Working for the Machine:

Patronage Jobs and Political Services in Argentina

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Why does the control of patronage increase an incumbent's chances of staying in power?[1] What do public employees do that might affect electoral competition? What motivates public employees to do it? This article describes what public employees do that might affect electoral outcomes and provides an explanation of why they do it. Across a vast number of cases, from the United States[2] to Italy,[3] Mexico,[4] Ghana,[5] and Argentina,[6] scholars have argued that patronage jobs help keep machines in power. Patronage jobs are assumed to be distributed to an incumbent's supporters in exchange for political services—such as helping with campaign events and mobilizing voters—that are essential for attracting and maintaining electoral support.[7] Thus, conventional wisdom posits that controlling patronage significantly increases an incumbent's chances of winning elections and staying in power. However, almost no systematic evidence details the political services that patronage employees provide in exchange for their jobs. There is no precise assessment of the types of services that are being provided, which employees provide these services, or the extent of this practice in public administration. Moreover, there is not a sound explanation as to why public employees provide these services.

Consistent with the general understanding in the literature, I argue that patronage jobs are distributed to supporters in exchange for political services. Besides doing their regular jobs, these supporters are expected to provide political services for the incumbent who hired them. Patronage jobs then provide incumbents with a "free" army of political workers. However, a citizen who receives a public sector job with the understanding that she will provide political support in return can easily renege on her side of the contract after getting the job. Why would public employees uphold their end

of the contract and provide political services even after receiving the benefit of the job? How can the patron make sure not to "waste" jobs on citizens who will not fulfill their side of the patronage contract? Existing explanations are based either on reciprocity (clients comply with the agreement because they want to help the person who has helped them) or threat of punishment (clients comply because they are afraid the patron will cut off the benefit if they fail to do so).

Departing from these explanations, I argue that patronage contracts are self-sustaining without reciprocity or the threat of punishment because incumbents distribute patronage jobs to supporters whose fates are tied to that of the incumbent who hires them. Public sector jobs (and, importantly, working conditions) enjoyed by supporters will be maintained by the incumbent but not by the opposition. This is because supporters of the incumbent cannot credibly commit to provide political services for the opposition. Supporters, then, have strong incentives to provide political services to help the incumbent stay in power, which makes their original commitment to provide these services a credible one. This alignment of interests between patrons and clients (or incumbents and patronage employees) makes patronage contracts incentive compatible, and therefore self-sustaining over time.

I test the empirical implications of the theory using a face-to-face survey of about 1,200 local public sector employees fielded from three Argentine municipalities. Using list experiments, I show that a considerable proportion of employees—particularly supporters—provide political services. To establish why public employees provide these services, I use two survey experiments that allow me to identify employees' comprehension of the likely effect of a change in the administration. The results strongly support the empirical expectations: public employees believe that their jobs are tied to the political success of the incumbent.

Patronage Contracts and Commitment

In contexts of weak civil service rules, as is the case in most Latin American countries,[8] the ability to discretionally appoint public sector workers provides incumbents with a powerful tool that can be used for political gain.[9] Patronage employees are expected to vote for the patron who appointed them, but the type of support that is expected in exchange for a public sector job goes far beyond electoral support. Based mainly on ethnographic work, existing studies suggest that political bias in hiring gets translated into political services.[10] Indeed, patronage employees in mid- and low-level positions—the focus of this article—are often involved in campaigning, organizing, and/or attending political meetings and rallies, mobilizing voters, and providing favors to citizens, among other activities. However, there is no systematic evidence of the provision of political services by public employees, likely because obtaining reliable data on these activities is extremely difficult. Using innovative techniques that provide anonymity and thus generate more reliable answers, this article focuses on three of these services: helping with campaigns, attending rallies, and monitoring elections.

Public employees under patronage contracts provide invaluable services to their patrons. However, patronage contracts are not easy to sustain. First, since the law cannot be used to enforce the contracts, they must be self-enforcing.[11] Second, since the exchange is sequenced, a citizen who provides political services with the expectation of getting a public sector job is always at risk of facing a politician who can decide not to hire her after she has already provided the services. Alternatively, a citizen who receives a job with the implicit or explicit understanding that she will provide political services can easily decide not to comply with her side of the agreement after getting the job.[12]

This article focuses on the second type of exchange, in which political support is provided after the benefit is received. In this case, incumbents are at risk of "wasting" jobs on citizens who, once hired, will not comply with their side of the agreement. Why would they comply after receiving the benefit of the job? The literature so far has provided two main answers to this question: norms of reciprocity and threat of punishment.[13] According to the first set of theories, clients fulfill their side of the agreement because they want to help those who have helped them. Receiving a benefit engenders feelings of obligation and gratitude, and clients help the patron because of these feelings.[14] From this perspective, public employees comply with the agreement and provide political services because they want to reciprocate the incumbent for their jobs.

According to the second line of arguments, clients comply because they are afraid that the patron will cut off their benefits if they fail to do so. Much of the contemporary literature has focused on the monitoring and commitment problems that are associated with this understanding of clientelism.[15] From this perspective, the defining feature of clientelistic exchanges is that they are contingent on the client's behavior. If the client does not behave according to her patron's wishes—which requires either the patron's ability to monitor or the client's belief that this is possible—the patron has the power to punish the client by withdrawing or withholding the benefit. For patrons to be able to ensure that the political support associated with the benefit is provided, they should be able to credibly commit to punish non-compliers (and/or reward compliers). Thus, the commitment problem is solved on the basis of fear of punishment. Employees provide services because they are afraid that the patron will cut off the job otherwise.

Following some insights from the continuation value aspect of Robinson and Verdier's model, this article presents a different solution to the commitment problem that arises in clientelistic agreements. I argue that it is neither reciprocity nor fear of punishment that ensures that public employees uphold their part of the deal; it is the fact that their fates are tied to the political fate of their patron. In this theory of self-enforcing patronage, clients' compliance with patronage agreements is ensured by the fact that their incentives are aligned with those of their patron—both patron and clients will benefit from the patron's success. Of course, this is not to say that fear of punishment or feelings of reciprocity are never present in clientelistic exchanges, or that they are not possible. Rather, I demonstrate here that neither of these two factors are necessary characteristics of these arrangements.[16]

A Theory of Self-Enforcing Patronage

What makes patronage contracts self-enforcing in the absence of the threat of punishment or feelings of reciprocity is that patronage jobs are distributed to supporters whose fates are tied to the political survival of their patron.[17] In the absence of robust civil service rules, patronage employees believe that if the incumbent loses the election, their own jobs could be in jeopardy. This provides a major incentive to help the incumbent stay in power, thus making their original commitment to provide political services a credible one. Since the interests of patronage employees are aligned with those of the incumbent—both want the politician to stay in office—the commitment problem associated with the non-simultaneity of the clientelistic exchange disappears and patronage contracts become self-enforcing.[18]

Politicians want to distribute jobs to those who are most likely to provide political services. Finding this type of employee, however, is not an easy task. Potential patronage employees can promise future compliance, but absent the threat of punishment for non-compliers or feelings of reciprocity, the provision of political services has to be incentive compatible for this promise to be credible. Only when potential employees can credibly commit to providing support in the future are patronage contracts self-sustaining without punishment and reciprocity. All potential employees can promise to provide political services in the future, but only supporters can make these promises credible. Patronage jobs held by supporters will be maintained as they are by the incumbent, but not by a competing politician. Supporters then have strong incentives to help the incumbent stay in power, which makes their commitment credible.

If politicians could somehow know *ex ante* how potential employees would behave once hired, then the commitment problem associated with the sequenced nature of the patronage agreement would of course disappear. Full information about the intentions of potential employees would prevent strategic defection and solve the commitment problem for politicians. While the intention of potential employees to provide political services once hired is private information, their declared (or perceived) political preferences regarding the politician at the time of hiring are not. When hiring is mainly conducted through informal channels—as is the case in countries with weak civil service systems—politicians can use that information for their own benefit.

Politicians then use referrals as well as personal and partisan connections to screen potential clients and to separate perceived supporters from non-supporters.[19] Supporters might like the politician or the politician's party for ideological or personal reasons, or because of past or expected benefits. They might have connections with the party or they might just be faking support to obtain benefits. In other words, perceived support is not about ideological affinity but rather about proximity to the politician's network. In line with Calvo and Murillo, who show that citizens' perception of the likelihood of being offered a public sector job increases among Argentineans who are more connected to partisan networks,[20] I argue that those citizens who are closer to these networks, those whom I call supporters for simplicity, will be the ones that the politician

chooses to hire. Citizens can make efforts to be visible to the patron (i.e., help with the campaign or attend political rallies) and thus make sure they are identified as supporters.[21] As explained by a Salta employee: "There are people who wait to get hired . . . they work for years and years, and the only thing they want is a job. . . . If they performed, showed up on time, if they attended the rallies . . . yes, it is possible."[22] Those who demonstrate support are more likely to comply with the agreement and provide the promised services. Note that whether those who demonstrate support are sincere (ideological) supporters or strategic actors with materialistic goals does not affect the empirical implications of the theory. For reasons developed below, being a "real" supporter or just pretending to be one creates the same incentive-compatible patronage contracts. Politicians use perceived preferences at the time of hiring as a proxy for citizens' future likelihood of providing political services. Empirically, then, we should observe public sector jobs disproportionally distributed to supporters.

This expectation is consistent with studies on clientelism and patronage in Argentina as well as with my own research. Most recent studies argue that politicians allocate resources based on preexisting partisan linkages with the beneficiaries of those resources, especially in the case of patronage jobs.[23] Indeed, data from the survey of public employees described in the following section show considerable discretion and political bias in hiring decisions in the Argentine public administration. Around 64 percent of respondents reported having found their job through an acquaintance, a friend, or a relative, and the majority of respondents (59 percent) considered personal connections to be "important" or "very important" for getting a job.[24] Proximity to partisan networks, however, does not necessarily mean ideological affinity. In their survey of Argentinean voters, Calvo and Murillo find no correlation between ideological self-placement (on a left-right scale) and distributive expectations regarding public jobs.[25] Data from the public employees' survey show a similar pattern. Public employees reported that personal connections were more important than partisan affiliation and political ideology as hiring criteria in the administration.[26]

Yet, the fact that public jobs are disproportionally distributed to perceived supporters does not solve the commitment problem. Perceptions could be misleading. Once citizens expect the distribution of public jobs on the basis of perceived political preferences, they have an incentive to get closer to the partisan network and misrepresent their preferences. Supporters are often neither completely partisan (or ideological) nor completely opportunistic (only motivated by their own welfare), but rather are some combination of both. Moreover, partisan preferences might be endogenous to the patronage exchange itself. In the words of Diaz-Cayeros et al.: "partisan attachments are constructed through reciprocal material and symbolic exchanges, past, present, and future."[27] As an employee bluntly explains: "I've told you that I got this job through politics, but the thing is that I was politically active *in order to* get a job."[28] Citizens can pretend to have certain political preferences to get hired, change their minds about their preferences, or simply reduce the effort they are willing to devote to political work. Since the exchange is sequenced, patronage employees still have the possibility of not complying with their side of the agreement.

Being a supporter—or, more accurately, being perceived as one—is not in itself enough to guarantee compliance with the patronage contract.

What makes patronage contracts self-enforcing is public employees' belief that their jobs are tied to their patron's political survival. Why do patronage employees believe that they could lose their jobs or suffer negative effects on their working conditions with a new administration? Perceived political preferences at the time of hiring have the same effect as the public pledges discussed by Kitschelt and Wilkinson: those whose support for the incumbent is publicly known "are effectively then cut off from any expectation of rewards if the opposition should win."[29] Since the distribution of patronage jobs is based on perceived political preferences (or proximity to the partisan network), once citizens are hired as supporters, their genuine political preferences do not matter anymore. Their perceived political preferences dictate the treatment they will get from the opposition. Whether patronage workers are motivated by ideology, opportunism, or—more frequently—some combination of both, they will be branded and treated as true supporters by the opposition. Using the same logic that the incumbent applied to hire her supporters, a new incumbent will want patronage jobs to be distributed to those more likely to provide political services for her. Only her supporters can credibly commit to do that in the future, so current employees will be replaced, demoted, or sidestepped. Once branded as an incumbent's supporters, patronage employees have low expectations for keeping their jobs and working conditions if the opposition were to win. Supporters with patronage jobs understand that it is in their best interest to help the incumbent remain in power. It is this alignment of interests between patrons (politicians) and clients (employees) that makes patronage contracts self-sustaining.[30]

In this theory of self-enforcing patronage, public employees comply with patronage contracts because they believe that it is in their best interest to provide political services to keep their patron in power. More precisely, employees believe that it is in their best interest that political services are provided by someone so the incumbent gets re-elected. What then is the individual's incentive to contribute? As with other public goods (i.e., the re-election of the incumbent) that depend on collective contributions (i.e., political services), there may be a temptation for each individual actor to let others expend the effort. Because the benefit is non-excludable, both those who comply with the patronage contract and provide political services and those who do not will equally enjoy the benefit of keeping the patron in power.

