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THE ROLE OF WAGES AND AUDITING DURING A CRACKDOWN ON CORRUPTION IN THE CITY OF BUENOS AIRES*

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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. We find a well-defined, negative effect on the measures used to capture corruption. Prices paid by hospitals for basic, homogeneous inputs decrease by 15 percent during the first 9 months of the crackdown. After this period prices increase, but they are still 10 percent lower than those prevailing before the crackdown. Relative to the precrackdown 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 Gary Becker and George Stigler.

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

I. Introduction

In a seminal paper, Gary Becker and George Stigler showed that high wages paired with a nonzero audit probability could be used to deter misbehavior.[1]

* We give thanks to Gary Becker, Sebastian Galiani, Jorge Mera, Susan Rose-Ackerman, Amanda Rubilar, Bill Savedoff, Pablo Spiller, and Chris Woodruff for helpful suggestions and to seminar participants at Harvard University, Stanford University, the University of California, Berkeley, Northwestern University, the University of California, Los Angeles, the University of California, San Diego, Universität Bielefeld, the Econometric Society, the Latin American and Caribbean Economic Association, the Centro de Estudios Macroeconómicos de Argentina, and the Universidad Torcuato Di Tella for useful comments. The second author thanks the Center for Research on Economic Development and Policy Reform at Stanford University for hospitality. Part of this research was carried out with the support of the Inter-American Development Bank. Fernanda Martijena and Damian Staffa provided excellent research assistance.

[1] Gary S. Becker & George J. Stigler, Law Enforcement, Malfeasance, and Compensation of Enforcers, 3 J. Legal Stud. 1 (1974).

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The particular setup considered by Becker and Stigler was one in which a bureaucrat had the opportunity to become corrupt. Over the course of the following years, the main ideas in that paper would have considerable theoretical importance.[2] Furthermore, the Becker-Stigler model has influenced much of the recent policy debate. International organizations routinely recommend fighting corruption by raising public servants' salaries.

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 (for example, at the country level), so the data on wages and those on corruption may refer to different groups of individuals. Furthermore, they 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" 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 expenditures), so it reduces 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 1996–97. 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 decrease in prices (equal to 15 percent) following the introduction of the monitoring policy. As in previous, informal accounts of corruption crackdowns, the estimated effects

[2] In particular, they would play a key role in the development of a theory to explain bureaucratic corruption (Susan Rose-Ackerman, Corruption: A Study in Political Economy (1978); Timothy Besley & John McLaren, Taxes and Bribery: The Role of Wage Incentives, 103 Econ. J. 119 (1993)), as well as in other areas in economics, including the theory of equilibrium unemployment (Carl Shapiro & Joseph E. Stiglitz, Equilibrium Unemployment as a Worker Discipline Device, 74 Am. Econ. Rev. 433 (1984)), and the crime literature (Dilip Mookherjee & I. P. L. Png, Monitoring vis-à-vis Investigation in Enforcement of Law, 82 Am. Econ. Rev. 556 (1992); Dilip Mookherjee & I. P. L. Png, Corruptible Law Enforcers: How Should They Be Compensated? 105 Econ. J. 145 (1995)).

of the policy decrease over time.[3] After the initial 9 months, average prices paid by the procurement officers increase but are still 10 percent lower than the precrackdown levels. We then use the time-series variation in audit policies to test the Becker-Stigler hypothesis. Relative to the precrackdown 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 precrackdown 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.[4]

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. Anecdotal evidence, however, suggests that corruption in input procurement in the city of Buenos Aires is high.[5] A focused survey conducted among 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.[6] Officially, the monitoring policy was 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 information asymmetries or underprovision of effort.[7]

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, which allows us to identify the importance of salaries in deterring corruption. Second, the

[3] See Francis T. Lui, A Dynamic Model of Corruption Deterrence, 31 J. Pub. Econ. 215 (1986); and Guido Bertucci & Elia Yi Armstrong, Why Anti-corruption Crusades Often Fail to Win Lasting Victories (paper presented at United Nations Anti-corruption Summit 2000, Arlington, Va., 2000).

[4] Becker & Stigler, *supra* note 1.

[5] Two former heads of the publicly provided health insurance for pensioners were accused of buying inputs at inflated prices (see Daniel Gutman, Piden el procesamiento de Matilde Menéndez, Clarin, March 15, 1999 (Politics)). For allegations of overpricing in purchases of diapers, liquid oxygen, and audiphones, see Mas Sobreprecios en el PAMI, La Nacion, December 23, 1999 (General Information).

[6] The ranking by Transparency International 2000 suggests that corruption in Argentina is high by international standards.

[7] See, for example, Salud Para Todos, January 1999, at 23.

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 to 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 organizational environment. This also implies that agents have a similar cultural background, something that seems desirable given that cultural factors may play a role in explaining corruption.[8] It is also important to note that 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.

