Political Screening:
Theory and Evidence from the Argentine Public Sector[*]

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Abstract

Politicians can benefit by ensuring that public sector positions requiring political services are occupied by partisans. We study a model in which this political screening is achieved by varying the amount of required political services and associated compensation in otherwise similar positions. Past vote shares reflect the population share of partisans, and we predict a U-shaped relationship between an employee's current salary and the incumbent politician's vote share at the time of hiring. We test for this effect using individual data from a large national income survey from Argentina, a country with widespread political patronage. The results are consistent with the model, showing that political conditions at the time of hiring have long-lasting effects on public employees' wages.

1 Introduction

Every incumbent government has at its disposal an army of potential political activists: the public employees whose livelihood is tied to the state (Wilson, 1961). In environments such as the present-day US, civil service laws prevent incumbent politicians from requiring political services from anyone but top-level political appointees. In other systems, a large portion of public employees from garbage collectors to ministry bureaucrats are in some form of "patronage contract" with the administration. Providing political services such as campaigning for votes or favoring politically connected contractors in procurement decisions is an implicit or explicit part of the job requirements.

This paper argues that political screening - the separation of employees based on political preferences - is an important but little-understood problem in public employment. We provide a theory of political patronage which highlights political screening through appropriately designed patronage contracts. The model predicts a novel relationship between past vote shares and current public-sector salaries, and we find support for this prediction in Argentina.

In line with conventional wisdom and previous studies, we treat patronage as an exchange relationship where public employees receive benefits in return for political support.[1] However, we recognize that there is heterogeneity in individuals' willingness to support a given politician. In this environment, different individuals enter into different patronage contracts: those willing to provide more support ("partisans") self-select into positions where this is valued more. As an example, consider a secretary applying for a ministry job who may be qualified to work in several different offices within the ministry. This individual may choose between the different jobs based on his/her preferences regarding the salary offered, the job requirements (such as hours of work), and the amount of political services required. Because a secretary who likes the incumbent is willing to provide political services for lower compensation, politicians should in turn take this into account when designing the positions. This idea, which we formalize in Section 4, implies that politicians will create different types of positions, and individuals will self-select between them based on partisanship. This system of patronage contracts fulfills a political screening function that enhances the effectiveness of large-scale patronage.[2]

In our model, political preferences regarding the incumbent politician are private information at the time of hiring. However, the population share of partisans is known from vote

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[1] The related literature is reviewed in Section 2, and Section 3 provides anecdotal evidence on patronage contracts from Argentina and the mid-20th century US.

[2] Screening contracts may complement other screening mechanisms (e.g., personal contacts) typically used on a smaller scale. See Section 3 as well as our empirical results on referrals.

shares in the previous election, and this will be reflected in the optimal structure of patronage contracts. We therefore predict an association between partisan shares at the time of hiring and current salaries. As we show in Section 4.2, there are two opposing effects on the salary of the average public employee. A negative *incentive effect* results because political support is more expensive when the share of partisans is low. This is both because support must be purchased predominantly from nonpartisans, and because partisans must be given large rents to choose positions designed for them. As the population share of partisans increases, the support of nonpartisans becomes less essential, allowing the politician to reduce everyone's salary.[3] A higher partisan share also has a positive *composition effect* because the average employee is more likely to be a partisan. Since partisans receive rents, this raises observed salaries. Under some assumptions these opposing effects imply a U-shaped relationship between vote shares and public wages. Employees hired in environments with low or high partisan shares earn more than others.

In Section 5, we test for the association between partisan shares and public-sector salaries predicted by the model. We take advantage of a large national income survey from Argentina, a country known to have a large public sector with well-developed patronage systems, where the authority to set public wages is decentralized to the province level. To measure partisan shares at the time of hiring, we match to each public employee in our sample the vote share of the current provincial governor at the time the employee was hired by this administration. Our identification strategy relies on within-province variation in the current salary of employees hired by the same governor after different elections. This approach, described in Section 5.2, allows us to control for *all* unobservable province-level differences affecting current wages, as well as a wide range of individual characteristics. Consistent with the theory, we find that the salary of the average public employee has a robust U-shaped relation to the vote share of the incumbent governor at the time of hiring. Employees hired following an election with a vote share between 50-60% receive the lowest salaries. A ten percentage points lower or higher vote share yields a 3-5% higher salary. These findings survive a variety of robustness checks, and are driven by employees not hired through referrals, which provides support for the information asymmetry highlighted in the model. We argue that standard models of politically targeted policies do not explain the data.[4]

Overall, our results show that the nature of patronage contracts is important in understanding wages in public sectors characterized by extensive patronage. In particular, we

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[3]In practice, the political support required from nonpartisans seems to be positive: In a survey of Argentine public employees, Oliveros (2011) finds that even workers who identify themselves with the *other* party provide political services for the incumbent politician.

[4]In particular, a swing-voter model implies the opposite relationship, with wages being *highest* around a 50% vote share.

show that patronage contracts that have a role in screening employees result in political conditions at the time of hiring having long-lasting impacts on public wages. In Section 6, we discuss some tentative policy implications of our model.

2 Related literature

This paper contributes to several literatures in economics and political science. Informational problems in government are at the heart of the political agency literature (see Besley (2006) for an overview). In this research, the relationship between politicians and public employees is a relatively under-studied area, but the question of how the right types of bureaucrats are selected figures prominently in Besley and McLaren (1993), Prendergast (2007), and Ujhelyi (2014a).[5] In these papers the problem is selecting bureaucrats who place the optimal weight on social welfare. Our paper offers an interesting contrast, since here the politician is trying to select employees with the right type of *political* preference. We briefly take up the issue of how these two dimensions might interact in Section 4.3.

There are few formal models of political patronage and they generally focus on moral hazard issues. These arise when the politician cannot observe the actions of his clients (e.g., how they vote), or faces difficulties in making credible promises to them (Robinson and Verdier, 2003; Stokes, 2005; Enikolopov, 2007; Keefer and Vlaicu, 2008). In many cases, however, political support is readily observable, so that moral hazard is not an issue. This is likely to be the case when political support takes the form of campaigning on behalf of the politician (e.g., attendance at rallies, turning out to vote, bringing friends or neighbors to party meetings, etc.), giving identifiable campaign contributions, or making politically motivated decisions in office (e.g., awarding procurement contracts to "friendly" contractors). In other cases, moral hazard may be impossible to resolve. For example, in democratic elections patrons will necessarily lack information on the political support received from their clients in the form of votes. In these cases, the adverse selection problem we study may be more relevant.[6,7]

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[5] See also Di Tella and Schargrodksy (2003), who study the effect of public wages on the honesty of bureaucrats, and Rauch and Evans (2000) and Gailmard and Patty (2007) who argue that a well-designed civil service system allows the self-selection of skilled bureaucrats.

[6] In some previous studies, political preferences matter but are readily observable so the adverse selection problem we study does not exist. This is the case when political patronage is based on closely knit social networks: see Clark (1975) on the Irish in Boston, and Banerjee and Pande (2007), Munshi and Rosenzweig (2008), and Iyer and Mani (2012) on Indian castes. See Padro i Miquel (2007) for a theory of targeted redistribution under this assumption.

[7] The empirical political science literature on patronage is surveyed in Kitschelt and Wilkinson (2007). Our paper builds on the studies by Calvo and Murillo (2004) and Remmer (2007) who investigate patronage in the Argentinian public sector using province-level data. Relative to these studies, our work benefits from

The theory of screening contracts has been applied to a wide range of issues from procurement and regulation to optimal income taxation and labor contracts (see Bolton and Dewatripont (2005, Ch 2) for a textbook treatment). To our knowledge, the application to political patronage is new.

There is a large literature on the public-sector labor market, focusing mostly on the public-private wage gap (for a survey, see Gregory and Borland, 1999). Only a handful of papers explore the political motivations behind public wages. In important work related to ours, Borjas (1980, 1986) argued that wage differentials among US government employees in the 1970s could be explained by the political importance of the constituents they served as well as their ability to provide those services. These political considerations were significant in explaining wage differences in a cross section of public employees working for different federal agencies (Borjas, 1980) or for different US state governments (Borjas, 1986). In contrast to Borjas's general model of political demand and supply, we focus specifically on the mechanism of political patronage. Our model highlights the role played by public employees' political preferences, and implies a novel relationship between current wages and the incumbent's vote share at the time of hiring. The Argentine data we use offers an interesting complement to the results regarding wages in the US government.[8]

More generally, our work is related to the literature investigating politicians' strategies for eliciting support through targeted redistribution (e.g., Cox and McCubbins, 1986; Lindbeck and Weibull, 1987; Dixit and Londregan, 1996, 1998; Manacorda, Miguel and Vigorito, 2011). We show that the incumbent's vote share at the time of hiring has an interesting long-run effect on one such strategy, the public wages embodied in patronage contracts.

Finally, our finding that vote shares at the time of hiring have long-lasting impacts on wages also relates this paper to work studying the long-run impact of economic conditions on workers starting a new job (e.g., Beaudry and DiNardo, 1991; Oreopoulos, von Wachter, and Heisz, 2012). To our knowledge, the long-run impact of political conditions on public employees has not been studied previously.

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an explicit theoretical framework and individual-level data, which allows us to be more precise about the mechanisms we explore.

[8]Alesina, Baqir and Easterly (2000) argue that in US cities, public employment serves redistributive purposes. Matsusaka (2009) studies the effect of citizen initiatives in US cities using a model in which the number of city employees reflect patronage but their wages, set through union bargaining, do not. While the current US civil service laws indeed shelter city employees' pay from political influence, Argentine provincial governors face no such constraints in setting wages.

3   Screening and the "patronage contract"

While every public sector has a certain number of political appointees, what is striking in many cases is (i) how many employees are *actually* providing some form of political service, and (ii) how explicit these arrangements are. The sociological study of Auyero (2000) documents how, in a poor suburb of Buenos Aires, the Peronist patronage network extends all the way down to social workers and trash collectors. Similarly, "in Illinois, as elsewhere, public employees like parks and sanitation workers, often formed the backbone of the teams that rang doorbells, collected signatures on petitions and gathered crowds for political candidates." (Tolchin, 1990). In the Chicago machine in the middle of the 20th century,

> "[e]veryone was expected to work for the party. High level office-holders [...] were excused from precinct work of the door-to-door variety, but the alderman expected them to give service in the form of free legal advice to the people of the ward. Job holders also had to buy tickets to various party fundraising events and were required to contribute a percentage of their salaries to the ward organization. Generally, they were assessed between 2% and 3% of their total pay. Patronage workers were held strictly accountable for their political performance, but not for their performance on the job." (Freedman, 1994, 40).

