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Inequality and Corruption: Evidence from US States

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Abstract

High-quality data on state-level inequality and incomes, panel data on corruption convictions, and careful attention to the consequences of including or excluding fixed effects in the panel specification allow us to estimate the impact of income considerations on the decision to undertake corrupt acts. Following efficiency wage arguments, for a given institutional environment the corruptible employee's or official's decision to engage in corruption is affected by relative wages and expected tenure in the public sector, the probability of detection, the cost of fines and jail terms, and the degree of inequality, which indicate diminished prospects facing those convicted of corruption. In US states over 25 years we show that inequality and higher government relative wages significantly and robustly produce less corruption. This reverses other findings of a positive association between inequality and corruption, which we show arises from long-run joint causation by unobserved factors.

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Keywords: corruption, rent seeking, inequality, Gini coefficient, efficiency wage, public sector wages

JEL-codes: D72, D73, P48

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I. Introduction

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 developing and developed democracies alike. 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. A very large literature has investigated the political, cultural, historical, and economic determinants of corruption. A much smaller, micro-based literature, on which we build, considers how an elected or publicly employed agent's key choices over alternative income sources affect the decision to act corruptly.[1]

A frequent empirical finding is that greater economic inequality is associated with greater corruption. Here, inequality is a sign of poverty: poorer publicly employed agents have greater incentives to engage in corrupt behavior. Thus, Paldam (2002) writes that income inequality increases "… the temptation to make illicit gains." As a consequence, higher inequality is widely believed to be associated with higher levels of corruption, as when Rose-Ackerman (2004: 11) argues that "in democracies in particular, inequality facilitates corruption, a result consistent with the state capture variant of corruption." You and Khagram (2005) argue that there is a "norm" of expecting and tolerating corruption where inequality is high (see also Uslaner, 2008).[2]

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[1] 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.

[2] You and Khagram (2005) argue that the norm arises because inequality means that when the rich have more resources to lobby and/or act corruptly, there is less of a middle class to resist corruption, while the poor only observe corruption in society.

Instead, our theoretical starting point is the factors that influence the individual decision to take money from corrupt as opposed to legal activities. Becker and Stigler (1974), and more recently Besley and McLaren (1993) and Van Rijckeghem and Weder (2001) among others, model a publicly employed agent who, at the margin, compares the size of the rents from illegal activities, allowing for the probability of being investigated, detected, charged, convicted, jailed, expelled from public service, and therefore compelled to earn a living privately, with the income from continuing to work legally in the public sector.[3] We show how the relationship between inequality and both government wages and private sector wages affect the calculus of corruption. We generate two testable, related hypotheses: first, that higher relative government wages decreases corruption, and second, that greater inequality (through the effect of wages in alternative employment) also *decreases* corruption. We support these claims empirically.

By contrast, You and Khagram (2005) find empirical support for a positive relationship between income inequality and corruption using data from a cross-section of countries. Glaeser and Saks (2006) find the same empirical result for a cross-section of US states.[4] However, Treisman (2007: 239) writes of looking for

> … evidence of a link between corruption perceptions and both income inequality (measured in various ways) and the relative wage in the public sector …. These were not generally significant in regressions that included basic controls.

We show that both those variables are indeed significant. Using high quality panel data to distinguish short and long run effects of changes in relative income streams and wage inequality

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[3] We focus on government wages. In Becker and Stigler, it is optimal to set government wages above private sector wages, as the wage premium discourages corruption in an efficiency wage fashion. Van Rijckeghem and Weder (2001) give supporting empirical evidence for this effect in a cross-section of countries.

[4] You and Khagram and Glaeser and Saks use multi-year averages to take out noise from the dependent variable, so all their estimates are ultimately cross-sectional. Not everyone finds these patterns – Park (2003) and Brown et al. (2005) find no evidence that greater inequality increases corruption.

on corruption, we find some evidence of long-run joint drift of inequality and corruption, but that the short-run, causal effect of inequality on corruption is negative, as is the effect of relative government wages. These results, with more inequality leading to fewer corruption convictions, reflect the importance of accounting for time trends or year effects and unit fixed effects, and hold up even while controlling for factors from the other literature reviewed below.

We take those controls from recent cross-national empirical research that 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.[5] Where appropriate, many findings of this comparative work 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). We extend this literature by using data from American states in this investigation.

By cross-national standards the US states are a sample with relatively high incomes and established democracy and rule of law, and we return to the implications of this below. Nevertheless, three strong advantages of studying them are: we now have a reliable panel of corruption data covering a quarter of a century which does not rely on surveys or expert opinions; we also have better data for inequality and relative public sector salaries than do the cross-country studies we review below; and, most of all, enforcement potential across units, important once the probability of detection is considered, is at least exogenous, and perhaps roughly constant across states if not over time; we evaluate this last claim empirically below.

[5] See Ades and di Tella 1999; Montinola and Jackman, 2002; Seldadyo and de Haan 2005; and Treisman 2000, 2007, 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.

The next section presents our theoretical framework and empirical hypotheses. The third section describes data and estimation strategy. Section four presents results, considering alternative indicators and specifications. Section five concludes.

II. Relative wages, wage inequality, and corruption: the core argument

One way to counter the temptation of corruption among public officials and employees is to increase the wage rate paid to the official. This efficiency wage result was derived in the context of bribery of law enforcers by Becker and Stigler (1974). Theoretically it has been critically analyzed and extended by Besley and McLaren (1993), Mookherjee and Png (1995) and Ades and di Tella (1999), among others.[6]

The basic Becker-Stigler argument is that a government employee maximizes the present discounted value of a stream of expected income. A government employee contemplating a corrupt act can end up in three situations. First, if no corrupt act is committed, the employee simply receives his wage. Second, if he engages in corruption but is not detected, he receives both the wage and the bribe. Third, if he engages in corruption and is detected and sentenced he receives neither the wage nor the bribe, but incurs a penalty and is fired from public employment. In this case, future income is generally 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 wages and expected tenure in the public sector, the probability of detection, the cost of fines and jail terms, and wages in the private sector *conditional on having been caught for corruption*. Becker and Stigler show that in their environment, setting a public sector wage above the market clearing wage decreases the

[6] Polinsky and Shavell (2001) analyze a more general model, where public employees can also engage in extortion. Besley and McLaren (1993) and Ades and di Tella (1999) assume that employees differ in their intrinsic honesty.

propensity of the public employee to engage in corrupt behavior, a now standard efficiency wage result. As noted above, this hypothesis has received some 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. di Tella and Schargrodsky (2003) show, in a detailed analysis of corruption at hospitals in Buenos Aires, that higher wages in periods of less than maximal monitoring decreased corruption, while higher wages had no effect under maximal enforcement, as suggested by the Becker-Stigler model. Using higher government wages as a carrot thus seems to lower corruption.[7]

This paper focuses on the stick as well as the carrot. In addition to enforcement and sentencing, an important part of the government employee's calculus of corruption is the expected wage in employment following job termination that results from sentencing or plea bargaining in corruption cases. In our view, a public employee convicted of corruption would not subsequently be employed at the average private sector wage: we assume that on average they would return at below-average wages. In standard efficiency wage models, lower wages upon termination from government employment decreases the expected present value of discounted lifetime earnings resulting from choosing to be corrupt. This reduces, for given values of public sector wages, bribes, and enforcement, the temptation to engage in corrupt behavior.[8]

Therefore, we need to look more closely at the wage structure actually facing government employees terminated from their jobs due to corruption charges or sentences. We cannot observe

[7] However, other studies do not confirm this (Gurgur and Shah 2005; Treisman 2000), maybe because government wages correlate with measures of the rule of law and (of course) the quality of the bureaucracy. The level of government wages could also correlate with aspects of the overall income distribution like inequality.

