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CORRUPTION IS BAD FOR GROWTH (Even in the United States)

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Abstract

We estimate the impact of corruption on growth of output per worker in U.S. states. We improve on existing studies of the cost of corruption by using a better specified empirical model, focusing on a study population that is less likely to be affected by parameter heterogeneity, and controlling for endogeneity using political variables to instrument for corruption. We find that corruption plays a significant and causal role in lowering growth and investment across the states.

JEL: D74, F51, O10
Keywords: corruption, investment, economic growth.

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I. Introduction

There is no doubt that corruption is part of the U.S. political scene.[1] In Illinois, Rod Blagojevich is the sixth governor of that state to be arrested or indicted on corruption charges (*Time*, "Illinois Corruption," Dec. 11, 2008). The recent scandals in New Jersey have grown to encompass 44 people, among them three mayors and two state assemblymen. These arrests are apparently so consistent with the history of New Jersey politics that the *New York Times* ran a symposium asking the question, "What's Up With New Jersey?" (*New York Times*, July 26, 2009). Despite the massive attention in the popular press, however, much more ink has been spilled in an attempt to explain why corruption exists rather than asking, "Does it matter?" Type into Google, "Cost, Corruption, Blagojevich" and the lone relevant article which appears warns that if Illinois doesn't shape up, it will go the way of Louisiana and scare away potential investors (Chicago Tribune, "The Cost of Corruption," Dec. 14, 2008). The article's argument is based on the actions of one company (ThyssenKrupp) choosing not to build a factory in Louisiana one time (in 2007). Is that it? Can we not do better? Does corruption really affect economic growth in U.S. states? If so, is investment diversion a channel through which it operates?

The academic literature, while never mute, also does not shout about this issue. Indeed, many early studies argue that corruption could be *good* for growth (Leff 1964, Huntington 1968). In a state rife with bureaucracy and ill-conceived regulation, the cost of getting things done is high and a well-placed bribe may move things along more efficiently. Myrdal (1968) critiques this early position by pointing out that bad laws and bureaucracy may be endogenous to corruption. However, the idea that corruption is good for growth persists. Lui (1985) contends that corruption favours the survival of the most efficient firms. More recently, an influential study by Glaeser and Saks (2006) finds little evidence that corruption slows U.S. state growth.

They conclude from this that causality runs from growth to political institutions, rather than the other way around. The stakes in this debate are high, but clear answers are hard to come by.

The cross-country growth literature identifies many ways that corruption distorts economic activity. Murphy, Shleifer, and Vishny (1991, 1993) find that corruption supports inefficient firms and misallocates talent, technology, and capital. Additional microeconometric evidence for the cost of corruption in developing countries comes from Fisman and Svensson (2005), De Soto (1989), Svensson (2003), Fisman (2001), and Olken (2007). In contrast to the micro evidence, however, it is unclear whether corruption has any effect on important macroeconomic outcomes like growth rates. In one of the most widely cited studies of corruption, Mauro (1995) fails to find robust evidence for a correlation between corruption and economic growth among a cross-section of countries. Svensson (2005) uses updated data but reaches the same conclusion.

While the idea that corruption is bad for growth is intuitively appealing, it is difficult to support econometrically for several reasons. First, corruption may affect different countries in different ways. This parameter heterogeneity problem is especially prevalent in the cross-country literature (Brock and Durlauf 2000). Ehrlich and Lui (1999), for example, identify a strong negative effect of corruption on relatively poor and democratic countries, but not for autocracies or richer democracies. Second, endogeneity is likely to be a serious problem. For example, an unobserved third variable, such as culture or urban politics, may drive the relationship between corruption and growth, or, there may be reverse causality with growth driving corruption (Huntington 1968). Finally, the measure of corruption itself may be biased. The most common measure used in cross-country work, the International Country Risk Guide (ICRG), is survey based. Aside from the obvious point that corruption may mean different

[1] We define corruption as the use of public office for private gain (Rose-Ackerman, 1978).

things to the average Nigerian as opposed to the average Italian, it is also entirely possible that perceptions of corruption are influenced by perceptions of economic success, thereby dooming attempts to estimate the effect of corruption on growth from the start.

We restrict our attention to the cost of corruption in U.S. states because the question is important in and of itself and because this allows us to address many of the empirical shortcomings that plague the cross-country literature. Since U.S. states share a common national culture and federal government, the potential for parameter heterogeneity is significantly reduced. Also, the economies, and by association, the growth experiences, of U.S. states are more similar than those of a typical cross-section of countries. Focusing on the U.S. also allows us to take advantage of a measure of corruption that is superior to the survey based measures used in the cross-country literature. We use state-level data on the number of corruption convictions reported by the U.S. Department of Justice between 1975 and 2000. This is the same measure used by Fredricksson, List, and Millimet (2003), Goel and Nelson (1998), Depken and LaFountain (2006), and Glaeser and Saks (2006). Finally, we address potential endogeneity issues by instrumenting corruption using political variables that are unlikely to be related to growth other than through their effect on a state's governance. We then estimate unbiased effects of corruption on growth and investment using both two-stage least squares (2SLS) and limited information maximum likelihood (LIML) methods along with the conditional likelihood ratio (CLR) test to control for weak instruments (Moriera 2003, Andrews, Moreira, and Stock 2006, 2007).

