

Zanoni López, Wladimir; He, Ailin

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Wladimir Zanon
Ailin He

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Citizenship and the Economic Assimilation of Canadian Immigrants*

Wladimir Zanoni*

Ailin He**

Abstract

In this paper, we examine whether acquiring citizenship improves the economic assimilation of Canadian migrants. We took advantage of a natural experiment made possible through changes in the Canadian Citizenship Act of 2014, which extended the physical presence requirement for citizenship from three to four years. Using quasi-experimental methods, we found that delaying citizenship eligibility by one year adversely affected Canadian residents' wages. Access to better jobs explains a citizenship premium of 11 percent in higher wages among naturalized migrants. Our estimates are robust to model specifications, differing sampling windows to form the treatment and comparison groups, and whether the estimator is a non-parametric rather than a parametric one. We discuss how our findings are relevant to the optimal design of naturalization policies regarding efficiency and equity.

Keywords: Citizenship Premium, Labor Supply

JEL Codes: K37, J61, E24

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* Inter-American Development Bank. Email: wladimirz@iadb.org

** McGill University, Department of Economics. Email: ailin.he@mail.mcgill.ca

1 Introduction

The question of what forces drive the economic assimilation of immigrants is still debated by economists (see, for instance, the work by Abramitzky, Boustan, and Eriksson, (2012) and (2014) regarding the USA). Within that broad question, we seek to determine whether *citizenship acquisition* (or naturalization) contributes to migrants' economic assimilation in Canada. Studies on the links between migrants' naturalization and economic assimilation have received increasing attention. For instance, the OECD (2011) compiles evidence of the relationship between labor market outcomes and migrants' citizenship acquisition for Sweden, France, Switzerland, Germany, Norway, USA, and Canada. Recent work has employed causal inference frameworks to show that citizenship acquisition affects the economic assimilation of migrants in Germany (Gathmann and Keller (2018); Riphon and Saif (2019)) and other European countries (Hainmueller, Hangartner, and Ward (2019); Helgertz, Bevelander, and Tegunimataka, (2014)). While the evidence on the causal link between naturalization and economic assimilation is growing, the field still needs more systematic evidence of that relationship across regions and countries.

Whether (and how) naturalization plays a role in migrants' economic assimilation can be a subject of empirical research. Answering those questions matter, for if we found that naturalization promotes migrants' economic assimilation, we could direct attention to its actual efficiency and equity implications (Gathmann (2015)). Providing evidence that naturalization policy affects migrants' income would help us better quantify the policy's fiscal implications: We could better understand the dynamics of income eligibility for participation in welfare programs, and the potential for migrant populations' tax contributions. Indeed, a deeper understanding of the assimilation impact of naturalization would help better qualify its efficacy as a tool to reduce economic disparities. While relevant for Canada, answering our research question is also vital for OECD countries, many of which are currently chosen as host countries by a continuously growing population of economic migrants, international students, refugees, and asylum seekers from across the developing world.¹

To estimate the effects of citizenship on migrants' economic assimilation, we exploit the variability in eligibility for claiming citizenship caused by a change in 2014 in the Canadian immigration law. The law called "Bill C-24, Strengthening Canadian Citizenship Act" (the "Act" hereafter) was announced in June 2014 and began to be enforced in June 2015. It called for immigrants to have lived in Canada for 48 months, rather than the previous 36 months, before being eligible to apply for citizenship. Once announced, the new migration law split the pool of legal migrants into two groups corresponding to how long they had been living in Canada. By June 2015, those immigrants who had resided in Canada for three or more years still had the right to apply for citizenship. Nevertheless, residents who by that date had lived in the country for less than three years would have to wait one additional year to be eligible to apply. We compare the wages of immigrants who would otherwise be similar but, given the change in the law and small differences in the date when they migrated to Canada, were assigned different waiting times for applying for citizenship.

We separate the pool of immigrants into two groups that we call the treatment and comparison groups. The treatment group unites those migrants who arrived in Canada 31-35 months before June 2015, and had to wait 48 months (instead of 36 months) because of the Act after their arrival to apply for citizenship. The comparison group unites migrants who arrived in Canada 36-40 months before June 2015 and were eligible to apply for citizenship after only 36 months of Canadian residency. Using a difference in difference (DID) estimator with data within the indicated arrival bandwidths, we evaluated the Act's wage effects for two years after it became effective (between June 2015 and May 2017).

¹ In 2018, 3.1 and 2.2 million new migrants arrived in the EU and the USA respectively. See <https://www.oecd.org/migration/mig/G20-migration-and-displacement-trends-and-policies-report-2019.pdf>

We found evidence of statistically significant lower log hourly wages among those migrants affected by the Act who, because of their landing date in Canada, had to wait an additional year to claim citizenship. Those negative wage effects persisted for two years after the policy became effective and did not fade out. The order of magnitude of the average effects is around a negative 11 percent hourly wage difference between migrants in the treatment and comparison group. The value of the DID results is robust irrespective of the addition of different demographic controls, including different bandwidths for landing months to define treatment and control groups, and, whether we used a semi-parametric DID estimator instead of a parametric one. We found no differences in the trends in wages between treatment and control groups before the Act was announced. Our DID estimates are not biased because migrant workers in the treatment and comparison groups differentially self-select into employment, as we found that the Act did not impact the employment decisions of migrants.

Our crucial hypothesis on the mechanisms that drive the effects of the Act on wages are that naturalization:

- 1) improved the information available to Canadian firms about attributes that define migrants' productivity;
- 2) enhanced migrants' bargaining power with firms, and;
- 3) given that skilled Canadian migrants who naturalized could now find jobs in the USA that had been out of their reach as non-Canadians, naturalization increased the competition among firms for highly skilled migrants.

We found that the effects of the Act on wages appear quickly as migrants naturalize. Moreover, those wage differences that occur early in the experience-earnings profile of migrants do not fade out dynamically. In our empirical analysis, we expected to find differences in job quality attributes between naturalized and non-naturalized migrants that would also appear early because those would drive the effects of the Act on wages. Differing from the wage effect dynamics, those differences in job quality attributes should only last while the citizenship eligibility rates between migrants in the treatment and control groups differed.

To assess if our data fit those hypotheses, we first evaluated the effects of the Act on indicators of job "quality". We found that while they were not eligible for citizenship employed migrants were less likely to hold a position in the public sector, more likely to be self-employed, and more likely to have a job with continuously varying work schedules. Migrant workers affected by the Act also worked more hours a week (and for a lower wage) than those not affected by it. Consistent with our hypothesis, all those effects lasted only until the migrants affected by the Act completed the new residency time of 48 months required by the immigration law. While the adverse earning effects of the Act appeared right after migrant workers naturalized and persisted during the whole period of the study (2015-2017), the impacts of the Act on the indicators of job quality were transitory, fading out as migrants in the treatment group reached eligibility.

This paper's main contribution is to show that the acquisition of citizenship does matter for Canadian immigrants' economic assimilation. Our results also indicate that imposing a waiting time for naturalization eligibility of three years, instead of four, is a comparatively more efficient strategy for Canada's immigration policy. We also show that the effects of naturalization on economic integration are apparently mediated by the increasing competition faced by Canadian firms when migrants naturalize, and by the fact that naturalization solves informational asymmetries between migrants and firms regarding migrant productivity for the jobs the firms are offering, as well as those workers' commitment to staying in Canada in the long term.

Policymaking has overlooked the role that naturalization rules play as tools to promote economic assimilation. Even if countries pre-screen migrants' potential for economic assimilation before accepting them as legal migrants (as Canada, the UK, Singapore, Australia, and New Zealand do), naturalization can still play a role as a tool in fostering their economic integration. Our research offers broad lessons for migration policy, especially since policymakers and politicians often refer to the Canadian immigration system as a benchmark.² Besides, with increasing waiting times for processing citizenship applications in countries like the United States, our results provide inputs grounded in evidence for considering the optimal waiting time for a migrant to become a citizen.³

The rest of the paper proceeds as follows. Chapter 2 reviews the literature and provides a conceptual framework that guides our empirical analysis. Chapter 3 describes the change in the law, the attributes of our database, and the empirical strategy. Chapter 4 presents the main results, including robustness tests and some analysis of the mechanisms driving the results. Chapter 5 concludes and offers some policy implications of our findings.

2 Naturalization and Migrants' Incomes

Our interest in the causal identification of the effects of naturalization on economic assimilation using nonexperimental methods does not mark the first time researchers have shown such interest. Notably, Gathmann and Keller (2018) isolated the impact of citizenship eligibility using policy changes in 1991 and 2000 that reduced the waiting time for German residents' naturalization. They found that women who naturalized earlier earned more than those who did so later (they found no impact on immigrant men's wages). Those effects were caused by the dual changes resulting from the policy in both the extensive and intensive labor supply margins.

Another attempt similar in intent to ours was Hainmueller, Hangartner, and Ward (2019). Those authors used the results from secret ballot referendums sent to citizens in 46 Swiss municipalities between 1976 and 2003. Citizens cast votes to decide on the naturalization applications of each immigrant resident in their municipality. They compared the long-term wages of migrants who narrowly won or lost their naturalization referendums and found an average 13.5 percent wage premium related to citizenship acquisition. In a related fashion, our research adds to that stream of causal inference literature by providing evidence of the effects of Canadian citizenship on wages employing a clear identification strategy.

In what pertains to the dynamics of the wage effects of naturalization, previous research has documented that a sharp increase in the wages of migrants immediately follows the acquisition of citizenship (see Jarreau (2015) for France; Steinhardt (2008) and Gathmann (2015) for Germany; Steinhardt and Wedemeier (2011) for Switzerland, and; Peters, Vink and Schmeets (2018) for The Netherlands). Such a discrete change in wages is consistent with our hypothesis: Firms rapidly respond to naturalization as a signaling device that facilitates access to better jobs and opens the job market to what otherwise would be legally restricted jobs (inside Canada and in the United States). We present evidence that this pattern of increases in wages immediately after naturalization also occurs in Canada.

² In a speech addressed to the US Congress in February 2017, US President Donald Trump called for "merit-based" immigration reform, citing the Canadian model as an example. The US Senate proposed new legislation in 2017 and 2019, entitled the Reforming American Immigration for Strong Employment Act, that, among other features, would have adopted a points-based system like the Canadian one. It did not become law. .

³ On February 12, 2019, 86 members of the US Congress sent a letter to the Director of Citizenship and Immigration Services expressing concern over documented delays in the processing of citizenship applications that effectively increased the time for becoming a US citizen.

Naturalization might increase the returns to skills and induce migrants to enter the labor force. Among those already working, citizenship would raise the reservation wages of migrants and push wages up. Naturalization would create more work opportunities, which would increase their bargaining power and promotions and lead to higher salaries or to a transition to better-paid jobs (Amuedo-Dorantes, Bansak, and Raphael (2009) describes how legalization of immigrants in the United States in 1986 had those effects).

Previous research testing the hypothesis that naturalization confers better job opportunities to migrants has found that compared to non-naturalized migrants, naturalized ones exhibit higher employment rates in the public sector. Naturalized migrants' jobs are more likely to be white collar and in unionized firms than the jobs held by other migrants. Self-employment is also more prevalent among migrants who are citizens than among migrants who are not. Papers also suggest that naturalization reduces discrimination in local labor markets and stimulates political participation (see, for instance, the evidence in Bratsberg et al. (2002); Gathmann and Keller (2018); Hainmueller, J., Hangartner, D. and Pietrantuono, G. (2017), Hainmueller, Hangartner, and, Ward (2019) and; Fougere and Safi (2006); OECD (2011)).

Prior research highlights that employers may be reluctant to hire and train migrant workers because they can only imperfectly observe the migrants' commitment to staying in the host country (or with the firm) over the long term.⁴ Besides, employers cannot quickly assess how familiar migrants are with the host country's customs and culture and with the institutions that affect labor productivity. The literature broadly shares the idea that naturalization gives a signal to employers that helps ease some of these informational problems, giving migrants more job opportunities and enhancing their bargaining power in the process (see Anderson and Huang (2019) for a recent review of this topic.) As proposed by Bratsberg et al. (2002), the transition to better jobs would mediate the Act's wage impacts, along with raises and promotions facilitated by naturalization. Consequently, we expect to find differences in the quality attributes of the jobs held by migrants who acquire citizenship compared with those who do not.

Recent work by Dostie, Li, Card, and Parent (2020) found that firm-specific pay and hiring policies in Canada contribute to the gap in earnings between immigrants and natives and that the earnings gap can be closed as migrants move from one employer to the next. If naturalization enables job mobility, then transitions in jobs from lower to higher quality ones would be a mechanism explaining differences in wages between naturalized and non-naturalized migrants. Evidence from France by Gianluca and Peri (2020) suggests that migration leads to a more effective practice of positive assortative matching. In local labor markets, high-quality firms screen job candidates (including migrants) to find high-quality workers. One can think of naturalization as lowering those screening costs that high-quality firms face when assessing migrant workers and influencing wage differences among migrants according to their citizenship status.

Notice that in Canada, the signal of migrant commitment that naturalization provides could convey different information to firms depending on where the migrant is positioned in the skills distribution. For less-skilled workers, naturalization would signal a commitment to staying in Canada. However, in tandem with opportunities for native-born Canadians, citizenship acquisition opens the labor market of the United States to migrant workers at the top of the skills distribution through the NAFTA agreement (DeVoretz and Pivnenko (2005)). De Matos and Parent (2019) document the cases of several highly skilled migrants who naturalized to leave Canada for the United States. (Bratsberg and Raquin (2012) document a similar phenomenon in Norway vis-à-vis the EU).

⁴ There is no empirical evidence of the magnitude of return migration in Canada. The total number of emigrants (including Canadian born) has been estimated at 7 percent of the Canadian population (Bérard-Chagnon(2018)). DeVoretz and Pivnenko (2005) estimate that 25 percent of the post-1986 migrants (up to 2004) had returned to Hong Kong with Canadian citizenship.

Naturalization might thus convey to Canadian firms some ambiguous information about the migrants' commitment to staying in Canada when they are highly skilled workers.

As highly skilled migrants naturalize, Canadian firms searching for them must compete with US firms to hire and retain them. The increased competition for them would press the wages of these highly skilled and naturalized migrants up. While naturalization would cause differences in wages between otherwise identical migrant workers, the mechanisms causing those effects differ throughout the migrants' skills distribution.