[8] This implies that comparisons to average wages in the manufacturing sector, as in Van Rijckeghem and Weder (2001), or to per capita GDP, as in Schiavo-Campo et al. (1997), may capture an occupational choice between the private and public sectors, to which we return below, rather than the threat from terminated employment.

this expected wage, and people at different points in the public sector wage distribution will presumably enter the private sector wage distribution at different points. Moreover, a more unequal wage distribution will tend to increase the distance (in dollars) between different percentiles in the wage distribution. In addition, just as the average private sector wage will vary between localities, so will other percentiles in the wage distribution: thus, the 10th percentile in the wage distribution could be lower in a locality with a more unequal wage distribution. Empirically, we therefore have to consider not only the *distribution* of wages (rather than just specific moments or percentiles) but also government sector wages relative to lower decile wages as well as the average wage.

Our conjecture is that, for given government salary levels, facing a more unequal private wage distribution increases the expected cost of engaging in corrupt behavior. In our empirical analysis below, we therefore include a measure of both the carrot, higher relative government salaries, and the stick, lower wages in alternate employment measured by the (inequality of the) distribution of wages and salaries. Both of these are expected to decrease corruption.[9] We capture the distribution of wages by an inequality measure: the Gini-coefficient for wages and salaries.[10] The larger the Gini-coefficient, the more unequal is the wage distribution.

So far we have analyzed the effects of changes in the compensation schedule on the choice of employees already in the public sector. However, if agents differ in some intrinsic characteristic like honesty -- as assumed by Besley and McLaren (1993) and Ades and di Tella (1999) -- the compensation schedule also affects selection into the public workforce. If this is the

[9] Note that if inequality "reduces" government wages, the effect of omitting the relative wage variable on the estimated effect of inequality will be to bias it in a positive direction. However, if high relative government wages reduce inequality, then the effect of omitting income inequality will be to bias the estimated effect of relative wages toward zero. In any case, empirically, there is an obvious case for simultaneously considering both relative government wages and income inequality.

[10] This coefficient measures the area (the sum of distances) between the Lorenz curve (the actual wage distribution) and the 45-degree line (the hypothetical case of equal wages).

case, the effects of the stick in the analysis above could differ between the short and the long run: In the short run, a changing compensation schedule (including outside opportunities) affects the moral hazard component of corruption: the calculation, conditional on being publicly employed, of whether to engage in corrupt behavior. In the long run, however, the compensation schedule affects the selection of people in and out of the public sector.

In this longer perspective, one might assume that positive public attitudes towards government and governance, which are positively correlated with support for greater equality, increase the relative compensation of government officials and employees. This in turn attracts more honest or highly-qualified people (as in Besley and McLaren, and Ades and di Tella) which lowers corruption and raises the quality of government (Besley 2004). A long run relationship, reflecting joint causation from factors like citizens' attitude to government thus leads us to expect a positive correlation between lower inequality, higher government salaries, and lower corruption across units in the long run, the converse of the short run hypothesis regarding the moral hazard dimension. This is the same result, if not the same mechanism, as in the many cross-sectional studies cited in the Introduction.

The effects of wages and wage inequality considered so far have been on the *supply* of corruption. If increases in inequality are mainly driven by changes (increases) in the share of income received by high earners, however, this may increase the *demand* for corruption (Shleifer and Vishny, 1993). For given public wages, this would also increase the level of corruption. In that case, any finding of a negative supply effect of inequality on corruption would be net of this positive demand effect. We evaluate this alternative explanation empirically below, using data specifically on highest incomes.

III. Data and Specification

III.1 Specification

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

$$corruption_{i,t} = \beta^{g} gini_{i,t} + \beta^{w} relwages_{i,t} + \gamma X_{i,t} + \mu_{i} + \tau_{t} + T_{t} + \varepsilon_{i,t}$$

where corruption in state *i* at time *t* is explained by economic inequality, measured by the Gini-coefficient, average government wages measured relative to average private sector wages, and additional variables contained in the *X* matrix, described below. In addition, we include state fixed effects to account for time invariant differences between states, time fixed effects to account for changes that affect all localities in the same way, for example changes in federal legislation and enforcement, and a time trend. The trend does not contribute anything in itself, as year effects have already been taken account of by the year fixed effects, but it helps us separate the trend from yearly changes. When we present our main results, we highlight the contributions of state and year fixed effects, respectively. We subsequently consider an alternative dynamic panel specification of the temporal aspects based on an error correction model, which confirms our inferences about the main quantities of interest.

Throughout, we distinguish short run and long run, or cross-sectional, effects of inequality on corruption. We argue that in the long run inequality and corruption may move together due to joint causation from third factors. However, we believe there is little reason to be concerned about endogeneity of inequality with respect to corruption in the short run. Corruption is a relatively rare occurrence in the US in the period we consider, and does not affect the income distribution, as might be the case if corruption were more widespread.

III.2 Dependent variable: Corruption convictions

While the comparative 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 from the Public Integrity Section of the US Department of Justice. Maxwell and Winters (2004, 2005) provide and describe in detail these annual data, aggregated to states from U.S. judicial districts. The Maxwell-Winters data includes some 17,000 cases between 1977 and 2002. Early on, local cases were the most common but more recently the proportion of federal officials prosecuted (along with those who corrupt them) has increased.

The Section, created by the 1977 Ethics in Government Act, reports “criminal abuses of public trust by government officials”. It prosecutes some cases, though most are handled by U.S. Attorneys. Corruption cases begin with criminal investigations which may or may not end in referrals to the U.S. Attorney’s office. In the last two decades 80 per cent came from the FBI. 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. The cases are not on the whole petty crimes: lead charges on the largest number of indictments pursued by US Attorneys were based on robbery or extortion affecting interstate commerce, theft 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 2008).[11]

Overall, we believe these convictions data to be of high quality and comparable across states. Of course, in principle there is an ambiguity about convictions data. For instance, if there

[11] The source is the Transactional Records Access Clearinghouse at Syracuse University: http://tracfed.syr.edu/.

was corruption in the judicial system, it would manifest itself in a low number of corruption convictions. We assume this is not a widespread problem in the US in the period we study, and we say more about issues of enforcement below. At the same time, we also note that Alt and Lassen (2008) show that estimates of effects on corruption are qualitatively similar in a cross-section when both expert ratings (of corruption and quality of governance) and data on convictions are available.[12]

Maxwell and Winters calculate the number of convictions relative to the number of elected officials, as a proxy for the number of all government officials. Figure 1a first shows (line; measured on the left-axis) the sum across all states of the number of convictions relative to the number of elected officials (in 10,000s) in each state. 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 13.7 convictions per 10,000 elected officials (corresponding to 1108 convictions), and the convictions rate in 1989 ranges from .01 in Vermont to 1.75 in Virginia.[13] As the distribution is skewed, Maxwell and Winters' (2005, p. 9) final measure (and our dependent variable in subsequent analyses) is the logarithm of convictions for corruption per 10,000 elected officials. The average of these logged values over all years is presented in Figure 1b, ranging from lows in Vermont and North Dakota to highs in Louisiana and Florida.

[Figures 1a and 1b about here]

Our sample covers the years 1977-1999 for the 48 contiguous states. Details, sources and a Table of summary statistics for data appear in the Appendix.

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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] Others (Glaeser and Saks 2006) adjust the number of convictions by state population. We return to this in the robustness section below.

