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ABSTRACT

The Wage Effects of Restricting Temporary Foreign Workers: Evidence from Canada's 2014 TFWP Reforms

This study examines the labor market impacts of Canada's 2014 reforms to the Temporary Foreign Worker Program (TFWP), which introduced stricter limits on hiring low-skilled foreign workers. Using a difference-in-differences framework with Labour Force Survey data from 2005 to 2019, we compare wage outcomes for domestic workers in TFW-intensive occupations to those in similar low-skill jobs unaffected by the reforms. Robustness checks— including event study analysis, propensity score matching, placebo tests, and additional validation with Census data (2006, 2011, 2016)—consistently show that the reforms led to a statistically significant increase in wages for affected domestic workers. The estimated impact ranges from 3.7% to 4.5%, suggesting that restricting access to temporary foreign labor modestly improves wage outcomes for low-wage Canadians. These findings offer timely insights amid renewed policy efforts to tighten immigration, highlighting the potential benefits of targeted reforms while cautioning against broader restrictions that could undermine labor market responsiveness and sectoral needs.

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1. Introduction

Canada has increasingly relied on temporary foreign workers (TFWs) to address persistent labor shortages, especially in sectors like agriculture, accommodation, and food services. Although TFWs represent a small proportion of Canada's overall workforce—approximately 4 percent in 2019—they constitute significantly larger shares in specific sectors: about 15 percent in agriculture and roughly 10 percent in accommodation and food services (Statistics Canada 2022). From 2011 to 2021, the total number of TFWs surged dramatically from around 356,000 to 845,000, reflecting their growing importance within Canada's labor market (Statistics Canada 2024).

Initially established in 1973 as a mechanism to address short-term, skilled-labor shortages, Canada's Temporary Foreign Worker Program (TFWP) expanded in 2002 to include low-skilled occupations, significantly broadening its scale and impact (Fudge and MacPhail 2009; Gross and Schmitt 2012). However, over the following decade, concerns emerged regarding employers' misuse of the program as a permanent source of cheaper and flexible labor, potentially displacing domestic workers and exposing TFWs to precarious conditions and exploitation (Byl 2010; Marsden 2011; Shantz 2015; Robillard et al. 2018). Criticisms intensified after the 2008–2009 recession, prompting debates about whether the TFWP genuinely filled labor shortages or inadvertently undermined wages and employment conditions for Canadian workers (Faraday 2014).

In response, the Canadian government enacted comprehensive reforms to the TFWP in 2014, imposing stricter labor market tests, higher administrative fees, shortened permit durations, tighter hiring quotas, and increased compliance enforcement (Bandali 2014; Government of Canada 2015). By increasing the program's costs and administrative hurdles, the reforms sought to ensure that TFWs would be recruited only when domestic labor shortages were acute rather than as a permanent substitute for local labor. These changes led

to a decline in TFW admissions under the Labor Market Impact Assessment (LMIA)-required streams from 103,910 in 2013 to approximately 46,060 by 2017 (Government of Canada 2022). Still, total TFW numbers continued to climb, mainly driven by exponential growth in the LMIA-exempt International Mobility Program (O'Donnell and Skuterud 2022).

Recent policy discussions surrounding the TFWP reforms have garnered renewed interest, driven by sustained labor shortages across the Canadian economy (BDC 2021; Morissette 2022) and evolving immigration policies aimed at scaling back reliance on temporary foreign workers (Government of Canada 2024; Bongiorno 2024). Restricting TFW access could improve wages and employment opportunities for low-skilled domestic workers facing direct competition from foreign labor (Borjas 2003; Brochu, Gross, and Worswick 2020). However, employers highlight the critical role of TFWs in filling sector-specific or regional labor gaps, cautioning that overly stringent limitations might exacerbate skill shortages and undermine economic competitiveness (Falconer 2020). Additionally, recent analyses suggest that expanded immigration policies have inadvertently intensified labor shortages by simultaneously boosting aggregate labor demand (Fortin 2025). Thus, policy discussions underscore the need for balanced, targeted reforms that safeguard domestic workers while maintaining economic dynamism.

Despite the robust policy discourse, empirical evidence on the causal wage effects of Canada's 2014 TFWP reforms remains limited. This study fills that gap by using nationally representative data from the Labour Force Survey (LFS) and Census to quantify wage changes among domestic workers in low-skilled, TFW-intensive occupations following the 2014 reforms. Applying robust econometric techniques—including difference-in-differences, event study analyses, and propensity score matching—our findings demonstrate a modest yet statistically significant wage increase (approximately 3–4 percent) attributable to the reforms.

These results offer timely evidence supporting targeted policy interventions while highlighting the complexities of comprehensive immigration reform.

The remainder of this paper proceeds as follows. Section 2 reviews the existing literature. Section 3 describes our estimation strategies and data. Section 4 presents empirical findings and robustness checks. Section 5 discusses policy implications and study limitations. Section 6 concludes.

2. Literature Review

Research on Canada's Temporary Foreign Worker Program (TFWP) has long focused on its implications for domestic labor. While TFWs help address labor shortages in sectors such as agriculture, accommodation, and food services, critics argue that their growing presence may harm local workers—particularly at the lower end of the labor market (Borjas 2003; Brochu, Gross, and Worswick 2020; Gross 2010). Basic economic theory supports this concern, suggesting that the influx of foreign labor could exert downward pressure on wages and employment if TFWs are highly substitutable with domestic workers (Borjas 2003; Worswick 2013). Empirical research in Canada offers partial support for this view. For instance, Brochu, Gross, and Worswick (2020) and Cardoso et al. (2023) find that greater reliance on low-skilled TFWs can modestly suppress domestic wages, largely because employers use foreign labor as a cost-saving strategy (Gross and Schmitt 2012; Foster and Barnetson 2017).

At the same time, not all research concludes that TFWs have negative effects. In some contexts, temporary foreign workers may complement native-born labor by alleviating production bottlenecks, thereby enhancing productivity and potentially raising domestic wages (Peri and Sparber 2009; Peri 2012). In smaller communities or specialized roles, TFWs may stimulate local economic activity without directly competing with domestic labor (Gross 2010). Gesualdi-Fecteau (2016) further complicates this picture by showing that

employment relationships involving TFWs often involve multiparty arrangements, with labor intermediaries playing a central role in shaping wages and working conditions—resulting in uneven labor market outcomes rather than uniformly downward wage pressure.

A separate strand of research has explored the longer-term implications of temporary labor migration, particularly Canada's shift toward a two-step immigration system in which temporary status increasingly serves as a pathway to permanent residency (O'Donnell and Skuterud 2022; Ci, Hou, and Morissette 2018; Hou and Bonikowska 2016). Ci, Hou, and Morissette (2018) document improved labor market outcomes for temporary foreign workers who transition to permanent residency, highlighting the broader economic benefits of integration. Similarly, Hou and Bonikowska (2016) find earnings advantages among former skilled TFWs who later became permanent residents, suggesting that early labor market experience under temporary status shapes longer-term outcomes.

The academic and policy debate surrounding TFWs intensified following Canada's 2014 reforms, which aimed explicitly at reducing employer dependence on low-skilled TFWs through enhanced labor market testing, higher administrative costs, stricter hiring quotas, and strengthened enforcement measures (Bandali 2014; Government of Canada 2015). Advocates of the reforms argued that they would improve employment opportunities and wages for domestic workers by limiting employers' reliance on a cheaper, more vulnerable foreign workforce (Shantz 2015). A substantial body of research had already documented concerns that employers were using the program not as a last resort, but as a long-term staffing strategy, often without adequately demonstrating genuine labor shortages (Foster 2012; Prokopenko and Hou 2018; Gross 2010; Brochu, Gross, and Worswick 2020). Others emphasized that lower-skilled TFWs frequently faced exploitative conditions, including substandard housing, limited access to healthcare and employment benefits, and heightened vulnerability to abuse—often exacerbated by weak enforcement mechanisms and restrictive

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work permits (Byl 2010; Marsden 2011; Fudge and MacPhail 2009; Foster and Barnetson 2017; Robillard et al. 2018; Gesualdi-Fecteau 2016). These concerns helped shape the political momentum behind the 2014 reforms, which were framed not only as labor market interventions but also as responses to mounting human rights and regulatory challenges. Nonetheless, critics warned that overly restrictive policies could constrain firms' ability to meet real labor demands and risk unintended consequences in sectors heavily reliant on TFWs (Falconer 2020).

