Studies in Comparative International Development
https://doi.org/10.1007/s12116-021-09324-x

The Most Vulnerable Poor: Clientelism Among Slum Dwellers

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Accepted: 12 March 2021

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 arrangements 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.

Keywords Clientelism · Vulnerability · Poverty · Slums · Argentina

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Published online: 07 April 2021

Clientelism—understood as the personal exchange of goods and favors for political support—has long been associated with poverty.[1] Yet, much 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 risks increase the vulnerability of slum dwellers. This vulnerability is used by politicians and brokers, who politicize access to scarce resources (Auerbach 2016; Auerbach and Thachil 2018, 2020; Auyero 2001; Holland 2017; Nichter 2018; Zarazaga 2014). Slum dwellers are therefore more exposed to clientelism than other similarly poor urban dwellers.

This population, however, tends to be excluded from quantitative analyses of clientelism relying 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 typically found on census maps and in census data (Lupu and 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] Outdated census data increases these difficulties. At the same time, when conducting representative surveys, polling firms in the developing world often substitute some selected sampling units that are "too remote, too dangerous, or inaccessible" (Lupu and 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 location in marginal land and lack of adequate public

[1] The association between poverty and clientelism has been 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 and Murillo 2004; Holland 2017; Kitschelt and Wilkinson 2007a; Mares and Young 2016; Stokes et al. 2013; Weitz-Shapiro 2014).

[2] A slum is a "contiguous settlement that lacks one or more of the following five conditions: access to clean water, access to improved sanitation, sufficient living area that is not overcrowded, durable housing, and secure tenure" (UN-Habitat 2016, 57). See next section for more details.

[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). 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.

services, which increases their vulnerability to environmental risks (e.g., floods).[5] Thus, even in the cases in which maps do exist, 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. Despite the conditions that make slum dwellers more exposed to clientelism, the quantitative scholarship on the topic is often based on national representative surveys which generally exclude these areas.[6] We argue 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.[7] 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.[8] Applying matching techniques, we compare the personal experiences and perceptions of voters in an Argentine slum with the experiences and perceptions of similarly poor non-slum voters.

Holding constant individual demographics usually associated with clientelism and using direct questions and a list experiment, we find that exposure to clientelism is higher among slum dwellers. To assess the implications of such 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. Slum dwellers were more likely to respond that not turning out to vote was riskier than voting for a rival candidate. This difference in exposure and understanding of clientelism between similarly poor urban voters underscores the need to include slum dwellers into our quantitative analyses.

To our knowledge, our study is the first systematic attempt to compare political attitudes and behavior associated with clientelism among slum dwellers with other poor urban 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

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

[6] See, for instance, Brusco et al. (2004), Calvo and Murillo (2013, 2019), and Stokes (2005) in Argentina; González-Ocantos et al. (2012; 2014), Holland and Palmer-Rubin (2015), and Schaffer and Baker (2015) in Latin America; and Jensen and Justesen (2014) and Kramon (2019) in Africa.

[7] On the 53(3) SCID special issue, Auerbach et al. (2018) discuss the multiple problems associated with the study of informal settlements around the developing world, suggesting that the difficulties we highlight here for the study of clientelism may apply to many other issues in other developing countries as well.

[8] 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.

poor voters' exposure to clientelism based on their vulnerability (e.g., Bobonis et al. 2017; Nichter 2018) 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 and Thachil 2018, 2020; Holland 2017; Post 2018).

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 one of the most urbanized regions 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 recent 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 estimated over 160 million households living in slums globally by 2020 (IDB 2016). According to estimates from international agencies, around 24% 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.[9] There are a total of 3,826 informal settlements in Argentina, home to an estimated 787,808 families. Around 35% of these settlements (1352 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.[10] Within this province, most slums, including our study cite, are concentrated in

[9] 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.

