Electoral Effects of Integrating Forced Migrants:
Evidence from a Southern Country[*]

Sandra V. Rozo[†]ⓡ   Alejandra Quintana[‡]ⓡ   María José Urbina[§]

May 9, 2024

Abstract

How does easing the economic integration of migrants affects voting behaviors? We assess how the regularization of half a million Venezuelan migrants affected the electoral behaviors of Colombians by comparing election results in municipalities with higher and lower program take-up rates. We document negligible impacts on native voting behavior. We also conducted a survey experiment to investigate the lack of voter response. Even after receiving information about the program, voters showed no changes in voting intentions or prosocial views toward migrants. Hence, their indifference did not stem from a lack of awareness about the program.

"Of course I want to help Venezuelan migrants, we are all migrants in some way, you know? I am just worried about the response from my people. The political backlash could be difficult for my party."

(Elected Official in Latin America, March 15, 2021)

I INTRODUCTION

Evidence from a robust group of studies document a strong correlation between the arrival of new migrants and host's electoral support for anti-migrant political parties.[1] Much less is known, however, on the dynamics of host's electoral behaviors *after* migrants have settled in hosting locations. Specifically, an extremely relevant question is how host's electoral behaviors are affected by policies that facilitate the integration of migrants *after settlement*. In fact, policies that facilitate economic integration for settled migrants might have extremely different effects than the immediate arrival of migrants. For example, voters may lack information about regulations concerning migrant integration, may believe those regulations do not affect overall migration inflows, or may support migrants now in the country as long as more migrants do not come because of those regulations.

We examine the electoral effects of the *Programa Especial de Permanencia* (PEP) in Colombia. The PEP was a large scale regularization program offered to approximately half a million Venezuelan forced migrants for up to two years.[2] The PEP visa granted working rights and access to public programs, such as full health and education services, plus access to financial services. It was offered to every Venezuelan in Colombia who registered in a census of irregular migrants between April and June 2018. As documented by Ibáñez et al. (2022) and Urbina et al. (2023), the PEP program profoundly affected the well-being of treated Venezuelan migrants through improvements in their labor income, consumption, and health. For its mere size and the generosity of its benefits, the PEP program is a

---

[1]See, for example, Gerdes and Wadensjö (2008); Otto and Steinhardt (2014); Mendez and Cutillas (2014); Barone et al. (2016); Harmon (2017); Halla et al. (2017); Dustmann et al. (2016). In one notable exception regarding the Global South by Zhou and Grossman (2021), the authors show migration inflows are related to greater support for the incumbent candidate since they elicited significant international aid.

[2]More than 5.6 million refugees have fled Venezuela's economic and humanitarian crises (UNHCR 2023). Colombia is the most common destination of Venezuelan refugees; by mid-2022, it hosted almost 2.5 million such migrants. Moreover, Colombia has committed to policies that promote the rapid social and economic integration of migrants.

great case study to examine the effects of migrant integration policies in host's electoral outcomes.

Our analysis assesses the impacts of the PEP program on electoral turnout, support for left-wing, center, and right-wing political ideologies, and electoral competition in Colombia.[3] We examine the causal effects of the PEP program by comparing mayoral and first-round presidential election results in municipalities with higher and lower PEP take-up rates, before and after the program's rollout in 2018.

We employ municipal data from six mayoral elections for 1,098 of the 1,122 municipalities in Colombia (recorded after the year 2000, when information about the electoral roll became available). Each municipal election is conducted independently and mayors are elected at the local level by plurality rule. As such, the analysis exploits data from more than six thousand individual election points. We also examine the effects of the PEP program on first-round presidential elections. Presidents are elected nationally in the first round if they receive fifty percent of the votes, plus one. Hence, the presidential election analysis includes six individual election points.

Our analysis controls for municipal and election-year fixed effects, a rich set of baseline municipal characteristics, their interactions with election-year trends, and department election-year trends. The data supports the validity of the empirical strategy as we observe parallel trends for mayoral and presidential elections in municipalities with higher and lower PEP program take-up rates before the program's onset in 2018. In line with new developments in the difference-in-difference methodology, we demonstrate that our results hold even amid potential violations of the parallel trend assumption and are independent of the algorithm and functional form used.

We document negligible effects of the PEP program on all the outcomes for mayoral and presidential elections. The results do not stem from low precision since the coefficients are close to zero. In fact, we establish the same results for the impacts of a similar but larger program called the *Estatuto Temporal de Protección para Migrantes Venezolanos* (ETPV). The

---

[3]We classified this following the methodology proposed by Fergusson et al. (2020) and employed in Rozo and Vargas (2021).

ETPV is a scaled-up version of the PEP that allowed every Venezuelan migrant who arrived in Colombia before January 31, 2021 to apply for a 10-year permit and receive the same rights as did the PEP. We evaluate the electoral impacts of the ETPV using an equivalent difference-in-difference methodology that exploits the variation in the location of applicants (by department, the only publicly available data) and time variation induced by the program's inception. Despite the large scale of this program (six times bigger than the PEP and representing four percent of the Colombian population), we show that it also had negligible effects on voting behaviors.

Although Colombian voters showed no response to migrant regularization programs, they reacted strongly to Venezuelan migration inflows, as reported in Rozo and Vargas (2021). In fact, the authors conclude that larger Venezuelan inflows increased voter turnout and shifted votes from left-wing to right-wing political ideologies.[4] We update and replicate their estimates with our data and confirm the results.[5] We also establish that even after employing the same cross-sectional variation as Rozo and Vargas (2021), the PEP program had no effect on voting behaviors.

Why would voters respond strongly to migration inflows but be indifferent to the PEP program? First, they may *lack information*: perhaps they are not informed about policies that regulate the labor rights of migrants. In fact, "residence and work permits granted by the Colombian government to Venezuelan refugees" are internet search terms Colombians do not often use. Moreover, mainstream newspapers in Colombia did not report on or heavily scrutinize the PEP program.[6] Under this scenario, voters might react differently if they were fully aware of the PEP. The second possibility is that voters are *indifferent to*

---

[4]These effects are predominantly driven by voter concerns about the economic effects of migrants as well as by a novel channel we call *strategic electoral misinformation*, whereby political parties make a migratory shock salient to voters to demonize the political agenda of rivals.

[5]Importantly, although early settlements of Venezuelan migrants and the concentration of PEP applicants are correlated, the correlation is low. Moreover, to evaluate the impacts of the PEP program, we compare electoral outcomes before and after 2018. In contrast, Rozo and Vargas (2021) evaluate the impacts of changes in annual migration inflows to Colombia.

[6]This is corroborated by the findings of Santamaria (2022), who exploits geographical variation in the internet search intensity of these keywords to identify where migrants settled in Colombia. We were only able to find 120 news articles related to the PEP program by the major Colombian news outlets in 2018. All of these articles were purely informative and had no negative messages on the program's potential impacts (see Appendix E).

arrived migrants. This will occur if the effect of immigration on voting behaviors is predominantly driven by voter concern about the overall economic impact of migrants when they arrive or about future inflows, but not by policies once they are in the country. This could be the case to the extent voters believe that policies such as the PEP program will not affect future migration inflows overall. This scenario is supported by the fact that inflows of Venezuelans to Colombia decelerated after 2018 (Figure A.1).

We test the validity of these channels through a survey experiment involving 1,040 Colombians between October and December of 2022. First, we informed the participants about the PEP program and its benefits. We then collected information on their attitudes towards migrants (including a list experiment and a dictator game to measure altruism), voting intentions, political views on migrants, and general knowledge about the PEP program. The experiment enabled us to measure causally how prosocial behaviors and voting intentions change when individuals have information about the PEP program. We also measured the amount of information the control group had about the PEP. Our results suggest that approximately half of the average adults in Colombia are informed about the PEP program. In addition, when participants received information about the program, no changes emerged in their prosocial behaviors toward migrants or their voting intentions. As such, we conclude that *lack of information* among voters does not explain their lack of response to the PEP. Instead, this may relate more to their *indifference* to policies that support migrants after their arrival.

The results are in-line with findings from previous work (Bahar et al., 2021, 2022; Ibáñez et al., 2022; Bahar et al., 2023), which documented that the PEP program did not prompt negative effects on labor or crime outcomes for native Colombians (Bahar et al., 2021, 2022). In fact, the program only induced a small change in the formalization rates of migrants (close to 10p.p), with most migrants remaining in the informal sector but improving their access to public programs and labor conditions (Ibáñez et al., 2022). Consequently, our paper shows that in developing countries with large informality, migration reforms have negligible effects on native's electoral behaviors because most migrants who are eligible for this reforms remain in the informal sector. As such, our results are most

relevant for countries that host forced migrants in the Global South, which corresponds to the lion's share of forced migration (around 85% of forcibly displaced populations live in the global south). Our findings suggest that conditional on having large informal sectors and controlled inflows of migrants, native's political behaviors are unaffected by policies that facilitate the economic integration of migrants who are already in their countries.

Contribution to the literature: This paper contributes to studies examining the political effects of immigration on host economies. With few exceptions in the Global South (Rozo and Vargas 2021, Zhou and Grossman 2021, Bedasso and Pascal 2020, Altindag and Kaushal 2020), most work evaluates the role of migration inflows in shaping election outcomes in the Global North.[7] The main results of this body of work suggest that more exposure to immigration flows is correlated with greater support for anti-immigration parties and less support among natives for redistributive policies.[8] We contribute to these strands by examining the electoral effects of policies that facilitate the economic integration of migrants.

This paper also adds to the literature concerning the impacts of migration reforms. Most studies in this area have examined the impacts of amnesties on labor markets in the Global North (Cobb-Clark et al. 1995, Kaushal 2006, Amuedo-Dorantes et al. 2007, Amuedo-Dorantes and Bansak 2011, Chassamboulli and Peri 2015, Amuedo-Dorantes and Antman 2017, Devillanova et al. 2018, Monras et al. 2018, Hainmueller et al. 2019), with others on the impacts of amnesties on crime behaviors (Baker 2015, Mastrobuoni and Pinotti 2015, Pinotti 2017) and a few on the impacts of amnesties in the Global South (Fallah et al. 2019). The work most closely related to ours considers the implications of the PEP for

---

[7]See Schaub et al. (2021), Otto and Steinhardt (2014), and Hennig (2021) for Germany; Schaub et al. (2021) and Otto and Steinhardt (2014) for Italy; Barone et al. (2016) for Austria; Brunner and Kuhn (2018) for Switzerland; Edo et al. (2019) for France; Dustmann et al. (2019) and Gerdes and Wadensjö (2008) for Denmark; Hangartner et al. (2019) for Greece; and Gimpel (2014) and Mayda et al. (2016) for the United States.

[8]See Alesina and Tabellini (2021), Mayda et al. (2016), and Otto and Steinhardt (2014) for seminal examples. The main mechanisms highlighted by the literature are the economic circumstances conditioning voters (Tomberg et al. 2021, Roupakias and Chletsos 2020, Edo et al. 2019, Barone et al. 2016, Halla et al. 2017, Hainmueller and Hopkins 2014), cultural values (Hainmueller and Hiscox 2007, Goldstein and Peters 2014), cultural differences between migrants and natives (Hainmueller and Hiscox 2007, Bansak et al. 2016, Bursztyn et al. 2021, Tabellini 2019, Alesina et al. 2019, Alrababa'h et al. 2021), and sociotropic explanations (Hainmueller and Hopkins 2015, Hainmueller et al. 2015b).

Colombian hosting communities, including impacts on labor markets (Bahar et al. 2021), crime (Bahar et al. 2022), firm development (Bahar et al. 2023), inequality (Lombardo et al. 2021), and migrant well-being (Ibáñez et al. 2022, Urbina et al. 2023). Our contribution relative to this work is the novel evaluation of the electoral implications of this type of program.

Our work also contributes to the relatively new studies on how humanitarian interventions affect voting behaviors and attitudes towards migrants. For example, Baseler et al. (2021) and Zhou et al. (2023) examine how aid influences attitudes towards refugees in Uganda. Baseler et al. (2021) find that grants tagged to aid sharing significantly increased support for inclusive policies, including the right of refugees to work and the immigration of additional refugees. However, Zhou et al. (2023) find no evidence that refugee presence has been associated with more negative (or positive) attitudes towards migrants or migration policy. On their behalf, Hainmueller et al. (2015a) show that naturalization caused long-lasting improvements in political integration in Switzerland: immigrants became likely to vote and attained considerably higher levels of political efficacy and knowledge, and some municipalities used referendums to determine naturalization permissions. Our study contributes to their work by analyzing how large regularization programs with no direct benefits to hosts (such as cash tied to aid) affect native voting behavior in the short run.

Finally, this paper also speaks to research on how low-cost interventions can affect attitudes towards migrants and preferences for migration policy. Some of these studies have shown high levels of misinformation among respondents in developed countries regarding the size and characteristics of the immigrant population (Alesina et al. 2018, Grigorieff et al. 2020) and have concluded that information-provision interventions can correct such misperceptions (Alesina et al. 2018, Hopkins et al. 2019). Nevertheless, the impacts of information effectiveness on policy preferences and behaviors are mixed (Hopkins et al. 2019, Haaland and Roth 2020, Alesina et al. 2018, Williamson 2020, Grigorieff et al. 2020.). We offer new data on how information-provision interventions affect native voting behavior, social capital, and attitudes toward migrants.

II CONTEXT: THE PEP PROGRAM

By mid-2022, more than 5.6 million Venezuelans had fled the humanitarian crisis in their country. Approximately 2.5 million of them had settled in Colombia, currently the primary recipient of Venezuelan migrants (UNHCR 2023). This number represents a shock equivalent to about three percent of Colombia's total population (Figure A.1).[9] Despite the size of the migration flows, the Colombian government has been generous, granting these migrants free mobility and opportunities to regularize their status. One of the largest initiatives was the PEP program in 2018.

The RAMV census: between April and June of 2018, the Colombian government undertook a countrywide survey to count the number of irregular (undocumented) migrants. It was known as the RAMV, *Registro Administrativo de Migrantes Venezolanos*. Colombian authorities administered the survey at 1,109 different stations in 441 of the 1,122 municipalities. The registration points 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, migrants had to go to the registration point with proof of Venezuelan citizenship through official identification documentation. Registration was voluntary and was only advertised as a statistic exercise. The RAMV identified 442,462 undocumented Venezuelan migrants.

The PEP program: In July 2018, just days before leaving office, President Juan Manuel Santos unexpectedly offered a regularization program to everyone who had registered in the survey. The PEP eligibility requirements were (i) registration in the survey, (ii) physical presence in Colombia at the time the decree was issued, and (iii) lack of a criminal record or a deportation order. The processing and issuance of a PEP was free, voluntary, and could only be done online. Sixty-four percent of all undocumented migrants who had registered in the RAMV received a PEP visa. Table A.2 describes the main sociodemographic characteristics of the Venezuelans who applied and did not apply for the PEP program. The data shows that PEP migrants are older, more educated, more likely to be

---

[9]Compared to the Colombian population, Venezuelan migrants are younger, more educated, and have lower employment rates (see Table A.1).

employed in the informal sector, and less connected to networks in Colombia, relative to non-PEP applicants.

PEP benefits: The PEP visa granted Venezuelan migrants the right to work, the possibility of being scored by SISBEN, [10] access to financial services, and a document to prove regular status in Colombia and thus avoid deportation. Table A.3 depicts the benefits granted by the PEP compared to the rights of all migrants in Colombia.

III   DATA

The main empirical analysis employs municipal-election panel data between 2000 and 2022. The sample is restricted to this period since municipal-level voting roll registries are available from 2000. We also did a survey experiment to clarify the mechanisms underlying our main results. Details on the survey are in section VI.

PEP take-up. The number of individuals who applied for the PEP program is available by municipality. Colombian migration authorities provided the data, which is illustrated in Figure 1.

Elections. We use data from six mayoral municipal elections (2000, 2003, 2007, 2011, 2015, and 2019). Each municipal election corresponds to an independent race where the elected official was chosen based on plurality rule. We also use data for the six most recent presidential elections (2002, 2006, 2010, 2014, 2018, and 2022). We only employ data for the first-round elections to maintain consistency across election years. The election data comes from the Colombian electoral agency. We use the information to examine the effects of the PEP program on (i) election turnout (measured by the individuals who voted as a share of the electoral roll); (ii) support for left-wing, center, or right-wing political ideologies; and (iii) electoral competition.[11] Appendix B describes in detail the steps we followed to create these variables.

Descriptive statistics for all variables used in the main specifications are in Table C.1.

---

[10]The score used to award anti-poverty social programs in Colombia.

[11]Calculated following Chacón et al. (2006) as: $1 - (\%1^{\text{st}}Candidate - \%2^{\text{nd}}Candidate)$. When the margin of victory among candidates is close to zero, the elections were competitive and the variable takes a value close to one.

There is significant variation in the turnout of the mayoral elections (the mean is 65 percent and the standard deviation is 11 percent). The dominance of a center-oriented political ideology is clear: on average, 66 percent of voters supported center parties, while 16 percent supported right-wing parties and only 6 percent supported left-wing candidates. On average, these mayoral elections were competitive (0.63).

Furthermore, Figures C.1 and C.2 illustrate the geographic distribution of outcomes in the mayoral and presidential elections. The figure illustrates several trends of political behavior in Colombia. First, there is a large geographical variation among political outcomes across municipalities. Second, the majority of political participation in Colombia is concentrated in the center and northern areas of the country. Finally, there is more support for right-wing political ideologies in the eastern and center regions of Colombia, whereas left-wing ideologies attract greater support in the west and south.

Other municipal controls. We also use several municipal characteristics measured before the 2018 implementation of PEP and interact them with election-year indicator variables to flexibly account for non-parametric trends in observable variables that may bias the main results. Descriptive statistics for all controls are illustrated in Table C.2. The data comes from multiple sources listed in the footnote of Table C.2.

