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# Local externalities in labor markets: congestion and information flow among peers

**Lucila Berniell**[1] | **Dolores de la Mata**[2] | **Federico Juncosa**[3]

[1]CAF-development bank of Latin America. Research department.
mberniell@caf.com

[2]CAF-development bank of Latin America. Research department.
mdelamata@caf.com

[3]CAF-development bank of Latin America. Research department.
cjuncosa@caf.com

We explore local externalities in labor markets, exploiting the random assignment of a large-scale internship program in Argentina. Examining the probability of registered employment in the 12 months after the program, we find that applicants are affected by two opposing external effects: those whose closest applicant received the internship have an employment rate 1.8 percentage points higher than the neighbors of non-beneficiary applicants, while those who face the top decile of program saturation in their neighborhood show an employment rate 2.98 percentage points lower than those in the first decile. We posit that the first effect is due to the transmission of relevant labor market information among peers, while the second is due to the increased competition from program beneficiaries. These findings are robust to different specifications and varying neighborhoods.

**KEYWORDS**

Labor market frictions, Networks, Externalities, Displacement effects

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# Externalidades locales en mercados laborales: congestión y transmisión de información entre pares

**Lucila Berniell**[1] | **Dolores de la Mata**[2] | **Federico Juncosa**[3]

[1]CAF-banco de desarrollo de América Latina. Dirección de investigaciones socioeconómicas.
mberniell@caf.com

[2]CAF-banco de desarrollo de América Latina. Dirección de investigaciones socioeconómicas.
mdelamata@caf.com

[3]CAF-banco de desarrollo de América Latina. Dirección de investigaciones socioeconómicas.
cjuncosa@caf.com

Estudiamos la presencia de externalidades locales en mercados laborales, explotando la asignación aleatorizada de un programa de pasantías en Argentina. Examinando la probabilidad de empleo registrado en los 12 meses posteriores a la culminación del programa, encontramos que los postulantes se ven afectados por dos externalidades contrapuestas: aquellos cuyo postulante más cercano recibió la pasantía tienen una tasa de empleo 1,8 puntos porcentuales más alta que los vecinos de postulantes no beneficiarios, mientras que aquellos que se enfrentan al decil superior de saturación de beneficiarios del programa en su vecindario muestran una tasa de empleo 2,98 puntos porcentuales inferior que los del primer decil. Estos hallazgos son robustos a controles socioeconómicos y a definiciones de vecindario variables. Sostenemos que el primer efecto se debe a la transmisión de información relevante sobre el mercado laboral entre pares, mientras que el segundo se debe a la mayor competencia de los beneficiarios del programa.

**KEYWORDS**
Fricciones del mercado laboral, Redes, Externalidades, Efectos de desplazamiento

## 1 | INTRODUCTION

Labor markets are paradigmatic examples of the importance of information asymmetries in equilibrium outcomes. These asymmetries are more salient for workers with little or no previous contact with employers, as in the case of young workers, not only because the young person seeking employment is unaware of many relevant aspects of the labor market (wages to which they can aspire, quality of potential employers, etc.), but also because an empty curriculum vitae conveys little information to the employer about the potential productivity of the person applying for a vacancy (Pallais, 2014). In absence of other sources of information, job-seekers often use their networks of contacts, family and friends, to learn about the job opportunities available, and use referral mechanisms to convey information to potential employers about their productivity (Schmutte, 2014).

On the other hand, labor markets are affected by significant spatial frictions that make distances and transit alternatives play a decisive role in the number of vacancies that open and the candidates that occupy them. These spatial frictions entail that there is a local aspect to labor markets, since people restrict their job-search effort to small areas of the city, particularly those that are more accessible and where they perceive less competition from potential competitors (Manning and Petrongolo, 2017).

In this paper we present causal evidence about the existence of the two mechanisms described above: the transmission of information about job opportunities among peers and the presence of local congestion. To study these mechanisms we rely on a large active labor market program (ALMP) in Argentina, called the First Step Program (PPP, for it's Spanish acronym). The program randomly assigns 12 months of a paid internship to beneficiaries, who significantly increase their likelihood of employment after the end of the benefit, as documented in Berniell and de la Mata (2017).[1] The evidence is constructed using exposure of applicants to two sources of exogenous variation: close neighbors and fraction of neighbors in the same local labor market that obtain –by chance– a job opportunity.

We find significant positive and negative external effects of the PPP program. First, being exposed to a close neighbor that received the PPP benefit increases the probability of employment, which we refer as the *information effect* of the program. Second, we find evidence showing the existence of a *competition effect*, that is, the negative effect caused by the treatment saturation among applicants residing nearby. These findings are consistent with the existence of local labor markets, and are robust to different specifications and varying neighborhoods.

The present work is linked to two branches of literature. First, there is a growing body of studies that emphasize the importance of the transmission of information through networks of contacts. For instance, Calvó-Armengol and Jackson (2004) considers a model where agents share information about job vacancies through a network of contacts and conclude that, in such a context, ALMPs would have local increasing returns to scale. Bayer et al. (2008), meanwhile, present empirical evidence on the existence of this mechanism in the context of workers residing in the City of New York. Assuming that individuals can choose the neighborhood where they live, but cannot choose a specific block within that neighborhood, the authors find a greater likelihood of a shared employer among workers who reside close to each other. They attribute this effect to the existence and use

[1] The PPP offers 12-month apprenticeships in firms operating in the formal sector of the economy, in which job quality is considerably better than in the informal sector. As shown in Berniell and de la Mata (2017), the PPP increases the probability of formal but not of informal employment of former beneficiaries. Importantly for this work, formal jobs are registered with the tax authority and we will use the resulting administrative records to compute impacts and external effects on the probability of employment. Therefore, from now on, every time we describe results regarding employment or work experience we refer to formal employment or formal work experience.

of connections between neighbors to seek employment. Hellerstein et al. (2014) develop a metric to assess the effects of networks on labor markets for different subgroups of the population and find that they are more prevalent among low-skills individuals and operate more strongly among people of the same race.

Second, this article is related to studies on the effectiveness of ALMPs to improve labor outcomes, particularly those that take displacement effects into account as a form of negative externality. The seminal work by Crépon et al. (2013) carries out an experimental field study of the impact of assistance services in the search for employment that allows for the identification of externalities by randomizing, not only job-seekers' treatment status, but also, the program saturation across cities. The authors find that part of the beneficial effects of the program are due to the displacement of non-beneficiaries, this externality being more important in the case of more competitive labor markets.

Our results provide three main contributions to the existing literature. First, this work contributes to empirical studies on referral mechanisms and information transmission between peers that use geographical proximity as a proxy for social ties. These studies usually rely on strong assumptions to deal with the problem of endogeneity in residence decisions. Our work, on the other hand, presents a clearer strategy of causal identification of the effects of interest, using three key characteristics of the PPP: the random assignment of a large number of beneficiaries, its positive direct impact on their employment rate, and the geographical proximity of applicants. These characteristics imply exogenous changes in relevant features of local labor markets, such as the amount information available through social contacts and local market tightness. To the best of our knowledge, this is the first attempt to measure these effects in the field, and in particular for the youth in developing countries, for whom information frictions in the labor market are particularly important.

Second, this article contributes to the literature that seeks to quantify displacement effects of ALMPs, in line with the work of Crépon et al. (2013) and the more recent of Berniell and de la Mata (2017), Marinescu (2017) and Abebe et al. (2017). Our specific contribution consists of disentangling two types of externalities or spillover effects that occur on a smaller geographical domain than those considered in the previous literature. These findings highlight the need to consider negative as well as positive external effects in the design and evaluation of ALMPs, since these externalities not only affect the impacts themselves but also challenge the causal interpretation of impact evaluations due to violation of the Stable Unit-Treatment-Value Assumption (SUTVA; Rubin 1980). Except in the special case of perfect balance between the two opposing forces, studies that omit them may report biased estimators.

Finally, this work contributes to the impact evaluation literature of youth employment programs, particularly those focused on developing countries. This literature has been recently summarized in McKenzie (2017) and Card et al. (2018), among others.

## 2 | INSTITUTIONAL FRAMEWORK: THE PPP PROGRAM

The PPP is an internship program administered by the Secretariat of Equity and Employment in the province of Cordoba (Argentina), whose main objective is to facilitate youth entry into the labor market. The Secretariat randomly selects beneficiaries among eligible candidates and provides them with 12 months of salaried employment. Young beneficiaries to the program receive the stipulated amount payed by the Secretariat, conditional on working 20 hours per week. The amount of the subsidy for the 2012 edition, analyzed in this paper, was around 90% of the legal minimum hourly wage [2].A distinctive aspect of the program is

[2]The gross monthly minimum wage was equivalent to $ 585 in June 2012.

that it seeks to improve the employability of young people through work experience in the formal sector of the economy, but does not require an instance of classroom training, apart from the one employers may impart at the time of incorporating the young intern to the firm. In addition to the employee salary subsidy, there is a benefit for firms associated with the PPP hiring requirements, which exempts them from the non-salary and administrative costs that formally hiring and/or dismissing a worker normally entail. In addition, firms are not required to continue with the employment relationship after the end of the 12-month program.

Eligibility conditions for young candidates consist of being between 16 and 25 years of age at the time of applying to the program, having a legal address in the province of Cordoba and not having been registered as a formal worker in the six months prior to the application deadline.[3]. Regarding firms, all those that are formally registered with the tax authority and that have at least one registered employee are eligible.

The PPP was launched for the first time in 1999 and kept repeated editions every year until 2007. During that period, the program maintained characteristics similar to the edition under study, with the main difference that the selection of beneficiaries was made in a fist-come first-served basis until the available quotas were filled. The PPP was suspended after 2007 and remained inactive until it was relaunched in 2012. From then on, beneficiaries were selected at random from a pool of applicants, which outnumber available quotas by about 3 to 1.[4] The selection of the beneficiaries is made through a public draft in the Lottery of the Province of Cordoba, which occurs annually in the month of May. To participate in the draw, applicants must submit an enrollment form collecting their personal data and that of the firm where they intend to work. This form must also be supported by the firm where the candidate would perform its functions if selected.

## 3 | DATA

This article uses data from three sources. First, administrative data of the program, provided by the Secretariat of Equity and Employment Promotion (former Employment Promotion Agency of Cordoba). This government agency collects data from the program registration forms, including sociodemographic characteristics of the applicants, such as sex, age, marital status, number of children, educational level, enrollment at educational institutions, among others. It also includes information about the firm that supports the candidate's application, such as their legal name, activity sector and number of formally registered employees. We keep all records corresponding to firms in the province's capital, the city of Cordoba, which is the second largest City in Argentina.[5]

A distinctive feature of the program's administrative records is that they include the residential address reported by the applicants and the address of the establishments where they applied. From these data, and making use of the Google Maps Application Programming Interface (API), we obtained the geographic coordinates of place of residence and place of

[3]As reported Berniell and de la Mata (2017) using data from the Permanent Household Survey, the set of applicants to the PPP program does not differ statistically in its observable characteristics of the population of young people of the same age group in Cordoba.

[4]The allocation mechanism includes quotas that imply that the probability of selection is not uniform among applications. First, there is a limit to the maximum number of beneficiaries per firm depending on the number of formally registered employees at the time of registration. Second, candidates can apply to multiple positions although they can be a PPP beneficiary in only one of them. Since these dimensions affect the selection probability, they are included as controls in all the specifications reported in the following sections.

[5]The city of Cordoba, the province's economic powerhouse, had a population of 1.39 million at the time of implementation of the edition under study (INDEC, 2010).

intended work detailed in each file, as well as the distance and travel-time between them.[6] This process had a high level of success: out of 8882 individuals who filled eligible forms in the city of Cordoba, we obtained 7339 geolocations.[7] Following the same procedure, we obtained the coordinates of 2998 firms, which amounts to 84% of firms in the city of Cordoba that received one or more PPP applications.

Table 1 presents the socioeconomic characteristics of the sample. Of the 7339 individuals who presented an eligible form and who were successfully geolocated, 51% are women, and their average age is 21. In addition, 95% are single, 69% of those over 18 have completed high school while 8% of those over 21 have completed higher education at the time of registration. Although it was possible to apply to the program online, around 51% of applicants completed their application manually. Finally, we report the rate of (registered) employment in the months prior to the start of the program, which is close to zero consistent with the eligibility criteria. As shown in the table 2, out of the 7339 individuals in this sample, 2604 were selected in the draft to receive the benefit, ie 35.5%. Compliance with the program assignment is notoriously high: 84.4% of those selected complete at least 2/3 of the internship, while 79% complete it fully.[8]

Figures 1 and 2 show the location of young people and firms applying to the program, respectively. In the figure 1, the beneficiary youngsters appear in blue, while those who were not selected appear in white. The figure shows that applicants are scattered throughout the urban area of the city, as well as beneficiaries, as a result of random assignment. Figure 2 shows a salient agglomeration of economic activity in the center of the city, while another important fraction of establishments are located along the main roads.

An important message of Figure 1 is that there is a high density of applicants in the city. Table 4 summarizes this fact, showing the distribution of distances between each candidate's residential address and that of its first, tenth and fiftieth nearest program candidate. The nearest one lives 62 meters away on average, while 75% of candidates have their nearest applicant less than 90 meters away from their place of residence. Even considering the fiftieth nearest applicant of each candidate, the average distance between them is 709 meters. Table 5 shows the density of applicants and beneficiaries in radii of 100, 500 and 1000 meters away from the residential address of each applicant. The exposure of candidates to nearing beneficiaries of the program in their vicinity is notorious: on average, each applicant to the program has about 16 beneficiaries within a radius of 500 meters from their place of residence.