III.3 Main explanatory variables

Gini-coefficient

Galbraith and Hale (2006) estimate a Gini coefficient for each state and the District of Columbia from 1969-2004 based on Bureau of Economic Analysis (BEA) wage data. For every year the BEA compiles data on wages and employment across dozens of industrial classifications for every state. Galbraith and Hale use a complicated iterative procedure of calculating a Theil-statistic from the BEA industry- and sector-level wage data, and then fitting it to Gini coefficients of family income from the Census Bureau's Current Population Survey yearly individual-level sample survey data. Galbraith and Hale report that from 1969 to 2004, the estimate of the Gini coefficient of family income increased for every state. The state average (not weighting by population) was .356 in 1969 and .427 in 2004, an average increase of 20 per cent, ranging from a 6 per cent increase in North Dakota to 46 per cent in Connecticut. Figure 2 shows the state averages and ranges by year for the period of our sample.

[Figure 2 about here]

Figure 2 also shows the trend in the share of income earned by the top decile income earners, averaged across states. This means, for example, that on average across states in 1996, the richest 10 per cent of the income distribution earned 40 per cent of total income. As is obvious from the figure, this share has been rising alongside the Gini coefficient, and it could, as noted in our theoretical discussion, be an alternative source of an increasing number of corruption convictions.[14]

Government average and relative wage

Data are calculated from average state and local government wages and salaries as provided by the Bureau of Economic Analysis: "Wage and Salary Disbursements by Industry", which was

[14] Data on top income share is from Frank (2008).

also the source for construction of the Gini-coefficients. We use this to construct three useful variables: the average wage for state and local government, in current dollars; the ratio of the average state and local government wage to the average wage in the state; and the average wage for state and local government, in constant dollars.[15] Figure 3 displays the second of these (solid line), the ratio of the average state and local government wage to the average wage in state.

[Figure 3 about here]

The third of these we use for another measure, which serves as an alternative to the Gini-coefficient: government wages relative to the income level of the first decile, shown as a dashed line in figure 3. While we do not have data on the distribution of wages within sectors, we have data on incomes by decile.[16] To the extent that those making the calculus of corruption contemplate future wages at the bottom of the scale, then incomes in the lower deciles are a better deflator of average government wages than average private sector wages. As such, this measure really combines, though imperfectly, the carrot, expressed by government wages, and the stick, expressed by the income distribution. It does so by relating government wages to a more appropriate point of comparison: bottom decile rather than average wages. However, as we do not know the specific point of comparison for government officials in a given state in a given year, it complements, but does not substitute for, the income distribution measure which captures the relative difference between high and low incomes.

Turnover, term limits, and the shadow of the future

Shleifer and Vishny (1993) argue that in unstable systems the ephemeral nature of public positions makes officials irresponsible and grasping. In the analysis of Becker and Stigler (1974), the public official is always corrupt in the final period of employment unless the promise of

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

[16] The data source is www.inequalitydata.org. Data were supplied by Andrew Gelman. See Gelman, 2008, ch. 5.

pensions is sufficient to keep him honest. Expected tenure in office or government employment thus always affects the expected present value of alternative income streams. Lacking systematic data on the probability of re-election of a gubernatorial administration, 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 a short horizon of officials or an administration. 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.[17] Indeed, if incumbents subject to a one-term limit have worse performance than incumbents under a two-term limit (Alt, Bueno de Mesquita, and Rose 2008) we expect this effect to be larger in the one-term limit case. Further data details appear in the Appendix.

<u>The probability of detection: prosecutor bias and effort and enforcement</u>

An important parameter in the efficiency wage framework is the risk of being caught. This risk depends on observability and enforcement. In US states, enforcement begins mostly with the FBI which is responsible for the vast majority of case referrals that begin the enforcement process (Gordon 2008). For our current purposes we treat relative effort across states as constant across years, and changes in effort from year-to-year as the same across all states.[18] What happens if an agent's corrupt acts are detected? He faces investigation (almost certainly), a considerable period (up to 18 months) of being under scrutiny while the case is considered (highly probable), and then possible indictment and trial (less than 50-50 overall), conviction or a plea deal (85%, conditional on getting here), and sentencing. Boylan (2005)

[17] 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.

[18] That is, we treat FBI effort as picked up by state and year fixed effects, and we assume the latter also pick up any effects of changes in federal sentencing guidelines. We have been unable to ascertain anything specific about local FBI effort (manpower, budget) for the period we study.

argues that "ambitious" prosecutors seek longer sentences rather than more convictions.[19] We have been unable to obtain systematic data on prosecutorial effort, and so empirically rely on year fixed effects to capture changes in federal prosecutorial effort and strategy, though subsequently we look briefly at possible politicization of prosecutions.

III.4 Other social, political, institutional factors

Our main specification includes, in addition to the variables suggested by the model, five variables most often found to influence corruption in other studies of US states: average real income per capita, population share with college education or higher, real government expenditures per capita, population, and divided government. Meier and Holbrook (1992), Goel and Nelson (1998), Adserà et al. (2003), Boylan and Long (2003), Alt and Lassen (2003, 2008), and Glaeser and Saks (2006) among others find effects of income and a better educated citizenry, though the results are mostly cross-sectional. Other studies including Goel and Nelson (1998) and Alt and Lassen (2003, 2008) find higher government expenditures (or a variant thereof) per capita to be associated with greater corruption. In their panel Maxwell and Winters (2004) find over-time instability in many estimated effects, but ultimately affirm a set of more stable correlates including population (positive), social homogeneity (negative), and citizen education and engagement (negative).

Cross-national institutional studies 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. Many of these variables are constant across US states in the period we consider. Maxwell and Winters (2004) find that the number of governmental units increases corruption

[19] This apparently increases the chance of becoming a judge or a partner in a good law firm after leaving office.

(more than linearly once there are more than a handful of governments[20]), Alt and Lassen (2008) show that divided government (legislature and executive controlled by different parties) in states is associated with lower corruption. In our sample, divided government occurs on average 45 per cent of the time. After increasing the first few years, it is stationary, and it is generally highly correlated with measures of political competition at the state level.

In robustness analysis, we consider several more variables, though not, of course, the entire span of variables that have appeared in the corruption literature.[21] Glaeser and Saks find that racial heterogeneity increases corruption, and similarly Maxwell and Winters find that social homogeneity reduces corruption. We consider the effects of racial heterogeneity, measured by the (logarithm of the) percentage of the state population which is Black. Fisman and Gatti (2002) find that a high share of federal transfers in revenues increases corruption.[22] We consider ease of raising funds and fiscal decentralization, measured as the share of state revenues generated by federal transfers and whether or not a state had formal tax and expenditure limits in a given year, and also experiment with other measures like state budget process transparency (Alt, Lassen and Rose 2006), but do not find precise results. Finally, unobserved or unchanging factors like

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[20] This would mean that opportunities to grab dominate better monitoring when there is more decentralization. Nevertheless, Dincer, Ellis, and Waddell (2006) show the opposite: fiscal decentralization (measured as the local share of state and local expenditures) reduces corruption.

[21] For example, Seldadyo and de Haan (2005) average indices to reduce noise and factor analyze correlates to reduce multicollinearity. They find the closest stable and robust correlates of corruption (itself based on indices) 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, recalling the Maxwell-Winters US findings for population, social homogeneity, and citizen education and engagement. Other 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).

[22] Glaeser and Saks acknowledge that the effects of inequality and race might reflect the prominence of federal programs creating opportunities for corruption. They also find convergence among states over time, which might reflect effort to secure more convictions early on wherever corruption was most visible, stabilizing later.

"cultures" of corruption (Peters and Welch, 1978; Johnston, 1983) are subsumed by state fixed effects. Sources of data and further details are provided in the Appendix.

IV. Estimation Results

In this part we first estimate the model above including the main controls but with and without state and year fixed effects. We examine the reasons for the different results that come out of these estimations. As a consequence, we next analyze the temporal patterns, distinguishing short and long run effects of changes in inequality and relative wages on changes in corruption convictions using a dynamic panel model. The estimates are qualitatively consistent with those from the earlier model. Then we consider alternative measures of inequality and relative wages, and again the results support inferences drawn from our theoretical approach. Finally, we consider additional explanatory variables, including racial heterogeneity, the business cycle, decentralization, federal transfers, tax and expenditure limits, and budget transparency, among other things, as well as additional robustness issues, including alternative explanations.