There are, however, at least three factors that reduce the likelihood of widespread free riding in the case of patronage. First, the benefit at stake (a job) might be important enough to provide a significant incentive for cooperation.[31] When the benefit is this large, more people are willing to pay the cost (provide services) to make sure that the desirable outcome will actually happen. Second, the cost of cooperation is not necessarily high. Political services are often provided during regular working hours, so the choice of public employees is not between, for instance, attending a rally and staying at home, but between attending a rally and performing their regular "on-the-job" duties. Third, individual contributions are not necessarily individually irrelevant to the

outcome. While, for instance, it may have little impact whether one particular individual attends a rally, the cooperation of an individual in charge of mobilizing many others to the rally may be quite important to its success.

The argument advanced in this article is related to Robinson and Verdier, who note that promises of public-sector employment tie "the continuation utility of a voter to the political success of a particular politician."[32] The theory of self-enforcing patronage, however, departs from their formal model in a couple of fundamental ways. First, in their model, the continuation value aspect is not enough to guarantee compliance: "(politicians) must be able to use policies that tie the continuation utility of a voter to their political success, or alternatively, if behavior is observable, allow voters to be punished if they renege on the exchange."[33] Reversibility (as a threat for punishment) is, for them, key to solving the commitment problem.[34] This also explains, in their view, why (true) supporters are the beneficiaries of jobs. To reduce the moral-hazard problem and solve the commitment issue, jobs need to be distributed to true believers so the "patron can observe their effort with relatively high probability."[35] In the theory outlined here, in contrast, reversibility only matters because employees believe that a new incumbent could fire or demote them, which creates the incentive to try to help keep the current incumbent in office, without any need for monitoring and punishment.[36]

The theory of self-enforcing patronage has two main empirical implications that are tested in the following sections. First, if patronage jobs are in fact disproportionally distributed to supporters in exchange for political services, we should observe that supporters are more involved than non-supporters in the actual provision of these services. Second, if supporters are more involved in the provision of services, it is because they believe that their jobs are tied to the incumbent's political success. Thus, I expect public sector employees who are supporters to be more afraid than non-supporters of a new politician replacing the incumbent. Importantly, and departing from existing accounts, it is not just the reversibility of public jobs that matters; the possibility of changes in working conditions such as being demoted or sidestepped with a new administration also creates incentives for patronage employees to comply.

The Empirical Strategy

Despite patronage being a widespread phenomenon, the difficulty in collecting systematic data means that we know very little about what public employees do, and why they do it. The approach I take in this article allows me to elicit accurate information from the actors involved while minimizing social response bias. I use an original survey of about 1,200 public sector employees that incorporates different strategies for encouraging truthful responses.

Case Selection Survey data were gathered in face-to-face interviews of 1,184 low- and mid-level local public sector employees in three Argentinean municipalities.[37]

While the patterns described in this article are common to many countries without robust civil service systems, Argentina—infamous for its clientelistic politics—offers a particularly good setting in which to study patronage. First, it lacks stable civil service rules and has a large public sector with "well-developed patronage systems."[38] Moreover, its extensive decentralization results in significant variation in the size and characteristics of public employment across provinces and municipalities. Finally, while some provincial regulations apply, control over local personnel is the exclusive responsibility of local governments.

To conduct the survey, I selected three similarly sized, but very distinct municipalities: Salta (Salta), Santa Fe (Santa Fe), and Tigre (*Conurbano Bonaerense*, Buenos Aires). Although the municipalities were not selected at random, they are illustrative of the diverse economic and political realities of the country, providing a good opportunity to study patronage across different environments.[39] By including a municipality from the poorer north dominated by the Peronist Party (Salta), a municipality from the relatively richer and more competitive center governed by the Radical Party (Santa Fe), and one from the Peronist *Conurbano Bonaerense* (Tigre), the area most infamous for its clientelistic politics, I intended to capture the regional diversity of Argentine politics.

The Survey   Together with a team of research assistants, we interviewed around 400 employees in each municipality.[40] I generated a random sample based on the official list of public employees (excluding elected officials and high-level positions).[41] The selected employees were then approached at public offices during working hours.

Asking directly about political services is problematic because respondents could refuse to answer or provide untruthful responses, especially if they think that their jobs could be jeopardized by their answers. To get around this problem of social desirability bias, increase the response rate, and produce more valid estimates, two distinct but complementary strategies were implemented. First, I use list experiments, a technique specially designed to study sensitive issues. Second, I follow Scacco's strategy and split the questionnaire into two parts.[42] Part A of the survey instrument contained the less sensitive questions and the list experiments described in the next section. Part B had the sensitive questions about political preferences and behavior. Each part of the questionnaire was marked with a different identification number that could only be matched with a document not available to the enumerators. Apart from this number, the second part of the questionnaire had no information that could be used to identify the respondent. Enumerators administered Part A of the questionnaire, while Part B was read and filled out by the respondents themselves. In this way, the other employees in the office were not able to hear the questions or the answers. Finally, the respondents were asked to store Part B in a sealed cardboard box similar to a ballot box.[43]

List Experiment Technique   The logic of the list experiment technique is straightforward.[44] First, the survey sample is split into random halves: a treatment and a control group. Each group is read the same question and shown a card with the response options.[45] Cards for each group differ only in the number of response

categories. List experiments work by aggregating the item of interest (the treatment) with a list of other items. Respondents are asked to report the number of items on the list that applies to them, but not which ones. The question does not ask respondents to mention specific activities, only how many of those activities they did. Thus, as long as respondents understand that the anonymity of responses is protected, list experiments generate more accurate responses and more valid estimates than direct questioning.[46] Since respondents were randomly assigned to the treatment and control groups, as long as the randomization was successful, the two groups would be identical, on average, on both observable and unobservable characteristics.[47] Therefore, an estimate of the proportion of respondents providing services can be obtained by comparing the average responses across groups.

Patronage Contracts and Political Services: Evidence from List Experiments

According to the theory of self-enforcing patronage, patronage contracts are distributed to supporters in exchange for political services. Politicians choose to hire supporters because their commitment to provide political services in the future is credible. This section presents empirical evidence that public employees—particularly supporters—do in fact provide these services. Using list experiments, I estimate the proportion of employees who (1) help with campaigns, (2) attend rallies, and (3) monitor elections.[48]

Electoral Campaigns   Since local politicians in Argentina have limited resources to finance professional campaigns through extensive use of the media,[49] participation of supporters is crucial. At the local level, "human-intensive" activities such as painting graffiti, plastering posters, and door-to-door visits are still essential parts of campaigning. Moreover, the availability of "real" volunteers has considerably decreased over the years, making the role of public employees even more important.

   One common activity among parties involves door-to-door campaigning—an activity that is usually accompanied by the distribution of paper ballots, the same ones that voters will find at the voting booth. Argentina does not use the Australian ballot (i.e., there is no standard official ballot with all candidates). In contrast, and although the government is in charge of the distribution of the ballots on Election Day, each political party is responsible for producing its own ballots. The distribution of these ballots in the weeks before the election is crucial. It helps voters get to know the candidates and familiarize themselves with the ballot they intend to use on Election Day. This could be key to finding the preferred option at the voting booth, which often contains an overwhelming number of ballots. The distribution of paper ballots before the election has also been related to vote buying.[50] Other important campaign activities include painting graffiti, hanging banners, and plastering posters. These are an essential part of campaigning, especially for local politicians who cannot afford other, more expensive types of advertising on billboards.[51] In addition, campaigning in Argentina usually

involves organizing meetings with neighbors and other activities such as seminars, social gatherings, and cultural events.

Political Rallies   Argentine political parties invest a lot of time and effort in organizing rallies. A crucial part of the organization effort is making sure that enough people will show up; qualitative research has shown that public employees are expected to turn out to rallies.[52] Rallies continue to play a number of important roles in Argentine politics. First, they serve the straightforward purpose of advertising and allowing candidates to display their power to voters and other politicians. Second, attendance at rallies is considered a way for potential clients to show loyalty to the party—an opportunity to publicly display support.[53] Finally, the number of followers whom each broker can mobilize to rallies provides party leaders with important information about the power of each broker. [54]

Monitoring Elections   Parties in Argentina consider the presence of party representatives in polling stations (partisan monitors) on Election Day essential to guaranteeing fair elections. Each party has the right to assign a head of partisan monitors by school (where polling stations are located), plus a monitor by voting booth. The electoral law also requires three polling station officials by voting booth selected by the government to monitor the election and count the votes.[55] Although these official monitors are the only ones with legal authority to decide on electoral issues, parties consider it crucial to have their own monitors protecting their votes. Monitors are also in charge of ensuring that enough ballots from their party are in the booth throughout the day. Accusations of missing ballots are frequent in Argentinean elections, and the conventional wisdom posits that parties steal other parties' ballots from the voting booth. Official monitors are in charge of ensuring that this does not happen, but parties consider having their own monitors essential to preventing their ballots from being stolen. Finally, partisan monitors are also considered to be essential to monitor turnout and vote buying.[56]

Table 1 presents the list experiments estimates where the treatment categories are "Work/help in the electoral campaign," "Attend political rallies," and "Be an election monitor."[57] Respondents in the control group report their average number of activities as 1.19, 1.39, and 0.93, respectively, while the average in the treatment group is 1.41, 1.60, and 1.05, respectively. List experiments generate an accurate estimate of the proportion of employees involved in each of these activities: 22 percent reported helping with the campaign, 21 percent reported attending political rallies, and 12 percent reported monitoring the election (all estimates are significant at the 99 percent level).[58] Thus, this section provides the first systematic evidence that public employees do indeed provide political services and offers an unbiased estimation of the rate at which they provide them in Argentina, one of the most studied cases in the literature on clientelism.

Heterogeneous Treatment Effects   According to the theory of self-enforcing patronage, a higher proportion of supporters should be involved in the provision of political services. To determine whether the provision of services differs across

Table 1 Political Services: List Experiments Estimates

	Electoral Campaign	**Rallies**	**Monitoring**
Treatment	1.41 (0.04) N=587	1.60 (0.04) N=586	1.05 (0.03) N=585
Control	1.19 (0.03) N=582	1.39 (0.04) N=584	0.93 (0.02) N=587
Treatment effect	0.22*** (0.05) N=1169	0.21*** (0.06) N=1170	0.12*** (0.04) N=1172

Note: Standard errors in parentheses. Two-sample t-test with unequal variance.
*** p<0.01, ** p<0.05, * p<0.1.

supporters and non-supporters, I estimate the difference-in-means across these subgroups. Support for the mayor is measured with a question that asked respondents whether they identified themselves with the mayor's party.[59] Figure 1 displays the list experiment estimates of the three services by support for the mayor.[60]

In line with expectations, Figure 1 shows that supporters provide more political services than non-supporters. Among the subgroup who self-identified with the party of the mayor, 34 percent helped with campaigns, as did 16 percent of those who did not identify with the party of the mayor. The 18-point difference is significant at the 90 percent level. I find a similar pattern for the other services. Among supporters, 28 percent reported attending rallies, while among non-supporters the proportion drops to 20 (although the 8-point difference is not significant). Finally, among supporters, 27

Figure 1 List Experiment Estimates of Political Services by Support for the Mayor
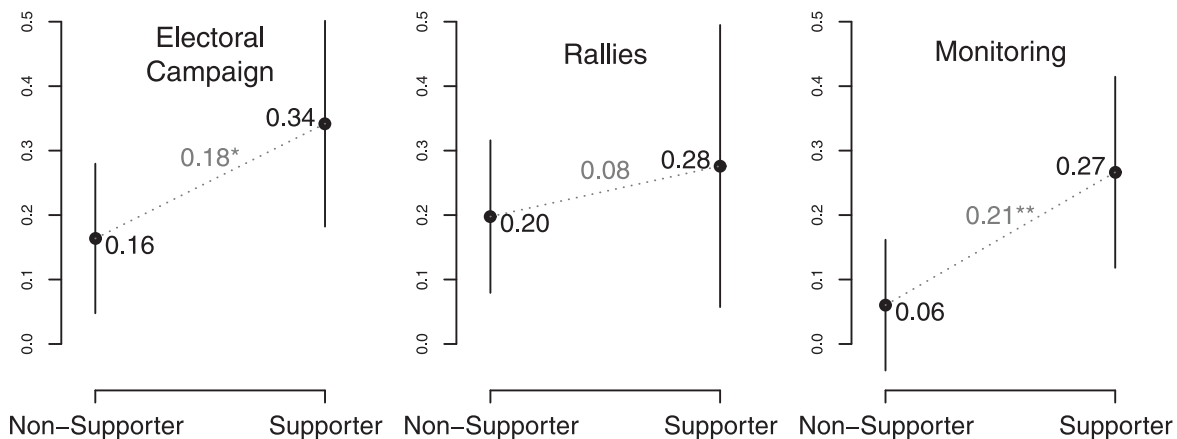
Note: Average treatment effects calculated as the difference between the treatment and the control group (*t*-test with unequal variance). Black circles indicate the proportion of employees in each subgroup who performed the service. Vertical lines represent 95 percent confidence intervals.
*p < 0.10, **p< 0.05, ***p< 0.01.

percent were election monitors in the last election, while among non-supporters the proportion was not significantly different from zero. The difference between the proportion of supporters and non-supporters who reported being monitors is a significant 21 percentage points (at the 95 percent level).