## 4 | EXTERNALITIES AND LOCAL LABOR MARKETS

The high density of PPP candidates and beneficiaries described in the previous section draw attention to the potential externalities that may result from implementing an active labor market policy in the scale of this program. This is because the program acts upon agents that interact with each other across a social network of friends and acquaintances, and who face significant spatial and informational frictions when accessing employment opportunities. These observations lead us to depart from a canonical labor market framework and consider the presence of positive and negative externalities.

First, there may be positive externalities due to social links between candidates and infor-

[6] This procedure required a first stage of cleaning and structuring the information, in which typing errors were corrected, non-alphanumeric characters were eliminated and the street terminology was standardized.

[7] Table A.1 in the appendix shows the number of geolocated individuals and the differences in observable characteristics relative to non-geolocated individuals.

[8] Among candidates not initially selected by the draft, a small percentage (less than 3%) managed to become beneficiaries of the program, presumably by appealing their rejection.

mational labor market frictions. Previous evidence on the effects of the PPP suggests that the main mechanism for its positive impact on employment is by a reduction in informational barriers (Berniell and de la Mata, 2017). The program may act upon informational barriers by providing beneficiaries with relevant information regarding the returns to salaried employment and job-search strategies. In turn, the signal of having held 12 months salaried employment in a formal-sector firm may be informative for potential employers on candidate types. However, both of these channels may also occur by the action of peers: a friend may be a relevant source of information on salaried employment for her peers and on the candidate types for firms by providing referrals for their acquaintances. Consequently, by relaxing informational barriers to employment for PPP beneficiaries, the program can also improve labor outcomes for beneficiaries' friends who receive a second-hand treatment.

Second, there may be negative externalities due to displacement. Within a job-matching framework, we can think of the PPP as causing a reduction on the costs of exerting search intensity for job-seekers who benefited by the PPP –directly or through their peers– which results in a higher optimal search intensity (Pissarides, 2000). The resulting increase in job-search intensity by a fraction of the population leads to a higher level of employment in equilibrium or, equivalently, in a higher probability of employment for the average worker. In turn, a higher level of employment carries an efficiency improvement in the efficiency of the matching function that leads to a tighter labor market, which bounds the creation of new employment (we refrain form considering changes in firms' choices at this point). If a fraction of workers chooses a higher search intensity in a tight labor market, their improved labor outcomes must partly come at the expense of the remaining workers.

The potential for externalities as described above is emphasized by the presence of spatial frictions. Even though cities, or metropolitan areas, are usually considered as a single labor market, the spatial structure of a city together with non-negligible pecuniary and non-pecuniary transportation costs have important consequences for the labor market. These affect the flow of information –labor-related or otherwise–, job-search patterns, social relationships, among others.

Figure 4 presents evidence on the role of space on job search, showing the correlation between proximity between candidates and the probability of same-firm applications. The blue line shows the ratio between the observed frequency of same-firm pairs in groups of the K nearest applicants and the relative frequency of same-firm applicants among candidate pairs taken at random. The red line shows the gradient of this metric, that is, how that ratio falls as larger groups of neighbors are considered. We observe that the probability of same-firm applicants among groups of five closest neighbors is about ten times greater than among candidate pairs taken at random.[9].

The pattern shown in Figure 4 may be due to two reasons. The first is the presence of significant transportation costs within the city that leads candidates to only consider commuting to positions nearby. Figure 3 provides further evidence on this, showing the distribution of times in public transit between worker-firm pairs observed in the applications to the program, compared to the times between worker-firm pairs taken at random. The figure shows that candidates' applications are strongly biased towards more accessible firms: the observed commute in public transit between candidates' residential address and their proposed establishment is 42 minutes on average, 23% lower than for worker-establishment pairs taken at random. The second reason is that it is likely that individuals who live nearby share personal characteristics that lead them to apply to firms of the same category or type.

Given the evidence on the importance of these spatial frictions, we propose to study the workings of both externalities –positive informative externality and negative competitive externality– in a local environment. To this aim the PPP and its detailed geographic data

[9]The baseline probability that a random pair of individuals from the sample apply to the same firm is 0.37%.

provides a valuable setting. Its draw of beneficiaries among applicants to the program results in an exogenous source of variability that acts upon a social network and alters the spatial equilibrium of the labor market. First, it randomly assigns a 12-month work experience that is verifiable because it is a formal employment relationship. After the end of the program, beneficiaries have, on average, a better CV due to more certifiable work experience to show to potential employers. In fact, program assignment causes a 25% increase in probability of employment one year after the end of the program, considering the average impact in the whole province (Berniell and de la Mata, 2017). Second, the program acts upon a social network, affecting beneficiary and non-beneficiary job-seekers by means of their mutual connections in the network. Third and last, the PPP randomly affects the competitive environment of candidates, who find different levels in average work experience among their close competitors and, hence, experience better or worse job market outcomes due to the program. In what follows, we will call these three channels *direct effect*, *informative effect* and *competitive effect*, respectively.

## 5 | IDENTIFICATION

In order to evaluate the potential contagion effects we first present the ideal experiment that would allow us to isolate both effects. Second, we present our strategy to identify those effects in the context of the PPP program, taking advantage of the locality of labor markets within the city and the random nature of the assignment of the benefits, which exogenously changes the local spatial equilibrium

To evaluate the hypotheses presented, i.e. the presence of an informative and a competitive effect, in the context of an ideal experiment, the researcher should alter the social network of each young candidate, by way of randomly choosing if they will be assigned to the treatment or control of the two following interventions: the first one referred to assigning salaried work experience to his closest friend, while the second refers to improving the curriculum of all job applicants living in their environment. In this way, four groups of individuals would be formed: controls in the first and second treatments (CC group), those whose closest friend **has no salaried employment experience** and are surrounded by job applicants with **low work experience**; TC group, those whose closest friend has **high work experience** and are surrounded by individuals with **low work experience**; CT group, those whose closest friend **high no work experience** and are surrounded by individuals with **high work experience**; and TT group, those, whose closest friend **high work experience** and who are surrounded by individuals with **high work experience**. Equipped with this experiment, the researcher could fit the following model:

$$E_i = \alpha + Exp_i^f \Gamma_1 + S_i \Gamma_2 + Exp_i^f S_i \Gamma_3 + u_i$$

where $E_i$ is a variable indicating the employment status of agent $i$, $Exp_i^f$ is the work experience of candidate $i$'s friend, and $S_i$ is the characterization of his competitive environment, taking value 1 if job-seekers in this environment have high work experience, and 0 otherwise. In this context, $\alpha$ captures the employment rate of individuals who did not receive any treatment, group CC, while $\Gamma_1$, $\Gamma_2$ and $\Gamma_3$ capture the increase in the employment rate that would result of assigning a candidate treatment 1 only, treatment 2 only, and finally, the joint impact of both treatments, respectively. The hypothesis posed on the existence of an informative effect implies that $\Gamma_1 > 0$, while the existence of a local competitive effect implies that $\Gamma_2 < 0$. In a more general design, it would be worthwhile to explore the effect of variable intensities of competitive environment, that is, when $S$ takes continuous values.

Finally, the theoretic framework of the present study is agnostic regarding the sign of $\Gamma_3$, the interaction between both treatments.

The available data allow us to approximate the ideal experiment, since the random selection of beneficiaries generates exogenous variability in the labor market's spatial equilibrium. Next, we discuss the strategy carried out to estimate each effect and the extent to which it departs from the ideal setting. First, studying the channel of transmission of information via a friend or connection's work experience, requires data on the personal relationships of candidates. In the absence of information on the applicants' social network, we infer a link between candidates based on proximity using their residential address: we consider the effect of the PPP beneficiary status of a candidate's nearest neighbor. The strategy of inferring a social network using geographic proximity is common in studies of networks and labor markets, such as Hellerstein et al. (2014) and Schmutte (2014).

Regarding the competitive effect, the program provides a valuable framework since it introduces exogenous variability in the employability of a candidates most likely competitors, those that live nearby and share her local labor market. Although the program was not explicitly designed to study the effects of a greater saturation of treatment within cities, there is sufficient sample variability in the fraction of beneficiaries in candidates' neighborhoods. The measure of competitive environment is then constructed as the share of beneficiary candidates to the program among the nearest $K$ applicants. The choice of this parameter $K$, that determines the breadth of the environment considered relevant for job-seekers is somewhat arbitrary. As a guide to choosing the relevant environment, we go back to Figure 4 since it shows how the probability of young neighbors postulating to the same firm decreases as the neighborhood considered expands, which is the basis for the existence of displacement. We choose $K = 15$, where the slope flattens.[10] Table 6 shows the sample variability exploited in the present study, for parameter values 10, 15 and 20. For our chosen value of $K = 15$, the observed fraction of beneficiary candidates among closest neighbors in the 10th and 90th percentiles is 0.20 and 0.53, respectively.

There are three more considerations in the empirical approach to take into account. First, the sample includes all georeferenced applicants, among whom around a third were beneficiaries to the program. Given the relevance of the first order effect of the PPP, the direct effect of the program is included in all specifications. The complete specification that results from the joint estimation of three interventions interacted –the first-order effect of the program, the information effect of the nearest neighbor, and the competitive effect of the fraction of beneficiaries among the nearest 15 neighbors– results in seven coefficients of interest: three direct effects, three double interactions and a triple interaction. This brings us to the second consideration: due to the lack of power of the sample to identify all the parameters jointly, our analyses focus on the estimation of the direct effects, omitting the interactions of the model. Therefore, the identified coefficients should be interpreted as the average effects of each variable across the treatment and control groups of the remaining variables, weighted by the fraction of individuals in each group. Third and last, our analysis will focus on the study of the impacts during the twelve months after the end of the program.

We fit the following specification for the joint estimation of the three effects: first order effect of the program, informative effect of the nearest neighbor's beneficiary status, and competition effect of program saturation in the relevant local labor market. We fit this specification separately for each month in order to avoid introducing structure to the evolution of the employment rate and to allow variable effects over time. The implemented

[10] The choice of the number of neighbors does not significantly alter the results. As a robustness exercise, we run every specification for parameter values $K = 10$ and $K = 20$ in Section 7.

model is:

$$E_i = \hat{\alpha} + T_i \hat{\gamma_1} + T_i^{vec} \hat{\gamma_2} + S_i^K \hat{\gamma_3} + Q_i \hat{\delta_1} + Q_i^{vec} \hat{\delta_2} + \overline{QN}_i \hat{\delta_3} + u_i \tag{1}$$

where $E_i$ is the employment rate of individual $i$, $T_i$ represents the beneficiary status of individual $i$ (time-invariant attribute), $S_i^K$ is the share of treated of individuals among the nearest $K$ candidates. We include a vector of stratification variables $Q_i$, that affects the individual's likelihood of being selected as beneficiary in the draft: number of files submitted, number of employees of the company to which she applies and the ratio between the number of applicants and the number of employees in the firm. Likewise, vectors $Q_i^{vec}$ and $\overline{QN}_i$ correspond to these quota variables of the nearest neighbor and the averages of the nearest $K$ applicants, respectively.

Table 7 shows balance statistics in observed characteristics for the variables *program beneficiary*, *nearest neighbor beneficiary* and *fraction of beneficiaries in 15 nearest candidates*. We observe that, for most personal characteristics and for the outcome variables in the months prior to the start of the program, there are no significant imbalances. In the case of the program beneficiary status, there is a slight imbalance in variables *has children*, the frequency being 1.4 percentage points (pp) lower for beneficiary applicants compared to non-beneficiary applicants, and *higher education*, 3.7 pp more frequent among beneficiaries. Regarding the nearest neighbor program status, those whose nearest applicant is a program beneficiary are single with a frequency of 1.2 pp greater, have 2.3 pp higher rate of completion of secondary studies and 2.1 pp higher rate of manual application to the program. None of these differences are economically relevant, given that they represent a change of 4% at most with respect to the sample mean. No imbalances are observed in individual characteristics for the fraction beneficiaries among the 15 nearest candidates.

For the three variables under study, we find minor but statistically significant imbalances in socioeconomic neighborhood characteristics. Candidates who were beneficiaries, have beneficiary nearest neighbor or have a larger fraction of beneficiaries in their environment, show better neighborhood indicators. The differences observed are statistically significant, although economically small: the greatest difference is observed in the labor informality variable, 0.8 pp lower for PPP beneficiaries, 1.1 pp lower for neighbors of a PPP beneficiary and 8.9 pp lower for individuals with a percentage of beneficiaries in their environment of 100% (15 of 15), with respect to those who have 0 treated neighbors in their environment. The magnitude of this last coefficient should be interpreted with caution because it refers to the comparison between those who have a fraction beneficiaries among their neighbors of 1 versus 0, even though that variability does not occur in the sample. Considering individuals with an average program saturation (of 0.35, the fraction of beneficiaries in the city of Cordoba as a whole) they show a rate of informality in the neighborhood 3.1 pp lower than those who have zero treated in their environment.[11]

Due to the presence of these small imbalances we include robustness exercises in Section 7, where we report estimates for the following specification, including the full-set of available

[11] The presence of imbalances in observable characteristics in our sample demands careful consideration as it poses a potential threat to our identification strategy. For example, if the socio-economic status of beneficiaries to the program is higher than that of non-beneficiaries, we will observe higher labor formality rates between the former, not only because they received the program, but also because they were more likely to secure employment in the formal sector to begin with. In that case, the observed imbalances may imply that our estimation of the information effect is biased upwards due to socio-economic clustering, and should be regarded as an upper bound. However, they would also tend to reinforce our findings on the competition effect (fraction of beneficiaries in neighborhood variable), because while a larger fraction of beneficiaries in the proximity should imply a lower probability of formal employment, it would also be associated with better socioeconomic indicators in the neighborhood, which imply a higher probability of formal employment.

individual and neighborhood controls:

$$E_i = \hat{\alpha} + T_i\hat{\gamma}_1 + T_i^{vec}\hat{\gamma}_2 + S_i^K\hat{\gamma}_3 + X_i\hat{\beta} + Y_i^{barr}\hat{\eta} + Q_i\hat{\delta_1} + Q_i^{vec}\hat{\delta_2} + \overline{QN}_i\hat{\delta_3} + u_i \quad (2)$$

where $X_i$ are individual characteristics (sex, age, marital status, children and education) and $Y_i^{barr}$ are characteristics of the neighborhood of residence of individual $i$: fraction of households with UBN, unemployment rate and informality rate. This produces no relevant changes to our estimations.