We find that corruption has a negative and significant effect on growth in U.S. states. Our preferred specification indicates that a one standard deviation increase in per capita corruption convictions between 1975 and 2000 (or about 0.013 additional convictions per

100,000 people) causes the average annual growth rate of gross state product per worker to fall by almost a third of a standard deviation (or about 0.19 percentage points), all else being equal. After we instrument for corruption using exogenous political variables, our estimates suggest that the same increase in corruption convictions causes average annual growth rates to fall by up to three-quarters of a standard deviation (or about 0.5 percentage points). Consistent with the cross country literature, we also find that corruption decreases investment.

Our finding that corruption is bad for growth is surprising in light of the fact that Glaeser and Saks (2006) fail to find the same thing in their study of corruption in the United States. They use the same corruption convictions data, the same growth data, and almost the same specification, but find an insignificant effect of corruption on growth.[2] We argue in the next section that this stems from their use of a misspecified regression equation. In their growth regressions, they use initial gross state product per worker in order to control for standard Solovian convergence effects. By doing so, they are implicitly assuming that the initial institutional endowment for all the states is identical. As we argue below, this is probably a poor assumption and introduces potentially serious multicollinearity into their regression equation that can explain their insignificant coefficient on corruption. Also, Glaeser and Saks (2006) control for endogeneity in their growth regression by looking at the effect of corruption between 1976 and 1980 on growth between 1980 and 2000. In contrast, we are able to exploit the entire time series variation in the data by instrumenting for corruption with state level political variables that have support in the theoretical and empirical literatures on the causes of corruption. Thus, we view our main result, that corruption is costly for growth, as a complementary to, but also an

[2] Compared to our results, Glaeser and Saks (p. 1067) report a one standard deviation increase in corruption leads to about a quarter of a standard deviation decrease in growth. We find that a one standard deviation increase in corruption leads to about 0.30 of a standard deviation decrease in growth. The difference is that our estimate is highly significant. We discuss this finding in greater detail in the next section.

important improvement on, the work by Glaeser and Saks. If we are going to worry about the causes of corruption in U.S. states and dedicate significant resources to preventing it, we should at least know how much it costs. Furthermore, if we are going to interpret corruption as a reflection of institutional quality, then knowing whether it causes, or is caused by, economic growth is a vital component of a much larger debate.

II. Identification Strategy and Data

We model the effects of corruption on growth as follows:

$$\textit{growth in output per worker}_s = \alpha + \beta\,\textit{corruption}_s + \gamma\,\textit{initial capital}_s + X_s'\eta + \varepsilon_s \qquad (1)$$

where $s = 1, \ldots, 50$ indexes states, *growth in output per worker* is the annual growth rate of real gross state product (GSP) per worker, *corruption* is the number of federal, state, and local public corruption convictions per 100,000 persons, *initial capital* is the initial capital stock per worker in 1970, X is a vector of other determinants of growth, and ε is an i.i.d. normal error term.

We use cross-section data for U.S. states (excluding the District of Columbia) for 1970–2000. We use 1975–2000 averages of *growth in output per worker* and *corruption*. To ensure exogeneity, we use 1970 values (or the closest year available) for control variables in X and for the instruments in our 2SLS and LIML regressions. We follow Mauro (1995) and Glaeser and Saks (2006) in selecting the variables for X. We include the initial share of the adult population with a high school degree or less; initial population level; population growth rate; initial ratios of state and local government consumption expenditures, capital outlays, and tax revenue to personal income. We include indicators for the North, Midwest and South regions (West is omitted) in all regressions. Table 1 contains descriptive statistics and data sources.

Our measure of corruption is the number of convictions of federal, state, and local employees by the Public Integrity Section of the U.S. Department of Justice. Examples of activities defined as corruption include accepting bribes or kickbacks from private entities engaged in business with the government, falsifying travel expense documents, stealing government property, possession of narcotics with intent to distribute, smuggling illegal aliens, or awarding government contracts without a competitive bidding process (Public Integrity Section, Criminal Division, U.S. Department of Justice 2001).

Figure 1 plots the average number of corruption convictions per 100,000 people against the annual growth rate of gross state product.[3] The most corrupt states are Arkansas, Mississippi, and Louisiana. The least corrupt states are Washington, Oregon, and Vermont. The simple correlation between corruption and growth is negative (ρ=-0.39) and significant at the 1 percent level. However, there is a lot of variation in growth rates for each level of corruption, and, as discussed below, the potential for endogeneity is high.

An obvious objection to our convictions measure is that corruption may extend to the judicial machinery of a state, thereby reducing the number of convictions and biasing reported corruption downwards. However, the fact that the convictions are pursued at the federal level with federal resources mitigates this concern. Since the U.S. Department of Justice applies an equal standard across the country, this is a potentially unbiased source of information on the extent of corruption. It is also important to note that our measure of corruption includes all convictions by federal authorities of federal, state, or local officials. However, we do not have data on state and local convictions of corrupt public employees. As such, our measure is

[3] We also did our entire analysis using corruption convictions per state employee and find no significant differences.

probably an understatement of the true extent of corruption.[4] We assume that the extent to which corruption is understated is not correlated with state growth rates.

