Political and Economic Incentives during an Anti-Corruption Crackdown

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Abstract

In September 1996, the newly appointed Health Secretary of the Government of the City of Buenos Aires (GCBA) launched an anti-corruption crackdown. Each hospital had to report the purchase prices paid for a sample of inputs acquired through a decentralized procurement process. Using this information, the Health Secretariat compiled price comparisons highlighting the hospitals paying the lowest and highest values for each product. The monitoring policy was at work through December 1997. This paper analyzes the economic incentives of the procurement officers under monitoring, and the political incentives of the political officers that implemented these controls. Our evidence shows that the policy induced an immediate fall in input prices. After this initial fall, prices rose, but they were still lower than those prevailing before the crackdown. The empirical results also show that the hospitals with the lower procurement officers' wages were the first hospitals where prices started to rise. We provide an explanation for these findings drawing on two previous papers by these authors.

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I. Introduction

The economic approach to the study of corruption made a significant leap forward with the appearance of the first empirical studies on the causes and consequences of corruption in the 1990's. Mauro (1995), for example, presented evidence on the effect of corruption on investment and growth using data for a cross section of countries. Besides settling an old debate in the development literature, basically by showing that there was a negative effect of corruption on investment regardless of the level of red tape in the country, this paper was important because it was one of the first attempts at using data to provide empirical discipline to the field of corruption.

The corruption data used in these papers were subjective in nature and were produced for the business community.[1] The first indexes were produced in the context of country risk assessment and collected the opinions of experts who made comparisons between countries. Others were simply produced as a by-product of more comprehensive surveys of business conditions in different countries. Those giving opinions were typically business people and high officials in government. Being based on opinions, such data were subject to relatively large margins of error. Progress then followed two different avenues. Some tried to improve the precision of cross country indicators, basically by aggregating the data produced by the different sources for each country. Examples of these are the important work of Kaufmann et al (1999) and Lambsdorff (1999), and it is the approach followed by *Transparency International*. A second avenue followed by some researchers tried to develop microeconomic measures of corruption.[2] An advantage of this micro data is, presumably, a higher degree of precision in the measurement of corruption. But a practical problem that soon had to be faced was that papers that study corruption within a single country seldom have available the kind of variation in the corruption and institutional data that allows to identify competing hypothesis. If, for example, one wants to study the effect of judicial independence on bribe taking, it is unlikely to find different

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

[2] Li (2001) and Reinikka and Svensson (2001) share with our study the use of hard corruption measures.

judicial regimes active within the same country. Yet, if one had an experiment where such a variation was present, these very same features would represent a considerable advantage as they would provide a controlled environment where to evaluate their effects.

In this paper we summarize work that exploits a unique event that has exactly these features (Di Tella and Schargrodsky (2001, 2002)). In September 1996, an anti-corruption crackdown took place in the City of Buenos Aires. By looking at data that is correlated with the degree of corruption, before, during and after the peak of the crackdown, we are able to study how corruption reacts to changes in auditing intensity. In the first of these papers we study how the prices paid by public hospitals for basic, homogeneous inputs changes as the crackdown progresses depending on the wage paid to the procurement officer. This allows us to evaluate the empirical performance of one of the basic hypothesis in the corruption literature, namely the Becker-Stigler hypothesis whereby higher wages at intermediate auditing levels bring about lower levels of corruption (see Becker and Stigler (1974); see also Rose-Ackerman (1978), Besley and McLaren (1993), Mookherjee and Png (1995) and Banerjee (1997)).

The Becker-Stigler model has been the framework for much of the recent policy debate on corruption. Yet, in spite of its importance, the Becker-Stigler model has received relatively little empirical attention. The relatively small body of work on the topic provides only weak support for the hypothesis that there is a negative relationship between corruption and wages. A number of issues, however, need to be addressed to make this work fully convincing. These include those surrounding the interpretation of the results, omitted variables and causality. Previous work uses highly aggregated data (e.g. at the country level), so the data on wages and those on corruption typically refer to different groups of individuals. As for omitted variables, previous work is cross-sectional, so a number of forces could be driving the results, including culture and others. In particular, previous papers do not control for auditing intensity, a variable that should take intermediate values for efficiency wages to have an effect on corruption levels. Lastly, previous work can be improved upon if more information is provided concerning the direction of causality. A negative correlation between corruption and wages across countries could be a reflection of reverse causality because 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 micro experiment allows us to overcome these problems.

