Enforcement and Public Corruption

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Abstract

We use high-quality panel data on corruption convictions, new panels of assistant U.S. attorneys and relative public sector wages, and careful attention to the consequences of modeling endogeneity to estimate the impact of prosecutorial resources on criminal convictions of those who undertake corrupt acts. Consistent with "system capacity" arguments, we find that greater prosecutor resources result in more convictions for corruption, other things equal. We find more limited, recent evidence for the deterrent effect of increased prosecutions. We control for and confirm in a panel context the effects of many previously identified correlates and causes of corruption. By explicitly determining the allocation of prosecutorial resources endogenously from past corruption convictions and political considerations, we show that this specification leads to larger estimates of the effect of resources on convictions. The results are robust to various ways of measuring the number of convictions as well as to various estimators.

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Academics and practitioners alike lavish attention on corruption, commonly defined as the misuse of public office for private gain. The misuse of political and administrative power at the expense of citizens remains a problem in both developing and developed democracies. The abuse of office takes many forms, from receiving direct payments for political favors to election tampering to enacting legislation or otherwise channeling public money for private benefit to groups of friends, clients, supporters, or voters. Quintessentially, corruption is about taking payment for an illegal act, or somehow inappropriately enriching oneself from the public purse. Many studies investigate corruption's political, cultural, historical, and economic determinants. There is less empirical research on what actually to do to combat it.[1] In this paper, we investigate the effects of increasing enforcement effort, specifically by increasing prosecutorial resources, on corruption.

On the one hand, deterrence theory, originating in economics with Becker (1968) and in the context of corruption Becker and Stigler (1974), predicts that more prosecutorial resources, by increasing the risk of successful prosecution, should deter public officials and employees from engaging in corrupt activities. Following Becker and Stigler (1974), Besley and McLaren (1993), Van Rijckeghem and Weder (2001), and Ferraz and Finan (2008, 2009) among others model publicly employed agents' decision to take money from corrupt as opposed to legal activities, at the margin comparing the consequences of honest behavior with the size of the rents from illegal activities, allowing for the probability of being investigated, detected, charged, convicted, jailed and expelled from public service. These models of "economic" factors suggest that more enforcement can produce *fewer* corruption convictions, as long as a higher probability of enforcement discourages enough public officials from choosing corrupt activities.

[1] Authors from Klitgaard (1988) through Rose-Ackerman (1999, p. 52-3) to Lambsdorff (2007, Ch. 2) remark on the lack of empirical research on enforcement. van Aaken et al. (2010) consider the effect of prosecutorial independence, but not their resources.

On the other hand, theories of “system capacity” or “system overload” originating largely in criminology (e.g. Pontell et al. 1994) argue that white-collar crimes, including corruption, are insufficiently prosecuted. Some overload followed the expansion of the legal basis for federal prosecution of state and local corruption in the period after Watergate (Archambeault and Elmore 1983; Maass 1987).[2] While this part of system strain reflects a lack of funds allocated to enforcement, it also can result from incentives facing prosecutors. For example, Rasmusen et al. (2009) show that in a rewards system focused on conviction rates, prosecutors can find it optimal to forego prosecutions of complex cases with more uncertain outcomes and instead prosecute only cases that can with reasonable certainty result in a prosecutorial victory. Thus, according to system capacity theory, increasing prosecutorial capacity and resources can *increase* the number of prosecutions and convictions. In the context of corruption cases, Meier and Holbrook (1992) write that if “convictions were simply the result of slack prosecution resources … we would expect that convictions would be positively related to the number of federal prosecutors….” Whitford (2002) makes the same argument about prosecutorial staff.

These models provide core intuitions underlying our empirical analysis. Since both effects can be present at the same time, the net result of more resources, that is, which effect dominates in practice, can go either way, depending on factors we discuss below. As an empirical matter in the United States, though the literature is not long, Whitford (2002) finds evidence for a positive relationship between prosecutor resources and convictions while Meier and Holbrook (1992) finds no such association. Levitt (1997, 2002) finds evidence that more police resources result in less crime. The latter, though not specifically about corruption, makes

[2] Legal scholars argue that corruption became a federal priority in the mid-1970s. See also Baxter (1982, p. 322) and Ogren (1973), who complains about a lack of resources.

clear how important it is to take account in estimation of the clearly endogenous decision about how many resources to allocate to combating public corruption.

However, political corruption cases are not only about corruption, but there can also be "partisan influences on prosecutorial discretion" (Beale 2009). The decision to prosecute ultimately rests with the US attorneys, who are appointed by the President with the advice and support of home-state co-partisans. Not only do partisan factors affect appointment decisions, but as shown by Gordon (2009) there is a clear and systematic partisan effect on US attorneys' priorities regarding public corruption cases. In our empirical approach, we put this political bias to use as an instrument to correct for the endogenous choice of enforcement resources.

We use panel data on corruption convictions in U.S. states from 1977-2003 to examine how public resources available for the investigation and prosecution of offenders affect the calculus of corruption.[3] For explanatory variables, instruments, and robustness considerations we create two new state-by-state panels: the number of general attorneys in EOUSA offices (prosecutorial resources) and relative wages for public employees (a deterrence consideration). In selecting control variables, we build on many findings of cross-national research on corruption that have already been incorporated in research on American state governments (Adserà et al. 2003; Alt and Lassen 2003, 2008; Glaeser and Saks 2006; Maxwell and Winters 2005).[4] By cross-national standards US states are a sample with relatively high incomes and established democracy and rule of law. Three important advantages of studying them are: we have a reliable

[3] Recent comparative (Treisman 2007) and American (Glaeser and Saks 2006) studies find that corruption has no effect on economic growth, despite a large earlier literature to the contrary. While in the states corruption may affect borrowing costs (Depken and LaFountain 2006), we leave the consequences of corruption for another paper.
[4] Cross-national empirical research examines how not only income and other social factors but also political factors like judicial independence, federal regimes, or electoral institutions (district magnitude, ballot structure, open list voting) shape the incentives for politicians to engage in illegal rent-seeking and corrupt activities. See Ades and di Tella 1999; Montinola and Jackman, 2002; Seldadyo and de Haan 2005; Treisman 2000, 2007; Uslaner 2008 and more specifically on institutions La Porta et al. 2004; Gerring and Thacker 2004; Persson, Tabellini and Trebbi 2003; Kunicová and Rose-Ackerman 2005; and Brown et al. 2006.

panel of corruption data covering over a quarter of a century which does not rely on surveys or expert opinions; we have better data for relative public sector salaries than do the cross-country studies; and, perhaps most of all, we have a panel measuring enforcement resources across units, important once capacity, detection, and prosecution are considered.

We find that greater prosecutor resources do indeed result in more convictions for corruption overall, other things equal, consistent with "system capacity" arguments. However, we also show that the strain on an overburdened system has diminished in more recent years, in ways consistent with an emerging deterrence effect. The analysis also takes into account, and confirms in a panel context, a number of previously identified correlates and causes of corruption. Moreover, we explicitly allow for the allocation of prosecutorial resources to be determined endogenously, by past corruption convictions and political considerations, and show that this specification leads to larger estimates of the effect of resources on convictions. Overall changes in priorities and incentive systems at the federal level are captured by year fixed effects, while district, here aggregated to state, fixed effects capture, *inter alia*, the fact that some judicial districts are more attractive for prosecutors building a career. The results are robust to various ways of measuring the number of convictions, including moving averages and deflation by both population and the number of state and local government employees, as well as to various estimators addressing complications arising from the nature of the data. The next section presents our theoretical framework and empirical hypotheses. Subsequent sections describe data and estimation strategy and present results, considering alternative indicators and specifications.

I. Enforcement and corruption: a theoretical review

Public employees and elected officials engaging in corruption run the risk of being caught. In some countries and time periods, corruption is and has been socially acceptable and a way of life generally proceeding without interference from the law. However, in most developed countries including the US in the period we study, corruption has been recognized as a serious problem and has been a subject of police investigations and prosecution. This section does two things. It reviews how enforcement resources affect the costs of engaging in corruption, considering both deterrence and system strain, with the aim of specifying hypotheses to be evaluated empirically. It also examines what determines the allocation of enforcement resources, with the aim of correcting for problems of endogeneity in this allocation, which could, if not addressed, lead to biased estimates of the effects of enforcement on corruption prosecutions. We also briefly discuss two additional hypotheses coming out of our main framework.

I.1 Economic effects of enforcement resources on corruption behaviour.

