Give me Your Tired and Your Poor: Impact of a Large-Scale Amnesty Program for Undocumented Refugees*

Dany Bahar † Ana María Ibáñez‡ Sandra V. Rozo§

Abstract

Between 2014 and 2019 over 1.4 million refugees fled from Venezuela to Colombia as a result of a humanitarian crisis, many of them without a regular migratory status. We study the labor market impacts in Colombia of the *Permiso Temporal de Permanencia* program, the largest migratory amnesty program offered to undocumented migrants in a developing country in modern history. The program granted regular migratory status and work permits to nearly half a million undocumented Venezuelan migrants in Colombia in August 2018. To identify the effects of the program, we match confidential administrative data on the location of undocumented migrants with department-monthly data from household surveys and compare labor outcomes in departments that were granted different average time windows to register for the amnesty online, before and after the program rollout. We do not distinguish any sizable or significant effects of the program.

JEL Classification: F22, O15, R23

Keywords: Migration, Work Permit, Labor Markets, Amnesties.

*We thank Felipe Muñoz from the Colombian Presidency and the Colombian statistics agency for providing the data for this study. We also thank Maria José Urbina and María Adelaida Ortega for their excellent work as research assistants. We are also grateful to seminar participants at the JDC Conference on Forced Displacement, LACEA, the University of Maryland, and Jobs and Development Conference and to the Inter-American Development Bank for useful suggestions. Ibáñez and Rozo gratefully acknowledge funding from the Inter-American Development Bank for this project. Bahar gratefully acknowledges funding from the Open Society Foundation to the Brookings Institution for this project.

†Brookings Institution. E-mail: DBahar@brookings.edu

‡Inter-American Development Bank. Email: anaib@iadb.org

§Corresponding author: E-mail: sandra.rozo@marshall.usc.edu. Address: USC Marshall School of Business. 701 Exposition Boulevard, Los Angeles, CA 90089.

I Introduction

What are the implications of granting working rights to all forced migrants in their hosting economies? Despite the alarming size of forced migrant flows, which reached 71 million people in 2018,[1] few hosting economies provide them with full work permits. The process of obtaining a work permit, if it even exists, often involves large amounts of red tape and rigid eligibility criteria due to fears of political backlash. Consequently, little evidence is available on the impacts of granting work permits to forced migrants on a large scale, especially within developing countries, which host more than 85% of the forcibly displaced populations worldwide and typically have large informal sectors.

We examine the labor market impacts of a large-scale amnesty program that granted work permits to nearly half a million Venezuelans in Colombia during the fall of 2018. The context of our study is the massive inflow of Venezuelans to Colombia, which significantly increased in 2014 due to the worsening humanitarian crisis caused by the authoritarian regimes of Hugo Chávez and his successor Nicolás Maduro. As of January 2020, the United Nations estimates that nearly 5 million Venezuelans have left the country, with about 1.6 million being hosted by Colombia. Many of these immigrants are undocumented, either having overstayed the 180 days allowed to all Venezuelans visiting Colombia or having entered the country at irregular crossing points.[2]

Between April and June 2018, the Colombian government ran a nationwide survey to register all undocumented Venezuelan migrants residing in Colombia. The survey registered about 442,000 Venezuelans.[3] The initial purpose of the survey was to identify and collect basic demographic information on the undocumented immigrants residing in Colombia; importantly, it was not intended to serve as a basis for revising individuals' migratory status. In July 2018, however, outgoing President Juan Manuel Santos unexpectedly announced that all Venezuelan immigrants who had registered in the survey would be eligible for a formal temporary migratory status. This

[1] See UNHCR (2018).

[2] Irregular entry was often due to not having a passport given the difficulties of obtaining one in Venezuela.

[3] The survey was known as Registro Administrativo de Migrantes Venezolanos (Administrative Registry of Venezuelan Immigrants).

renewable two-year visa, known as the *Permiso Temporal de Permanencia* (PEP for its Spanish acronym), granted undocumented Venezuelan immigrants the legal right to work as well as access to basic public services. Unlike other cases of countries granting regular migratory status on a large scale to undocumented migrants,[4] access to the PEP program was not conditional on any eligibility criteria aside from having registered in the aforementioned survey.

We focus our analysis on the impacts of the program on weekly hours worked, monthly wages, unemployment, and labor force participation. Additionally, we study the effects of the program on firm creation and exit. Throughout the paper we examine the impacts of the PEP program across these dimensions on three groups of individuals: (i) Colombians, (ii) Venezuelans, and (iii) Colombian returnees (i.e., Colombian citizens who lived in Venezuela and returned to Colombia within the last five years).

To carry out our analysis, we use confidential administrative data on the number of undocumented immigrants who obtained the PEP status, linked to department-level (i.e., state-level) labor market outcomes. The data are based on monthly household and labor-force surveys between January 2017 and June 2019.

To deal with endogeneity concerns regarding the self-selection of undocumented migrants into certain geographic areas, we exploit the fact that each undocumented migrant registered in the nationwide government survey was granted a registry number exogenous to the migrant. Based on this number, individuals were randomly assigned to one of 22 time windows for registering in the PEP amnesty online. According to government officials, this process was used to even the public load when processing the regularization. Based on the random assignment of time windows, we estimate an average time window available to register in the PEP program by department. Consequently, our empirical strategy compares labor outcomes between departments with different average time windows to register in the program online, before and after the program was rolled out in August 2018. As we show in our analysis, the departments that had longer average time windows also had a disproportionately high number of PEP holders.

[4] In places such as the United States, Europe, and Jordan.

We also estimate an event study that examines the evolution of quarter-by-quarter labor outcomes in departments that had different average time windows to register in the PEP program. The event study supports the validity of our identification strategy, as we observe parallel trends before the program implementation in all the outcomes that we examine.

We do not find any large or significant effects of the PEP program on any of the outcomes or samples that we study. We do observe, however, marginal negative effects of the program on the unemployment and labor force participation of Venezuelan migrants. However, these negative marginal effects are not consistently observed across the different specifications that we use to test the effects of the program.

Our results are not explained by lower-than-expected take-up rates for undocumented migrants deemed eligible for regularization. Following August 2018, 64% of registered irregular immigrants applied and received a PEP visa. There are several explanations for our results. The first is that the composition of the labor force supply remained unchanged after the rollout of the program. In other words, PEP holders' main motive for obtaining the new migratory status was to access public services, such as health and education for themselves and their dependants, and not to switch jobs from the informal to the formal sector. Forced migrants, for instance, may already have a job in the informal sector—which is large in Colombia—and may not perceive any benefits from becoming formal employees. In fact, migrants may perceive that getting a formal job only means the additional cost of paying taxes. Although our estimates suggest that formal workers in Colombia earn almost twice the wages of informal workers, migrants may not be aware of the existing wage premium. Another possibility arises from labor demand dynamics; migrants may be trying to get a formal job, but they are unable to get one. For instance, it is unclear whether Colombian firms will offer a formal job to migrants, or even if the firms have information on what a PEP is and the fact that it is indeed a valid work permit. Our conversations with Colombian local officials working in the program implementation are consistent with this possibility being part of the explanation. The third possibility is that the sudden increase in labor supply of immigrants in the formal sector creates other general equilibrium effects (e.g., increase in aggregate demand, or

skill complementarity with natives) that result in dynamics such that we do not observe negative labor market effects. In fact, the null effect we find is in line with other studies on the effects of inflows of migrants on labor market outcomes.[5] Finally, it is also possible that the time horizon under consideration is too short, and more time is necessary to observe any impact on the studied outcomes.

Our results are robust to a battery of tests. First, all our estimates include department and month-year fixed effects, and consequently, they are not sensitive to time-invariant differences across departments or to aggregate time trends. We also control for full interactions of a rich set of pre-crisis department characteristics and year fixed effects to account for the possibility that their time evolution may bias our results.[6] Second, we test for the possibility that the time window available for PEP registration may be correlated with observable individual or departmental variables. We find that our measure of average registration days for a PEP is indeed correlated with the distance to the Colombia-Venezuela border. Hence, we show that our results are robust to the inclusion of interactions of year dummies and the closest distance to the Venezuela-Colombia border. Third, we account for the possibility that our estimates may be noisy due to the continuous and strong flows of Venezuelan migrants to Colombia. For this purpose, we include controls for the interaction of year dummies and early settlements of Venezuelan migrants.[7] Lastly, we exclude the departments at the Colombia-Venezuela border from our sample to account for the possibility that other time-varying factors, such as trade or demand patterns, may be confounding our estimates. Our conclusions remain unchanged.

Our paper contributes to two large groups of studies. First, it adds to the studies that examine the impacts of forced migrants on labor markets.[8] Most of these studies find evidence of negative

[5] See Clemens and Hunt, 2017 for a review of this literature.

[6] These characteristics include conflict, violent crime, government presence, municipal income, central government transfers, GDP composition, inequality, poverty, size of the informal sector, night light density, and exports to Venezuela.

[7] As noted by Rozo and Vargas (2019), early settlements of Venezuelan migrants are strong predictors of subsequent Venezuelan inflows to Colombia.

[8] See Borjas and Monras (2017); Peri and Yasenov (2018); Clemens and Hunt (2017); Del Carpio and Wagner (2015); Fallah et al. (2019); and Altindag et al. (2018) for notable examples. Other studies that explore the impacts of immigration on labor markets include Card (2001), Borjas (2003), Card (2005), Ottaviano and Peri (2012), Dustmann

effects of forced migrants on low-skilled workers employed in the informal sector, coupled with a positive impact on the firms operating in formal markets.

Second, our work contributes to the studies that explore the impacts of migrant amnesty in hosting countries.[9] These studies focus on examining the impacts of large-scale amnesties of migrants in the United States and Europe. In general, these studies find negative effects for blue-collar workers who compete with the migrants who are naturalized and positive effects on the labor market outcomes of migrants. Our work contributes to these streams of literature by being the first, to the best of our knowledge, to study the effects of a large-scale program that grants work permits to forced migrants inside a developing country with a large informal sector.

Large-scale amnesty programs represent a politically sensitive issue. Some countries prefer a piecemeal approach or ban working rights altogether to prevent labor conditions from deteriorating within the host population. Imposing strict regulations for accessing formal labor markets may push migrants to work in informal labor markets, which affects the most vulnerable workers. These decisions are not based on evidence and mostly reflect the fears and prejudices of the government and the population. Our paper can offer consequential lessons for developing countries that lack comparable data or opportunities for identification but are also hosting large flows of forced migrants and may be considering implementation of similar policies.

II Context

II.1 The humanitarian crisis in Venezuela

The Venezuelan political crisis traces back to the election of President Hugo Chávez in late 1998. Chávez's regime was characterized by expropriations, nationalizations, restrictions on private businesses, and the implementation of large social programs using resources from the oil boom of the

et al. (2016), Foged and Peri (2016), and Dustmann et al. (2017).

[9] See Kossoudji and Cobb-Clark (2002), Bratsberg et al. (2002), Orrenius and Zavodny (2003), Kaushal (2006), Amuedo-Dorantes et al. (2007) for examples.

2000s. His policies were continued by his appointed successor, Nicolás Maduro, who was first elected president of Venezuela in 2013. Under Maduro's tenure, authoritarianism increased and the ongoing economic and social crises significantly worsened. Chávez's legacy of relying on pro-cyclical policies with unsustainable levels of public debt and massive amounts of regulations over the private sector induced a deep economic recession when oil prices collapsed in 2014.

By 2018, the situation in Venezuela became a full-fledged humanitarian crisis. Independent sources claim that by 2018, 87% of households were below the poverty line, up from less than 50% in 1996. About three-quarters of the population had involuntarily lost an average of 20 pounds of weight, and one-third of the population reported eating twice per day or less (Sequera, 2018). Infant death rates increased by at least 30%, and maternal mortality rose by 65% (The Guardian, 2017). Additionally, patients with chronic diseases such as cancer, renal failure, or diabetes reported that they could not access necessary medicines on a regular basis (Jones and Pozzebon, 2018).

II.2 Venezuelan immigration into Colombia

Owing to the crisis, many Venezuelans decided to leave their country. According to data from the United Nations Refugee Agency (UNHCR), as of June 2019, over 4 million Venezuelans had left, citing violence, shortages of basic goods, political persecution, and the humanitarian crisis as reasons for their departure (see UNHCR, 2018). The vast majority of these migrants are being hosted in neighboring Colombia, but other common destinations include Peru, Ecuador, Argentina, Chile, and Brazil. Data from the Colombian government suggest that as of 2019, 1.4 million Venezuelans were residing in Colombia.

In light of the large inflows of immigrants from Venezuela, the Colombian government created a new migratory status—a temporary resident visa—that is renewable every two years, known as *Permiso Temporal de Permanencia* (PEP). The PEP was granted in waves. The first two waves—the first in January 2017 and the second in February 2018—were targeted to Venezuelans who had proper documentation and had a migratory status recognized by the law. The PEP was an instru-

ment to allow immigrants to join the formal labor force and get access to government-provided services, such as education and health. These first two waves were largely endogenous to the characteristics of migrants. Under the first two waves of the PEP program, nearly 182,000 permits were issued.

II.2.1 National registry of undocumented immigrants

By early 2018 the number of undocumented Venezuelan immigrants who were not eligible for the first two rounds of PEP was high and growing. The undocumented Venezuelan immigrants are those who either overstayed their initial tourist visa, which is valid for 180 days only (automatically given to any Venezuelan visiting Colombia), or entered Colombia at non-official border crossings (immigrants often enter unofficially because they do not have a valid passport, which is costly to get in Venezuela due to the collapse of government services).[10] Without a formal migratory status, undocumented immigrants can only work in the informal sector, facing a higher risk of exploitation and often being exposed to poor working conditions with no access to social security programs.

In order to count and characterize the population of undocumented Venezuelan immigrants residing in Colombia, the government implemented a nation-wide survey between April and June of 2018. This survey, known as the *Registro Administrativo de Migrantes Venezolanos* (RAMV), was administered by the Colombian authorities at 1,109 different stations throughout the country in 441 of the 1,122 Colombian municipalities. These registration points are represented in Figure I. The official registration stations for the RAMV were located in border municipalities, in municipalities with a large population of Venezuelan migrants, and in municipalities where local authorities requested them. In order to register in the RAMV, migrants had to show proof of Venezuelan citizenship. This requirement prevented registration of migrants who lacked any official identification

[10] Another possibility is that some immigrants abused a permit issued by the Colombian known as *Tarjeta de Movilidad Fronteriza* designed to facilitate the access of Venezuelans to Colombian areas bordering Venezuela (e.g., to shop for groceries and medicines, visit family members, or go to school). Once in Colombia, a permit holder's movement is limited to these bordering areas only. Yet, due to a lack of enforcement, some permit holders, once in Colombia, could have continued on to other parts of the country and stayed there.

documents.[11]

While the government explicitly stated that registering in the RAMV would not result in deportations or have any negative legal consequences, ex ante there was no clear benefit from registering. Importantly, registering was not explicitly or implicitly linked to a future possibility of formal migratory status. To encourage registration, the Colombian government rolled out a massive information campaign throughout the country, but "word of mouth" was likely an important determinant of actual registration in the RAMV. For all these reasons, we presume the survey did not cover the whole population of undocumented migrants. Furthermore, those left out were likely the less connected, less informed, and most vulnerable people. Nevertheless, RAMV identified 442,462 undocumented Venezuelan migrants in Colombia, belonging to 253,575 different households.

Using the data from the RAMV, we characterize the undocumented migrant population living in Colombia in 2018 in Tables I through IV (column 1). According to our analysis, the undocumented migrants in the RAMV are balanced in term of gender and are young (on average 26 years old), generally single (35% are married and cohabiting), and primarily from Venezuela (2.2% registered as Colombians). The average household size in the sample is 1.7 with an average of 0.45 children under 18 years of age. Sixty-one percent are single-parent households, which suggests many families strategically separated and a number of members remained in Venezuela. One quarter of households have a child (see Table I).

The data also allow us to make six general observations of the undocumented migrant population living in Colombian who registered for the RAMV:

1. *RAMV migrants have large disparities in their education levels, with the majority having complete secondary.* While 50% of the migrants have secondary education, 12.3% report having no education, 18% have only primary, and 15.5% report having university or technical education (see Table I).

2. *RAMV migrants are facing tight labor conditions and work predominantly in services.* One

[11] A Venezuelan national ID, a document that is much more common than a valid passport, was useful for registering.

quarter of the RAMV sample that is working age is unemployed, while the unemployment rate for all Venezuelan migrants in Colombia is 11.6% (according to labor surveys between 2017 and 2019). The educational degree of 89% of RAMV migrants is not officially recognized by the Colombian government, and only 12.9% can certify their labor experience. Moreover, 36% of the RAMV sample works in services and sales (see Table II).

3. *RAMV migrants have low access to education and health services.* Enrollment of children in primary and secondary school is only 40.7% and 35%, respectively. Although being an undocumented migrant may partially explain the low access to education, other factors such as discrimination, low income levels, lack of information on how to enroll in school, poor health conditions, and child labor may also play a role. In fact, 7.2% of children between 12 and 17 years of age are working. Access to health is practically nonexistent (see Table III).

4. *RAMV migrants want to stay in Colombia.* Eighty-nine percent of undocumented migrants want to stay in Colombia, and only 8.6% intended to return to Venezuela. This finding is surprising because 66.1% still have a large number of household members in Venezuela (see Table IV).

5. *Nearly half of RAMV migrants have access to networks in Colombia.* Forty-four percent have family in Colombia besides their nuclear household, and on average, 10.2% of the migrant population in the host municipalities in Colombia come from the same municipality in Venezuela (see Table IV).

6. *The majority of RAMV migrants are of working age.* Seventy-three percent of registered migrants are between 18 and 65 years of age, and 26.4% are children under 18 (see Table V).

II.2.2 PEP: Migratory amnesty to undocumented Venezuelan immigrants

On July 25, 2018, days before leaving office, President Juan Manuel Santos unexpectedly decreed that all undocumented Venezuelans registered in the RAMV would be eligible to apply for a new wave of PEP, which so far had only been offered to documented Venezuelan migrants. This decree represented an unexpected and widespread migratory amnesty to everyone in the registry, regardless of their characteristics.

The third wave of the PEP rollouts—the one on which we focus our analysis and will refer to simply as PEP from hereon—was issued to undocumented migrants during the fall of 2018.[12]

The PEP not only grants the right to work in the formal sector but also gives migrants the possibility of being scored by SISBEN, the general instrument used in Colombia for targeting anti-poverty social programs. The SISBEN score is required for accessing most social services in Colombia including the subsidized health system. The PEP is also used by immigrants as a document to prove regular status in Colombia and thus avoid possible deportation. Therefore, the PEP program is perhaps one of the largest and most generous amnesty programs for undocumented migrants in modern history.

