The Most Vulnerable Poor:

Clientelism among Slum Dwellers

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Abstract

Are slum dwellers more involved in clientelistic arrangements than other (urban poor) voters? While poverty is a key predictor of clientelism, some urban poor voters are more involved in clientelistic arrangement than others. Insecure tenure, lack of access to public resources, and location in areas exposed to environmental shocks increase the vulnerability of slum dwellers. This vulnerability is used by politicians and brokers, who politicize access to scarce resources, and thus make slum dweller more exposed to clientelism. The qualitative literature has long highlighted how clientelism provides a strategy for slum dwellers to cope with their vulnerability, but this population is often excluded from quantitative analyses of clientelism. Using survey data from Argentina and a matching technique that allows us to compare slum dwellers with similar non-slum dwellers, we find that there is indeed a higher prevalence of clientelism among the former. We use a survey experiment on monitoring and sanctions to show that this different exposure to clientelism is consequential. We find different responses across similarly poor slum dwellers and non-slum dwellers regarding the potential consequences of defecting from clientelistic arrangements. Our findings suggest that including slum dwellers in quantitative analyses would improve our understanding of clientelism.

Clientelism—understood as the personal exchange of goods and favors for political support—has long been associated with poverty.[1] Yet, a lot of what we know today about clientelism is informed by quantitative studies that routinely exclude the poorest and most vulnerable populations: people living in slums.[2] The qualitative literature on clientelism, in contrast, has long highlighted the extreme dependence of slum dwellers on political brokerage (e.g., Auyero, 2001; Collier, 1976; Gay, 1994). Insecure tenure, scarce and often discretionary access to public services and resources, and location in areas exposed to environmental shocks increase the vulnerability of slum dwellers. This vulnerability is used by politicians and brokers, who politicize access to scarce resources (Auerbach, 2016; Auerbach & Thachil, 2018, 2019; Auyero, 2001; Holland, 2015; Zarazaga, 2014). Slum dwellers are therefore more exposed to clientelism than other urban poor.

This population, however, tends to be excluded from quantitative analyses of clientelism that rely on public opinion surveys. For logistical, budgetary, and security reasons, slum residents are typically excluded from nationally representative surveys conducted in most developing countries. Most representative surveys are conducted face-to-face using some form of multistage area sampling, which usually goes from geographic units, to households to individuals. This kind of sampling requires information about how to divide the target population both geographically and numerically; information that is typically found on census maps and

[1] According to the literature, the connection between poverty and clientelism is explained by the marginal value of handouts, the scarcity of labor market opportunities, shorter time horizons of poorer voters, or dependence on political discretion to access scarce resources; all of which increase the current value of immediate assistance over uncertain policy promises for the future (e.g., Auyero 2001; Calvo & Murillo, 2004; Holland 2017; Kitschelt & Wilkinson, 2007a; Mares & Young, 2016; Stokes et al., 2013; Weitz-Shapiro, 2014).

[2] A slum household is one "in which the inhabitants suffer *one or more* of the following *'household deprivations'*: lack of access to improved water source, lack of access to improved sanitation facilities, lack of sufficient living area, lack of housing durability, and lack of security of tenure" (UN-Habitat 2016, 2). See the next section for more details.

census data (Lupu & Michelitch, 2018). Slums, however, are rarely mapped or surveyed by government agencies (Auerbach et al., 2018, pp. 270–272), and even when they are, slum maps often do not cover the inner streets and alleys that are needed to properly select households for surveys.[3] These difficulties heightened when census data is outdated. At the same time, when conducting representative surveys, polling firms in the developing world often have to substitute some selected sampling units that are "too remote, too dangerous, or inaccessible" (Lupu & Michelitch, 2018, p. 207).[4] Compared to other poor neighborhoods, slums tend to exhibit higher crime rates and have more accessibility problems due to their frequent location in marginal land and lack of adequate public services, which increases their vulnerability to environmental shocks (e.g. floods).[5] Thus, even in the cases in which maps do exist, for logistical or security reasons, slums tend to be substituted for other similar points (when possible) within the same primary sampling unit.

This omission is particularly problematic for our understanding of clientelism. In spite of the conditions that make slum dwellers more exposed to clientelism, the quantitative scholarship on the topic is often based on national representative surveys that exclude these areas.[6] We argue

[3] The slum we study appears in the municipal map as a large empty spot.

[4] Half of the requests for substitution of sampling points received in the 2016/17 Latin American Public Opinion Project (LAPOP), the biggest ongoing survey in the Americas, from local polling firms implementing the survey were for security reasons. Other reasons included abandoned locations, commercial areas, and areas inaccessible due to flooding (Personal communication with Noam Lupu, LAPOP Associate Director, July 3, 2018). The survey manual for the Afrobarometer, the biggest ongoing survey in Africa, in turn, states that: "In some cases, a few EAs [Census Enumeration Areas] may be so inaccessible or so dangerous that substitution becomes necessary" (Afrobarometer, 2017, p. 34). Substitutions should not exceed 5% of EAs and should be done with other EAs with similar characteristics (except the reason that generated the substitution). Substitutions are reported on Afrobarometers country codebooks. In Argentina, we talked to three of the main national polling firms and they confirmed the difficulties in surveying slum dwellers, who are thereby excluded from samples.

[5] In fact, our own pilot for the slum survey had to be postponed because the slum was flooded.

[6] For instance, see Brusco et. al. (2004), Calvo and Murillo (2013, 2019), and Stokes (2005), for the Argentinean case. Other examples from Latin America include: Gonzalez-Ocantos et al. (2012; 2014), Holland and Palmer-Rubin (2015), and Schaffer & Baker (2015).

here that this mismatch between theories of clientelism that invoke the urban poor and empirics that often excludes key portions of this population, may have important consequences for what we think we know about clientelism. The goal of this paper is then twofold. First, we want to provide systematic quantitative evidence that slum dwellers are indeed more involved in clientelistic exchanges than other urban poor voters. Second, we want to show how this may be consequential for theories of clientelism based on public opinion surveys that excludes slum dwellers. To do this, we field our own slum survey simultaneously with a national representative survey around the 2015 presidential election in Argentina.[7] Relying on matching techniques, we compare the personal experiences and perceptions of voters of an Argentine slum with similarly poor non-slum voters.