Despite considerable policy attention, few studies have rigorously assessed the causal wage impacts of the 2014 reforms. Much of the existing literature offers descriptive accounts of TFW vulnerabilities or theoretical discussions of their labor market effects, without rigorous causal estimates. For example, Byl (2010) and Robillard et al. (2018) highlight precarious working conditions among TFWs but do not evaluate impacts on domestic wage outcomes. Worswick (2013) presents a theoretical model analyzing four major TFWP reforms but underscores the need for empirical validation. One notable exception is Brochu, Gross, and Worswick (2020), who develop an efficiency-wage model to examine the effects of TFWs on domestic wages and employment. While they find evidence of adverse effects, they acknowledge limitations in establishing causal inference and call for further research using credible identification strategies.

Against this backdrop, the present study contributes new causal evidence on the wage impacts of the 2014 TFWP reforms for low-skilled Canadian workers. By using large-scale data and applying quasi-experimental methods, the analysis sheds light on whether and how restricting temporary foreign labor influences domestic wage dynamics—offering policy-relevant insights amid ongoing debates on immigration, labor shortages, and worker protection.

3.   Estimation Framework and Data

This section outlines the empirical strategies and datasets used to assess the causal impact of the 2014 TFWP reforms on domestic workers' wages. The main analysis uses Canadian Labor Force Survey Data from 2005 to 2019. We employ five complementary approaches: (1) difference-in-differences (DiD), (2) an event study design, (3) propensity score matching (PSM), (4) a placebo test, and (5) validation using an alternative data source.

3.1.   Estimation Strategies

Difference-in-Differences (DiD). The central hypothesis is that tightening restrictions on TFWs in 2014 conferred wage benefits on domestic workers in certain low-skilled, TFW-intensive occupations. We define these occupations as "treated" and compare them to similar low-skilled occupations that are unaffected by the reforms (the "control" group). By contrasting outcomes pre- and post-reform for both groups, the DiD estimator captures the net policy effect. Specifically, we estimate:

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_i + \beta_3 Treat_i \cdot Post_i + \gamma X_{it} + \delta_p + \pi_t + Z_{rt} + \varepsilon_{it} \qquad (1)$$

where Y_i refers to the logarithm of the weekly wages of domestic worker i in year t. The variable $Treat_i$ indicates whether the worker belongs to a TFW-stream occupation, $Post_i$ denotes whether the observation occurs in the post-reform period (years>=2014). The interaction term ($Treat_i \cdot Post_i$) is the main parameter of interest and its coefficient β_3 (i.e., the DiD estimator) captures the causal impact of the TFWP reforms on wages for the treated relative to the control group. The vector X_{it} is a vector of demographic and labor-related characteristics. We also include the year (π_t), provincial (δ_p), and municipality (Z_r) fixed effects to account for time-varying and region-specific unobservables, and ε_{it} is idiosyncratic the error term.

Event Study Design (ESD). The DiD approach relies on the assumption that both the treatment and control groups would follow parallel trends over two periods in the absence of

treatment. Violation of parallel trends could severely undermine the credibility of DiD findings (Angrist and Pischke 2009; Callaway and Sant'Anna 2021). To validate whether our DiD findings satisfy the parallel trends assumption, we employ an event study approach (Sun and Abraham 2020; Callaway and Sant'Anna 2021).

Specifically, we define the policy implementation year (2014) as the event date. For each individual i in year t, we let $K_{it} = t - 2014$ measure the relative time to the policy: $K_{it} < 0$ indicates years before the policy took effect, $K_{it} = 0$ corresponds to the policy year, and $K_{it} > 0$ for years after the policy took effect. The estimation replaces the single post-reform indicator in our DiD model with a series of dummy variables for these relative time periods. A typical specification can be written as:

$$Y_{it} = \alpha_1 + \sum_{k \neq 0} \phi_k \cdot D_k \cdot treat_{it} + \gamma X_{it} + \delta_p + \pi_t + Z_r + \epsilon_{it} \tag{2}$$

where D_k is an indicator for being k years from the policy (e.g., $D_{-2} = 1$ if two years before treatment, 0 otherwise; $D_1 = 1$ if one year after treatment, 0 otherwise). The model includes both leads ($k < 0$) and lags ($k > 0$) to gauge pre-treatment trends and post-treatment dynamic effects, with the year immediately prior to treatment ($k = -1$) serving as the reference period. The coefficients ϕ_k capture the estimated difference between the treated and control groups at each relative period, allowing us to assess whether wage trends diverged before the reforms and how quickly and strongly wages responded afterward. Insignificant pre-treatment coefficients (ϕ_k, for $k < 0$) bolster the credibility of the parallel trends assumption, while significant post-treatment coefficients (ϕ_k, for $k > 0$) indicate whether wage impacts emerged immediately or developed gradually over time.

Propensity Score Matching (PSM). Despite controlling for observable covariates in DiD, significant differences between treated and control groups could still bias estimates if certain attributes remain unbalanced (Angrist and Pischke 2009; Imbens and Wooldridge 2009; Lechner 2011; Stuart 2010; Bertrand et al. 2004; Abadie 2005). To mitigate such bias,

we estimate propensity scores—the probability of being in a TFW-stream occupation—based on observable characteristics, such as age, education, and job tenure, and match treated individuals to "similar" controls. The matched sample thus comprises a balanced and statistically comparable control group. We then re-run the DiD on this matched sample, thereby reducing confounding arising from unbalanced covariates (Dehejia 2005; Glazerman et al. 2003).

Placebo Test. A placebo or falsification test further helps confirm that observed effects truly stem from the 2014 reforms rather than from confounding trends. Empirical studies frequently employ placebo comparisons of unaffected groups to validate causal inference, ensuring that estimated treatment effects are not artifacts of the research design (Card and Krueger 1994; Autor 2003; Clemens and Wither 2019). For example, we can estimate the DiD model using high-skilled occupations (theoretically unaffected by the TFWP changes) as a "placebo" control group. If the true policy effect on the treated is genuinely present, we should still observe a robust and significant difference-in-differences estimate, even in high-skilled occupations. If, by contrast, the DiD effect disappears or greatly diminishes with this new control group, it suggests the original effect might have been driven by broad labor-market forces or something else that affected both the original control group (low-skill, non-TFW-stream) and the treatment group, rather than the policy itself.

Validation with Census Data. Lastly, we replicate the DiD approach using data from the 2006, 2011, and 2016 Canadian Censuses. Although the Census provides fewer observation points than the Labour Force Survey (LFS), it offers a considerably larger sample size and measures annual wages rather than weekly wages, thus serving as a valuable cross-check of our DiD estimates. Using annual wages as the dependent variable allows us to capture additional employment dimensions, such as hours worked and the number of weeks employed throughout the year. Moreover, the Census's broad coverage provides increased

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statistical power, enabling a more nuanced examination of the policy's impact across various demographic groups. If the Census-based findings largely mirror those from the LFS, it strengthens the argument that any observed wage effects are robust to different data sources and measurement scales.

Through these five strategies, we aim to obtain a reliable picture of how restricting TFW admissions influences wages among the most directly affected domestic workers. The next subsection details the data sources and construction of the treatment and control groups.