In the economic "efficiency wage" framework of the traditional deterrence literature, the expected costs of engaging in corruption rise with the risk of being caught and prosecuted, everything else equal. The government employee who maximizes the present discounted value of a stream of expected income and contemplates a corrupt act can end up in three situations. First, if no corrupt act is committed, the employee simply continues to receive his wage. Second, if he engages in corruption but is not detected, he receives both the wage and the "bribe", the value of the corrupt act. Third, if he engages in corruption and is detected and successfully prosecuted he receives neither the wage nor the bribe, but incurs a penalty and is fired from public employment. In this case, future income is assumed to derive from employment in the private sector. Consequently, for a given institutional environment, the corruptible employee's or official's decision to engage in corruption is affected by (1) wages and expected tenure in the public sector, (2) the probability of detection, and (3) the cost of fines and jail terms, and wages in the private sector conditional on having been caught for corruption. Holding (1) and (3) constant, an increase in the probability of detection leads to a lower net benefit from corruption. From this perspective, we expect the number of corruption prosecutions to decrease as enforcement resources increase.

In Becker and Stigler (1974), the level of enforcement is the audit probability entering the maximization problem of the public employee, but is given from outside the model.[5] Generally, public enforcement of law (see Polinsky and Shavell 2001, 2007) involves the choice of costly enforcement under a budget constraint, so maximal enforcement is typically not socially optimal. This creates room to incorporate theories of "system capacity" or "system overload" originating in criminology.

System capacity (e.g. Pontell 1984) refers to the ability of formal agencies to sanction crime. According to Pontell, Calavita, and Tillman (1994, p. 393), "exceedingly high workload demands in criminal justice agencies [imply that] institutional limits to crime control are present for all offences. Yet perhaps they manifest themselves most clearly in the case of white-collar and corporate crime" of which corruption is one example. Corruption cases, like other white-collar crimes, are high effort cases (Richman, 2009), and work on local enforcement of white-collar crime (e.g. Benson et al. 1990) reports that local prosecutors see the level of resources available to them as the primary obstacle for prosecuting white collar crime. Under this view, increasing the level of resources, primarily prosecutorial resources, for a given in-flow of referrals would tend to increase prosecutions. Over enough time, however, vastly increased resources to combat corruption would, we suspect, eventually be internalized by potential perpetrators, leading to a lower supply of corruption witnessed by a lower number of referrals and ultimately convictions.

[5] Experimental results (Ferraz and Finan 2008) show that release of unfavorable audit outcomes does reduce incumbents' subsequent electoral success.

Thus, increasing resources spent on enforcement can have countervailing effects. In the short run, increasing enforcement resources can increase the number of prosecutions, in particular under system strain where prosecution of cases is abstained from for budgetary reasons. If the increase in enforcement resources is accompanied by increased relative public wages, there could be an offsetting effect. Increased resources can also decrease prosecutions if perpetrator adjustment to higher probabilities of enforcement is instantaneous (or such increases were pre-announced) or if prosecutors have political ambitions (Rasmusen et al. 2009). If deterrence effects are present, that is if the supply of corruption is not entirely inelastic with respect to the probability of being prosecuted, the longer run effect can be ambiguous, while if there are no deterrence effects, long-run effects would continue to be positive.[6]

Returning to the effects of wages and penalties in the deterrence approach, to our knowledge, no one has ever systematically investigated the loss of income suffered by those convicted of corruption offenses. We cannot do this either. Anecdotal investigation of individual cases where information could be obtained from publicly available sources suggests that there is usually some loss, frequently severe, consistent with the literature on post-incarceration incomes of white-collar criminals (Waldfogel 1994; Western et al. 2001). Setting a public sector wage above the market-clearing wage also decreases the propensity of the public employee to engage in corrupt behavior, a now standard efficiency wage result. di Tella and Schargrodsky (2003) show, in a detailed analysis of corruption at hospitals in Buenos Aires, that higher wages decreased corruption, at least conditional on less than maximal monitoring, as suggested by the

[6] Moreover, "when the effects of the crime rate on the expected sanction are taken into account, the optimal investment in law enforcement … may be greater or smaller than predicted by traditional analysis" (Bar-Gill and Harel 2001, p. 499). For example, higher crime rates can change the incentives for citizens to inform police about criminal activities; investigations vary in the extent to which information gained in one aids in another.

Becker-Stigler model. Goel and Rich (1989) find the same effect in a cross-section of US states.[7] Using higher government wages as an incentive can thus lower corruption, since the official contemplating a corrupt act has more to lose.

Finally, the shadow of the future is also important within this framework: expected tenure in office or government employment affects the expected present value of alternative income streams. The Becker-Stigler public official is always corrupt in the final period of employment unless the promise of pensions is sufficient to keep him honest. Shleifer and Vishny (1993) argue that in unstable systems the ephemeral nature of public positions makes officials irresponsible and grasping: for instance, an elected politician with little or chance of re-election has less incentive to do things voters want, like controlling corruption. Again, however, the effect of the future is actually ambiguous, as we cannot say in principle whether supply or demand effects dominate. Consider a politician with little chance of continuing: while she (or her friends) has more incentive to grab what she can while she can, someone who is not going to be in office for long is correspondingly less worth "bribing".

I.2 The allocation of enforcement resources

In US states, corruption cases usually begin with criminal investigations, which may or may not end in referrals to the U.S. Attorney's office. For most of the last two decades 80 per cent of such referrals came from the FBI.[8] Cases are not petty: most frequent lead charges on indictments pursued by US Attorneys were based on robbery or extortion affecting interstate commerce, theft

[7] There is other empirical support: Van Rijckeghem and Weder (2001) find, in a cross-country analysis, that countries with higher average government wages relative to average wages in the manufacturing sector have less corruption as measured by expert surveys, though Treisman (2000) does not find this. Paldam (2002) notes that poorer publicly-employed agents have greater "…temptation to make illicit gains." Ferraz and Finan (2009) find that higher pay improves legislator performance and public-good provision in Brazilian municipalities, though they do not investigate corruption directly.

[8] While only one case in three is actually prosecuted, the time from a referral to a decision to decline a prosecution is a year and a half, which could itself be a serious sanction.

and bribery in entities receiving more than $10,000 in federal funds, the mail fraud statute, conspiracies to defraud the federal government, and the RICO statute (Gordon 2009).[9] At this stage decisions are made about whether to pursue or decline a referred case, which also depend on the allocation or resources between corruption cases and other crimes.

How do enforcement agents, including police and prosecutors, allocate resources between different types of crimes? At the level of individual US Attorney offices, research on prosecutors argues that they choose which cases to prosecute and which to decline so as to maximize their conviction rate (e.g. Rasmusen et al., 2009). For efficiency reasons, we would expect decision-makers concerned with the number of convictions to allocate scarce prosecutorial resources in a way such as to equalize the marginal propensity to convict across judicial districts, and for resources within districts to be allocated in the same way. We assume therefore that if enforcers, here the EOUSA, want to maximize the number of cases that are successfully prosecuted, they "hunt where the ducks are". More specifically, to maximize successful prosecutions, following Knowles, Persico, and Todd's (2001) analysis of racial profiling in motor vehicle searches, enforcers prioritize resources to equalize the probability of detection of corrupt cases across districts and subpopulations. They choose "hit rates" in proportion to the expected tendency to commit crimes. If hit rates are chosen this way, the distribution of enforcement resources is endogenous, with more federal investigative and prosecutorial personnel and resources allocated to districts with past patterns of more corruption and other crimes, or perhaps other factors that affect, or are correlated with, the likelihood of such crimes.

However, U.S. Attorneys are presidential appointees, and so the allocation of resources could reflect partisan factors (Schlesinger and Meier 2002; Whitford 2002; Beale 2009; Gordon

[9] The source is the Transactional Records Access Clearinghouse "TRACFED" at Syracuse University: http://tracfed.syr.edu/. Data used under university license.

2009; Gordon and Huber 2009). Gordon discusses partisan prosecutions during the Clinton and Bush administrations, while Beale describes the US Attorney firings in 2007. Richard Posner found partisan bias an obvious possibility, blogging in August 2005:

> Another factor is that most big cities have Democratic mayors …. Republican attorneys-general are more likely to investigate and prosecute public corruption in Democratic-controlled cities than Democratic attorneys-general are.

Richman ascribes this to the rise of federal criminal enforcement, which

> as a distinct and valuable component of local ecologies, particularly in urban areas, owes a lot to a disjunction between those with national political power and those who hold sway locally. The risk that partisan prosecutors with allegiance to the president's party will target local political opponents – either at the behest of the White House or on their own – is real. (Richman 2009, p. 2122)

If the appointment process is politicized in this way, fewer resources would be allocated to districts in which co-partisans of the President were in power, in order to skew the distribution of prosecutions and convictions away from one's allies and toward one's opponents.

