

DISCUSSION PAPER SERIES

IZA DP No. 13743

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for Undocumented Refugees**

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ABSTRACT

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

Between 2014 and 2020 over 1.8 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 short- to medium-term 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 roll-out. We are only able to distinguish negative albeit negligible effects of the program on the formal employment of Colombian workers. These effects are predominantly concentrated in highly educated and in female workers.

JEL Classification: F22, O15, R23

Keywords: migration, work permit, labor markets, amnesties

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I Introduction

What are the implications of granting working rights to forced migrants in their hosting economies? Despite the alarming size of forced migrant flows, which reached approximately 80 million people in 2019,¹ 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. The United Nations estimates that up until 2020 about 5.2 million Venezuelans have left the country, with about 1.8 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 through irregular or unofficial crossing points.²

Between April and June 2018, the Colombian government ran a nationwide census to register all undocumented Venezuelan migrants residing in Colombia. The census registered about 442,000 Venezuelans.³ The initial purpose of the census was to identify and collect basic demographic information on the undocumented immigrants residing in Colombia; importantly, it was not originally 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 im-

¹See [UNHCR \(2018\)](#).

²Many Venezuelan immigrants choose to cross over to Colombia through an irregular crossing point simply because they do not have a national identification or a valid passport given the prohibitively large costs of obtaining one in Venezuela due to the collapse of public services.

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

migrants who had registered in the survey would be eligible for a formal temporary migratory status. This 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, employment, and labor force participation on three samples of workers: Colombian natives, Venezuelan migrants, and Colombian workers who returned in the last 5 years to Colombia. We examine the impacts of the PEP program in workers in both the informal and formal sectors. 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 February 2020.

To deal with endogeneity concerns regarding the self-selection of undocumented migrants into certain geographic areas, we exploit the fact that each undocumented migrant who registered in the nationwide government survey was granted a registry number that was exogenously allocated at the time of registration. Based on this number, individuals were exogenously allocated to one of 22 time windows for applying for the PEP visa online. According to government officials, scattering the applications across several months was done to avoid overloading public servants in charge of processing the amnesty. Using the exogenous assignment of time windows, we estimate an average time window available to register in the PEP program for the immigrants residing in each 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 resulted in a disproportionately high number of PEP holders. As part of our

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

analysis, we test whether the average registration days are correlated with observable covariates before the onset of the program and find no statistically significant coefficients for any of the variables, consistently with the claim that the numbers were exogenously allocated (see Tables B.1 and B.2 in the Online Appendix).

We also estimate event studies examining the evolution of quarter-by-quarter labor outcomes in departments that had different average time windows to register in the PEP program. The event studies support the validity of our identification strategy, as we observe parallel trends before the program implementation in the vast majority of outcomes that we examine.

We do not find any large or significant effects of the PEP program on any of the outcomes that we study, except for the case of formal employment. Particularly, we observe that a twofold increase in PEP holders (per 100,000 inhabitants) results in a drop in formal employment of 0.1 percentage points. These correspond to small effects considering that a twofold increase in PEP holders per 100,000 inhabitants is about 1.3 standard deviations and that average employment rate in the formal sector is 65 percent for Colombian workers. Consequently, our estimates on the impacts of the PEP program corresponds to 0.15 percent deviation from the mean of formal employment for Colombian workers.

These effects are predominantly concentrated on the employment rates of workers who are highly educated (e.g., having at least a high-school degree) as well as female workers. The disproportionate effects of the PEP program on educated workers may be explained by the fact that Venezuelan workers are, on average, more educated than Colombian workers. The disproportionate impacts of PEP on female Colombian workers may be associated to the discrimination against women in the workplace.

Interestingly, we do not observe any significant effects of the program on the hours worked, wages, or labor force participation of Colombian workers on the formal or informal sectors; nor do we observe changes on the employment rate of Colombian workers in the informal sector.

In line with the effects documented for formal Colombian workers, we observe a positive effect

of the PEP program on the formal employment rate of Venezuelan workers. These effects, however, are not statistically significant once we correct our inference for the small number of clusters we have in our data.

How is it possible that the regularization of approximately half a million migrants has only induced negligible effects on the formal employment of native workers? 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.

However, there are several explanations for our results. The first one is that the composition of the labor force supply remained unchanged after the roll-out 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.

A second 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 negligible size of the effect we document are in line with other studies

on the effects of inflows of migrants on labor market outcomes.⁵

Finally, while it is also possible that the time horizon under consideration is too short to observe any large impact on the studied outcomes, we believe that 14 months after full implementation of the program is a reasonable period to observe the main impacts of the program.

Our results are robust to a battery of tests. First, all our estimates include department, month, and 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 departmental variables. We find that our measure of average registration days for a PEP is indeed correlated with distance to the Colombia-Venezuela border. Hence, we show that our results are robust to the inclusion of interactions of year dummies and the distance between each department and the closest point of 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 in developed economies (see [Borjas and Monras, 2017](#); [Peri and Yasenov, 2018](#) for notable examples).⁸ Although historically there has been con-

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

⁶These 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.

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

⁸Other studies that explore the impacts of general immigration on labor markets include [Card \(2001\)](#), [Borjas \(2003\)](#), [Card \(2005\)](#), [Ottaviano and Peri \(2012\)](#), [Dustmann et al. \(2016\)](#), [Foged and Peri \(2016\)](#), and [Dustmann et al.](#)

flicting conclusions about the effects of noted refugee waves such as the Mariel Boatlift in Miami, recent papers reinforce the consensus that the impact of immigration on average native-born workers is small, and fails to substantiate claims of large detrimental impacts on workers with less than high school (Clemens and Hunt, 2017).

Second, our work contributes to the studies that explore the impacts of migrant amnesty in hosting countries (see Kossoudji and Cobb-Clark, 2002; Bratsberg et al., 2002; Orrenius and Zavadny, 2003; Kaushal, 2006; Amuedo-Dorantes et al., 2007 for examples). Most of these studies focus on examining the impacts of large-scale amnesties on the legalized migrants in developed countries. Kossoudji and Cobb-Clark (2002), for example, document that the legalization of 1.7 million unauthorized workers in the United States was reflected on higher wages for the legalized migrants. Similar results are presented by Bratsberg et al. (2002) when analyzing the impacts of migrant's naturalization and Kaushal (2006), who examine the impacts of the 1997 Nicaraguan Adjustment and Central American Relief Act. Yet, many other studies examine the impacts of inflows of forced migrants in the native's labor outcomes (see Maystadt and Verwimp, 2014; Tumen, 2016; Ruiz and Vargas-Silva, 2016; Alix-Garcia et al., 2018; Fallah et al., 2019; Altındağ et al., 2020). Our study falls in between these two groups by focusing on the impacts of a large amnesty on natives workers, while focusing on a developing country with a large informal labor market.⁹

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 gov-

(2017).

⁹Our paper also contributes to a number of other recent studies that have looked at the effects of the overall inflow of Venezuelan immigrants in Colombia finding negative, yet, small effects on labor market outcomes (e.g., Caruso et al., 2019; Pacheco, 2019; Bonilla-Mejía et al., 2020). The context of the natural experiment our paper exploits deals with endogeneity concerns that could still be present in these other broader studies, namely disentangling between the effect on labor markets created by the migration flows from the effect that follows the collapse of the Venezuelan economy, Colombia's historical main trading partner. Other studies of labor impacts of the Venezuelan migration crisis include Morales and Pierola (2020) who focus on the case of Peru.

ernment 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 Venezuelan Crisis

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 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 procyclical 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 left their country. According to data from the United Nations Refugee Agency (UNHCR), as of 2020, over 5.2 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 2020, 1.8 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 instrument 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 prohibitively costly to get for most Venezuelans due to the collapse of government services and

rampant corruption).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 census between April and June of 2018. This census, 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 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

¹⁰Another possibility is that some immigrants abused a permit issued by the Colombian known as *Tarjeta de Movilidad Fronteriza* designed to facilitate short-term 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.

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

in Colombia in 2018 who registered in the census. We present the main descriptive statistics in Table I. According to our analysis, the undocumented migrants in the RAMV are equally split in terms 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 0.57 children. Sixty-one percent are single-parent households, which suggests that many families strategically separated and a number of members remained in Venezuela.

The data also allow us to make six general observations of the undocumented migrant population living in Colombia 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. The average years of education for the whole sample is 10.5.
2. *RAMV migrants are facing tight labor conditions and work predominantly in services.* One 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. Thirty six percent of the RAMV sample works in services and sales.
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.

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.
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.
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.

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, a mean targeted test instrument to identify eligible beneficiaries to 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

¹²Ever 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.

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 number exogenously allocated to each migrant at the time of registration. 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 II plots the distribution of RAMV-registered migrants assigned to each of the 22 available time windows (the specific share of the RAMV-registered 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 present the main descriptive statistics for the population that applied and did not apply for the PEP program in Table I (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. Because the registration in PEP provides access to the subsidized health regime, families with children have an additional incentive for registering.

II.3 Selection on the PEP beneficiaries

The RAMV and PEP populations are self-selected. First, the RAMV covered undocumented migrants that are presumably more vulnerable than legal migrants. Second, although the government designed the RAMV to cover all undocumented migrants, some features of the collection process might have left out a group of migrants. Third, the registration process for PEP is voluntary. The decision of eligible migrants to register depends on the constraints to registration or the expected net benefits from registration.

In order to register in the RAMV, migrants had to show proof of Venezuelan citizenship. Owing to the collapse of the Venezuelan State, many Venezuelan citizens lack official identification documents. In fact, many crossed through an unofficial border crossing because they lacked the valid documents to enter Colombia through an official checkpoint. Since the RAMV covered only documented population, it is likely that in the absence of a valid proof of citizenship a large fraction of Venezuelan immigrants were unable to register.

The official registration points for the RAMV were not widely spread across the Colombian territory. These points were located in border municipalities, municipalities with a large population of Venezuelan migrants, and municipalities in which local boards specifically requested it. As a result, 35 percent of the Colombian municipalities had a registration point (see Figure I). The National Government did a massive information campaign and relied on organizations of Venezuelan

migrants to reach the largest number of migrants. However, it is safe to assume that many were left out and those left out are probably the less connected, less informed and most vulnerable persons. Networks and word of mouth are important determinants of registration in the RAMV by providing information of State programs initiatives, their benefits, and the actual procedure to register.

Although all persons in RAMV were eligible for PEP, people had to enroll online. Constraints, such as no access to internet, perceived net benefits from enrolling, and high mobility of the migrant population might have determined the decision to register in PEP. Also, lack of information about the process and the benefits of enrolling may hinder the registration process.¹³

Considering the aforementioned selection issues, our analysis is only valid to the specific group of immigrants sample in the RAMV who applied for the PEP.

III Data

We use departmental-monthly data from January 2017 to February 2020 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, employment, and labor force participation on Colombian workers. We examine the impacts of the PEP program on three samples of workers: (i) Colombian, (ii) Venezuelan, and (iii) Colombians who returned from Venezuela in the last five years. We test the effects on (ii) and (iii) since it is possible that the PEP program may have prompted different effects on other Venezuelan migrants or Colombian returnees from Venezuela, who probably have weaker ties to the Colombian labor markets relative to the rest of Colombian workers.¹⁴

¹³For example, each person was randomly allocated to a specific time period to register. This time window lasted two months. If not properly informed, migrants may not register timely and lose the benefits of PEP.

¹⁴Colombians migrated to Venezuela when the internal conflict in their country intensified during the 1990s. There-

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.¹⁵ Descriptive statistics for our sample are presented in Table II. Relative to Colombian workers, Venezuelan migrants and Colombian returnees tend to work for longer weekly hours for lower wages. They also have lower employment 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 assess the robustness of our estimates including night light density, conflict-related variables, homicide rates, 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 of the control variables are presented in Appendix A.

fore, there was a significant Colombian diaspora in Venezuela, which by now also include second generation migrants. Some of these Colombians returned during the current Venezuelan crisis. For identifying Colombian returned migrants in the sample of the GEIH, they should have arrived to Colombia in the last 5 years prior to the survey.

¹⁵Legal working age begins at 10 years in Colombian rural areas, whereas it is 12 years in urban areas.

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 areas that are more prosperous and less violent. As such, a simple mean comparison of areas with different sizes of PEP holders may be biased.

Moreover, migrant's decisions are dynamic, and hence, concerns related to the endogenous choices of migrant's location into specific areas are not solved by merely adding fixed effects by geographic location or time. Fixed effects by area account for all the time-invariant characteristics of geographic units; and time fixed effects account for all the aggregate trends that are consistent across the country. Yet, only including area and time fixed effects ignores the dynamic behavior of migrants. To account for that dynamic behavior, we need to instrument the treatment of the PEP program with a variable that has geographic and monthly variation. Our identification strategy aims to correct for these biases by using a 2SLS estimation.

Considering that the timing of the PEP registration 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 to each undocumented immigrant to request a PEP was exogenous to the immigrant and depended on the form number in the individual's RAMV registration. As mentioned above, government officials report that this scattered approach was done to distribute the registration load on public officials evenly across time.

Consequently, we estimate the following 2SLS difference-in-difference specification¹⁶

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

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

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, employment, and labor force participation), PEP corresponds to the logarithm of PEP holders per 100,000 inhabitants,¹⁷ and $I(\text{Post August 2018})$ 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 roll-out 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 2005,¹⁸ (ii) percentage of households

¹⁶Applying instrumental variables in a difference-in-difference setting is common in the economics literature. As a pioneering example of combining these methods see the seminal study by [Duflo \(2001\)](#).

¹⁷Specifically, it corresponds to the number of PEP holders (between 10 to 64 years) divided by the population between 10 and 64 years multiplied by 100,000.

¹⁸The variables available in 2005 were calculated with the population census of 2005, the last census available before the PEP program began.

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 and monthly fixed effects.

Finally, standard errors are clustered at the department level to account for geographic serial correlation. Considering that we have a small number of clusters, we correct our statistical inference using cluster-robust wild-bootstrap p-values.¹⁹

Note that our main specification is estimated using monthly data from January 2017 to February 2020. 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). Based on the individual time window available to request a PEP, we estimate the average registration days (*Reg. Days* in equation 2) by

¹⁹See [Cameron et al. \(2008\)](#) for details on the issues caused by a small number of clusters and [Roodman et al. \(2019\)](#) for corrections. 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](#)).

department as

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

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 different average number of days available for requesting a PEP online, before and after the program implementation in August 2018, based on the registration number given to each one of the undocumented immigrants residing in each department.

