

# Conceptual and methodological note to measure discrimination against slum dwellers and the impact on discrimination of slum upgrading interventions:

## Case study of Buenos Aires

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## Abstract

Conceptual and Methodological Note on "urban discrimination". The conceptual part is focused on the definition of the term "urban discrimination" (discrimination against slum dwellers) and the development of an economic model representing the theoretical approach. The methodological part describes how to implement an impact evaluation to measure discrimination using the methodology of "audit contracts", and to measure how slum upgrading interventions affect "urban discrimination".

## Jel Codes

- J2 Demand and Supply of Labor
- J21 Labor Force and Employment, Size, and Structure
- J24 Human Capital • Skills • Occupational Choice • Labor Productivity
- J3 Wages, Compensation, and Labor Costs
- J41 Labor Contracts
- J6 Mobility, Unemployment, Vacancies, and Immigrant Workers
- J64 Unemployment: Models, Duration, Incidence, and Job Search
- J7 Labor Discrimination
- O18 Urban, Rural, Regional, and Transportation Analysis • Housing • Infrastructure
- R12 Size and Spatial Distributions of Regional Economic Activity
- R2 Household Analysis

## Keywords

Impact Evaluation, Experimental, Urban Discrimination, “Audit Contracts”, Cities, Urban Development.

# Conceptual and Methodological Note to Measure Discrimination Against Slum Dwellers and the Impact on Discrimination of Slum Upgrading Interventions. Case Study of Buenos Aires

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## Contents

Section 1. Introduction and Background.....	3
Section 2. Research questions and literature review .....	6
2.1. <i>Research question number 1: Do slum dwellers face “urban discrimination” in the labor market? .</i>	6
2.2. <i>Research question number 2: Can SUPs improve social integration by diminishing discrimination and, therefore, promoting inclusion in the labor market among slum dwellers? .....</i>	7
Section 3. Conceptual framework.....	8
3.1. Discrimination and SUPs: A theory of change .....	13
Section 4. Empirical approach .....	14
4.1. The case of urban discrimination in Buenos Aires, Argentina .....	14
4.2. A correspondence study of urban discrimination.....	15
4.3. Implementation of the correspondence study.....	21
4.4. Specification of empirical analysis .....	23
4.4.1. Answering question number 1: .....	23
4.4.2. Answering question number 2: .....	24
Section 5. Expected Results and policy implications.....	24
Section 6. Methodological considerations and limitations of the study .....	25
Section 7. References.....	28

## Section 1. Introduction and Background

In this document, we describe the methodology and conceptual framework that guides our ongoing study of the Buenos Aires job market, which seeks for evidence of discrimination against slum dwellers based on their place of residency (which we call urban discrimination). We look into whether two identical job applicants that reside one in an informal settlement, known as *villas*, and the other in a formal one, known as *barrios*, have different employment success probabilities. We use correspondence studies' methodology to answer two separate questions regarding this issue: 1) are slum dwellers discriminated against in the labor market because of the place where they reside? and 2) can SUPs diminish discrimination and promote inclusion in the labor market among slum dwellers?

Population growth in major cities is a current global phenomenon. Between 1990 and 2016, the number of people living in urban areas has increased from 43 % to 54 % worldwide.<sup>1</sup> Since 1960, the number of people living in urban centers in Latin America and the Caribbean (LAC) has grown nearly by 400 % (compared to a worldwide increase of 300 % during the same time span). As of 2020, 80 % of LAC's inhabitants live in urban areas<sup>2</sup>. While LAC's urban population is currently increasing, 22 of its biggest cities have recently experienced economic contractions (Parilla et al., 2015). These demographic trends are expected to continue in the coming decades, as a result of expected increases in migration flows, primarily motivated by climate change-related phenomena (Rigaud et al., 2018).

Addressing some of these challenges has been at the core of the United Nations' Sustainable Development Goals (SDGs). While urbanization affects most areas of human development, goals #8 and #11<sup>3</sup> are the most closely associated with LAC's urbanization problems. LAC has not been able to enjoy the same benefits from urbanization as developed countries have (CAF, 2017). The region's rapid urbanization has resulted in reduced productivity (Ahrend et al., 2017; Duque et al., 2019) and increased socio-spatial segregation (Aquino and Gainza, 2014; Yunda and Sletto, 2020), which in turn has hindered cities' ability to manage growth sustainably (Belsky et al., 2013; Nagendra et al., 2018). Nearly 20 % of LAC's urban population lives in informal dwellings (slums) characterized by hazardous locations and having poor access to public services (UN Habitat, 2016).

Policy makers in LAC, who often lack sufficient fiscal resources, actively need effective urban policies to deal with the challenges of rapid population and urban growth, such as increasing traffic congestion, shortages and low quality of housing, and diseconomies of scale in the provision of public goods in big cities of the region, all of which threaten economic productivity and the welfare of citizens. Governments, often with the support of multilateral development banks (MDBs), have historically dedicated a large part of their budgets to "slum upgrading programs" (SUPs), which seek to address urbanization and integrate slums into the formal city. SUPs typically call for comprehensive interventions using three main pillars: (i) improving housing property rights (urban planning, titling, etc.), (ii) investing in physical infrastructure (paving roads and streets, building sewage and sanitation systems, providing street lighting, building urban equipment, designing

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<sup>1</sup> World Development Indicators (<http://wdi.worldbank.org/table/3.12>)

<sup>2</sup> World Development Indicators (<http://wdi.worldbank.org/table/3.12>)

<sup>3</sup> Sustainable Development Goals: #8 access to decent jobs and inclusive economic growth, and #11: "Building Sustainable cities and communities"



type of integration with a proxy variable that summarizes the likelihood that a job applicant receives a call back to a job application.<sup>4</sup>

The underlying theory of change that we adopt assumes that SUPs can help achieve integration to the labor market by lowering of transaction costs and removing information asymmetries in that market. Two examples of mechanisms that characterize the barriers to integration to the formal labor market are:

- i. Poor access to quality housing and transportation increases the transaction cost of searching for and maintaining high-quality jobs (CAF, 2017), as neighborhood violence coupled with low-quality roads, as well as poor electricity, drainage, sewage, sanitation, and transportation services, all make it costly for slum residents to find good-quality jobs.
- ii. Access to the labor market might also be limited because some employers dislike working with people who reside in slums based on prejudices and/or negative stereotypes. However, as we will explain later in this document, even when employers are not prejudiced, living in a slum might represent a disadvantage for a job candidate if, because of transaction costs and informational asymmetries, the employer associates the negative attributes of the slum with the productivity of the applicant.

To the extent that SUPs are effective at lowering the housing and infrastructure-related transaction costs that have an impact on the productivity of slum residents, and eventually their costs of searching for quality jobs, we expect the labor market prospects of those people to increase when their neighborhoods receive those interventions. Notice, however, that even when SUPs are successful in increasing the former type of transaction costs, we might still not see a substantial impact on their labor market prospects, as for SUPs to be effective at substantially improving such an outcome, the sources of labor market discrimination would have to be addressed.

Framed within this perspective that highlights the relevance of labor market discrimination to achieve the social integration goal associated with SUPs, our study addresses the following two policy questions:

1. Do slum dwellers face discrimination in the labor market? (Henceforth, we will call this type of discrimination “urban discrimination.”)
2. Do SUP interventions contribute to labor market integration by changing how employers perceive slum dwellers (decreasing “urban discrimination”) and therefore increase their integration into the labor markets?