IV EMPIRICAL STRATEGY

PEP program impacts cannot be estimated by comparing electoral outcomes in municipalities with different program take-up. This is because migrants "vote with their feet" and consider the characteristics of each place when deciding where to reside. For example, Venezuelan migrants may choose to locate in areas that are more prosperous, less violent, or where locals are more welcoming. As such, a simple mean comparison of areas with different program take-up rates may be biased. For this reason, we employ a difference-in-difference methodology. The main specification uses municipal election-year variation that exploits the unexpected timing of PEP implementation and the municipal location of PEP holders. Specifically, the following specification was used:

$$Y_{mdt} = \alpha[\text{PEP}_{md} \times I(\text{Post 2018})_t] + \sum_{c \epsilon Z}[c_d \times \psi_y] + \gamma_m + \gamma_t + \gamma_{dt} + \epsilon_{mdt} \qquad (1)$$

where m stands for municipality (the equivalent of a county in the United States), d stands for the department (the equivalent of a state in the United States), and t stands for election-year variation. Y represents the electoral outcomes of interest, *PEP* corresponds to the standardized values of the number of PEP holders as a share of population, and *I(Post 2018)* is an indicator variable that takes the value of one after 2018. C is a rich set of pre-determined municipal characteristics measured before the PEP program launched. We included interactions of these variables and year indicator variables in all estimates to flexibly account for potential differential non-parametric trends in a number of municipal characteristics observed prior to each migrant's regularization. The variables included as baseline controls in Z include (i) *conflict and violence-related variables* such as homicide rates and number of robberies; (ii) *public-finance-related variables* including revenue, expenditures, capital expenditures, and central government transfers to the municipalities (as a total and by type); (iii) *poverty and inequality* measured by the number of people subsidized by the health system and the percentage of the population living in rural areas; (iv) *economic growth* approximated by night light density; and (v) *previously regularized population* measured as the number of applicants to past smaller regularization programs. These regularization programs only targeted highly educated migrants with passports in Colombia. These variables are listed in Table C.2. Equation 1 also includes municipal (γ_m), election-year (γ_t), and department election-year (γ_{dt}) fixed effects. Finally, standard errors were clustered at the municipal level to account for geographic serial correlation.

IV. A Internal validity

Considering that there is no staggered treatment and all municipalities are treated at the same time, we begin by illustrating the validity of the parallel trend assumption for different specifications of the treatment variable. We explore potential violations of this assumption in the next section.

The canonical difference-in-difference estimates should be valid as long as municipalities

with different PEP take-up rates experienced parallel dynamic behaviors in the outcomes examined before the implementation of the program in 2018 (after controlling for the baseline covariates). We examine the validity of the parallel trend assumption through an event study in Figures 2, 3, 4, and 5. The figures illustrate the coefficients of an event study that excludes the elections closest to 2018 for the five electoral outcomes studied. We estimate the event study for (i) the continuous treatment variable (PEP), defined as the standardized values of the variable PEP holders, and (ii) the discrete treatment variable equal to one if the municipality had positive program take-up. Generally, the figures suggest there are no differential trends in the outcome evolution before the implementation of the PEP program. In any case, we formally tested the sensitivity of our estimates to potential violations of the parallel trend assumption (see details in the next section). Our results are robust to all the tests employed.

V ELECTORAL IMPACTS OF THE PEP PROGRAM

Table 1 displays the coefficient estimates of equation (1) using different versions of the treatment variable for the mayoral and presidential elections. Independent of the type of election and the definition of the treatment variable, we do not distinguish significant effects of the PEP program on any electoral outcome we examine. Moreover, we also evaluate potential heterogeneous effects of the PEP program in municipalities with different conflict incidence and state presence, but we do not identify any effects statistically different from zero.[12]

V. A Robustness tests

We evaluated the robustness of our main findings to a series of empirical exercises outlined below. Our main results remain unchanged.

Algorithm choice: We inspected overall time trends of municipalities with and without PEP program take-up using the raw data in Figures D.1 and D.2. Although we generally see parallel trends in the raw data, a few exceptions prompted us to test for potential static differences in the municipal baseline outcomes (illustrated in Table D.1). Since we found

---

[12]For reasons of brevity, the heterogeneous effects analysis is not presented in the tables but is available upon request.

statistical differences in some of these static baseline outcomes, we tested the validity of our main results to a matching difference-in-difference and produced similar results (see Table D.2).[13] We also implemented the non-inferiority test that allows for the existence of potential pre-treatment linear trends between the treatment and control groups (as proposed by Bilinski and Hatfield, 2018) in Table D.3; the results remain unchanged.

Electoral response to larger migrant regularization programs: In Table D.4, we further test the robustness of our main results by exploring the effects of a similar but larger regularization program offered by the Colombian government in 2021. The *Estatuto Temporal de Permanencia* scaled PEP benefits to any Venezuelan migrant who arrived in Colombia before January 2021. The program grants the same benefits as PEP but is more than six times larger. We employ a similar empirical strategy that exploits geographic variation in the program take-up rate by department (the only information available) and time variation in the onset of the program after 2021. The results confirm that voters had no reaction to migrant regularization programs in Colombia. Moreover, this second finding suggests that the negligible effects observed for the PEP program were not driven by power issues.

Do voter reactions to migration flows differ?: So far we have found negligible effects of the PEP program on the voting behaviors of Colombian natives. Yet, Rozo and Vargas (2021) have shown that Venezuelan migration inflows change voter behaviors. The authors exploit geographical variation in the early settlements of migrants (before the onset of the crisis) and annual variation in the aggregate migration flows. They demonstrate that larger migration shocks increase voter turnout and shift votes from left-wing to right-wing political ideologies. We replicate Rozo and Vargas (2021)'s estimates, adding data for the last elections (2019 for mayoral elections and 2022 for presidential elections), and observe similar effects. Particularly, we see that larger migration inflows translate into higher voter turnout and a shift of votes from left-wing to right-wing ideologies (see Table D.5). We illustrate in Figure D.5 that both estimates use different sources of cross-sectional variation. Furthermore, we confirm that even when using the cross-sectional

---

[13]We did this by predicting the probability of PEP take-up based on the significant covariates and restricting the sample to the common support in the treatment and control groups based on the overlap of those probabilities (see Figures D.3 and D.4).

variation from Rozo and Vargas (2021), we do not find significant effects of the PEP program (see Table D.6).

Are the impacts in municipalities with lower/higher take-up? We also explore whether the program induced different impacts in municipalities that had higher or lower program take-up. For this purpose, we divide the municipalities in the sample in three terciles for program take-up and a control group with no take-up. We first show that the parallel trend assumption is satisfied for municipalities in tercile groups 1, 2, or 3 relative to the control group (Figures D.6-D.8). We also confirm that there are negligible effects of the program for all outcomes and groups in Table D.7, except for the case of political competition where we document a positive impact for municipalities in Tercile 3.

All in all, our results suggest that Colombian voters did not change their voting behaviors in response to migrant regularization programs. The effects observed may be explained by *lack of information* about the PEP program among voters or by their *indifference* to the program. We explore the validity of these channels in the next section.

VI ARE COLOMBIANS UNINFORMED ABOUT THE PEP PROGRAM?

We designed a survey experiment to evaluate whether Colombian natives: (i) are informed about the PEP program and (ii) conditional on receiving information on the PEP program, change their prosocial views and voting behaviors.

For this purpose, we conducted an in-person survey experiment in Bogotá, the Colombian city with the highest number of Venezuelan migrants.[14] The randomized trial offered treated individuals information about the PEP program in order to later collect measures on their prosocial views and voting intentions. Particularly, everyone read the following information (translated into English here): *"4.6 million Venezuelans have been forcibly displaced and 4 of every 10 live in Colombia (1.7 million)."* In addition, the treatment group read the following statement (translated into English here):

"281,000 irregular Venezuelan migrants have been legalized through the Permiso Especial de Permanencia, which grants them a work permit, access to social programs (such as a subsidized health

---

[14]According to the last population census (2018), 166,566 Venezuelans live in Bogotá; this corresponds to 2.32 percent of its population.

regime), and access to financial services."

Figure G.1 illustrates how the information was presented. The survey was executed between October and December of 2022. The sample size was 1,040 individuals and included Colombian residents who were older than 21 years old and living in Bogotá. Appendix G describes the survey's structure and design.

Table G.1 confirms that the randomization was successful as there are no significant differences in any of the 15 sociodemographic variables we collected. Moreover, the test of joint significance also confirms the experiment's success in maintaining balance across the treatment and control groups.

VI. A Are Colombians informed about the PEP program?

Table 2 examines the control group's knowledge about the PEP program. Importantly, we asked these questions at the end of the survey to prevent priming the control group. We found that roughly half of the control group knew about the PEP program and understood who was eligible to apply (44.6 and 52.1 percent, respectively). When characterizing the individuals with correct information on the PEP program (panel B), we found they were mature in age, employed, educated, interested in politics, and active voters in the last elections.

VI. B Once informed, do natives change their voting intentions?

To evaluate the effects of the program, we estimate the following specification:

$$Y_i = \alpha_0 + \alpha_1 T_i + X_i + \epsilon_i \tag{2}$$

where i represents the individual and Y denotes the primary outcomes of interest as pre-registered. These outcomes include: (i) voting intentions; (ii) self-reported measures of social capital (positive reciprocity, negative reciprocity, altruism, and trust);[15] (iii) experimental measures of altruism toward migrants through a dictator game; and (iv) measures of political attitudes toward migrants. Finally, X includes controls for the stratification

---

[15]All the questions have been validated in Colombia and taken from Gallup surveys as reported in Falk et al. (2018).

variables.

Tables 3, 4, 5, and 6 illustrate the effects of the program on the primary outcomes of interest. They show that even after treated participants received information about the program, there were no changes in prosocial behaviors or voting intentions in any of our outcomes. Although the coefficients are small, they are generally bigger than two percent. This suggests that the lack of significance is not due to statistical imprecision since our sample size was designed to identify minimum effects of at least two percent.

Table G.6 tests our results for social desirability. In other words, respondents may have guessed our hypothesis and changed their responses to match what they thought we wanted to hear. Particularly, the table reports the results of a list experiment in which we gave everyone in the survey a list of things they could dislike. The list includes individuals who mistreat others, are poor, and are of a different religion. In the experiment, we randomized the whole sample again. Next, the treatment group for the list experiment was given an additional choice: "Venezuelan migrants." We then asked respondents to tell us the number of things on that list they disliked but not the specific things they disliked. We tested for the interaction effects of both treatments on the number of things respondents reported they disliked (see Table G.6). These results confirm that our findings were not biased by social desirability as there are no significant differences between groups. Furthermore, we also tested for heterogeneous effects of the program among individuals with higher social desirability as measured by the 13-item, Marlowe-Crowne social desirability scale (see Crowne and Marlowe (1960) for details on the scale). Tables G.2–G.5 show no significant heterogeneous effects of the program among respondents who scored higher.[16] Consequently, we conclude that the lack of response to the PEP program among voters in Colombia is not due to *lack of information* about it.

VII WHAT OTHER FACTORS MAY BE DRIVING THE RESULTS?

So far we have shown that there was a negligible electoral response from Colombian natives toward the PEP program. Moreover, this results are not explained by a lack of

---

[16]We also evaluated them for heterogeneous effects of the treatment on the stratification variables but found no effects on any of the groups of interest. The results are available upon request.

awareness about the program. In this section, we combine our results with previous research on the impacts of the PEP program to offer a plausible explanation. Particularly, our main results are consistent with findings from previous work on the impacts of the PEP program in labor markets, crime behaviors, and migrant's welfare (Bahar et al., 2021, 2022; Ibáñez et al., 2022; Bahar et al., 2023). In contrast to what is typically documented for the global north, these studies showed that the PEP program did not induced negative effects on labor or crime outcomes for native Colombians in the short term (Bahar et al., 2021, 2022). This occurred since the program only induced a small change in the formalization rates of migrants (close to 10p.p), with most migrants remaining in the informal sector and in their same jobs but improving their access to public programs and labor conditions in the same jobs (Ibáñez et al., 2022). Consequently, our paper shows that in developing countries with large informality, migration reforms most likely have negligible effects on native's electoral behaviors because most migrants who are eligible for this reforms remain in the informal sector. All in all, our findings suggest that conditional on having a large informal sector and controlled inflows of migrants, native's political behaviors are unaffected by policies that ease the economic integration of migrants.

VIII CONCLUDING REMARKS

This paper provides evidence that a large migrant regularization program that granted job permits and social benefits to half a million undocumented Venezuelan migrants did not change the voting behavior of Colombian natives. This lack of voter response does not stem from *lack of information* about the PEP program.

We speculate that the lack of response is because Venezuelan migration inflows have stalled. We suggest that while Colombian natives worry about actual migration inflows, they are not concerned with the policies regulating those migrants post-arrival once the inflows are controlled.

The economic integration of forced migrants is politically sensitive. Although natives may sympathize with this difficult situation, they may also worry about its effects on their community. For example, voters may fret about job displacement, crime, and the fiscal consequences of generous financial support for forced migrants. We provide rigorous

evidence that one of the largest regularization programs offered in a developing country in recent history had no effects on native voting behavior. Although previous research showed that massive forced migration flows from Venezuela induced dramatic responses in Colombia, our paper establishes that the same is not true of public policies that ease the integration of migrants after their arrival.

References

Alesina, Alberto, Armando Miano, and Stefanie Stantcheva (2018) "Immigration and redistribution,"Technical report, National Bureau of Economic Research.

Alesina, Alberto, Elie Murard, and Hillel Rapoport (2019) "Immigration and Preferences for Redistribution in Europe," Working Papers 2019-15, CEPII research center.

Alesina, Alberto and Marco Tabellini (2021) "The Political Effects of Immigration: Culture or Economics?," IZA Discussion Papers 14354, Institute of Labor Economics (IZA).

Alrababa'h, Ala', Andrea Dillon, Scott Williamson, Jens Hainmueller, Dominik Hangartner, and Jeremy Weinstein (2021) "Attitudes Toward Migrants in a Highly Impacted Economy: Evidence From the Syrian Refugee Crisis in Jordan," *Comparative Political Studies*, 54 (1), 33–76.

Altindag, Onur and Neeraj Kaushal (2020) "Do Refugees Impact Voting Behavior in the Host Country? Evidence from Syrian Refugee Inflows in Turkey," *Public Choice* (10849).

Amuedo-Dorantes, Catalina and Francisca Antman (2017) "Schooling and Labor Market Effects of Temporary Authorization: Evidence from DACA," *Journal of Population Economics*, 30 (1), 339–373.

Amuedo-Dorantes, Catalina and Cynthia Bansak (2011) "The Impact of Amnesty on Labor Market Outcomes: A Panel Study using the Legalized Population Survey," *Industrial Relations: A Journal of Economy and Society*, 50 (3), 443–471.

Amuedo-Dorantes, Catalina, Cynthia Bansak, and Steven Raphael (2007) "Gender Differences in the Labor Market: Impact of IRCA's Amnesty Provisions," *The American Economic Review*, 97 (2), 412–416.

Bahar, Dany, Bo Cowgill, and Jorge Guzman (2023) "Refugee Entrepreneurship: The Case of Venezuelans in Colombia," *American Economic Association Papers Proceedings. Forthcoming*.

Bahar, Dany, Ana María Ibáñez, and Sandra V. Rozo (2021) "Give me your Tired and your

Poor: Impact of a Large-scale Amnesty Program for Undocumented Refugees," *Journal of Development Economics*, 151, 102652.

———— (2022) "Empowering Migrant Women: Impacts of Amnesties on Crime Reports," *World Bank*.

Baker, Scott R. (2015) "Effects of Immigrant Legalization on Crime," *American Economic Review*, 105 (5), 210–13.

Bansak, Kirk, Jens Hainmueller, and Dominik Hangartner (2016) "How economic, humanitarian, and religious concerns shape European attitudes toward asylum seekers," *Science*, 354.

Barone, Guglielmo, Alessio D'Ignazio, Guido de Blasio, and Paolo Naticchioni (2016) "Mr. Rossi, Mr. Hu and politics. The role of immigration in shaping natives' voting behavior," *Journal of Public Economics*, 136, 1–13.

Baseler, Travis, Thomas Ginn, Robert Hakiza, Helidah Ogude, and Olivia Woldemikael (2021) "Can Aid Change Attitudes toward Refugees? Experimental Evidence from Microentrepreneurs in Urban Uganda."

Bedasso, Biniam and Jaupart Pascal (2020) "South-South migration and elections: evidence from post-apartheid South Africa," *IZA Journal of Development and Migration*, 11 (1), 1–47.

Bilinski, Alyssa and Laura A. Hatfield (2018) "Nothing to see here? Non-inferiority approaches to parallel trends and other model assumptions," *arXiv e-prints*.

Brunner, Beatrice and Andreas Kuhn (2018) "Immigration, Cultural Distance and Natives' Attitudes Towards Immigrants: Evidence from Swiss Voting Results," *Kyklos*, 7, 28–58.

Bursztyn, Leonardo, Thomas Chaney, Tarek Alexander Hassan, and Aakash Rao (2021) "The Immigrant Next Door: Exposure, Prejudice, and Altruism," Working Papers 2021-16, Becker Friedman Institute for Research In Economics.

Chacón, Mario, James Robinson, and Ragnar Torvik (2006) "When is Democracy an Equi-

librium?: Theory and Evidence from Colombia's La Violencia," *Journal of Conflict Resolution*, 55.

Chassamboulli, Andri and Giovanni Peri (2015) "The Labor Market Effects of Reducing the Number of Illegal Immigrants," *Review of Economic Dynamics*, 18 (4), 792–821.

Cobb-Clark, Deborah A., Clinton R. Shiells, and B. Lindsay Lowell (1995) "Immigration Reform: The Effects of Employer Sanctions and Legalization on Wages," *Journal of Labor Economics*, 13 (3), 472–498.

Crowne, Douglas and David Marlowe (1960) "A New Scale of Social Desirability Independent of Psychopathology," *Journal of consulting psychology*, 24, 349–54.

Devillanova, Carlo, Francesco Fasani, and Tommaso Frattini (2018) "Employment of Undocumented Immigrants and the Prospect of Legal Status: Evidence from an Amnesty Program," *Industrial and Labor Relations Review*, 71 (4), 853–881.

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

Dustmann, Christian, Kristine Vasiljeva, and Anna Pill Damm (2019) "Refugee Migration and Electoral Outcomes," *Review of Economics Studies*.

Edo, Anthony, Yvonne Giesing, Jonathan Öztunc, and Panu Poutvaara (2019) "Immigration and electoral support for the far-left and the far-right," *European Economic Review*, 115, 99–143.

Falk, Armin, Anke Becker, Thomas Dohmen, Benjamin Enke, David Huffman, and Uwe Sunde (2018) "Global Evidence on Economic Preferences*," *The Quarterly Journal of Economics*, 133 (4), 1645–1692.

Fallah, Belal, Caroline Krafft, and Jackline Wahba (2019) "The impact of refugees on employment and wages in Jordan," *Journal of Development Economics*.

Fergusson, Leopoldo, Pablo Querubin, Nelson A. Ruiz, and Juan F. Vargas (2020) "The Real Winner's Curse," *America Journal of Political Science*, 00, 1–17.

Gerdes, Christer and Eskil Wadensjö (2008) "The Impact of Immigration on Election Outcomes in Danish Municipalities,"Technical Report 3586, Institute of Labor Economics (IZA).

Gimpel, James (2014) "Immigration's Impact on Republican Political Prospects, 1980 to 2012," *Center for Immigration Studies*.

