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Abstract

We study the prices paid for basic inputs during a crackdown on corruption in the public hospitals of the city of Buenos Aires, Argentina during 1996-97. As in previous, informal accounts of corruption-crackdowns, there is a well defined, negative effect on the measures used to capture corruption. Prices paid by hospitals for basic, homogeneous inputs fall by 17% during the first nine months of the crackdown. After this period prices rise, but they are still 10% lower than those prevailing before the crackdown. Relative to the pre-crackdown period, higher wages play no role in inducing lower input prices when audit intensity can be expected to be maximal (during the first phase of the crackdown), but have a negative and well-defined effect when audit intensity takes intermediate levels (the last phase of the crackdown). Controlling for fixed effects, we find that the wage elasticity of input prices exceeds 20%. These results are consistent with the standard model of bribes of Becker and Stigler (1974).

JEL: K42.
Keywords: Anti-corruption crackdown, efficiency wages, audit, procurement.

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I. Introduction

People are good. But if you monitor them, they are better.
Juan Domingo Peron

In a seminal paper, Becker and Stigler (1974) showed that high wages paired with a non-zero audit probability could be used to deter misbehavior by agents under supervision. The particular set-up considered by Becker-Stigler was one in which a bureaucrat had the opportunity to become corrupt. Over the course of the following 25 years, the main ideas in that paper would have considerable theoretical and practical importance. In particular, they would play a key role in the development of a theory explaining bureaucratic corruption (Rose-Ackerman (1978), Besley and McLaren (1993)). They would also contribute to other areas in economics, including the development of the principal-agent literature (Mirrlees (1971)), the theory of hierarchies (Calvo and Wellisz (1978)), the theory of equilibrium unemployment (Shapiro and Stiglitz (1984), MacLeod and Malcomson (1988)), and the crime literature (Mookherjee and Png (1992, 1995)). Furthermore, the Becker-Stigler model has been the framework for much of the recent policy debate on corruption. The international organizations routinely include recommendations to fight corruption by raising public servants' salaries.[1]

In spite of its importance, the Becker-Stigler model has received relatively little empirical attention. The small body of work that is available provides weak support for a negative relationship between corruption and wages. However, this literature still has a number of issues to address. The first is one of interpretation: prior work uses highly aggregated data (e.g. at the country level), so the data on wages and those on corruption may refer to different groups of individuals. Furthermore, they also rely on survey data. These papers typically study the relationship between public sector wages and subjective corruption rankings. The second issue is a potential omitted variable bias. The evidence available up to now is cross-sectional, so a number of forces could be driving the results. The candidates are not only the classic omitted variables (culture, for example), but also auditing intensity, a variable that the Becker-Stigler model requires to take *intermediate*

[1] See, for example, World Bank (1997). A description of how wage increases are part of the current effort to reduce corruption in the Mexico appears in "Reforming Mexico's police", *The Economist*, December 11, 1999.

values for wages to have any effect on corruption. The third issue concerns the direction of causality. Corruption is a drain on public resources (lowering tax collections and increasing procurement expenses), so it constrains the ability of the bureaucracy to pay high wages.

Our paper presents a different approach that takes advantage of a crackdown on corruption that occurred in the city of Buenos Aires, Argentina, in 1996-7. Following allegations of widespread corruption under the previous administration, the newly elected city government collected and compared the prices paid by all public hospitals in the city for a number of very basic supplies, such as ethyl alcohol and hydrogen peroxide. These are homogeneous inputs, so differences in their prices could not be attributed to quality differences. We estimate a large and well-defined fall in prices (equal to 17%) following the introduction of the monitoring policy. As in previous, informal accounts of corruption-crackdowns, the estimated effects of the policy fall over time. After the initial nine months, average prices paid by the procurement officers rise, but are still 10% lower than the pre-crackdown levels. We then use the time series variation in audit policies to test the Becker-Stigler hypothesis. Relative to the pre-crackdown period, the effect of wages on input prices is negative but insignificant during the first phase of the crackdown, when audit intensity is likely to be maximal. The effect, however, is larger in absolute value (more negative) and well defined during the last phase of the crackdown, when monitoring intensity can be expected to be higher than in the pre-crackdown period but lower than during its initial phase. Controlling for fixed effects, we find that the wage elasticity of input prices exceeds 20%. Thus, and in contrast to previous research, we find evidence consistent with the basic model of bribes of Becker and Stigler (1974).

There are advantages and disadvantages to our approach. One problem, for example, is that only one of the reasons for high procurement prices can be traced back to dishonesty. Other potential reasons include lack of motivation for good performance or lack of information. We believe that these are relatively minor factors compared to corruption as causes of price differences in our sample. Anecdotal evidence is overwhelmingly suggestive that corruption in input procurement in the city of Buenos

Aires is high.[2] A focused survey conducted amongst 360 doctors and nurses in Buenos Aires hospitals shows that corruption in input purchases in public hospitals was perceived to be moderate to high. Respondents also considered corruption in the health sector to be at the average level for the country.[3] More importantly, perhaps, the monitoring policy was officially designed to attack a problem of corruption. In several public speeches, the Secretary of Health of the City of Buenos Aires presented the policy of monitoring prices as an attempt to control corruption with no reference to informational asymmetries or under-provision of effort.[4]

Our approach, however, has some advantages over previous work. First, we study the effect of wages at different levels of auditing, as suggested in the theoretical literature. The distinctive feature of our approach is its use of an event that generates time series variation in auditing levels, allowing us to identify the importance of salaries in deterring corruption. Second, the fact that our study is at the micro level implies that the identification strategy used is relatively clean. Inputs and wages are paid out from two different budgets, so it cannot be argued that hospitals that pay high prices and spend a lot of money on input purchases have little money left to pay the procurement officer's wage. This helps avoid simultaneity problems. Third, the incidence of omitted variables can be expected to be very low. All the hospitals are in the city of Buenos Aires and operate under a similar managerial and organizational environment. This also implies that agents have a similar cultural background, something that seems desirable given that some authors have argued that cultural factors play a big role in explaining corruption differences across countries (e.g. Huntington (1968)). Importantly, procurement officers who are caught taking bribes face identical punishment, which basically amounts to dismissal from the job. Fourth, the interpretation of the results is not obscured by aggregation. Our wage data correspond to the persons actually making the purchases.

[2] The former head of the PAMI, the publicly provided health insurance for pensioners, was accused of buying services and inputs at inflated prices (see, for example, *Clarin*, March 15, 1999). The present head is under investigation for similar crimes at the time of writing (see, for example, *Clarin*, November 9, 1999). For allegations of overpricing in procurement of medicines, diapers, liquid oxygen and audiphones, see *La Nacion*, December 23, 1999.

[3] Survey data suggests that corruption in Argentina is very high by international standards. See, for example, the ranking of *Transparency International* 2000.