IV.1 Basic results

Table 1 shows the results of a generic specification that we use to illustrate the main results. It estimates logged convictions per 10,000 elected officials in 48 states over 23 years from 1977-99. The first reported regression (column 1) is simply a pooled OLS omitting both year and state fixed effects. Columns two and three include year and state fixed effects, respectively, while the fourth, our main specification, includes both. Each set of fixed effects (and both sets together) is jointly statistically significant. Hausman tests on random effects as opposed to fixed effects confirm the choice of unit fixed effects, both in specifications with and without year effects.

[Table 1 about here]

In Table 1, column four, the income and inequality effects are consistent with what we would expect given an efficiency wage argument: the more the detected, convicted official gives up in terms of the benefits from an honest future and the worse the re-entry prospects after conviction, the fewer convictions we observe. Where the average public wage is higher, relative to the average private wage, there is less corruption.[23] Higher income inequality also apparently deters corruption. To repeat, this negative effect of inequality is the opposite of what has been found in other literature. Both coefficients are statistically significant. Other results (not shown) indicate that any one or more other controls (other than the fixed effects) can be deleted from this regression without altering the sign or significance of these variables, The inequality result is no artifact, though the sign reversals between columns 1 and 4 indicate that it does depend on whether one is studying cross-sectional or over-time variation.

The four “standard” controls all have expected signs: states with higher incomes and a more educated citizenry have, on average, less corruption, though the effect of education is not nearly statistically significant in this model. Larger fiscal scale (more real spending per capita) and a larger population both are associated with increased corruption. The latter is to some extent an artifact of, but at the same time controlling for, the public goods nature of some elected officials. That is, larger states have proportionately fewer elected officials, but supposedly a proportional number of public employees, than smaller states. This means that corruption convictions per 10,000 elected officials will tend to overstate corruption in larger states. At the same time, however, if larger states also mean states with major metropolitan areas (such as IL or NY), this in itself could be a cause of higher corruption. Including or excluding other controls discussed next does not make any difference to the qualitative pattern involving these four

[23] Note that this effect of relative wages is independent of the effect of overall government spending, which goes the other way. That is, once one controls for the negative effect of relatively high public wages, the effect of (the rest of) high spending is positive, reflecting temptation.

variables, though significance levels go up and down.[24] Corruption convictions also rise under term-limited incumbents, and the effect is larger for one-term lame ducks.[25] Convictions fall when there is divided party control of government in the states. Divided party government has a negative effect on corruption, as in Alt and Lassen (2008), but now this effect holds up in a panel with an even larger set of other controls. Possibly both those results would be better cast with lags to allow for the two-year gap between corrupt action and conviction in most cases.[26]

Relative wages has a big substantive effect. The standardized coefficient is about -.35, so a one standard deviation change in the relative wages of government to private employment reduces corruption convictions by about a third of a standard deviation. The effect of inequality is even larger: the standardized coefficient is about 0.7, and this estimate allows for the fact that the effect of inequality is conditional on conviction, which occurs in less than a third of referrals. Turning to term limits, when a one-term limit binds the effect is larger (0.4) than when a two-term limit binds (0.12) but the former is much less common than the latter in the sample (about 2 per cent versus 18 per cent of observations). The effect of a two-term limit is similar in magnitude to the effect of divided government, which is 0.08 (but occurs nearly half the time).

IV. 2 The impact of time and state fixed effects.

In the pooled regression of Table 1, column 1, inequality enters with a positive sign and is strongly significant, and a similar result obtains in repeated cross sections for each year (not reported). Including state and time fixed effects changes this completely: The coefficient on inequality is now negative and strongly significant. Such sign reversals are not uncommon, and

[24] There are some correlations among the explanatory variables. Income, spending, and education are all correlated, and they and the Gini all trend upward. We return to this below.

[25] The fact that one-term limits are more common in the early period, when there are fewer corruption convictions, would work against our finding a positive coefficient.

[26] The incumbency variables do relate to the Governor's whole term (usually four years) though divided government could change every two years if the legislature changes hands.

indeed uncovering them is one of the advantages of panel data, as Wilson and Butler (2007) show. Here we explore this result further, and Figure 4 is a first step: it plots the year-by-year averages of convictions and inequality.

[Figure 4 about here]

One can see two things happening in Figure 4. First, there is obvious movement from lower left to upper right, as both inequality and convictions increase (the ubiquitously-found positive relationship). Nevertheless, what is equally apparent is that from 1983-1997 (approximately), the relationship is negative. From 1983-86 the data are moving along a downward-sloping line, and then both variables move out a bit. The relationship from 1987-97 again has a downward slope, and then in the last two years the variables jointly move out again. This is a pattern of long-term joint movement with positive correlation, but short-term movements (often called feedback or error correction, see below) that go in the opposite direction.

What do the estimates in the individual states look like for the relationship between corruption and inequality, stripped of this time pattern? If we remove the global time effects from the data and graph the predictions from the de-trended data by states, we get Figure 5. Each solid line represents the linear fit (across the range of observed data) between detrended convictions and inequality for an individual state. The main regression line, based on a pooled bivariate OLS, and shown as a dashed line, denoted LR, in Figure 5, is indeed positively sloped.

[Figure 5 about here]

However, one can see that in most cases, once the effects of time on all units are taken away, the relationship *within* each unit is in fact negative. There is not really a problem with unit heterogeneity in this case: a look at all 48 reveals only six states (Idaho, Iowa, Minnesota, North

Dakota, South Dakota and Wyoming) with coefficients that are positive or close to zero, mostly at very small values of both variables. Similarly, this has nothing to do with controls: while this Figure is based on the regression in Table 1, deleting any or all of the controls (other than inequality and relative public wages!) would allow us to present the same picture. So in fact the impact of inequality on convictions in the short run is negative, even though in the long run other factors may cause both corruption and inequality to trend higher together. We explore this dynamic relationship in detail in the following section.

IV.3 Short and long run effects of inequality: Dynamic panel estimation

The combination of short-term negative and long-term positive dynamic effects – suggested both by Figures 4 and 5 and by the results in the two-way fixed effects model as opposed to the cross-sectional model – raises issues of potential error correction and cointegration. If any linear combination of the variables or, of course, the variables themselves, is stationary then an error correction model (ECM) can estimate both such long- and short-run effects.[27] The general (panel) error correction model, which is a re-parameterization of the standard ADL(1,1) model, is given by

$$\Delta y_{i,t} = \beta_i \Delta x_{i,t} + \phi_i \left(y_{i,t-1} - X_{i,t} \lambda_i \right) + \mu_i + \tau_t + \varepsilon_{i,t}$$

where $\Delta y_{i,t}$ is the dependent variable in state *i* during year *t*. In the most general formulation, we allow all slopes and intercepts to be state-specific; we return to this below. The current change in *y* is the sum of two components relating to the independent variables (as well as state fixed effects and common year effects, as above). First, the error correction mechanism consists of the

[27] Standard tests show that while superficially the series might appear to cointegrate, only the Gini-coefficient has a unit root while corruption is stationary. See Figures 1 and 2. However, some believe an ECM is appropriate even if the dependent and explanatory variables do not cointegrate (De Boef and Keele 2008).

equilibrium error $\left(y_{i,t-1} - X_{i,t}\lambda_i\right)$, which is a partial correction for the extent to which the dependent variable deviated from the equilibrium value, corresponding to $\lambda_i X_{i,t}$. The parameter ϕ_i captures how quickly the relationship returns to equilibrium. Second, $\beta_i \Delta X_{i,t}$ is the direct, short-term effect of a change in the independent variable. Below, we refer to λ_i as the long-run effect and β_i as the short-run effect.