Section II reviews the literature, and Section III describes our data and the events under study. Section IV presents the empirical results, while Section V 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 make. If he decides to be honest, he will take home the wage w with certainty. Choosing malfeasance implies playing a lottery: with some probability $(1 - \theta)$ 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. Assuming that agents detected taking bribes are not fined but are just fired instead, the penalty depends on the wage earned in alternative employment, which we call w^0. 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 to deter corruption.[9] The key equation in the Becker-Stigler model, indicating when an agent will be honest, can be written as follows:

$$w > (1 - \theta)(w + b) + \theta w^0. \tag{1}$$

[8] See Samuel P. Huntington, Political Order in Changing Societies (1968).

[9] Models that develop these ideas include Jennifer F. Reinganum & Louis L. Wilde, Income Tax Compliance in a Principal-Agent Framework, 26 J. Pub. Econ. 1 (1985); Besley & McLaren, *supra* note 2; Mookherjee & Png, Corruptible Law Enforcers, *supra* note 2; Abhijit V. Banerjee, A Theory of Misgovernance, 112 Q. J. Econ. 1289 (1997); Alberto Ades & Rafael Di Tella, Rents, Competition, and Corruption, 89 Am. Econ. Rev. 982 (1999).

Rearranging yields

$$\theta(w - w^0) > (1 - \theta)b. \tag{2}$$

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 $w - w^0$, the "efficiency wage" (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 $\theta = 1$, the condition is satisfied for any positive efficiency wage, and for $\theta = 0$, the condition is never satisfied. Furthermore, assume that opportunity wages w^0 are distributed following the cumulative function $F(w^0)$. The probability that w^0 is higher than a cutoff value w^{0^*} is given by $1 - F(w^{0^*})$, which represents the frequency of corruption in society. For any reasonable distribution of opportunity wages, implying a lower 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 $-f(w^{0^*})$. More extreme values of the audit probability θ lead to more extreme values of w^{0^*} and, hence, to lower values of the density.

Three recent papers study the effect of bureaucratic wages on survey measures of corruption across countries.[10] The first paper, by James Rauch and Peter Evans, uses wage data for 35 countries collected by the authors in a survey of country experts who come mainly from the academic community. It finds no evidence that wages deter corruption. A paper by Daniel Treisman uses a new data set compiled by Salvatore Schiavo-Campo and coauthors in which efficiency wages are proxied by the ratio of average central government wages to gross domestic product per capita. It also reports an insignificant coefficient on wages in a corruption regression. The third paper, by Caroline Van Rijckeghem and Beatrice Weder, finds evidence consistent with the theory in a cross section of 28 developing countries, in which

[10] Empirical papers using subjective measures of corruption include Paolo Mauro, Corruption and Growth, 110 Q. J. Econ. 681 (1995); James Hines, Forbidden Payment: Foreign Bribery and American Business after 1977 (Working Paper No. 5266, Nat'l Bur. Econ. Res. 1995); Vito Tanzi & Hamid Davoodi, Corruption, Public Investment and Growth, in The Welfare State, Public Investment, and Growth (Hirofumi Shibata & Toshihiro Hiori eds. 1998); Ades & Di Tella, *supra* note 9; Jakob Svensson, Who Must Pay Bribes and How Much? Evidence from a Cross Section of Firms, 118 Q. J. Econ. 207 (2003); Rafael La Porta *et al.*, The Quality of Government, 15 J. L. Econ. & Org. 222 (1999); Daniel Kaufmann & Shang Jin Wei, Does "Grease" Money Speed up the Wheels of Commerce? (Working Paper No. 7093, Nat'l Bur. Econ. Res. 1999); Alberto Alesina & Beatrice Weder, Do Corrupt Governments Receive Less Foreign Aid? 92 Am. Econ. Rev. 1126 (2002); Shang-Jin Wei, How Taxing Is Corruption on International Investors? 82 Rev. Econ. & Stat. 1 (2000). The paper by Wei Li, Corruption and Resource Allocation: Evidence from China (unpublished manuscript, Univ. Virginia 2001), shares with our paper the use of hard corruption measures.

public wages are obtained by dividing the wage bill by employment in the public sector and the alternative wage is earned in the manufacturing sector. The evidence is not favorable to the standard model once fixed effects are included.[11]

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 in which there is no active audit and the probability of being punished for corruption is near zero or in which 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 anticorruption strategies based on auditing is quite limited. In practice, there are several cases of anticorruption crackdowns emphasizing the auditing of areas in which there are suspicions of corruption. Examples of this approach include the "Mani Pulite" prosecutions in Italy in the early 1990s and the crackdown on judicial corruption in Venezuela.[12] China is a classic example of a country where attempts to control widespread corruption include recurrent anticorruption campaigns. These often include "exemplary" punishments (including death). One of the characteristics of these episodes is that their effects do not seem to last very long.[13]