This information is important for the IADB since it would contribute to the Bank’s goals of (i) reducing social exclusion and inequality and (ii) improving productivity and innovation in the region’s urban centers (IADB, 2015).

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<sup>4</sup> There are several labor market outcomes that are relevant to measure integration to this market. The labor economics literature develops the idea that hiring is a sequential process and receiving a call back from a job application is the first step that might lead to hiring (for instance, as in Heckman and Siegelman (1993)). As will be clear later, the choice of that indicator of integration into the labor market is dominated by the empirical approach we undertake.



The present document provides a conceptual framework to understand urban discrimination and offers a theory of change through which SUP interventions would affect such discrimination. We also describe our empirical approach to studying those phenomena with a case study of urban discrimination: Buenos Aires, Argentina (a country where IADB has invested 1.1 billion USD in SUP programs between 2010 and 2020). In Section 2, we state our research questions and provide a literature review on the state of the art for each of them. In Section 3, we provide a conceptual framework that guides the economic intuition of our empirical research. In Section 4, we describe the empirical aspects including the case of Argentina, the empirical strategy and the methodological aspects to set up the audit study, and the specification of the estimations. At the end of the document (Section 5), we highlight some shortcomings, limitations, and challenges of this study and describe what actions we undertook to minimize their impact on the quality of our research.

## Section 2. Research questions and literature review

### 2.1. Research question number 1: Do slum dwellers face “urban discrimination” in the labor market?

According to Besbris et al. (2018), people who reside in neighborhoods associated with negative characteristics such as poverty or crime may face discrimination when required to disclose their place of residency. They argue that as a result of this “spatial stigma,” those residing in these neighborhoods face the risk of losing job opportunities. To empirically evaluate the existence/magnitude of spatial stigma, Besbris et al. (2015) advanced a correspondence study of discriminatory call-back rates for a sales ad for secondhand iPhones posted from impoverished and black or Latino neighborhoods in the USA.<sup>5</sup>

Bertrand and Mullainathan (2004) pioneered correspondence studies of discrimination in the cities of Boston and Chicago. They found that job applicants from whiter backgrounds had a higher call-back probability than their observationally equivalent, black peers, even after controlling for factors such as education level or job industry. Interestingly for the present research, they found quite strong evidence of discrimination based on ZIP code, which does not vary with race. Since then, correspondence studies have been widely used to measure labor force discrimination. Baert (2018) compiled 90 correspondence studies that evaluated labor force discrimination for a range of traits<sup>6</sup> distributed across all continents but Africa. For Latin America, Boo et al. (2013) found discriminatory call-back rates among less attractive applicants in Argentina, and Galarza and Yamada (2014) and Arceo-Gomez and Campos-Vazquez (2014) found evidence of discrimination against indigenous populations in Peru and Mexico, respectively. Blanco and Boo (2010) also

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<sup>5</sup> Correspondence studies are common in the study of discrimination. In the cited study secondhand cell phones were offered through an online platform for secondhand goods named Craigslist. Sale offers of identical cell phone were made, but each of those offers was associated to an individual who live in a neighborhood that could be easily associated to join large majority of either black, white or Latino residents. Then a comparison of call back rates would allow to identify

<sup>6</sup> Baert (2017) only looks for correspondence studies that cover a trait that is explicitly prohibited in at least one state in the USA. The list of traits includes race and national origin, gender and pregnancy, religion, disability, (older) age, military service or affiliation, wealth, genetic information, citizenship status, motherhood, marital status, sexual orientation and gender identity, political affiliation, union affiliation, and physical appearance.

identified positive discriminatory call-back rates among applicants with information and communication technology skills in Buenos Aires. More recently, Yamada and Galarza (2019) found discrimination based on traits like beauty and sex in Peru.

With reference to Argentina (our case study), the Argentinian National Institute Against Discrimination, Xenophobia, and Racism (INADI) identified several instances of discriminatory hiring practices based on place of residency that limit those living in informal settlements from accessing formal jobs regardless of their qualifications (INADI, 2017). Furthermore, some empirical evidence from Callegari et al. (2017)<sup>7</sup> and Boo and Trako (2009) points to the existence of discrimination based on socioeconomic condition using correspondence studies. Additionally, focus groups and structured interviews (conducted by the researchers in this study) provided accounts of a market for addresses in Argentina, where residents of slums “rent” addresses from formal areas to avoid urban discrimination when searching for jobs.

## 2.2. *Research question number 2: Can SUPs improve social integration by diminishing discrimination and, therefore, promoting inclusion in the labor market among slum dwellers?*

Because of the magnitude of the investments represented by SUPs in the developing world, and their expected capacity to integrate slum dwellers with markets, policy makers are interested in assessing the welfare effects of SUPs. However, evidence in this respect is limited. In part, this happens because the evaluation of these programs presents important methodological challenges such as small number of treated units<sup>8</sup> (Brakarz and Jaitman, 2013).

One example of an integral program evaluated was the quasi-experimental evaluation of the SUP program Favela Bairro in Brazil (Soares and Soares, 2005). Results showed positive impacts on infrastructure-related outcomes, such as the use of sewage water and garbage collection, but failed to determine the program’s impact on outcomes such as literacy or household income, most likely because of the data limitations that the study faced.

However, some evidence has showed that partial interventions, which involves components of SUPs (such as interventions on infrastructure or access to financial resources), have positive impacts on wellbeing and integration. For instance, an experimental evaluation of a program that facilitated access to credit for households to finance water services connection improvements in Morocco showed positive impacts on disposable time, wellbeing, and community bondage but no effect on income, labor market participation, or schooling (Devoto et al, 2011). Other evidence in India from a randomized evaluation of microfinance projects found that treated areas presented higher spending on durable goods and lower on “temptation goods” but no change in health, education, or women’s empowerment (Banerjee et al, 2015).

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<sup>7</sup> Callegari et al. (2017) evaluate call-back rates from identical applicants that reside in different nearby locations where one corresponds to a *villa* (Villa 31) and the other to a *barrio* (Recoleta) with higher socioeconomic status. This research was conducted as an undergraduate dissertation for the School of Economics at the Torcuato di Tella University in Buenos Aires, Argentina.

<sup>8</sup> In the program evaluation literature, the term “treated” refers to subjects selected to receive the intervention being evaluated.

Some impact evaluations have measured whether training programs for youth living in deprived neighborhoods in Latin America affected their employment probability, hours worked and/or wages. Results show that short-term impacts on labor outcomes tend to dissipate in the long term. Ibararán and Shady (2009) focused on seven labor force capacity-building programs for youth living in impoverished neighborhoods, and their results suggest an increase of up to 5 % in employment rates among recipients (greater among women in Colombia and Panama) as well as a positive impact on the quality of jobs. Furthermore, Attanasio et al. (2008) studied a similar program in Colombia and found generalized positive effects, which were found to be greater in women than in men. Their evidence suggests that women who receive the training have a higher probability of finding a job and tend to work more days per week and hours per day than women who do not receive the training. Monthly income for treated women is reported to be 18 % higher than that of their counterparts. Acevedo et al. (2020) found that vocational training in the Dominican Republic had positive impacts in the short run for women.

Generating knowledge about the effects of SUPs on slum dwellers' access to the labor market is a public policy priority that could complement the cost-effectiveness assessments of SUP interventions and/or improve the effectiveness of these programs. In the next section, we summarize the theoretical framework that provides a conceptual basis for the empirical strategy we proposed to answer these questions.

