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## Perceptions of Ballot Integrity and Clientelism

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Argentine elections are often surrounded by claims of electoral manipulation. In every electoral cycle, accusations of clientelism, driving voters to the polls, voter intimidation, and missing or stolen ballots are regularly made months before the election, on Election Day, and in the days after the election. One of the most common allegations is of clientelism, the particularistic distribution of material benefits and favors in exchange for electoral support. According to these allegations, voters get some benefit from a political broker, are driven to the polling station by that same broker, and then vote according to the broker's request. In the months before the 2015 elections, newspapers reported several such stories.<sup>1</sup> These stories, in turn, raise serious questions about the secrecy of the ballot. Although these concerns are rarely raised in public discourse, doubts about ballot secrecy implicitly underlie accusations of clientelism. If citizens are not fully con-

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1. See, for example, "Clientelismo en El Impenetrable: inmersión electoral en la selva de los suplicantes," *La Nación*, September 13, 2015; "Clientelismo en el Norte: un paseo por la feria del extravagante proselitismo tucumano," *La Nación*, August 22, 2015; "Jorge Lanata reveló las maniobras de clientelismo electoral," *Perfil*, August 31, 2015; "Lanata mostró el clientelismo en distintos lugares del país," *Clarín*, August 30, 2015.

vinced that voting is secret, then their vote choices are more easily manipulated and more vulnerable to clientelism.<sup>2</sup> While neither clientelism nor violations of ballot secrecy are considered widespread enough to support allegations of general fraud,<sup>3</sup> several incidents are reported in the press and, more recently, social media during every Argentine electoral cycle. These claims rarely became formal legal accusations though.<sup>4</sup>

Argentine voters' perceptions, or at least the perceptions of *some* voters, seem to be in line with these accusations. Indeed, despite the fact that general trust in the democratic system is high and similar to the most stable democracies in the region (see Lupu, Oliveros, and Schiumerini, Introduction, this vol.), voters' perceptions of electoral malfeasance are far from negligible. When asked in the months before the 2015 election if they believed that voting was secret in Argentina, 32 percent of survey respondents responded negatively, while 3 percent responded that they did not know.<sup>5</sup> An equal proportion, 32 percent, reported that their neighbors were targeted with clientelistic offers.<sup>6</sup> The goal of this chapter is to shed some light on the origins of these perceptions. In particular, are these perceptions related to personal experiences?

Individuals' perceptions and beliefs are, of course, fundamental for electoral outcomes. We know, for instance, that perceptions of party polarization make individuals more likely to form a party attachment (Lupu 2015); perceptions of incumbent corruption may cause voters to punish the incumbent (Ferraz and Finan 2008) or withdraw from the political process (Chong et al. 2015); negative perceptions of the economy affect support for the incumbent (Lewis-Beck and Stegmaier 2000); and, importantly for this study, perceptions of political clientelism and electoral malfeasance affect citizens' electoral behavior. Indeed, doubts about the secrecy of the ballot

2. In 1999, a candidate running for the governorship of the province of Buenos Aires, Graciela Fernández Meijide, famously made this concern part of her campaign. She reminded voters that voting was secret, so she advised them to "take the goods with one hand and vote with the other one" (cited in Szwarcberg 2015).

3. Indeed, according to the expert survey conducted by the Electoral Integrity Project, Argentina is considered among the countries with a high degree of electoral integrity. With a score of 65 out of 100, Argentina scores just higher than the United States (61) and slightly below Chile (67). In the Americas, the highest-scoring countries are Costa Rica (81), Canada (75), and Uruguay (75) (Norris et al. 2017).

4. "Lo que faltó fueron las denuncias," *Página12*, August 8, 2015; "Ningún candidato formalizó denuncias por robo de boletas," *Infobae*, August 9, 2015.

5. All numbers and results throughout this chapter come from APES 2015 (Lupu et al. 2015) and were calculated using post-stratification weights (included in the APES dataset) to adjust for unit nonresponse and attrition based on three demographic characteristics: gender, age, and education.

6. Responses for these questions in the second wave are reported below.

may affect turnout (Gerber, Huber, Doherty, Dowling, and Hill 2013; Gerber et al. 2014)<sup>7</sup> and vote choice (Gerber, Huber, Doherty, and Dowling 2013), while perceptions of clientelism seem to affect the vote choice of the nonpoor (Weitz-Shapiro 2014). But where do these perceptions of electoral manipulation come from? Are these perceptions related to individuals' actual experiences during elections? These are important questions if we are to understand how voters choose their leaders in democracies. Yet political scientists have so far paid little attention to them.

How citizens make voting decisions has important consequences for the nature of representation. Democratic theory presumes that voters' decisions are based on programmatic policy considerations, while accountability models expect them to vote against politicians who underperform. Persistent clientelism and electoral malfeasance undermine these theories of democratic representation. If ballot integrity is not guaranteed, then citizens might fear that a sincere vote may have some negative repercussions, making them abstain (Birch 2010; Gerber et al. 2014; Gerber, Huber, Doherty, Dowling, and Hill 2013) or vote for a different candidate than the one they actually prefer (Gerber, Huber, Doherty, and Dowling 2013). At the same time, doubts about ballot secrecy create a market in which voters might be targeted with positive and negative inducements (promises and threats) that are contingent on vote choice (Mares and Young 2016).<sup>8</sup> And if voters sell their votes in exchange for favors or material benefits, politicians have no reason to take voters' policy preferences into consideration (Stokes 2005; Stokes et al. 2013). Yet we still know little about how clientelism and ballot integrity enter into voters' calculations when casting their ballots. We know that perceptions of clientelism and ballot integrity affect vote choice. But these perceptions may bear little resemblance to reality.

This chapter leverages a combination of different measures of electoral malfeasance, taken twice over the course of the 2015 Argentine presidential campaign, to uncover the relationship between personal experiences and perceptions of ballot integrity and clientelism. The 2015 election in Argentina is a particularly good setting for studying these issues: accusations of political clientelism and electoral manipulation both played prominent roles during the campaign. The panel structure of the 2015 Argentine Panel Election Study (APES) allows me to study perceptions and personal

7. Similarly, using data from the Comparative Study of Electoral Systems (CSES) from 33 countries, Birch (2010) finds that those who believe that elections are not conducted fairly are less likely to vote.

8. On the difference between positive and negative inducements, see Mares and Young (2016, 268–71).

experiences before and after the election. And since the 2015 election in Argentina resulted in a transfer of power, transforming the opposition voters from “losers” into “winners,” it provides a particularly good environment for exploring the relationship between perceptions and experiences.

Are perceptions of ballot secrecy and clientelism based on personal experiences? Are those who believed that voting is not secret before the election the same as those who believed it afterward? Are those who reported having experienced and/or witnessed clientelistic offers before the election the same as those who reported it afterward? Are the individual-level correlates about clientelism and ballot secrecy stable over time? Are the correlates the same for personal experiences and perceptions? To explore these questions, I use survey data from the 2015 APES. APES conducted two national waves of interviews: the first one before the mandatory primaries and the second one right after the runoff election between Daniel Scioli, representing the incumbent party, and the eventual winner, Mauricio Macri.<sup>9</sup> I exploit the panel design of APES to study perceptions and reported personal experiences with electoral malfeasance before and after the 2015 presidential election in Argentina. I focus on two particularly salient issues: clientelism and ballot secrecy.