[4] See, for example, page 23 of *Salud Para Todos*, January 1999.

Section II describes the previous literature and its relationship to the empirical exercise we conduct. Section III describes our data and the sequence of events during the period under study, while the results are presented in section IV. Section V briefly discusses policy implications and concludes.

II. Corruption, Auditing and Wages

The Becker-Stigler model is built around the choice that an agent who has the opportunity to be corrupt must take. If he decides to be honest, he will take home the wage w with certainty. Choosing malfeasance implies playing a lottery: with some probability θ the agent will escape detection and take home the wage and b, the value of a bribe. With the complementary probability there will be an audit and the agent will be penalized. The agent is assumed to be able to compare the certain wage with the expected payoff from being corrupt in making his choice. Thus, the model is entirely built around what we now call an "incentive compatibility constraint" and the key prediction is that, *ceteris paribus*, high wages help deter corruption. Models that extend these ideas include Besley and McLaren (1993), Mookherjee and Png (1995), Ades and Di Tella (1999), Banerjee (1997), *inter alia*.

Given the difficulty in prosecuting corruption cases, we need to make further assumptions concerning both the probability and the magnitude of the penalty. It is realistic to assume that agents found taking bribes do not receive any monetary fines but are just fired instead. Thus the penalty depends on the wage earned in alternative employment that we can call w^0. Furthermore, not all cases of corruption will end with the agent actually being penalized. Call q the agent's estimate of the probability that an agent found taking bribes is actually fired. The key equation in the Becker-Stigler model, indicating when an agent will be honest, can be written as follows:

$$w > (1-\theta)(w+b) + \theta\left(w(1-q) + w^0 q\right) \qquad (1)$$

Rearranging yields:

$$q\left(w - w^0\right) > \frac{1-\theta}{\theta} b \tag{2}$$

This formulation allows us to focus on two important aspects of the problem. The first is that the correct measure for an agent's incentive to be honest depends on $q\left(w - w^0\right)$, or what we can call the "efficiency wage". This consists of the product of the perceived probability of punishment and the wage premium (defined as the difference between the nominal wage and the opportunity wage). The second is that audit intensity plays a crucial role. Indeed, the model itself predicts no relationship between efficiency wages and the frequency of corruption both when the audit probability is one and when it is zero. For θ=1, the condition is satisfied for any positive efficiency wage, and for θ=0, the condition is never satisfied. Furthermore, assume that opportunity wages w^0 are distributed following the cumulative function $F(w^O)$. The probability that w^0 is higher than a cut-off value w^{O*} is given by $1 - F(w^{O*})$, which represents the frequency of corruption in society. For any reasonable distribution of opportunity wages, implying less frequency of extreme values, the more extreme the values taken by the audit probability θ, the lower the effect of wages on the frequency of corruption. To see this, note that the effect of increasing wages on the frequency of corruption in society is $-f(w^{O*})$. More extreme values of the audit probability θ, lead to more extreme values of w^{O*} and hence to lower values of the density. Our working paper presents a model with these features but where agents also choose the amount of bribes, and shows that their size is negatively correlated with wages at intermediate auditing levels.

Three recent papers study the effect of bureaucratic wages on survey measures of corruption across countries.[5] The first paper, Rauch and Evans (2000), uses wage data for 35 countries collected by the authors in a survey of country experts coming mainly from the academic community. It finds no evidence that wages deter corruption. Furthermore, wages sometimes enter with the wrong sign and the coefficient is always very small when

[5] Recent empirical papers using subjective measures of corruption include Mauro (1995, 1998), Hines (1995), Ades and Di Tella (1999), Wei (2000), Tanzi and Davoodi (1998), Treisman (2000), Svensson (1999), La Porta *et al* (1999), Kaufmann and Wei (1999), Alesina and Weder (1999), Fisman and Gatti (1999), *inter alia*.

compared with other variables included to capture other aspects of bureaucratic efficiency, such as meritocratic recruitment or career stability. A paper by Treisman (2000) uses a new data set compiled by Schiavo-Campo *et al* (1997) where efficiency wages in the bureaucracy are proxied by the ratio of average central government wages to GDP per capita. Again, it reports an insignificant coefficient on wages in a corruption regression. The third paper, by Van Rijckeghem and Weder (1997), finds evidence consistent with the theory in a cross section of 28 developing countries.[6] The evidence is not favorable to the standard model once fixed effects are included. The concern for a simultaneity bias in this problem has led some economists to take the next logical step and examine the impact of exogenous forces on the variables that capture the quality of government (of which *both* corruption and wages are indicators). This is the case for La Porta *et al* (1999) who study the impact of variables such as religion and geography. Although the estimated effects are exogenous, the policy implications are less direct.[7]

One potential explanation for the apparent empirical failure of the Becker-Stigler hypothesis is that these studies include a number of observations drawn from environments where there is no active audit and the probability of being punished for corruption is near zero, or where there is very high audit and the probability of being punished for corruption is near one. Since theory predicts that wages should have no effect on corruption in such circumstances, the coefficient on wages in a corruption regression that does not control for audit intensity will tend to zero.

Unfortunately, the literature on anti-corruption strategies based on auditing is quite limited (but see the crime literature; in particular, Andreoni (1991) who uses a similar break-up in the punishment probability, and Mookherjee and Png (1992) who compare deterrence strategies based on monitoring with those based on investigations). In practice, there are several cases of anti-corruption crackdowns emphasizing the auditing of areas where there are suspicions of corruption. Examples of this approach include the

[6] They present three types of specifications: i) a univariate, ii) a regression that controls for the quality of the bureaucracy, an index of law and order, and dummies for Nicaragua and Korea, and iii) a full specification with a large set of controls, and dummies for Singapore and Korea. Their paper also presents an interesting model where corruption and wages are linked through fairness.

[7] Although wages are not the focus of their analysis, Goel and Nelson (1998) find some evidence that the proportion of government employees who are convicted of bribery in the US in a given year is negatively correlated with wage premiums in the public sector.

"mani-pulite" prosecutions in Italy in the early nineties and the more recent crackdown on judicial corruption in Venezuela.[8] China is a classic example of a country where attempts to control widespread corruption include recurrent anti-corruption campaigns. These often include "exemplary" punishments (including death). Lui (1986) provides a detailed account of the three main crackdowns on corruption during the period 1949-83. One of the characteristics of these episodes is that their effects do not seem to last very long. Liu (1983) describes corruption-related news reports during the purges in the Chinese Communist Party.