A related body of empirical work has advanced significantly our under-

[11] See James E. Rauch & Peter B. Evans, Bureaucratic Structure and Bureaucratic Performance in Less Developed Countries, 75 J. Pub. Econ. 49 (2000); Salvatore Schiavo-Campo, Giulio de Tommaso, & Amitabha Mukherjee, An International Statistical Survey of Government Employment and Wages (Policy Research Working Paper No. 1806, World Bank 1997); Daniel Treisman, The Causes of Corruption: A Cross-National Study, 76 J. Pub. Econ. 399 (2000); Caroline Van Rijckeghem & Beatrice Weder, Bureaucratic Corruption and the Rate of Temptation: Do Wages in the Civil Service Affect Corruption, and by How Much? 65 J. Dev. Econ. 307 (2001). Rajeev K. Goel & Michael N. Nelson, Corruption and Government Size: A Disaggregated Analysis, 97 Pub. Choice 107 (1998), finds some evidence that the proportion of government employees who are convicted of bribery in the United States in a given year is negatively correlated with wage premiums in the public sector. Furthermore, the concern for a simultaneity bias in the estimate of the effect of wages on corruption has led some economists to take the next logical step and examine the impact of exogenous forces on both variables. This is the case for La Porta *et al.*, *supra* note 10, which studies the impact of variables such as religion and geography. Although the estimated effects are exogenous, the policy implications are less direct.

[12] In 1999, President Hugo Chavez had 195 allegedly corrupt judges fired. Early steps are described in Caribbean Jacobinism, Economist, August 12, 1999. In the crime literature, James Andreoni, Reasonable Doubt and the Optimal Magnitude of Fines: Should the Penalty Fit the Crime? 22 Rand J. Econ. 385 (1991), and Mookherjee & Png, Monitoring vis-à-vis Investigation, *supra* note 2, analyze deterrence strategies based on auditing.

[13] Lui, *supra* note 3, provides a detailed account of the three main crackdowns on corruption during the period 1949–83. Alan P. Liu, The Politics of Corruption in the People's Republic of China, 77 Am. Pol. Sci. Rev. 602 (1983), describes corruption-related news reports during the purges in the Chinese Communist Party.

standing of the ways in which procurement processes can be manipulated and how the public sector can end up paying higher-than-market-clearing prices.[14] 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 who 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 from the hospitals with the officers or with firms that acquiesce to bribe demands in order to stay in business.

III. Description of the Crackdown and Empirical Strategy

A. 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 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).[15]

Public hospitals that depend 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 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 direct monetary incentives to obtain savings in input purchases.[16] 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 other purposes, even within the same hospital.

[14] See Robert H. Porter & Douglas J. Zona, Detection of Bid Rigging in Procurement Auctions, 101 J. Pol. Econ. 518 (1993); Robert H. Porter & Douglas J. Zona, Ohio School Milk Markets: An Analysis of Bidding, 30 Rand J. Econ. 263 (1999); Jonathan M. Karpoff, D. Scott Lee, & Valaria P. Vendrzyk, Defense Procurement Fraud, Penalties, and Contractor Influence, 107 J. Pol. Econ. 809 (1999).

[15] The GCBA is the largest single supplier of health services in Buenos Aires, accounting for over 36 percent of the hospital beds available in the city. The city supply of beds is supplemented by the private sector (45 percent), the unions (7 percent), university and federal institutions (7 percent), and the armed forces (5 percent). While access to this second group is often restricted by affiliation or ability to pay, access to GCBA hospitals is free.

[16] There are no bonuses or prizes related to input savings. But, of course, there might be other benefits (reputation or moral satisfaction, for example) associated with proper performance.

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, and month of each purchase for a limited group of inputs. The information was to be copied directly from the invoices of each purchase in a format that enabled auditing by including the invoice number. The method used by the government was to start with very homogeneous products where price differences could not be explained in terms of quality, so as to make price comparisons as powerful as possible. 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 gradually incorporated into the price lists but are not considered here because there are 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 milliliters), ethyl alcohol (96 percent), iodine povidone (5 percent), and hydrogen peroxide (100 volume).

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 prices 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 these products during the sample period. 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 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.

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. The original officer had retired, moved to another job, or 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. In no

case did the procurement officer change during our period of analysis. This reduces our sample to 28 hospitals. For these four products, 544 transactions were registered. The data are described in Appendix A.

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 3 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 9 months after the introduction of the policy, when the auditing is expected to be maximal. The third period is the last 7 months of the sample period, when auditing intensity is expected to have declined relative to the initial crackdown period. We can speculate that, as time goes by, prosecution of corrupt agents becomes more costly to publicly elected principals because corruption is more and more likely to be the result of lack of control on their part (and hence be blamed on them), even if they take remedial action.