## Perceptions of Electoral Manipulation and Expectations

### *Clientelism*

The literature on clientelism has grown substantially in the last decade. Many scholars have provided insights into how clientelism works, its causes and consequences, what sustains clientelistic political relationships, and whether these arrangements are efficient (e.g., Calvo and Murillo 2013; Stokes et al. 2013; Szwarcberg 2015; Weitz-Shapiro 2014). Recently, novel methods have allowed scholars to get more accurate estimates of the extent of clientelism across different countries (e.g., Calvo and Murillo 2013; González-Ocantos et al. 2012; González-Ocantos, Kiewiet de Jonge, and Nickerson 2015). Yet, we still know very little about how clientelism affects

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9. According to Argentine electoral laws, all parties must select their presidential candidates in primaries that are mandatory for both parties and voters. In 2015, that election took place on August 9. The presidential election took place on October 25, and Scioli got 37 percent of the votes while Macri got 34 percent. Because no candidate obtained more than 45 percent of the votes, there was a runoff election on November 22, which was won by Macri. For more details on the 2015 electoral cycle, see Calvo (this vol.).

the voting behavior of clients and potential clients, let alone how *perceptions* about clientelism are formed and affect the voting behavior of non-clients.

Scholars continue to debate the effectiveness of clientelism. Given the almost universal secrecy of the ballot in electoral democracies, we might expect clients to promise their vote in return for material benefits and favors from politicians but then renege on their side of the bargain in the voting booth. Some scholars have argued, however, that clientelism works because norms of reciprocity motivate clients to follow through on their commitments (Finan and Schechter 2012; Lawson and Greene 2014). Others have argued that clients believe that their vote can be observed even in the presence of the secret ballot and that, as a result, they fear punishment (e.g., Brusco, Nazareno, and Stokes 2004; Stokes 2005; Weitz-Shapiro 2014). Still others have argued that voters may choose to support clientelistic politicians primarily because they want to keep the continuous flow of benefits, which requires the politician to remain in office (Oliveros 2016b; Zarazaga 2014, 2015). Empirical findings remain mixed and contradictory.

Less studied is the effect of clientelistic practices on the perceptions and voting behavior of non-clients. Can perceptions about clientelism have an effect on the vote choice of non-clients? Where do these perceptions come from? With the exception of Weitz-Shapiro (2014), these questions have received no attention. Using a survey experiment, Weitz-Shapiro finds that nonpoor citizens, who are less likely to be part of clientelistic exchanges, are less likely to support politicians who engage in clientelism. But while her survey experiment is useful for isolating the effect of perceptions about clientelism on political attitudes, it leaves unanswered the question of how citizens form these perceptions. After all, nonpoor voters are typically not approached by politicians with offers of clientelistic exchanges, nor do they witness politicians engaging in clientelism (which typically takes place in poor neighborhoods). How, then, do they learn about clientelism? Where do these perceptions come from? What types of voters perceive higher levels of clientelism?

### *Ballot Secrecy*

The 1912 electoral reform made voting secret, universal, and mandatory for all Argentine males over 18 years of age.<sup>10</sup> Since then, the secret ballot

10. Law 8871, known as the Sáenz Peña Law, established secret, compulsory, and universal suffrage for male citizens over 18 years of age. The goal of the introduction of compulsory and secret voting (suffrage was already universal for men) was to increase levels of participa-

in Argentina has been well established.<sup>11</sup> However, regardless of whether voting is actually secret, people's perceptions about it seem essential to understanding political behavior.<sup>12</sup> Citizens' confidence in the secrecy of the ballot is surprisingly low across both young and advanced democracies. For instance, in their study of ballot secrecy perceptions in the United States, Gerber, Huber, Doherty, and Dowling (2013) found that 25 percent of respondents did not believe their ballot choices are kept secret. Kiewiet de Jonge and Nickerson (2014) found widespread uncertainty about ballot secrecy in countries like Honduras (52 percent), Nicaragua (36 percent), and Uruguay (33 percent).<sup>13</sup> For the case of Argentina, in a survey conducted in 2012, they found that 13 percent of the respondents did not believe their own ballots were secret and 7 percent were not sure (Kiewiet de Jonge and Nickerson 2014).

At the same time, doubts about ballot integrity are particularly troubling in contexts like the Argentine one, in which clientelism is thought to be a common practice. The practice is presumed to be so widespread that clientelism in Argentina has received greater attention from scholars than that in any other single country.<sup>14</sup> According to a substantial part of the literature, clientelistic exchanges are based on fear of the punishment that clients expect to receive if they fail to deliver their votes as requested (e.g., Brusco, Nazareno, and Stokes 2004; Kitschelt and Wilkinson 2007a; Stokes 2005; Stokes et al. 2013; Weitz-Shapiro 2014). Monitoring voting behavior, or making clients believe that monitoring is possible, becomes then fundamental to making clientelistic exchanges work. Indeed, prior studies of US elections from 1860 to 1930 (Kuo and Teorell 2017) and Chilean

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tion, reduce electoral corruption, and encourage the formation of opposition parties (Alonso 1996). For more on the 1912 reform, see Botana (1985, 217–345).

11. This, of course, does not mean that other types of electoral manipulation were not used. See, for instance, Cantú and Saiegh (2011) for an interesting analysis of electoral fraud committed in Argentina during the so-called Infamous Decade (1931–41).

12. To distinguish between actual ballot secrecy and people's beliefs about the issue, Gerber, Huber, Doherty, and Dowling (2013, 78) propose the concept of "psychologically secret": the ballot is considered psychologically secret "when the voter believes that election administration is such that her ballot choices are secret."

13. The exact wording of their question is as follows: "Do you believe that the government or the parties can discover for whom you have voted?" (Kiewiet de Jonge and Nickerson 2014).

14. See, among others, Auyero (2001); Brusco, Nazareno, and Stokes (2004); Calvo and Murillo (2004, 2013); Camp (2017); Giraudy (2007); Kemahlioglu (2012); Levitsky (2003); Lodola (2005); Nichter (2008); Oliveros (2016a, 2016b); Remmer (2007); Stokes (2005); Stokes et al. (2013); Szwarcberg (2012, 2015); Weitz-Shapiro (2012, 2014); and Zarazaga (2014, 2015).

elections after 1958 (Baland and Robinson 2007) show that improvements in the protection of ballot secrecy reduce clientelism.<sup>15</sup> If clientelism actually works through monitoring vote choice and punishing non-compliers, then citizens' beliefs about the secrecy of the ballot become central.<sup>16</sup> If the secrecy of the ballot is taken for granted, on the other hand, then either clientelism is not possible or clientelistic exchanges need to be sustained on something other than monitoring and the fear of punishment.

But what explains variation in perceptions about ballot secrecy? Recent literature on perceptions about electoral integrity more broadly has considered three types of arguments: electoral institutions, voters' partisan affiliations or electoral preferences, and voters' sociodemographic characteristics. The first group of explanations has focused on factors such as the electoral system (e.g., Anderson et al. 2005; Birch 2008), the funding of political parties (e.g., Birch 2008), and the type of voting technology (e.g., Alvarez et al. 2013). The second group of explanations follows Anderson et al.'s (2005) theory of the winner-loser gap, which posits that the outcome of the election produces different levels of trust in the electoral process for winners and losers. Along these lines, Anderson et al. (2005) in the United States and Cantú and García-Ponce (2015) in Mexico found strong partisan effects: electoral losers tend to have more negative perceptions of the integrity of the electoral process.<sup>17</sup> The third group of explanations has focused on voters' demographic and socioeconomic characteristics. For example, Gerber, Huber, Doherty, and Dowling (2013) in the United States and Kiewiet de Jonge and Nickerson (2014) in Latin America found a negative correlation between socioeconomic status and confidence in ballot secrecy.<sup>18</sup>

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15. In both cases those improvements were caused by the introduction of the Australian ballot, which makes state authorities responsible for printing ballots that include all candidates. Before the introduction of the Australian ballot, and up to this day in Argentina, ballots were/are printed by the political parties themselves and voters could/can bring the paper ballots to the polling station. For more on the effects of improving ballot secrecy, see Mares and Young (2016, 274–75) and Teorrel, Lehoucq, and Ziblatt (2017).

16. As noted, not all the literature on clientelism agrees that monitoring voting behavior is what makes clientelism work. For an alternative explanation based on reciprocity, see Finan and Schechter (2012) and Lawson and Greene (2014). For another alternative based on self-interest, see Oliveros (2016b) and Zarazaga (2014, 2015).