3.2. Data

Our analyses draw on two complementary data sources: the Canadian Labour Force Survey (LFS) from 2005 to 2019 and the long-form Canadian Census for 2006, 2011, and 2016. Both datasets provide rich individual-level information on demographic characteristics, labor market outcomes, and detailed occupational codes, allowing us to construct treatment and control groups and observe wage changes over time.

3.2.1. Labour Force Survey (LFS)

The LFS is a monthly survey of approximately 54,000 Canadian households, designed to represent the civilian, non-institutionalized population aged 15 and older. For this study, we pool data from the May files in each year between 2005 and 2019, ensuring consistent occupation coding in line with the 2016 National Occupational Classification (NOC).[1] The LFS captures weekly wages (including tips, commissions, and bonuses), hours worked, job tenure, union status, and a range of worker and job characteristics that help isolate the impact of policy changes.

We restrict our attention to wage-earning employees aged 15 to 64 who are neither full-time students nor self-employed, thus aligning with prior research on low-skilled labor markets (Gross and Schmitt 2012; Prokopenko and Hou 2018). Following the literature, we

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[1] LFS data are collected monthly. In this study, we use surveys from the month of May to avoid seasonality.

adjust nominal wages to 2020 constant dollars using the Consumer Price Index (CPI). Our main outcome variable is the natural logarithm of weekly wages.

3.2.2. Census of Population

As a complementary data source, we draw on the long-form Canadian Population Census from 2006, 2011, and 2016. Though the Census does not have annual files like the LFS, it offers three notable advantages. First, its substantially larger sample size provides greater statistical power, facilitating subgroup analyses by gender, age, and other demographic splits. Second, it measures annual wages and the total weeks worked in the previous year, enabling us to assess broader employment-intensity changes (e.g., shifts between full-time and part-time or full-year and part-year employment) that may arise from policy reforms. Third, whereas our primary LFS analysis strategically employs May data to minimize seasonality, the Census inherently captures annual averages, offering an additional robustness check by verifying that the identified wage effects are not driven by residual seasonal variations. Annual wage data from the Census are deflated to constant 2020 dollars, matching our approach with the LFS data.

We did not use Census data in the main analysis for two reasons. First, some crucial job-related variables in the LFS (e.g., job tenure, union membership, contract type, and firm size) that may affect the wage outcome are not available in census data. Additionally, the census data are collected only every 5 years. With only three observation points (2006, 2011, and 2016), it limits our ability to test the validity of the parallel trends assumption and the timing of treatment effects using the Event Study Design (ESD) model (Angrist and Pischke 2009; Bertrand et al. 2004).

3.2.3. Treatment and Control Groups

Central to our approach is distinguishing between TFW-stream (treated) occupations and non-TFW-stream (control) occupations. Drawing on program documentation (Employment

and Social Development Canada 2022; Elgersma 2014) and prior studies (Fudge and MacPhail 2009; Nakache and Kinoshita 2010; Foster 2012), we identify 10 low-skilled occupations in which employers heavily relied on temporary foreign workers. These occupations constitute the "treatment" group. Our "control" group consists of 22 low-skilled occupations at the same skill (level D) as defined in the National Occupation Classification (NOC) but historically less reliant on TFWs.[2] In additional analyses, we also test a placebo scenario, replacing the control group with high-skilled occupations presumed to be unaffected by the TFWP reforms.

Both datasets enable us to classify occupations at the four-digit NOC level, which is crucial for identifying occupations in the TFW and non-TFW streams. We employ consistent occupational definitions across years, using the concordances provided by Statistics Canada to match NOC codes from earlier years to the 2016 NOC classification system.[3] This consistency is vital for accurately assigning individuals to TFW-intensive occupations and ensuring comparability over time.

3.2.4. Summary Statistics

We begin with a broad comparison of weekly wages (in constant 2020 dollars) and employment counts in TFW-stream ("treated") versus non-TFW-stream ("control") occupations for 2005 to 2019 based on the LFS data (Table 1). A few insights stand out. First, TFW-stream occupations generally have lower median weekly wages compared to those in the control group, although the wage gap varies across occupations and years. Second, some TFW-stream occupations—such as food-counter attendants—experience modest wage gains in specific intervals, whereas others, including security guards, show relatively little change.

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[2] The lists of treatment and control occupations, along with their associated NOC codes (2016 version), are displayed in Table 1.

[3] See Statistics Canada's website for NOC code concordances.

Third, the overall wage and employment patterns do not reveal a strictly uniform trend before or after 2014, instead displaying pockets of growth or stagnation in both groups. This variability across occupations and over time underscores the need for formal econometric methods to rigorously identify the reforms' impacts.

Table 1. Weekly Wages and Employment Counts of Treatment and Control Groups

NOC Code (2016)	TFWP stream occupations	Median Weekly Wages ($2020)					Weighted Counts (in thousands)				
		2005-2007	2008-2010	2011-2013	2014-2016	2017-0219	2005-2007	2008-2010	2011-2013	2014-2016	2017-0219
6541	Security Guards	527.5	574.8	585.4	576.2	606.1	291.2	332.5	338.6	358.2	309.0
6611	Cashiers	254.3	249.9	281.1	272.7	282.1	1,041.3	1,090.8	1,089.2	1,120.7	1,089.6
6622	Grocery Clerks and Store Shelf	303.9	330.5	364.7	387.4	386.1	505.1	544.0	583.2	602.9	690.8
6711	Food Counter Attendants	256.1	264.6	290.0	306.8	307.3	871.7	982.8	1,044.6	1,082.2	1,187.5
6721	Support occupations in hotels	753.4	628.7	675.4	613.6	544.0	18.5	17.2	22.4	20.7	10.1
6731	Light Duty Cleaners	452.1	479.0	472.8	490.2	514.8	470.0	509.9	520.3	563.0	587.8
6732	Specialized Cleaners	508.6	564.5	580.1	554.8	603.9	100.7	106.5	97.2	107.5	116.0
6733	Janitors, Caretakers	602.7	646.7	635.0	678.6	680.0	391.8	421.6	418.1	391.8	393.9
7611	Construction Trades Labourers	775.1	839.2	857.0	853.6	813.4	366.6	322.5	423.8	407.3	413.3
8612	Landscaping Labourers	592.2	617.4	617.0	618.8	655.2	234.8	232.7	253.5	275.7	250.1
NOC Code (2016)	Non-TFWP stream occupations	Median Weekly Wages ($2020)					Weighted Counts (in thousands)				
		2005-2007	2008-2010	2011-2013	2014-2016	2017-0219	2005-2007	2008-2010	2011-2013	2014-2016	2017-0219
6621	Service Station Attendants	235.9	231.1	292.9	270.5	348.3	68.8	58.5	50.0	51.2	41.0
6623	Other Elemental Sales	417.8	413.0	446.3	435.0	384.5	111.5	71.4	74.0	66.0	68.9
6722	Operators in Recreation	354.4	440.9	371.5	416.1	368.5	97.1	110.4	113.3	132.6	163.4
6741	Dry Cleaning and Laundry	502.3	564.5	557.8	601.8	581.0	64.7	74.8	54.1	61.5	50.2
6742	Ironing, Pressing Occupations	308.6	268.3	380.7	403.3	298.4	75.2	80.6	89.8	81.9	78.8
7612	Other Trades Labourers	590.8	670.6	731.3	699.9	677.8	30.9	31.5	36.8	32.8	25.7
7621	Public Works and Labourers	918.6	886.2	891.6	909.0	961.3	71.7	64.1	56.4	68.8	85.2
7622	Railway and Transport Labourers	717.0	705.6	685.6	601.8	788.0	10.9	10.6	12.1	24.9	20.2
8611	Harvesting Labourers	467.9	574.8	585.4	623.3	640.7	105.2	82.7	77.8	93.8	100.3
8613	Aquaculture Labourers	568.0	700.6	567.4	742.6	739.4	6.3	7.3	4.4	6.5	9.0
8614	Mine Labourers	901.4	1,200.7	1,249.5	1,084.4	1,224.0	12.2	9.1	18.2	7.8	11.0
8615	Oil and Gas Drilling Labourers	1,081.3	1,270.0	1,229.3	1,313.1	1,050.6	28.4	26.3	24.5	18.7	18.8
8616	Logging and Forestry Labourers	791.1	756.4	714.0	820.7	756.4	28.8	22.0	21.2	16.8	9.2
9611	Labourers in Metal Processing	901.4	922.0	777.0	1,025.0	820.0	24.1	19.2	32.3	20.9	24.7
9612	Labourers in Metal Fabrication	761.2	718.5	675.4	744.1	720.9	75.4	56.6	69.3	76.0	75.2
9613	Labourers in Chemical Products	617.8	615.5	594.2	770.5	739.4	24.2	21.1	33.8	20.5	23.3
9614	Labourers in Wood and Paper	835.5	803.8	787.2	692.6	840.5	109.4	66.5	55.4	90.2	58.8
9615	Labourers in Rubber Products	614.6	705.6	765.5	612.8	664.9	41.0	23.2	22.1	24.6	31.8
9616	Labourers in Textile Processing	551.1	557.4	493.2	649.3	604.4	30.5	17.8	16.4	15.5	18.5
9617	Labourers in Food, Beverage	653.0	660.4	630.4	639.0	630.4	138.3	142.0	127.9	141.4	162.0
9618	Labourers in Fish and Seafood	537.8	571.8	565.6	569.8	604.4	29.1	23.2	24.3	21.0	19.8
9619	Other Labourers	535.7	528.3	513.2	509.9	604.4	172.0	144.9	116.5	117.0	82.8