The requirements for entering the PEP program were (i) being registered in the RAMV; (ii) being in Colombia by the time the decree was issued; and (iii) not having any criminal record or a deportation order. The processing and issuance of a PEP were free and voluntary and could only be done online.

Each migrant registered in the RAMV was assigned a registration number that was issued in ways exogenous to the migrant. Subsequently, each individual was allocated a specific time window to register based on his or her number. Government officials report that this approach was used to evenly distribute the registration load on public officials across time. Figure III shows the distribution of RAMV migrants assigned to each of the 22 available time windows (the specific

[12]Ever since, there have been other waves of PEP, albeit much smaller and not to undocumented immigrants. The third wave of PEP, the one we focus on, covers the most vulnerable migrants.

share of the RAMV population in each time window is presented in Appendix A).

II.2.3 Who are the PEP holders?

Sixty-four percent of the total undocumented migrants registered in the RAMV actually received a PEP. We compare the population that applied and did not applied for the PEP program in Tables I through IV (columns 3 and 4). Our analysis of the data allowed us to identify four main differences between these groups of migrants:

1. *PEP holders may get higher returns from joining formal markets.* They are more educated and more likely to have their degree and experience officially recognized.

2. *PEP holders seem to be more integrated into labor markets, specifically inside the informal sector.* Their unemployment rates are lower (23.8% for holders vs. 25.7% for non-holders), and they report being workers in higher proportions (64% vs. 60%, respectively).

3. *PEP holders have weaker social networks.* They have less family in Colombia, more household members in Venezuela, and a lower percentage of contacts from their municipality of origin at their destination (8.7% vs. 12.8%). Given stronger social networks, non-PEP holders may be able to rely on them more for seeking employment, which reduces the need to have a formal working permit.

4. *PEP holders have more children and less access to health services.* The percentage of households with children (and the average number of children) is higher for PEP holders. Yet, their access to health services is lower. Given that the registration in PEP provides access to the subsidized health regime, families with children have an additional incentive for registering.

III Data

We use departmental-monthly data from January 2017 to June 2019 to conduct our empirical analysis. The data that we use can be grouped into three categories.

1. *Labor Force Surveys.* We use the Colombian labor force surveys compiled by the Colombian statistics Agency (DANE in Spanish). We use these surveys to study the impacts of the amnesty on monthly wages, weekly hours worked, unemployment, and labor force participation[13] on three samples of workers: (i) Colombian, (ii) Venezuelan, and (iii) Colombians who returned from Venezuela in the last five years. The labor force surveys correspond to monthly repeated cross sections that characterize individual socio-demographics as well as labor outcomes. The surveys are representative for 23 of the 36 departments of the country. These correspond to the departments where the most populated cities of the country are located. Consequently, we use the surveys to study the impacts of the amnesty on labor markets using department-monthly variation. We restrict our sample to all individuals in the labor force (i.e., those who are working or actively looking for employment) ages 12 to 65 years who are not house workers.[14]

   Descriptive statistics for our sample are presented in Table VI. Relative to Colombian workers, Venezuelan migrants and Colombian returnees tend to work for longer weekly hours for lower wages. They also have higher unemployment rates despite being more educated.

2. *RAMV Data*. We use the confidential RAMV registry data to compute the share of undocumented immigrants who requested and received PEP status in each department.

3. *Other Municipal Controls.* We use a number of municipal covariates to asses the robustness of our estimates including night light density, conflict-related variables, homicide rates,

[13] Unemployment is a dummy variable equal to one for individuals without a job, in search for a job, or available to start working. Labor force is a dummy variable that takes the value of one if the individual has a job, is looking for a job, or had income or a business during the previous week.

[14] Legal working age begins at 10 years in Colombian rural areas, whereas it is 12 years in urban areas.

GDP municipal composition, exports to Venezuela, and proxies for government activity. Administrative information at the municipal level comes from the CEDE municipal panel, the Ministry of Defense, the National Planning Department, and DANE. Night light density comes from the National Oceanic and Atmospheric Administration. We also use controls for early settlements of Venezuelans in Colombia from the Colombian population census of 2005. Descriptive statistics for all variables are presented in Appendix I.

4. *Firm Entry and Exit.* We use department-monthly counts on firm entries and deaths from the Colombian Association of Chambers of Commerce (Confecámaras). The number of firm entries in a department is the number of firms that obtained a new *Registro Mercantil* (the Colombian business license). Firm exit is estimated by Confecámaras according to the number of firms by month and department that report their status as cancelled.[15]

IV Identification Strategy

We cannot correctly assess the impacts of the PEP program by simply comparing the socioeconomic outcomes in areas with different relative sizes of recently regularized populations. This restriction is because undocumented migrants presumably consider the characteristics of each place when deciding where to reside. It is possible, for instance, that Venezuelan migrants choose municipalities that are more prosperous and less violent. As such, a simple mean comparison of areas with different sizes of PEP holders may be biased. Our identification strategy aims to correct for these biases by using a 2SLS estimation.

Considering that the timing of the PEP implementation was exogenous, we only instrument for the treatment—the share of undocumented migrants who received PEP status in each department—by exploiting the size of the window that undocumented immigrants had to request a PEP in each department. This approach is based on the fact that, as explained above, the time window allotted

[15]We acknowledge the exit measure might be prone to measurement error because many firms might have stopped operating but did not complete the process of officially cancelling their business license.

to each undocumented immigrant to request a PEP was exogenous to the immigrant and depended on the form number in that individual's RAMV registration. Government officials report that this scattered approach was done to distribute the registration load on public officials evenly across time.

Consequently we use the following IV difference-in-difference specification

$$Y_{imdt} = \alpha \underbrace{\Big[\text{PEP}_d \times I(\text{Post Aug. 2018})_t\Big]}_{X_{dt}} + W'_{imdt}\Gamma + \sum_{c \epsilon Z}[c_{md} \times \psi_y] + \gamma_d + \gamma_t + \epsilon_{imdt} \quad (1)$$

$$X_{dt} = \alpha\Big[\text{Reg. Days}_d \times I(\text{Post Aug. 2018})_t\Big] + W'_{imdt}\Psi + \sum_{c \epsilon Z}[c_{md} \times \psi_y] + \beta_d + \beta_t + \mu_{imdt} \quad (2)$$

where i stands for individual *i*, m stands for municipality, d stands for department (the equivalent of a state in the United States), t stands for year-month variation, and y for year variation. Y represents the outcomes of interest (including the logarithm of monthly wages, the logarithm of weekly hours worked, unemployment, and labor force participation), PEP corresponds to the logarithm of the share of population who registered for a PEP in each department, and *I(Post August 2018)* is a dummy variable that takes the value of one for any observation for which the month-year is after August of 2018 (when the PEP rollout began).

W is a matrix of individual controls, which includes years of education, marital status, age, gender, and a dummy variable for the head of households.

Z is a full set of predetermined municipal characteristics measured before the beginning of our period of analysis (in order to reduce endogeneity concerns). Interactions of these variables and year dummies are included in all our estimates to flexibly account for potential differential non-parametric trends on a number of municipal characteristics observed prior to the migrant's

legalization. The variables included in Z are (i) Gini index in 2005,[16] (ii) percentage of households in Colombia with at least one unsatisfied basic need in 2005, (iii) percentage of households in Colombia with at least one informal worker in 2005, (iv) homicide rates in 2014, (v) terrorist attacks in 1995, (vi) night light density in 2013, (vii) number of financial institution in 1995, (viii) number of tax collection offices in 1995, (ix) agriculture, industry, and services GDP in 2009, (x) central government transfers in 2009, (xi) transfers in education in 2009, (xii) transfers in health in 2009, (xiii) total municipal income in 2016, (xiv) total municipal expenditures in 2016, and (xv) total exports to Venezuela in 2016.

γ_d, γ_t, β_d, and β_t are department and year-monthly fixed effects. Finally, standard errors were clustered at the department-monthly level.[17]

Note that our main specification is estimated using monthly data from January 2017 to June 2019. As such, our estimates for α represents the average effect for all months post August 2018, compared with all months beforehand.

IV.1 Average registration days

The total number of days available for the PEP registration for all migrants in the RAMV ranges between 78 and 141 (with 22 different time windows). The distribution of the available registration days to request a PEP for each migrant in the RAMV is presented in Figure III.

Based on the individual time window available to request a PEP, we estimate the average reg-

[16] The variables available in 2005 were calculated with the population census of 2005, the last census available before the PEP program began.

[17] We did not use sample weights in our estimates because the sample design is exogenous to the dependent variables conditional on all the covariates in our regressions. In other words, the distribution of the dependent variables in our sample is identical to the distribution of the variables in the population. This is called the ignorability condition, and it describes a situation in which the sample weights can be omitted when the probability of being in the sample is independent of the variable of interest (and the error term) and the OLS estimator will be consistent and efficient (Pfeffermann, 1993). Including sample weights could affect the efficiency of the estimators: if errors were homoscedastic, an estimation using Weighted Least Square (WLS) will produce heteroscedastic errors (Solon et al., 2015).

istration days (*Reg. Days* in equation 2) by department as

$$\text{Reg. Days}_d = \sum_{j \epsilon K} \frac{\text{Individuals assigned to time window j}_d}{\text{Total RAMV migrants}_d} \times \text{[Days in time window j]} \quad (3)$$

where K represents each of the 22 possible individual time windows assigned to migrants in the RAMV to request a PEP online.

Consequently, in our empirical strategy, we compare the evolution of labor outcomes in departments with higher and lower average number of days available for requesting a PEP online, before and after the program implementation in August 2018.

Figure IV confirms that a positive and strong correlation exists between our measure of average registration days and the share of regularized migrant population (i.e., PEP holders) in each department.

IV.2 Event study

We complement our baseline empirical analysis by estimating an event study using the following specification

$$Y_{imdt} = \sum_{j=Q1-2017}^{Q2-2018} \alpha_j \text{Reg. Days}_d + \sum_{j=Q4-2018}^{Q2-2019} \alpha_j \text{Reg. Days}_d + W'_{imdt}\Gamma + \sum_{c \epsilon Z} [c_{md} \times \psi_y] + \gamma_d + \gamma_t + \epsilon_{idt} \quad (4)$$

where all the symbols represent the same variables described earlier. We ran this specification using quarters instead of months to make the figures clear. Our estimates exclude the third quarter of 2018 since the PEP program began to be implemented in August 2018.

The estimation of the event study is extremely useful because it serves two purposes. First, it allows testing the validity of the parallel trend assumption between departments with different

average days for registration to request a PEP. Second, it evaluates the time evolution of labor outcomes after the program began to be implemented on a quarter-by-quarter basis.

V Results

Given that a PEP granted undocumented migrants access to formal labor markets, but they could already be part of the informal labor markets in Colombia, we only study the impacts of PEP on hours worked and wages in the formal sector. In our study, an individual is considered a formal worker if he or she is affiliated with health services, as typically defined in Colombia.

The estimated coefficient of interest α, as defined in equation 1, is presented for each sample and dependent variable in Tables VII through IX. Each table presents the estimates of the OLS (Panel A), reduced form (Panel B), and 2SLS or IV-dif-in-diff estimates (Panels C and D, respectively). Columns (1) and (2) of each table have a smaller number of observations because they only include formal workers.

Our first main observation is that our instrument is strong for all the samples that we study, which eliminates concerns about weak instrumentation. There is also a positive and statistically significant correlation between the average registration days (*Reg. Days*) and the share of PEP holders in each department.

Our second main result is that, in general, we do not distinguish significant or large effects of the PEP implementation, albeit a few exceptions do exist. In particular, a 10% increase in the share of PEP holders in the population is associated with

1. A reduction of 0.001 percentage points in labor force participation of Colombian workers. Similar, but slightly larger effects are observed for the sample of Venezuelan workers. In this sample, a 10% increase in the share of PEP holders reduced labor force participation by 0.04 percentage points.

2. A 0.02 percentage point increase in the unemployment of Venezuelan workers.

3. An increment of monthly wages for Colombian returnees. This result, however, is not robust to any of the exercises we carry out in section VI.

4. A 0.003 percentage point increase in the unemployment of Colombian returned workers.

V.1 Event study

The estimates for the event study described by equation 4 are presented in Figures V through VII. The figures show the estimates for α_j, as defined in equation 4, the coefficients that test the quarter-by-quarter evolution of any outcome differences between departments with a higher and lower average number of days for registering for the PEP program.

The figures show that the parallel trend assumption of no significant differences before the PEP implementation is predominantly satisfied for all the samples and outcomes. The figures also suggest that PEP implementation has not been reflected in any significant differences between areas with different average number of days for registering for a PEP after the program implementation. In fact, all the confidence intervals contain zero and they overlap before and after the program implementation. Furthermore, the figures do not show a trend for the coefficients that may result in significant impacts in the medium term, suggesting the short-term null effects may persist in the medium term. The figures largely suggest that the program had no significant effects on any of the outcomes that we study.

V.2 Heterogeneous effects

We also test if the program had heterogeneous effects by characteristics of the individuals, including gender, education level (e.g., beyond high school or less than high school), economic sector,

and firm size.[18] For this purpose, we split the sample according to the different categories in each case. The results of this analysis are presented in Figures VIII through X.

In short, we are not able to distinguish any effects of the program for any of the samples that we study. The only exception pertains to the individuals working in the economic sector of "real estate" for which we find positive effects in hours worked (in line with the demand shock for housing). We also distinguish a marginal increase in the unemployment rates of male Colombian workers (0.001 percentage point increase for an additional 10% share of PEP holders). Despite the statistical significance, the economic significance of this estimator is negligible.

V.3 Firm creation

As an additional test we also examine the effects of the PEP program in firm creation and destruction using the specification described by equations 1 and 2. Our estimates use the logarithm of the total number of new firms created (i.e., firm births) and closed (i.e., firm deaths) by department and month as dependent variables. The results of these exercises are presented in Table X. We do not distinguish any significant effects of the PEP program in the firm outcomes that we study.

In sum, despite marginal negative effects in early Venezuelan migrants, our estimates largely suggest that the large amnesty of undocumented migrants in Colombia has not had any significant effects on the Colombian formal labor markets in the short run.

VI Robustness Tests

Controlling for Early Settlements of Migrants

Given the continuous incoming flow of Venezuelan migrants during our period of analysis, an important concern regarding our estimates is that they might account for the effects of this con-

[18] For the case of economic sector and firm size, unemployment was not included in the analysis because economic sector and firm size are only observed for employed individuals.

tinuous migration on top of the effect of the PEP rollout we are exploring. To account for this issue, we test for the robustness of our main results to the inclusion of the interaction of early settlements of Venezuelan migrants (as observed in the population Census of 2005, the last census available before PEP status was granted) and year fixed effects. As shown by Rozo and Vargas (2019) a high and strong correlation exists between early settlements of Venezuelan migrants and the Venezuelan migration observed after 2005. We present the results of this exercise in Appendix B. Our results are robust to the inclusion of these controls. The only exception is that the impacts of the PEP program on the wages of Colombian workers who returned from Venezuela in the last five years is no longer significant and we observe a marginal increase in unemployment for these workers (equivalent to an increment of 0.003 percentage points per each additional 10% share of PEP holders).

Is the Number of Days Available for Registration Exogenous?

We first test whether our measure of average registration days available by department has any correlation with observable covariates. For this purpose, we regress *Reg. Days* (as defined by equation 3) on the observable individual covariates available in the labor force surveys (excluding our outcomes) and all the static controls listed in Table I of Appendix A. We find a positive and statistically significant correlation between the average number of days available to request a PEP and the distance to the closest Colombia-Venezuela border crossing. [19]

Consequently, we proceed to test whether including the interaction of the distance to the Colombia-Venezuela border and time dummies changes our general results in all our estimates. We present the results of this exercise in Appendix B. Our results are robust to the inclusion of these controls. [20]

[19] Our measure of distance is actually an inverse distance to the closest Colombia-Venezuela border crossing estimated in the following way. We first estimate the distance between the centroid of each municipality or department to each of the five points of entry at the Venezuela-Colombia border. These points are located in Cúcuta, Maicao, Arauca, Puerto Carreño, and Puerto Inírida. Then, we aggregate all the distances using a weighted average that weights the distance to each migration point using a share of the total number of Venezuelan migrants that entered Colombia by each point during 2014 and 2017. Our final step is to create the inverse distance measure as the ratio of 1 to the final weighted average.

[20] The only exception is that the impacts of the PEP program on the wages of Colombian workers who returned from Venezuela in the last five years is no longer significant.

Excluding Border Municipalities

Another important threat to the validity of our results is that regions at the Colombia-Venezuela border may be evolving in different ways relative to the rest of the country due to the intensification of the Venezuelan crisis. Bordering departments, for instance, may be experiencing higher economic activity since many Venezuelans come temporarily to Colombia to get food or medicines and then go back to their country. Alternatively, they could also be experiencing harsher economic conditions due to the collapse of the Venezuelan economy because Venezuela has historically been Colombia's main trading partner. Although, in practice, such situations will only constitute a threat to the validity of our estimates if these municipalities experience disproportionate changes only after August 2018, we still wanted to test whether our results will change if we remove these departments from our sample. For this purpose we exclude La Guajira, Norte de Santander, Boyacá, and Cesar (the bordering departments in our sample) and re-estimate the effects of the PEP program. The results of this exercise are presented in Appendix B and confirm that the PEP program has no significant effects on any of the outcomes or samples that we study.

Controlling for the Previous Waves of the PEP

Considering that two previous PEP waves occurred before the implementation of the program, we evaluate if it is plausible that the areas that legalized more migrants in waves 1 and 2 may have evolved in different ways relative to the other municipalities. Such differences may be confounding our estimates. To account for this possibility, we obtained information on the number of individuals regularized under PEP 1 and PEP 2 and their respective locations from the Colombian government and included interactions of these variables and year fixed effects in all our estimates. Our results remain completely unchanged with or without the inclusion of these controls. Hence, for brevity, our preferred specifications include these controls.

Restricting our Sample to Different Time Periods

We also test for the sensitivity of our results based on the time period used in the estimation, by excluding one month at a time and thus shrinking the sample consecutively. The results of

these exercise are presented in Appendix B, and they confirm that our findings are not sensitive to choosing a different period for the estimation. Naturally, as the sample uses fewer months for the estimation, the standard errors of the estimates become larger and thus the estimates become imprecise. Yet, the point estimates remain highly similar across all estimations.

VII Potential Explanations

Overall, our analysis suggests that granting work permits to undocumented migrants on a large scale has not translated to any observable effects in the Colombian labor markets. We know that 64% of the Venezuelan undocumented migrants identified in 2018 applied for a temporary job permit. Hence, our results are not explained by low take-up rates. However, several other factors can potentially explain why we do not observe any impacts of this program.