The *Rutan v. Republican Party of Illinois* case of the Supreme Court provides an example of a particularly explicit patronage contract. In the 1970s, applicants to public-sector jobs in Illinois were required to sign a "promotion form" that included the following questions:

> "Would you be willing to become an active Sangamon Country Republican Foundation Member? (The foundation is a voluntary, financial assistance organization)
> Would you be willing to canvass and work your precinct or neighborhood for candidates the Central committee recommends as qualified for local, state, and national offices?" (Freedman, 1994, p106).

In other cases, the patronage contract is more implicit. In the Buenos Aires patronage network "'Gratitude' goes without saying because it almost always comes without saying. [...] On a few occasions, attendance [at a political rally] is explicitly required. Yet such requests are seldom phrased as orders, obligations; rather, they are usually phrased as invitations." (Auyero, 2000, 161) As one person employed as a social worker put it, "I know that I have to go to [the politician's] rally in order to fulfill my obligation to her, to show my gratitude.

Because, if I do not go to her rally, then, when I need something, she won't give it to me."
(160) Similarly, during litigations against the patronage machine of Nassau County, NY,
workers described how information about the required political support was passed along
among co-workers without explicit threats or instructions from party representatives.[9] For
example, employees told each-other about the requirement to pay an assessment of 1% of
each paycheck to the party. This "norm" was specific enough to include the provision that
the payments should be made in lump sums, not in installments. Everyone obeyed, even
though most workers were never directly approached or threatened by a party official.

Because political preferences affect employees' willingness to participate in these patron-
age contracts but are rarely observable, hidden information is an inherent problem in every
patronage system. This seems well understood in the Peronist network in Buenos Aires,
where potential clients use attendance at political rallies to signal their political loyalties
to the organizer: "Attendance at rallies is also considered a way of showing a broker that
one is loyal, responsible, and ready to help out when needed - and therefore deserving of a
job if and when one becomes available." (Auyero, 2000, 163) In the US patronage machines
of the mid-20th century, political screening was made easier by voter registration being a
matter of public record. In the Rutan case mentioned above C. Rutan was repeatedly denied
promotion at the Illinois Department of Rehabilitation Services. When she asked why she
was not being promoted, "she was told that her voting record had been checked and that
her name didn't "clear" the governor's office." (Freedman, 1994, 105). In most settings, this
type of information is not available, and checking individual applicants' backgrounds would
be very costly. In large-scale patronage systems information is likely to remain asymmetric,
and this will affect the patronage contracts between politicians and public employees.

When employees have unobservable political preferences, patronage contracts (and, more
generally, public sector jobs) have a screening function. Any given position with a combina-
tion of political requirements and associated pay will induce the self-selection of specific types
of workers. E.g., jobs with more political requirements will be more attractive to partisans.
When politicians realize this, a dual system should emerge, with some positions specifically
targeting partisans and others targeting nonpartisans through their mix of pay, expected
performance, and political requirements. This is reminiscent of a duplication of positions
that is common in several European countries as well as in the European Commission. Here,
a large ministerial cabinet often coexists with a non-political bureaucracy performing similar
tasks - a system that is sometimes referred to as "parallel bureaucracies" (Cini, 1996, Ch
4). Similarly, many bureaucracies, including those of the US federal and local governments,

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[9]The source on Nassau County patronage is Freedman (1994, Ch 4) as well as the series "Hempstead
Party Favors" in Newsday, September 22-24, 1991.

feature a combination of political and civil service positions. Presumably, the former are designed to attract partisans of the incumbent politicians, while civil service positions are a better match for those less willing to provide political services.

Our model below shows how such a dual structure might emerge even in weakly institutionalized environments.[10] A vivid example of partisan and nonpartisan contracts in such settings is provided by Sorauf's (1956) famous study of patronage among highway workers in Pennsylvania in the 1950s. There, highway maintenance crews consisted of two types of jobs: "caretakers" and "laborers," *both* of which were politically appointed. Although the professional qualifications required for the two jobs appear to be similar, caretakers were better paid and more politically active. In Sorauf's sample, 88% of interviewed caretakers participated in a political campaign, all of them contributed money at least once to the party in office, and their median contribution was $15. By contrast, 33% of the laborers campaigned and 72% contributed money, with a median contribution of $2-3. Sorauf's summary echoes our model below:

> "[t]he data [...] suggest that there are many levels of patronage with varying degrees of political effectiveness. Even within the narrow range of this study, two patronage systems emerge - the more effective system of appointments to the caretakers' positions and the appointments to other jobs. In the better-paid caretakers' jobs the party secured markedly greater political activity, rewarded more loyal partisans, and extracted greater party effort." (p1055).

Of course, systematic evidence that would allow the identification of partisan and nonpartisan positions and corresponding wage differentials is scarce. Our work below provides an indirect way to test for political screening by exploiting the implications of our model for the optimally designed system of patronage contracts.

4 The model

4.1 Setup

Consider the problem of a politician in office who is designing a patronage system to fill medium- to low-level public-sector jobs.[11] Suppose that each of these jobs can constitute a

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[10]While in established democracies civil service regulations prohibit political services in the nonpartisan positions, in other environments any official classification may not be informative about the actual duties performed. Case studies suggest that in Argentina more than 50% of a typical agency's workforce works in positions that are effectively partisan (Bambaci et al., 2007).

[11]High-level positions where performance is crucial for policy and electoral success are likely to be filled by hand-picked appointees whom the politician knows personally. The screening system we consider below

"patronage job" that Wilson (1961, p370) defines as positions "the pay for which is greater than the value of the public services performed. This 'unearned increment' permits the machine to require that the holder perform party services as well." Thus, we assume that employees have the opportunity to provide some political support $x \geq 0$ to the politician. The variable x can correspond to different types of behavior, from taking decisions favoring the politician while performing the job to outright campaigning. We assume that the level of support is contractible: the politician can require support x as a condition of employment, and if the applicant accepts, the contract is perfectly enforced. As described in Section 3, the politician could contract on x explicitly, or implicitly, by maintaining an organizational culture inside an institution, such as a ministry, that requires a certain amount of partisan support x. The existence of such expectations may be common knowledge among potential applicants even if they are not part of the formal job requirements.

The politician's payoff in period t from an employee who provides political support x^t and who earns salary s^t is

$$v_t(x^t) - s^t, \tag{1}$$

with v_t increasing and concave. Thus, the politician values political support, especially when it is scarce, but does not like to pay for it, perhaps because he has less money to spend on providing public goods. Political support may be valued because it increases future electoral success, or it might increase the utility derived from holding office (e.g., by raising the politician's status). Note that we allow the value of political support to change over time. For example, changes in the political environment can affect the politician's tradeoff between providing public goods and targeted transfers, as in the standard models of pork-barrel politics (e.g., Dixit and Londregan, 1998). The politician's payoff in (1) is a reduced form that is consistent with several underlying models of political competition. This makes our model more general, and highlights the fact that the identification strategy in our empirical work is not contingent on a particular model of elections. As we make clear below, nothing important changes if the function v_t is stochastic.[12]

There is a large population of applicants, each of whom can be of two type, either Partisan or Nonpartisan ($i = P, NP$) and this is private information. Partisans and nonpartisans differ in their willingness to provide political support to the incumbent politician. We capture this by assuming that partisans have a lower cost and lower marginal cost of providing political support: $c_P(x) < c_{NP}(x)$, $c'_P(x) < c'_{NP}(x)$, where these cost functions are increasing and

is different.

[12]We do not assume that the politician faces a binding budget constraint in setting public wages. While such constraints may be relevant for a local politician, our empirical work studies provincial governors who have considerable latitude in redistributing resources across departments and projects. If creating additional positions is costly, the payoff in equation (1) received from each employee will determine how many are hired.

convex. In period $t = 0$, partisanship is private information, but the population share of partisans, $p \in (0, 1)$, is known (for example, from the politician's vote share in the last election).[13]

This definition of partisanship is consistent with several interpretations. Partisans may like the politician because of personal or ideological reasons, or they (or their families) may have ties to the party machine. Alternatively, "partisans" may in fact be independent voters while "nonpartisans" have ties to *another* party's political machine. In this case, the higher cost of nonpartisans from providing political support comes from the fact that they have to renege on their engagement with the other party. Finally, a person's cost of supporting the politician may be lower simply due to the act of having voted for him in the past. Mullainathan and Washington (2009) show that such cognitive dissonance effects are empirically important. In the extreme, this implies that even if ex ante voters view candidates as being identical and vote randomly, after the election the population will be divided into partisans and nonpartisans.

An applicant's payoff in period t from working in a job that requires political support x^t and pays salary s^t is $s^t - c_i(x^t)$. If he works somewhere else, the applicant gets an outside option worth u^t, such as a wage in the private sector. As we discuss below, the results we derive are robust to whether this outside option is deterministic or stochastic.[14]

4.2   Optimal patronage contracts

Since politicians are typically in office for several years, contracts may be signed for the long term. In period $t = 0$, the politician commits to a contract which specifies the political support, output and corresponding salary in each future period, contingent on the relevant parameters of the model. Let $(\mathbf{x}, \mathbf{s})$ denote a contract for T periods, where $\mathbf{x} = (x^0, ..., x^T)$ and $\mathbf{s} = (s^0, ..., s^T)$. In optimum, these contracts will depend on the future political environment (captured by v^t) and the outside option u^t of the employee. For example, a politician may require more political support in periods where his valuation for such support is higher, or may promise to pay the employee more if the outside option increases.[15]

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[13]In reality, people may differ in other relevant dimensions, for example some people may like the politician but may be unwilling to engage in patronage. Here, we ignore these differences and focus on basic political preferences. In a country where voting is compulsory, such as Argentina, vote shares provide a measure of partisanship that is both accurate and easily accessible to a politician. There are of course other proxies politicians may be able to use in specific settings, and we will control for some of these in our empirical work below.

[14]The assumption that partisans' and non-partisans' outside options are equal is not crucial. On the other hand, if the two types *only* differed in their outside option, the screening problem would disappear.

[15]Section 4.3 and the Online Appendix discuss the issues that arise if parameters of the model evolve stochastically *and* contracts cannot be written to depend on them.

At the start of the game, the politician decides on a set of contracts $(\mathbf{x}, \mathbf{s})$ to be offered. Multiple contracts can be interpreted, e.g., as corresponding to separate positions within the bureaucracy, as in the examples discussed in Section 3. Next, an applicant is randomly selected from the population to decide whether to accept one of the contracts offered or to reject all of them. Accepted contracts are enforced, i.e., the politician commits to not exploiting the information gained when an applicant reveals his type by selecting a particular contract. Similarly, employees commit to abide by the terms of a signed contract. However, once hired, the employee may decide to quit in any future period, with no possibility of being re-hired.