By focusing on a population that is most likely to be targeted by clientelism yet often ignored in quantitative research, we expect to highlight the importance of understanding diversity among poor voters in their exposure to clientelism. Holding constant individual demographics usually associated with clientelism and using direct questions and a list experiment, we find that exposure is higher among slum dwellers. To assess the implications of this different exposure, we rely on a survey experiment about the potential costs of voter defection from a clientelistic agreement, by randomly varying whether the voter a) does not turnout to vote, or b) votes for another candidate. We find that slum dwellers are more likely to respond that voters' risk is higher if they do not turnout to vote than if they vote for a rival candidate. This difference in exposure and understanding of the clientelistic experience between similarly poor voters underscores the importance of including slum dwellers into quantitative analysis of clientelism.

[7] Although our slum sample is not a representative sample of Argentinean slum dwellers, it provides an initial view into the political reality of this understudied population. The slum we study, however, is fairly representative in terms of its characteristics. See Table A2 in the Online Appendix and the next section.

To our knowledge, our study is the first systematic attempt to compare political attitudes and behavior associated with clientelism among slum dwellers with other urban poor voters. Our findings call attention to the potential limitations of prior survey research, which informs most of the existing literature on clientelism while excluding the respondents most exposed to this phenomenon: slum dwellers. In so doing, we contribute both to an emerging literature distinguishing among poor voters' exposure to clientelism based on their vulnerability (e.g., Bobonis et al., 2017) as well as to an incipient scholarship seeking to devise new research strategies to study the politics of informal urban settings in the democracies of the Global South (Auerbach et al., 2018, pp. 270–272; Auerbach & Thachil, 2018, 2019; Holland, 2017; Post, 2018).

1. Poverty and Vulnerability among Slum Dwellers

We focus on slum dwellers because, as the most vulnerable urban poor, they are crucial for understanding clientelism in the most urbanized region in the world, Latin America. Not only are slum populations significant in Latin America, but slum dwellers are also the most exposed to hazardous conditions challenging their daily life, thereby increasing their vulnerability to politicized interactions. It is this vulnerability what makes them more dependent than other urban poor on clientelistic arrangements.

Although the proportion of slum dwellers in urban areas has diminished worldwide in the last decades, their absolute numbers have increased (UN-Habitat 2016). In 2012, 113 million people were living in slums in Latin America and the Caribbean, and projections based on growth rates show that by 2020 there will be over 160 million households living in slums (IDB 2016). According to estimates from international agencies, around 24 percent of the urban population in Latin America and the Caribbean inhabits informal settlements. Living and

environmental conditions in these areas are distressing. Residents endure inadequate water supplies and sanitation, overcrowded and dilapidated housing, hazardous locations, tenure insecurity, and are vulnerable to serious health risks. Furthermore, life in slums is notoriously marked by fear, socioeconomic stigmatization and discrimination, and exclusion from formal services and employment opportunities (UN-Habitat 2013).

In Argentina, according to a report by TECHO (2016), one in ten people live in informal settlements.[8] There are a total of 3,826 informal settlements, which are home to an estimated 787,808 Argentine families. Around 35% of these settlements (1,352 total) and the majority (50,5%) of households living in these settlements (397,705 families) are located in the province of Buenos Aires, which houses one third of the Argentine population with its 16 million inhabitants.[9] Within this province, most slums are concentrated in the suburban belt surrounding the City of Buenos Aires known as Greater Buenos Aires (*Gran Buenos Aires* - GBA).[10] The slum we study is located in this area.

The vulnerability of slum dwellers is not just a consequence of their low incomes, but also of the risks they face, which make their welfare uncertain and volatile (Ligon and Schechter 2003; Bobonis et al. 2017). Negative shocks are more likely to affect households in slums because residents have fewer safety nets and are more exposed to environmental risks due to low

[8] This report is based on mapping all cities with more than 10,000 inhabitants. Informal settlements are defined by the NGO TECHO (2016, p.12) as groups of at least 8 families in which more than half of the population does not have deeds certifying property rights over the land and no regular access to at least two of the most basic public services (running water, sewers, and/or electricity with an individual electric meter). Slums are a subtype of settlement characterized by high population density and irregular urban layout. These are the most common types of informal settlements in the area we study.

[9] A more recent official report identifies 4228 informal settlements in cities over 10,000 inhabitants (39% of them in Buenos Aires), and estimates that around 3,5 million people (around 9% of the population) live in informal settlements (ReNaBAP 2017).

[10] Greater Buenos Aires refers to the 24 municipalities that are closer to the City of Buenos Aires, excluding the City itself. See a map of this area (Figure A1) in the Online Appendix.

quality infrastructure and location in areas not appropriate for housing. Like other urban poor, slum dwellers are affected by food insecurity, are less educated than the rest of the population, are often unemployed or employed in the informal sector, and they often have more children than the non-poor. All these characteristics have been found to be correlated with vulnerability (Ligon & Schechter, 2003).

Slum dwellers, however, are more vulnerable than other urban poor due to tenure insecurity, paucity of basic public services and programs, and environmental shocks. First, slum dwellers face land tenure insecurity as they usually lack deeds certifying property rights over the land they occupy. According to TECHO (2006), in 79 percent of Argentinean informal settlements, the majority of households have no legal document regarding the property they occupy.[11] In addition to the threat of eviction (even when its likelihood, at least in Argentina, is small), the lack of legally recognized physical addresses reduces their access to jobs and public resources.[12] The slum dwellers we study occupy (or rent) the area they inhabit without clear, legally recognized tenure arrangements, a vulnerable situation that only affects three percent of households in the GBA area (2010 Census).