For now, we leave open the motivation of (assistant) US attorneys. We note that models of prosecutorial decision-making (e.g. Rasmusen et al. 2009) need not imply that increasing resources increases the number of prosecutions. For instance, if attorneys are politically motivated, the number of prosecutions can fall when the level of resources increases, if the attorney chooses to focus instead on conviction rates.[10] In practice, some US Attorney districts give greater possibilities for establishing and maintaining a high profile than others. Thus, prosecutor motivations and objective functions may differ across districts. As such differences

[10] Gordon (2009) argues that prosecutors pursue "high profile" cases. Boylan (2005) argues that "ambitious" prosecutors seek longer sentences rather than more convictions. This apparently increases the chance of becoming a judge or a partner in a good law firm after leaving office.

are (mostly) unobservable, cross-district estimates implicitly assuming similar motivations may be biased; in our empirical analysis below, however, the panel structure of the data allows us to control for (state) fixed effects, subsuming stable differences in motivations across districts.

II. Data and Specification

II.1 Dependent variable: Corruption convictions

The cross-national literature on corruption uses surveys of experts and firms to construct corruption measures. A number of studies of corruption in the US have used data on corruption convictions ("criminal abuses of public trust by government officials") reported annually by the Public Integrity Section of the US Department of Justice (Meier and Holbrook 1992; Glaeser and Saks 2006; Maxwell and Winters 2004, 2005) since the 1978 Ethics in Government Act. The Section, created in 1976, prosecutes some cases, but most are handled by U.S. Attorneys. While the Act spoke of corrupt elected officials, its data clearly includes both elected and appointed officials, as well as at least some of those who sought to corrupt them. Early on, local cases were the most common but more recently the proportion of federal officials prosecuted has increased. The annual data, aggregated to states from U.S. judicial districts, includes some 21,000 cases between 1977 and 2003.

The coverage of these reports far exceeds any available alternative. TRACFED Convictions data reported by the Executive Office of the U.S. Attorneys begins a decade later, in 1986. When both datasets are available, the positive correlation between them is evident. The National Incident-Based Reporting System annual report of white-collar crimes begins a decade later still.[11] It does not identify crimes involving public officials. Where such identification is possible (e.g., bribery) the number of incidents is tiny: Barnett (No date: 3) reports 191 bribery

[11] Earlier print editions of *Crime in the United States* do not contain the state-by-state data.

offenses 1997-99, in which years the Public Integrity Section reports 2932 corruption convictions. Finally, no repeated survey or expert data is available annually, but note that when expert ratings (of corruption and quality of governance) and data on convictions are both available, Peters and Welch (1978) and Alt and Lassen (2008) show that convictions and expert estimates are positively correlated.[12]

There are issues with the data we use, including jurisdictional issues about federal prosecution of state and local corruption. Maass (1987) and Gordon (2009) review developments and sources. As they make clear, federal prosecutions have more or less eliminated corruption prosecutions arising at other levels of government. Data might also be "lumpy" if a large sting or investigation results in several convictions in a state in one year followed by a period of apparent quiet. We ensure that this does not affect the results we report. Politicized prosecutions are a possibility, and we deal with it directly. If corruption in the judicial system manifests itself in lower numbers of corruption convictions, convictions data will be ambiguous. We assume this is not a widespread problem in the US in the period we study. Overall, we believe these convictions data to be of satisfactory quality and comparable across states.

Following Glaeser and Saks (2006) we normalize convictions by state population. Other normalizations are possible[13] and we show below that results using different bases do not differ qualitatively. The average values by state over all years appear in Figure 1(a), ranging from lows in Oregon and Washington to highs in Louisiana and Mississippi. Figure 1(b) first shows (line;

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[12] Expert data are not without their own problems. Treisman (2007) reports that for "reported experience" of corruption nothing else matters once income is controlled, but "reputed" corruption responds inversely to a country's income, a free press, women in the labor force, and the extent of trade. The risk, of course, is that experts rating a country are inferring honesty from observables like a lot of trade, while people conducting that trade report a different experience.

[13] Maxwell and Winters (who generously made their data available) calculate the number of convictions relative to the number of elected officials, as a proxy for the number of all government officials, interpolating and extrapolating number of elected officials from a limited number of observations, as do Meier and Holbrook 1992. However, this variable has not been updated since 1992 so we eschew that option here.

measured on the left-axis) the sum across all states of the number of convictions relative to population in each state, year by year. Second (bars; measured on the right-axis), it shows the range across states of convictions measured this way. For example, in 1989 the sum across states was 198 convictions per million inhabitants (corresponding to 1108 convictions), and the convictions rate in 1989 ranges from .4 in Washington state to 9.3 in Mississippi. As the distribution is highly skewed, our dependent variable in most subsequent analyses is the logarithm of convictions for corruption relative to population measured in millions.[14]

[Figure 1 about here]

Our sample covers the years 1977-2003 for the 48 contiguous states. Details, sources and a Table of summary statistics for data appear in the Appendix.

II.2 Core explanatory variables from the model

Endogenous enforcement and prosecutor resources

The quantity of interest is the effect of (endogenous) prosecutor resources on the choice of corruption. In the efficiency wage framework this works through its effect on the risk of being caught. Since enforcement begins mostly with FBI investigations and case referrals, an ideal measure would be the geographical distribution of criminal investigators employed by the FBI. However, for (probably obvious) reasons, the FBI apparently does not release that information.[15] An alternative that has considerable variation across time and space arises one step further along the investigative process: the staff of the U.S. Attorney's office, which Whitford (2002) shows to

[14] Since we cannot take logs of zero, we add a shift parameter to make sure that state-years with zero convictions are not excluded from the analysis. Below, we confirm the robustness of the results using a tobit estimator with the non-logged number of convictions relative to population.

[15] Other agencies of the Department of Justice (DEA, INS) do release investigator data. However, those agencies are not involved much in corruption investigations, nor does the distribution of their staff generate significant empirical results for corruption. Other measures, like auditor powers and selection in the states, lack significant time variation in our period. See Schelker (2006). Some studies (Cordis 2009; Goel and Nelson 2009) less appropriately employ an intermittently-available series for "federal law enforcers", most of whom have no connection with corruption cases or the Department of Justice.

be important in the productivity of district offices. Since the dependent variable, convictions, is a function of prosecutions, we measure resources as federal full-time-equivalent (FTE) positions in the U.S. Attorney's office for "general attorneys" for each year and judicial district, and then aggregate to the state level, again normalizing by state population.[16] The data, with ranges across states, appears in Figure 2.

[Figure 2 about here]

Government average and relative wage

How much, if at all, do we estimate that higher public wages reduce corruption? For every year the Bureau of Economic Analysis (BEA) compiles data on wages and employment across dozens of industrial classifications for every state in "Wage and Salary Disbursements by Industry". For state and local government employees, we calculate the average wage and salary disbursements by state in current dollars reflecting "Full time and part time wage and salary employment". In the data below we omit part-timers. We use this data to construct three useful variables. First is the average wage for state and local government, in current dollars.[17] As the measure of the current income advantage to being in the public sector, we calculate the ratio of this average state and local government wage to the average non-government wage in the state.[18] We also calculate the average wage for state and local government, in constant dollars, adjusted for the BLS regional CPI. Across years, the ratio never exceeds unity on average, though the range for individual state-years is from 0.78 to 1.17.

[16] Data are available under academic license from tracfed.syr.edu .We measure code 0905, full-time federal general attorneys. In years before 1986, the US Attorneys' offices were combined with Department of Justice "Offices and Boards". Printed reports of the Executive Office of the US Attorney list "Assistant US Attorneys" by district and clarify that these data are effectively equivalent to the number of attorneys employed by Offices and Boards. (The print data series was discontinued after 1985.) We are confident that the change in description does not introduce important errors into the data series.

[17] These are a single code up to 1979 in the BEA data, and two separate codes thereafter.

[18] We calculated the correlations between average legislator compensation and these other relative wage averages. They are all positive. Salary data supplied by Thad Kousser.

Shadow of the future

The arguments about considering the agent's expected tenure or probability of retention at any time (which could apply to both elected and appointed officials), suggest a way to estimate the effect of the "shadow of the future". This is to assume that the Governor is responsible for health of the state. Higher corruption convictions, a bad outcome, indicate that Governor is, at the margin, of lower ability or putting less effort into the job. We are not assuming the Governor is convicted nor controlling for Governor's expected ability.

We lack systematic data on the probability of re-election of a gubernatorial administration at any time. Instead we look at the effect of having a Governor who is a lame duck (that is, in the last term of a constitutionally-allowed incumbency) as an indicator of officials or an administration with a short horizon. Whether the short tenure induces less monitoring effort by the Governor or whether it induces associates of the Governor (some of whom may leave public office at the same time) to value corrupt acts more, we expect more corruption under term-limited incumbents.[19] Moreover, if incumbents subject to a one-term limit have worse performance than incumbents under a two-term limit (Alt, Bueno de Mesquita, and Rose 2011) we expect this effect to be larger in the one-term limit case. Further details appear in the Appendix.