As a benchmark, consider the first best case in which the politician observes the worker's type, and so can offer different contracts to partisan and nonpartisan applicants. For a given type $i = P, NP$, the politician maximizes for each period t his payoff $v_t(x_i^t) - s_i^t$ subject to the participation constraint $s_i^t - c_i(x_i^t) \geq u^t$. The first-best pair of contracts $(\hat{\mathbf{x}}_P, \hat{\mathbf{s}}_P)$ and $(\hat{\mathbf{x}}_{NP}, \hat{\mathbf{s}}_{NP})$ is characterized by $c_i'(\hat{x}_i^t) = v_t'(\hat{x}_i^t)$ and $\hat{s}_i^t = c_i(\hat{x}_i^t) + u^t$. In the first best, partisans provide a higher level of support, and both agents are paid according to the services they provide and their outside options. Because types are observable, the population share p of partisans plays no role in the equilibrium contracts.

With asymmetric information, the politician's optimal strategy is to offer multiple contracts, and give partisans and nonpartisans the incentive to self-select into the appropriate positions.[16] These optimal contracts, described below, achieve the political screening of public employees. In some cases, real-world politicians may also choose to reduce the informational asymmetry by using referrals and personal connections to screen applicants individually. However, in a large public sector with many applicants, this might be very costly, and draw considerably more public scrutiny than the contracting solution based on self-selection. We therefore model a situation where such direct screening is not feasible and information remains asymmetric. Our empirical work below will control for employees hired through referrals.

Letting δ denote the common discount factor, the optimally designed screening contracts solve the following problem:

$$\max_{\substack{\mathbf{x}_P, \mathbf{s}_P \\ \mathbf{x}_{NP}, \mathbf{s}_{NP}}} \sum_{t=0}^{T} \delta^t \left[p(v_t(x_P^t) - s_P^t) + (1-p)(v_t(x_{NP}^t) - s_{NP}^t) \right],$$

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[16] For now, we will simply assume that it is optimal to hire both partisans and nonpartisans. We justify this assumption after describing our results.

subject to the incentive constraints

$$\sum_{t=0}^{T} \delta^t \left[s_P^t - c_P(x_P^t) \right] \geq \sum_{t=0}^{T} \delta^t \left[s_{NP}^t - c_P(x_{NP}^t) \right] \qquad (IC_P)$$

$$\sum_{t=0}^{T} \delta^t \left[s_{NP}^t - c_{NP}(x_{NP}^t) \right] \geq \sum_{t=0}^{T} \delta^t \left[s_P^t - c_{NP}(x_P^t) \right], \qquad (IC_{NP})$$

and two series of participation constraints

$$\sum_{\tau=t}^{T} \delta^{\tau-t} [s_P^\tau - c_P(x_P^\tau)] \geq \sum_{\tau=t}^{T} \delta^{\tau-t} u^\tau \qquad (PC_P)$$

$$\sum_{\tau=t}^{T} \delta^{\tau-t} [s_{NP}^\tau - c_{NP}(x_{NP}^\tau)] \geq \sum_{\tau=t}^{T} \delta^{\tau-t} u^\tau \qquad (PC_{NP})$$

for $t = 0, ..., T$.

The incentive constraints ensure that in period $t = 0$ the agent chooses the contract designed for him. The participation constraints ensure that, in any period t, applicants are willing to work in the public sector even if that involves providing political support.

In optimum, the nonpartisans' participation constraints and the partisans' incentive constraint bind. This argument is relatively standard and left to the Appendix. Solving the resulting problem, we find that the optimal pair of contracts is defined by the following equations

$$x_P^t = \hat{x}_P^t \qquad (2)$$

$$c'_{NP}(x_{NP}^t) = (1-p)v'_t(x_{NP}^t) + pc'_P(x_{NP}^t) \qquad (3)$$

$$s_{NP}^t = c_{NP}(x_{NP}^t) + u^t \qquad (4)$$

$$s_P^t = c_P(x_P^t) + u^t + c_{NP}(x_{NP}^t) - c_P(x_{NP}^t) \qquad (5)$$

In this screening solution, partisans provide the efficient level of political support in every period (equation (2)), and receive a high salary which not only compensates them for the effort provided and the foregone outside option, but also contains a rent of $(c_{NP}(x_{NP}^t) - c_P(x_{NP}^t))$ (equation (5)). This rent gives partisans an incentive to self-select into the high-x contract designed for them. To minimize the partisan rent, the politician distorts the support from nonpartisans relative to the first best ($x_{NP}^t < \hat{x}_{NP}$ from equation (3)). This allows him to offer a lower salary in the nonpartisan contract (equation (4)) and hence receive the partisans' political services at a lower price.

Comparing equations (4) and (5), we see that partisans get a higher salary than nonpar-

tisans: $s_P^t - s_{NP}^t = c_P(x_P^t) - c_P(x_{NP}^t) > 0$. This "partisan premium" reflects the different levels of political support required in the positions designed for partisans and nonpartisans, as well as the rents associated with the partisan contracts.[17]

This simple model implies that in any period t, public-sector salaries are affected by the initial partisan share p: in equation (3), x_{NP}^t is a function of p, and in turn both s_P^t and s_{NP}^t depend on x_{NP}^t. Without information on individual partisanship, the salaries s_P^t and s_{NP}^t are unobserved. However, for a given cohort of employees, we can observe the period-t salary of an average employee (an employee chosen at random): $\tilde{s}^t(p) \equiv p s_P^t + (1-p) s_{NP}^t$. Using (4) and (5), we have the following result.

Proposition 1 *For a given cohort of employees, the period-t salary of the average worker depends on the population share of partisans at the time of hiring. In particular,*

$$\tilde{s}^t(p) = p[c_P(x_P^t) - c_P(x_{NP}^t(p))] + c_{NP}(x_{NP}^t(p)) + u^t. \tag{6}$$

Proposition 1 predicts that partisan shares at the time of hiring will have long-lasting effects on workers' salaries. How do salaries change as a function of the initial share of partisans? Taking the derivative of (6) and using equations (2) and (3), we get

$$\frac{\partial \tilde{s}^t}{\partial p} = [c_P(x_P^t) - c_P(x_{NP}^t)] + (1-p)v'(x_{NP}^t)\frac{\partial x_{NP}^t}{\partial p}, \tag{7}$$

The term in brackets is a positive *composition effect*. As the share of partisans p rises, more partisans are hired, and because partisans' salary contains a premium, this raises the observed average salary. The second term is a negative *incentive effect*. When p is low, the politician is forced to rely on the expensive support purchased from nonpartisan workers. This also requires giving high salaries to partisans as an incentive to choose the contract designed for them. As p rises, the politician can substitute the support of nonpartisans with the cheaper support of partisans ($\frac{\partial x_{NP}^t}{\partial p} < 0$), allowing all salaries to be reduced.

The presence of the positive composition effect and the negative incentive effect in (7) creates the possibility for nonmonotonicity. When the share of partisans p is *large*, the incentive effect vanishes and (7) is always positive. When almost all employees are partisans, a small change in partisan shares has a negligible effect on the screening contracts: the composition effect dominates, and the average salary is increasing. On the other hand, when p is *small*, the composition effect is small as well. This is because for low p, nonpartisans

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[17]For any given t, equations (2)-(5) are equivalent to the second best contract of the corresponding one-period screening problem. This is a version of the "false dynamics" result for long-term contracting in deterministic environments (see Laffont and Tirole, 1993).

provide more support, hence the difference between the salaries of partisans and nonpartisans is small.[18] This creates the possibility of a U-shaped effect of p on the average salary: when the negative incentive effect is large, (7) is negative for low values of p. All else equal, this is more likely to be the case when the marginal value of political support $(v'(x_{NP}^t))$ is large, or when the politician is quick to replace the support from nonpartisans with that of partisans as the share of partisans rises ($\frac{\partial x_{NP}^t}{\partial p}$ is large in absolute value).

It is worth emphasizing that the nonmonotonicity uncovered here depends crucially on the screening contracts resolving the adverse selection problem faced by the politician. Only then do we have both a negative incentive and a positive composition effect on the average wage. For example, if the politician had perfect information on applicants' types, everyone would be given the first best contract. The incentive effect would disappear, and the effect of p on average salaries would be monotonic (a composition effect). The nonmonotonicity we find in our empirical work below rejects this alternative in favor of screening contracts.

In deriving Proposition 1, we have assumed that both partisan and nonpartisan workers are employed in the optimal screening solution. In general, this need not be the case: the politician could find it optimal to offer a single contract targeted towards partisans, and to completely screen out nonpartisans from the public sector. We have two reasons to believe that the two-contract solution is relevant in our empirical context. First, in a survey of Argentine public employees, Oliveros (2011) finds that even employees who identify with and vote for the party in opposition provide political services to the incumbent politician. Not only are nonpartisan employees not screened out from the public sector, they engage in political activities on behalf of their employer. Second, our empirical results below document a robust association between partisan shares and wages. By contrast if only partisan employees were hired, they would all receive the first best contract, and p would have no effect on wages. Thus, both of these patterns support our focus on two-contract screening solutions.

4.3   Discussion of modeling assumptions and extensions

The above model makes a first step in applying the theory of screening contracts to the study of the patronage relationship. In doing so, it makes a number of simplifying assumptions.

First, we have assumed that the politician does not attempt to exploit the information gained from observing employees' self-selection. This may be supported by a reputational argument: if contracts were renegotiated, this would jeopardize the ability to screen future cohorts of employees. Studying this mechanism explicitly would provide an interesting ex-

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[18]Formally, $\frac{d}{dp}[c_P(x_P^t) - c_P(x_{NP}^t)] = -c_P' \frac{\partial x_{NP}^t}{\partial p} > 0$.

tension and would require considering relational contracts as in Levin (2003). Second, we have modeled the provision of political services as a cost incurred by employees that does not depend on the political environment. This allows us to assume that the share of partisan applicants to the public sector reflects their share in the population. More generally, individuals may obtain different benefits from helping their preferred politician depending on the fraction of partisans p, which would create an additional channel through which this variable might influence the optimal contracts. Third, we have considered a deterministic environment. In the Online Appendix, we discuss the issue of extending the model to uncertainty regarding applicants' outside options or political preferences (partisanship). Fourth, we did not consider the role of employees providing useful public services while working on the job. In the Online Appendix, we show how to incorporate this without affecting the above results. We also show that simple extensions along these lines can lead to interesting implications regarding the impact of patronage on the efficiency of public services.

All these extensions present interesting possibilities for future research. However, as we show below, our simpler model above is sufficient to explain the pattern of public wages in the Argentine provinces.

5   Testing for partisan effects in public-sector wages in Argentina

In this section, we take the theory to the data. After describing the background and our data, we present estimates of the relationship between partisan shares and public sector wages.