Source: Canadian Labour Force Survey, May files (2014–2019).

Table 2 provides more detailed descriptive statistics on worker and job characteristics during the post-reform period (2014–2019) for both treatment (TFW-stream) and control

(non-TFW-stream) groups, again using the Canadian Labour Force Survey (LFS). On average, workers in TFW-stream occupations earn lower weekly wages ($485) than those in the control group ($651). They also tend to be younger and have shorter job tenures (54 months vs. 71 months), suggesting higher turnover or job instability. Moreover, TFW-stream occupations exhibit a larger share of female workers (49.7% vs. 34.9%), more part-time employment (44.1% vs. 22.3%), and lower union membership (24.6% vs. 27.6%), all of which can contribute to wage differentials.

Substantial differences also appear in firm and sector-related characteristics. Workers in treatment occupations disproportionately work in large enterprises (over 500 employees), accounting for 51.5% of their group compared to 36.4% in the control group. These TFW-stream jobs are heavily concentrated on retail trade (33.1%) and service sectors (57.4%), whereas the control group's employment is more evenly spread across manufacturing (39.8%), services (35.4%), and agriculture (13.8%). Although the two groups are similarly distributed across Canada's provinces—particularly Ontario, Quebec, and British Columbia—TFW-stream occupations are more prevalent in major metropolitan areas like Toronto, Montreal, and Vancouver.

Overall, these descriptive patterns underscore the importance of explicitly controlling for differences in observables between the treatment and control groups when estimating the causal effects of the 2014 TFWP reforms. They also highlight the need for robustness checks of the Difference-in-Differences (DiD) estimates, which motivates our use of an event study design (ESD), propensity score matching (PSM), and a placebo test in the analyses that follow.

Table 2. Summary Statistics of Treatment and Control Groups, Post-Reform 2014-2019

Variable	Treatment (N = 34,850)		Control (N = 8,670)	
	Mean	Std. Dev.	Mean	Std. Dev.
Weekly wages ($2020)	485.0	326.1	650.9	374.9
Age	34.5	16.3	38.0	16.1
Job tenure (in mos)	53.6	79.4	70.7	98.7
Female	0.4967	0.5000	0.3486	0.4765
Foreign-born (=1 yes)	0.2631	0.4403	0.2792	0.4486
Marital status				
1. Single	0.5563	0.4968	0.4549	0.4980
2. Married	0.3735	0.4837	0.4721	0.4992
3. Divorced	0.0702	0.2554	0.0730	0.2602
Education				
1. HS or less	0.5611	0.4963	0.5695	0.4952
2. Some PSE	0.3624	0.4807	0.3524	0.4777
3. BA & +	0.0766	0.2659	0.0781	0.2683
FT work (=1 yes)	0.5586	0.4966	0.7766	0.4166
Union (=1 yes)	0.2462	0.4308	0.2757	0.4469
Perm. Job (=1 yes)	0.7717	0.4197	0.7270	0.4455
Firm size				
1. < 20	0.2123	0.4089	0.2313	0.4217
2. 20-99	0.1527	0.3597	0.2176	0.4126
3. 100-500	0.1197	0.3246	0.1876	0.3904
4. Over 500	0.5153	0.4998	0.3636	0.4811
Sector				
1. Agri (11/23)	0.0844	0.2779	0.1380	0.3449
2. Manufacturing (31/33)	0.0107	0.1027	0.3984	0.4896
3. Retail trade (41/48)	0.3307	0.4705	0.1100	0.3129
4. Services (51/91)	0.5742	0.4945	0.3536	0.4781
Province				
NL	0.0154	0.1229	0.0149	0.1213
PE	0.0049	0.0695	0.0072	0.0844
NS	0.0295	0.1693	0.0226	0.1487
NB	0.0228	0.1492	0.0261	0.1595
QC	0.2155	0.4112	0.2399	0.4270
ON	0.3903	0.4878	0.3938	0.4886
MB	0.0348	0.1832	0.0418	0.2002
SK	0.0309	0.1732	0.0330	0.1786
AB	0.1189	0.3237	0.1072	0.3094
BC	0.1371	0.3440	0.1135	0.3172
Major CMA				
Quebec	0.0223	0.1475	0.0192	0.1373
Montreal	0.1053	0.3070	0.1089	0.3115
Ottawa	0.0358	0.1859	0.0170	0.1292
Tornoro	0.1586	0.3653	0.1722	0.3776
Winnipeg	0.0239	0.1528	0.0222	0.1473
Calgary	0.0423	0.2013	0.0294	0.1689
Edmonton	0.0384	0.1922	0.0312	0.1738
Vancouver	0.0715	0.2576	0.0527	0.2235
Other CMAs	0.3056	0.4607	0.2743	0.4462
Non-CMA	0.1963	0.3972	0.2729	0.4455

Source: Canadian Labour Force Survey, May files (2014–2019).

4. Empirical findings

4.1. Difference-in-Differences (DiD) Regressions

Table 3 presents the Difference-in-Differences (DiD) estimates to assess the impact of the 2014 TFWP reforms on the wages of Canadian workers in low-skilled occupations. The key parameter for identifying the causal effect of the reforms is the interaction term (TreatTFW × post2014), often referred to as the DiD estimator. This interaction captures how wage growth in TFW-stream (treated) occupations differs from that of comparable non-TFW-stream (control) occupations after 2014.

Specification (1) is the most parsimonious model, which includes only the treatment and post-reform indicators, along with year-fixed effects. The negative treatment indicator confirms that, prior to 2014, workers in TFW-stream occupations earned significantly lower weekly wages than those in the control group. Meanwhile, the positive post-reform indicator shows that wages rose across all low-skilled occupations following 2014. Most notably, the interaction term is positive (0.0452) and significant at the 1% level. This result suggests that the 2014 TFWP reforms led to an additional increase in weekly wages of approximately 4.5% for domestic workers in TFW-stream occupations relative to those in the control group. For an average weekly wage of $485, this effect translates into roughly $22 in extra weekly earnings.

Specification (2) adds demographic controls such as age, gender, marital status, education, and immigration status. The magnitude of the treatment effect remains nearly the same at 0.0461 ($p < 0.01$), underscoring that demographic differences between the groups do not drive the observed wage gains. Among the newly added controls, gender is particularly significant, with female workers in low-skilled occupations earning about 35 percent less than male counterparts, all else being equal. Marital status also matters significantly, as single and

divorced individuals earn roughly 34% and 11% less, respectively, compared to married workers.