[10] 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).

the suburban belt surrounding the City of Buenos Aires known as Greater Buenos Aires (GBA).[11]

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 (Bobonis et al. 2017; Ligon and Schechter 2003; Nichter 2018). Like other urban poor, slum dwellers are affected by food insecurity, have lower levels of education, are more likely to be unemployed or employed in the informal sector, and to have more children than the non-poor—characteristics which all correlate with vulnerability (Ligon and 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 risks. First, slum dwellers face land tenure insecurity as they usually lack deeds certifying property rights over the land they occupy. According to TECHO (2016), in 79% of Argentinean informal settlements, the majority of households have no legal document regarding the property they occupy.[12] In addition to the threat of eviction, the lack of legally recognized physical addresses reduces slum dwellers' access to jobs and public resources.[13] Only 3% of households in the GBA area inhabit in areas without legally recognized tenure arrangements (Census 2010), including the slum dwellers we study.

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% of informal settlements lack paved streets, while 74% lack sewerage (TECHO 2016). Moreover, garbage collection is only available in 62% of settlements and, even in these cases, is often insufficient. In 13% of settlement, there are open garbage dumps (TECHO 2016). In the slum we study, garbage collection is infrequent and does not reach the entire slum, forcing 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% of the respondents in our sample live in brick houses), unpaved streets and corridors (only 7.5% 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% 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, but only one third of households in

[11] GBA 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.

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

[13] 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.

the province of Buenos Aires (Census 2010) including the slum we study, do not have access to gas from public networks forcing their inhabitants to rely on propane tanks, which are more expensive than gas from the public grid (TECHO 2016). Whereas the majority of Argentine households have access to running water and electricity (with individual meters) from the public grid, only 40% of households in the slum we study have access to running water, and while they are connected to the electricity network, individual users are not paying for that energy.

In addition to the vulnerability created by tenure insecurity, poor access to public services and resources, and higher exposure to environmental risks, slum residents are also subjected to physical insecurity, stigmatization, and discrimination. In Argentina, insecurity has been associated with the lack of formal streets, streetlights, and police patrols (Kessler and Bruno 2018). An official report from the local Judiciary Department states that 75% of homicides in the area occurred in informal settlements (Departamento Judicial de San Martín 2012, p. 12). In our survey, crime is mentioned as the main problem in the slum. All these conditions heighten the vulnerability of a slum dweller, even compared with similarly poor voters outside the slum.

Vulnerability and Clientelism

Vulnerability makes slum dwellers more likely to rely on political brokers to access scarce resources and solve their daily problems. Our argument builds on recent work by Nichter (2018) who shows how Brazilian voters who are more exposed to droughts, unemployment, and health risks are more likely to engage in clientelism to improve their access to water, employment, and health care, respectively.[14] Similarly, Post (2018, p. 120) points out that research examining 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 2017; Zarazaga 2014). Insecure property rights have also been shown to extend voters' exposure to clientelism and dependence on politicians' discretion (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.[15] Beyond material resources, brokers also provide help accessing other benefits, for instance, getting

[14] For the effect of droughts on vulnerability and clientelism, see also Bobonis et al. (2017).

[15] When the slum we study floods, for instance, brokers provide clothes, mattresses, and medicines to residents. A broker was reported to have rescued people with his own boat and another one to have delivered mattresses for 100 slum residents relocated to the church during a flood (personal communication with the slum priest).

an appointment at the local hospital, or filing a form to access some benefit provided by the government. Moreover, 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) describe in detail how 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 and Thachil 2018, 2020), South Africa (e.g., Dawson 2014), and Ghana (e.g., Paller 2014). Embedded in local communities, brokers are intermediaries 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 the distribution of those resources, and the success of the political mobilization (Calvo and Murillo 2019; Zarazaga 2014). Like the Indian slum brokers described by Auerbach and Thachil (2018), brokers with better access to state resources are the most successful in delivering political mobilization for Argentine politicians (Szwarcberg 2015; Zarazaga 2014; Stokes et al. 2013). 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 breaking a clientelistic agreement, comparing 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 easily observable by brokers (e.g., Nichter 2008), monitoring specific choices at the ballot box requires the ability to tamper with ballot secrecy, which is significantly harder, if possible at all.[16] Of the 120 Argentinean