5.1   Background and data

To test for the effect of partisan shares at the time of hiring on the salary of public employees, we take advantage of a large income survey conducted in the twenty four Argentine provinces in 2001. Argentina provides a perfect laboratory to test for patronage in public-sector wages for a number of reasons. First, patronage and the use of public employment for political gain has been extensively documented in the sociology and political science literature (for example, Auyero, 2000, Brusco et al. 2004, Calvo and Murillo, 2004, Stokes, 2005, Remmer, 2007, Szwarcberg, 2012). In particular, Argentine politicians face few constraints in hiring and firing public employees. As a vivid example, in 2000, when discussing the possibility that mass layoffs in the *federal* public sector (where civil service rules are in fact more established than at the provincial level we study) might allow them to be replaced with more loyal

employees, the current president, then a House representative, explained that this could be done without legislative action. "I accept the rationale for being elected into office and wanting to have collaborators that ministers and secretaries can trust, but for this we do not need [new legislation]. We just need to modify the policies that regulate the Directors so that they can make new hires, discussing then the possibility of severance packages"[19] In the Online Appendix, we provide direct evidence on the high turnover of public employees and its correlation with the electoral cycle.

A second important feature of the Argentine case is that provincial public-sector employment is regulated by provincial institutions, therefore control over personnel and wages is the exclusive responsibility of the local government. In contrast to countries with enforceable national service rules, provincial employment in Argentina is not under the supervision of federal regulatory agencies. The provincial executives, embodied in the office of the governor and their local ministries of economy, make all relevant decisions on public-sector wages and employment.[20] As a result, there is significant variation in public-sector wages and employment across provinces. Public employees represent approximately 20 percent of total employment in the metropolitan provinces of Buenos Aires, Cordoba, and Santa Fe, but include over 40 percent of total employment in the Northern provinces of Salta, La Rioja, and Santiago del Estero. Similar differences can be observed in public-sector wages, with average salaries in the Tierra del Fuego and the City of Buenos Aires more than doubling those of poorer provinces such as Jujuy or Catamarca. This variation in public-sector wages is explained by more competitive private sector salaries, socio-demographic features of the employee population, and, we argue, partisan differences that deserve further scrutiny.

Finally, an important aspect of the Argentinian case is that voting is compulsory. Even though enforcement is not perfect, turnout in elections tends to be extremely high compared to the typical numbers in Western countries. Since the 1983 democratic transition, turnout levels have remained above 70% for both presidential and congressional elections. Vote shares, consequently, provide a readily available and accurate measure of district level partisanship (p in the theory).[21] While there are two large parties (the Perionists (PJ) and

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[19]Current President of Argentina (then house representative) Cristina Fernández de Kirchner, March 23, 2000, 3rd Meeting, Argentine House, Discussion of Initiative 114-PE-99, Law of Economic Emergency proposed by the National Executive.

[20]Collective bargaining in the Argentine public sector is a relatively recent phenomenon. Unions were suspended by the military government until 1983, and democratic governments through the 1990s routinely restricted collective negotiations with public employees when those interfered with national economic policies. In the period we consider, few provinces had collective bargaining laws, although in practice informal negotiation mechanisms may have existed (Cetrangolo, 1997). See also Etchmendy and Collier (2007) on the "resurgence" of Argentine unions after our period of study.

[21]In turn, we assume that the relative propensity to apply for a public sector job between partisans and nonpartisans does not vary systematically with the vote share (see the discussion in Section 4.3).

the Radicals (UCR)), there are also a number of strong provincial parties in Argentina. As a result, it is possible to win an election with less than half of the votes. This implies that, importantly, our measure of partisanship for the incumbent politician is not bounded from below at 50%.

The survey we use in this article was conducted by the Argentine census bureau (INDEC) in 2001 for the SIEMPRO agency. The survey includes individual level data measuring wages, employment status and sector, year of hiring, educational achievement, type of search that resulted in employment, and a battery of socio-demographic questions for a total of 23,430 respondents who are currently employed full time in the public or private sector. The sampling frame, which also forms the basis of the official census, is designed to be representative of the Argentine population living in towns of 5,000 or more (84% of the population).[22] In this sample, a total of 5,610 respondents are employed in the public sector, representing a remarkable 23.9% of the economically active population (we use 4,742 observations with no missing variables). The share of public employees in the survey conforms to the national statistical figures of the Argentine census bureau, and is comparable to the share of public employees in Scandinavian countries or France (Gregory and Borland, 1999).[23]

Because we concentrate on within public-sector wage differences, rather than the public-private wage gaps, we use as a dependent variable the log of the individual public employee's monthly salary in 2001.[24] All contracts in Argentina are signed on the basis of a monthly salary, which conforms to working weeks of 44 hours. While some differences in the contracted working hours could affect the estimates of the public-sector wage gap vis-à-vis the private sector, the weekly hours of all public-sector employees is similar across provinces and salaries are readily comparable. Definitions and summary statistics of our variables are in Table 1.

We merge the SIEMPRO survey with information on political and economic conditions in the provinces. The merging is explained in detail in the next section. We measure partisan shares at the time of contracting using the vote shares of the 2001 incumbent governor in elections prior to 2001.[25] Given the importance of parties in Argentine politics,

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[22]Details on the survey methodology can be found in SIEMPRO (2001).

[23]Although we cannot distinguish between employees working for the federal government or the provinces, provincial governments in Argentina employ almost 5 times as many workers as the federal government (4.6 in 2002 according to the INDEC. By comparison, the corresponding ratio for the US is around 1.4.) We also check that our results below are robust to excluding the province of Buenos Aires and the Federal Capital, where most federal employees are located.

[24]The lognormal distribution of wages is a standard assumption in the labor economics literature and is not rejected in our regressions below. We also show that our results do not depend on the specification being loglinear.

[25]Most governor's elections were conducted in 1999 and every 4 years before that. Provinces have the constitutional authority to schedule the election date. The electoral calendar can be retrieved from http://andy.towsa.com/totalpais/calendario.html.

we also present results where we identify the governor by his or her party affiliation (i.e., treat successive governors from the same party as one governor). Table 2 gives the year each administration first took office.[26] To measure economic conditions that may have long-lasting effects on public wages, we collected data on provincial GDP per capita, unemployment, and the two major sources of provincial government revenue: taxes and federal transfers. As described in Table 2, this last data has some limitations, in particular, government revenues are only available for three years, 1980, 1990 and 2001.

5.2 Identification

The main challenge in identifying the effect of provincial vote shares on wages comes from the wide range of political and economic factors which may potentially be correlated with both. In the context of our theory a natural solution presents itself, because we predict that current wages will differ within a province between individuals hired after different elections. Thus, our identification strategy relies on within-province variation in the *current* (2001) salary of employees hired by the same governor after different elections.

Proposition 1 predicts that the current wage $Wage_{jrl}^{2001}$ of employee j in province r hired by the current governor between elections l and $l + 1$ will be a function of (i) the vote share $Votes_{rl}$ of the current governor in election l, (ii) the current value v_{2001} that the governor places on political support (through the equilibrium political support x_{NP} and x_P), (iii) the current outside option u^{2001}, and (iv) the effort costs c_P and c_{NP}.

Based on this, employees hired by the current (2001) governor are matched with the vote share in the election preceding the hiring. For example, if the current governor was first elected in 1995 (taking office in 1996) and then reelected in 1999, then based on our theory wages of employees hired between 1996 and 1999 will be affected by the 1995 vote share, while the wages of those hired after 1999 will be affected by the 1999 vote share. A question arises however on how to deal with employees hired by a previous administration. In our benchmark results, we simply drop them from the sample. Because incoming provincial governors in Argentina can easily fire existing employees or manipulate their wages in order to extract political services from them, we also pursue a second strategy. In this case, we treat all employees as new hires. Thus, we effectively assume that a newly elected governor fires all existing employees, and offers new contracts, possibly re-hiring some of them. This allows for the fact that an incoming governor may not offer a contract to an employee who

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[26]In the period we consider, 3 elections (Tierra del Fuego 1999, Corrientes 1997, and Chaco 1995) had runoffs. For these, we take the first round vote share of the eventual winner. For several elections, candidates were supported by a coalition of one major and several smaller parties. In these cases, we take the vote share of the entire coalition. In each case our choice is guided by which vote share is likely to be the most informative about partisanship.

is very likely to be nonpartisan (i.e., loyal to the previous administration).[27] Those who are rehired are offered new contracts based on the politician's vote share from the previous election, as in our theory. Thus, in the previous example, employees hired before 1995 and still working in the public sector are matched with the incoming governor's 1995 vote share.

We control for cross-province differences in outside options and the value of political support using province fixed effects μ_r. In addition, we ensure that jobs and individuals are comparable to each other by including a detailed vector $\mathbf{X}_j$ of individual characteristics. Our specifications therefore take the form

$$Wage_{jrl}^{2001} = S(Votes_{rl}, \mu_r, \mathbf{X}_j). \tag{8}$$

The variation used to identify the effect of vote share $Votes_{rl}$ is illustrated in Figure 1. The Figure shows the average salary in 2001 among employees hired after a given election in a province. In Catamarca, shown on the first panel, the current party has been in power for three election cycles. Employees hired after the election with the middle vote share earn considerably less than those hired in the other two cycles. A similar picture emerges in the other three provinces shown on the graph. In each case, being hired after an election with low or high vote shares yields higher future salaries than elections in the middle.

The vector of individual characteristics $\mathbf{X}_j$ includes the following (see Table 1): the number of years since the employee was hired (experience) and its square; age and its square, highest educational attainment (dummies for elementary education (the omitted category), some high-school, completed high-school, some college education, completed college education), gender, 52 dummies for the individual's job classification coded at the 2-digit level, and an indicator for the size of the locality. When considering all employees, we also add an indicator for employees hired under the current administration, and a dummy for employees hired before the 1983 democratic transition. Note that by construction the experience variables control for common time-effects across provinces. To control for such effects in a more flexible way, we sometimes add election-year and year-of-hiring indicators. Finally, in some specifications we also include information about whether the employee found the job through an acquaintance who also works there. If such referrals transmit information on political preferences, these applicants may face a different screening process.

It is worth emphasizing that when estimating (8), the province fixed effects control for a wider range of possible confounding effects than in panel studies (i.e., if $Wage_{jrl}^t$ was varying in t). These fixed effects capture *all* province-level factors that affect the 2001 wages, whether or not these factors vary over time. For example, different political environments

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[27]Theoretically, it is optimal for the politician to fire (and not rehire) those existing employees who have a higher probability of being nonpartisans than the rest of the population.

might lead to different opportunities for patronage politics vs. public goods provision for the incumbent politician, and this is likely to be reflected in public wages. However, this implies a relationship between salaries and the *current* political environment within a province, and this is controlled for by the province fixed effect. Thus, factors such as the degree of political competition, whether incumbent politicians feel secure or threatened in their seats, differences between current governors' "abilities", differences in the most recent electoral results, or the current state of provincial government budgets are all controlled for by μ_r. Similarly, province-level differences in the current economic environment presumably have a major impact on public wages, but any such factor will again be captured by the fixed effects.