To evaluate these results more rigorously and control for the main alternative explanations—reciprocity and punishment—I conducted multivariate analysis. If reciprocity theories of clientelism were correct, respondents with more favorable attitudes toward reciprocity should be more involved in the provision of services (out of feelings of gratitude). If punishment theories were correct, respondents without tenure rights should be more involved in the provision of services (out of fear of being fired). To measure attitudes about reciprocity (*Reciprocal*), respondents were asked about their agreement with the following statement: "We always have to return the favors that others have done for us." Those who strongly agreed or agreed more than disagreed were coded as reciprocal (1); those who strongly disagreed or disagreed more than agreed were coded as non-reciprocal (0). *Tenure* takes the value of one for those with a permanent contract, and zero otherwise. Control variables include *Female* (0–1), *College* (0–1), *Age* (1–5), and dummies for municipalities. In all the models, the main variable of interest (*Supporter*) is measured as in the previous section with a question that asks about self-identification with the mayor's party.

Table 2 presents the multivariate analysis. All the models are ordinary least squares regressions with the list experiment counts as dependent variables. Following Gonzalez-Ocantos et al.[61] and Gonzalez-Ocantos, de Jonge, and Nickerson,[62] 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 non-1ptinteracted versions of all the variables. The estimates for the political services are derived from the interacted coefficients, while the non-interacted coefficients (not reported) provide estimates for the activities in the control lists.[63] All models include controls for age, gender, education, and dummy variables for municipality (not shown).[64]

The results show that, in general, being a supporter is positively associated with the provision of services, even when controlling for the two main alternative explanations. In all models the sign of the coefficients for the *Supporter* variable is positive and statistically significant for helping with the campaign and monitoring elections, but not for attending rallies. Neither the *Reciprocal* variable nor the *Tenure* variable is statistically distinguishable from zero in any of the models. Even after controlling for the main alternative explanations, being a supporter seems to be the best predictor for the provision of services.

Indeed, the theory of self-enforcing patronage posits that the main explanatory variable for the provision of services is support for the mayor. Supporters are more involved in the provision of services because they have more to lose from a change in administration. Non-supporters, with or without tenure rights, more or less reciprocal, have nothing to fear from a new government. The reason why employees comply with their side of the agreement is not the threat of punishment or feelings of reciprocity, but

Table 2 Political Services; OLS Regressions

	Political Campaigns		Political Rallies		Election Monitors	
Treatment List						
Supporter (0-1)	0.217**	0.177*	0.089	0.055	0.236**	0.254**
	(0.103)	(0.103)	(0.133)	(0.133)	(0.099)	(0.100)
Tenure (0-1)	0.076	0.081	-0.055	-0.044	-0.175	-0.167
	(0.120)	(0.121)	(0.139)	(0.138)	(0.109)	(0.109)
Reciprocal (0-1)		0.015		0.112		0.031
		(0.135)		(0.143)		(0.128)
Constant	0.214	0.217	0.035	-0.048	0.214	0.184
	(0.162)	(0.198)	(0.181)	(0.218)	(0.146)	(0.191)
Observations	1,165	1,157	1,166	1,159	1,168	1,160
R-squared	0.124	0.129	0.165	0.165	0.090	0.094

Note: OLS regressions with the list experiment counts as dependent variables. Tables C6a and C6b in the Appendix report the coefficients for all the variables in these models and the non-interacted variables. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

fear of losing their jobs (or negative changes in working conditions) with a change in the administration. The next section tests this claim.

Self-Enforcing Patronage Contracts

Why do public employees provide political services? This section tests the main empirical implication of the theory: patronage employees believe that their fates are tied to the electoral fate of their patron. To identify the potential effect of a change in the administration on different types of public employees, I use two survey experiments. A randomly selected subset of respondents was asked to estimate the likelihood of losing their jobs and suffering changes in working conditions if the opposition won the next election. The control group was asked the same questions but without providing any information about the hypothetical electoral outcome. I test the theory of self-enforcing patronage by estimating heterogeneous treatment effects across supporters and non-supporters. I expect that supporters will be more likely than non-supporters to fear losing their jobs or experiencing negative changes in their working conditions if a politician from a different party were elected. The results of the survey experiments are consistent with this expectation.

One of the main advantages of experiments is that randomization ensures that the populations in the control and treatment groups are, on average, equivalent on both observable and unobservable characteristics.[65] This allows me to use difference-in-means (t-tests) to analyze the results. Because the individuals who received the treatment were randomly selected, differences in responses across groups can be

attributed to the extra information received by the treatment group.[66] I have also corroborated the main results with regression analyses in which the treatment is included as an independent variable along with controls for tenure, age, gender, education, and municipality.[67] After analyzing the main treatment effects, the core of the argument is tested by examining how support for the incumbent conditions the size of the treatment effect.

Perception of Job Stability   To measure perceptions of job stability, respondents were asked to estimate the likelihood of keeping their jobs after the next election as follows: "On a scale from 0 to 10, where 0 means 'Not at all likely,' and 10 means 'Very likely,' how likely is it that you will continue working at the municipality next year, after the 2011 mayoral elections?" The treatment group was asked the same question with the addition of: "if the incumbent mayor is not reelected and the opposition wins?" [68] Respondents who were told this hypothetical about the incumbent losing the next election and the opposition winning responded differently from those who did not hear any information about the outcome of the election. Whereas the average response among the control group is 8.15, the figure drops to 7.75 among those who received the treatment. The average treatment effect is a significant 0.41 difference (at the 95 percent level).[69] Employees in general fear losing their jobs if the next election were to be won by the opposition.[70]

The main empirical implication of the theory developed here is that supporters of the incumbent have more to fear than non-supporters from a change in the administration. I expect, then, that supporters estimate a higher likelihood of losing their jobs if the incumbent were to lose the next election. Employees without tenure might also fear more with a new administration. Indeed, tenured employees—who cannot be legally fired—should not fear losing their jobs, regardless of the electoral outcome.

To estimate supporters' reaction to the hypothetical electoral outcome, I estimate conditional average treatment effects (CATE). I do this simply by estimating causal effects separately for different subgroups of the population.[71] To identify support for the mayor, two questions were used. First, respondents were asked about their identification with the mayor's party (*Mayor Party*). The second asked for whom they had voted in the last mayoral election (*Mayor Voter*). Results are presented graphically. For example, the left plot in Figure 2 shows the treatment effect of hearing about the hypothetical electoral outcome when asked about the likelihood of staying in the job across employees with tenure ("Yes") and those without tenure ("No"). The dashed line represents the quantity of greatest interest since a steeper slope indicates that hearing about the hypothetical electoral outcome affects the subgroups differently. Since tenured employees cannot be fired, I present the effect for the whole sample (black dots) and the effect without tenured employees (white dots) for each subgroup. Excluding tenured employees makes all the effects stronger in the predicted directions.

First, and in line with expectations, the treatment effect is much stronger for the non-tenured employees (left plot). Among employees without job security, the

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Figure 2 Perception of Job Stability, Heterogeneous Treatment Effects
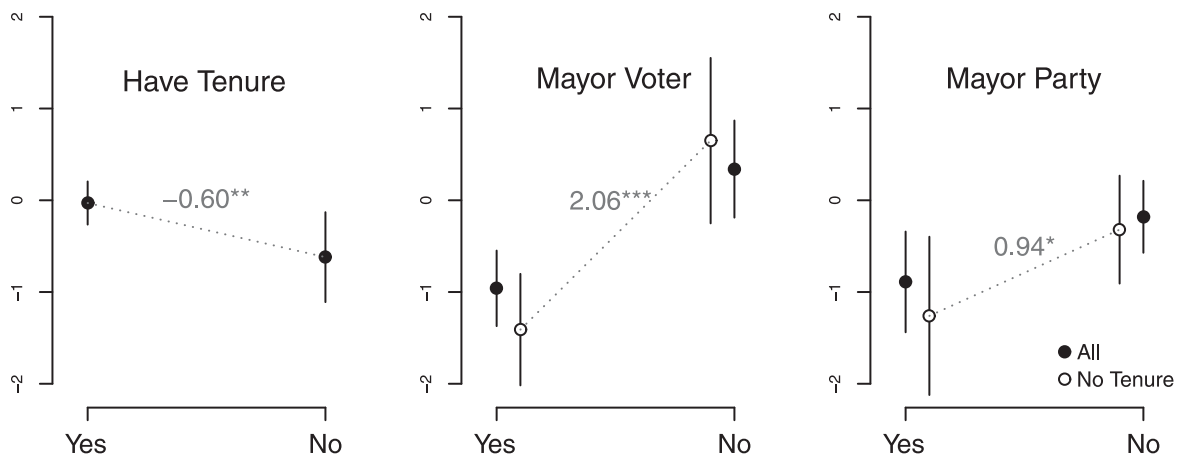
Note: Average treatment effects calculated as the difference between the treatment and the control group (*t*-test with unequal variance). Black circles indicate the treatment effect within subgroups; white circles restrict the sample to non-tenured employees. Vertical lines represent 95 percent confidence intervals. ***p<0.01, **p<0.05, *p<0.1.

treatment effect is a significant 0.62 (compared to a nonsignificant 0.03 for tenured employees). The difference between tenured and non-tenured employees is a significant 0.60 (at the 95 percent level). Second, as the figure clearly shows, supporters who receive the hypothetical about a candidate from the opposition winning the election respond quite differently to the question about the likelihood of keeping their jobs than supporters who do not hear the hypothetical. Hearing the hypothetical about the opposition winning, in contrast, has no effect on the expectations of non-supporters.

The difference in effects between those non-tenured employees (white dots) who reported having voted for the incumbent and those who did not (middle plot) is a significant 2.06 difference (at the 99 percent level). Recall that the scale is 0 to 10; this means that those who had voted for the current mayor feel, on average, 20 percent less confident about keeping their jobs if the opposition wins. Results are similar when using the alternative measure of support. The difference between non-tenured employees who identified themselves with the party of the mayor and those who did not (right plot) is a significant 0.94 (at the 90 percent level), indicating that supporters of the mayor's party feel on average around 10 percent less secure about keeping their jobs if the opposition wins.

Perception of Change   The fear of losing one's job if the opposition wins the next election is not the only mechanism that sustains patronage contracts. Especially for tenured employees, other incentives are in place. If an opposition politician wins the next election, "disloyal" employees—the ones perceived as supporters of the old administration—might be transferred, sidestepped, demoted, or assigned to different activities. A tenured employee who has been working at the municipality of Santa Fe since 1985 explained this clearly: "The fear [for a tenured employee] is about changing

jobs, changing the place of work; it is about being sent somewhere else, somewhere one does not know how to do the job, or where one doesn't have much to do. . . . A lot of things can be changed." In fact, she continued, there were a few cases like this with the change of administration in 2007: "Old employees have been sidestepped a little, their participation has been restricted. I know of people that had to ask to be transferred to another area because there was no room for them anymore where they used to work." And she finished her description by adding: "I am not saying this happens, all I am saying is that *one is afraid of it; it is one's salary, one's livelihood.*"[72]

The story of another employee from Santa Fe illustrates how the fear associated with a change in the administration sometimes becomes real. When I met him, he was sitting alone in an empty office at the municipal Art Center with nothing but an empty desk and a couple of chairs. When asked about his job, he said that, formally, he was the director of photography at the Art Center, but he was not doing that anymore because the new administration (that took office in 2007) appointed someone else to that position. He had gotten his job at the municipality in 1983 and he held, at the time of the interview, the highest rank in the local civil service system. He had tenure so the new administration could not fire him and he was still getting his full salary as director, but when asked about what he was actually doing every day at the office, he replied: "nothing." The new person was doing his former job and there was nothing for him to do. At the time of the interview, he was fifty years old.[73]

To determine whether employees are afraid of these types of changes, I again use a hypothetical about the electoral outcome. All respondents were first asked how satisfied they were with their jobs and then to estimate the likelihood of a change after the election as follows: "On a scale from 0 to 10, where 0 means 'Not at all likely,' and 10 means 'Very likely,' how likely do you think it is that that level of satisfaction with your job will change next year, after the 2011 mayoral elections?" Respondents selected into the treatment group received additional information about the outcome of the election: "if the incumbent mayor is not reelected and the opposition wins?" Immediately after this question, all respondents were asked: *"Do you think that your situation will be better, the same, or worse?"* Responses were coded 1 for better, 0 for no change expected, and -1 for worse.