In the second of these papers, Di Tella and Schargrodsky (2002), we focus on the political incentives of the government to control corruption by second-tier officials. We note that the negative effect of the crackdown on input prices tends to fall over time. A similar experience, whereby the effect of an anti-corruption effort disappears over time, has been noted during other anti-corruption campaigns. In policy debates, this is often taken as a sign of the government's wavering commitment in the fight against graft. We take a different stand. We propose a model where we can study the incentives of the government to uncover corruption by a non-elected bureaucrat. Our model mimics the behavior of the data by noting that a newly elected government has full incentives to detect corruption by lower level officials, but that governments that have been in office for a while do not. Each discovery of an act of corruption conveys two kinds of information. First, it conveys information on the government's commitment to fighting corruption this period. But it also provides some information on the government's performance in controlling corruption in previous periods. When the government has been in office for only one period, it can always blame the previous government for corruption in former periods. But as time goes by, each discovery of an act of corruption is more damaging for the current government. Such a mechanism is also present in more general contexts and it is a force arguing that changes in government may be intrinsically good by reducing the incentives to cover up. This should be contrasted with standard political economy models that suggest that preventing elected officials from seeking re-election is a negative feature of electoral systems (see, for example, Besley and Case (1995) and Persson, Roland and Tabellini (1997)).

Section II describes the institutional features of the anti-corruption crackdown while section III describes our empirical approach to detect the effects of the price monitoring policy. Section IV proposes a theory of political incentives to control corruption that leads to the kind of political corruption cycle detected in the data. Section V describes the economic incentives faced by procurement officers that are paid different official salaries and have different opportunity wages. Section VI concludes.

II. The Anti-Corruption Crackdown

In the city of Buenos Aires, the GCBA is the largest individual provider of health services through its network of 33 hospitals.[3] The GCBA hospitals purchase their inputs in a decentralized way. Each hospital has a procurement office responsible for buying supplies that is headed by a procurement manager. The hospitals finance their purchases from an annual budget assigned to them by the Health Secretariat. Hospitals have little incentives to generate savings in supply purchases. The funds are earmarked for input procurement and cannot be utilized for any other purposes, even within the same hospital. Any savings can only be used for purchasing supplies, and unutilized funds have to be returned to the GCBA at the end of the year.

After an electoral campaign focused on accusations of corruption by the previous administration, a new government was formed in the city of Buenos Aires in August 1996. Motivated by a number of informal reports of corrupt practices in the health sector, one of the first initiatives of the new authorities was aimed at controlling corruption in input procurement in public hospitals. On September 9, 1996, the GCBA Health Secretariat, through the Sub-Secretariat of Strategic Management, ordered the 33 institutions in its jurisdiction to supply detailed information, including date, price, quantity, brand, supplier, and supplementary information, for each purchase for a sample of supplies. From October 7, 1996, the information supplied by the hospitals was compiled and regularly sent back to all the hospitals. The price comparisons sent to the hospitals highlighted the highest and lowest prices for each input. Prizes or punishments were not announced at the time (nor were they applied on the basis of this information throughout the period), but at that time the procurement officers did not know how the Secretariat would use the information. The monitoring policy was interrupted in December 1997.

The government selected homogeneous products, with no significant differences in quality between articles produced by different suppliers. For normal saline, the Health

[3] The GCBA operates 29 treatment centers with hospitalization (13 general hospitals, 2 children hospitals and 14 specialized hospitals), 3 dental hospitals without hospitalization (two for adults and

authorities collected information going back to June 1996. For 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 in this study because there is no price data prior to the implementation of the monitoring policy. Thus, the four products included in the study are normal saline (500 ml.), ethyl alcohol (96%), iodine povidone (5%), and hydrogen peroxide (100 vol.). We study the effect of the anti-corruption crackdown using the unique database that was generated by this very same policy.[4]

III. The Effects of the Price Monitoring Policy

Under the monitoring policy, the GCBA Health Secretariat required the 33 institutions under its authority to send information on their purchases of a specific set of supplies. The auditing policy operated from September 1996 through December 1997, but, as explained before, the requested price information went back to June 1996.