## 6 | RESULTS

Table 8 and Figure 6 show the three coefficients of interest: direct program effect, informative effect and competitive effect, resulting from specification 1 for each month up to 12 months after program completion. In the first place, we observe that the sample mean employment rate in this period is increasing, starting at 17.4% in July 2013 and reaching 23% 11 months later. Second, regarding the direct effect of receiving the PPP benefit, column 1 shows a positive significant effect for all the months reported. Beneficiaries have, on average, a employment rate 5.08 percentage points higher than non-beneficiary individuals, which represents 24% relative to the employment rate observed in the entire sample (column 2).

Column 4 of Table 8 shows the informative effect on employment. The coefficient is consistently positive throughout the analyzed period, and is statistically significant in 8 of the 12 months reported. Individuals whose nearest neighbor was a PPP beneficiary show, on average for the entire period reported, a 1.8 pp higher employment rate than the rest, which represents 8.7% of the average employment rate. Considering that this affects 35% of the sample, this amounts to a 0.64 percentage points aggregate employment rate increase.

Finally, column 7 of Table 8 shows that individuals in a more competitive environment have, on average, a lower employment rate. We observe negative coefficients for all months, while they are statistically significant for ten of the twelve months in the sample. The estimated coefficient is -8.9 pp on average for the entire period and reaches a minimum of -12.9 pp six months after the end of the program. A back of the envelope calculation to assess this effect in light of the variability observed in the sample reveals that those who face the top decile of program saturation in their proximity (with 53% of beneficiaries among their 15 closest applicants, see table 6), show an average employment rate 14.01% (2.98 percentage points) lower than candidates in the first decile (with 20% beneficiaries)[12].

The empirical analysis so far focuses on studying the average of the first order effect of the PPP program and the two contagion effects –information and competence– omitting the interaction between these three variables in both specifications. Below, we present the results of a completely interacted model in these variables, obtained from the following equation:

$$\begin{aligned} E_i = \alpha_0 + T_i\psi_1 + T_i^{vec}\psi_2 + S_i^K\psi_3 \\ + T_iT_i^{vec}\psi_4 + T_iS_i^K\psi_5 + T_i^{vec}S_i^K\psi_6 + T_iT_i^{vec}S_i^K\psi_7 \\ + Y_i^{barr}\eta + Q_i\delta_1 + Q_i^{vec}\delta_2 + \overline{QN}_i\delta_3 + \nu_i \quad (3) \end{aligned}$$

Table 9 and Figure 7 present the results of this specification, excluding and including

[12]This results from considering the difference between the fraction of beneficiaries in the first and last decile (33.3%, see table 6), more precisely, it is the average across the reported months of: $\left[(S^{15}_{p90} - S^{15}_{p10})\hat{\gamma}_3/\overline{E}\right]$, where $\overline{E}$ is the average employment rate of the entire sample in that month.

controls for individual and neighborhood characteristics.[13] The results of this exercise are consistent with the findings described so far, albeit with a significant loss of power and with less stable estimators between months. We observe important direct program effects, larger than those obtained in previous specifications, with an average value for the entire period of 8.6 pp. The first interference effect described $\psi_2$, caused by allocation of the PPP benefit to a candidate's nearest neighbor has a positive impact that averages 5.6 pp for the full period. These coefficients are significantly higher than those obtained in specification 1, although they are statistically significant in just four of the twelve months under study. The last direct effect, the competitive effect caused by program saturation among the 15 closest neighbors ($\psi_3$), shows effects close to zero. This finding is surprising when compared to the results of specification 1, where the estimators show a clear negative impact.

Regarding the estimators for the interactions, the differential effect of having a beneficiary nearest neighbor among young beneficiaries $\psi_4$ is null; in this case, the direct effects of both treatments capture most of the impact. On the other hand, when considering the interactions of both the first order effect and the informative effect with the competitive effect, coefficients $\psi_5$ and $\psi_6$, we observe negative and large coefficients. This suggests a fact that reconciles the findings of the present exercise and the models without interactions: the competitive effect, caused by a high program saturation among the closest neighbors, has the potential of reversing the positive impacts of the program, although it does not alter labor market outcomes of the less favored individuals, that is, those who were neither beneficiaries themselves nor neighbors of a beneficiary.[14]

Having found two interference effects with opposing signs, raises the question whether the omission of these variables results in a biased estimation of the first-order effect of the program. In this regard, Table 10 reports the estimator of the direct effect that results from a naive model that excludes all interference effects:

$$E_i = \tilde{\tilde{\alpha}} + T_i \tilde{\tilde{\gamma}}_1 + Q_i \tilde{\tilde{\delta}}_1 + w_i \tag{4}$$

Columns 1 and 2 of Table 10 report the coefficient of the treatment variable in equation 4. In this specification, the average value for the treatment effect of the program is 4.98 pp, virtually indistinguishable from the estimate from specification 1 (0.1 pp smaller). Column 3 reports the estimator bias if we regard specification 1 to be the true model (ie. $\tilde{\tilde{\gamma}}_1 - \tilde{\gamma}_1$), whereas columns 4 and 5 report the statistic F and the P-value of the test of differences between both coefficients under the null hypothesis that model 1 is correct. Although the difference is negative for all months (column 3), they are not statistically different and they are all very close to zero.

## 7 | ROBUSTNESS

The first robustness check tackles the slight imbalances observed in Table 7. For that matter, we fit specification 2 including the following controls for individual characteristics (before starting the program) and neighborhood: sex, age, education, marital status, children, manual application to the program[15], regarding individuals; and UBN (fraction of households

[13] Due to the lack of stability of the coefficient associated with the triple interaction, we exclude it from Figure 7, although they are reported in Table 9, column 7.

[14] The lower panel of Figure 7 shows the results of fitting specification 3 including controls for individual and neighborhood characteristics. We observe that including controls to this exercise does not qualitatively affect the results discussed.

[15] The latter is included as a potential indicator of skills and/or socioeconomic status of applicants, insofar as it requires access to a computer with internet and the expertise to handle it.

with at least one unmet basic need), informality rate (percentage of heads of households without access to health insurance) and unemployment rate, regarding the neighborhood.

Table 12 and Figure 8, show the results of specification 2, that incorporates the full set of controls. The inclusion of these controls for individual and neighborhood characteristics does not produce significant changes in our estimates. We observe a slight reduction of the estimators for the first order effect of the program in all months, which averages -0.27 pp (or -5.6% with respect to the previous estimate) for the period. The controls tend to reduce the estimator of the informative effect in all months, although in an equally small amount, which averages -0.16 pp (-9.3%). Finally, no systematic changes are observed our estimates of the competitive effect.

An important aspect when assessing the results presented is to what extent to which they depend on the neighborhood chosen for the analysis. Our second robustness exercise tackles this issue: tables 9 and 10 show the results of fitting specification 1, considering 10 and 20 nearest neighbors instead of 15. Altering this parameter does not qualitatively alter our main outcomes. Increasing the neighborhood however, results in an increase in the magnitude of the estimators of the competitive effect (7.6 pp, 8.9 pp and 9.4 pp for 10, 15 and 20 neighbors, respectively), while reducing the accuracy of the estimator. In fact, the average of the standard error in the specification 1 takes the values 0.032, 0.038 and 0.043 for 10, 15 and 20 neighbors, respectively. This finding is expected because of the changing interpretation of the estimator: it corresponds to the effect of going from 0% to 100% of beneficiaries among treated neighbors, which on the one hand is more drastic the greater the number of neighbors considered, while on the other, results in a variable $S_i^K$ with less sample variability, given that it is less frequent to find observations exposed to a fraction of beneficiaries far from the aggregate average, the greater the neighborhood considered.

In order to assess the likelihood of observing estimates of the magnitude we find, we carry out the following placebo tests to provide additional evidence that support our proposed causal link. We do permutations on the residential locations of PPP candidates, randomly assigning a placebo residential address to each candidate from the list of all addresses, without replacement. For each location permutation, we build our interference variables by finding each candidate's new nearest neighbor PPP assignment status and the share of beneficiaries in the 15 closest neighbors, and fit our main specification 1 for each post-treatment month. Figure 11 displays our main results, together with the regression outcomes resulting from the 1000 location permutations, showing that the chances of observing estimates of this magnitude are slim. Also, the figure shows a marked stability of our both estimates across the 12 months.

Finally, when assessing the robustness of our findings we have so far been looking at the significance level of the outcomes for each of the twelve post-treatment months separately. This is the most conservative approach to analyzing these outcomes, as it does not account for the observed stability of our estimates across time. To put this into perspective, we make use of our location permutations once again and compare the point estimate average across the 12 post-treatment months of our main specification to the distribution of average point estimates of the placebo regressions. Table 16 and Figure 12 portray the results of this analysis: our estimates for the informative effect are larger than 98.2% of the placebo estimates, while those for the competitive effect are smaller than 99% of the placebo estimates.

## 8 | CONCLUSION

Employability of vulnerable populations, and in particular youth employability, has been extensively studied and is of great interest for the design of public policies. In fact, there are numerous impact evaluations of ALMPs aimed at improving employment prospects of these population subgroups that, nonetheless, show ambiguous results. These mixed results are likely to depend on the specifics of design, implementation, and context of each program. Moreover, part of the ambiguous results may come from the presence of counteracting external effects implied by those programs.

In this work we find that such external effects can be sizeable in local labor markets. More precisely, we provide robust evidence on the existence of two types of externalities, with opposite impacts on young job-seekers: an information externality, that impacts youngsters positively and are thought to imply the transmission of labor market information through close contacts in a social network (here represented by spatial proximity); and a competition externality, which harms job-seekers by exposing them to a high saturation of program beneficiaries in their relevant local labor market. Each one of these effects is backed-up by separated strands of literature, and our results provide evidence that links them.

Regarding the informative effect, we find that being exposed to a neighbor (nearest applicant to the program) that received the benefit of the program increases in 1.8 percentage points the probability of employment in the 12 months following the end of the program. Regarding the competition effect, we document the existence of an economically significant negative displacement effect. When comparing applicants in the first and last decile of program saturation, we observe a reduction of 2.98 percentage points in the probability of employment. At the aggregate (city) level, however, these two external effects, compensate each other.

Our results have direct implications for public policy. First, the informative effect can be a valuable resource to enhance the impact of labor insertion programs, exploiting existing social networks. For example, the random allocation of benefits could be stratified by areas where lasting bonds of social interaction are usually built, such as the educational establishment attended by young people. In this way, the assignment could be designed so as to affect as many social nuclei as possible and thus exploit the transmission of information among peers.

On the other hand, there are policy instruments with the potential to avoid the observed negative displacement effects, by way of expanding the relevant labor market of the population targeted and reduce spacial frictions in labor markets. Examples of this could be transport subsidy policies and the use of employment agencies that inform of job opportunities throughout the city. Finally, the existence of this competitive effect demands caution when considering scaling up this type of programs.