Suppose naturalization reveals information to firms about migrants' actual productivity and strengthens their positive assortative matching practice. Then those migrants who naturalize would have higher wages from higher quality jobs than those held by their otherwise identical counterparts who do not naturalize. Moreover, the differences in salaries induced by citizenship acquisition should appear according to the timing of naturalization, as this will reflect the speed with which profit-maximizing firms incorporate this valuable information into their screening, hiring, promotion, and pay practices.⁵

If naturalization leads to higher wages because it eases the information asymmetry problems that we described and facilitates the screening of highly skilled migrants by high-quality firms, then the Act should have differentially impacted job quality attributes that migrants held while some were eligible for citizenship, but others were not. Furthermore, those impacts on job quality attributes should only appear while those differences last. They should fade out as all migrants become eligible to claim citizenship. The dynamics of the effects of the Act on indicators of quality should reflect that temporal pattern of impacts.

Migrant workers who naturalize broaden their job opportunities, which improves their position to negotiate promotions and wage increases with their current employers. At the same time, naturalized migrants find it easier to transition to better paid and more stable jobs. Naturalization prospects encourage immigrant investment in country-specific human capital, which positions them better in the labor market. Assimilation is faster if transaction costs to acquire citizenship are low.

3 Empirical Strategy

3.1 The Canadian Citizenship Reform of 2014

Bill C-24, also known as the Strengthening Canadian Citizenship Act, was an initiative of the Canadian Government to amend citizenship eligibility requirements. It became law in June 2014.⁶ The objective of Bill C-24 was to “...*update eligibility requirements for Canadian Citizenship, strengthen security and fraud provisions, and amend provisions governing the processing of applications and the review of decisions*” (Bill C-24, 2014). The Government gradually rolled out the changes in the law between June 2014 and June 2015. In Appendix A, we summarize all changes associated with the Act that occurred before June 2017.⁷

⁵ While they are not referring to the explicit differences between migrants with and without citizenship, Fang, T., & Heywood, J.S. (2010) indicate that in Canada workplace characteristics, including the concentration of immigrants, might influence the ethnic gap in wages.

⁶ In Canada, the formal process through which an Act of Parliament becomes law is the receipt of Royal Assent. It signals that a bill has been passed by the two Houses of Parliament. The complete amendment can be found at <http://www.parl.ca/DocumentViewer/en/41-2/bill/C-24/royal-assent>.

⁷ In June 2017, three years after Bill C-24 was passed, Bill C-6 received Royal Assent, thus reversing most of the changes promoted by Bill C-24.

The most relevant impact of the Act on naturalization rules was a lengthening of the time required for citizenship eligibility beginning in June 2015. Under the previous law, legal residents could apply for citizenship if they resided in Canada for 36 out of the 60 months immediately preceding the date of their application submission. The new rule lengthened the requirement for a physical presence in the country to 48 out of the last 72 months. Consistent with those legislative changes, Figure 1 shows that in June 2015 there was a noticeable drop in the number of citizenship applications made by Canadian permanent residents. These residents kept their citizenship applications at low levels for about one year while they gained eligibility under the new rule. Only after June 2016 did the number of applications start to pick up again.

Figure 2 illustrates how the Act affected the number of citizenship applications from Canadian residents depending on how long they lived in the country. The Figure distinguishes three cohorts of immigrants. The first cohort unites all permanent residents who applied for citizenship between January 2013 and June 2014 (before the Act became law). The second cohort combines all applications made between June 2014 and June 2015 (within a year after the policy became law, yet before the Government began to enforce it). The third cohort shows naturalization requests that migrants made between June 2015 and May 2016 (within a year after the Government started implementing the new policy).

As we can see in Figure 2, immigrants applied for citizenship as soon as they reached eligibility. Notice that before June 2015 most migrants submitted their citizenship applications immediately after they had completed their 36 months in Canada. After June 2015, migrants submitted most applications after they had been in the country 48 months. The announcement in June 2014 of the new law that the Government would begin to enforce in June 2015 effectively split the pool of Canadian permanent residents into two groups: one group affected and another group unaffected by the new law. As time passed, those migrants affected by the Act, who had to wait 48 months to be eligible for naturalization, could apply for citizenship because their time living in Canada approached that limit. The Act thus caused migrants to "delay" their citizenship applications for a year, applications that, had the law not passed, would have been submitted after only 36 months living in the country.

In Figure 3, we illustrate how the Act affected citizenship eligibility for Canadian migrants over time to highlight some key aspects of our research design. The rows in the box represent cohorts that group migrants by the months when they arrived in Canada (between February and November 2012). The columns in the Figure split the time between June 2014 and June 2017 into four periods defined by the Act: the announcement of the Act, its implementation, and two subsequent periods that we call the mid-run and long-run.

To interpret Figure 3 with our identification strategy, first, imagine the timeline of an immigrant who arrived in Canada in June 2012 (represented in the Figure's mid-horizontal panel). When the Government announced the Act in June 2014, that migrant was not eligible for citizenship because he had only lived in Canada for 24 months. However, by June 2015, when the Government began to enforce the Act, he would have been living in the country for 36 months. He would therefore be eligible to apply for citizenship under the old rules. Let's now compare the timeline of this migrant to that of another migrant with the same background characteristics who arrived one month later (in July 2012.). The latter migrant would not be able to apply for citizenship in June 2015. He would have to wait until June 2016 to be eligible for citizenship (an entire additional year compared to the former migrant).

Our study exploits this exogenous assignment of the time for eligibility of citizenship created by the Act to identify the causal effects of citizenship acquisition on wages. Moreover, since there was a one-year gap between when the government enacted and enforced the law, we examined the policy's impacts before and after these points in time to investigate its dynamic effects. We split the post-Act period in four as follows:

1. We estimated the impact of the Act on wages during the transitional period leading to the Act's full enforcement. We call this period the "*post-announcement*" period (from June 2014 to May 2015). Starting in December 2014, the migrants in the treatment group (those who had resided in Canada for 36-40 months by June 2015) began to be eligible for citizenship. The effect of the Act on wages during the post-announcement period can be interpreted as the joint effect of reaching eligibility and applying for citizenship (without receiving it). We hypothesize that if there are effects, they should be small for those are averages across time periods when no migrant was eligible and when some migrants were eligible.
2. We also estimated "short-run" policy effects for the "post-implementation" period between June 2015 and May 2016.
3. We computed the Act's impacts during the "*mid-run post-implementation period*" (from June 2016 to May 2017). We followed the migrants until enough time had passed so that most of the migrants who joined the control group would have been able to obtain their citizenship. In contrast, application eligibility was only beginning to take effect for the treatment group.
4. We tried to capture the "*long-run post-implementation*" (from June 2017 to December 2017) effects to see if any differential outcomes had persisted while the treatment group also became citizens.

The choice of these analytic periods responds to the hypothesized dynamics of the effects of the Act on wages that we laid out in the introduction and reiterate here. The effects of the Act on wages should appear as soon as migrants acquire citizenship and should not fade out. As more migrants naturalize, firms' competition for skills, and migrants' bargaining power with firms also increase. The impact of the Act on indicators of job quality enabled by those processes should be contemporaneous with the wage effects. Nevertheless, those early impacts on job quality should fade out as migrants in the treatment group reach eligibility for citizenship.

2.2 The Analytical Database

Our analytical dataset is comprised of data from two sources. The first data source combines immigrants who were interviewed between 2013 and 2017 by the Canadian Labor Force Survey (LFS), a monthly cross-sectional, nation-wide survey that has been conducted by Statistics Canada since 1976. Every month, the LFS provides information on the demographics of respondents and closely tracks their employment status, hours worked, wages and salaries, work schedules, and respective occupations. The LFS also determines if the respondent ever previously arrived as a landed immigrant in Canada. If so, the landing year and month is recorded.⁸

We pulled out monthly extracts of the LFS data between June 2013 and December 2017. We recovered the landing date information from those monthly databases and classified immigrants affected and unaffected by the 2014 Act. We formed a treatment group with immigrants who landed in Canada 31 to 35 months before June 2015, and a comparison group with those who landed 36 to 40 months before that date.

A second dataset that complemented the information from LFS was employed to add certain characteristics of Canadian permanent residents at landing time. The Permanent Residence Landing File (PRLF) is a database of administrative records covering immigrants who landed in Canada between 2003 and 2013. In addition to the exact landing date, PRLF records permanent residents' characteristics at

⁸ Only the landing year (not the landing month) was recorded for immigrants who landed more than five years prior to the time of the interview. This limited our ability to track the immigrants' labor market outcomes after December 2017, since we were not able to precisely identify our cohorts' months of landing and determine their treatment status.

landing: It includes their country of birth and citizenship, immigration categories, and the intended province of destination in Canada. The PRLF also collects the demographic attributes, educational attainment, and occupation of the newcomers to Canada. We used the PRLF data to impute migrants' characteristics when they arrived in Canada and extend the control variables available from LFS. We first aggregated the data based on landing date, source country, age, and gender and then linked those aggregates to individuals who shared these common attributes across the two databases.⁹

Our unit of analysis is migrant males aged 25–64 at landing time. Migrant males were more likely to be the principal applicant for both permanent residency and citizenship than women and were also more active in the labor force.¹⁰ We identified approximately 250 male immigrants in each monthly sample from the LFS, which the LFS had evenly split between treated and comparison groups. Our analytical database combined 6,313 individuals. In Appendix B, we discuss in more detail how we selected the migrants that would be part of our analytic sample, how we cleaned the variables in both sources, and how we constructed key indicator variables that we use in the present research. We highlight that, while the Act might have resulted in return migration for those residents in Canada who were arriving near the time when the Act was enacted, we rule out the possibility that those effects would be binding for individuals who had already resided in Canada for some time in our sample.

In Table 1, we present descriptive statistics of predictors of wages for all immigrants in our sample, and by their treatment status. We report sample means and standard deviations (in parentheses) of those predictors of wages during the pre-policy announcement period (June 2013 to May 2014) for the comparison group (column 3) and the treatment group (column 5). In column 7, we present the differences in the means during the pre-policy announcement period. The asterisks next to the estimates in column 7 indicate that the mean difference across those two groups is statistically significant.

We found that at the time of landing, the immigrants that we grouped in the treated and comparison groups had balanced characteristics across most dimensions. There were, however, some differences in educational attainment across the groups, as those migrants in the treatment group were 6 percent more likely to hold a university degree than the migrants in the comparison group. Migrants in the treatment group were also 6.2 percent less likely to come from a developed country, and a similar percentage was likely to come from an English-speaking country. We also observed some differences between the migrants in each group in terms of their occupation when they arrived in Canada.

In the study we examine the effects of the Act on log hourly wages, and across other labor market outcomes. Those other outcomes include indicators for employment status (= 1 if employed; = 0 if unemployed); counts of the number of usual hours worked per week at all jobs (conditional on being employed); indicators of having varying working hours from week to week, 5) indicators of working in the public sector, and 6) indicators of being self-employed.

In Table 2, we show descriptive statistics of all these outcomes. The migrants in our data exhibit a very high labor force participation and employment rates (89.7 percent and 93.2 percent, respectively). The immigrants also engaged in long work hours, with average weekly hours reaching 40.5. Also, around 30 percent of the immigrant population had varying work schedules. Approximately 10 percent of them worked in the public sector, and another 10 percent chose to be self-employed.

⁹ Both datasets, i.e., the LFS and the PRLF, are confidential, and access to them can only be granted through Statistics Canada's Research Data Centres.

¹⁰ Analysis of the impacts of the Act on female labor supply is slated as a topic for future research.

2.3 Identification Strategy

Let y_{it} represent the wages of an immigrant who has Citizenship status, i (with $i = 1$ if the migrant is in the treatment group of those affected by the Act, and $i = 0$ otherwise), at time t (time is denoted as a two-period indicator, with $t = 0$ and $t = 1$ referring to the periods before and after the policy announcement, respectively).¹¹ The wages are a function of citizenship status, C_{it} (also denoted as an indicator variable that turns 1 if the individual is a citizen and 0 otherwise) and a set of additive separable unobserved factors as in the following variance components model:

$$y_{it} = C_{it}\delta + U_i + b_{it} + a_{it}. \quad (1)$$

Notice that $C_{i0} = 0$ is true for all immigrants in our sample, as citizenship eligibility status only diverged after policy announcement at $t = 1$. According to LaLonde and Topel (1991), the first error factor, U_i , denotes the mean value of an unobserved time invariant of migrant i . The second factor, b_{it} , represents the impact of labor market conditions at time t on migrant i . Finally, a_{it} describes the average, country-specific, accumulated stock of human capital of migrant i up to time t . The parameter of interest δ represents the mean difference in wages between migrants who were eligible for citizenship and those who were not after changes to the citizenship law were enforced.

Subtracting the outcome equation by treatment status ($i = 1$ and $i = 0$) in ($t = 1$ and $t = 0$), we obtain:

$$\begin{aligned} y_{11} - y_{01} &= (C_{11} - C_{01})\delta + (U_1 - U_0) + (b_{11} - b_{01}) + (a_{11} - a_{01}) \quad (2) \\ y_{10} - y_{00} &= (U_1 - U_0) + (b_{11} - b_{01}) + (a_{11} - a_{01}) \quad (3) \end{aligned}$$

Subtracting the first of those first-difference equations from the second leads to:

$$\Delta_{(y)} = \delta + \Delta_{(b)} + \Delta_{(a)} \quad (4)$$

where, to simplify the notation, we have denoted $\Delta_{(y)} = (y_{11} - y_{01}) - (y_{10} - y_{00})$; $\Delta_{(b)} = (b_{11} - b_{01}) - (b_{10} - b_{00})$; and $\Delta_{(a)} = (a_{11} - a_{01}) - (a_{10} - a_{00})$, and takes advantage of the fact that $C_{11} = 1$ and $C_{01} = 0$.

Equation 4 suggests that we can recover an unbiased estimate of the age effect δ using a difference-in-differences (DID) estimator. However, in our context, DID might be a necessary but, perhaps, insufficient strategy for identifying the wage effect of citizenship acquisition. Besides differencing out time-invariant unobservable factors (as we can with the DID estimator), two empirical challenges remain. First, we need to ensure that the migrants in each group were not affected differently by labor market conditions characteristic of different stages of the economic cycle (so that $E[\delta|\Delta(b)] = \delta$). Second, we would like to equate the rate of accumulation of country-specific skills ($\Delta(a)$) across the migrants in both groups.

To deal with those threats to identification, we selected migrants to minimize the differences in the landing dates between those in the treatment and comparison groups. These small differences reduce the opportunity for individuals in the control group to, by the time the Act becomes effective, have obtained more country-specific human capital than those in the treatment

¹¹ There was a “vacatio legis” of one year after the policy announcement; thus, treatment status could be changed in subsequent periods for those in the comparison group.

group (or vice versa). Also, as will be shown next, our empirical models would control the set of factors presented in Table 1, which can be considered both predictors of wages and human capital investment decisions. Simultaneously, the short window of time that differentiates when migrants in the treatment and comparison groups arrived in Canada reduces the possibility that migrants in different groups would have come to Canada at different stages of the economic cycle.