### Section 3. Conceptual framework

To guide our intuition, we use insights from the economic models for employer taste-based and statistical discrimination. *Taste-based discrimination* (Becker, 1957) takes place when the employer's valuation of the employee's traits (for instance, race, gender, and/or place of residency) affects the wage those employers are willing to offer to a job applicant independently of how these traits correlate with the employee's marginal productivity. When making hiring decisions, employers will act as if there is an additional cost (a prejudice prime) to be discounted from the wage they offer to people who have the trait. Charles and Guryan (2008) estimate that one-quarter of the racial wage gap in the USA could be attributed to that prejudice prime. Rooth (2010) and Nunley et al. (2015) found evidence of taste-based discrimination on race through field experiments.

On the other hand, *statistical discrimination* (Phelps, 1972) could occur when an employer is faced with a choice between two candidates who are revealed to have the same observable qualifications, yet those qualifications provide a noisy signal of their productivity. If the employer knows that the applicants belong to different groups (they differ in race, gender, place of residency, etc.), the employer could draw information from the characteristics of those groups to improve his assessment about the productivity of the worker. When that happens, even a non-prejudiced employer might discriminate if the group characteristics disfavor the productivity assessment of the members of a group.

There can be multiple interactions between taste-based and statistical discrimination (Bertrand and Duflo, 2017). In this document, we argue that some of those interactions occur because (1) as in statistical discrimination theory, obtaining information about workers' skills is costly, and (2) the

capacity of employers to process such information is limited (bounded rationality).<sup>9</sup> When information is costly and rationality is bounded, one can abandon the assumption that employers form accurate depictions about the productivity of the members of a group they are prejudiced against. Instead, when a prejudiced employer is considering hiring workers from a group he dislikes, his own prejudice will influence how he forms expectations about that group's productivity.

Under this perspective, prejudice is not only a prime to compensate for the disutility of the employer when hiring a member of the disliked group (as in Becker) but also a factor that defines how the employer forms expectations about the productivity of the members of the disliked groups.

The prior that the employer has about the distribution of skills of the disliked group does not necessarily have to represent the “true” distribution of skills (as assumed in the canonical model of statistical discrimination). As several authors have noticed, representation heuristics, confirmation bias and/or probabilistic extrapolation might dominate the formation of judgements about the attributes that characterize group members, as those aid simplifying a rather complex task (Kahneman and Tversky, 1972; Rabin and Schrag 1999 and Barberis; Shleifer, and Vishny 1998 ). In influencing those priors (true or mistaken), several factors might be relevant, such as previous direct interaction with (and indirect observations of) people, as well as opinions stated by mass media and, perhaps, social media from and about the group discriminated against.<sup>10</sup> That employers might profile a mistaken distribution of skills is also consistent with the idea that there are “kernels of truth” that consolidate as people tend to overweight the representative type in target groups (Bordalo, Coffman and Shleifer, 2016).

To more clearly illustrate our view on how taste-based and statistical discrimination could interact with each other, we show a conceptual model that described such interrelatedness. Our model assumes that hiring decisions are most likely subject to a certain degree of discrimination whose sources, as we will show, can only be readily distinguished in extreme cases. Becker's taste-based discrimination model, as formalized by Charles and Guryan (2008), is our starting point.

Let us assume a perfectly competitive market in which the utility  $V_e$  of employer  $e$  increases in the profits he makes  $\pi_e$ . Profits are a function of the total number of units of labor hired =  $L_b + L_v$ , which adds units of labor from workers who do not have the trait that the employer dislikes,  $L_b$ , and individuals who exhibit the trait  $v$  he dislikes,  $L_v$ . The employer's utility function is also assumed to decrease in  $d_e$  with each unit of labor that is hired from the pool of  $L_v$  workers who have the trait  $v$ . The employer chooses labor from groups with traits  $b$  and  $v$  to maximize the utility function:

$$1. V_e = f_e(L_b + L_v) - w_b L_b - w_v L_v - d_e L_v ,$$

where  $f_e$  is a production function with constant returns to scale and  $w_b$  and  $w_v$  are the wages of the workers in each group. The first three terms of the utility function  $V_e$  add up to the profit of the firm:  $\pi_e = f_e(L_b + L_v) - w_b L_b - w_v L_v$ .

<sup>9</sup> See Varshney and Varshney (2017) for a bounded rationality approach to statistical discrimination [https://krvarshney.github.io/pubs/VarshneyV\\_pieeee2017.pdf](https://krvarshney.github.io/pubs/VarshneyV_pieeee2017.pdf) .

<sup>10</sup> Pager and Karafin (2009)

One of the first-order conditions for maximizing such utility indicates that, in equilibrium, the employer would hire the number  $L_v^*$  and  $L_b^*$  of workers so that the value of the marginal product of their labor equals the marginal cost that the employer faces. Compared to the classic theory of the firm, a non-standard result from Becker's theory of discrimination is that the marginal product of labor is influenced by the employer's distaste parameter  $d_e$ . From the equilibrium equivalence between marginal product and costs, the distaste parameter becomes the difference between what the employer is willing to pay the workers with the conspicuous trait for which he has a distaste ( $w_v$ ) and what would have been the marginal product of those workers' labor in the absence of such employer distaste:

$$2. \quad d_e = w_v - f_e'(L_b^* + L_v^*).$$

One conclusion from Becker's model that is relevant for this study is that, in the market, the short-term equilibrium wage gap between members of groups  $v$  and  $b$  will be determined by the marginal discriminator (the employer who is the most prejudiced among those who happen to hire members of group  $v$ ). For a given distribution of prejudice among employers, the equilibrium wage of individuals who belong to group  $v$  in terms of those wages in group  $b$  (the relative wage) is affected by the supply of workers in group  $v$  relative to the supply of workers in group  $b$ . If the size of group  $v$  is sufficiently small, so that all of them can be hired by non-prejudiced employers, the discriminators will not have influence on the equilibrium relative wages, and there will be no inequality. If the distribution of prejudice does not change, as the supply of workers in group  $v$  relative to that of group  $b$  grows, members of group  $v$  could not all be hired by non-discriminating employers, and so the most prejudiced employer to hire members of  $v$  will determine the equilibrium wage gap. In equilibrium, inequality in wages would result from prejudice at the margin and not from the influence of average prejudice among employers.

Let us assume a given supply of workers in group  $v$ , all of whom are hired by non-discriminating employers. For inequality in wages to exist, it is necessary that at least one member of  $v$  is hired by a discriminating employer. The wage gap could arise if, for instance, the distribution of prejudice among all the employers now changes, and some of them become prejudiced: differences in the relative wages in equilibrium would result from changes in the extensive margin of prejudice. Relatedly, if some wage inequality in equilibrium already exists, the equilibrium wage gap could increase if the employers' distaste about working with members of group  $v$  increases, changing the distribution of prejudice. Such changes in the intensive margin of prejudice would also affect the equilibrium wage gap.

Introducing the assumptions of imperfect information and bounded rationality to this model allows us to illustrate the interrelatedness between statistical and taste-based discrimination. Under these assumptions, the employer faces costs of assessing the valuable attributes that determine the employees' skill set, including their intelligence, motivation, grit, perseverance, family, cultural background, and several other factors. If the value of the marginal product of labor (net of prejudice) is imperfectly observed, then employers optimize their expectations of such values based on the information available to them and within the constraints of their computational abilities.