## 9 | TABLES

TABLE 1 Descriptive statistics

| | Mean | SD | p25 | p50 | p75 | N |
|---|---|---|---|---|---|---|
| Female | 0.5093 | (0.4999) | 0 | 1 | 1 | 7339 |
| Age | 21.14 | (2.5015) | 19 | 21 | 23 | 7339 |
| Single | 0.9509 | (0.2160) | 1 | 1 | 1 | 7339 |
| Has children | 0.0813 | (0.2734) | 0 | 0 | 0 | 7339 |
| Finished highschool (18+) | 0.6944 | (0.4607) | 0 | 1 | 1 | 4742 |
| Finished College (21+) | 0.0805 | (0.2722) | 0 | 0 | 0 | 2148 |
| Manual application | 0.5051 | (0.5000) | 0 | 1 | 1 | 7339 |
| Emp. Jan-2012 | 0.0139 | (0.1171) | 0 | 0 | 0 | 7339 |
| Emp. Feb-2012 | 0.0041 | (0.0638) | 0 | 0 | 0 | 7339 |
| Emp. Mar-2012 | 0.0083 | (0.0908) | 0 | 0 | 0 | 7339 |
| Emp. Apr-2012 | 0.0134 | (0.1148) | 0 | 0 | 0 | 7339 |
| Emp. May-2012 | 0.0258 | (0.1584) | 0 | 0 | 0 | 7339 |
| UBI (neighborhood) | 0.0839 | (0.0744) | 0.0334 | 0.0639 | 0.1145 | 7313 |
| Informality (neighborhood) | 0.3107 | (0.1465) | 0.1852 | 0.2971 | 0.4064 | 7313 |
| Unemployement (neighborhood) | 0.0714 | (0.0205) | 0.0594 | 0.0693 | 0.0855 | 7313 |

TABLE 2 PPP allocation compliance

| | | PPP beneficiary | | | | | |
|---|---|---|---|---|---|---|---|
| | | No | | Yes | | Total | |
| | | N | (%) | N | (%) | N | (%) |
| 360+ days of attendance | No | 4,606 | (97.28) | 540 | (20.74) | 5,146 | (70.12) |
| | Yes | 129 | (2.72) | 2,064 | (79.26) | 2,193 | (29.88) |
| | Total | 4,735 | (100.00) | 2,604 | (100.00) | 7,339 | (100.00) |
| 240+ days of attendance | No | 4,595 | (97.04) | 406 | (15.59) | 5,001 | (68.14) |
| | Yes | 140 | (2.96) | 2,198 | (84.41) | 2,338 | (31.86) |
| | Total | 4,735 | (100.00) | 2,604 | (100.00) | 7,339 | (100.00) |

TABLE 3 Residence-establishment commutes

| | Distance walking | Distance in public transit | Time in public transit |
|---|---|---|---|
| Mean | 5.39 | 6.78 | 39.72 |
| SD | 4.31 | 5.49 | 25.18 |
| Skewness | 1.06 | 1.08 | 0.95 |
| p25 | 1.84 | 2.26 | 22.15 |
| Median | 4.68 | 5.90 | 37.57 |
| p75 | 7.73 | 9.80 | 52.99 |
| N | 9425 | 9400 | 9400 |

*Notes:* The table reports descriptive statistics on commutes from candidates' residence to their proposed establishment for all PPP applications, as reported by Google Maps API for regular business days at 8:30 am.

TABLE 4 Distance to the 1st, 10th and 50th nearest neighbor

| | 1st nearest neighbor | 10th nearest in neighbor | 50th nearest neighbor |
|---|---|---|---|
| Mean | 62.49 | 284.22 | 709.26 |
| SD | 83.38 | 236.56 | 507.37 |
| Skewness | 12.98 | 9.09 | 6.06 |
| p25 | 9.54 | 197.09 | 531.30 |
| Median | 49.27 | 252.72 | 603.39 |
| p75 | 90.34 | 313.73 | 733.62 |
| N | 7339 | 7339 | 7339 |

*Notes:* The statistics reported correspond to straight-line distances to the closest, 10th closest and 50th closest neighbor of each PPP candidate.

TABLE 5 Number of applicants and beneficiaries in radii of 100, 500 and 1000 meters (m)

| | Neighbors | | | Beneficiary neighbors | | |
|---|---|---|---|---|---|---|
| | 100 m | 500 m | 1000 m | 100 m | 500 m | 1000 m |
| Mean | 2.60 | 43.06 | 142.84 | 0.95 | 15.70 | 51.57 |
| SD | 3.07 | 38.66 | 103.40 | 1.37 | 15.12 | 39.37 |
| Skewness | 2.46 | 2.83 | 2.10 | 2.32 | 2.72 | 2.08 |
| p25 | 1 | 23 | 87 | 0 | 8 | 30 |
| Median | 2 | 34 | 128 | 0 | 12 | 45 |
| p75 | 3 | 45 | 155 | 1 | 17 | 56 |
| N | 7339 | 7339 | 7339 | 7339 | 7339 | 7339 |

*Notes:* The table reports the number of candidates and beneficiaries within 100, 500 and 1000 meters of straight-line distance.

TABLE 6 Program saturation by number of nearest neighbors considered

| | 10 nearest neighbors | 15 nearest neighbors | 20 nearest neighbors |
|---|---|---|---|
| Mean | 0.36 | 0.35 | 0.35 |
| SD | 0.16 | 0.13 | 0.11 |
| Skewness | 0.21 | 0.17 | 0.17 |
| p10 | 0.20 | 0.20 | 0.20 |
| Median | 0.40 | 0.33 | 0.35 |
| p90 | 0.60 | 0.53 | 0.50 |
| N | 7339 | 7339 | 7339 |

*Notes:* the table shows descriptive statistics for the program saturation variable in K closest neighbors, for K values of 10, 15 and 20 PPP candidates.

TABLE 7 Balance statistics for direct program assignment, nearest neighbor assignment and program saturation

| | Mean | PPP beneficiary | | NN beneficiary | | Saturation 15 NNs | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | | (3) | | (4) | |
| Female | 0.5094 | -0.0132 | | 0.0000 | | 0.0026 | |
| | | (0.0122) | | (0.0122) | | (0.0449) | |
| Age | 21.138 | -0.0325 | | 0.0548 | | -0.1214 | |
| | | (0.0600) | | (0.0602) | | (0.2191) | |
| Single | 0.9509 | 0.0043 | | -0.0118 | ** | -0.0100 | |
| | | (0.0052) | | (0.0055) | | (0.0199) | |
| Children | 0.0814 | -0.0142 | ** | -0.0011 | | -0.0256 | |
| | | (0.0066) | | (0.0067) | | (0.0245) | |
| Highschool (18+) | 0.6942 | 0.0117 | | 0.0280 | ** | 0.0713 | |
| | | (0.0143) | | (0.0138) | | (0.0513) | |
| Higher ed. (21+) | 0.0806 | 0.0368 | *** | -0.0020 | | -0.0172 | |
| | | (0.0137) | | (0.0123) | | (0.0492) | |
| Manual application | 0.5050 | 0.0064 | | 0.0208 | * | 0.0664 | |
| | | (0.0121) | | (0.0121) | | (0.0445) | |
| Emp. Jan-2012 | 0.0139 | -0.0009 | | -0.0007 | | 0.0093 | |
| | | (0.0029) | | (0.0028) | | (0.0107) | |
| Emp. Feb-2012 | 0.0041 | 0.0004 | | 0.0022 | | -0.0014 | |
| | | (0.0016) | | (0.0017) | | (0.0058) | |
| Emp. Mar-2012 | 0.0083 | 0.0016 | | 0.0016 | | -0.0022 | |
| | | (0.0022) | | (0.0023) | | (0.0090) | |
| Emp. Apr-2012 | 0.0134 | 0.0021 | | 0.0014 | | -0.0039 | |
| | | (0.0029) | | (0.0029) | | (0.0113) | |
| Emp. May-2012 | 0.0258 | 0.0027 | | 0.0003 | | 0.0112 | |
| | | (0.0039) | | (0.0039) | | (0.0145) | |
| UBI (nbhood) | 0.0839 | -0.0049 | *** | -0.0062 | *** | -0.0448 | *** |
| | | (0.0018) | | (0.0018) | | (0.0061) | |
| Informality (nbhood) | 0.3108 | -0.0080 | ** | -0.0111 | *** | -0.0899 | *** |
| | | (0.0036) | | (0.0036) | | (0.0127) | |
| Unemp. (nbhood) | 0.0714 | -0.0012 | ** | -0.0013 | ** | -0.0113 | *** |
| | | (0.0005) | | (0.0005) | | (0.0017) | |

*Notes:* the table shows the full sample mean of each characteristic and the difference relative to baseline value for variables direct program assignment, nearest neighbor assignment and share of beneficiaries in 15 nearest neighbors.

TABLE 8 Direct and externality effects on employment rate
Specification 1, no controls, 15 nearest neighbors

| | | | PPP beneficiary | | NN beneficiary | | Share of beneficiaries 15 NNs | |
|---|---|---|---|---|---|---|---|---|
| | | | Diference | SE | Diference | SE | Slope | SE |
| Month | Baseline emp. | N | (1) | (2) | (3) | (4) | (5) | (6) |
| Jul-13 | 0.1744 | 7334 | 0.0397 *** | (0.0099) | 0.0099 | (0.0097) | -0.0582 | (0.0357) |
| Aug-13 | 0.1867 | 7334 | 0.0493 *** | (0.0102) | 0.0131 | (0.0100) | -0.0523 | (0.0368) |
| Sep-13 | 0.1942 | 7334 | 0.0525 *** | (0.0103) | 0.0212 ** | (0.0101) | -0.0711 * | (0.0369) |
| Oct-13 | 0.2036 | 7334 | 0.0545 *** | (0.0105) | 0.0239 ** | (0.0103) | -0.0770 ** | (0.0373) |
| Nov-13 | 0.2128 | 7334 | 0.0511 *** | (0.0106) | 0.0192 * | (0.0105) | -0.0960 ** | (0.0382) |
| Dec-13 | 0.2209 | 7334 | 0.0507 *** | (0.0108) | 0.0203 * | (0.0106) | -0.1291 *** | (0.0386) |
| Jan-14 | 0.2206 | 7334 | 0.0530 *** | (0.0108) | 0.0207 * | (0.0106) | -0.1148 *** | (0.0392) |
| Feb-14 | 0.2190 | 7334 | 0.0501 *** | (0.0107) | 0.0221 ** | (0.0106) | -0.1088 *** | (0.0391) |
| Mar-14 | 0.2210 | 7334 | 0.0467 *** | (0.0108) | 0.0210 ** | (0.0107) | -0.0806 ** | (0.0393) |
| Apr-14 | 0.2176 | 7334 | 0.0516 *** | (0.0107) | 0.0181 * | (0.0106) | -0.0863 ** | (0.0388) |
| May-14 | 0.2213 | 7334 | 0.0504 *** | (0.0108) | 0.0168 | (0.0106) | -0.0882 ** | (0.0391) |
| Jun-14 | 0.2306 | 7334 | 0.0595 *** | (0.0109) | 0.0130 | (0.0108) | -0.1092 *** | (0.0396) |

*Notes:* the table shows the mean employment rate, direct and externality coefficients and standard errors (in parenthesis) for each post-treatment period.

TABLE 9 Direct and externality effects on employment rate
Specification 3 - fully interacted model, no controls, 15 nearest neighbors

| | | Direct effect (T) | Informative effect (N) | Competitive effect (S) | F x N | F x S | N x S | F x N x S |
|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Jul-13 | Coefficient | 0.0894 *** | 0.0334 | -0.0065 | -0.0423 | -0.1212 | -0.0438 | 0.0581 |
| | *P-value* | (0.0095) | (0.3536) | (0.8999) | (0.5110) | (0.2083) | (0.6270) | (0.7142) |
| Aug-13 | Coefficient | 0.0849 ** | 0.0435 | -0.0115 | -0.0447 | -0.0749 | -0.0559 | 0.0475 |
| | *P-value* | (0.0160) | (0.2374) | (0.8238) | (0.5073) | (0.4491) | (0.5421) | (0.7755) |
| Sep-13 | Coefficient | 0.0925 *** | 0.0671 * | -0.0042 | -0.0437 | -0.1058 | -0.1137 | 0.0911 |
| | *P-value* | (0.0089) | (0.0700) | (0.9351) | (0.5227) | (0.2848) | (0.2151) | (0.5874) |
| Oct-13 | Coefficient | 0.0886 ** | 0.0818 ** | -0.0087 | -0.0438 | -0.0786 | -0.1359 | 0.0649 |
| | *P-value* | (0.0136) | (0.0305) | (0.8692) | (0.5240) | (0.4337) | (0.1455) | (0.6999) |
| Nov-13 | Coefficient | 0.0613 * | 0.0504 | -0.0564 | 0.0163 | -0.0124 | -0.0691 | -0.0811 |
| | *P-value* | (0.0929) | (0.1891) | (0.3088) | (0.8156) | (0.9037) | (0.4678) | (0.6347) |
| Dec-13 | Coefficient | 0.0816 ** | 0.0693 * | -0.0626 | -0.0187 | -0.0732 | -0.1171 | 0.0120 |
| | *P-value* | (0.0272) | (0.0801) | (0.2648) | (0.7920) | (0.4768) | (0.2313) | (0.9446) |
| Jan-14 | Coefficient | 0.1118 *** | 0.0911 ** | -0.0122 | -0.0638 | -0.1594 | -0.1787 * | 0.1429 |
| | *P-value* | (0.0027) | (0.0229) | (0.8292) | (0.3746) | (0.1260) | (0.0717) | (0.4170) |
| Feb-14 | Coefficient | 0.0778 ** | 0.0621 | -0.0341 | 0.0117 | -0.0908 | -0.1191 | 0.0046 |
| | *P-value* | (0.0357) | (0.1184) | (0.5477) | (0.8707) | (0.3802) | (0.2278) | (0.9791) |
| Mar-14 | Coefficient | 0.0753 ** | 0.0391 | -0.0167 | 0.0297 | -0.1013 | -0.0691 | -0.0196 |
| | *P-value* | (0.0422) | (0.3285) | (0.7711) | (0.6784) | (0.3291) | (0.4901) | (0.9111) |
| Apr-14 | Coefficient | 0.0871 ** | 0.0477 | -0.0133 | 0.0068 | -0.1180 | -0.0957 | 0.0295 |
| | *P-value* | (0.0177) | (0.2271) | (0.8148) | (0.9233) | (0.2505) | (0.3318) | (0.8652) |
| May-14 | Coefficient | 0.0810 ** | 0.0361 | -0.0194 | 0.0389 | -0.1028 | -0.0690 | -0.0533 |
| | *P-value* | (0.0276) | (0.3669) | (0.7367) | (0.5853) | (0.3180) | (0.4919) | (0.7601) |
| Jun-14 | Coefficient | 0.1056 *** | 0.0491 | -0.0297 | -0.0272 | -0.1511 | -0.1140 | 0.1216 |
| | *P-value* | (0.0047) | (0.2257) | (0.6124) | (0.7083) | (0.1465) | (0.2596) | (0.4942) |

*Notes:* the table shows coefficients for the *direct effect*, *informative effect*, and *competitive effect*, three double interactions, and a triple interaction among these variables. These are the outcome of specification 3 estimated by OLS for each month, without controls.