Specification (3) further introduces job-related characteristics—job tenure, full-time status, union membership, permanent employment, firm size, and industry fixed effects—improving the model's explanatory power (R-squared increases from 0.241 to 0.709). The interaction coefficient remains positive and highly significant, although it drops slightly to 0.0371, suggesting that part of the original 4.6 percent wage gap was explained by differences in job characteristics across treatment and control groups. Notably, working full-time, holding a permanent contract, being unionized, and working for a larger firm each contribute significantly to higher weekly earnings in these low-wage occupations.

Specification (4) incorporates additional fixed effects for provinces and metropolitan areas (CMA). Even after accounting for regional labor market variations, the estimated wage effect of the reforms remains stable at around 0.037 ($p < 0.01$). This implies that the 2014 TFWP reforms were associated with a 3.7 percent wage gain for domestic workers in TFW-stream occupations, relative to control occupations, after controlling for a wide range of demographic, job-related, and regional factors.

Overall, the DiD regressions in Table 3 consistently indicate a modest but statistically significant post-reform wage improvement—ranging from roughly 3.7 to 4.5 percent—for Canadian workers in TFW-intensive occupations. These findings align with the study's main hypothesis that restricting temporary foreign labor alleviates at least some wage pressure on low-skilled domestic workers. Subsequent subsections complement this analysis by validating the parallel trends assumption, performing robustness checks via propensity score matching, using a placebo control group, and replicating the main results with Census data.

Table 3. Difference-in-Differences Regressions

Dependent variable: ln(weekly wage) in $2020

Explanatory variables	Coefficients (Std. Err.)			
	(1)	(2)	(3)	(4)
TreatTFW (=1 treatment)	-0.3821***	-0.2604***	-0.0387***	-0.0400***
	(0.0087)	(0.0081)	(0.0068)	(0.0068)
Post2014 (=1 if 2014 or later)	0.1146***	0.0978***	0.1461***	0.1520***
	(0.0196)	(0.0180)	(0.0110)	(0.0110)
TreatTFW*post2014	0.0452***	0.0461***	0.0371***	0.0370***
	(0.0144)	(0.0133)	(0.0080)	(0.0079)
Female		-0.3531***	-0.0902***	-0.0926***
		(0.0053)	(0.0037)	(0.0037)
Age		0.0106***	0.0019***	0.0021***
		(0.0002)	(0.0002)	(0.0002)
Immigrant (=1 yes)		-0.0327***	-0.0398***	-0.0692***
		(0.0069)	(0.0044)	(0.0049)
Marital status (Ref=married)				
Single		-0.3425***	-0.1125***	-0.1152***
		(0.0072)	(0.0045)	(0.0044)
Divorced		-0.1060***	-0.0400***	-0.0430***
		(0.0104)	(0.0063)	(0.0062)
Education (Ref: Some PSE)				
HS or less		-0.1301***	-0.0734***	-0.0813***
		(0.0056)	(0.0035)	(0.0035)
BA or higher		-0.0342***	0.0093	-0.0003
		(0.0121)	(0.0076)	(0.0076)
ln(job tenure)			0.0335***	0.0347***
			(0.0013)	(0.0013)
Full-time work (=1 yes)			0.9970***	0.9930***
			(0.0042)	(0.0042)
Union (=1 yes)			0.1658***	0.1765***
			(0.0043)	(0.0044)
Permanent job (=1 yes)			0.0699***	0.0644***
			(0.0046)	(0.0046)
Firm size (ref: 20-99)				
< 20			-0.0368***	-0.0368***
			(0.0056)	(0.0056)
100-500			0.0307***	0.0283***
			(0.0058)	(0.0057)
Over 500			0.0458***	0.0373***
			(0.0047)	(0.0047)
Year-fixed effects	Yes	Yes	Yes	Yes
Industry-fixed effects	No	No	Yes	Yes
Province-fixed effects	No	No	No	Yes
CMA-fixed effects	No	No	No	Yes
Constant	6.2008***	6.1865***	5.3471***	5.3145***
	(0.0131)	(0.0164)	(0.0161)	(0.0208)
Observations	111,030	111,030	111,030	111,030
R-squared	0.0398	0.2406	0.7088	0.7143

Source: Canadian Labour Force Survey, May files (2005–2019).

4.2. Event Study Design (ESD) Regressions

In a standard difference-in-differences (DiD) framework, the parallel trends assumption is critical for attributing observed outcome changes to the policy intervention rather than to pre-existing or coincidental trends. To further test the robustness of our DiD findings, we use an Event Study Design (ESD), which explicitly checks whether the treatment and control groups followed similar wage trajectories prior to the 2014 TFWP reforms and captures how the wage effects evolve over time.

Table 4 divides the sample into five time periods: 2005–2007, 2008–2010, 2011–2013, 2014–2016, and 2017–2019. We designate 2011–2013 (the immediate pre-reform interval) as the baseline. The regressions include the treatment indicator (TreatTFW) plus four interaction terms combining this indicator with the period dummies—two for the pre-reform segments (2005–2007 and 2008–2010) and two for the post-reform segments (2014–2016 and 2017–2019). The estimated coefficients on these interaction terms capture the wage differences between treated and control occupations for each period relative to the 2011–13 baseline. All additional controls from the most comprehensive DiD specification (Table 3, Model 4) are retained.

The baseline treatment indicator is negative and statistically significant (–0.0274), confirming that TFW-stream occupations paid lower wages overall than non-TFW-stream occupations throughout the study period. In the pre-treatment periods (2005–2007 and 2008–2010), the interaction terms are small and insignificant (–0.0171 and –0.0199, respectively), indicating no detectable differences in wage trends between treatment and control occupations before 2014. This supports the parallel trends assumption, a critical prerequisite for interpreting the DiD estimates causally.

Turning to the post-treatment results, the interaction coefficient for 2014–2016 (0.0197) is positive but not statistically significant. However, for 2017–2019, it rises to 0.0292 ($p < 0.05$),

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implying a wage increase of roughly 2.9 percent in treated occupations relative to unaffected ones. This delayed but significant effect suggests that the policy's full impact may require several years to manifest, potentially reflecting employers' gradual adjustments in hiring or wage-setting practices.

Overall, the ESD analysis reinforces the DiD findings by confirming that wage trajectories were similar before 2014 and by highlighting a meaningful wage increase in the later post-reform period. This dynamic evidence further supports the conclusion that the 2014 TFWP reforms resulted in a modest but significant improvement in weekly wages for domestic workers in TFW-stream occupations.

Table 4. Event Study Design (ESD) Regression

Dependent variable: ln(weekly wage) in $2020

Explanatory variables	ESD regression
	Coefficients
	(Std. Err.)
Reference: year_2011_13	
TreatTFW*year_2005_07	-0.0171
	(0.0121)
TreatTFW*year_2008_10	-0.0199
	(0.0125)
TreatTFW*year_2014_16	0.0197
	(0.0126)
TreatTFW*year_2017_19	0.0292**
	(0.0127)
TreatTFW	-0.0274***
	(0.0102)
All other controls in Table 3(4)	Yes
Constant	5.3178***
	(0.0213)
Observations*	111,030
R-squared	0.7143

Source: Canadian Labour Force Survey, May files (2005–2019).

## 4.3. Propensity Score Matching Analysis

Although difference-in-differences (DiD) regressions control for observable covariates, imbalances between the treatment and control groups may still introduce bias if their

characteristics differ systematically (Stuart, 2010; Lechner, 2011). To address this concern, we employ Propensity Score Matching (PSM), which creates more comparable subsets of treated and untreated observations based on the estimated probability—i.e., the propensity score.