At the same time, because slums are often situated in hazardous locations and lack basic public services, such as sewerage and pavement, slums dwellers are more exposed to environmental and health risks. In Argentina, 70 percent of informal settlements lack paved streets and 74 percent do not have sewerage (TECHO, 2016). Moreover, garbage collection is only available in 62 percent of settlements and, even in these cases, is often insufficient. In 13

[11] Table A2 in the Appendix summaries the characteristics of Argentinean informal settlements in comparison with the slum we surveyed.

[12] According to the priest from the slum where we conducted the survey, all his young parishioners use non-slum relatives' addresses when applying for jobs.

percent of settlement there are open garbage dumps (TECHO, 2016). In the slum we study, garbage collection is infrequent, and it does not reach the entire slum forcing its inhabitants to also rely on an open garbage dump. Two thirds of informal settlements experience flooding whenever there is heavy rainfall (TECHO, 2016), and the slum we study is no exception—it is the third most mentioned issue when we asked dwellers about their main problems. Its location on low land, bordered by a small river, which regularly overflows as sewage drains from other neighborhoods, heightens its exposure to flooding, which happens every time it rains heavily. Precarious housing (only 5 percent of the respondents in our sample live in brick houses), unpaved streets and corridors (only 7.5 percent of our respondents live on paved streets), and lack of sewers increase exposure to flooding while aggravating slum dwellers' vulnerability.

Finally, slums are characterized by poor (or lack of) access to basic public services. In Argentina, 73 percent of informal settlements have pit latrines, including the one we study (compared to one quarter of households in the province of Buenos Aires). Eighty-eight percent of informal settlements, including the slum we study here, do not have access to gas from public networks forcing their inhabitants to rely on propane tanks, which is more expensive than gas from the public grid (TECHO, 2016). By contrast, only a third of households in the province of Buenos Aires are not connected to the gas networks (2010 Census). In only about four percent of informal settlements, the majority of households have access to running water from the public grid; while 30 percent have access to electricity from the public grid with an individual meter (TECHO, 2016). In our slum, around 40 percent of households have access to running water, while electricity comes from the public grid but with no individual meter.

In addition to the vulnerability created by tenure insecurity, poor access to public services and resources, and higher exposure to environmental shocks, slum residents are also subjected to

physical insecurity, stigmatization, and discrimination. In Argentina, insecurity has been associated with the lack of formal streets, street lights, and police patrols (Kessler & Bruno, 2018). An official report from the local Judiciary Department states that 75 percent of homicides in the area occurred in informal settlements (*Departamento Judicial de San Martín*, 2012, p. 12). Indeed, crime is mentioned as their main problem by the slum dwellers in our survey.

2. Vulnerability and Clientelism

The vulnerability of slum dwellers makes them more likely to rely on political brokers to access scarce resources and solve their daily problems. Our argument builds on recent work by Bobonis et al. (2017), which studies the interplay between droughts, vulnerability, and clientelism. Pointing to the same effects of vulnerability on a poor rural population, Bobonis, et al. (2017) show how voters in Brazil who are more exposed to droughts are more likely to engage in clientelism to improve their access to water. Similarly, Post (2018, p. 120) points out that research that examines how intermediaries exchange the votes of their followers for local infrastructure (e.g., Auerbach, 2016; Gay, 1994), "implicitly suggests that greater infrastructural needs in dense urban slums may provide more fodder for clientelism than in rural areas."

Vulnerable slum dwellers often resort to brokers (called *referentes* or *punteros* in Argentina) and politicians to solve their daily problems, access basic public services and resources, and cope with negative shocks. The politicization of access to scarce public services heightens this practice. Even in cases in which public resources and services are available, slum residents are unable to rely on continuous and non-discretional access (Auerbach, 2016; Auyero, 2001; Holland, 2015, 2016; Zarazaga, 2014). Insecure property rights have also been shown to extend the exposure of voters' to clientelism and their dependence on politicians' arbitrariness (Holland, 2017;

Larreguy et al., 2018). Brokers are essential for demanding development and distributing resources, from unemployment programs and public sector jobs to food, medicine, clothes, and constructions materials.[13] Beyond material resources, brokers also provide help accessing other benefits, for instance, getting an appointment at the local hospital, or filing a form to access some benefit provided by the government, and basic information. Indeed, brokers often provide the information necessary to access resources that are, in principle, available to everyone. For the Argentinian case, the work of Auyero (2001), Zarazaga (2014), and Szwarcberg (2015) provide a detailed description of the ways in which political brokers are crucial for solving the everyday problems of slum dwellers.

Beyond Argentina, this reliance of slum dwellers on political brokers to cope with their vulnerability has been documented in the case of Brazil (e.g., Gay, 1994), Mexico (e.g., Cornelius, 1975), Peru (e.g., Collier, 1976; Stokes, 1995), India (e.g., Auerbach, 2016; Auerbach & Thachil, 2018, 2019), South Africa (e.g., Dawson, 2014), and Ghana (e.g., Paller, 2014). Embedded in local communities, these brokers are intermediaries in the relationship between politicians and voters. Whereas their linkages to politicians allows them to access resources, their personal connections to voters and their knowledge about voters' needs are crucial for the electoral effectiveness of distribution and the success of their political mobilization (Calvo & Murillo, 2019; Zarazaga, 2014). Like the Indian slum brokers described by Auerbach & Thachil (2018), those with better access to state resources are the most successful in Argentina (Szwarcberg, 2015; Zarazaga, 2014). In exchange for access to scarce resources, brokers expect political support at the ballot box and attendance at rallies, which allows them to signal their capacity for political mobilization to

