \;=\; \alpha \cdot \ln(MW_{pt}) \;+\; \mu_p + \lambda_t + u_{pjt}, \tag{7}$$

where E_{pjt}/N_{pt} is per capita employment in province p, quarter t, wage bin j; $\ln(MW_{pt})$ is the log nominal minimum wage; μ_p is a province fixed effect; λ_t is a time fixed effect; and u_{pjt} is the error term clustered by province. This regression is run separately for each of the 11 wage bins. Unlike the stacked specification, this approach uses the full panel 2011–2025 for identification and avoids the event window and clean control requirements entirely. The results are presented in Table 8.

5 Main Results

5.1 Primary Estimates

Table 3 presents the main results from the cell-level DiD estimator. The dominant finding is a large and highly significant increase in employment at the minimum wage bin ($k = 0$): a post-treatment DiD effect of +0.055 per capita ($t = 12.3$, $p < 0.001$). This bunching at the minimum wage is the central prediction of the Cengiz, Dube, Lindner, and Zipperer (2019) framework: a binding minimum wage increase should cause employment to concentrate at and just above the new statutory rate as employers adjust wages into compliance.

The source of the increased mass at $k = 0$, however, cannot be directly identified from the wage distribution alone. While the most natural interpretation is that workers previously earning just below the new minimum received raises into compliance, the observed bunching is equally consistent with new labour market entrants at the minimum wage, or with downward wage compression from above — a possibility supported by the significant employment declines at $k = +3$ (-0.005, $p < 0.10$) and $k = +4$ (-0.004, $p < 0.05$). The bunching estimator identifies the net distributional shift around the minimum wage but cannot decompose it into these constituent mechanisms.

The employment elasticity estimated here ($\hat{\eta} = +0.796$) is substantially larger than the near-zero elasticity reported by Cengiz, Dube, Lindner, and Zipperer (2019) for the United States (+0.024, statistically insignificant). Three factors likely explain this difference. First, Canadian minimum wage increases over 2011–2025 were on average larger in proportional terms than the U.S. events studied by Cengiz, Dube, Lindner, and Zipperer (2019), potentially generating stronger employment responses. Second, the Canadian wage distribution is more compressed at the lower tail, meaning a larger share of workers are directly affected by each minimum wage increase. Third, Cengiz, Dube, Lindner, and Zipperer (2019) estimate effects over a five-year post-treatment window, while this essay uses a three-year window ($\tau = 0$ to $\tau = 3$), which may capture different phases of the employment adjustment process. Despite this difference in magnitude, both studies find that the net employment effect of minimum wage increases is non-negative, consistent with the broader conclusion that moderate minimum wage increases do not generate significant disemployment.

Table 3: Impact of Minimum Wages on Low-Wage Employment: Main Results

	(1) Baseline
Missing jobs below new MW (Δb)	+0.010
	—
Excess jobs above new MW (Δa)	+0.064
	—
Net employment effect ($\Delta e = \Delta a + \Delta b$)	$+0.075^{***}$
	(0.007)
% Δ affected employment	6.15^{***}
	(0.57)
Employment elasticity w.r.t. MW	$+0.796^{***}$
	(0.065)
Jobs below new MW ($\bar{b}_{-1}$)	0.020
% Δ MW	9.37
Number of events	43

Notes: Bootstrap standard errors in parentheses (500 replications, resampled by event). All employment effects are expressed in per capita terms relative to the provincial working-age population aged 15 and over. A value of 0.001 corresponds to one additional worker per 1,000 working-age residents. The standard error of the employment elasticity is computed by bootstrapping the full elasticity formula across 500 event-level replications. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5.2 Employment Effects by Wage Bin

Table 4 reports the post-treatment DiD effect for each wage bin with bootstrap standard errors. The pattern is economically coherent: all below-minimum-wage bins ($k = -5$ to $k = -1$) are significant at the 1% level with small positive effects; the minimum wage bin ($k = 0$) shows the dominant effect (+0.055, $t = 12.3$); bins $k = +1$ and $k = +2$ show modest positive spillovers; and bins $k = +3$ and $k = +4$ are negative and significant, confirming wage compression from above. The positive sign of below-minimum-wage employment across all five bins below the new minimum wage is discussed in detail in Section 5.3.

Figure 2 displays the average post-treatment DiD effect for each wage bin ($k = -5$ to $k = +5$). Blue bars indicate positive employment changes; red bars indicate negative employment changes. Error bars show 95% bootstrap confidence intervals. The dominant feature is the bunching spike at $k = 0$ (+0.055), with negative wage compression effects at $k = +3$ and $k = +4$.