In line with the results from the previous section, respondents who were told the hypothetical about the incumbent losing the election responded quite differently from those who did not hear this information. Whereas the average response among the control group was 0.36, the average for those who received the treatment was 0.13.[74] The average treatment effect is a significant 0.23 difference. On average, public employees think that their situation would be worse if the opposition were to win.[75] Figure 3 presents the differences in the size of the treatment effect across different subsets of employees—*Tenure*, *Mayor Voter*, and *Mayor Party*. Again, I expect supporters of the incumbent to be more prone to think that the change would be for the worse.[76] Although the theory of self-enforcing patronage does not provide a clear prediction in this case, tenured employees—who have generally been in the job longer and possibly already experienced a change in administration—might be less afraid of suffering negative changes.

Figure 3   Likelihood of Change, Heterogeneous Treatment Effects
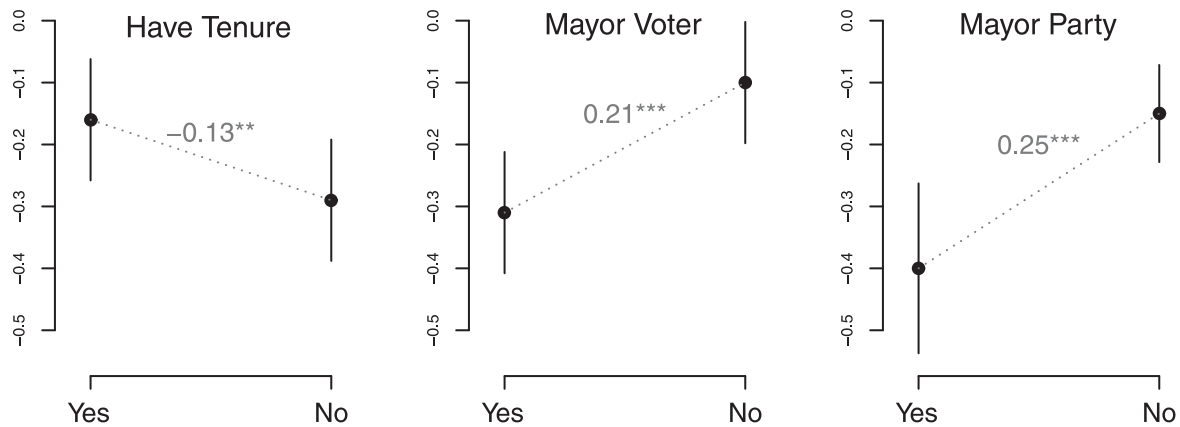
Note: Average treatment effects calculated as the difference between the treatment and the control group (*t*-test with unequal variance). Black circles indicate the treatment effect within subgroups. Vertical lines represent 95 percent confidence intervals. ***p<0.01, **p<0.05, *p<0.1.

Supporters who receive information about the electoral outcome respond differently to the question about changes in working conditions than supporters who do not hear any information. Hearing the hypothetical about the opposition winning has a considerably smaller effect on the future expectations of change among non-supporters. In other words, the strength of the main treatment effect is conditional on the characteristics of the respondents predicted by the theory of self-enforcing patronage—namely support for the mayor. The difference in effects between employees who reported having voted for the incumbent and those who did not (middle plot) is a significant 0.21 (at the 99 percent level). Recall that the scale in this case is -1 to 1; so 0.21 indicates that incumbent voters are, on average, 10 percent more negative about potential changes in working conditions if the opposition wins. The difference in effects between those who identify with the party of the mayor and those who do not (right plot) is a significant 0.25 (at the 99 percent level). Tenured employees also feel less afraid than non-tenured employees of the opposition winning (left plot). The difference in effects across respondents with and without tenure is a significant 0.13 (at the 95 percent level).

In sum, the two survey experiments show that incumbent's supporters have strong incentives to try to keep things as they are.[77] The results clearly indicate that those who could be perceived as supporters by the opposition are afraid of losing their jobs or work conditions changing for the worse with a new administration, which is a strong incentive for providing political services that could help keep the incumbent in office.

Conclusion

This article has set out to answer two questions: what do public sector employees do that affects electoral competition and why do they do it. Using an unobtrusive measurement technique that generates unbiased estimates, I provide systematic evidence

that public employees under patronage contracts do indeed provide political services to the patron who hires them. These political services—attending rallies, helping with campaigns, and monitoring elections, just to mention the ones studied here—are essential for obtaining and maintaining electoral support. In contexts of weak civil service systems, the ability to distribute jobs to those who would provide these services gives incumbents a powerful electoral tool. Patronage contracts, however, are risky. Since the exchange of jobs for political support is sequenced and the law cannot be used to enforce such agreements, defection and betrayal are always a possibility. Here, I have focused on the commitment problems that arise when the job is distributed with the expectation of obtaining political support from the client in the future. A citizen who receives a job with the understanding that she will provide political services in return can easily renege on the agreement after getting the job.

Departing from existing explanations, the theory of self-enforcing patronage posits that public employees comply with the agreement because they believe that their fates are tied to that of their patron. Patrons do not need to monitor employees and threaten to punish non-compliers, nor do they have to encourage feelings of reciprocity among them. When patronage jobs are distributed to perceived supporters, patronage contracts are self-sustaining. Only supporters can credibly commit to provide political support in the future. Patronage jobs and working conditions held by perceived supporters will be maintained by the incumbent (their patron), but not by the opposition (because incumbents' supporters cannot credibly commit to provide political services for the opposition). Once perceived or branded as a supporter of the incumbent, patronage employees have low expectations of keeping their jobs and working conditions if the opposition were to win.

The actual firing or demotion of employees may happen only rarely. Nonetheless, the fact that employees believe in this possibility is enough of an incentive to support their patron. When something as valuable as one's livelihood is at stake, clients might be less willing to risk being wrong. Supporters understand that it is in their best interest to provide political services to help the incumbent remain in power, and this alignment of interests between patrons and clients makes patronage contracts self-sustaining. The empirical evidence provided in this article is consistent with this theory. The list and survey experiment results show that supporters are indeed more likely to be involved in the provision of political services and that they are more afraid than non-supporters of losing their jobs or suffering negative changes in working conditions with a change in administration.

The theory of self-enforcing patronage suggests that clients do not act qualitatively differently from other voters. Clients, as do other citizens, care about their own well-being. They are not necessarily more altruistic than others, and they do not need to be "forced into" supporting a politician who makes them better off. As does any other voter, clients can choose to support the politician who guarantees the continuity of the benefit because they understand it is in their best interest to do so. To the extent that clients believe that the continuation of the benefit is conditional on the patron remaining in a position of power, there is an incentive to help the patron achieve this goal. In the specific clientelistic case discussed here, public sector employees have such an incentive, which

encourages them to comply with the patronage agreement and provide the services needed to ensure the incumbent's electoral success. When the clientelistic exchange is incentive compatible, neither feelings of reciprocity nor monitoring and the threat of punishment are necessary to sustain the exchange. To the extent that patrons and clients share the same interests, there is no need for external enforcement mechanisms.

NOTES

1. Patronage is the discretionary and personalized exchange of public sector jobs for political support. Following Martin Shefter, "Party and Patronage: Germany, England, and Italy," *Politics & Society*, 7 (1977), 403–51 and Rebecca Weitz-Shapiro, *Curbing Clientelism in Argentina: Politics, Poverty, and Social Policy* (New York: Cambridge University Press, 2014), among others, I consider patronage as a subtype of clientelism in which the good being exchanged for political support is a public sector job. I use "patronage contract" to denote that patrons and clients engage in contract-like exchange relationships. Patronage contracts are implicit or explicit agreements between those who get (or expect to get) a job (the client) and those who get (or expect to get) political support in return (the patron). Patronage employees are those who get their jobs with the *expectation* of providing political support in return.

2. Olle Folke, Shigeo Hirano, and James M. Snyder, "Patronage and Elections in U.S. States," *American Political Science Review*, 105 (August 2011), 567–85.

3. Judith Chubb, *Patronage, Power and Poverty in Southern Italy: A Tale of Two Cities* (New York: Cambridge University Press, 1982).

4. Kenneth F. Greene, *Why Dominant Parties Lose: Mexico's Democratization in Comparative Perspective* (New York: Cambridge University Press, 2007).

5. Sarah Brierley, "Combining Patronage and Merit in Public Sector Recruitment," *Journal of Politics* (forthcoming).

6. Ernesto Calvo and María Victoria Murillo, "Who Delivers? Partisan Clients in the Argentine Electoral Market," *American Journal of Political Science*, 48 (October 2004), 742–57.

7. I define "supporter" simply as a public employee who supports the incumbent. At the very minimum, this support involves electoral support. Note that, as explained below, support may not be fully orthogonal to the distribution of the job itself.

8. Merilee S. Grindle, *Jobs for the Boys: Patronage and the State in Comparative Perspective* (Cambridge: Harvard University Press, 2012).

9. Most studies of clientelism today agree on the importance of public sector jobs for financing the work of political brokers (Isabela Mares and Lauren Young, "Buying, Expropriating, and Stealing Votes," *Annual Review of Political Science*, 19 (May 2016), 267–88). However, not all patronage employees are brokers, and brokers are not the only ones in the public administration who are involved in political activities. This article presents evidence that public employees other than brokers engage in political activities that are fundamental for attracting and maintaining electoral support. We know very little about the activities of these less studied and less influential, but still fundamental, political workers.

10. Javier Auyero, *Poor People's Politics: Peronist Survival Networks and the Legacy of Evita* (Durham: Duke University Press, 2001); Mariela Szwarcberg, *Mobilizing Poor Voters: Machine Politics, Clientelism, and Social Networks in Argentina* (New York: Cambridge University Press, 2015); Rodrigo Zarazaga, "Brokers beyond Clientelism: A New Perspective through the Argentine Case," *Latin American Politics and Society*, 56 (2014), 23–45.

11. Simona Piattoni, "Clientelism in Historical and Comparative Perspective," in Simona Piattoni, ed.*, Clientelism, Interests, and Democratic Representation* (Cambridge: Cambridge University Press, 2001), 1–29; James A. Robinson and Thierry Verdier, "The Political Economy of Clientelism," *Scandinavian Journal of Economics*, 115 (April 2013), 260–91.

12. Ernesto Calvo and Gergely Ujhelyi, "Political Screening: Theory and Evidence from the Argentine Public Sector," Working paper (Houston: University of Houston, 2012), http://econpapers.repec.org/paper/houwpaper/201303201.htm;Scott C. James, "Patronage Regimes and American Party Development from 'The Age of Jackson' to the Progressive Era," *British Journal of Political Science*, 36 (January 2006), 39–60; Robinson and Verdier.

13. For the state of this debate in Latin America, see Ezequiel Gonzalez-Ocantos and Virginia Oliveros, "Clientelism in Latin American Politics," in Gary Prevost and Harry Vanden, eds., *The Encyclopedia of Latin American Politics* (Oxford: Oxford University Press, 2019).

14. Frederico Finan and Laura Schechter, "Vote-Buying and Reciprocity," *Econometrica*, 80 (March 2012), 863–81; Chappell Lawson and Kenneth F. Greene, "Making Clientelism Work: How Norms of Reciprocity Increase Voter Compliance," *Comparative Politics*, 47 (October 2014), 61–85; James C. Scott, "Patron-Client Politics and Political Change in Southeast Asia," *American Political Science Review*, 66 (March 1972), 91–113.

15. Valeria Brusco, Marcelo Nazareno, and Susan Stokes, "Vote Buying in Argentina," *Latin American Research Review*, 39 (June 2004), 66–88; Simeon Nichter, "Vote Buying or Turnout Buying? Machine Politics and the Secret Ballot," *American Political Science Review,* 102 (February 2008), 19–31; Robinson and Verdier; Susan Stokes, "Perverse Accountability: A Formal Model of Machine Politics with Evidence from Argentina," *American Political Science Review,* 99 (August 2005), 315–25; Weitz-Shapiro.

16. The literature on clientelism has devoted considerable attention to the problem of monitoring voting behavior under the secret ballot (e.g., Brusco et al.; Stokes). The activities studied here are visible and thus potentially easy to monitor. But that does not mean that they are in fact being monitored. To the extent that the theory of patronage developed here is correct and patrons and clients share the same interests, *monitoring is not necessary*.

17. Note that the patron can be, depending on the context, an individual politician or a political party. Similarly, opposition can refer to a party or to an opposition faction within the same party.

18. As with other public goods (in this case, the incumbent's reelection) that depend on collective contributions (political services), there may be a temptation for free riding. This is discussed at the end of this section.

19. Scholars, such as Auyero, Szwarcberg, and Zarazaga, who think about clientelism as a long-lasting relationship emphasize that patrons and brokers know their clients. Finan and Schechter and Susan Stokes, Thad Dunning, Marcelo Nazareno, and Valeria Brusco, *Brokers, Voters, and Clientelism* (New York: Cambridge University Press, 2013) provide systematic evidence of this.