The analysis of PSM involves three sequential steps. First, propensity scores are estimated using a logistic regression model, where the treatment indicator—being employed in TFW-stream occupations—is regressed on various observed covariates, including demographic and job-related factors such as age, gender, education, job tenure, full-time status, union membership, firm size, and region. Second, based on these estimated propensity scores, we match each treated individual with a comparable set of control individuals using one-to-one, one-to-five, and one-to-ten nearest neighbor (NN) matching. Each match pairs (or groups) observations with similar covariate profiles, thereby reducing confounding from differences in observed characteristics. Third, we then re-estimate the DiD regressions on these matched samples, effectively comparing like with like, controlling explicitly for observed covariates.

We first conducted balance tests to evaluate how well the matched samples align across key covariates based on one-to-ten nearest-neighbor matching (Figure 1). Before matching, substantial imbalances existed, particularly in gender (female), firm size, full-time employment status, and education level, with biases reaching nearly 50%. These biases are reduced dramatically and fall well below 5 percent following matching, a commonly accepted threshold for adequate balance (Stuart 2010). This indicates that the PSM procedure substantially reduced observable differences between the treatment and control groups.

Figure 1. Balance Tests of the Covariates in the Unmatched and Matched Samples
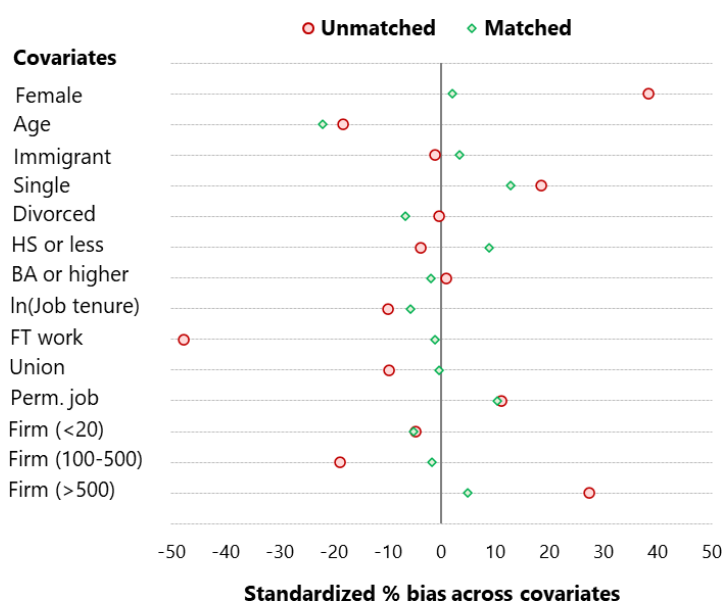
Source: Canadian Labour Force Survey, May files (2005–2019).

Table 5 presents the main DiD estimates from the matched samples. Across all three matching specifications, the interaction term (TreatTFW × Post2014) remains positive and highly significant ($p < 0.01$). For instance, one-to-one NN matching yields an estimated coefficient of 0.039, indicating a roughly 3.9 percent wage increase for domestic workers in TFW-stream occupations compared to their matched peers in non-TFW-stream occupations. The estimates for one-to-five and one-to-ten matches are similarly robust, at approximately 0.038 and 0.036, respectively. For brevity, we do not discuss each control coefficient in detail. Their signs, magnitudes, and significance levels broadly mirror those in Table 3, indicating that the PSM procedure yields no notable changes in how observed worker or job characteristics correlate with wage outcomes

These PSM-based findings align closely with the earlier unmatched DiD results, reinforcing confidence that the 2014 TFWP reforms generated a modest but significant wage boost for treated workers. Together with the balance tests, this consistency underscores that the reported treatment effects are not merely artifacts of pre-existing demographic or

occupational differences but rather reflect a genuine policy impact of restricting low-skilled TFW admissions.

Table 5. Propensity Score Matching Regressions

Dependent variable: ln(weekly wage) in $2020

Explanatory variables	Coefficients (Std. Err.)		
	One-to-one NN matching	Five-to-one NN matching	Ten-to-one NN matching
	(1)	(2)	(3)
TreatTFW	-0.0586***	-0.0534***	-0.0487***
	(0.0163)	(0.0136)	(0.0130)
Post2014	0.1694***	0.1888***	0.1887***
	(0.0356)	(0.0279)	(0.0275)
TreatTFW*post2014	0.0687**	0.0600**	0.0555**
	(0.0319)	(0.0287)	(0.0273)
Female	-0.0810***	-0.0840***	-0.0869***
	(0.0151)	(0.0134)	(0.0130)
Age	0.0016***	0.0010**	0.0010**
	(0.0005)	(0.0004)	(0.0004)
Immigrant (=1 yes)	-0.0504**	-0.0461**	-0.0506***
	(0.0212)	(0.0189)	(0.0179)
Marital status (Ref=married)			
Single	-0.1098***	-0.1249***	-0.1286***
	(0.0151)	(0.0130)	(0.0129)
Divorced	-0.0536***	-0.0629***	-0.0607***
	(0.0195)	(0.0226)	(0.0224)
Education (Ref: Some PSE)			
HS or less	-0.0949***	-0.0976***	-0.0956***
	(0.0149)	(0.0132)	(0.0121)
BA or higher	0.0396	0.0147	0.0058
	(0.0293)	(0.0353)	(0.0412)
ln(job tenure)	0.0460***	0.0480***	0.0476***
	(0.0060)	(0.0053)	(0.0050)
Full-time work (=1 yes)	1.0047***	1.0171***	1.0197***
	(0.0139)	(0.0126)	(0.0125)
Union (=1 yes)	0.1530***	0.1590***	0.1587***
	(0.0164)	(0.0160)	(0.0166)
Permanent job (=1 yes)	0.1117***	0.1085***	0.1106***
	(0.0180)	(0.0159)	(0.0153)
Firm size (ref: 20-99)			
< 20	-0.0396*	-0.0276	-0.0221
	(0.0236)	(0.0198)	(0.0187)
100-500	0.0066	0.0239	0.0284
	(0.0262)	(0.0212)	(0.0201)
Over 500	0.0009	0.0040	0.0067
	(0.0255)	(0.0208)	(0.0191)
Year-fixed effects	Yes	Yes	Yes
Industry-fixed effects	Yes	Yes	Yes
Province-fixed effects	Yes	Yes	Yes
CMA-fixed effects	Yes	Yes	Yes
Constant	5.2469***	5.2372***	5.2587***
	(0.0772)	(0.0634)	(0.0605)
Observations	97,385	103,149	105,970
R-squared	0.6974	0.6968	0.6952

Source: Canadian Labour Force Survey, May files (2005–2019).

4.4. A Placebo Test

Although our main DiD estimates indicate a post-reform wage gain in TFW-intensive occupations, these findings could still reflect other unobserved labor market shocks or data artifacts (Bertrand et al. 2004). As an additional robustness check, we conduct a placebo (falsification) test by replacing our original control group of low-skilled, non-TFW-stream occupations with high-skilled occupations that are theoretically unaffected by the 2014 TFWP reforms. Specifically, we identify 89 skill-level A occupations in the 2016 National Occupational Classification (NOC), which generally require a university degree and thus should not be directly impacted by restrictions on low-skilled TFWs. If the 2014 reforms genuinely drove the wage increases among TFW-stream workers, then estimating the difference-in-differences model with this placebo control group should still yield a robust and significant interaction term (TreatTFW×Post2014). Conversely, if the estimated effect substantially diminishes or disappears, it would suggest that the original DiD results were confounded by broader trends affecting both the treatment and control groups.