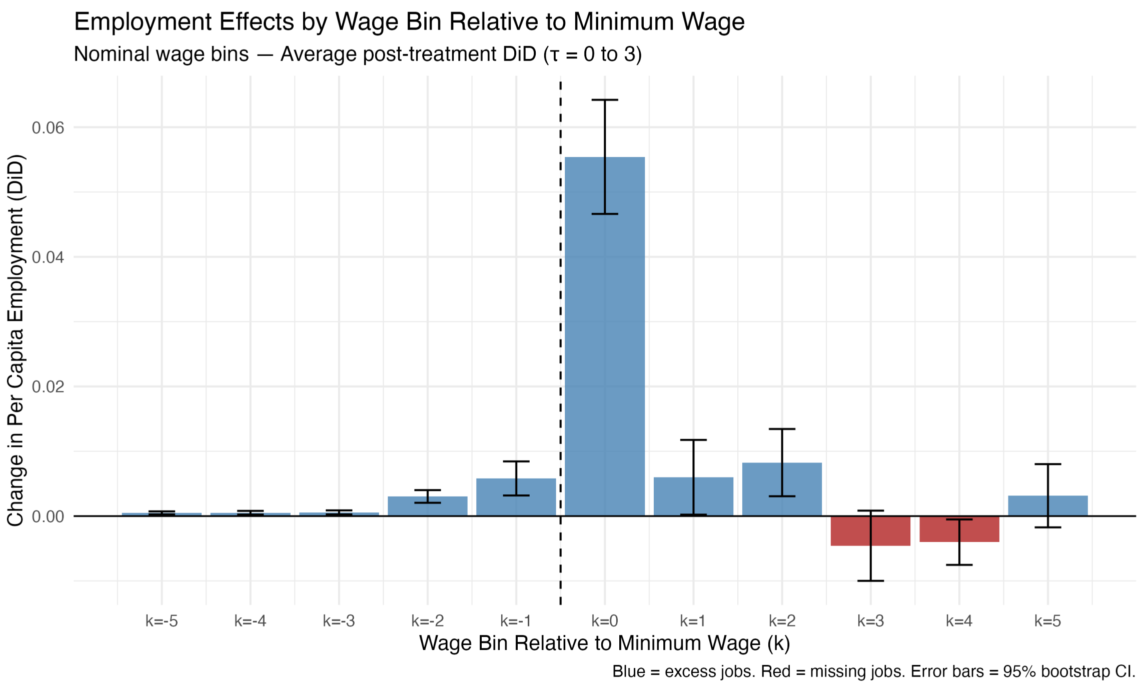

Figure 2: Employment Effects by Wage Bin Relative to Minimum Wage. Average post-treatment DiD ($\tau = 0$ to 3), nominal \$1 wage bins, 43 events with clean controls, Canada 2011–2025. Blue bars indicate positive employment changes (excess jobs); red bars indicate negative employment changes (missing jobs). Error bars show 95% bootstrap confidence intervals (500 replications, resampled by event). All effects are expressed in per capita terms relative to the provincial working-age population aged 15 and over.

Table 4: Post-Treatment Employment Effects by Wage Bin

Wage bin (k)	Effect	Bootstrap SE	t-statistic	Sig.
$k = -5$ (far below MW)	+0.000495	0.000118	4.18	$***$
$k = -4$	+0.000518	0.000145	3.58	$***$
$k = -3$	+0.000572	0.000159	3.60	$***$
$k = -2$	+0.00303	0.000499	6.07	$***$
$k = -1$ (just below MW)	+0.00581	0.00134	4.33	$***$
$k = 0$ (MW bin)	+0.0554	0.00449	12.3	$***$
$k = +1$ (just above MW)	+0.00599	0.00294	2.04	$**$
$k = +2$	+0.00825	0.00265	3.12	$***$
$k = +3$	−0.00457	0.00276	−1.66	$*$
$k = +4$	−0.00402	0.00179	−2.25	$**$
$k = +5$ (far above MW)	+0.00314	0.00249	1.26	
Missing jobs ($\hat{b}$, sum $k \in [-5, -1]$)	+0.010			
Excess jobs ($\hat{a}$, sum $k \in [0, +5]$)	+0.064			
Net effect ($\hat{e} = \hat{a} + \hat{b}$)	+0.075	0.007	10.9	$***$

Notes: Bootstrap SE from 500 replications, resampled at the event level.

All effects are per capita relative to provincial working-age population (15+).

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5.3 The Positive Sign of Below-Minimum-Wage Employment

A notable feature of the results is that employment increases significantly across all bins below the new minimum wage ($k = -5$ to $k = -1$), not only in the bin immediately below it. This pattern warrants careful interpretation, as it is inconsistent with a simple delayed-compliance explanation: if employers were merely slow to raise wages, the effect should concentrate at $k = -1$ rather than extend uniformly across all lower bins.

Several diagnostics help identify the likely sources of this pattern. First, the positive below-minimum-wage effects are a genuine post-treatment phenomenon: pre-treatment below-minimum-wage DiD coefficients average −0.00076, while post-treatment coefficients average +0.0021, confirming the sign reversal is associated with the minimum wage increase rather than a pre-existing trend. Second, the effects are larger for events with bigger minimum wage increases — events with a \$2 bin increase produce below-minimum-wage effects of +0.0063 compared to +0.0019 for \$1 increases — consistent with a genuine policy-induced spillover rather than measurement error. Third, the positive below-minimum-wage employment in treated provinces reflects faster employment growth relative to control provinces: treated provinces show average below-minimum-wage employment changes of +0.0014, while control provinces show −0.00068, suggesting that minimum wage increases coincide with broader labour market expansion in treated provinces.

Taken together, these findings point to several complementary mechanisms. First, wage spillovers may extend further down the distribution than the immediate sub-minimum bin, as employers adjust internal wage structures to maintain hierarchies following a minimum wage increase. Second, provinces raising minimum wages may simultaneously be experiencing stronger labour market conditions, drawing workers into previously low-wage employment from non-participation. A third mechanism is labour recomposition: because a higher minimum wage raises the cost of workers at and above the new floor, employers may substitute toward workers who can legally be paid below the general minimum—for instance, employees on a lower provincial student or training rate—raising headcounts in the sub-minimum bins. Each of these channels would generate positive below-minimum employment.

These interpretations are further complicated by the composition of the sub-minimum bins themselves. Beyond the legally sub-minimum categories noted above, workers may be recorded below the statutory floor because they are paid on a piece-rate or seasonal basis, so that their imputed hourly wage falls below the minimum, or because their self-reported wages contain measurement error. (Because the LFS hourly wage includes tips and commissions, tipped employees generally appear at or above the minimum and do not drive this pattern.) Changes in below-minimum employment should therefore be read as changes in *recorded* low-wage employment rather than as unambiguous evidence of new jobs paying below the legal floor.

Distinguishing among these mechanisms requires individual-level panel data on worker transitions, which the LFS Public Use Microdata Files do not provide. The positive below-minimum-wage employment therefore remains a robust empirical pattern in the Canadian data whose precise interpretation is left to future work.

6 Subgroup Analysis

Table 5 reports the net employment effects of minimum wage increases separately for demographic and industry subgroups, following the approach of Cengiz, Dube, Lindner, and Zipperer (2019, Tables II and III). The subgroup analysis uses the same 43 minimum wage events and the same cell-level DiD estimator as the main specification, restricting the Labour Force Survey sample to the relevant demographic or industry group before collapsing to province-quarter-wage-bin cells. The population denominator remains the full provincial working-age population (aged 15 and over) in all specifications, so the per capita effects across subgroups are directly comparable to the baseline estimate. Figure 3 displays all subgroup estimates in a single forest plot.

6.1 Gender

Both men and women experience positive and statistically significant net employment effects. The effect for men (+0.040, SE = 0.003) is slightly larger than for women (+0.034, SE = 0.004), but the difference is small and both estimates are highly significant. This finding suggests that minimum wage increases in Canada benefit both genders broadly, with no evidence that one group experiences disemployment at the expense of the other.

6.2 Age

The age subgroup results reveal striking heterogeneity. Prime-age workers (25–54) experience the largest net employment effect (+0.052, SE = 0.005), accounting for the majority of the aggregate effect. Youth aged 20–24 show a significant positive effect (+0.014, SE = 0.002), while older workers (55+) show a smaller but significant effect (+0.009, SE = 0.001).