Goldstein, Judith L. and Margaret E. Peters (2014) "Nativism or Economic Threat: Attitudes Toward Immigrants During the Great Recession," *International Interactions*, 40 (3), 376–401.

Grigorieff, Alexis, Christopher Roth, and Diego Ubfal (2020) "Does information change attitudes toward immigrants?" *Demography*, 57 (3), 1117–1143.

Haaland, Ingar and Christopher Roth (2020) "Labor market concerns and support for immigration," *Journal of Public Economics*, 191, 104256.

Hainmueller, Jens, Dominik Hangartner, and Giuseppe Pietrantuono (2015a) "Naturalization Fosters the Long-term Political Integration of Immigrants," *Proceedings of the National Academy of Sciences*, 112 (41), 12651–12656.

Hainmueller, Jens, Dominik Hangartner, and Dalston Ward (2019) "The effect of citizenship on the long-term earnings of marginalized immigrants: Quasi-experimental evidence from Switzerland," *Science Advances*, 5 (12), eaay1610.

Hainmueller, Jens and Michael J. Hiscox (2007) "Educated Preferences: Explaining Attitudes toward Immigration in Europe," *International Organization*, 61 (2), 399–442.

Hainmueller, Jens, Michael J. Hiscox, and Yotam Margalit (2015b) "Do concerns about labor market competition shape attitudes toward immigration? New evidence," *Journal of International Economics*, 97 (1), 193–207.

Hainmueller, Jens and Daniel J. Hopkins (2014) "Public Attitudes Toward Immigration," *Annual Review of Political Science*, 17 (1), 225–249.

———— (2015) "The Hidden American Immigration Consensus: A Conjoint Analysis of Attitudes toward Immigrants," *American Journal of Political Science*, 59 (3), 529–548.

Halla, Martin, Alexander F Wagner, and Josef Zweimüller (2017) "Immigration and Voting for the Far Right," *Journal of the European Economic Association*, 15 (6), 1341–1385.

Hangartner, Dominik, Elias Dinas, Moritz Marbach, Konstantinos Matakos, and Dimitrios Xefteris (2019) "Does Exposure to the Refugee Crisis Make Natives More Hostile?" *American Political Science Review*, 113 (2), 442–455.

Harmon, Nikolaj A (2017) "Immigration, ethnic diversity, and political outcomes: Evidence from Denmark," *The Scandinavian Journal of Economics*.

Hennig, Jakob (2021) "Neighborhood quality and opposition to immigration: Evidence from German refugee shelters," *Journal of Development Economics*, 150, 102604.

Hopkins, Daniel J, John Sides, and Jack Citrin (2019) "The muted consequences of correct information about immigration," *The Journal of Politics*, 81 (1), 315–320.

Ibáñez, Ana María, Andres Moya, María Adelaida Ortega, Sandra V Rozo, and María Jose Urbina (2022) "Life out of the Shadows."

Kaushal, Neeraj (2006) "Amnesty Programs and the Labor Market Outcomes of Undocumented Workers," *Journal of Human resources*, 41 (3), 631–647.

Kling, Jeffrey R, Jeffrey B Liebman, and Lawrence F Katz (2007) "Experimental Analysis of Neighborhood Effects," *Econometrica*, 75, 83–119.

Lombardo, Carlo, Julián Martínez Correa, Leonardo José Peñaloza Pacheco, and Leonardo Carlos Gasparini (2021) "The distributional effect of a massive exodus in Latin America and the role of downgrading and regularization," *Documentos de Trabajo del CEDLAS*.

Mastrobuoni, Giovanni and Paolo Pinotti (2015) "Legal Status and the Criminal Activity of Immigrants," *American Economic Journal: Applied Economics*, 7 (2), 175–206.

Mayda, A.-M., G. Peri, and W. Steingress (2016) "Immigration to the U.S.: A problem for the Republicans or the Democrats?," Working papers 597, Banque de France.

Mendez, Ildefonso and Isabel M Cutillas (2014) "Has immigration affected Spanish presidential elections results?" *Journal of Population Economics*, 27 (1), 135–171.

Monras, Joan, Javier Vázquez-Grenno, and Ferran Elias (2018) "Understanding the Effects of Legalizing Undocumented Immigrants."

Otto, Alkis Henri and Max Friedrich Steinhardt (2014) "Immigration and election outcomes. Evidence from city districts in Hamburg," *Regional Science and Urban Economics*, 45, 67–79.

Pinotti, Paolo (2017) "Clicking on Heaven's Door: The Effect of Immigrant Legalization on Crime," *American Economic Review*, 107 (1), 138–68.

Roupakias, Stelios and Michael Chletsos (2020) ""Immigration and far-right voting: evidence from Greece," *The Annals of Regional Science*.

Rozo, Sandra V. and Juan F. Vargas (2021) "Brothers or invaders? How crisis-driven migrants shape voting behavior," *Journal of Development Economics*, 150, 102636.

Santamaria, Julieth (2022) "When a Stranger Shall Sojourn with Thee': The Impact of the Venezuelan Exodus on Colombian Labor Markets," *Alianza EFI. Working Paper*.

Schaub, Max, Johanna Gereke, and Delia Baldassarri (2021) "Strangers in Hostile Lands: Exposure to Refugees and Right-Wing Support in Germany's Eastern Regions," *Comparative Political Studies*, 54 (3-4), 686–717.

Tabellini, Marco (2019) "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration," *Review of Economic Studies*.

Tomberg, Lukas, Karen Smith Stegen, and Colin Vance (2021) ""The mother of all political problems"? On asylum seekers and elections," *European Journal of Political Economy*, 67, 101981.

UNHCR (2023) "Refugee statistics," https://www.unhcr.org/refugee-statistics/.

Urbina, Maria, Sandra Rozo, Andres Moya, and Ana Ibáñez (2023) "Least Protected, Most

Affected: Impacts of Migration Regularization Programs on Pandemic Resilience," *American Economic Association Papers Proceedings. Forthcoming*.

Williamson, Scott (2020) "Countering misperceptions to reduce prejudice: An experiment on attitudes toward Muslim Americans," *Journal of Experimental Political Science*, 7 (3), 167–178.

Zhou, Yang-Yang and Guy Grossman (2021) "When Refugee Exposure Increases Incumbent Support through Development: Evidence from Uganda," *OSF Preprints. October*, 7.

Zhou, Yang-Yang, Guy Grossman, and Shuning Ge (2023) "Inclusive refugee-hosting can improve local development and prevent public backlash," *World Development*, 166, 106203.

IX FIGURES

Figure 1. Share of PEP holders (% of population)

PEP Holders (% Pop. in 2018)
- 0.00
- 0.01 - 0.10
- 0.11 - 0.80
- 0.81 - 6.00
- 6.01 - 12.40
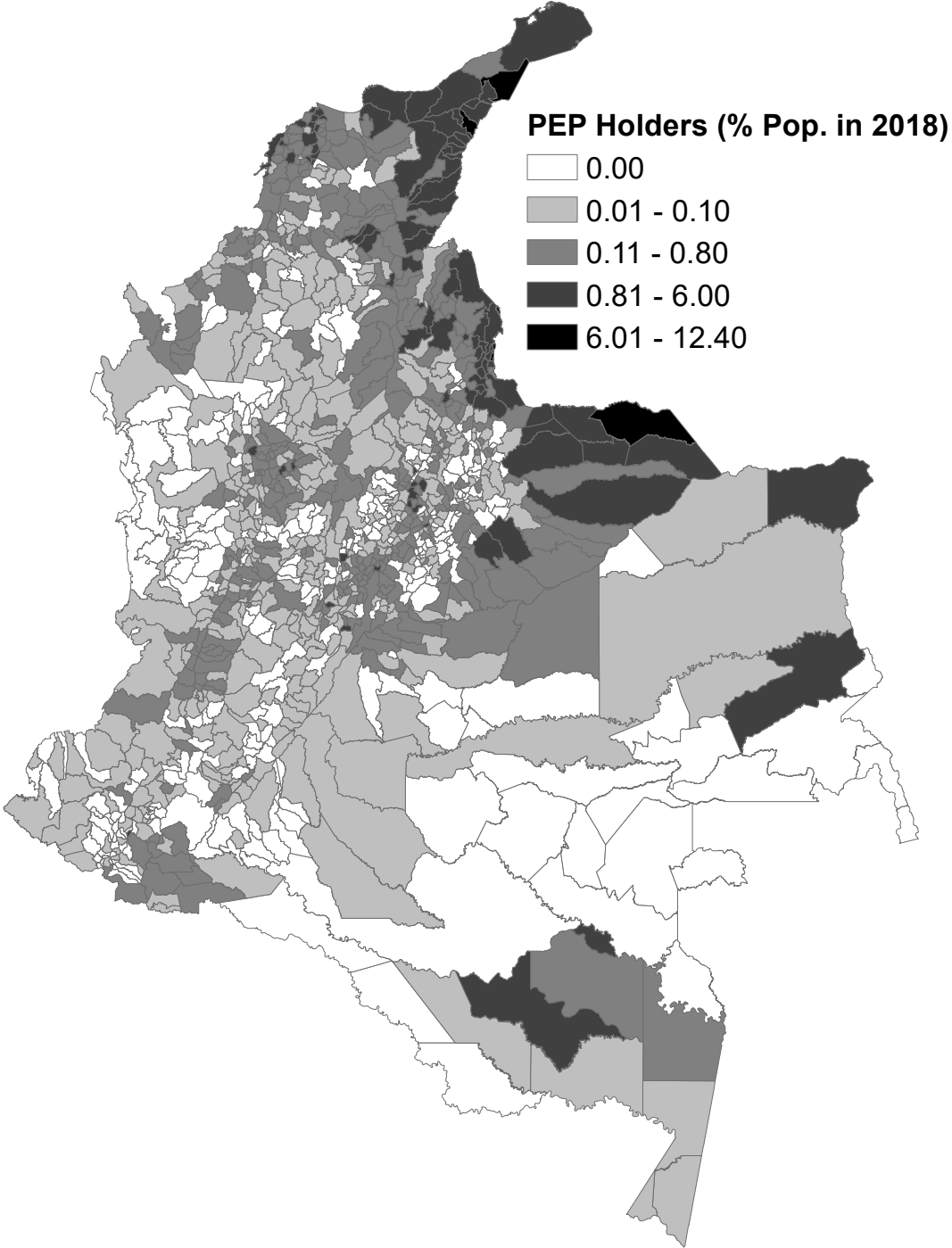
Figure 2. Parallel Trend Assumption – Mayoral Elections
Continuous Treatment Variable

Notes: The figure illustrates the results of an event study estimation. We interacted the standardized values of the variable PEP holders with mayoral electoral years excluding the 2015 election, the last one before 2018, when the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.

Figure 3. Parallel Trend Assumption – Mayoral Elections
Discrete Treatment Variable

Notes: The figure illustrates the results of an event study estimation. We interacted the discrete variable PEP, which takes the value of one in municipalities where there are PEP holders, with mayoral electoral years excluding the 2015 election—the last one before 2018, when the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.

Figure 4. Parallel Trend Assumption – Presidential Elections
Continuous Treatment Variable
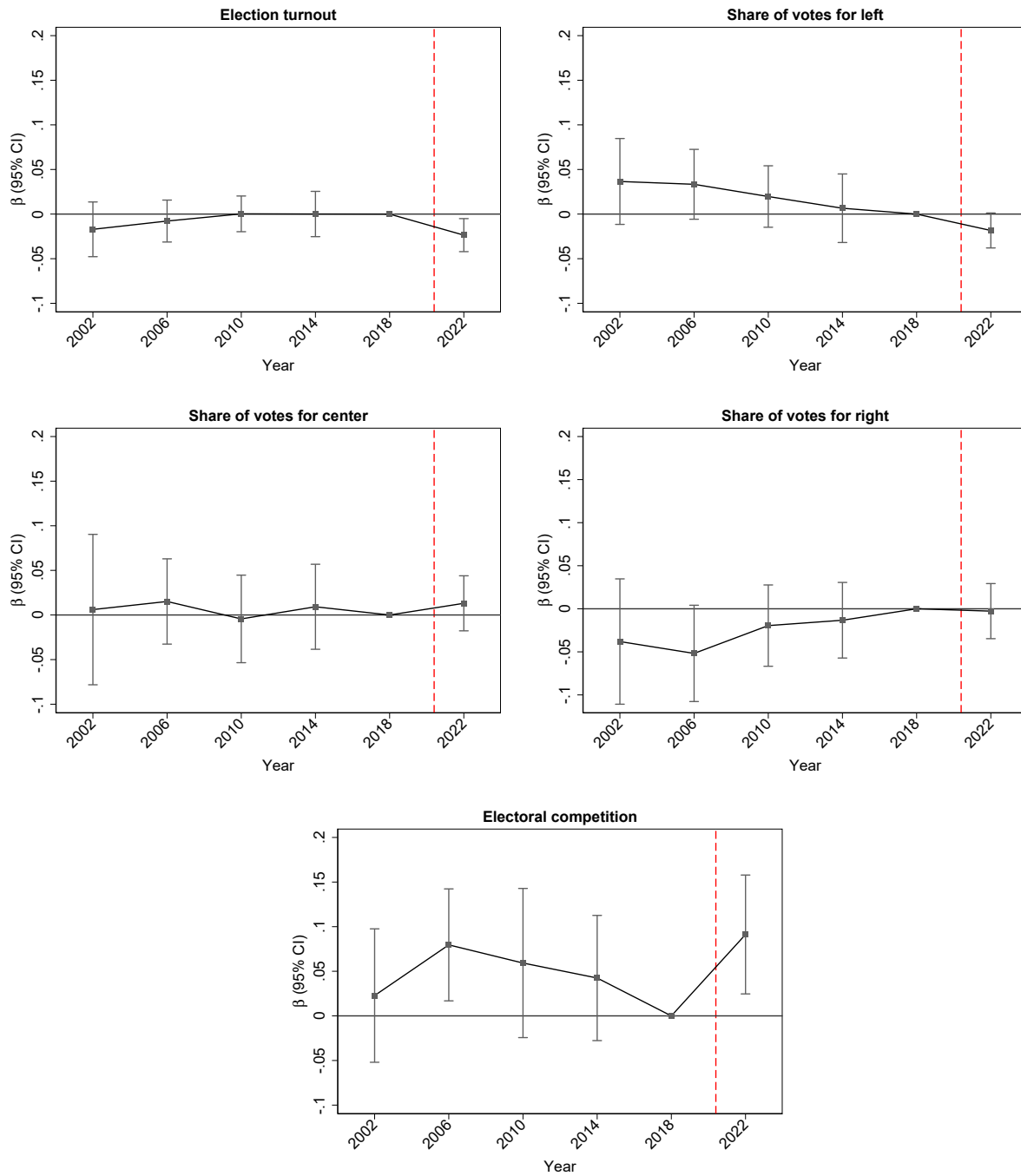
Notes: The figure illustrates the results of an event study estimation. We interacted the standardized values of the variable PEP holders with presidential electoral years excluding the election of 2018, the year the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.
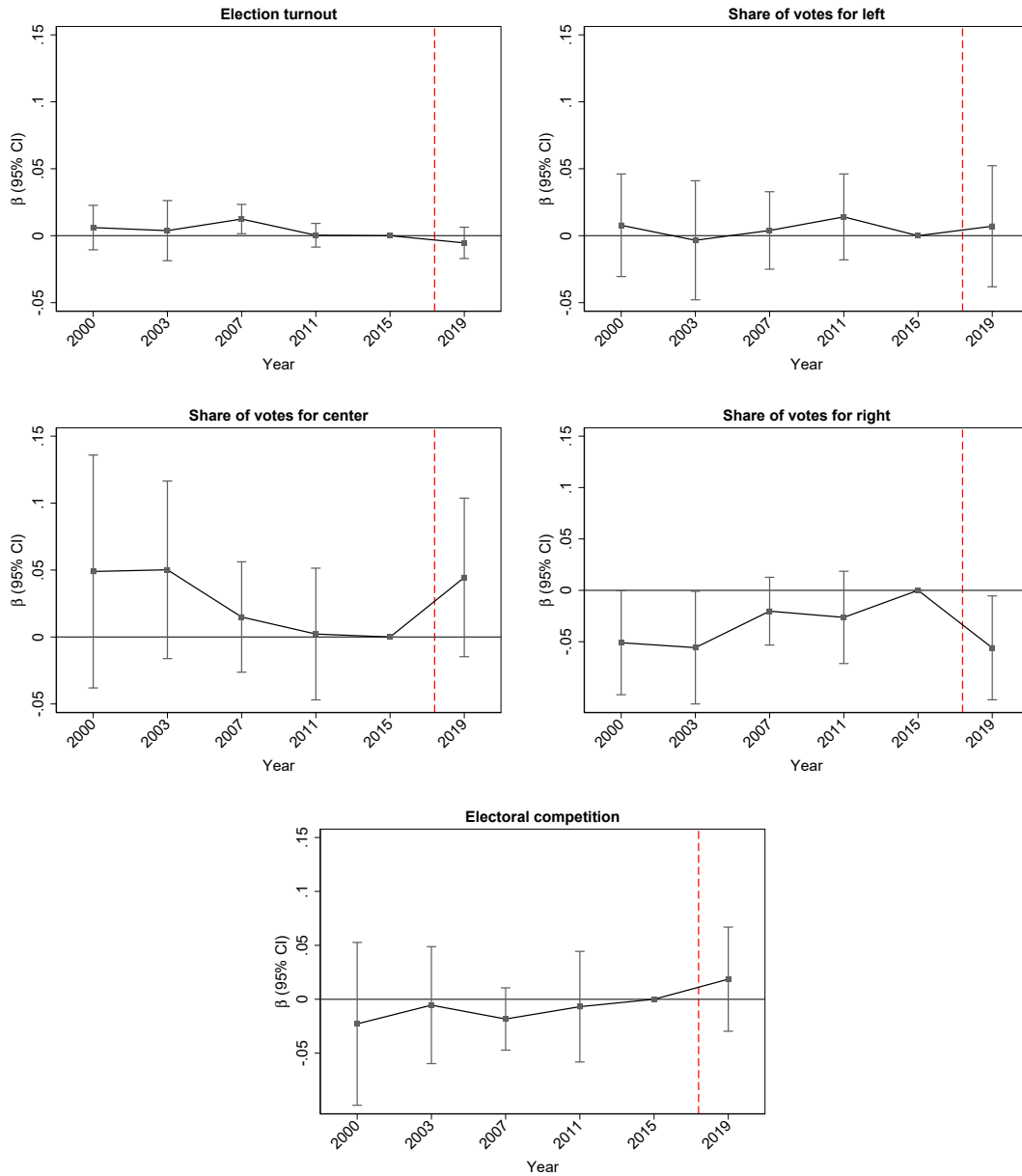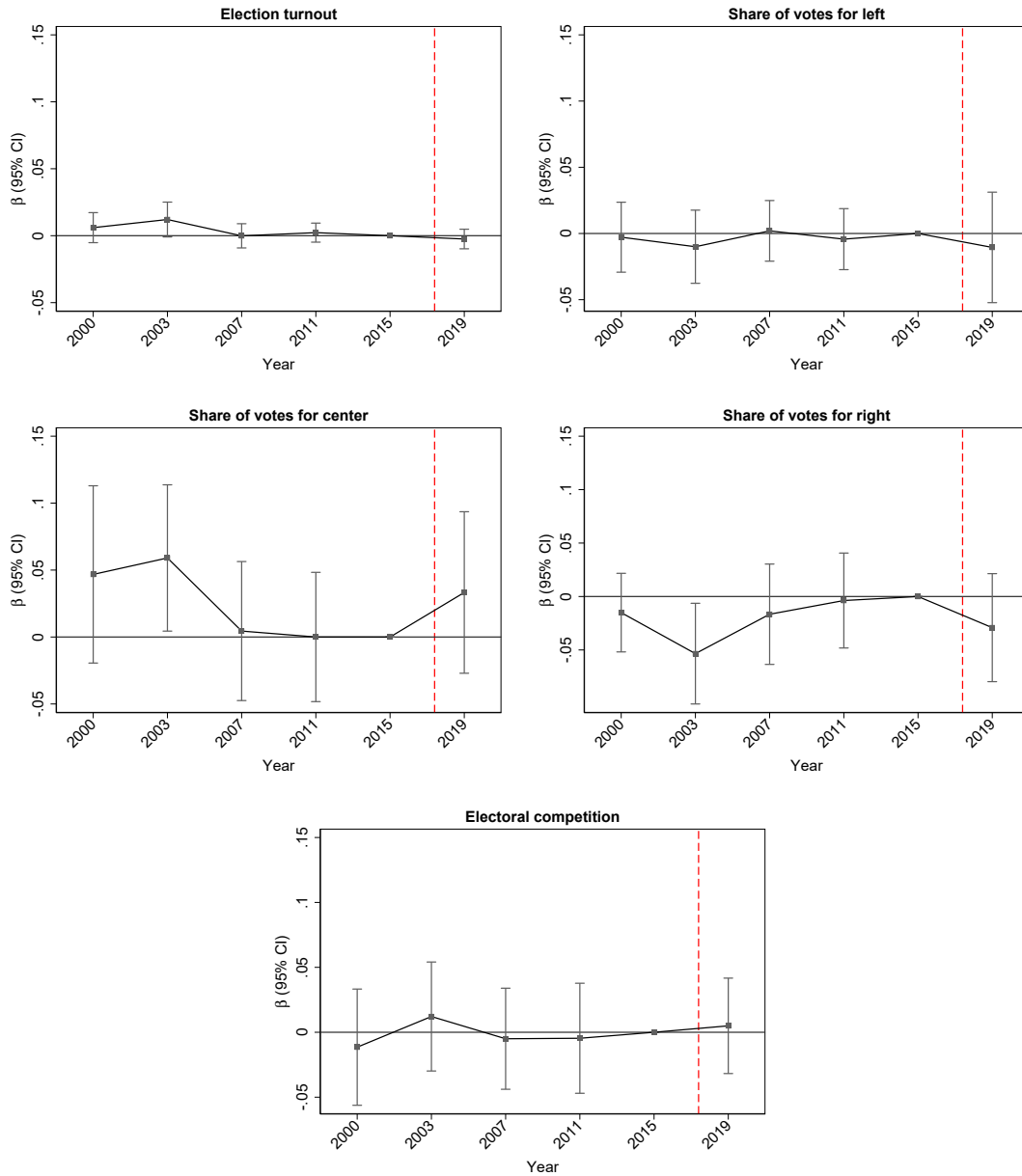
Figure 5. Parallel Trend Assumption – Presidential Elections
Discrete Treatment Variable