Our empirical model is a parametric analog to the DID estimator presented in equation 4. Cross-sectional data was pooled to estimate the parameters for the following equation by ordinary least squares (OLS):

$$y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \delta Post_t \times Treat_i + \gamma_1 X_{i0} + \gamma_2 Z_{it} + u_{it}, \quad (5)$$

where $Treat_i$ is an indicator for treatment membership and $Post_t$ is an indicator variable that equals 1 if the observation corresponds to the post program Period 1 and 0 otherwise (Period 1 spans from June 2014 to December 2017). As we will describe in more detail next, X_{i0} is a vector of pre-determined characteristics measured at the baseline period $t = 0$, which includes individual controls at landing. To account for macroeconomic and labor market trends, as well as an individual's accumulated experience in Canada, we obtain vector Z_{it} , consisting of year and quarter fixed effects, and the time since landing as a proxy for the accumulation of country-specific human capital. δ is the treatment effect of interest. y_{it} denotes the short-run labor market outcomes of these immigrants. Estimates of treatment effects on wages were estimated for the four post-reform time periods ($t = 1$) described in Section 2.1, i.e., post-announcement, post-implementation, and mid-run and long-run post-implementation periods. We also calculated the effects of the Act by averaging the outcome observed between 0 and 2.5 years after the policy announcement.

We specify five alternative models (Model 1-Model 5), each with an increasing set of control variables as detailed in Table 3. As we indicated before, several of the control variables (denoted with the label "Pro" in Table 3) were imputed to the LFS from the PRLF by linking aggregate data by gender, year and month when landing and country of birth across the two databases (see Appendix B for details).

Model 1 in Table 3 represents an unconditional comparison of the outcome with no controls. In Model 2, we account for macroeconomic and labor market trends, controlling for year and quarter fixed effects when observing the outcome variable. We also include a variable that captures the probability of landing in each of the 10 Canadian Provinces. In Model 2, we also add the unemployment rate in each province when the immigrant landed in Canada. In Model 3, we expand the set of control variables to include demographics, education, and occupation controls at the landing time. We added control variables to hold constant the migrant's age and that of his children (categories). In Model 3 we also added some imputed variables that aim to control the migrant's education level at the landing time. Model 4 adds country of birth fixed effects, one indicator variable turning one if that country is a developed one, and another turning one if English is the primary language (in both cases, the variable turns a value of zero otherwise). Lastly, Model 5 adds controls for the probability that the migrant migrated into three immigration categories (economic, refugee, or student).

As we will show next, after controlling for pre-existing differences in observable characteristics and differentiating time-invariant unobservables, the wages of immigrants who had landed in a close enough period were not systematically affected by any other factors except for those associated with the Act. What differentiates landed immigrants in June 2014 (when the Canadian Government announced the law) is that some of them who arrived in Canada later were ineligible for citizenship applications after three years of physical residence in the country.

4 Results

4.1 Effects of Citizenship on Log Hourly Wages

In this section, we present the key results of this research. We first show that the DID estimates of the impact of the Act on wages are robust to the covariates we include in the models we use. We also show that those effects are dynamically heterogeneous as their magnitude changes over time after the Government announced the policy in June 2014. Second, we demonstrate that we can hold valid the "parallel pre-trends trends" assumption underlying the DID estimator (CITE). Third, we highlight that the estimates of the wage effects change little if we extend the time frame to sample migrants at each side of the threshold that defines citizenship eligibility. Fourth, we show that the wage effect estimates are similar if the DID estimator is semiparametric instead of parametric. Finally, we show that the DID estimates of the wage effects are similar to those that we computed on yearly earnings using an alternative administrative database that links migrants with their tax records.

4.1.1 Main Estimates

In Figure 4 we present estimates of the effects of the Act on log hourly wages computed for five sets of covariates presented in columns 1-5 in Table 3. We computed those estimates in the four time periods that we described in Section 3.1 that details our empirical strategy: 1) the policy announcement period (shown in the upper-left graph), 2) the policy implementation period (upper-right graph), 3) the mid-run policy implementation period (lower-left graph), and 4) the long-run policy implementation period (lower-right graph). Dispersion in each estimate is represented by 95 percent confidence intervals for robust standard errors, as depicted by the capped lines extending from the mean estimates. The solid dots indicate that the mean estimate is statistically significant at 10 percent.

The DID estimates in Figure 4 show that the Act adversely impacted immigrants' wages. Those migrants who waited an additional year to claim citizenship were paid lower wages than those who could apply as soon as they become eligible after 36 months of residing in Canada. As we can see in each of the four graphs, the estimates are very similar in magnitude, irrespective of the set of control variables for which we controlled. This systematic pattern suggests that we achieve identification of the treatment effect by differencing out time invariant unobservable factors with the DID estimator, and that covariates play a minor (yet not negligible) role in our design.

The effects of the Act are always negative in sign yet vary dynamically. The citizenship premium changes from 7 percent during the policy announcement period to 13 percent during the policy implementation period. The policy effects of the Act on wages do not dissipate toward the end of the period under analysis. Because of the importance that early wages have for longer-term earnings trajectories, those lower effects on wages could have long-lasting implications throughout the age-earnings profile of migrants affected by the Act.

4.1.2 Testing the Parallel Trends Assumption

An essential assumption for identifying policy effects using the DID estimator is that the treatment and comparison groups follow parallel trends in the outcome variable of interest before treatment. To conduct formal tests on this assumption, we follow the work of Autor (2003) and introduce leads and lags into the DID framework. We estimated the following equation:

$$y_{ict} = \beta_t D_{ct} X_{ict} + \gamma_c + \varphi_t + \epsilon_{ict} \quad (6)$$

where y_{ict} is the labor market outcome for individual i in landing cohort c at time period t .

As explained before, the Government announced the Act in June 2014, so we use that time to anchor the pre- and post-program periods and treat $t = 0$ at that time. We include 12 lags to study the pre-trends and 36 leads to study the dynamic effects after the policy announcement. D_{it} is an indicator variable turning to 1 for the treated cohort observed in the post-announcement periods $t > 0$. We also control for observable characteristics using the same set of variables described in the DID estimates that we presented in Figure 4 (those come from the specification that we call “Model 5”. In Table 3) In Figure 5 we show the β 's estimates from equation 6 to illustrate the differences in wages between treated and comparison groups in the pre- and post-treatment assignment periods. The Figure shows that there are no significant differences in log hourly wages before June 2014; i.e., the common pre-trend assumption holds.

As we previously indicated, the treatment and comparison groups combined immigrants who landed 31-35 months before June 2015 in the former and 36-40 months before that date in the latter group. To explore how sensitive the estimates are to choosing different sampling bandwidths, we reproduce estimates of the post-implementation period in the top panel of Table 4 from Figure 4 (where we presented estimates of the effects of the Act on hourly wages across periods), but employ first a one month and then a three-month bandwidth. As we can see in columns 2, 3 and 4 of the Table, the computed effects that employ different bandwidths are all negative in sign and statistically significant at conventional statistical precision levels. Notice also that, despite the fact that the magnitude of the effect is bigger when we computed it using a one-month bandwidth, it is not statistically different from those estimated using either a three- or a five-month bandwidth.

In addition, since we are simultaneously testing the hypothesis of multiple outcomes, the probability of false positives increases accordingly. To address this issue, we monitored the False Discovery Rate (FDR) or the Type I error at a conventional level of significance. We followed a linear step-up testing procedure developed by Benjamini et al. (2006), and computed q-values (or the adjusted p -values). We provide those results in parentheses, with asterisks indicating the level of significance for that estimate. All results are statistically significant, when the baselines are also significant, after correcting for FDR.

4.1.3 Estimates of the Effects of the Act Using Other Estimators

If the relationship between the wages of Canadian immigrants and their citizenship status is non-linear, then it is possible that our choice of a linear DID estimator would induce bias due to specification error. We would like to rule out the possibility that linear extrapolations of what in fact is a non-linear link function drives our results. We computed semi-parametric estimates of the effects of interest in the post-announcement period using a propensity-score-matching difference-in-differences (PSM-DID) estimator (Smith and Todd, 2005). Such an estimator deals with selection based on observables and time-invariant unobservables and addresses non-linearities between the treatment status and the wage outcome. We calculated those PSM-DID estimates over common support areas, which we defined after calculating a propensity score with all covariates included in the previous models. In computing the PSM-DID estimates, we selected the weighting scheme and the bandwidth to create counterfactual outcomes that balance the characteristics that exhibit significant differences between the treatment and control groups at baseline. We show the PSM-DID estimate in Column 5 of Table 4. We highlight that the value of the wage effect estimated with the PSM-DID is very similar to the other linear DID estimates presented in the Table, despite being less precisely estimated.¹²

¹² Implementation of the PSM-DID estimator was conducted in two stages. We calculated a propensity score in the first stage, including those pre-determined variables found to have statistical significance in both treatment and comparison groups in Table 1. In the second stage, we computed two sets of point estimates of the effects of interest:

4.1.4 Selection on Unobservables

As we noted before, there are differences (statistically significant) between migrants in the treated and comparison groups in some variables that predict earnings, such as age, educational attainment, and occupation at landing. One remaining concern might be that, while the difference in difference approach will remove any time-fixed differences across the treatment and control groups, time-varying differences across these groups might drive our results. We address that concern by comparing estimates from DID and OLS and exploring how much was removed by accounting for time-fixed differences. If we obtained estimates that are very similar across the two estimators, then the primary source of bias could be unaccounted time-invariant unobservables.

In the top panel of Table 4, we compare estimates of the effects of the Act on log hourly wages estimated using OLS with DID estimates of that same effect. As can be seen in Column 1, the OLS estimate is considerably smaller than any of the DID estimates (it is not statistically significant). Differences between OLS and DID suggest that the DID models address selection on unobservables factors. If we relied on the OLS model, whose identification assumption is one of "selection on observables," we could be mistakenly underestimating the policy's effects on wages.

Despite the fact that 90 percent of the migrant sample is employed at baseline, as we showed in Table 2, we would like to discard the possibility that the factors that eventually drive the self-selection of migrants' into employment also drive their wage effects. To test that hypothesis, we first evaluated whether the Act impacted the migrants' employment decisions in our sample. In Figure 6, we show estimates of the impact of the Act on the probability of being employed using an indicator of "employment status." The variable takes value one if the migrant is employed and zero otherwise. As we can deduce from the Figure, the Act did not impact the extensive labor supply margin. In most of the graphs in Figure 5, the effects are very close to zero in magnitude and not statistically significant. That result holds irrespective of the specifications that we use and the reference period we analyze, yet it is particularly prominent in our preferred specification (Model 5 from Table 3).

We provide a last validity check to rule out the possibility that selection on unobservables drives our results. In Table 5, we report policy effects of the Act on log hourly earnings for the four relevant periods estimated before using DID (Column 1) and a Heckman's two-steps selection correction estimator (H2SLS Column 2). In both models, we control for all covariates in Model 5 of Table 3. The H2SLS estimator includes, as instruments, indicators for whether the spouse is employed (if a spouse is present) and her employment status and earnings.

The estimates that we present in Table 5 show that when we use a Heckman selection-corrected estimator, an estimator that explicitly models unobservables that explain employment status decisions, the value of the citizenship premium remains unaltered. Our results suggest that the DID estimator dealt with unobservable factors that could have led some migrants in one group to seek jobs more actively than those in the other group (such as differences in ability or motivation). A harmful impact from extending the waiting time for citizenship eligibility by an additional year appears to drive the difference in wages found in our data consistently.

one concerning the outcome measured before the policy announcement and the other regarding the policy implementation period. The differences between these estimates are our DID treatment estimates. We bootstrapped the standard errors. Balancing tests using the weighting schemes implied by the estimators indicate that the samples are balanced ex-ante in most of the observables (+ 95 percent of the variables).

4.1.5. Evidence of Effects of the Act on Yearly Earnings

To verify how robust our estimates are to the use of alternative data sources, we computed the effects of the Act on yearly earnings using a longitudinal database of administrative records—the Longitudinal Immigration Database (IMDB, also produced by Statistics Canada). The IMDB combines administrative records from the Immigration, Refugees and Citizenship Canada department with annual tax records from the Canada Revenue Agency for all immigrants who landed in Canada between 1980 and 2016. IMDB contains the characteristics of immigrants at the time of their landing and income information from their tax records. To compute citizenship's impacts, we restricted the migrants' population from the IMDB in a similar fashion as we did with the LFS survey.

The IMDB data that we use combines the population of migrants who are male, aged 25-64 at the time of landing, and who would have lived in Canada between 33 and 38 months by June 2015. We estimated the DID model from Equation 5 using yearly earnings from the IMDB between 2012 to 2016 (excluding 2014 -- the year when the Canadian Government announced the Act). As with previous estimations, we control for covariates resembling those used in Model 5 from Table 3. Those covariates include the age of the migrant (and its squared term), and a battery of indicators for educational achievement levels, marital status, occupation, and children's ages (grouped in categories). We also added indicator variables for knowledge of either French or English (or both), and fixed effects for the type of immigration category, source country, and the year when the earnings outcomes were recorded.

In Table 6, we present the DID estimates of the effects of the Act on the yearly earnings of immigrants and on their employment status (with an indicator variable for positive earnings). We offer estimates computed with three alternative samples of immigrants that landed +/-1, +/-2, and +/-3 months around June 2012. Three observations are worth making about those results in Table 6. First, notice that the three estimates' magnitudes in columns 1, 2, and 3 are very similar. As we enlarge the landing time windows, the yearly earning premiums estimated by DID do not change significantly. Second, notice that the treatment effects (all of which are statistically significant) are signed negative, reinforcing our previous conclusion that the Act depressed migrants' economic prospects (the magnitude of the impact is between 11 and 13 percent lower earnings for migrants in the treatment group). Finally, as with the LFS data, we also found that the effects of the Act are not explained by unobservable factors that explain self-selection in the labor market, as the effects of the Act on employment are zero.

To reconcile the results in Table 6 with our estimates of the effect of the Act on hourly wages, we remind the reader that we found that migrants in the treatment group increased their working hours during the policy implementation period (from June 2015 to June 2016). Those effects faded out after that. In parallel, we also found that migrants affected by the Act consistently exhibited lower wages after June 2014. The impact of the Act on yearly earnings averaged over the years 2015 and 2016 portrays a weighted average of effects of the interaction between working hours and hourly wages that changes dynamically during those years (the weights will average data of that interaction over: 1) six months during the announcement period (January-June 2015); 2) one year during the implementation period (July 2015-June 2016) and; 3) six months during the mid-run period (July-December 2016)).