The panel structure presents some additional complications. In particular, a recurring issue in applications of the ECM in a panel context is the heterogeneity of cross sectional units, here states. Three overall approaches exist (see Pesaran, Shin and Smith, 1999): First, the traditional dynamic fixed effects (DFE) model pools the data, allowing only intercepts, the state fixed effects, to differ. Second, the Pooled Mean Group (PMG) estimator pools the data and constrains the long run coefficient λ to be the same across states, while allowing intercepts, short-run coefficients and error variances to differ. Finally, the mean group (MG) estimator does not pool the data at all, but simply averages estimates from separate state-by-state estimations. These models are increasingly unrestricted in the order mentioned, and thus possible to test against each other using Hausman-type specification tests.

Table 2 presents the results.[28] The upper part of the table reports results for the three different estimators of a parsimonious specification, where *gini* is the only explanatory variable. The lower part of the table reports results for a more comprehensive specification, which includes additional short run control variables. For each of the restricted models is reported the *p*-value associated with a Hausman-test of the model against the MG estimator. While the MG estimator is consistent under both the null hypothesis and the alternative hypothesis, the restricted models (PMG and DFE) are inconsistent under the alternative but efficient under the

[28] Estimation was carried out in Stata 10, using the xtpmg-procedure developed by Blackburne and Frank (2007).

null. Thus, a low *p*-value suggests rejection of the null, i.e. that the unrestricted MG-model is preferred.

[Table 2 about here]

Throughout, we observe a positive and strongly significant long-run coefficient on inequality. At the same time, the estimated short-run effect of inequality is persistently negative, and significantly so in most specifications. The specification tests suggest a preference for the unrestricted MG model against the alternatives, most evidently in the parsimonious specifications. This means that the long-run relation between inequality and corruption is not the same across states. It does not mean, however, that the effects of inequality on corruption follow completely different patterns across states: While the MG model imposes no a priori restriction on parameters, a Wald-test of equality all 48 short-run coefficients resulting from the MG-estimation strongly suggests (with a *p*-value of .93) that we cannot reject that the short run effects are identical in the MG-specification.

Together, these results support the hypothesis that the long-run relationship is driven by joint causation: inequality and corruption trend together for reasons we turn to next. At the same time, the short run, causal effects work mainly through affecting incentives in the public official's maximization problem, as we have modeled them.[29] The magnitude of these strongly significant incentive effects (though not the underlying values of inequality) may well be the same across all states, and the effect is about 60 per cent larger than that estimated using our standard panel specification above. The standardized effect of the Gini coefficient is -1.2, meaning that a one standard deviation increase in inequality decreases corruption convictions by

[29] Real per capita income and education (% with college degree) also trended strongly upwards in the period we consider. Including these in the long-run relation weakens the long-run result on the Gini coefficient in some specifications, but does not affect the short-run estimates. These two variables are never significant in the long-run relation. Results are available on request.

a half standard deviation, a considerable effect (given the larger standard deviation of the corruption variable due to the log-transformation). The effect of government relative wages is similar to that in the previous specification.

But what about the long run effects, which differ across states? Briefly, we believe they combine attitudes towards government with social and institutional characteristics. The long run coefficients describe the covariance (proportion or disproportion) between changes in corruption and changes in inequality over time. There is one long run coefficient per state, and, for example, the long run coefficients are smaller in states that have larger metropolitan areas. This is because there is more corruption in metro areas, so a unit change in inequality makes less difference there.[30] In the cross-section of states, conditional on metropolitan population, we find that the long run coefficient linking inequality and corruption is positively associated with a state having left-leaning citizens.[31] There a unit change in inequality produces more corruption, perhaps because inequality is itself less variable. But this is describing why corruption increases more and less with respect to inequality in the long run, not why the two move together to begin with. What we can say is that causally, if inequality increases, the immediate effect is that corruption declines, though eventually inequality and corruption will trend together.

IV.4 An alternative measure of relative wages

This section estimates the panel error-correction model using our alternative, or rather composite, measure of relative government wages, where average government wages are compared to income in the bottom decile instead of average private sector wages. To highlight the contribution of this relative government wage measure, we consider six different specifications,

[30] For the years that we have data, metropolitan population share within the state essentially doesn't move, so in our dynamic model its effect is picked up by state fixed effects.
[31] "Left-leaning", is citizen ideology, accessed at http://www.uky.edu/~rford/Home_files/page0005.htm (Berry et al. 1998). See the Appendix for details.

all based on the MG-estimator: Parsimonious and full specifications including the Gini-coefficient and the relative government wage measure on their own and combined. The results are presented in Table 3.

[Table 3 about here]

The top panel reports results from the parsimonious specifications. The first specification (1) is identical to specification (3) reported in Table 2, and included for comparison. Specification (2) includes the relative government wage on its own: The long run effect is positive and weakly significant, while the short run effect, as expected, is negative and strongly significant. This pattern is confirmed in the third specification, where both measures are included. The coefficients remain almost unchanged. The pattern continues in the bottom panel, where the additional explanatory variables are included, except in this case the long run effect of relative government wages ceases to be significant. The estimated short run effects of this definition of government relative wages are reasonably large: Based on specification (6) of Table 3, a one standard deviation increase in the relative government wage measure decreases corruption convictions by 0.6 standard deviations. The remaining coefficients are unchanged by the inclusion of the relative government wage variable.

In additional results (not reported), we explored the relative contributions of real average government wages and real bottom decile income: While real average government wages are almost significant, the main short-run effect seems to come from income levels at the bottom decile: When such incomes increase, corruptions convictions increase significantly in the short-run, as the expected income loss due to terminated employment decreases, altering the corruption calculus. Finally, we note that replacing income in the bottom decile with income in the second or third deciles yields qualitatively similar effects, but these are generally insignificant, as was the relative wage measure based on average private sector wages reported in Table 2.

IV.5 Further variables and robustness considerations

<u>Additional potential determinants of corruption</u>

There are, as noted above, many potential variables that could influence, or be associated with, corruption. Here, we briefly comment on a limited selection, especially those shown to be associated with corruption in (cross-sectional) analysis of US states. Generally, including these additional variables has no effect on our other estimates. An interesting result is that, in contrast to existing findings in Glaeser and Saks (2006) and Maxwell and Winters (2004), the proportion of the state population that is Black is *negatively* related to corruption, but with a point estimate less than its standard error. However, this variable has a skewed distribution. Taking the log of the proportion Black, this transformed variable is weakly significant. The sign reversal relative to existing research clearly comes from the inclusion of state fixed effects. Repeating this robustness exercise for the panel ECM specification produces similar results.

If our conjecture about the calculation of job prospects is correct, it also ought to be the case that in good (bad) times, when it would be easier (harder) to find alternative employment, the effects of inequality and relative wages should be larger (smaller), other things equal. For a simple test of this we ran the model of Table 1, col. 4 in periods separated according to whether the state's unemployment rate was below (good times) or above (bad) it's average for the state. The coefficients of both variables were larger in good times, and the difference was statistically significant in the case of relative wages. This lends further support to our interpretation.

We also examined whether the increasing share of income controlled by the richest decile, shown in Figure 2, is a cause of corruption, either jointly with or instead of inequality and relative government wages. The evidence does not support this in any way. In the standard panel data model, top decile income enters with a negative sign, but is smaller than its standard error. In the dynamic panel (error-correction) model, it enters with a positive sign but remains insignificant, without altering results for inequality and relative government wages. Further, like top decile incomes, we find no evidence that ease of funds, measured by per capita federal grants and/or tax and expenditure limits, have significant effects on corruption, and the same holds for measures of fiscal transparency. Finally, various state level measures of ideology and partisanship, which could be correlated with inequality, were never significant and did not affect other estimates. Political variables could affect convictions, though, through politicization of the prosecution decision; we turn to this next.