[13] When the slum we study is flooded, for instance, brokers become important as providers of basic welfare, such as clothes, mattresses, and medicines. In one occasion, a broker was seen rescuing people with his own boat. On a different flood, 100 slum residents relocated to the church and a broker managed to deliver two mattresses to each family (Personal communication with the slum priest).

politicians (Stokes et al., 2013; Szwarcberg, 2015). For all these reasons, we expect *slum dwellers to be more exposed to clientelism than similar non-slum urban poor.*

To test whether higher exposure to clientelism among slum dwellers is consequential for their understanding of how this exchange works, we conduct a survey experiment on the potential consequences of not fulfilling a clientelistic agreement and compare the survey experiment responses across slum dweller and similarly poor non-slum dwellers. Our goal here is to study whether excluding slum dwellers from quantitative studies of clientelism could affect the conclusions of existing theories. If the responses of slum dwellers are different from those of the non-slum dwellers, then excluding the former from quantitative studies of clientelism is an important and consequential omission. Our theories of clientelism based on "representative" samples that exclude slums may simply not apply to slum dwellers—those who are expected to be more exposed to clientelism. The experiment asked respondents about the consequences of reneging on a clientelistic agreement, while randomly varying the options of whether the potential client a) does not turnout to vote or b) votes for another candidate. Whereas turnout is easy to monitor by brokers, monitoring specific choices at the ballot box requires the ability to tamper with the secrecy of the ballot box.[14]

[14] There is an ongoing debate about brokers' capacity to monitor voters' behavior. The sequenced nature of clientelistic exchanges generate commitment problems (e.g., Oliveros, 2018; Robinson & Verdier, 2013). Clients may comply because they are afraid of punishment if they fail to deliver the requested political support (e.g., Brusco et al., 2004; Stokes, 2005; Stokes et al., 2013; Weitz-Shapiro, 2014). In this case, a broker's capacity to monitor voting behavior—or making clients believe so, even with secret ballot (Chandra, 2007; Kitschelt & Wilkinson, 2007a)—becomes fundamental. Alternatively, brokers may monitor visible political support, such as turnout (Nichter, 2008) or rally attendance (Stokes et al., 2013; Szwarcberg, 2015) to evaluate the "loyalty" of their clients, or they can monitor collective behavior at the level of the polling station (Cooperman, 2019; Gingerich & Medina, 2013; Rueda, 2017). Monitoring, however, is not necessary if clients support brokers due to a sense of obligation or reciprocity (Finan & Schechter, 2012; Lawson & Greene, 2014; Scott, 1972) or if voters perceive such support as part of their self-interest to maintain the flow of resources coming to the broker to whom they are connected (Auerbach & Tachil 2018; Diaz-Cayeros et al., 2016; Oliveros, 2016, 2018; Zarazaga, 2014, 2015). See Gonzalez-Ocantos & Oliveros (2019) for a discussion of this debate in the Latin American context.

3. Empirical Strategy

Our data come from two different surveys conducted around the 2015 presidential election in Argentina. One is a national representative survey: the 2015 Argentine Panel Election Study (APES) (Lupu et al., 2015).[15] The other one is a shorten version of the same questionnaire conducted in one slum located in the municipality of San Miguel (GBA). APES was conducted by MORI—one of the largest public opinion companies in Argentina— and it involved two national waves of face-to-face interviews with Argentine citizens and permanent residents18 years old and over. All interviews were conducted using PDAs. The first wave of interviews (June-August 2015) was based on a nationally representative sample of voters living in cities with 10,000 inhabitants or more, while the second wave (November-December 2015) consisted of a panel sample of respondents from the first wave that agreed to participate again, plus a refresh sample. The first wave includes 1,149 respondents, while the second wave includes 780 of the 1,149 respondents from the first wave plus 626 respondents drawn from a refresh sample.[16] The second wave therefore has a sample size of 1,406.[17] In this paper, we use data mainly from the first wave.[18]

The survey among slum dwellers (citizens or permanent residents aged 18 years old and over) consisted of a reduced version of the 2015 APES questionnaire and was administrated

[15] Information on APES 2015 can be found on http://www.noamlupu.com/data.html; more information on the slum survey can be found in the Appendix.

[16] To compensate for sample attrition, a refresh sample of 626 respondents was drawn, selected according to the same procedures use for the first wave.

[17] Table A3 in the Appendix shows the sample representativeness for the APES survey and the comparison with our slum sample for age, education, and gender.

[18] We use data from the first wave because some of the questions analyzed here were not included on the second wave.

between December 1, 2015 and January 3, 2016 by a team of local enumerators that we recruited, trained, and supervised. All of the enumerators were familiar with the slum—which was key for the success of the survey— since they were affiliated with a non-profit organization that has been providing social services in the community for twenty years. One of the main challenges in conducting surveys in slums is that official maps from which households can be selected do not exist. To get around this issue, we took advantage of maps that were drawn by the aforementioned NGO. These maps included all the streets and alleys in the slum and the number of houses on each block. Our enumerators were randomly assigned a starting point in the slum and were instructed to conduct interviews in every other household.[19]

We selected a slum located in San Miguel, one of the northwestern municipalities of Greater Buenos Aires where a third of the population lives in poverty.[20] Since the 1960s, GBA has attracted economic migration from northern provinces and neighboring countries, which resulted in the establishment of hundreds of informal settlements and a high population density of around 2700 people per Km^2.[21] The slum we selected started to populate in the late 1960s, with migrants mainly from the Argentine provinces of Chaco and Santiago del Estero, as well as from Chile and Paraguay. Residents established precarious homes in a private vacant rural property with disputed property rights. Today, the slum occupies 100 acres and in 2010—the last official record— had a population of 10,000 inhabitants. An NGO that works in the area calculates roughly 15,000 inhabitants. As described in the prior section, the slum lacks basic

[19] This is an imperfect methodology since houses may contain more than one household – particularly in slums – but it is a reasonable, feasible solution to the challenge of drawing a representative sample from these communities.