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 a leading national newspaper (*La Nacion*). The keywords used were *corrupcion* and *ciudad de Buenos Aires*. There are only two candidate stories in spite of the fact that one of the main campaign promises was to root out corruption in the city. Both are stories that give 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. 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. The two publication dates are February 16 and May 19, 1997. All our results are robust to using either of these two dates (both yield very similar estimates, as indeed does choosing any of the months in between). We present the results using May as the break point, as this produces coefficients that are marginally more precisely estimated.[17]

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

$$\text{PRICE}_{iht} = \lambda\Omega_{iht} + \alpha_t\theta_t + \delta_t\theta_t(w_h - w_h^0) + \Sigma_h + \varepsilon_{iht}, \tag{3}$$

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

where 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 independence of the error term within hospitals and for the potential presence of unobservable hospital and officer characteristics, we include fixed effects Σ_h. 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 500 milliliters. The term Ω_{iht} is the log of the size of each purchase (to control for quantity discounts). Product dummies are included in all regressions. We allow auditing levels θ_t to have a direct effect on prices. The coefficient α_t captures any general effects of auditing affecting hospital procurement, regardless of the wage received by procurement officers.

The main coefficient of interest is δ_t, the effect of the "efficiency wages" $(w_h - w_h^0)$ on input prices at different levels of auditing θ_t. The variable w_h is the log of the procurement officer's wage. Wages of GCBA public employees follow a government scale based on educational attainment, seniority, and rank. The scale is based on observable characteristics so as to remove discretion and clientelism in wage setting in the public sector. Although all our officers share the same rank (chief of the procurement office), there is some variation in nominal wages in our sample that comes from differences in the other determinants. As an estimate of the opportunity wage of each procurement officer, we use w_h^0, defined 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. For details, see Appendix B.[18]

We also exploit heterogeneity across procurement officers in their perceptions of the level of enforcement of laws designed to punish corrupt practices in the public sector. Although the monitoring initiative was uniform across hospitals, procurement officers may have different perceptions of this enforcement. To capture this heterogeneity, we also asked them in the survey, "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?"[19] We interact the log of this perceived probability of punishment with the efficiency wage to obtain "perceived efficiency wages." In three hospitals, the procurement officers did not provide an answer to this question. Thus, we restrict to 499 transactions for the 25 hospitals for which we have complete answers when we utilize this variable.

[18] The average ratio of nominal to opportunity wages in our sample is $\exp(.519) = 1.68$. Note that Goel & Nelson, *supra* note 11, reports a public sector premium of 1.66 in the United States.

[19] Martin Gaynor & Paul Gertler, Moral Hazard and Risk Spreading in Partnerships, 26 Rand J. Econ. 591 (1995), uses survey data to measure individual attitudes toward risk.

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 the level of 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.

Finally, it has been argued that when corruption is rampant, the principal may be better off paying very low wages.[20] 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 anticorruption policy on prices. Regression (1) in Table 1 includes a dummy for the period when the monitoring policy was active (Policy) and a basic set of controls. These include the size of each purchase (Quantity) and four product dummies. There is strong evidence of quantity discounts. The monitoring policy had an economically and statistically significant effect on prices. Prices decreased 12.3 percent after the policy was implemented.[21] Note that, during the period of analysis, the pharmaceutical wholesale price index for Argentina remained stable (decreased .5 percent). The index shows no seasonality and very low variability.

In order to exploit the time-series variation in the monitoring policy, we define two period dummies. 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). Again, the baseline corresponds to the period prior to the implementation of the monitoring policy.

[20] The "capitulation" and "reservation" wage regimes of Besley & McLaren, *supra* note 2.

[21] An intercept coefficient of β in the regressions is equivalent to a percentage change $\exp(\beta) - 1$.

TABLE 1

THE EFFECT OF THE CORRUPTION CRACKDOWN ON PRICES

	(1)	(2)
Quantity	−.05297**	−.04792**
	(6.196)	(5.534)
Policy	−.13076**	
	(4.945)	
Period 2		−.15869**
		(5.686)
Period 3		−.10153**
		(3.619)
F-statistic[a]		8.69**
R^2	.79	.80

NOTE.—Dependent variable: log of unit price. Policy, Period 2, and Period 3 are dummy variables that take the value of 1 for September 1996–December 1997, September 1996–May 1997, and June 1997–December 1997, respectively. All models include fixed effects and product dummies; *t*-statistics are in parentheses (absolute values). Number of observations = 544.

[a] Null hypothesis: Period 2 = Period 3.

** Significant at the 1% level.

Regression (2) in Table 1 studies the effect of the monitoring policy partitioning the period of analysis in this way. Prices decreased by 14.6 percent in Period 2, relative to their original levels, but recovered by 5 percentage points in Period 3. Taken on their own, prices during Period 3 were still 9.7 percent lower than in the precrackdown period. The magnitude of the estimated effects is not out of line with anecdotal evidence on the size of bribes in Argentina.[22] We reject the equality of the Period 2 and Period 3 coefficients at a 1 percent 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 anticorruption crackdowns.