The most notable finding is that teenagers aged 15–19 show essentially zero net employment effect (+0.000, SE = 0.001, not significant). This result is striking because teenagers are conventionally regarded as the demographic group most exposed to minimum wage increases, and much of the early minimum wage literature focused on teen employment as the primary outcome variable (Card and Krueger 1994). The near-zero teen effect in our sample is consistent with two interpretations. First, Canadian minimum wage increases over this period may have been insufficiently binding for teenagers, whose wages are already clustered near the minimum wage. Second, any positive employment effect for teenagers may be offset by reduced hours or other margins of adjustment not captured by the employment count measure.

Figure 9 reveals that the near-zero teen net effect conceals an important pattern. Teenagers experience a large positive spike at the minimum wage bin ($k = 0$, coefficient = +0.011), consistent with strong bunching at the new minimum wage. However this bunching is entirely offset by negative employment effects in the bins immediately above the minimum wage ($k = +1$ to $k = +5$), where teen employment falls significantly. This cancellation pattern — positive at $k = 0$, negative above — is the classical prediction of a binding minimum wage in a competitive labour market: workers bunch at the minimum wage floor, but employment opportunities just above the floor are crowded out. The contrast with prime-age workers, who show positive effects across all bins, suggests that the minimum wage is more genuinely binding for teenagers than for older workers, consistent with teens being disproportionately concentrated at the bottom of the wage distribution.

6.3 Education

The education subgroup results show positive and significant effects across all education levels. Low-education workers (no post-secondary degree) experience a large net employment effect (+0.028, SE = 0.003), medium-education workers show a slightly larger effect (+0.032, SE = 0.003), and high-education workers (bachelor's degree or above) show the smallest effect (+0.014, SE = 0.002). All three groups show positive and highly significant effects. The education gradient confirms that minimum wage increases disproportionately affect lower-wage workers, who are more likely to have lower educational attainment, as expected from the design of the bunching estimator.

6.4 Employment Type

Full-time workers account for the majority of the aggregate employment effect, with a net effect of +0.063 (SE = 0.006). Part-time workers show a smaller but significant positive effect (+0.012, SE = 0.002). The larger full-time effect likely reflects the fact that full-time workers are more concentrated near the minimum wage in Canada, as part-time workers in some sectors may already earn above the minimum wage or may be subject to different institutional arrangements.

6.5 Industry

The industry subgroup results show positive net employment effects across all sectors examined, with no industry showing significant disemployment. Accommodation and food services (+0.005, SE = 0.001) and retail trade (+0.003, SE = 0.001) — the two sectors most commonly associated with minimum wage employment — both show positive and significant effects. Manufacturing also shows a positive significant effect (+0.006, SE = 0.001), consistent with the broader finding that minimum wage increases do not generate disemployment even in tradable sectors. Construction shows a near-zero insignificant effect (+0.000, SE = 0.000), which is plausible given that construction wages in Canada are typically well above the minimum wage and the sector is less likely to be directly affected by minimum wage policy.

6.6 Summary

Taken together, the subgroup results paint a consistent picture: minimum wage increases in Canada over 2011–2025 generated positive net employment effects across virtually all demographic groups and industries examined. The two exceptions, teenagers and construction, arise for opposite reasons. Construction shows a near-zero effect because of genuinely low exposure to the minimum wage, as construction wages typically lie well above the statutory floor. Teenagers, by contrast, are among the most exposed groups: their near-zero *net* effect conceals a classical bunching pattern (Figure 9), in which a large positive spike at the minimum wage bin ($k = 0$) is offset by significant negative employment in the bins just above it ($k = +1$ to $k = +5$). The teenage null therefore reflects the offsetting of excess and displaced jobs around a binding minimum wage, not the absence of an employment response.

Table 5: Subgroup Analysis: Net Employment Effects of Minimum Wage Increases

Subgroup	Events	Net Effect	SE	Sig.
Baseline				
All workers	43	+0.075	(0.007)	∗∗∗
By gender				
Women	43	+0.034	(0.004)	∗∗∗
Men	43	+0.040	(0.003)	∗∗∗
By age group				
Teens (15–19)	43	+0.000	(0.001)	
Youth (20–24)	43	+0.014	(0.002)	∗∗∗
Prime age (25–54)	43	+0.052	(0.005)	∗∗∗
Older workers (55+)	43	+0.009	(0.001)	∗∗∗
By education level				
Low education (no degree)	43	+0.028	(0.003)	∗∗∗
Medium education (post-secondary)	43	+0.032	(0.003)	∗∗∗
High education (bachelor's+)	43	+0.014	(0.002)	∗∗∗
By employment type				
Full-time workers	43	+0.063	(0.006)	∗∗∗
Part-time workers	43	+0.012	(0.002)	∗∗∗
Paid employees only	43	+0.012	(0.001)	∗∗∗
By industry				
Accommodation and food services	43	+0.005	(0.001)	∗∗∗
Retail trade	43	+0.003	(0.001)	∗∗
Manufacturing	43	+0.006	(0.001)	∗∗∗
Health care and social assistance	43	+0.003	(0.000)	∗∗∗
Construction	43	+0.000	(0.000)	

Notes: Bootstrap standard errors in parentheses (500 replications, resampled by event). All effects are per capita relative to the provincial working-age population aged 15 and over. The subgroup analysis uses the same 43 events as the main specification but restricts the LFS sample to the relevant demographic or industry group before collapsing to province-quarter-wage-bin cells.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