17. In contrast, in her cross-national study, Birch (2010) finds no evidence of a winner-loser gap.

18. Gerber, Huber, Doherty, and Dowling (2013) also found that Hispanics and blacks in the United States tend to distrust ballot secrecy more than whites.

### *Expectations*

Most of these studies, however, are based on voters' perceptions of electoral malfeasance (both clientelism and violations of ballot integrity) measured at one specific point in time.<sup>19</sup> But there are good reasons to suspect that measures taken *before* the election capture expectations about the electoral process, while measures taken *after* the election capture both the experience of the electoral process as well as "the disappointment or approval of voters with regard to the outcome of the election" (Cantú and García-Ponce 2015, 1). One would expect voters' perceptions to be based on experiences and thus present an accurate picture of the integrity of the electoral process. As mentioned above, however, prior research has found that perceptions of electoral integrity are often affected by voters' personal characteristics (Gerber, Huber, Doherty, and Dowling 2013; Kiewiet de Jonge and Nickerson 2014) and by the outcome of the election itself (Anderson et al. 2005; Cantú and García-Ponce 2015).

Are perceptions of electoral manipulation, then, related to experiences, personal bias, or postelection emotions? Based on the existing literature and to shed some light on the relationship between personal experiences and perceptions, this chapter focuses on the following hypotheses. First, I take advantage of the fact that APES asked about both experiences and perceptions of electoral integrity during the 2015 election, so I simply compare individual correlates across questions. If perceptions were informed by personal experiences, then one would expect to find the same individual correlates across questions. Alternatively, if perceptions were not based on personal experiences, then the opposite should be observed.

Second, if perceptions were informed by experiences, the actual experience of voting in the 2015 election could affect postelection perceptions. Measuring perceptions *after* the election would then provide an estimate of the performance of the electoral administration, either reinforcing or changing prior perceptions. However, there was no evidence of widespread electoral manipulation during the 2015 election; nor was there evidence that the election was different from previous elections. Thus, there is no reason to expect that the 2015 electoral experience itself would change perceptions about the integrity of the electoral process.

Alternatively, perceptions of electoral integrity after the election could be biased by the outcome of the election (Anderson et al. 2005; Cantú and

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19. An important exception is Cantú and García-Ponce (2015), who study perceptions of electoral integrity at different stages of the 2012 Mexican presidential election.

García-Ponce 2015). Regardless of the actual experiences of voters during the 2015 election, electoral losers may have more negative perceptions of the integrity of the electoral process. In line with the winner-loser gap theory, I expect Scioli voters (the losers of the election) to have a more negative perception of the integrity of the electoral process after the election.

Finally, as noted, much of the existing literature on clientelism argues that clientelism survives despite ballot secrecy because voters often *believe* that their vote is not secret (e.g., Brusco, Nazareno, and Stokes 2004; Stokes 2005; Weitz-Shapiro 2014).<sup>20</sup> When clients believe that their votes could be monitored, then the punishment of non-compliers becomes possible and clientelism works. If clientelism in fact works this way, and perceptions are based on experiences, doubts about ballot integrity should be positively correlated with clientelism. Alternatively, if clientelistic exchanges are not sustained by fear of monitoring and punishment, or perceptions are not actually based on personal experiences, there should not be an association between clientelism and beliefs about ballot integrity.

### Empirical Strategy

To study personal experiences with and perceptions of clientelism and ballot secrecy, I use survey data from the 2015 APES. APES conducted two national, face-to-face waves of interviews, the first between June 24 and August 7 (before the primaries on August 9) and the second between November 23 and December 30 (after the runoff election between Scioli and the winner, Macri).<sup>21</sup>

To address problems of social desirability bias and get a reliable estimate of the extent of clientelistic offers in Argentina, the 2015 APES included three different questions about clientelism. First, following González-Ocantos et al. (2012), the survey included a list experiment, a useful technique for estimating the incidence of sensitive behaviors.<sup>22</sup> The logic of this technique is very simple. First, the survey sample is randomly split into treatment and control groups. Respondents in each group are read the same question and shown a list with a different number of response

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20. Note that this literature pays little attention to whether questions about ballot secrecy are asked before or after an election takes place.

21. Twelve interviews (1 percent) that were conducted on November 21 (the day before the runoff election) were dropped. See the methodological appendix in Lupu, Oliveros, and Schiumerini (this vol.) for further details about the survey.

22. For general advice on how to design list experiments, see Glynn (2013).

options. List experiments work by integrating the item we care about (the “sensitive” item) into a list of other items. Thus, the list presented to the treatment group has one more response option, the sensitive item, than the list presented to the control group.<sup>23</sup> The question does not ask respondents to tell the enumerator about specific activities but rather only to indicate *how many* of those activities they were involved with, so the question provides the respondents with full anonymity. The sensitive item in this case was “Received any material benefit—like clothes or food—or personal favor from a political broker.”<sup>24</sup>

The survey also included two direct questions about clientelism. The first one asked respondents directly if they had received any material benefit or personal favor (*Personal clientelism*).<sup>25</sup> The second one inquired about whether the respondent’s neighbors have received any benefit or favor (*Neighborhood clientelism*).<sup>26</sup> Although I use this question to measure perceptions of clientelism, I acknowledge that it is far from ideal. Responses to this question could reflect actual knowledge of whether the neighbors were targeted with clientelistic offers as well as preconceptions or perceptions of the existence of such offers in the neighborhood. In addition, it might not be a good measure for capturing the perceptions of the middle class, who live in non-poor neighborhoods where clientelism is less prevalent. Even if the neighborhood clientelism question is imperfect, it is reasonable to assume that it better captures perceptions about clientelism than the personal clientelism question, which clearly relates to personal experience.

To measure perceptions and experiences with ballot integrity, the APES included two questions. The first one aimed to capture general perceptions

23. The randomization was programmed into the tablets so respondents only saw the intended items, and the order of the items on the lists was rotated at random to prevent order effects.

24. The list experiment wording was as follows: “Now I am going to show you a list where various activities related to politics are listed. I would like for you to tell me HOW MANY of those have you done this year. Do not tell me which ones, only HOW MANY.”

Saw campaign posters  
 Talked about politics with someone  
 Received any material benefit—like clothes or food—or personal favor from a political broker (treatment item)  
 Saw campaign ads on TV and radio  
 Was a candidate for political office

25. The question asked, “During this year, did you receive any material benefit—like clothes or food—or a personal favor from a political broker?”

26. The question asked, “During this year, did your neighbors receive any material benefit—like clothes or food—or a personal favor from a political broker?”

about ballot secrecy (*General perception*).<sup>27</sup> The second, more specific question was designed to capture more personal experiences with violations of ballot integrity (*Personal experience*).<sup>28</sup>

To capture the winner-loser gap, the main explanatory variable is *Scioli* (the loser of the election). For wave 1 (before the primary election), this variable takes the value of 1 for those who reported having an intention to vote for Scioli in the primary election and 0 otherwise. For wave 2 (after the runoff election), the variable *Scioli* takes the value of 1 for those who reported having voted for Scioli in the runoff election and 0 otherwise.<sup>29</sup>

### Personal Experience with and Perceptions of Clientelism

How widespread is clientelism in Argentina? Who are the targets of clientelistic offers? Who perceives higher levels of clientelism? What are the individual correlates of personal and perceived clientelism before and after the election? Figure 10.1 presents the percentages of respondents who reported having received a benefit or favor themselves (top bar charts) and those who reported that their neighbors had received such a benefit (bottom bar charts). The first bar in each chart presents results from the survey conducted before the primaries (wave 1), while the second bar in each chart presents results from the survey conducted after the runoff election (wave 2). The left bar charts include all respondents, while the right bar charts include only those who participated in both waves.

