Appendix A
Argentina Brokers' Survey

To gather fine-grained information about the preferences and behaviors of political brokers, we surveyed elected city councilors and non-elected activists who work for those councilors in the Argentine provinces of Córdoba, San Luis, and Misiones, as well as the Conurbano area of greater Buenos Aires.[1] In this book, we refer to both councilors and non-elected activists as "brokers." This is appropriate, as councilors may work as operatives for mayors or other politicians at higher levels of the political system, whereas many councilors also had worked as neighborhood operatives before rising to elected office. The non-elected activists we surveyed, meanwhile, work directly as political operatives for councilors. We therefore believe that both elected councilors and their non-elected operatives should be considered local brokers. Surveying them gives us important insights into their preferences and behaviors.

The major difficulty involved in surveying brokers involves how to generate a representative sample. Previous researchers working in Argentina, such as Auyero and Levitsky, have generated valuable insights into the political function and behaviors of brokers.[2] Yet it is difficult to know how results from these convenience samples may or may not extend to the many tens of thousands of political operatives who comprise the population of interest. Generating a probability sample of these operatives is challenging, however, because a ready-made sampling frame – that is, a list of brokers from which one could draw a random sample – does not exist.

As outlined in Chapter 4, our approach to this problem was two-fold. First, we drew a probability sample of councilors by randomly sampling municipalities from four Argentine provinces and then randomly sampling city councilors

[1] Our surveys were approved by the Yale Human Subjects Expedited Review Committee under 1RB Protocol #0906005355.

[2] Auyero 2001, Levitsky 2003.

in each of those municipalities. Once municipalities were sampled, it was straightforward to obtain a list of councilors and thus a sampling frame for councilors in each municipality. Second, our survey instrument then asked sampled councilors for a list of the non-elected activists who work for them. This generated, for each sampled councilor, a sampling frame of brokers, from which we could then sample at random.

As far as we know, our data provide the first large, representative sample of brokers in any country. The subjects of the survey bear some resemblance to those sampled for the European Political Party Middle Level Elites study,[3] but our samples operate at a lower level and come from much more bottom-heavy parties or machines. Possible bias from nonresponse or from the failure of councilors to provide complete lists of their non-elected brokers – discussed later – could compromise the strict probability sampling of non-elected brokers. The elected councilors' selection would not be thus compromised, however.[4] Another innovative aspect of our effort was that we asked some questions as survey experiments, meaning that we recruited our experimental subjects in an unusual but valuable way. The value of our approach is that we are confident that our results can be reliably projected to the population of councilors from which our sample was drawn, and our procedure also generates systematic and likely quite representative data on non-elected brokers – a difficult population to study systematically.

In this Appendix we discuss our survey instrument, including its embedded survey experiments; describe our sampling design, including our procedure for drawing a probability sample of city councilors and a semiprobability sample of the non-elected operatives who work for them; and discuss challenges in data analysis, such as the bootstrapping of standard errors, that arise from the complex sampling design. The survey instrument was written and administered in Spanish; it was piloted in July 2009, and interviews took place between 2009 and 2011. In all, our sampling design called for us to interview approximately 800 brokers. Interviews were conducted by the authors, by a team of research assistants in each of the four provinces, and by Edwin Camp of Yale University, who was instrumental in planning and implementing the survey. Mariela Szwarzberg and Luis Schiumerini also helped us to develop the survey instrument; the surveys were implemented by us and by the team of research assistants we thank in the acknowledgments.

A.1 SURVEY INSTRUMENT

Our survey instrument sought to elicit several types of information from brokers.[5] At the start of each interview, we asked a battery of questions about the

3 See Reif, Cayrol, and Niedermayer 1980.

4 We might then describe our procedure as having generated a probability sample of councilors and a semiprobability sample of non-elected brokers.

5 The survey instrument is posted in its entirety at http://www.thaddunning.com/data/brokers, along with the replication files and other materials.

broker's personal history working in politics: the party or parties the broker had worked for and elective offices sought or obtained. We also asked several questions tapping individual brokers' attitudes toward risk. At the conclusion of each interview, several questions also sought information on brokers' age, education, income, and other occupations.

Next, we asked several questions about the numbers and party affiliations of other brokers and voters in the neighborhood where the broker works. We also asked about the quantity and origin of resources obtained by brokers from party leaders and other sources, perceptions of the extent and nature of rent seeking by brokers, and the nature of relationships between individual brokers and "their" voters, including the kind and quality of information voters have about individual voters' preferences and behaviors. Finally, we asked a series of questions posing hypothetical scenarios for brokers, for instance, asking them to evaluate the types of voters that would be targeted for benefits in each scenario, what voters would do if they stopped receiving benefits, or what brokers would do if party leaders deprived them of resources.