[16] The debate about brokers' capacity (and need) to monitor voters' behavior is not settled. Clients may comply with the clientelistic agreement 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, brokers' capacity to monitor voting behavior—or making clients believe so, even with secret ballot (Chandra 2007; Kitschelt and 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 polling station (Cooperman 2019; Gingerich and Medina 2013; Rueda 2017). Monitoring, however, is not necessary if clients support brokers due to feelings of reciprocity (Finan and Schechter 2012; Lawson and Greene 2014; Scott 1972) or if voters perceive such support as part of their self-interest to maintain the flow of resources (Auerbach and Thachil 2018; Diaz-Cayeros et al. 2016; Oliveros 2021; Zarazaga 2014, 2015). See González-Ocantos and Oliveros (2019) and Hicken & Nathan (2020) for a discussion of this debate.

brokers interviewed by Zarazaga (2014), none of them thought it was possible to find out how an individual had voted. More broadly, "systematic evidence of the monitoring of vote choices is surprisingly rare" (Hicken and Nathan 2020, p.281).[17] At the same time, voters' perceptions of ballot secrecy may be based on preconceptions and not in actual experiences with ballot tampering. In her study of voters' perceptions of ballot integrity in Argentina, Oliveros (2019) finds that voters who report that their neighbors received clientelistic offers are less likely to believe that voting is secret, while personal experience with clientelism is not correlated with beliefs about ballot secrecy, suggesting that skepticism about the secret ballot is not informed by personal experiences. If clientelism is indeed more prevalent in slums, slum dwellers' responses to the survey experiment will reflect their own experiences with clientelism, while non-slum dwellers are more likely to base their responses on perceptions or misconceptions. Since most recent studies suggest that monitoring at the ballot booth is indeed extremely rare, we expect slum dwellers to be more aware of brokers' difficulties in finding out about how people voted. Slum dwellers will thus, more accurately, believe that the consequences of reneging on the clientelistic agreement will be more serious if the potential client does not turn out to vote than if she votes for another candidate. In contrast, we expect non-slum dwellers to believe that the consequences of reneging on the clientelistic agreement will be more serious if the potential client votes for another candidate because their lower exposure to clientelism reduces their awareness of the difficulties of monitoring individual electoral choices at the ballot box.[18]

Empirical Strategy

Our data come from two surveys conducted around Argentina's 2015 presidential election. One is a nationally representative survey: the 2015 Argentine Panel Election Study (APES) (Lupu et al. 2015).[19] The other one is a shortened 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 eligible voters 18 years old and over. All interviews were conducted using Personal Digital Assistants. The first wave of interviews

[17] After reviewing all publications on clientelism from 2008 to 2018, the only studies found by Hicken and Nathan (2020) showing concrete evidence of monitoring electoral behavior were historical or non-fully democratic cases.

[18] Whereas an alternative explanation would be that monitoring turnout is easier than monitoring vote choice in the slum, but not outside the slum, we find no theoretical argument to support this interpretation. Moreover, the slum dwellers in our sample reported voting in around 30 different schools outside the slum (the assignment of polling places is based both on the address of the voter and the first letter of his/her last name).

[19] 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.

(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 who agreed to participate again, plus a refresh sample. The first wave includes 1149 respondents, while the second wave includes 780 respondents from the first wave plus 626 respondents drawn from a refresh sample.[20] The second wave therefore has a sample size of 1406.[21] In this paper, we use data mainly from the first wave.[22]

The survey among slum dwellers (eligible voters aged 18 and over) consisted of a reduced version of the APES questionnaire and was administrated 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 this 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.[23]

We selected a slum located in San Miguel, one of the northwestern municipalities of Greater Buenos Aires (GBA) where a third of the population lives in poverty.[24] 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 (3335.6 people per km^2 in San Miguel).[25] The slum we selected developed 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 124 acres and, according to estimates of the local government, the population is 7000

[20] To compensate for sample attrition, a refresh sample was drawn, selected according to the same procedures used for the first wave.