Figure 10.1 shows a rate of self-reported clientelism that ranges between 1.7 percent and 3.4 percent, depending on the wave and the respondents included. When asked a similar question in 2001–2 in a survey conducted in the provinces of Buenos Aires, Córdoba, and Misiones, 7 percent of people said they had received gifts (Brusco, Nazareno, and Stokes 2004).<sup>30</sup> Nevertheless, as shown by González-Ocantos et al. (2012), we know that

27. The question asked, “In general, do you believe that voting is secret in Argentina?”

28. The question asked, “And thinking specifically about someone like you, who votes at a school in your neighborhood, do you think that local political operatives, political parties, or the government can find out how that person votes?” Note that polling stations in Argentina are set up in schools.

29. Alternative specifications based on the outcome of the October general election yield basically the same results.

30. Note that this survey was conducted in the midst of the biggest economic crisis in Argentine history, so more material distribution would have been expected. I thank Vicky Murillo for pointing this out.

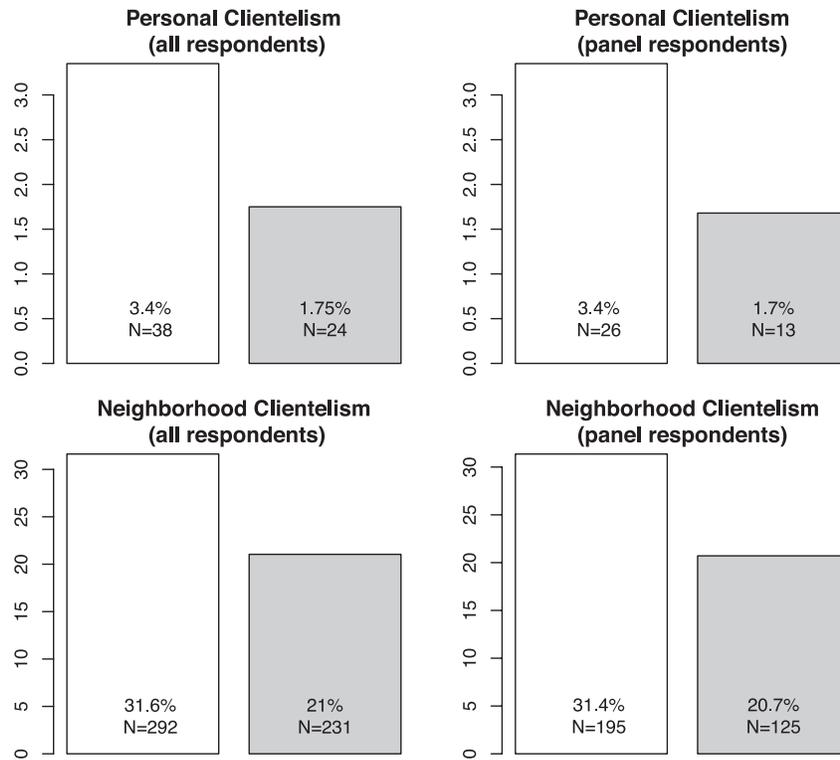


Fig. 10.1. Personal and Neighborhood Clientelism, across Waves  
 Note: Top charts correspond to *Personal clientelism*; bottom charts correspond to *Neighborhood clientelism*. First bars (in white) correspond to wave 1, while second bars (in gray) correspond to wave 2. DN/NA were coded as missing. All numbers calculated using weights.

voters might not be willing to admit having *personally* received benefits.<sup>31</sup> When survey respondents were asked whether parties had distributed goods in their neighborhoods before the primary (wave 1), 31.6 percent of respondents ( $N = 292$ ) reported that political brokers made favors or gave things out to their neighbors during the year. When asked the same question after the runoff election (wave 2), 21 percent ( $N = 231$ ) reported the same.<sup>32</sup> For comparison, in the survey conducted by Brusco, Nazareno, and

31. In the survey they conducted in 2008 in Nicaragua, 2.4 percent of respondents admitted receiving a gift or favor personally when asked directly, while the list experiment estimate was 24 percent (González-Ocantos et al. 2012).

32. Except for three respondents in wave 1 and four respondents in wave 2, all those who reported *Personal Clientelism* also reported *Neighborhood Clientelism*.

Stokes (2004) in 2001–2, 44 percent of respondents reported that parties gave things out to individuals in their neighborhoods during the campaign. Restricting the sample to panel respondents (right charts) yields similar numbers. So, despite the fact that both questions asked about clientelism “during this year,” and thus we should not observe a decline, responses about clientelistic offers went down over the course of the campaign.

It is possible that changes in responses across waves are related to measurement issues. Indeed, questions about clientelism (particularly in the case of *Personal clientelism*) are socially sensitive, so respondents may not be willing to provide honest answers when asked directly. This, in turn, may cause instability of answers across waves. To address this problem, the APES included a third question about personal clientelism, using a list experiment to reduce social desirability bias. The list experiment estimates do not show the decrease in personal clientelism observed in the direct questions. For wave 1, the estimated percentage of respondents receiving a gift or a favor during the year is 11 percent (although only significant at the 90 percent level). For wave 2, the estimated percentage of respondents receiving a favor or a gift during the year is 15 percent (significant at the 99 percent level). When restricting the sample only to those respondents who participated in both waves, the estimated percentage of respondents receiving a gift or a favor is 17 percent for wave 1 and 14 percent for wave 2 (significant at the 99 percent and 95 percent level, respectively).<sup>33</sup> These list experiment estimates are in between the ones obtained from the direct questions—significantly higher than the personal measure but smaller than the neighborhood one. This is in line with the expectations of Brusco et al. (2004), who suggested that the “true” level of clientelism may lie somewhere in between the self-reported individual measure and the neighborhood one.<sup>34</sup>

Going back to the direct measures of clientelism, the observed changes across waves may also reflect changes in individual postelection perceptions (or willingness to report) rather than real changes in the frequency of clientelistic transactions. The election (or the outcome of the election) may have biased ex post reporting of clientelism when asked directly.<sup>35</sup> To

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33. These estimates were all calculated with two-sample *t*-tests with unequal variance (see table 1 in the online appendix). Unfortunately, the APES did not include a similar list experiment to measure neighborhood clientelism.

34. This is different from what González-Ocantos et al. (2012) find in the case of Nicaragua, in which the list experiment estimate (24.3 percent) is higher than both the self-reported measure (2.4 percent) and the neighborhood one (17.9 percent).

35. Note that the list experiment estimates (which do not show a significant decline in

explore the sources of this bias and the stability (or instability) of responses across waves, I conducted a multivariate analysis using both direct measures of clientelism as the dependent variable for each wave of the APES (fig. 10.2).<sup>36</sup> To capture the winner-loser gap, the main explanatory variable is *Scioli*, taking the value of 1 for those who reported having an intention to vote for Scioli in the primary election (wave 1) or having voted for Scioli on the runoff election (wave 2) and 0 otherwise.<sup>37</sup> To test for the relationship between clientelism and general beliefs about ballot integrity, *Ballot secrecy* (Yes = 1) is included.<sup>38</sup> All four models also include controls for *Relative Wealth* (1–5) and *Political Knowledge* (0–3), as well as controls for *Female* (Female = 1), *Age* (1–5), and level of *Education* (0–5) (not reported in fig. 10.2).<sup>39</sup> Personal characteristics (*Female*, *Age*, *Education*, and *Relative wealth*) were measured during wave 1 for panel respondents and during wave 2 for the refreshment sample. *Political knowledge* and *Ballot secrecy* were measured in both waves. Figure 10.2 presents the results from a series of regressions in which the dependent variables are the two direct measures of clientelism in wave 1 (left panel) and wave 2 (right panel). Black circles indicate results from regressions using the self-reported experience measure of clientelism (*Personal clientelism*), while white circles use the more general perception

clientelism across waves) are an indirect measure of personal clientelism, not neighborhood clientelism.