The obvious threat to our identification strategy is if other factors *at the time of hiring* besides vote shares have long-term effects on wages. As above, we expect political factors to mainly affect contemporaneous wages, and we know of no other study that finds long-term wage impacts. We do however deal with the concern that economic factors may have long-term effects. In some specifications we add to (8) a term $\mathbf{Z}_{rl}$ containing province characteristics that may have long-lasting effects on salaries between election l and 2001. We were able to find data on three factors that may be correlated with vote shares *and* have long-lasting effects on salaries: provincial GDP, unemployment, and provincial government revenues at the time of hiring (see Table 2). Once the vote shares are merged to the income survey as explained above, we merge the $\mathbf{Z}_{rl}$ variables based on the same year as the vote shares. For government revenues, we take the 1980 value for all elections in the 80s, the 1990 value for elections in the 90s, and the 2001 value for elections in 2000.

5.3   Benchmark results

We start by estimating the following specification:

$$\ln Wage_{jrl}^{2001} = \alpha_1 Votes_{rl} + \alpha_2 (Votes_{rl})^2 + \boldsymbol{\beta}\mathbf{X}_j + \mu_r + \varepsilon_j, \tag{9}$$

where ε_j is a normally distributed error term that may be heteroskedastic and correlated within a province. We expect $\alpha_1 < 0$ and $\alpha_2 > 0$, consistent with the U-shape suggested by Figure 1 and our theory.

Table 3 presents the results from estimating equation (9), restricting the sample to workers hired under the current governor (excluding individuals hired under a different party or a different governor from the same party). We report heteroskedasticity-robust standard errors that allow for two-way clustering at the province and election-year level following the

method proposed by Cameron, Gelbach and Miller (2011).[28] Column (1) confirms the U-shaped relationship between vote shares at the time of hiring and individual salaries when individual and province characteristics are controlled for. The negative coefficient on the main effect of $Votes$ and the positive coefficient on the squared term are both individually and jointly significant (F-test p-value = 0.000). Figure 2 graphs the estimated marginal effects. The impact of $Votes$ on salaries is initially negative, but the magnitude of the effect declines, and becomes positive at a vote share of 59% (the last row of Table 3 reports the estimated turning points). According to our estimates, employees hired at this lowest point earn approximately 5.1% lower salaries compared to a 10 percentage point change in vote shares in either direction

From our theory, the negative initial wage effect of the vote share is explained by the incentive effect: in provinces with a low partisan share, the political support purchased from nonpartisan employees is very valuable. This results in high salaries for nonpartisans, and hence also for partisans, who need to be compensated for selecting the contracts designed for them. As the share of partisans rises, the support purchased from nonpartisans can be replaced with the support of partisans, consequently the salaries can decrease. Based on the theory, the positive wage effect of higher vote shares is explained by the composition effect: the high partisan share results in many partisan employees. In equilibrium, partisans are paid more than their nonpartisan colleagues both because they provide more political support and because they receive rents to ensure self-selection.

Columns (2)-(7) of Table 3 present a variety of robustness checks. First, we ask whether our findings could be driven by outliers. In Column (2), we drop the Federal Capital and the province of Buenos Aires to make sure that the potentially different dynamics of the provincial public sector close to the federal center do not drive the results. This causes very little change in the coefficient estimates on $Votes$ and $Votes^2$ in both panels: our findings are robust. In Column (3), we drop the provinces of La Rioja, Tierra del Fuego, and Tucuman. La Rioja has particularly large vote shares in our sample (in Table 2, it is the only province whose vote share is more than two standard deviations away from the mean across all provinces), while Tierra del Fuego and Tucuman have the lowest average vote share over this period. As Column (3) shows, dropping these three provinces yields even larger coefficient estimates on the vote share terms.

Next, we ask whether our results are robust to including province characteristics at the time of hiring which might have long-term effect on wages. This is the only type of province

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[28]Since our dependent variable is for a single year, clustering at the province × year level is inappropriate as this would assume that, in a given province, *only* employees hired in the same year face correlated shocks in their current wages.

level effect that would not be captured by the fixed effects. In particular, Beaudry and DiNardo (1991) and subsequent authors have argued that, in the private sector, economic conditions at the time of hiring can have long-lasting effects on workers' salaries. Although it is unclear whether these factors would be correlated with the vote share of the incumbent governor,[29] they certainly could be, so it is useful to check if our results are robust. In Column (4) of Table 3, we include provincial GDP per capita and provincial unemployment at the time of hiring (we match these variables based on the same year as the vote shares).[30] Including these variables does not affect our results. Another relevant factor that may potentially have long-term wage impacts is the size of state government budgets. As described above the available information is very limited, but in an attempt to address this issue, Column (5) includes state government revenues from their two main sources: taxes, and transfers from the federal government. Our previous results are robust to including these additional variables.[31] Next, Columns (6) and (7) add election-year indicators and year-of-hiring indicators, which also leaves our results intact.[32] The findings in Columns (4-7) confirm that in the public sector, apart from the economic factors identified in previous research, *political* factors at the time of hiring can also have long term impacts on wages.

Finally, Column (8) performs a simple falsification test by running regression (9) on the private sector employees in our dataset. Compared to the public sector regressions, the coefficient estimates on the vote share variables drop by a factor of 10 and become highly insignificant. As expected, the implications of our model of political screening do not show up in the private sector data.

5.4   Robustness

In this section, we examine the robustness of the above findings through a series of further tests. We begin by expanding the sample to include all public employees, even if they were first hired under a different administration. As argued above, provincial politicians in

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[29] Recall that we measure the incumbent's vote share independently of the party of the incumbent. In fact, the difference in mean winning vote shares between elections won by the Peronists (51.5%) and the Radicals (48.9%) between 1983-2001 is not statistically significant ($t = -1.29$). Thus, omitted province characteristics correlated with vote shares *conditional* on the party would not necessarily be correlated with them in our regressions.

[30] We lose several observations due to missing values, including all of Rio Negro and the Federal Capital. See Table 2 for details.

[31] We find a significant negative effect on transfers, which could reflect increased federal assistance when a province's budget worsens.

[32] Note that, based on our theory of screening contracts, common shocks to incumbent governors' popularity should be reflected in public wages. Therefore the year effects introduced in Columns (6) and (7) will capture part of the variation in wages that is caused by vote shares as predicted by our model. Here the estimated effect of $Votes$ only captures deviations across provinces and cohorts from these common shocks.

Argentina in this period faced few constraints in firing employees, and the decision to retain an employee can be modeled similarly to the hiring decision. In these regressions, we identify the governor by his or her party, i.e., we treat different governors from the same party in a province as one politician. We control for employees hired under the current governor's party (*Current party hire*) and those hired before 1983 (*Old system hire*).

Column (1) of Table 4 shows that the U-shaped pattern predicted by our theory is present in this expanded sample as well.[33] Columns (2)-(3) show that this finding is not driven by central provinces or outliers. In Columns (4)-(7), we check that the findings are robust to including various controls that vary depending on the year employees were hired (in addition to the *experience* variables): GDP, unemployment, government revenues, election-year indicators, and year-of-hiring indicators. The vote share coefficients are now somewhat more sensitive than in Table 3 (in particular, the main effect of *Votes* loses significance in Column (6)). However, the qualitative findings remain robust.[34] One difference relative to the earlier results is that the estimated turning point where the effect of vote shares becomes positive is now lower, at around 50%. In these regressions, a 10 percentage point change in the vote share relative to this lowest point is estimated to increase wages by around 3%.

As above, in Column (8) we perform a falsification test on private sector employees. As expected, the effect of vote shares disappears. Finally, Column (9) excludes all employees hired since the most recent election. These are the workers most susceptible to a reverse causality concern, where wages may affect election results rather than the other way around (we discuss this further in Section 5.6 below). Our findings are robust.

A potential concern with the above results is whether they could be explained away by salaries being more noisy in some province-election cells than others. This is motivated by the fact that the number employees in our sample varies by province and election: across the 77 cells, the average sample size is 62, with a median of 40 and a standard deviation of 61 (see the Online Appendix for more details). This might be worrisome if cell sizes happen to be small for very large or very small vote shares, so that salaries in the survey are measured with more noise in exactly these cases. We do not find this to be the case. In fact the three elections with the highest vote share have an average sample size above the median (42), while the three elections with the lowest vote share are among the largest cells in the sample (with an average size of 184). In any event, as discussed above, dropping La Rioja, the

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[33]We can test if the two samples behave differently by including an indicator for employees that were also present in the restricted sample in Table 3 and interacting it with the vote share variables. In the Online Appendix, we confirm that this larger sample behaves similarly to the restricted sample.

[34]Surprisingly, GDP at the time of hiring is estimated to have a significant negative effect on current wages in Columns (4) and (5). Columns (6) and (7) suggest that this variable might pick up time effects not captured by experience (employees hired earlier were hired under lower GDP levels *and* are currently paid more). In these columns, the effect of GDP is not significant.

province with the two highest vote shares does not change our results. We also checked that our results are robust to dropping elections with a cell size below 20, the lowest 5 percentile of our sample (regressions not shown).

Finally, we perform two tests to assess whether the functional form assumption in equation (9) is important. We first relax the loglinearity assumption, and then allow the effect of $Votes$ to vary arbitrarily across different ranges.

Because wages are measured on the log scale, equation (9) assumes that the data generating process is loglinear. While this is a common assumption in the labor literature, it is not implied by our theory, and therefore it is useful to check whether our results are robust. To keep the lognormal distribution of wages, we estimate

$$Wage_{jrl}^{2001} = \alpha_1 Votes_{rl} + \alpha_2 (Votes_{rl})^2 + \boldsymbol{\beta} \mathbf{X}_j + \mu_r + \varepsilon_j, \tag{10}$$

where $Wage_{jrl}$ is wages in hundred pesos, and ε_j is lognormal with a parent distribution $N(\mu, \sigma^2)$. Note that this is exactly the quadratic approximation of equation (6) in Proposition 1, assuming that for each individual j the outside option u_j is given by $u_j = \boldsymbol{\beta} \mathbf{X}_j + \mu_r + \varepsilon_j$. We include all the controls listed in Table 4, and estimate equation (10) by Maximum Likelihood. The results, shown in Table 5, are similar to those seen earlier, confirming the U-shaped effect of $Votes$. Employees hired after a vote share around 46% earn the lowest wages, and a change in vote shares in either direction has a significant positive effect.