The distribution of the available registration days to request a PEP for each migrant in the RAMV is presented in Figure II. The figure suggests that there is significant variation in the number of days available for registration for the sample of individuals registered in the RAMV census. Figures III and IV show that the average number of registration days has strong geographic and time variation. Figure III presents the daily flow and total stock of undocumented migrants that registered for the PEP between August and December of 2018. The figure confirms that there is positive growth on the number of individuals who registered for the PEP across the complete window of time open for registration. Figure IV shows the distribution on the number of individuals registered for the PEP across time and department. Figure V confirms that a positive and strong correlation exists between the average registration days and the share of regularized migrant population (i.e., PEP holders) in each department. Moreover, the figure also illustrates the departmental variation in the instrument. Figure B.1, of Appendix B also illustrates that there is a positive correlation between the average registration days and the number of individuals who registered for the PEP in each department.

Considering the plausibility of our exclusion restriction, we formally 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 a large battery of department covariates observed before the program onset in Appendix B (see Tables B.1 and B.2). These controls include variables related to population, labor markets, geographic characteristics, economic growth, social disparities, violence and conflict, governmental presence, early settlements of Venezuelan refugees in 2005, and total foreign population observed at the beginning of 2018. None of the coefficients is statistically significant, which confirms that the cross-regional variation of average registration days is not explained by department level observable characteristics at baseline. This is consistent with the fact that the registration windows are exogenous.

IV.2 Event study

We complement our baseline empirical analysis by estimating an event study using the following (reduced-form) specification:

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

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

The estimated coefficient of interest α , as defined in equation 1, are presented in Tables III through V. The table presents the estimates of the OLS (Panel A), reduced form (Panel B), 2SLS (Panel C) and first-stage (Panel D). Columns (1) to (3) estimate the effects of the program in the formal sector and columns (4) to (6) present our estimates for the effects of the program in the informal sector. Informality is an indicator variable that takes the value of one for all the workers that do not contribute monetarily to the health system. Note that, in all of our estimates, dependent variables that are continuous are transformed using a logarithm (such as hours worked and monthly wages) whereas dependent variables that are defined as binary variables (such as being employed and participating in the labor force) are not transformed in the estimations.

Our first main observation is that our instrument is strong as there is a statistically significant and positive correlation between the average registration days (*Reg. Days*) and the share of PEP holders in each department, as noted in Panel D of the three tables.

Our second main result is that, for the most part, we do not distinguish significant or large effects of the PEP implementation in the outcomes we examine, except for the formal employment of Colombian workers.

In particular, our results suggest that a twofold increase in PEP holders (per 100,000 inhabitants) results in a drop of 0.1 percentage points in formal employment for Colombian workers (or a reduction of 0.01 on the employment indicator variable). Note, however, that these correspond to negligible effects when considering its economic significance. First, a twofold increase of our variable of interest is quite an unusually large number as it represents about 1.3 standard deviations of our sample (e.g., since our sample mean is about 790 PEP holders per 100,000, a twofold increase would imply 1580 PEP holders per 100,000 inhabitants, and the standard deviation is 1,155). Second, the average employment rate of Colombian workers in the formal sector in our sample is 65 percent. Hence, the estimated effect correspond to a change of 0.15% on the formal employment mean.

Beyond employment, importantly, we do not observe any other significant effects of the PEP amnesty program in any of the other outcomes we study for the sample of Colombian workers.

Table IV presents results of outcomes for other Venezuelan immigrants to Colombia. Here, we have another result worth mentioning. As expected, we observe a positive and significant effect of the program on the formal employment of Venezuelan migrants, implying that the PEP program is indeed resulting in Venezuelan workers joining the formal labor market. Although the effects are statistically significant when standard errors are clustered by department (shown in parenthesis), we are not able to reject the null hypothesis that this point estimate is different from zero when correcting our statistical inference for the small number of clusters in our data (see the cluster-robust wild-bootstrap p-values in brackets).

Table V presents results exploring outcomes of Colombian returnees where we cannot distinguish any effects.

V.1 Event study

The estimates for the event study described by equation 4 are presented in Figures VI and VII for the sample of Colombian workers. 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 the vast majority of outcomes. The figures also confirm the results reported earlier. We do not distinguish any significant effects of the program in any outcomes except for the employment rate in the formal sector and monthly wages in the informal sector. The employment rate exhibits a positive trend beginning in the first quarter of 2019. Given the large standard errors, the coefficient estimates after PEP are not statistically different from those before PEP (the confidence intervals overlap). The first quarter of 2020 shows a sharp reduction. Monthly wages for the informal sector drop the second quarter after PEP started.

Although the coefficient estimates are noisy, for some quarters the effect is statistically significant.

We only present the event study for the sample of Colombian workers due to space concerns. Yet, the estimates of the impacts of the PEP program for the other two samples of workers are not statistically different from zero in any quarter and are available upon request.

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), age (for individuals with more and less than 33 years), economic sector,²⁰ and firm size. For this purpose, we split the sample according to the workers belonging to the different categories in each case.²¹ The results of this analysis are summarized and presented in Figures VIII and IX.²² We only carry out this analysis for the sample of Colombian workers as we only observe a significant effect of the program for that group of workers.

Across all these different categories, we are only able to distinguish a negative yet negligible effect of the program in the employment rate on the formal sector for highly educated workers as well as of female workers.

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 continuous

²⁰We split workers working in high vs. low-skill sectors. High-skill sectors are real estate and services, whereas low-skill sectors include agriculture, manufacturing, transportation, communication, and construction.

²¹Note that, since economic sector and firm size is observed only for employed individuals, we do not estimate our specifications using employment as the dependent variable for the different categories of economic sector and firm size.

²²The corresponding tables are available in Appendix D.

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 \(2019\)](#) there is a high and strong correlation between early settlements of Venezuelan migrants and the Venezuelan migration observed after 2005. We present the results of this exercise in Table [VI](#). Our results are robust to the inclusion of these controls.

Controlling for distance to the border

In a separate exercise we test for the correlation between the average registration days and the distance to Venezuela. 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.²³ 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 Table [VII](#). Our results are robust to the inclusion of these controls.

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

²³Our 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.

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 C (Figures C.1 and C.2) and confirm the validity of our results.

Controlling for the previous waves of the PEP

Considering that two previous PEP waves ²⁴ 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.

Sensitivity of coefficients estimates to departmental controls

Our main estimates include a large variety of department-level controls. To assess the sensitivity of our analysis to these controls we replicate the estimates of Table III excluding all the controls. The results of this exercise are presented in the Appendix C, Table C.1 and confirm the validity of our estimates.

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 C (Figure C.3), and they confirm that our findings are not sensitive to choosing a different period for the estimation. Naturally, as the sample uses fewer months

²⁴The process to grant the first two waves of PEP were different. PEP was granted to Venezuelan migrants with legal visas. These migrants migrated legally to the country and were in much less vulnerable conditions than the ones we are studying in this paper

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 large effects in the Colombian labor markets (other to the negative but small effects observed in formal employment). 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. Even if these individuals apply for a PEP visa, 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 VIII.²⁵ The estimates confirm that formal workers earn almost double the

²⁵The regression includes all the controls included in our main estimates. Standard errors were clustered at the department level.

wages received by informal workers in the Colombian labor markets, which suggests that a large premium is associated with being a formal worker. Therefore, 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.

A second 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.

A third explanation is that –even if important composition changes are occurring in the labor markets as immigrants move to the formal sector– the overall effects we measure in the short- to medium-term are such that the negative effects on labor markets one would expect following a sudden increase in the labor supply are offset by an increase on aggregate demand due to the same inflow or, alternatively, are not there given high complementarities between immigrants and the the local labor force. In such case, Venezuelans and Colombians would not be competing in the same labor pool.

A possible additional explanation is that, simply put, we do not observe any impacts of the program because the length of our data in terms of time. However, while this is a possibility, our data includes observations for 18 months after the program was announced in August of 2018, and 14 months after all undocumented immigrants formally received their PEP visa. Thus, even if labor market adjustments take time, we believe that if there were to be tangible effects of this program we would have been able to distinguish some of them in the period of time we analyze.

All in all, 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](#)).

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 hours worked, wages, and labor force in the short to medium run (14 months after the program was fully implemented). The only exception is formal employment, for which we find negative negligible effects of the program, mostly for Colombian workers who are highly educated and female.

Why do we observe limited effects of the program? 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. Moreover, workers see a large premium when they join the formal sector for which migrants should be interested in joining the formal sector. The remaining explanations include: hesitance from firms to hire migrants, a rise in aggregate demand due to the newly arrived immigrants that offset any possible negative effects created by a boost in labor supply, or the fact that we only observe limited impacts due to the 14 months time horizon we explore in our data after the program was implemented. Future research should update our analysis as more data becomes available to study the long term impacts of the program.

Another interesting avenue for future research, as more data on population dynamic becomes available, is to examine the impacts of the PEP program on native out-migration. It is possible that the large regularization of migrants may be prompting natives to leave regions with a high share of migrants.

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. For instance, policies to inform both migrants about the benefits of working in the formal sector as well as incentives for firms to hire migrants. These and other examples might be a crucial complement to the decision of governments to grant the right to work.

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Table (I) Characterizing Migrants Registered in the RAMV Census

| | RAMV sample | Obs | Without PEP | With PEP | Mean Diff |
|-----------------------------------|--------------------|---------|--------------------|--------------------|-----------|
| PEP holder [=1] | 0.638 (0.481) | 441,237 | - | - | - |
| Female [=1] | 0.497 (0.500) | 440,904 | 0.499 (0.500) | 0.496 (0.500) | 0.003 |
| Age | 25.889 (14.570) | 439,491 | 23.944 (15.126) | 26.982 (14.131) | -3.039*** |
| Married or cohabiting [=1] | 0.347 (0.476) | 441,237 | 0.293 (0.455) | 0.377 (0.485) | -0.084*** |
| Household Head [=1] | 0.524 (0.499) | 441,237 | 0.484 (0.500) | 0.546 (0.498) | -0.062*** |
| Years of education | 10.481 (5.409) | 441,234 | 9.435 (5.662) | 11.075 (5.166) | -1.640*** |
| Labor force [=1] | 0.626 (0.484) | 322,716 | 0.596 (0.491) | 0.640 (0.480) | -0.044*** |
| Informal worker [=1] | 0.320 (0.466) | 322,716 | 0.278 (0.448) | 0.340 (0.474) | -0.063*** |
| Unemployed [=1] | 0.244 (0.430) | 322,716 | 0.257 (0.437) | 0.238 (0.426) | 0.019*** |
| Family in Colombia [=1] | 0.435 (0.496) | 441,209 | 0.473 (0.499) | 0.413 (0.492) | 0.060*** |
| Family in Venezuela [=1] | 0.661 (0.473) | 441,209 | 0.652 (0.476) | 0.666 (0.472) | -0.015*** |
| Size of family in Venezuela | 2.556 (2.723) | 434,397 | 2.510 (2.728) | 2.582 (2.720) | -0.072*** |
| Network, migrants from same munip | 0.102 (0.148) | 441,237 | 0.128 (0.175) | 0.087 (0.129) | 0.040*** |
| Parents [=1] | 0.202 (0.401) | 441,237 | 0.163 (0.369) | 0.224 (0.417) | -0.061*** |
| Number of children | 0.575 (1.017) | 278,969 | 0.526 (1.013) | 0.598 (1.017) | -0.072*** |
| Access to health system [=1] | 0.011 (0.105) | 441,222 | 0.015 (0.120) | 0.009 (0.095) | 0.005*** |

Notes: Standard deviation in parentheses.

Table (II) Descriptive Statistics - Labor Force Survey

| | Observations | Average | St. Deviation |
|---|---------------------|----------------|----------------------|
| Panel A. Identification | | | |
| Registration Days | 1,598,415 | 107.35 | 4.84 |
| PEP / Pop. aged 10 - 64 x 100,000 | 1,598,415 | 787.87 | 1,155.5 |
| Log (PEP / Pop. aged 10 -64 x 100,000) | 1,598,4154 | 5.86 | 1.32 |
| Panel B. Colombian Labor Force Sample | | | |
| Weekly Hours Worked [Formal Sector] | 536,000 | 46.41 | 13.86 |
| Monthly Wages in Thousands of COL pesos [Formal Sector] | 480,780 | 1,419.44 | 1,689.99 |
| Weekly Hours Worked [Informal Sector] | 341,529 | 43.76 | 17.68 |
| Monthly Wages in Thousands of COL pesos [Informal Sector] | 299,486 | 597.81 | 482.21 |
| Employed [Formal Sector]* | 813,491 | 0.65 | 0.48 |
| Employed [Informal Sector]* | 667,012 | 0.49 | 0.49 |
| Labor Force | 1,552,222 | 0.60 | 0.49 |
| Gender [=1 Female] | 1,552,222 | 0.46 | 0.50 |
| Age | 1,552,222 | 35.40 | 15.13 |
| Married | 1,552,222 | 0.46 | 0.50 |
| Household Head | 1,552,222 | 0.34 | 0.47 |
| Years of Education | 1,552,222 | 6.46 | 3.71 |
| Informal [=1 if does not contribute to health system]** | 1,463,362 | 0.45 | 0.50 |

Notes: *Some individuals have more than one job, hence, they may be part of both the informal and formal sectors.** The indicator variable is equal to one if the individual does not contribute to the health system through his/her primary job.