36. I also conducted ordinary least squares regressions with the list experiment count as the dependent variable. Following González-Ocantos et al. (2012) and González-Ocantos, Kiewiet de Jonge, and Nickerson (2015), the models include a dummy variable indicating the treatment assignment (i.e., the list experiment condition), interactions between this variable and all the independent variables, as well as noninteracted versions of all the variables. The estimates for clientelism are derived from the interacted coefficients, while the noninteracted coefficients provide estimates for the items in the control list. Unfortunately, while this method yields unbiased estimates (Imai 2011), it is quite inefficient. Probably because of this, none of the variables of interest achieved statistical significance, and because the standard errors were significantly bigger than for the other measures, including these results in the same graph made it hard to understand. These results are reported in tables 4 and 5 in the online appendix.

37. Respondents who reported that they were not planning to vote ( $N = 38$ ) in wave 1 and those who reported that they did not vote in the runoff election in wave 2 ( $N = 114$ ) were coded as missing.

38. This question is the more general one: “In general, do you believe that voting is secret in Argentina?”

39. *Political knowledge* (0–3) is based on correct answers to three questions: the name of the Argentine minister of the economy, the number of Argentine provinces, and the name of the Brazilian president. *Education* (0–5): without formal education or incomplete primary (0), complete primary (1), incomplete secondary (2), complete secondary (3), incomplete tertiary or university (4), and complete tertiary or university (5). *Relative wealth* (1–5) is measured in quintiles of a factored index constructed from a series of questions about household assets. *Age group* (1–5): 18–25 (1), 26–35 (2), 36–45 (3), 46–55 (4), and more than 55 (5).

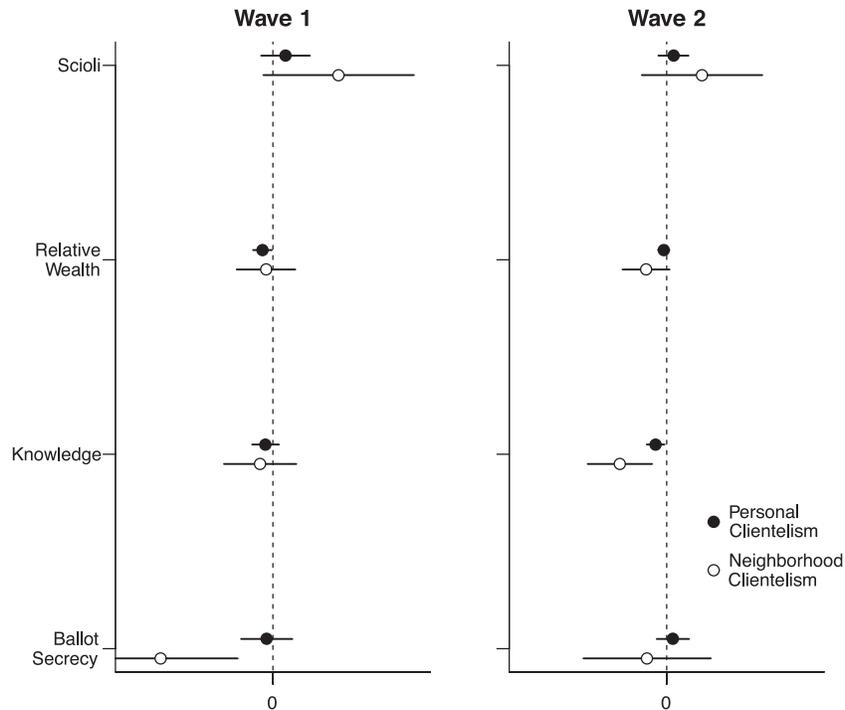


Fig. 10.2. Individual Determinants of Personal and Neighborhood Clientelism, across Waves

Note: The panel on the left presents the results from wave 1; the panel on the right presents the results from wave 2. Black circles indicate results for regressions using *Personal clientelism* as the outcome variable; white circles indicate results from regressions using *Neighborhood clientelism* as the outcome variable. All models were estimated using weights and include controls for gender, age, and education (not reported). Horizontal bars represent 95% confidence intervals (robust standard errors). Results from logit models were essentially equivalent, so OLS results are reported for simplicity.

measure (*Neighborhood clientelism*). All models are ordinary least squares (OLS) regressions with robust standard errors, estimated using weights for age, education, and gender.<sup>40</sup>

First, across both waves, results on *Personal clientelism* suggest that benefits are distributed to all types of voters.<sup>41</sup> Contrary to conventional wisdom

40. Tables 2 and 3 in the online appendix contain the numeric values represented in the figure.

41. Using partisanship as the main exploratory variable instead of vote choice (in the same model) yields similar results. Those who identify themselves with the Peronist party (both Peronism and Frente para la Victoria) are not more likely than the rest of the population or partisans from other parties to be targeted with clientelistic offers.

and previous findings (e.g., Brusco, Nazareno, and Stokes 2004), Peronist voters or, at least, Scioli voters in the 2015 election do not seem to be disproportionately targeted by clientelistic offers. In terms of perceptions, results across waves show no relationship between voting for Scioli and believing/reporting that neighbors received benefits.<sup>42</sup> In contrast to the predictions of the winner-loser gap theory, the losers of the election are not more likely to report higher levels of clientelism in their neighborhoods.

Although most of the existing literature finds a strong and negative correlation between income and clientelism (e.g., Calvo and Murillo 2004; Kitschelt and Wilkinson 2007b; Stokes et al. 2013),<sup>43</sup> figure 10.2 shows a much weaker relationship. Indeed, while always negative, the relationship only achieves statistical significance (at the 95 percent level) for the measure of *Personal clientelism* in wave 1. The poor are more likely to report being offered material benefits and favors before but not after the election, and, surprisingly, they are not more likely to report that their neighbors were the targets of clientelistic offers.<sup>44</sup>

*Political knowledge*, in turn, is not correlated to either of the measures of clientelism before the election (wave 1) but is negatively and significantly correlated with both measures after the election (wave 2). Respondents with less political knowledge are more likely to report both personal clientelism and clientelism among their neighbors, but only after the election is over.

Finally, believing that voting is not secret is strongly correlated with *Neighborhood clientelism* before the election, but not with *Personal clientelism* (wave 1). In other words, before the election, those respondents who reported having themselves been the target of clientelistic offers are not more likely to express doubts about ballot integrity, which seems to go against the assumptions of the vast literature that associates clientelism with monitoring and punishment (e.g., Brusco, Nazareno, and Stokes 2004; Stokes 2005). Respondents who are more likely to believe that voting is not secret report that their neighbors (but not necessarily themselves) received

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42. Using partisanship as the main explanatory variable instead of vote choice yields similar results. Those who identify themselves with the Peronist Party are more likely than the rest of the population to report that their neighbors received clientelistic offers before the election (wave 1). This relationship, while still positive, loses statistical significance after the election (wave 2). However, when compared to other partisans, Peronists are not more likely to report *Neighborhood clientelism* in either wave.

43. For a discussion of the relationship between poverty and clientelism, see Stokes et al. (2013, chap. 6).

44. There is some variation across different model specifications; see table 2 and table 3 in the online appendix.

favors or material benefits—a perception that is not necessarily based on personal experience. I return to this point in more detail in the next section. After the election, beliefs about ballot secrecy, while still negative, are no longer significantly correlated with *Neighborhood clientelism*, while *Personal clientelism* is still not correlated with ballot integrity after the election. Those who reported being the targets of clientelistic offers are not more likely to express doubts about the secrecy of the ballot, either before or after the election.

In sum, although the models across waves do not necessarily exhibit contradictory results, some of the statistically significant relationships found in wave 1, before the election, disappear in wave 2, after the election, while new ones appear. The only consistent (null) finding across waves and measures is that Scioli voters are neither more nor less likely to be targets of clientelistic offers, nor are they more or less likely to perceive more clientelism in their neighborhoods.<sup>45</sup> In terms of other predictors of reported clientelism, the fact that the same measure taken at two different points in time yields somewhat different results suggests that perceptions of clientelism might not be informed by personal experiences. This should be a call for caution. As González-Ocantos et al. (2012) argue, researchers should be extremely careful when measuring sensitive issues. Perceptions of clientelism are not necessarily based on personal experiences, so scholars should be cautious when drawing conclusions from indirect proxies such as neighborhood clientelism. Moreover, my findings show that the timing of a survey is also extremely consequential. Responses seem to be highly dependent on both *how* and *when* the variables are measured.