More formally, let us assume that employers assess the wages  $w_g$  that they will offer to job candidates who belong to a group  $g(b, v)$  as in the canonical statistical discrimination model: wages offered are a linear combination of the assessments about both the worker's observed productivity  $Q_g$  and the productivity of the group to which that worker belongs  $Q_{\bar{g}}$ :

$$3. \quad w_g = \theta_g(Q_g) + \theta_{\bar{g}}(Q_{\bar{g}}),$$

Our key hypothesis is that when information is imperfect, so that the productivity of a prospective worker within a firm is manifested as a noisy signal and the capacity of the employer to process that information is also limited (rationality is bounded), the employers form expectations about the productivity of job applicants that are influenced by their own prejudices. Resorting to prejudiced convictions and ideas about the productivity of the members of a group is an effective behavioral response to deal with uncertainty, considering the limited cognitive capabilities that characterize humans. In the words of Herbert Simon, this hypothesized behavioral heuristic might be consistent with "...the kind of rational behavior that is compatible with the access to information and the computational capacities that are actually possessed..." (1955, pp. 99)

In its simplest form, the statistical discrimination approach assumes that, when assessing group productivity  $\theta_{\bar{g}}(Q_{\bar{g}})$ , the employers draw inferences from a prior distribution that represents the "true" distribution of skills characterizing the average member of the  $g$  group. We relax the assumption that the prior distribution is necessarily the true distribution of skills in the group of reference. Instead, we acknowledge that the employers' assessments of productivity might also be affected by the informational content of a distribution of reference that reinforces the employer beliefs (prejudiced or not).

To formalize the previous point, let us call  $\rho(Q_{\bar{g}})$  the true distribution of skills that characterizes workers in group  $v$  and further assume that with  $\frac{\partial w_v}{\partial \rho(\cdot)} > 0$ .<sup>11</sup> In addition, let us call  $\tau(Q_{\bar{g}})$  with (with  $\frac{\partial w_v}{\partial \tau(\cdot)} < 0$ ) the distribution of skills that is perceived by the prejudiced employers who, in addition to facing noisy signals of productivity when workers apply for jobs at their firms, are also rationally bounded. The  $\tau(\cdot)$  distribution reinforces the employer's beliefs about negative attributes of those workers in group  $v$ . For the prejudiced employer,  $\tau(Q_{\bar{b}}) = 0$  and  $\tau(Q_{\bar{v}}) < 0$ .<sup>12</sup> The employer's group productivity appraisal can be modeled as a linear combination of the prejudice-biased and the true distributions. If we assume additive separability and linearity, expectations of group skills can be modeled as follows:

$$4. \quad \theta_{\bar{g}}(Q_{\bar{g}}) = \rho(Q_{\bar{g}}) + D[\tau(Q_{\bar{g}})],$$

where:  $D = 1$  if  $d_e > 0$  and  $D = 0$  otherwise.

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<sup>11</sup> This is an arbitrary assumption; however, it does not affect our main argument or conclusions regarding the process of expectation formation and how it affects the employers' evaluation of the productivity of an individual.

<sup>12</sup> A similar assumption about the prior group ability distribution formed as the product of the true and the prejudice-influenced distribution is found in Farmer, A., & Terrell, D. (1996). Discrimination, Bayesian updating of employer beliefs, and human capital accumulation. *Economic Inquiry*, 34(2), 204–219.

Equation 4 implies that for group  $b$ , which is not subject to prejudice, the employer will form group expectations based on the true distribution of skills; while for group  $v$ , the employer will resort to both the true and prejudice-biased distributions of skills, more formally:

- 4.1.  $\theta_{\bar{b}}(Q_{\bar{b}}) = \rho(Q_{\bar{b}})$ , for the non-discriminated group, and;
- 4.2.  $\theta(Q_{\bar{v}}) = \rho(Q_{\bar{v}}) + \tau(Q_{\bar{v}})$  for the discriminated group.

The point of this model is to illustrate that prejudice could influence the process through which the expectations of group productivity are formed. Replacing equation 4 with 3, we obtain:

5.  $w_g = \theta_g(Q_g) + \rho(Q_{\bar{g}}) + D[\tau(Q_{\bar{g}})]$ , which for groups  $b$  and  $v$  becomes correspondingly:

- 5.1.  $w_b = \theta_b(Q_b) + \rho(Q_{\bar{b}})$
- 5.2.  $w_v = \theta_v(Q_v) + \rho(Q_{\bar{v}}) + \tau(Q_{\bar{v}})$ .

Equation 5 showcases how the statistical and taste-based discrimination models are related through the distaste parameter  $d_e$ . When equation 5 is replaced with 1 and maximizing utility, the first-order conditions are as follows:

6.  $f'_{e|b}(L_b^* + L_v^*) = \theta_b(Q_b) + \rho(Q_{\bar{b}})$
7.  $f'_{e|v}(L_b^* + L_v^*) = \theta_v(Q_v) + \rho(Q_{\bar{v}}) + \tau(Q_{\bar{v}}) + d_e$ .

So, in equilibrium, the value of the marginal products from changes in  $L_b$  ( $f'_{e|b}$ ) and  $L_v$  ( $f'_{e|v}$ ) each equals the marginal cost for the employer. This simple model illustrates two important features of the discrimination process. On the one hand, the discrimination parameter is the difference between the marginal product of labor in the absence of discrimination and the perceived contribution of the employee to the productive process, which is affected by the employer's bias. Under imperfect information and bounded rationality, discrimination not only affects wages through distaste but also through the impact it has on the way how the employer forms expectations about the productivity of employees.

This model also accommodates a general interpretation of which marginal costs are associated to the equilibrium choice of units of labor. For instance, when the employer is prejudiced ( $d_e > 0$ ), there is perfect information about the valuable attributes of the worker, and there exists no bounded rationality. In this case, the marginal cost in equation 6 for group  $v$  boils down to  $\theta_v(Q_v) + d_e = w_v + d_e$ , and we are back to the canonical Becker's model of taste discrimination<sup>13</sup>. In the absence of prejudice ( $d_e = 0$ ) and assuming that information is imperfect, but rationality is not bounded, the model boils down to the statistical discrimination model. In this case, noisy signals affect the allocation of weights into the group's and the individual's perceptions of the marginal cost of workers in group  $v$ . Equation 6 becomes  $f'_{e|v}(L_b^* + L_v^*) = \theta_v(Q_v) + \rho(Q_{\bar{v}})$ . Equation 6

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<sup>13</sup> Notice that because there is perfect information, the employer's assessments of the group productivity of members of group  $v$  ( $\tau(Q_{\bar{v}}), \rho(Q_{\bar{v}})$ ) are unnecessary.



highlights that, empirically, the equilibrium wage results from valuations that, at the margin, might be both influenced by prejudice and affected by informational costs.

The discrimination model of wage determination represented in equations 6 and 7 leads to a key conclusion about who would get a job offer that is relevant for the empirical strategy that we describe in the next sections of this document. The prejudiced employer will extend job offers to candidates from the group prejudiced against (excluding distaste considerations) if they have higher expected productivity than workers from the group that is not subject to prejudice. While this conclusion is the same as can be obtained from Becker's model of discrimination, we highlight some potential sources of discrimination often overlooked in discrimination studies in economics that, in fact, can be relevant for policy.

### 3.1. Discrimination and SUPs: A theory of change

We are now able to give some formal content to our account about how SUP interventions can affect labor market discrimination, with reference to our general model of discrimination and to the components that characterize those interventions.