A key difference between the growth specification we outline in (1) and the specification used by Glaeser and Saks (who fail to find an effect of corruption on growth) is that we use the initial capital-to-labor ratio rather than initial real GSP per worker. There are good reasons for doing this. The standard justification for including initial output per worker in growth regressions is to capture convergence driven by the diminishing marginal product of capital. Early derivations of regression specifications from Solow's theoretical model by Barro and Sala-i-Martin (1992) and Mankiw, Romer, and Weil (1992) explicitly assumed that technology is fixed across states in order to eliminate the need to find proxy values for the technology terms that flowed out of the model.[5] In effect, by assuming the initial level and rate of change of technology are identical across states, one can collapse these terms into the intercept. No harm, no foul, and more importantly, no bias.

There are well known objections to this assumption, including those of Solow himself:

> The usual presumption seems to be that technology is universal, if only because handbooks of science and engineering are easily and promptly available everywhere. But that seems superficial to me. Abstract technological knowledge by itself butters no parsnips. For two countries to have effectively the same technology is very much a matter of workers' skills and attitudes towards work, managerial and administrative habits, interpersonal attitudes, social norms and institutions, and no doubt many other hard and soft characteristics of the economic and social environment (Solow 2000, p. 103).

If technology is interpreted as capturing broader institutional variables, like corruption, then the assumption of constant technologies when specifying growth regressions does not seem valid.

[4] We also investigated the possibility that corruption conviction are endogenous to Justive Department Resources dedicated to a given state. We collected data on attorney hours allocated by Justice towards the Public Integrity Section in each state and regressed this on corruption convictions per capita. The coefficient on attorney hours is both economically and statistically insignificant (elasticity = 0.04 and p-value=0.33).
[5] See McQuinn and Whelan (2007) for a discussion of this assumption.

This is not an original critique. Given the problematic nature of the "fixed technology" assumption, researchers started relaxing it by including variables which proxy for technological factors like those mentioned by Solow. However, our argument is that this is not enough. The constant technologies assumption also carries with it another identifying assumption which is less acknowledged in the literature. To be precise, if one wants to capture convergence using initial output per worker, then one has to also assume that the initial technological endowment of states are also identical (Kocherlakota and Yi 1995). To see this, simply recall that the Solow model is derived from a constant returns to scale production function like the familiar Cobb-Douglas form:

$$Y_t = K_t^{\alpha}\left(A_t L_t\right)^{1-\alpha} \qquad (2)$$

where L is labor, K is physical capital, A is the level of technology, and α represents the elasticity of output with respect to capital. If we take logs, set $t=0$, and then rearrange (2) in order to get an expression for initial output per worker, the result is:

$$\ln\left(\frac{Y_0}{L_0}\right) = (1-\alpha)\ln\left(A_0\right) + \alpha\ln\left(\frac{K_0}{L_0}\right) \qquad (3)$$

Initial output per worker is an additive function of *both* initial technology and initial capital per worker. The term is supposed to capture convergence driven by the diminishing marginal product of capital; $\left(A_0\right)$ is just along for the ride. If the original Mankiw, Romer, and Weil assumption of fixed technologies across states holds, then this is not a problem, since in its logged form, the technology term in (3) simply collapses into the constant. Unfortunately, if this assumption does not hold, and if one includes a term to capture the effect of technological endowment on growth on the right hand side of the regression equation, then using initial output per worker to capture convergence also introduces unwanted multicollinearity. Simply stated, if technological endowment matters for growth, then initial output per worker and the proxy for technology will be correlated and parameter estimates cannot be trusted. No wonder it is difficult to find an effect of corruption on growth. Scholars have been confounding its effects with those of convergence.

Questions immediately leap to mind. First, if $\frac{Y_0}{L_0}$ is a problem, then why doesn't everybody just use initial capital per worker instead? $\frac{K_0}{L_0}$ is, after all, the variable we focus on when we actually do theory with the Solow model. The answer is, of course, "data availability." Especially in the cross country setting, there are simply no consistent data on capital per worker. Output per worker, on the other hand, is easy to find. Happily, there *are* consistent data on capital per worker for U.S. states. We use data constructed by Garofalo and Yamarik (2002), who generate state-level capital stock series by apportioning the national capital stock to states according to their share of industrial income. Their procedure, which classifies income using one-digit Bureau of Economic Analysis industry codes, is an improvement on the capital series provided by Munnell (1990) who separates income into just three categories (agriculture, manufacturing, and non-agricultural manufacturing from five-year census intervals).

A second, more challenging question is, "So what?" Multicollinearity is almost always a problem in regression analysis, so why should we worry about it in this particular circumstance? Our results suggest that it is of first-order importance when discussing corruption and growth in U.S. states. The correlation between corruption and initial output per worker is 0.27 and is significant at the 6 percent level. The correlation between initial capital per worker and corruption is, in effect, zero. Furthermore, our point estimate for the impact of corruption on growth is virtually identical to that of Glaeser and Saks (2006). We both find that a one standard deviation increase in corruption leads to a one-quarter to one-third of a standard deviation reduction in growth per worker. The difference is that their estimate is insignificant whereas ours is significant at the 1 percent level. This is consistent with multicollinearity problems.