Figure 1 shows the evolution of an index of GCBA input prices from June 1996 through December 1997. Prices fell significantly after September 1996, and this decline reached a minimum in February 1997.[5] After that time, prices increased, although, in average, they remained below their initial levels.[6] Although other factors may have affected these purchase prices, the price evolution suggests that the transparency policy had a strong initial effect, and that its effectiveness declined later.

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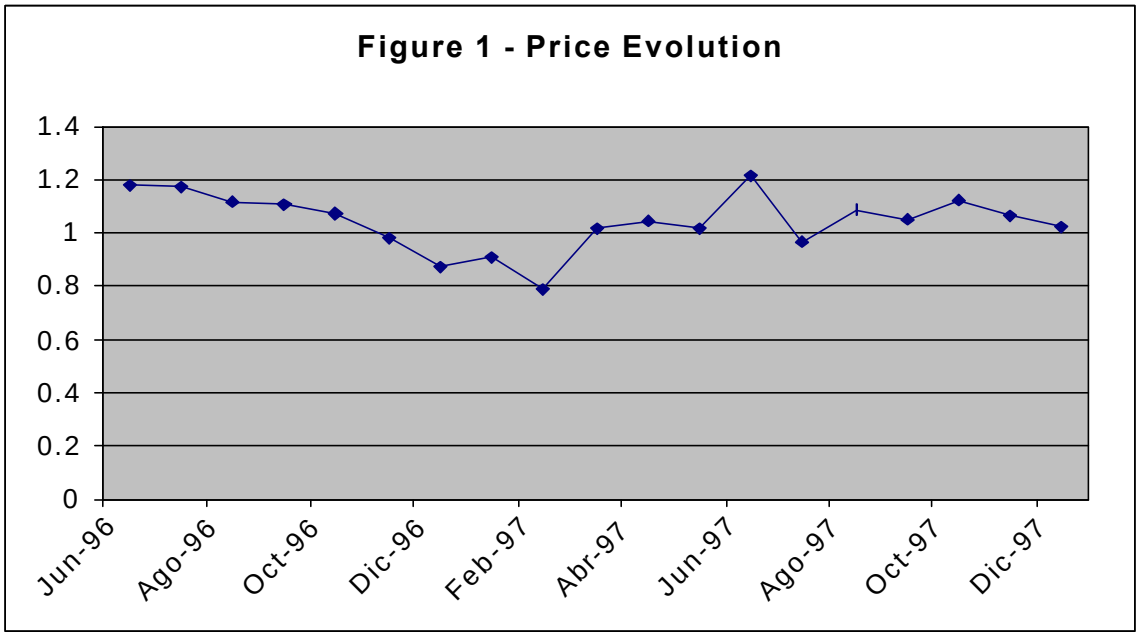
one for children), and a zoonosis institute. In a broad sense, we refer to all these institutions as "hospitals".

[4] The database is described in Di Tella and Schargrodsky (2001).

[5] Indeed, it could be argued that prices start to fall in August 1996, when the new government took office. Perhaps the implementation of the anti-corruption crackdown was correctly anticipated by the procurement agents.

[6] During the period of analysis, the pharmaceutical wholesale price index for Argentina shows no seasonality and very low variability (dropped 0.5%).



To evaluate more carefully the effect of the anti-corruption policy on procurement prices controlling for the effect of purchase size, hospital characteristics, and procurement officer characteristics, we estimate an equation of the following form:

$$PRICE_{iht} = \lambda Quantity_{iht} + \alpha Crackdown_t + \Sigma_h + \varepsilon_{iht}, \qquad (1)$$

where $PRICE_{iht}$ is the (log of the) price of the input bought in purchase i by hospital h in period t, Σ_h is an hospital fixed effect that controls for potential lack of independence of the error term within hospitals and for the potential effect of fixed hospital and officer characteristics, $Quantity_{iht}$ is the log of the size of each purchase (to control for quantity discounts), and $Crackdown_t$ is a dummy variable that equals one for the period when the monitoring policy was active, and zero otherwise. This dummy divides the sample period into two: the period June 1996-August 1996 before the policy was implemented, and the period September 1996-December 1997 when the policy was active. Product dummies are included in all regressions.