II.3 Other social, political, institutional factors

Our specification also includes as controls variables found to influence corruption in other studies of US states for which annual data is available. These include average constant-dollar income per capita in the state (Meier and Holbrook 1992; Goel and Nelson 1998; Adserà et al. 2003; Boylan and Long 2003; Alt and Lassen 2003, 2008; Glaeser and Saks 2006), the

[19] Just as ignoring the fact that those who are there longer could also be more worth bribing, using the Governor's time horizon to stand for "all agents" is a necessary modeling simplification.

population share with high school education or higher (same sources plus Maxwell and Winters 2004), inequality (Uslaner, 2008), the scale of government, per capita constant-dollar state government revenues or expenditures (Goel and Nelson 1998; Alt and Lassen 2003, 2008), state population (Maxwell and Winters 2004), divided government, where legislature and executive are controlled by different parties (Alt and Lassen 2008), degree of urbanization (Alt and Lassen 2003), decentralization (Goel and Nelson 2010), and citizen ideology. Corruption is expected to be higher where government scale is larger (more temptation), where population is bigger, and where urbanization is higher, and lower where incomes and average education are higher and where there is divided government. Details and sources of data are provided in the Appendix.[20]

Cross-national institutional studies of corruption suggest other underlying causes including political-civil liberty, decentralization of power, checks and balances, presidential government, participation, political competition, instability, electoral rules, and constraints on the chief executive.[21] Many of these factors are constant or nearly so across the period we consider within states as well as across states in some cases. Effects of unchanging factors, including “cultures” of corruption (Peters and Welch, 1978; Johnston, 1983), are subsumed by state fixed effects. Changes that affect all states equally (aspects of federal enforcement like sentencing guidelines, for instance) are picked up in the year fixed effects.

II.4 Specification and identification

[20] Since corruption is a white-collar crime, the white-collar crime rate (fraud, embezzlement, forgery, and bribery) could also be useful as an instrument for enforcement, but despite extensive searches and requests of published data we have been unable obtain this by district or state and year earlier than 1996.

[21] Seldadyo and de Haan (2005) find the closest stable and robust correlates of corruption to be a dozen clustered variables (rule of law, judicial independence and impartial courts, government effectiveness, GDP per capita, political stability, regulatory quality, bureaucratic quality, law and order, labor market regulation, international trade, internal conflict, and secondary school enrollment) reflecting “the capacity of government to regulate and enforce law.” Controlling for this factor in a version of extreme bounds analysis reveals (after 713,460 regressions) that population density (negative for corruption), Scandinavian legal origin (negative), and ethnic conflict (positive) are other robust predictors. Less stably correlated variables include the illiteracy rate (negative) or primary school enrollment (positive), the government wage (positive), dependence on fuel exports (positive), presidential government (negative), and female labor force participation (negative).

The panel structure of our data allows us to investigate the effect of prosecutorial resources on corruption convictions taking into account both invariant differences across states and common changes across time. The basic model that we estimate is

$$(1)\ \ corruption_{i,t} = \beta^{e} EOUSA_{i,t} + X'_{i,t}\beta + \alpha_i + \tau_t + \varepsilon_{i,t}$$
$$(2)\ \ EOUSA_{i,t} = Z'_{i,t}\gamma + \nu_{i,t},\ \ Z_t = \left[corruption_{i,t-2}, partisan_{i,t-2} \right]$$

First, we focus on the main estimating equation (1).[22] Here, corruption in state *i* at time *t* is explained by prosecutorial resources, denoted EOUSA, measured by the EOUSA's FTE attorneys per million population. The matrix *X* contains the other variables reviewed above and α and τ are the state and year fixed-effects, respectively.

Equation (2) allows for the possibility, introduced above, that prosecutorial resources are chosen endogenously. To estimate the effect of prosecutorial resources in a consistent way, we introduce a set of instrumental variables, *Z*, which includes past corruption convictions as of two periods ago (since more convictions in a district could affect future staffing levels) and partisan factors.[23] To estimate how far partisan Presidents target prosecutorial resources toward opponents and away from supporters (Schlesinger and Meier 2002), we include a variable to measure the congruence between the President currently appointing US attorneys and the ideology of the state population. This variable, Republican congruence, is equal to the share of self-declared conservative voters when the appointing President is a Republican and zero otherwise. In addition, we include a variable allowing for enforcement resources to be influenced by the degree of urbanization, which also influences corruption directly, interacted with the share of Democrats in the state senate, again to get at political motivations for the allocation of

[22] For an explicit derivation of an empirical model very similar to our equation 1, though lacking any reference to endogeneity, see Cordis (2009).
[23] Whitford (2002) shows that prosecutor staffing is affected by national political trends, including ideology and partisanship.

resources. As identifying the causes of prosecutorial resource allocation is interesting in its own right, we examine this separately. However, for our instrumental variables strategy to work, we do not need to include all potential determinants in *Z*, and we select the configuration of partisan factors to maximize our potential for credible inference.

The viability of the instrumental variables approach depends, in addition to the requirement that the instrumental variables *Z* affect the potentially endogenous variable, on an exclusion restriction, which here is the assumption that partisan factors and past corruption convictions affect contemporaneous corruption convictions only though their effect on enforcement resources and priorities. In fact, our panel data instrumental variables model with fixed effects requires an assumption of strongly exogenous instruments, i.e. that $E\left(\varepsilon_{i,t} \mid \alpha_i, z_{i,1}, ..., z_{i,T}\right) = 0, \forall i$, for the estimation to be consistent (e.g. Cameron and Trivedi, ch. 22). This is a strong assumption, as it implies that the contemporaneous error is uncorrelated with all, both past and future, values of the instruments. In particular, since we include a lagged dependent variable as instrument, this implies that we assume that the number of corruption prosecutions at the state level at time $t-2$ affects current prosecutions only through its effect on prosecutorial resources. We do, however, note below that our results are unaltered by excluding this particular instrumental variable from the analysis and examine, if indirectly, the appropriateness of the exclusion restriction through tests for overidentification. Regarding the effect of partisan forces on corruption decisions, the exclusion restriction is that partisan factors affect the number of corruption convictions only through enforcement resources, controlling for state fixed effects and common year effects.

Finally, we estimate the model with panel-robust (clustered) standard errors, which both corrects for heteroskedasticity and potential serial correlation in the errors and results in consistent estimates of the variance matrix, in contrast to the standard heteroskedasticity-robust correction for panel data models (Stock and Watson, 2008). Instrumental variables regression in a panel context with time and unit fixed effects is demanding on the data, as both state and time fixed effects enter also in the estimation of EOUSA, the first stage, together with all other explanatory variables *X*. While our instrumental variables have good explanatory power on their own, they are not sufficiently strong in the first-stage regression for a standard two-stage least squares estimation to be unbiased. As noted by the literature on weak instruments (e.g. Staiger and Stock, 1997), this can cause considerable bias in the TSLS-estimates. For this reason, we employ two alternative IV estimators, the limited information maximum likelihood (LIML) and the GMM Continually Updating Estimator (CUE), both of which can provide unbiased estimators even when instruments are too weak for TSLS to be trusted (e.g. Stock, Wright and Yogo, 2002). Again, however, the relative performance depends on the presence of heteroskedasticity as well as clustering and serial correlation in the standard errors. While research on this issue is on-going, current best practice seems to be to estimate the model by CUE in the presence of weak and/or many instruments as well as clustering.

III. Estimation Results

Table 1 reports, as a benchmark, results from a standard fixed effects estimation where attorneys per million population in the state ("EOUSA" above; henceforth "Attorneys") is treated as being exogenously determined. The first column shows results from a model with only state fixed effects in addition to the Attorneys variable, the second column includes year effects and the third column includes the full set of controls. The estimate for the effect of Attorneys is positive and similar across specifications and consistently significant at the 5 or 1 per cent levels.

[Table 1 about here]

One feature of corruption cases is that convictions can come in clumps since many investigations involve multiple targets. This means that an annual count will appear noisy, even though underlying developments are actually reasonably smooth. A consequence of this is that our dependent variable will have considerable measurement error, possibly affecting estimates. For this reason, we show in columns 4-6 results for the first three specifications for the case of a three-year unweighted moving-average measure of the dependent variable. Here, estimates are smaller, consistent with the idea that the moving average transformation has eliminated some measurement error. The number of observations is also slightly higher here, as the smoothing of the time series eliminates a number of missing observations.

We find little impact of relative wages in these estimates. However, some alternate estimators discussed below do yield estimates consistent with the underlying deterrence theory. Binding gubernatorial term limits also, weakly, tend to be associated with more corruption convictions, as predicted. Overall, these estimates also offer very limited support for deterrence effects.

Estimated effects of other control variables generally reflect findings elsewhere in the literature. Divided government is consistently and in a significant way associated with lower corruption, as in Alt and Lassen (2008). States with higher levels of per capita government revenues see significantly more corruption convictions, as in Goel and Nelson (1998), while states with a larger share of the population living in urban areas see more corruption, as in some specifications in Glaeser and Saks (2006). Higher per capita income levels are weakly associated with lower corruption, as are larger shares of the population having a high school degree, consistent with the cross-sectional evidence of Meier and Holbrook (1992) and Glaeser and Saks (2006). The results for inequality are variable, usually negative and not significant in the first stage but positive, and sometimes significant, in the second stage. While not shown, state and year fixed effects are significant, the former subsuming all institutional state characteristics that did not change in the period we consider.