A related body of empirical work has advanced significantly our understanding of the ways in which procurement processes can be manipulated and how the public sector can end up paying higher-than-market clearing prices (on bid rigging in highway construction contracts and the supply of milk for public schools, see Porter and Zona (1993, 1999); on fraud in the defense procurement industry, see Karpoff, Lee and Vendrzyk (1999)). The focus in this literature is the behavior of firms who act as suppliers. As a consequence, the motivation of procurement officers is kept in the background in the institutional settings studied. In general the results are equally consistent with "innocent" procurement officers or with officers that take an active part in the bid rigging process. Our paper can be thought of as complementary to this literature. We focus on the behavior of procurement officers and provide no information about the actions of supplier firms. Thus, our results are equally consistent with firms that coordinate rent extraction for the hospitals with the officers or with firms that acquiesce to bribe demands in order to stay in business.[9]

III. Description of the Crackdown and Empirical Strategy

III. a. Description of the Crackdown

In August 1996, after an electoral campaign focused on the issue of corruption by the outgoing administration, a new government was formed in the city of Buenos Aires. One

[8] Between August and November 1999, President Chavez had 195 allegedly corrupt judges fired. Early steps are described in "Caribbean Jacobinism," *The Economist*, August 14, 1999.

[9] Our paper is also related to work studying cost functions for hospitals and other institutions in the health sector (see Carey (1997), Gertler and Waldman (1992), *inter alia*).

of the first initiatives of the new authorities in the Health Secretariat was aimed at controlling corruption in input procurement in public hospitals. The focus of this initiative was all public hospitals dependent on the Government of the City of Buenos Aires (GCBA).[10]

Public hospitals depending on the GCBA acquire their inputs in a decentralized way. Each hospital acquires its own inputs. Input purchases are financed by an annual budget assigned to each hospital by the GCBA's Health Secretariat. Each hospital has an employee in charge of a small procurement office. This office must acquire all the supplies required for the normal operation of the hospital. Procurement officers have no monetary incentives to obtain savings in input procurement. The only incentive for an officer to save money on these purchases is to make funds available to the hospital to buy other inputs. The funds cannot be used for purposes other than input procurement, even within the same hospital. To acquire a given input, procurement officers have to follow one of six alternative procedures, depending on the amounts of money involved and the urgency with which the inputs are needed (public bidding, private bidding, *Decreto 69*, direct purchase, special account, and emergency purchase).

Motivated by a number of informal reports of corrupt practices in the health sector, the newly appointed Health Secretary implemented a monitoring initiative on hospital procurement on September 9, 1996, which required the 33 GCBA public hospitals to report information on price, quantity, brand, supplier, procedure, and month of each purchase for a limited group of inputs. The information was to be collected directly from the invoices of each purchase. The criterion used by the government was to select products where price differences could not be explained in terms of quality so as to make price comparisons as powerful as possible. The first group consisted of homogeneous products, with product differentiation small or non-existent. For the first product - normal saline - the Health authorities collected information going back to June 1996. For the next three products - ethyl alcohol, iodine povidone, and hydrogen peroxide - the information collected went back only to August 1996. Other products were

[10] The GCBA is the largest single supplier of health services in the city, receiving more than eight million annual visits. GCBA hospitals account for over 36% of the hospital beds available in the city. The city supply of beds is supplemented by the private sector (45%), the unions (7%),

gradually incorporated into the price lists, but are not considered here because there is no price data prior to the implementation of the monitoring policy and their product definitions are less homogeneous. Thus, the four products included in our study are normal saline (500 ml.), ethyl alcohol (96°), iodine povidone (5%), and hydrogen peroxide (100 vol.).

The information was compiled by the Health Secretariat and periodically returned to the hospitals. This was done by circulating a list showing the price paid for the inputs by each hospital, starting October 7, 1996. The list highlighted the hospitals that paid the lowest and the highest price for each product. No prizes or punishments were announced at the time (nor were they applied on the basis of this information throughout the period). The information was compiled until December 1997. No price information was collected after this date. Not all the institutions acquired the four sample inputs during the period considered. One of the 33 city hospitals, a psychiatric hospital, did not acquire any of these four inputs during the period of analysis.

The wage information was obtained through personalized interviews in which procurement officers in each hospital were asked their nominal wage and their personal characteristics: gender, age, tenure on the job, marital status, head of household status, and education. The interviews were conducted in 1998 and required a special permission from the Health Secretary. The support of the Health Secretary ensured that all officers answered the survey and provided good quality data on sensitive issues such as earnings.

The monitoring initiative was uniform across hospitals. Nevertheless, procurement officers may have different perceptions of the level of enforcement of laws designed to punish corrupt practices in the public sector. To capture this heterogeneity, we also asked them: *"On a scale from 0 to 100, what is the probability that somebody who commits an act of corruption in a public hospital ends up being fired?"* The responses to this question were varied. Some officers provided further details. In one hospital the procurement officer added: *"Never in 30 years."* In another hospital the response was: *"I do not know of a single case in the city government in 20 years."* There is, however, a group of hospitals where the perception of control is very high. In one

university and federal institutions (7%), and the armed forces (5%). While access to this second group is often restricted by affiliation and/or by ability to pay, access to GCBA hospitals is free.

hospital, the procurement officer added, *"The office here is very small. If there were somebody committing acts of corruption it would be known immediately. I never heard of such a case."* In another hospital the answer was more specific: *"In our hospital, there was the case of two employees who came here from another hospital under accusations of corruption. There was a judiciary process. When there was a verdict, they were both fired."*

In four cases, we found that the person in charge of the office at the time of the survey had been appointed after our period of analysis.[11] The original officer had retired, moved to another job or had been promoted. In none of these cases was the replacement of the procurement officer related to the results of the monitoring policy. Unfortunately, for these four hospitals we cannot relate the input prices to the procurement officer's efficiency wage, as we were unable to collect the information on the wage and personal characteristics of the person who was in charge at the time of the purchases. This reduces our sample to 28 hospitals.

For these four products, 548 transactions were registered. However, two observations were excluded because the quantity acquired was not provided, and two observations were interpreted as misreport and eliminated because the reported prices were in excess of 3.5 times the average weighted product price (see Borenstein and Rose (1994)). Thus, our final sample contains 544 observations and 28 hospitals. In three hospitals, the procurement officers did not provide an answer to the question on the perceived probability of punishment. We first concentrate on the 499 transactions for the 25 hospitals for which we have complete answers, and then include these three hospitals. The data are described in Appendix 1.

III. b. Empirical Strategy

A key element of our approach is the use of variations in the monitoring policy to proxy for audit intensity (θ in our model). We construct a set of dummy variables, θ_t, dividing the sample period into three. The first three months cover the period prior to the introduction of the monitoring policy, when auditing is expected to be lowest. The second period is the first nine months after the introduction of the policy, when the auditing is

[11] In no case did the procurement officer change during our period of analysis.

expected to be maximal. The third period is the last seven months of the sample period, when auditing intensity is expected to have declined relative to the initial crackdown period. In order to choose the break point month between the last two periods analyzed we study media focus on corruption.