The effect of the anti-corruption policy on prices is presented in Table 1. Column A shows that the monitoring policy had an economically and statistically significant effect on

prices. Prices dropped 12.3% after the policy was implemented.[7] There is also evidence of quantity discounts. However, our preliminary observation showed that the immediate policy effect was stronger than the longer-term effect. To analyze these dynamics, we split the crackdown period into two: *Crackdown 1*, right after the introduction of the policy when the auditing is expected to be maximal, and *Crackdown 2*, when, as time goes by, auditing intensity could have declined.

In order to choose the break point between the two crackdown periods, we analyzed media focus on corruption. We reviewed the stories about corruption and the new administration at that time, and we found two stories giving wide coverage to the results of large opinion polls reporting that, regarding corruption, the inhabitants of the city of Buenos Aires consider that the performance of the new government is no better than that of its predecessor. Our hypothesis is that these stories signal the moment when procurement officers receive independent information on the weakness of the new government's commitment to fighting corruption. The publication dates of these polls are February 16, 1997, and May 19, 1997. 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. Thus, we now consider three periods: June 1996-August 1996, prior to the implementation of the monitoring policy; September 1996-May 1997, that starts when 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; and June 1997-December 1997, which covers the final period until the Health Secretariat stops the policy (and, thus, the data compilation).

Column B in Table 1 studies the effect of the monitoring policy partitioning the period of analysis in this way. Prices dropped by 14.6% in *Crackdown 1*, relative to their original levels, but recovered by five percentage points in *Crackdown 2*. Taken on their own, prices during *Crackdown 2* were still 9.7% lower than in the pre-crackdown period. The magnitude of the estimated effects is not out of line with anecdotal evidence on the size

[7] An intercept coefficient of a in these regressions is equivalent to a percentage change $exp(a)$-1.

of bribes in Argentina.[8] We reject the equality of the *Crackdown 1* and *Crackdown 2* coefficients at a 1% significance level. These results suggest that the monitoring policy appears to have provoked a fall in purchase prices, but that the transitory effect was greater than the longer-term effect. After a few months, prices began to rise again, although without regaining their initial level.

It could be argued, however, that although the GCBA Health Secretariat implemented the monitoring policy to fight corruption, there were actually no overprices in hospital procurement. The monitoring policy may have affected prices through other channels. One alternative hypothesis is that the price differentials across hospitals were a consequence of information asymmetries. If the hospitals paying high prices were unaware of the existence of cheaper suppliers, or if they were victims of discriminatory pricing, the distribution of price comparisons generated by the monitoring policy could have allowed a price reduction. However, under this interpretation is unclear why prices rebounded afterwards, before the interruption in the distribution of the price comparisons.

Another alternative hypothesis is that the price evolution was due to changes in the provision of effort by procurement officers that were induced by the policy. Lazy (but honest) officers only started to exert effort under the pressure from the Health Secretariat. In this case, the price rebound could be explained by the relaxation in the monitoring pressure. However, anecdotal evidence is overwhelmingly suggestive that corruption in input procurement in the city of Buenos Aires is high.[9] 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 significant. Respondents also considered corruption in the health sector to be not lower than the average country level.[10] More importantly, perhaps, the monitoring policy was officially designed to attack a problem of corruption. In several public speeches, the Health Secretary presented the

[8] Media investigations revealed that the price paid by the pensioners' social security agency for several services was inflated by more than 20%. A survey of German exporters indicates that bribes of 10% to 15% of the price of the exported goods were paid in order to place exports in state owned Argentine companies (Neumann (1994)).