We now explore the role of wages. As a benchmark, we first follow the previous literature by considering the effect of efficiency wages without exploiting the time-series variation in the monitoring policy. As wages do not vary during the sample period, for these regressions we use a random effects model that includes the log of the number of beds to control for

[22] Investigations revealed that the price paid by the pensioners' social security agency for funeral services was inflated by 20 percent, the price for dental services was inflated by 27 percent (Jueces Federales están investigando a Alderete, Clarin, May 28, 1998 (Society)), and that for psychiatric services by 25 percent (Gutman, *supra* note 5). A survey of German exporters carried out in 1994 indicated that German businessmen paid between 10 and 15 percent of the price of the exported goods in bribes in order to place exports in state-owned Argentine companies (Peter Neumann, Bose: Fast Alle Bestechen, 4 Impulse 12–16 (1994)).

TABLE 2

THE ROLE OF WAGES DURING THE CORRUPTION CRACKDOWN

Variables	(1)	(2)	(3)	(4)
Quantity	−.03714**	−.04775**	−.03697**	−.04766**
	(4.913)	(5.538)	(4.926)	(5.511)
Beds	.00920		.00868	
	(1.020)		(.987)	
Period 2	−.15532**	−.10420	−.15525**	.90829
	(5.546)	(1.484)	(5.545)	(1.170)
Period 3	−.10081**	.03165	−.10057**	1.41566*
	(3.631)	(.467)	(3.624)	(1.860)
Efficiency Wage	−.01020			
	(.216)			
Efficiency Wage × Period 2		−.10679		
		(.884)		
Efficiency Wage × Period 3		−.25061*		
		(2.151)		
Wage			−.00109	
			(.029)	
Wage × Period 2				−.14886
				(1.375)
Wage × Period 3				−.21193*
				(1.995)
Fixed effects	No	Yes	No	Yes
Random effects	Yes	No	Yes	No
R^2	.80	.79	.80	.78

NOTE.—Dependent variable: log of unit price. Efficiency Wage is the difference between the log of the nominal wage and the log of the opportunity wage. Wage is the log of the nominal wage. Regressions (1) and (3) are random effects models (with *z*-statistics in parentheses). Regressions (2) and (4) are fixed effects models (with *t*-statistics in parentheses). All regressions include product dummies. Number of observations = 544.

* Significant at the 5% level.

** Significant at the 1% level.

hospital size.[23] In regression (1) of Table 2, the effect of Efficiency Wage, the difference between the nominal wage and the opportunity wage, on Price is statistically insignificant. This is similar to the results obtained in previous studies: without controlling for audit intensity, there is no evidence that wages 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 Efficiency Wage as a step function in regression (2) of Table 2. Relative to the precrackdown period, the effect of efficiency wages on input prices is negative but not significant during the first phase of the crackdown, when audit intensity is expected to be at its

[23] We obtain similar results when we control for hospital size by using outpatient visits, discharges, or the total amount of funds spent in the purchases of these inputs. Note that the effect of hospital size is absorbed in the fixed effects models.

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 precrackdown 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 .25. Put differently, relative to the precrackdown period, a 1-standard-deviation increase in Efficiency Wage 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 decrease 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 does not have such a strong average effect on prices. It does, however, still help keep prices in check in hospitals where officers are paid well.

Regressions (3) and (4) consider Wage as our key independent variable instead of Efficiency Wage. The estimates use only variations in nominal wages across agents but are somewhat easier to interpret.[24] The results are similar to those obtained in the first two columns. In regression (3), wages are not correlated with prices over the whole sample in random-effects estimation. However, when the sample period is partitioned to capture different audit probabilities in regression (4), 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 1-standard-deviation increase in Wage leads to a reduction of one-third of a standard deviation in Price.

Table 3 repeats the results from Table 2 incorporating the 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 regression (1), we exploit this heterogeneity across officers by defining Perceived Efficiency Wage 1 (PEW1) as the product of Efficiency Wage multiplied by the Perceived Probability of Punishment (PPP) if caught in an act of corruption (as

[24] The strategy here is to use the simplest specification that still captures the intuition behind the Becker-Stigler model. It could be that ability or other unobservables affect the opportunity wage. However, the government scale leaves little room for discretion, so the actual wage does not reflect such influences. Nonetheless, the results are robust to using other proxies for the wage premium, such as one in which the nominal wage is predicted from procurement officers' observable characteristics. The results are also robust to alternative estimates of the outside opportunity wage (see Appendix B).