[21] 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.

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

[23] 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.

[24] 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).

[25] The province of Buenos Aires has a population density of 50.8 people per km^2 and Argentina, of 10.7 people per km^2.

people—a population density of 14,000 people per km^2. As described, the slum lacks basic public services and infrastructure, and is located in 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.[26] Because clientelism could be associated with negative social stigma, clients have incentives to misreport clientelistic exchanges, thereby increasing the probability of under-estimation when relying on direct questions.[27] Following González-Ocantos et al. (2012), to address problems of social desirability bias and obtain reliable estimates of clientelism, we use a list experiment. This technique is simple to implement, and useful for generating unbiased estimates when dealing with sensitive topics. List experiments randomly split the survey sample into treatment and control groups. 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 have done, but only to indicate *how many* of those activities were done, so the question provides the respondents with full anonymity.[28]

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.

Given the sensitivity of asking directly about the potential consequences of breaking a clientelistic agreement, we randomly assigned respondents to hear

[26] Question wording is in the Appendix.

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

[28] 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 ≤ 0.05) for design effects. Table A8 in the Appendix reports the distribution 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).

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.[29] We compare the average responses across groups 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 [Pedro/Susana] [delivers/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 [**not to vote/to vote for another candidate].**

Information about the broker's gender, whether the broker promises or delivers the government sponsored job, and the behavior of the voter on Election Day were all randomized across respondents.[30] Here, we focus only on the third treatment, the electoral behavior of the voter. Immediately following the vignette, respondents were asked, "How likely are you to believe that the voter would face any problems **[for not turning out to vote/ for not voting for [Pedro/Susana]'s candidate]**? Very likely, somewhat likely, somewhat unlikely, or 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 and Zubizarreta 2018; Zubizarreta and Keele 2017; Zubizarreta et al. 2014).[31] Using this method,

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

[30] There were then $2\times2\times2=8$ 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.

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

respondents in the slum are matched to respondents who do not live in the slum but are otherwise as similar as possible (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.[32]

In order to produce groups that have a similar distribution of observed covariates, we included 15 covariates in the matching procedure.[33] We selected covariates based on their relation to the outcome (clientelism), the treatment assignment (living in the slum), or both.[34] 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 and Murillo 2004, 2013), our matching procedure includes a covariate for Peronism. We measure Peronism 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 the respondent or anyone else in the household receives benefits from the 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 and Pessino 2014). While we expect beneficiaries of government programs to be more 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 restricting wave 1 of the APES national representative sample ($N = 1149$) to only GBA respondents, we are left with 328 respondents in the non-slum group (our control) and 385 respondents in the slum group (our treatment). After the matching procedure, we are left with a sample of 234 respondents in each group.[35] 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.[36]

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

[33] 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.

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

[35] 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.

[36] Standardized differences of greater than 0.25 generally indicate serious imbalance in covariates (see, for instance, Stuart 2010).

Table 1 Mean balance, before and after matching

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

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 successfully created balance in covariate values between slum respondents and non-slum respondents, we calculated standardized differences in covariate means before and after our matching procedure. Table 1 presents the mean balance and the absolute standardized differences in means before and after matching for our included matching covariates.[37]

As expected, slum dwellers are different in important ways from those who do not live in the slum, leading to 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% of the population of GBA lives in poverty. Note, however, that the conditional cash program (AUH) is 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).

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

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.

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 base our analysis on the matched data, which 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.[38]

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%). 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%).[39]

Note that the observed differences may be related to measurement issues. Indeed, questions about clientelism (particularly *self-reported clientelism*) are sensitive, so respondents may not be willing to provide honest answers when asked directly (González-Ocantos et al. 2012, 2014). If slum respondents are less subject to social desirability bias about their personal connections with clientelism, 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.