First, when SUPs include interventions that provide effective job and skills training and help facilitate job searches (for instance, by enhancing social capital networks and facilitating links between enterprises and workers), the productivity of individuals who reside in slums is expected to increase. Productivity could also increase as people residing in urban slums who are subject to interventions by SUPs are safer in their own neighborhoods, have cheaper access to transportation networks and public services (including sanitation, education, and health), and face lower risks of homelessness because of strengthened property rights. This is a straightforward effect from the prototypical investments represented in SUPs that would improve job market prospects of slum dwellers irrespective of the type of discrimination exercised by prejudiced employers.

Second, for the reasons just described, a SUP intervention would not only positively affect individual productivity but could also do the same to the average productivity level  $\rho(Q_{\bar{v}})$  of workers in group  $v$ . If, as we described, SUPs reduce job market barriers for certain workers and reduce their transaction costs, while also improving their skills, then we could expect average productivity to increase. If information is imperfect, then improvements in the true distribution of skills could make slum dwellers more employable.

Third, if the information about the skills of a worker who lives in a slum gives a noisy signal of his/her productivity (which is a plausible assumption to make in LAC), SUP interventions could also help reduce the noise in that signal when they submit job applications. SUPs can be loaded with informational content to teach employers about the true distribution of the skills of workers in group  $v$ , including not only references to the mean levels of productivity and education across occupations but also about how dispersed and skewed those distributions are.



As will be described in detail next, in our empirical analysis, we compare call-backs to job applicants from formal<sup>14</sup> and informal neighborhoods when both apply for a job and assume that the productivity signals that they submit their job applications are noisy indicators of their skills. Call-back rates will be based on expected productivity differences between two workers with identical skills whose unobservable factors distribute equally, and who have different places of residence (formal vs. informal), yet the source of discrimination will not be distinguished. Thus, the employer will call the applicant with the highest expected marginal productivity.

## Section 4. Empirical approach

### 4.1. The case of urban discrimination in Buenos Aires, Argentina

Greater Buenos Aires, Argentina, faces most of the problems inherent in rapid urbanization. In this urban agglomeration, there are 1071 informal settlements where 329.464 families live<sup>15</sup>. Of these informal settlements, 340 are urban slums (112,957 families), denoted more commonly as “villas”. These villas are defined as a type of informal settlement that lacks basic infrastructure and has inadequate land tenure regime relative to local legislation (Clichevsky, 2003). According to Bonfiglio and Marquez’s (2017) characterization of *villas* in Buenos Aires, the population is younger on average with a higher proportion of children per household and fewer households, formed only by younger couples or elderly persons without children. Approximately 60 % of inhabitants live in precarious conditions that include both living and environmental conditions. Additionally, the unemployment rate is higher, and the size of the labor force is smaller (with a higher negative impact for women). Furthermore, four of every ten households report having no social protection from any type of institutions whether public or private. In general, for residents of Buenos Aires *villas*, difficulties associated with socioeconomic integration, education deficits, and other types of social vulnerabilities tend to be more extended and more intense than for the rest of the urban population (Bonfiglio and Marquez, 2017).

In this context, the unequal distribution of resources, urban goods, and services makes the City of Buenos Aires the place with one of the strongest contrasts between wealth and social deprivations in Argentina (Bonfiglio and Marquez, 2017). A clear illustration of this disparity is Villa 31, the oldest and largest villa of the city, located near the center of Buenos Aires and next to the richest neighborhoods, called Recoleta. In this informal settlement, there are marked differences in the labor market and in education with respect to the formal city. For example, Villa 31 doubles the share of non-registered wage workers of the city (51% vs 26%), and school dropout for adolescents aged 13-17 years is more than the double (10% vs 4%)<sup>16</sup>.

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<sup>14</sup> Formal in this context refers to a territory that has been urbanized according to a urban plan by the government and is not considered a slum. Along this document it can also be called *barrio*. Informal in this context refers to a neighborhood that has not been planned. Along this document it can also be called *villa*.

<sup>15</sup> According to the Villas and Precarious Settlement Public Provincial Registry (Law# 14.449) of the Ministry of Infrastructure of the Province of Buenos Aires.

<sup>16</sup> Diagnóstico socioeconómico del Barrio 31 Relevamiento Socio-Demográfico 2016. Secretaria de Integración Social y Urbana. Gobierno de la Ciudad de Buenos Aires.

The Argentinian government has undertaken several efforts to mitigate these problems. In 1996, the government started the first national Urban Upgrading Program (PROMEBA, for its acronym in Spanish) funded by the IADB, and in 2007, the government established a Conditional Credit Line for Investment Projects (CCLIP) with the objective of improving the quality of life and of contributing to the urban and social inclusion of Argentine households in the poorest segments of the population. Between 2010 and 2019, the IADB has invested more than 1000 million USD to projects with the objective of integrating *villas* into the formal framework surrounding the city of Buenos Aires. As of today, the CCLIP has financed three operations inside the Barrio Improvement Program (PROMEBA) framework aimed at improving the habitability of households living in informal neighborhoods, with a target of approximately 250,000 households in shantytowns and unregulated settlements.

After PROMEBA, more SUPs have followed financed by the IADB as well as the World Bank or the Argentinian Government. For instance, in 2016, the Government of the Autonomous City of Buenos Aires initiated an integration policy of *villas* to the formal city. With special emphasis on Villa 31, the Social and Urban Integration Department was created within a comprehensive urban plan aiming at improving education, health and labor access, and providing social facilities, basic infrastructure, urban connectivity, and housing habitability. The implementation of this plan counts with the support of the IADB and the World Bank. Also, in 2019, the IADB financed the Social and Urban Integration Program which targets 5,000 households in about 14 informal neighborhoods included in the National Registry of Informal Neighborhoods (RENABAP). This program also includes expansion of community participation in social programs and integration into the productive fabric of the formal city. The Social and Urban Integration Program in the Province of Buenos Aires represents the first operation within a second CCLIP to support the Province of Buenos Aires government. It aims to help 23,600 people living in informal neighborhoods in the province and includes integrating them into the municipal urban fabric and increasing opportunities for social, labor, and productive community.

#### 4.2. A correspondence study of urban discrimination

To answer our policy questions, we employ a correspondence study methodology following the seminal work of Bertrand and Mullainathan (2004) based in urban areas of Buenos Aires. In correspondence studies, researchers reply to publicly available real job offers with fictitious job applications tailored to fit the description of those jobs. Two or more job applications are designed to be observationally equivalent but to reflect only differences in the trait(s) of interest(s) upon which basis discrimination is expected to occur (the place of residence in our case). After submitting the job applications, researchers measure the call-back rate differences among fictitious applicants with different traits. Call-back rate differences provide a measure of discrimination in the labor market against that trait.

To answer our **first research question**, our correspondence study will measure pairwise differences in call-back rates to fictitious job applications from residents of *villas* and residents of

formal neighborhoods, commonly known as *barrios*<sup>17</sup>. Following standard practices in correspondence studies, we will build profiles of job applicants (CVs) that are observationally identical in all dimensions, but the denomination of the place of residence is listed as a “villa.” Duplets of applications will be created, in which one applicant will report living in a *villa* and another in a *barrio*, and each duplet will be submitted to all job offers identified. Notice that in each duplet, both the *villa* and *barrio* will be selected to pair socioeconomic status; in this way, we can come as close as possible to establishing discrimination from the fact that somebody resides in a *villa*, and not the fact that the *villa* and *barrio* have different poverty levels.