TABLE 3

THE ROLE OF WAGES AND PUNISHMENT PERCEPTIONS DURING THE CORRUPTION CRACKDOWN

Variables	(1)	(2)	(3)	(4)
Quantity	−.03130**	−.04331**	−.02705**	−.04404**
	(4.054)	(4.696)	(3.391)	(4.817)
Beds	.01748*		.01555+	
	(2.063)		(1.847)	
Period 2	−.15774**	−.14389**	−.15725**	−.15111**
	(5.226)	(3.517)	(5.214)	(3.410)
Period 3	−.10493**	−.03420	−.10238**	−.00583
	(3.522)	(.831)	(3.443)	(.130)
PEW1	−.02015**			
	(3.048)			
PEW1 × Period 2		−.00996		
		(.492)		
PEW1 × Period 3		−.04604*		
		(2.267)		
PEW2			−.00203**	
			(3.137)	
PEW2 × Period 2				−.00035
				(.188)
PEW3 × Period 3				−.00528**
				(2.748)
Fixed effects	No	Yes	No	Yes
Random effects	Yes	No	Yes	No
R^2	.80	.80	.80	.80

NOTE.—Dependent variable: log of unit price. PEW1 is the product of Efficiency Wage multiplied by the Perceived Punishment Probability. PEW2 is the product of Wage multiplied by the Perceived Punishment Probability. Regressions (1) and (3) are random effects models (with *z*-statistics in parentheses). Regressions (2) and (4) are fixed effects models (with *t*-statistics in parentheses). All regressions include product dummies. Number of observations = 499.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

defined in Appendix A). The estimated effect is negative and significant, even without interacting this variable with the period dummies.

Regression (2) in Table 3 studies the role of PEW1 at different levels of audit intensity. Relative to the precrackdown period, the new definition of efficiency wages does not have a significant effect on prices in Period 2. Again, the effect is negative and well defined in Period 3. Now, a 1-standard-deviation increase in Perceived Efficiency Wage 1 is associated with a decrease of one-third of a standard deviation in Price.[25]

Regression (3) studies a different construction of the perceived efficiency wage, interacting the perceived punishment probabilities with nominal wages.

[25] We also considered whether our results are driven by PPP in auxiliary regressions. We find no evidence of this when we repeat regression (2) of Table 3, adding PPP interacted with the period dummies. None of the coefficients are well defined (probably because of multicollinearity) although the interaction of Perceived Efficiency Wage 1 and Period 3 is still negative.

TABLE 4

ROBUSTNESS

	(1)	(2)	(3)	(4)
Quantity	−.04364**	−.04417**	−.04593**	−.04621**
	(4.716)	(4.838)	(5.328)	(5.408)
Period 2	−.07876+	.01096	−.14552**	−.15088**
	(1.697)	(.216)	(3.665)	(3.479)
Period 3	−.05465	−.00878	−.03626	−.00621
	(1.532)	(.239)	(.911)	(−.142)
PEW3	−.03173*			
	(2.215)			
PEW4		−.00509**		
		(4.061)		
PEW1 × Period 2			−.00866	
			(.454)	
PEW1 × Period 3			−.04385*	
			(2.306)	
PEW2 × Period 2				−.00039
				(.216)
PEW2 × Period 3				−.00512**
				(2.806)
Observations	499	499	544	544
R^2	.79	.80	.80	.80

NOTE.—Dependent variable: log of unit price. PEW3 is the product of Efficiency Wage multiplied by the Adjusted Punishment Probability. PEW4 is the product of Wage multiplied by the Adjusted Punishment Probability. PEW1 is the product of Efficiency Wage times the Perceived Punishment Probability. PEW2 is the product of Wage times the Perceived Punishment Probability. All models include fixed effects (with *t*-statistics in parentheses) and product dummies.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

The effect of Perceived Efficiency Wage 2 (PEW2) (defined as Wage × PPP) on prices is negative and well defined. This regression controls only for random effects, however. Exploiting the time-series variation in the auditing policy, we find in regression (4) that PEW2 has a stronger effect during Period 3. A 1-standard-deviation increase in this variable leads to a reduction in Price of one-third of a standard deviation.

Table 4 shows a number of checks on our results.[26] It may be argued that officers replied to the survey question on the perceived punishment probability thinking only about the last period. This would mean that such information is relevant for Period 3 only. Regressions (1) and (2) repeat regressions (1) and (3) from Table 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

[26] Furthermore, all our estimates are robust to including monthly dummies instead of period dummies. The significance of the results is also unaffected when we consider robust standard errors (unclustered or clustered by product, hospital, and period).

2, and the value given by the respondents in Period 3. These regressions yield similar results. Finally, our regressions in Table 3 limited the sample to the 25 hospitals where the officers answered the question regarding the PPP. Having excluded three hospitals, it could be argued that the nonresponse of these officers might generate a selection bias in the regressions that use PPP. Regressions (3) and (4) in Table 4 show that the results are robust to the inclusion of the nonrespondent officers using an instrumental variables procedure.[27]

V. Policy Implications and Conclusions

More than 25 years ago, Becker and Stigler argued that agents under supervision would tend to be less corrupt if they are paid high wages.[28] 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 anticorruption policy debate in which 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 probability of audit is very low or very high. Including observations with these characteristics will bias the results toward 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 that 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 are not aggregated but correspond to the persons who are actually in charge of making the purchases. 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

[27] These three hospitals represent a relatively low fraction of the sample (11 percent of the hospitals and 8 percent of the observations). First, we run the perceived punishment probability on personal and hospital characteristics for the respondent procurement officers. We then use the estimated coefficients from this regression and the nonrespondent officers' characteristics to extrapolate their responses (James J. Heckman, Sample Selection Bias as a Specification Error, 47 Econometrica 153 (1979)). The results are robust to considering only the officers' personal characteristics and their responses to other related questions in the survey as instruments.