Table 6 presents the placebo test regressions, focusing on the key treatment indicators and their interactions. Across four progressively comprehensive specifications mirroring those in Table 3, the interaction term remains positive and highly significant, with coefficients ranging from 0.0196 to 0.0446 ($p < 0.01$). Notably, these effect sizes align closely with the baseline DiD and propensity-score matching results, reinforcing the conclusion that low-skilled workers in TFW-stream occupations experienced a meaningful wage benefit following the reforms. Since the policy did not target high-skilled occupations, these findings suggest that the estimated treatment effect indeed reflects a direct consequence of the 2014 TFWP reforms, rather than unaccounted-for macroeconomic shocks.

Table 6. DiD Regressions with alternative comparison groups

Dependent variable: ln(weekly wage) in $2020

Explanatory variables	Coefficients (Std. Err.)			
	(1)	(2)	(3)	(4)
TreatTFW	-1.2306***	-0.9099***	-0.5796***	-0.5750***
	(0.0052)	(0.0061)	(0.0056)	(0.0055)
Post2014	0.1357***	0.1307***	0.1079***	0.1144***
	(0.0104)	(0.0096)	(0.0071)	(0.0070)
TreatTFW*post2014	0.0307***	0.0196***	0.0446***	0.0439***
	(0.0081)	(0.0073)	(0.0050)	(0.0049)
Controlled variables	Table 3(1)	Table 3(2)	Table 3(3)	Table 3(4)
Constant	7.0219***	6.6875***	5.6292***	5.6216***
	(0.0076)	(0.0108)	(0.0227)	(0.0238)
Observations	217,756	217,756	217,756	217,756
R-squared	0.4579	0.5466	0.7725	0.7768

Source: Canadian Labour Force Survey, May files (2005–2019).

4.5. Additional Validation from Census Data

As a further robustness check, we replicate the core difference-in-differences analysis using data from the 2006, 2011, and 2016 Canadian Censuses. Unlike the Labour Force Survey (LFS), the Census captures annual earnings rather than weekly wages, and the cross-sectional design provides only three time points rather than a continuous yearly series. Nonetheless, its larger sample size offers additional statistical power, and measuring annual wages can reveal any broader changes in employment intensity—such as the total weeks or hours worked per year—that might also be influenced by the 2014 TFWP reforms.

Table 7 presents the DiD estimates based on Census data. In specifications that do not explicitly control for total weeks worked, the coefficient on the interaction term (TreatTFW×Post2014) is somewhat larger—around 0.0737 (p < 0.01)—reflecting the possibility that restricting TFW admissions increased both wage rates and overall employment intensity for treated workers. Based on the group's mean annual wage of $23,285, this represents approximately $1,723 in additional annual earnings.
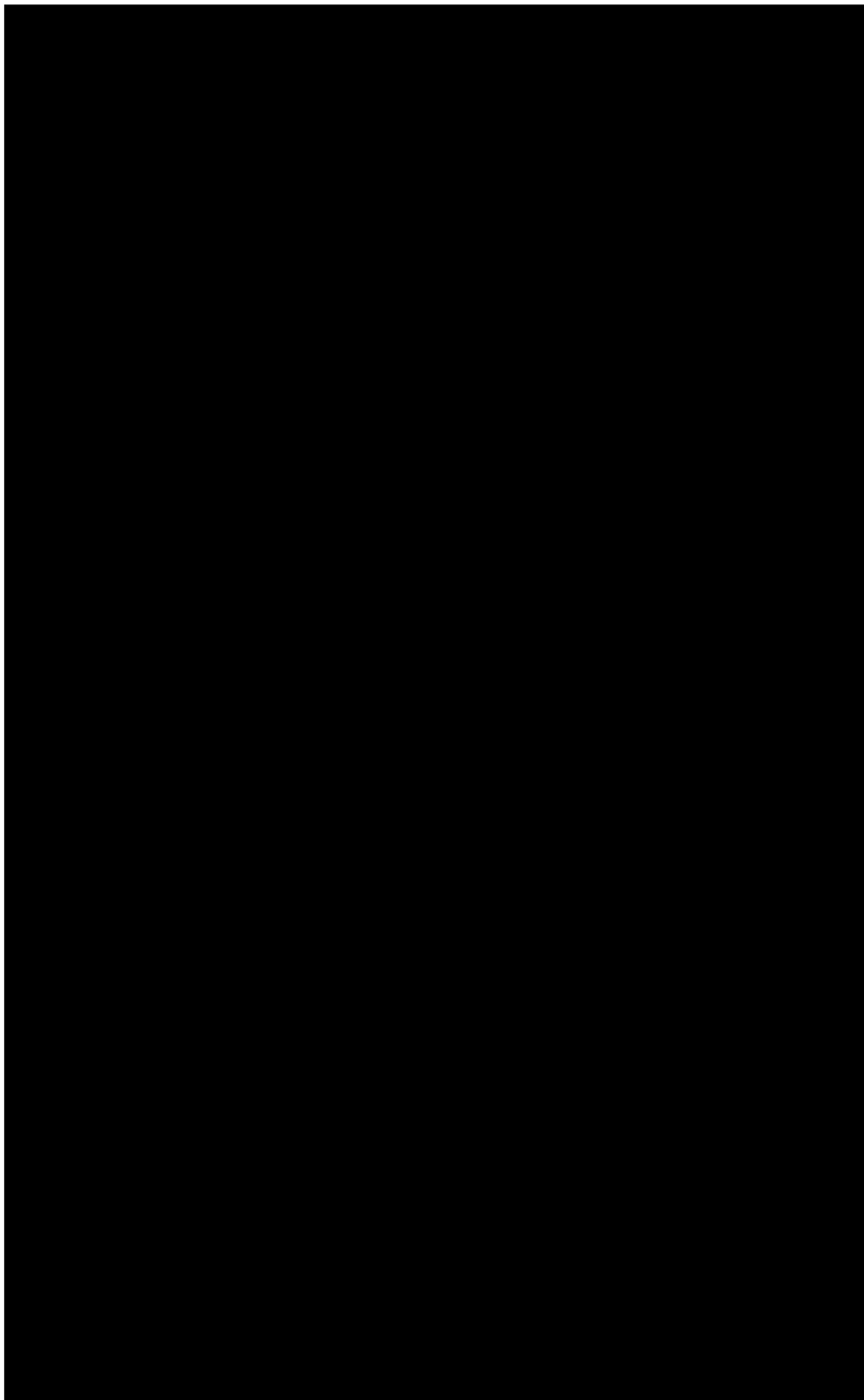
Once we add weeks worked and other job-related characteristics, the estimated treatment effects converge to roughly 0.0350, closely mirroring the LFS-based DiD estimate of 0.0370 in Table 3. This similarity arises because controlling for weeks worked effectively removes much of the employment-intensity dimension, rendering annual wages more directly comparable to weekly earnings.

Overall, the Census-based analysis strongly validates our primary findings that the 2014 TFWP reforms raised the wages of domestic workers in TFW-stream jobs. Furthermore, consistency across both weekly and annual wage measures suggests that the reforms enhanced the economic well-being of the treatment group, not only through higher weekly wages but also potentially through improved employment stability and greater overall labor market attachment.

Varying Treatment Effects

Utilizing the Census's large sample size, we also investigate how the 2014 TFWP reforms affect different demographic groups. Specifically, we extend our core difference-in-differences framework by introducing three-way interactions between the treatment and post-reform indicators and various demographic attributes, including gender, age, immigration status, official language proficiency, and education level. Table 8 presents the results, with each column corresponding to a different subgroup analysis. All models incorporate the same covariates used in Table 7(4) (i.e., demographic, job-related, and regional controls) along with separate main and two-way interaction terms for the post-reform period and each demographic characteristic. This approach ensures the three-way interaction coefficients capture the true differential effect of restricting TFW admissions for each subgroup, beyond baseline group differences or secular trends.

Table 7. DiD Regressions from Census Data

Source: Canadian Population Census 2006, 2016, National Household Survey (NHS) 2011.