Politics in the short run: politicization

Richard Posner (blog) wrote 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."

In the corruption convictions data we study, referrals mostly pass through the offices of U.S. Attorneys. The recent controversy over U.S. Attorney firings reminds us that their appointment and behavior may be politicized.[32] We lack data for a systematic investigation, but we can look for evidence that changes of partisanship across Presidential administrations alter prosecutorial effort in the way Posner and Gordon suggest.

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[32] As Gordon (2008) shows, if corruption convictions are "political" and politics is partisan, U.S. Attorneys in Republican (Democratic) administrations will have more desire to "go after" elected Democrats (Republicans) and/or their bureaucratic agents. This has two effects: it inhibits the choice of corruption by the "target" but increases pursuit of the case (conditional on corruption being chosen) by the "enforcer". There are conditions under which each effect dominates, but,the comparative statics of Gordon's model are for sentence severity. We also lack the partisan affiliation of the convicted official in most cases, though biographical research could in principle determine the partisanship of the President appointing the judge in each case in the Tracfed (post-1986) data.

There is indeed a little such evidence, but very little. We use Berry et al.'s (1998) institutional ideology (updated to 2006) as a measure of broad state-level political orientation: the question is whether after Bill Clinton (George W. Bush) became President, the volume of cases referred or pursued decreased (increased) disproportionately in more liberal areas. We base the investigation on comparisons of the two years before and after the transition.[33] First we look at referrals or cases processed in the Clinton transition: relative to 1991-92, more liberal areas saw significantly *more* cases processed in 1993-94. This is the opposite of what would be expected if detection and referral were politicized. Once a case is referred, of the cases processed in any year, U.S. Attorneys choose to file charges in some and decline to in others: think of this ratio of (charges filed/cases processed) as reflecting prosecutorial "effort". In the case of the Clinton administration, U.S. Attorneys in 1993-94 filed slightly more cases relative to 1991-92, but not nearly as many more as were referred, so effort in 1993-94 (relative to 1991-92 in the same locale) certainly 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.[34]

The Bush transition reveals similarities and differences. Referrals are disproportionately (and significantly) up in liberal areas, as they were eight years earlier, though this time the result appears consistent with partisan politicization. However, U.S. Attorneys do not respond with more filings where there are more cases, so effort appears to decline where (in ideological terms) it should increase. Of course, perhaps 2001-2 are not good years to study: maybe investigations

[33] Ideology data was accessed at http://www.uky.edu/~rford/Home_files/page0005.htm. We lack data for the Carter-Reagan transition, since the Tracfed data begin in 1986. The section summarizes results from a variety of specifications involving different lags and time periods, changes in Congressional as well as Presidential control, and alternative measures of local partisanship.

[34] However, we cannot reject the counterhypothesis that workload expanded faster than capacity to file charges, leading to the apparent decline in effort.

increased for reasons unrelated to domestic corruption. By 2003-4 the ideological bias has disappeared from cases referred, relative to 1999-2000, but charges filed go up, and so prosecutorial effort appears politicized in exactly the way a reader of Posner would expect.[35] In any case, we draw three inferences from this investigation: first, though teasing out better data would involve a lot of work, it might well be worth it;[36] second, the results are tantalizing but mixed; and finally, the mixed results make it unlikely that our model is fundamentally misspecified because it omits partisan political factors.

Alternative variable definitions and specifications

In their cross-sectional analysis, Glaeser and Saks (2006) adjust the number of corruption convictions by state population instead of the number of elected officials. If we do the same, we get qualitatively similar results in both the standard and dynamic panel specifications, except for population as an independent variable. If anything, the results for inequality are stronger using this adjustment. The ranking of states shown in Figure 1b changes somewhat using this alternative deflator, but the overall grouping of states does not.

As a robustness check, we also consider Langer's (1999) annual estimates of state Gini coefficients of household income from 1976 to 1995 using CPS data. The Langer estimates do not quite cover our sample period. They are very volatile, and Galbraith and Hale report that the average time series correlation for states between their measure that uses industrial sector data and Langer's is .58. Nevertheless, the effect of inequality remains negative, and is weakly significant in the parsimonious dynamic panel data specification, but not significant when

[35] The coefficient of the interaction term is 1.9 times its standard error. However, any apparent ideological bias in effort fades out in 2005-6. Maybe that is why some U.S. Attorneys were subsequently fired!

[36] Some states (for example, Illinois or New York) contain multiple judicial districts with very different political characteristics, so further disaggregation might pay off.

including all controls. However, if we use a Gini coefficient derived from the same source as the bottom decile income (www.inequalitydata.org), inequality is negative and strongly significant.

Experimentation with sub-period estimation in the dynamic panel data specification reveals no important qualitative changes regarding the short run results if the earlier period (years before 1991) is considered separately. Many of the effects, including long-run results on inequality, reduce or flatten out if only more recent years (since 1983) are considered. Experimentation with lags on the decision variables reveals that lags of one or two years to the Gini coefficient make little difference, but the government relative wages variable is less stable.

V. Concluding Summary

Dramatically better data on state-level inequality and incomes, the use of panel data on corruption convictions, and careful attention to time dynamics as well as the consequences of including or excluding state fixed effects in the panel specification allow us to estimate the impact of income considerations on the decision to undertake corrupt acts. Our main findings from panel data regressions show that higher income inequality and higher public relative to average as well as lower-than-average private wages (a better outcome if the agent stays on the right side of the law) both *deter* corruption, at least in the short run. Following efficiency wage arguments, we show that in the 48 contiguous states over nearly a quarter of a century, where average government wages were higher relative to private wages, so that public officials had more to lose in lifetime income terms by losing their jobs, corruption was lower. At the same time, where local inequality was higher, corruption was also lower, other things equal, because those convicted of corruption faced poorer prospects from probably re-entering private life toward the bottom of the income scale. We do not interpret that as a defense of inequality. Rather, we see it as a caution to those who might have believed that reducing inequality would reduce corruption: our finding reinterprets and in some ways reverses an established literature on the usually-assumed-to-be-positive relationship between inequality and corruption. We argue that this positive association arises from joint causation by unobserved factors rather than because of a theoretical relationship in which higher inequality causes more corruption.

That higher government wages relative to average private sector wages deters corruption was known from the cross-national literature (e.g. Van Rijckeghem and Weder, 2001), but the results for both inequality and lower-than-average wages are novel and stronger, and turn out to be the result of paying attention to time dynamics and state fixed effects rather than some special features of the data. While we observe in any given year a positive relationship between inequality and corruption convictions, accounting for time trends or year effects and, most importantly, state fixed effects, results in the opposite conclusion, with more inequality leading to fewer corruption convictions. These results hold up independently of other political and economic factors that affect corruption in American states, like the effects of checks and balances described in Alt and Lassen (2008), and the results are statistically significant and reasonably sizeable. Additional empirical analyses based on panel error-correction models confirm this pattern. In the short run, higher wage inequality decreases corruption convictions, but in the long run, inequality and corruption have a positive co-movement, possibly due to joint causation from factors such as citizens' views of government.

At the same time this result fits comfortably into the earlier literature in that many past findings continue to hold. Income and education reduce, and population and fiscal scale increase corruption. Divided government, at least in its party control of separated branches form, appears associated with lower corruption despite all these other consideration. Term limits, often held responsible for poor political performance, appear to be associated with higher corruption. Our results also suggest how consequences of omitted variable bias may explain some other discrepancies in the findings among papers. For example, bear in mind that we estimate negative effects on corruption from income and government relative wages, but a positive effect for government expenditure (fiscal scale). Since income is generally positively correlated with fiscal scale, omitting fiscal scale from a corruption regression (as many do) could bias the estimated income effect toward zero. Similarly, if a higher relative government wage increases the scale of government (likely), omitting the government relative wage variable could bias the estimated fiscal scale effect toward zero. For these and other possible determinants, like the mixed results on politicization, opportunities to do more research await.