Table 2 reports results from our main instrumental variables specifications for our two measures of the dependent variable. The first column shows the first-stage regression, which is the same for the LIML-estimator and the CUE shown in columns 2 and 3, respectively. The instruments all contribute to explaining the allocation of enforcement resources in a significant way, both individually and jointly: past corruption convictions increase enforcement resources, as does the combination of a strong Democratic presence in the state senate and more urbanized populations, while states with more voters identifying themselves as “conservatives” are allocated less prosecutorial resources under Republican presidential administrations. An F-test for joint significance of the instruments returns a value of 7.31, which has a p-value of .0004. The F-test suggests that the jointly significant instruments are well beyond the critical values necessary for unbiased estimation using LIML and CUE, though (as discussed above) not sufficiently strong for us to rely on standard two-state least squares estimation.[24]

[Table 2 about here]

Results for the control variables are largely as above. Some differences exist, both relative to the OLS results and between the two dependent variables. Overall, higher revenue levels, urbanization rates, and a more right-leaning citizenry are all associated with more convictions. With less consistency across specifications, inequality is associated with more convictions and in contrast, divided government is associated with fewer convictions. .

[24] Our F-test statistics are 7.31 and 7.23, respectively, above the most restrictive critical value of 6.46 as tabulated by Stock and Yogo (2005) and reported in the output of the Stata-routine xtivreg2.

However, correcting for the endogenous allocation of enforcement resources results in a considerably larger estimate of the effect of enforcement resources on corruption prosecutions than that produced by the OLS-specification reported in Table 1. For both estimation methods, the estimated coefficient is two to three times the least squares panel estimate. The LIML estimates on enforcement resources are slightly larger than those based on CUE. Of course, in contrast to the OLS estimate, the IV-estimate takes into account the endogenous allocation of resources aimed at achieving an optimal allocation of resources, measuring the effect of a marginal FTE attorney assuming that the EOUSA wishes to optimize resources, allowing for political considerations.

The magnitude of this estimated effect can be approximated as follows. An estimated coefficient of .642 suggests that hiring one more FTE assistant US attorney (per inhabitant, measured in millions) on average will increase the (logged) number of corruption convictions (per inhabitant, measured in millions) by .642, or increase the *number* by 1.9 (+/- .9) convictions. This should be compared to the corresponding OLS estimate of .8 +/- .3 (Table 1, column 6), which reflects the average, across the sample, relationship between the number of FTE assistant district attorneys and the number of convictions for corruption.

III.1. Robustness: Instruments, Measurement and Estimation

The results for prosecutor resources presented in Table 2 are robust to exclusion of individual instruments or subsets of instruments in a qualitative sense. In every model we look at, Attorneys has a positive effect on the number of corruption convictions, though with some variations in magnitude. The estimated coefficients are always significant at least at the 10 percent level. Furthermore, diagnostics generally support the IV-identification strategy.

We undertook a variety of robustness tests, including alternative estimators focusing on particular characteristics of the data as well as alternative measurements in addition to the moving averages already utilized in Tables 1 and 2. In the analysis above, a correction was needed to the log transformation of the number of convictions relative to population (as it is not possible to take the natural log of zero) in order to include observations with zero in the analysis. We investigated various transformations and corrections, and all yielded similar results. Table 3 presents some results. To avoid transformations, the first two columns report results from an instrumental-variables tobit analysis, using as dependent variables the number of convictions relative to population and a moving average version of this, in both cases explicitly allowing for zero convictions as a special outcome.

[Table 3 about here]

Using the IV-tobit comes at a price, however, as it is not possible to include the panel structure (and, thus, state fixed effects) in the analysis. The results are, however, broadly similar to what we observed above, the main difference being stronger results on a number of control variables. While divided government continues to be associated with lower corruption in a significant way, we now also observe strongly significant relationships for the share of the population with high school, the relative wage, and unemployment, all associated with lower corruption, and state population size and a more left-leaning citizenry, both associated with higher corruption, where the latter result is the reverse of the results so far. The strong result on the relative wage is particularly noteworthy, as it is consistent with cross-country and cross-state evidence cited above. It is not, nor are results on the other control variables, an artifact of the lack of state fixed effects, but rather attributable to the tobit-specification.[25]

[25] Our other specifications, like those in Tables 1 and 2 but with fixed effects omitted, give similar results.

In this specification the coefficient of Attorneys measures the marginal effect of increasing enforcement on convictions under a latent variable interpretation. Since the dependent variable is no longer log-transformed, the coefficient of 1.2 can be compared directly to the effects of increasing resources reported above. These were equal to .8 and 1.9 for the OLS- and IV-specifications, respectively. In sum, the IV-tobit analysis suggests results between the others, closer to the OLS-results and considerably smaller than the IV-panel results.

Columns 3 and 4 report a different standardization, showing results when the dependent variable is defined as the log of the number of corruption convictions relative to the number of FTE state and local government employees in millions, as above for both the annual and the moving average definitions.[26] The qualitative results are the same, which is reassuring but not too surprising as the number of government employees is highly correlated with state population. The results here are slightly larger than those reported above, owing to the smaller denominator; for example, the estimate for the MA-measure is .8, compared to .65 above, which is equivalent to a marginal effect of 2.2 +/- 1.0.

III.2. The Alleviation of System Strain and Deterrence

According to deterrence theory, increased resources for enforcement should, by increasing overall expected penalties for corruption, deter public officials and public sector employees contemplating corrupt acts. In our case, that would appear as a negative estimate for the effect of enforcement resources. Do the consistently positive signs on enforcement resources imply that there is no deterrence effect? In this section, we investigate this question in an exploratory way.

Since the effects of system strain and deterrence go in opposite directions, the (strong) alleviation of system strain or overload could mask contemporaneous lagged deterrence effects.

[26] Since the convictions number reported includes also federal employees in a state, we implicitly assume that the number of federal employees in a state is proportional to the number of state and local government employees.

There is no question that the number of attorneys steadily increased (refer back to Figure 2) as the EOUSA built up the framework for the legal attack on corruption (Maass 1987 and Archambeault and Elmore 1983). We begin by examining whether the workload of EOUSA attorneys changed. One way to examine that is to chart the ratio of cases pursued (or filed) to total number of cases (pursued and declined) referred from law enforcement agencies. Figure 3 reports raw and moving-average--smoothed numbers from TRACFED beginning in 1986.[27]

[Figure 3 about here]

Initially, the ratio of cases filed to total number of cases was approximately one-third, with some yearly fluctuations, shown by the solid blue line in the figure. The stability of the early period is more clear in the smoothed version of the same data (the dashed, red line). Based on this, evidence points toward a structural change in 1994 or 1995, when the share of filings begins trending upwards. This shift in the ratio reflects a drop in the number of declinations, as the average number of filings (the dashed-dotted green line) is reasonably constant in the period we consider. At the same time, enforcement resources grew steadily in the period, though at a slower pace post 1990 (Figure 2 again). Together, these observations suggest that system strain eased somewhat in the period, as relatively more personnel were available to handle relatively fewer referrals. With system strain easing, the probability of convicting a corrupt offender should increase, opening up a potentially larger role for deterrence.[28]

In our empirical framework, the alleviation of system strain should represent itself as a smaller coefficient on enforcement resources from the early 1990s onwards. A simple way to

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[27] Of course, referrals could have involved better or worse cases: we assume a constant level of quality in terms of referrals over time.

[28] Indeed, the proportion of cases pursued that result in convictions has risen in recent years. However, that need not be because system strain as we have defined it has eased: for example, the quality of referrals could have improved. Evidence from the most recent years is also not yet comparable to historical data due the large share of cases still pending (OIGs Audit Report 09-03 on Resource Management of United States' Attorneys Offices).

model this is by incorporating a shift parameter for the effect of enforcement resources post-1994 in the regression. This is shown in column 1 in Table 4; the specification is as in Table 1 above, though it allows for the fixed effects as well as the coefficient on enforcement resources to be different before and after 1994. The interaction term is negative, and close to significant, consistent with the interpretation that system strain was easing post-1994. As above, year fixed effects control for changes in priorities affecting all states in the same way. If, however, some states but not other experienced changes in priorities, this is modeled explicitly here by allowing the fixed effects to differ across time. Thus, the results presented here remain robust to such state-specific changes in priorities.