TABLE 10 Omitted-variable bias test, model without interactions

| Month | Coefficient (1) | SE (2) | Difference (3) | F-test (4) | P-value (5) |
|---|---|---|---|---|---|
| Jul-13 | 0.039059 | 0.009876 | -0.00061 | 0.003758 | 0.951123 |
| Aug-13 | 0.048726 | 0.010174 | -0.00058 | 0.003276 | 0.95436 |
| Sep-13 | 0.051652 | 0.01033 | -0.00082 | 0.006344 | 0.936518 |
| Oct-13 | 0.053632 | 0.010501 | -0.0009 | 0.007348 | 0.931692 |
| Nov-13 | 0.050101 | 0.01063 | -0.00103 | 0.009298 | 0.923186 |
| Dec-13 | 0.049356 | 0.010754 | -0.00133 | 0.015242 | 0.901746 |
| Jan-14 | 0.051765 | 0.010752 | -0.0012 | 0.012549 | 0.910808 |
| Feb-14 | 0.048952 | 0.010723 | -0.00116 | 0.011789 | 0.913542 |
| Mar-14 | 0.045794 | 0.010759 | -0.0009 | 0.007065 | 0.933016 |
| Apr-14 | 0.050649 | 0.010732 | -0.00093 | 0.007487 | 0.931047 |
| May-14 | 0.049492 | 0.010781 | -0.00093 | 0.007485 | 0.931058 |
| Jun-14 | 0.058443 | 0.010913 | -0.00108 | 0.009863 | 0.920894 |

*Notes:* the table shows the direct program effect coefficient resulting from specification 4, the difference relative to the estimation from specification 1 and the corresponding Wald test considering specification 1 to be correct under the null hypothesis.

TABLE 11 Omitted-variable bias test, model with interactions

| Month | Coefficient (1) | SE (2) | Difference (3) | F-test (4) | P-value (5) |
|---|---|---|---|---|---|
| Jul-13 | 0.039059 | 0.034443 | -0.05031 | 2.133714 | 0.144134 |
| Aug-13 | 0.048726 | 0.035234 | -0.03613 | 1.051618 | 0.305169 |
| Sep-13 | 0.051652 | 0.035371 | -0.04085 | 1.333617 | 0.248201 |
| Oct-13 | 0.053632 | 0.035893 | -0.03498 | 0.949844 | 0.329791 |
| Nov-13 | 0.050101 | 0.03649 | -0.01121 | 0.094416 | 0.758645 |
| Dec-13 | 0.049356 | 0.036921 | -0.03219 | 0.760373 | 0.38324 |
| Jan-14 | 0.051765 | 0.03727 | -0.06002 | 2.593823 | 0.107324 |
| Feb-14 | 0.048952 | 0.037006 | -0.02881 | 0.605984 | 0.436329 |
| Mar-14 | 0.045794 | 0.037038 | -0.02947 | 0.633044 | 0.426267 |
| Apr-14 | 0.050649 | 0.036728 | -0.03645 | 0.985073 | 0.320982 |
| May-14 | 0.049492 | 0.036775 | -0.03155 | 0.735988 | 0.390977 |
| Jun-14 | 0.058443 | 0.037361 | -0.0472 | 1.596163 | 0.206488 |

*Notes:* the table shows the direct program effect coefficient resulting from specification 4, the difference relative to the estimation from specification 3 and the corresponding Wald test considering specification 3 to be correct under the null hypothesis.

TABLE 12 Direct and externality effects on employment rate
Specification 2, with worker and neighborhood controls, 15 nearest neighbors

| | | | PPP beneficiary | | Nearest neighbor beneficiary | | Share of beneficiaries in 15 nearest | |
|---|---|---|---|---|---|---|---|---|
| | | | Diference | SE | Diference | SE | Slope | SE |
| Month | Baseline emp. | N | (1) | (2) | (3) | (4) | (5) | (6) |
| Jul-13 | 0.1744 | 7256 | 0.0371 *** | (0.0099) | 0.0084 | (0.0097) | -0.0640 | (0.0362) |
| Aug-13 | 0.1867 | 7256 | 0.0471 *** | (0.0102) | 0.0116 | (0.0100) | -0.0622 | (0.0372) |
| Sep-13 | 0.1942 | 7256 | 0.0500 *** | (0.0103) | 0.0208 ** | (0.0101) | -0.0783 * | (0.0372) |
| Oct-13 | 0.2036 | 7256 | 0.0521 *** | (0.0105) | 0.0228 ** | (0.0103) | -0.0811 ** | (0.0376) |
| Nov-13 | 0.2128 | 7256 | 0.0480 *** | (0.0106) | 0.0180 * | (0.0104) | -0.1004 ** | (0.0384) |
| Dec-13 | 0.2209 | 7256 | 0.0475 *** | (0.0107) | 0.0182 * | (0.0106) | -0.1280 *** | (0.0388) |
| Jan-14 | 0.2206 | 7256 | 0.0498 *** | (0.0107) | 0.0186 * | (0.0106) | -0.1096 *** | (0.0394) |
| Feb-14 | 0.2190 | 7256 | 0.0482 *** | (0.0107) | 0.0198 ** | (0.0106) | -0.1031 *** | (0.0391) |
| Mar-14 | 0.2210 | 7256 | 0.0444 *** | (0.0107) | 0.0183 ** | (0.0106) | -0.0774 ** | (0.0393) |
| Apr-14 | 0.2176 | 7256 | 0.0487 *** | (0.0107) | 0.0161 * | (0.0105) | -0.0902 ** | (0.0389) |
| May-14 | 0.2213 | 7256 | 0.0475 *** | (0.0108) | 0.0154 | (0.0106) | -0.0970 ** | (0.0392) |
| Jun-14 | 0.2306 | 7256 | 0.0566 *** | (0.0109) | 0.0120 | (0.0107) | -0.1199 *** | (0.0398) |

*Notes:* the table shows the mean employment rate, direct and externality coefficients and standard errors (in parenthesis) for each post-treatment period, including controls for the full set of worker and neighborhood characteristics.

TABLE 13 Direct and externality effects on employment rate
Specification 1, no controls, 10 nearest neighbors

| | | | PPP beneficiary | | Nearest neighbor beneficiary | | Share of beneficiaries in 15 nearest | |
|---|---|---|---|---|---|---|---|---|
| | | | Diference | SE | Diference | SE | Slope | SE |
| Month | Baseline emp. | N | (1) | (2) | (3) | (4) | (5) | (6) |
| Jul-13 | 0.1744 | 7334 | 0.0401 *** | (0.0099) | 0.0121 | (0.0099) | -0.0579 * | (0.0302) |
| Aug-13 | 0.1867 | 7334 | 0.0496 *** | (0.0102) | 0.0143 | (0.0102) | -0.0443 | (0.0311) |
| Sep-13 | 0.1942 | 7334 | 0.0529 *** | (0.0103) | 0.0230 ** | (0.0104) | -0.0626 ** | (0.0313) |
| Oct-13 | 0.2036 | 7334 | 0.0548 *** | (0.0105) | 0.0253 ** | (0.0106) | -0.0625 ** | (0.0318) |
| Nov-13 | 0.2128 | 7334 | 0.0514 *** | (0.0106) | 0.0204 * | (0.0108) | -0.0759 ** | (0.0323) |
| Dec-13 | 0.2209 | 7334 | 0.0509 *** | (0.0108) | 0.0218 ** | (0.0109) | -0.1021 *** | (0.0328) |
| Jan-14 | 0.2206 | 7334 | 0.0534 *** | (0.0107) | 0.0234 ** | (0.0109) | -0.1036 *** | (0.0331) |
| Feb-14 | 0.2190 | 7334 | 0.0505 *** | (0.0107) | 0.0256 ** | (0.0109) | -0.1075 *** | (0.0331) |
| Mar-14 | 0.2210 | 7334 | 0.0470 *** | (0.0108) | 0.0235 ** | (0.0109) | -0.0779 ** | (0.0332) |
| Apr-14 | 0.2176 | 7334 | 0.0519 *** | (0.0107) | 0.0197 * | (0.0108) | -0.0731 ** | (0.0329) |
| May-14 | 0.2213 | 7334 | 0.0507 *** | (0.0108) | 0.0174 | (0.0109) | -0.0656 ** | (0.0333) |
| Jun-14 | 0.2306 | 7334 | 0.0597 *** | (0.0109) | 0.0134 | (0.0110) | -0.0774 ** | (0.0336) |

*Notes:* the table shows the mean employment rate, direct and externality coefficients and standard errors (in parenthesis) for each post-treatment period.

TABLE 14 Direct and externality effects on employment rate
Specification 1, no controls, 20 nearest neighbors

| | | | PPP beneficiary | | | Nearest neighbor beneficiary | | | Share of beneficiaries in 15 nearest | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Difference | | SE | Difference | | SE | Slope | | SE |
| Month | Baseline emp. | N | (1) | | (2) | (3) | | (4) | (5) | | (6) |
| Jul-13 | 0.1744 | 7334 | 0.0399 | *** | (0.0099) | 0.0098 | | (0.0095) | -0.0723 | * | (0.0403) |
| Aug-13 | 0.1867 | 7334 | 0.0495 | *** | (0.0102) | 0.0127 | | (0.0098) | -0.0594 | | (0.0415) |
| Sep-13 | 0.1942 | 7334 | 0.0526 | *** | (0.0103) | 0.0201 | ** | (0.0100) | -0.0731 | * | (0.0420) |
| Oct-13 | 0.2036 | 7334 | 0.0546 | *** | (0.0105) | 0.0234 | ** | (0.0102) | -0.0906 | ** | (0.0425) |
| Nov-13 | 0.2128 | 7334 | 0.0511 | *** | (0.0106) | 0.0176 | * | (0.0104) | -0.0966 | ** | (0.0436) |
| Dec-13 | 0.2209 | 7334 | 0.0507 | *** | (0.0107) | 0.0191 | * | (0.0105) | -0.1482 | *** | (0.0440) |
| Jan-14 | 0.2206 | 7334 | 0.0529 | *** | (0.0107) | 0.0196 | * | (0.0105) | -0.1340 | *** | (0.0445) |
| Feb-14 | 0.2190 | 7334 | 0.0499 | *** | (0.0107) | 0.0201 | * | (0.0105) | -0.1110 | ** | (0.0442) |
| Mar-14 | 0.2210 | 7334 | 0.0466 | *** | (0.0108) | 0.0191 | * | (0.0105) | -0.0720 | | (0.0446) |
| Apr-14 | 0.2176 | 7334 | 0.0515 | *** | (0.0107) | 0.0171 | | (0.0104) | -0.0969 | ** | (0.0443) |
| May-14 | 0.2213 | 7334 | 0.0502 | *** | (0.0108) | 0.0148 | | (0.0105) | -0.0801 | * | (0.0448) |
| Jun-14 | 0.2306 | 7334 | 0.0593 | *** | (0.0109) | 0.0106 | | (0.0106) | -0.0997 | ** | (0.0456) |

*Notes:* the table shows the mean employment rate, direct and externality coefficients and standard errors (in parenthesis) for each post-treatment period.

TABLE 15 Robustness to neighborhood extent, specification 1 – 15 vs. 10 and 15 vs. 20 nearest neighbors

| | 15 vs. 10 neighbors | | | | | | 15 vs. 20 neighbors | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Direct effect | | Informative effect | | Competitive effect | | Direct effect | | Informative effect | | Competitive effect | |
| | Difference | % | Difference | % | Difference | % | Difference | % | Difference | % | Difference | % |
| Month | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Jul-13 | 0.0004 | 1.03 | 0.0021 | 21.40 | 0.0003 | -0.58 | 0.0002 | 0.59 | -0.0001 | -1.36 | -0.0140 | 24.10 |
| Aug-13 | 0.0003 | 0.64 | 0.0012 | 9.27 | 0.0080 | -15.35 | 0.0002 | 0.39 | -0.0004 | -3.21 | -0.0071 | 13.59 |
| Sep-13 | 0.0004 | 0.76 | 0.0018 | 8.58 | 0.0085 | -11.96 | 0.0001 | 0.20 | -0.0011 | -5.06 | -0.0020 | 2.83 |
| Oct-13 | 0.0003 | 0.57 | 0.0013 | 5.62 | 0.0145 | -18.86 | 0.0001 | 0.21 | -0.0006 | -2.31 | -0.0136 | 17.65 |
| Nov-13 | 0.0003 | 0.54 | 0.0012 | 6.15 | 0.0201 | -20.90 | 0.0000 | 0.01 | -0.0016 | -8.24 | -0.0006 | 0.59 |
| Dec-13 | 0.0003 | 0.51 | 0.0015 | 7.53 | 0.0270 | -20.89 | 0.0000 | -0.02 | -0.0012 | -5.82 | -0.0191 | 14.81 |
| Jan-14 | 0.0004 | 0.74 | 0.0028 | 13.44 | 0.0112 | -9.74 | -0.0001 | -0.17 | -0.0011 | -5.25 | -0.0192 | 16.68 |
| Feb-14 | 0.0004 | 0.83 | 0.0035 | 16.02 | 0.0014 | -1.27 | -0.0002 | -0.40 | -0.0019 | -8.65 | -0.0021 | 1.96 |
| Mar-14 | 0.0003 | 0.67 | 0.0026 | 12.19 | 0.0027 | -3.39 | -0.0001 | -0.31 | -0.0019 | -9.04 | 0.0086 | -10.66 |
| Apr-14 | 0.0003 | 0.64 | 0.0016 | 8.71 | 0.0132 | -15.31 | -0.0001 | -0.16 | -0.0009 | -5.23 | -0.0105 | 12.21 |
| May-14 | 0.0002 | 0.47 | 0.0007 | 4.04 | 0.0226 | -25.59 | -0.0002 | -0.45 | -0.0020 | -11.80 | 0.0081 | -9.14 |
| Jun-14 | 0.0002 | 0.35 | 0.0004 | 3.30 | 0.0318 | -29.09 | -0.0002 | -0.38 | -0.0024 | -18.25 | 0.0095 | -8.66 |
| Avearge | 0.0003 | 0.65 | 0.0017 | 9.69 | 0.0134 | -14.41 | 0.0000 | -0.04 | -0.0013 | -7.02 | -0.0052 | 6.33 |

*Notes:* the table shows the difference in estimated coefficients, considering 10 and 20 nearest neighbors relative to our chosen value of 15 nearest neighbors, using our main specification 1.