Notes: The figure illustrates the results of an event study estimation. We interacted the discrete variable PEP, which takes the value of one in municipalities where there are PEP holders, with presidential electoral years excluding the election of 2018, the year the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.

X TABLES

Table 1. Impacts of PEP Program on Electoral Outcomes

	Election	Share of Votes for			Electoral
	Turnout	Left	Center	Right	Competition
	(1)	(2)	(3)	(4)	(5)
Panel A. Mayoral Election - Discrete Treatment Variable					
$I(PEP_m) \times I(Post2018)_t$	-0.006	-0.008	0.015	-0.013	0.007
	(0.003)	(0.019)	(0.025)	(0.020)	(0.014)
q-values	[0.653]	[1.00]	[1.00]	[1.00]	[1.00]
R-squared	0.853	0.450	0.363	0.422	0.386
Observations	6,174	6,174	6,174	6,174	5,969
Panel B. Mayoral Election - Continuous Treatment Variable					
$PEP_m \times I(Post2018)_t$	-0.004	0.001	0.011	-0.012	0.010
	(0.002)	(0.007)	(0.009)	(0.008)	(0.006)
q-values	[0.283]	[0.490]	[0.357]	[0.283]	[0.283]
R-squared	0.853	0.450	0.363	0.422	0.386
Observations	6,174	6,174	6,174	6,174	5,969
Panel C. Presidential Election - Discrete Treatment Variable					
$I(PEP_m) \times I(Post2018)_t$	0.002	-0.009	0.004	-0.001	-0.004
	(0.004)	(0.007)	(0.008)	(0.007)	(0.013)
q-values	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
R-squared	0.877	0.898	0.845	0.866	0.682
Observations	6,561	6,561	6,561	6,561	6,549
Panel D. Presidential Election - Continuous Treatment Variable					
$PEP_m \times I(Post2018)_t$	-0.019	-0.038	0.008	0.022	0.050
	(0.015)	(0.016)	(0.016)	(0.021)	(0.026)
q-values	[0.250]	[0.105]	[0.500]	[0.280]	[0.129]
R-squared	0.877	0.898	0.845	0.866	0.682
Observations	6,561	6,561	6,561	6,561	6,549
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Department FE $\times$ Year	Yes	Yes	Yes	Yes	Yes
Municipal controls $\times$ Year	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of our main specification in equation 1. $I(PEP_m)$ is a discrete variable that takes the value of one if the municipality had positive take-up rates and PEP_m is the standardized value of the ratio of PEP migrants to population. Electoral competition was estimated following Chacón et al. (2006):1 - (% winning candidate - % second-place candidate). The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Clustered standard errors at the municipal level are reported in parentheses and False Discovery Rate (FDR) q-value in brackets *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table 2. Knowledge about the PEP Program
Control Group Only

	Average	STD	Observations
	(1)	(2)	(3)
Panel A. People with correct knowledge about PEP			
Knowledge about PEP [=1]	0.446	0.497	543
Knowledge of who can access the PEP [=1]	0.521	0.501	261
Panel B. Demographic Characteristics of People with knowledge about PEP			
Male [=1]	0.54	0.50	242
Age	48.12	15.24	242
Education: Primary school or less [=1]	0.08	0.28	242
Education: Secondary school [=1]	0.30	0.46	242
Education: Technician, university or more [=1]	0.62	0.49	242
Married or Cohabitating [=1]	0.52	0.50	242
Economic Strata: Low [=1]	0.43	0.50	242
Economic Strata: Medium [=1]	0.33	0.47	242
Economic Strata: High [=1]	0.23	0.42	242
Employed [=1]	0.89	0.31	151
Labor Contract [=1]	0.44	0.50	124
Student [=1]	0.13	0.34	242
Political Interest [=1]	0.86	0.34	242
Voted in 2019 Mayoral Elections [=1]	0.75	0.43	242
Voted in 2002 Presidential Elections [=1]	0.81	0.39	242

Table 3. Treatment Effect on Voting Intentions

	Vote intention in 2023 Mayoral elections	Vote intention in 2026 Presidential elections
	(1)	(2)
I(Treatment)	-0.024	-0.025
	(0.023)	(0.023)
R-squared	0.018	0.013
Observations	1,040	1,040
Mean values (Control Group)	0.839	0.855

Notes: This table reports an OLS estimate. Dependent variables in columns (i)–(ii) are indicator variables [=1] if the respondent has the intention to vote in the next mayoral election in 2023 and in the next presidential election in 2026. All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table 4. Treatment Effect on Social Capital

	Positive Reciprocity Index	Negative Reciprocity Index	Altruism	Trust
	(1)	(2)	(3)	(4)
I(Treatment)	0.019	-0.023	-0.017	-0.036
	(0.061)	(0.063)	(0.061)	(0.061)
R-squared	0.029	0.000	0.022	0.013
Observations	1,040	1,040	1,040	1,040
Mean (Control Group)	0.000	0.000	0.000	0.000

Notes: This table reports an OLS estimate. The variable in Column (i) is an index constructed using the methodology of Kling et al. (2007) and the reported answer on a 1 to 5 scale of the approval of the statement: *when someone does me a favor, I am willing to return it*, and the answer of the hypothetical money the respondent may give to a stranger as a thank-you for helping him on the street; (ii) is an index constructed using the methodology of Kling et al. (2007) and the reported answer on a 1 to 5 scale of the approval of the following statements: *How willing are you to punish someone who treats you unfairly, even when there are risks to you of personal consequences*; *How willing are you to punish someone who treats others unfairly, even when there are risks to you of personal consequences*; and *If I am treated very unfairly, I will take revenge on the first occasion, even if I have to pay a cost for it*; (iii) is the standardized reported answer on a 1 to 5 scale of the approval of the statement: *How willing are you to donate to charitable causes without expecting anything in return*; (iv) is the standardized reported answer on a 1 to 5 scale of the approval of the statement: *I always assume that people have only the best intentions*. All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table 5. Treatment Effect on Altruism Towards Migrants

	Money will kept by their-self (log)	Money will share with vulnerable Venezuelan (log)	Money will share with vulnerable Colombian (log)
	(1)	(2)	(3)
I(Treatment)	0.011	-0.024	-0.043
	(0.063)	(0.044)	(0.032)
R-squared	0.028	0.010	0.017
Observations	332	434	796
Mean values (Control Group)	8.077	7.737	8.131

Notes: This table reports an OLS estimate. Dependent variables are the logarithm of the answer of the respondent about the distribution of 5,000 Colombian pesos between him (column (i)), a vulnerable Venezuelan migrant (column (ii)), and a vulnerable Colombian (column (iii)). All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table 6. Treatment Effect on Political Attitudes towards Migrants

	Colombian government has to help Venezuelans	In favor to a law that helps Venezuelans	Venezuelans compete with Colombians jobs	Venezuelans increase crime	Venezuelans improve Colombian culture	Positive effect of Venezuelans in Colombia
	(1)	(2)	(3)	(4)	(5)	(6)
I(Treatment)	0.026	0.025	0.065	-0.066	-0.051	-0.028
	(0.060)	(0.061)	(0.061)	(0.063)	(0.062)	(0.031)
R-squared	0.032	0.036	0.007	0.021	0.027	0.027
Observations	1,040	1,040	1,040	1,040	1,040	1,040
Mean (Control Group)	0.000	0.000	0.000	0.000	0.000	0.566

Notes: This table reports an OLS estimate. Dependent variables in columns (i)–(vi) are the standardized reported answer on a 1 to 5 scale of the approval of the statements:(i) *The Colombian government is obliged to help Venezuelan migrants*; (ii) *Would vote for a policy to increase government spending to help Venezuelan migrants*; (iii) *Venezuelan migrants come to compete for our jobs*; (iv) *Venezuelan migrants increase crime*; and (v) *Venezuelan migrants improve Colombian society by bringing new ideas and cultures.*. All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

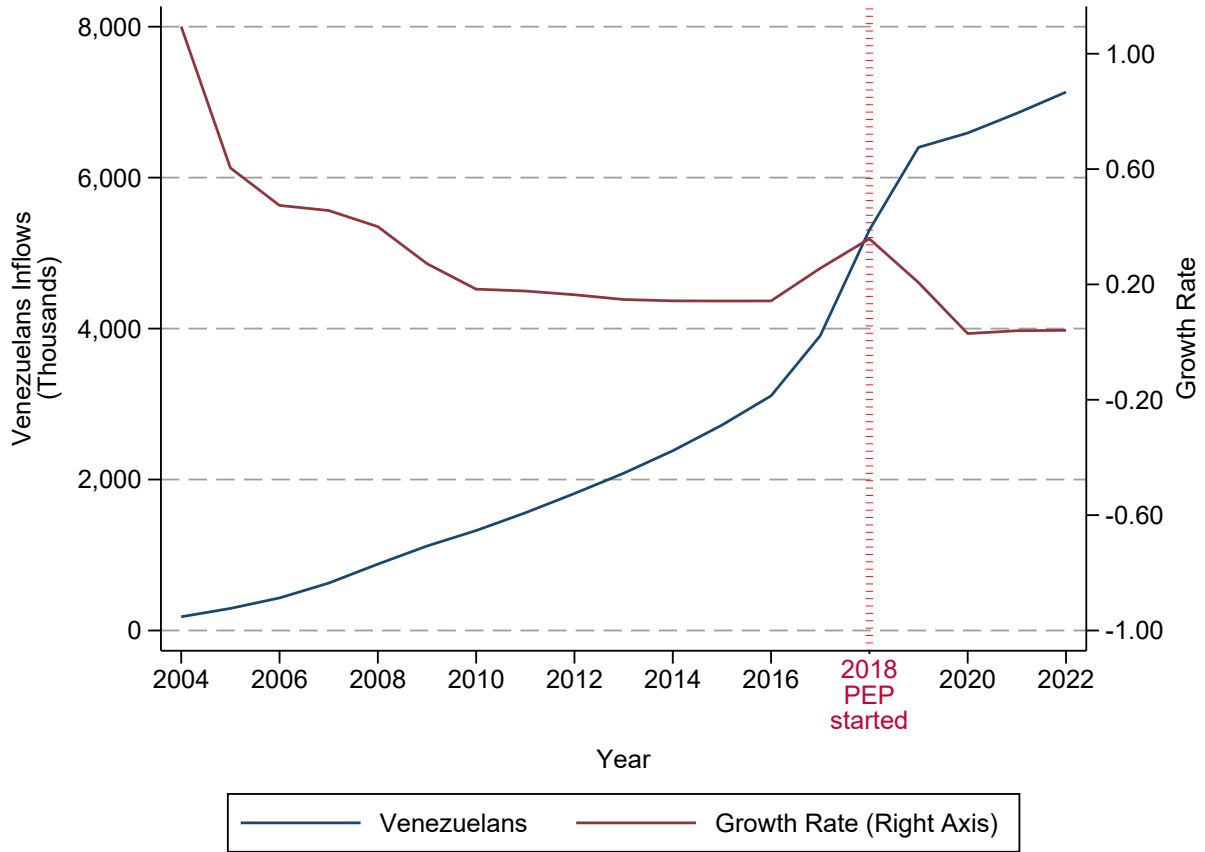# APPENDIX A: VENEZUELAN POPULATION, RAMV, AND PEP BENEFITS

Figure A.1. Venezuelan Inflows to Colombia

Notes: The data comes from the Colombian migration agency, *Migración Colombia*, and includes transitory migration. There is no data available before 2003.

Table A.1. Characterizing Venezuelan Migrants vs. Colombian population

	Colombians	Venezuelans	Mean Diff. (p-value)
	(1)	(2)	(3)
Female [=1]	0.513	0.491	0.000
	(0.500)	(0.500)	
Age: 19-	0.312	0.401	0.000
	(0.463)	(0.490)	
Age: 20-39	0.318	0.451	0.000
	(0.466)	(0.498)	
Age: 40-59	0.235	0.116	0.000
	(0.424)	(0.320)	
Age: 60+	0.135	0.032	0.000
	(0.342)	(0.175)	
Married or Cohabitating [=1]	0.472	0.468	0.056
	(0.499)	(0.499)	
Literate [=1]	0.939	0.957	0.000
	(0.239)	(0.203)	
Student [=1]	0.284	0.222	0.000
	(0.451)	(0.416)	
Education: Nothing or Preschool	0.068	0.062	0.000
	(0.251)	(0.241)	
Education: Elementary School	0.299	0.199	0.000
	(0.458)	(0.399)	
Education: Middle or High School	0.419	0.489	0.000
	(0.493)	(0.500)	
Education: University or More	0.214	0.250	0.000
	(0.410)	(0.433)	
Employed [=1]	0.897	0.803	0.000
	(0.304)	(0.398)	
Observations	4,263,965	95,356	4,359,321

Source: Colombian census of 2018.

Table A.2. Characterizing Documented vs. Undocumented Migrants

	PEP	Non-PEP	Mean Diff. (P-value)
	(1)	(2)	(3)
Female [=1]	0.496	0.499	0.135
	(0.500)	(0.500)	
Age (Years)	26.986	23.988	0.000
	(14.141)	(15.121)	
Primary or Less [=1]	0.294	0.423	0.000
	(0.456)	(0.494)	
Secondary [=1]	0.592	0.506	0.000
	(0.492)	(0.500)	
University or more [=1]	0.115	0.071	0.000
	(0.318)	(0.257)	
Married or cohabitating [=1]	0.377	0.294	0.000
	(0.485)	(0.456)	
Pregnant [=1]	0.037	0.038	0.196
	(0.188)	(0.191)	
Sector: Services and Sales	0.815	0.822	0.002
	(0.388)	(0.382)	
Sector: Manufacturing	0.060	0.054	0.000
	(0.237)	(0.226)	
Sector: Extraction and Transp.	0.078	0.077	0.226
	(0.269)	(0.266)	
Sector: Finance and Adm.	0.009	0.009	0.417
	(0.093)	(0.095)	
Sector: Other	0.038	0.038	0.758
	(0.190)	(0.191)	
Occupation: Formally Employed (% of Pop.)	0.008	0.007	0.582
	(0.087)	(0.086)	
Occupation: Informally Employed (% of Pop.)	0.264	0.188	0.000
	(0.441)	(0.391)	
Occupation: Self-Employed (% of Pop.)	0.225	0.207	0.000
	(0.418)	(0.405)	
Occupation: Unemployed (% of Pop.)	0.186	0.174	0.000
	(0.389)	(0.379)	
Occupation: Student (% of Pop.)	0.071	0.088	0.000
	(0.257)	(0.283)	
Occupation: Housework (% of Pop.)	0.093	0.104	0.000
	(0.291)	(0.305)	
Family size	3.507	3.517	0.134
	(2.042)	(2.092)	
Family in Colombia [=1]	0.413	0.473	0.000
	(0.492)	(0.499)	
Family in Venezuela [=1]	0.666	0.652	0.000
	(0.472)	(0.476)	
Migration Intent: Stay in Colombia	0.889	0.903	0.000
	(0.315)	(0.296)	
Migration Intent: Move to another country	0.019	0.021	0.000
	(0.136)	(0.143)	
Migration Intent: Return to Venezuela	0.092	0.076	0.000
	(0.290)	(0.265)	
Observations	281,307	161,310	442,617

Source: RAMV census.