[9] Two former heads of PAMI, the publicly provided health insurance for pensioners, were accused of buying services and inputs at inflated prices. The media has recently published allegations of overpricing in procurement of medicines, diapers, liquid oxygen and audiphones.

policy of monitoring prices as an attempt to control corruption with no reference to informational asymmetries or under-provision of effort.[11]

IV. Political Corruption Cycles

A key result in the study of this event is the temporary nature of the crackdown. As shown in Table 1, prices fell at the start of the crackdown, but finally rebounded. In a few months, prices were significantly higher than the lowest level reached at the peak of the crackdown. The literature has documented other instances of anti-corruption campaigns that are short-lived. Lui (1986) documents a number of anti-corruption campaigns that took place in China, over the last few decades. He describes how these are launched with great initial strength, only to subsequently dwindle. The phenomenon has also been discussed in policy circles. A United Nations study contrasts the fanfare of the inauguration of anti-corruption crusades with their unglamorous end (Bertucci and Armstrong, 2000). The reason for this short duration is often attributed during political debate to a lack of moral commitment to the cause of honesty. But another possibility that is not usually discussed has to do with the fact that the political benefits of such crackdowns change over time. Indeed, during the first phase of an anti-corruption campaign all the political benefits go to the reformer who has launched the initiative. As time goes by, each discovery of an act of corruption seems to convey more ambiguous information. On the one hand it is still good news because it shows that the reformer is trying to control corruption. But on the other hand the act of corruption has now taken place during the reformer's term in office. Thus, at the very least, the reformer is "guilty" of not having discovered it before. And he/she may even be guilty of having appointed the corrupt official, or of not having supervised him/her appropriately. Thus, the reformer will have stronger incentives to be tough on corruption early on in his/her term, but it will become more and more difficult to keep the auditing pressure as time goes by. Of course, the agents under supervision understand these incentives.

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

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

The experience of the crackdown on Buenos Aires hospitals provides some support for this view, where the incentives to controlling corruption are tightly linked to the political cycle. We can call this the political economy of anti-corruption campaigns. For simplicity, assume that acts of corruption can be discovered only after they are committed. When the government is new, it gets only credit from the discovery of acts of corruption. When the government is in office for some time, the discovery of an act of corruption is also an indication that the government was ineffective at controlling corruption in previous periods. The discovery itself is of course a good sign, and suggests that the government is not afraid of exposing corruption even when committed by its own employees. But if there is some possibility that forces beyond the government's control, such as the judiciary, the private sector (for example, the media), or some independent agency, denounce the act of corruption, then exposing corruption would only be harmful to the government.

This suggests that the timing of these crackdowns *within* each political cycle is not random. From the point of view of the government's incentives, anti-corruption campaigns should be started at the beginning of the government tenure in office. At that point in time, the political costs of discovering corruption are borne by the previous government, under whom the acts of corruption can be presumed to have taken place. In other words, the present government cannot be blamed for not controlling (because it was not in charge). As time goes by, the discovery of acts of corruption is informative of the present government's commitment to investigate corruption but also, and this is crucial, of the governments inability to prevent acts of corruption through monitoring and bureaucratic controls.

A simple model illustrates this is as follows. Suppose that the government has to choose the level of monitoring p that exerts on the procurement agents. Let's consider the incentives of the government to monitor the procurement officer. The government is honest but cares about its reputation amongst voters for exerting high monitoring effort. Consider an economy where elections take place every other year. For simplicity, re-elections are not allowed so that the government is in one of two states: just elected, or in its second (and last) year in office. As a benchmark, we consider the amount of monitoring put forward by a government that is just elected. Call this level p_1 and assume that some cost of monitoring (as in Becker (1968)) ensures that at the optimum $0<p_1<1$. Note that the benefit to the government is the increase in the government reputation for exerting high

effort. Although the act of corruption may have taken place in the past, the new government has controlled corruption at the first opportunity.