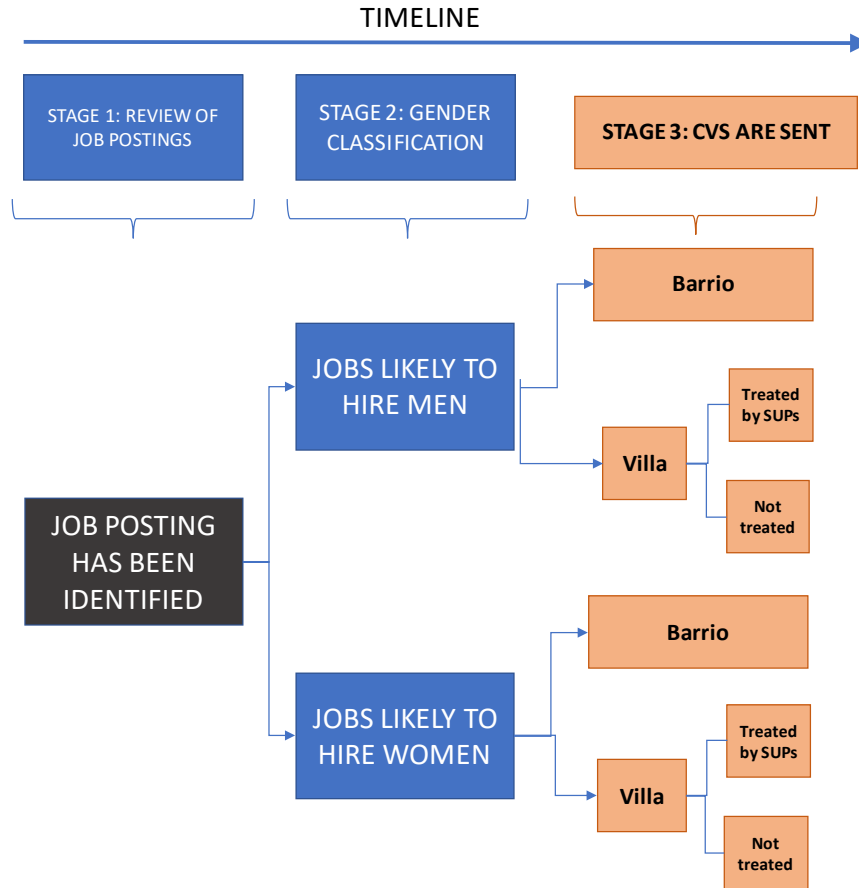
To answer our **second research question**, the correspondence study will allow us to compare differences in call-back rates in duplets of fictitious job applications made to represent people who reside in *villas* that have been subject to interventions by SUPs (or “treated,” in the language of program evaluation: *treated villas*) and people who reside in *villas* that have not received interventions by SUPs (*untreated villas*).

We will send fictional duplets of CVs to a pool of real explicit job posts gathered from a daily inventory of job offers in websites and newspapers between January and July 2020. Figure 2 illustrates a timeline of the process of submission of duplets for a job application. For each cell combining an eligible job offer (inside the occupational category), gender, and application type, one pair of fictitious applicants’ CVs will be submitted (one representing a resident of a *barrio* and another one a resident of a *villa*).

**Figure 2:** Summary of the process of submission to a job posting

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<sup>17</sup> *Barrios* present a regular urban network, subjected to official regulation with regards to land ownership and occupancy.



The study will run from October 2019 to August 2020. Table 1 shows the timeline and main milestones for the implementation of the study. In the first 3 months, professional profiles were created, and several presentations and focus groups were held with local academia, human resource recruiters, government departments, slum dwellers, and civil organizations of the *villas* with the twofold objective to validate the profiles of the CV – making them realistic and observationally equivalent for a human resource recruiter – and to determine what the key criteria are to match *villas* and *barrios* and ensure that we are not affecting the probability of hiring by factors other than “urban discrimination,” such as the distance to the job opportunity. In the following trimester, we conducted a pre-test of the delivery options and a pilot using *Villa 31* (an iconic *villa* that has been receiving very relevant interventions in the last 4 years) as the treated *villa*, *Villa 21–24* as untreated *villa* (a *villa* in the southern part of the city that has not received an intervention besides some very partial interventions in very localized areas), and Montserrat as the *barrio* of control (Montserrat is a low-income *barrio* localized in the center of Buenos Aires). During the pilot, almost 1,000 duplets were sent. After reviewing the results of the pilot, we scaled to the rest of the duplets, and analysis and delivery of the results are planned to finalize the study.

**Table 1.** Timeline of the study

	Months										
	1	2	3	4	5	6	7	8	9	10	11

Creation of the CVs	X										
Focus groups to:		X									
- Validate CVs' profiles											
- Determine the criteria for matching <i>villa</i> – <i>barrio</i> duplets											
Pre-test			X								
Pilot				X	X						
Sample size calculations and selection of duplets					X	X					
Massive CVs delivery						X	X	X	X		
Analysis										X	
Results											X

### **Sample selection and sample size considerations**

Call-back response rates in similar studies to the one we propose have ranged between 5 % and 25% of the duplets submitted. Researchers are limited in the conclusions they can draw because of weak statistical power when duplets receive few call-backs in correspondence studies. Lessons learned from our pilot study, and from focus groups, suggest that to maximize the response rates from job applications, the *villa*–*barrio* duplets should be selected based on the following criteria:

- 1) The geographical location of the duplet should be proximal to where the job is posted, which would lower a potential “distance-to-job” effect that gives priority to job applicants who signal low commuting costs. In cases in which the job’s location is not specified, we randomly assign a geographical location to the duplet in Buenos Aires.
- 2) The names of the *villas* should be conspicuous, so that we maximize the ability of employers to identify where job applicants reside and capture the dimension of discrimination we are researching.
- 3) The time span between when the job is posted and the duplet application is submitted should be no longer than 2 days.
- 4) The qualifications of the applicants are carefully customized to match the profile of the workers that is specified in each job post.

When constructing the *villa*–*barrio* duplets, each selected *villa* is coupled with an observationally (in proximity and socioeconomic characteristics) equivalent *barrio*. Twenty *villas* enter our research design, with 10 treated and 10 untreated. *Villas* for the study are selected to ensure conspicuity (since the employer needs to be knowledgeable of the *villa*), proximity to an area of economic activity<sup>18</sup> and, for the treated *villas*, that they have received an SUP. The ten untreated *villas* were those that had not yet received any intervention; however, five of them are due to receive interventions in the near future, allowing us to create a baseline for a potential prospective evaluation.

The selection of control *barrios* is based on proximity to the *villa* (*barrios* should be within the same locality/municipality as *villas*) and similarity in demographic and economic characteristics

<sup>18</sup> To ensure there are enough job vacancies for the study, which ended up with the selection of *villas* in the City of Buenos Aires and in its nearest belts – located in areas of the first and second belts of greater Buenos Aires

(which we identify using the Argentinian census data from 2010 measured at the census-tract level). The similarities in socioeconomic status between the *villas* and *barrios* in each duplet will also be validated qualitatively by stakeholders in focus groups and interviews.