20. Ernesto Calvo and Maria Victoria Murillo, "When Parties Meet Voters Assessing Political Linkages Through Partisan Networks and Distributive Expectations in Argentina and Chile," *Comparative Political Studies*, 46 (July 2013), 851–82; Ernesto Calvo and María Victoria Murillo, *Non-Policy Politics: Richer Voters, Poorer Voters, and the Diversification of Electoral Strategies* (New York: Cambridge University Press, 2019).

21. Auyero; Simeon Nichter, "Declared Choice: Citizen Strategies and Dual Commitment Problems in Clientelism," APSA 2009 Toronto Meeting Paper, 2009; Szwarcberg.

22. Interview, Salta, August 1, 2011. Politicians take these efforts seriously, as described by another employee who works in close proximity to the mayor: "If a person has contributed to the mayor's victory, she has a right ("*tiene cierto derecho*") [to get a job]" (Interview, Salta, August 10, 2011).

23. Calvo and Murillo, 2004; 2013; 2019; Özge Kemahlioğlu, *Agents or Bosses? Patronage and Intra-Party Politics in Argentina and Turkey* (Colchester: ECPR Press, 2012); Stokes et al.

24. See Table C1 in the Appendix. Moreover, Table C3 shows that employees hired during the current administration are far more likely to report having voted for the current mayor and being party supporters than employees hired during previous administrations.

25. Calvo and Murillo, 2019.

26. While 59 percent reported that personal connections were important and very important, 32 percent reported the same for partisan affiliation and 29 percent for ideology. See Table C2 in the Appendix.

27. Alberto Diaz-Cayeros, Federico Estévez, and Beatriz Magaloni, *The Political Logic of Poverty Relief: Electoral Strategies and Social Policy in Mexico* (New York: Cambridge University Press, 2016), 10.

28. Interview, Salta, June 8, 2011.

29. Herbert Kitschelt and Steven Wilkinson, "Citizen-Politician Linkages: An Introduction," in Herbert Kitschelt and Steven Wilkinson, eds., *Patrons, Clients, and Policies*, 1–49 (Cambridge: Cambridge University Press, 2007), 15. See Nichter (2009) for an example of how clients in Brazil strategically decide to reveal or hide their political preferences according to their expectations about the upcoming election.

30. The scope conditions of the theory outlined here are few. First, patronage is only possible in contexts of weak civil service systems. Second, the theory presumes a minimal level of competition. Finally, political preferences need to be somewhat informative. If citizens can switch at no cost, then being perceived as a supporter does not provide any valuable information.

31. Public sector jobs are indeed highly valued. In 2012, the average public employee in Latin America earned 38 percent more than a formal private employee and twice as much as an informal worker. And the wage gap is generally higher for less-educated employees in Latin America (Malena Arcidiácono et al., "El Empleo Público En América Latina. Evidencia de Las Encuestas de Hogares," *CAF Banco de Desarrollo de America Latina* CAF Documento de Trabajo 2014/05 (2014)) and Argentina (Calvo and Murillo 2004, 2019).

32. Robinson and Verdier, 285.

33. Ibid., 261.

34. ". . .a job has the additional advantage that it can be withdrawn as punishment," ibid., 262.

35. Ibid., 267.

36. Two other aspects separate their formal model from the argument advanced here: 1) it focuses on voting behavior (vs. political services), and 2) it focuses on the reversibility aspect of the job without any consideration for the possibility of changing working conditions.

37. Civil service systems usually allow for political appointments at the higher levels of the bureaucracy. Top positions were, therefore, excluded from the sample.

38. Calvo and Ujhelyi.

39. See Table A1 in the Appendix for information on the municipalities.

40. Appendix A provides more information about the survey.

41. Information on public employment is not publicly available and politicians are usually reluctant to share it. Obtaining these data was particularly challenging and time consuming, illustrating the opacity of the Argentine civil service.

42. Alexandra Scacco, "Who Riots? Explaining Individual Participation in Ethnic Violence" (New York: Columbia University, 2010) https://search.proquest.com/openview/7c75f21fd4ed9bb58991c034783c9713/1?pq-origsite=gscholar&cbl=18750&diss=y

43. To test the strategy, I included an additional question in Salta about the upcoming presidential election. Half of the respondents were asked this question directly (in Part A); the other half found this question at the end of Part B to answer by themselves. The results (available upon request) show that the technique was successful, and employees responded differently when asked under the protected form.

44. See Adam N. Glynn, "What Can We Learn with Statistical Truth Serum? Design and Analysis of the List Experiment," *Public Opinion Quarterly*, 77 (January 2013), 159–72.

45. Since most interviews were conducted in front of others, response options were not read aloud.

46. Elisabeth Coutts and Ben Jann, "Sensitive Questions in Online Surveys: Experimental Results for the Randomized Response Technique (RRT) and the Unmatched Count Technique (UCT)," *Sociological Methods & Research*, 40 (January 2011), 169–93; Chad P. Kiewiet De Jonge and David W. Nickerson, "Artificial Inflation or Deflation? Assessing the Item Count Technique in Comparative Surveys," *Political Behavior*, 36 (September 2014), 659–82. To protect anonymity in list experiments, it is crucial to avoid lists that would result in respondents choosing none or all of the items, generating "floor" or "ceiling" effects, respectively. To minimize ceiling effects, the three lists included rare activities or activities that were not possible to perform concurrently. The strategy was successful since only around 1 percent of respondents who received the control list reported all four of the control items. The inclusion of high prevalence activities to minimize the risk of floor effects was less successful (see Table C4). Note, however, that the presence of either ceiling or floor effects would lead to the underestimation of the sensitive activity (Graeme Blair and Kosuke Imai, "Statistical Analysis of List Experiments," *Political Analysis,* 20 (Winter 2012), 47–77); the estimates presented here are thus conservative. Using the method developed by Blair and Imai to test the validity of the

experiments, I fail to reject the null hypothesis for design effects in the three list experiments. See Appendix B for experiments wording and Table C4 for the distribution of responses across groups.

47. Table A6 in the Appendix reports the average age, gender, and education for both groups. The balanced distribution across conditions suggests that the randomization was successful.

48. In Virginia Oliveros, "Making It Personal: Clientelism, Favors, and the Personalization of Public Administration in Argentina," *Comparative Politics*, 48 (April 2016), 373–91, I study another political service: the provision of favors.

49. Kemahlioğlu.

50. Brusco et al..

51. Zarazaga.

52. Auyero; Szwarcberg; Zarazaga.

53. Auyero; Szwarcberg.

54. Stokes et al.; Szwarcberg; Zarazaga.

55. Official monitors are responsible for checking voters' identifications, counting votes, and filling the forms reporting the electoral results.

56. Brusco et al.; Szwarcberg.

57. See Appendix B for the baseline categories and question wording.

58. Assessing the magnitude of these estimates is difficult without information about how many monitors, campaign workers, and rallies attendees are "needed" in an election. For the case of monitors, however, the data actually exist. In Tigre, for instance, 14 percent of respondents participated as monitors, which means that around half of all the monitors deployed during the 2009 election were public employees. See Table C4 in the Appendix.

59. See Appendix B for variable operationalization and descriptive statistics.

60. Table C5 presents the numeric values displayed in the figure.

61. Ezequiel Gonzalez-Ocantos, Chad Kiewiet de Jonge, Carlos Meléndez, Javier Osorio, and David Nickerson, "Vote Buying and Social Desirability Bias: Experimental Evidence from Nicaragua," *American Journal of Political Science*, 56 (January 2012), 202–17.

62. Ezequiel Gonzalez-Ocantos, Chad Kiewiet de Jonge, and David Nickerson, "Legitimacy Buying: The Dynamics of Clientelism in the Face of Legitimacy Challenges," *Comparative Political Studies*, 48 (August 2015), 1127–58.

63. Unfortunately, my attempts to use the more efficient estimator proposed by Blair and Imai and Kosuke Imai, "Multivariate Regression Analysis for the Item Count Technique," *Journal of the American Statistical Association*, 106 (2011), 407–16 were unsuccessful and the models failed to converge. Note, however, that the less efficient (and therefore conservative) fixed interaction approach used here still produces unbiased estimates.

64. As a robustness check, I ran additional models (Table C7a and C7b in the Appendix) in which I included interaction terms between the main variables. Results are generally consistent with the ones presented here.

65. See Table A6 for randomization checks.

66. Note that both experiments can only manipulate perceptions if respondents thought that reelection was possible. This seems likely considering the reelection rates in the provinces studied here range from 40 to 47 percent (Juan Pablo Micozzi, "The Electoral Connection in Multi-Level Systems with Non-Static Ambition" (Houston: Rice University, 2009), https://scholarship.rice.edu/handle/1911/61922, Table A1).

67. See Tables C9 and C12.

68. A slightly different question was used in Salta. See Appendix B.

69. Tables C8 and C9 present the t-test and the regression analysis results.

70. The question may also be capturing cases of employees who would resign. Yet, the comparison of the conditional average treatment effect for tenured and non-tenured employees (below) suggests that the question is more likely capturing the likelihood of being fired than resigning.

71. Table C10 shows the exact numeric effects within subgroups.

72. Interview, Santa Fe, August 16, 2011.

73. Interview, Santa Fe, July 22, 2011.

74. Regardless of the treatment, few respondents were expecting worsen conditions. Since salary increases, promotions, and tenure rights are tied to years in the job, most employees think that, all else equal, working conditions will get better over time.

75. Tables C11 and C12 show the t-test and the regression analysis results.

76. Table C13 displays the exact numeric effects within each subgroup.

77. The results by municipality are broadly consistent with the ones obtained by grouping the three municipalities together (results available upon request).

APPENDIX

Appendix A: Survey Methodology, Representativeness, and Randomization

The survey consisted of face-to-face interviews of low- and mid-level local public sector employees in the Argentinean municipalities of Salta (province of Salta), Santa Fe (province of Santa Fe), and Tigre (province of Buenos Aires).[1] Together with a team of research assistants, we interviewed 1,184 public sector employees (around 400 in each municipality). The survey was administered between August 10 and December 30, 2010, in Santa Fe; between August 11 and November 26, 2010, in Tigre; and from June 6 to August 11, 2011, in Salta.[2] It was preceded by a pilot administered in Santa Fe between July 22 and July 29, 2010, consisting of 40 cases. The contact rate for the survey was 59 percent, the response rate was 56 percent, the cooperation rate 95 percent, and the refusal rate 3 percent. The margin of error was 2.7 percent.[3] On average, survey interviews lasted 24 minutes.

A random sample based on the official list of public employees (excluding elected officials and high-level positions) was generated within each municipality. The randomly selected employees were then directly approached for interviews at public offices during their working hours. Since the random sample was drawn from an official list of public employees and the survey was administered during office hours at the municipality, both the permission and the collaboration of the local authorities were critical. To maximize the chances of getting the authorities' approval for the survey, I took two precautions. First, I excluded particularly direct sensitive questions—especially the ones related to the mayor. Second, I designed the survey instrument to be as short as possible to make sure employees would not be kept away from their jobs for long periods of time. Local authorities in each municipality read the survey instrument but did not censor any of the proposed questions.

I provided interviewers with lists of randomly selected public employees and the addresses of their respective workplaces, which ranged from offices at the city hall or at decentralized offices (*delegaciones*) to construction sites, cemeteries, health centers, hospitals, parks, and the street itself. When the selected employee was not at the workplace at the time of the visit or preferred to answer the survey at a different time of day, interviewers were instructed to make an appointment to return later. If the selected employee refused to answer the survey or the interview could not be conducted after the second attempt, the respondent was replaced with the following name on the list of public employees. Interviewers made detailed records of failed interviews. Because survey and list experiment questions were embedded in the survey instrument with two conditions each—treatment and control—two versions of the questionnaire were used.

With the exception of the survey and the list experiment questions, respondents were asked questions from identical questionnaires. Interviewers used the two questionnaires in sequential order, assigning respondents alternatively to either the treatment or the control group.

Since the survey was conducted face-to-face at public offices, getting truthful answers presented a challenge. While high-ranking public officials usually have their own offices, most public employees in Argentina share their work spaces with others. Public employees could be unwilling to reveal sensitive information in front of others, especially if they think that their jobs could be jeopardized by their answers. Following standard IRB procedures, all interviews started with the enumerators explaining the purpose of the survey and the confidentiality of all the data collected. Enumerators were also instructed to emphasize the strictly academic purpose of the survey and to ensure that respondents understood that the information would not be shared with anyone. Besides this standard procedure, I implemented two distinct but complementary strategies to minimize social response bias—whether in the form of refusals or inaccurate answers.