Now consider the incentives for a government that has been in office already one year and is in its second year. The discovery of an act of corruption now reveals two things. Just as in the case with a newly elected government, it reveals that the government is keen in exposing corruption when it sees it. But it also shows that the government was ineffective at monitoring corruption that had indeed taken place during the previous period. This combination of an imperfect detection technology that depends on the government efforts and the fact that the process is repeated over time, suggests that as the time goes by the incentives to "cover up" would tend to increase. Thus, we have that the probability of detecting corruption is higher for a newly elected government than for those who have been in office for longer periods of time, or that $p_1 > p_2$.

How do procurement officers react to these different detection probabilities? Consider a procurement agent that has the opportunity of committing an act of corruption that would provide a bribe b. Assuming that agents detected taking bribes do not receive any fine but are fired instead, the penalty depends on the wage earned in alternative employment, which we can call w^0. His current wage is w, and the perceived probability of getting caught is given by p. There is no discounting, so the condition for honesty is:

$$w > (1-p)(w+b) - pw^0 \qquad (2)$$

This is a standard incentive compatibility constraint of the kind introduced by Becker and Stigler (1974). If outside wages are distributed following the distribution function $F(w^0)$, one can write the equilibrium amount of corruption as $F(w^{0*})$, where w^{0*} is the level of w^0 that makes equation (2) above hold with equality. It is easy to observe that the equilibrium level of corruption will be declining in p. In particular, $w_1^{0*} < w_2^{0*}$ for $p_1 > p_2$.

This explanation gives predictions that coincide with the described experience. First, the anticorruption crackdown was introduced by a newly elected. Second, the program was discontinued 16 months later (the government had been appointed for four years). Third, corruption (overprices in our setup) is low during initial government periods

but rises afterwards. During *Crackdown 1,* 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. In *Crackdown 2*, 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 short life of anti-corruption crackdowns is consistent with previous informal descriptions (e.g. Lui (1986), Bertucci and Armstrong (2000)).

V. Corruption, Auditing and Wages

We can also analyze the role of wages during the anti-corruption crackdown. In their seminal paper, Becker and Stigler (1974) showed that above-market-clearing wages ("efficiency wages") paired with a non-zero audit probability could be used to deter misbehavior. An efficiency wage is the difference between an agent's nominal wage and the wage that an individual with similar personal characteristics could earn in an average job in the rest of the economy. This difference represents the magnitude of what the agent risks losing if he or she were to be caught in an act of corruption and dismissed. Presumably, higher efficiency wages induce workers to perform more efficiently (Yellen, 1984; Shapiro and Stiglitz, 1984). In our context, this argument would indicate that hospitals with better-paid procurement officers should be paying less for supplies. This is clear from rearranging equation (2), which yields:

$$p\left(w - w^0\right) > (1-p)b \tag{3}$$

This incentive compatibility constraint shows that the level of corruption depends negatively on efficiency wages $\left(w - w^0\right)$. However, a more subtle reading reveals that there is no relationship between wage premia and the frequency of corruption when the audit probability is very high or very low. As p tends to one, the condition is satisfied for

any positive wage premium, and none of the agents is corrupt. When p tends to zero, the condition is never satisfied and all the agents are corrupt. Thus, wages only affect corruption under intermediate levels of auditing.

In spite of its popularity, the corruption deterring effect of efficiency wages has received little empirical support.[12] Three recent papers that study the effect of efficiency wages on survey measures of corruption across countries provide weak support for a negative relationship between corruption and wages. Rauch and Evans (2000) finds no evidence that wages deter corruption. Furthermore, wages sometimes enter with the wrong sign and the coefficient is always very small when compared with other variables included to capture other aspects of bureaucratic efficiency. Treisman (2000) reports an insignificant coefficient on wages in a corruption regression. Van Rijckeghem and Weder (1997) finds evidence consistent with the theory in a cross section of 28 developing countries, but the evidence is not favorable to the standard model once fixed effects are included.

One potential explanation for the apparent empirical failure of the Becker-Stigler hypothesis is that, without controlling for auditing levels, 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 inclusion of those observations will induce the coefficient on wages in a corruption regression to tend to zero. Instead, we use the time series variation in auditing levels generated by the GCBA anticorruption crackdown to identify the importance of salaries in deterring corruption.