Another question is whether or not corruption is really the same thing as initial technology? The rest of the literature answers this question in the affirmative. Glaeser and Saks (2006) implicitly assume this is the case with their identification strategy of using initial corruption to explain second period growth. In both the empirical and theoretical literatures, corruption has been associated with relatively permanent state characteristics such as constitutional rules governing judicial appointments (Alt and Lassen, 2007), access to the franchise (Rose-Ackerman, 1978), penetration of the news media in the population (Adsera *et. al.*, 2003), racial and income inequality (Glaeser and Saks, 2006), and ethnic fractionalization (Mauro, 1995). While we acknowledge that corruption may also affect the rate of growth of technology, we follow most of the literature in interpreting it as a relatively ingrained characteristic of a state.

One last potential concern with specification (1) is that corruption and economic outcomes may be simultaneously determined. For example, southern states tend to have relatively high degrees of ethnic fractionalization. Perhaps that fractionalization drives both corruption (as argued by Mauro in the cross country context, for example) and economic growth directly via its effect on public goods provision (as argued by Alesina *et. al.*, 1999). Thus, negative correlation observed between corruption and growth would be spurious. Stories like this can be multiplied *ad infinitum*.

In order to minimize potential bias in our estimates of the effect of corruption on growth and investment, we identify state level instruments that are correlated with corruption, but independent of the error term in equation (1). Good cross-country instruments for corruption are difficult to find (Shaw, Katsaiti, and Jurgilas 2006) and similar difficulties arise in the state context. One source of potential instruments is the theoretical literature on the causes of corruption, which suggests focusing on institutions that impose constraints on the actions of public servants. For example, Rose-Ackerman (1978) argues that a powerful channel through which corrupt officials are constrained is through the franchise, and Adsera et. al. (2003) argue that a more informed electorate is better able to punish errant public servants at the polls. As such, our first two instruments are directly related to the costs to the electorate of punishing corrupt acts by individuals or parties at the polls.

The first instrument is the number of days an individual had to be in residence in a state in 1970 in order to be eligible to vote. The longer the waiting period, the more likely the state has a history of disenfranchisement. This suspicion is confirmed by the scatter plot of residency requirements against corruption convictions in figure 3a. The correlation between the two variables is 0.42 and is significant at the 1 percent level. Mississippi, Louisiana, and Alabama led the pack in 1970 by requiring an individual to have a full year of residency before being eligible to vote. Somewhat further behind are several other southern states including Georgia, Florida, and South Carolina. The residency requirement is probably capturing a whole slew of historical institutions associated with Jim Crow, both formal and informal, which served to sever the link between large classes of citizens and the ballot box (Kousser 1999).

The second instrument is an index of campaign finance restrictions in place in 1970. There is ample support from the literature on campaign finance that contributions to candidates translate into more votes and that the reasons for these donations are often for some sort of *quid pro quo* (Grier 1989, Coates 1998, Stratmann 1995, 1998). As such, the more lax are campaign finance restrictions, the more likely it is that a candidate will be elected or defeated for supporting the private interests of donors rather than the general public interest of reducing corrupt practices. Figure 3b shows the scatter plot of our campaign finance restriction index against corruption. A higher value for the index indicates more severe restrictions. The correlation between the two variables is -0.30 and is significant at the 5 percent level.

Our last instrument is motivated by the observation that a state's constitution plays a fundamental role in defining the rules governing its politics. Sometimes those rules need to be changed or updated to be consistent with changes in other social or cultural institutions. This can be done by amending the current constitution (usually through the legislature) or by adopting a new constitution. Every state except Vermont has used both methods, but there is a great deal of variation. For example, Louisiana had four constitutions between 1901 and 1996 whereas Ohio had just one (Tarr 1998, p. 137). We argue that amending the existing constitution is a relatively nuanced way to alter the rules dictating the governance of a state, while rejecting the existing constitution and remaking the fundamental social contract between a state and its citizens is a relatively blunt way to do so. When the old rules are invalidated and a constitutional convention is held to establish new rules, many more issues are on the table than are with a run-of-the-mill amendment. As such, the durability of a state's constitution is a measure of the quality of the rules by which it is governed. The quality of these rules should in turn be related to corruption. We choose to measure the durability of a state's constitutional framework by measuring the age of its current constitution as of 1970. Figure 3c illustrates the strong negative relationship between this measure and corruption rates. The correlation between the two variables is -0.38 and is significant at the 1 percent level.

III. Results

Table 2 reports estimates of the impact of corruption on state-level growth using equation (1). Columns (1) and (2) present the OLS results. In column (1), we estimate the impact of corruption on GSP growth controlling only for initial capital per worker. Half of the variation in growth is explained by just these two variables with each coefficient being negative and highly significant. The point estimate for corruption indicates that a one standard deviation (0.013 in our data) increase in corruption convictions per 100,000 persons reduces growth by 0.23 percentage points. In other words, a one standard deviation increase in corruption reduces growth by a little more than one third (0.35) of a standard deviation. Column (2) adds controls for initial education, initial population, population growth, and state and local fiscal policy. Adding additional controls reduces the estimated impact of corruption, but not by much, and the effect remains economically and statistically significant. In this case, a one standard deviation increase in corruption reduces growth by a little less than one-third (0.30) of a standard deviation. As noted in the previous section, these point estimates are consistent with those of Glaeser and Saks (2006). The difference is that our estimates are statistically significant.