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: See main text for definitions, sources and explanations.

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.[37] 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: 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.

[37] 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)

Racial composition: Data up to 1990 was provided by Tim Besley, updated with annual estimates from the Joint Center for Political and Economic Studies. See for instance http://www.jointcenter.org/.

Fiscal transparency: Alt, Lassen and Rose (2006).

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.

[Table A.1 about here]

Table A.1: Summary statistics

	N	Mean	SD	min	max
Raw number of convictions	1088	14.89	22.29	0	155.00
Log(number of convictions per 10,000 elected officials)	1103	-3.14	2.50	-9.86	0.76
Gini coefficient	1104	0.39	0.02	0.33	0.48
Government wages relative to average wage	1104	0.97	0.07	0.78	1.17
Government wages relative to bottom decile income	1104	1.03	0.16	0.65	1.61
Divided government	1104	0.45	0.50	0	1
Real income per capita (1,000 $)	1104	23.32	4.05	14.76	40.59
Real government expenditures per capita (1,000 $)	1104	4.08	0.89	2.40	7.37
College educated, per cent	1104	18.94	4.79	10.30	34.98
Population, millions	1104	5.07	5.30	0.41	33.15
Binding one-term limit	1104	0.06	0.24	0	1
Binding two-term limit	1104	0.20	0.40	0	1
Log(number of convictions per million population)	1088	-1.00	4.44	-11.51	3.09
Black population, per cent	1104	0.11	0.09	0	0.37
Real per capita federal grants received	1104	835.73	230.02	443.87	1958.29
Tax and expenditure limits	1104	0.39	0.49	0	1
Fiscal Transparency index	1104	0.45	0.19	0	1
Share of income received by top decile	1104	0.36	0.04	0.28	0.51

Table 1: Determinants of corruption convictions per 10,000 elected officials, 1977-1999

	Pooled OLS	Year FEs	State FEs	State and Year FEs
	(1)	(2)	(3)	(4)
Gini index	41.758	34.998	8.879	-26.999
	[2.634]***	[3.257]***	[11.277]	[11.585]**
Relative government wages	2.424	0.777	6.315	-5.038
	[1.078]**	[1.118]	[2.545]**	[3.914]
Divided government	-0.311	-0.385	-0.129	-0.194
	[0.130]**	[0.129]***	[0.142]	[0.124]
Real per capita income (1000$)	0.194	0.190	-0.026	-0.147
	[0.033]***	[0.034]***	[0.085]	[0.086]*
Real per capita gov expentitures (1000$)	-0.420	-0.419	0.252	0.511
	[0.112]***	[0.115]***	[0.313]	[0.297]*
Percent college graduates or higher	-0.091	-0.129	0.072	-0.036
	[0.029]***	[0.033]***	[0.056]	[0.083]
Population (millions)	0.076	0.090	0.053	0.138
	[0.010]***	[0.011]***	[0.084]	[0.071]*
Binding one-term limit	1.129	1.315	0.760	0.995
	[0.218]***	[0.251]***	[0.532]	[0.427]**
Binding two-term limit	0.529	0.555	0.305	0.305
	[0.151]***	[0.150]***	[0.206]	[0.191]
Year effects and time trend	No	Yes***	No	Yes***
State fixed effects	No	No	Yes***	Yes***
Observations	1103	1103	1103	1103
R-squared	0.31	0.37	0.06	0.19

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
Significance levels for fixed effects relate to test of joint significance.
A constant was included in all regressions, but results are not reported.
Calculation carried out in Stata 10.0 using reg and xtreg.

Table 2: Dynamic panel error-correction estimates of corruption convictions and inequality

	DFE	PMG	MG
	(1)	(2)	(3)
Long-run coefficient (λ)	28.23 ***	20.45 ***	55.01 ***
	(5.07)	(2.04)	(13.13)
Adjustment parameter (ϕ)	-0.83 ***	-0.81 ***	-0.91 ***
	(0.03)	(0.04)	(0.03)
Short-run coefficient (β): Gini	-29.18 **	-16.31	-44.07 ***
	(14.16)	(15.99)	(14.50)
p-value, Hausman test against MG estimator[1)]	0.002	0.015	
No. of states	48	48	48
No. of observations	1054	1054	1054
	DFE	**PMG**	**MG**
	(4)	(5)	(6)
Long-run coefficient (λ)	13.25 **	13.84 ***	36.56 ***
	(6.06)	(2.05)	(12.29)
Adjustment parameter (ϕ)	-0.84 ***	-0.95 ***	-1.05 ***
	(0.03)	(0.04)	(0.04)
Short-run coefficients (β):			
Gini	-29.07 **	-26.52	-48.55 **
	(14.55)	(20.19)	(19.99)
Relative government wages	-3.90	-4.71	-5.09
(to average wages)	(4.08)	(6.31)	(6.51)
Divided government	-0.22	-0.54 *	-0.45
	(0.15)	(0.32)	(0.31)
Real income per capita	0.26 *	0.61 ***	0.52 **
	(0.15)	(0.23)	(0.25)
Real gov. exp. per capita	0.45	0.52	0.42
	(0.60)	(0.90)	(0.90)
College educated	-0.58 ***	-1.03 **	-1.48 **
	(0.17)	(0.42)	(0.72)
Population (mill.)	0.02	-26.17 **	-21.32 **
	(0.55)	(12.42)	(9.91)
p-value, Hausman test against MG estimator[1)]	0.095	0.142	
No. of states	48	48	48
No. of observations	1054	1054	1054

1) H0: No difference between restricted (DFE and PMG) and unrestricted (MG) model. See text for details.

A constant was included in all regressions, but results are not reported.

***, **, * denote significance at 1%, 5% and 10% levels, respectively.

Calculation carried out in Stata 10.0 using xtpmg.

Table 3: Dynamic panel error-correction estimates of corruption convictions, inequality and relative government wages

	MG (1)	MG (2)	MG (3)
Long-run coefficient (λ)			
Gini	55.01 ***		64.03 ***
	(13.13)		(14.21)
Relative government wages		3.51 *	3.19 *
(to lowest decile income)		(1.82)	(1.93)
Adjustment parameter (ϕ)	-0.91 ***	-0.86 ***	-1.03 ***
	(0.03)	(0.03)	(0.03)
Short-run coefficient (β)			
Gini	-44.07 ***		-57.03 ***
	(14.50)		(14.09)
Relative government wages		-4.18 **	-4.79 **
(to lowest decile income)		(2.04)	(2.29)
p-value, Hausman test against PMG estimator1)	0.015	0.500	0.030
No. of states	48	48	48
No. of observations	1054	1054	1054
	MG (4)	**MG** (5)	**MG** (6)
Long-run coefficient (λ)			
Gini	46.00 **		44.33 **
	(15.94)		(13.13)
Relative government wages		1.89	2.78
(to lowest decile income)		(2.77)	(2.98)
Adjustment parameter (ϕ)	-1.05 ***	-1.04 ***	-1.16 ***
	(0.04)	(0.04)	(0.04)
Short-run coefficients (β):			
Gini	-57.10 ***		-63.23 ***
	(17.40)		(24.46)
Relative government wages		-11.30 ***	-9.26 **
(to lowest decile income)		(3.75)	(4.14)
Divided government	-0.49	-0.51	-0.27
	(0.30)	(0.32)	(0.30)
Real income per capita	0.52 **	0.49 **	0.33
	(0.25)	(0.24)	(0.27)
Real gov. exp. per capita	0.42	1.18	1.27
	(0.87)	(1.11)	(1.31)
College educated	-1.32 **	-1.24 ***	-1.40 ***
	(0.64)	(0.34)	(0.61)
Population (mill.)	-23.02 **	-12.42	-11.12
	(10.62)	(13.43)	(12.95)
p-value, Hausman test against MG estimator1)	0.059	0.853	0.300
No. of states	48	48	48
No. of observations	1054	1054	1054

1) H0: No difference between restricted (PMG) and unrestricted (MG) model. See text for details.
A constant was included in all regressions, but results are not reported.
***, **, * denote significance at 1%, 5% and 10% levels, respectively.
Calculations carried out in Stata 10.0 using xtpmg.