Table II (cont'd): Descriptive Statistics - Labor Force Survey

| | Observations | Average | St. Deviation |
|---|---------------------|----------------|----------------------|
| Panel C. Venezuelan Sample | | | |
| Weekly Hours Worked [Formal Sector] | 2,786 | 49.45 | 12.31 |
| Monthly Wages in Thousands of COL pesos [Formal Sector] | 2,623 | 1,323.6 | 1,651.74 |
| Employed [Formal Sector] | 3,538 | 0.780 | 0.414 |
| Weekly Hours Worked [Informal Sector] | 2,346 | 46.75 | 17.40 |
| Monthly Wages in Thousands of COL pesos [Informal Sector] | 2,136 | 622.73 | 500.73 |
| Employed [Informal Sector] | 4,484 | 0.502 | 0.500 |
| Labor Force | 36,039 | 0.72 | 0.45 |
| Gender [=1 Female] | 36,039 | 0.49 | 0.50 |
| Age | 36,039 | 28.75 | 10.93 |
| Married | 36,039 | 0.54 | 0.50 |
| Household Head | 36,039 | 0.27 | 0.44 |
| Years of Education | 36,039 | 7.37 | 3.89 |
| Informal | 8,022 | 0.56 | 0.50 |
| Panel D. Colombian Returnees Sample | | | |
| Weekly Hours Worked [Formal Sector] | 1,875 | 50.051 | 12.31 |
| Monthly Wages in Thousands of COL pesos [Formal Sector] | 1,758 | 1,038.88 | 1,107.15 |
| Employed [Formal Sector] | 2,338 | 0.795 | 0.403 |
| Weekly Hours Worked [Informal Sector] | 4,116 | 45.32 | 17.21 |
| Monthly Wages in Thousands of COL pesos [Informal Sector] | 3,674 | 586.81 | 524.13 |
| Employed [Informal Sector] | 6,781 | 0.583 | 0.493 |
| Labor Force | 12,453 | 0.72 | 0.45 |
| Gender [=1 Female] | 12,453 | 0.48 | 0.50 |
| Age | 12,453 | 37.71 | 14.12 |
| Married | 12,453 | 0.54 | 0.50 |
| Household Head | 12,453 | 0.34 | 0.47 |
| Years of Education | 12,453 | 7.05 | 3.68 |
| Informal | 9,119 | 0.74 | 0.44 |

Notes: *Some individuals have more than one job, hence, they may be part of both the informal and formal sectors.** The indicator variable is equal to one if the individual does not contribute to the health system through his/her primary job.

Table (III) Impacts of PEP on Colombian Workers

| Var in logs* | Formal Sector | | | Informal Sector | | | |
|-------------------------------------|--|--|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (7) | |
| | Hours Worked* | Monthly Wages* | Employed | Hours Worked* | Monthly Wages* | Employed | |
| Panel A: OLS | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.001) [0.956] 0.057 | -0.001 (0.002) [0.544] 0.020 | -0.001*** (0.000) [0.000] 0.016 | -0.000 (0.001) [0.716] 0.117 | -0.008* (0.004) [0.132] 0.040 | -0.001 (0.001) [0.150] 0.079 | 0.000 (0.000) [0.450] 0.198 |
| Panel B: Reduced Form | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.000 (0.000) [0.888] 0.057 | -0.000 (0.000) [0.918] 0.021 | -0.0001*** (0.000) [0.016] 0.016 | -0.000 (0.000) [0.394] 0.117 | -0.000 (0.000) [0.334] 0.117 | -0.000 (0.000) [0.262] 0.040 | -0.000 (0.000) [0.812] 0.079 |
| Panel C: 2SLS (Second Stage) | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.001) [0.846] 0.057 | -0.000 (0.003) [0.904] 0.020 | -0.001*** (0.000) [0.006] 0.016 | -0.001 (0.001) [0.378] 0.117 | -0.005 (0.004) [0.312] 0.040 | -0.001 (0.001) [0.260] 0.079 | -0.001 (0.001) [0.814] 0.079 |
| Panel D: First Stage | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.054*** (0.002) [0.000] 725.9 475,179 | 0.054*** (0.002) [0.000] 725.9 475,179 | 0.054*** (0.002) [0.000] 696.18 504,876 | 0.055*** (0.003) [0.000] 419.67 292,394 | 0.055*** (0.003) [0.000] 419.67 292,394 | 0.055*** (0.003) [0.004] 400.22 363,667 | 0.054*** (0.002) [0.000] 511.61 1,545,487 |
| Controls (All Panels) | | | | | | | |
| Dep. FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Add. Controls | Yes | Yes | Yes | 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) an indicator variable for household heads. Standard errors clustered by department are in parentheses. Cluster-robust wild-bootstrap p-values are reported in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (IV) Impacts of PEP on Venezuelan Migrants

| Var in logs* | Formal Sector | | | Informal Sector | | | |
|-------------------------------------|--------------------------------|--------------------------------|--------------------------------|--------------------------------|--------------------------------|--------------------------------|--------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: OLS | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.002 (0.005) [0.684] | -0.023 (0.029) [0.422] | 0.004* (0.002) [0.088] | 0.002 (0.007) [0.740] | -0.018 (0.026) [0.462] | -0.003 (0.004) [0.572] | -0.003* (0.002) [0.194] |
| Adj.R-squared | 0.096 | 0.056 | 0.058 | 0.128 | 0.083 | 0.081 | 0.216 |
| Panel B: Reduced Form | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | -0.000 (0.000) [0.740] | -0.001 (0.002) [0.370] | 0.0002* (0.000) [0.122] | 0.000 (0.000) [0.716] | -0.000 (0.002) [0.858] | -0.000 (0.000) [0.558] | -0.000 (0.000) [0.242] |
| Adj.R-squared | 0.096 | 0.057 | 0.058 | 0.128 | 0.083 | 0.081 | 0.216 |
| Panel C: 2SLS (Second Stage) | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.002 (0.005) [0.754] | -0.025 (0.028) [0.392] | 0.004* (0.002) [0.124] | 0.003 (0.007) [0.758] | -0.006 (0.027) [0.850] | -0.003 (0.005) [0.580] | -0.003 (0.002) [0.244] |
| Adj.R-squared | 0.096 | 0.057 | 0.058 | 0.128 | 0.084 | 0.081 | 0.216 |
| Panel D: First Stage | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.058*** (0.002) [0.000] | 0.058*** (0.002) [0.000] | 0.059*** (0.002) [0.000] | 0.063*** (0.003) [0.000] | 0.063*** (0.003) [0.000] | 0.063*** (0.003) [0.000] | 0.064*** (0.002) [0.000] |
| F-Test | 1226.98 | 1226.98 | 1121.54 | 636.05 | 636.05 | 685.39 | 795.3 |
| Observations (All Panels) | 2,867 | 2,867 | 3,078 | 2,417 | 2,417 | 2,965 | 39,878 |
| Controls (All Panels) | | | | | | | |
| Dep. FE | Yes |
| Year FE | Yes |
| Month FE | Yes |
| Individual Covariates | Yes |
| Add. Controls | Yes |

Notes: We are including the same controls described in Table III. Standard errors clustered by department are in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (V) Impacts of PEP on Colombian Returnees

| Var in logs* | Formal Sector | | | Informal Sector | | | |
|-------------------------------------|---|---|---|---|---|---|--|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: OLS | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.002 (0.006) [0.808] 0.127 | -0.002 (0.024) [0.912] 0.058 | 0.002 (0.004) [0.632] 0.095 | 0.002 (0.001) [0.126] 0.124 | -0.016 (0.017) [0.388] 0.047 | -0.000 (0.003) [0.884] 0.063 | -0.000 (0.003) [0.952] 0.171 |
| Adj.R-squared | | | | | | | |
| Panel B: Reduced Form | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | -0.000 (0.000) [0.720] 0.127 | -0.000 (0.001) [0.734] 0.058 | 0.000 (0.000) [0.558] 0.095 | 0.000 (0.000) [0.368] 0.124 | -0.001 (0.001) [0.354] 0.047 | 0.000 (0.000) [0.996] 0.063 | 0.000 (0.000) [0.970] 0.171 |
| Adj.R-squared | | | | | | | |
| Panel C: 2SLS (Second Stage) | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.002 (0.006) [0.756] 0.127 | -0.007 (0.023) [0.750] 0.058 | 0.002 (0.003) [0.616] 0.095 | 0.002 (0.002) [0.358] 0.124 | -0.016 (0.016) [0.362] 0.047 | 0.000 (0.003) [0.996] 0.063 | 0.000 (0.003) [0.978] 0.171 |
| Adj.R-squared | | | | | | | |
| Panel D: First Stage | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.060*** (0.002) [0.000] 739.87 1,865 | 0.060*** (0.002) [0.000] 739.87 1,865 | 0.060*** (0.002) [0.000] 713.42 1,993 | 0.064*** (0.003) [0.000] 652.38 3,870 | 0.064*** (0.003) [0.000] 652.38 3,870 | 0.064*** (0.002) [0.000] 735.19 4,826 | 0.064*** (0.002) [0.000] 767.23 13,050 |
| F-Test | | | | | | | |
| Observations (All Panels) | | | | | | | |
| Controls (All Panels) | | | | | | | |
| Dep. FE | Yes |
| Year FE | Yes |
| Month FE | Yes |
| Individual Covariates | Yes |
| Add. Controls | Yes |

Notes: We are including the same controls described in Table III. Standard errors clustered by department are in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (VI) Impacts of PEP on Colombian Workers Controlling for Early Venezuelans Settlements

| Var in logs* | Formal Sector | | | Informal Sector | | | |
|-------------------------------------|--------------------------------|--------------------------------|---------------------------------|--------------------------------|--------------------------------|--------------------------------|--------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: OLS | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.001) [0.928] | -0.001 (0.002) [0.624] | -0.001*** (0.000) [0.004] | -0.000 (0.001) [0.698] | -0.010 (0.004) [0.168] | -0.001 (0.001) [0.130] | 0.000 (0.000) [0.490] |
| Adj.R-squared | 0.057 | 0.02 | 0.016 | 0.116 | 0.04 | 0.079 | 0.198 |
| Panel B: Reduced Form | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.000 (0.000) [0.892] | -0.000 (0.000) [0.918] | -0.000*** (0.000) [0.002] | -0.000 (0.000) [0.426] | -0.000 (0.000) [0.328] | -0.000 (0.000) [0.238] | 0.000 (0.000) [0.836] |
| Adj.R-squared | 0.057 | 0.020 | 0.016 | 0.117 | 0.040 | 0.079 | 0.198 |
| Panel C: 2SLS (Second Stage) | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.001) [0.876] | -0.000 (0.003) [0.926] | -0.001*** (0.000) [0.004] | -0.001 (0.001) [0.384] | -0.004 (0.004) [0.360] | -0.001 (0.001) [0.264] | 0.000 (0.000) [0.812] |
| Adj.R-squared | 0.057 | 0.020 | 0.016 | 0.117 | 0.040 | 0.079 | 0.198 |
| Panel D: First Stage | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.054*** (0.002) [0.000] | 0.054*** (0.002) [0.000] | 0.054*** (0.002) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.003) [0.000] | 0.054*** (0.002) [0.000] |
| F-Test | 731.51 | 731.51 | 699.6 | 423.14 | 423.14 | 403.11 | 514.55 |
| Observations (All Panels) | 475,179 | 475,179 | 504,876 | 292,394 | 292,394 | 363,667 | 1,545,487 |
| Controls (All Panels) | | | | | | | |
| Dep. FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Add. Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: We are including the same controls of Table III. 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 (VII) Impacts of PEP on Colombian Workers Controlling for Distance to the Border

| Var in logs* | Formal Sector | | | Informal Sector | | | |
|-------------------------------------|--------------------------------|--------------------------------|---------------------------------|--------------------------------|--------------------------------|--------------------------------|--------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: OLS | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.001) [0.930] | -0.001 (0.002) [0.648] | -0.001*** (0.000) [0.004] | -0.000 (0.001) [0.774] | -0.007 (0.004) [0.150] | -0.001 (0.000) [0.128] | 0.000 (0.000) [0.436] |
| Adj.R-squared | 0.057 | 0.020 | 0.016 | 0.117 | 0.040 | 0.080 | 0.197 |
| Panel B: Reduced Form | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.000 (0.000) [0.786] | -0.000 (0.000) [0.958] | -0.000*** (0.000) [0.002] | -0.000 (0.000) [0.482] | -0.000 (0.000) [0.368] | -0.000 (0.000) [0.244] | 0.000 (0.000) [0.818] |
| Adj.R-squared | 0.057 | 0.020 | 0.016 | 0.117 | 0.040 | 0.080 | 0.197 |
| Panel C: 2SLS (Second Stage) | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.001) [0.806] | -0.000 (0.003) [0.928] | -0.001*** (0.000) [0.002] | -0.001 (0.001) [0.474] | -0.004 (0.004) [0.314] | -0.001 (0.001) [0.280] | 0.000 (0.000) [0.848] |
| Adj.R-squared | 0.057 | 0.020 | 0.016 | 0.117 | 0.040 | 0.080 | 0.197 |
| Panel D: First Stage | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.054*** (0.002) [0.000] | 0.054*** (0.002) [0.000] | 0.054*** (0.002) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.002) [0.000] |
| F-Test | 715.17 | 715.17 | 685.87 | 418.41 | 418.41 | 398.84 | 507.06 |
| Observations (All Panels) | 469,100 | 469,100 | 498,702 | 290,562 | 290,562 | 361,518 | 1,531,734 |
| Controls (All Panels) | | | | | | | |
| Dep. FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Add. Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: We are including the same controls of Table III. Clustered standard errors at the municipality-monthly level are reported in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

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

| Dep. Variable | Wages |
|---|---------------------------------|
| Gender [=1 if male] | 0.336*** (0.022) [0.000] |
| Age | 0.001 (0.001) [0.506] |
| Married [=1 if Married or Cohabiting] | 0.035*** (0.008) [0.000] |
| Head of Household | 0.217*** (0.014) [0.000] |
| Years of Education | 0.009*** (0.004) [0.008] |
| Years of Education ² | -0.001*** (0.000) [0.000] |
| Informality: Does not Contribute to Health System | -0.951*** (0.036) [0.000] |
| Adj. R-squared | 0.084 |
| Observations | 767,573 |

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 III](#). Standard errors clustered by department are in parentheses. Cluster-robust wild-bootstrap p-values are reported in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%

Figure (I) RAMV Registration Points and Geographic Location of Irregular Migrants Identified with the RAMV