[20] See Table A1 in the Appendix for more information. 35.7% of the population of GBA was poor in 2016, compared to 32.5% for the entire country (Source: http://observatorioconurbano.ungs.edu.ar/ with data from *EPH* - INDEC, 2016).

[21] For comparison, the population density of the province of Buenos Aires and Argentina is 50.8 people per Km^2 and 10.7 people per Km^2, respectively. Note also that GBA is 99.8% urban.

public services and infrastructure and it is located on an area prone to flooding.

The wording for all the questions was the same in both surveys and was kept as simple as possible, considering the challenges of conducting a survey among a population with low levels of education.[22] Because clientelism could be associated with negative social stigma, both patrons and clients have incentives to misreport clientelistic exchanges, thereby increasing the probability of under-estimation when relying on direct questions.[23] Following Gonzalez-Ocantos et al. (2012), to address problems of social desirability bias and obtain reliable estimates of clientelistic practices, we use a list experiment. This technique is useful for generating unbiased estimates when dealing with sensitive topics and it is simple to implement. First, the survey sample is randomly split into two halves, the treatment and the control group. Respondents in each group are read the same question and shown a list with different number of response options. List experiments work by aggregating the item we care about (the "sensitive" item) with a list of other items. The only difference between lists for the two groups is the number of response categories—the list for the treatment includes the sensitive item whereas the control list does not. The sensitive item in this case was: "Received any material benefit—like clothes or food—or personal favor from a political broker" The question does not ask respondents to tell the enumerator the specific activities they witnessed, but only to indicate *how many* of those activities were witnessed, so the question provides the respondents with full anonymity.[24]

[22] Question wording is in the Appendix.

[23] See Kitschelt & Wilkinson (2007b, pp. 323–327) for a discussion.

[24] To protect the privacy of the responses, it is crucial to avoid lists that would result in respondents choosing none or all the items, generating "floor" or "ceiling" effects, respectively. To minimize ceiling effects, we included one-low prevalence activity (being a candidate); to minimize floor effects, we included two high-prevalence activities (saw campaign posters and saw campaign adds on TV and radio). The strategy seemed successful since very few of the respondents who received the control list reported either zero or four of the control items. To test the validity of the experiment, we used the method developed by Blair and Imai (2012) and we failed to reject the null hypothesis in the test (*ict.test*, rejection criteria of $\alpha = 0.05$) for design effects. Table A8 in the Appendix reports the distribution

The survey also includes two direct questions about clientelism asking respondents if they have received any material benefit or personal favor from a candidate or a political broker (*self-reported clientelism*) and whether the respondent's neighbors have received any benefit or favor (*witnessed clientelism*), respectively. Note that all three measures refer to experiences with the supply side of clientelism, but it does not inquire about voters' response to these clientelistic offers. We use these three alternative measures to assess differences in exposure to clientelism.

Similarly, given the sensitivity of asking directly about the potential consequences of breaking a clientelistic agreement, we randomly assigned respondents to hear vignettes presenting different outcomes of a hypothetical exchange of political support for a government sponsored temporary job. Random assignment to the different vignettes creates groups that, on expectation, are equivalent on observable and unobservable characteristics.[25] We compare the average responses across groups in order to isolate the causal effect of the treatment (in our case, a hypothetical voter's electoral behavior). The vignette read as follows (with the phrases in brackets randomized across respondents):

> Now imagine that another political broker named [RV1a: Pedro/RV1b: Susana] [RV2a: delivers/RV2b: promises] a government sponsored temporary job (*plan de empleo*) to a resident of the neighborhood and asks him/her to vote for the broker's candidate in the next election. The resident accepts the job but on election day he/she decides [**RV3a: not to vote/RV3b: to vote for another candidate].**

of responses across groups for the list experiment estimates; the experiment wording is reported on pages 11-12 of the Appendix. For advice on designing list experiments, see Glynn (2013).

[25] Tables A4-A7 in the Appendix show the distribution across the different treatment conditions and balance on pre-treatment characteristics for both surveys.

Information about the gender of the broker (RV1), whether the broker promises or delivers the government sponsored job (RV2), and the behavior of the voter on election day (RV3) were all randomized across respondents.[26] Here we focus only on the third treatment, the electoral behavior of the neighbor. Immediately following the vignette, respondents were asked, "How likely are you to believe that the voter would face any problems **[RV3a: for not turning out to vote/RV3b: for not voting for [Pedro/Susana]'s candidate]**? Very likely, somewhat likely, somewhat unlikely, not at all likely?" We collapse this outcome variable to a dichotomous variable that takes the value of one for the responses "very likely" or "somewhat likely" and zero for "somewhat unlikely" or "not at all likely."

To compare responses of slum dwellers with other similar urban poor voters who do not live in the slum, we combine two strategies. First, we reduce heterogeneity and improve comparability across groups by using only APES 2015 data from GBA, the area where the slum we study is situated. Second, we use matching to produce groups that are as similar as possible in observed covariates. Matching seeks to improve or create balance understood as "the degree to which the treatment and control covariate distributions resemble each other" (Ho et al., 2007, p. 215). Like experiments, matching can produce groups that are comparable on observables; unlike experiments, matching cannot guarantee comparable groups on unobservable characteristics.

We use a matching technique called cardinality matching, which maximizes the size of the matched sampled that is balanced according to the requirements for covariate balance set by the researcher before matching (Visconti & Zubizarreta, 2018; Zubizarreta & Keele, 2017;

[26] There were then 2x2x2=8 randomly assigned vignettes. For the national survey, randomization was programmed into the PDAs that the enumerators used to administrate the survey. For the slum survey, enumerators received printed questionnaires with the different vignettes.