Power calculations are not straightforward in correspondence studies. Sample sizes in these studies can vary from 100 (Neumark, Bank, and Van Nort, 1996) to 3,000 (Yinger, 1991). In the most-cited study in this literature, Bertrand and Mullainathan (2004), whose outcome of interest was race discrimination, sent nearly 5,000 CVs in response to approximately 1,300 job vacancies. A similar correspondence study in Chile was also conducted to measure social class and neighborhood discrimination, where researchers sent 11,016 CVs in response to nearly 1,377 job vacancies for a response rate of 14.65 % (Bravo, Sanhueza, and Urzúa, 2009). Our study counts on one of the largest samples in the empirical literature, in terms of both job vacancy applications (5,000) and numbers of CVs submitted (10,000).

Vuolo et al. (2015) have discussed some statistical tests to address sample size in correspondence studies and provide some statistical tests to measure them. According to the suggested McNemar's test, a sample size of at least 153 observations for a power of 0.8 and statistical significance of 0.05, with 40 % of positive responses for the treatment (and no response for the control) and 60 % of positive responses for the control (with no response for the treatment), is required to find statistical significance for each *villa*. Since we are sending applications in response to at least 250 vacancies per experimental unit (trials, duplets of each of the 20 *barrio-villa* pairings), we consider our study to be sufficiently powerful to measure discrimination. Table 2 represents the outline of the sample selection and sample size.

**Table 2.** Outline of the sample selection and sample size

		Villas	Barrios	Duplets	# Job offers per duplet
Untreated	Not in pipeline	Villa 1 (pilot)	Barrio 1 (pilot)	Duplet 1 (pilot)	500
		Villa 2	Barrio 2	Duplet 2	225
		Villa 3	Barrio 3	Duplet 3	225
		Villa 4	Barrio 4	Duplet 4	225
		Villa 5	Barrio 5	Duplet 5	225
	In Pipeline	Villa 6	Barrio 6	Duplet 6	225
		Villa 7	Barrio 7	Duplet 7	225
		Villa 8	Barrio 8	Duplet 8	225
		Villa 9	Barrio 9	Duplet 9	225
		Villa 10	Barrio 10	Duplet 10	225
Treated	Villa 11 (pilot)	Barrio 11 (pilot)	Duplet 11 (pilot)	500	
	Villa 12	Barrio 12	Duplet 12	225	
	Villa 13	Barrio 13	Duplet 13	225	
	Villa 14	Barrio 14	Duplet 14	225	
	Villa 15	Barrio 15	Duplet 15	225	
	Villa 16	Barrio 16	Duplet 16	225	
	Villa 17	Barrio 17	Duplet 17	225	
	Villa 18	Barrio 18	Duplet 18	225	
	Villa 19	Barrio 19	Duplet 19	225	
	Villa 20	Barrio 20	Duplet 20	225	
TOTAL:				5050	

To ensure we have sufficient statistical power, we conduct an additional exercise in power calculations, assuming that this would be an experimental exercise. We assume a power of 80 % and a confidence level of 95 %. For the first question, we will be comparing 20 *villas* to 20 *barrios*, so we will have 20 treatment and 20 control clusters. To answer the second question, we will be comparing treated and untreated *villas*; therefore, we will only have 10 treatment and 10 control clusters. Since we lack evidence regarding intra-cluster correlation and discrimination prevalence in the area, we propose four scenarios to calculate a possible range of minimum detectable effects. With the sample of the study, for the first question, we could detect a range of impacts in call-back

rates from 0.2 % to 8.8 %. For the second research question, we could detect impacts ranging from 0.3 % to 12.5 %. Therefore, differences in call-back rates below 8.8 % for the first question and 12.5 % for the second question may not be sufficient evidence of “urban discrimination.”

**Table 3.** Power sample calculations

	Research question 1				Research question 2			
	Scenario 1	Scenario 2	Scenario 3	Scenario 4	Scenario 1	Scenario 2	Scenario 3	Scenario 4
Power	80%	80%	80%	80%	80%	80%	80%	80%
Confidence Level	95%	95%	95%	95%	95%	95%	95%	95%
Cluster size	250	250	250	250	250	250	250	250
Treatment clusters	20	20	20	20	10	10	10	10
Control clusters	20	20	20	20	10	10	10	10
Intra-cluster correlation	1	0	1	0	1	0	1	0
Prevalence	100%	100%	1%	1%	100%	100%	1%	1%
Minimum Detectable Effects	2.80%	0.20%	8.80%	0.60%	4.00%	0.30%	12.50%	0.80%

Each job submission provides an observation for the overall analytical database. The attributes of each job submission will be recovered in that database. Those attributes include characteristics of the individuals who form the duplet in the submission, attributes of the job offer (company, occupation, gender requested for the job, date of the job posting, and the submitted application, among others), and the outcome of the job application with three possible values: call-back to one of the applicants, call-back to both of the applicants, or no call-back to either applicant.

### 4.3. Implementation of the correspondence study

#### **Building resumes**

A database of resumes was created using real resumes in a predefined format. Additional aesthetic elements will be modified depending on the job level on the application. Resumes will have three modules: (1) personal information, (2) professional experience, and (3) educational background.

#### ***Module 1: personal information***

Each synthetic applicant will be assigned an identity consisting of a name, phone number, and address. The name for the applicants will be obtained from the National People Registry.<sup>19</sup> Specifically, we selected 4 of the 20 most common names in 1990, to avoid resume individualization. Table 4 shows the name and phone number assigned. Each profile will keep the same name for every application. In addition to phone number, each profile will have an email address that will be used in all applications. In this way, we ensure a tracking method for call-backs. Therefore, there will be four different phone numbers and email addresses, one for each

<sup>19</sup> <http://nombres.historias.datos.gob.ar/#seccion5>



profile, to avoid employers identifying repeated contact information. Resumes will not include photos to avoid biases based on personal appearance.

Table 4: Synthetic applicants’ contact information

Sex	Place of residence	Name	Phone number
Female	Barrio	Laura Pérez	1122385234
Male		Matías Ezequiel Fernández	1164854392
Female	Villa	Florencia López	1164854432
Male		Juan Ignacio González	1139520502

We defined each profile’s address based on reference areas for the city of Buenos Aires, as well as internal administrative divisions for the province of Buenos Aires. In the Buenos Aires Metropolitan Area case, we did not implement a distance between the jobsite and place of residence criteria since it is a relatively homogeneous district without major differences in access to transportation that could affect selection. Our focus is on the Autonomous City Buenos Aires as this is where most of the economic activity occurs and to maximize the call-back rates (which would not be influenced by distance from home to work).

### ***Modules 2 and 3: Professional experience and educational background***

For the same application, modules 2 and 3 must contain equivalent information such that employers are unable to discriminate since skill sets are observable. Additionally, the content in these modules will be tailored based on job requirements to maximize employability. For this purpose, we will use a bank of secondary and tertiary education institutions, as well as possible professional experience (by job area) and special aptitudes and skills, that can be used regardless of the application. Comparability of CVs was assessed in focus groups that included participants who work on labor-related community organizations, academics, and recruiters.<sup>20</sup>

### **Job Selection and Application Process**

The first step in applying for jobs will be the creation of a data bank of potential jobs. This bank gathers daily job posts from several Argentinian online job-search engines such as “hoytrabajo,” “zonajobs,” and “computrabajo” and national government job-search websites such as “portalempleo.gob.ar” and “trabajo.buenosaires.gob.ar,” as well as daily newspaper publications. Each post will be added to the bank if it complies with two criteria. Most popular jobs fall under one of the following areas: (a) administrative, accounting, and finances; (b) commercial, sales, and business; (c) manufacturing and production; (d) tourism and gastronomy; (e) offices; (f) customer support call centers and telemarketing; (g) construction; (h) secretaries and receptionists; and (i)

<sup>20</sup> The organizations are AMIA y CEDEL, (<https://www.buenosaires.gob.ar/jefaturadegabinete/secretaria-de-integracion-social-y-urbana/centro-de-desarrollo-emprendedor-y>), ACIJ (<https://acij.org.ar/>) y BIDlab (<https://bidlab.org/es>)

nursing. To increase the probability of receiving a call-back, that the gap between the date of the job posting and the date of the application must not exceed two days.