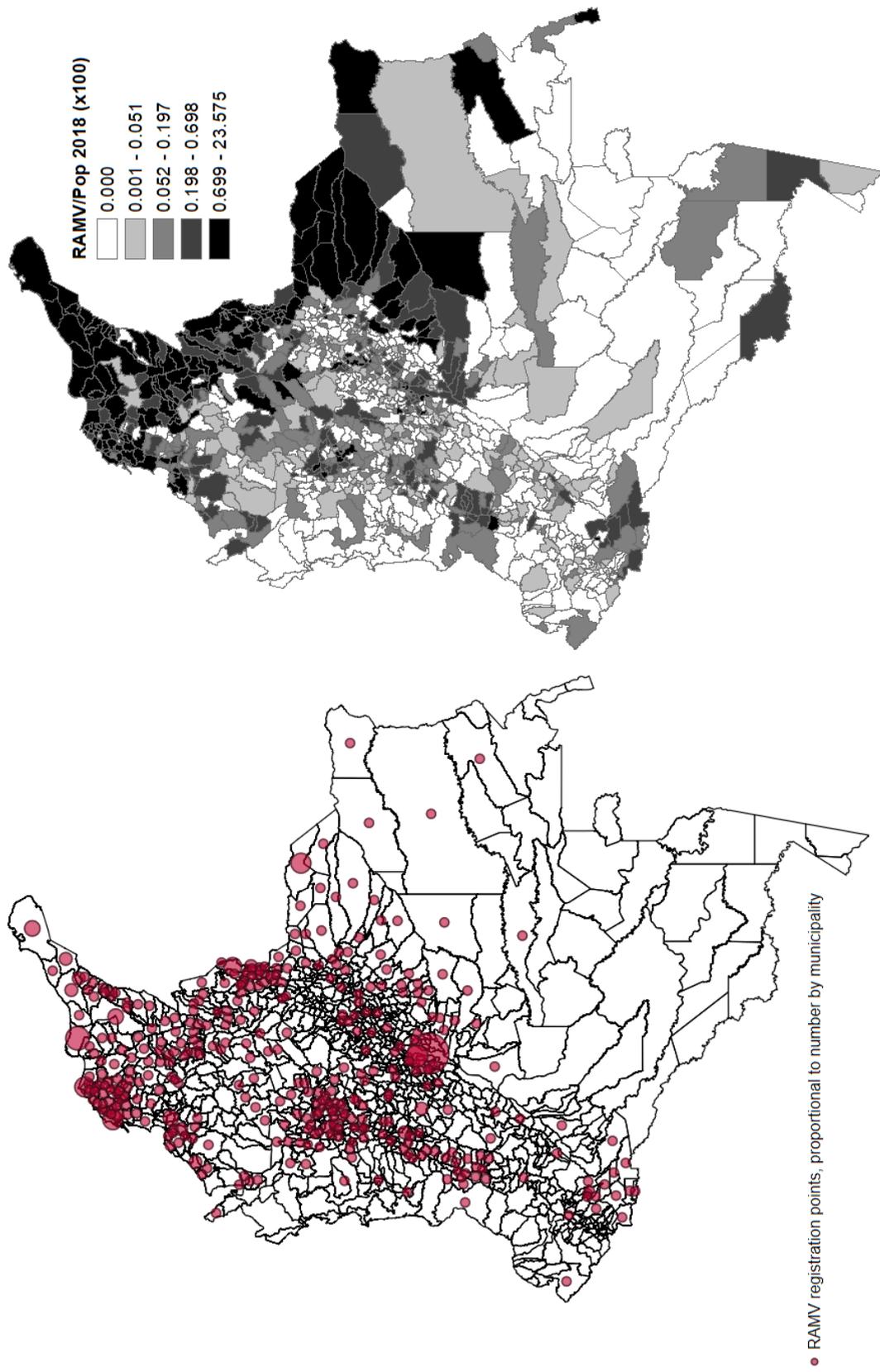


Figure (II) Distribution of Registration Days for PEP

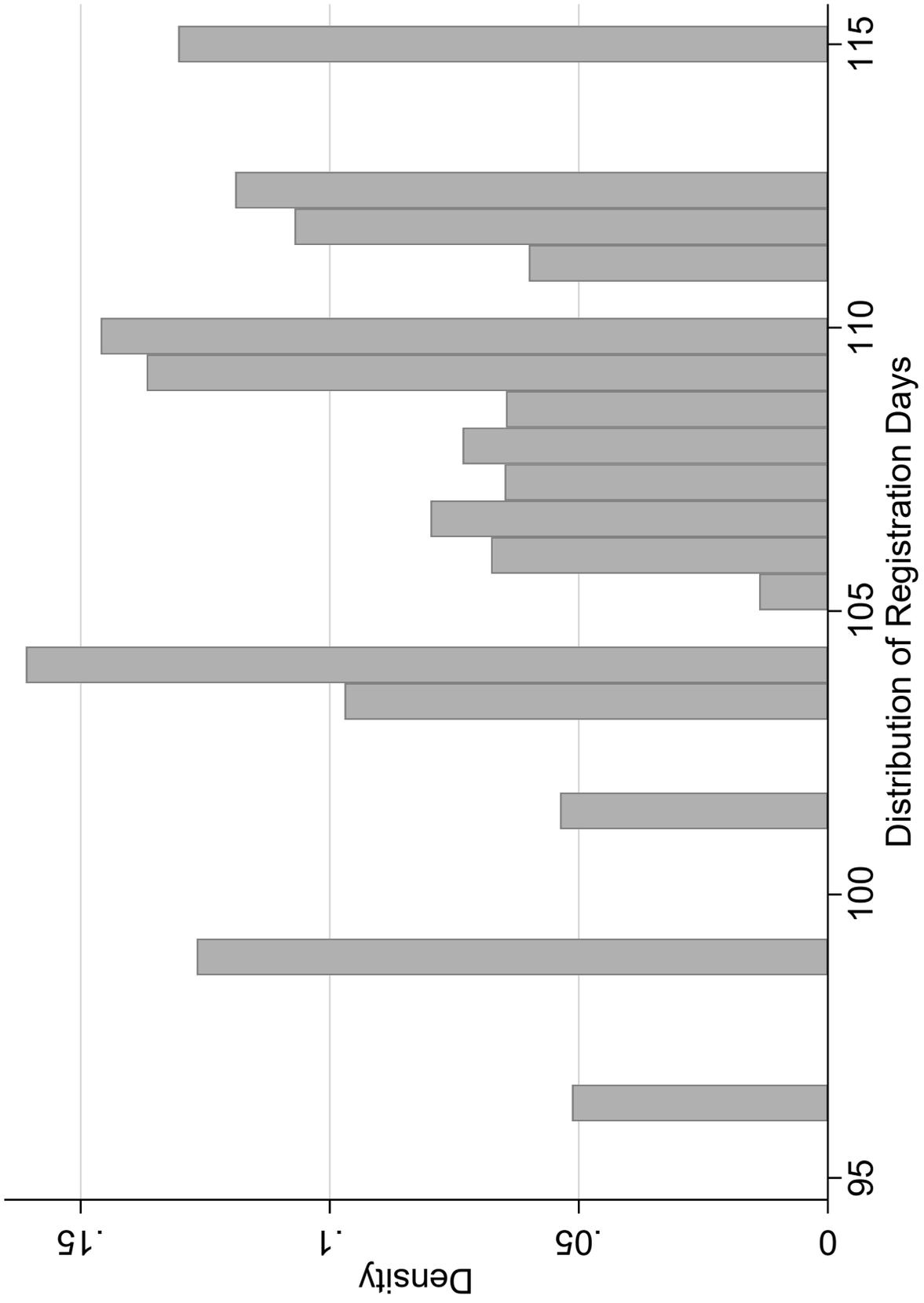
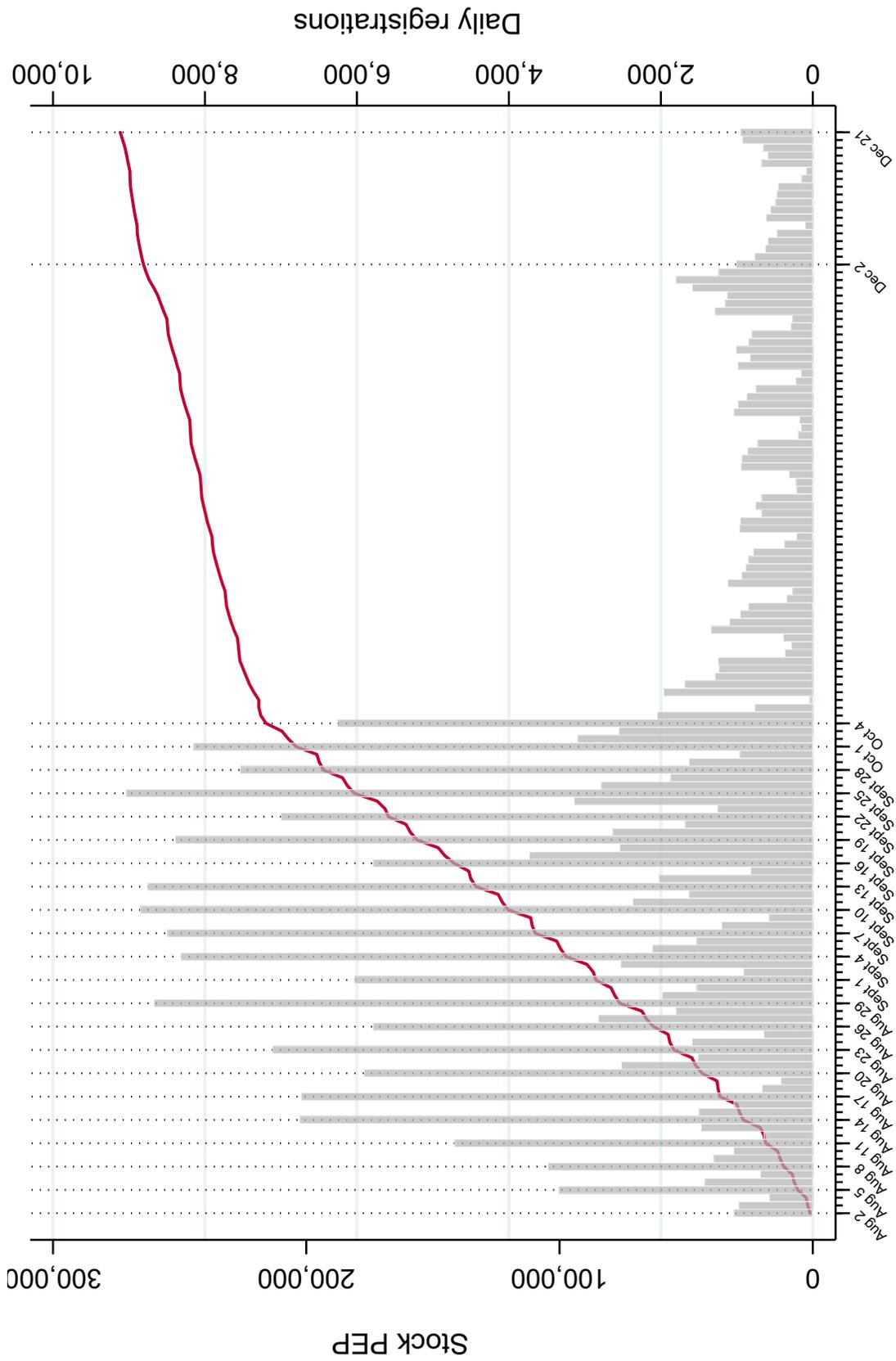


Figure (III) PEP registrations: Daily Flow and Total Stock



Daily PEP registrations (right axis)
 Stock PEP (left-axis)