Table A.3. PEP Benefits

	All Refugees	Refugees with RAMV	Refugees with PEP
Education	Nursery, pr. and sec.	Nursery, pr. and sec.	Nursery, pr. and sec.
	Food and school bus	Food and school bus	Food and school bus
	No	No	Promotion across levels
	No	No	Degree recognition
SISBEN	No	No	Yes
Health	Emergency care	Emergency care	Emergency care
	Public health programs	Public health programs	Public health programs
	Vaccines	Vaccines	Vaccines
	Prenatal care	Prenatal care	Prenatal care
	Prevention campaigns	Prevention campaigns	Prevention campaigns
	No	No	Subsidized regime
ICBF**	No	No	Childcare
	No	No	Early childhood service
Formal Labor	No	No	Job permit
Financial Services	No	No	banking access

Source: Ibáñez et al. (2022). *SISBEN: score used to target social safety net programs in Colombia, and ** ICBF: Colombian Family Welfare Institute.

APPENDIX B: DATA BASE CONSTRUCTION

Mayoral elections

The database was constructed with original data from the Colombian electoral authority, *Registraduría Nacional del Estado Civil*. Our database begins after the 2000 elections, since after that year the electoral data has information on the electoral roll and the total votes received for all candidates. To begin, we identified the political party of each candidate or the political movement that endorsed the candidate's campaign[17] and then classified its ideology as left, center, or right following the methodology proposed by Fergusson et al. (2020). The classification for each candidate's ideology includes three steps.

1. Check party names, mottos, and slogans for words that identify the candidate's party clearly as left-leaning or right-leaning (e.g., communist or socialist for left-wing, and conservative or Christian for right-wing).

2. In the event that the previous step did not work, we checked the party statutes (when available) for policy stances that clearly leaned either to left or right. We coded a party as left-wing if the party statutes included at least three of the following five leftist policy positions: (1) pro-peasant, (2) advocates greater market regulation, (3) thinks workers should be defended against exploitation, (4) advocates state-owned or communal property rights, and (5) anti-imperialist. We coded a party as right-wing if its statutes included at least three of the following five right-wing policy positions: (1) economic growth is emphasized over redistribution; (2) advocates free market, orthodox policies, and privatization; (3) believes that family and religion are the moral pillars of society; (4) appeals to patriotism and nationalism, and accepts the suspension of some freedoms in order to guarantee security; and (5) prioritizes law and order. We classified parties that did not include at least three of the policy stances from either list in their statutes as neither left-wing nor right-wing.

---

[17]In Colombia, a mayoral candidate may register for the elections with the endorsement of a political movement or party, with legal status recognized by the National Electoral Council, or with the support of a significant group of citizens, in which case it must provide the total of the corresponding signatures.

3. For parties for which official statutes were not available, we checked the government plan that candidates submit to the electoral authority before elections and—when available—searched them for the same policy stances as in the second step.[18]

Finally, we used electoral roll information to calculate the municipality's turnout and the share of votes obtained in each election by left-wing parties, center parties (the residual of neither left-wing nor right-wing parties), and right-wing parties.

Presidential elections

Our analysis of presidential elections focuses on the first-round elections that took place between 2000 and 2022. The 2022 presidential elections took place May 29; the results are publicly available on Colombia's electoral agency web page[19] but are not compiled in a single document, so we scraped the web page to gather all the information. As in the mayoral elections, we classified all candidates according to their apparent political ideology, following Fergusson et al. (2020).

---

[18]For example: for the 2019 elections, Colombia's electoral authority official website gathered information on all government plans https://wapp.registraduria.gov.co/electoral/Elecciones-2019/infocandidatos2019.php
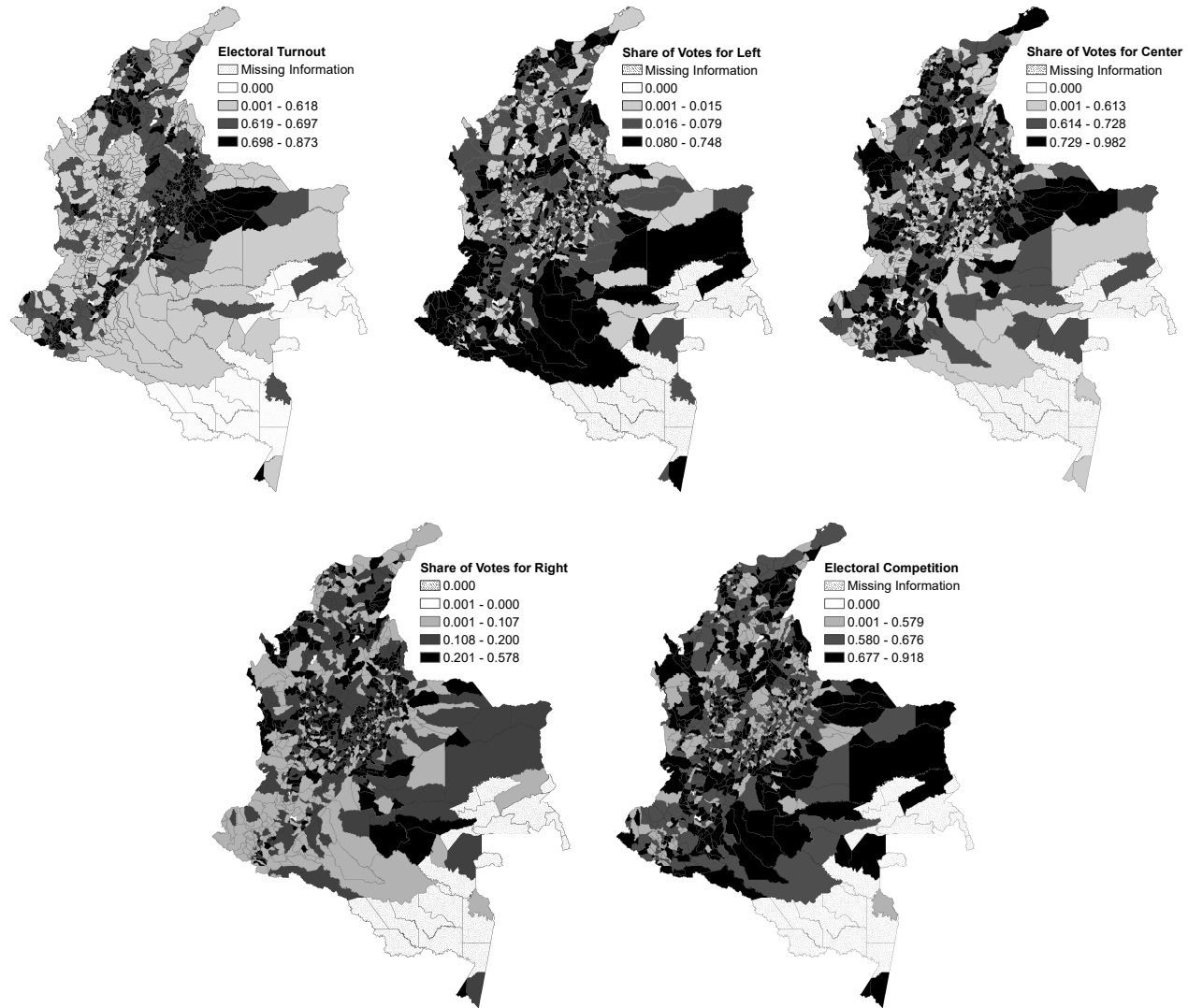
[19]https://resultados.registraduria.gov.co/presidente/0/co

APPENDIX C: DESCRIPTIVE STATISTICS

Table C.1. Descriptive Statistics – Electoral Outcomes

	Year	Observations	Average	St. Deviation
	(1)	(2)	(3)	(4)
Panel A. PEP Take-up				
PEP holders*	2018	1,098	0.003	0.043
Municipality with PEP Holders	2018	1,098	0.740	0.439
Panel B. Mayoral Elections (2000, 2003, 2007, 2011, 2015 and 2019)				
Registered voters	2000-2019	6,212	27,701.96	166,629.00
Total votes	2000-2019	6,212	15,677.75	82,668.48
Votes of left	2000-2019	6,212	1,724.70	26,780.75
Votes of center	2000-2019	6,212	8,813.67	39,424.01
Votes of right	2000-2019	6,212	2,212.02	10,870.86
Election turnout (Votes / Registered Voters)	2000-2019	6,212	0.65	0.11
Left (% of Votes)	2000-2019	6,212	0.06	0.15
Center (% of Votes)	2000-2019	6,212	0.66	0.26
Right (% of Votes)	2000-2019	6,212	0.16	0.21
Electoral competition	2000-2019	5,992	0.63	0.19
Panel C. Presidential Elections (2002, 2006,2010, 2014, 2018, and 2022)				
Registered voters	2002-2022	6,564	23,532.14	80,693.46
Total votes	2002-2022	6,564	11,145.94	41,003.64
Votes of left	2002-2022	6,564	2,375.6	11,788.09
Votes of center	2002-2022	6,564	3,059.43	13,397.85
Votes of right	2002-2022	6,564	5,327.39	20,775.21
Election turnout (Votes/Registered voters)	2002-2022	6,564	0.46	0.49
Left (% of Votes)	2002-2022	6,564	0.17	0.18
Center (% of Votes)	2002-2022	6,564	0.29	0.22
Right (% of Votes)	2002-2022	6,564	0.50	0.22
Electoral competition	2002-2022	6,557	0.54	0.19

Notes: PEP holders is the share of migrants who reported having PEP divided by the total population in 2018 in each municipality. Electoral competition among candidates and political ideologies at municipal level is estimated following Chacón et al. (2006): 1 - (% winning candidate - % second-place candidate).

Figure C.1. Electoral Outcomes – Mayoral Elections

Electoral Turnout
- Missing Information
- 0.000
- 0.001 - 0.618
- 0.619 - 0.697
- 0.698 - 0.873

Share of Votes for Left
- Missing Information
- 0.000
- 0.001 - 0.015
- 0.016 - 0.079
- 0.080 - 0.748

Share of Votes for Center
- Missing Information
- 0.000
- 0.001 - 0.613
- 0.614 - 0.728
- 0.729 - 0.982

Share of Votes for Right
- 0.000
- 0.001 - 0.000
- 0.001 - 0.107
- 0.108 - 0.200
- 0.201 - 0.578

Electoral Competition
- Missing Information
- 0.000
- 0.001 - 0.579
- 0.580 - 0.676
- 0.677 - 0.918

Notes: The figure depicts the electoral variables average of the 2000, 2003, 2007, 2011, 2015, and 2019 mayoral elections.

Figure C.2. Electoral Outcomes – Presidential Elections
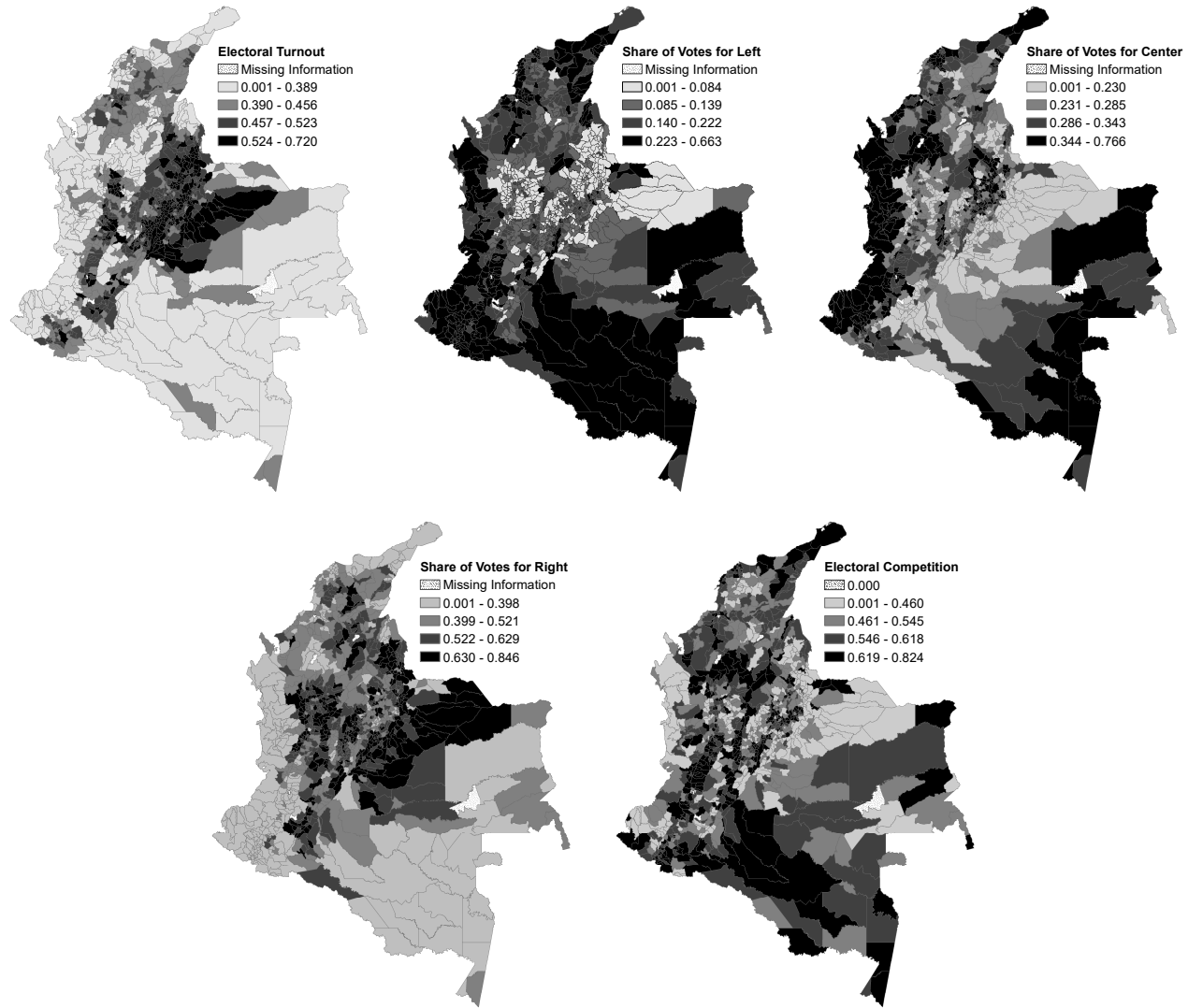
Notes: The figure depicts the electoral variables average of the 2002, 2006, 2010, 2014, 2018, and 2022 presidential elections.

Table C.2. Descriptive Statistics – Municipal Baseline Controls

|	Year	Observations	Average	St. Deviation
	(1)	(2)	(3)	(4)
Panel A. Conflict and Violence				
Homicide rates (per 100,000 inh.)	2017	1,098	10.07	46.91
Number of robberies	2017	1,098	200.24	1,218.37
Panel B. Public Finance				
Revenue	2017	1,098	49,932.28	218,740.95
Expenditure	2017	1,098	49,655.99	220,118.23
Capital Expenditures	2017	1,098	42,716.56	188,503.99
Central Government Transfers (SPG)*	2017	1,098	19,218.23	62,347.32
SPG in education	2017	1,098	7,769.44	41,083.67
SPG in health	2017	1,098	6,138.15	15,432.11
SPG in sewage and water	2017	1,098	1,298.5	2.304.58
SPG in child nutrition programs	2017	1,098	151.73	269.86
SPG in children	2017	1,098	133.75	246.87
Panel C. Poverty and inequality				
Subsidized Regime Affiliates	2016	1,098	14,330.32	86,453.46
Rural index (% Rural population)	2017	1,098	0.55	0.24
Panel D. Economic Growth				
Night Light Density	2009	1,098	3.85	7.21
Panel E. Previous Regularized Population				
Number of Applicants PEP 1 (August 2017-October 2017)	2017	1,098	36.63	293.95
Number of Applicants PEP 2 (February 2018-June 2018)	2018	1,098	58.3	454.21

Notes: *SPG stands for *Sistema General de Participaciones* and represents central government transfers to municipalities. Variables are expressed in millions of Colombian pesos, except for expenditures, which are expressed in thousands of Colombian pesos. *Data source:* (i) homicide rates (per 100,000 inh.) and number of robberies come from the Colombian National Police; (ii) revenue, expenditure, and capital expenditure come from the municipal panel of the Center for Economic Studies at Universidad de los Andes (CEDE); (iii) central government transfers (SPG), and SGP in education, health sewage and water, child nutrition programs, and children come from the Colombian National Planning Department; (iv) subsidized regime affiliates come from the Colombian Ministry of Health; (v) rural index (% rural population) comes from the municipal panel of CEDE; (vi) night light density comes from the National Oceanic and Atmospheric Administration (NOAA); and (vii) number of applicants PEP1 and PEP2 come from Migracioń Colombia

APPENDIX D: ROBUSTNESS TESTS

Raw data trends

Figure D.1. Evolution of Outcomes by PEP and Non-PEP Status – Mayoral Elections
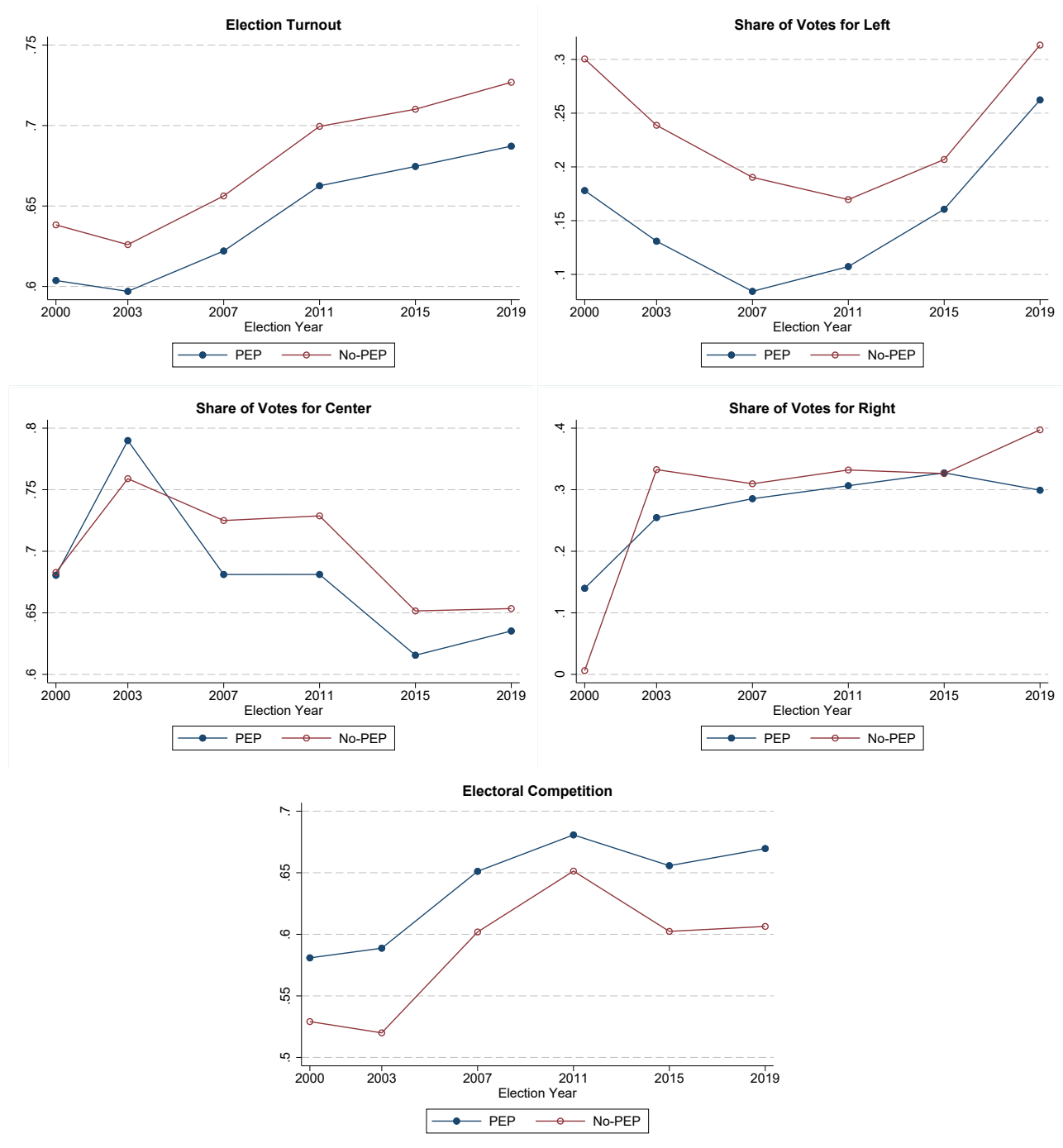
Notes: The figure illustrates the yearly evolution of the electoral outcomes in municipalities with and without PEP take-up. We changed the variables with zero for missing values.