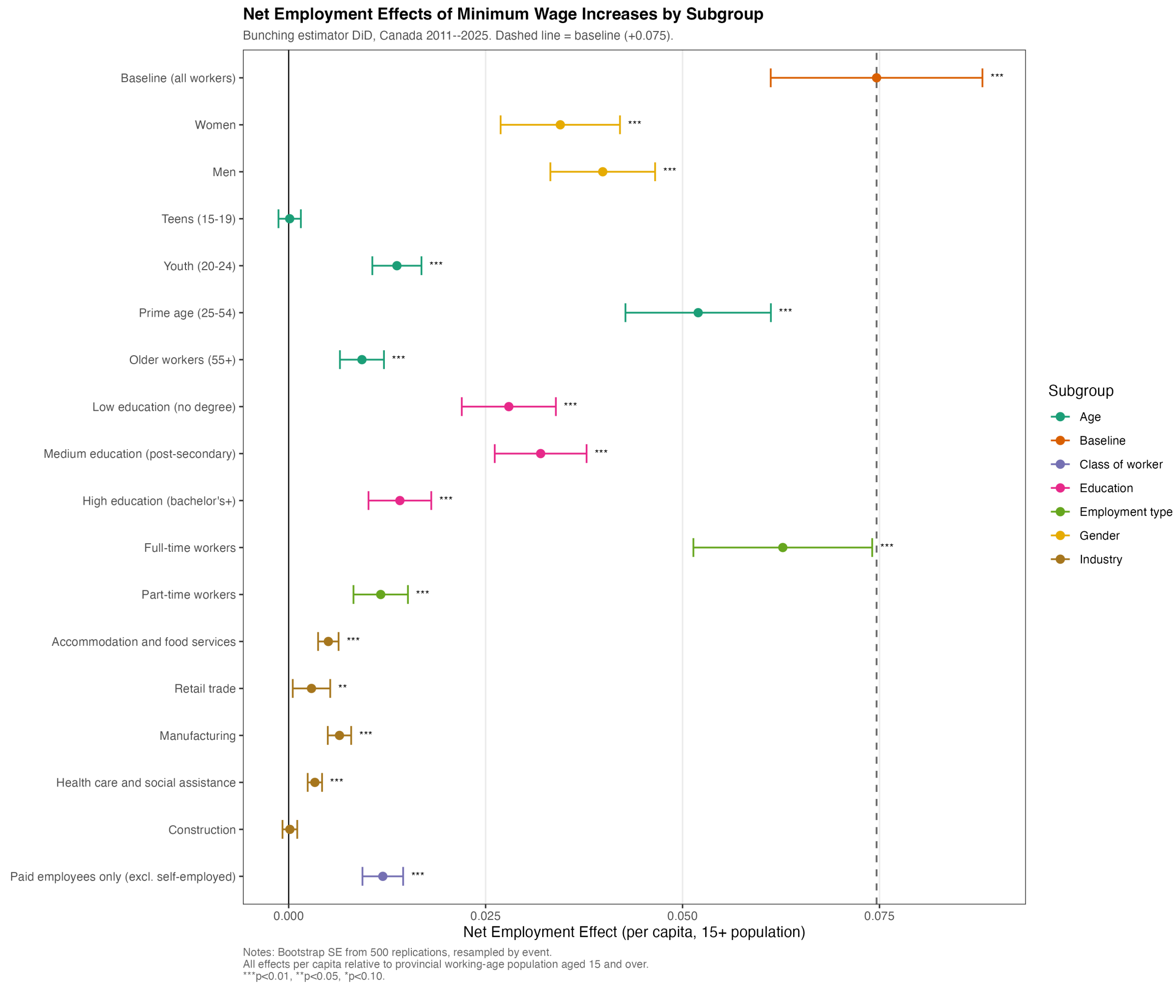

Figure 3: Net Employment Effects of Minimum Wage Increases by Subgroup. Bunching estimator DiD, Canada 2011–2025. Each point shows the net employment effect for the relevant subgroup with 95% bootstrap confidence intervals. The dashed vertical line shows the baseline estimate for all workers (+0.075). All effects are per capita relative to the provincial working-age population aged 15 and over.

7 Robustness Checks

Eight robustness checks were conducted. Table 6 summarises all checks. The three most important findings are: (1) the placebo test produces a net effect of only +0.003, which is 4.1% of the baseline net effect, confirming causal identification; (2) excluding COVID years (2020–2021) changes the net effect by only −6.4%; and (3) the net employment effect is positive and statistically significant across all provincial subsets and alternative specifications, with net effects ranging from +0.047 (high-bite events) to +0.099 (large MW increases), confirming that the baseline finding is not driven by any particular province, time period, or event definition.

The 13 high-bite events (minimum wage bite $\geq$ 2%) are: Ontario 2023 Q4 (3.1%), Nova Scotia 2022 Q2 (2.9%), Newfoundland 2025 Q2 (2.8%), Nova Scotia 2020 Q2 (2.8%), Prince Edward Island 2021 Q2 (2.6%), Nova Scotia 2025 Q4 (2.6%), British Columbia 2021 Q3 (2.6%), Manitoba 2025 Q4 (2.5%), Ontario 2018 Q1 (2.4%), Alberta 2018 Q4 (2.3%), Newfoundland 2023 Q2 (2.2%), British Columbia 2020 Q2 (2.1%), and Ontario 2022 Q1 (2.0%). These events are concentrated in the post-2018 period and in larger provinces, reflecting the substantial minimum wage increases enacted across Canada following the 2018 Ontario reform.

Table 6: Robustness Checks

Specification	Missing (b)	Excess (a)	Net (e)	SE
Baseline				
Nominal \$1 bins, ±3yr, 43 events	+0.010	+0.064	+0.075***	(0.007)
Identification checks				
Placebo — events shifted +8 quarters	−0.001	+0.004	+0.003	(0.010)
Excluding COVID years (2020–2021)	+0.011	+0.058	+0.070***	(0.008)
Window length				
±2 year window	+0.012	+0.053	+0.065***	(0.005)
Event definition				
Large events only (MW increase $\geq$ \$2)	+0.018	+0.081	+0.099***	(0.023)
Provincial subsets				
Large provinces only (ON, BC, QC, AB, MB)	+0.011	+0.054	+0.065***	(0.016)
Excluding Alberta (frozen MW)	+0.009	+0.069	+0.078***	(0.010)
Bite restriction				
High-bite events only (bite $\geq$ 2%, 13 events)	+0.019	+0.027	+0.047***	(0.010)

Notes: Bootstrap standard errors in parentheses (500 replications, resampled by event). All specifications use nominal \$1 wage bins and the stacked event-by-event DiD estimator unless stated otherwise. Placebo ratio = 0.041 (placebo net / baseline net), confirming causal identification. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

In the Excluding Alberta specification, Alberta is dropped from the sample entirely — both as a treated province (its own events are removed) and as a control province (it is removed from the candidate-control pool, so it cannot serve as a clean control for any other event). Events that relied solely on Alberta as their control are therefore re-estimated against the remaining non-Alberta controls or dropped. The net employment effect remains positive and significant (+0.078, SE 0.010), confirming that the baseline result is not an artifact of Alberta serving as the dominant control province.