If, as we have found, the wage effect dominates the hours' effect over time, then seeing that yearly earnings in 2015-2016 are also adversely affected by the Act reinforces our main result: the Act harmed the wages of migrants in the treatment group. We think this consistency in results across databases (and outcome variables) speaks to our empirical strategy's robustness to recover the causal effects of the Act on wages.

4.2 Analysis of Mechanisms Driving the Effects of Citizenship on Wages

Why would there be differences in hourly wages between migrants who seem identical in all other attributes but in their citizenship status? As we previously indicated, our central hypothesis is that when a migrant acquires citizenship, this status conveys information to employers about commitment to staying in the country and sharing country and firm values that affect job productivity. From that perspective, one would expect that until they become citizens, migrants would be offered jobs of lower quality than their naturalized counterparts. That differential access would impact the hourly wages they earn. Furthermore, we can expect that if wages start at a lower level, they remain lower over time. However, the differences in job quality attributes that drive those initial differences in wages would only be temporary and dissipate as all migrants acquire citizenship.

To falsify those hypotheses, we use the LFS data to compute the effects of the Act on several outcome variables that proxy for job "quality" attributes that predict wages. Those variables include work in the public sector,¹³ self-employment and having a work schedule with hours that continuously vary. If we find that the Act adversely correlates with those proxies for job quality attributes, it can be plausibly suggested that the costs of accessing quality jobs drives the impact of the Act on wages.

In Figures 6, 7, and 8, we show DID estimates of the effects of the Act on the likelihoods of: 1) being employed in the public sector ("Public Employee" in Figure 6); having a work schedule with hours that constantly vary ("Working Hours Vary" in Figure 7), and the likelihood of being a self-employed worker ("Self-Employed" in Figure 8)." We computed those effects using the same five model specifications and present those across the same four analytics periods that we employed to estimate the effects of the Act on hourly wages (presented in Figure 4). Our narrative next focuses on the results from "Model 5" (the specification with the most inclusive set of covariates). To validate the use of the DID estimator, and to resemble what we did with the estimates of the effects of the Act on wages, we show in Figure 9 that there are no differences in the trends of those outcome variables by treatment status before the Government announced the Act in June 2014.

Our findings suggest that immigrants in the treatment group were less likely to work in the public sector (Figure 6) and more likely to be self-employed (Figure 8) than other migrants.¹⁴ Notice that those patterns only persisted while the treatment group was ineligible to apply for citizenship (i.e., during the policy-implementation and medium-run periods). The effects dissipated after that. We also found that immigrants whose eligibility was delayed by the Act were more likely to work in jobs with irregular schedules. These effects were particularly evident during the policy implementation period but gradually faded out later on, as did the two previous outcomes (Figure 7).

¹³ In Canada, working for the public sector involves job stability, decent wages, and numerous perks. In some cases, appointments are legally restricted to citizens.

¹⁴ In addition, several institutional barriers also would make it costlier for employers with high quality jobs to hire non-citizens, compared to citizens with equivalent levels of on-the-job productivity. Employment in some public sector and unionized jobs are limited to citizens. Canadian employers hiring non-citizens also must paid costs from scrutinizing the documentation that is needed to determine a workers' legal status and avoid breaking labor laws (in Canada hiring an illegal worker is subject to employer fines under Canada's Immigration and Refugee Protection Act). Lastly, International travel restrictions for non-Canadian citizens who require visas to enter other countries might also affect their likelihood of being hired in certain specific occupations with well-paid jobs for which they could be equally qualified than other immigrants with Citizenship status.

If, as it is usually assumed, leisure is a normal good, then non-naturalized migrants who earn less per hour than their peers who have citizenship could compensate for their income differential by working more intensely. Working longer hours while achieving a lower salary would suggest that non-naturalized migrants have poor access to quality jobs. In other words, with less effort, naturalized migrants could achieve the same income levels as their non-naturalized counterparts. If the Act positively impacted hours worked and negatively affected wages, then finding that the Act induces longer working hours at lower wages would suggest that lower-quality jobs drive the impact of citizenship on wages.

We validate this adaptative mechanism by inspecting the effects of the Act on the intensive margin of labor supply. In Graph B of Figure 10, we show that working hours increased among employed immigrants in the treatment group after the Government started to enforce the Act. Soon after the Canadian Government implemented the policy, there was a mean difference of nearly two hours per week between those affected and not by the Act. In a similar fashion as with the other job quality indicators, those effects of the Act faded out through time (there are no effects either in the medium-run or beyond).

Our results are consistent with the hypothesis posed before: that Citizenship acquisition could be communicating to employers a signal of immigrants' commitment to the country in the longer term. Gaining citizenship would increase an immigrant's likelihood of being hired for a well-paid and stable job. However, the Act could have also flattened the growth rate of some immigrants' wages by, for instance, reducing their likelihood of being promoted within a firm or taking on a different job at another firm. Those hypotheses, however, cannot be disproven with the current data.

The evidence presented in this section suggests that the Act affected migrants' wages by enabling better access to jobs to those able to acquire citizenship earlier than their not yet naturalized counterparts. These effects on job quality attributes lasted until those migrants that the Government required to reside in Canada for 48, rather than 36 months, lived long enough in Canada to reach the new time threshold for residency. However, by depressing their wages early on, the Act appears to have somehow permanently depressed (or at least during the time we can observe from current Canadian data) the salaries of those immigrants required to reside longer in the country to be eligible for citizenship.

6 Conclusions

At the beginning of this paper, we asked whether citizenship acquisition improves migrants' economic assimilation in Canada. Our empirical analysis shows evidence suggesting that other factors being equal, naturalized citizens earn higher wages than their non-naturalized counterparts (approximately 11 percent more).

That under the Act, some migrants had to wait one additional year to claim citizenship, while others did not, was a "naturally" occurring sorting process. The enactment of the Act put exogenous variation into the likelihood of becoming a citizen that allowed us to simulate random assignment conditions as it would have happened in a randomized controlled experiment. We provide evidence of citizenship's causal effects on economic assimilation with a clean identification strategy that ties an immigration policy to the behavioral responses of immigrants affected by it.

Within that causal inference framework, we found that those immigrants able to acquire citizenship after living for three years in Canada were better positioned in the labor market than

those who had to delay their citizenship applications an additional year because of the policy change. In the short term, both earning capacities and the likelihood of landing a job with deserved “job quality” were negatively affected by the Act. Our results also suggest that, because of those baseline differences in hourly wages induced by changes in the migration policy, the longer-term wage growth trajectory differs across the two groups, favoring naturalized migrants.

Our results indicate that, on efficiency grounds, delaying citizenship acquisition can be costly for society: An initial 11 percent difference in earnings can result in a substantial portion of the migrant population being permanently below the threshold where tax contributions are above welfare transfers. On equity grounds, naturalization policy should provide a predictable and stable plan with clear and stable rules for all migrants. We have shown that society pays the price when policymakers manipulate elements of migration policy to favor their political clientele. Providing stable perceptions of fairness around migration policy may benefit members of society, beyond migrants. Suppose the objective is to compete efficiently with developed countries to attract the world's most talented human capital. In that case, establishing an evidence-based time for naturalization eligibility, and committing to its stability through time, is a priority.

Our analysis suggests that firms value the clear signal of migrants' commitment that citizenship reveals. This signaling might be particularly important for those firms that heavily invest in their employees' human capital because their associated risk of losing those investments is inversely proportional to that commitment. Lack of citizenship might have impacted hiring decisions and the timing and likelihood of promotions, with longer-term implications for wage growth. When migrants lack citizenship beyond a specific time threshold, they appear to become systematically disconnected from opportunities in the labor market for gaining access to well-paid, stable jobs and those characterized by steep growth in wages.

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Tables

Table 1: Descriptive Statistics of Covariates (Full Sample: June 2013 to December 2017)

Explanatory Variables	(1) NOB	(2) Sample Mean	(3) Control Pre	(4) Control Post	(5) Treatment Pre	(6) Treatment Post	(7) First Diff. (5)-(3)
Age at landing	6,313	36.355 (8.648)	36.923 (9.423)	36.180 (8.769)	36.081 (8.014)	36.452 (8.506)	-0.842* [0.421]
Youngest child 0-5 at landing	6,313	0.207 (0.405)	0.206 (0.405)	0.170 (0.376)	0.241 (0.428)	0.230 (0.421)	0.036 [0.02]
Youngest child 6-12 at landing	6,313	0.130 (0.336)	0.139 (0.346)	0.140 (0.347)	0.131 (0.338)	0.117 (0.321)	-0.008 [0.016]
Youngest child 13-18 at landing	6,313	0.040 (0.196)	0.052 (0.222)	0.030 (0.171)	0.053 (0.223)	0.040 (0.197)	0.001 [0.011]
Prob(Married) at landing	6,292	0.739 (0.257)	0.725 (0.277)	0.742 (0.249)	0.744 (0.245)	0.739 (0.262)	0.018 [0.013]
Educated in Canada	6,313	0.087 (0.282)	0.066 (0.248)	0.101 (0.301)	0.065 (0.246)	0.090 (0.286)	-0.001 [0.012]
Educ: Secondary or less at landing	6,313	0.169 (0.375)	0.159 (0.366)	0.159 (0.366)	0.158 (0.365)	0.188 (0.391)	-0.002 [0.017]
Educ: Cert. or some Univ. at landing	6,313	0.312 (0.463)	0.324 (0.468)	0.326 (0.469)	0.268 (0.443)	0.312 (0.464)	-0.057** [0.022]
Educ: Univ. and above at landing	6,313	0.519 (0.500)	0.516 (0.500)	0.514 (0.500)	0.575 (0.495)	0.500 (0.500)	0.058* [0.024]
Prob(Mgmt/Prof) at landing	5,916	0.509 (0.284)	0.482 (0.296)	0.508 (0.291)	0.526 (0.278)	0.512 (0.275)	0.044** [0.014]
Prob(Tech/Trade) at landing	5,916	0.319 (0.216)	0.334 (0.241)	0.309 (0.214)	0.327 (0.219)	0.320 (0.205)	-0.007 [0.011]
Prob(Manual) at landing	5,916	0.172 (0.200)	0.184 (0.204)	0.183 (0.214)	0.148 (0.168)	0.168 (0.195)	-0.037*** [0.009]
Prob(Economic class)	6,292	0.711 (0.279)	0.706 (0.284)	0.682 (0.290)	0.724 (0.284)	0.735 (0.262)	0.018 [0.014]
Prob(Refugee)	6,292	0.015 (0.091)	0.010 (0.064)	0.020 (0.114)	0.017 (0.092)	0.011 (0.070)	0.007 [0.004]
Prob(Student)	6,229	0.096 (0.295)	0.145 (0.353)	0.076 (0.264)	0.124 (0.329)	0.087 (0.282)	-0.022 [0.016]
Source Cntry: use English	6,313	0.372 (0.483)	0.404 (0.491)	0.350 (0.477)	0.346 (0.476)	0.395 (0.489)	-0.058* [0.023]
Source Cntry: developed	6,313	0.263 (0.440)	0.317 (0.466)	0.238 (0.426)	0.255 (0.436)	0.272 (0.445)	-0.062** [0.022]
Unemployment rate in current province at time of landing	6,313	0.060 (0.020)	0.060 (0.018)	0.059 (0.020)	0.062 (0.021)	0.060 (0.019)	0.001 [0.001]

Note: Other independent variables not listed in this table include: birth country dummies and probability of intending to land in a province group. Pre-policy refers to the period before June 2014. Standard Deviations are in (parentheses); Standard Errors are in [brackets]. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 2: Descriptive Statistics of Outcome Variables (Full sample: 06/ 2013 to 12/2017)

	(1) NOB	(2) Mean	(3) Std. Dev.	(4) Universe
In the labor force	6,298	0.897	(0.304)	All respondents 15+
Employment Status (E/U)	5,651	0.932	(0.252)	In the labor force
Usual Total Hours Worked, cond. Inlf	5,266	40.545	(10.249)	Currently employed
Log Hourly Earnings	4,668	2.870	(0.453)	Currently employed
Working Hours Vary	5,266	0.299	(0.458)	Currently employed
Multiple Job Holder	5,266	0.083	(0.275)	Currently employed
Self-employed	5,668	0.109	(0.312)	Currently employed, multiple job holders and job changers
Work in Public Sector	5,668	0.100	(0.300)	Currently employed, multiple job holders and job changers

Notes: These are descriptive statistics of the outcome variables of interest in the full sample, i.e. from June 2013 to December 2017. "In the Labor Force" is a dummy variable equals to 1 if individual is in the labor force, 0 otherwise; "Employment Status (E/U)" is defined among workers in the labor force. It equals to 1 if person is employed, and 0 if unemployed; Similarly "Usual Total Hours Worked" is reported from workers being in the labor force; "Log Hourly Earnings" is the log of total hourly earnings in constant 2002 dollars; "Working Hours Vary" and "Multiple Job Holders" are defined for currently employed workers as indicators for having varying working hours and being a multiple job holder; Lastly, for currently employed, multiple job holders and job changers, two additional job-related characteristics are created. They are dichotomous variables indicating if the worker is currently "Working in the Public Sector", or being "Self-employed". Column (1) to (3) presents number of observations, means and standard deviations. Column (4) indicates the population from which valid records of the dependent variable is recorded.

Table 3: Model Specifications

		Specifications				
	Detail Variables	(1)	(2)	(3)	(4)	(5)
Var. of Interest	Policy variables	Y	Y	Y	Y	Y
Demographics	Age at landing			Y	Y	Y
	Age ² at landing			Y	Y	Y
	Youngest child 0-5 at landing			Y	Y	Y
	Youngest child 6-12 at landing			Y	Y	Y
	Youngest child 13-18 at landing			Y	Y	Y
	Prob(Married) at landing			Y	Y	Y
Education & Occupation	Educated in Canada			Y	Y	Y
	Prob(Univ. and above) at landing			Y	Y	Y
	Prob(Cert. or some Univ.) at landing			Y	Y	Y
	Prob(Secondary or less) at landing			Y	Y	Y
	Prob(Mgmt/Prof) at landing			Y	Y	Y
	Prob(Tech/Trade) at landing			Y	Y	Y
	Prob(Manual) at landing			Y	Y	Y
Immigration Category	Prob(Economic class)					Y
	Prob(Refugee)					Y
	Prob(Student)					Y
Country of Origin	Source Cntry: use English				Y	Y
	Source Cntry: Developed				Y	Y
	Source Cntry FE				Y	Y
Landing related controls	Prob(Intended Prov.) at landing		Y	Y	Y	Y
	Unemployment rate in current province at time of landing		Y	Y	Y	Y
Other Controls	Year FE		Y	Y	Y	Y
	Quarter FE		Y	Y	Y	Y

Notes: See Appendix A for details on variable constructions.