TABLE 16 Permutations-based P-values

| | Coefficient | One-sided P-value | Two-sided P-value | Coefficient | One-sided P-value | Two-sided P-value |
|---|---|---|---|---|---|---|
| Informative effect | 0.01827 | 0.018 | 0.034 | 0.016663 | 0.027 | 0.034 |
| Competititive effect | -0.08931 | 0.006 | 0.009 | -0.09259 | 0.003 | 0.009 |
| Individual and neighborhood controls | | No | | | Yes | |
| Permutations | | 1000 | | | 1000 | |

*Notes:* the table shows the estimates to the *informative effect* and *competitive effect* resulting from 1 and their associated P-values calculated by 1000 location permutations.

# 10 | FIGURES

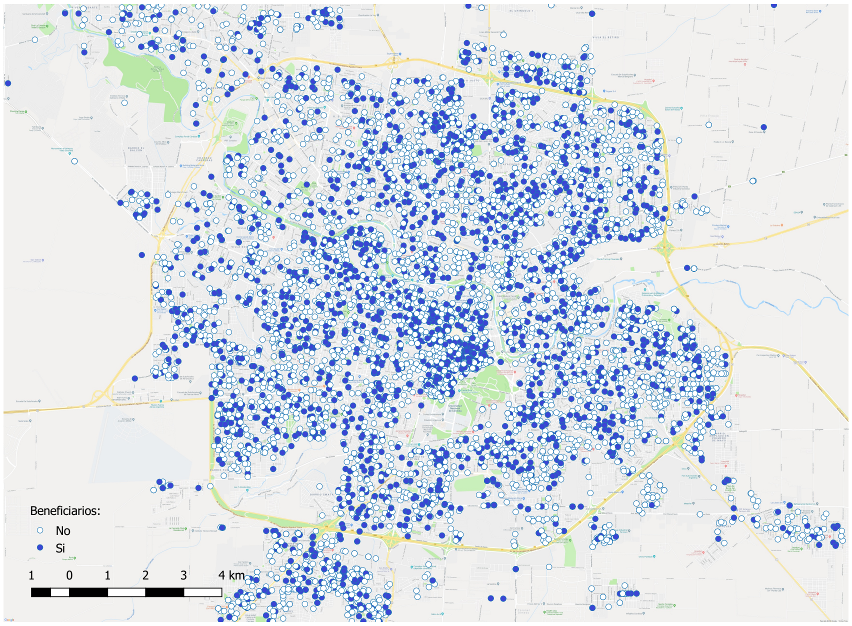


FIGURE 1 Location of beneficiary and non-beneficiary candidates in Cordoba city. *Notes:* The figure presents all applicants that presented a valid PPP application and were successfully geolocated. Beneficiary and non-beneficiary candidates appear in blue and white, respectively.

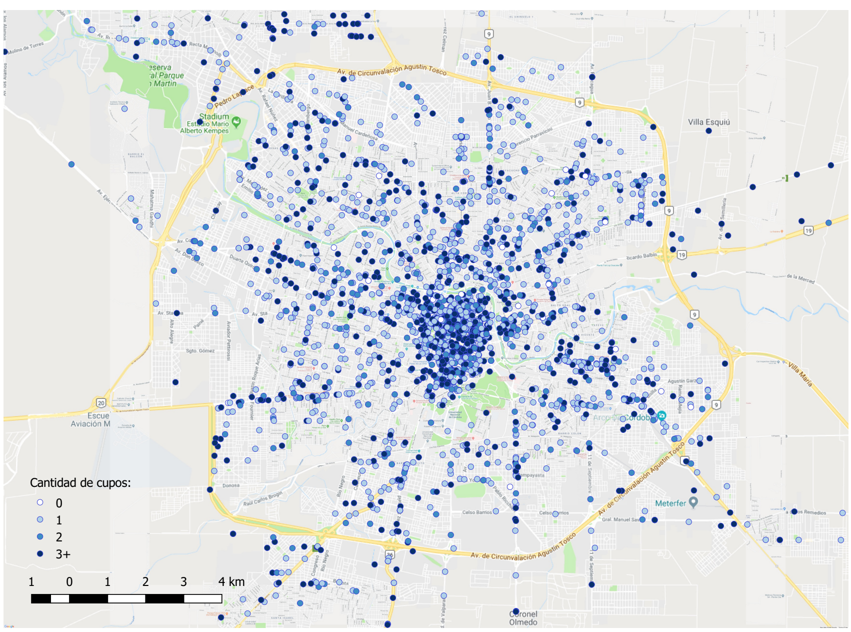


FIGURE 2 Location of eligible firms in Cordoba city. *Notes:* The figure presents all firms that received valid PPP applications, by maximum allowed number of PPP beneficiaries.

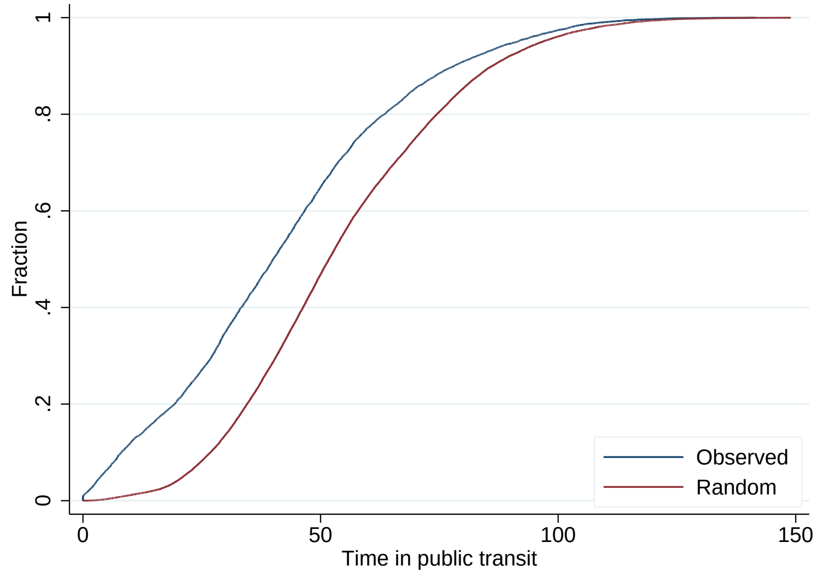


FIGURE 3 Distribution of residence-workplace commuting times, observed vs. random worker-firm pairs. *Notes:* the figure shows the distribution of commuting times in public transit between the residential and workplace addresses declared in all valid PPP applications, compared to residence-workplace pairs chosen at random. Time calculations based on Google Maps Directions API.

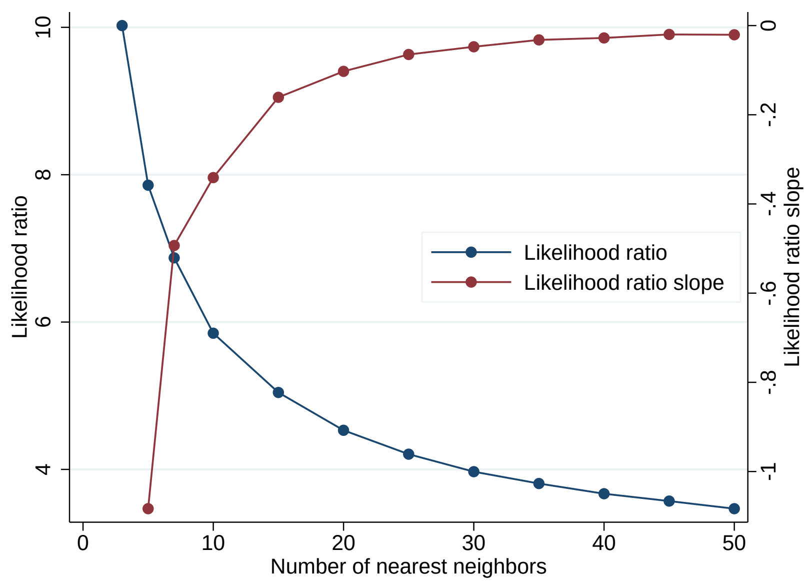


FIGURE 4 Likelihood ratio of same-same firm applicants by number of closest neighbors. *Notes:* the figure shows the share of applicant pairs that choose the same firm for groups of closest neighbors of increasing size relative to pairs of applicants chosen at random.

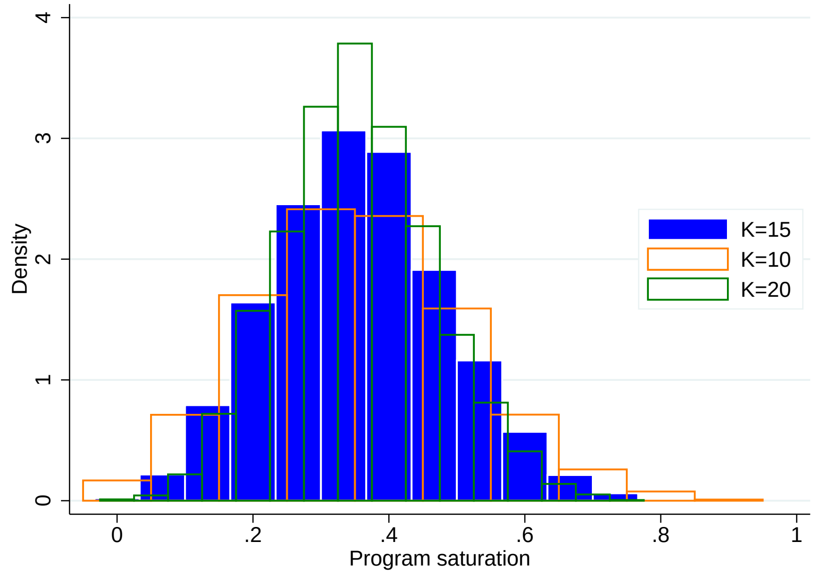


FIGURE 5 Histogram of program saturation by number of closest neighbors considered

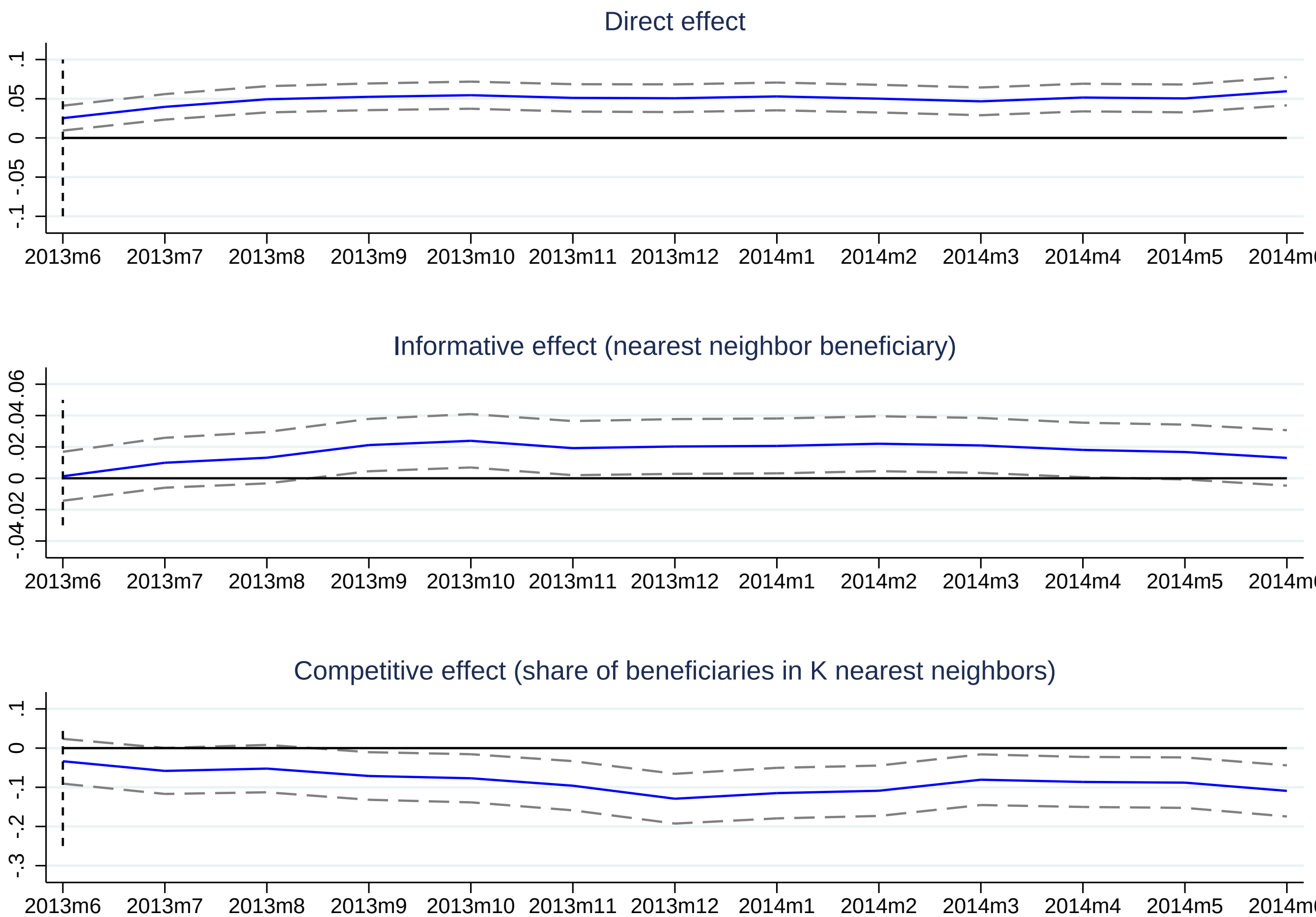


FIGURE 6 Specification 1, 15 nearest neighbors, no controls. *Notes:* The figure shows coefficients and 90% confidence intervals for *direct effect*, *informative effect* and *competitive effect*, for 12 post-treatment months, resulting from specification 1. Competitive effect corresponds to estimation on share of beneficiaries among 15 nearest neighbors.