* Dec 2: Original end date; Dec 21: Extension end date

Figure (IV) PEP Registrations by Department

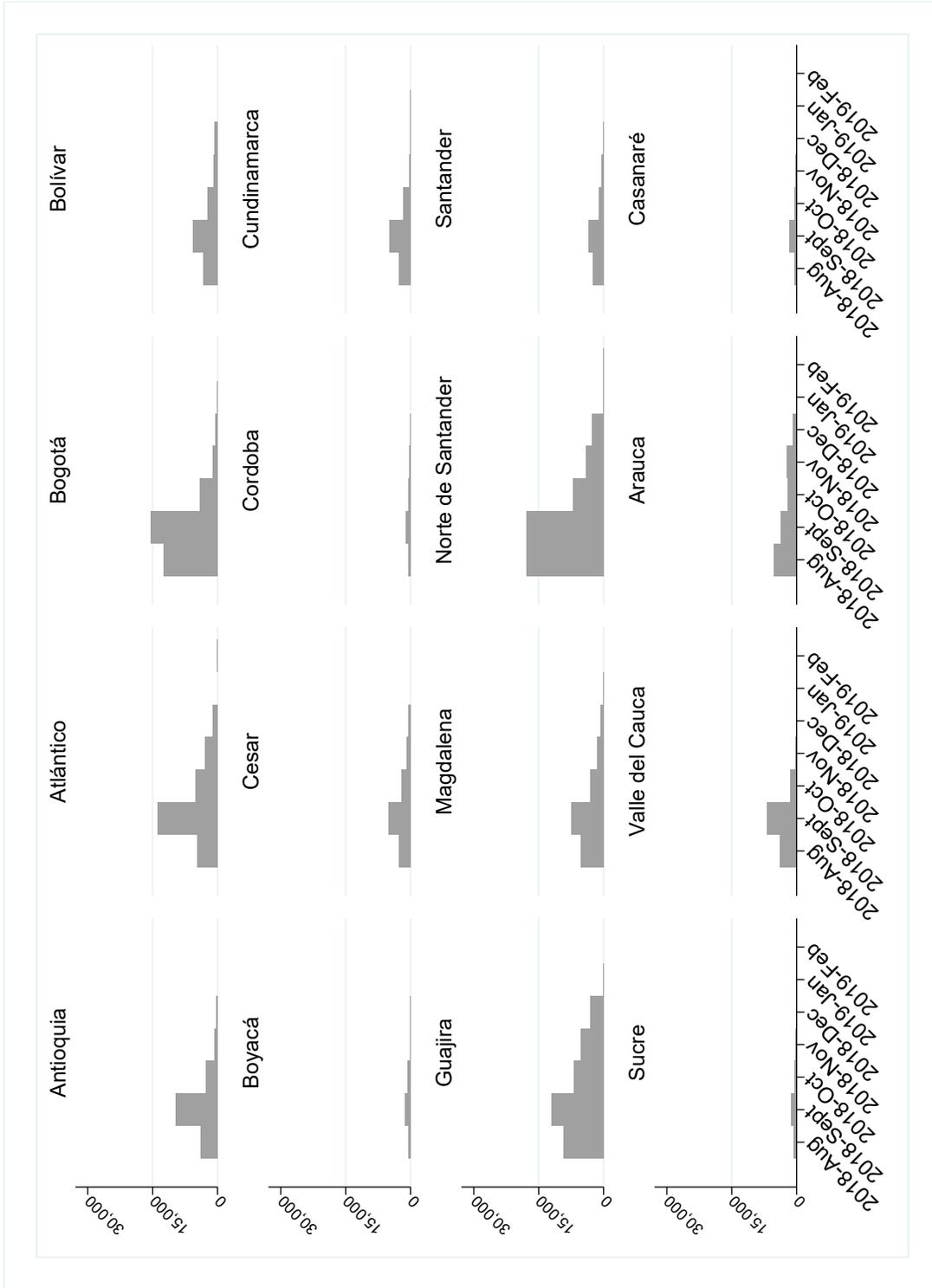


Figure (V) Average Registration Days and PEP Registrations

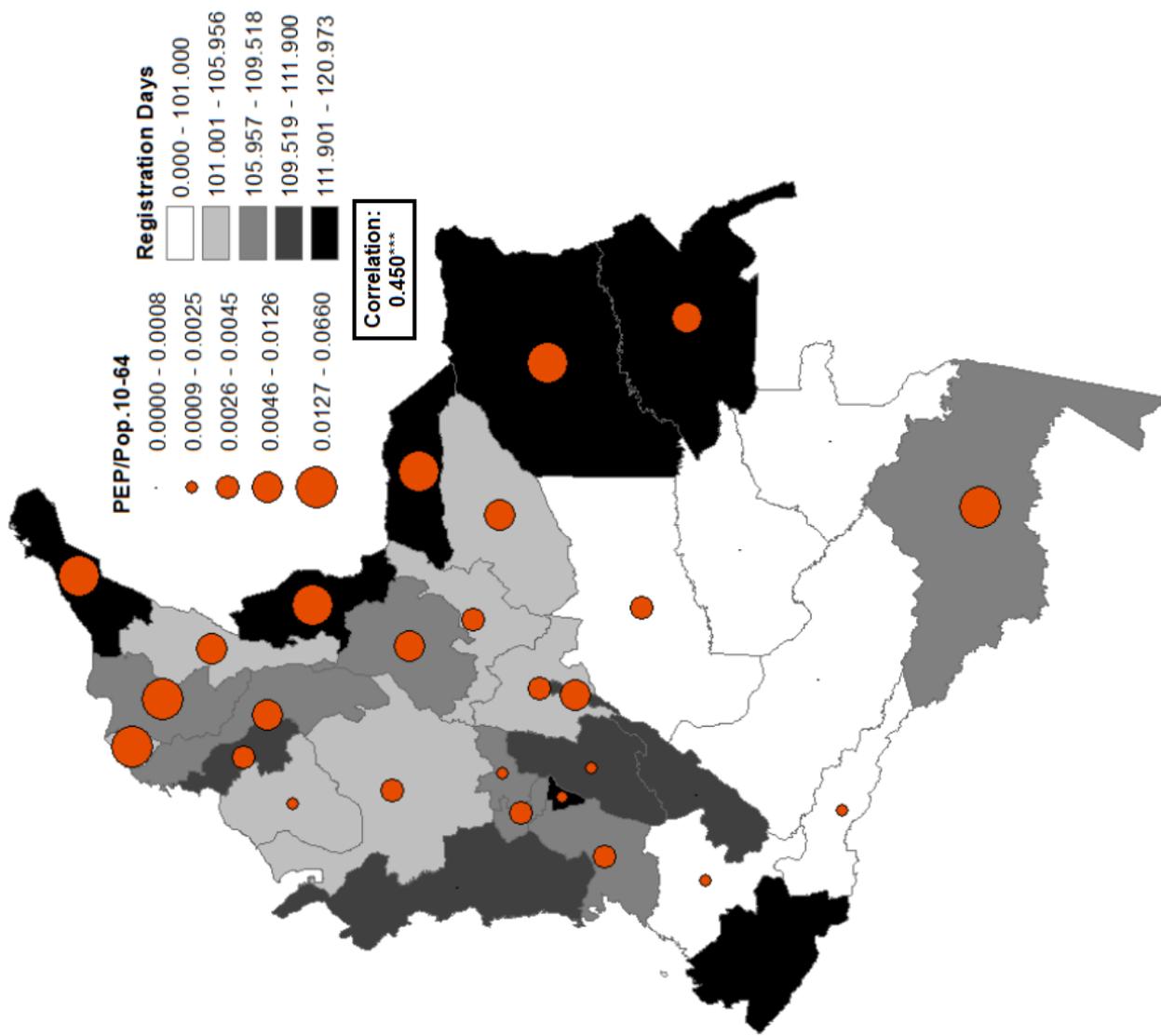


Figure (VI) Event Study: Impacts of PEP on the Formal Sector

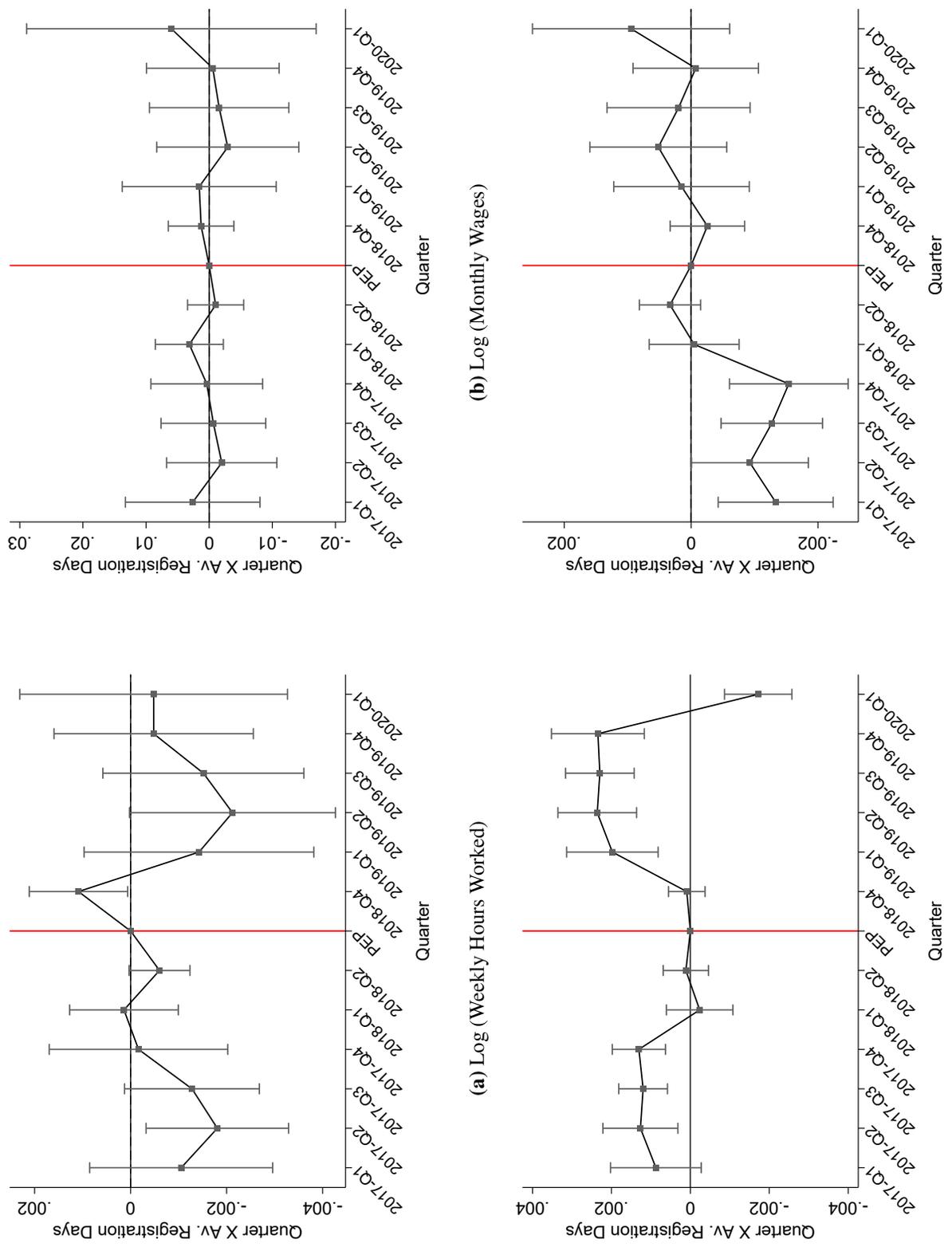
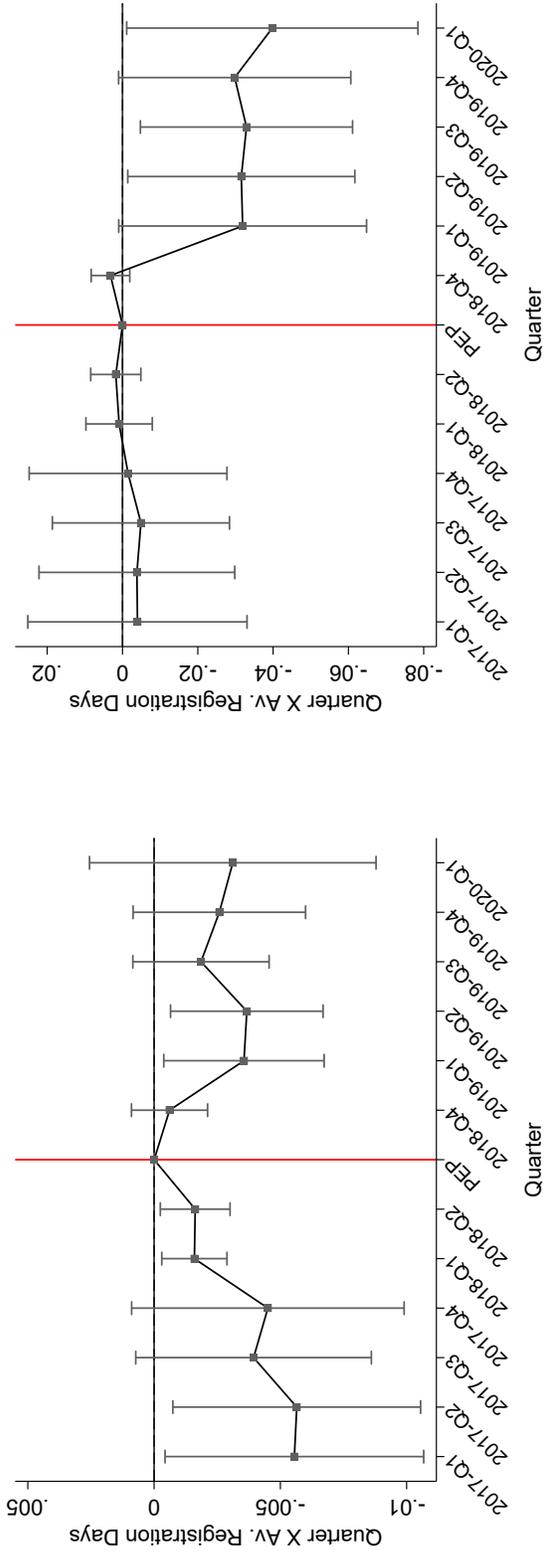
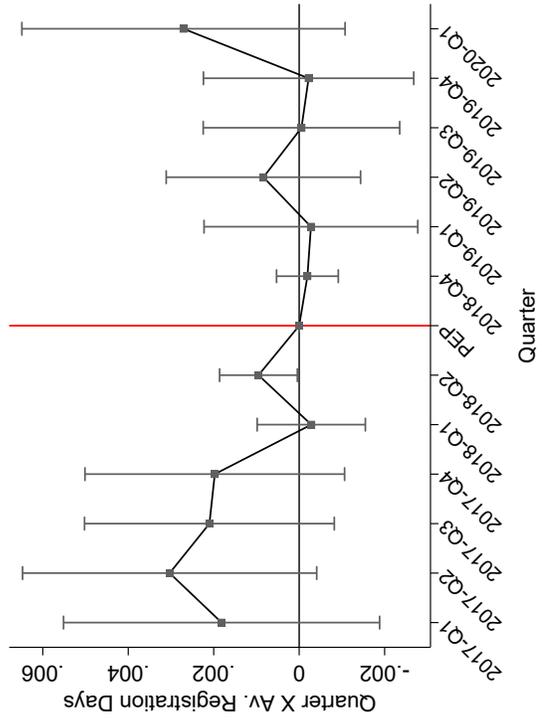