Several of these latter questions were asked in the form of survey experiments. Thus we used four different versions of our questionnaire, with the version assigned at random to particular respondents.[6] Several survey-experimental questions were identical on versions 1 and 3 and versions 2 and 4; thus, for these questions, approximately one-half of respondents were assigned to one version of the question and one-half of respondents to the other. The main rationale for asking these questions in the form of survey experiments was that we were concerned that two questions posed to the same respondent, in which aspects of the scenario presented to the respondent varied across the two questions, would not provide valid counterfactuals for each other. In particular, we were concerned that exposure to one version of the question would condition responses to a second question – making it impossible to separate the effects of the particular scenario being posed from the effects of exposure to a different scenario earlier in the survey. One obvious alternative would have been to ask each broker every question but to randomize the question order, so that we could evaluate empirically the possibility of contamination by earlier questions. For logistical and cost reasons, however, we opted to confine the survey to different versions of paper-based questionnaires.[7] Because most survey-experimental questions had only two versions, moreover, we projected

[6] In practice, we implemented this by sorting stacks of questionnaires and working through the stacks in each municipality. There were many more respondents in each municipality than versions of the questionnaire (see Tables A.3.1 and A.3.2), and this ordered rotation very likely ensures that the version administered is statistically independent of respondents' characteristics or potential responses.

[7] We considered the purchase and use of electronic PDAs that would allow us to randomize question order more seamlessly. However, we did not pursue this alternative for the present study, for various reasons.

that we would have sufficient statistical power for detecting substantial effects of exposure to different versions of the questions.

A.2 SAMPLING DESIGN

We purposively chose four Argentine provinces or subprovinces from which to sample brokers: Córdoba, Misiones, San Luis, and the Conurbano area of greater Buenos Aires. These areas vary with respect to the competitiveness of the party system, the strength of Peronist and Radical party organizations, and other factors such as urbanization that may be related to the efficacy or character of clientelism. Thus our chosen provinces include a large province with a substantial Radical Party presence (Córdoba), an example of monopolistic clientelism in which a single Peronist-affiliated family has long been politically dominant (the Rodrguez Saá family in San Luis), a rural northeastern province dominated by a single regional party (the *Partido Renovador* in Misiones, which combines an important Radical faction and an important Peronist faction),[8] and the highly urban area of greater Buenos Aires that has historically provided an important base for the Peronist party (the Conurbano). The sample therefore contains two relatively competitive provinces and two monopolistic provinces.

The Conurbano of Buenos Aires, with its heavy concentration of poor urban voters, is judged to be of such importance to understanding clientelism in Argentina that fine-grained information about the role of brokers there was a particular priority. Moreover, as Szwarcberg and others emphasized, around 60 percent of registered voters in the province of Buenos Aires and one-quarter of Argentina's total population live in the 24 municipalities of the Conurbano – giving this area important influence over national electoral outcomes.[9] Thus, we chose not to sample municipalities from the entire province of Buenos Aires but focused instead on the Conurbano area.

Because these provinces were chosen purposively, we can only project results from our survey to the population of brokers in these provinces. Still, these four provinces or subprovinces contain a substantial proportion of the Argentine population – around 52 percent of all Argentines.[10] They also include highly politically relevant areas such the Conurbano, which makes these areas of substantial interest and importance. We now describe how our samples of municipalities and brokers were selected in each of the provinces.

[8] Perhaps 95 percent of mayors in Misiones are from the *Partido Renovador*, and they are quite dominant in the province. Edwin Camp, personal communication.

[9] Szwarcberg 2009.

[10] Rounded, 2010 census figures give the population numbers as follows: Córdoba, 3.3 million; Misiones 1 million; San Luis, 432,000; Conurbano, 16 million. These figures total to 20.7 million, or 52 percent of the 40 million residents in Argentina. Source: INDEC.

A.2.1 Sampling Municipalities

Within provinces, our design involved a multistage cluster sample. The primary sampling units were municipalities (*municipios*).[11] Within each of the three provinces and one sub-province (the Conurbano in Buenos Aires province), we sampled municipalities at random by assigning each municipality a quasi-random number uniformly distributed on the $[0, 1]$ interval and sorting the municipalities in order of these numbers. We then worked down the lists in each until we had reached the overall target number of interviews of 800. We don't know the population of brokers in each province so could not design a strict probability-proportionate-to-size sample, but we drew more brokers from larger, more populated areas likely to have more brokers, such as the Conurbano (257 out of 800 brokers) and Córdoba (179 out of 800 brokers).[12] Our sampling design called for a particular fraction of the councilors in each municipality, and of the brokers working for these councilors, to be surveyed. Because we lacked a sampling frame for non-elected brokers in advance of data collection, and thus did not know how many brokers would enter our sample from each municipality, we opted to take the approach of working down the lists in each province until the required number of interviewees had been surveyed.[13]

Table A.1 shows the municipalities in each province that were selected into our sample. In the Conurbano of greater Buenos Aires, we sampled 10 of 24 municipalities; in Córdoba, 10 out of 249; in San Luis, 9 out of 18; and in Misiones, 20 out of 75.[14] In the Conurbano and Córdoba, the samples were self-weighting, whereas in San Luis and Misiones, municipalities with larger populations were weighted more heavily and thus had a larger probability of selection.[15] In San Luis, we excluded extremely small villages called communes ("*comunas*") from the universe of primary sampling units. More municipalities appear in Misiones than in other provinces in Table A.1 because municipalities in that province are on average smaller and have fewer councilors – necessitating sampling in more municipalities to reach our intended size of the broker sample in the province. Thus considering the universe of brokers in the four provinces as a whole, brokers from Misiones may be overrepresented.