First and most importantly, approximately 50% of undocumented Venezuelan migrants reported being part of the Colombian labor markets as workers in the informal sector or as self-employed individuals before a PEP was granted (see Table II). Even if these individuals apply for a PEP, they may have little intention to get a formal job if they perceive that becoming a formal employee may only translate to higher costs for them. For instance, once they have a formal job, migrants are required to pay payroll taxes. Paying those contributions may not give migrants additional benefits because they may not receive pensions in Colombia. Under these assumptions, many migrants may be applying for a PEP to get health and education access rather than to get a formal job.

We examine this possibility by testing whether a wage premium is associated with being part of the formal sector in the Colombian labor markets. For this purpose, we estimate a Mincer equation for wages, in which we include a dummy variable for being an informal worker as a regressor. We estimate the regression for all the sample and for Venezuelan migrants. The results of this exercise are presented in Table XI.[21] The estimates confirm that formal workers earn almost double

[21]The regression includes all the controls included in our main estimates. Standard errors were clustered at the

the wages received by informal workers in the Colombian labor markets, which suggests that a large premium is associated with being a formal worker. Hence, it is unlikely that undocumented migrants perceive no benefits from getting a formal job. It remains true, however, that migrants may not have this information.

Another explanation for our results is that even if migrants are trying to get a formal job, it is unclear whether they will obtain one once a PEP is granted. For example, firms may not be willing to hire migrants or may not have information about how to use a PEP and what its implications are. Moreover, migrants may not have enough information on how to use a PEP in practice to apply for a formal job. For instance, they may not have connections in the formal sector. Our conversations with Colombian local authorities support this explanation, with local officials indicating that a large number of PEP holders report having difficulty with their PEP being recognized by firms. In some instances, firms lack information about how to use a PEP and do not trust it.

It is also plausible that we do not observe any impacts of the program because we focus on its effects in the short term and it may take more time to distinguish any observable effects.

More generally, however, it is not obvious that we should expect to see these effects at all, even if important composition changes are occurring in the labor markets as immigrants move to the formal sector. This may be because immigrants are complementary to local workers and thus are not competing in the same labor pool. In fact, some of our results show that the small effects exist when looking at the sample of Venezuelan workers for whom PEP workers are more likely to be substitutes.

While our study looks at the rollout of a formal migratory status for incumbent immigrants, the null effects we find across the board are consistent with several other studies that look at the effect of large inflows of immigrants and refugees on labor markets, which also show little to no effects with regard to labor outcomes of the native population (see Clemens and Hunt, 2017).

department-month level.

VIII Concluding Remarks

We examine the labor market impacts of a large-scale amnesty program that granted job permits to approximately half a million undocumented Venezuelan migrants. To our knowledge, our paper is the first to examine the impacts of this type of policy in a developing country with a large informal sector. This distinction is important given that illegal migrants may already be part of the informal sector, and as such, may have less incentive to formalize their situation.

All in all, we are unable to identify any significant impacts of this amnesty program on labor markets in the short run. While a number of potential explanations may exist for this result, we claim our results cannot be explained by low take-up rates of the program, since over two thirds of those eligible for the program were granted a regular migratory status.

While large-scale amnesty programs are a politically sensitive issue, we hope this study, based on the case of Venezuelans in Colombia, can shed some light on the possible economic implications of such policies and the need to establish complementary policies. The lack of impact in the current study may be due to the short-term period after PEP was put in place, the decision of immigrants to stay in informal labor markets, or the unwillingness of firms to hire migrants. If the latter two are the driving forces, policies to inform migrants and firms about the program as well as incentives for firms that hire migrants may complement the decision of governments to grant the right to work.

References

Altindag, O., O. Bakis, and S. Rozo (2018). Blessing or burden? the impact of refugees on businesses and the informal economy. *SSRN Working Paper N.3188406*.

Amuedo-Dorantes, C., C. Bansak, and S. Raphael (2007). Gender differences in the labor market: Impact of irca. *American Economic Review 97*(2), 412–416.

Borjas, G. J. (2003). The labor demand curve is downward sloping: Reexamining the impact of

immigration on the labor market. *The Quarterly Journal of Economics 118*(4), 1335–1374.

Borjas, G. J. and J. Monras (2017). The labour market consequences of refugee supply shocks. *Economic Policy 32*(91), 361–413.

Bratsberg, B., J. F. Ragan, Jr, and Z. M. Nasir (2002). The effect of naturalization on wage growth: A panel study of young male immigrants. *Journal of Labor Economics 20*(3), 568–597.

Burbidge, J. B., L. Magee, and A. L. Robb (1988). Alternative transformations to handle extreme values of the dependent variable. *Journal of the American Statistical Association 83*(401), 123–127.

Card, D. (2001). Immigrant inflows, native outflows, and the local labor market impacts of higher immigration. *Journal of Labor Economics 19*(1), 22–64.

Card, D. (2005). Is the new immigration really so bad? *The Economic Journal 115*(507), F300–F323.

Clemens, M. A. and J. Hunt (2017). The labor market effects of refugee waves: reconciling conflicting results. *NBER Working Paper n.23433*.

Del Carpio, X. V. and M. Wagner (2015). The impact of Syrians refugees on the Turkish labor market. *World Bank Policy Research Paper No. 7402*.

Dustmann, C., U. Schönberg, and J. Stuhler (2016). The impact of immigration: Why do studies reach such different results? *Journal of Economic Perspectives 30*(4), 31–56.

Dustmann, C., U. Schönberg, and J. Stuhler (2017). Labor supply shocks, native wages, and the adjustment of local employment. *The Quarterly Journal of Economics 132*(1), 435–483.

Fallah, B., C. Krafft, and J. Wahba (2019). The impact of refugees on employment and wages in Jordan. *Journal of Development Economics (Forthcoming)*.

Foged, M. and G. Peri (2016). Immigrants' effect on native workers: New analysis on longitudinal data. *American Economic Journal: Applied Economics 8*(2), 1–34.

Jones, J. and S. Pozzebon (2018, March). Venezuela's Health System Is in Worse Condition than Expected, Survey Finds. https://www.cnn.com/2018/03/28/americas/venezuela-hospitals-report/index.html.

Kaushal, N. (2006). Amnesty programs and the labor market outcomes of undocumented workers. *Journal of Human Resources 41*(3), 631–647.

Kossoudji, S. A. and D. A. Cobb-Clark (2002). Coming out of the shadows: Learning about legal status and wages from the legalized population. *Journal of Labor Economics 20*(3), 598–628.

MacKinnon, J. G. and L. Magee (1990). Transforming the dependent variable in regression models. *International Economic Review*, 315–339.

Orrenius, P. M. and M. Zavodny (2003). Do amnesty programs reduce undocumented immigration? evidence from irca. *Demography 40*(3), 437–450.

Ottaviano, G. I. and G. Peri (2012). Rethinking the effect of immigration on wages. *Journal of the European Economic Association 10*(1), 152–197.

Peri, G. and V. Yasenov (2018). The labor market effects of a refugee wave: Synthetic control method meets the Mariel boatlift. *Journal of Human Resources (Forthcoming)*.

Pfeffermann, D. (1993). The role of sampling weights when modeling survey data. *International Statistical Review / Revue Internationale de Statistique 61*(2), 317–337.

Rozo, S. and J. F. Vargas (2019). Brothers or invaders? how crisis-driven migrants shape voting behavior. *SSRN N.3401036*.

Sequera, V. (2018, February). Venezuelans report big weight losses in 2017 as hunger hits. https://www.reuters.com/article/us-venezuela-food/venezuelans-report-big-weight-losses-in-2017-as-hunger-hits-idUSKCN1G52HA.

Solon, G., S. Haider, and J. Wooldridge (2015). What are we weighting for? *Journal of Human Resources 50*(2), 301–316.

The Guardian (2017, May). Venezuela's Infant Mortality, Maternal Mortality and Malaria Cases Soar. https://www.theguardian.com/global-development/2017/may/09/venezuela-public-health-crisis-infant-mortality-maternal-malaria.

UNHCR (2018). Global Trends: Forced Displacement in 2018. Technical report, The UN Refugee Agency.

Table (I) Characterizing Migrants Registered in RAMV: Individual Characteristics

	RAMV sample	Obs	Without PEP	With PEP	Mean Diff
Panel A: Individual					
Female* [=1]	0.497	441,237	0.498	0.496	0.003
	(0.500)		(0.500)	(0.500)	
Age	25.889	439,491	23.944	26.982	-3.039***
	(14.570)		(15.126)	(14.131)	
Married or Cohabiting	0.347	441,237	0.293	0.377	-0.084***
	(0.476)		(0.455)	(0.485)	
Born in Venezuela	0.975	441,237	0.973	0.976	-0.003***
	(0.155)		(0.161)	(0.152)	
Born in Colombia	0.022	441,237	0.023	0.021	0.002***
	(0.147)		(0.151)	(0.145)	
No education [=1]	0.123	441,234	0.169	0.097	0.072***
	(0.329)		(0.375)	(0.296)	
Primary [=1]	0.180	441,234	0.209	0.163	0.046***
	(0.384)		(0.407)	(0.369)	
Secondary [=1]	0.500	441,234	0.463	0.521	-0.058***
	(0.500)		(0.499)	(0.500)	
University or Higher [=1]	0.155	441,234	0.110	0.182	-0.072***
	(0.362)		(0.312)	(0.385)	
PEP holder [=1]	0.638	441,237			
	(0.481)				
Panel B: Household					
Family size	1.736	254,202	1.548	1.827	-0.280***
	(1.260)		(1.105)	(1.319)	
Children in household	0.251	254,202	0.198	0.277	-0.080***
	(0.434)		(0.398)	(0.448)	
Single parent HH [=1]	0.608	63,874	0.705	0.574	0.132***
	(0.488)		(0.456)	(0.495)	
Number of children	0.450	250,523	0.360	0.494	-0.133***
	(0.922)		(0.863)	(0.946)	

Notes: Standard deviation in parentheses.

Table (II) Characterizing Migrants Registered in RAMV: Work Status and Sector

	RAMV sample	Obs	Without PEP	With PEP	Mean Diff
Worker [=1]	0.626	322,716	0.596	0.640	-0.044***
	(0.484)		(0.491)	(0.480)	
Formal worker [=1]	0.010	322,716	0.011	0.010	0.001*
	(0.100)		(0.102)	(0.098)	
Informal worker [=1]	0.320	322,716	0.278	0.340	-0.063***
	(0.466)		(0.448)	(0.474)	
Self-employed [=1]	0.296	322,716	0.308	0.290	0.018***
	(0.456)		(0.462)	(0.454)	
Unemployed [=1]	0.244	322,716	0.257	0.238	0.019***
	(0.430)		(0.437)	(0.426)	
Degree recognized [=1]	0.113	441,182	0.094	0.124	-0.029***
	(0.317)		(0.292)	(0.329)	
Labor certificate	0.129	441,174	0.098	0.147	-0.048***
	(0.335)		(0.297)	(0.354)	
Sector: Finance, Admin	0.003	320,391	0.002	0.003	-0.001**
	(0.050)		(0.046)	(0.052)	
Sector: Health and Nat.Science	0.003	320,391	0.002	0.004	-0.001***
	(0.058)		(0.050)	(0.062)	
Sector: Soc.Science, Art, Sports	0.010	320,391	0.008	0.011	-0.002***
	(0.099)		(0.092)	(0.103)	
Sector: Services/Sales	0.361	320,391	0.302	0.395	-0.093***
	(0.480)		(0.459)	(0.489)	
Sector: Extractive Indust	0.007	320,391	0.008	0.007	0.001***
	(0.084)		(0.090)	(0.081)	
Sector: Transport, Equip. Operation	0.026	320,391	0.019	0.030	-0.012***
	(0.160)		(0.136)	(0.172)	
Sector: Process, Fabrication, Assembly	0.024	320,391	0.018	0.028	-0.009***
	(0.154)		(0.134)	(0.164)	

Notes: Standard deviation in parentheses. Labor force variables are calculated only for the population between 12 and 65 years of age. Worker=1 for formal, informal and self-employed individuals.

Table (III) Characterizing Migrants Registered in RAMV: Schooling and Health Access

	RAMV sample	Obs	Without PEP	With PEP	Mean Diff
Active student	0.264	116,300	0.246	0.279	-0.033***
	(0.441)		(0.431)	(0.449)	
Enrolled in school, age 6-11	0.407	38,823	0.386	0.424	-0.038***
	(0.491)		(0.487)	(0.494)	
Enrolled in school, age 12-17	0.350	29,732	0.339	0.358	-0.019***
	(0.477)		(0.473)	(0.479)	
Children working, age 6-11	0.006	38,823	0.006	0.006	-0.000
	(0.078)		(0.076)	(0.079)	
Children working, age 12-17	0.072	29,732	0.077	0.069	0.009**
	(0.259)		(0.267)	(0.253)	
In school x I[Arrival 2016]	0.385	12,547	0.367	0.402	-0.035***
	(0.487)		(0.482)	(0.490)	
In school x I[Arrival 2017]	0.322	45,814	0.297	0.342	-0.045***
	(0.467)		(0.457)	(0.474)	
In school x I[Arrival 2018]	0.192	57,861	0.178	0.204	-0.026***
	(0.394)		(0.382)	(0.403)	
Access to Col health system	0.011	441,222	0.015	0.009	0.005***
	(0.105)		(0.120)	(0.095)	

Notes: Standard deviation in parentheses. Education variable for children under 18 years of age. Access to health for the full sample.

Table (IV) Characterizing Migrants Registered in RAMV: Migration Dynamics and Networks

	RAMV sample	Obs	Without PEP	With PEP	Mean Diff
Longterm: stay in Colombia [=1]	0.894	441,222	0.903	0.889	0.015***
	(0.308)		(0.295)	(0.315)	
Longterm: move different country [=1]	0.020	441,222	0.021	0.019	0.002***
	(0.138)		(0.142)	(0.136)	
Longterm: return to Venezuela [=1]	0.086	441,222	0.076	0.092	-0.017***
	(0.281)		(0.265)	(0.290)	
Family in Colombia [=1]	0.435	441,209	0.473	0.413	0.060***
	(0.496)		(0.499)	(0.492)	
Family in Venezuela [=1]	0.661	441,209	0.652	0.666	-0.015***
	(0.473)		(0.476)	(0.472)	
Size of family in Venezuela	2.556	434,397	2.510	2.582	-0.072***
	(2.723)		(2.728)	(2.720)	
Network, Immig. from same munip	0.102	440,848	0.128	0.088	0.040***
	(0.148)		(0.175)	(0.129)	

Notes: Standard deviation in parentheses. For an immigrant whose last municipality of residence in Venezuela was *i* and is now living in a municipality *j* in Colombia, *Network size* is the sum of all immigrants from municipality of origin *i* and *Network share* is the ratio of Network size *i* to the total number of immigrants in the host municipality *j*.

Table (V) Characterizing Migrants Registered in RAMV: Age Distribution

	RAMV Full Sample	RAMV Female	RAMV Male	PEP Full Sample	PEP Female	PEP Male
Age: 0-5 years	47,745	23,476	24,241	25,114	12,282	12,816
	(10.82)	(10.71)	(10.93)	(8.93)	(8.81)	(9.05)
Age: 6-11 years	38,823	19,034	19,779	21,427	10,546	10,876
	(8.80)	(8.68)	(8.92)	(7.62)	(7.56)	(7.68)
Age: 12-17 years	29,732	14,863	14,863	17,200	8,608	8,587
	(6.74)	(6.78)	(6.70)	(6.11)	(6.17)	(6.06)
Age: 18-65 years	320,524	159,505	160,731	216,065	107,199	108,685
	(72.64)	(72.76)	(72.50)	(76.81)	(76.86)	(76.75)
Age: 66+ years	4,413	2,328	2,084	1,488	846	641
	(1.00)	(1.06)	(0.94)	(0.53)	(0.61)	(0.45)
Total	441,237	219,206	221,698	281,294	139,481	141,605
	(100.00)	(100.00)	(100.00)	(100.00)	(100.00)	(100.00)

Table (VI) Descriptive Statistics Labor Force Surveys

Panel A: Colombian Sample					
Dep. Variable	**Observations**	**Average**	**St. Deviation**	**Min**	**Max**
Hours Worked	727,607	45.36	15.75	1.00	130.00
Wages	635,254	1,070,935	1,373,314	0.00	102,000,000
Unemployed	1,230,548	0.08	0.27	0.00	1.00
Labor Force	1,230,548	0.60	0.49	0.00	1.00
Sex [=1 Female]	1,230,548	0.46	0.50	0.00	1.00
Age	1,230,548	35.35	15.12	12.00	65.00
Married	1,230,548	0.46	0.50	0.00	1.00
Household Chief	1,230,548	0.34	0.47	0.00	1.00
Years of Education	1,230,548	6.45	3.71	0.00	99.00
Informality	1,160,241	0.45	0.50	0.00	1.00
Panel B: Venezuelan Sample					
Hours Worked	16,122	49.54	16.22	1.00	130.00
Wages	14,560	683,679	801,622	0.00	30,000,000
Unemployed	25,413	0.12	0.32	0.00	1.00
Labor Force	25,413	0.73	0.45	0.00	1.00
Sex [=1 Female]	25,413	0.49	0.50	0.00	1.00
Age	25,413	28.69	10.78	12.00	65.00
Married	25,413	0.54	0.50	0.00	1.00
Household Chief	25,413	0.26	0.44	0.00	1.00
Years of Education	25,413	7.31	4.02	0.00	99.00
Informality	5,623	0.54	0.50	0.00	1.00
Panel C. Colombian Returnees Sample					
Hours Worked	6,670	47.25	16.31	1.00	130.00
Wages	5,898	691,530	718,314	0.00	20,000,000
Unemployed	10,343	0.12	0.32	0.00	1.00
Labor Force	10,343	0.72	0.45	0.00	1.00
Sex [=1 Female]	10,343	0.48	0.50	0.00	1.00
Age	10,343	37.39	14.03	12.00	65.00
Married	10,343	0.54	0.50	0.00	1.00
Household Chief	10,343	0.34	0.47	0.00	1.00
Years of Education	10,343	7.04	3.71	0.00	99.00
Informality	7,473	0.74	0.44	0.00	1.00