We control for the potential endogeneity of corruption in columns (3)–(6) by using 2SLS and LIML estimators. In each first-stage regression, the political instruments enter with their correct signs and are jointly significant. Moreover, the Sargan chi-squared statistic is insignificant at any reasonable level, indicating that the instruments are exogenous. The cost of corruption doubles under both 2SLS and LIML estimation. The full specifications in columns (4) and (6) indicate that a one standard deviation increase in corruption reduces growth by about two-thirds of a standard deviation. By contrast, the estimated effects of a one standard deviation increase in either education or initial population are about a third of standard deviation increase in growth. Thus, according to our best estimates, corruption plays at least as large a role in state growth as do the more traditional macro variables that have received greater attention.

In the cross-national literature, Shaw, Katsaiti, and Jurgilas (2006) show that the instruments for corruption used by Mauro (1995) are "weak" as defined by Staiger and Stock (1997). Staiger and Stock suggest that a first-stage F-statistic of ten or higher indicates acceptably strong instruments. In the presence of weak instruments, instrumental variable (IV) estimators, such as 2SLS, lose consistency resulting in bias and size distortion in hypothesis testing (Staiger and Stock, 1997). Our low F-statistics also suggest that our instruments may be weak.[6]

We employ two methods for point estimation and testing that are robust to the presence of weak instruments. First, we use LIML for point estimation. LIML typically has better small sample properties than 2SLS, especially with weak instruments (Hausman, Stock and Yogo, 2005). Unlike 2SLS, LIML does not have finite moments when the number of overidentifying restrictions is small. As a consequence, the variance of LIML is typically large, but the distribution of the LIML estimator is generally centered near the true value.[7] Second, we use Moreira's (2003) CLR test statistic to derive 95-percent confidence intervals.[8] The CLR test has correct coverage probability in the presence of weak instruments. If the instruments are irrelevant, the CLR test will return an unbounded confidence interval correctly $(1-\alpha)$x100% of

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[6] Stock and Yogo (2005) present a formal test of weak instruments. The test compares the minimum eigenvalue of the generalized *F*-statistic (the *F*-statistic itself with one endogenous variable) to critical values based upon the worst possible case of weak instruments. The testing procedure compares the minimum eigenvalue to pre-determined critical values under which the size of a nominal 5% test about a parameter of interest were actually r percent. For 2SLS, the test *fails* to reject the null of weak instruments at r = 30% in both columns (3) and (4). However, for LIML, the test rejects the null of weak instruments at r = 20% in both columns (5) and (6).
[7] If there is exact identification, LIML and 2SLS are identical.
[8] Andrews, Moreira, and Stock (2006) show that the CLR test is more powerful than other available tests on endogenous variables in an IVmodel.

the time. However, if the instruments are sufficiently strong then the CLR test will return a bounded confidence interval centered on the LIML point estimate.

The results in columns (5) and (6) show that corruption reduces state-level growth. In each column, the LIML point estimate for corruption is negative and significant at the 1 percent level. Moreover, the 95 percent confidence interval generated by the CLR statistic is bounded and negative. Given that LIML and CRL are robust to weak instruments, we can conclude that the negative link between corruption and growth is not a mere artifact of invalid instruments.

One possible channel through which corruption affects growth is by reducing investment. This is the mechanism alluded to by the article on the cost of corruption in Louisiana cited in the Introduction. More generally, entrepreneurs may respond to corruption by choosing "fly-by-night" technologies with too little fixed capital so they can credibly threaten to disappear should bribe demands become too high. At the same time, uncertainty and the need for secrecy raise transactions costs surrounding a bribe above those for a tax of the same magnitude (see Svensson 2005 and the references therein). If corruption is bad enough, entrepreneurs shift production from the formal to the informal sector of the economy (Johnson, Kaufmann, and Shleifer 1997). The necessity of flying under the radar in this case also reduces investment.

In order to identify a possible effect of corruption on investment, we estimate the following model:

$$investment\ share_s = \alpha + \beta\, corruption_s + \gamma\, initial\ capital_s + X_s'\mu + \varepsilon_s \qquad (4)$$

where *investment share* is the ratio of investment to GSP. *Corruption* and the controls in *X* are identical to those in our growth regressions.

Table 3 presents estimates of the impact of corruption on investment. In column (1), we explain investment using just corruption and initial capital. Our estimates suggest that a one standard deviation increase in corruption convictions reduces investment as a share of GSP by 0.44 percentage points, or a little less than one-third (0.31) of a standard deviation, and the effect is statistically significant at the 5 percent level. When we include controls for education, initial population, population growth, and state and local public finance, the coefficient on corruption retains its significance but is reduced in size. A one standard deviation increase in corruption convictions reduces investment as a share of GSP in this case by 0.32 percentage points, or a little less than one quarter (0.23) of a standard deviation.