11 *Municipios* are administrative units somewhat akin to counties in the United States that, however, have city councils.

12 See Table A.2. However, we also wound up with a large sample of 170 brokers from Misiones. San Luis, at 102 brokers, is also our least populous province, as described in a previous note in this Appendix A.

13 We worked roughly synchronously in the different provinces, though started slightly earlier in Buenos Aires and Córdoba.

14 Our bootstrapped standard errors, discussed below, take account of our sampling from small finite populations, which is especially important in the Conurbano and San Luis.

15 In Misiones and San Luis, we weighted municipalities by population size: we divided each municipality's population by the province's total population and multiplied this ratio by the realization of a quasi-random number distributed uniformly on [0,1]. We then sorted the list in descending order according to the product of the population size ratio and the random number.

TABLE A.1. *Sampled Municipalities by Province*

Buenos Aires (Conurbano)	**Córdoba**	
Almirante Brown	Arroyito	
Avellaneda	Córdoba Capital	
Ezeiza	General Cabrera	
Florencio Varela	Huanchillas	
General San Martín	Las Varas	
Ituzaingó	San Francisco del Chañar	
Lanús	Santa María de Punilla	
Malvinas Argentinas	Tio Pujio	
San Martín	Villa Carlos Paz	
Tigre	Villa Fontana	
San Luis	**Misiones**	
Juana Koslay	25 de Mayo	Garupá
Justo Daract	Apóstoles	Jardín America
La Toma	Bernardo de Irigoyen	Oberá
Villa de Merlo	Campo Grande	Posadas
Naschel	Campo Ramón	Puerto Esperzanza
Quines	Candelaria	Puerto Iguazú
San Francisco del M. de O.	Com. Andres Guacurari	San Antonio
San Luis	Dos de Mayo	San Ignacio
Villa Mercedes	El Soberbio	San Pedro
	El Dorado	San Vicente

A.2.2 Sampling Brokers

Our method for sampling city councilors was simple. Once we had sampled municipalities, we obtained lists of the elected members of the city council in each sampled municipality. We then selected at random one-half of the councilors on each list and requested in-person, face-to-face interviews with these selected councilors.

Without a readily available sampling frame for non-elected brokers, the procedure for drawing a probability sample from that population was less straightforward. Indeed, the absence of such a sampling frame is a major obstacle to characterizing this population and constitutes an important contribution of our research.

Our strategy was as follows. During the interview with each councilor, we asked:

How many brokers [*referentes*[16]] work for you? Please, think only of those that you know by name.

[16] We used the word"*referente*" for broker, which might also be translated as "activist" or "operative" and which is more neutral than the often-used but sometimes pejorative term "*puntero*."

The interviewer recorded this number. Among those who answered this question, the mean number of brokers was 19, with a standard deviation of 23.[17] We found substantial heterogeneity in answers to this question across provinces – from 23 in Buenos Aires and 22 in Misiones to 13 in Córdoba and 12 in San Luis.

Then, at the end of the interview, the councilor was read the following statement:

> We thank you very much for your participation. The success of this academic study depends on the collaboration of many people like you. Thus, just as we have asked leaders throughout the country, we desire your collaboration to choose some of your brokers to interview. To assure that we interview a representative group, it is necessary that the selection of these people be done at random. I would like to ask for your help to sample some of the brokers who work for you, using a simple procedure that we have used in the other cases. Would you accept to help me?

Councilors who accepted this request were then asked for the name and contact information of each of their brokers. In most municipalities, we then sampled one-third of the brokers on these lists at random and attempted to contact these brokers to request interviews.[18] Two exceptions arose. When the interviewed councilor only named one broker, we interviewed that broker with probability one; when the councilor named two brokers, we interviewed one of them with probability one-half.

In principle, this procedure produces a probability sample of the non-elected brokers who work with city council members in our selected provinces. However, there are at least two important concerns about the representativeness of our sample of non-elected brokers. One is that councilors may not faithfully report the number of brokers who work for them, in response to our initial question. For instance, they may tend to inflate the number of brokers who they say work with them and then be unable to name this number of brokers at the end of the interview, making it difficult to evaluate the true nonresponse rate in our attempts to survey brokers. Our interviewers were asked to record whether the number of brokers each councilor gave in response to the initial question matched the number of names he or she ultimately provided on the list. These two numbers matched in about 42 percent of the cases; however, in 58 percent of the interviews, the numbers differed. Moreover, as Table A.2 shows, we sampled non-elected brokers at an approximate rate of about 1.7 per councilor – that is, we interviewed 516 non-elected brokers and 284 elected councilors. This implies that councilors each provided us lists of approximately

[17] Including one extreme outlier – a councilor who reported working with 1,000 brokers – raises the mean to 23 and the standard deviation to 69.