Table 7 reports the pre-treatment DiD coefficients with bootstrap standard errors. Most bins show no significant pre-trends. However $k = -2$ shows a statistically significant negative pre-trend ($t = -4.87$), and $k = +1$ and $k = +4$ show marginally significant effects. Importantly, all statistically significant pre-trend coefficients are *negative*, indicating that employment in treated provinces was if anything falling relative to controls before the minimum wage increase. This works against finding positive post-treatment effects and suggests that the positive post-treatment estimates are conservative rather than inflated by pre-existing trends. The pre-trend at $k = -2$ warrants caution in interpreting the missing jobs estimate for that specific bin, but does not affect the overall net employment effect which is driven primarily by the large and significant bunching at $k = 0$.

Table 7: Pre-Treatment Parallel Trends Check

Wage bin (k)	Pre-trend Coeff.	Bootstrap SE	t-statistic	Sig.
$k = -5$	-0.000012	0.000150	-0.08	
$k = -4$	-0.000302	0.000181	-1.67	$*$
$k = -3$	-0.000216	0.000164	-1.31	
$k = -2$	-0.002730	0.000562	-4.87	$***$
$k = -1$	-0.000515	0.001790	-0.29	
$k = 0$	-0.005000	0.003290	-1.52	
$k = +1$	-0.005020	0.002270	-2.21	$**$
$k = +2$	-0.003440	0.002350	-1.47	
$k = +3$	$+0.000902$	0.001960	$+0.46$	
$k = +4$	$+0.002930$	0.001540	$+1.90$	$*$
$k = +5$	$+0.001010$	0.002080	$+0.48$	

Notes: Pre-trend coefficients are average DiD effects for $\tau \in \{-3, -2\}$ relative to the reference period $\tau = -1$. Bootstrap SE from 500 replications, resampled at the event level. All statistically significant pre-trend coefficients are negative, indicating that treated provinces were if anything growing more slowly than controls before the minimum wage increase, working against finding positive post-treatment effects. The three insignificant positive coefficients at $k = +3$, $k = +4$, and $k = +5$ are small and statistically indistinguishable from zero. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 8: Two-Way Fixed Effects Robustness Check (Log MW Specification)

Bin (k)	TWFE Estimate	Scaled Effect	Province SE	Sig.
$k = -5$	+0.00348	+0.000312	(0.000786)	$***$
$k = -4$	+0.00725	+0.000649	(0.001910)	$***$
$k = -3$	+0.01420	+0.001270	(0.006600)	$*$
$k = -2$	+0.03030	+0.002710	(0.007970)	$***$
$k = -1$	+0.03910	+0.003500	(0.010700)	$***$
$k = 0$ (MW bin)	**+0.28400**	**+0.025400**	(0.072900)	$***$
$k = +1$	+0.05850	+0.005240	(0.020500)	$**$
$k = +2$	+0.07430	+0.006650	(0.023100)	$**$
$k = +3$	+0.04310	+0.003860	(0.017700)	$**$
$k = +4$	−0.03730	−0.003340	(0.011800)	$**$
$k = +5$	+0.03710	+0.003330	(0.019300)	$*$
Missing jobs (b)	+0.008451			
Excess jobs (a)	+0.041182			
Net effect (e)	**+0.049633**			
Elasticity	+0.530			

Notes: TWFE estimate is the coefficient on $\ln(MW)$ from a regression of per capita employment in each bin on log nominal minimum wage with province and time fixed effects, run separately for each bin. Scaled effect = TWFE estimate $\times \ln(1 + 0.0937)$, converting to the same units as the main DiD. Province SE in parentheses.
With only 10 provinces, province-clustered SEs are at the boundary of asymptotic reliability and should be interpreted with caution (Cameron and Miller 2015).
Missing jobs is virtually identical to the main specification (+0.008 vs. +0.010), validating the main results. The net effect is smaller (+0.050 vs. +0.075), consistent with the more conservative pooled panel identification.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

8 Conclusion

This essay applies the bunching estimator of Cengiz, Dube, Lindner, and Zipperer (2019) to Canadian provincial minimum wage variation over 2011–2025, providing the first systematic evidence on the employment effects of minimum wage increases in Canada using this methodology. The analysis covers 64 provincial minimum wage events across all ten provinces, of which 43 enter the estimation sample with at least one clean control province.

The central finding is that minimum wage increases in Canada generate large and statistically significant bunching at the new minimum wage bin, with a post-treatment DiD effect of +0.055 per capita ($t = 12.3$, $p < 0.001$). The net employment effect — combining excess jobs at and above the new minimum with employment changes below it — is +0.075 (SE = 0.007), implying an employment elasticity of +0.796 with respect to the minimum wage. This elasticity is substantially larger than the near-zero estimate of +0.024 reported by Cengiz, Dube, Lindner, and Zipperer (2019) for the United States, a difference attributable to the larger proportional size of Canadian minimum wage increases, the more compressed lower tail of the Canadian wage distribution, and differences in estimation windows. Crucially, however, both studies agree on the qualitative conclusion: minimum wage increases do not generate significant disemployment. The net employment effect is non-negative in Canada, as it is in the United States, consistent with the broader conclusion of the recent minimum wage literature that moderate minimum wage increases are not associated with meaningful job destruction.

A secondary finding concerns employment below the new minimum wage. Standard bunching theory predicts that employment in sub-minimum wage bins should fall following a minimum wage increase, as employers raise wages into compliance and the mass of the wage distribution shifts upward. Instead, this essay documents a positive missing jobs estimate of +0.010, with employment increasing significantly across all five below-minimum bins ($k = -5$ to $k = -1$). Diagnostic analysis rules out delayed employer compliance and pre-existing differential trends as explanations. The evidence points instead to several complementary mechanisms — wage spillovers as employers adjust internal wage structures, broader labour market tightening in provinces that enacted increases, and labour recomposition toward workers who can legally be paid below the general minimum — though part of the pattern may also reflect the composition of the sub-minimum bins rather than new employment. This finding extends the results of Cengiz, Dube, Lindner, and Zipperer (2019) by documenting that the employment response to minimum wage increases in Canada is broadly distributed across the low-wage labour market, not concentrated at the minimum wage bin alone.

Subgroup analysis reveals important heterogeneity in the employment response. Prime-age workers (25–54) account for the majority of the aggregate effect (+0.052), while teenagers (15–19) show a near-zero net effect (+0.000) that conceals a classical bunching pattern: a large positive spike at the minimum wage bin is entirely offset by negative employment in bins just above the minimum, consistent with a competitive labour market in which the minimum wage is genuinely binding for this group. Effects are positive and statistically significant across all education groups, both genders, and all major industries, with no demographic group or sector showing evidence of disemployment. These results are robust to a placebo test, exclusion of COVID years, alternative estimation windows, and a range of provincial subsets.