First, I designed a series of survey list experiments with the goal of providing respondents with the anonymity needed to induce them to give accurate information. Second, I followed Scacco's (2010) strategy (originally developed to survey rioters in Africa) and split the questionnaire into two parts. Part A of the questionnaire had background and general information about the respondent, as well as the less sensitive questions and the list experiments. Part B had the more sensitive and direct questions about voting behavior, ideology, and political preferences. Each part of the questionnaire was marked with a different survey identification number that could only be matched with a document not available to the enumerators. Apart from this number, the second part of the questionnaire had no information—such as age, gender, education, or occupation—that could be used to identify the respondent. Enumerators administered Part A of the questionnaire, while Part B (the sensitive part) was read and filled out by the respondents themselves.[4] Thus, the other employees in the office were not able to hear the questions or the answers. Part B of the questionnaire was designed to be short and easy to understand with only closed-ended questions. Finally, the respondents were asked to store the second part of the questionnaire in a sealed cardboard box similar to a ballot box (something familiar to Argentineans since cardboard ballot boxes and paper ballots are still used in Argentinean elections).[5] Enumerators were instructed to provide a detailed explanation of these procedures before handing out Part B of the questionnaire to the respondents and to make sure respondents understood that the confidentiality of their responses was fully protected. Their understanding was fundamental to guaranteeing the success of the technique.

Interviewers were recruited from Humanities and Social Science Departments in Buenos Aires, Salta, and Santa Fe, and were either advanced undergraduate or recently graduated students. For the purpose of survey verification, basic information about the public sector employees (age, years in the position, and type of contract) was obtained from each of the municipalities and was not shared with the enumerators. If this

information did not match that reported by the interviewer in the survey instrument beyond the reasonable expected mistakes, further verification was conducted on the interviews administered by that interviewer. This second round of verification was done in person (by me) with the respondents.[6]

Table A1:   Socio-demographic and political characteristics of the municipalities

	Salta	**Santa Fe**	**Tigre**
Province	Salta (North)	Santa Fe (Center)	Buenos Aires
Mayoral reelection rate (province)	40%	47%	46%
Mayor	Miguel Angel Isa	Mario Barletta	Sergio Massa
Period	2003-2015	2007-2011	2007-2013
Mayor's party	Peronist	Radical	Peronist
Electoral Competition (municipality)	Low	High	Middle/Low
Population 2001	472,971	369,589	301,223
Population 2010	536,113	485,345	376,381
% w/college	9%	11%	7%
% w/health insurance	48%	59%	45%
% poverty	21%	14%	20%

Note: Data from the 2001 Census (except for population, 2010 data was not available at the time of the survey). Mayors are the ones who were in power at the time of the survey (2010/2011). Mayoral reelection rates correspond to the 1983-2007 period (Micozzi 2009).

Table A2:   Characteristics of public sectors across municipalities

	Salta	**Santa Fe**	**Tigre**
Total N in payroll	4,619	5,070	2,569
As a % of the population	0.86%	1.04%	0.68%
Total N in sample frame	4,263	4,528	2,406
Tenured employees	77%	55%	20%
Got job in current administration	47%	45%	45%
Women	37%	36%	45%
Older than 40	70%	63%	53%

Note: Data provided by each municipal government. Data from Santa Fe is from June 2010, data from Tigre is from July 2010, and data from Salta is from May 2011.

Table A3: Survey sample representativeness (Salta)

Variable	Surveyed employees		Employees in the sample	
	N	**%**	**N**	**%**
Observations	389		4263	
Gender				
Male	228	58.61	2701	63.35
Female	161	41.39	1562	36.65
Start working with current mayor	211*	54.24	1491**	46.75
Older than 40 years old	230	59.13	2121***	70.04
Tenure				
With tenure	242	62.21	2992****	77.11
No tenure	146	37.53	888****	22.89

*1 missing observation
**1074 missing observations
***1235 missing observations
****383 missing observations

Table A4: Survey sample representativeness (Santa Fe)

Variable	Surveyed employees		Employees in the sample	
	N	**%**	**N**	**%**
Observations	395		4528	
Gender				
Male	235	59.49	2917	64.42
Female	160	40.51	1611	35.58
Start working with current mayor	141	35.70	1949**	44.87
Older than 40 years old	270*	67.94	2765***	63.34
Tenure				
With tenure	256	64.81	2484	54.86
No tenure	139	35.19	2044	45.14

*2 observations missing
**184 observations missing
***163 observations missing

4

Table A5:   Survey sample representativeness (Tigre)

Variable	Surveyed employees		Employees in the sample	
	N	**%**	**N**	**%**
Observations	400		2406	
Gender				
Male	193	48.25	1323	54.99
Female	207	51.75	1083	45.01
Start working with current mayor	184*	46.00	1034***	45.37
Older than 40 years old	192**	48.61	1201***	52.70
Tenure				
With tenure	88	22.00	475	19.74
No tenure	312	78.00	1931	80.26

*2 missing observations
**5 missing observations
***127 missing observations

Table A6:   Covariate balance across type of questionnaires, by municipality

	Tigre		Santa Fe		Salta		Whole Sample	
Variable	Type 1	Type 2	Type 1	Type 2	Type 1	Type 2	Type 1	Type 2
Observations	199	201	196	199	196	193	591	593
Female	0.55	0.48	0.41	0.40	0.40	0.42	0.46	0.44
Age	39.33	39.65	44.63	44.80	43.86	43.11	42.61	42.51
Education	6.37	6.11	5.30	5.11	5.56	5.63	5.74	5.62

Note: The balanced distribution of the variables across the two conditions suggests that the groups are fairly equivalent on observable characteristics and that the randomization was successful. None of the differences between control and treatment groups are statistically significant (at the 95 percent level).

Appendix B: Variable Operationalization and Descriptive Statistics

LIST EXPERIMENTS
Treatment activity in bold; Spanish translation in brackets and italics.

I am going to hand you a card that mentions a number of activities. Please, I would like for you to tell me HOW MANY of those you did in <u>the last elections (2009/2011).</u> Please, do not tell me which ones, just HOW MANY.
(*Le voy a entregar una tarjeta donde figuran una serie de actividades, quisiera que me señale CUÁNTAS de ellas realizó Ud. <u>en las elecciones del último año (2009/2011).</u> Por favor, no me diga cuáles, sino solamente CUÁNTAS*)

Note: 2009 for Santa Fe and Tigre; 2011 for Salta.

A) Electoral Campaigns
- Be a candidate (*Ser candidato*)
- Get informed about the different candidates *(Informarse sobre los distintos candidatos)*
- **Work/help in the electoral campaign** *(Trabajar/ ayudar en la campaña electoral)*
- Get disenfranchised *(Impugnar el voto)*
- Cast a straight-ticket vote for any of the parties *(Votar la lista completa de algún partido)*

B) Political Rallies
- Participate in political meetings (*Participar de reuniones políticas*)
- Vote in the primaries of any party *(Votar en las internas de algún partido)*
- **Attend political rallies** *(Concurrir a movilizaciones o actos electorales)*
- Abstain from voting *(Abstenerse de votar)*
- Get informed about the election on the news *(Informarse acerca de la elección en las noticias)*

C) Monitoring Elections
- Decide whom to vote for at the last minute (*Decidir el voto a último momento*)
- Split the ticket *(Cortar boleta)*
- **Be an election monitor** *(Ser fiscal de mesa)*
- Abstain (*No votar*)
- Cast a null vote (*Anular el voto*)

INDEPENDENT VARIABLES

Mayor Supporter

Do you identify yourself with any party? (*¿Se identifica Ud. con algún partido político?*)
Those who replied YES were asked:
Which party do you identify yourself with? *(¿Con cuál partido se identifica Ud.?)*
Respondents who mentioned the mayor's party in each municipality were coded as Supporters; all the rest were coded as zero.
Supporter (1)
Non-supporter (0)

Mayor Voter[7]

Which party did you vote for in the last local elections for mayor (2007/2011)? (*¿A qué partido/candidato votó en las últimas elecciones municipales para intendente (2007/ 2001)?*)
Respondents who reported the party of the incumbent mayor in each municipality were coded as mayor voters; all the rest were coded as zero. Respondents who were not registered to vote in the municipality where they worked were coded as missing.
Mayor Voter (1)
Nonvoter (0)

College

Which is your maximum level of education? (*¿Cuál es el máximo nivel educativo alcanzado por Ud.?*)
Recoded into two categories:
College (1)
No College (0)

Age

Could you tell me your age? (*¿Podría decirme su edad?*)
Recoded into five age categories:
18–25 (1)
26–35 (2)
36–45 (3)
46–55 (4)
More than 55 (5)

Female

Note the sex of the respondent (*Registrar el sexo del entrevistado*)
Female (1)
Male (0)

Tenure

What type of contract do you have in the municipality? (*¿Qué tipo de contrato tiene Ud. con la municipalidad?*)
Tenure (1) (*Permanente*)
No Tenure (0) (*Temporario/Contratado*)

Reciprocal

How much do you agree or disagree with the following statement: "We always have to return the favors that others have done for us." Would you say you … (*Qué tan de acuerdo o en desacuerdo está Ud. con la siguiente afirmación: "Siempre hay que regresar los favores que alguien nos hace". Diría Ud. que está…*)
- Strongly agree (*Muy de acuerdo*)
- Agree more than disagree (*Más de acuerdo que en desacuerdo*)
- Disagree more than agree (*Más en desacuerdo que de acuerdo*)
- Strongly disagree (*Muy en desacuerdo*)

Recoded into two categories:
Reciprocal: Strongly agree + Agree more than disagree (1)
Nonreciprocal: Strongly disagree + disagree more than agree (0)

SURVEY EXPERIMENTS
Treatments in bold and underlined

Job Stability

[SHOW CARD]. On a scale from 0 to 10, where 0 means 'Not at all likely,' and 10 means 'Very likely,' how likely is it that you will continue working at the municipality next year, after the 2011/2015 mayoral elections **if the incumbent mayor is not reelected and the opposition wins**?

(*[ENTREGAR TARJETA] En una escala de 0 a 10, donde 0 es NADA Probable, y 10 es MUY Probable, ¿Cuan probable es que Ud. siga trabajando en la municipalidad el año que viene, luego de las elecciones para intendente de 2011/2015 **si el actual intendente no fuese reelecto y ganara la oposición**?*)

Not at all likely (*Nada Probable*)					Likely (*Probable*)				Very likely (*Muy Probable*)		DK (*NS*)
0	1	2	3	4	5	6	7	8	9	10	99

Note: 2011 for Santa Fe and Tigre; 2015 for Salta

Job Stability bis (for Salta)

Now imagine that the next mayoral elections, instead of being in 2015, would be next year. In this same scale, how likely is it that you will continue working at the municipality next year, after these hypothetical elections **if the incumbent mayor is not reelected and the opposition wins**?

(*Ahora imagínese que las próximas elecciones para intendente, en lugar de ser en 2015, fueran EL AÑO QUE VIENE. En esta misma escala, ¿Cuan probable es que Ud. siga trabajando en la municipalidad el año que viene, luego de estas supuestas elecciones, **si el actual intendente no fuese reelecto y ganara la oposición**?*)

Not at all likely (*Nada Probable*)					Likely (*Probable*)				Very likely (*Muy Probable*)		DK (*NS*)
0	1	2	3	4	5	6	7	8	9	10	99

Note: For reasons beyond my control (explained in Appendix A), the survey in Salta had to be postponed. Consequently, the surveys in Santa Fe and Tigre were conducted around a year before the local elections, whereas the survey in Salta was conducted almost four years before the next elections. Thus, many non-tenured employees were expecting to get tenure in the near future, and this expectation would affect their responses about job stability. To get around this problem, a follow-up question (Job Stability bis) was included in Salta. Both questions are highly correlated (0.8 for tenured employees and 0.67 for non-tenured), and results are consistent across measures; so I use the question about the hypothetical election for consistency with the other municipalities.