[18] In the Conurbano of Buenos Aires, we sampled one-fifth of the brokers in every municipality except Malvinas Argentinas and Tigre. We did this because we wanted to sample a larger number of municipalities to avoid excessive clustering of respondents within municipalities; in the Conurbano, councilors often have many non-elected operatives, so with a smaller sampling fraction we could have met our rough provincial target for brokers with just a few sampled municipalities.

TABLE A.2. *Sampled Brokers Numbers of Councilors and Non-Elected Brokers by Province*

	Buenos Aires (Conurbano)	Córdoba	San Luis	Misiones	Totals
Councilors	99	57	89	55	300
Non-elected brokers (*referentes*)	158	122	105	115	500
Totals	257	179	102	170	800

$(1.7)(3) = 5.1$ brokers' names, on average – which is substantially below the mean number of 19 brokers reported by councilors in response to the initial non-specific question about the numbers of brokers who work with them. This proportion varied somewhat by province – from 1.7 in Buenos Aires (where we sampled only one-fifth of brokers from each list) – to 1.9 in Córdoba and San Luis and 1.6 in Misiones.[19]

Another related, perhaps even more germane concern is that councilors may selectively name brokers at the end of the interview, or forget to report brokers with whom they work less frequently or less well. This tendency is very difficult to analyze systematically. If the characteristics of brokers that lead them to be included or excluded from the lists are related to their answers to our questions, councilors' selective reporting would compromise representativeness and generate bias.[20]

These potential problems are much less serious for our survey of elected brokers (i.e., councilors). We therefore think it is correct to call our survey of councilors a probability sample. Our survey of non-elected brokers might be called a semiprobability sample: probability procedures were used to select brokers from the sampling frame we built using our interviews of elected councilors, but that frame itself could be flawed.

Tables A.3.1 and A.3.2 break down Table A.2 by municipality.

[19] In Buenos Aires, we sampled only one-fifth of the brokers for some municipalities. Also, some city council members simply did not provide lists, and we were not able to interview every broker that we selected. The relatively low proportion of brokers to city council members, relative to provinces like Córdoba or Misiones (see Table A.2), is due to the brokers not being included, either because they were not named or were not found. We experienced particular difficulty interviewing non-elected brokers in Ezeiza.

[20] Another source of bias would arise if some of the brokers we identify refused to be interviewed. As Tables A.3.1 and A.3.2 show, summing across all provinces, we successfully surveyed 300 of 336 randomly selected councilors, for a response rate of 89.3 percent. Among non-elected brokers in the province of Buenos Aires, the response rate was 66.7 percent. Unfortunately, because lists of brokers' names from which we sampled in other provinces were inadvertently discarded, we cannot readily calculate the true response rate for non-elected brokers in those provinces. Note that substitutions were not allowed for either councilors or non-elected brokers, i.e., if a selected broker was not found, we did not substitute another who had not initially been randomly selected. In a few municipalities in Córdoba and San Luis, an additional councilor was inadvertently interviewed, which is why the number of surveyed councilors is occasionally greater than the number of sampled councilors.

TABLE A.3.1. *Survey Completion Rates by Province and Municipality (Buenos Aires, Córdoba, and San Luis)*

Province Municipality	Sampled Councilors	Surveyed Councilors	Surveyed Non-Elected Brokers
Buenos Aires (Conurbano)	**105**	**99**	**158**
Almirante Brown	12	11	26
Avellaneda	13	13	17
Ezeiza	10	7	0
Florencio Varela	11	11	26
General San Martín	13	12	12
Ituzaingó	10	10	28
Lanús	12	11	12
Malvinas Argentinas	12	12	26
Tigre	12	12	11
Córdoba	**58**	**57**	**122**
Arroyito	4	5	9
Córdoba Capital	16	22	69
General Cabrera	4	4	14
Huanchillas	4	1	0
Las Varas	4	4	1
Lozada	4	0	0
San Francisco del Chañar	4	5	29
Santa María de Punilla	4	4	3
Tio Pujio	4	2	1
Villa Carlos Paz	6	7	9
Villa Fontana	4	3	1
San Luis	**62**	**55**	**115**
Buena Esperanza	3	0	0
Candelaria	2	2	3
Concaran	3	3	8
Juana Koslay	5	4	5
Justo Daract	5	7	15
La Toma	4	4	14
Lujan	2	2	2
Villa de Merlo	5	5	10
Naschel	3	3	4
Quines	4	4	3
San Francisco del M. de O.	3	3	3
San Luis	7	6	13
Santa Rosa de Conlara	3	3	17
Tilisarao	4	5	3
Unión	2	0	0
Villa Mercedes	7	4	15