[Table 4 about here]

As long as the “system strain” and “deterrence” effects of enforcement go in opposite directions, the observed decline in the effect of enforcement resources post 1994 could also be interpreted as an increase in the relative importance of deterrence. Beyond this recent trend, suppose further that the observed empirical estimate of the causal effect of enforcement resources on corruption convictions is at any time a net effect, combining both short run effects of system strain with longer-run effects of deterrence. To explore this possibility in more depth, we estimate the temporal impact of enforcement resources using a distributed lag model. We do this by including in the IV-specification of Table 2 a (twice) lagged value of enforcement, alongside the current level, mirroring the two-period lag in the instruments. We also include a lagged dependent variable to estimate the long-term impact of enforcement. While there are well-known issues with estimating dynamic fixed effects models by including directly the lagged dependent variable into a standard OLS fixed effects framework, the long time series that we have available here suggests that this is a minor concern.

Columns 2 and 3 in Table 4 show the results for the IV-specifications, now with two endogenous variables. While IV-diagnostics are not as strong as before, owing to the fact that we have to instrument two endogenous variables with the same set of instruments, they are still supportive of the IV-specifications. As before, the contemporaneous effect is significant and there is some suggestive evidence of a negative lagged effect suggesting a delayed role for deterrence, consistent with a timeline where contemporaneous increases in enforcement resources affect contemporaneous decisions to engage in corruption, (sometimes) resulting in referrals and convictions cases showing up in the statistics in the following years. We calculate long-run estimates as $\left(\beta_1+\beta_2\right)/(1-\omega)$ where β_1 and β_2 are the contemporaneous and lagged effects of (endogenous) Attorneys, respectively, while ω is the coefficient on the lagged dependent variable. These long-run net effects, of .5 and .32, respectively, are considerably smaller than the direct effect estimates shown in Table 2.

This exploratory analysis suggests that important features of the system of corruption prosecutions have changed over time. However, there are limits to our ability from these data to pinpoint what changed and exactly when changes occurred. Our IV-specification does not allow us to split the sample at or around 1994, as the IVs in this case become quite weak both before and after 1994. Similarly, split sample analysis of the OLS specification (not shown) produces estimates that are specification-dependent, with inter-period differences that do not rise to conventional levels of significance.

IV. Concluding Summary

The use of panel data on corruption convictions, new data on prosecutor resources along with better data on state-level relative incomes, and careful attention to endogeneity as well as fixed effects in the panel specification allow us to estimate the impact of prosecutorial resources on convictions of those who undertake corrupt acts. We find that greater prosecutor resources result in more convictions for corruption, other things equal. The results are robust to various ways of measuring the number of convictions, including moving averages and deflation by both population and the number of state and local government employees, and to various estimators addressing complications arising from the nature of the data. The results suggest that effects of system overload dominate those of deterrence on convictions, though probably to a lesser extent in more recent years. Moreover, we explicitly allow for the allocation of prosecutorial resources to be determined endogenously, by past corruption convictions and political considerations, and show that this specification leads to larger, though not unrealistic, estimates of the effect of resources on convictions.

Moreover, every specification that allows for endogenous prosecutor resources estimates a negative effect of relative public sector pay on corruption, as predicted by deterrence theory, though only in some specifications does the estimate rise to conventional levels of statistical significance. Furthermore, divided government, at least in its party control of separated branches form, appears associated with lower corruption despite all these other consideration, while term limits, often held responsible for poor political performance, appear to be associated with higher corruption. Finally, the analysis takes into account and confirms in a panel context, a number of previously identified political and economic correlates and causes of corruption, like the effects of checks and balances, income, education, population, and fiscal scale, though the magnitudes and significance of their effects are somewhat specification-dependent

A valuable next step is to push further the analysis of partisan forces. While we lack data in TRACFED before 1986, there is evidence that changes of partisanship across some

Presidential administrations alter prosecutorial effort in the way Posner and Gordon suggest. If we look again at cases referred and cases chosen for prosecution or declined, as above, we can see, in the case of the Clinton administration, that U.S. Attorneys in 1993-94 filed slightly more cases relative to 1991-92, but not nearly as many more as were referred. This revealed “effort” in 1993-94 (relative to 1991-92 in the same locale) clearly declined relatively more in more liberal areas. In simple regressions (results not shown), the interaction of ideology and lagged effort is negative and more than twice its standard error. Of course, we also cannot reject the counterhypothesis that workload expanded faster than capacity to file charges, leading to the apparent decline in effort, consistent with the “system strain” results reported above. In the case of the recent Bush administration, prosecutorial effort appears politicized in the same way, though with a slightly greater delay. Also, any apparent ideological bias in effort fades out in 2005-6, which maybe is why some U.S. Attorneys were subsequently fired! Further research using this data and other sources, though not easy, could determine the partisan affiliation of convicted officials in many cases, and biographical research could in principle even determine the partisanship of the President appointing the judge in each case.

Finally, how general are our results? Several things distinguish our sample from a broad cross-section of countries: higher incomes, the serious nature of the offenses (unlike the ubiquitous petty corruption reported elsewhere), and omnipresent enforcement, without which our model does not work. In the literature there is more or less a consensus that democracy reduces corruption, especially when democracy is synonymous with other related variables like freedom of the press and the rule of law (Brunetti and Weder, 2003). We see no obvious reason that, conditional on the presence of democracy or the rule of law which proxy for enforcement, other effects like those of government wages and inequality on corruption should not appear in a cross-national analysis. Finding out whether that is right is a challenge that remains before us.

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A. Data appendix

Corruption Convictions: The Public Integrity Section of the US Department of Justice (Maxwell and Winters 2004, 2005) reports "criminal abuses of public trust by government officials", based primarily on reports from U.S. Attorney offices. Originally the statute mandated reporting such abuses by *elected* officials, but individual cases reported in detail make it clear that the reports include non-elected public officials as well and others involved in corrupting them. The Section, created by the 1977 Ethics in Government Act, prosecutes some cases, but the great majority of cases are prosecuted by U. S. Attorneys. The 1983 Report of the Public Integrity Sections notes a change in the reporting and counting practice, notably including lower level employees, which caused in increase in the number of convictions from then on. These changes are subsumed by the year fixed effects.

From 1986 on more detailed data is available by judicial district at tracfed.syr.edu. Data on individual cases can be retrieved, offering the possibility of breaking down cases by the level of official involved within districts and states, as well as referrals, charges filed, and cases declined. The data here are a subset of the Public Integrity data, and it is not clear what causes the differences. The number of filings from the Tracfed data and the number of convictions from the Public Integrity Sections has a correlation coefficient of .74. Total number of referrals equal filings and declinations as reported by Tracfed. Effort equals filings divided by referrals. In 1993-94, referred to in the text, the average number of referrals across states was 37.9 (sd 45.0), ranging from 0 to 231. Average effort was .30, ranging from 0 to 1.

Elected officials: Data on the number of popularly elected state and local officials for the years 1977, 1987, and 1992 used in the calculation are from Table 2 of Volume 1, no. 2, "Popularly Elected Officials" of the U.S. Census Bureau, 1992 Census of Governments. These are available at http://www.census.gov/govs/www/cog92.html. Data for the intervening years were interpolated by averaging over time. Since numbers reported by the Section evidently include non-elected officials and some non-officials it is in fact not an entirely correct deflation.

Inequality: Ratio of 90^{th} to 10^{th} percentile of male wage income, from Gelman (2008).

Relative wages: We use the BEA data, which are in current dollars for state and local government employees (in total, and from 1979- by state and local separately), adjusted by "Full time and part time wage and salary employment" for the same categories, to calculate the average wage in current dollars of state and local government employees.[29] To be in real terms, we adjusted this for inflation using the regional CPI from the Bureau of Labor Statistics, divided into West, Midwest, Northeast and South. We also obtained in a similar way the average wage and salary disbursements by state, to compare with public sector wages.

Real per capita income, government expenditures and federal transfers: *Statistical Abstract of the United States*, various years

Divided government and tax and expenditure limits: *The Book of the States*, various years.

Education and percent urban population: Bureau of the Census.

Population: State Politics & Policy Data Resource, http://www.ipsr.ku.edu/SPPQ/datasets.shtml

Term limits: Data from Alt et al. (2008). For the whole sample period 14 states had no gubernatorial term limits, 18 states had two-term limits, and one state, VA, had a one-term limit throughout. Seven further states began with one-term limits but switched to two-term limits, while eight further states switched from no to two-term limits, The last group all switched in the early 1990s. The breakdown of states is as follows:

States with no effective term limits during the sample period: CT, ID, IL, IA, MA, MN, NH, NY, ND, TX, UT, VT, WA, WI.

[29] Part timers are problematic, and we omit them.

States with 2-term limits: AL, DE, FL, IN, KS, LA, ME, MD, MO, NE, NV, NJ, OH, OK, OR, PA, SD, and WV.

States switching within the sample period from 1- to 2-term limits (with year of switch): GA (1976), KY (1994), MS (1994), NM (1990), NC (1977), SC (1980), and TN (1978).