Perception of Change

General satisfaction (framing question)
In general terms, how satisfied are you with your job? (*En términos generales, ¿Cuan conforme diría Ud. que está con su trabajo?*)
- Very satisfied (*Muy conforme*)
- Satisfied *(Conforme)*
- Not very satisfied (*Poco conforme*)
- Not at all satisfied *(Nada conforme)*

Perception of change

On a scale from 0 to 10, where 0 means 'Not at all likely,' and 10 means 'Very likely,' how likely do you think it is that the level of satisfaction with your job will change next year, after the 2011 mayoral elections **if the incumbent mayor is not reelected and the opposition wins**? (*[ENTREGAR TARJETA] En una escala de 0 a 10, donde 0 es NADA Probable, y 10 es MUY Probable ¿Cuan probable cree Ud. que es que ese nivel de conformidad con su trabajo cambie el año que viene, luego de las elecciones para intendente de 2011 si el actual intendente no fuese reelecto y ganara la oposición?*)

Not at all likely (*Nada Probable*)					Likely (*Probable*)			Very likely (*Muy Probable*)			DK (*NS*)
0	1	2	3	4	5	6	7	8	9	10	99

Direction of change
Do you think that your situation will be better, the same, or worse? (*¿Ud. cree que su situación será mejor, igual o peor?*)
- Better (*Mejor*) (1)
- The same (*Igual*) (0)
- Worse (*Peor*) (-1)

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Table B1:   Descriptive Statistics

Variable	N	Mean	Std. Dev.	Min	Max
Campaigns	1169	1.30	0.83	0	5
Rallies	1170	1.50	0.97	0	5
Monitoring	1172	0.99	0.74	0	4
Mayor Supporter	1184	0.33	0.47	0	1
Mayor Voter	1113	0.61	0.49	0	1
College	1181	0.23	0.42	0	1
Age	1184	3.20	1.21	1	5
Female	1184	0.45	0.50	0	1
Tenure	1183	0.50	0.50	0	1
Current Mayor	1181	0.45	0.50	0	1
Reciprocal	1176	0.86	0.34	0	1
Stability	1131	7.95	2.74	0	10
Change	1027	0.25	0.55	-1	1

Appendix C: Additional Tables and Robustness Checks

Table C1: How did you find out about this job?[8]

	N	%	
Through someone/an acquaintance who worked at the municipality	352	30%	
Through a friend/relative who worked at the municipality	328	28%	**64%**
"Through politics"	69	6%	
Submitted a resume to the municipality	156	13%	
Previous beneficiary of a welfare program	55	5%	
Through an employment agency/the media/ newspapers/ ads	40	3%	
Previous internship at the municipality	25	2%	
Entrance examinations	18	2%	
Some other way & missing	141	12%	
	1184	100%	

Note: Some examples of answers that specifically mentioned politics ("Through politics") as the means of finding out about the job: "Through politics"; "Through a politician"; "Through a local broker"; "I used to work for a Peronist councilman"; "I used to work in politics with someone at the municipality"; "Because of political activism, social activism."; "Because my (husband/wife/relative) works in politics." This, of course, does not mean that there were no political contacts on the other informal channels. In the cases of those who reported finding out about the job through an acquaintance, a relative, or a friend, that connection may have been political. As explained by an employee from Santa Fe, admitting the political connection might not be everyone's first choice: "It's always more dignified, I think, to earn one's job. I think we would all prefer that. No one wants to get the job because of one's best friend . . . or later being singled out, 'look, he is the friend. . .'"[9] Some employees, however, openly admitted the political connection.

Table C2: Importance of different criteria for hiring[10]

	Very important	Important	A little important	Not important at all	Total N
Education	32% (N=376)	42% (N=481)	20% (N=228)	6% (N=72)	1157
	74%		26%		
Work experience	30% (N=342)	36% (N=419)	24% (N=282)	10% (N=114)	1157
	66%		34%		
Personal connections	30% (N=342)	29% (N=334)	17% (N=191)	24% (N=281)	1148
	59%		41%		
Partisan affiliation	13% (N=151)	19% (N=217)	19% (N=213)	49% (N=552)	1133
	32%		68%		
Political ideology	12% (N=130)	18% (N=197)	17% (N=190)	53% (N=594)	1111
	29%		71%		

Note: Percentages exclude respondents who did not answer the question.

Table C3: Relationship between current administration jobs and support for the mayor

	Mayor Voters	Mayor's Party Supporters
Current Administration	0.72	0.39
	(0.02)	(0.02)
	N=492	N=536
Previous Administrations	0.53	0.27
	(0.02)	(0.02)
	N=619	N=645
Difference	0.19**	0.12**
	(0.03)	(0.03)
	N=1111	N=1181

Note: Two-sample t-test with unequal variances and standard errors in parentheses

Table C4: Distribution of responses for the list experiment, across treatment and control

	Rallies				Electoral Campaign				Monitoring			
	Control		Treatment		Control		Treatment		Control		Treatment	
0	76	13%	61	10%	79	14%	79	13%	162	28%	130	22%
1	277	47%	259	44%	334	57%	271	46%	319	54%	319	55%
2	162	28%	158	27%	153	26%	175	30%	93	16%	112	19%
3	66	11%	70	12%	13	2%	47	8%	12	2%	24	4%
4	3	1%	36	6%	3	1%	12	2%	1	0%		0%
5			2	0%			3	1%				0%
Estimate												
	0.21***				0.22***				0.12***			
N	584		586		582		587		587		585	

Note: Assessing the magnitude of these estimates can be difficult without information about how many monitors, campaign workers, and rallies attendees are "needed" in an election. For the case of monitors, however, there is some data that allows me to provide a precise estimate of the importance of public employees' contribution. In Tigre, for example, 225,493 citizens were registered to vote in the 2009 election (the one held before the survey was conducted) and there were 652 voting booths or "*mesas*" (official data from the provincial electoral authorities, *Junta Electoral, provincia de Buenos Aires*). Political parties usually assigned one monitor by booth plus a head monitor —"*fiscal general*"— by school (where elections take place in Argentina), so 652 is a conservative estimate since it does not include the head monitor who is usually a relatively important broker. The total number of public employees in Tigre is 2406 (excluding elected officials and high rank position). Thus, provided that the sample was properly drawn, we can infer that 289 (12 percent of 2406) of those 2406 employees served as election monitors in the 2009 election. Therefore, almost half (44 percent, 289 out of 652) of the people needed to monitor the 2009 election were local public sector employees, though only around 1 percent of Tigre's voters are public employees. In the specific case of Tigre, the estimate for the proportion of employees that were election monitors in the previous election is actually 14% (not 12%), which means that 52 percent of all the monitors needed during the election were public employees. Similar data for the other two municipalities was not available.

Table C5: List Experiment estimates of political services by support for the mayor

		Electoral Campaign	Political Rallies	Election monitors
Mayor Party Supporter	Yes	0.34***	0.28**	0.27***
		(0.08)	(0.11)	(0.08)
		N=379	N=380	N=381
	No	0.16***	0.20***	0.06
		(0.06)	(0.06)	(0.05)
		N=790	N=790	N=791
	Difference in means	0.18*	0.08	0.21**
		(0.10)	(0.13)	(0.09)
		N=1169	N=1170	N=1172

Note: *** p<0.01, ** p<0.05, * p<0.1.

Table C6a: Political Services. OLS Regressions. Coefficients for the Treatment Lists.

	Political Campaigns			Political Rallies			Election Monitors		
Treatment List									
Supporter (0-1)	0.194*	0.217**	0.177*	0.078	0.089	0.055	0.212**	0.236**	0.254**
	(0.102)	(0.103)	(0.103)	(0.129)	(0.133)	(0.133)	(0.093)	(0.099)	(0.100)
Tenure (0-1)		0.076	0.081		-0.055	-0.044		-0.175	-0.167
		(0.120)	(0.121)		(0.139)	(0.138)		(0.109)	(0.109)
Reciprocal (0-1)			0.015			0.112			0.031
			(0.135)			(0.143)			(0.128)
Female (0-1)	-0.019	-0.043	-0.023	0.090	0.044	0.062	-0.021	-0.020	-0.025
	(0.102)	(0.098)	(0.097)	(0.116)	(0.111)	(0.111)	(0.091)	(0.090)	(0.090)
College (0-1)	-0.087	-0.033	-0.038	-0.084	-0.086	-0.076	0.011	0.005	0.009
	(0.110)	(0.111)	(0.111)	(0.130)	(0.128)	(0.129)	(0.108)	(0.112)	(0.113)
Age (1-5)	-0.024	-0.045	-0.047	0.041	0.060	0.052	-0.069*	-0.036	-0.040
	(0.042)	(0.046)	(0.046)	(0.047)	(0.054)	(0.054)	(0.037)	(0.041)	(0.042)
Salta		0.096	0.099		-0.003	0.004		0.005	-0.001
		(0.120)	(0.120)		(0.147)	(0.147)		(0.111)	(0.111)
Santa Fe		0.139	0.134		-0.006	-0.011		0.137	0.148
		(0.125)	(0.126)		(0.129)	(0.130)		(0.123)	(0.123)
Treatment constant	0.260	0.214	0.217	0.036	0.035	-0.048	0.288**	0.214	0.184
	(0.167)	(0.162)	(0.198)	(0.180)	(0.181)	(0.218)	(0.144)	(0.146)	(0.191)
Observations	1,166	1,165	1,157	1,167	1,166	1,159	1,169	1,168	1,160
R-squared	0.037	0.124	0.129	0.082	0.165	0.165	0.043	0.090	0.094

Note: OLS regressions with the list experiment counts as dependent variables. All coefficients are interactions between each independent variable and the treatment (the list experiment condition including the sensitive activity). Table C6b below reports the coefficients for the non-interacted variables. The municipality of Tigre (the base category) was excluded. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table C6b: Political Services. OLS Regressions. Coefficients for the Control Lists.

Control List	Political Campaigns			Political Rallies			Election Monitors		
Supporter (0-1)	0.065	0.163***	0.170***	0.471***	0.532***	0.533***	-0.376***	-0.293***	-0.309***
	(0.059)	(0.062)	(0.062)	(0.080)	(0.081)	(0.081)	(0.062)	(0.066)	(0.067)
Tenure (0-1)		-0.198***	-0.192***		-0.131	-0.126		-0.004	-0.002
		(0.071)	(0.071)		(0.083)	(0.083)		(0.076)	(0.076)
Reciprocal (0-1)			0.089			0.005			0.117
			(0.094)			(0.103)			(0.075)
Female (0-1)	-0.003	0.007	-0.002	0.023	0.049	0.042	0.038	0.037	0.044
	(0.062)	(0.058)	(0.058)	(0.073)	(0.068)	(0.069)	(0.061)	(0.061)	(0.061)
College (0-1)	-0.075	-0.010	0.000	-0.113	0.033	0.040	-0.017	0.004	0.012
	(0.066)	(0.066)	(0.066)	(0.076)	(0.072)	(0.073)	(0.072)	(0.076)	(0.076)
Age (1-5)	0.038	0.055**	0.052*	-0.035	-0.046	-0.050	0.035	0.026	0.029
	(0.026)	(0.028)	(0.028)	(0.031)	(0.033)	(0.033)	(0.024)	(0.027)	(0.028)
Salta		0.097	0.084		0.416***	0.406***		-0.106	-0.107
		(0.070)	(0.071)		(0.092)	(0.092)		(0.070)	(0.071)
Santa Fe		0.539***	0.544***		0.745***	0.748***		0.207**	0.198**
		(0.078)	(0.078)		(0.078)	(0.078)		(0.085)	(0.085)
Control constant	1.060***	0.837***	0.765***	1.359***	1.007***	1.018***	0.921***	0.886***	0.783***
	(0.108)	(0.100)	(0.135)	(0.122)	(0.112)	(0.144)	(0.092)	(0.094)	(0.111)
Observations	1,166	1,165	1,157	1,167	1,166	1,159	1,169	1,168	1,160
R-squared	0.037	0.124	0.129	0.082	0.165	0.165	0.043	0.090	0.094

Note: OLS regressions with the list experiment counts as dependent variables. Coefficients for the non-interacted variables from regression reported on Table 2 on the main document. Interacted variables display on Table C6a. The municipality of Tigre (the base category) was excluded. Robust Standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C7a: Political Services. OLS Regressions. Coefficients for the Treatment Lists (with interactions)

Treatment List	Political Campaigns			Political Rallies			Election Monitors		
Supporter (0-1)	0.177*	0.268**	0.379	0.055	0.117	-0.375	0.254**	0.257*	0.506*
	(0.103)	(0.136)	(0.266)	(0.133)	(0.183)	(0.332)	(0.100)	(0.133)	(0.263)
Tenure (0-1)	0.081	0.116		-0.044	-0.034		-0.167	-0.160	
	(0.121)	(0.142)		(0.138)	(0.148)		(0.109)	(0.130)	
Reciprocal (0-1)	0.015		0.056	0.112		-0.080	0.031		0.141
	(0.135)		(0.169)	(0.143)		(0.151)	(0.128)		(0.150)
Supporter*Tenure		-0.111			-0.059			-0.042	
		(0.207)			(0.256)			(0.187)	
Supporter*Reciprocal			-0.248			0.486			-0.270
			(0.288)			(0.357)			(0.281)
Female (0-1)	-0.023	-0.015	-0.040	0.062	0.046	0.080	-0.025	-0.018	-0.026
	(0.097)	(0.098)	(0.098)	(0.111)	(0.112)	(0.111)	(0.090)	(0.090)	(0.090)
College (0-1)	-0.038	-0.049	-0.029	-0.076	-0.083	-0.082	0.009	0.007	0.017
	(0.111)	(0.111)	(0.111)	(0.129)	(0.128)	(0.130)	(0.113)	(0.112)	(0.113)
Age (1-5)	-0.047	-0.033	-0.047	0.052	0.058	0.041	-0.040	-0.037	-0.074**
	(0.046)	(0.040)	(0.040)	(0.054)	(0.054)	(0.045)	(0.042)	(0.041)	(0.037)
Salta	0.099	0.136	0.101	0.004	0.002	-0.008	-0.001	0.008	-0.061
	(0.120)	(0.112)	(0.120)	(0.147)	(0.147)	(0.141)	(0.111)	(0.111)	(0.103)
Santa Fe	0.134	0.156	0.130	-0.011	-0.011	-0.020	0.148	0.134	0.097
	(0.126)	(0.121)	(0.128)	(0.130)	(0.147)	(0.125)	(0.123)	(0.124)	(0.115)
Treatment constant	0.217	0.160	0.200	-0.048	0.029	0.131	0.184	0.209	0.143
	(0.198)	(0.214)	(0.164)	(0.218)	(0.181)	(0.220)	(0.191)	(0.148)	(0.193)
Observations	1,157	1,158	1,165	1,159	1,166	1,160	1,160	1,168	1,161
R-squared	0.129	0.125	0.124	0.165	0.165	0.164	0.094	0.090	0.093