We review the stories regarding corruption and the new administration appearing in one of the two main newspapers in the country (*La Nacion*).[12] There are only a few such stories in spite of the fact that one of the main campaign promises was to root out corruption from the city. There are two possibilities. Both are stories giving wide coverage to the results of large opinion polls reporting that corruption in government is one of the main concerns of the inhabitants of the city of Buenos Aires, and that a large proportion of them view the performance of the new government as no better than that of its predecessor. Our hypothesis is that these stories signal the moment when procurement officers receive independent information regarding the new government's commitment to fighting corruption. Up until then, the procurement officers knew that the new administration had launched a monitoring initiative but they were uncertain about how serious it was regarding taking action against offenders. As time goes by, prosecution of procurement officers becomes more costly to the administration as corruption is more and more likely to be the result of lack of control on the part of (and hence be blamed on) the new authorities, even if they take remedial action. The two publication dates are February 16 and May 19. Using either of the two dates yields very similar results (as indeed does choosing any of the months in between). We present the results using May as the break point as this yields coefficients that are marginally more precisely estimated.

We study the effect of efficiency wages on prices at different auditing levels by estimating an equation of the following form:

$$PRICE_{iht} = \lambda\Omega_{iht} + \alpha_t\theta_t + \delta_t\,\theta_t q_h\left(w_h - w_h^0\right) + \Sigma_h + p_{iht}\,, \tag{3}$$

where the true price p_{iht} acts as our error term, and $PRICE_{iht}$ is the log of the price of the input bought in purchase i by hospital h in period t. To control for potential lack of

[12] The keywords used where *corrupcion* and *ciudad de Buenos Aires*. The other newspaper (*Clarin*) does not have the newspapers on line for the sample period.

independence of the error term within hospitals and for the potential presence of unobservable hospital and officer characteristics we estimate a fixed effects model. As a benchmark we will also estimate some regressions that do not exploit the time series variation in the monitoring policy. For these regressions we use a random effects model. Where possible, we consider packages of identical size in order to minimize problems of comparability. Thus, for example, all the purchases of normal saline included in our sample are of bottles of 500ml. Ω_{iht} is the log of the size of each purchase (to control for quantity discounts). Dummies for products are included in all regressions. To proxy for the size of the hospitals in the random effects models we also include the log of the number of beds. Size may affect prices because bigger hospitals may have more bargaining power (in addition to quantity discounts) or because they may be more disorganized.

The main coefficients of interest are δ_t, the effect of the "efficiency wages" $q_h(w_h - w_h^0)$ on input prices at different levels of auditing θ_t. Efficiency wages are constructed using three sources of heterogeneity. The variable w_h is the log of the procurement officer's wage. This exploits the variation in wages introduced by the government scale where there is some element of pay for seniority and education. We can also exploit heterogeneity introduced by the officers' personal characteristics. For the procurement officer of hospital *h,* we define the opportunity wage w_h^0, as the log of the wage predicted for an individual with his or her observed characteristics (gender, education, experience, seniority, marital status, and head of household status) from an earnings equation for inhabitants of the city of Buenos Aires. We define the wage premium as $(w_h - w_h^0)$.[13] The details of the estimation are presented in Appendix 2. The third variable is the perceived probability of punishment, q_h, and is the answer to a survey question asking *"On a scale from 0 to 100, what is the probability that somebody who carries out an act of corruption in a public hospital ends up being fired?"*[14] Lastly, we also allow auditing to have a direct effect on prices. The coefficients α_t capture any

[13] Note that the average ratio of nominal to opportunity wages in our sample is exp(0.519)=1.68. Interestingly, Goel and Nelson (1998) report a public sector premium of 1.66 in the US.

[14] Gaynor and Gertler (1995) use survey data on individual attitudes towards risk in their study of moral hazard in medical partnerships.

general effects of auditing affecting hospital procurement, regardless of the wage received by officers.

As we mentioned briefly in the introduction, the institutional features of public hospitals in the city of Buenos Aires imply that the identification strategy used is relatively clean. The resources received by the hospitals from the city government to pay wages and inputs are earmarked separately for each particular use. Funds received to pay wages cannot be used to pay inputs or vice versa. Thus, it cannot be argued that hospitals that pay very high prices for their inputs are then left with less money to pay the wage of the procurement officer, because the latter comes from the central government's wage bill of public employees, and the funds received to acquire inputs cannot be used to pay wages. It can be argued, of course, that when corruption in procurement is high, resources to pay out wages are low for *all* the hospitals. But the link going from purchase prices to procurement officer's wage at the individual hospital level is broken.

Lastly, it has been argued that when corruption is rampant the principal may be better off paying very low wages (the "capitulation" and "reservation" wage regimes of Besley and McLaren (1993)). First, wages for procurement officers in public sector hospitals are higher than what a simple earnings equation would predict for individuals with their characteristics. Second, wages follow the same scale across all hospitals so this feature would explain low wages in the sector, not variations across individual hospitals.

IV. Empirical Results

We start by analyzing the effect of the anti-corruption policy on prices. Column A in Table 1 includes a dummy for the period when the monitoring policy was active (*Policy*) and a basic set of controls. These include a proxy for the size of each purchase (*Quantity*) and four product dummies.[15] There is strong evidence of quantity discounts. The monitoring policy had an economically and statistically significant effect on prices. Prices dropped 13.8% after the policy was implemented.[16] Note that, during the period of

[15] The sample is restricted to 499 purchases so as to allow comparisons with regressions C and D in Tables 2 and 3. Results using the maximum feasible sample are presented in Table 4.
[16] An intercept coefficient of β in these regressions is equivalent to a percentage change $exp(\beta)$-1.

analysis, the pharmaceutical wholesale price index for Argentina remained stable (dropped 0.5%). The index shows no seasonality and very low variability.

In order to exploit the time series variation in the monitoring policy, we define three period dummies. *Period 1* (June 1996-August 1996) corresponds to the period prior to the implementation of the monitoring policy. *Period 2* (September 1996-May 1997) starts the month the monitoring policy was implemented and ends the month when there is independent information that there is weak commitment of the new government to fight corruption. *Period 3* (June 1997-December 1997) covers the period until the Health Secretariat stops compiling the information altogether (end of the sample period).

Column B in Table 1 studies the effect of the monitoring policy partitioning the period of analysis in this way. Prices dropped by 17% in *Period 2*, relative to their original levels, but recovered by seven percentage points in *Period 3*. Taken on their own, prices during *Period 3* were still 10% lower than in the pre-crackdown period. The magnitude of the estimated effects is not out of line with anecdotal evidence on the size of bribes in Argentina.[17] We reject the equality of the *Period 2* and *Period 3* coefficients at a 1% significance level. It suggests that the immediate effect of the crackdown (*Period 2*) was stronger than its longer-term effect (*Period 3*). This is consistent with what is found in informal descriptions of anti-corruption crackdowns (e.g. Lui (1986)).