Figure (VII) Event Study: Impacts of PEP Informal Sector



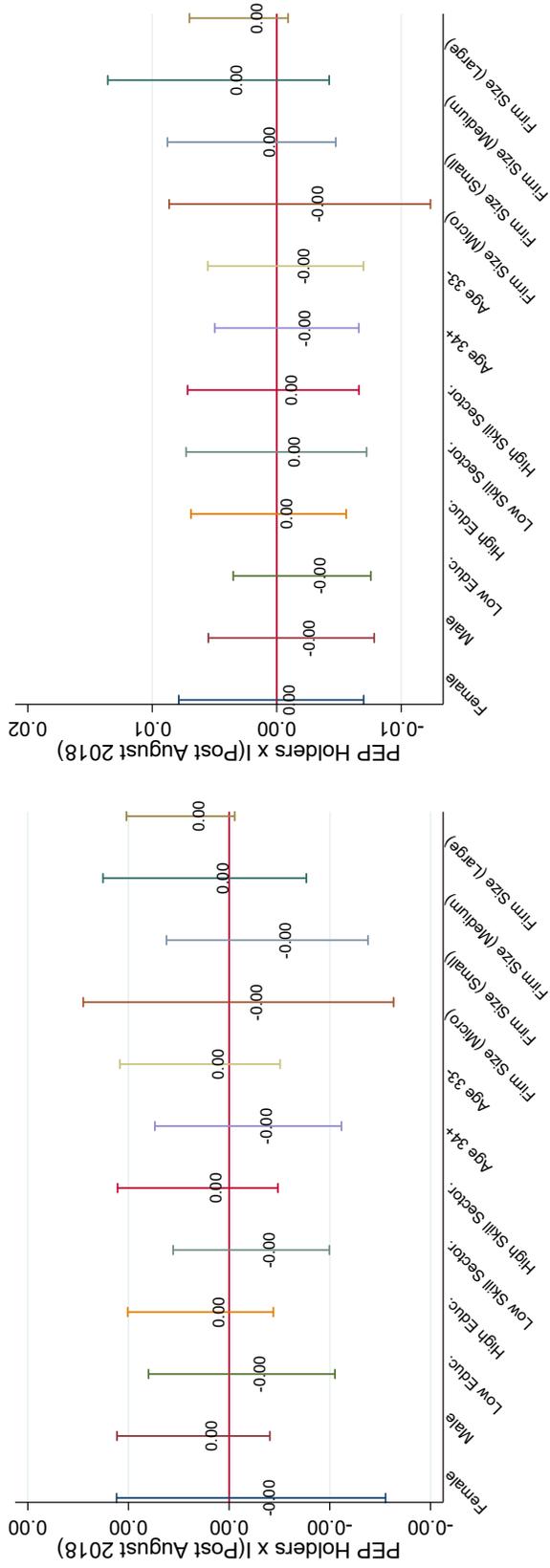
(b) Log (Monthly Wages)

(a) Log (Weekly Hours Worked)

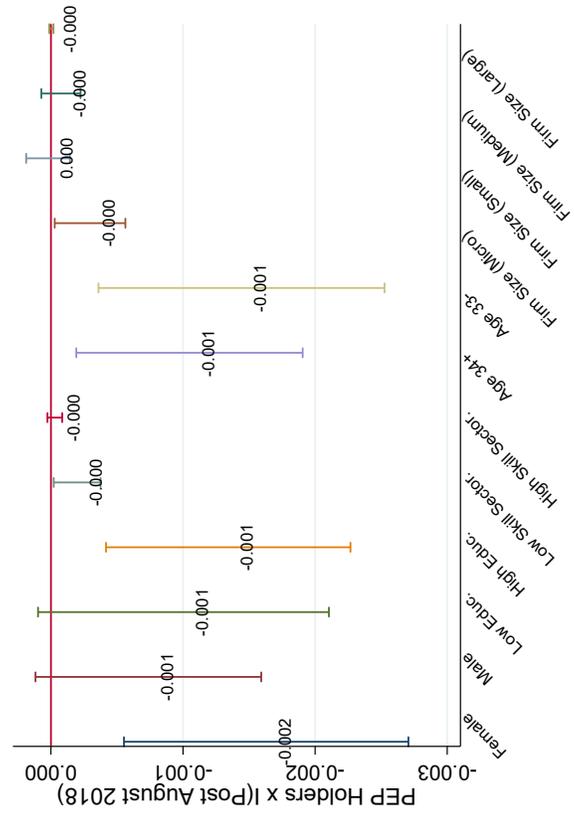


(c) Employed

Figure (VIII) Heterogeneous Effects: Formal Sector

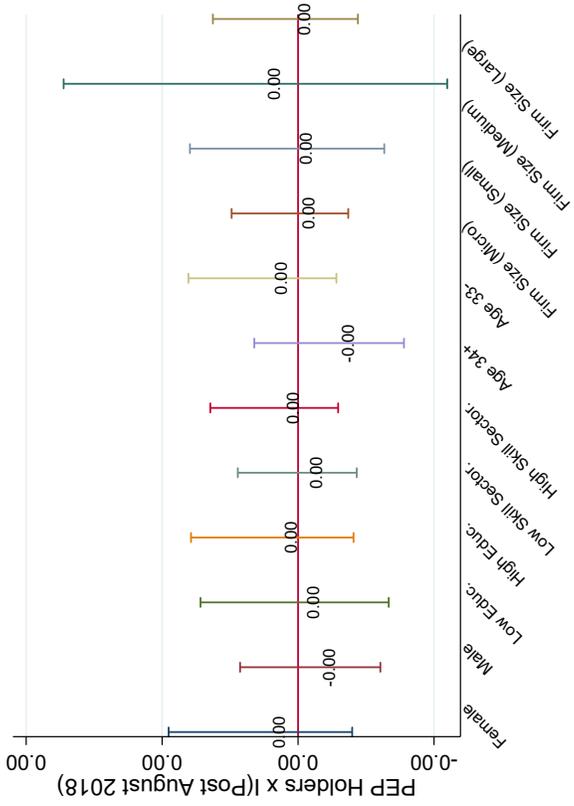


(a) Log(Weekly Hours Worked)



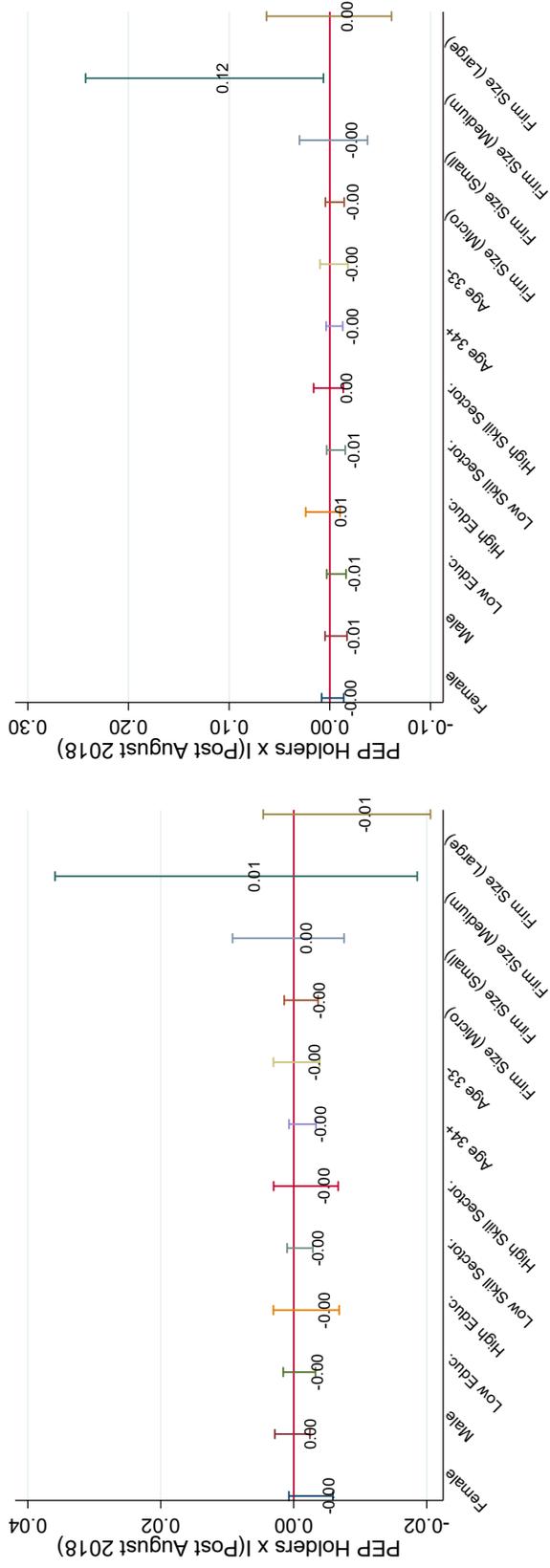
(c) Employed

(b) Log(Monthly Wages)



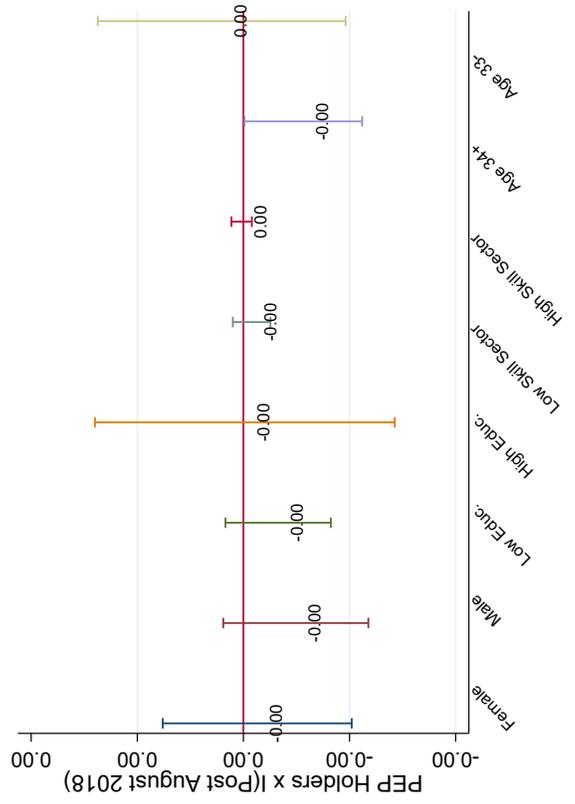
(d) Labor Force (All Sample)

Figure (IX) Heterogeneous Effects: Informal Sector



(a) Log(Weekly Hours Worked)

(b) Log(Monthly Wages)



(c) Employed

Appendix A: Descriptive Statistics

Table (A.1) Descriptive Statistics - Control Variables

| Variable | Year | Observations | Average | St. Deviation |
|---|------|--------------|---------|---------------|
| GINI | 2005 | 24 | 0.45 | 0.03 |
| Unsatisfied Basic Needs (UBN, % Households) | 2005 | 24 | 44.47 | 72.33 |
| Informal Labor* (% Household) | 2005 | 24 | 94.69 | 4.59 |
| Homicide (% Pop by 100,000 Individuals) | 2014 | 24 | 33.90 | 19.31 |
| N. of Terrorist Attacks | 1995 | 24 | 1.08 | 1.95 |
| Night Light Density | 2013 | 24 | 4.71 | 4.95 |
| Number of Financial Institutions | 1995 | 24 | 74.17 | 65.38 |
| Number of Tax Collection Office | 1995 | 24 | 29.25 | 25.60 |
| GDP in agriculture (Millions) | 2009 | 24 | 1.37 | 1.74 |
| GDP in industry (Millions) | 2009 | 24 | 1.38 | 1.70 |
| GDP in services (Millions) | 2009 | 24 | 1.14 | 0.88 |
| Central Gov. Transfers (Millions) | 2010 | 24 | 4.73 | 5.73 |
| Educ. Central Transfers (Millions) | 2010 | 24 | 8.85 | 15.36 |
| Health Central Transfers (Millions) | 2010 | 24 | 578,723 | 455,661 |
| Total Municipal Income (Millions) | 2016 | 24 | 271.94 | 295.77 |
| Mun. Public Expenditure (Millions) | 2016 | 24 | 244.89 | 445.22 |
| Total Exports to Venezuela | 2005 | 24 | 16.53 | 27.98 |
| PEPI (August 2017-October 2017) | 2017 | 24 | 2,816 | 5,835 |
| PEP2 (February 2018-June 2018) | 2018 | 24 | 4,618 | 9,932 |
| PEP4 (January 2019-) | 2019 | 24 | 4,812 | 10,712 |
| Inverse of Distance to the Border | 1995 | 23 | 0.04 | 0.21 |
| Venezuelan Population | 2005 | 24 | 1,529 | 2,002 |

Table (A.2) Descriptive Statistics - Registration Windows

| Registration Number | | Registration Date | | Pop. Share by Registration Window |
|---------------------|-----------|-------------------|-----------|-----------------------------------|
| 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: Identification

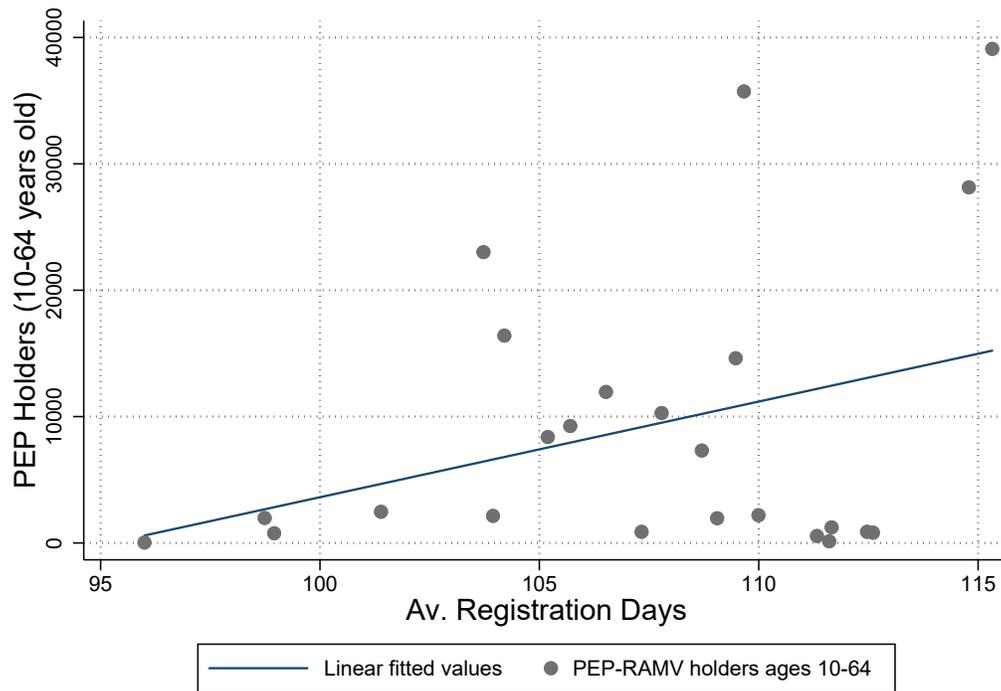
Table (B.1) Av. Registration Days - Department Controls

| Dep. Variable | Av. Registration Days by Dep. |
|--|-------------------------------|
| Population in 2017 | 0.000 (0.000) |
| Av. Wage 2017 | -0.062 (0.084) |
| Av. Hours Worked 2017 | -2.273 (4.785) |
| Av. Unemployment 2017 | -252.819 (510.323) |
| Informal labor (% HH) | 1.492 (9.227) |
| Index for unsatisfied basic needs 2005 | -0.046 (0.819) |
| Subsidized health care system | -0.000 (0.000) |
| GDP per capita 2017 | 0.000 (0.000) |
| Av. Night light density 2013 | 2.385 (3.414) |
| Terrorist events 2017 | 0.760 (2.211) |
| Homicide rate 2017 | -0.007 (0.019) |
| Hectares coca 2016 | 0.001 (0.001) |
| N. of financial institutions | 0.556 (0.739) |
| N. of tax collection offices | -0.392 (0.767) |
| N. of hospitals | -0.480 (1.019) |
| N. of health centers | -0.422 (0.773) |
| N. education establishments | 0.006 (0.059) |
| Land fertility index | -1.145 (1.803) |
| Height (MASL) | -0.018 (0.039) |
| Venezuelan early settlements in 2005 | 0.004 (0.007) |
| Foreigners 2018 | -0.000 (0.001) |
| Constant | 175.587 (975.915) |
| R-squared | 0.848 |
| Observations | 23 |