States switching within the sample period from no to 2-term limits (with year). AZ (1992), AR (1992), CA (1990), CO (1990), MI (1992), MT (1992), RI (1994), and WY (1992)

Ideology: Berry et al.'s (1998) measures of citizens and government ideology, 0 (conservative) - 100 (liberal). For the years 1993-94 referred to in the text, the average of the government ideology variable was 52.9 (sd equal 22.3), ranging from 1.7 to 93.0.

Voter ideology shares: Aggregated CBS News/New York Times national polls [electronic file], collected by Gerald C. Wright, John P. McIver and Robert S. Erikson (http://php.indiana.edu/~wright1/cbs7603_pct.zip).

[Table A.1 about here]

Table A.1: Summary statistics

	mean	sd	min	max
Convictions / population^	2.96	2.87	0	25.29
log (Convictions / population)^	-1.09	5.54	-16.12	3.23
EOUSA FTE attorneys per million population	13.25	5.45	2.18	36.53
Relative government wages	0.97	0.07	0.78	1.18
Inequality: Male wages	16.92	3.77	8.24	31.42
Divided government	0.46	0.50	0	1
Real per capita income ($1000)	13.36	2.75	7.71	23.28
Real per capita gov revenues ($1000)	1.70	0.44	0.83	3.83
Percent high school graduates	0.49	0.07	0.30	0.62
Log of Population (millions)	1.21	0.99	-0.79	3.56
Binding one-term limit	0.05	0.21	0	1
Binding two-term limit	0.22	0.41	0	1
Unemployment	5.95	2.08	2.30	17.40
Citizen ideology measure	47.57	14.73	8.45	95.97
Percent living in urban areas	67.27	21.20	26.03	100.00
Republican congruence	20.09	17.81	0	66.70
Urbanization*Share of Democrats in state senate^^	38.70	17.42	3.37	86.49

Sample: n = 1152 (sample from moving average regressions), except ^ which has 1138 and ^^ which has 1128.

Table 1. Enforcement and corruption convictions: OLS with fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Annual corruption measure			Moving average corruption measure		
	OLS	OLS	OLS	OLS	OLS	OLS
EOUSA FTE attorneys per million population	0.29***	0.25**	0.28**	0.28***	0.20*	0.20*
	[0.05]	[0.10]	[0.11]	[0.06]	[0.10]	[0.10]
Relative government wages			-31.45			-8.14
			[83.25]			[73.16]
Inequality: Male wages			0.09			0.06
			[0.07]			[0.07]
Divided government			-0.71*			-0.58**
			[0.37]			[0.28]
Real per capita income ($1000)			-0.49			-0.40
			[0.33]			[0.32]
Real per capita gov revenues ($1000)^			3.24**			2.73**
			[1.50]			[1.27]
Percent high school graduates			-3.25			-8.53
			[9.78]			[10.32]
Log of Population (millions)			4.99			1.75
			[3.51]			[2.90]
Binding one-term limit			1.75			1.34
			[1.37]			[1.31]
Binding two-term limit			0.58			0.29
			[0.41]			[0.33]
Unemployment^			-0.04			-0.04
			[0.17]			[0.15]
Citizen ideology measure			-0.06*			-0.05*
			[0.03]			[0.03]
Percent living in urban areas^			0.27			0.25
			[0.18]			[0.16]
Observations	1,327	1,327	1,138	1,343	1,343	1,152
R-squared	0.06	0.14	0.13	0.11	0.22	0.18
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes

Robust standard errors corrected for clustering at the state level in brackets.
*** p<0.01, ** p<0.05, * p<0.1. ^ denotes lagged twice.
A constant was included in all regressions, but results are not reported.
Calculation carried out in Stata 11.1 using xtreg.

Table 2: Enforcement and corruption convictions: Panel IV-analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Annual corruption measure			Moving average corruption measure		
	OLS	IV: LIML	IV: CUE	OLS	IV: LIML	IV: CUE
EOUSA FTE attorneys per million population		0.91**	0.87**		0.70**	0.64**
		[0.46]	[0.37]		[0.32]	[0.30]
Relative government wages	53.02	-39.99	-42.35	55.59	-25.62	-9.80
	[49.91]	[90.34]	[87.20]	[50.61]	[77.88]	[75.45]
Inequality: Male wages	-0.06	0.16*	0.17**	-0.06	0.11	0.09
	[0.05]	[0.09]	[0.09]	[0.05]	[0.08]	[0.07]
Divided government	0.09	-0.51	-0.43	0.10	-0.47*	-0.44*
	[0.20]	[0.39]	[0.37]	[0.20]	[0.28]	[0.26]
Real per capita income ($1000)	-0.27	-0.14	-0.18	-0.24	-0.25	-0.23
	[0.24]	[0.44]	[0.42]	[0.25]	[0.36]	[0.35]
Real per capita gov revenues ($1000)^	-1.87	4.44**	4.55***	-1.95	3.85**	3.75**
	[1.34]	[1.84]	[1.76]	[1.31]	[1.58]	[1.55]
Percent high school graduates	5.55	-6.42	-8.06	5.55	-11.57	-11.37
	[6.95]	[8.08]	[7.59]	[7.09]	[8.39]	[8.17]
Log of Population (millions)	-11.74***	13.07	13.67*	-11.66***	8.20	7.95
	[3.53]	[8.49]	[7.27]	[3.55]	[5.79]	[5.17]
Binding one-term limit	-2.19*	2.78	2.64	-2.20*	2.34	2.2
	[1.23]	[1.85]	[1.73]	[1.20]	[1.80]	[1.76]
Binding two-term limit	0.25	0.41	0.41	0.24	0.18	0.18
	[0.26]	[0.36]	[0.37]	[0.26]	[0.29]	[0.28]
Unemployment^	0.44***	-0.28	-0.27	0.43***	-0.22	-0.16
	[0.11]	[0.23]	[0.21]	[0.11]	[0.19]	[0.17]
Citizen ideology measure	0.05*	-0.09***	-0.10***	0.05*	-0.07**	-0.07**
	[0.02]	[0.03]	[0.03]	[0.02]	[0.03]	[0.03]
Percent living in urban areas^	-0.05	0.35*	0.40**	-0.05	0.29*	0.27*
	[0.14]	[0.19]	[0.18]	[0.13]	[0.16]	[0.15]
Instrument: Dependent variable_t - 2	0.05**			0.04**		
	[0.02]			[0.02]		
Instrument: Republican congruence_t-2	-0.06***			-0.06***		
	[0.02]			[0.02]		
Instrument: Urbanization*Share of Democrats	0.05*			0.05*		
in state senate_t-2	[0.03]			[0.03]		
Observations	1,101	1,101	1,101	1,113	1,113	1,113
R-squared	0.79			0.79		
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
J-statistic		0.985	0.979		0.712	0.731
p-value		0.611	0.613		0.701	0.694
F 1st stage		7.308	7.308		7.233	7.233

Robust standard errors corrected for clustering at the state level in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$. ^ denotes lagged twice.
A constant was included in all regressions, but results are not reported.
Calculation carried out in Stata 11.1 using xtreg and xtivreg2.

Table 3: Enforcement and corruption convictions: Robustness

	(1)	(2)	(3)	(4)
	convictions / mill. population		log(conv / state + local gov empl)	
	Annual	MA	Annual	MA
	IVTOBIT	IVTOBIT	CUE	CUE
EOUSA FTE attorneys per million population	1.17***	1.06***	1.01**	0.75**
	[0.21]	[0.216]	[0.43]	[0.34]
Relative government wages	-145.03***	-129.80***	-48.78	-13.50
	[37.11]	[32.413]	[100.41]	[86.81]
Inequality: Male wages	0.06	0.04	0.20**	0.11
	[0.06]	[0.047]	[0.10]	[0.08]
Divided government	-0.57*	-0.49*	-0.50	-0.51*
	[0.32]	[0.258]	[0.43]	[0.30]
Real per capita income ($1000)	-0.03	-0.00	-0.19	-0.25
	[0.16]	[0.000]	[0.49]	[0.40]
Real per capita gov revenues ($1000)^	-0.37	-0.00	5.18**	4.26**
	[0.62]	[0.001]	[2.03]	[1.79]
Percent high school graduates	-16.59***	-14.93***	-9.09	-12.91
	[5.70]	[3.974]	[8.76]	[9.42]
Log of Population (millions)	2.06***	1.76***	16.04*	9.43
	[0.40]	[0.378]	[8.43]	[5.98]
Binding one-term limit	-0.12	-0.30	3.14	2.67
	[0.76]	[0.457]	[2.00]	[2.02]
Binding two-term limit	0.31	0.11	0.48	0.21
	[0.39]	[0.328]	[0.43]	[0.33]
Unemployment^	-0.62***	-0.58***	-0.32	-0.19
	[0.18]	[0.173]	[0.24]	[0.19]
Citizen ideology measure	0.03**	0.03**	-0.12***	-0.08**
	[0.02]	[0.015]	[0.03]	[0.03]
Percent living in urban areas^	-0.01	-0.01	0.46**	0.31*
	[0.01]	[0.011]	[0.20]	[0.17]
Observations	1,101	1,113	1,101	1,113
State FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
J-statistic			0.914	0.644
p-value			0.633	0.725
F 1st stage			7.308	7.233

Robust standard errors (cols 1 and 2) corrected for clustering at the state level (cols 3 and 4) in brackets.
*** p<0.01, ** p<0.05, * p<0.1. ^ denotes lagged twice.
A constant was included in all regressions, but results are not reported.
Calculation carried out in Stata 11.1 using ivtobit and xtivreg2.