We now explore the role of wages. The theory section suggests the use of a variable that combines the wage premium and the perceived probability of punishment. Since the use of the latter can be viewed as an elaboration of the basic model, we first present results using the wage premium as our independent variable and later we incorporate data on the perception of punishment. In column A of Table 2, we include the log of the procurement officer's wage premium (and hospital beds) in a random effects version of the model presented in column B of Table 1. The effect of wage premiums on prices is statistically insignificant. This is similar to the results obtained in the previous

[17] Recent investigations revealed that the price paid by the pensioners' social security agency for funeral services was inflated by 20%, while the price for dental services was inflated by 27% (see *Clarin*, May 28, 1998), and that for psychiatric services by 25% (see *Clarin*, March 15, 1999). A survey of German exporters carried out in 1994 indicated that German businessmen paid between 10% and 15% of the price of the exported goods in bribes in order to place exports in state owned Argentine companies (Neumann (1994)).

literature using aggregate data. In a cross-section regression, where there are no controls for audit intensity, there is no evidence that wages help deter corruption.

We now exploit variations over time in the intensity of audit. Given that the auditing conditions faced by these officers seem to have changed during the period of analysis, we treat the wage premium as a step function in column B of Table 2. Relative to the pre-crackdown period, the effect of wage premiums on input prices is negative but not significant during the first phase of the crackdown, when audit intensity is expected to be at its peak. The effect, however, is negative, larger in absolute size and significant at conventional levels during the last phase of the crackdown, when monitoring intensity can be expected to take intermediate values (higher than in the pre-crackdown period but lower than during its initial phase). The estimated effects are also economically significant. During period 3, the wage premium elasticity of input prices is 26%. Put differently, relative to the pre-crackdown period, a one standard deviation increase in *Wage Premium* leads to a reduction of one third of a standard deviation in *Price*.

This is consistent with the basic Becker-Stigler model. Applied to this setting, the predictions of the model suggest that, with no monitoring in place in *Period 1*, prices should be high and not sensitive to wages. In *Period 2*, the implementation of the monitoring policy should induce a general increase in detection probabilities and a fall in prices for all the hospitals. If the increase in oversight is large enough, this reduction should not depend on the wage paid out to the procurement officers. Finally, when the intensity of the monitoring policy has weakened in *Period 3*, the monitoring policy no longer has a strong average effect on prices. It does, however, still help keep prices in check in hospitals where officers are paid well.

Other interpretations are possible. It could be that higher wage premiums are standing in for high ability. Maybe officers with low wage premiums are just incompetent people who cannot get a good job in spite of extensive education. However, it is unclear why ability would have a differential effect during the crackdown. The inclusion of fixed effects rules out any average effect. Ability could still play a role if able procurement officers respond more to auditing. Note that this argument would require, for example, that buying cheaply standardized products is tiresome and under no-monitoring able officers provide no effort. In that case, ability would only play a role when procurement

officers are being audited. In that situation, however, it is still hard to see how the incompetent procurement officers manage to buy at similar prices to those paid by their more able colleagues at the peak of the crackdown. During *Period 2*, the effect of wage premiums on prices should be stronger - not weaker - than in *Period 3*. Moreover, our results also survive the inclusion of other variables that are plausibly correlated with ability, such as the officer's educational attainment (interacted with the period dummies).[18]

A related interpretation argues that the wage premium proxies for connections. Officers with good connections would be able to get these positions despite their lack of credentials, i.e. they would end up with a high wage premium. However, the protection provided by these connections suggests that these agents should respond less -not more- during the crackdown. Furthermore, our results survive controlling for other variables that ought to be correlated with connections, such as the officer's seniority (interacted with the period dummies).

Column C considers the role of the "efficiency wage", hence exploiting the substantial differences in the way procurement officers perceive the probability of being punished after committing an act of corruption. A number of officers declare that punishment (separation from the job) follows after corruption with certainty. Another group of officers declare that this is never the case. In column C we exploit this heterogeneity across officers by including the wage premium multiplied by the perceived probability of punishment if caught in an act of corruption (*Efficiency Wage1*). The estimated effect is negative and significant.

Column D in Table 2 studies the role of efficiency wages at different levels of audit intensity. Relative to the pre-crackdown period, efficiency wages do not have a significant effect on prices in period 2. However, the effect is negative and well defined in period 3. Now, a one standard deviation increase in *Efficiency Wage1* is associated with a fall of one third of a standard deviation in *Price*.

We also considered whether these results are driven by *PPP* in auxiliary regressions. We find no evidence of this when we repeat regression D in Table 2, adding *PPP* interacted with the period dummies. None of the coefficients are well defined

[18] All results reported but not presented are available upon request.

(probably because of multicollinearity) although the interaction of *Efficiency Wage1* and *Period 3* is still negative. All our estimates are also robust to including monthly dummies instead of period dummies and to including procedure dummies.

Table 3 presents a similar set of estimates but using the log of the wage as our key independent variable instead of the wage premium. The estimates use only variations in nominal wages across individuals but are somewhat easier to interpret. The results are similar to those in Table 2. Again, wages are not correlated to prices over the whole sample in random effects estimation. When the sample period is partitioned to capture different audit probabilities, the officers' wages have a role in inducing lower prices in the last period. The coefficient on *Wage * Period 3* is negative and significant. The implied wage elasticity of prices is 21%. A one standard deviation increase in *Wage* leads to a reduction of one third of a standard deviation in *Price*.

Regression C studies a different construction of the efficiency wage, using just wages. The effect of *Efficiency Wage2* (*Wage * PPP*) on prices is negative and well defined. This regression only controls for random effects, however. Exploiting the time series variation in the auditing policy we find that *Efficiency Wage2* has a stronger effect during period 3. A one standard deviation increase in *Efficiency Wage2* leads to a reduction in *Price* of one third of a standard deviation in this variable.

Table 4 shows a number of checks on our results. It may be argued that officers replied to the survey thinking only about the last period. This would mean that such information is relevant for the last period only. Regressions A and D repeat regression C of tables 2 and 3 using a time-varying version of *PPP* and controlling for fixed effects. We now assume that the perceived punishment probability takes the value of 0 prior to the implementation of the policy in period 1, the value of 100 when the monitoring policy was in its initial phase in period 2, and the value given by the respondents in period 3. These regressions yield similar results.