Table (VII) Impacts of PEP on Colombian Workers

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	-0.001	-0.011	0.001	0.002*
	(0.001)	(0.007)	(0.001)	(0.001)
Adj.R-squared	0.058	0.020	0.044	0.200
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	0.000	-0.002	0.000	-0.001**
	(0.000)	(0.002)	(0.000)	(0.000)
Adj.R-squared	0.058	0.020	0.044	0.200
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	0.002	-0.021	0.001	-0.006**
	(0.005)	(0.023)	(0.003)	(0.003)
Adj.R-squared	0.058	0.020	0.044	0.200
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.093***	0.093***	0.107***	0.113***
	(0.016)	(0.016)	(0.017)	(0.017)
F-Test	32.03	32.03	39.4	43.29
Observations (All Panels)	373,460	373,460	737,690	1,230,548
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: Additional controls include full interactions of year dummies and i) Gini index in 2005, ii)% of households in Colombia with at least one unsatisfied basic need in 2005, iii) % of Households in Colombia with at least one informal worker in 2005, iv) homicide rates in 2014, v) terrorist attacked in 1995, vi) night light density in 2013, vii) number of financial institution in 1995, viii) number of tax collection offices in 1995, ix) agriculture, industry, and services GDP in 2009, x) central government transfers in 2009, xi) transfers in education in 2009, xii) transfers in health in 2009, xiii) total municipal income in 2016, xiv) total municipal expenditures in 2016, and xv) total exports to Venezuela in 2016. Individual covariates include i) years of education, ii) marital status, iii) age, iv) gender, and v) a dummy for household heads. Clustered standard errors at the department-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (VIII) Impacts of PEP on Venezuelan Workers

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	0.012	0.113	0.018***	-0.018***
	(0.019)	(0.207)	(0.007)	(0.006)
Adj.R-squared	0.118	0.065	0.059	0.216
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	0.003	0.042	0.004*	-0.005***
	(0.005)	(0.047)	(0.002)	(0.001)
Adj.R-squared	0.118	0.066	0.059	0.216
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	0.028	0.417	0.023*	-0.032***
	(0.052)	(0.471)	(0.013)	(0.009)
Adj.R-squared	0.118	0.062	0.059	0.216
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.100***	0.100***	0.157***	0.158***
	(0.019)	(0.019)	(0.014)	(0.014)
F-Test	29.39	29.39	122.93	128.11
Observations (All Panels)	1,890	1,890	18,425	25,413
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table VII. Clustered standard errors at the department-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (IX) Impacts of PEP on Colombia Returnees Workers

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	0.005	0.125	0.013	-0.004
	(0.017)	(0.078)	(0.010)	(0.010)
Adj.R-squared	0.140	0.082	0.055	0.177
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	-0.002	0.050**	0.004*	0.004
	(0.005)	(0.023)	(0.002)	(0.003)
Adj.R-squared	0.140	0.083	0.055	0.177
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	-0.026	0.539**	0.028*	0.028
	(0.055)	(0.273)	(0.016)	(0.019)
Adj.R-squared	0.139	0.070	0.055	0.176
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.093***	0.093***	0.135***	0.138***
	(0.024)	(0.024)	(0.017)	(0.017)
F-Test	16.59	16.59	60.27	67.49
Observations (All Panels)	1,440	1,440	7,473	10,343
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table VII. Clustered standard errors at the department-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (X) Impacts of Regularization on Firms

Var in logs*	**Births***	**Deaths***
	(1)	(2)
Panel A: OLS		
Pep Holders x I(Post Aug. 2018)	0.034***	-0.052**
	(0.011)	(0.026)
Adj.R-squared	0.981	0.904
Panel B: Reduced Form		
Reg. Days x I(Post Aug. 2018)	0.003	-0.009
	(0.004)	(0.006)
Adj.R-squared	0.981	0.904
Panel C: 2SLS (Second Stage)		
Pep Holders x I(Post Aug. 2018)	0.023	-0.060
	(0.026)	(0.041)
Adj.R-squared	0.981	0.904
Panel D: First Stage		
Reg. Days x I(Post Aug. 2018)	0.146***	0.146***
	(0.019)	(0.019)
F-Test	67.71	67.71
Observations (All Panels)	660	660
Controls (All Panels)		
Dep. FE	Yes	Yes
Year-Monthly FE.	Yes	Yes
Add. Controls	Yes	Yes

Notes: We are including the same controls of Table VII. Clustered standard errors at the department-monthly level are reported in parentheses. . *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (XI) Mincer Equation: Testing for a Formal Sector Premium

Dep. Variable	Wages
Gender [=1 if male]	0.341***
	(0.006)
Age	0.001***
	(0.000)
Married [=1 if Married or Cohabiting]	0.036***
	(0.005)
Head of Household	0.220***
	(0.005)
Years of Education	0.007***
	(0.002)
Years of Education^2	-0.001***
	(0.000)
Informality: Does not Contribute to Health System	-0.935***
	(0.009)
Adj. R-squared	0.083
Observations	602,935

Notes: Wages were transformed using the inverse hyperbolic sine transformation (see Burbidge et al., 1988 and MacKinnon and Magee, 1990 for details). The coefficients can be interpreted as a log transformation on the dependent variable. We are including the same controls of Table VII. Clustered standard errors at the department-monthly level are reported in parentheses.