[28] Becker & Stigler, *supra* note 2.

procurement. Wages and input payments are made 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 examines different agents in a relatively homogeneous environment.

As in previous informal accounts of corruption crackdowns, after a large initial success, the estimated effects of the policy decrease over time. During the first 9 months of the crackdown, prices paid by hospitals for a homogeneous group of inputs decrease by almost 15 percent. After the initial crackdown period, purchase prices increase but are still 10 percent lower than their precrackdown levels. We then estimate the effect of wages on prices. Controlling for hospital fixed effects and relative to the precrackdown period, the effect of wages on input prices is negative but insignificant during the first phase of the crackdown, when audit intensity is 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 of these hospitals, our estimates suggest that anticorruption 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 model of Becker and Stigler.

Our findings suggest that the degree of audit intensity is crucial for the effectiveness of anticorruption wage policies. Exclusive emphasis on wage raises may be misplaced, as such policies would work only if there were audit policies in place. On the other hand, exclusive emphasis on auditing may be difficult to sustain over time. 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."[29]

APPENDIX A

Data Description and Sources

Price_{iht}: Log of unit price of the input bought in purchase i by hospital h at time t (Government of the City of Buenos Aires (GCBA), Health Secretariat, Under-

[29] Cited in Robert E. Klitgaard, Controlling Corruption 80–81 (1988).

Secretariat of Strategic Management).

Quantity$_{iht}$: Log of quantity of input bought in purchase i by hospital h at time t (GCBA, Health Secretariat).

Beds$_h$: Log of the annual average daily availability of beds (plus 1) in hospital h (GCBA, Health Secretariat, Office of Health Statistics, Sintesis Estadistica (1997)).

Period 2$_t$: Dummy variable that equals one if purchase at time t was performed from September 1996 through May 1997 and zero otherwise (GCBA, Health Secretariat).

Period 3$_t$: Dummy variable that equals one if purchase at time t was performed from June 1997 through December 1997 and zero otherwise (GCBA, Health Secretariat).

Policy$_t$: Dummy variable that equals one if purchase at time t was performed from September 1996 through December 1997 and zero otherwise (GCBA, Health Secretariat).

Wage$_h$: The log of the monthly wage received by the procurement officer of hospital h (survey, available from the authors upon request).

Efficiency Wage$_h$: EW, 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 (Instituto Nacional de Estadistica y Censos de la Republica Argentina, Encuesta Permanente de Hogares (see Appendix B)).

Perceived Punishment Probability$_h$: PPP, 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 for ln(0)) (survey).

PEW1$_h$: Perceived Punishment Probability$_h$ × Efficiency Wage$_h$ (see Perceived Punishment Probability and Efficiency Wage).

PEW2$_h$: Perceived Punishment Probability$_h$ × Wage$_h$ (see Perceived Punishment Probability and Wage).

Adjusted Punishment Probability$_{ht}$: Variable that equals zero if Period 2$_t$ and Period 3$_t$ equal zero, equals ln(100) if Period 2$_t$ equals one, and equals Perceived Punishment Probability$_h$ if Period 3$_t$ equals one (see Perceived Punishment Probability, Period 2, and Period 3).

PEW3$_{ht}$: Adjusted Punishment Probability$_{ht}$ × Efficiency Wage$_h$ (see Adjusted Punishment Probability and Efficiency Wage).

PEW4$_{ht}$: Adjusted Punishment Probability$_{ht}$ × Wage$_h$ (see Adjusted Punishment Probability and Wage).

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 (GCBA, Health Secretariat).

TABLE A2

SUMMARY STATISTICS

Variable	Observations	Mean	SD	Min	Max
Price	544	0	.215	−.863	1.364
Quantity	544	0	1.283	−4.059	3.724
Period 2	544	.391	.488	0	1
Period 3	544	.479	.500	0	1
Beds	28	4.593	2.109	0	7.377
Wage	28	7.111	.316	6.620	7.718
Efficiency Wage	28	.519	.262	−.057	.939
PPP	25	2.629	2.033	0	4.605
PEW1	25	1.486	1.469	−.266	4.250
PEW2	25	18.867	14.631	0	35.545
PEW3	25	1.688	1.480	−.266	4.328
PEW4	25	23.364	14.679	0	35.545

NOTE.—Price and Quantity have been centered on product means.