Columns (3)–(6) show that when we control for endogeneity, our estimates of corruption's impact on investment are larger but less precise. Under 2SLS with the full set of controls (column (4)), a one standard deviation increase in corruption implies a reduction in investment by half a standard deviation, though the estimated coefficient is no longer significant at traditional levels. We have the same concerns about weak instruments in the investment regressions as with the growth regressions. Thus, in columns (5) and (6) we report LIML estimates along with the CLR test to provide a robust estimate of the effect of corruption on investment. The point estimates in columns (5) and (6) are negative and also imply a cost of corruption of about the same magnitude as 2SLS. Furthermore, the coefficients are now significant at the 10 percent level. The 95 percent confidence interval generated by the CLR statistic is no longer bounded and negative, but most of the interval is consistent with corruption reducing investment across the U.S. states.

Overall, our results suggest that corruption may affect state growth through its effect on investment. However, these results are somewhat fragile, indicating that corruption probably also affects growth through other channels.

V. Conclusion

It is often assumed in the United States that corruption is somebody else's problem. It is in the developing world where dictator's wives buy thousands of shoes with taxpayer money and rulers build mansions in the face of grinding poverty. It is unlikely that this belief stems from a mass self-delusion on the part of American citizens that their public servants are angels. The evidence to the contrary is all too apparent in the media. Rather, this belief probably stems from two sources. First, as Tullock (1990) observes, the dollar amounts associated with American corruption seem ludicrously small. There are no palaces being built by corrupt American officials, nor is there theft on the scale perpetrated by Zaire's President Mobutu Sese Seko, who had looted an amount equal to his country's entire external debt at the time of his ouster in 1997 (Svensson, 2005). By contrast, the numbers involved in the recent New Jersey scandals are laughable. The Mayor of Hoboken, Peter Cammarano, is accused of accepting some $10,000 in bribes (New York Times, July 23, 2009). The true social cost of these payments, however, may be much higher. Officials compete for bribes, so the actual amount paid may be well below the value of the service provided. Similarly, public officials in a democracy are not always in a position to extract full value from their corrupt activities because they can be "outed" by the very people who are trying to influence them. African dictators rarely face similar issues.

A second reason why the cost of corruption may be underappreciated in the United States is because we assume that, even though a few officials may be bad, the "system" is good. We provide evidence that this is not the case. Higher conviction rates of public officials are strongly related to lower economic growth. Our OLS results suggest that corruption plays at least as great a role in explaining the growth experiences of U.S. states as other macroeconomic determinants like population and education. Under instrumental variables estimation, we find that corruption becomes even more of a drag on growth. We also provide evidence for a plausible channel through which the corruption operates. Capital markets in the United States are heavily integrated and capital flows are highly elastic. When a culture of corruption in a state raises uncertainty or the cost of doing business, capital flows to more amenable institutional environments. Our finding that investment is reduced in states with higher corruption rates provides plausible support for this view.

So why does the cost of corruption matter? First, if corruption actually affects economic growth, then we should condemn it for more than purely ethical reasons. Identifying the causes of corruption and implementing costly strategies to reduce it are justified by our findings. Increasing the pay of existing public employees or hiring new ones to monitor the old ones costs money and how much we want to spend should depend on the cost of corruption itself.[9]

More broadly, establishing whether or not political corruption causes lower growth influences debates about reform.[10] If an absence of corruption is simply a by-product of a well-educated populace or good economic policies that generates no real benefits, then investing in costly political reform is unnecessary. However, if corruption causes low growth, then the reform of political institutions deserves to take center stage. Our finding that corruption is bad for growth undermines one of the main claims of Glaeser and Saks (2006, p.1068) that "development improves political institutions, rather than political institutions determining development". At least in the United States, this appears to be far from the whole story.

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[9] For the theoretical development of this view, see Acemoglu and Verdier (1998).

[10] Glaeser et. al. (2004) contend that good economic policy is necessary for good growth. Acemoglu et. al. (2001) and North (1990) argue that good political institutions cause good growth.

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Table 1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Growth Rate in Real GSP per worker (Annual Average 1975-2000)	0.0104602	0.0066268	-0.0064123	0.0295616
Investment/GSP (1975-2000 avg.)	0.0820067	0.0141802	0.0610942	0.1230727
Corruption Convictions per 100,000 pop. (1975-2000 avg.)	0.0272564	0.0129466	0.0074948	0.0609403
Ln(capital per worker in 1970)	10.78953	0.2067082	10.39079	11.42784
Proportion with High School Degree or Less in 1970	0.7829862	0.0434299	0.6851989	0.8589363
Ln(population in 1970)	0.8948371	1.062384	-1.186369	2.99802
Population growth (1970-2000)	0.0115564	0.00928	0.000826	0.0467236
State & Local Government Consumption (1972)	0.1567057	0.025119	0.1214099	0.2580106
State & Local Government Capital Outlays (1972)	0.0394628	0.016725	0.0221152	0.1426708
Tax Burden (1972)	0.0203485	0.0099421	0.0047538	0.0401973
Residency (number of days to vote in 1970)	110.7	86.98469	0	365
Campaign Finance Restrictions (index, 1970)	0.52	0.292417	0	1
Number of Years with Current Constitution (1970)	85.52	44.09602	1	190

Notes: Corruption convictions are reported by U.S. Department of Justice (various years). Real investment and capital stock data are from Garofalo and Yamarik (2002). Real GSP data are from Beemiller and Downey (1988) and Renshaw, Trott and Friedenberg (1998). Labor and population data are from the Bureau of Labor Statistics (www.bls.gov). Education data are from the 1970 Census of Population and Housing. Fiscal variables are from the 1972 Census of Governments. Residency requirements, campaign finance, and constitution adoption data are from Book of the States (1970).