Figure (I) RAMV Registration Points

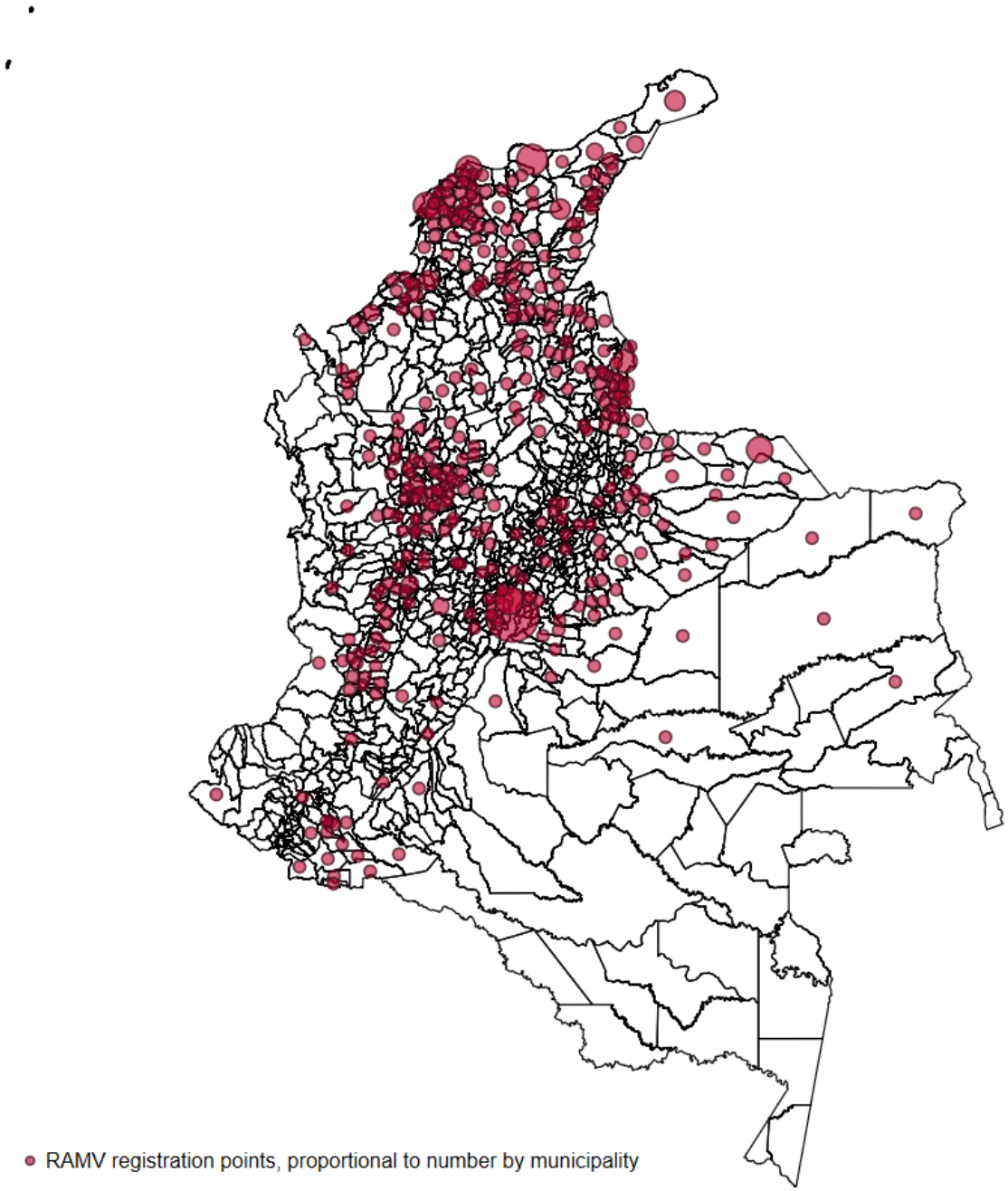


Figure (II) Geographic Location of Irregular Migrants Identified with the RAMV

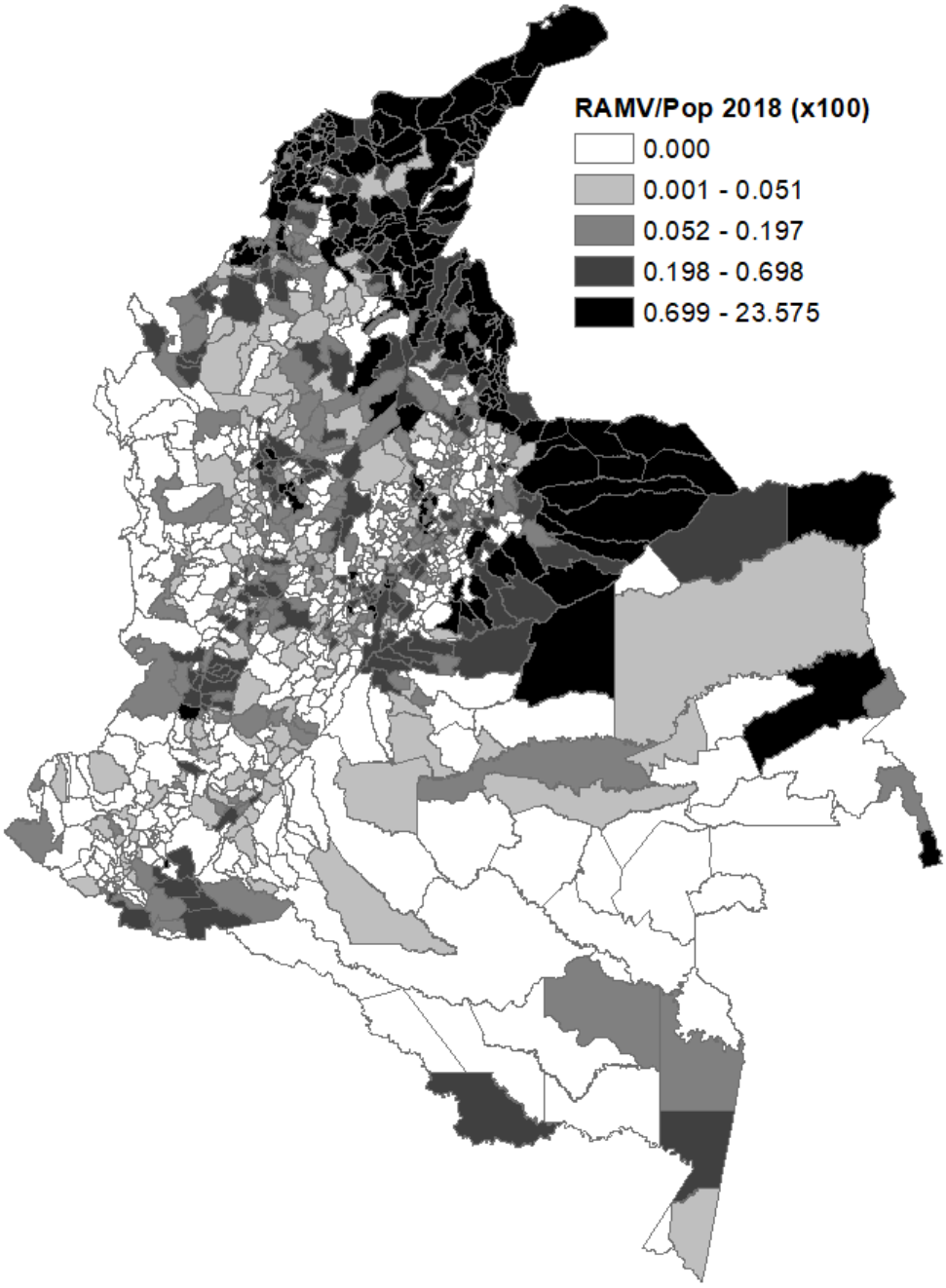


Figure (III) Distribution of Registration Days for PEP

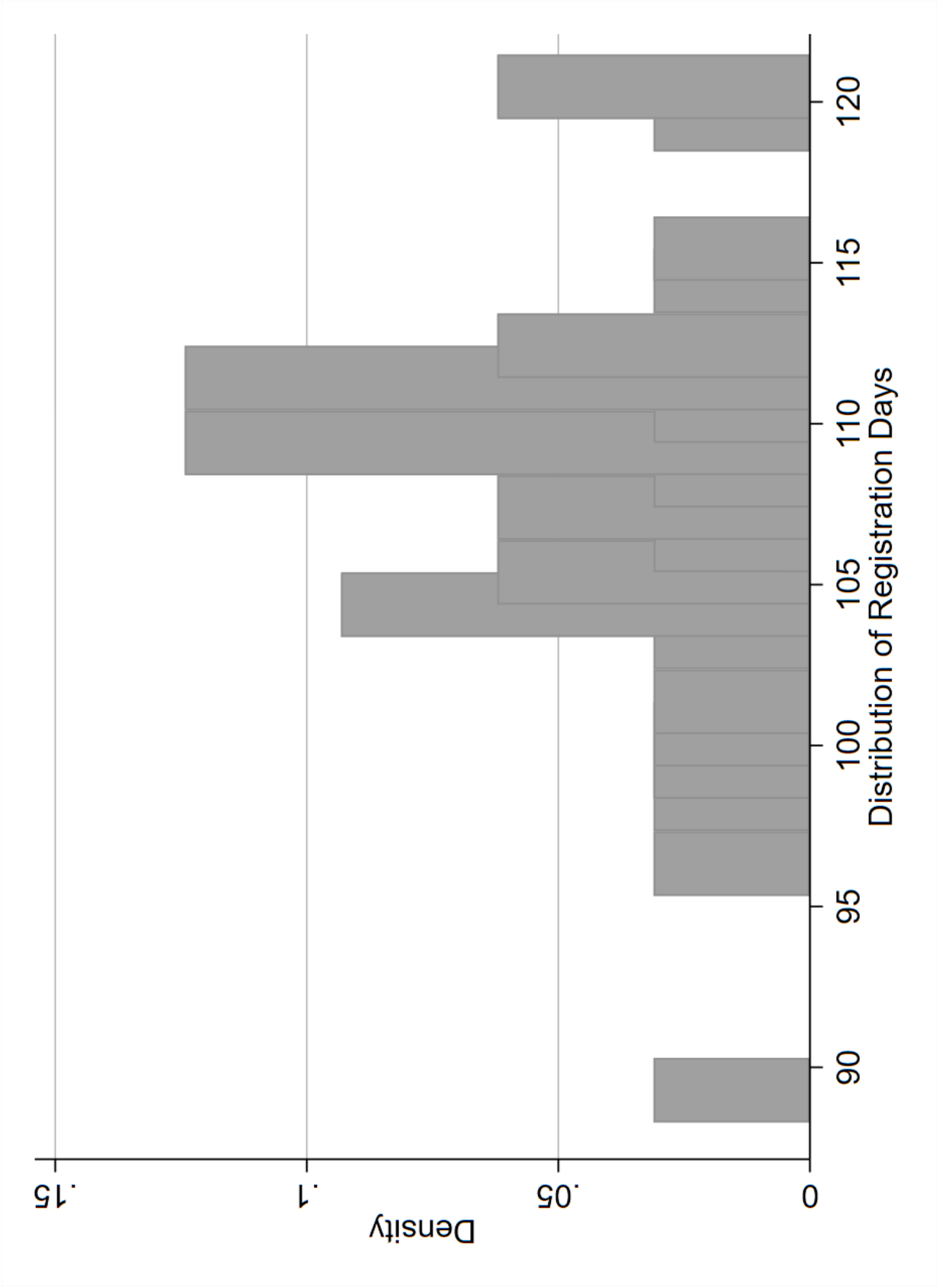


Figure (IV) Average Registration Days and PEP Requests

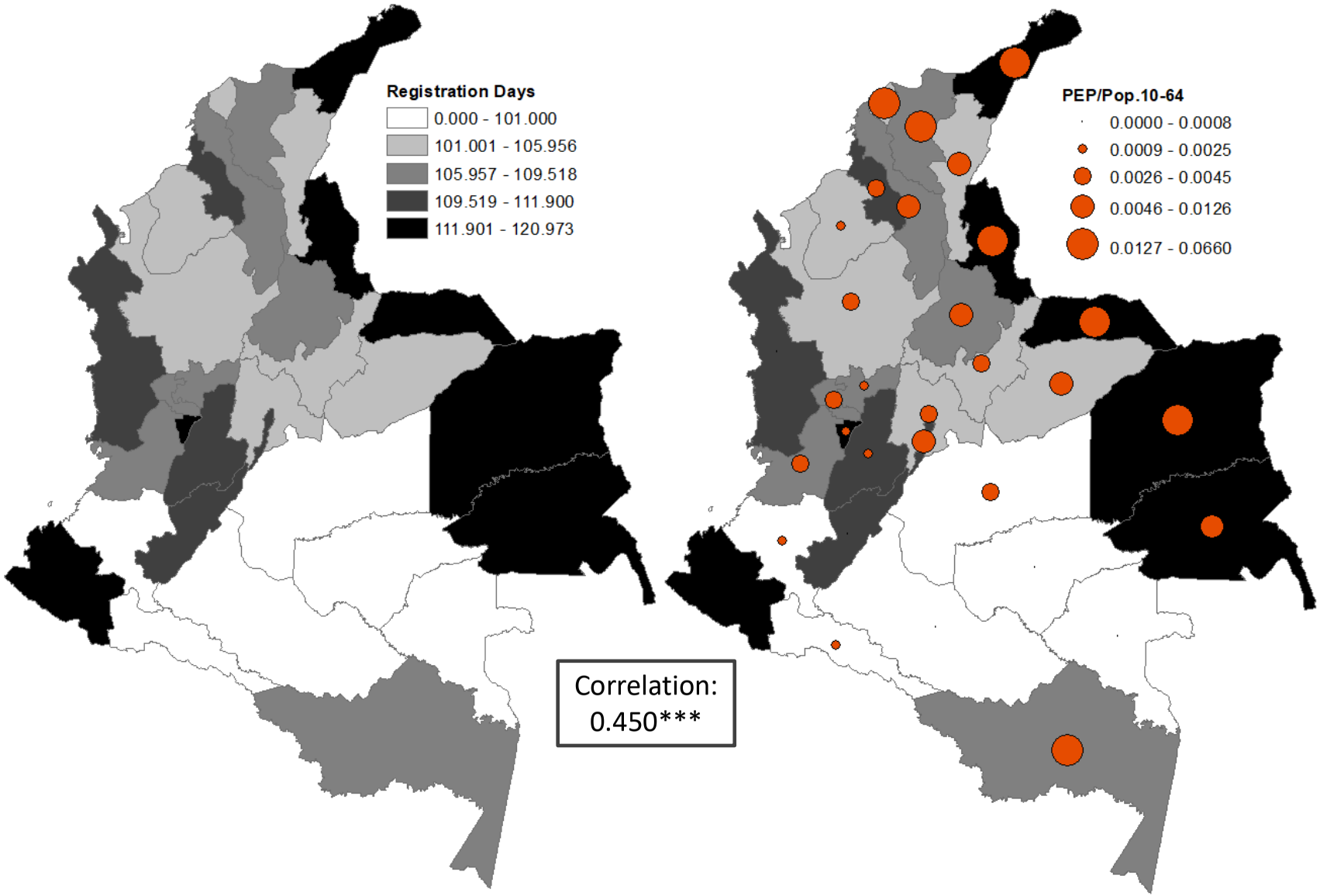
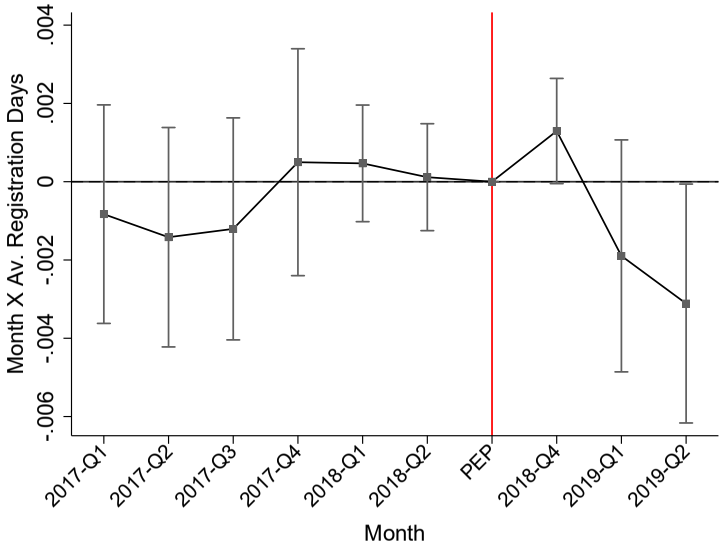
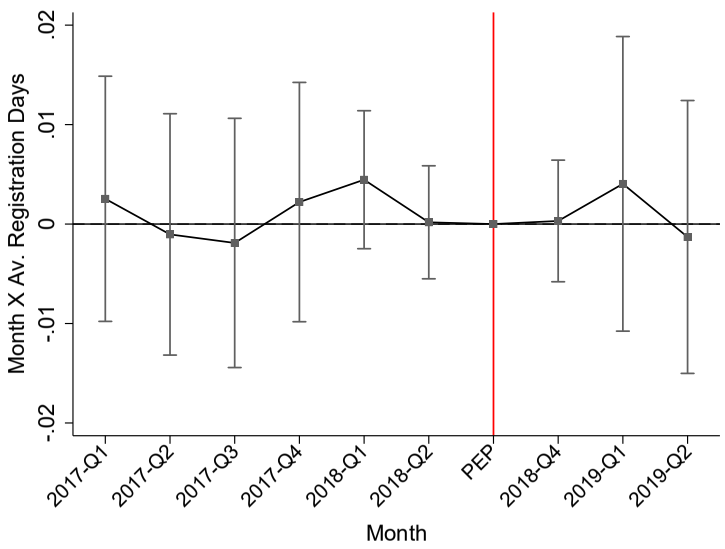


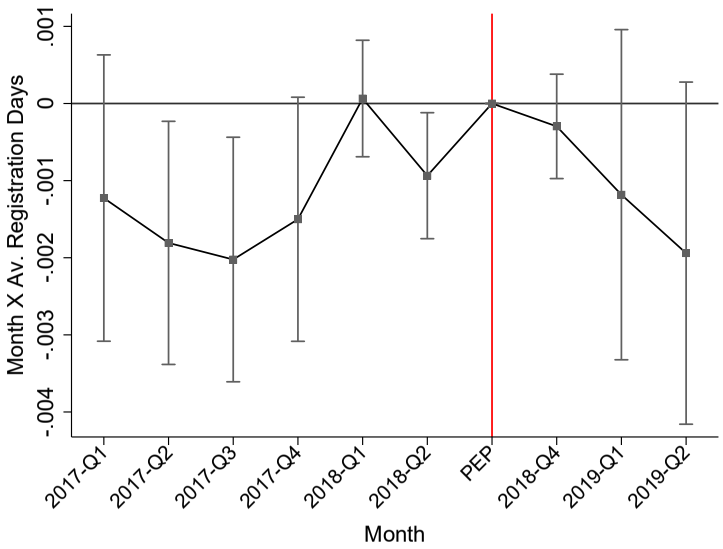
Figure (V) Event Study: Colombian Sample



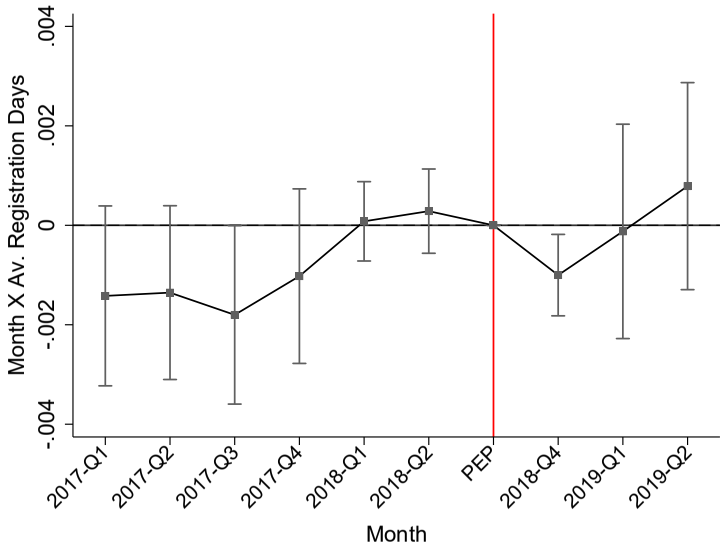
(a) Weekly Hours Worked



(b) Monthly Wages

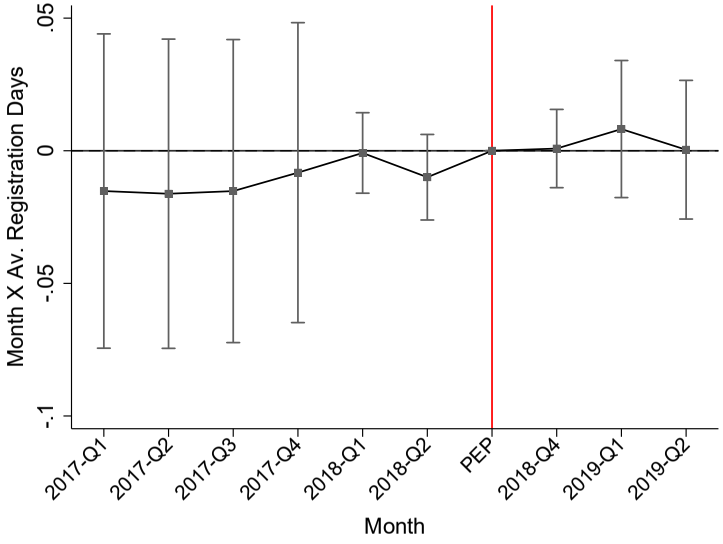


(c) Unemployed

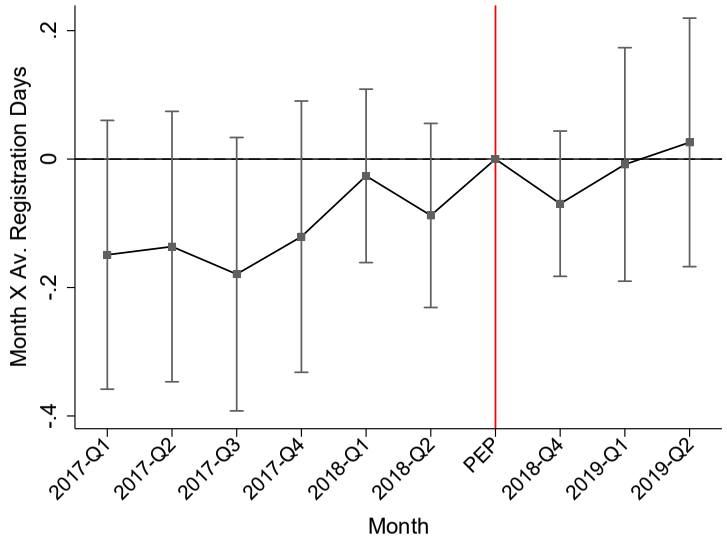


(d) Labor Force

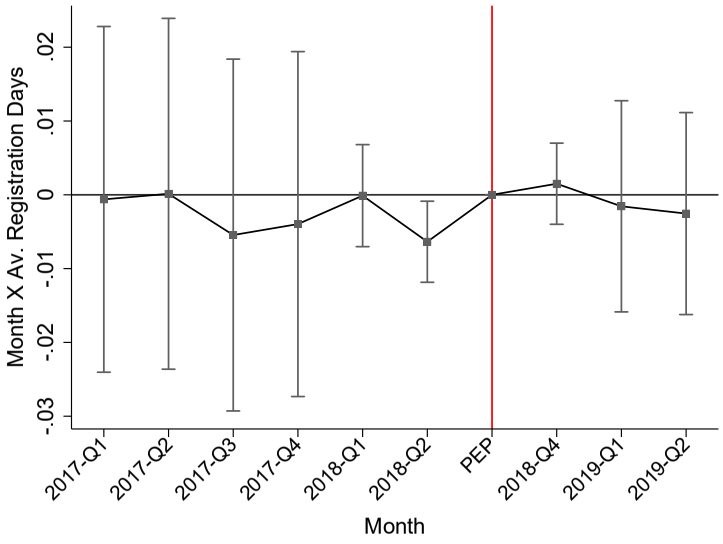
Figure (VI) Event Study: Venezuelan Sample



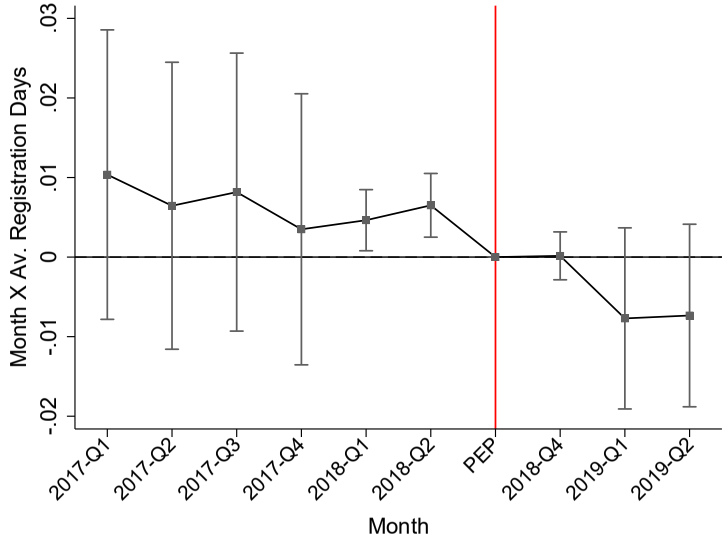
(a) Weekly Hours Worked



(b) Monthly Wages

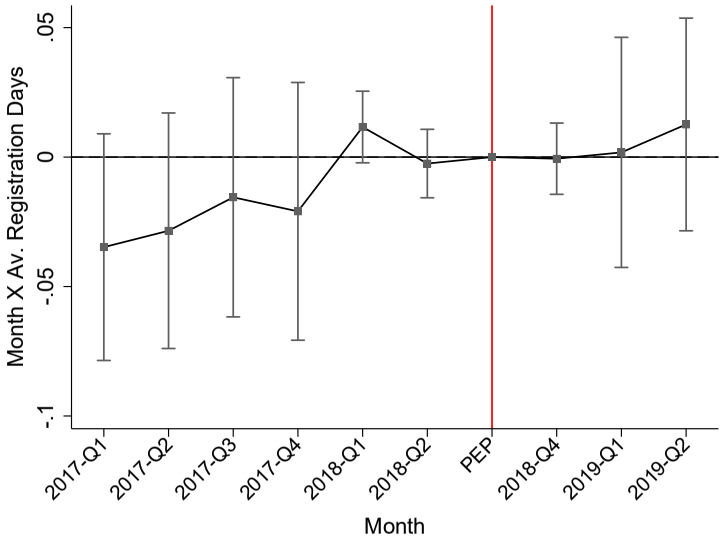


(c) Unemployed

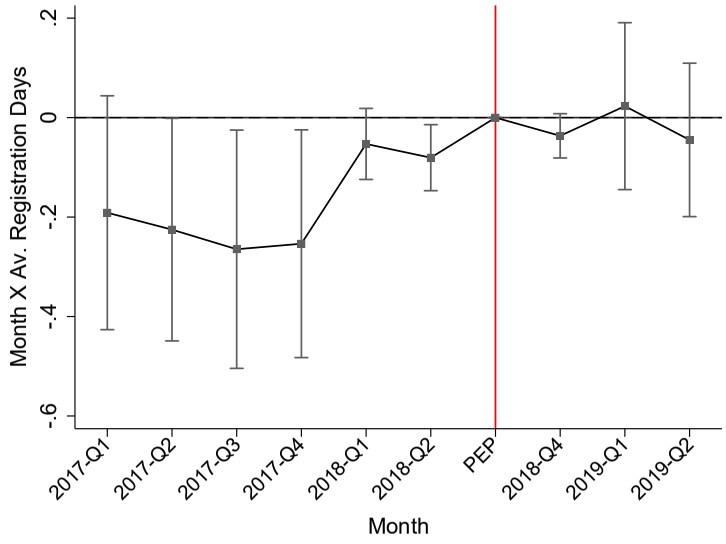


(d) Labor Force

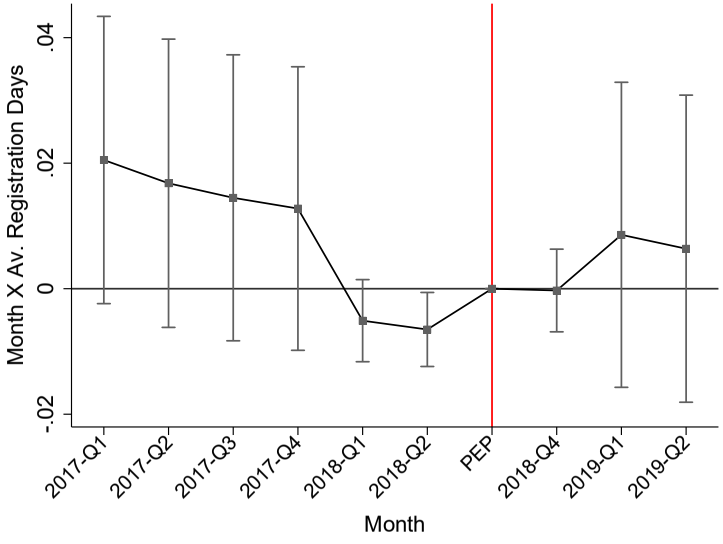
Figure (VII) Event Study: Colombian Returnees Sample



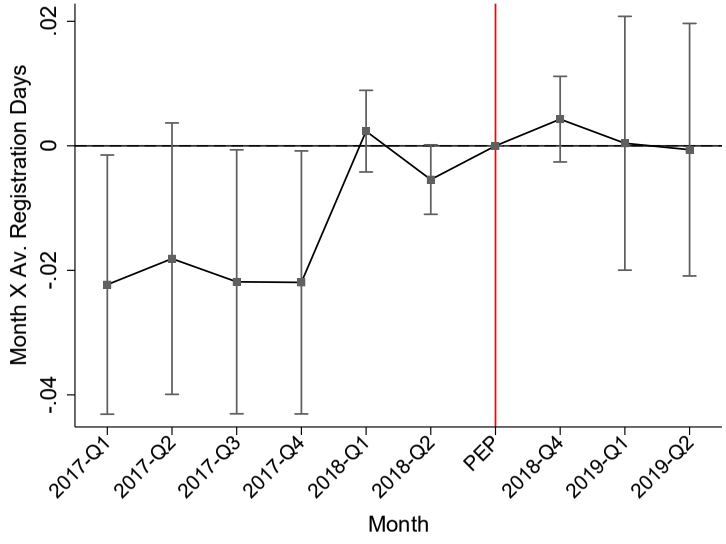
(a) Weekly Hours Worked



(b) Monthly Wages

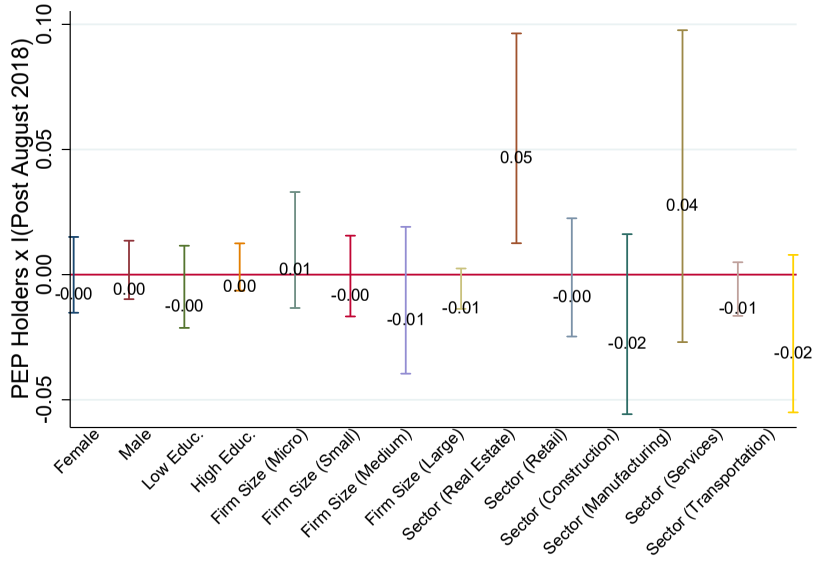


(c) Unemployed

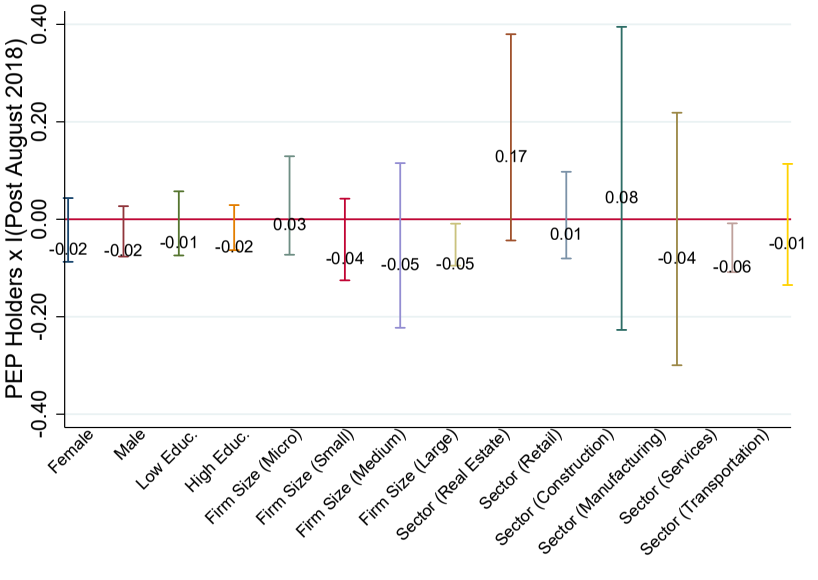


(d) Labor Force

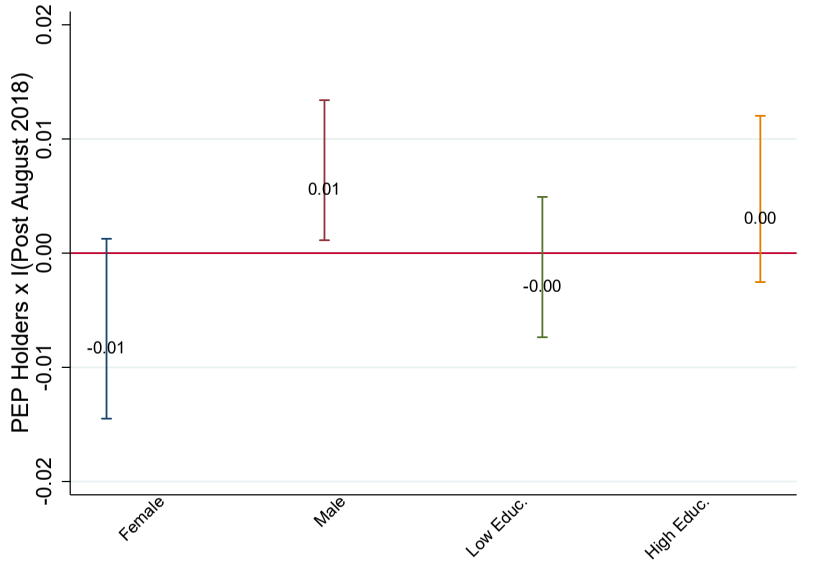
Figure (VIII) Heterogeneous Effects: Colombian Workers