Table 4: Estimates of the Effects of the Act on Different Outcomes: Policy Implementation Period

	(1)	(2)	(3)	(4)	(5)
Outcomes	OLS(5m)	DID(1m)	DID(3m)	DID(5m)	PSM-DID
Log Hourly Wages	-0.021 (0.025) [0.540]	-0.298*** (0.079) [0.001]***	-0.160*** (0.044) [0.001]***	-0.132*** (0.033) [0.001]***	-0.136 (0.067)
Employed	0.030 (0.022) [0.024]*	0.046 (0.044) [0.590]	-0.000 (0.023) [0.987]	0.013 (0.019) [0.489]	-0.017 (0.032)
Hours Worked	0.019 (0.025) [0.016]*	0.038 (0.083) [0.645]	0.034 (0.045) [0.515]	0.059 (0.035) [0.151]	0.022 (0.058)
Self-employed	0.029 (0.017) [0.076]	-0.037 (0.050) [0.615]	0.064* (0.029) [0.100]	0.096*** (0.022) [0.001]***	0.091 (0.044)
Job in the Public Sector	-0.004 (0.017) [0.016]*	-0.063 (0.038) [0.275]	0.050 (0.026) [0.147]	-0.028 (0.021) [0.255]	-0.063 (0.048)
Usual Hours Worked Vary	1.053 (0.548) [0.062]	1.182 (1.585) [0.615]	1.253 (0.946) [0.296]	1.933* (0.759) [0.022]*	1.508 (1.22)

Notes: Each row corresponds to one outcome. In Column (1), we run a simple OLS to compare labor market outcomes between treatment and control groups, with a +5/-5 landing window, only in the post-policy period, i.e. after June 2014. Column (2) to (4) employed the difference-in-differences model shown in Equation 5. We vary the sample of treatment and control groups by using those with different landing windows—they are +/-1, +/-3 and +/-5 months farther away from the 35/36 months residency requirement threshold by June 2015. Column (5) reports difference-in-difference estimates after propensity score matching on observable characteristics, where treatment and control groups are defined with a +5/-5 landing window. All of these models control for the most extensive sets of variables as listed in Column (5) of Table 3. These variables are also variables used for balancing characteristics between treatment and control groups in the PSM-DID model. Standard errors are in (parentheses). q-values are calculated to correct for FDR due to multiple outcome tests. They are displayed in [brackets]. For the PSM-DID models, standard errors are bootstrapped. * p or q < 0.05, ** p or q < 0.01, *** p or q < 0.001.

Table 5: Estimates of the Effects of the Act on Log Hourly Earnings Addressing Selection on Unobservables with a Heckman Two-Steps Estimator

Est: POST×TREAT	Depvar: Log Hourly Earnings	
	(1)	(2)
Policy Announcement	-0.072* (0.033)	-0.066* (0.032)
Policy Implement	-0.132*** (0.033)	-0.116** (0.036)
Mid-run	-0.095** (0.034)	-0.105** (0.034)
Long-run	-0.103* (0.042)	-0.116* (0.045)
Controls		
Model 5	Y	Y
Spouse characteristics		
Heckman Selection		Y

Notes: This table reports policy effects of the Act on Log Hourly Earnings for four time periods—Policy Announcement (June 2014 to May 2015), Policy Implementation (June 2015 to May 2016), mid-run (June 2016 to May 2017) and long-run (June 2017 to December 2017) relative to the reference period (June 2013 to May 2014). Column (1) and (2) are respectively DID and Heckman two-steps selection correction estimates from using covariates specified in Model 5 of Table 3. Column (1) controls for all variables included in Model 5 as listed in Table 3. Column (2) uses Heckman correction to adjust for participation, where a spouse indicator, employment status and earnings along with the policy variables, i.e. POST, TREAT, POST×TREAT, are instruments used to identify immigrant's probability of being in the labor force. Standard errors are in (parentheses). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: DID estimates of the effects of the Act on Yearly Earnings Using the Immigration Landing Database (IMDB-2012-2016)

Est: POST×TREAT	(1)	(2)	(3)
Depvar: Log Annual Earnings	-0.118 (0.083)	-0.105** (0.036)	-0.128*** (0.028)
Depvar: I(Earnings > 0)	-0.024 (0.028)	-0.010 (0.012)	0.015 (0.010)

Notes: Table presents policy estimates for two outcome variables: Log annual earnings and a dichotomous variable indicating if individual has positive earnings. Column (1) to (3) use $+/-1$, $+/-2$, $+/-3$ months of landing windows to define treatment and control groups, respectively. Standard errors in (parentheses). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Control variables include: age (and its squared term), education categories, marital status, occupation categories, child's age group dummies, knowledge of official languages, immigration categories (all measured at the time of landing), as well as source country FEs and tax year FEs.