Our approach has some other advantages over prior work. Previous studies use data aggregated at the country level. Thus, the data on wages and corruption may refer to different individuals. Furthermore, they rely on survey measures of corruption. Instead, our hard data (prices, wages, and characteristics) correspond to the persons and hospitals actually making the purchases. Another issue concerns the direction of causality.

[12] International organizations routinely include recommendations to fight corruption by raising public servants' salaries (see, for example, World Bank, 1997). In a United Nations presentation, Bertucci

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. In our case, 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. This avoids simultaneity problems. 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 wage of the procurement officer, because the funds received to acquire inputs cannot be used to pay wages. Moreover, the incidence of omitted variables can be expected to be high in cross-country studies, but low in our micro exercise. All the hospitals are in the city of Buenos Aires and operate under a similar managerial, organizational, legal and cultural environment. In particular, procurement officers who are caught taking bribes face identical punishment, which basically amounts to dismissal from the job.

In order to analyze the effect of efficiency wages on prices at different auditing levels, we interact efficiency wages and our crackdown dummies by estimating the following equation:

$$PRICE_{iht} = \lambda Quantity_{iht} + \alpha_1 Crackdown1_t + \alpha_2 Crackdown2_t + \\ + \beta_1 Crackdown1_t * EW_h + \beta_2 Crackdown2_t * EW_h + \Sigma_h + \varepsilon_{iht}, \qquad (3)$$

where EW_h is the "efficiency wage" $(w_h - w_h^0)$, w_h is the log of the procurement officer's wage, and w_h^0, the opportunity wage, is 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.[13] Similar empirical results are obtained when we consider the (log of the) wage as our independent variable instead of the efficiency wage. In addition to the

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and Armstrong (2000) state that: *"Unless people can attain a minimum living standard through their employment, survival corruption will take place*".

[13] The details of the estimation of the efficiency wages are presented in Di Tella and Schargrodsky (2001), Appendix 2. According to our estimates, the average ratio of nominal to opportunity wages for our GCBA procurement officers is 1.68.

interaction with the efficiency wages, we allow the crackdown dummies to have a direct effect on prices.

Our results on the effect of procurement officers' efficiency wages on input prices are presented in Table 2. In column A, we include *Efficiency Wage* without interacting it with auditing levels.[14] As obtained in the previous literature, the effect of efficiency wages on prices is statistically insignificant. Without controlling for audit intensity, there is no evidence that wages help deter corruption.

We now exploit variations over time in the monitoring policy to proxy for 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 column B of Table 2.[15] Relative to the pre-crackdown 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 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 the second crackdown period, the wage premium elasticity of input prices is 25%.

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 before the crackdown, prices should be high and not sensitive to wages. In *Crackdown 1*, 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 *Crackdown 2*, the monitoring policy no

[14] As procurement officers' efficiency wages are fixed during the period of analysis, in this regression we estimate a random effects model. To proxy for the size of the hospitals in this model 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.

[15] We do not include independently the efficiency wages in this regression because they are fixed during the period of analysis and, thus, their effect is collapsed with the fixed effects.

longer has a strong average effect on prices. It does, however, still help keep prices in check in hospitals where officers are paid well.[16]

In Table 3 we summarize our theoretical predictions. We expect new governments to be tough on corruption, and that the strength of anti-corruption actions declines overtime. Consequently, corruption should be increasing as government ages. Moreover, we do not expect efficiency wages to affect corruption when auditing levels are very high or very low, but only at intermediate levels. Our empirical results from the analysis of the anti-corruption crackdown on hospital procurement in the City of Buenos Aires are consistent with these predictions.

Table 3

Electoral Cycle	Incoming Government (*Crackdown 1*)	Transition (*Crackdown 2*)	Old Government
Auditing	High	Intermediate	Low
Corruption	Low	Medium	High
Effect of Wages on Corruption	Low	Negative	Low

VI. Conclusions

The first wave of empirical work on corruption used mainly cross-country subjective data. In an attempt to complement this work we take advantage of a crackdown on corruption that occurred in the city of Buenos Aires, Argentina, in 1996-7. In an original and interesting initiative adopted to fight corruption in procurement procedures, the newly elected city government collected and compared the prices paid by all public hospitals in the city for a number of very homogeneous supplies. Beginning in October 1996, the information was summarized and sent regularly to all the hospitals in a form that highlighted the institutions that had paid the highest and lowest prices for each supply item. The policy was interrupted in December 1997. This experiment provides an ideal environment where to

[16] Di Tella and Schargrodsky (2001) extends and interprets these results. The evidence is not consistent with the alternative interpretations that efficiency wages proxy for high ability or for better connections.

study the role of economic and political incentives in determining the observed levels of corruption.