Table 2: The Effect of Corruption on Growth

Dependent variable: annual average growth rate in real GSP per worker (1975-2000)

	(1)	(2)	(3)	(4)	(5)	(6)
Corruption convictions per 100,000 pop. (1975-2000 avg.)	-0.177***	-0.148**	-0.277**	-0.353**	-0.281***	-0.385***
	(0.065)	(0.067)	(0.111)	(0.151)	(0.106)	(0.143)
Wald 95% confidence interval	[-0.308, -0.046]	[-0.284, -0.012]	[-0.501, -0.054]	[-0.657, -0.048]	[-0.488, -0.074]	[-0.666, -0.104]
CLR 95% confidence interval					[-0.629, -0.043]	[-1.168, -0.088]
ln(capital per worker) (1970)	-0.010**	-0.010**	-0.009**	-0.011**	-0.0090**	-0.011**
	(0.004)	(0.005)	(0.004)	(0.005)	(0.0040)	(0.004)
Share of adult pop. with high school degree or less (1970)		0.036		0.036		0.047*
		(0.023)		(0.023)		(0.027)
ln(population) (1970)		0.0013**		0.0016*		0.0017**
		(0.0006)		(0.0008)		(0.0008)
Population growth (1970-2000)		0.047		-0.059		-0.076
		(0.128)		(0.156)		(0.143)
State & local gov. cons'n. share of personal income (1972)		-0.020		0.00024		0.0034
		(0.055)		(0.05310)		(0.0481)
State & local gov. cap. outlays share of personal income (1972)		-0.030		0.035		0.046
		(0.062)		(0.090)		(0.082)
State & local tax revenue share of personal income (1972)		0.018		-0.062		-0.075
		(0.087)		(0.103)		(0.095)
Estimator	OLS	OLS	2SLS	2SLS	LIML	LIML
R^2	0.52	0.61				
First-stage statistics						
Hansen's J-statistic			0.49	1.75	0.50	1.75
(p-value)			(0.78)	(0.42)	(0.78)	(0.42)
Partial R^2			0.29	0.25	0.29	0.25
F-statistic			5.61	3.92	5.61	3.92
(p-value)			(0.00)	(0.01)	(0.00)	(0.01)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Wald and CLR 95% confidence intervals for the coefficient on corruption are in brackets. First-state statistics refer to instrumental variables regressions that instrument for corruption with variables measuring states' residency requirements, campaign finance restrictions', and constitution durability. All regressions include a constant term and regional dummies. See the note to Table 1 for data sources.

Table 3: The Effect of Corruption on Investment
Dependent variable: investment/GSP (1975-2000 average)

	(1)	(2)	(3)	(4)	(5)	(6)
Corruption convictions per 100,000 pop. (1975-2000 avg.)	-0.343**	-0.247**	-0.538**	-0.551	-0.538**	-0.561*
	(0.144)	(0.121)	(0.280)	(0.337)	(0.280)	(0.299)
Wald 95% confidence interval	[-0.633, -0.053]	[-0.492, -0.002]	[-1.073, +0.056]	[-1.234, +0.133]	[-1.087, +0.011]	[-1.147, +0.025]
CLR 95% confidence interval					[-1.431, +0.125]	[-1.977, +0.214]
ln(capital per worker) (1970)	0.027**	0.035***	0.029***	0.034***	0.029***	0.034***
	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)	(0.009)
Share of adult pop. with high school degree or less (1970)		-0.133**		-0.119*		-0.119**
		(0.057)		(0.067)		(0.059)
ln(population) (1970)		-0.000		0.00042		0.00043
		(0.002)		(0.0020)		(0.0017)
Population growth (1970-2000)		0.213		0.056		0.050
		(0.266)		(0.350)		(0.307)
State & local gov. cons'n. share of personal income (1972)		-0.011		0.019		0.020
		(0.109)		(0.119)		(0.104)
State & local tax revenue share of personal income (1972)		-0.137		-0.039		-0.036
		(0.140)		(0.202)		(0.177)
State & local tax revenue share of personal income (1972)		-0.421**		-0.541**		-0.545**
		(0.199)		(0.232)		(0.204)
Estimator	OLS	OLS	2SLS	2SLS	LIML	LIML
R^2	0.31	0.52				
First-stage statistics						
Hansen's J-statistic			2.17	0.41	2.26	0.41
(p-value)			(0.34)	(0.82)	(0.32)	(0.82)
Partial R^2			0.29	0.25	0.29	0.25
F-statistic			5.61	3.92	5.61	3.92
(p-value)			(0.00)	(0.01)	(0.00)	(0.01)

Notes: See the notes to Table 1.