Figures

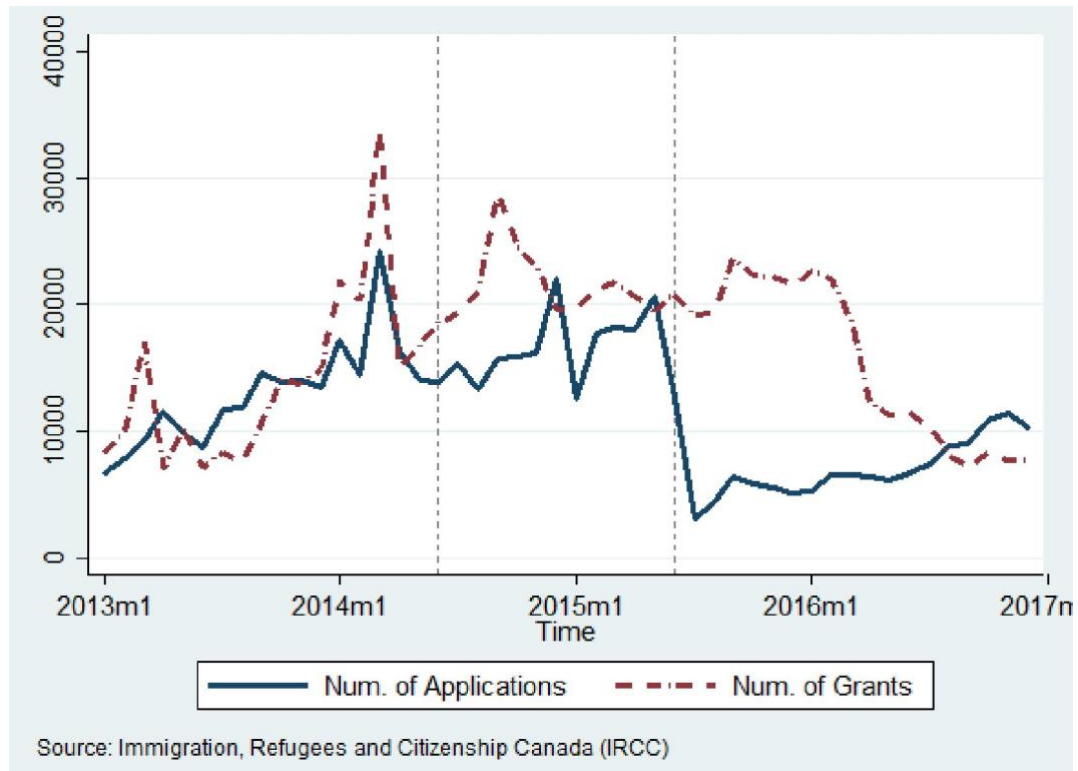


Figure 1: Citizenship Applications and Grants, 2011-2017

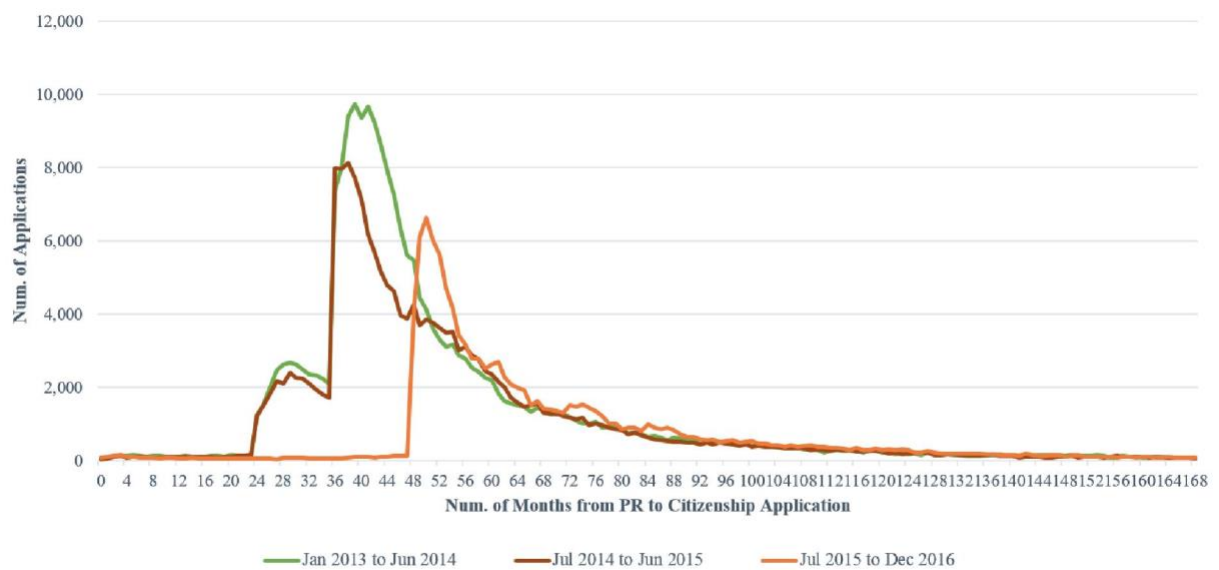


Figure 2: Number of Months from PR to Citizenship Application

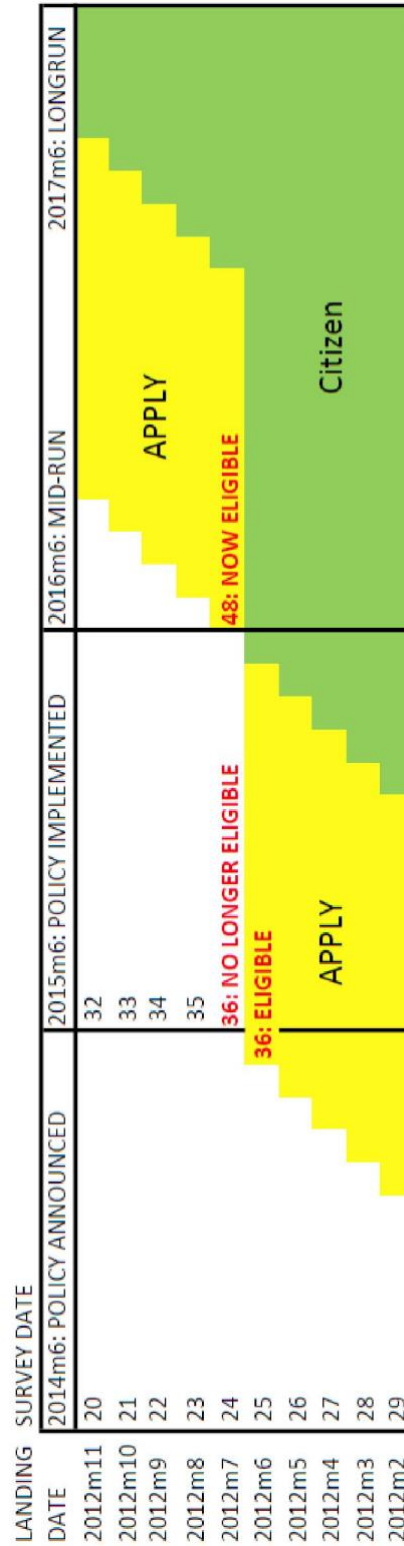


Figure 3: Timeline of the Act and Citizenship Eligibility

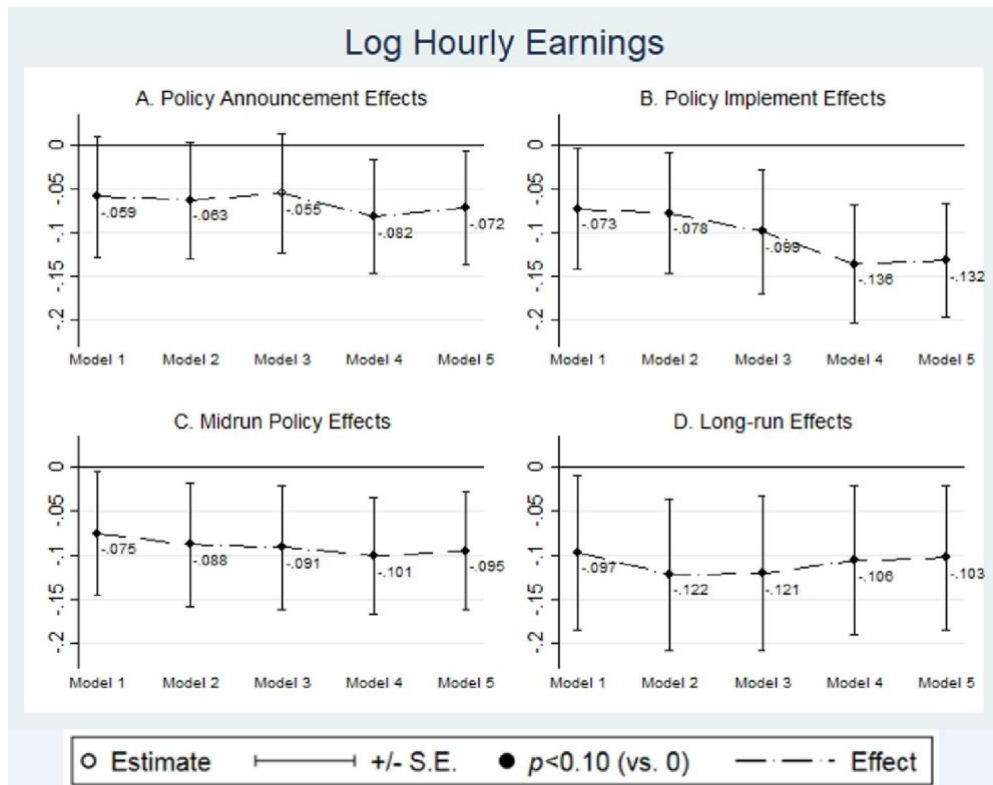


Figure 4: Effects of the Act on Log Hourly Earnings

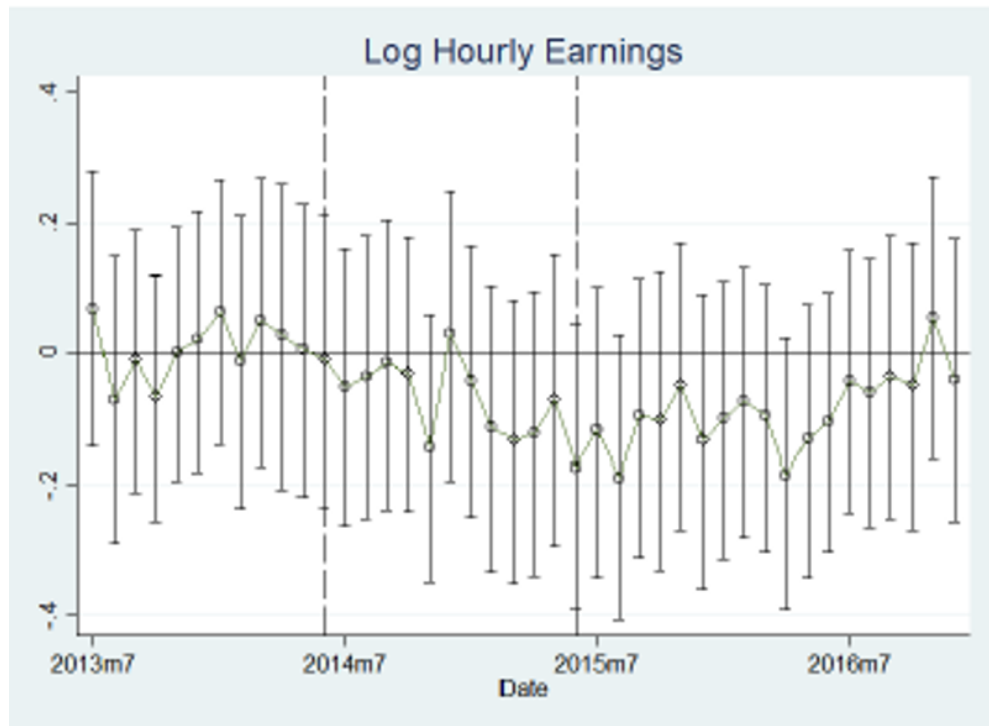


Figure 5: Evaluation of the Common Trends Assumption
Differences in Log Hourly Wages by Treatment Status Over Time

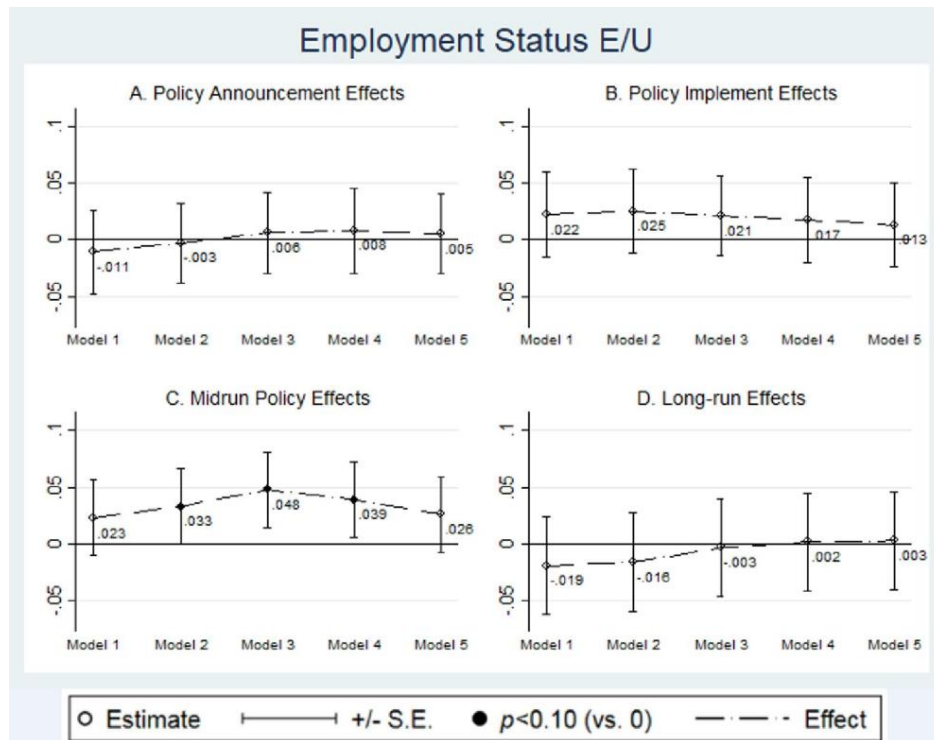


Figure 5: Employment Status (E/U: Employed/Unemployed)

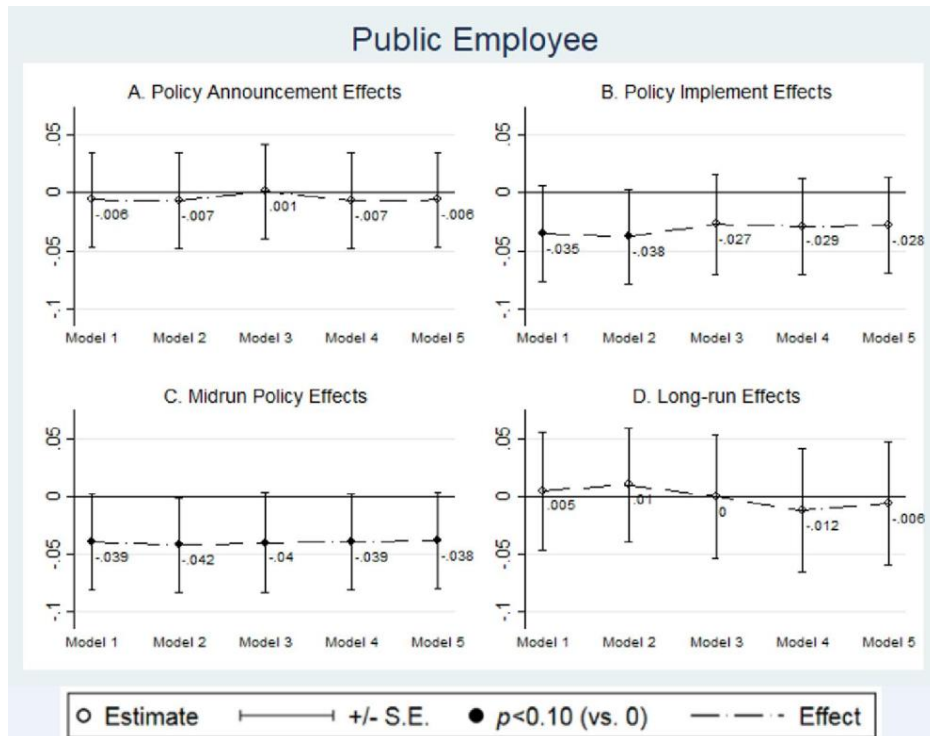


Figure 6: Effects of the Act on the Likelihood of Holding a Job in the Public Sector (Public Employee)

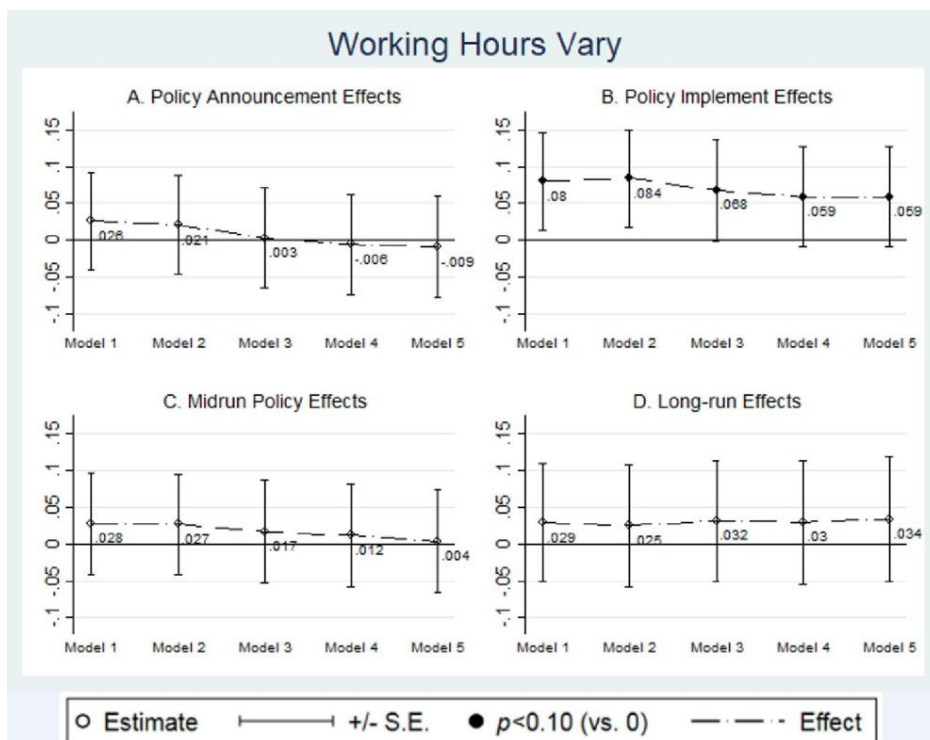


Figure 7: Effects of the Act on the Likelihood of Holding a Job with a Changing Schedule (Working Hours Vary)

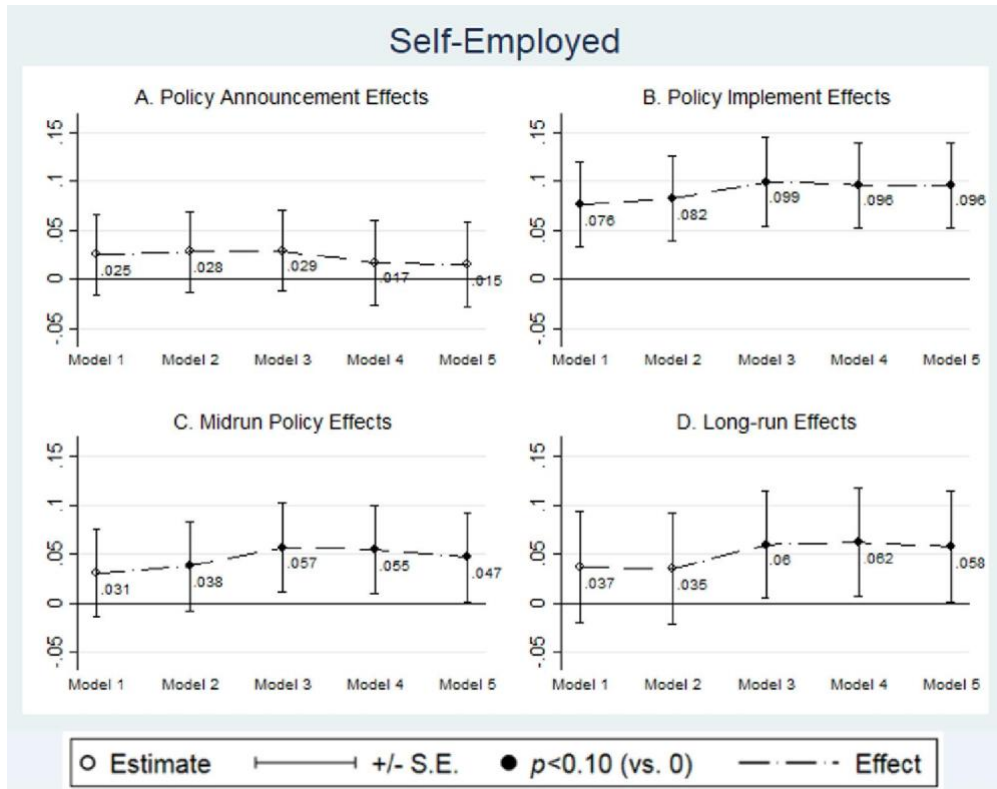


Figure 8: Effects of the Act on the Likelihood of Being a Self-Employed Worker (Self-Employed)



Figure 9: Evaluation of the Common Trends Assumption
Differences in Job Quality Attributes by Treatment Status Over Time

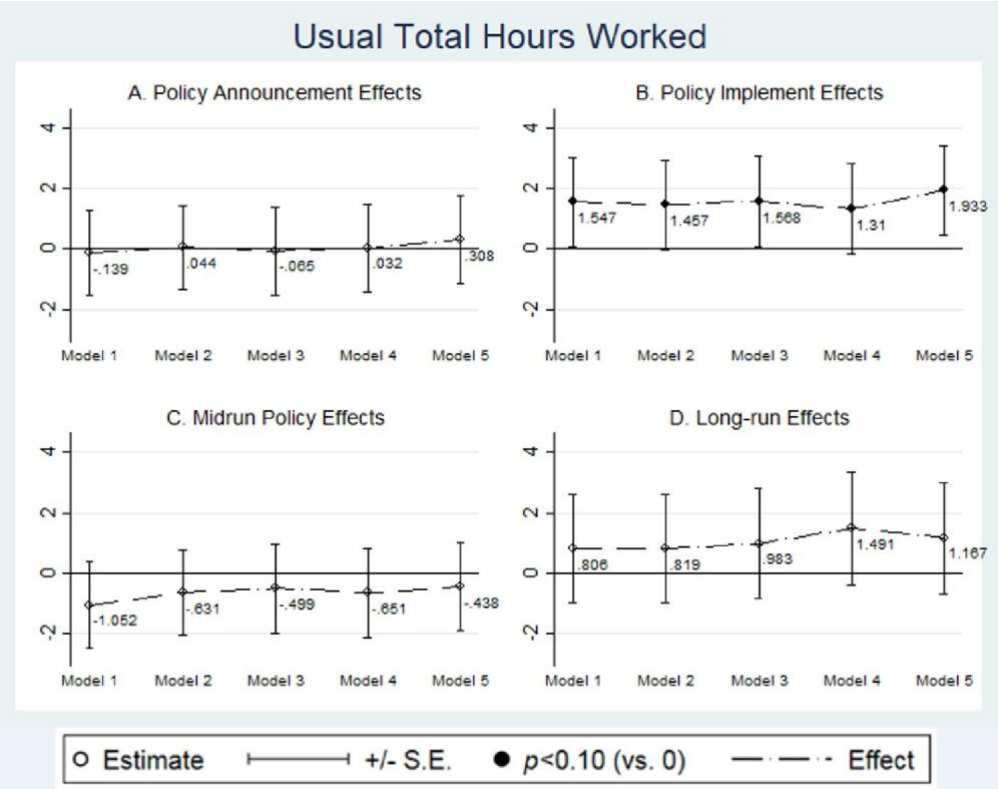


Figure 10: Effects of the Act on the Number of Usual Hours Worked Per Week

Appendix B. Sample Selection, Data Cleaning and Variable Constructions

Our main sample was collected from the Labor Force Survey (LFS) and encompasses June 2013 to December 2017. We restrict the immigrant sample to males, aged 25—64. In order to exploit the change in the citizenship law, which was announced in June 2014 and became effective in June 2015, we selected immigrants based on their landing dates. We define immigrants landed between July 2012 and November 2012 as the treatment group, since they would have resided in the country for less than 36 months before the citizenship law became effective on June 2015. An "observationally equivalent" group of immigrants, composed of those who landed between February 2012 and June 2012, is defined as the control group. They would have potentially resided in Canada for 36 to 40 months and therefore be eligible to apply for citizenship given the new law. Table 6 shows how each variable is constructed and the source of data. The majority of the current characteristics come from the Labor Force Survey (LFS). The landing characteristics are merged from the Permanent Residence Landing File (PRLF) under the following steps:

1. Separate groups of individuals by three identifying characteristics: gender, time of landing and country of birth.
2. Merge all individuals from PRLF that share the same identifying characteristics
3. Generate birth year for each individual, drop merged observations from PRLF if birth year is outside of the +1/-1 window.
4. Compare education level in LFS with that in PRLF, drop merged observations from PRLF if education level in LFS (current) is lower than education level in PRLF (at the time of landing); Drop if education level in LFS is not equal to education level in PRLF if individual did not obtain highest degree in Canada; Drop if education level in LFS is two levels higher than Education level in PRLF
5. For all the "possible matches" based on the above selection criteria, we calculate the mean of all matches on variables of interest, including marital status, occupation categories and intended province of landing, as probability of having corresponding characteristic at landing.