A job post will be selected to maximize the variance across qualifications and professional experience by distributing applications for “professionals,” “technicians,” and “unskilled jobs” in an equivalent manner. We will not use the same profile to apply to two different jobs in the same workplace.

Receiving and recording the employers’ responses constitutes as the last step in the application process. We will consider four possible response types: (1) no response of any sort, (2) a call or email rejecting the candidate, (3) a call or email requesting further information, and (4) a call or email for an interview appointment. In terms of positive or negative responses, we will consider #1 and #2 as negative and #3 and #4 as positive. For a response type to be considered a #1, at least 15 days must have elapsed without the employer responding to the application.

#### 4.4. Specification of empirical analysis

##### 4.4.1. Answering question number 1:

The described correspondence study will allow us to determine an estimate that measures the urban discrimination effect of living in a *villa* on the call-back rates from employers. Because, as explained, each *villas–barrio* duplet has been selected so that they are observationally equivalent on socioeconomic and distance-to-jobs characteristics, the difference in call-back rates will be computed net of those characteristics. The source of variability in the matching estimate will come from differences in the call-back rates across duplet with treatment being living in a *villa* instead of a *barrio*. The following difference in conditional expectations isolates the urban discrimination effect  $UD$ :

$$UD = E[Y|V = 1, X, D, U] - E[Y|V = 0, X, D, U]$$

where  $Y$  is the call-back rate for a specific subpopulation of applicants;  $V$  is an indicator variable turning into 1 if the neighborhood where applicants live is a *villa* and zero if it is a *barrio*;  $X$  is a vector of observable characteristics that would make each *villa–barrio* duplet observational identical;  $D$  is a distance parameter measuring geographical distance between the duplet and the employer (which is identifiable whenever the job application has an associated address or location); and the parameter  $U$  portrays unobservable characteristics that are assumed to be distributed equally in the two geographical units (the assumption of matching identification).

The  $UD$  parameter measures the expected value of call-back from job applications considering that the place of residence is a *villa* (instead of a *barrio*). Estimation of such parameter will take place in stages. In the design stage, we warrant that  $X$  and  $D$  are matched between *barrios* and *villas* by preselecting *barrios* near where *villas* are located (taking account of the fact that both are close to the employers) and warranting that those *barrios* have similar socioeconomic characteristics than *villas* (so that we hold constant the factors that across neighborhoods make individuals in *villas*

and *barrios* observationally different). In a second stage, the call-back rate differences was observed using the application submissions as the unit of analysis and controlling for the differences in characteristics of the job offers. In that second stage, estimation of the parameter will be made using parametric (OLS) and semi-parametric methods (such as propensity score matching and/or non-parametric regression).

#### 4.4.2. Answering question number 2:

The correspondence study also allows us to compute an estimate that measures the average effect of living in a *villa* subject to an SUP intervention on the call-back rates from employers. Let  $T$  be an indicator variable that turns into 1 whenever SUP interventions have occurred and 0 otherwise. Notice that because *barrios* have already been subject to interventions by components that characterize SUPs, we assume that  $T = 1$  for all of them. The difference in conditional expectations,  $E[Y|V = 1, T = 0, X, D, U] - E[Y|V = 0, T = 1, X, U]$ , measures the effect of the SUP treatment on the subset of untreated *villas*. Similarly, the difference  $E[Y|V = 1, T = 1, X, D, U] - E[Y|V = 0, T = 1, X, U]$  measures the effect of the SUP treatment on the subset of treated *villas*. The difference between the two differences measures the effect of SUPs on labor market discrimination ( $L$ ):

$$L = E[Y|V = 1, T = 0, X, D, U] - E[Y|V = 1, T = 1, X, D, U].$$

Given that we are also collecting information on untreated *villas* that are in the pipeline to receive SUPs, we are laying the basis for a prospective evaluation on urban discrimination that could control for baseline characteristics using a differences-in-differences approach.

## Section 5. Expected Results and policy implications

The results of this research have important policy implications. If our results show that slum dwellers have a significant lower rate of call backs than non-slum dwellers, this will present evidence on the existence of “urban discrimination”. If we find discrimination still persists after SUPs, this implies that these programs should be more effective in increasing social integration.

In that case, some specific actions focused in enhancing integration should be put in place. Some of these actions could include information campaigns to show the results of the SUPs in the formal city (so that people become aware of the improvements in the former slums in terms of infrastructure and security), or campaigns in the media to highlight profiles of the slum dwellers

as skilled professionals and trustful citizens. Other interventions carried out in Argentina include organized walks for residents in the formal city in slums as in the case of Buenos Aires<sup>21</sup>.

## Section 6. Methodological considerations and limitations of the study

This section discusses some methodological limitations of the study.

Matching assumptions do not hold.  $U$  are the unobservable characteristics that are supposed to be distributed equally along the two geographic units. To mitigate this limitation, we conducted focus groups with public officials, NGOs, specialists in MDBs, and neighboring residents to validate the choice of the matching pair of *villas* and *barrios*.

Conspicuity of the differences in the duplets is assumed. Although we validated it qualitatively, the notoriousness of the difference between a *villa* and a *barrio* might not be credibly interpreted by a prospective employer. In that case, the selection of the neighborhoods, *villa* or *barrio*, will be random, even though the employer would in fact discriminate against workers from *villas* (irrespective of whether discrimination is of taste or statistical origin). In order to mitigate this risk, we have taken the approach of identifying very clearly in the job applications the neighborhoods where applicants reside. In addition, we have validated in focus groups with experts that the address denominations of “manzana XX, calle ZZ” are the common way to identify that people reside in *villas*, while people who reside in the formal city have the address denomination of the type “calle XX, numero ZZ.” As described in Besbris et al. (2018), correspondence studies that seek to find differences in places of residence face the reality that the “challenge for measuring spatial stigma is the need to effectively capture the local schemas that people use to cognitively map their city.”

The macroeconomic risk of an economic crisis<sup>22</sup> might lower the supply of job offers and increase the number of competitors per application, which results in a lower response rate. In addressing these challenges, we have optimized the profile of the applicants to match the observable attributes described in job offers. Additionally, we have minimized the time span between the date of the job posting and the date of the application.

With regard to the timeframe of the study, correspondence studies face the challenge of claiming timewise comparability of the applications. If studies have an excessive timeframe, the unit of analysis (duplets in our case) might lose comparability because of the changes in conditions of supply and demand that relate to seasonality and economic changes. To deal with this issue, we

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<sup>21</sup> Some initiatives are Jane’s Walk Buenos Aires, or Running 31 organized by the Municipality of Buenos Aires.

<sup>22</sup> As of the moment of the design of the study, the country is undergoing a delicate economic situation with a high level of poverty, experiencing high levels of inflation, drop of the exchange rate among others.

will test the results of our model with and without fixed effects for weeks and/or months, so that units appear observationally equivalent timewise and effects are not driven by these changes.



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