Figure 1: Corruption Convictions per capita and the Growth of Gross State Product

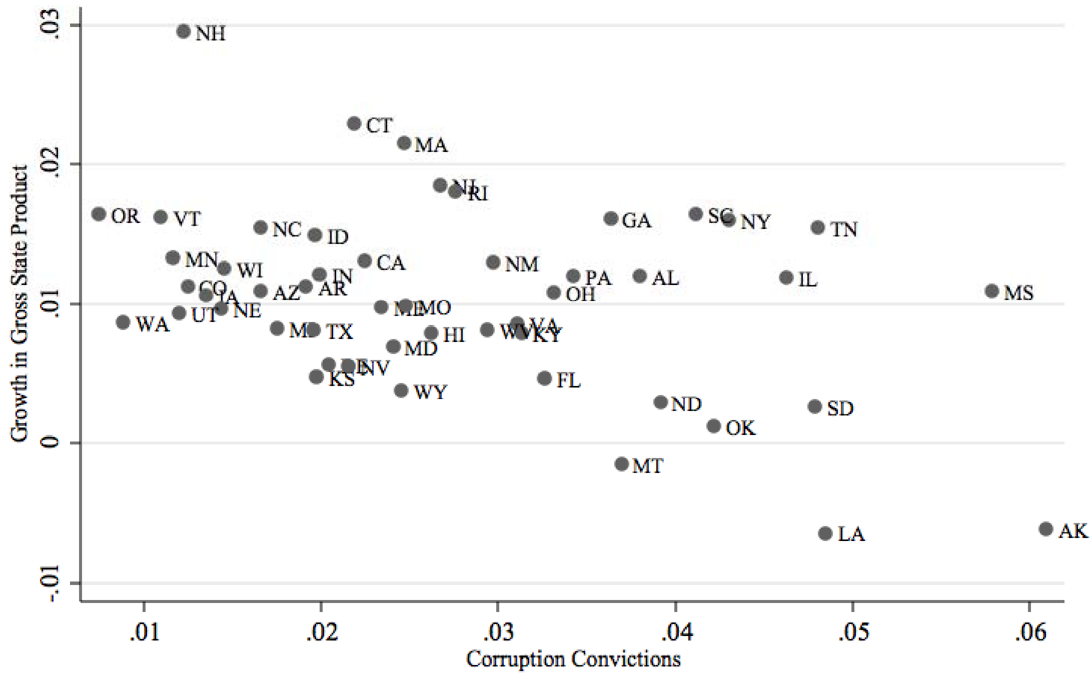

Figure 2a: Corruption Convictions per capita and Voting Residency Requirements in 1964

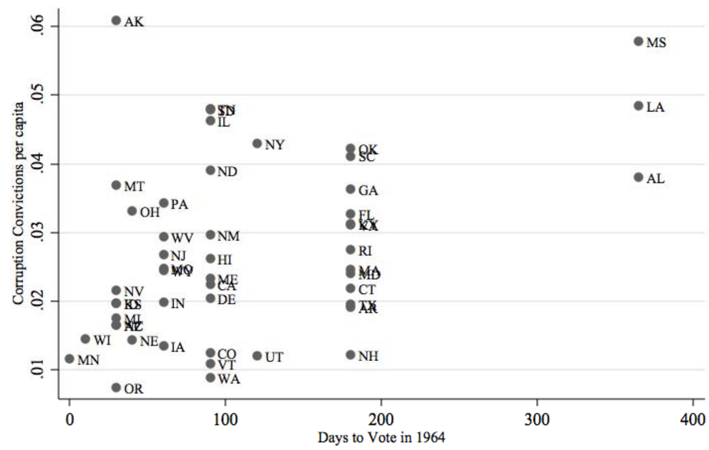

Figure 2b: Corruption Convictions per capita and Number of Campaign Finance Restrictions

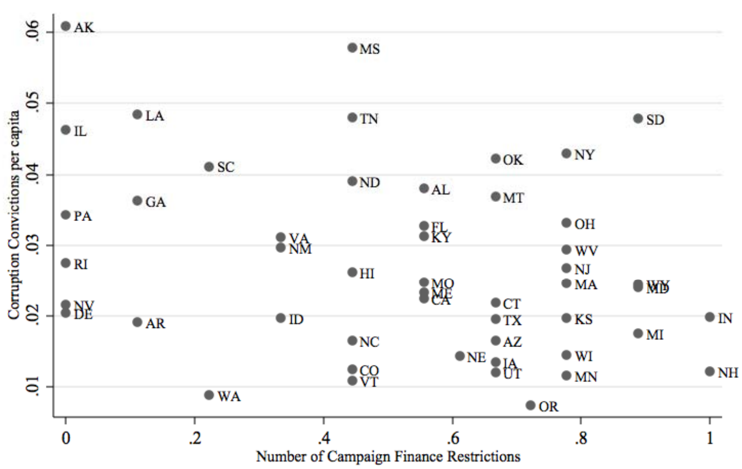

Figure 2c: Corruption Convictions per capita and Longevity of Constitution