Figure D.2. Presidential Elections' Evolution of Outcomes by PEP and Non-PEP Status
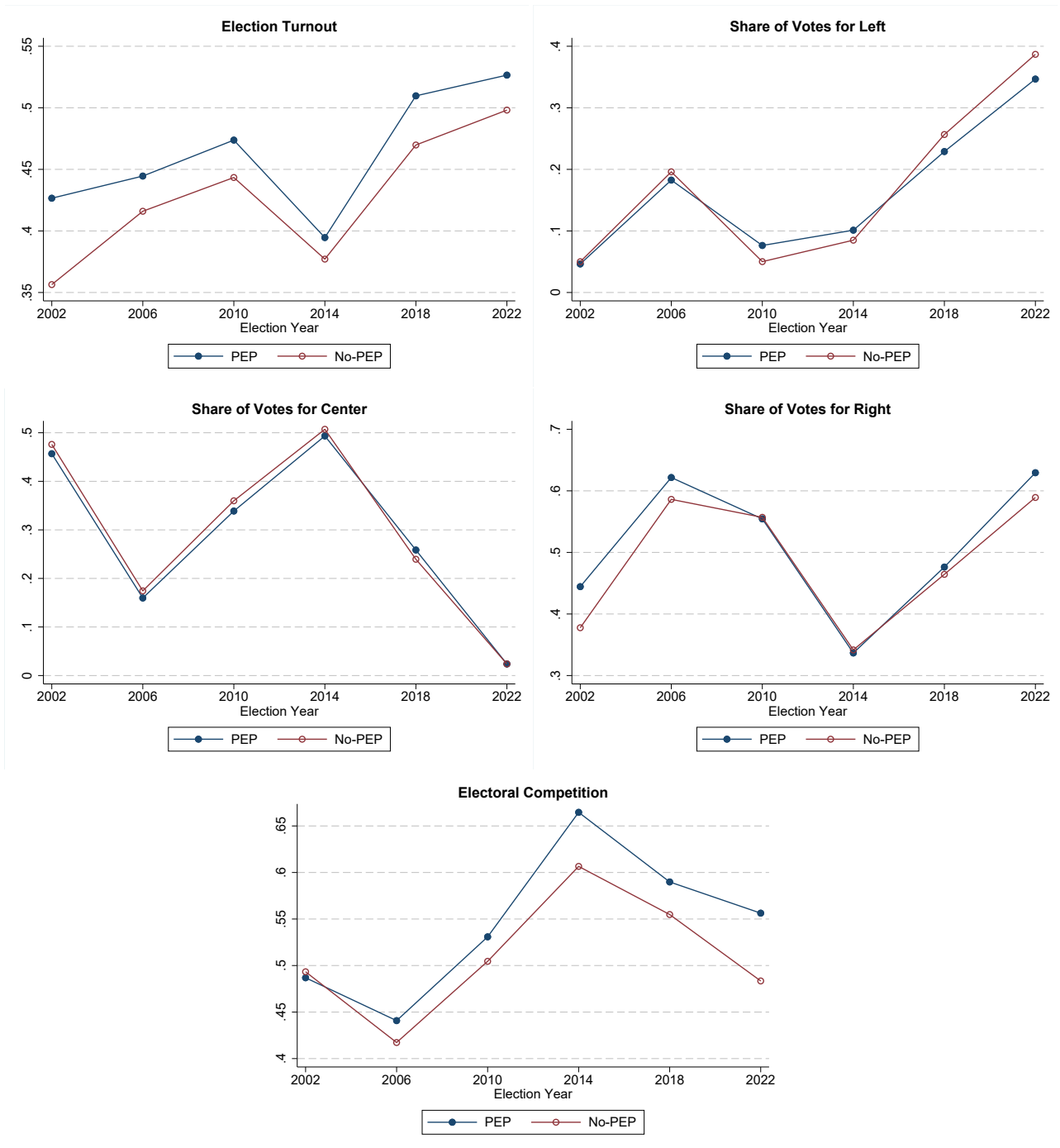
Notes: The figure illustrates the yearly evolution of the electoral outcomes in municipalities with and without PEP take-up.
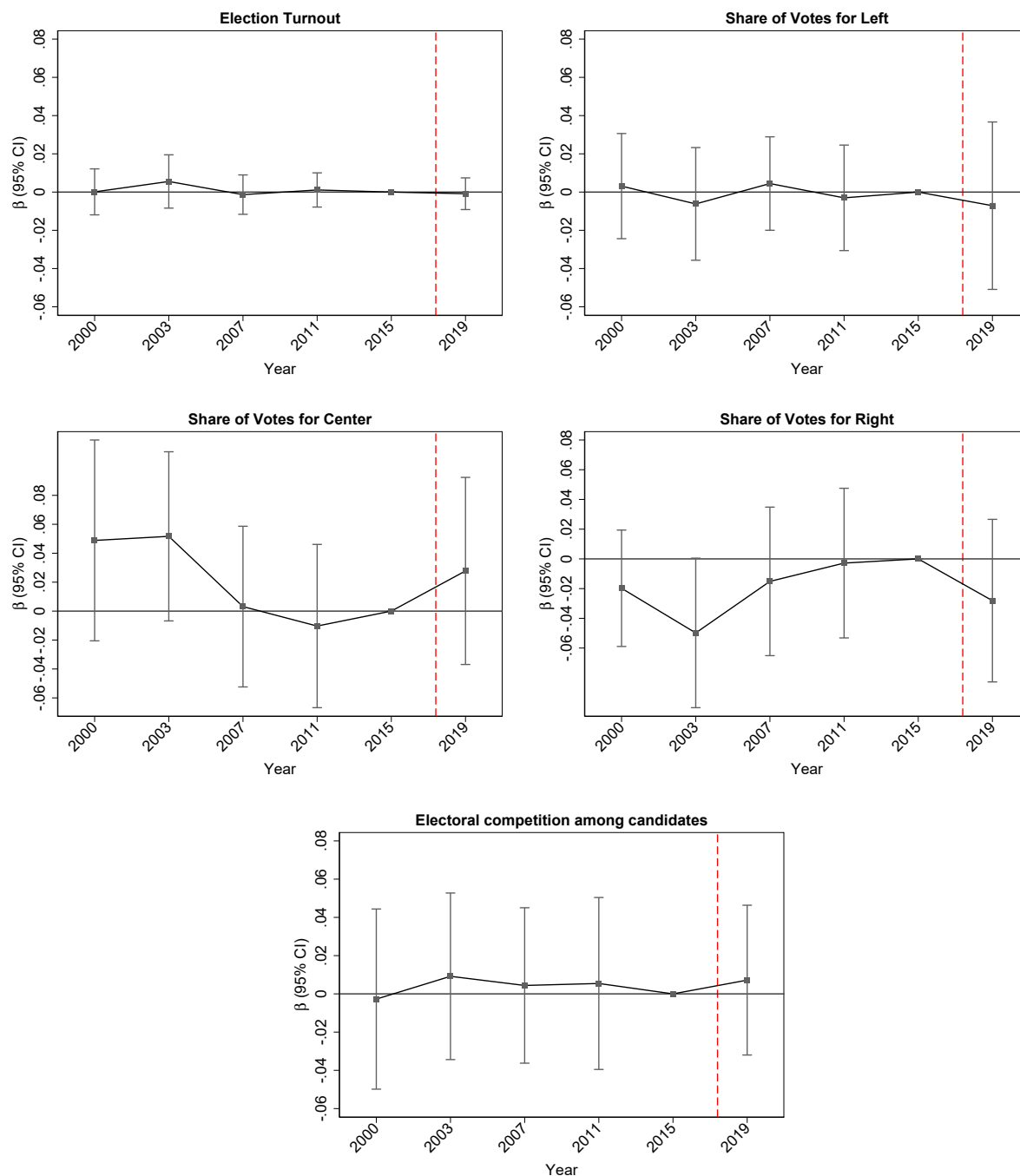
Algorithm Choice

Matching Difference-in-Difference Algorithm

Table D.1. Descriptive Statistics – Electoral and Municipal Baseline Controls

	PEP	No-PEP	Mean diff. (p-value)
	(1)	(2)	(3)
Panel A. Conflict and Violence			
Homicide rates (per 100,000 inh.)	14.190	2.388	0.003
	(67.448)	(3.972)	
Number of robberies	385.816	11.175	0.083
	(3,653)	(14.634)	
Panel B. Public Finance			
Revenue	81.554	13.620	0.051
	(588.875)	(7.113)	
Expenditure	32.850	2.899	0.146
	(347.791)	(1.676)	
Capital Expenditures	82.252	13.515	0.060
	(616.664)	(7.173)	
Central Government Transfers (SPG)*	27,070	6,878	0.005
	(123,000)	(3,726)	
SPG in education	12,728	425	0.011
	(82,056)	(391)	
SPG in health	8,127	2,320	0.000
	(25,355)	(1,969)	
SPG in sewage and water	1,641	727	0.002
	(4,889)	(450)	
SPG in child nutrition programs	188.248	73.683	0.000
	(403.757)	(83.888)	
SPG in children	162.116	72.740	0.000
	(348.286)	(87.952)	
Panel C. Poverty and inequality			
Rural index (% Rural population)	0.505	0.678	0.000
	(0.248)	(0.172)	
Panel D. Economic Growth			
Night Light Density	4.755	1.346	0.000
	(8.126)	(2.130)	
Panel E. Previous Regularized Population			
PEP1 (August 2017-October 2017)	83.540	0.640	0.174
	(1,029)	(3.94)	
PEP2 (February 2018-June 2018)	136.957	1.168	0.188
	(1,743)	(4.866)	
Observations	811	286	1,097

Notes: *SPG stands for *Sistema General de Participaciones* and represents central government transfers to municipalities. Variables are expressed in millions of Colombian pesos, except for expenditures, which are expressed in thousands of Colombian pesos.

Figure D.3. Parallel Trends for the Common Support – Mayoral Elections
Discrete Treatment Variable

Notes: The figure illustrates the results of an event study estimation for the municipalities in the common support of the predicted pscores. We interacted the discrete variable PEP, which takes the value of one in the municipalities where there are PEP holders, with mayoral electoral years excluding the 2015 election, the last one before 2018, when the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.
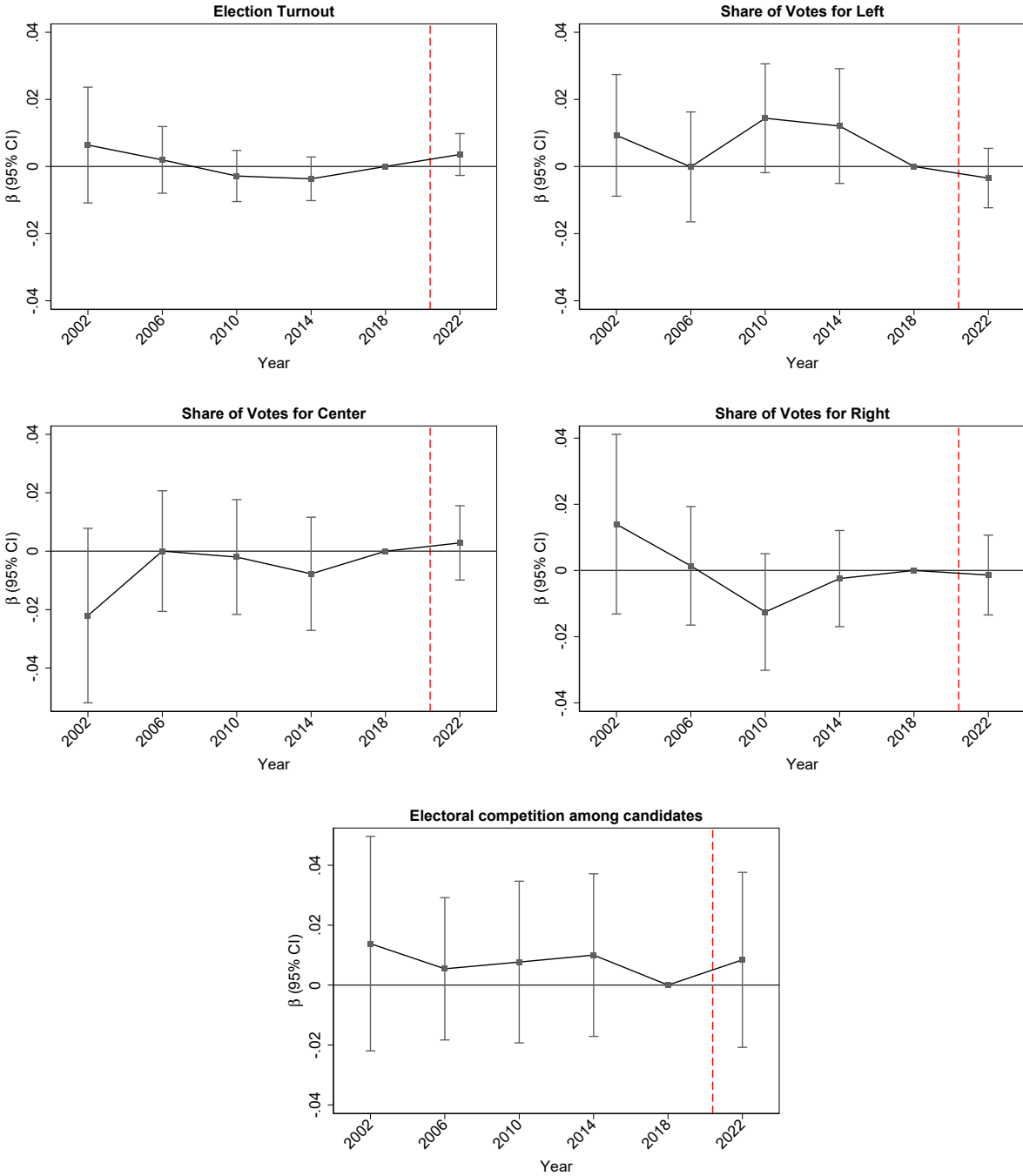
Figure D.4. Parallel Trends for the Common Support – Presidential Elections
Discrete Treatment Variable

Notes: The figure illustrates the results of an event study estimation for the municipalities in the common support of the predicted pscores. We interacted the discrete variable PEP, which takes the value of one in the municipalities where there are PEP holders, with mayor electoral years excluding the 2015 election, the last one before 2018, when the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.