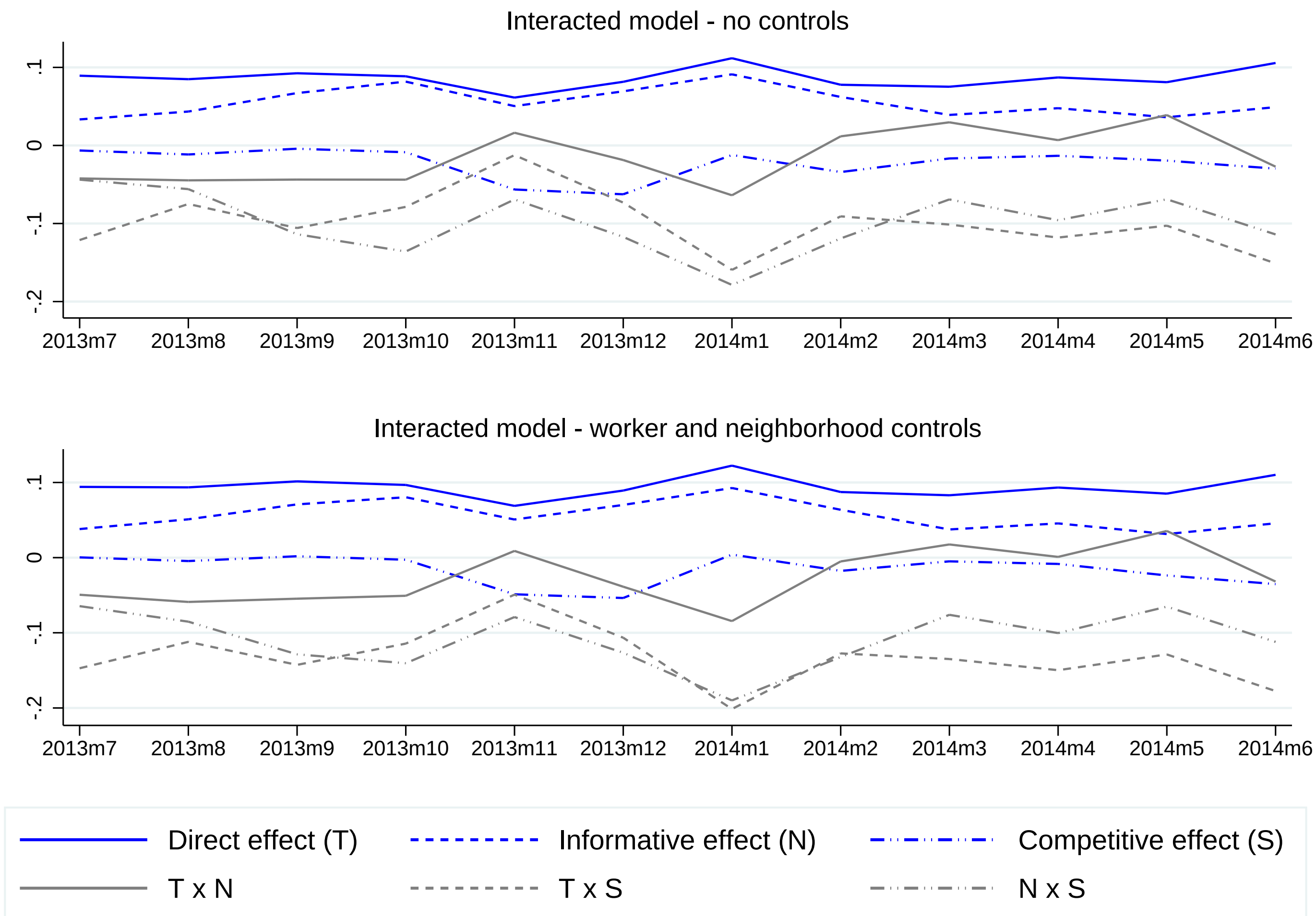


FIGURE 7 Fully interacted specification. *Notes:* The figure shows coefficients for *direct effect*, *informative effect*, *competitive effect*, and three double interactions for 12 post-treatment months, resulting from specification 3. Competitive effect corresponds to estimation on share of beneficiaries among 15 nearest neighbors. The top panel does not include control variables, while the bottom panel includes controls for worker and neighborhood characteristics.

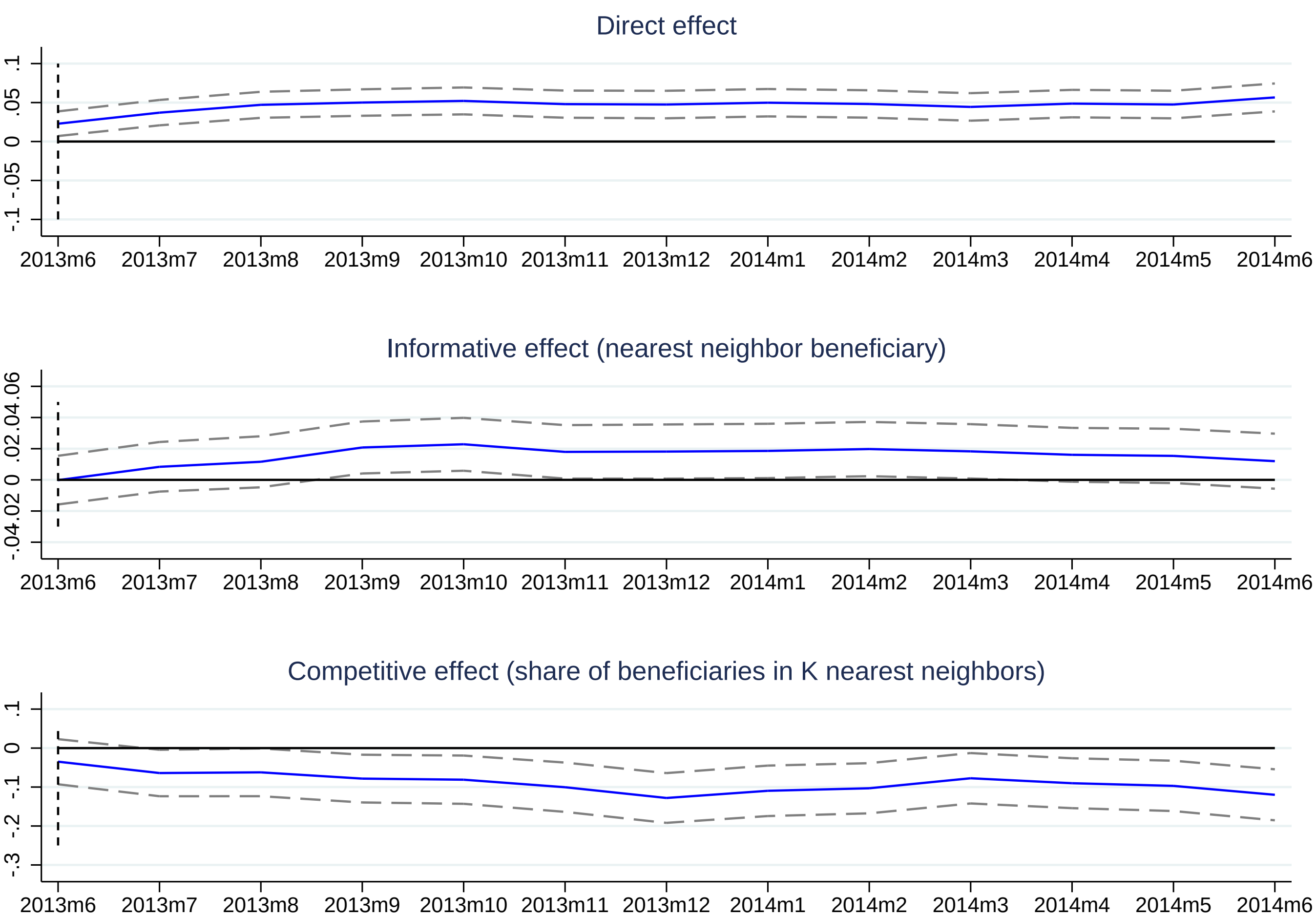


FIGURE 8 Specification 2, 15 nearest neighbors, worker and neighborhood controls. *Notes:* the figure shows coefficients and 90% confidence intervals for *direct effect*, *informative effect* and *competitive effect*, for 12 post-treatment months, resulting from specification 2. Competitive effect corresponds to estimation on share of beneficiaries among 15 nearest neighbors. The model includes the full set of worker and neighborhood controls.

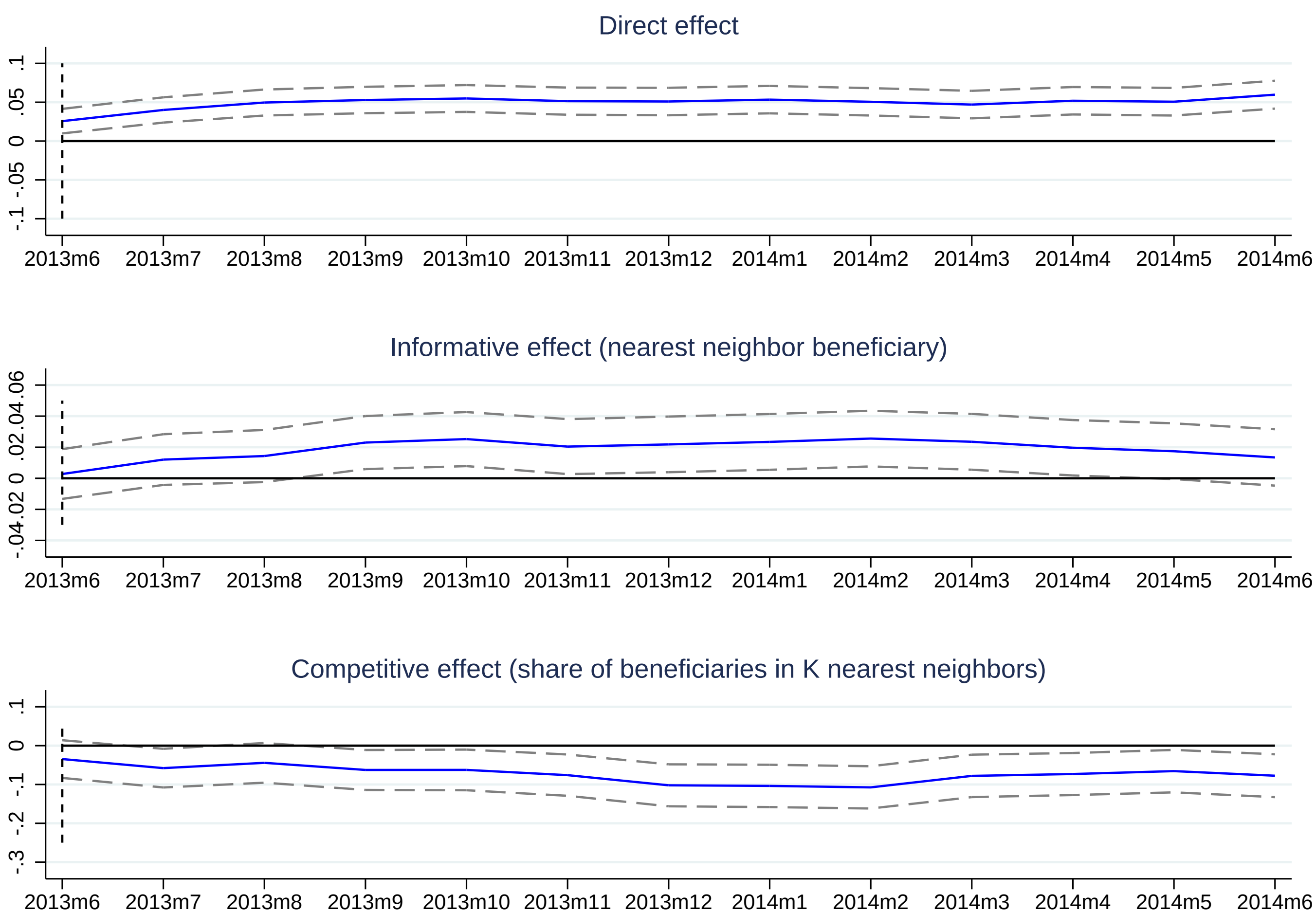


FIGURE 9 Specification 1, 10 nearest neighbors, no controls.*Notes:* The figure shows coefficients and 90% confidence intervals for *direct effect*, *informative effect* and *competitive effect*, for 12 post-treatment months, resulting from specification 1. Competitive effect corresponds to estimation on share of beneficiaries among 10 nearest neighbors.