Table 6: Variable Constructions

	Variable	Source of Data	Variable Construction
Policy	treat	LFS	=1 if land before June 2012; =0 o.w.
	post	LFS	=1 if observe before June 2014; =0 o.w.
Demographics	age	LFS	
	age at landing	imputed	=age-(survey year-landing year)
	youngest child 0 - 5 at landing	Imputed	=age of youngest-(survey year-landing year)
	youngest child 6 - 12 at landing	Imputed	same as above
	youngest child 13 - 18 at landing	Imputed	same as above
	Prob(married) at landing	Merged from PRLF	
Education	Educated in Canada	LFS	=1 if country of education (of highest degree) is Canada; 0, o.w.
	Education categories at landing	Imputed	=current education, if highest education received in Canada
	Years of schooling (approx.)	Imputed	=current education-1 o.w.
			=6 if Grade 8 or lower
			=9.5 if Grade 9-10
			=12 if Grade 11-13, non graduate
			=12.5 if Grade 11-13, graduate
			=13.5 if some post-secondary
			=14.5 if trades cert. or diploma
	Year of schooling at landing	Imputed	=15.5 if community college, CEGEP
			=16 if Univ. cert. below Bachelor's
			=16.5 if Bachelor's degree
			=20 if above Bachelor's degree
			=years of schooling-(survey year-landing year)

Table 6: Variable Constructions (Cont.)

Variable		Source of Data		Variable Construction	
Occupation	Occupation categories	LFS		=1 if Managerial or Professionals =2 if Skilled and Technical or Intermediate and Clerical =3 if Elemental and Labourers	
	Prob(Mngt/Prof) at landing	Merged from PRLF			
	Prob(Tech/Trade) at landing	Merged from PRLF			
	Prob(Manual) at landing	Merged from PRLF			
Immigration Category	Prob(Econ Immi.) at landing	Merged from PRLF			
	Prob(Refug Immi.) at landing	Merged from PRLF			
	Prob(Student Immi.) at landing	Merged from PRLF			
Cntry of Origin	Use English	Merged by source country		=1 if source country use English as language of instruction or over 50% of the population speaks English	
	Developed country	Merged by source country		=1 if source country is a developed country (Based human development index (HDI) published by United Nations Developed Program.); 0 o.w.	
	Continent/country FE	LFS		=1 if North America =2 if Latin America =3 if Europe =4 if Africa =5 if Asia =6 if Oceania =7 if Phillipine =8 if India =9 if China	
				=(survey time-landing time)	
Landing related	months since landing	Imputed			
	Prob(Intended Prov.) at landing	Merged from PRLF			
	unemployment rate at landing (in current province)			unemployment rate merge by landing month, year and current province of residence	
Other controls	Year FE	LFS			
	Quarter FE	LFS			
	Province FE	LFS			

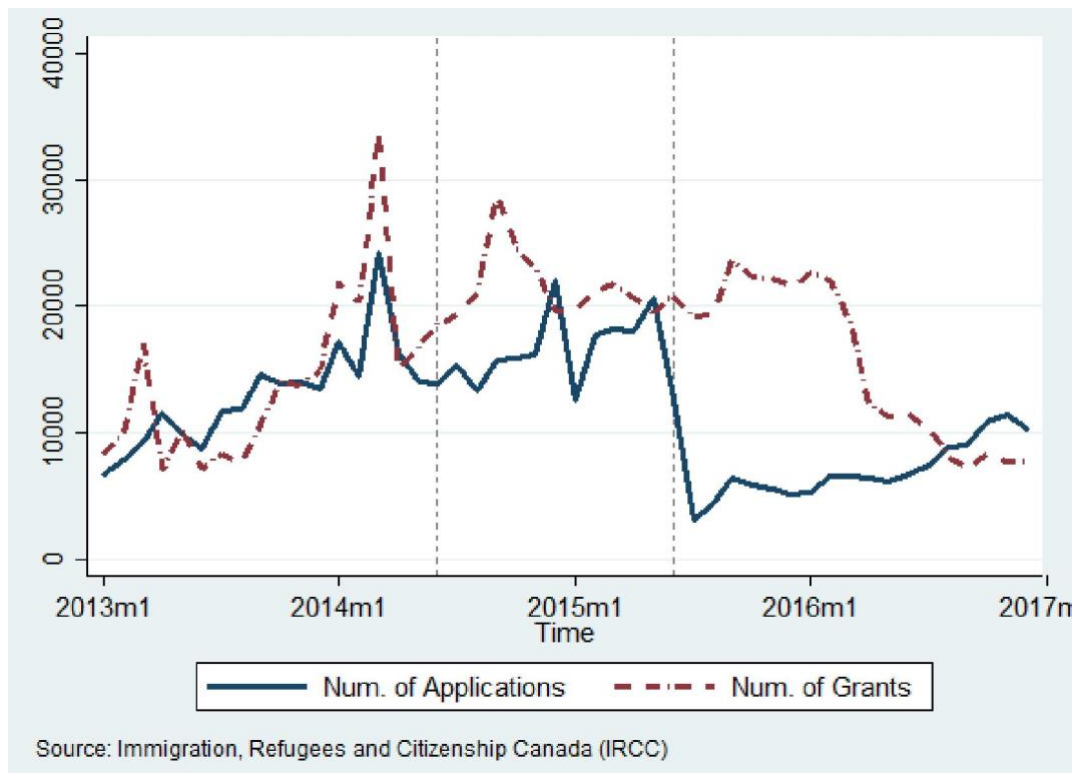


Figure 1: Citizenship Applications and Grants, 2011-2017

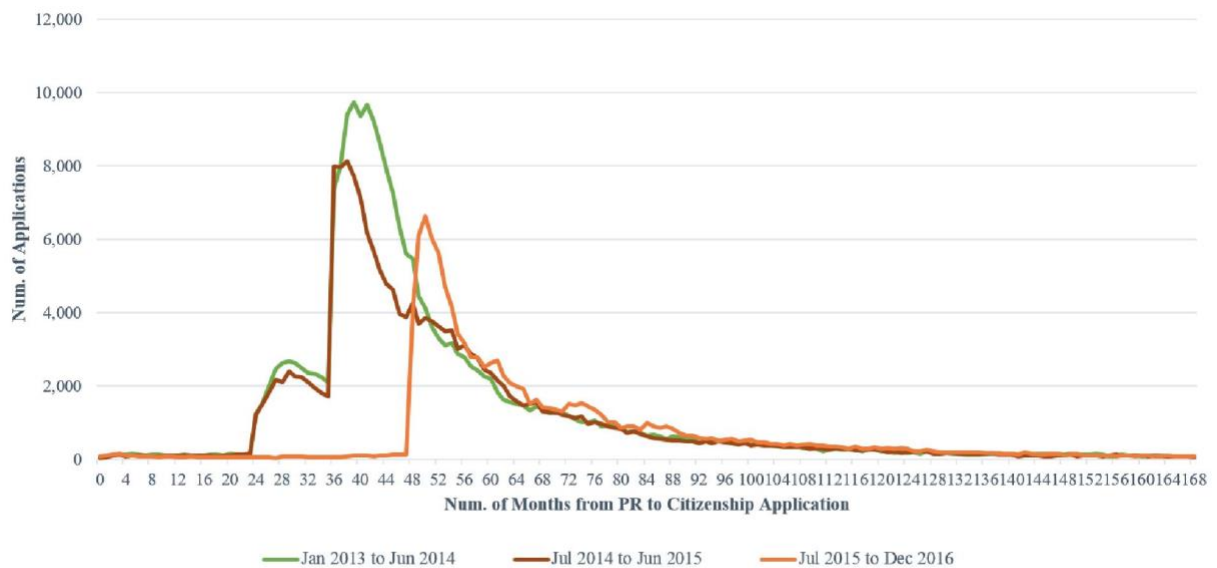


Figure 2: Number of Months from PR to Citizenship Application

In the Labor Force

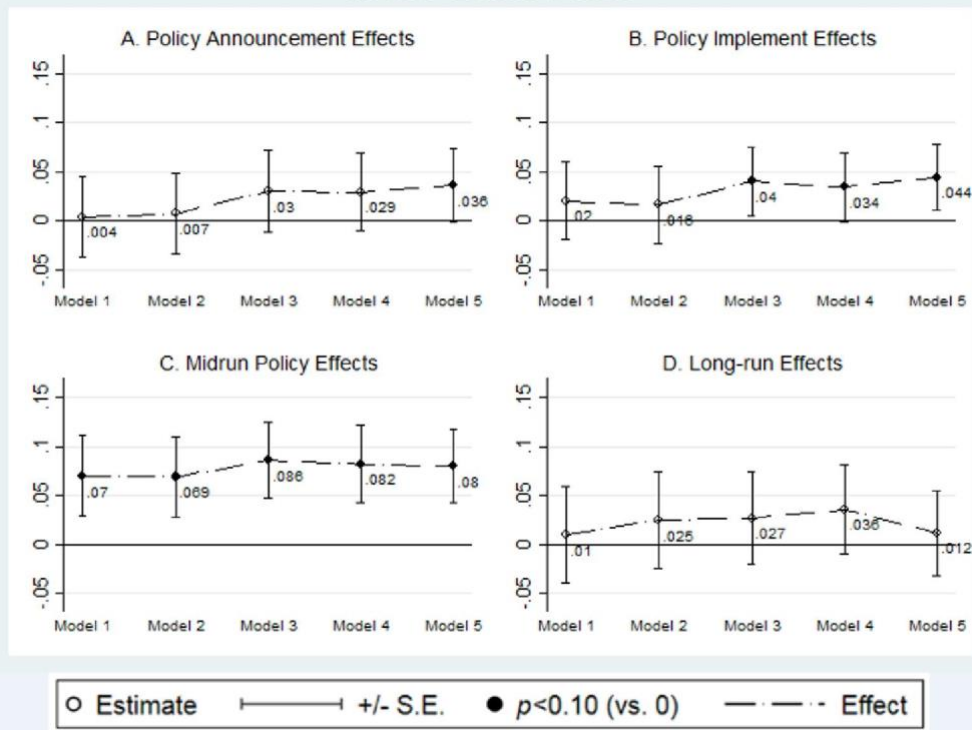


Figure 4: In the Labor Force

Employment Status E/U

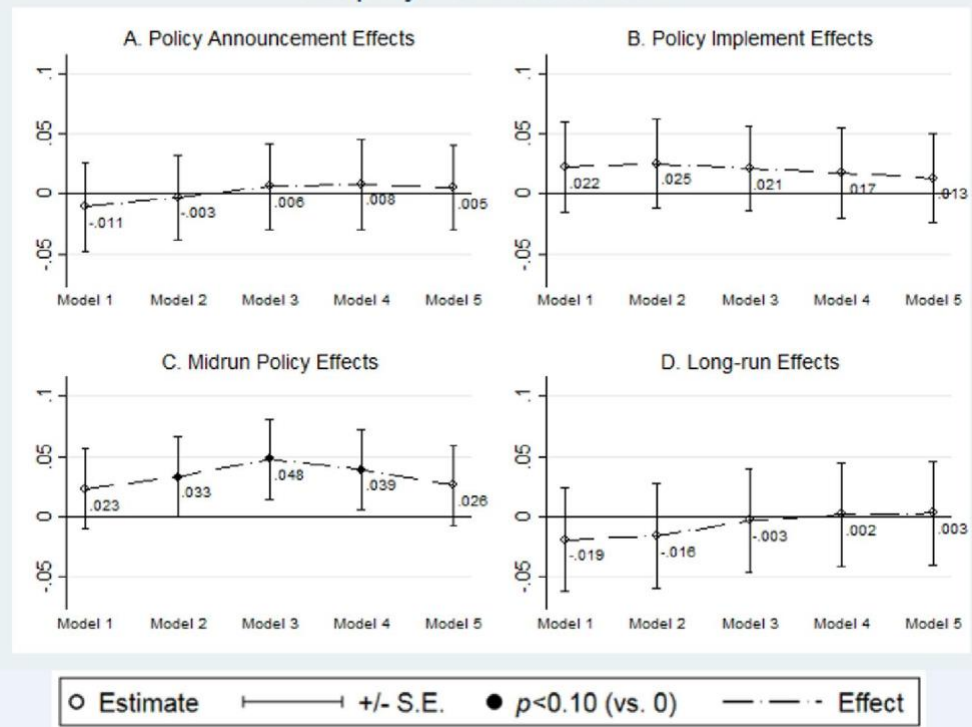


Figure 5: Employment Status (E/U)