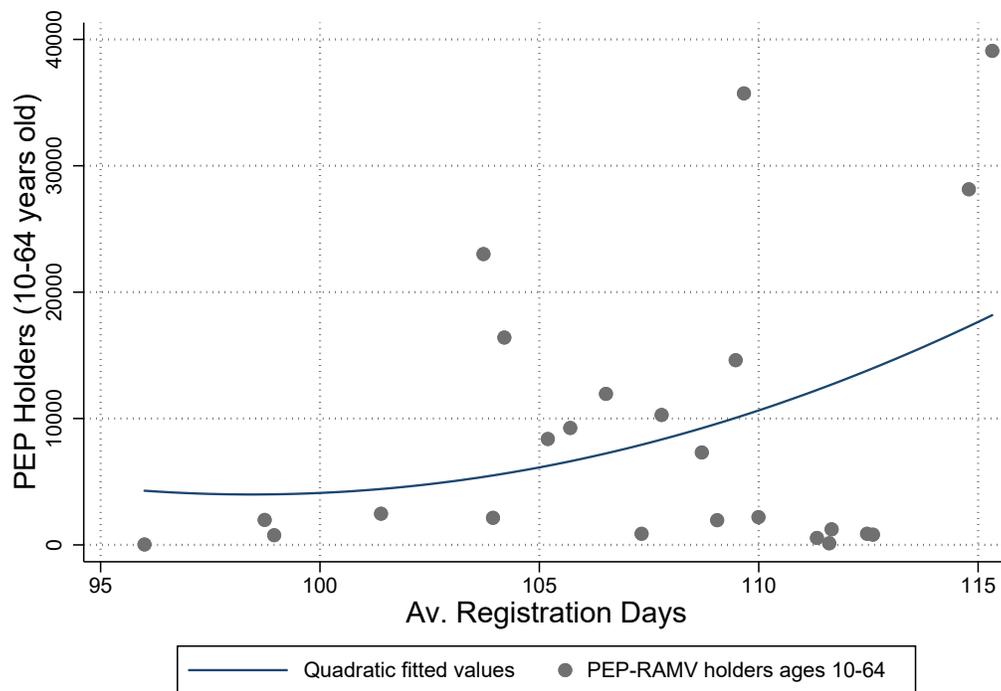
Table (B.2) Av. Registration Days - Municipal Controls

| Dep. Variable | Av. Registration Days by Mun. |
|--|-------------------------------|
| Population in 2017 | -0.000 (0.000) |
| Informal labor (% HH) | 0.136 (0.120) |
| Index for unsatisfied basic needs 2005 | -0.043 (0.031) |
| Subsidized health care System | -0.000 (0.000) |
| GDP per capita 2017 | -0.000 (0.000) |
| Terrorist events 2017 | -0.592 (0.692) |
| Homicide rate 2017 | 0.012 (0.018) |
| Hectares coca 2016 | 0.000 (0.000) |
| Av. Night light density 2013 | 0.016 (0.076) |
| N. of financial institutions | -0.128 (0.425) |
| N. of tax collection officies | -2.680*** (0.819) |
| N. of hospitals | 2.172*** (0.759) |
| N. of health centers | 0.233 (0.576) |
| N. education establishments | -0.018 (0.045) |
| Land fertility index | -0.043 (0.029) |
| Height (MASL) | -0.001 (0.000) |
| Tot. Early Ven.settlements in 2005 | 0.001 (0.005) |
| Foreigners 2018 | 0.000 (0.000) |
| Constant | 95.386*** (11.188) |
| R-squared | 0.033 |
| Observations | 808 |

Figure (B.1) Av. Registration Days and PEP Holders ages 10-64



Note: Correlation 0.334***



Note: Correlation 0.334***

Appendix C: Robustness Test

Figure (C.1) Robustness Test: Exclude Border Departments (Formal Sample)

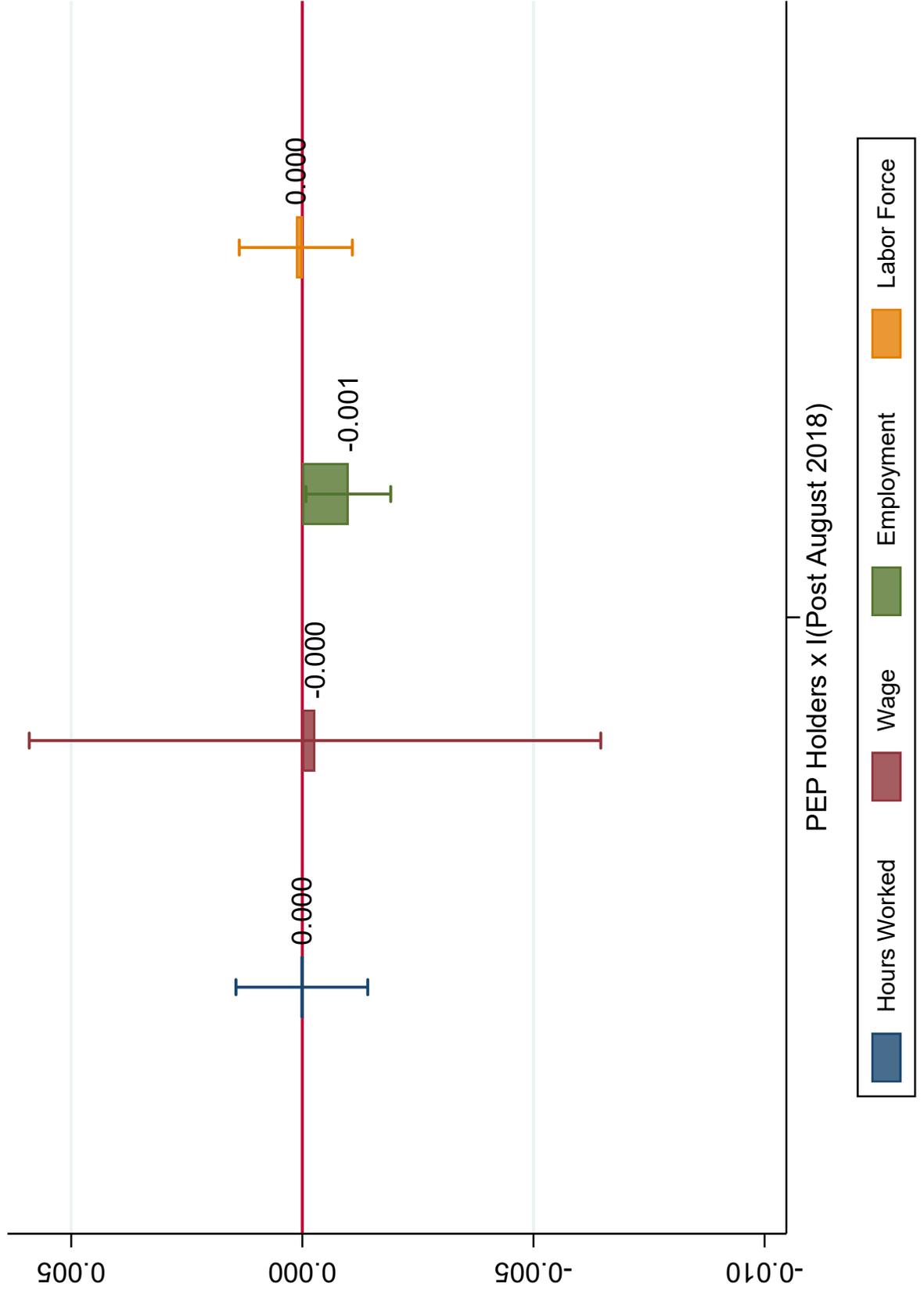


Figure (C.2) Robustness Test: Exclude Border Departments (Informal Sample)

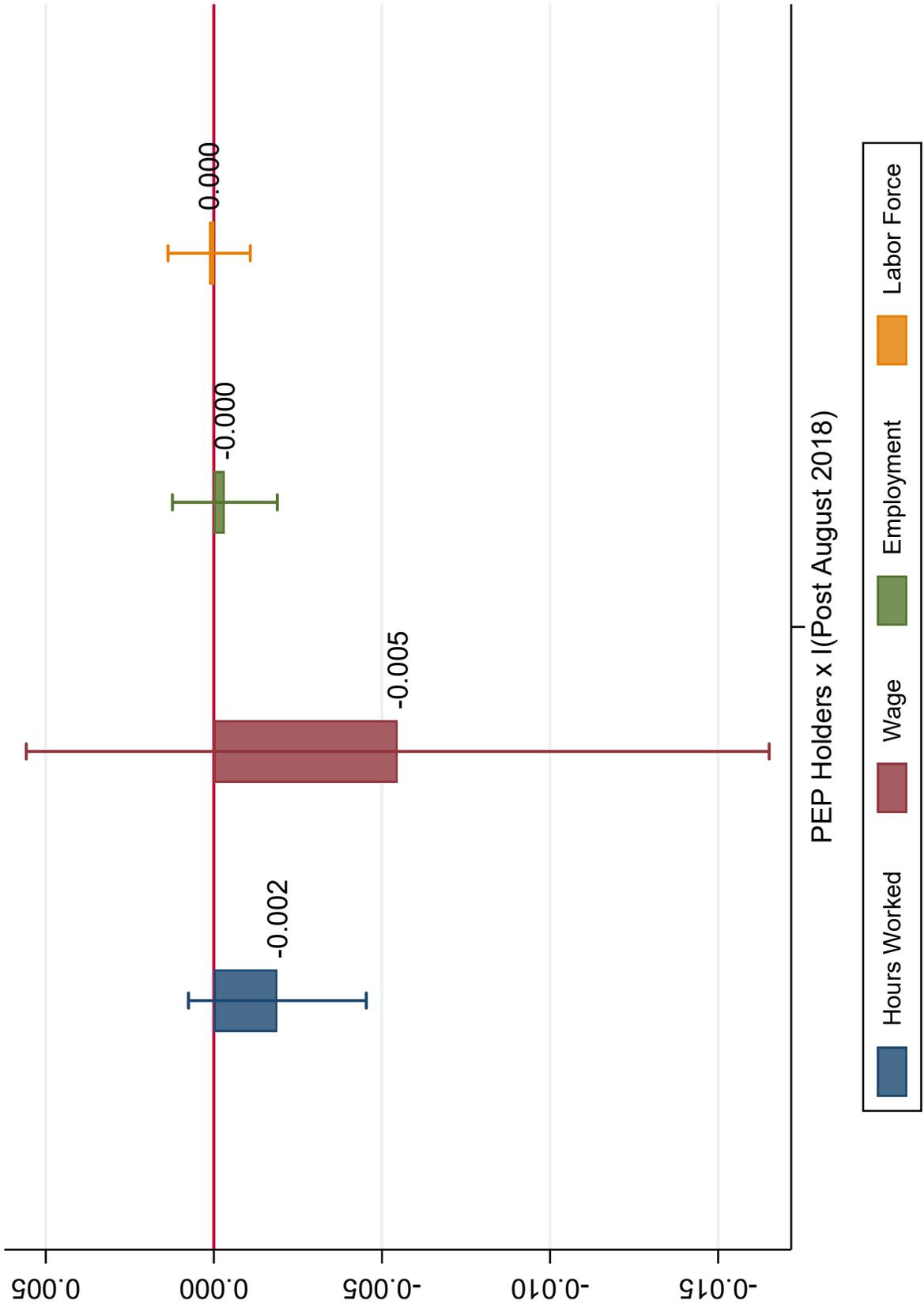
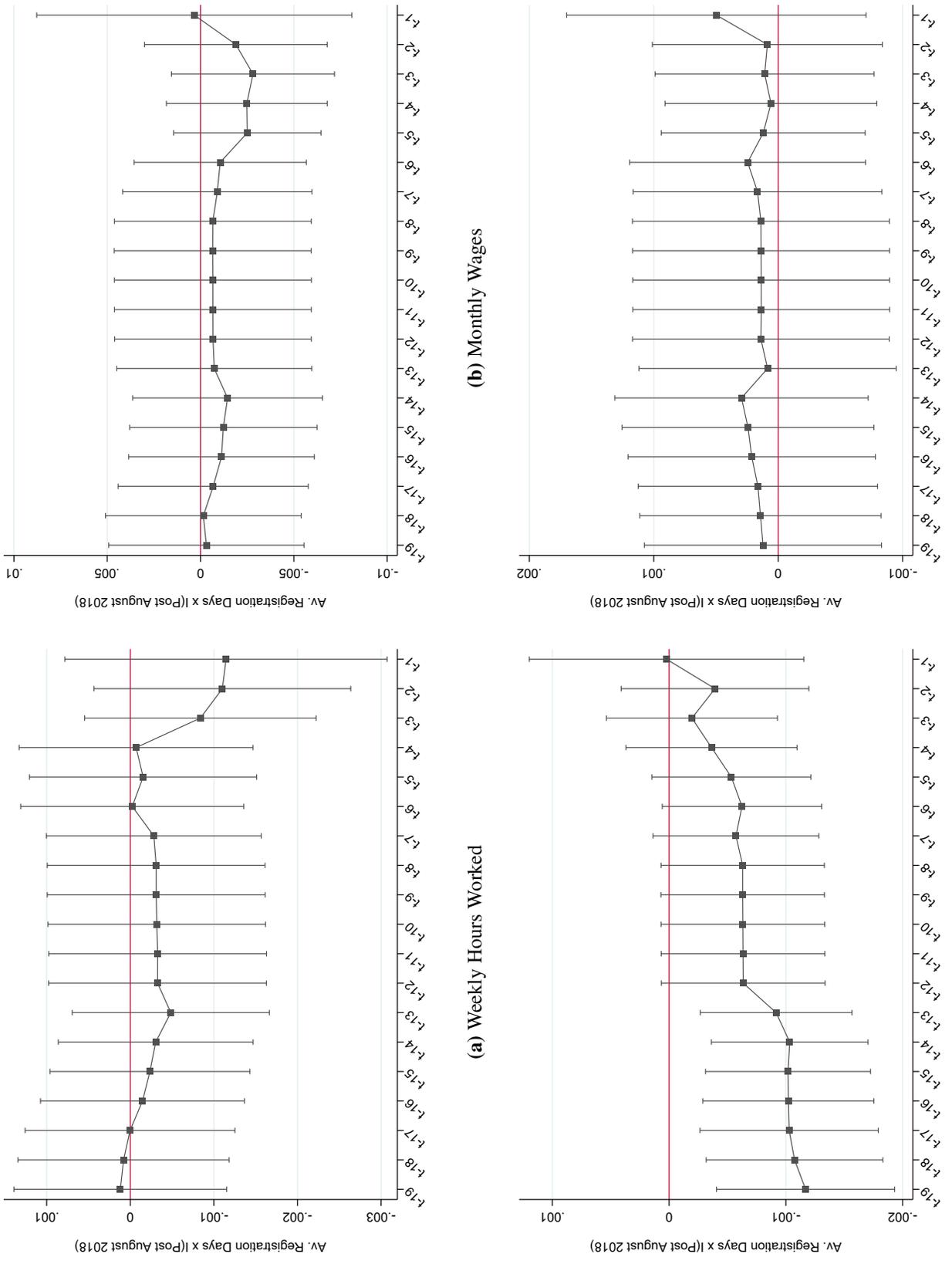