TABLE A3

CORRELATION COEFFICIENTS

	Price	Quantity	Beds	Period 2	Period 3	Wage	EW	PPP	PEW1	PEW2	PEW3
Quantity	−.21										
Beds	−.01	.19									
Period 2	−.19	.17	.03								
Period 3	.07	−.17	.04	−.77							
Wage	−.01	.07	.20	.04	−.05						
EW	.02	−.06	.31	.02	−.01	.66					
PPP	−.17	.29	.07	.01	−.03	.18	.15				
PEW1	−.13	.14	.12	.01	−.03	.56	.65	.78			
PEW2	−.17	.29	.09	.01	−.03	.25	.19	.99	.81		
PEW3	−.23	.13	.20	.50	−.21	.48	.61	.37	.62	.40	
PEW4	−.32	.22	.12	.65	−.28	.16	.12	.45	.37	.45	.80

NOTE.—Price and Quantity have been centered on product means. All correlations are obtained for 544 observations except the last five rows, which involve 499.

APPENDIX B

CONSTRUCTION OF THE EFFICIENCY WAGE

Step 1. We first estimate two earnings regressions (Table B1) for 1,833 employed men and 1,163 employed women (excluding those self-employed) with only one job in the Buenos Aires metropolitan area for the October 1998 wave of the Argentine Permanent Household Survey (Instituto Nacional de Estadistica y Censos de la Republica Argentina, Encuesta Permanente de Hogares (http://www.indec.mecon

TABLE B1

TWO EARNINGS EQUATIONS, CITY OF BUENOS AIRES, 1998

Variables	Women	Men
Primary school, complete	28.8316	76.11078**
	(.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)
$(\text{Experience})^2$	−.2346**	−.26659**
	(5.262)	(6.684)
Seniority	12.4664**	11.67066**
	(10.203)	(12.131)
Live with partner	40.5321	16.63089
	(1.402)	(.607)
Married	30.8005	65.4536**
	(1.353)	(2.543)
Divorced	30.1723	−19.1853
	(.966)	(.485)
Widowed	−31.4816	−32.8301
	(.734)	(.467)
Head of household	71.1932**	99.29729**
	(2.835)	(4.356)

NOTE.—Dependent variable: monthly income. Head of household is a dummy variable that equals one when the respondent is the household head and equals zero otherwise. Primary school, complete; high school, incomplete; high school, complete; vocational school, incomplete; vocational school, complete; university, incomplete; and university, complete are dummy variables that equal one when this is the maximum educational level of the respondent and zero otherwise (the base category is primary school, incomplete). Experience = age minus 16 if primary school, incomplete; primary school, complete, or high school, incomplete equal one. Experience = age minus 18 if high school, complete equals one; experience = age minus 20 if vocational school, incomplete or university, incomplete equal one; experience = age minus 22 if vocational school, complete equals one; experience = age minus 23 if university, complete equals one. Seniority = years of employment with the current employer. Live with partner, married, divorced, widowed are dummy variables that equal one when this is the marital status of the respondent and zero otherwise (the base category is single). *t*-statistics are in parentheses (absolute value). A constant is included. The R^2 value in the regression for women (men) is .37 (.44). The number of observations in the regression for women (men) is 1,163 (1,833).

* Significant at the 5% level.

** Significant at the 1% level.

.gov.ar)).[30] Thus, earnings are assumed to be the product of B times personal characteristics.

Step 2. We obtained permission from the health secretary to run a focused survey that asked the procurement officers' age, gender, education level, seniority, head-of-household status, and marital status. The interviews were preceded by a letter from the health secretary requesting officers to participate in the survey. With this information, we predicted the opportunity wage these agents could earn working elsewhere in the city of Buenos Aires had they lost their appointments in the public hospitals, given their personal characteristics C_h (experience, seniority, education, head-of-household status, and marital status) and the estimated coefficients $\hat{B}$ from the earnings equation presented above.[31]

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

$$\mathrm{EW}_h = w_h - w_h^0 = w_h - \ln(\hat{B}C_h),$$

where, for procurement officer h, EW_h is the efficiency wage, w_h is the log of the nominal wage, and w_h^0 is the log of the estimated opportunity wage.

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[30] On wage determination see Gary S. Becker, Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education (1964). Earnings equations are usually run in logs since we normally think about earnings as changing by percentages, not absolute levels. Besides, a log specification takes care of outliers. However, this approach yields a model with a very low R^2 values for this sample (.16 for women and .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 higher R^2 values (.27 and .32 for women and men, respectively). In order to deal with outliers, we use robust regression techniques. The R^2 values for these regressions are .37 for women and .44 for men. Other studies have also found low R^2 values in earnings equations for Argentina (for example, Sebastian Galiani, Wage Determination in Argentina: An Econometric Analysis with Methodology Discussion (Working Paper No. 218, Inst. Torcuato Di Tella 1999)). The results are very similar if ordinary least squares (nonrobust) techniques are used.

[31] This procedure implicitly assumes that the procurement officers would immediately find 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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