#### Personal Experience with and Perceptions of Ballot Secrecy

Surprisingly for a country where the secrecy of the ballot has been well established for decades, many of the respondents on the 2015 APES were not that confident about the integrity of the electoral process. When asked before the election if they believed that voting was secret in Argentina, 32 percent of respondents replied negatively and 3 percent said that they did not know (fig. 10.3). When asked the same question after the election, the proportion of respondents who questioned ballot secrecy decreased to 29

45. This result is consistent across different model specifications. See table 2 and table 3 in the online appendix.

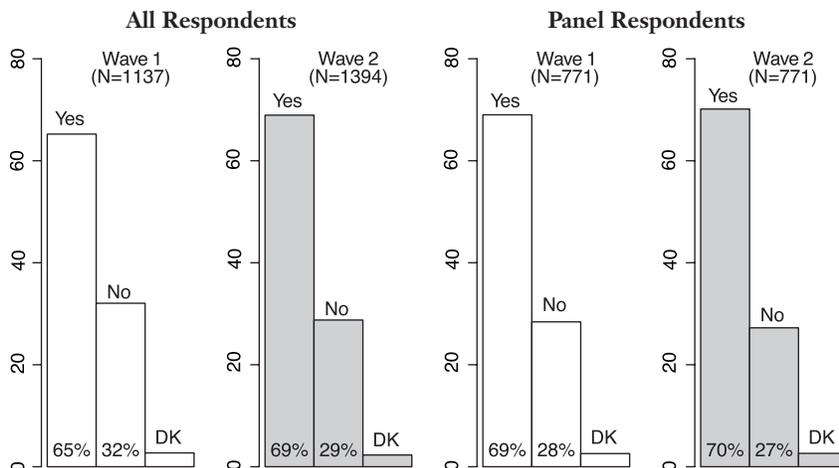


Fig. 10.3. Beliefs about Ballot Secrecy, before and after the Election  
 Note: All numbers and percentages calculated using weights.

percent. Restricting the sample to panel respondents (the two plots on the right) yields a similar picture.

Figure 10.3 shows a surprisingly high number of citizens questioning the secrecy of the ballot. Where are these perceptions coming from? A second question asking respondents to think more carefully about their personal experience, about “someone like you, who votes at a school in your neighborhood,” provides a more accurate measure of respondents’ personal experiences.<sup>46</sup> This alternative question shows that, at least in some cases, the responses to the general question were not based on personal experience. Indeed, of the 364 respondents who replied that voting was not generally secret during wave 1, 30 percent replied that for the specific case of someone like them, voting in their neighborhood, it was not possible for activists, parties, or the government to find out about vote choice; 7 percent replied that they did not know; and 64 percent replied that indeed it was possible to find out about voting behavior. In other words, this perception seems to be based on a personal or close experience for only 64 percent of those who believe that voting was not secret in general. A similar pattern is found when comparing the responses on general perceptions about ballot integrity and personal experience during wave 2.<sup>47</sup>

46. Recall the wording: “And thinking specifically about someone like you, who votes at a school in your neighborhood, do you think that local political activists, political parties, or the government can find out how that person votes?”

47. Of the 400 respondents who replied that voting was not secret during wave 2, 25 per-

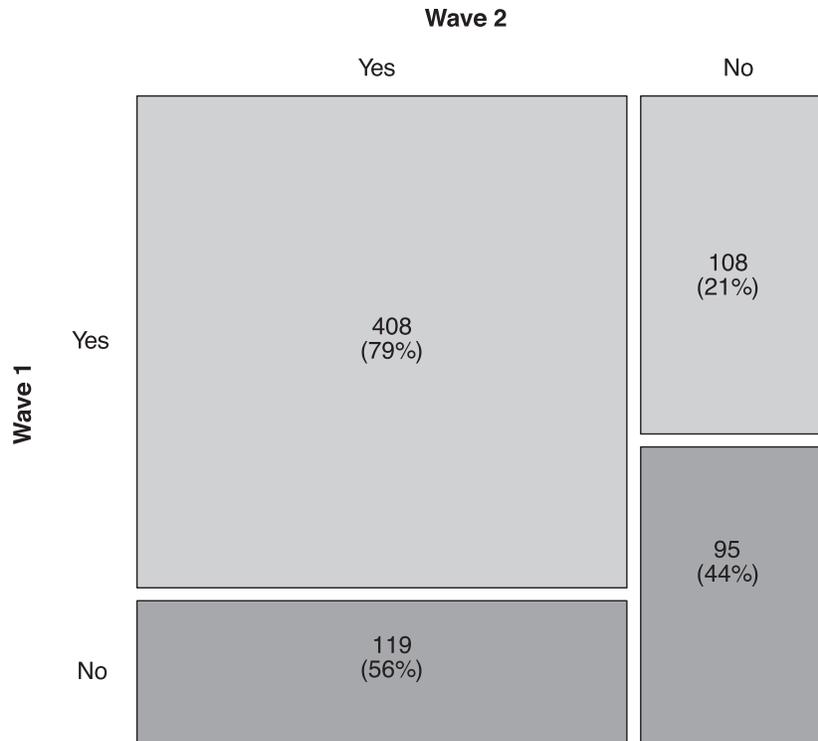


Fig. 10.4. Changes in Beliefs about Ballot Secrecy, Panel Respondents  
 Note: Figure excludes respondents that answered DN/NA on wave 1 or wave 2. All numbers and percentages calculated using weights.

As in the case of clientelism, the descriptive statistics suggest that, at least for some respondents, perceptions about the integrity of the ballot are not based on personal experiences. Where are these perceptions coming from? To explore this further, figure 10.4 presents the responses from before and after the election for the group of respondents that participated in both waves.

Of the 516 who reported before the primary (wave 1) that voting was in fact secret, 21 percent changed their minds and questioned the secrecy of the ballot after the runoff election (wave 2). Among the 214 who reported before the election that they did not believe that voting was secret (wave 1), 56 percent reported the opposite after the election (wave 2). Figure 10.4

cent replied that, for someone like them voting in a school like the one they vote at, it was not possible to find out about vote choice; 3 percent replied that they didn't know; and 72 percent replied that indeed it was possible to find out about voting behavior.

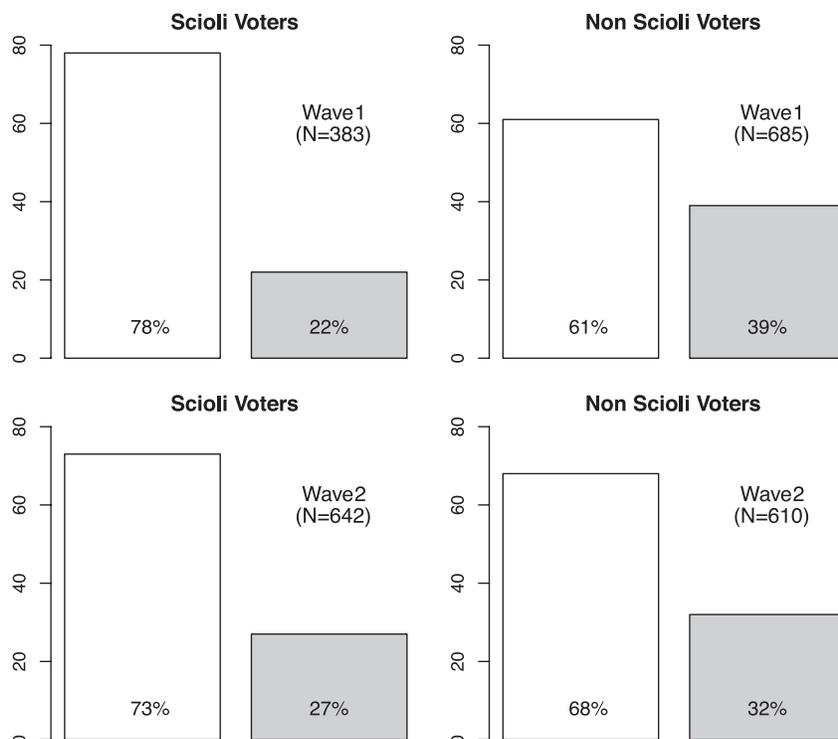


Fig. 10.5. Beliefs about Ballot Secrecy, by Vote Choice  
 Note: DN/NA were excluded. All figures were calculated using weights.