Next, we replace the quadratic specification in equation (9) with a series of dummies for different Π^h ranges of $Votes$:

$$\ln Wage_{jrl}^{2001} = \sum_h \alpha_h \times I(Votes_{rl} \in \Pi^h) + \boldsymbol{\beta} \mathbf{X}_j + \mu_r + \varepsilon_j. \tag{11}$$

We create an indicator for vote shares less than 0.4, and then one each for ranges of 7.5 percentage points, the last one being 0.625 and above.[35] Figure 3 presents the results from this regression, where the omitted category is $Votes \in [0.475, 0.55)$. Again a clear U-shaped pattern emerges, with employees hired after elections with very low or very high vote shares being paid more than others.

5.5 Information regarding partisanship

A strong implication of our model which we have not tested above is that partisan workers are paid more than nonpartisans. This wage difference reflects the more extensive political

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[35] This is based on Table 2, where the standard deviation of the average vote share across provinces is 7.2 percentage points.

services provided by partisans, as well as the rents that such workers are offered to take these more demanding positions. Thus, our model predicts a form of wage discrimination in the public sector.

The difficulty with testing this implication directly is that partisanship in unobservable. If the researcher could identify partisans, so could the politician, and the screening problem we study would not exist. In this section we tackle this issue first by looking at referral hires and then by exploiting the correlation between education and partisanship described by previous literature.

We first include information on whether employees found their current position through a referral (28% of the sample did so). While we do not know the type of information transmitted in a referral, it could be that at least some referrals contain credible information on an applicant's partisanship. Such information could affect the patronage contracts in one of two ways. First, referrals could be used specifically to hire partisan employees. In this case, we would expect employees hired through referrals to earn a wage premium. Including an indicator for referral hires in our regressions provides little support for this. Referral hires are estimated to earn 2.7% higher wages, but this is not statistically significant (see the Online Appendix).

A second possibility is that referral hires include both partisan and nonpartisan employees. This would be the case if referrals solve the screening problem, and allow each type to be assigned in the "right" position even without giving them rents (i.e., using the first best contract). Based on our theory, this would imply that individual salaries among referral hires do not respond to the population share of partisans. Instead, the only effect of the vote share on wages would be the positive composition effect (see the discussion at the end of section 4.2). Thus, if screening contracts play an important role in patronage, we expect the U-shaped patterns found above to be driven by employees *not* hired through referrals.

The findings in Figure 4 are consistent with this second interpretation. The figure shows the estimated effect of $Votes$ from regressions that allow this effect to vary between referral and non-referral hires, by including the $Referral$ dummy as well as its interaction with the $Votes$ variables. The estimated marginal effects are graphed separately for employees hired with and without referrals. The Online Appendix reports the detailed regressions and additional robustness checks. As before, the main effects of $Votes$ and $Votes^2$ show a robust U-shape for workers not hired through referrals, with the effect increasing from negative to positive as vote shares go up. By contrast, the interaction of both vote share terms with the indicator for referral hires always has the opposite sign of the main effects, resulting in a flatter relationship. On the right-hand side panels, the estimated effect of vote shares for referral hires is statistically insignificant in most of the sample, with a significant positive

effect for very large vote shares. This is entirely consistent with our theory: if referrals result in perfect information about partisanship, only the positive composition effect remains. These findings support our model of screening contracts: the predictions are confirmed exactly for those employees where asymmetric information is more likely to be a problem.

In a second attempt to measure partisanship, we turn to an extensive literature on the social bases of the Argentine political parties. A recent review by Lupu and Stokes (2009) argues that educational attainment provides the most consistent distinction between the core supporters of the Peronist party (low education) and of the UCR (high education). Based on this, we create an indicator for workers who did not finish high-school *and* are working for a province governed by the PJ, and another one for workers who finished high-school *and* are working under a UCR governor. We have 1103 (23%) workers in the former and 818 (17%) workers in the latter category. Table 6 shows the results from including these variables in our regressions. In Columns (1) and (2), workers who are most likely to be partisans of the PJ earn around 13.7% higher wages in the provinces governed by this party. Interestingly, we do not see a corresponding effect of being among the partisans of the UCR in the provinces that they control. Clearly, one should be cautious in interpreting the results from this crude measure. Nevertheless, the contrast with the private sector in Column (3) does indicate that the combination of educational attainment with the "right" political party can yield a wage premium for the public employees in our sample.

5.6 Alternative explanations

We have shown that the pattern of public-sector wages and partisan shares in Argentina is consistent with a model of political screening, where patronage contracts must deal with asymmetric information on employees' political preferences. In this section we briefly consider potential alternative models and argue that there are no obvious alternatives that would explain the data.

First, we note that in standard models of targeted redistribution where politicians acquire votes by favoring pivotal voter groups (swing voters) or by increasing turnout among loyal supporters (core voters), expenditures are correlated with *current* electoral results. In models such as Cox and McCubbins (1986), Lindbeck and Weibull (1987), and Dixit and Londregan (1996, 1998), politicians allocate resources among voters based on partisanship to win the next election (or as a reward for votes after an election). Since a politician cannot increase his vote share retrospectively and there is no reason to reward supporters for electoral results in the distant past, once current electoral results are controlled for these models have no implications for the relationship between *past* vote shares and *current* wages. By contrast,

we find that past vote shares matter for wages in our regressions even controlling for province fixed effects (which pick up any impact of the current electoral environment).

Second, in these standard models the relationship between vote shares and expenditures is very different from the U-shaped pattern we find. In swing-voter models, expenditures as a function of vote shares tend to be *inverse* U-shaped as very low or high vote shares usually indicate the presence of few swing voters. In core-voter models, expenditures increase monotonically with the share of partisans. (See, e.g., Case (2001) and Ansolabehere and Snyder (2006) for empirical work on these models.)

Could some other theory based on recent elections drive our empirical results? The province fixed effects would pick up any impact that the last election may have on all employees in a province, but differential impacts across cohorts may be possible. Column (9) of Table 4 estimates our baseline regression omitting every employee hired since the most recent election. These are the employees most likely to be affected by the standard mechanisms of targeted redistribution. Our findings remain robust, these employees do not drive the results. Consistent with our screening theory of patronage, elections at the time of hiring, in some cases going back as much as 18 years, have an impact on wages currently paid.

6 Conclusion

We have presented a model of political patronage in which a politician attempts to maximize the level of political support extracted from public employees and minimize its cost. Political preferences are private information. In settings where personal screening is impractical and there are no readily available indicators of preferences (such as family ties or ethnicity), the optimal patronage contracts have a screening function. Positions with different combinations of political requirements and compensation are offered, and partisans self-select into positions that require a large degree of political support in exchange for high pay. At the time of hiring, the politician's vote share reflects the population share of partisans, and this affects the structure of optimal patronage contracts. Thus, the model predicts an association between the incumbent's past vote shares and current wages in the public sector. Individual-level data across provinces in Argentina is consistent with the theory, indicating that public-sector salaries have a U-shaped relation to the vote share of the incumbent governor at the time of hiring.

The possibility of political screening through patronage contracts has interesting policy implications for civil service rules and other provisions aimed at curbing patronage.[36] First,

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[36]See Ujhelyi (2014a, b) for detailed analyses of the impact of civil service rules in different models.

the model underscores the importance of policies that tie public wages to observable employee characteristics, such as civil service pay scales based on qualifications and entrance exams. In a world without such restrictions, politicians can use salary differences in otherwise similar positions to screen applicants based on political preferences. Tying salaries to education or experience prevents the politician from offering a partisan premium, a necessary ingredient of the system of screening contracts. Thus, pay-setting policies in the public sector can help reduce political screening.

Second, consider the standard feature of civil service systems that designate "political" positions in which patronage is legal and "career" positions in which it is not, as in the US system. In our model, the regulation of career positions can be represented as putting restrictions on the nonpartisan contracts that the politician can offer. This has an interesting implication: the self-selection constraints imply that the politician's optimal response to lower s_{NP} and x_{NP} is to reduce the rents offered in the partisan contracts. Thus, the regulation of career positions may lead to lower payments for political services in political positions, even if those are not regulated directly.

Finally, our model suggests an alternative to typical accounts of civil service reforms that view them as driven by public demands for more transparency and efficiency in government. Relative to a world with implicit screening contracts, separating political and career positions codifies the existence of partisan and nonpartisan contracts and makes it explicit that political support is expected from employees in the latter but not in the former. If self-selection based on partisanship is important, as in our model, then civil service reform may be attractive to incumbent politicians because it can enhance the efficiency of political screening. Investigating these and other implications of our approach for the regulation of public employment is an interesting topic for future research.

7   Appendix: Derivation of the optimal contracts

We first solve the problem ignoring, for each type, all but the first participation constraint. We show that the contract given in (2)-(5) solves this problem. Since this solution satisfies the deleted constraints, it is also a solution to the complete problem.

Consider the politician's problem subject to the two incentive constraints (IC_P) and (IC_{NP}) and two participation constraints, $i = P, NP$

$$\sum_{t=0}^{T} \delta^t \left[s_i^t - c_i(x_i^t) \right] \geq \sum_{t=0}^{T} \delta^t u^t. \qquad (PC_i^0)$$

Assume that (PC_P^0) binds. Then (IC_P) is $\sum\limits_{t=0}^{T}\delta^t u^t \geq \sum\limits_{t=0}^{T}\delta^t[s_{NP}^t - c_P(x_{NP}^t)]$, but since $c_{NP}(x_{NP}^t) > c_P(x_{NP}^t)$, this would imply $\sum\limits_{t=0}^{T}\delta^t u^t > \sum\limits_{t=0}^{T}\delta^t[s_{NP}^t - c_{NP}(x_{NP}^t)]$, contradicting (PC_{NP}^0). Since (PC_P^0) does not bind, (IC_P) has to, or else all payments s_p^t could be reduced. Next, assume that (IC_{NP}) binds. Using the binding (IC_P), (IC_{NP}) would then become $\sum\limits_{t=0}^{T}\delta^t \left[c_P(x_P^t) - c_P(x_{NP}^t)\right] \geq \sum\limits_{t=0}^{T}\delta^t \left[c_{NP}(x_P^t) - c_{NP}(x_{NP}^t)\right]$. But since $c_P'(x) < c_{NP}'(x)$ for all x, this cannot be the case. Therefore (PC_{NP}^0) has to bind. Substituting the binding constraints into the objective and solving gives (2)-(5).