Usual Total Hours Worked

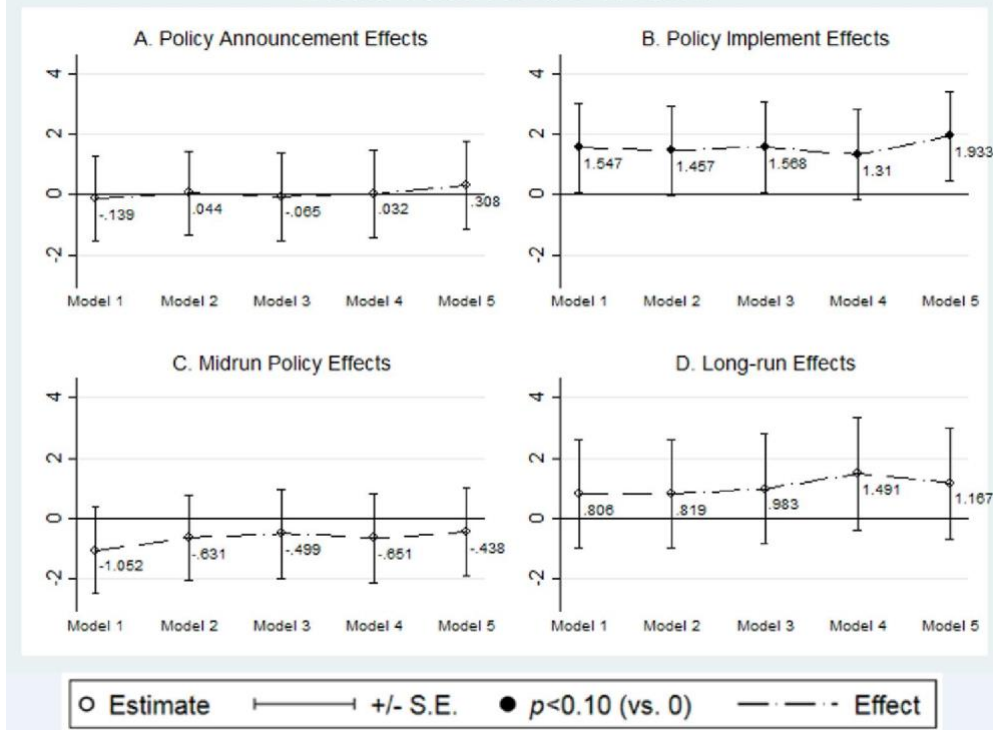


Figure 6: Usual Hours Worked Per Week

Log Hourly Earnings

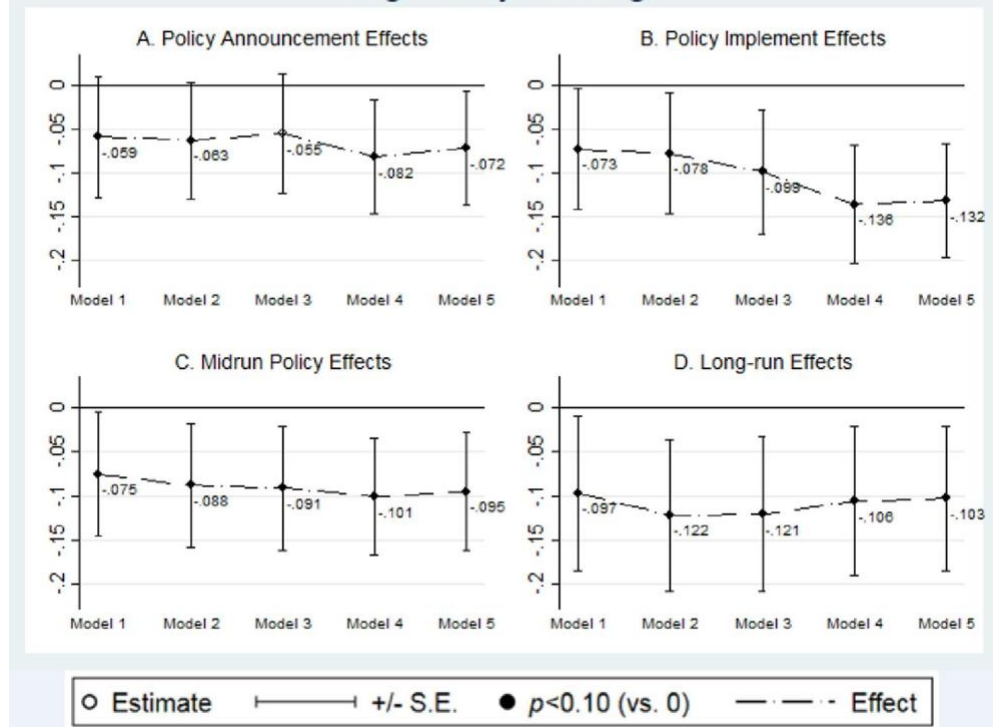


Figure 7: Log Hourly Earnings

Working Hours Vary

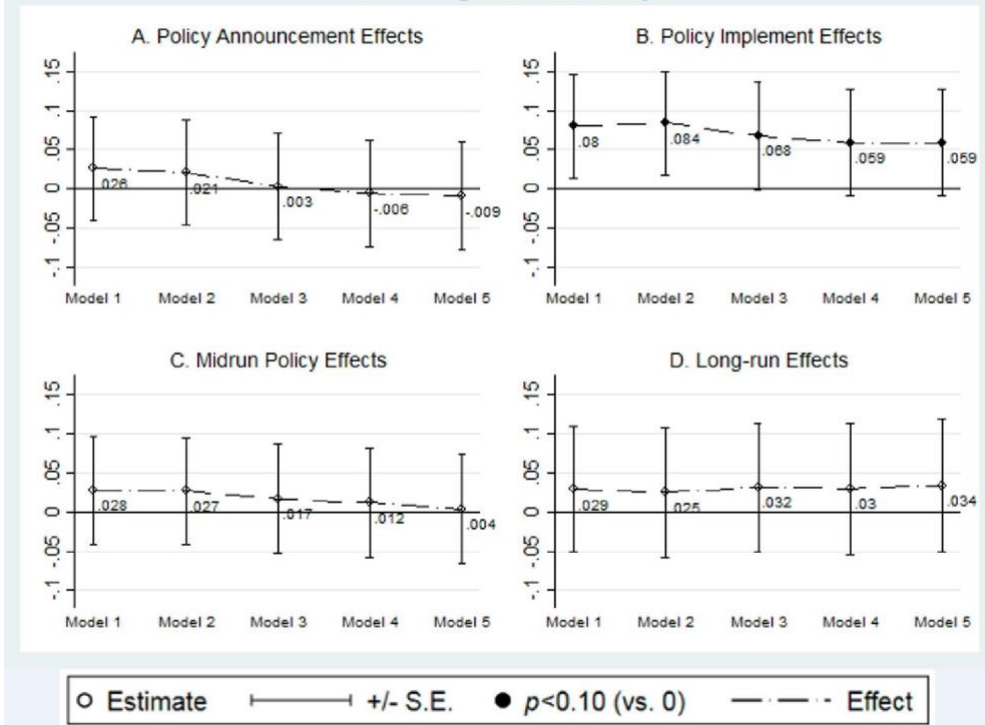


Figure 8: Job Characteristic: Working Hours Vary

Public Employee

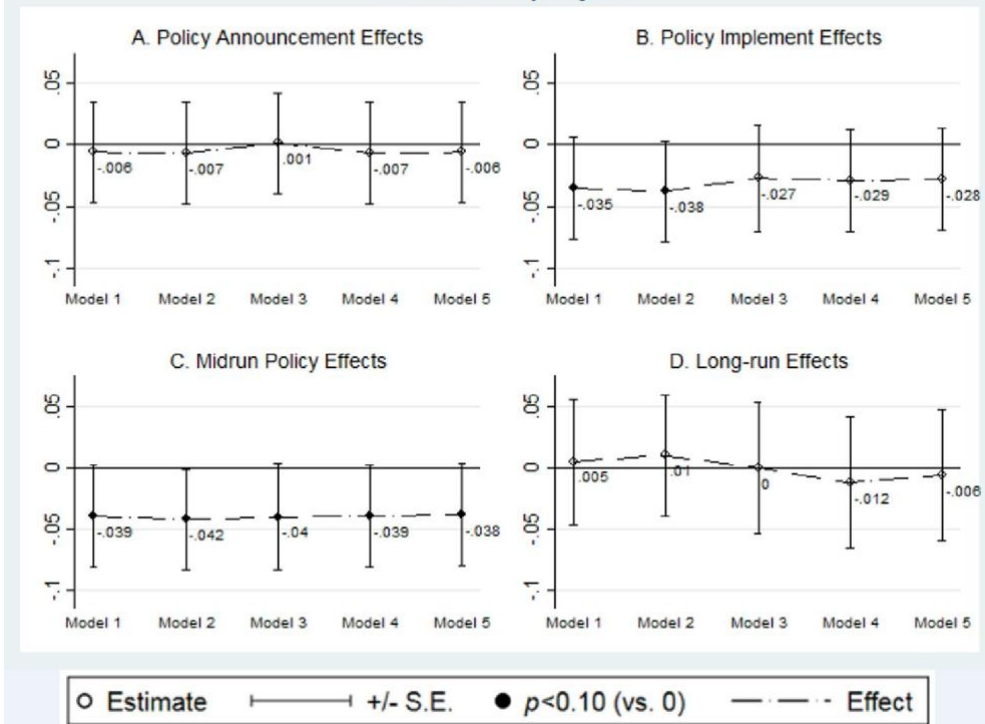


Figure 9: Job Characteristic: Work in Public Sector

Self-Employed

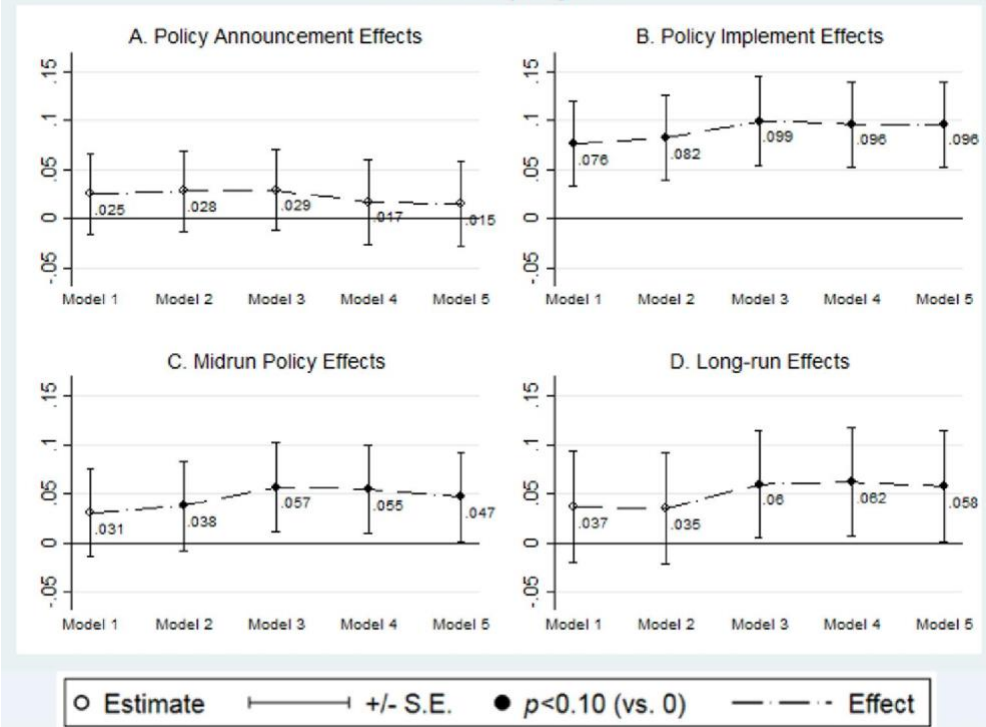


Figure 10: Job Characteristic: Self-employed

Appendix A. Summary of Changes to Bill C-24

Before	After
Effective on June 19, 2014	
No fast-track for citizenship for members of the Canadian Armed Forces	Fast-track mechanism for citizenship for individuals serving or on exchange with the Canadian Armed Forces
Effective on August 1, 2014	
Limited authority to define what constitutes a complete application	Establishes authority to define what constitutes a complete application and what evidence applicants must provide
Citizenship grant is a three-step decision-making process	Changes citizenship grant to a single-step process for most applications that reduces duplication and improves processing times
Effective May 28, 2015	
Governor in Council (GIC) final decision maker for citizenship revocation	Gives Minister of Citizenship and Immigration authority to decide on most revocation cases; Complex revocation cases such as war crimes, crimes against humanity, security, other human or international rights violations, and organized criminality decided by the Federal Court
Effective on June 11, 2015	
Residence for three out of four years (1,095 days)	Four years (1,460 days) out of six years
No requirement that residents be physically present	Requires 183 days minimum of physical presence in Canada during each of the four calendar years that are fully or partially within the six years before the date of application
Time as a non-permanent resident may be counted toward residence for citizenship	Eliminates use of time spent in Canada as a non-permanent resident (non-PR) for most applicants

No “intent to reside” provision	Introduces “intent to reside” provision
Applicants aged 18–54 must meet language requirements and pass knowledge test.	Applicants aged 14–64 to meet language requirements and pass knowledge test
Some “Lost Canadians” are ineligible for citizenship	Extends citizenship to “Lost Canadians” born before 1947 as well as their 1st generation children born abroad
Bars to citizenship for people with domestic criminal charges and convictions	Expansion of criminal prohibitions to bar applicants for crimes committed abroad
Consultants not required to be registered or regulated in order to represent individuals in citizenship matters; Few tools to deter fraud and ensure program integrity; Fines and penalties for fraud are a maximum of \$1,000 and/or one year in prison	Clearly defines who is authorized to provide representation or advice in a consultant capacity on citizenship matters and receive consideration (i.e. compensation) newly-designated Immigration Consultants of Canada Regulatory Council is the new regulatory body for citizenship consultants; Gives the Minister of Citizenship and Immigration Canada the authority to refuse an application for recognized status as a consultant if applicant commits fraud; Fines and penalties for fraud are up to a maximum \$100,000 and/or up to five years in prison; New offences and penalties will be implemented for a person who counsels known misrepresentation or represents or advises a citizenship applicant and are unauthorized to do so
No requirement to file Canadian income taxes to be eligible for a grant of citizenship	Requires adult applicants to file Canadian income taxes, if required under the Income Tax Act, to be eligible for citizenship

<p>No authority to revoke citizenship for grounds beyond fraud and misrepresentation</p>	<p>Authority to revoke Canadian citizenship from dual citizens who served as members of an armed force of a country or an organized armed group engaged in armed conflict with Canada; who are convicted of terrorism, high treason, treason, or spying offences, depending on the sentence received; Authority to deny Canadian citizenship to permanent residents who served as members of an armed force of a country or an organized armed group engaged in armed conflict with Canada or who are convicted of terrorism, high treason, treason, or spying offences, depending on the sentence received</p>
<p>Misrepresentation on applications could only be pursued through the laying of charges by the Royal Canadian Mounted Police.</p>	<p>Applicants can be refused for misrepresenting or withholding material facts on applications and are subsequently barred from being granted citizenship for five years</p>