TABLE A.3.2. *Survey Completion Rates by Municipality (Misiones)*

Province Municipality	Sampled Councilors	Surveyed Councilors	Surveyed Non-Elected Brokers
Misiones	**111**	**89**	**105**
25 de Mayo	3	3	12
9 de Julio	2	1	2
Apóstoles	3	3	6
Aristobulo del Valle	3	2	2
Bernardo de Irigoyen	3	3	1
Campo Grande	3	3	2
Campo Ramón	3	2	4
Campo Viera	2	2	1
Candelaria	3	2	5
Capiovi	2	2	0
Caraguatay	2	1	0
Cerro Azul	2	1	0
Colonia Alberdi	2	2	0
Colonia Aurora	2	0	0
Colonia Victoria	2	2	0
Com. Andres Guacurari	3	1	1
Concepción de la Sierra	2	2	1
Dos Arroyos	2	1	0
Dos de Mayo	3	3	0
El Alcazar	2	2	3
El Soberbio	4	4	5
Eldorado	4	2	5
Garupá	3	2	9
Guaraní	2	0	0
Itacaruaré	2	1	0
Jardín America	3	3	5
Leandro N. Alem	4	3	3
Los Helechos	2	2	0
Oberá	3	2	9
Posadas	7	5	15
Puerto Esperzanza	3	3	4
Puerto Iguazú	4	2	1
Puerto Rico	3	3	1
San Antonio	2	2	2
San Ignacio	3	3	1
San José	2	2	0
San Pedro	3	2	3
San Vicente	5	5	2

A.3 BOOTSTRAPPING STANDARD ERRORS

Our design for sampling brokers was complex, as noted in the previous sections. After selecting municipalities in each province at random, we randomly sampled one-half of the city councilors for interviews. The number of councilors on each municipalities is an increasing function of municipal population, and hence the number we interviewed was not uniform across the selected municipalities. We then sampled one-half of the elected councilors in each municipality. Finally, we sampled one-third of the non-elected brokers working with each councilor (the sampling fraction was one-fifth in the Conurbano – again, to reduce clustering of brokers within municipalities). The procedure thus produced a multistage cluster sample, in which councilors are clustered by municipality, and brokers are clustered by councilor. Recall that we sampled municipalities without replacement from the small finite population of municipalities in each of four provinces (Córdoba, San Luis, Misiones, and Buenos Aires).[21] Given that the number of brokers in the sample depends on the particular municipalities sampled (because municipalities have unequal numbers of councilors) and on the particular councilors sampled (because different councilors may have different numbers of brokers working with them), the survey sample size is itself a random variable.

Our sampling procedures call for caution when we use the sample to estimate parameters in the population of brokers. Cluster sampling may make variance formulas based on simple random sampling inappropriate. In addition, because we sampled without replacement from small populations, we cannot assume i.i.d. sampling.[22] Finally, when we use the mean of the sample to estimate population means, we may have some ratio-estimator bias – because both the numerator (e.g., the sum of responses in the sample) and denominator (the sample size) are random variables.

This is a good situation in which to use the bootstrap, which is a procedure for using computer simulations to investigate the properties of statistical estimators and to estimate standard errors.[23] We begin by briefly reviewing the theory of the bootstrap before turning to a description of our use of it to estimate standard errors as well as the extent of ratio-estimator bias. Although the bootstrap is most helpful when analytic variance formulas may not apply, or when we want to estimate the degree of bias in certain estimators, the procedure is most easily understood for simple random samples.

[21] As noted in connection with Table A.1, the sample is especially large relative to the population of municipalities in the Conurbano, where we sampled 10 of 28 municipalities, and San Luis, where we sampled 9 out of 18. In Córdoba, we sampled 10 out of 249 municipalities, and in Misiones, 20 out of 75.

[22] Our sample of municipalities in Buenos Aires is especially large, relative to the population of municipalities in the Conurbano.

[23] Important references on the bootstrap include Efron 1979, Bickel and Freedman 1981, 1984, and Chao and Lo 1985. A very clear introduction is in Freedman 2005.

Suppose that we have drawn a simple random sample of size n from some (large) population. The parameters of this original population – say, the mean μ or the variance σ^2 – are unknown. Of course, we know from statistical theory that the mean of the sample, denoted $\overline{X}$, is an unbiased estimator of μ, and we know the sampling variance of the mean is σ^2/n. Also, the sample variance $\hat{\sigma}^2$ is an unbiased estimator for σ^2.[24]

However, suppose we have forgotten statistical theory. We can use the bootstrap to assess the unbiasedness and variance of the estimator $\overline{X}$. The procedure is as follows.