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Table 1: Definitions and summary statistics

Variable	Definition	Obs	Mean	Std. Dev.	Min	Max
ln*Wage*	Natural logarithm of individual respondent's salary	4742	6.200	0.704	1.386	9.210
Votes	Vote share of the incumbent governor	4742	0.493	0.090	0.354	0.820
Women	1 if the respondent is a woman	4742	0.519	0.500	0	1
Age	Respondent's age × 0.1	4742	4.078	1.068	1.5	7.5
Schooling	Education achieved by respondent (0 = some primary school or less, 1 = completed primary school, ..., 5 = completed college or higher)	4742	3.125	1.691	0	5
Experience	Number of years since hired × 0.1	4742	1.163	0.898	0	5.4
Occupation	Respondent's occupation coded at the 2-digit level from the Argentine National Job Classification, 1998. (52 occupations are represented in our sample.)	4742				
City	1 if respondent lives in a town of at least 100,000	4742	0.543	0.498	0	1
Old system hire	1 if hired before 1983	4742	0.255	0.436	0	1
Current governor hire	1 if respondent was hired under the current governor	4742	0.223	0.417	0	1
Current party hire	1 if respondent was hired under the current governor's party	4742	0.507	0.500	0	1
Referral	1 if the respondent found this job through an acquaintance who also works there	4691	0.283	0.450	0	1

Source: Individual data is from SIEMPRO (2001). The electoral data is from the Ministry of the Interior, available at http://andy.towsa.com/totalpais/index.html. The electoral data was matched to individuals based on the year they were hired, as explained in the text.

Table 2: Province characteristics

Province	Party of 2001 governor[a]	Year party first elected	Year governor first elected	Avg. vote share of party since first elected (%)	Avg. GDP per capita 1983-2000[b]	Avg. unemploy-ment 1983-2000 (%)[c]	Avg. govt. revenue per capita 1980-2000[d]
Buenos Aires	pj	1987	1999	49.6	0.284	15.1	0.428
Capital Federal	ucr	1996	2000	41.8	n/a	9.7	n/a
Catamarca	ucr	1991	1999	51.4	0.244	11.5	1.141
Chaco	ucr	1995	1995	44.7	0.133	10.8	0.731
Chubut	ucr	1991	1999	54.8	0.629	12.4	0.862
Cordoba	pj	1998	1998	49.6	0.283	12.1	0.540
Corrientes	Other	1997	1997	48.5	0.233	11.9	0.623
Entre Rios	ucr	1999	1999	49.1	0.28	10.7	0.665
Formosa	pj	1983	1995	52.5	0.163	8.0	1.002
Jujuy	pj	1983	1998	48.6	0.216	9.3	0.723
La Pampa	pj	1983	1991	50.0	0.378	5.8	1.131
La Rioja	pj	1983	1995	69.8	0.324	7.2	1.342
Mendoza	ucr	1999	1999	37.9	0.228	5.0	0.529
Misiones	pj	1987	1999	50.2	0.183	6.4	0.583
Neuquen	Other	1983	1999	52.9	0.721	9.0	0.915
Rio Negro	ucr	1983	1995	45.8	0.369	n/a	0.882
Salta	pj	1995	1995	51.1	0.213	13.4	0.599
San Juan	ucr	1999	1999	55.7	0.212	9.5	0.789
San Luis	pj	1983	1991	54.0	0.593	6.2	1.038
Santa Cruz	pj	1983	1999	57.7	0.687	3.7	1.806
Santa Fe	pj	1983	1995	47.1	0.365	14.7	0.573
Santiago del Estero	pj	1983	1999	54.8	0.139	5.5	0.655
Tierra del Fuego	pj	1999	1999	36.8	1.165	9.2	1.929
Tucuman	pj	1999	1999	36.4	0.257	16.0	0.565
Mean				49.6	0.361	9.7	0.872
Std. dev.				7.2	0.245	3.4	0.391

Notes: [a] If the governor was supported by a party alliance, the dominant party is listed. UCR: Union Civil Radical, PJ: Partido Justicialista, Other: other provincial party.
[b] 1000 pesos at 1986 prices. Source: Universidad Nacional de La Plata as described in Porto (2004).
[c] Source: Porto (2004) and INDEC, www.indec.mecon.ar. Four missing values (Cordoba 1996, Neuquen 1991, Misiones 1989, Santiago del Estero 1988) were inputed using the average of the neighboring years. Observations prior to 1991 are missing for the province of Buenos Aires and Santa Fe.
[d] Sum of intergovernmental transfers from the federal government and provincial tax revenues, in 1000 pesos at 2001 prices. Available only for years 1980, 1990 and 2000. Source: Porto (2004).

Table 3: The effect of partisan shares on public-sector wages (only current governor's hires)

	(1)	(2) without Federal Capital and Buenos Aires	(3) without La Rioja, Tierra del Fuego, and Tucuman	(4)	(5)	(6) with election year FE	(7) with year-of-hiring FE	(8) private sector workers
Votes	-6.235***	-6.394***	-8.909***	-6.089**	-6.055**	-5.006**	-5.473**	-0.568
	(1.645)	(1.561)	(1.083)	(2.932)	(2.952)	(2.282)	(2.340)	(0.874)
Votes^2	5.273***	5.350***	7.850***	5.173**	5.114**	4.150**	4.596**	0.364
	(1.309)	(1.244)	(1.018)	(2.600)	(2.594)	(1.914)	(2.008)	(0.725)
Unemployment				-0.000	0.002	0.013	0.017	
				(0.017)	(0.015)	(0.011)	(0.012)	
GDP per capita				0.054	0.313	1.709***	1.896***	
				(0.490)	(0.552)	(0.601)	(0.608)	
Tax revenues					-0.004			
					(0.003)			
IG transfers					-0.001*			
					(0.000)			
R^2	0.55	0.56	0.56	0.55	0.55	0.55	0.56	0.36
N	1,059	963	914	993	993	993	993	5,930
Provinces	24	22	21	22	22	22	22	24
Estimated turning point	0.59	0.60	0.57	0.59	0.59	0.60	0.60	

Notes: Dependent variable is ln*Wage*. All regressions include *Women, Age, Age^2, Experience, Experience^2, City*, dummies for *Schooling* and *Occupation*, and a full set of province fixed effects. Column (8) is a falsification exercise on private sector employees. Robust standard errors clustered two-way by province and election year in parentheses. The last row reports the value of *Votes* for which the estimated effect becomes positive. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4: The effect of partisan shares on public-sector wages (all hires)

	(1)	(2) without Federal Capital and Buenos Aires	(3) without La Rioja, Tierra del Fuego, and Tucuman	(4)	(5)	(6) with election-year FE	(7) with year-of-hiring FE	(8) private sector workers	(9) without recent hires
Votes	-2.763***	-2.691***	-4.790***	-2.638**	-2.629**	-1.304	-1.716*	-0.590	-3.747***
	(0.807)	(1.005)	(1.684)	(1.255)	(1.244)	(1.105)	(0.990)	(0.979)	(0.688)
Votes^2	2.834***	2.767***	4.760***	2.983***	2.954***	1.612*	1.907**	0.862	3.106***
	(0.642)	(0.785)	(1.549)	(1.023)	(1.015)	(0.955)	(0.821)	(0.758)	(0.565)
Unemployment				-0.009	-0.009	-0.021***	-0.015**		
				(0.007)	(0.008)	(0.006)	(0.007)		
GDP per capita				-0.529*	-0.529*	-0.259	-0.175		
				(0.281)	(0.313)	(0.262)	(0.225)		
Tax revenues					0.001				
					(0.001)				
IG transfers					-0.000				
					(0.000)				
R^2	0.50	0.50	0.48	0.51	0.51	0.51	0.52	0.43	0.46
N	4,742	4,050	4,049	4,048	4,048	4,048	4,048	14,409	4,120
Provinces	24	22	21	22	22	22	22	24	24
Estimated turning point	0.49	0.49	0.50	0.44	0.44	0.40	0.45		0.60

Notes: Dependent variable is ln*Wage*. All regressions include *Women, Age, Age^2, Experience, Experience^2, City, Old system hire, Current party hire*, dummies for *Schooling* and *Occupation*, and a full set of province fixed effects. Column (8) presents a falsification exercise on private sector employees. Column (9) excludes employees hired since the most recent election. Robust standard errors clustered two-way by province and election year in parentheses. The last row reports the value of *Votes* for which the estimated effect becomes positive. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Linear specification with lognormal errors (Maximum Likelihood)

	Coefficient estimate	Standard error
Votes	-7.865	0.740
Votes^2	8.481	0.844
μ	1.639	0.022
σ	0.565	0.014
N	4742	
Provinces	24	
Log-likelihood	-11792.49	
Estimated turning point	0.46	

Notes: Results from estimating equation (10). The dependent variable is respondents' wage in 100 pesos. The regression includes *Women, Age, Age^2, Experience, Experience^2, City, Old system hire, Current party hire*, dummies for *Schooling* and *Occupation*, and a full set of province fixed effects. The asymptotic covariance matrix was estimated using the inverse of the Hessian of the likelihood function (Wooldridge, 2002, p352).

Table 6: Partisanship

	(1)	(2)	(3) private sector workers
PJ base	0.137***	0.138***	0.044
	(0.037)	(0.038)	(0.040)
UCR base	-0.118	-0.115	-0.006
	(0.077)	(0.077)	(0.050)
Votes		-2.800***	
		(0.792)	
Votes^2		2.865***	
		(0.638)	
R^2	0.50	0.50	0.43
N	4,742	4,742	14,409