Zubizarreta et al., 2014).[27] Using this method, respondents in the slum are matched to respondents who do not live in the slum but are as similar as possible to the first (particularly in terms of poverty and other predictors normally associated with clientelism). After creating a matched subset of the data in this way, the rest of our analysis is based on calculating the differences across the matched groups.[28]

In order to produce groups that have similar distribution of observed covariates, we included 15 covariates in the matching procedure.[29] Included covariates were selected based on their relation to the outcome (clientelism), the treatment assignment (living in the slum), or both.[30] These include personal characteristics (age, gender, education, marital status, employment, and number of people and children in the household) and household assets (freezer, cellular phone, washing machine, computer, and flat screen TV) to proxy for income. Given the connection between the Peronist Party and clientelistic practices (e.g., Brusco et al., 2004; Calvo & Murillo, 2004, 2013), our matching procedure includes a covariate for Peronism. We measure this covariate using the respondent's report about her father's partisanship when she was younger. Finally, we include two covariates to account for state dependency: whether respondents or anyone else in their household receives benefits from the federal conditional cash transfer program (*Asignación Universal por Hijo- AUH*) or were beneficiaries of the non-contributory pension program for those not covered by social security (*moratoria previsional*) (Lustig & Pessino, 2014). While we expect beneficiaries of government programs to be more

[27] For an overview of this method, see Zubizarreta et al. (2014) and Zubizarreta & Keele (2017); for a discussion of its advantages see Visconti & Zubizarreta (2018); for an application to economic voting, see Visconti (2017).

[28] We use the designmatch package available in CRAN (Zubizarreta & Kilcioglu, 2016). To conduct the optimization, we use the Gurobi 9.0.0 solver.

[29] The details for each covariate are included in Table A9 in the Appendix. Covariates for the treatment assignment of the experiments were also included to make sure groups remained balanced.

[30] For advice on determining which covariates to include in the matching procedure, see Stuart (2010).

susceptible to clientelism given their dependence on state largesse, *AUH* is a relatively well-targeted program (De La O, 2015; Garay, 2016), which also serves as an alternative (and good) indicator of poverty.

The first step is to obtain a matched sample. After restringing wave 1 of the APES national representative sample (N=1,149) to only those respondents who live in GBA, we are left with 328 subjects in the non-slum group (our control) and 385 subjects in the slum group (our treatment). After the matching procedure, we are left with a sample of 234 respondents in each group.[31] Cardinality matching maximizes the size of the sample that achieves the mean balance constraints imposed beforehand. In our case, we imposed a tolerance for imbalances that does not allow differences >1/10 of a standard deviation.

Matching can only be considered successful in holding constant the influence of covariates if it creates balance in the distribution of covariates across groups. To verify that our matching procedure was indeed successful in creating balance in covariate values between slum respondents and non-slum respondents, we calculated standardized differences in covariate means before and after matching for all the covariates included in the matching procedure. Table 1 presents the mean balance, the absolute standardized differences in means, and the P-values before and after matching for the covariates included in the matching procedure.[32]

[31] Regarding missing values for covariates, we impute the median and generate binary variables for missingness. These variables indicating missing values are also included in the mean balance optimization.

[32] To preserve the balance across groups in the survey experiments, the matching procedure also includes the variables for treatment assignment.

Table 1: Mean Balance, before and after Matching

Covariate	BEFORE MATCHING				AFTER MATCHING			
	Slum residents	Non-Slum residents	Stand. diff. in means	P-value	Slum residents	Non-Slum residents	Stand. diff. in means	P-value
Age(18-84)	38.24	42.26	0.25	0.00	38.45	40	0.1	0.26
Female	0.63	0.53	0.19	0.01	0.62	0.58	0.08	0.30
Education (0-5)	2.04	2.13	0.07	0.35	2.09	2.18	0.07	0.46
Married	0.32	0.33	0.03	0.72	0.32	0.33	0.01	0.92
#of children (0-8)	1.66	1.19	0.35	0.00	1.41	1.39	0.01	0.89
#of people (1-13)	4.57	3.77	0.42	0.00	4.18	4.01	0.09	0.34
Employed	0.53	0.50	0.05	0.47	0.56	0.51	0.09	0.36
Freezer	0.89	0.89	0	0.99	0.88	0.86	0.08	0.41
Cellular phone	0.95	0.87	0.29	0.00	0.93	0.91	0.09	0.31
Washing machine	0.89	0.88	0.05	0.51	0.87	0.86	0.03	0.79
Computer	0.42	0.51	0.19	0.01	0.46	0.5	0.09	0.31
Flat screen TV	0.67	0.53	0.29	0.00	0.65	0.61	0.08	0.39
Peronist father	0.89	0.94	0.17	0.02	0.89	0.92	0.09	0.34
AUH	0.51	0.21	0.65	0.00	0.34	0.3	0.09	0.32
Moratoria	0.21	0.04	0.53	0.00	0.08	0.05	0.09	0.19
Observations	385	328			234	234		

As expected, slum dwellers are different in important ways from those who do not live in the slum so there were several serious imbalances in the full sample. In particular, slum residents tend to have more children, share their households with more people, and are more likely to receive benefits through the conditional cash transfer program (*AUH*) and the non-contributory pension (*moratoria*). We do not find important differences in terms of material possessions, which could reflect the crudeness of our measures but also likely reflects the fact that 36 percent of the population of GBA lives in poverty.[33] Note, however, that the conditional cash program (*AUH*) is considered to be a well-targeted program (De la O 2015, Garay 2016) and we find significantly more beneficiaries of this program among slum residents. Table 1 also shows that after the matching procedure, none of the imbalances are >1/10 of a standard deviation (our imposed tolerance for imbalance).

4. Results

We begin our analysis by showing that there is indeed significant variation in exposure to clientelism across the respondents to the nationally representative survey, respondents who live in GBA, and respondents who live in the slum. Table 2 presents our estimates of clientelism across these three samples using the three different measures.