- The empirical sample of size n becomes a new "bootstrap population." We know the true parameters of this population – for example, the mean $\overline{X}$ and the variance $\hat{\sigma}^2$.
- We use the computer to draw a sample of size n at random with replacement from this bootstrap population. This sample is the first "bootstrap replicate." Drawing with replacement simulates the process of simple random sampling from a large population.
- Then, we calculate the estimator of interest – say, the sample mean – for this first bootstrap replicate. Denote this estimator by $\overline{X}^{(1)}$.
- We repeat this procedure, say, 1,000 times. Thus we create 1,000 bootstrap replicates, with means $\overline{X}^{(1)}.,\overline{X}^{(2)},\ldots,\overline{X}^{(999)},\overline{X}^{(1000)}$.[25]

The *bootstrap principle* says that the sampling distribution of each bootstrap estimator $\overline{X}^{(i)}$ approximates the sampling distribution of the original estimator $\overline{X}$ – because the process of drawing bootstrap replicates with replacement is akin to the simple random sampling that produced the original data. Thus for any bootstrap replicate i, the distribution of $\overline{X}^{(i)} - \overline{X}$ approximates the distribution of $\overline{X} - \mu$. Moreover, the 1,000 bootstrap replicates trace out the sampling distribution of $\overline{X}^{(i)}$. For simple random samples, the mean of the 1,000 bootstrap replicates should be about equal to the mean of the bootstrap population – because the mean is unbiased – and the standard deviation of the 1,000 bootstrap replicates approximates the true standard error $\sigma/\sqrt{n}$.

Although this example shows how the bootstrap works, we typically want to use the bootstrap in more complex settings, where estimators may not be unbiased or analytic formulas for their variances may be unavailable. The key to bootstrapping is to mimic the actual sampling design that produced the empirical sample, for this is what will allow us to estimate the true variance as well as any bias in estimators. For the Argentina brokers' survey, this means replicating the two-stage clustering and other features of the sampling design.

[24] For this to be true, we must form the sample variance by dividing the sum of squared deviations from $\overline{X}$ by $(n-1)$.

[25] There is no requirement that we draw 1,000 bootstrap replicates; sometimes, a smaller number may suffice, or a larger number may be required.

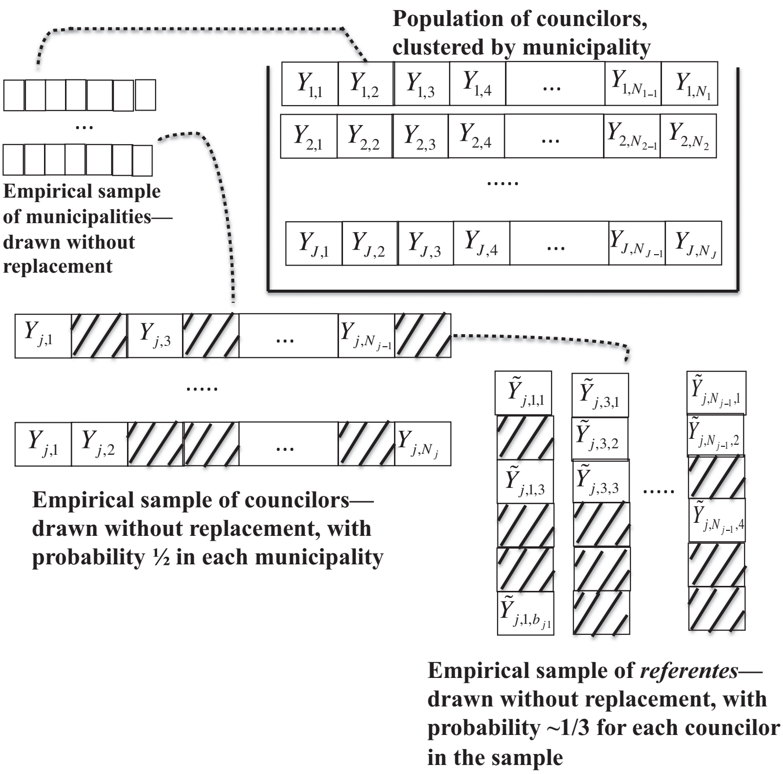

FIGURE A.1. Sampling Design: Argentina Brokers' Survey.

We now explain the use of the bootstrap in our setting.[26] First, we introduce the following notation. Each municipality $j = 1, \ldots, J$ in each of our four provinces has N_j councilors (recall that N_j varies as a function of municipal size). Next, index the responses for councilor i in municipality j by $Y_{j,i}$. Finally, each councilor in turn has b_{ij} non-elected brokers whose responses are indexed by $\tilde{Y}_{j,i,b_k}$, where j is the municipality, i is the councilor, and b_k is the kth non-elected broker or *referente* working for that councilor.

Then, we can depict the original sampling design as in Figure A.1. First, we draw n municipalities without replacement from the population of J municipalities, where $n < J$. Each sampled municipality j has N_j councilors. We then sample councilors at random from each of these municipalities. This is shown in Figure A.1 by crossing out with slashes those councilors in each municipality that are *not* sampled. Finally, for each selected councilor in each selected municipality, we select *referentes* – that is, non-elected brokers – at random.

[26] A Stata routine to implement the procedure described here was written by Joel Middleton and Edwin Camp.

In Figure A.1, we show the brokers for each of the councilors selected in the *first* municipality only, again crossing out with slashes those brokers that are not selected. Note that this sampling process takes place in each of the four provinces in our universe.