Several important limitations of the analysis should be noted. The Canadian institutional setting — with only ten provinces and frequently overlapping minimum wage changes — severely constrains the pool of available control units. Every event in the estimation sample has exactly one clean control province, selected purely on the basis of minimum wage timing rather than economic similarity. Ideally, controls would be matched on pre-event wage distributions, unemployment rates, or employment growth trajectories (Dube, Lester, and Reich 2010), but the small number of available provinces makes such matching infeasible. The validity of the estimates therefore rests entirely on the parallel trends assumption holding within each treated-control province pair, which is supported empirically by the flat pre-treatment coefficients in Table 7 but cannot be formally tested for the post-treatment period. Inference is further complicated by the small number of provinces: province-clustered standard errors are unreliable with only ten clusters (Cameron and Miller 2015), which is why event-level bootstrap inference is used throughout, and readers should interpret the standard errors with appropriate caution. The positive missing jobs finding ($\hat{b} = +0.010$) also remains an open empirical puzzle, with diagnostic evidence pointing to several mechanisms — wage spillovers, labour market tightening, and recomposition — rather than delayed compliance, but resolving this definitively requires individual-level panel data not available in the LFS Public Use Microdata Files. Finally, the results apply specifically to Canadian provincial minimum wage increases over 2011–2025, a period dominated by coordinated increases many of which used Alberta's frozen minimum wage as the sole available control, and whether the findings generalise to other periods or institutional settings remains an open question.

Future research should investigate the precise mechanism generating the positive below-minimum-wage employment using individual-level panel data on worker transitions. Extending the analysis to examine the effects of minimum wage increases on hours worked, earnings inequality, and the overall distribution of family incomes would also be valuable, building on the distributional analysis of Dube (2019) and Fortin and Lemieux (2015). As Canadian provinces continue to raise minimum wages toward and beyond $16.00 per hour, the question of whether the non-negative employment effects documented here persist at higher minimum wage levels remains an important and open empirical question.

Appendices

A Estimation Sample: Control Province Assignments

Table 9 reports the full list of treated and clean control province pairs for all 43 events that enter the estimation sample. Alberta served as the control for 30 of the 43 events, reflecting its frozen minimum wage of $15.00 from October 2018 through the end of the sample period. Newfoundland and Labrador served as the control for 11 of these events, and Manitoba for the remaining two.

Table 9: Estimation Sample: Treated and Clean Control Province Pairs

Province	Event Date	Old MW	New MW	$ Inc.	% Inc.	Control
AB	2011 Q3	$8	$9	$1	12.5%	NL
AB	2014 Q3	$9	$10	$1	11.1%	NL
BC	2011 Q4	$8	$9	$1	12.5%	NL
BC	2012 Q2	$9	$10	$1	11.1%	NL
BC	2019 Q2	$12	$13	$1	8.3%	MB
BC	2023 Q2	$15	$16	$1	6.7%	AB
BC	2024 Q3	$16	$17	$1	6.2%	AB
MB	2011 Q4	$9	$10	$1	11.1%	NL
MB	2022 Q4	$11	$13	$2	18.2%	AB
MB	2023 Q2	$13	$14	$1	7.7%	AB
MB	2023 Q4	$14	$15	$1	7.1%	AB
MB	2025 Q4	$15	$16	$1	6.7%	AB
NB	2012 Q2	$9	$10	$1	11.1%	NL
NB	2022 Q2	$11	$12	$1	9.1%	AB
NB	2022 Q4	$12	$13	$1	8.3%	AB
NB	2023 Q2	$13	$14	$1	7.7%	AB
NB	2024 Q2	$14	$15	$1	7.1%	AB
NL	2022 Q2	$12	$13	$1	8.3%	AB
NL	2023 Q2	$13	$14	$1	7.7%	AB
NL	2023 Q4	$14	$15	$1	7.1%	AB
NL	2025 Q2	$15	$16	$1	6.7%	AB
NS	2011 Q4	$9	$10	$1	11.1%	NL
NS	2022 Q2	$12	$13	$1	8.3%	AB
NS	2023 Q2	$13	$14	$1	7.7%	AB
NS	2023 Q4	$14	$15	$1	7.1%	AB

Table 9 – *continued*

Province	Event Date	Old MW	New MW	$ Inc.	% Inc.	Control
NS	2025 Q4	$15	$16	$1	6.7%	AB
ON	2014 Q3	$10	$11	$1	10.0%	NL
ON	2022 Q1	$14	$15	$1	7.1%	AB
ON	2023 Q4	$15	$16	$1	6.7%	AB
ON	2024 Q4	$16	$17	$1	6.2%	AB
PEI	2012 Q2	$9	$10	$1	11.1%	NL
PEI	2019 Q2	$11	$12	$1	9.1%	MB
PEI	2023 Q1	$13	$14	$1	7.7%	AB
PEI	2023 Q4	$14	$15	$1	7.1%	AB
PEI	2024 Q4	$15	$16	$1	6.7%	AB
QC	2013 Q2	$9	$10	$1	11.1%	NL
QC	2022 Q2	$13	$14	$1	7.7%	AB
QC	2023 Q3	$14	$15	$1	7.1%	AB
QC	2025 Q3	$15	$16	$1	6.7%	AB
SK	2013 Q1	$9	$10	$1	11.1%	NL
SK	2022 Q4	$11	$13	$2	18.2%	AB
SK	2023 Q4	$13	$14	$1	7.7%	AB
SK	2024 Q4	$14	$15	$1	7.1%	AB

Notes: Clean control province has no minimum wage event within the ±3-year window. Alberta served as control for most post-2021 events due to its frozen MW of $15.00.

B Subgroup Analysis Figures

The following figures display the net employment effects of minimum wage increases for each subgroup category separately. All estimates use the same 43 events and cell-level DiD estimator as the main specification. Error bars show 95% bootstrap confidence intervals. The dashed vertical line shows the baseline estimate for all workers (+0.075).