then shows that, at least for some respondents, the election (or the outcome of the election) changed perceptions of ballot secrecy. As noted, this could be related to the actual experience of voting, in which case perceptions about ballot integrity after the election are shaped by the electoral process itself. But, following Anderson et al. (2005), it could also be evidence of a winner-loser gap. Maybe those who became losers in the election changed their perceptions about the process precisely because they lost. I explore these two alternatives by focusing on the individual determinants of beliefs about ballot integrity before and after the election.

As a first look at these differences, figure 10.5 displays beliefs about ballot secrecy by vote choice. Top panels (wave 1) display beliefs about ballot secrecy for those who reported intentions to vote for the incumbent candidate and the eventual loser, Scioli, in the primaries (top-left chart) and those who did not (top-right chart); bottom panels (wave 2) display beliefs about ballot secrecy for those who reported having voted

for Scioli in the runoff election (bottom-left chart) and those who did not (bottom-right chart).<sup>48</sup>

Figure 10.5 shows that before the election (wave 1), Scioli voters responded significantly differently than non-Scioli voters. About 78 percent of Scioli voters responded that voting was indeed secret (top-left panel), compared to 61 percent among non-Scioli voters (top-right panel); this 17 percentage point difference is significant at the 99 percent level. When voters were asked the same question after the election (now measuring Scioli voters based on their vote on the runoff), the difference between the groups is reduced to a non-significant 5 percentage points.<sup>49</sup> This evidence seems to point to the existence of a winner-loser gap. Indeed, the outcome of the election, in which Scioli lost, seems to have changed voters perceptions. To further explore this possibility, I now turn to regression analysis.

Figure 10.6 presents the results from a series of regressions in which the dependent variables are the two different measures of beliefs about ballot secrecy in wave 1 (left panel) and wave 2 (right panel). *General perception* (“In general, do you believe that voting is secret in Argentina?”) takes values of 1 (Yes, voting is secret) and 0 (No, voting is not secret).<sup>50</sup> To facilitate the comparison, *Personal experience* also takes values of 1 (Yes, voting is secret) when the answer is “No, it’s not possible to find out” and 0 (No, voting is not secret) when the answer is “Yes, it is possible to find out.”<sup>51</sup> As in the previous section, the main explanatory variable is *Scioli*, taking the value of 1 for those who reported having an intention to vote for Scioli in the primary (wave 1) or having voted for Scioli in the runoff election (wave 2) and 0 otherwise. To explore the relationship with clientelism, both measures of clientelism are included: *Personal clientelism* (1 = Yes) and *Neighborhood clientelism* (1 = Yes). All models also include controls for *Relative wealth* (1–5) and *Political knowledge* (0–3), as explained before, as well as controls for *Female* (1 = Yes), *Age* (1–5), and level of *Education* (0–5) (not reported in the figure). Personal characteristics (*Female*, *Age*, *Education*, and *Relative wealth*) were measured during wave 1 for panel respondents and

48. Using the vote in the general election instead of the vote in the runoff election yields similar percentages.

49. The same pattern can be observed when restricting the sample to panel respondents only. Using intention to vote for Scioli in the primary instead of voting for Scioli in the runoff election (for panel respondents) also yields similar results.

50. Don’t Know / No Answer responses were coded as missing.

51. “And thinking specifically about someone like you, who votes at a school in your neighborhood, do you think that local political activists, political parties, or the government can find out how that person votes?”

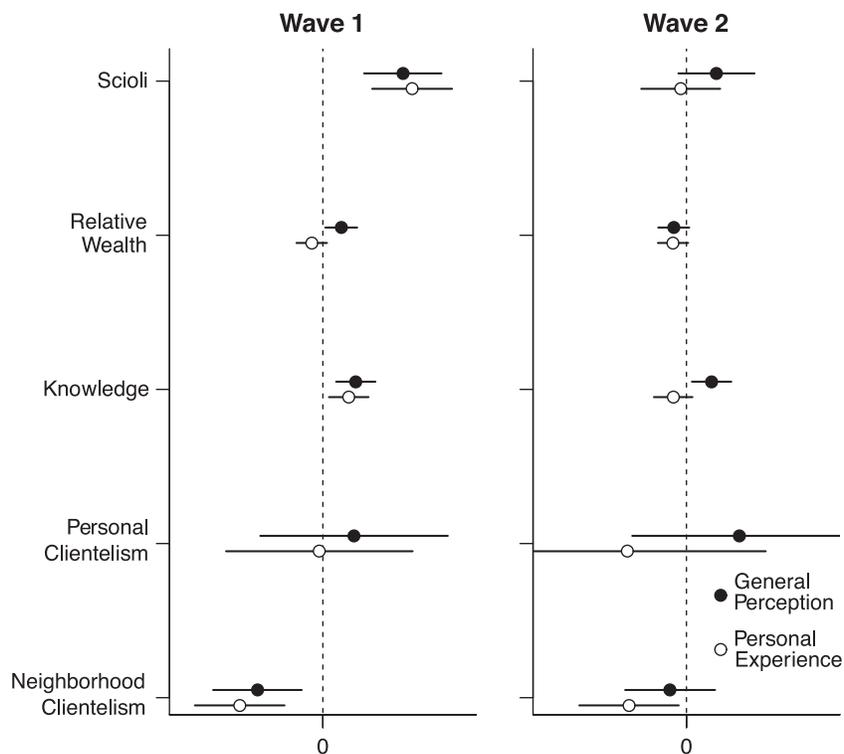


Fig. 10.6. Individual Determinants of Beliefs about Ballot Secrecy, across Waves  
 Note: The panel on the left presents the results from wave 1; the panel on the right presents the results from wave 2. Black circles indicate results from regressions using *General perception* as the outcome variable; white circles indicate results from regressions using *Personal experience* as the outcome variable. All models were estimated using weights and include controls for gender, age, and education (not reported). Horizontal bars represent 95% confidence intervals (robust standard errors). Results from logit models were essentially equivalent, so OLS results are reported for simplicity.

during wave 2 for the refresh sample. Political knowledge and both variables for clientelism were measured again for all respondents in wave 2.<sup>52</sup> Black circles indicate results from regressions using the general perception of ballot secrecy (*General perception*), while white circles use the more personal one (*Personal experience*). All models are OLS regressions with robust standard errors and estimated using weights for age, education, and gender.

First, and in line with expectations, figure 10.6 shows evidence of a significant winner-loser gap. Indeed, Scioli voters changed their perceptions of ballot integrity after losing the election. During wave 1 (left panel),

52. The online appendix contains the numeric values represented in the figure.

those who reported having intentions to vote for Scioli on the primary show higher levels of trust in ballot secrecy. Across both measures of ballot secrecy, *Scioli* is both substantively and statistically the most important factor in explaining beliefs about ballot secrecy before the election. To illustrate, consider a female Scioli voter between the ages of 36 and 46 who holds a high school degree, has a middling level of wealth (3), possesses medium political knowledge (2), and does not report personal or neighborhood clientelism. She has a 79 percent predicted probability of believing that voting is secret (measured as *General perception*). For a non-Scioli voter with the same characteristics, the predicted probability of believing that voting is secret decreases to 64 percent.<sup>53</sup> However, once the election is over (wave 2, right panel) and their candidate has lost, Scioli voters are no longer more likely to believe in the integrity of the electoral process than non-Scioli voters. Note that Scioli was the candidate of the incumbent party, so the positive correlation (in wave 1) may also indicate that Scioli voters had more confidence in the government running a clean election. They apparently lost some of this confidence after the election, either because of their experience while voting or, more likely (since there were no serious allegations of fraud), because their preferred candidate had lost.