To bootstrap estimators such as the sample mean, we want to simulate this sampling design using the computer. We describe the procedure before discussing why it works.[27]

1. First, we copy each municipality in the empirical sample k times, where k is inverse of the sampling fraction; for instance, if there are 30 municipalities in the province or subprovince, and we sampled 15, then $k = 2$.[28] This creates a "bootstrap population" of municipalities, which has size $J * k$.[29]
2. Now, draw a random sample of size J *without replacement* from the bootstrap population of municipalities. Note that each of the J sampled municipalities has $\frac{N_j}{2}$ councilors.[30]
3. In each municipality in the empirical sample, copy each councilor once (because the sampling fraction is $\frac{1}{2}$ so $k = 2$); this creates a "bootstrap population" of N_j councilors for every municipality j.
4. Now, draw a random sample of $\frac{N_j}{2}$ councilors *without replacement* from the bootstrap population of councilors for every municipality j in each bootstrap replicate. Calculate and save the mean (or other estimator) of councilors' responses.
5. For each councilor in this bootstrap replicate, create a "bootstrap population" of *referentes* (non-elected brokers) as follows:
   - If a given councilor has 1 *referente* – in the original data – denote this sole *referente* as the bootstrap population of brokers for this councilor.
   - If a given councilor has 2 or more *referentes* – again in the original data – copy the responses for each *referente* three times (because the sampling fraction was one-third) to form the bootstrap population
6. Now for each sampled councilor, draw at random without replacement a number of *referentes* equal to the number of *referentes* originally sampled for this councilor.[31] Calculate and save the mean of brokers' responses in this bootstrap replicate.
7. Finally, repeat all these steps 1,000 times.

[27] The following procedure must be conducted separately for each province (or subprovince, in the case of the Conurbano of Buenos Aires).

[28] Recall that in Buenos Aires we only sample municipalities located in the Conurbano region of greater Buenos Aires.

[29] For now, assume k is an integer; we discuss the case when it is not later.

[30] Assume for now that N_j is even.

[31] For instance, if a councilor named 6 *referentes*, and we had originally sampled 2, we would copy responses for these two *referentes* three times to create a bootstrap population of size 6; then, we would draw twice at random without replacement from this bootstrap population.

Just as in the example of simple random sampling given above, the *bootstrap principle* here applies. That is, the distribution of any bootstrapped estimator, such as the mean of a bootstrap sample, should approximate the distribution of the original estimator. Thus, for any particular survey question, the standard deviation of the 1,000 replicates approximates the standard error. (These are calculated separately for councilors and non-elected brokers, as the sampling variances of the means should differ for these groups.) Also, if there is an appreciable difference between the mean of the empirical sample and the mean of the 1,000 bootstrap replicates, this suggests appreciable ratio-estimator bias.

Why does this procedure work? Consider first the problem raised by clustering: in our data, councilors are clustered within municipalities, and non-elected brokers (*referentes*) are clustered by councilor. The issue is that the responses of councilors in the same municipality, or of brokers working for the same councilor, may be less variable than responses of councilors and brokers in the universe as a whole; thus variance formulas that assume a simple random sample are inappropriate.[32] Our bootstrap procedure works because it preserves the clustered nature of the sampling process: councilors are sampled within municipal clusters, and brokers are sampled within clusters of councilors. If responses within clusters are much less variable than responses across clusters, the standard deviation of the 1,000 bootstrap replicates will be larger than for a simple random sample.

Next, consider the problem of sampling both municipalities, councilors, and non-elected brokers (*referentes*) without replacement. Our procedure creates several bootstrap populations:

- A population of municipalities of size $J * k$, by copying each sampled municipality k times
- J populations of councilors, each of size N_j, by copying the sampled councilors in each municipality 2 times
- $\frac{J * \sum_{j=1}^{J} N_j}{2}$ populations of non-elected brokers, one for each councilor.

Why do we do this copying, that is, why do we create a bootstrap population of municipalities of size $J * k$ by copying each municipality in the original sample k times? By creating a larger population of size (say) $J * k$ and drawing J municipalities without replacement, we mimic the procedure of sampling without replacement from a small finite population.[33] Importantly, copying each element the same number of times does not change the distribution of outcomes in the bootstrap populations; the moments (e.g., mean and variance) of the bootstrap populations should be the same as for the original empirical samples (and thus approximately equal to those of the true distribution).[34]

[32] In the survey literature, the ratio of the variance under clustered sampling to the variance from a simple random sample of equivalent size is known as the *design effect*; see Kish 1965.

[33] Bickel and Freedman 1984 and Chao and Lo 1985 discussed this strategy.

[34] Next some important wrinkles are discussed, however.

Finally, our bootstrap procedure also helps us estimate the bias that is due to the use of a ratio estimator (where random variables are in the numerator and denominator of the estimator). The reason is that for each bootstrap replicate, we can calculate a ratio estimator such as the sample mean – which is the sum of responses in the bootstrap replicate divided by the sample size, both of which are random variables due to our sampling design. The mean of the 1,000 replicates then tells us how far this ratio estimator is off, on average, from the true mean response in the population. If there is bias due to the fact that a nonlinear operation (i.e., division) is used to estimate the population parameter (the mean), this will be reflected naturally in the bootstrap estimates.