Table D.2. Impacts of PEP Program on Electoral Outcomes
Matching Diff-in-Diff

	Election Turnout	Share of Votes for			Electoral Competition
	Turnout	Left	Center	Right	Competition
	(1)	(2)	(3)	(4)	(5)
Panel A. Mayoral Elections					
$I(PEP_m) \times I(Post2018)_t$	-0.001	-0.007	0.012	-0.014	0.006
	(0.004)	(0.020)	(0.027)	(0.022)	(0.016)
q-values	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
R-squared	0.847	0.464	0.359	0.428	0.384
Observations	5,035	5,035	5,035	5,035	4,842
Panel B. Presidential Elections					
$I(PEP_m) \times I(Post2018)_t$	0.002	-0.011	0.008	-0.002	0.001
	(0.004)	(0.007)	(0.008)	(0.007)	(0.013)
q-values	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
R-squared	0.868	0.901	0.843	0.869	0.679
Observations	5,439	5,439	5,439	5,439	5,426
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Department FE $\times$ Year	Yes	Yes	Yes	Yes	Yes
Municipal controls $\times$ Year	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of our main specification in equation 1 using a matching difference-in-difference. We predicted the probability of having PEP take-up by municipality using the municipal characteristics that were significant in Table D.1 and restricted the sample to the common support sample in the treatment and control groups where the pscores overlapped. Panel A shows the results for the mayoral elections, and panel B for the presidential elections. The variable PEP_m is the standardized value of the share of PEP holders over the population in 2018. Electoral competition was estimated following Chacón et al. (2006):1 - (% winning candidate - % second-place candidate). The estimates include municipality fixed effects. Clustered standard errors at the municipal level are reported in parentheses and False Discovery Rate (FDR) q-value in brackets *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Non-Inferiority Test

Table D.3. Impacts of PEP Program on Mayoral and Presidential Elections
Non-Inferiority Test for Potential Violation of Parallel Trend Assumption

| | Election | Share of Votes for | | | Electoral |
| | Turnout | Left | Center | Right | Competition |
	(1)	(2)	(3)	(4)	(5)
Panel A. Mayoral Elections - Canonical 2x2 variable					
$I(PEP_m) \times I(Post2018)_t$	-0.006	-0.008	0.015	-0.013	0.007
	(0.003)	(0.019)	(0.025)	(0.020)	(0.014)
q-values	[0.653]	[1.00]	[1.00]	[1.00]	[1.00]
R-squared	0.853	0.450	0.363	0.422	0.386
Observations	6,174	6,174	6,174	6,174	5,969
Panel B. Mayoral Elections - Continuos variable					
$PEP_m \times I(Post2018)_t$	-0.003	0.001	0.010	-0.011	0.010
	(0.002)	(0.007)	(0.009)	(0.008)	(0.007)
q-values	[0.316]	[0.530]	[0.383]	[0.316]	[0.316]
R-squared	0.853	0.450	0.363	0.422	0.386
Observations	6,174	6,174	6,174	6,174	5,969
Panel C. Presidential Elections - Canonical 2x2 variable					
$I(PEP_m) \times I(Post2018)_t$	0.002	-0.009	0.004	-0.001	-0.004
	(0.004)	(0.007)	(0.008)	(0.007)	(0.013)
q-values	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
R-squared	0.877	0.898	0.845	0.866	0.682
Observations	6,561	6,561	6,561	6,561	6,549
Panel B. Presidential Elections - Continuos variable					
$PEP_m \times I(Post2018)_t$	-0.019	-0.036	0.007	0.022	0.051
	(0.014)	(0.016)	(0.016)	(0.021)	(0.026)
q-values	[0.237]	[0.124]	[0.467]	[0.276]	[0.124]
R-squared	0.877	0.898	0.845	0.866	0.682
Observations	6,561	6,561	6,561	6,561	6,549
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Department FE $\times$ Year	Yes	Yes	Yes	Yes	Yes
Municipal Controls $\times$ Year	Yes	Yes	Yes	Yes	Yes
Differential Trend	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results for the non-inferiority test proposed by Bilinski and Hatfield 2018. It shows the results of our main specification in equation 1, controlling for an indicator variable of differential linear pre-trends between the treatment and control groups. Electoral competition was estimated following Chacón et al. (2006):1 - (% winning candidate - % second-place candidate). The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Clustered standard errors at the municipal level are reported in parentheses and False Discovery Rate (FDR) q-value in brackets *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Impacts of a larger regularization program

Table D.4. Electoral Impacts of ETPV Program – Presidential Elections

	Election	Share of Votes for			Electoral
	Turnout	Left	Center	Right	Competition
	(1)	(2)	(3)	(4)	(5)
Panel A. Presidential Elections - Continuous Variable					
$\text{RUMV}_d \times I(\text{Post2018})_t$	-0.014	-0.047	0.019	0.027	-0.080
	(0.006)	(0.027)	(0.015)	(0.020)	(0.040)
q-values	[0.157]	[0.157]	[0.157]	[0.157]	[0.157]
R-squared	0.988	0.959	0.969	0.953	0.879
Observations	198	198	198	198	198
Department FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of the effect of the ETPV program on electoral outcomes. The variable RUMV_d is the total number of migrants who were issued the Registro Único de Migrantes Venezolanos (RUMV) over the population in 2020 by department. Electoral competition was estimated following Chacón et al. (2006):1 - (% winning candidate - % second-place candidate). The estimates include department and electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined department characteristics, measured before the beginning of our period and listed in Table C.2. Clustered standard errors at the municipal level are reported in parentheses and False Discovery Rate (FDR) q-value in brackets *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.
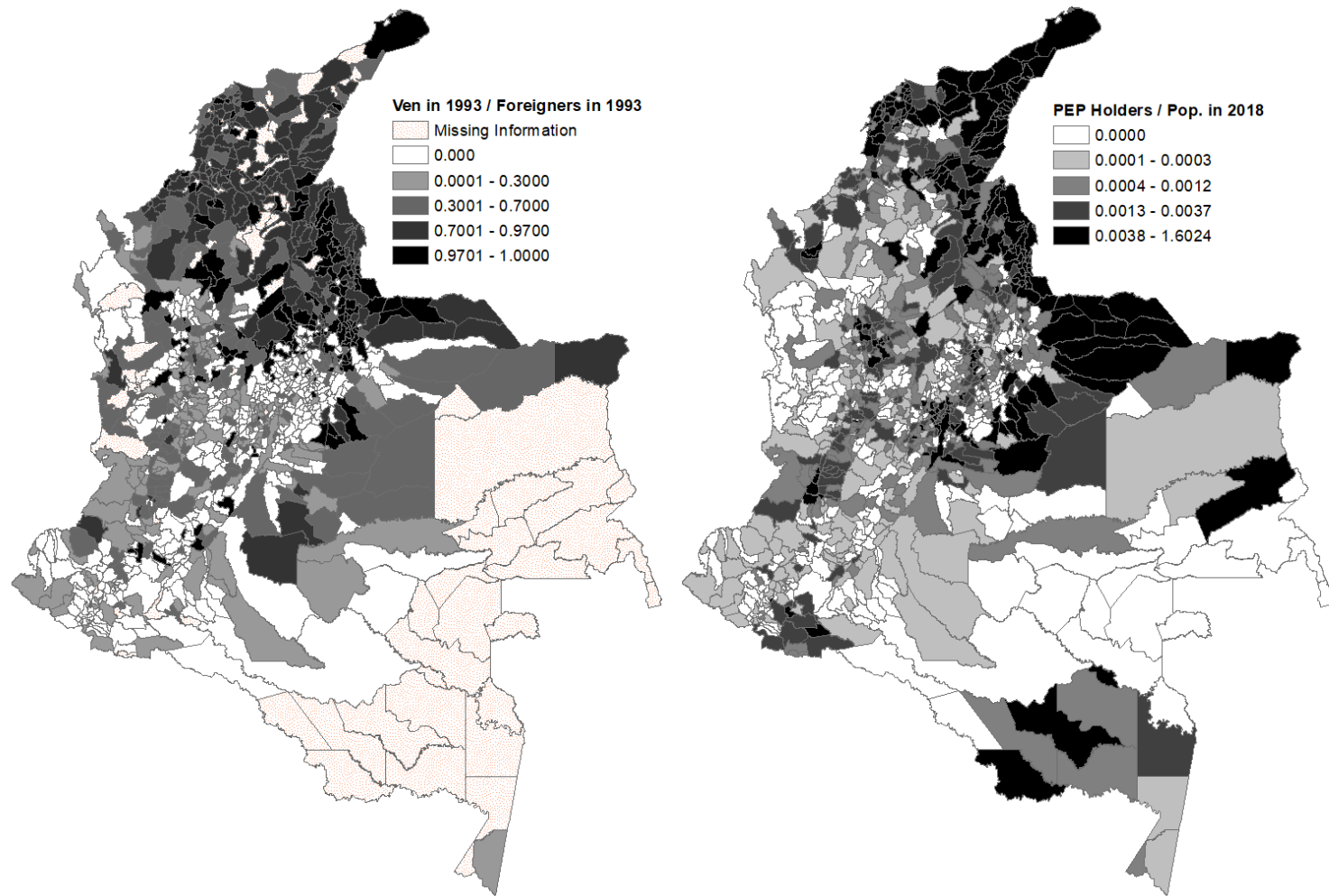
Impacts of migration flows are different

Table D.5. Effects of Venezuelan Migration on Mayoral and Presidential Elections

| | Election Turnout | Share of Votes for | | |
| | | Left | Center | Right |
	(1)	(2)	(3)	(4)
Panel A. Mayoral Elections				
Predicted Venezuelan Inflows	0.022*	-0.013*	0.003	0.017*
	(0.010)	(0.005)	(0.014)	(0.011)
q-values	[0.064]	[0.064]	[0.263]	[0.087]
R-squared	0.797	0.437	0.441	0.484
Observations	4,693	4,693	4,693	4,693
Panel B. Presidential Elections				
Predicted Venezuelan Inflows	0.002	-0.011***	0.003*	0.008***
	(0.002)	(0.002)	(0.002)	(0.003)
q-values	[0.122]	[0.001]	[0.058]	[0.004]
R-squared	0.823	0.852	0.942	0.917
Observations	6,768	6,768	6,768	6,768
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Department FE × Year	Yes	Yes	Yes	Yes
Municipal controls × Year	Yes	Yes	Yes	Yes

Notes: This figure replicates the estimate by Rozo and Vargas (2021), adding the last elections for mayors in 2019 and president in 2022. Panel A shows the results for the mayoral elections between 1997 and 2019, and panel B for the presidential elections between 1994 and 2022. Predicted Venezuelan Migration is the interaction between the shift share of early settlements of Venezuelans in 1993 and the cumulative number of individuals arriving in Colombia from Venezuela each year, over the total Colombian population each year. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period of analysis. Clustered standard errors at the municipal level are reported in parentheses and False Discovery Rate (FDR) q-value in brackets *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Figure D.5. Venezuelan Settlements in 1993 and PEP Holders in 2018

Ven in 1993 / Foreigners in 1993
Missing Information
0.000
0.0001 - 0.3000
0.3001 - 0.7000
0.7001 - 0.9700
0.9701 - 1.0000

PEP Holders / Pop. in 2018
0.0000
0.0001 - 0.0003
0.0004 - 0.0012
0.0013 - 0.0037
0.0038 - 1.6024

Notes: Municipalities with missing information were created after 1993. The maps were constructed using information from the population census of 1993 and the RAMV census.

Table D.6. Impacts of PEP Program on Mayoral and Presidential Elections
Migration Test

| | Election | Share of Votes for | | | Electoral |
| | Turnout | Left | Center | Right | Competition |
	(1)	(2)	(3)	(4)	(5)
Panel A. Mayoral Elections					
EarlySettlements1993$_m$ × I(Post2018)$_t$	0.003	0.016	-0.010	-0.012	-0.000
	(0.002)	(0.011)	(0.015)	(0.012)	(0.008)
q-values	[0.557]	[0.557]	[0.673]	[0.557]	[1.00]
R-squared	0.861	0.461	0.368	0.430	0.392
Observations	4,954	4,954	4,954	4,954	4,815
Panel B. Presidential Elections					
EarlySettlements1993$_m$ × I(Post2018)$_t$	-0.005	0.003	-0.006	0.003	0.004
	(0.002)	(0.004)	(0.005)	(0.005)	(0.007)
q-values	[0.227]	[0.863]	[0.780]	[0.863]	[0.863]
R-squared	0.894	0.902	0.860	0.876	0.691
Observations	5,177	5,177	5,177	5,177	5,173
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Department FE × Year	Yes	Yes	Yes	Yes	Yes
Municipal Controls × Year	Yes	Yes	Yes	Yes	Yes

Notes: Early Settlements is the geographical variation from Rozo and Vargas (2021), defined as the ratio of Venezuelan migrants by municipality over total foreigners in Colombia in 1993 (before the onset of the Venezuelan crisis). Electoral competition was estimated following Chacón et al. (2006):1 - (% winning candidate - % second-place candidate). The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Clustered standard errors at the municipal level are reported in parentheses and False Discovery Rate (FDR) q-value in brackets *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

X. A   Heterogeneous treatments effects

### Figure D.6. Parallel Trend Assumption – Mayoral Elections
Continuous Treatment Variable – Tercile 1
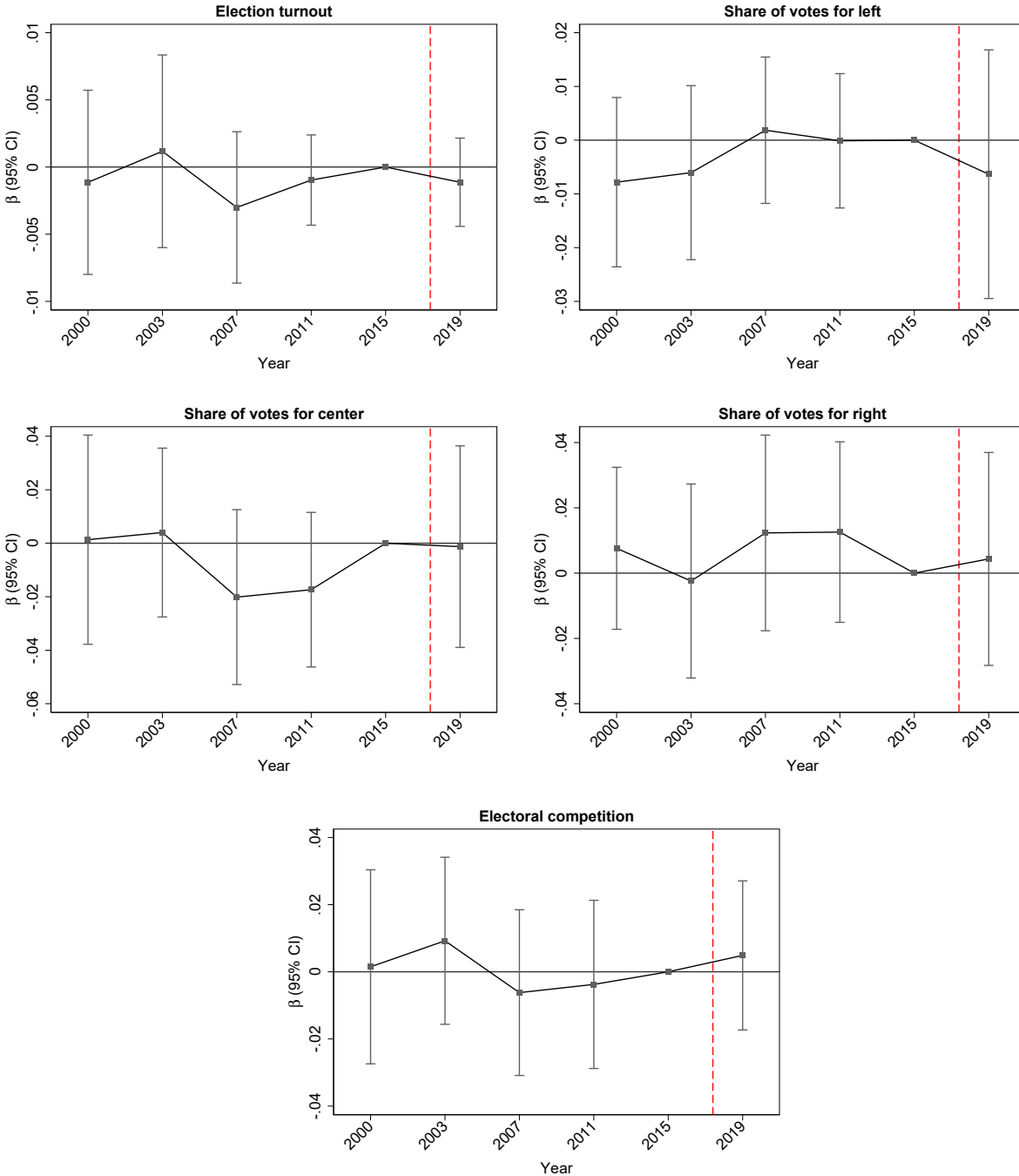
Notes: The figure illustrates the results of an event study estimation for the tercile 1 of the PEP holders intensity. We interacted the standardized values of the variable PEP holders with mayoral electoral years excluding the election of 2015, the year the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.