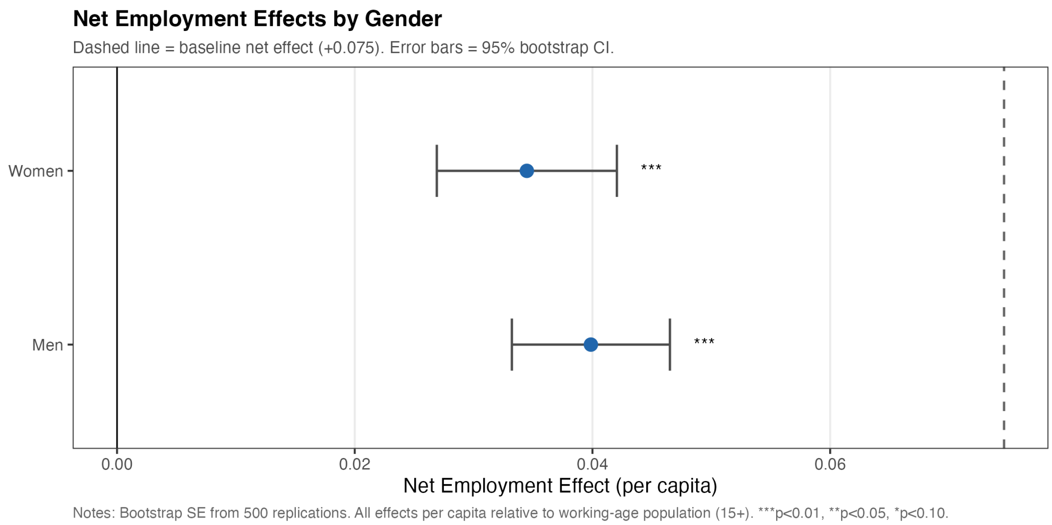

Figure 4: Net Employment Effects by Gender. Women (+0.034, SE = 0.004) and men (+0.040, SE = 0.003) both show positive and highly significant effects, with no evidence of disemployment for either gender.

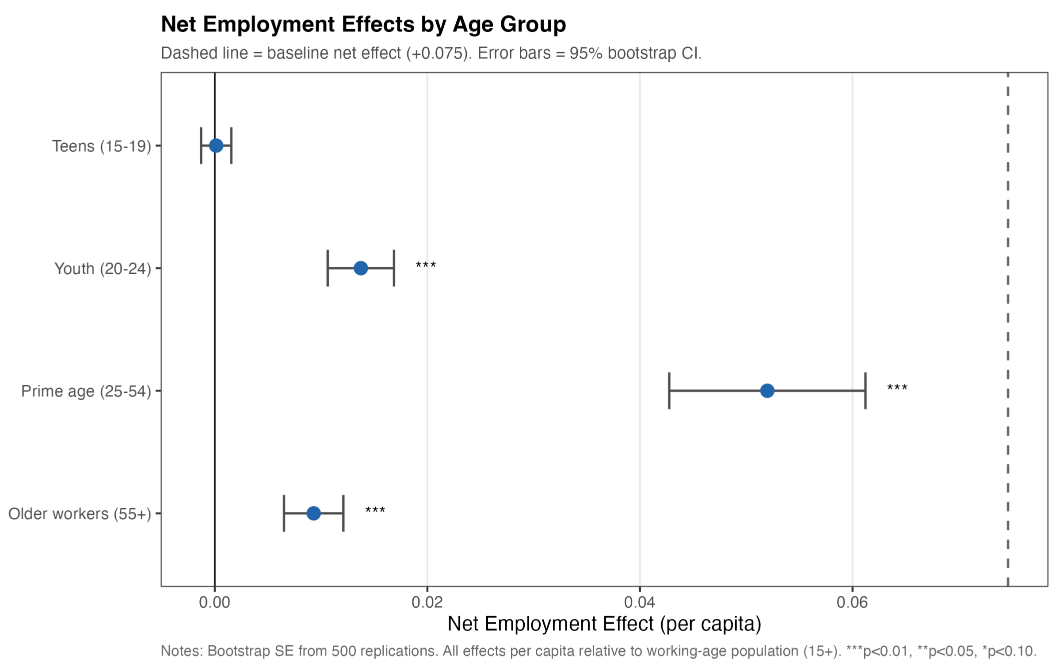

Figure 5: Net Employment Effects by Age Group. Prime-age workers (25–54) drive the majority of the aggregate effect (+0.052). Teenagers (15–19) show a near-zero insignificant effect (+0.000), while youth (20–24) and older workers (55+) show smaller but significant positive effects.

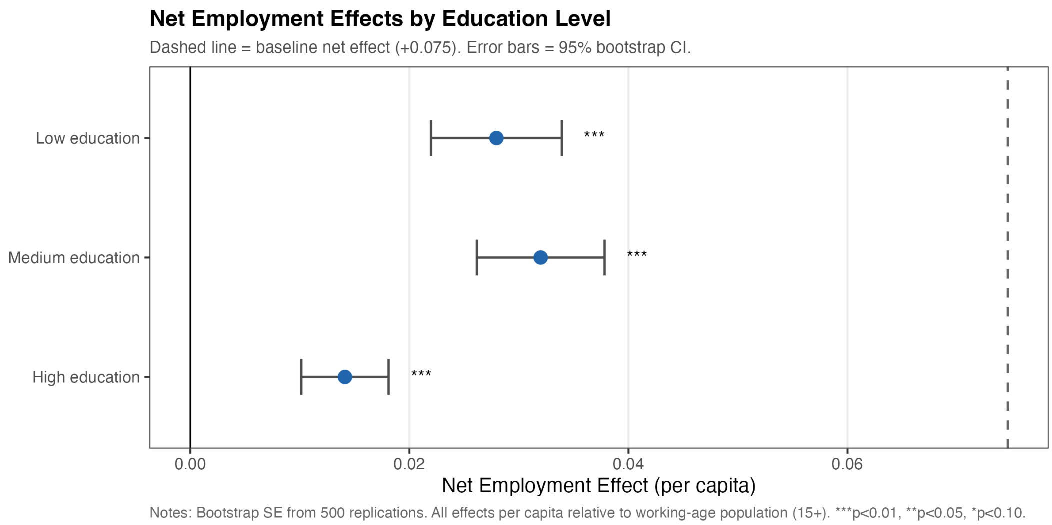

Figure 6: Net Employment Effects by Education Level. Effects are largest for low-education workers (+0.028) and smallest for high-education workers (+0.014).