Notes: Dependent variable is ln*Wage*. *PJ base* is 1 for workers who did not finish high-school in provinces where the governor's party is the PJ. *UCR base* is 1 for workers who finished high-school in provinces where the governors' party is the UCR. All regressions include *Women, Age, Age^2, Experience, Experience^2, City, Old system hire, Current party hire*, dummies for *Schooling* and *Occupation*, and a full set of province fixed effects. Robust standard errors clustered two-way by province and election year in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
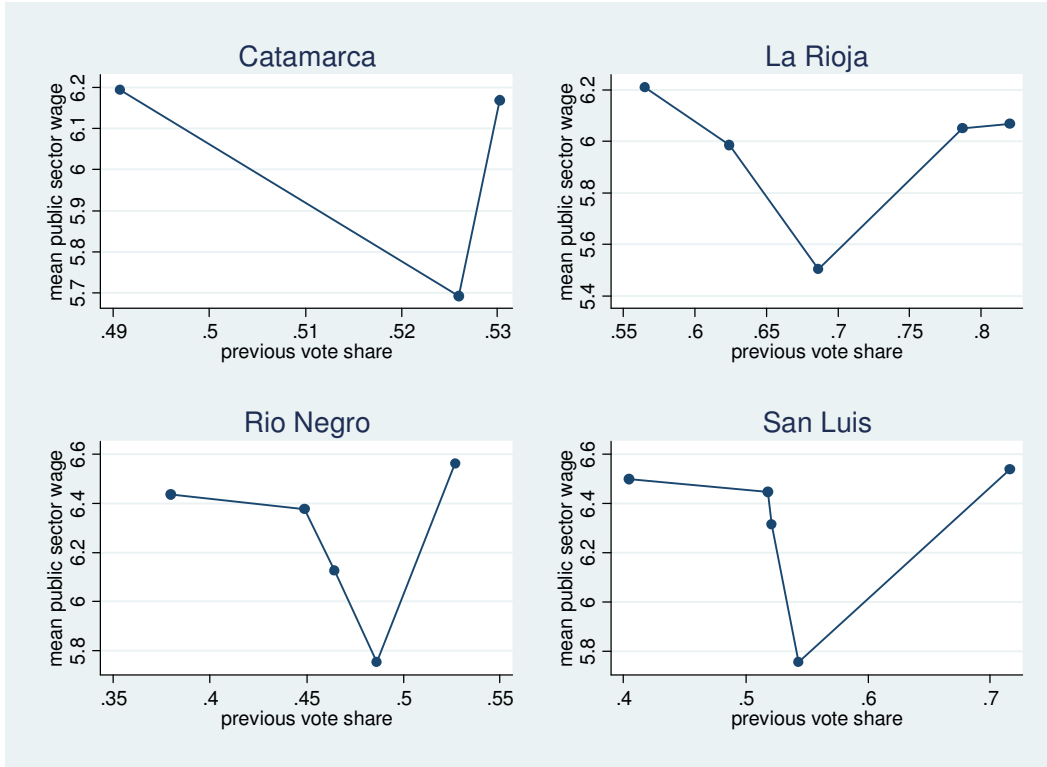
Figure 1: Mean 2001 public sector salaries as a function of vote shares at the time of hiring in four provinces

Each dot represents the 2001 mean log salary of public sector employees hired or retained by the 2001 incumbent after a given election.
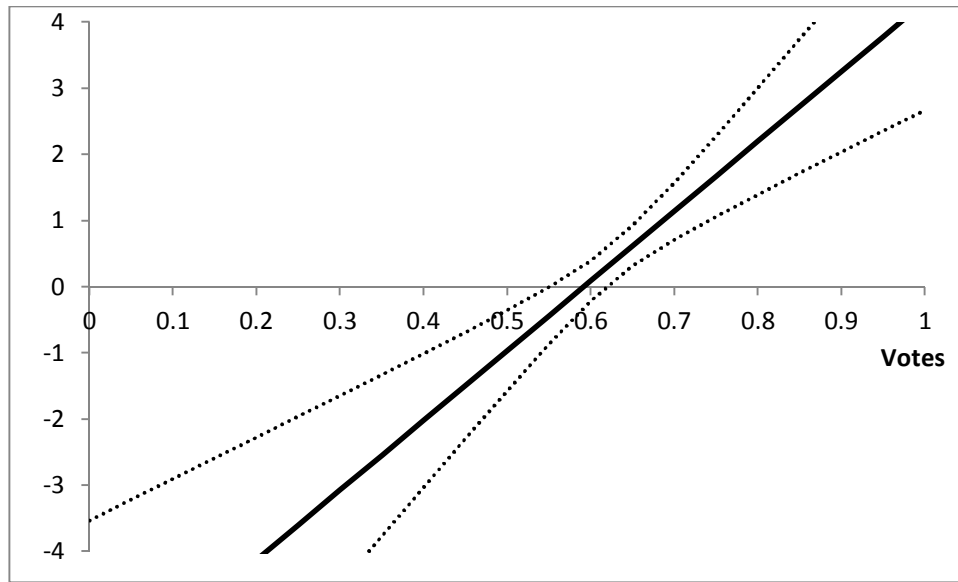
Figure 2: Effect of the vote share on wages

The figure displays the estimated marginal effect of *Votes* from column (1) in Table 3. Dotted curves denote the 90% confidence interval.
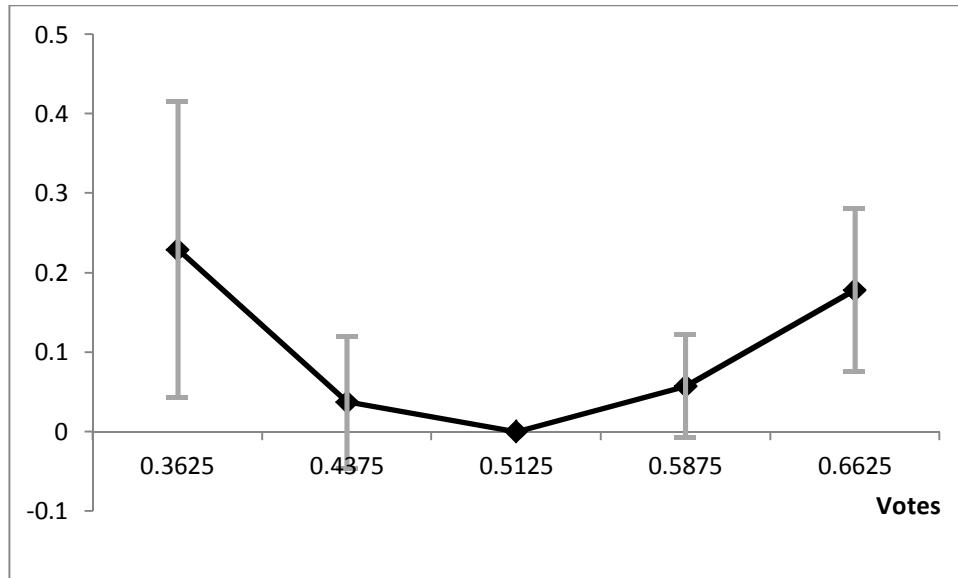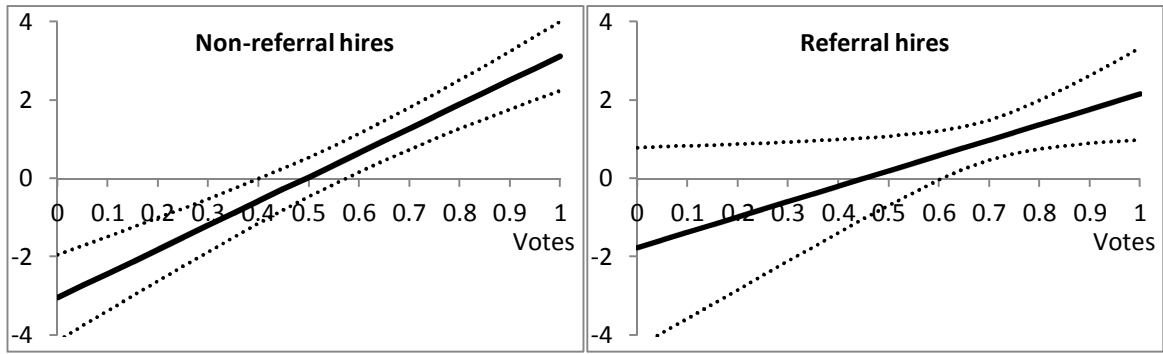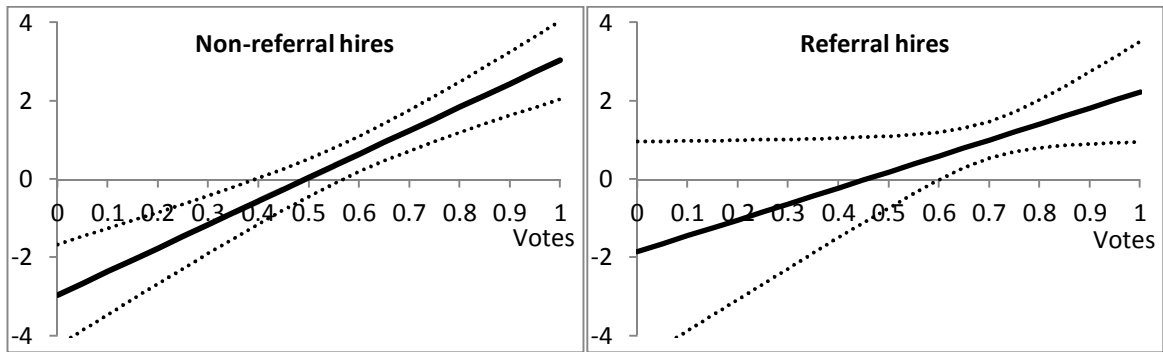
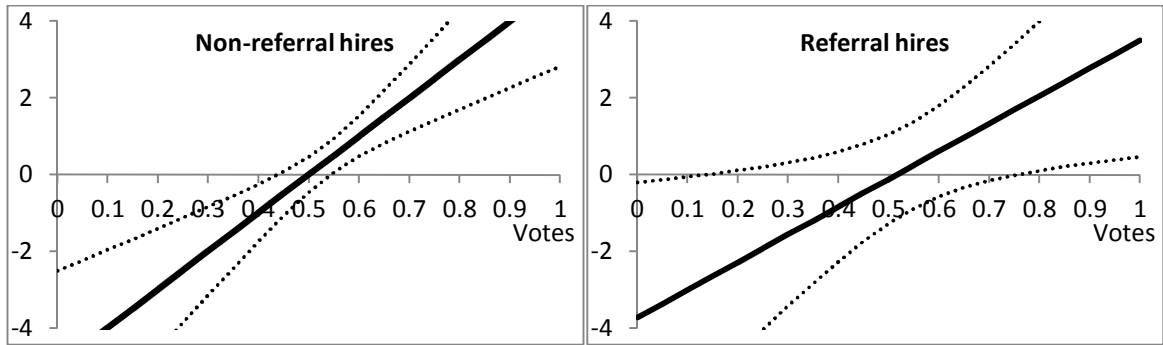Figure 3: Effect of the vote share on wages for various ranges of *Votes*

The diamonds represent point estimates from regression (16) with five ranges of *Votes*, as described in the text. Labels on the horizontal axis correspond to the midpoint of each category (the omitted category is 0.475-0.55). The vertical bands represent the 90% confidence intervals.

Panel A

Panel B: Without Federal Capital and Buenos Aires

Panel C: Without La Rioja, Tierra del Fuego, and Tucuman

Figure 4: Effect of the vote share on wages for employees hired with and without referrals

The figures display the estimated marginal effect of *Votes* for *Referral* = 0 (left panel) and *Referral* = 1 (right panel) from regressing *Wage* on (*Votes*, *Votes*^{2}, *Votes*×*Referral, Votes*^{2}×*Referral, Referral*) as well as the basic controls in Table 4 (*Women, Age, Age*^{2}*, Experience, Experience*^{2}*, City, Old system hire, Current party hire*, dummies for *Schooling* and *Occupation*, and a full set of province fixed effects). Detailed regressions are reported in the Online Appendix. The sample includes all hires. Dotted curves represent the 90% confidence intervals.