Figure D.7. Parallel Trend Assumption – Mayoral Elections
Continuous Treatment Variable
Tercile 2
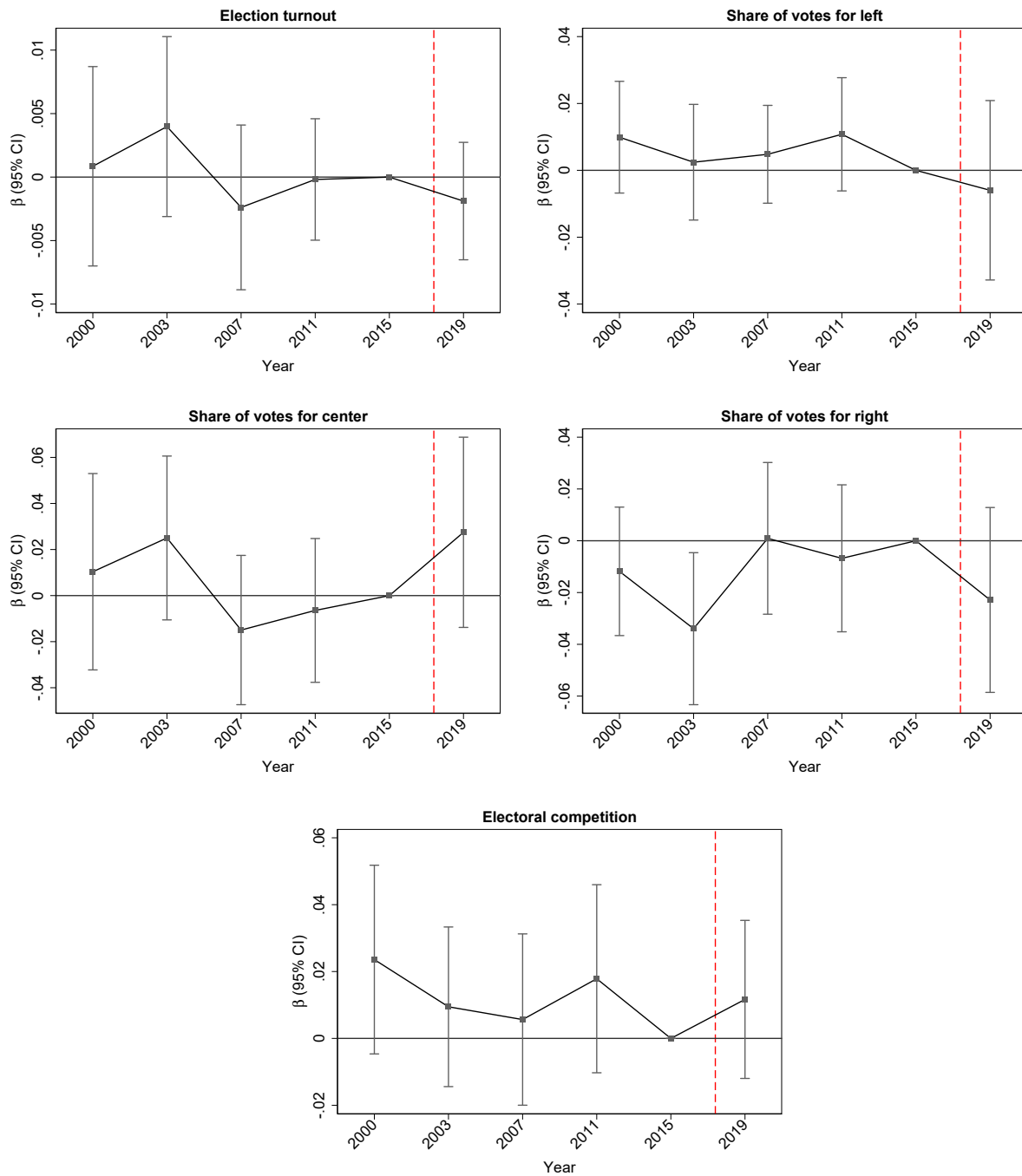
Notes: The figure illustrates the results of an event study estimation for the tercil 2 of the PEP holders intensity. We interacted the standardized values of the variable PEP holders with mayoral electoral years excluding the election of 2015, the year the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.
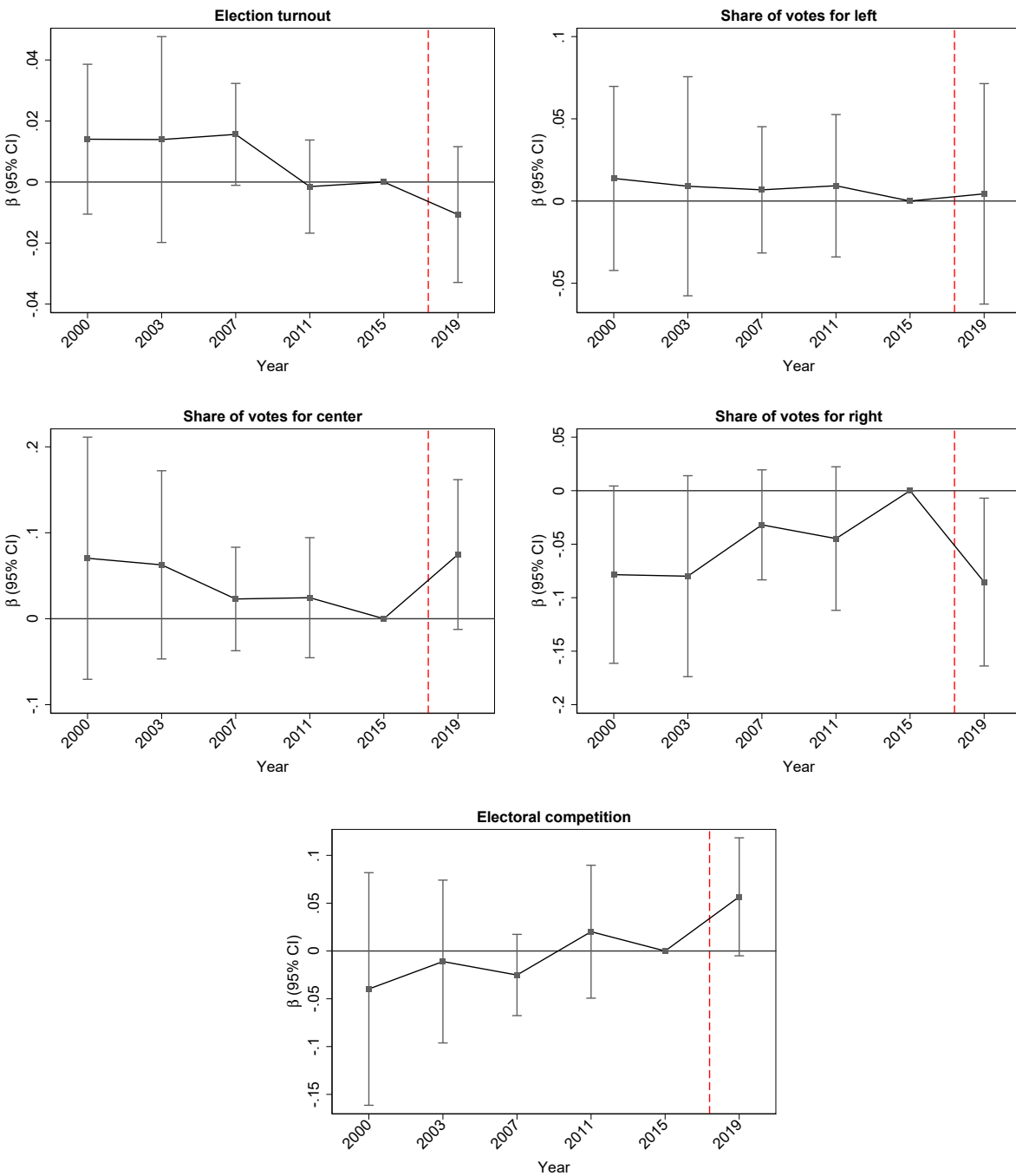
Figure D.8. Parallel Trend Assumption – Mayoral Elections
Continuous Treatment Variable
Tercile 3

Notes: The figure illustrates the results of an event study estimation for the tercile 3 of the PEP holders intensity. We interacted the standardized values of the variable PEP holders with mayoral electoral years excluding the election of 2015, the year the PEP program was implemented. The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Standard errors were clustered at the municipality level and bars represent a 95% confidence interval.

Table D.7. Impacts of PEP Program on Electoral Outcomes by Treatment Intensity
Mayoral Elections

| | Election | Share of Votes for | | | Electoral |
| | Turnout | Left | Center | Right | Competition |
	(1)	(2)	(3)	(4)	(5)
Panel A. Tercile 1 - PEP Holders Municipalities					
$PEP_m \times I(Post2018)_t$	-0.000	-0.004	0.006	-0.002	0.005
	(0.002)	(0.011)	(0.016)	(0.013)	(0.009)
q-values	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
R-squared	0.876	0.482	0.382	0.439	0.398
Observations	3,107	3,107	3,107	3,107	2,977
Panel B. Tercile 2 - PEP Holders Municipalities					
$PEP_m \times I(Post2018)_t$	-0.002	-0.012	0.026	-0.014	0.001
	(0.002)	(0.012)	(0.017)	(0.014)	(0.009)
q-values	[0.763]	[0.763]	[0.763]	[0.763]	[0.763]
R-squared	0.867	0.456	0.375	0.441	0.411
Observations	3,134	3,134	3,134	3,134	3,005
Panel C. Tercile 3 - PEP Holders Municipalities					
$PEP_m \times I(Post2018)_t$	-0.017	-0.002	0.048	-0.048	0.064**
	(0.009)	(0.028)	(0.037)	(0.035)	(0.024)
q-values	[0.124]	[0.310]	[0.166]	[0.166]	[0.042]
R-squared	0.880	0.451	0.383	0.436	0.420
Observations	3,126	3,126	3,126	3,126	3,002
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Department FE x Year	Yes	Yes	Yes	Yes	Yes
Municipal controls x Year	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results of our main specification in equation 1. $I(PEP_m)$ is a discrete variable that takes the value of one if the municipality had positive take-up rates and PEP_m is the standardized value of the ratio of PEP migrants to population. Electoral competition was estimated following Chacón et al. (2006):1 - (% winning candidate - % second-place candidate). The estimates include municipality, electoral year, and department electoral-year fixed effects, and they control for the interaction between electoral year dummies and a set of predetermined municipal characteristics, measured before the beginning of our period and listed in Table C.2. Clustered standard errors at the municipal level are reported in parentheses and False Discovery Rate (FDR) q-value in brackets *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

APPENDIX E: MEDIA DISSEMINATION OF THE PEP PROGRAM

Table D.1. News in National and Regional Newspapers Mentioning
the Permiso Especial de Permanencia

Main topic of the news	Number of the news
Information about PEP program and RAMV census	33
Border Enforcement and Migration Control	14
Causes of Migration and Venezuelan Context	5
Consequences of accepting Velenzuelan Migrants in Colombia	22
Government benefits for Venezuelans in Colombia	10
Situation of Venezuelan Migrants in Colombia	21
Statistics of Venezuelan Migration	15
Total Number	120

Notes: This table presents the total number of news articles found when we searched the *Permiso Especial de Permanencia* or the acronym PEP in the principal newspapers of Colombia in 2018. The newspaper sources used in the analysis were El Tiempo, El Espectador and Publimetro. To complement the analysis, we used the Google news tool to check for regional newspapers.

APPENDIX G: SURVEY EXPERIMENT

Survey Details The sample size was chosen to guarantee two conditions: (i) that the sample represents the population universe with an estimated margin of error of ± 2.5%, and that (ii) we would be able to detect a difference of at least 2% in the primary outcomes between treatment and control groups. The sampling method was probabilistic and stratified using the geostatistical tool of the Colombian Statistics Agency (DANE), which contains information about the urban blocks, locality, and economic strata in Bogotá. It was stratified by gender (female and male), two age groups (21–28 and 29+), and economic strata (high, medium, and low). The number of respondents needed in each subgroup was calculated so that the proportion of respondents in each stratum corresponded to the real proportion of inhabitants in that stratum out of the total sample universe. We only surveyed one member per household. The surveyed individual was the one with the closest birthday date to the day when the survey was carried out. The survey involved face-to-face interviews conducted in Spanish. The participants were asked 35 questions, which took approximately 45 minutes and were structured in 6 modules: (i) sociodemographics characteristics, (ii) social desirability measurements, (iii) list experiment, (iv) attitudes toward Venezuelan migrants, (v) political views on migrant integration policies and voting intentions, and (vi) general knowledge about the PEP program. Respondents answered the modules in the order listed above; this order was carefully chosen to prevent priming the control group before asking about their prosocial behaviors and attitudes towards migrants. The last module was used to assess the control group's knowledge about the PEP program.

Figure G.1. Experiment Booklet (Treatment and Control Groups)

Treatment Group

- 4.6 millones de venezolanos han sido desplazados, 4 de cada 10 viven en Colombia (1.7 millones)

- 281 mil venezolanos indocumentados han sido legalizados a través del Permiso Especial de Permanencia que les da: permiso de trabajo, acceso a programas sociales (Ej: régimen subsidiado de salud) y acceso al sistema financiero.

Fuente iconos: https://www.flaticon.es/iconos-gratis/

Control Group

- 4.6 millones de venezolanos han sido desplazados, 4 de cada 10 viven en Colombia (1.7 millones)

Fuente iconos: https://www.flaticon.es/iconos-gratis/

Table G.1. Survey Experiment: Successful Covariate Balance

Variable	Control	Treatment	P- value
Age	51.007	49.362	0.106
Male [=1]	0.476	0.526	0.107
Ed: Primary school or less [=1]	0.163	0.172	0.696
Ed: Secondary school or less [=1]	0.364	0.370	0.842
Ed: Tchnician, university or more [=1]	0.473	0.457	0.628
Married or Cohabitating [=1]	0.535	0.526	0.785
Economic Strata: Low [=1]	0.498	0.500	0.953
Economic Strata: Medium [=1]	0.308	0.314	0.833
Economic Strata: High [=1]	0.194	0.186	0.746
Employed [=1]	0.899	0.872	0.293
Labor Contract [=1]	0.404	0.440	0.418
Student [=1]	0.132	0.123	0.686
Political Interest [=1]	0.771	0.743	0.291
Voted in mayoral 2019 elections	0.731	0.717	0.610
Voted in presidential 2022 elections	0.789	0.765	0.349
Join F-Test			0.394
Observations	546	494	1,040

Notes: Column (1) presents the control sample mean and Column (2) the treatment sample mean. Column (3) depicts the p-value of the t-test regression. We performed a joint orthogonality test by running a multinomial logit where the dependent variable is the assigned treatment, the explanatory variables are all the covariates in this table, and the base group is the control group. The joint orthogonality test p-value is 0.217. Definition dependent variables: labor contract is an indicator [=1] if the respondent reported a labor contract in his actual job. Political interest is an indicator [=1] if the respondent reported awareness of the current political situation in the country.

Table G.2. Heterogeneous Effects on Voting Intentions by Social Desirability Score

	Vote intention in 2023 Mayoral elections	Vote intention in 2026 Presidential elections
	(1)	(2)
β_1=I(Treat)×I(High Soc. Desirability)	0.057**	0.049**
	(0.023)	(0.022)
β_2=I(Treatment)	-0.025	-0.025
	(0.023)	(0.022)
Social Desirability Diff. Effect= $\beta_1 + \beta_2$	0.032	0.024
	(0.033)	(0.032)
R-squared	0.024	0.018
Observations	1,040	1,040

Notes: This table reports an OLS estimate interacting the treatment variable with the Social Desirability Score (SDS). The Social Desirability Score (SDS) is a measure of the individual's propensity to report socially desirable answers. High SDS refers to having an above-median score among all participants. Dependent variables in Columns (i)–(ii) are indicator variables [=1] if the respondent had the intention to vote in the next mayoral election in 2023 and in the next presidential election in 2026. All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table G.3. Heterogeneous Effects on Social Capital by Social Desirability Score

	Positive Reciprocity Index	Negative Reciprocity Index	Altruism	Trust
	(1)	(2)	(3)	(4)
β_1=I(Treat)×I(High SDS)	-0.054	-0.069	-0.000	0.043
	(0.061)	(0.061)	(0.061)	(0.061)
β_2=I(Treatment)	0.019	-0.023	-0.017	-0.036
	(0.061)	(0.061)	(0.061)	(0.061)
SDS Diff. Effect= $\beta_1 + \beta_2$	-0.035	-0.092	-0.017	0.007
	(0.087)	(0.086)	(0.086)	(0.086)
R-squared	0.031	0.066	0.027	0.019
Observations	1,040	1,040	1,040	1,040

Notes: This table reports an OLS estimate interacting the treatment variable with the Social Desirability Score (SDS). The Social Desirability Score (SDS) is a measure of the individual's propensity to report socially desirable answers. High SDS refers to having an above-median score among all participants. The variable in column (i) is an index constructed using the methodology of Kling et al. (2007) and the answer on a 1 to 5 scale of the approval of the statement: *when someone does me a favor, I am willing to return it*, and the answer regarding the hypothetical money the respondent may give to a stranger as a thank-you for helping him on the street; (ii) is an index constructed using the methodology of Kling et al. (2007) and the reported answer on a 1 to 5 scale of the approval of the following statements: *How willing you are to punish someone who treats you unfairly, even when there are risks to you of personal consequences*; *How willing are you to punish someone who treats others unfairly, even when there are risks to you of personal consequences*; and *If I am treated very unfairly, I will take revenge on the first occasion, even if I have to pay a cost for it*; (iii) is the standardized reported answer on a 1 to 5 scale of the approval of the statement: *How willing are you to donate to charitable causes without expecting anything in return*; (iv) is the standardized reported answer on a 1 to 5 scale of the approval of the statement: *I always assume that people have only the best intentions*. All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table G.4. Heterogeneous Effects on Altruism Towards Migrants by Social Desirability Score

	Money will kept by their-self	Money will share with vulnerable Venezuelan	Money will share with vulnerable Colombian
	(1)	(2)	(3)
β_1=I(Treat)×I(High SDS)	-0.067	0.004	-0.007
	(0.060)	(0.045)	(0.032)
β_2=I(Treatment)	0.004	-0.029	-0.043
	(0.063)	(0.044)	(0.032)
SDS Diff. Effect= $\beta_1 + \beta_2$	-0.064	-0.025	-0.050
	(0.092)	(0.062)	(0.045)
R-squared	0.032	0.018	0.022
Observations	1,040	1,040	1,040

Notes: This table reports an OLS estimate interacting the treatment variable with the Social Desirability Score (SDS). The Social Desirability Score (SDS) is a measure of the individual's propensity to report socially desirable answers. High SDS refers to having an above-median score among all participants. Dependent variables are the logarithm of the answer of the respondent about the distribution of 5,000 Colombian pesos between him (Column (i)), a vulnerable Venezuelan migrant (Column (ii)), and a vulnerable Colombian (Column (iii)). All the columns control for sex (female and male), two age groups (21– 28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table G.5. Heterogeneous Effects on Political Attitudes towards Migrants by Social Desirability Score

	Colombian government has to help Venezuelans	In favor to a law that helps Venezuelans	Venezuelans compete with Colombians jobs	Venezuelans increase crime	Venezuelans improve Colombian culture	Positive effects of Venezuelans in Colombia
	(1)	(2)	(3)	(4)	(5)	(6)
β_1=I(Treat)×I(High SDS)	-0.018	0.032	0.064	-0.091	0.133**	0.007
	(0.060)	(0.060)	(0.061)	(0.063)	(0.062)	(0.031)
β_2=I(Treatment)	0.026	0.024	0.064	-0.066	-0.052	-0.028
	(0.060)	(0.061)	(0.061)	(0.063)	(0.062)	(0.031)
SDS Diff. Effect= $\beta_1 + \beta_2$	0.008	0.056	0.129*	-0.157	0.081	-0.021
	(0.085)	(0.085)	(0.086)	(0.089)	(0.088)	(0.043)
R-squared	0.033	0.036	0.008	0.027	0.032	0.028
Observations	1,040	1,040	1,040	1,040	1,040	1,040

Notes: This table reports an OLS estimate interacting the treatment variable with the Social Desirability Score (SDS). The Social Desirability Score (SDS) is a measure of the individual's propensity to report socially desirable answers. High SDS refers to having an above-median score among all participants. Dependent variables in columns (i)–(vi) are the standardized reported answer on a 1 to 5 scale of the approval of the statements:(i) *The Colombian government is obliged to help Venezuelan migrants*, (ii) *Would vote for a policy to increase government spending to help Venezuelan migrants*, (iii) *Venezuelan migrants come to compete for our jobs*, (iv) *Venezuelan migrants increase crime*, and (v) *Venezuelan migrants improve Colombian society by bringing new ideas and cultures*. All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table G.6. List Experiment

	List Experiment
	(1)
β_1=I(Treatment)×I(List Treatment)	0.176
	(0.112)
β_2=I(Treatment)	-0.059
	(0.080)
List Treatment Diff. Effect= $\beta_1 + \beta_2$	0.117
	(0.078)
R-squared	0.050
Observations	1,040

Notes: The table depicts the results of the listing experiment randomly assigned to all participants. The respondents were asked: how many of these individuals would you not want to have as neighbors?. "[Please respond how many, not which of them]." The options were: "a. Abusive people"; "b. People in poverty"; "c. People who profess a religion different from yours"; "d. Venezuelan migrants." Option d was the statement assigned randomly so that only half the participants got this statement. The variable List Treatment is an indicator variable [=1] if the respondent received option d on the questionnaire. This table reports OLS estimates from equation xx. All the columns control for sex (female and male), two age groups (21–28 and 29+) and three economic strata (high, medium, and low). *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.