Second, in wave 1 (left panel) and in line with the findings presented in the previous section, those voters who reported that their neighbors received clientelistic offers in the last year (*Neighborhood clientelism*) are less likely to believe that voting is in fact secret, according to both the general perception measure and the more personalized one. Consider again the modal voter: a 36-to-46-year-old female Scioli voter with a high school degree, medium relative wealth, medium political knowledge, and who does not report personal or neighborhood clientelism. Recall that she has a 79 percent predicted probability of believing that voting is secret (measured as *General perception*). The predicted probability of believing that voting is secret decreases to 67 percent for a voter with the same characteristics but who reports neighborhood clientelism.<sup>54</sup> This goes in line with the findings of Kiewiet de Jonge and Nickerson (2014), who found that in 10 of the 13 Latin American countries in their study, neighborhood clientelism

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53. Predicted probabilities were calculated by holding all categorical variables at their sample modes and all ordered variables at their sample medians. Using the *Personal experience* measure yields similar results: 83 percent for the Scioli voter versus 67 percent for a non-Scioli voter with the same characteristics.

54. Predicted probabilities were calculated by holding all categorical variables at their sample modes and all ordered variables at their sample medians. When using the *Personal experience* measure, the difference between a modal voter who reports neighborhood clientelism and one who does not is slightly larger: 68 percent versus 83 percent.

was positively and significantly (at the 95 percent level) related to doubting ballot secrecy. In Argentina, they found that 82 percent of respondents who reported not witnessing clientelism believed in ballot secrecy, compared to only 59 percent of those who reported having witnessed clientelism.<sup>55</sup> Results from the 2015 APES (wave 1) show a similar pattern, with a slightly smaller difference: 69 percent of respondents who reported not witnessing clientelism believed in ballot secrecy (*General perception*) compared to 57 percent who reported witnessing clientelism (a significant 12 percentage point difference).

However, and also in line with the results presented in the previous section, personal experience with clientelism (*Personal clientelism*) is not significantly correlated with beliefs about ballot secrecy for either measure. The fact that the neighborhood measure of clientelism is significantly correlated with doubts about the secret ballot, while the personal measure is not, indicates that those doubts are not informed by personal experiences with clientelism. This result is also consistent with Kiewiet de Jonge and Nickerson (2014), who found statistically significant correlations between individual clientelism and ballot secrecy in only a few countries, and the relationship was not significant when pooling all the countries in the sample together. After the runoff election (wave 2, right panel), *Personal clientelism* remains not associated with beliefs about ballot integrity using either of the measures, while *Neighborhood clientelism* remains negatively associated with beliefs in ballot integrity only for the more personalized measure of trust in ballot secrecy (*Personal experience*); in wave 2 it is now undistinguishable from zero for the general measure (*General perception*).

Third, in wave 1 (left panel), political knowledge (*Knowledge*) is positively and significantly correlated with both measures of ballot secrecy, while in wave 2 (right panels) the effect remains only for the general perception measure, but not for the more personalized one. As mentioned, ballot secrecy is well established in Argentina, so it is expected that those with more political knowledge would believe that voting is in fact secret.

Finally, before the election (wave 1) richer voters were more likely to believe that voting is generally secret (*General perception*), while the estimate for *Personal experience* is statistically indistinguishable from zero. After the election (wave 2), however, the relationship between income and ballot secrecy seems negative for both measures, although not significant.

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55. The difference between those who witnessed clientelism and those who did not, when pooling the data across all 13 countries, is a significant 12 percentage points (Kiewiet de Jonge and Nickerson 2014, 20).

### Discussion

Argentine elections are frequently surrounded by allegations of electoral malfeasance—the 2015 electoral cycle was no exception. And these accusations, as shown here, seem to align with the perceptions of some voters. The results in this chapter, however, suggest that those perceptions bear little resemblance to reality. By focusing on ballot integrity and clientelism, this chapter shows that, in both cases, perceptions do not seem to be informed by personal experiences. The variation across measures and waves indicates changes in perceptions that were not generated by changes in voters' actual experiences with electoral malfeasance. This is consistent with recent studies of corruption that find no relationship between personal experiences of corruption and perceptions of corruption among politicians (Klašnja and Tucker 2013; Klašnja, Tucker, and Deegan-Krause 2016).<sup>56</sup>

Are changes in perceptions, then, a product of the outcome of the election? Do losers tend to have a more negative perception of the electoral process after losing the election? In the case of clientelism, there is no evidence of a winner-loser gap: Scioli voters were not more likely to report clientelism after the election. In contrast, perceptions about ballot secrecy do seem to be affected by the outcome of the election. Scioli voters were more likely than non-Scioli voters to believe that voting was secret before the election but not after their candidate lost.

In terms of how clientelism works, the lack of a significant correlation across waves and measures between personal experiences with clientelism and skepticism about the secret ballot casts doubt on how most of the literature understands clientelism. According to several scholars (e.g., Brusco, Nazareno, and Stokes 2004; Kitschelt and Wilkinson 2007a; Stokes 2005; Stokes et al. 2013; Weitz-Shapiro 2014), for a clientelistic exchange to take place, the patron should be able to identify non-compliers (or make clients believe that this is possible) and credibly commit to punishing them. The results in this chapter challenge the claim that monitoring and the threat of punishment are intrinsic components of clientelistic exchanges. Indeed, if those exposed to clientelism do not think that monitoring is possible, then either clientelistic exchanges are not enforceable or the logic of these exchanges has to be something other than monitoring and fear. Interestingly, in most cases, perceiving clientelism in the neighborhood does cor-

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56. Corbacho et al. (2016), however, find evidence of a different type of relationship between personal experience with corruption and corruption perceptions. Using an information experiment, they find that learning about rising levels of corruption in the country makes citizens more willing to pay a bribe.

relate with doubts about the secrecy of the ballot. That the neighborhood measure of clientelism is statistically correlated with doubts about the secret ballot, while the personal measure is not, indicates that those who express doubts about ballot secrecy are not informed by personal experience with clientelism.

Elections in Argentina might be a lot cleaner than a naive spectator would believe from following the 2015 campaign. Despite the perceptions of some voters, electoral malfeasance seems to be less pervasive than one would expect from the space it occupies in public discussion. Why then would the opposition and the media spend time and energy on these allegations? One possibility is that the opposition uses these accusations to present themselves as the “clean” option against the incumbent. This campaign strategy was particularly appealing in the 2015 election, in which the incumbent Peronist party (a party more traditionally identified with the lower classes) was running against a center-right party that was perceived to represent the middle and upper classes. We know that non-poor voters dislike clientelism more than poor voters and are more willing to punish politicians who rely on it (Weitz-Shapiro 2014). Consequently, if non-poor voters are more prone to punish electoral manipulation, and Macri voters were in fact more affluent than Scioli voters (as shown by Lupu, this vol.), accusations of electoral malfeasance could have been a rational strategy for the opposition.

Would such accusations work as an electoral strategy? Although this question is beyond the scope of this chapter, existing research suggests that the answer might be negative. Literature on corruption has recently emphasized the role of the source of information in affecting individual political beliefs and behavior (Botero et al. 2015; Weitz-Shapiro and Winters 2017; Winters and Weitz-Shapiro 2016). If accusations of electoral malfeasance are part of campaigning, different parties might present different pictures that reinforce the existing bias and preconceptions of their own supporters but fail to convince opposition voters. As an electoral strategy, then, allegations of electoral malfeasance might not be very effective.