[33] Surprisingly, slum residents do not appear to be more Peronists than non-slum residents. Since there is some literature that relates the Peronist party to more clientelism (e.g., Brusco et al., 2004; Calvo & Murillo, 2004, 2013), we were expecting to find more Peronists among slum residents. There is, however, at least one recent paper that does not find this relationship. Using data from APES 2015, Oliveros (2019) finds that Peronist voters were not more likely to be targeted by clientelistic offers than non-Peronist voters.

Table 2: Self-reported and witnessed clientelism, across samples

	SLUM DWELLERS	APES NATIONAL SURVEY			
		Wave 1		Wave 2	
		All respondents	GBA	All respondents	GBA
Self-reported clientelism	14%	3.4%	3.5%	1.8%	2.4%
Witnessed clientelism	54%	25%	24%	17%	23%
List experiment estimates	43%***	11%*	21%**	15%***	27%***

Note: DN/NA for self-reported clientelism and the list experiment are coded as missing; DN/NA for witnessed clientelism are coded as zero. Direct questions from APES were calculated using post-stratification weights (included in the APES dataset) to adjust for unit nonresponse and attrition based on gender, age, and education. ***p<0.01, **p<0.05, *p<0.1

Across the three different measures and both waves of APES, slum respondents reported higher levels of clientelism than non-slum respondents. Thus, a first look at the data is indeed consistent with our argument that slum dwellers, more vulnerable than others, are more exposed to clientelism, even when compared to those who live in the same area (GBA). Of course, individuals who live in slums may be fundamentally different from those who do not live in slums in terms of personal characteristics, experiences, and environment in ways that could be associated with clientelism. In particular, we know that poverty is a strong predictor of clientelism. We therefore based the rest of our analysis on the matched data that holds constant the potentially confounding influence of the respondents' characteristics.

Table 3 presents the results of a series of OLS regressions using our matched sample (234 slum dwellers plus 234 non-slum dwellers) and the two direct measures of clientelism. Columns

3 and 6 show the results for the main specification including the covariates that were used in the matching procedure. Columns 1 and 4 do not include any controls, while columns 2 and 5 include only the socioeconomic controls.[34]

[34] Full table can be found in the Appendix (Table A10).

Table 3: Living in a slum on reporting clientelism, OLS regression results

	Self-reported clientelism			Witnessed clientelism		
	(1)	(2)	(3)	(4)	(5)	(6)
Living in the slum	0.06**	0.06**	0.06**	0.21***	0.21***	0.21***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
Socioeconomic controls	No	Yes	Yes	No	Yes	Yes
All controls	No	No	Yes	No	No	Yes
Observations	468	468	468	468	468	468

Note: Standard errors in parentheses; ***p<0.01, **p<0.05, *p<0.1

The results across the different specifications clearly show that living in a slum is positively and significantly correlated with the probability of experiencing and witnessing clientelism. The magnitude is substantial. Living in the slum increases the probability of reporting personal clientelism by 6 percentage points (from around 6 to 11 percent). In the case of witnessed clientelism, the estimated effect is an increase in the probability of a positive response of 21 percentage points (from around 30 to 52 percent).[35]

Two important caveats, however, are worth noting. It is possible that part of the estimated effect of living in the slum on *Witnessed clientelism* could be explained by the higher population density in the slum. Due to overcrowding, our slum respondents live in closer contact with more people than our non-slum respondents, which may affect their likelihood of witnessing their neighbors' actions. Although we lack a measure of population density in the slum, its high levels

[35] The percentage of non-slum residents in our matched sample reporting *Self-reported clientelism* is 5.56%, compared to 11.11% reported by the slum residents in the matched sample. The percentage of non-slum residents in the matched sample reporting *Witnessed clientelism* is 30.34%; compared to 51.71% reported by the slum residents.

in the GBA, with 2700 people per km^2, reduces our concern about the impact of this variable on our results.[36] Second, the observed differences may also be related to measurement issues. Indeed, questions about clientelism (particularly in the case of *self-reported clientelism*) are sensitive, so respondents may not be willing to provide honest answers when asked directly (Gonzalez-Ocantos et al., 2012; Gonzalez-Ocantos et al., 2014). If slum respondents are less subject to social desirability bias about their personal connections with clientelism than non-slum respondents, we would also find higher rates of self-reported clientelism in the slum. To address this issue, Table 4 presents the list experiment estimates for the matched sample.

[36] See Table A1 in the Appendix.

Table 4: Clientelism among slum and non- slum residents, list experiment estimates

	Slum residents	Non Slum residents
Treatment	2.93	2.14
	(0.08)	(0.09)
	N=122	N=117
Control	2.63	2.04
	(0.06)	(0.08)
	N=112	N=117
Estimated Proportion	0.30***	0.10
	(0.10)	(0.12)
	N=234	N=234

Note: Two-sample *t*-tests with unequal variance; ***p<0.01, **p<0.05, *p<0.1.

Among slum dwellers, the estimated percentage of respondents receiving a favor or a gift in the last year is a significant 30 percent; while the estimated percentage among the non-slum dwellers is a non- significant 10 percent. This, of course, does not mean that there is no clientelism among the non-slum respondents. Most likely this is caused by the fact that we do not have enough statistical power, considering the higher demands of list experiments in this regard. Note, however, that we do find a significant estimate for the slum respondents with the same sample size (N=234) which indicates that, in line with the responses from the direct questions, clientelism is simply less prevalent among the non-slum respondents. Moreover, there is no reason to expect social desirability bias on the question about witnessing clientelism, which also shows a significant higher proportion among slum respondents. In sum, living in a slum is

significantly and positively correlated with the probability of reporting clientelism across all three measures.

In line with our expectations, clientelism is higher among slum respondents. We now move to show that this difference may have important implications for our theories of clientelism. Table 5 presents the results of our survey experiment design to show the potential consequences of studying clientelism with a biased sample of the population. The survey experiment explores respondents' perceptions of the potential costs for a client who decides to defect from a clientelistic agreement. Specifically, Table 5 displays the proportion of respondents in each of the two treatment categories who say that it was very likely or somewhat likely that a voter who had entered a clientelistic agreement would face a problem if he or she did not turn out to vote or if he or she voted for a candidate different than the one supported by the broker, across slum and non-slum respondents.