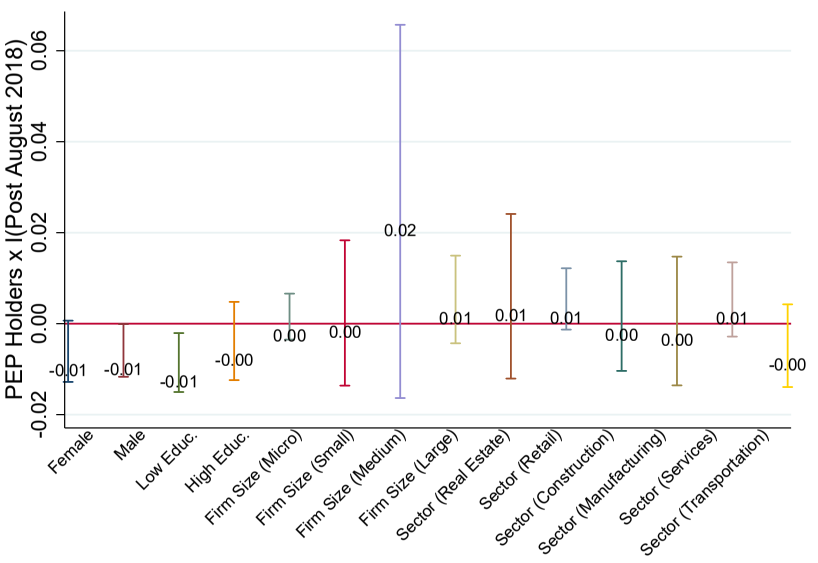
(a) Weekly Hours Worked



(b) Monthly Wages

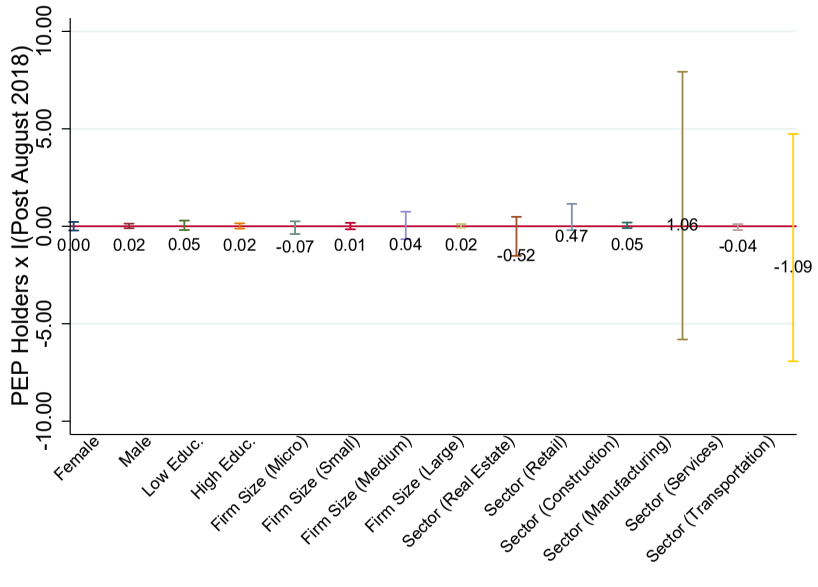


(c) Unemployed

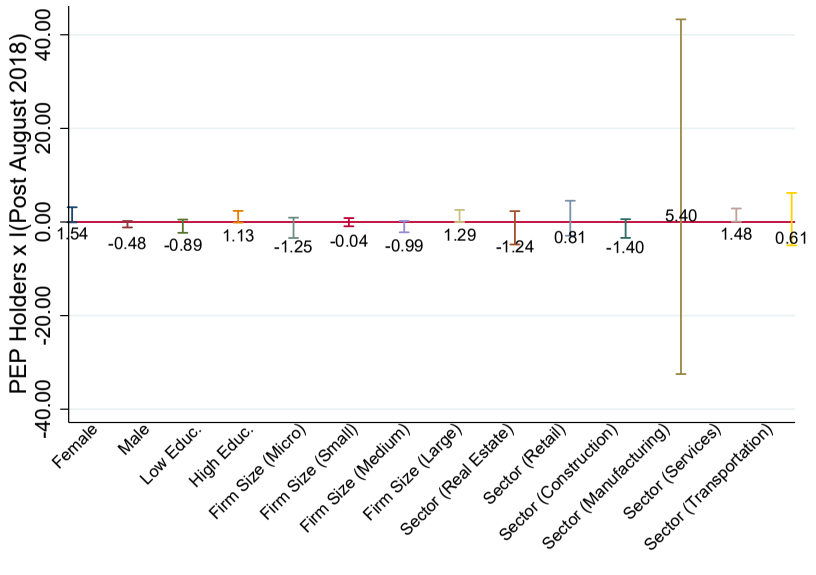


(d) Labor Force

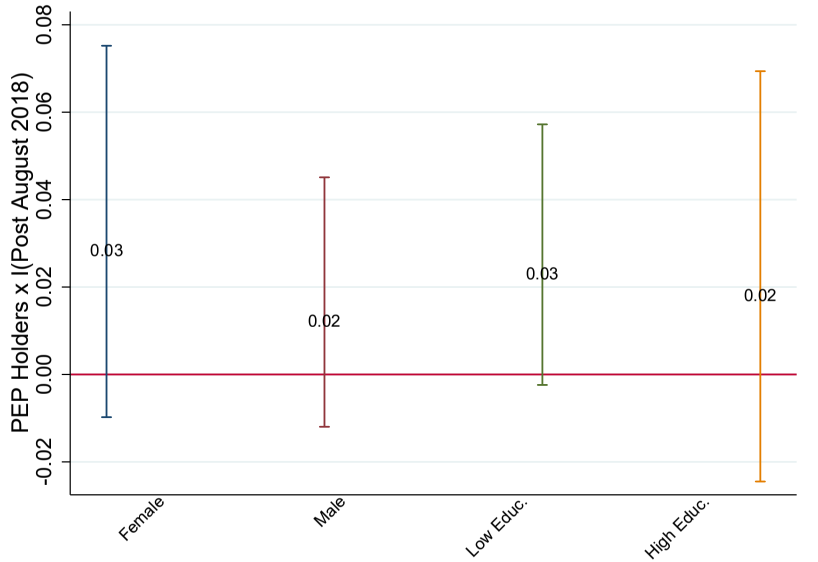
Figure (IX) Heterogeneous Effects: Venezuelan Workers



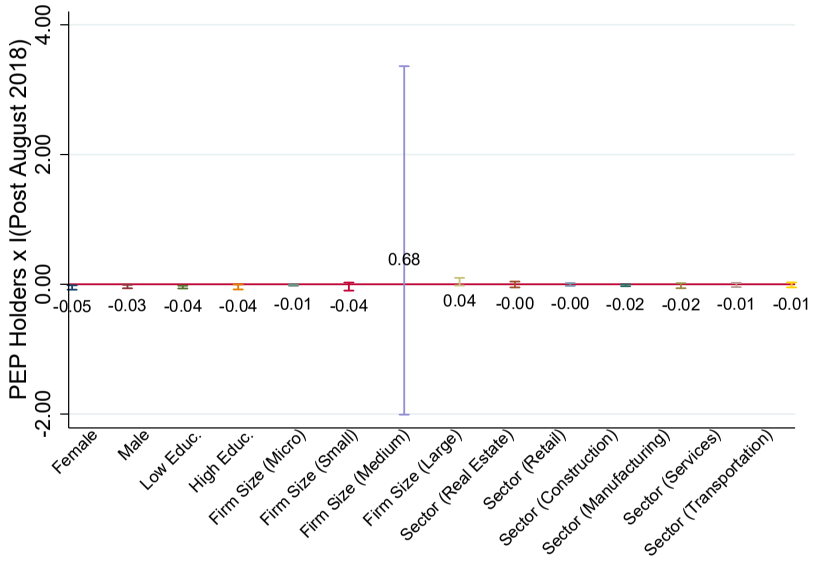
(a) Weekly Hours Worked



(b) Monthly Wages

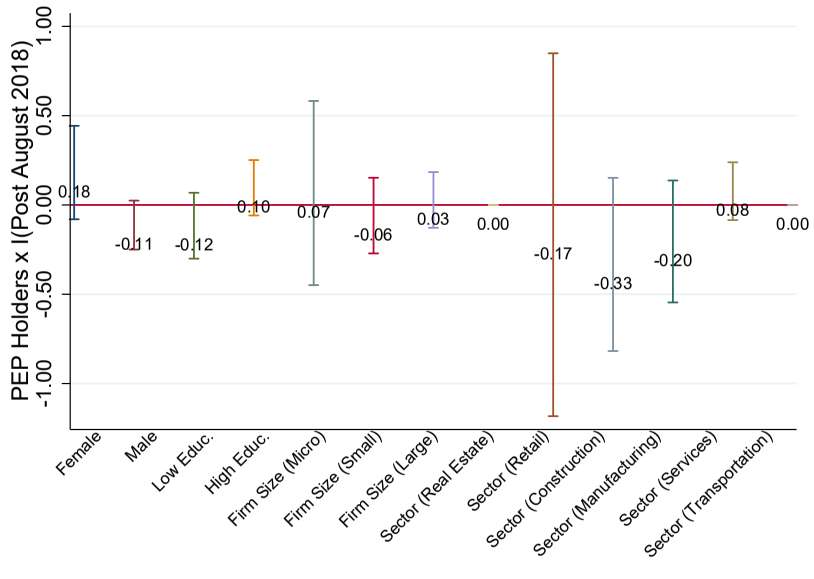


(c) Unemployed

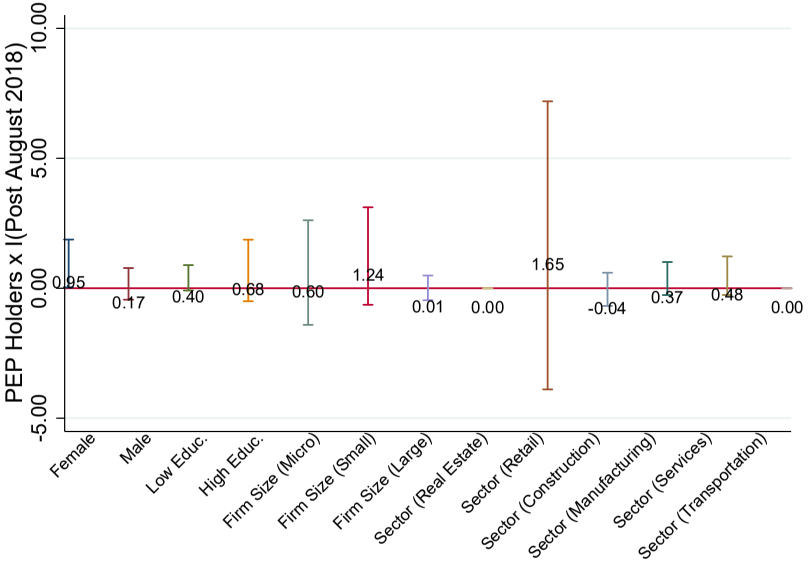


(d) Labor Force

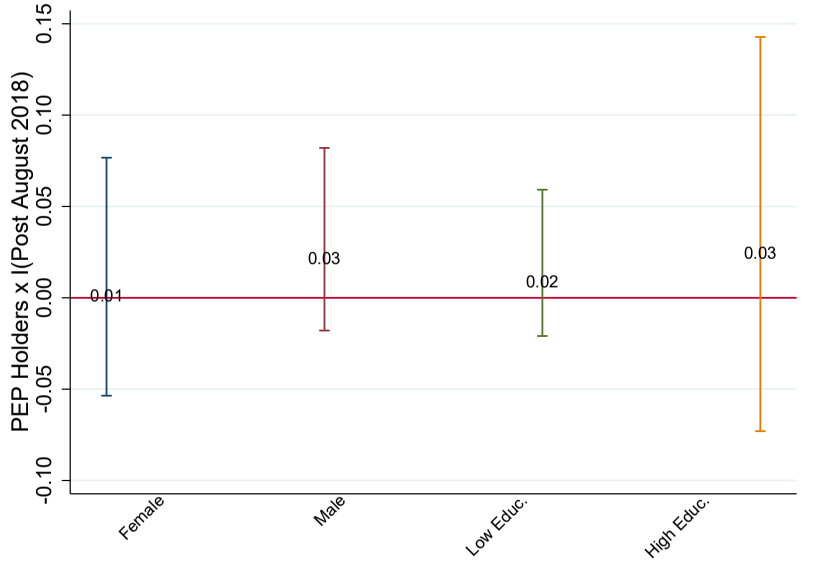
Figure (X) Heterogeneous Effects: Colombian Returnees Workers



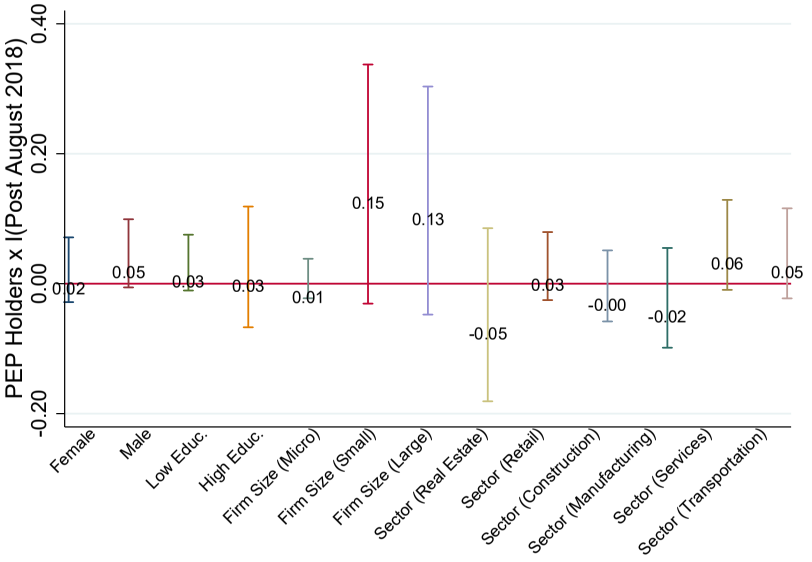
(a) Weekly Hours Worked



(b) Monthly Wages



(c) Unemployed



(d) Labor Force

Appendix A: Descriptive Statistics

Table (A.1) Descriptive Statistics - Control Variables

Variable	Year	Observations	Average	St. Deviation	Min	Max
GINI	2005	24	0.45	0.03	0.42	0.54
Unsatisfied Basic Needs (UBN, % Households)	2005	24	44.47	16.57	9.16	72.33
Informal Labor* (% Household)	2005	24	94.69	4.59	77.16	98.49
Homicide (% Pop by 100.000 Individuals)	2014	24	33.90	19.31	10.20	74.12
N. of Terrorist Attacks	1995	24	1.08	1.95	0.00	8.00
Night Light Density	2013	24	4.71	4.95	0.08	21.54
Number of Financial Institutions	1995	24	74.17	65.38	11.00	252.00
Number of Tax Collection Office	1995	24	29.25	25.60	4.00	99.00
GDP in agriculture (Millions)	2009	24	1.37	1.74	0.25	8.13
GDP in industry (Millions)	2009	24	1.38	1.70	0.25	7.55
GDP in services (Millions)	2009	24	1.14	0.88	0.08	3.82
Central Gov. Transfers (Millions)	2010	24	4.73	5.73	0.24	22.97
Educ. Central Transfers (Millions)	2010	24	8.85	15.36	0.70	72.70
Health Central Transfers (Millions)	2010	24	578,723	455,661	157,098	2,048,441
Total Municipal Income (Millions)	2016	24	271.94	295.77	40.89	1,333.02
Mun. Public Expenditure (Millions)	2016	24	244.89	445.22	45.70	2,267.07
Total Exports to Venezuela	2005	24	16.53	27.98	0.00	102.51
PEP1 (August 2017-October 2017)	2017	24	2,816	5,835	45.00	27,703
PEP2 (February 2018-June 2018)	2018	24	4,618	9,932	37.00	47,389
PEP4 (January 2019-)	2019	24	4,812	10,712	61.00	48,843
Inverse of Distance to the Border	1995	23	0.04	0.21	0.00	1.00
Venezuelan Population	2005	24	1,529	2,002	17	8,303