Among slum dwellers, the estimated percentage of respondents receiving a favor or a gift in the last year is a significant 30%; while the estimated

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

[39] 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.

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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

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

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

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)
	$N=234$	$N=234$

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

percentage among the non-slum dwellers is a non-significant 10%. 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 power demands of list experiments. However, that we do find a significant estimate for the slum respondents with the same sample size ($N = 234$), indicating 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, highlighting 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.

Columns 1 to 3 present the results from the unmatched sample and show that slum respondents are significantly more likely to consider not voting riskier than

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

	APES all respondents (wave 1)	Unmatched sample		Matched sample	
		All slum respondents	APES GBA	Slum respondents	Non-slum respondents
Not voting	0.55	0.60	0.45	0.58	0.50
	(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	0.10***	0.26***	0.03	0.21***	0.06
	(0.03)	(0.05)	(0.05)	(0.06)	(0.07)
	$N = 1053$	$N = 385$	$N = 328$	$N = 234$	$N = 234$

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; ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

voting for another candidate.[40] Slum residents and non-slum residents, however, may be fundamentally different in ways that could be associated with their response to this question. We focus our analysis on the matched sample (columns 4 and 5), which 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% (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% (middle row), a statistically significant difference of 21 percentage points. This difference in perceptions of the level of risk associated with voting for a different candidate or not voting at all may suggest that most slum respondents are not expecting political brokers to be able to find out about individual electoral choices. 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 turn out. In fact, for two thirds of slum respondents (63%), voting for another candidate is very unlikely or somewhat 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 turn out is 50%, while the percentage believing she would get punished for voting for a different candidate is 44%. This non-statistically significant difference of 6 percentage points is 15 percentage points lower than among slum respondents.[41] 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 personal experience with clientelism (slum respondents) believe that the negative consequences for defecting clients are significantly higher if they do not turn out than if they support another candidate. In line with our expectations, slum respondents (with more exposure to clientelism) 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.

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

[40] The 23 percentage point's difference between slum respondents and GBA APES respondents (columns 2 and 3) is significant at the 99% level.

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

that *some* poor voters are more susceptible to clientelistic arrangement than others. By comparing two similar populations—based on our slum survey and matching technique—we 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 list and survey experiments, 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 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 that non-slum respondents make the same distinction. Although we do not test this expectation directly, we believe this is simply a consequence of slum dwellers (more expose to clientelism) having a better understanding of how clientelism works than non-slum dwellers (with less exposure).

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 disproportionally exposed to clientelism. Our results suggest that including slum dwellers in surveys is crucial for understanding clientelism in Argentina and other countries. Recent scholarship in India is at the forefront of this research agenda (e.g., Auerbach 2016; Auerbach and Thachil 2018, 2020; Spater and Wibbels 2018). Future studies should continue exploring electoral politics in informal settlements in other countries.

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 clientelism and the comparative political behavior of slum dwellers more generally.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s12116-021-09324-x.

Acknowledgements We thank Ernesto Calvo, Jessica Gottlieb, Noam Lupu, Raúl L. Madrid, Thomas Mustillo, Aníbal Pérez-Liñan, David Samuels, Luis Schiumerini, Julie Weaver, Rebecca Weitz-Shapiro, Liz Zechmeister, and colleagues at Tulane University for helpful suggestions on the design of the survey and comments on a previous version of this paper. Participants at seminars at University of Notre Dame and University of Chicago as well as LASA 2017, SAAP 2017, and APSA 2018 also provided valuable feedback. Giancarlo Visconti provided indispensable and generous methodological help. Mercedes Sidders and Gonzalo Elizondo helped us with the implementation of the survey. We also want to acknowledge the assistance of Ezra Spira-Cohen. The

slum survey was funded by Centro de Investigación y Acción Social (CIAS, Argentina) and the Center for Inter-American Politics & Research (CIPR) at Tulane University. Part of this paper was completed while Virginia Oliveros was a Visiting Fellow at the Kellogg Institute for International Studies, University of Notre Dame.

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