Table 5: Respondents' beliefs about the potential consequences of not voting vs. voting for another candidate, across slum and non-slum respondents

		Unmatched Sample		Matched Sample	
	APES all respondents (Wave 1)	**All slum respondents**	**APES GBA**	**Slum respondents**	**Non-Slum respondents**
Not voting	0.55	0.60	0.45	0.58	0.5
	(0.02)	(0.04)	(0.04)	(0.05)	(0.05)
	N=594	N=193	N=172	N=115	N=124
Voting for another candidate	0.45	0.34	0.42	0.37	0.44
	(0.02)	(0.03)	(0.04)	(0.04)	(0.05)
	N=459	N=192	N=156	N=119	N=110
Difference	0.1***	0.26***	0.03	0.21***	0.06
	(0.03)	(0.05)	(0.05)	(0.06)	(0.07)
	N=1053	N=385	N=328	N=234	N=234

Note: Rows 1 and 2 report the proportion of respondents in each group who said that it was very likely or somewhat likely that the voter would get in trouble for his/her electoral behavior. Two-sample *t*-tests with unequal variance; ***p<0.01, **p<0.05, *p<0.1

Columns 1 to 3 present the results from the unmatched sample and show that slum respondents are significantly more likely to consider not voting more risky than voting for another candidate.[37] Slum residents and non-slum residents, however, may be fundamentally different in ways that could be associated with their response to this question. We then based the

[37] The 23-percentage points difference between slum respondents and GBA APES respondents (columns 2 and 3) is significant at the 99% level.

analysis below on the matched sample (columns 4 and 5) that holds constant the potentially confounding influence of respondents' personal characteristics.

The share of slum respondents (column 4) in the matched sample who believe that the voter would get in trouble if she decides not to turnout to vote is 58 percent (top row), while those who believe that the voter would get in trouble if she votes for a different candidate than the one requested is 37 percent (middle row), a statistically significant difference of 21 percentage points. This difference in perceptions of the level of risk associated to voting for a different candidate or not voting at all suggests that most respondents are not expecting political brokers to find out about their individual electoral choices, which are less observable than turning out. Even when voting for another candidate is more damaging for the broker, respondents believe that the consequences would be worse if the client fails to turnout. In fact, for the majority of slum respondents (63 percent), voting for another candidate is very unlikely or somehow unlikely to cause any trouble to the client.

Among the GBA respondents (non-slum dwellers in the matched sample, column 5), the treatment effect is in the same direction but smaller and not significant. Indeed, the percentage of respondents who believe that the client will face serious consequences by failing to turnout is 50 percent, while the percentage believing she would get punished for voting for a different candidate is 44 percent. This non-statistically significant difference of 6 percentage points is 15 percentage points lower than among slum respondents.[38] Thus, non-slum respondents with lower exposure to clientelism in the national sample do not differentiate between failing to turnout or voting for another candidate in terms of costs for the client. By contrast, those with more

[38] The 15 points difference is, however, not significant— probably due to the low number of observations.

personal experience with clientelism (slum respondents) believe that the negative consequences for defecting clients are significantly higher if they do not turnout than if they support another candidate. In line with our expectations, slum respondents and non-slum respondents have different perceptions of how clientelism works, which may be a cause of bias in theories of clientelism that are based on "representative" samples that excludes slums.

5. Conclusions

Insecure tenure, lack of access to public services and resources, and exposure to environmental shocks increase the vulnerability of slum dwellers. Politicians and brokers exploit this vulnerability by politicizing access to scarce resources which makes slum dwellers more exposed to clientelism. Whereas the literature on clientelism tends to portray "the poor" as an undifferentiated category, our results reveal that *some* poor voters are more susceptible to clientelistic arrangement than others. By comparing two similar populations—thanks to our slum survey and matching technique—we are able to provide systematic quantitative evidence that Argentine slum dwellers are more exposed to clientelism than other urban poor voters who do not inhabit slums. This finding suggests that prior empirical analyses and related theories of clientelism may obscure important forms of heterogeneity among poor voters.

Using a matching technique that allows us to compare respondents who live in a slum with similar non-slum respondents, along with a list and a survey experiment, we show not only the higher prevalence of clientelism among slum dwellers, but also their different understanding of clientelistic exchanges. Our results confirm the conventional wisdom in the qualitative literature regarding the higher prevalence of clientelism among slum dwellers while also

providing suggestive evidence regarding debates about monitoring and sanctioning in clientelistic exchanges. Slum respondents assign a higher probability of punishment to hypothetical clients who do not turn out to vote than to those who vote for a different candidate. We do not find the same difference among non-slum respondents.

The evidence presented here is, in our view, a strong indicator of the importance of including slum dwellers in quantitative analysis of clientelism based on surveys. Most of our survey-based knowledge of this political phenomenon excludes slum dwellers, who are not only a significant part of the population in developing countries but also one that is disproportionally exposed to clientelism. The results of our survey experiment suggest that our understanding of clientelism may change if slum dweller populations were included in the surveys we use. Recent scholarship in India is at the forefront of this research agenda (e.g., Auerbach 2016; Auerbach & Thachil 2018, 2019; Spater & Wibbels 2018). We believe that future studies should continue exploring electoral politics in informal settlements.

To the best of our knowledge, we provide the first systematic comparison of the extent and views of clientelism among slum dwellers and similarly poor non-slum urban dwellers. We show the importance of analyzing clientelism among this extremely vulnerable population despite the difficulties involved in data collection. We make a call for quantitative studies of clientelism to include slums and encourage others to follow this research path for advancing our understanding of both the comparative political behavior of slum dwellers and clientelism more generally.

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