Table (A.2) Descriptive Statistics - Registration Shift

Registration Number		Registration Date		Pop. Share by Registration Shift
Since	**Until**	**Since**	**Until**	
1	14,752	2-Aug-18	21-Dec-18	0.04652
14,753	30,213	5-Aug-18	21-Dec-18	0.04885
30,214	4,002,617	8-Aug-18	21-Dec-18	0.04714
4,002,618	4,014,997	11-Aug-18	21-Dec-18	0.04648
4,014,998	4,027,640	14-Aug-18	21-Dec-18	0.04634
4,027,641	4,040,663	17-Aug-18	21-Dec-18	0.04667
4,040,664	4,053,186	20-Aug-18	21-Dec-18	0.04630
4,053,187	4,065,677	23-Aug-18	21-Dec-18	0.04619
4,065,678	4,078,492	26-Aug-18	21-Dec-18	0.04620
4,078,493	4,091,505	29-Aug-18	21-Dec-18	0.04613
4,091,506	4,104,531	1-Sep-18	21-Dec-18	0.04616
4,104,532	4,117,421	4-Sep-18	21-Dec-18	0.04631
4,117,422	4,130,322	7-Sep-18	21-Dec-18	0.04610
4,130,323	4,142,976	10-Sep-18	21-Dec-18	0.04565
4,142,977	4,156,009	13-Sep-18	21-Dec-18	0.04572
4,156,010	4,168,922	16-Sep-18	21-Dec-18	0.04575
4,168,923	4,182,673	19-Sep-18	21-Dec-18	0.04576
4,182,674	4,196,951	22-Sep-18	21-Dec-18	0.04578
4,196,952	4,209,778	25-Sep-18	21-Dec-18	0.04576
4,209,779	4,222,027	28-Sep-18	21-Dec-18	0.04575
4,222,028	4,234,070	1-Oct-18	21-Dec-18	0.04582
4,234,071	4,242,447	4-Oct-18	21-Dec-18	0.02861

Appendix B: Robustness Test

Table (B.1) Impacts of Regularization on Colombian Workers with the control Venezuelans Settlements 2005

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	-0.001	-0.009	0.001	0.002
	(0.001)	(0.007)	(0.001)	(0.001)
Adj.R-squared	0.058	0.020	0.044	0.200
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	0.000	-0.002	0.000	-0.001**
	(0.000)	(0.002)	(0.000)	(0.000)
Adj.R-squared	0.058	0.020	0.044	0.200
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	0.002	-0.021	0.002	-0.006**
	(0.005)	(0.023)	(0.003)	(0.003)
Adj.R-squared	0.058	0.020	0.044	0.200
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.094***	0.094***	0.108***	0.114***
	(0.016)	(0.016)	(0.017)	(0.017)
F-Test	33.84	33.84	41.51	45.28
Observations (All Panels)	373,460	373,460	737,690	1,230,548
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Pre-Settlementts 2005	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (B.2) Impacts of Regularization on Venezuelan Workers with the control Venezuelans Settlements 2005

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	0.025	0.137	0.020***	-0.019***
	(0.017)	(0.211)	(0.007)	(0.006)
Adj.R-squared	0.123	0.067	0.059	0.216
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	0.003	0.045	0.004*	-0.005***
	(0.005)	(0.048)	(0.002)	(0.001)
Adj.R-squared	0.123	0.067	0.059	0.216
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	0.033	0.437	0.023*	-0.032***
	(0.050)	(0.462)	(0.013)	(0.009)
Adj.R-squared	0.123	0.063	0.059	0.216
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.104***	0.104***	0.157***	0.159***
	(0.019)	(0.019)	(0.014)	(0.014)
F-Test	30.38	30.38	122.6	128.75
Observations (All Panels)	1,890	1,890	18,425	25,413
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Pre-Settlementts 2005	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (B.3) Impacts of Regularization on Colombian Returnees Workers with the control Venezuelans Settlements 2005

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	0.003	0.116	0.014	-0.003
	(0.017)	(0.076)	(0.010)	(0.010)
Adj.R-squared	0.142	0.084	0.056	0.177
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	-0.004	0.046**	0.004*	0.004
	(0.005)	(0.023)	(0.002)	(0.003)
Adj.R-squared	0.143	0.085	0.056	0.177
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	-0.044	0.494*	0.028*	0.027
	(0.055)	(0.269)	(0.016)	(0.019)
Adj.R-squared	0.140	0.074	0.056	0.177
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.093***	0.093***	0.136***	0.139***
	(0.024)	(0.024)	(0.017)	(0.017)
F-Test	15.82	15.82	61.71	68.81
Observations (All Panels)	1,440	1,440	7,473	10,343
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Pre-Settlementts 2005	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (B.4) Impacts of Regularization on Colombian Workers with the control Distance to the Border

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	-0.001	-0.007	-0.000	0.001
	(0.002)	(0.008)	(0.001)	(0.001)
Adj.R-squared	0.058	0.020	0.045	0.199
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	0.000	-0.002	0.000	-0.001***
	(0.000)	(0.002)	(0.000)	(0.000)
Adj.R-squared	0.058	0.020	0.045	0.199
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	0.006	-0.027	0.000	-0.009***
	(0.006)	(0.026)	(0.003)	(0.003)
Adj.R-squared	0.058	0.020	0.045	0.199
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.086***	0.086***	0.099***	0.105***
	(0.017)	(0.017)	(0.018)	(0.018)
F-Test	24.68	24.68	29.84	33.12
Observations (All Panels)	368,426	368,426	730,116	1,219,328
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Pre-Settlementts 2005	Yes	Yes	Yes	Yes
Distance to the Borderl	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (B.5) Impacts of Regularization on Venezuelan Workers with the control Distance to the Border

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	0.026	0.163	0.021***	-0.019***
	(0.018)	(0.252)	(0.007)	(0.006)
Adj.R-squared	0.124	0.072	0.059	0.216
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	0.001	0.037	0.004*	-0.005***
	(0.005)	(0.050)	(0.002)	(0.001)
Adj.R-squared	0.124	0.072	0.058	0.216
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	0.013	0.415	0.026*	-0.033***
	(0.058)	(0.555)	(0.013)	(0.009)
Adj.R-squared	0.124	0.070	0.059	0.216
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.089***	0.089***	0.150***	0.152***
	(0.020)	(0.020)	(0.015)	(0.015)
F-Test	20.2	20.2	103.66	109.8
Observations (All Panels)	1,869	1,869	18,265	25,230
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Pre-Settlementts 2005	Yes	Yes	Yes	Yes
Distance to the Border	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (B.6) Impacts of Regularization on Colombian Returnees Workers with the control Distance to the Border

Var in logs*	**Hours Worked***	**Monthly Wages***	**Unemployed**	**Labor Force**
	(1)	(2)	(3)	(4)
Panel A: OLS				
Pep Holders x I(Post Aug. 2018)	-0.004	0.083	0.016	-0.008
	(0.019)	(0.086)	(0.010)	(0.011)
Adj.R-squared	0.143	0.084	0.056	0.177
Panel B: Reduced Form				
Reg. Days x I(Post Aug. 2018)	-0.004	0.043*	0.004*	0.003
	(0.005)	(0.023)	(0.002)	(0.003)
Adj.R-squared	0.143	0.086	0.056	0.177
Panel C: 2SLS (Second Stage)				
Pep Holders x I(Post Aug. 2018)	-0.047	0.554	0.031*	0.025
	(0.069)	(0.339)	(0.018)	(0.021)
Adj.R-squared	0.141	0.073	0.056	0.176
Panel D: First Stage				
Reg. Days x I(Post Aug. 2018)	0.077***	0.077***	0.126***	0.129***
	(0.026)	(0.026)	(0.018)	(0.018)
F-Test	9.72	9.72	47.82	54.22
Observations (All Panels)	1,422	1,422	7,432	10,291
Controls (All Panels)				
Dep. FE	Yes	Yes	Yes	Yes
Year-Monthly FE.	Yes	Yes	Yes	Yes
Pre-Settlementts 2005	Yes	Yes	Yes	Yes
Distance to the Border	Yes	Yes	Yes	Yes
Individual Covariates	Yes	Yes	Yes	Yes
Add. Controls	Yes	Yes	Yes	Yes

Notes: We are including the same controls of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Figure (B.1) Robustness Test: Exclude Border Departments (Colombian Sample)

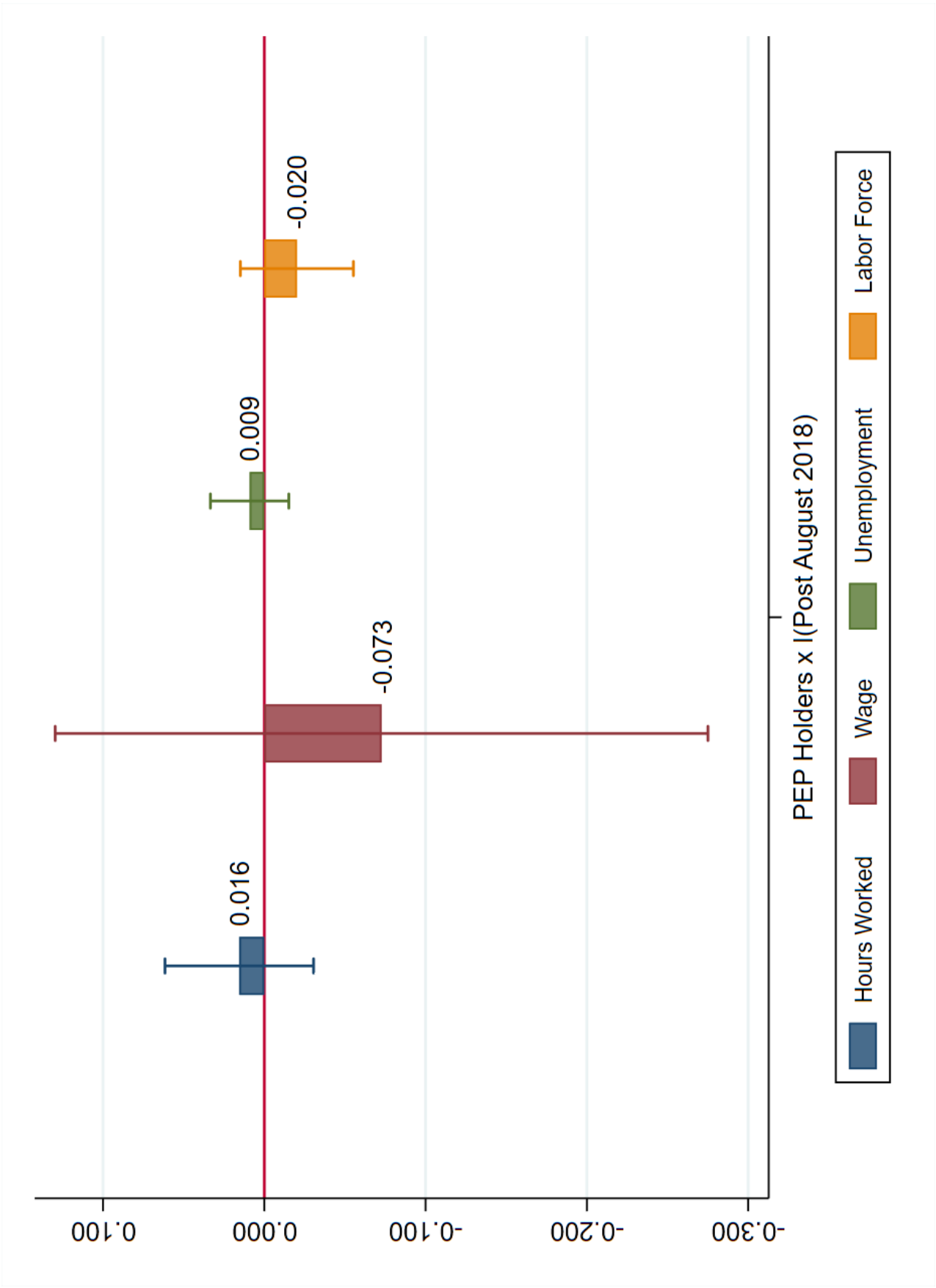


Figure (B.2) Robustness Test: Exclude Border Departments (Venezuelan Sample)

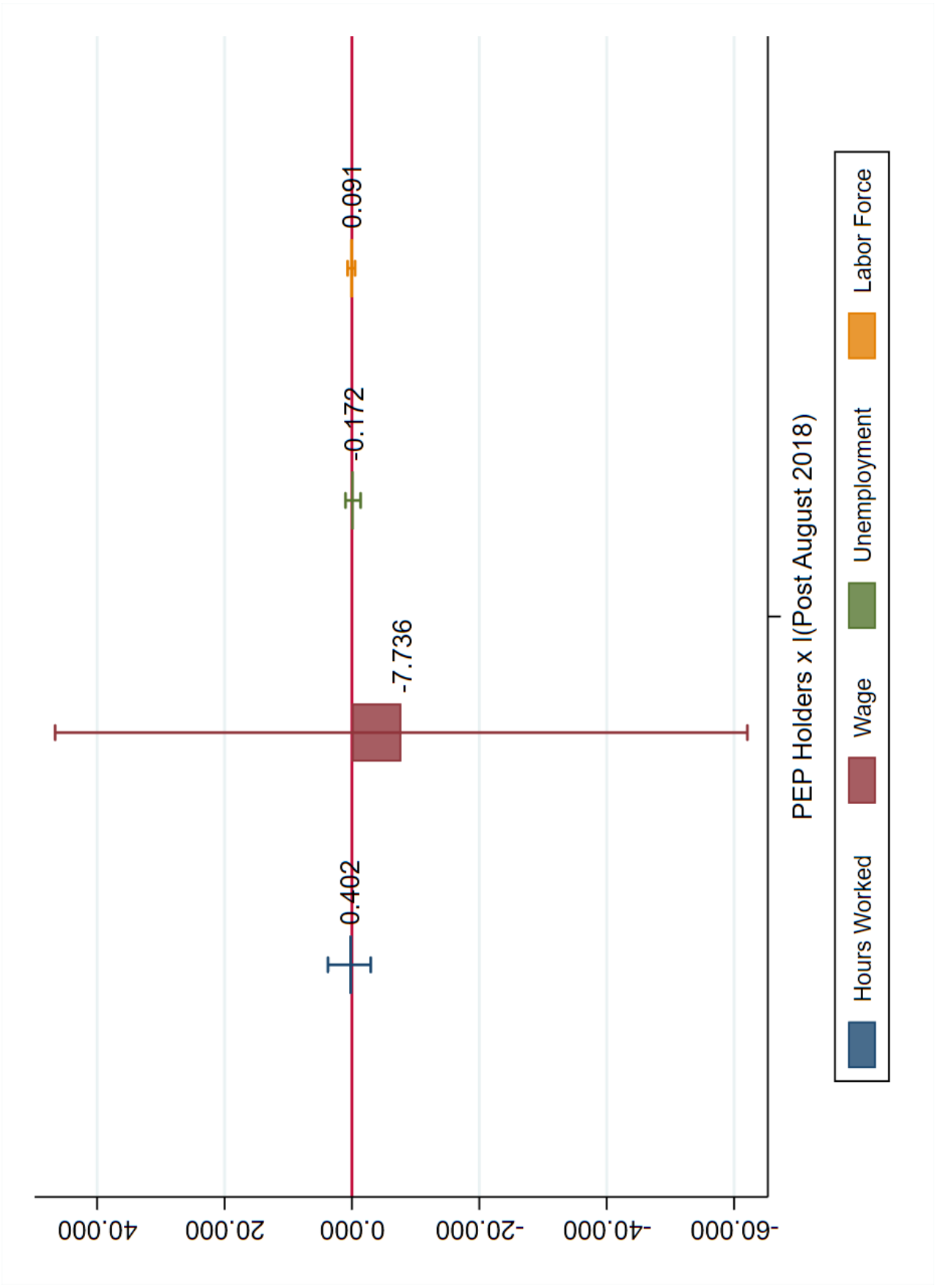


Figure (B.3) Robustness Test: Exclude Border Departments (Colombian Returnees Sample)