We limited the sample to all the hospitals where the officers answered the question regarding the *PPP*. But there are three more hospitals that could potentially be included in our sample. Regressions B and E in Table 4 repeat regression B (which does not make use of *PPP*) from Tables 2 and 3, and show that our results remain basically unchanged when this bigger sample is considered. Having excluded these three hospitals

from regression D in Tables 2 and 3, it could be argued that the non-response of these hospitals might generate a selection bias. For example, if the procurement officers of these hospitals take bribes that inflate the medical input prices and have high wages, the negative effect of wages on prices that we found could not be robust to the inclusion of these hospitals. However, the missing procurement officers seem to pay medium-to-low prices and receive medium-to-high wage premiums relative to other hospitals in our sample.[19] To look further into this issue, however, we use an instrumental variable procedure. First, we run the perceived punishment probability on personal characteristics of the purchase officer (gender, seniority, education, and nominal wage) and hospital characteristics (beds, computers, and employees) for the respondent procurement officers. We then utilize the estimated coefficients from this regression and the non-respondent officers' personal and hospital characteristics to extrapolate their responses. Columns C and F in Table 4 show that the results are robust to the inclusion of the non-respondent officers with this procedure.[20]

V. Policy Implications and Conclusions

More than 25 years ago, Becker and Stigler (1974) argued that agents that are under supervision should tend to be less corrupt if they are paid high wages. This hypothesis has influenced a large body of work in areas such as labor economics, theory of the firm, and macroeconomics. It has also affected the anti-corruption policy debate where one of the most influential policy proposals consists of increasing the salaries of public officials. There is, however, little empirical evidence in its favor. The previous literature finds very weak or no effects of wages in cross-country corruption regressions. One difficulty with previous work is that it is very hard to control for audit intensity at the country level with the data available. Theory predicts no correlation between wages and corruption when the

[19] According to their average price, these three hospitals rank in ascending order 9th, 12th, and 19th among our 28 hospitals. According to their wage premiums, the procurement officers of these three hospitals rank 23rd, 11th, and 20th respectively. They also represent a relatively low fraction of the sample (10.7% of the hospitals and 8.3% of the observations).

[20] See Heckman (1979). The results are robust to considering only the officer's personal characteristics as instruments, or to instrumenting with the officer's response to other related

probability of audit is very low or when it is very high. Including observations with these characteristics will bias the results towards finding no significant effects of wages on corruption. Simultaneity of corruption and wages, and omitted variable bias are also potential sources of concern in previous work.

In this paper we study the effect of bureaucratic wages on corruption and procurement efficiency. The distinctive feature of our approach is that we exploit a unique event of corruption control in the public hospitals of the city of Buenos Aires. After a change of government, the new authorities implemented a policy of monitoring input prices. The implementation of this monitoring policy allows us to study the effect of the procurement officers' wages on the prices paid by the hospitals at different levels of audit. Our wage data have the considerable advantage of corresponding to the persons who are actually in charge of making the purchases. In other words, our data is not aggregated, which helps the interpretation of the results. Another important advantage is that the funds available to pay the wages of the procurement officer are not affected by the amount of money spent on input procurement. Wages and input payments are made out from two different budgets. This reduces the possibility that our measures of bureaucratic wages and corruption are simultaneously determined. Furthermore, the incidence of omitted variables can be expected to be low as the study corresponds to different agents in a relatively homogeneous environment.

As in previous informal accounts of corruption-crackdowns there is a well-defined, negative effect of the monitoring policy on the measures used to capture corruption. During the first nine months after the crackdown, prices paid by hospitals for a homogeneous group of inputs fall by almost 17%. This number is not out of line with estimates of the size of bribes paid in Argentina based on surveys of businesspersons (see Neumann (1994)). After the initial crackdown period, purchase prices rise but are still 10% lower than their pre-crackdown levels.

We then estimate the effect of wages on prices. Controlling for hospital fixed effects and relative to the pre-crackdown period, the effect of wages on input prices is negative but insignificant during the first phase of the crackdown, when audit intensity is

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questions in the survey. These include the respondents' answer regarding the efficacy of controls in the procurement process, and their answers concerning the discretion of procurement officers.

expected to be maximal. The effect, however, is negative and well defined during the last phase of the crackdown, when monitoring intensity can be expected to take intermediate values. The wage elasticity of input prices exceeds 20%. Given the volume of purchases in the GCBA hospitals, our estimates suggest that anti-corruption wage policies would be cost effective even for implausibly large costs of implementing audits of the procurement officers. Thus, and in contrast to previous research, we find evidence consistent with the basic model of bribes of Becker and Stigler (1974).

Our findings suggest that the degree of audit intensity is crucial for the effectiveness of anti-corruption wage policies. Exclusive emphasis on wage raises may be misplaced, as such policies would only work if there were audit policies in place. In other words, we provide empirical evidence that carrots and sticks should be viewed as complementary tools in fighting corruption. This idea is not new. Historian Thomas Macaulay provides an example in his account of Lord Clive's experience in 1765 India:

> *"But Clive was too wise a man not to see that the recent abuses were partly ascribed to a cause which could not fail to produce similar abuses as soon as the pressure of his strong hand was withdrawn. The Company had followed a mistaken policy with respect to the remuneration of its servants. The salaries were too low to afford even those indulgences which are necessary to the health and comfort of Europeans in a tropical climate..."* (Macaulay, "Lord Clive," cited in Klitgaard (1988), pp. 80-81).

Table 1: The Effect of the Monitoring Initiative on Prices

	A	B
Quantity	-0.05081***	-0.04474***
	(5.578)	(4.823)
Policy	-0.12926***	
	(4.579)	
Period 2		-0.15795***
		(5.316)
Period 3		-0.09754***
		(3.243)
Fixed Effects	Yes	Yes
F-stat.†		8.38***
Observations	499	499
R^2	0.79	0.79

Notes: t-statistics are in parentheses (absolute values). Regressions include product dummies. † Null hypothesis: "Period 2" = "Period 3". *** Significant at the 1% level. Dependent Variable: log of price.

Table 2: The Role of Wage Premiums during the Monitoring Initiative

Variables	A	B	C	D
Quantity	-0.03655***	-0.04467***	-0.03130***	-0.04331***
	(4.491)	(4.835)	(4.054)	(4.696)
Beds	0.01433		0.01748**	
	(1.462)		(2.063)	
Period 2	-0.15683***	-0.09731	-0.15774***	-0.14389***
	(5.263)	(1.323)	(5.226)	(3.517)
Period 3	-0.09936***	0.03879	-0.10493***	-0.03420
	(3.354)	(0.546)	(3.522)	(0.831)
Wage Premium (WP)	0.00030			
	(0.006)			
WP * Period 2		-0.12038		
		(0.944)		
WP * Period 3		-0.26037**		
		(2.112)		
Efficiency Wage1 (EW1)			-0.02015***	
			(3.048)	
EW1 * Period 2				-0.00996
				(0.492)
EW1 * Period 3				-0.04604**
				(2.267)
Fixed Effects	No	Yes	No	Yes
Random Effects	Yes	No	Yes	No
Observations	499	499	499	499
R^2	0.79	0.78	0.80	0.80

Notes: t-statistics are in parentheses (absolute values) in Fixed Effects models. z-statistics are in parentheses (absolute values) in Random Effects models. All regressions include dummies for product. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level. Dependent Variable: log of price.