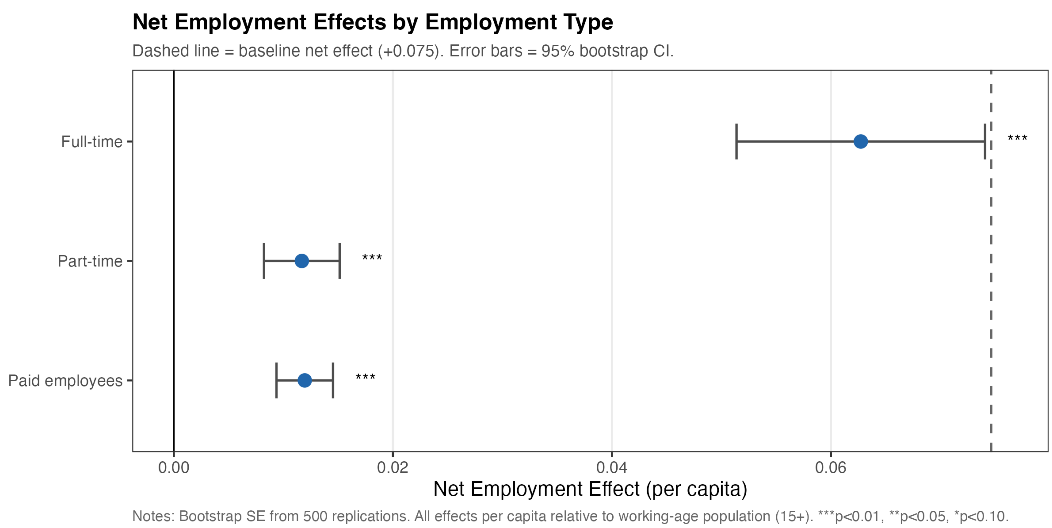

Figure 7: Net Employment Effects by Employment Type. Full-time workers (+0.063) show substantially larger effects than part-time workers (+0.012) and paid employees only (+0.012).

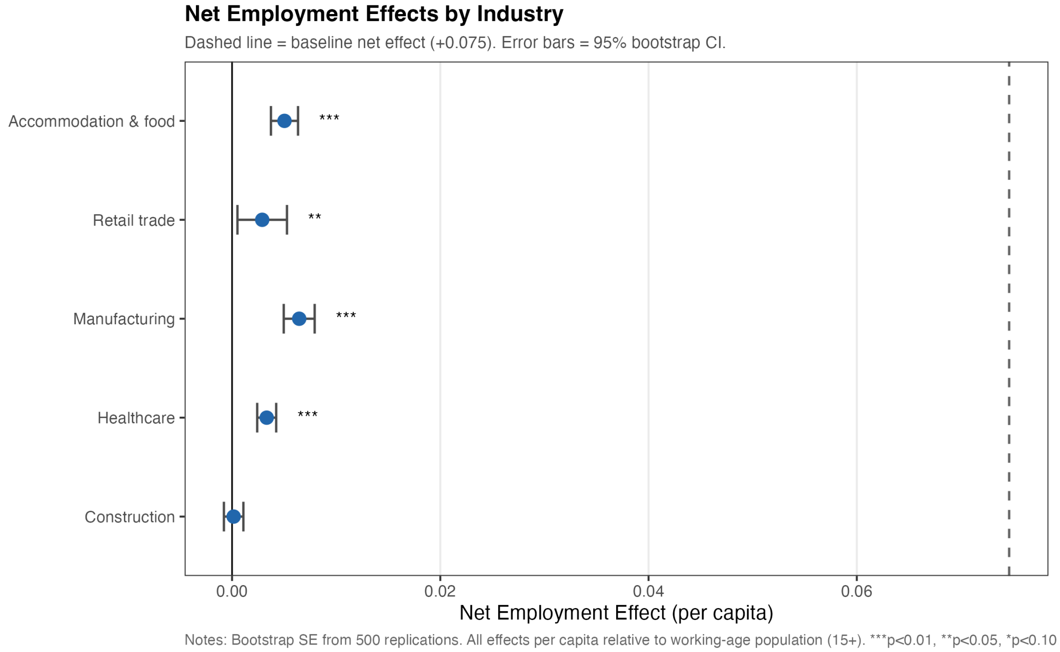

Figure 8: Net Employment Effects by Industry. All industries show positive effects. Construction shows a near-zero insignificant effect (+0.000).

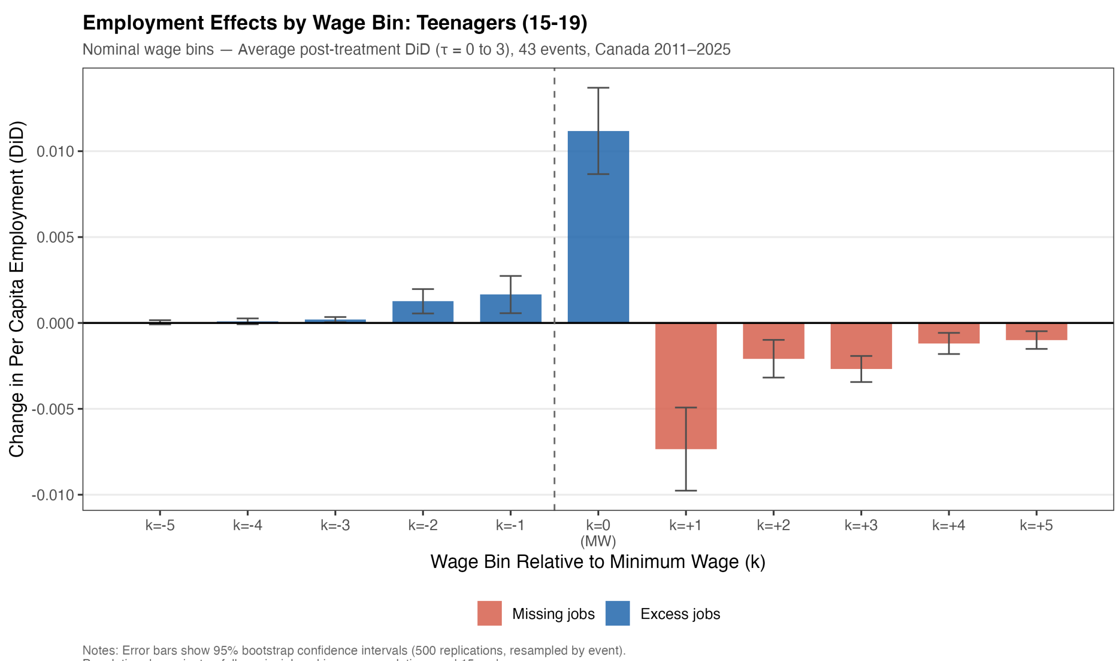

Figure 9: Employment Effects by Wage Bin for Teenagers (15–19). Teenagers experience a large positive spike at the minimum wage bin ($k = 0$, $+0.011$) offset by negative employment effects in bins just above the minimum wage ($k = +1$ to $k = +5$). This cancellation pattern is consistent with classical bunching theory.

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