Note: OLS regressions with the list experiment counts as dependent variables. All coefficients are interactions between each independent variable and the treatment. Table C7b below reports the coefficients for the non-interacted variables. The municipality of Tigre (the base category) was excluded. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table C7b: Political Services. OLS Regressions. Coefficients for the Control Lists (with interactions)

Control List	Political Campaigns			Political Rallies			Election Monitors		
Supporter (0-1)	0.170***	0.148*	0.207	0.533***	0.558***	1.016***	-0.309***	-0.324***	-0.250
	(0.062)	(0.078)	(0.165)	(0.081)	(0.108)	(0.210)	(0.067)	(0.089)	(0.168)
Tenure (0-1)	-0.192***	-0.210**		-0.126	-0.110		-0.002	-0.028	
	(0.071)	(0.087)		(0.083)	(0.090)		(0.076)	(0.089)	
Reciprocal (0-1)	0.089		0.102	0.005		0.201*	0.117		0.132
	(0.094)		(0.129)	(0.103)		(0.110)	(0.075)		(0.083)
Supporter*Tenure		0.032			-0.056			0.064	
		(0.115)			(0.155)			(0.127)	
Supporter*Reciprocal			-0.018			-0.538**			-0.065
			(0.176)			(0.224)			(0.182)
Female (0-1)	-0.002	0.005	0.006	0.042	0.051	0.036	0.044	0.035	0.044
	(0.058)	(0.058)	(0.059)	(0.069)	(0.069)	(0.068)	(0.061)	(0.061)	(0.061)
College (0-1)	0.000	-0.011	0.020	0.040	0.035	0.048	0.012	0.001	0.019
	(0.066)	(0.066)	(0.067)	(0.073)	(0.072)	(0.072)	(0.076)	(0.076)	(0.075)
Age (1-5)	0.052*	0.055**	0.015	-0.050	-0.047	-0.076**	0.029	0.027	0.030
	(0.028)	(0.028)	(0.025)	(0.033)	(0.033)	(0.030)	(0.028)	(0.027)	(0.024)
Salta	0.084	0.096	0.019	0.406***	0.416***	0.365***	-0.107	-0.109	-0.104
	(0.071)	(0.070)	(0.065)	(0.092)	(0.092)	(0.087)	(0.071)	(0.071)	(0.067)
Santa Fe	0.544***	0.541***	0.491***	0.748***	0.741***	0.708***	0.198**	0.212**	0.198**
	(0.078)	(0.080)	(0.077)	(0.078)	(0.079)	(0.078)	(0.085)	(0.085)	(0.079)
Control Constant	0.765***	0.841***	0.806***	1.018***	1.000***	0.892***	0.783***	0.893***	0.766***
	(0.135)	(0.101)	(0.154)	(0.144)	(0.113)	(0.148)	(0.111)	(0.096)	(0.112)
Observations	1,157	1,165	1,158	1,159	1,166	1,160	1,160	1,168	1,161
R-squared	0.129	0.124	0.125	0.165	0.165	0.164	0.094	0.090	0.093

Note: OLS regressions with the list experiment counts as dependent variables. Coefficients for the non-interacted variables. Interacted variables display on Table C7a. The municipality of Tigre (the base category) was excluded. Robust Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table C8: Perception of Job Stability

... if the incumbent mayor is not be reelected and the opposition wins? (treatment)	7.75
	(0.12)
	N=563
... ? (control)	8.15
	(0.11)
	N=568
Treatment effect	-0.41**
	(0.16)
	N=1131

Note: Two-sample t-test with unequal variance. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table C9: OLS Regressions. Perception of Job Stability

	Likelihood of staying in the job after the next election			
Treatment	-0.41**	-0.33**	0.30	-0.10
	(0.16)	(0.13)	(0.21)	(0.16)
Treatment* Mayor Voter			-1.08***	
			(0.27)	
Treatment* Mayor Party				-0.72**
				(0.29)
Mayor Voter			0.57***	
			(0.20)	
Mayor Party				0.49**
				(0.20)
Tenure		2.49***	2.53***	2.47***
		(0.20)	(0.21)	(0.20)
College		0.07	0.09	0.08
		(0.18)	(0.19)	(0.18)
Current Mayor		-0.93***	-1.03***	-0.98***
		(0.18)	(0.19)	(0.18)
Female		-0.16	-0.06	
		(0.17)	(0.15)	
Age		0.09	0.08	0.08
		(0.08)	(0.09)	(0.08)
Salta		-0.52***	-0.55***	-0.53***
		(0.20)	(0.21)	(0.20)
Santa Fe		-1.55***	-1.56***	-1.52***
		(0.20)	(0.20)	(0.20)
Constant	8.15***	7.70***	7.44***	7.60***
	(0.11)	(0.31)	(0.34)	(0.31)
Observations	1,131	1,125	1,059	1,125
R-squared	0.01	0.34	0.36	0.34

Note: Since the outcome variable is measured on a 0 to 10 scale, an alternative specification would be an ordered probit. The results were substantively equivalent using either specification so OLS results are reported for simplicity. The tenure variable takes the value of 1 for tenure employees, and zero otherwise. The college variable takes the value of 1 for employees with a college degree, and zero otherwise. The female variable takes the value of 1 for women, and zero otherwise. The age variable takes on values from 1 to 5, corresponding to respondents who are 18-25, 26-35, 36-45, 46-55 and more than 55. The municipality of Tigre (the base category) was excluded. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table C10: Perception of job stability, heterogeneous treatment effects

Characteristic	Whole Sample				Employees without Tenure			
	No	Yes	Difference in Effects	Difference in Effects	No	Yes	Difference in Effects	Difference in Effects
Have Tenure	-0.62**	-0.03	-0.60**	-0.59**				
	(0.25)	(0.12)	(0.28)	(0.27)				
	N=572	N=559	N=1131	N=1131				
Mayor Voter	0.34	-0.96***	1.30***	1.21***	0.65	-1.41***	2.06***	1.79***
	(0.27)	[0.21]	(0.35)	(0.34)	(0.46)	(0.31)	(0.55)	(0.48)
	N=417	N=647	N=1064	N=1064	N=184	N=348	N=532	N=532
Mayor Party	-0.18	-0.89***	0.71**	0.71**	-0.32	-1.26***	0.94*	0.99**
	(0.20)	(0.28)	(0.35)	(0.35)	(0.30)	(0.44)	(0.53)	(0.47)
	N=764	N=367	N=1131	N=1131	N=378	N=194	N=572	N=572
Municipal Dummies			NO	YES			NO	YES
					Employees with Tenure			
Mayor Voter					-0.16	0.18	0.34	0.38
					(0.18)	(0.17)	(0.25)	(0.25)
					N=233	N=299	N=532	N=532
Mayor Party					-0.07	0.25	0.32	0.34
					(0.16)	(0.18)	(0.24)	(0.24)
					N=386	N=173	N=559	N=559
Municipal Dummies							NO	YES

Note: In each panel, the first two columns show the average treatment effects calculated as the difference between the treatment and the control group (t-test with unequal variance). The last two columns show the difference in effects across respondents with and without each characteristic, and the last column includes controls for municipalities. Values in the left panel refer to the whole sample. Values in the right panels refer to employees with and without tenure. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

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Table C11: Do you think that your situation would be better, the same or worse?

. . . if the incumbent mayor is not be reelected and the opposition wins? (treatment)	0.13 (0.03) N=499
. . .? (control)	0.36 (0.02) N=528
Treatment effect	-0.23*** (0.03) N=1027

Two-sample t-test with unequal variance. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table C12: OLS Regressions. Likelihood of change

	Likelihood of changes in the job after the next election			
Treatment	-0.23***	-0.23***	-0.10**	-0.15***
	(0.03)	(0.03)	(0.05)	(0.04)
Treatment* Mayor Voter			-0.21***	
			(0.07)	
Treatment* Mayor Party				-0.26***
				(0.08)
Mayor Voter			0.09*	
			(0.05)	
Mayor Party				0.21***
				(0.05)
Tenure		-0.05	-0.05	-0.06
		(0.05)	(0.05)	(0.05)
College		-0.10**	-0.13***	-0.10**
		(0.04)	(0.04)	(0.04)
Current Mayor		0.02	0.05	-0.00
		(0.05)	(0.05)	(0.05)
Female		0.01	0.02	0.01
		(0.04)	(0.04)	(0.04)
Age		-0.01	-0.01	-0.02
		(0.02)	(0.02)	(0.02)
Salta		0.17***	0.18***	0.17***
		(0.05)	(0.05)	(0.05)
Santa Fe		0.08*	0.08*	0.10**
		(0.05)	(0.05)	(0.05)
Constant	0.36***	0.35***	0.29***	0.31***
	(0.02)	(0.07)	(0.08)	(0.07)
Observations	1,027	1,021	966	1,021
R-squared	0.04	0.07	0.08	0.09

Note: The results were substantively equivalent when using ordered probit so OLS results are reported for simplicity. The tenure variable takes the value of 1 for tenure employees, and zero otherwise. The college variable takes the value of 1 for employees with a college degree, and zero otherwise. The female variable takes the value of 1 for women, and zero otherwise. The age variable takes on values from 1 to 5, corresponding to respondents who are 18-25, 26-35, 36-45, 46-55 and more than 55. The municipality of Tigre (the base category) was excluded. Robust standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C13: Likelihood of changes for better or worse, heterogeneous treatment effects

Characteristic	No	Yes	Difference in Effect	
Have Tenure	-0.29***	-0.16***	-0.13**	-0.13*
	(0.05)	(0.05)	(0.07)	(0.07)
	N=521	N=505	N=1026	N=1026
Mayor Voter	-0.10**	-0.31***	0.21***	0.21***
	(0.05)	(0.05)	(0.07)	(0.07)
	N=381	N=590	N=971	N=971
Mayor Party	-0.15***	-0.40***	0.25***	0.25***
	(0.04)	(0.07)	(0.07)	(0.07)
	N=701	N=326	N=1027	N=1027
Municipal dummies			NO	YES

Note: First two columns show the average treatment effects calculated as the difference between the treatment and the control group (t-test with unequal variance). The last two columns show the difference in effects across respondents with and without each characteristic. The last column includes controls for municipalities. Robust standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

References

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NOTES

1. The survey methodology and the survey instrument were approved under Columbia University IRB protocol IRB-AAAE9968.

2. The survey in Salta was scheduled to be conducted in November and December 2010, but changes in the electoral calendar generated by the death of Néstor Kirchner (the main presidential pre-candidate) made the authorities in Salta reluctant to allow me to conduct the survey on the scheduled dates. Therefore, the survey in Salta was administered after the April 2011 local elections (when both the mayor and the governor were reelected), but before the October 2011 national elections.

3. Rates calculated according to the American Association of Public Opinion Research.

4. Literacy rates are very high in Argentina; there was no concern that the respondents would not be able to fill out Part B of the questionnaires by themselves. According to the Argentina 2010 census, only 1.96 percent of the total population older than age 10 is illiterate.

5. In a few cases, respondents asked enumerators to fill out Part B for them. Enumerators were instructed to agree with these requests, but Part B was still stored in the cardboard box when the survey was completed.

6. With this methodology, one enumerator was identified who was fabricating the responses. Her full set of interviews was replaced by finding and interviewing her original set of respondents.

7. Note that voting is mandatory in Argentina, and turnout is usually above 70 percent.

8. In Spanish: "*¿Se acuerda cómo se enteró de este trabajo?*"

9. Author interview, Santa Fe, August 16, 2011.

10. In Spanish: "*A continuación le voy a leer una lista de criterios que se pueden utilizar a la hora de contratar empleados. Dígame, por favor, cuáles de ellos cree Ud. que son criterios importantes a la hora de contratar un empleado nuevo en el área que Ud. trabaja. Dígame, por favor, si Ud. cree que importan mucho, importan, importan poco o no importan nada.*" The question was follow by a table that included the criteria: "*Experiencia laboral/Antecedentes laborales, Estudios, Afiliacion partidaria, Ideología política, Conexiones personales.*"