Table 3: The Role of Wages during the Monitoring Initiative

Variables	A	B	C	D
Quantity	-0.03654***	-0.04435***	-0.02705***	-0.04404***
	(4.506)	(4.784)	(3.391)	(4.817)
Beds	0.01467		0.01555*	
	(1.527)		(1.847)	
Period 2	-0.15692***	0.88101	-0.15725***	-0.15111***
	(5.267)	(1.108)	(5.214)	(3.410)
Period 3	-0.09959***	1.43069*	-0.10238***	-0.00583
	(3.362)	(1.836)	(3.443)	(0.130)
Wage (W)	-0.00603			
	(0.158)			
W * Period 2		-0.14488		
		(1.307)		
W * Period 3		-0.21340**		
		(1.964)		
Efficiency Wage2 (EW2)			-0.00203***	
			(3.137)	
EW2 * Period 2				-0.00035
				(0.188)
EW2 * Period 3				-0.00528***
				(2.748)
Fixed Effects	No	Yes	No	Yes
Random Effects	Yes	No	Yes	No
Observations	499	499	499	499
R^2	0.79	0.78	0.80	0.80

Notes: t-statistics are in parentheses (absolute values) in Fixed Effects models. z-statistics are in parentheses (absolute values) in Random Effects models. All regressions include dummies for product. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level. Dependent Variable: log of price.

Table 4: Robustness

	A	B	C	D	E	F
Quantity	-0.04364*** (4.716)	-0.04775*** (5.538)	-0.04593*** (5.328)	-0.04417*** (4.838)	-0.04766*** (5.511)	-0.04621*** (5.408)
Period 2	-0.07876* (1.697)	-0.10420 (1.484)	-0.14552*** (3.665)	0.01096 (0.216)	0.90829 (1.170)	-0.15088*** (3.479)
Period 3	-0.05465 (1.532)	0.03165 (0.467)	-0.03626 (-0.911)	-0.00878 (0.239)	1.41566* (1.860)	-0.00621 (-0.142)
Efficiency Wage3 (EW3)	-0.03173** (2.215)					
WP * Period 2		-0.10679 (0.884)				
WP * Period 3		-0.25061** (2.151)				
EW1* Period 2			-0.00866 (0.454)			
EW1* Period 3			-0.04385** (2.306)			
Efficiency Wage4 (EW4)				-0.00509*** (4.061)		
W * Period 2					-0.14886 (1.375)	
W * Period 3					-0.21193** (1.995)	
EW2* Period 2						-0.00039 (0.216)
EW2* Period 3						-0.00512*** (2.806)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	499	544	544	499	544	544
R^2	0.79	0.79	0.80	0.80	0.78	0.80

Notes: t-statistics are in parentheses (absolute values). All regressions include dummies for product. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level. Dependent Variable: log of price.

Appendix 1: Description of the Data

Summary Statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
Price	544	0	0.215	-0.863	1.364
Quantity	544	0	1.283	-4.059	3.724
Period 1	544	0.128	0.335	0	1
Period 2	544	0.391	0.488	0	1
Period 3	544	0.479	0.500	0	1
Beds	28	4.593	2.109	0	7.377
Wage	28	7.111	0.316	6.620	7.718
Wage Premium	28	0.519	0.262	-0.057	0.939
P. P. P.	25	2.629	2.033	0	4.605
Efficiency Wage1	25	1.486	1.469	-0.266	4.250
Efficiency Wage2	25	18.867	14.631	0	35.545
Efficiency Wage3	25	1.688	1.480	-0.266	4.328
Efficiency Wage4	25	23.364	14.679	0	35.545

Correlation Coefficients

	Price	*Quan*	*Beds*	*Per 1*	*Per 2*	*Per 3*	*Wage*	*WP*	*PPP*	*EW1*	*EW2*	*EW3*
Quantity	-0.21											
Beds	-0.01	0.19										
Period 1	0.19	0.01	-0.11									
Period 2	-0.19	0.17	0.03	-0.31								
Period 3	0.07	-0.17	0.04	-0.37	-0.77							
Wage	-0.01	0.07	0.20	0.02	0.04	-0.05						
WP	0.02	-0.06	0.31	-0.01	0.02	-0.01	0.66					
P. P. P.	-0.17	0.29	0.07	0.04	0.01	-0.03	0.18	0.15				
EW1	-0.13	0.14	0.12	0.04	0.01	-0.03	0.56	0.65	0.78			
EW2	-0.17	0.29	0.09	0.03	0.01	-0.03	0.25	0.19	0.99	0.81		
EW3	-0.23	0.13	0.20	-0.41	0.50	-0.21	0.48	0.61	0.37	0.62	0.40	
EW4	-0.32	0.22	0.12	-0.52	0.65	-0.28	0.16	0.12	0.45	0.37	0.45	0.80

Note: *Price* and *Quantity* in these two tables have been centered on product means. All correlations obtained for 544 observations except the last five rows, which involve 499.

Data Definitions and Sources

Variable	Definition	Source
$Price_{iht}$	Log of unit price of the input bought in purchase *i* by hospital *h* at time *t*.	Health Secretariat, GCBA
$Quantity_{iht}$	Log of quantity of input bought in purchase *i* by hospital *h* at time *t*.	Health Secretariat, GCBA
$Beds_h$	Log of the annual average daily availability of beds (plus 1) in hospital *h* for 1997.	*Sintesis Estadistica*, Health Secretariat, GCBA (1997)
$Policy_t$	Dummy variable which equals: 1 if purchase at time *t* was performed from September 1996 through December 1997, and 0 otherwise.	Health Secretariat, GCBA
Period 1_t	Dummy variable which equals: 1 if purchase at time *t* was performed from June 1996 through August 1996, and 0 otherwise.	Health Secretariat, GCBA
Period 2_t	Dummy variable which equals: 1 if purchase at time *t* was performed from September 1996 through May 1997, and 0 otherwise.	Health Secretariat, GCBA
Period 3_t	Dummy variable which equals: 1 if purchase at time *t* was performed from June 1997 through December 1997, and 0 otherwise.	Health Secretariat, GCBA
$Wage_h$	The log of monthly wages received by the procurement officer of hospital *h*.	Survey
Wage Premium$_h$	The log of the ratio of the actual monthly wage received by the procurement officer of hospital *h* to the monthly wage predicted by an earnings equation estimated on permanent household survey data for a person with procurement officer *h*'s personal characteristics.	Calculated from Survey and Argentine Permanent Household Survey (see Appendix 2)
Perceived Punishment Probability$_h$	The log of the answer given by the procurement officer of hospital *h* to the question *"On a scale from 0 to 100, what is the probability that somebody who carries out an act of corruption in a public hospital ends up being fired?"* (normalized to 0 when the answer is 0).	Survey
Efficiency Wage1$_h$	*EW1 = Perceived Punishment Probability*$_h$ * *Wage Premium*$_h$.	See *Perceived Punishment Probability* and *Wage Premium*.
Efficiency Wage2$_h$	*EW2 = Perceived Punishment Probability*$_h$ * *Wage*$_h$.	See *Perceived Punishment Probability* and *Wage*.
Adjusted Punishment Probability$_{ht}$	Variable which equals: 0 if *Period* 1_t equals 1, ln(100) if *Period* 2_t equals 1, and *Perceived Punishment Probability*$_h$ if *Period* 3_t equals 1.	See *Perceived Punishment Probability*, *Period 1*, *Period 2* and *Period 3*.
Efficiency Wage3$_{ht}$	*EW3 = Adjusted Punishment Probability*$_{ht}$ * *Wage Premium*$_h$.	See *Adjusted Punishment Probability* and *Wage Premium*.
Efficiency Wage4$_{ht}$	*EW4 = Adjusted Punishment Probability*$_{ht}$ * *Wage*$_h$.	See *Adjusted Punishment Probability* and *Wage*.
Procedure Dummies$_{iht}$	Dummy variables for purchase procedure (public bidding, private bidding, direct purchase, *Decreto 69*, special account, and emergency purchase) utilized in purchase *i* by hospital *h* at time *t*.	Health Secretariat, GCBA
Product Dummies$_{iht}$	Dummy variables for product (normal saline, ethyl alcohol, iodine povidone, and hydrogen peroxide) acquired in purchase *i* by hospital *h* at time *t*.	Health Secretariat, GCBA