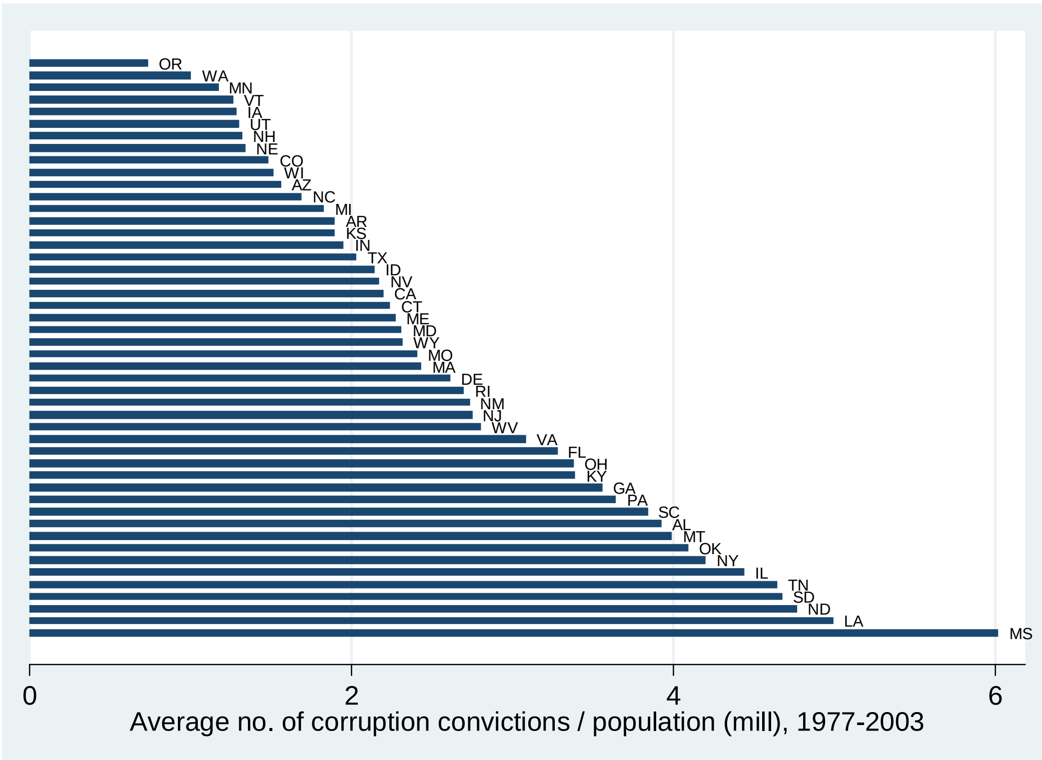
Table 4: Enforcement and corruption convictions: Dynamic issues

	(1) OLS	(2) IV	(3) IV
EOUSA FTE attorneys per million population	0.54***	2.31**	1.63*
	[0.14]	[1.13]	[0.88]
EOUSA FTE attorneys per million population,	-0.43		
post 1994	[0.30]		
EOUSA FTE attorneys per million population		-1.84	-1.31
lagged: L1 in column 2, L2 in column 3		[1.31]	[0.86]
Lagged dependent variable		0.06	0.01
L1 in column 2, L3 in column 3		[0.04]	[0.05]
Controls	Yes	Yes	Yes
Observations	1,042	1,100	1,081
R-squared	0.15		
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
J-statistic		0.060	0.246
p-value		0.807	0.620
F 1st stage		2.393	3.309

Robust standard errors corrected for clustering at the state level in brackets.
*** p<0.01, ** p<0.05, * p<0.1
A constant was included in all regressions, but results are not reported.
Calculations carried out in Stata 11.1 using xtreg and xtivreg2.

Figure 1: Corruption convictions 1977-2003

a) Corruption convictions by state



b) Corruption convictions / population (mill.) by year

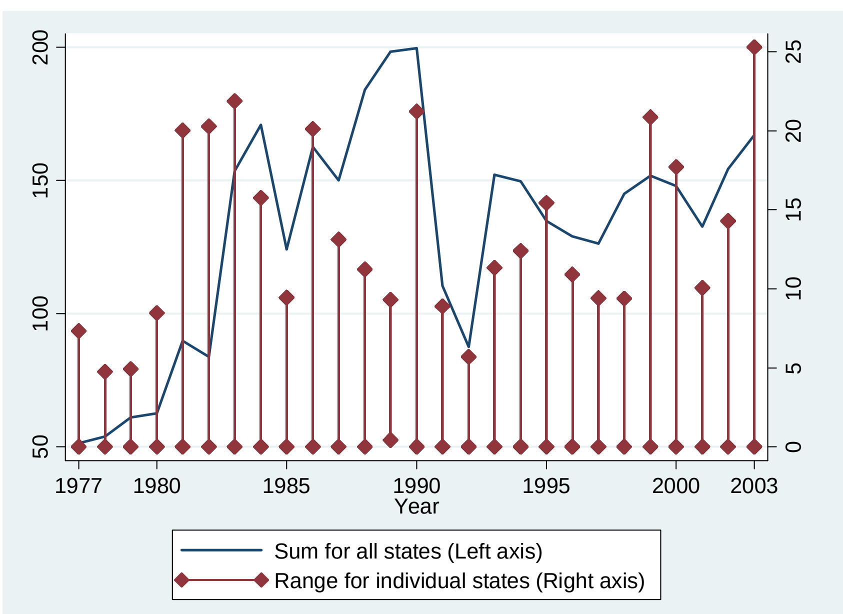


Figure 2: Range and average of enforcement resources, by year

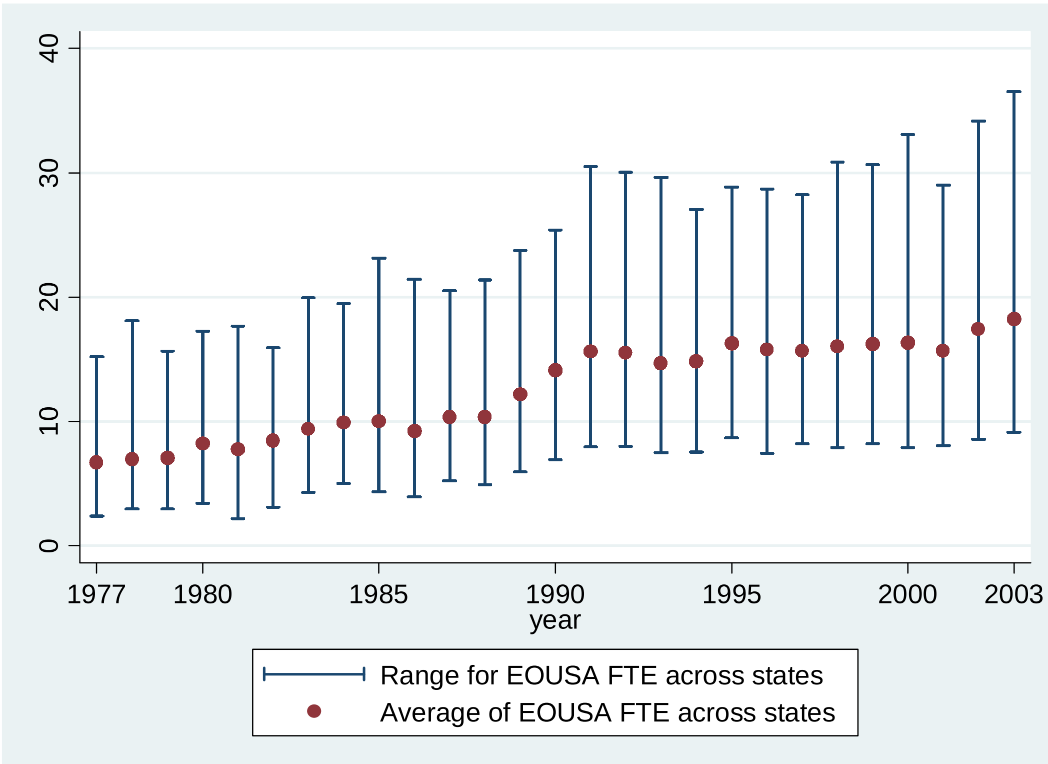


Figure 3: The increase of system capacity after 1994