In the first set of results, we examine gender differences by estimating (TreatTFW×Post2014×Female). The coefficient (-0.0017) is small and statistically insignificant, suggesting male and female workers in TFW-stream occupations benefited similarly from the reforms. Turning to age, an additional three-way interaction (TreatTFW×Post2014×Age) also yields an insignificant coefficient (-0.0002), indicating that older and younger workers experienced comparable wage gains.

By contrast, immigration status (specification 3) appears more consequential: The interaction term has a positive and statistically significant coefficient of 0.0302 ($p < 0.01$), indicating that immigrant workers in TFW-intensive jobs experienced notably larger wage increases, likely due to their increased labor-market substitutability with temporary foreign workers. Specification (4) shows that official language proficiency similarly emerges as a relevant factor. The interaction term yields a positive and significant coefficient (0.0652, $p < 0.05$), suggesting workers fluent in English or French experience more pronounced wage increases following the reforms. Finally, comparing education levels (specification 5) reveals that while those with a high school diploma or less do not exhibit significantly different outcomes from the baseline group, workers holding a bachelor's degree or higher experience marginally larger post-reform wage gains.

Taken together, these subgroup analyses reveal that the 2014 TFWP reforms did not distribute benefits uniformly across the labor force. Although overall wage effects remained positive, immigrant workers, those proficient in official languages, and higher-educated employees in TFW-stream occupations tended to capture somewhat greater improvements in earnings. Consequently, policymakers should be aware that reforms aimed at tightening access to temporary foreign labor may produce heterogeneous outcomes, potentially reinforcing existing labor market advantages for certain groups while still providing moderate gains to others.

Table 8. DiD Regressions (Census Data) with Varying Treatment Effects

Dependent variable: ln(annual wage) in $2020

Thee-way interactions	Coefficients (Std. Err.)				
	(1)	(2)	(3)	(4)	(5)
	By sex	By age	By immigration status	By official language	By education
TreatTFW#Post14#Female	-0.0017				
	(0.0082)				
TreatTFW#Post14#Age		-0.0002			
		(0.0003)			
TreatTFW#Post14#Immigration			0.0302***		
			(0.0091)		
TreatTFW#Post14#Official_language				0.0652**	
				(0.0259)	
TreatTFW#Post14#HS					0.0142
					(0.0096)
TreatTFW#Post14#BA					0.0316*
					(0.0181)
Main effects	Yes	Yes	Yes	Yes	Yes
Two-way interactions	Yes	Yes	Yes	Yes	Yes
Other Table A2(4) variables	Yes	Yes	Yes	Yes	Yes
Constant	7.4202***	6.3311***	7.5009***	7.3974***	7.4450***
	(0.0121)	(0.0150)	(0.0120)	(0.0174)	(0.0126)
Weighted Obs*	6,510,810	6,510,810	6,510,810	6,510,810	6,510,810
Adjusted R-squared	0.5322	0.5475	0.5323	0.532	0.5321

Source: Canadian Population Census 2006, 2016, National Household Survey (NHS) 2011.

5. Discussion

5.1. Policy Implications

These findings have significant implications for Canadian immigration and labor policy, particularly given ongoing efforts to balance labor supply needs with the protection of domestic workers. Our results indicate that limiting low-skilled TFW admissions can modestly alleviate downward wage pressures for vulnerable Canadian workers. In particular, the reforms appear to have helped raise pay in occupations where TFWs were most prevalent, echoing theoretical arguments that foreign labor can suppress domestic wages when workers are close substitutes (Borjas 2003; Brochu, Gross, and Worswick, 2020; Cardoso et al. 2023). Still, the modest size of the estimated wage gains suggests that TFWs were not the sole driver

of low pay in these sectors—an insight consistent with prior work highlighting the limited substitutability between foreign and domestic labor in certain industries (Peri and Sparber, 2009; Gu, Hou, and Picot, 2020).

This nuance highlights the need for broader structural interventions in policy debates. A substantial reduction in TFWs, without careful consideration of sectoral or regional dependencies, could exacerbate labor shortages in sectors such as agriculture or food services, potentially constraining productivity and economic growth (Falconer 2020; BDC 2021). As the Government of Canada contemplates additional changes to immigration targets for 2025–2027 (Government of Canada 2024), any reforms should be calibrated to preserve the positive impact on domestic workers' wages without undermining the viability of businesses that rely on foreign labor. Balancing targeted protections for vulnerable workers with the need for a flexible labor supply may therefore be key to sustaining both equity and economic vitality in Canada's labor market.

Additionally, our findings provide insights into Canada's evolving immigration policies, which increasingly rely on a two-step process wherein temporary foreign workers transition to permanent residency, often through the International Mobility Program (O'Donnell and Skuterud 2022). The modest yet significant wage gains we document for domestic workers in TFW-stream occupations imply that stricter regulations on low-skilled TFWs may not only improve labor outcomes for local workers but also alter employers' strategies for accessing foreign labor. Higher costs or more stringent rules under the TFWP may encourage greater reliance on LMIA-exempt pathways, reshaping Canada's temporary labor flows, long-term skill composition, and immigrant settlement patterns.

5.2. Limitations and Future Research

Although the findings offer important insights into the impacts of Canada's 2014 TFWP reforms on domestic workers' wages, several limitations warrant attention. First, our analysis

focuses solely on employed individuals, as occupation-level data—crucial for classifying TFW-stream occupations—are unavailable for the unemployed or those out of the labor force. Consequently, we cannot assess how the reforms might have influenced labor market participation or unemployment rates in TFW-intensive occupations.

Second, our data do not allow us to identify TFWs directly, which limits our ability to assess the specific behavior or outcomes of foreign workers themselves. As a result, we cannot observe which individuals are TFWs, nor can we analyze their labor market trajectories or measure the precise degree of TFW concentration at the sectoral or firm level. Instead, the analysis focuses on domestic workers in occupations historically reliant on TFWs. While this approach aligns with our objective of evaluating wage effects on Canadian workers, future research using immigration-linked microdata could more precisely assess the distributional impacts of TFW presence across different segments of the labor market.

Third, the study centers on wage outcomes, leaving firm-level and broader macroeconomic consequences relatively unexplored. Future research could benefit from utilizing richer administrative datasets that provide matched employer-employee records and direct identifiers for TFWs (e.g., Gu, Hou, and Picot 2020; Cardoso et al. 2023) to investigate how restricting TFW admissions affects firm behavior, including investments in capital or training, productivity gains, and competitive dynamics across different industries. Additionally, more granular data on actual TFW numbers and hiring practices would facilitate a deeper look into whether and how businesses adjusted recruitment strategies, potentially shifting toward alternative labor pools or adopting labor-saving technologies.

Lastly, the relatively short post-reform window means our estimates capture primarily medium-term effects. A longer observation horizon could reveal whether wage gains persist, attenuate, or become overshadowed by factors such as shifts in global labor demand or subsequent policy changes. Addressing these gaps would provide a more comprehensive

understanding of the long-term consequences of TFW-related policies, thereby guiding more effective immigration and labor reforms going forward.

6.   Conclusion

This study examined the wage impacts of Canada's 2014 reforms to the Temporary Foreign Worker Program (TFWP), which tightened access to low-skilled foreign labor. Using a difference-in-differences framework, supplemented by event study analysis, propensity score matching, placebo tests, and additional validation with Census data, we consistently find a modest but statistically significant wage increase—on the order of three to four percent—for Canadian workers in TFW-intensive occupations. The results suggest that, although restricting TFW admissions can help alleviate downward wage pressures for vulnerable domestic workers, the overall magnitude of the effect is relatively small. TFWs, therefore, appear to have contributed to, but were not the primary cause of, low pay in these occupations. Moreover, concerns remain about possible unintended consequences, such as labor shortages in certain sectors and reduced flexibility for employers. These findings suggest that while restrictions on temporary foreign labor can yield modest wage gains, such reforms are unlikely to resolve broader labor market challenges on their own and should be considered alongside complementary policies that strengthen wages and working conditions more broadly.

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