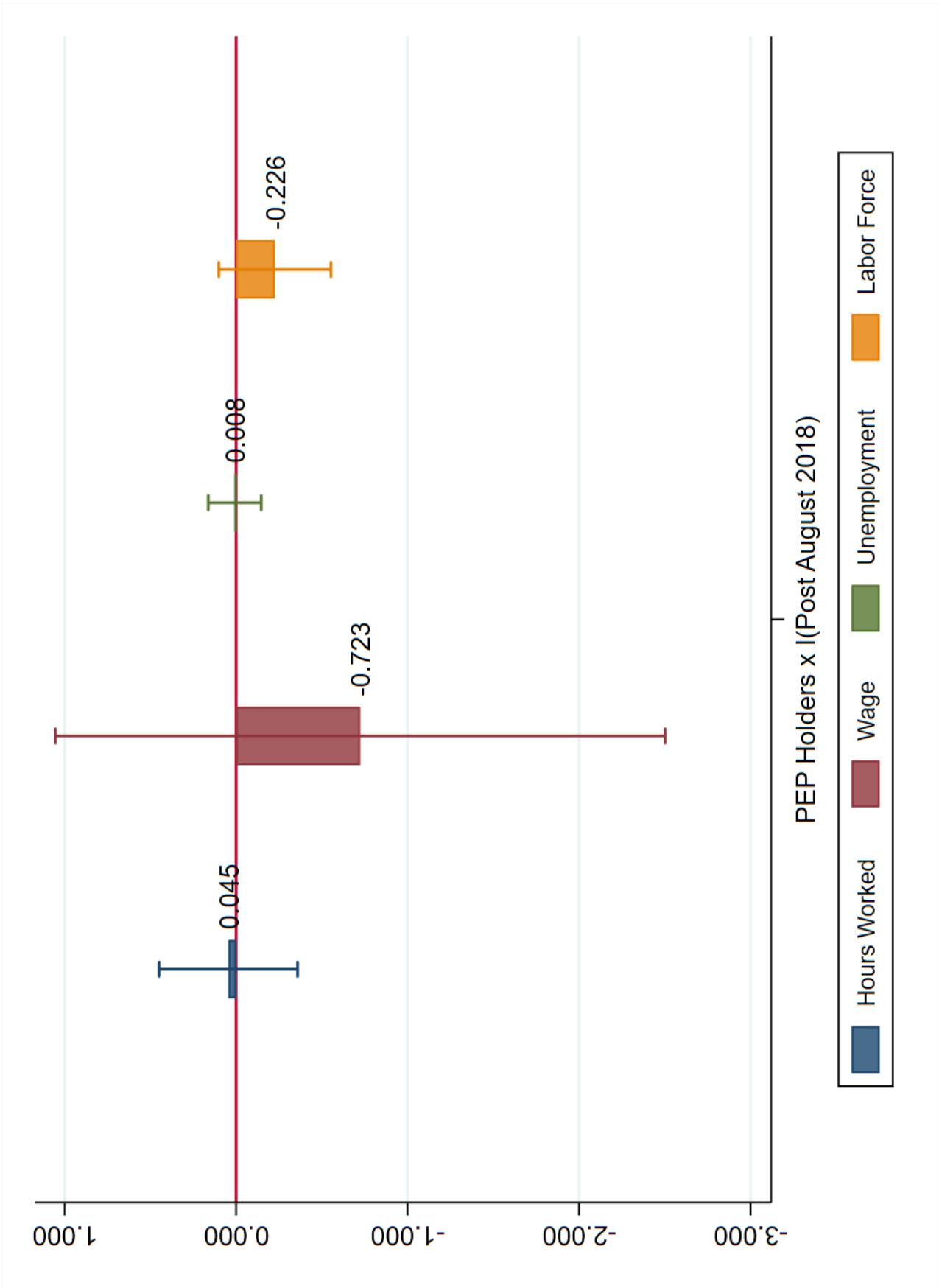
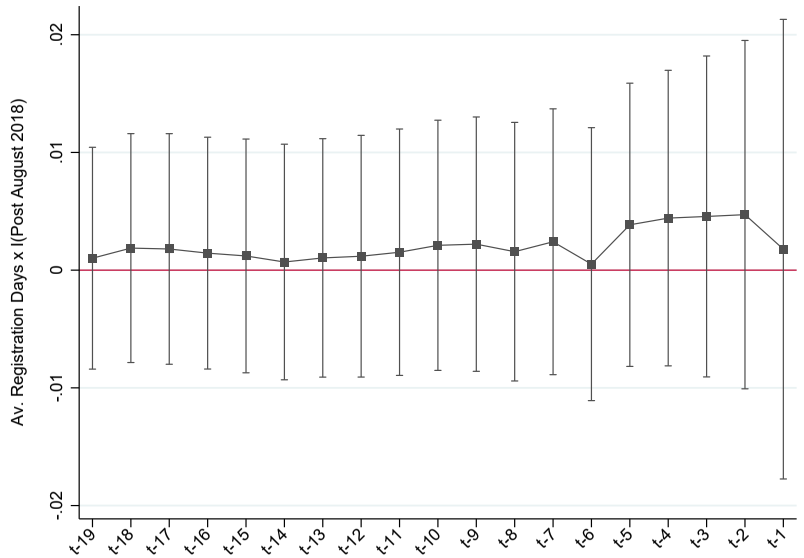
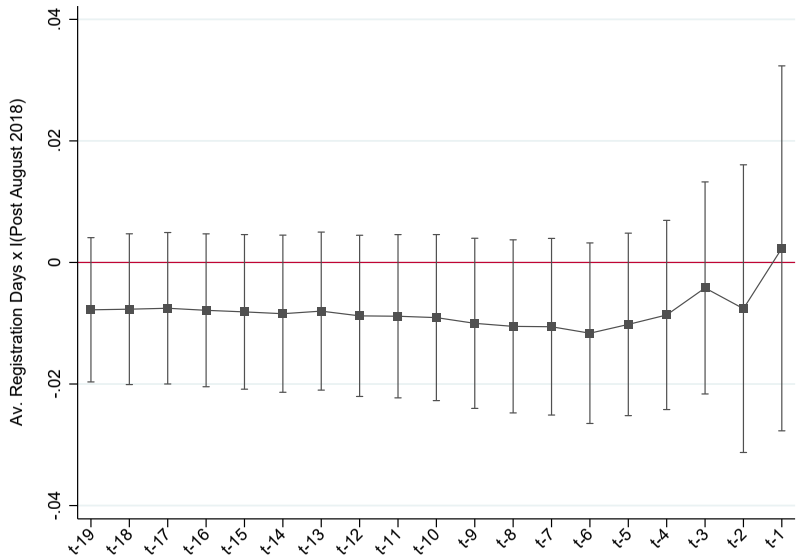


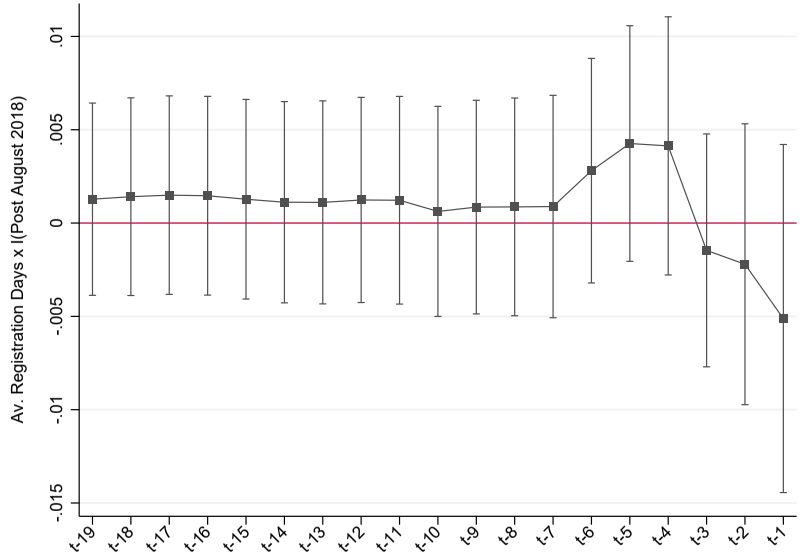
Figure (B.4) Robustness Test Different time periods before PEP - Colombian Workers



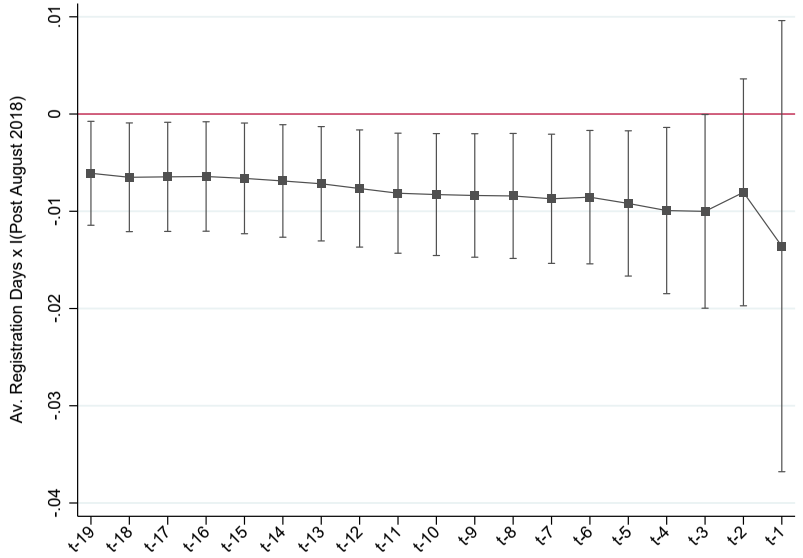
(a) Weekly Hours Worked



(b) Monthly Wages

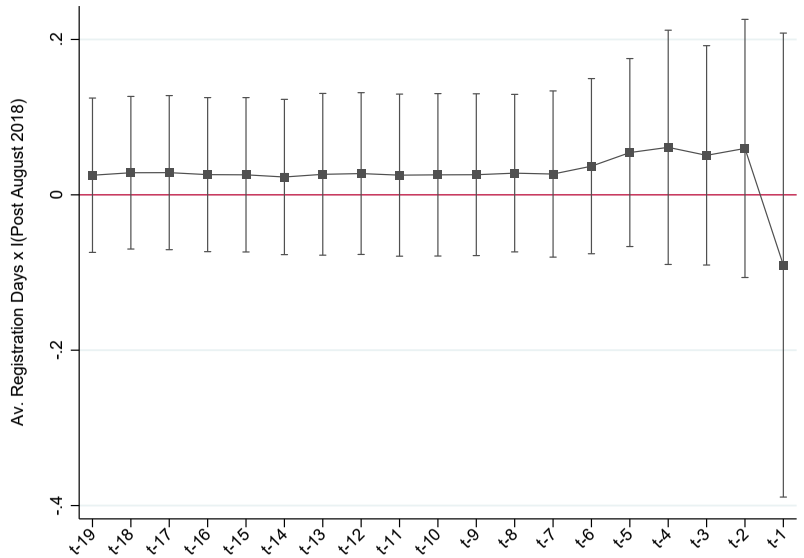


(c) Unemployed

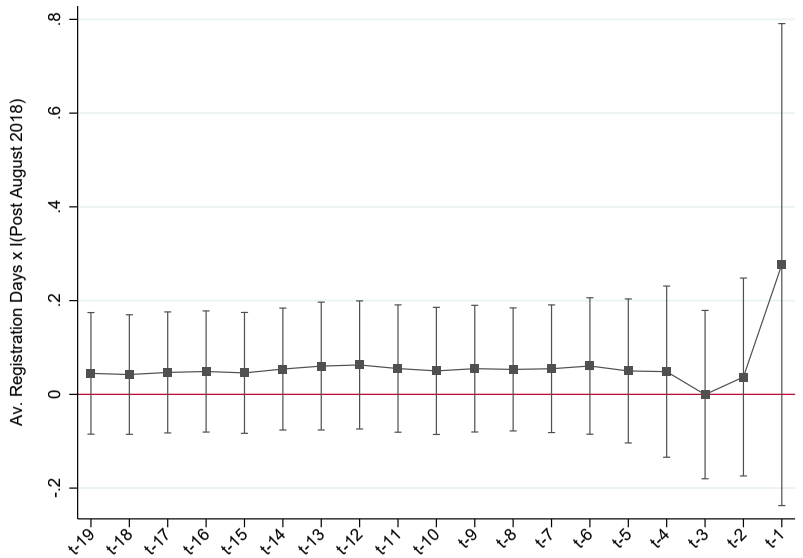


(d) Labor Force

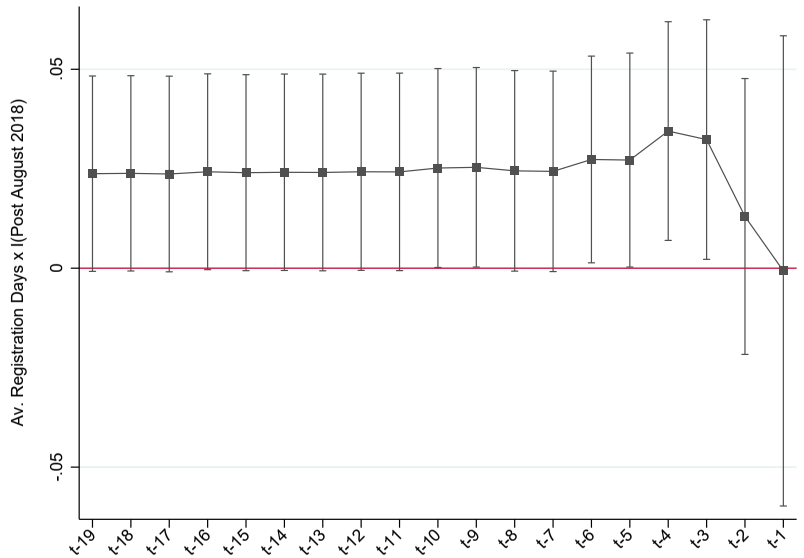
Figure (B.5) Robustness Test Different time periods before PEP - Venezuelan Workers



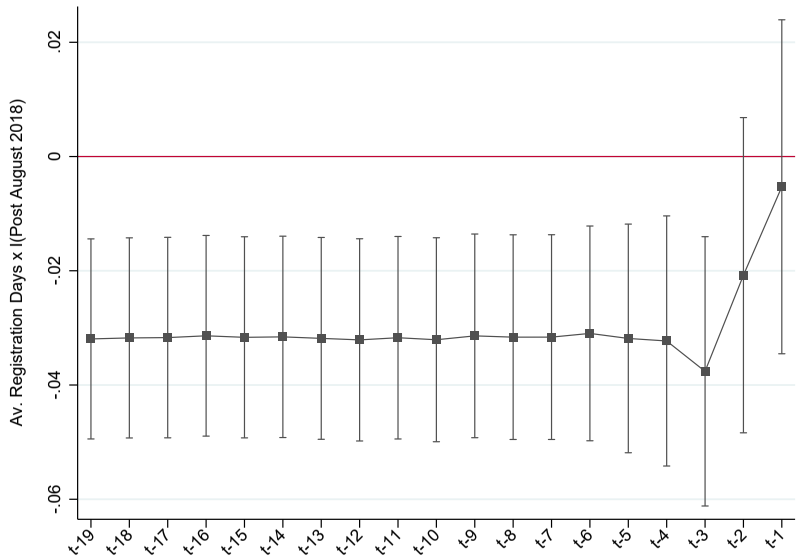
(a) Weekly Hours Worked



(b) Monthly Wages

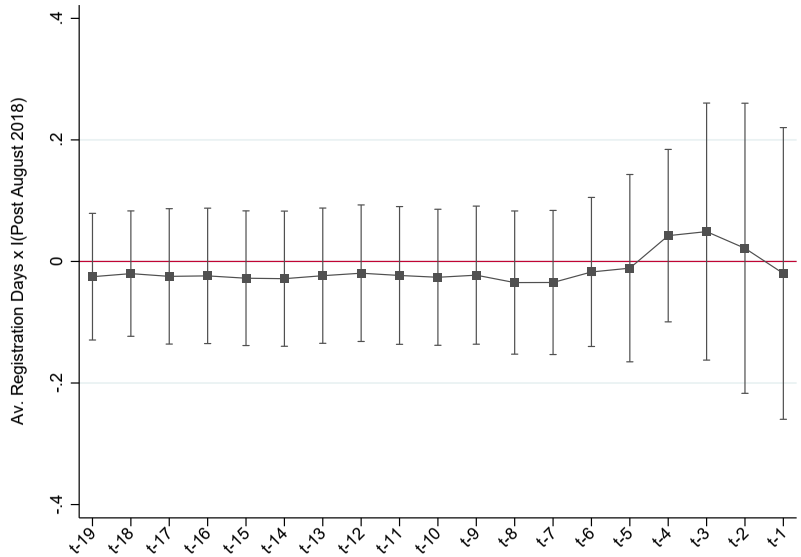


(c) Unemployed

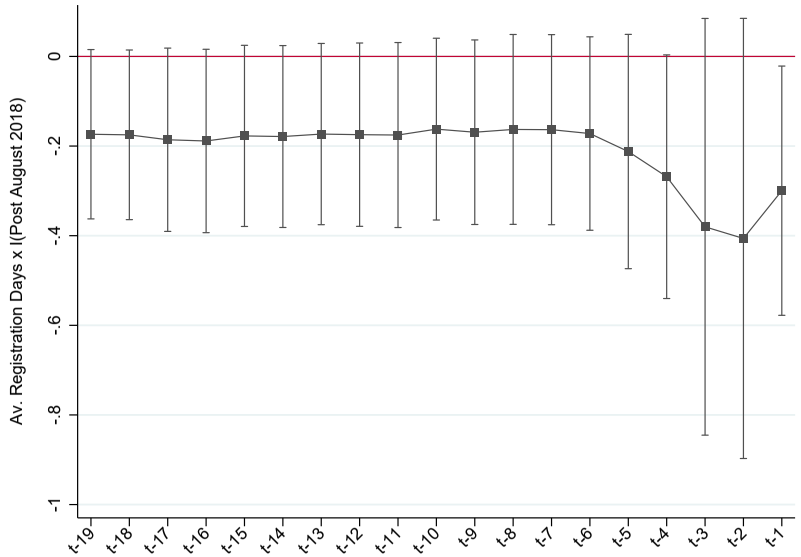


(d) Labor Force

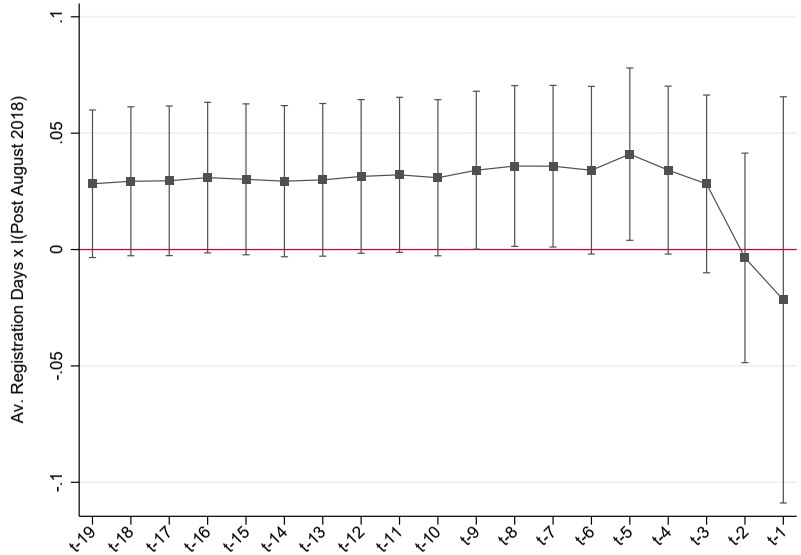
Figure (B.6) Robustness Test Different time periods before PEP - Colombian Returnees Workers



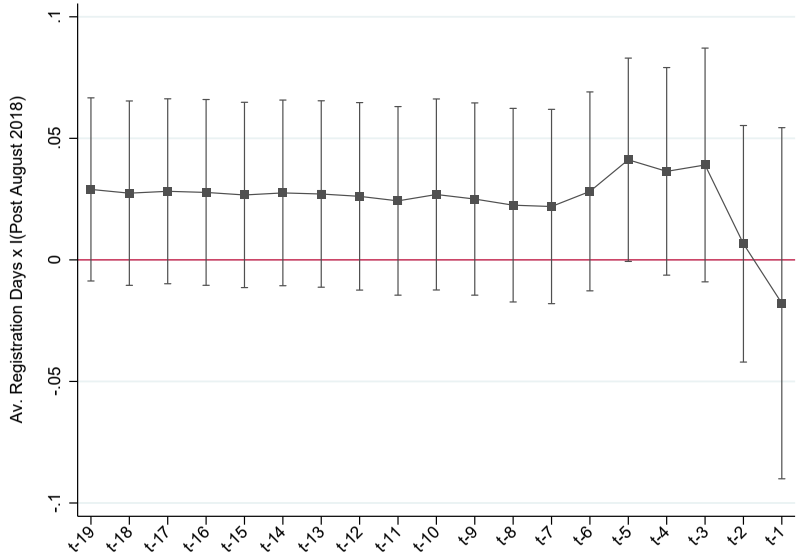
(a) Weekly Hours Worked



(b) Monthly Wages



(c) Unemployed



(d) Labor Force