Table (C.1) Impacts of PEP on Colombian Workers without department controls

| Var in logs* | Formal Sector | | | Informal Sector | | | |
|-------------------------------------|--------------------------------|--------------------------------|---------------------------------|--------------------------------|--------------------------------|--------------------------------|--------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: OLS | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.000 (0.000) [0.778] | -0.003 (0.002) [0.220] | -0.001** (0.000) [0.002] | -0.001 (0.002) [0.638] | -0.012** (0.004) [0.022] | 0.000 (0.001) [0.706] | 0.000 (0.000) [0.996] |
| Adj.R-squared | 0.056 | 0.019 | 0.016 | 0.116 | 0.039 | 0.079 | 0.198 |
| Panel B: Reduced Form | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.000 (0.000) [0.784] | -0.000 (0.000) [0.840] | -0.000*** (0.000) [0.018] | -0.000 (0.000) [0.510] | -0.000 (0.000) [0.330] | -0.000 (0.000) [0.208] | 0.000 (0.000) [0.954] |
| Adj.R-squared | 0.056 | 0.020 | 0.016 | 0.116 | 0.039 | 0.079 | 0.198 |
| Panel C: 2SLS (Second Stage) | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.001) [0.706] | -0.001 (0.003) [0.834] | -0.001*** (0.000) [0.010] | -0.001 (0.001) [0.458] | -0.005 (0.004) [0.292] | -0.001 (0.001) [0.192] | 0.000 (0.000) [0.936] |
| Adj.R-squared | 0.056 | 0.020 | 0.016 | 0.116 | 0.039 | 0.079 | 0.198 |
| Panel D: First Stage | | | | | | | |
| Reg. Days x I(Post Aug. 2018) | 0.054*** (0.002) [0.000] | 0.054*** (0.002) [0.000] | 0.054*** (0.002) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.003) [0.000] | 0.055*** (0.003) [0.000] |
| F-Test | 694.13 | 694.13 | 654.04 | 403.15 | 403.15 | 373.19 | 481.9 |
| Observations (All Panels) | 475,179 | 475,179 | 504,876 | 292,394 | 292,394 | 363,667 | 1,545,487 |
| Controls (All Panels) | | | | | | | |
| Dep. FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Add. Controls | No | No | No | No | No | No | No |

Notes: We are including the same individuals covariates of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Figure (C.3) Robustness Test Different time periods before PEP - Colombian Workers - Formal Sector



Appendix D: Heterogeneous Effects Tables

Table (D.1) Heterogeneous Effects: Informal Sector

| | Female | Male | Low Education | High Education | Age 34+ | Age 33- | Low Skill Working Sector | High Skill Working Sector |
|---------------------------------|--------------------|-------------------|-------------------|-------------------|-------------------|-------------------|--------------------------|---------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Panel A. Hours Worked | | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.003* (0.002) | 0.000 (0.001) | -0.001 (0.001) | -0.002 (0.002) | -0.001 (0.001) | -0.002 (0.002) | -0.001 (0.001) | -0.000 (0.002) |
| R-squared | [0.146] | [0.876] | [0.486] | [0.404] | [0.360] | [0.452] | [0.202] | [0.804] |
| F-Test | 0.026 | 0.062 | 0.123 | 0.079 | 0.126 | 0.047 | 0.135 | 0.116 |
| Observations | 412.13 | 426.51 | 436.49 | 303.21 | 413.27 | 439.44 | 432.04 | 396.43 |
| | 135,417 | 156,977 | 250,699 | 41,682 | 209,347 | 67,971 | 184,978 | 107,416 |
| Panel B. Wages | | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.003 (0.005) | -0.006 (0.005) | -0.007 (0.005) | 0.007 (0.008) | -0.010 (0.004) | 0.001 (0.007) | -0.005 (0.004) | -0.004 (0.007) |
| R-squared | [0.564] | [0.340] | [0.212] | [0.452] | [0.180] | [0.878] | [0.288] | [0.560] |
| F-Test | 0.018 | 0.027 | 0.044 | 0.041 | 0.047 | 0.026 | 0.048 | 0.049 |
| Observations | 412.13 | 426.51 | 436.49 | 303.21 | 413.27 | 439.44 | 432.04 | 396.43 |
| | 135,417 | 156,977 | 250,699 | 41,682 | 209,347 | 67,971 | 184,978 | 107,416 |
| Panel C. Employed | | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.000 (0.001) | -0.001 (0.001) | -0.001 (0.000) | 0.000 (0.001) | -0.000 (0.000) | 0.000 (0.000) | -0.001 (0.001) | 0.000 (0.001) |
| R-squared | [0.774] | [0.138] | [0.174] | [0.996] | [0.374] | [0.726] | [0.060] | [0.712] |
| F-Test | 0.093 | 0.063 | 0.065 | 0.110 | 0.013 | 0.003 | 0.034 | 0.079 |
| Observations | 393.78 | 407.09 | 419.44 | 262.17 | 403.92 | 428.35 | 409.3 | 382.78 |
| | 174,055 | 189,612 | 312,092 | 51,530 | 220,106 | 67,636 | 226,273 | 137,394 |

Notes: We are including the same individuals covariates of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (D.2) Heterogeneous Effects: Informal Sector

| | Firm Size (0 - 9 Workers) (1) | Firm Size (10 - 49 Workers) (2) | Firm Size (50 - 99 Workers) (3) | Firm Size (100+ Workers) (4) |
|---------------------------------|----------------------------------|------------------------------------|------------------------------------|---------------------------------|
| Panel A. Hours Worked | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.001 (0.001) [0.392] | 0.001 (0.004) [0.828] | 0.008 (0.012) [0.502] | -0.008 (0.006) [0.188] |
| R-squared | 0.121 | 0.098 | 0.193 | 0.096 |
| F-Test | 417.17 | 503.4 | 359.35 | 275.14 |
| Observations | 278,739 | 9,512 | 779 | 3,364 |
| Panel B. Wages | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.005 (0.004) [0.300] | -0.004 (0.016) [0.794] | 0.124** (0.053) [0.072] | 0.001 (0.029) [0.998] |
| R-squared | 0.042 | 0.045 | 0.176 | 0.066 |
| F-Test | 417.17 | 503.4 | 359.35 | 275.14 |
| Observations | 278,739 | 9,512 | 779 | 3,364 |

Notes: We are including the same individuals covariates of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (D.3) Heterogeneous Effects: Formal Sector

| | Female (1) | Male (2) | Low Education (3) | High Education (4) | Age 34+ (5) | Age 33- (6) | Low Skill Working Sector (7) | High Skill Working Sector (8) |
|---------------------------------|---------------------------------|---------------------------------|------------------------------|---------------------------------|--------------------------------|---------------------------------|------------------------------------|-------------------------------------|
| Panel A. Hours Worked | | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.000 (0.001) [0.760] | 0.001 (0.001) [0.352] | -0.000 (0.001) [0.774] | 0.001 (0.001) [0.436] | -0.000 (0.001) [0.608] | 0.001 (0.001) [0.438] | -0.000 (0.001) [0.666] | 0.001 (0.001) [0.460] |
| R-squared | 0.021 | 0.034 | 0.095 | 0.028 | 0.070 | 0.034 | 0.087 | 0.064 |
| F-Test | 678.05 | 709.64 | 819.69 | 573.09 | 775.57 | 588.32 | 668.74 | 735.33 |
| Observations | 230,040 | 245,139 | 227,302 | 247,870 | 254,997 | 195,760 | 292,046 | 183,133 |
| Panel B. Wages | | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.003) [0.908] | -0.001 (0.003) [0.760] | -0.002 (0.003) [0.376] | 0.001 (0.003) [0.836] | 0.000 (0.003) [0.992] | 0.000 (0.003) [0.944] | -0.001 (0.003) [0.778] | -0.001 (0.003) [0.820] |
| R-squared | 0.014 | 0.018 | 0.045 | 0.092 | 0.030 | 0.030 | 0.027 | 0.050 |
| F-Test | 678.05 | 709.64 | 819.69 | 573.09 | 775.57 | 588.32 | 668.74 | 735.33 |
| Observations | 230,040 | 245,139 | 227,302 | 247,870 | 254,997 | 195,760 | 292,046 | 183,133 |
| Panel C. Employed | | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.002*** (0.001) [0.002] | -0.001* (0.000) [0.094] | -0.001 (0.001) [0.066] | -0.001*** (0.000) [0.004] | -0.001** (0.000) [0.006] | -0.001*** (0.000) [0.290] | -0.0001* (0.000) [0.014] | -0.001 (0.001) [0.012] |
| R-squared | 0.018 | 0.011 | 0.019 | 0.015 | 0.009 | 0.001 | 0.010 | 0.036 |
| F-Test | 644.73 | 714.19 | 824.82 | 556.06 | 789.41 | 571.5 | 672.05 | 741.21 |
| Observations | 244,710 | 260,166 | 254,838 | 250,020 | 257,138 | 195,399 | 331,106 | 173,770 |
| Panel D. Labor Force | | | | | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.001 (0.001) [0.408] | -0.000178 (0.000) [0.750] | 0.000 (0.001) [0.944] | 0.000 (0.001) [0.532] | 0.000 (0.000) [0.986] | 0.000 (0.000) [0.400] | -0.000 (0.001) [0.398] | 0.000 (0.001) [0.342] |
| R-squared | 0.094 | 0.319 | 0.250 | 0.198 | 0.146 | 0.128 | 0.141 | 0.403 |
| F-Test | 476.57 | 506.23 | 501.2 | 454.5 | 550.74 | 509.87 | 521.36 | 457.1 |
| Observations | 831,977 | 713,510 | 1,027,322 | 518,039 | 563,932 | 301,377 | 782,133 | 763,354 |

Notes: We are including the same individuals covariates of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table (D.4) Heterogeneous Effects: Formal Sector

| | Firm Size (0 - 9 Workers) (1) | Firm Size (10 - 49 Workers) (2) | Firm Size (50 - 99 Workers) (3) | Firm Size (100+ Workers) (4) |
|---------------------------------|----------------------------------|------------------------------------|------------------------------------|---------------------------------|
| Panel A. Hours Worked | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.000 (0.001) [0.916] | -0.001 (0.001) [0.430] | 0.000 (0.001) [0.618] | 0.001* (0.001) [0.092] |
| R-squared | 0.089 | 0.053 | 0.052 | 0.079 |
| F-Test | 775.1 | 805.25 | 848.64 | 585.95 |
| Observations | 173,388 | 64,089 | 20,031 | 217,671 |
| Panel B. Wages | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.002 (0.005) [0.746] | 0.002 (0.003) [0.546] | 0.005 (0.004) [0.292] | 0.003 (0.002) [0.120] |
| R-squared | 0.033 | 0.028 | 0.033 | 0.058 |
| F-Test | 775.1 | 805.25 | 848.64 | 585.95 |
| Observations | 173,388 | 64,089 | 20,031 | 217,671 |
| Panel C. Employed | | | | |
| Pep Holders x I(Post Aug. 2018) | -0.000** (0.000) [0.016] | 0.000 (0.000) [0.792] | -0.000 (0.000) [0.062] | -0.000 (0.000) [0.844] |
| R-squared | 0.012 | 0.003 | 0.007 | 0.000 |
| F-Test | 792.59 | 823.84 | 850.4 | 572.34 |
| Observations | 178,810 | 62,658 | 19,642 | 214,876 |
| Panel D. Labor Force | | | | |
| Pep Holders x I(Post Aug. 2018) | 0.000 (0.000) [0.766] | 0.000 (0.001) [0.856] | 0.001 (0.001) [0.644] | 0.000 (0.001) [0.716] |
| R-squared | 0.149 | 0.157 | 0.138 | 0.111 |
| F-Test | 495.25 | 743.75 | 799.82 | 547.5 |
| Observations | 561,197 | 81,550 | 22,680 | 243,677 |

Notes: We are including the same individuals covariates of Table 1. Clustered standard errors at the municipality-monthly level are reported in parentheses. Cluster-robust wild-bootstrap p-values are presented in square brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.