A.3.1 Bootstrapping Standard Errors for Treatment Effects

The discussion of bootstrapping thus far applies to the estimation of the variance of estimators of certain population parameters. For instance, suppose we want to know what percentage of brokers have never switched political parties. The mean percentage in our empirical sample estimates this quantity.[35] To attach a standard error to this estimate, however, we would want to conduct a bootstrap simulation, using observed responses to this particular question to form our bootstrap population. The standard deviation of this percentage across the 1,000 bootstrap replicates would estimate the standard error associated with the percentage.[36]

How should we estimate the standard errors for differences-of-means, as in treatment effects for our survey-experimental questions? Here, we are comparing responses of respondents exposed at random to different scenarios. One possibility is simply to calculate the usual standard error for treatment-effect estimators, for instance, as the square root of the sum of the variances in the treatment and control groups.[37] This procedure would produce estimate treatment effects for the sample (the so-called sample average treatment effect, or SATE). However, this procedure for estimating the standard error of treatment effects does not take into account the variability induced by the sampling design – it only takes into account the variability due to random assignment to different versions of our questionnaire, for the sample at hand.

[35] This is because our sampling design is "self-weighting" within provinces – we sample a fixed fraction of councilors and a fixed fraction of councilors' brokers in each municipality – so we do not need to adjust for the over- or under sampling of brokers from certain areas, at least within provinces.

[36] In principle, the bootstrap should be redone for each survey question, because the distribution of responses will vary across different questions; however, using one bootstrap estimate of the standard error for similar questions may suffice.

[37] See Freedman, Pisani, and Purves 2007, pp. A32–A34, n. 11) or Dunning 2012, Appendix 6.2, for discussion and justification of this procedure for estimating the standard error of treatment effects in experiments.

If we are interested in estimating the so-called population average treatment effect (PATE) – that is, the difference of means across any two treatment conditions, for the whole population of brokers from which we drew our sample – we must again account for the sampling design. We can use the bootstrap here as well. We simply add the following procedures to the steps in the bootstrap outlined previously.

- After step 4 in the description of the bootstrap above, divide the $\frac{N_j}{2}$ councilors sampled for the *i*th bootstrap replicate according to the version of the survey-experimental question to which they were assigned (i.e., whether they were assigned to one treatment condition or another). Now, sample $(\frac{N_j}{2})(\frac{1}{2})$ councilors at random *with replacement* from each group. Calculate the difference of mean outcomes in each group, and save this difference.
- After step 6, divide the non-elected brokers (*referentes*) sampled for each bootstrap replicate according to the version of the survey questionnaire to which they were assigned. Now, sample $(\frac{N_j}{2})(\frac{1}{2})$ councilors at random *with replacement* from each group. Calculate the difference of mean outcomes in each group, and save this difference.

Now, the standard deviation of the difference of means across the 1,000 bootstrap replicates estimates the standard error of the estimated PATE. This standard error is calculated separately for councilors and *referentes*, though results may certainly be combined. This procedure should give an accurate assessment of the uncertainty due to random assignment of brokers to different versions of the questionnaire – because analyzing experiments assuming sampling with replacement generates reasonable, though sometimes conservative, standard errors for treatment effects (Freedman, Pisani, and Purves 2007: A32–34, n. 11; Dunning 2012, Appendix 6.2) – while also taking into account the uncertainty introduced by the complex design for sampling brokers from the population.

Two further points may be made about the bootstrap procedure described in this section. First, notice that the sampling fraction k for municipalities may not be an integer, for example, in a province in which we sampled, say, 10 out of 25 municipalities (see step 1 earlier). In this case, we may use the approach described by Bickel and Freedman 1984 and Chao and Lo 1985. In the case of municipalities, let $J_1 < J * k < J_2$ be the two nearest integer multiples of $J * k$. The variance of the bootstrap mean is $F(J * k) = (1 - \frac{n}{J*k})(\frac{J*k(n-1)}{(J*k-1)}s^2$, where s^2 is the variance of responses in the bootstrap population. Now, because $F(\cdot)$ is increasing in its argument, there exists $\alpha \in (0, 1)$ such that

$$F(J * k) = (J_1) + (1 - \alpha)F(J_2). \qquad (1)$$

Chao and Lo 1985 suggested randomizing between J_1 and J_2 with probability α and $1 - \alpha$, respectively, across the different bootstrap replicates.

Second, it is important to note that our procedure will understate sampling variability for brokers who are the only sampled broker from their councilor, because there will be no variance in responses in the bootstrap population. Thus, in cases in which the councilor in truth had two or three brokers, and we sampled one, our bootstrap procedure doesn't represent the sampling variability well. In addition, in cases in which we sampled two brokers in the empirical sample, the councilor may have had more or fewer than six brokers from which we sampled – that is, the true sampling fraction was not exactly one-third. In other words, the inverse of the sampling fraction for brokers may also not be an integer and may not be the same for all councilors. Our procedure thus provides only an approximation to the true sampling variance in these cases. Note, however, that the average number of brokers per councilor, at 5.1, was relatively large.