We find a large and well-defined fall in input prices equal to almost 15%, following the introduction of the monitoring policy. An important first finding is that, as in previous accounts of corruption-crackdowns, the estimated effects of the policy fall over time (Lui (1986), Bertucci and Armstrong (2000)). In other words, the effects of the anti-corruption initiative are temporary. After the initial nine months, average prices paid by the procurement officers rise, but are still 10% lower than the pre-crackdown levels. We depart from previous work that sees the temporary nature of such campaigns as evidence in favor of multiple equilibria models of corruption or to a lack of moral stamina of the reformers and consider the political economy of such campaigns. We note that a government typically performs two tasks. It controls corruption that takes place today, and it reports any bribery that is found to have taken place in previous periods. As a benchmark, consider a government that has just been elected. The government has full incentives to report corruption. The public will assume that the government discovered corruption as soon as it could. Corruption either took place this period and was immediately reported by the government, or it took place last period under the previous government who failed to report it and should be blamed for it. Different is the case of a government that has been in power for a couple of periods. Detecting corruption now reveals that corruption has taken place while this government was in office. The option of blaming the previous administration is not open to it. This problem is particularly acute when a second source of information (for example the media) could be responsible for detecting corruption.

A second application of the crackdown is to use the time series variation in audit policies to test the hypothesis that high wages paired with intermediate auditing policies tend to reduce corruption (Becker and Stigler (1974)). 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 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 our sample, just as predicted by the theory, the effect of wages on input prices is insignificant during the pre-crackdown period and during the first phase of the crackdown, when audit intensity is likely to be nil and maximal, respectively. The effect, however, is negative and statistically well defined during the last phase of the crackdown, when monitoring intensity can be expected to take intermediate levels (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%.

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 been influential in the literature and has also affected the policy debates where one of the most often heard policy proposals is to increase the salaries of public officials. Our findings suggest that the degree of audit intensity is crucial for the effectiveness of anti-corruption policies based on salary increases. 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. Our findings also suggest that the political incentives for implementing tough auditing practices may diminish over the time that the government is in office, creating what can be called political corruption cycles.

Table 1: The Effect of the Corruption Crackdown on Prices

	A	B
Quantity	-0.05297***	-0.04792***
	(6.196)	(5.534)
Crackdown	-0.13076***	
	(4.945)	
Crackdown 1		-0.15869***
		(5.686)
Crackdown 2		-0.10153***
		(3.619)
Fixed Effects	Yes	Yes
F-stat.†		8.69***
Observations	544	544
R^2	0.79	0.80

Source: Di Tella and Schargrodsky (2001), Table 1.
Notes: t-statistics are in parentheses (absolute values). Regressions include product dummies. † Null hypothesis: "Crackdown 1" = "Crackdown 2". *** Significant at the 1% level. Dependent Variable: log of price.

Table 2: The Role of Wages during the Corruption Crackdown

Variables	A	B
Quantity	-0.03714***	-0.04775***
	(4.913)	(5.538)
Beds	0.00920	
	(1.020)	
Crackdown 1	-0.15532***	-0.10420
	(5.546)	(1.484)
Crackdown 2	-0.10081***	0.03165
	(3.631)	(0.467)
Efficiency Wage (EW)	-0.01020	
	(0.216)	
EW * Crackdown 1		-0.10679
		(0.884)
EW * Crackdown 2		-0.25061**
		(2.151)
Fixed Effects	No	Yes
Random Effects	Yes	No
Observations	544	544
R^2	0.80	0.79

Source: Di Tella and Schargrodsky (2001), Table 2.
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.

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