Appendix 2: Construction of the Wage Premium

The wage premium for each procurement officer is constructed in three steps. We first run an earnings equation for 1,833 employed men and 1,163 employed women (excluding self-employed) with only one job in the Buenos Aires metropolitan area using data from the October 1998 wave of the Argentine Permanent Household Survey (*Encuesta Permanente de Hogares*). We then use the coefficients from these equations to predict the procurement officer's opportunity wage, i.e. the wage officers can expect to earn in the city's labor market given their personal characteristics. We then subtract the log of the predicted wage from the log of the actual wage. On wage determination see Becker (1964). For a recent application to Buenos Aires, see Galiani (1999).

Earnings equations are usually run in logs since we normally think about earnings as changing by percentages, not absolute levels. Besides, a log specification also takes care of outliers. However, this approach yields a model with a very low R^2 for this sample (0.16 for women and 0.09 in the men regression). Thus, using such a model to generate the predicted wage would introduce a lot of noise. A linear earnings equation, on the other hand, yields a higher R^2 (0.27 and 0.32 for women and men respectively). In order to deal with outliers, we use robust regression techniques (which reduce their weight) as our first stage estimation. The R^2 for these regressions are 0.37 for women and 0.44 for men.[21]

Step 1: We first estimate two earnings regressions for the city of Buenos Aires using the following variables:

Head of Household = dummy variable which equals 1 when the respondent is the household head, and equals 0 otherwise.

Primary School-Complete, *High School-Incomplete*, *High School-Complete*, *Vocational School-Incomplete*, *Vocational School-Complete*, *University-Incomplete*, *University-Complete* = dummy variables which equal 1 when this is the maximum educational level of the respondent, and 0 otherwise. The base category is Primary School-Incomplete.

Experience = age minus 16 if *Primary School-Incomplete*, *Primary School-Complete* or *High School-Incomplete* equal 1; = age minus 18 if *High School-Complete* equals 1; =

[21] Other studies have also found low R^2 in earnings equations for Argentina (e.g. Lee (1999)). The results are very similar if OLS (non-robust) techniques are used.

age minus 20 if *Vocational School–Incomplete* or *University-Incomplete* equal 1; = age minus 22 if *Vocational School-Complete* equals 1; age minus 23 if *University-Complete* equals 1.

Seniority = years of employment with the current employer.

Live with Partner, *Married*, *Divorced*, *Widowed* = dummy variables which equal 1 when this is the marital status of the respondent, and equal 0 otherwise. The base category is Single.

Two Earnings Equations, City of Buenos Aires, 1998.

Variables	Women	Men
Primary School–Complete	28.8316	76.11078***
	(0.785)	(2.711)
High School–Incomplete	85.0262**	137.2359***
	(2.201)	(4.672)
High School–Complete	188.6255***	253.8986***
	(4.953)	(8.303)
Vocational School–Incomplete	154.1066***	384.3582***
	(2.679)	(4.807)
Vocational School–Complete	206.3188***	339.1074***
	(4.466)	(5.699)
University–Incomplete	361.9371***	417.209***
	(8.598)	(12.385)
University–Complete	559.9386***	889.1461***
	(12.674)	(22.799)
Experience	9.1507***	11.64709***
	(4.000)	(5.234)
$(Experience)^2$	-0.2346***	-0.26659***
	(5.262)	(6.684)
Seniority	12.4664***	11.67066***
	(10.203)	(12.131)
Live with Partner	40.5321	16.63089
	(1.402)	(0.607)
Married	30.8005	65.4536***
	(1.353)	(2.543)
Divorced	30.1723	-19.1853
	(0.966)	(0.485)
Widowed	-31.4816	-32.8301
	(0.734)	(0.467)
Head of Household	71.1932***	99.29729***
	(2.835)	(4.356)
Constant	127.9271***	161.1841***
	(3.171)	(5.018)
Observations	1,163	1,833
R^2	0.37	0.44

Notes: Dependent variable: monthly income. t-statistics are in parentheses (absolute value).
* Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level

Step 2: In order to obtain data on the procurement officers, we obtained permission from the Health Secretary to run a focused survey on procurement officers. This was preceded by a letter from the Health Secretary requesting officers to participate in the survey. In the interviews, we asked the procurement officer's age, gender, education level, seniority, head of household status, and marital status. With this information we calculated the wage that they would earn given their personal characteristics, C_h (experience, seniority, education, head of household status, and marital status), and the estimated coefficients from the earnings equation presented above, which we denote $\hat{\mathrm{B}}$. The opportunity wage these agents could earn working elsewhere in the city of Buenos Aires had they lost their appointments in the public hospitals is $\hat{\mathrm{B}}C_h$.[22]

Step 3: The survey also asked the procurement officers' nominal wage. The wage premium was then obtained as the difference between the log of the nominal wage and the log of the opportunity wage:

$$WP_h = w_h - w_h^0 = w_h - \ln\left(\hat{B}C_h\right),$$

where: WP_h = procurement officer h's wage premium,

w_h = log of procurement officer h's nominal wage.

w_h^0 = log of procurement officer h's estimated opportunity wage.

[22] This procedure implicitly assumes that the purchase officers would find immediately a new job if they are fired from their jobs. Given the high unemployment rate in Buenos Aires during this period, these agents would probably spend some time in the unemployment pool before getting a new job. Thus, our procedure overestimates the market wages.

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