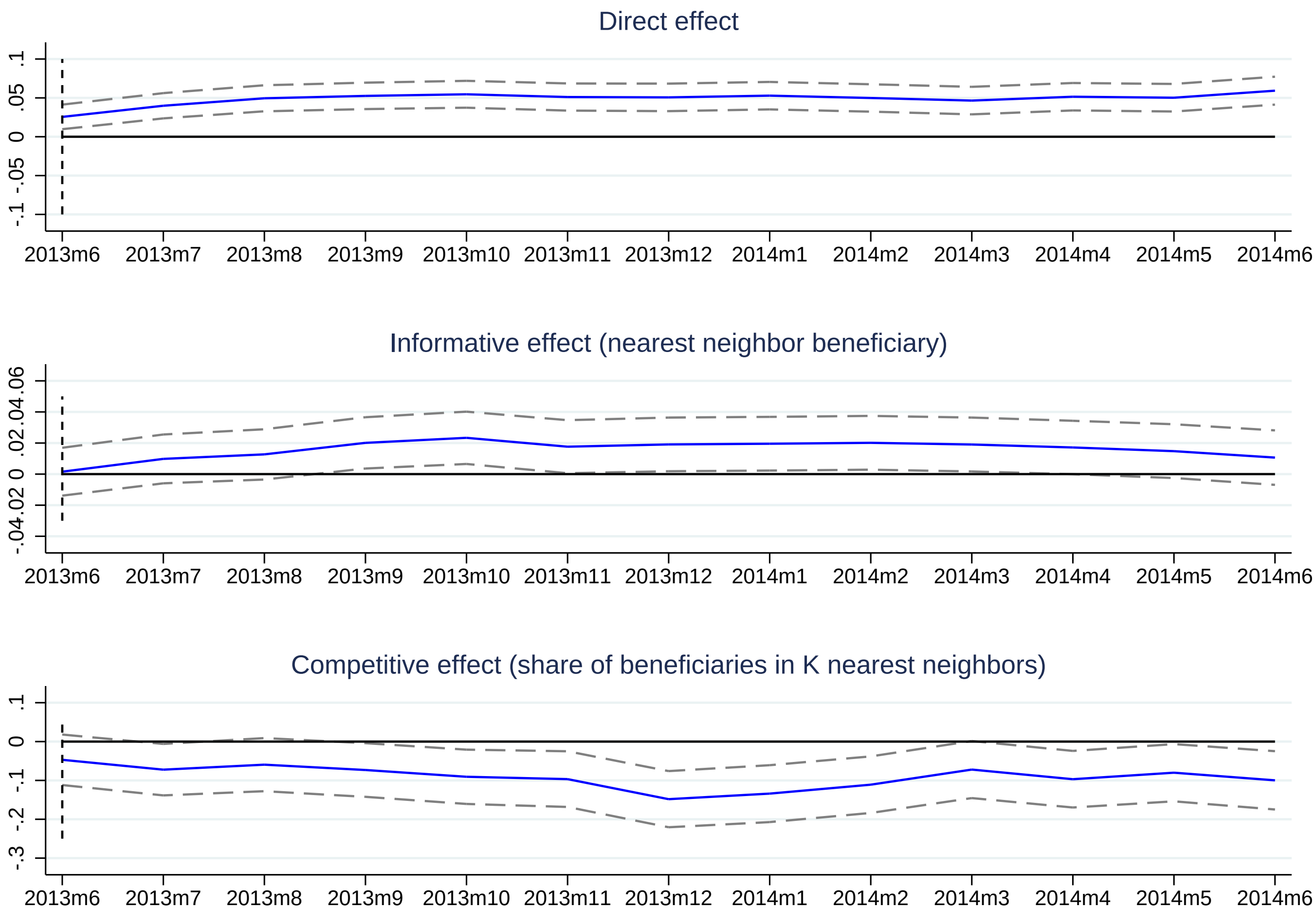


FIGURE 10 Specification 1, 20 nearest neighbors, no controls. *Notes:* The figure shows coefficients and 90% confidence intervals for *direct effect*, *informative effect* and *competitive effect*, for 12 post-treatment months, resulting from specification 1. Competitive effect corresponds to estimation on share of beneficiaries among 20 nearest neighbors.

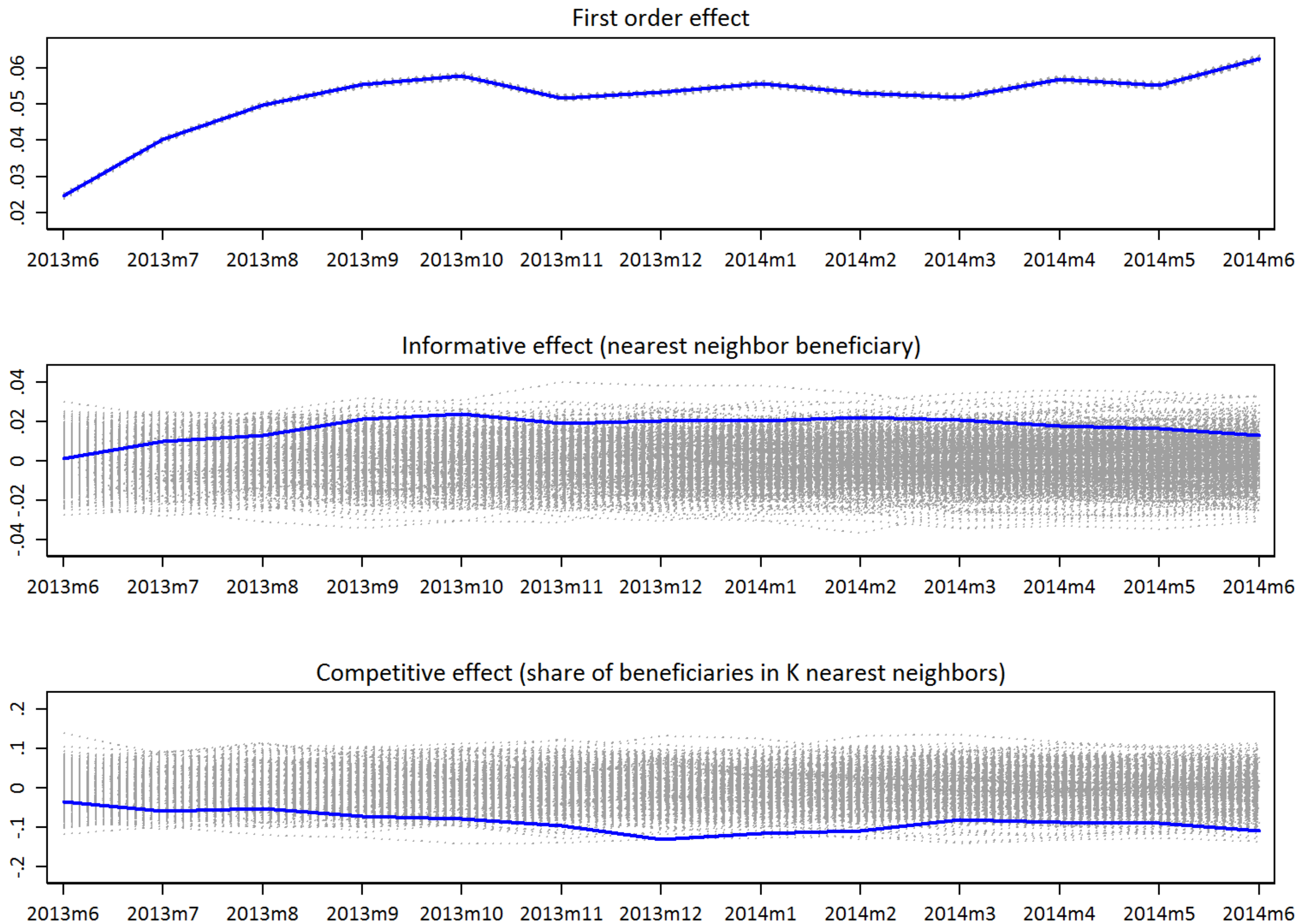


FIGURE 11 Observed vs. placebo locations, specification 1, 15 nearest neighbors, no controls. *Notes:* The figure shows main specification coefficients using observed locations and 1000 location permutations on *direct effect*, *informative effect* and *competitive effect*, for 12 post-treatment months, resulting from specification 1.

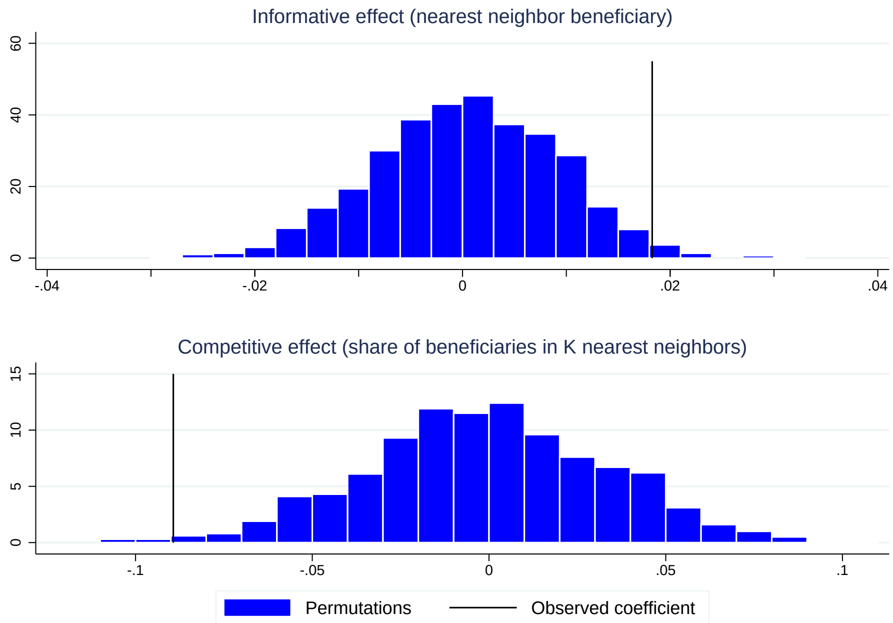


FIGURE 12 Observed vs. placebo locations, period mean effects, specification 1, 15 nearest neighbors, no controls. *Notes:* The figure shows the distribution of period mean coefficients from 1000 location permutations and the observed coefficient for the *direct effect*, the *informative effect* and the *competitive effect*, resulting from specification 1.

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# A | APPENDIX

TABLE A.1 Sample selection. Descriptive statistics and balance tests of georeferenced candidates [a]

| | | Non-georref. candidates | | | | |
|---|---|---|---|---|---|---|
| | Mean | Difference | | SE | Ratio | N |
| | (1) | (2) | | (4) | (3) | (5) |
| Female | 0.5094 | 0.0210 | | (0.0140) | 0.0411 | 8882 |
| Age | 21.138 | -0.3534 | *** | (0.0665) | -0.0167 | 8881 |
| Single | 0.9509 | -0.0129 | * | (0.0066) | -0.0136 | 8882 |
| Children | 0.0814 | 0.0394 | *** | (0.0089) | 0.4840 | 8882 |
| Highschool (18+) | 0.6942 | -0.0919 | *** | (0.0173) | -0.1323 | 5686 |
| Higher education (21+) | 0.0806 | -0.0127 | | (0.0144) | -0.1573 | 2514 |
| Manual application | 0.5050 | 0.0589 | *** | (0.0139) | 0.1166 | 8882 |
| Emp. Jan-2012 | 0.0139 | 0.0022 | | (0.0035) | 0.1612 | 8882 |
| Emp. Feb-2012 | 0.0041 | 0.0011 | | (0.0020) | 0.2634 | 8882 |
| Emp. Mar-2012 | 0.0083 | -0.0019 | | (0.0023) | -0.2233 | 8882 |
| Emp. Apr-2012 | 0.0134 | -0.0056 | ** | (0.0026) | -0.4199 | 8882 |
| Emp. May-2012 | 0.0258 | -0.0096 | *** | (0.0037) | -0.3733 | 8882 |
| UBI (nbhood) | 0.0839 | 0.0376 | *** | (0.0029) | 0.4477 | 8839 |
| EAP no health insurance (nbhood) | 0.3108 | 0.0778 | *** | (0.0052) | 0.2504 | 8839 |
| Unemployment (nbhood) | 0.0714 | 0.0075 | *** | (0.0007) | 0.1053 | 8839 |

*Notes:* The table shows descriptive statistics for the candidates who were successfully georeferenced, together with balance tests between them and non-georeferenced candidates. We report OLS coefficients (column 2) for the non-georeferenced dummy variable taking each listed characteristic as dependent variable. Column 3 shows the ratio of columns 2 and 1, and column 4 shows standard errors in parentheses.

[a] We found GPS coordinates for 7339 candidates, 83% of all PPP candidates in the city of Cordoba.