Figure 1a: Corruption convictions by year, 1977-1999

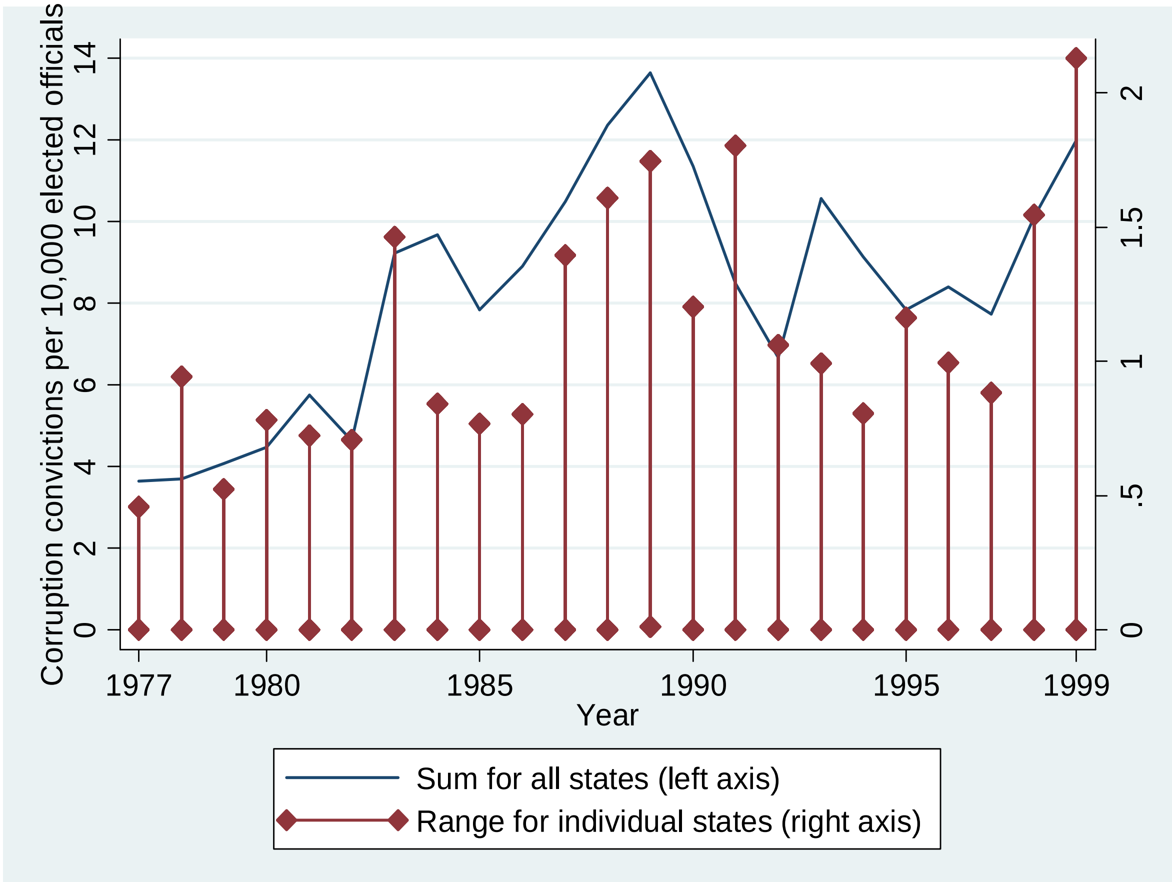

Figure 1b: Corruption convictions by state, 1977-2000

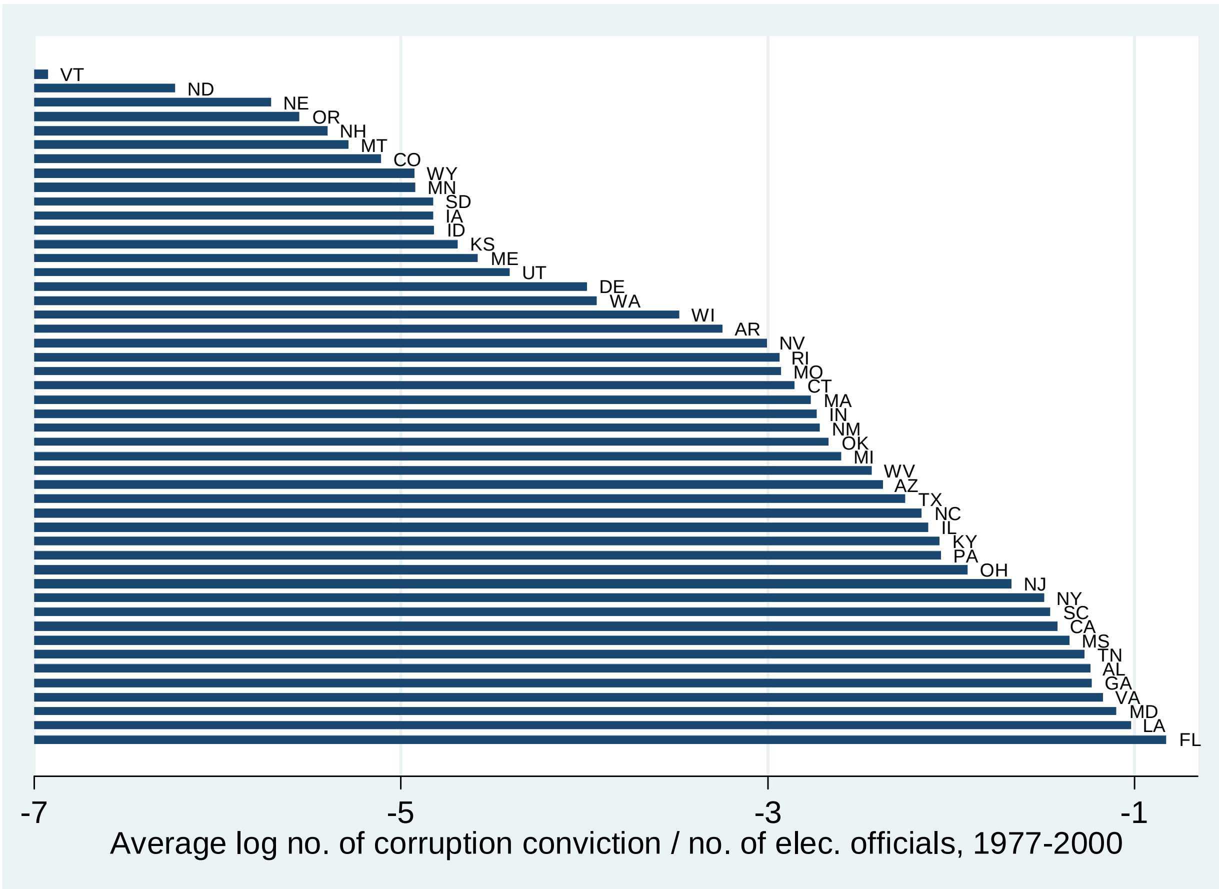

Figure 2: Range and average of Gini coefficients and top decile income shares, by year

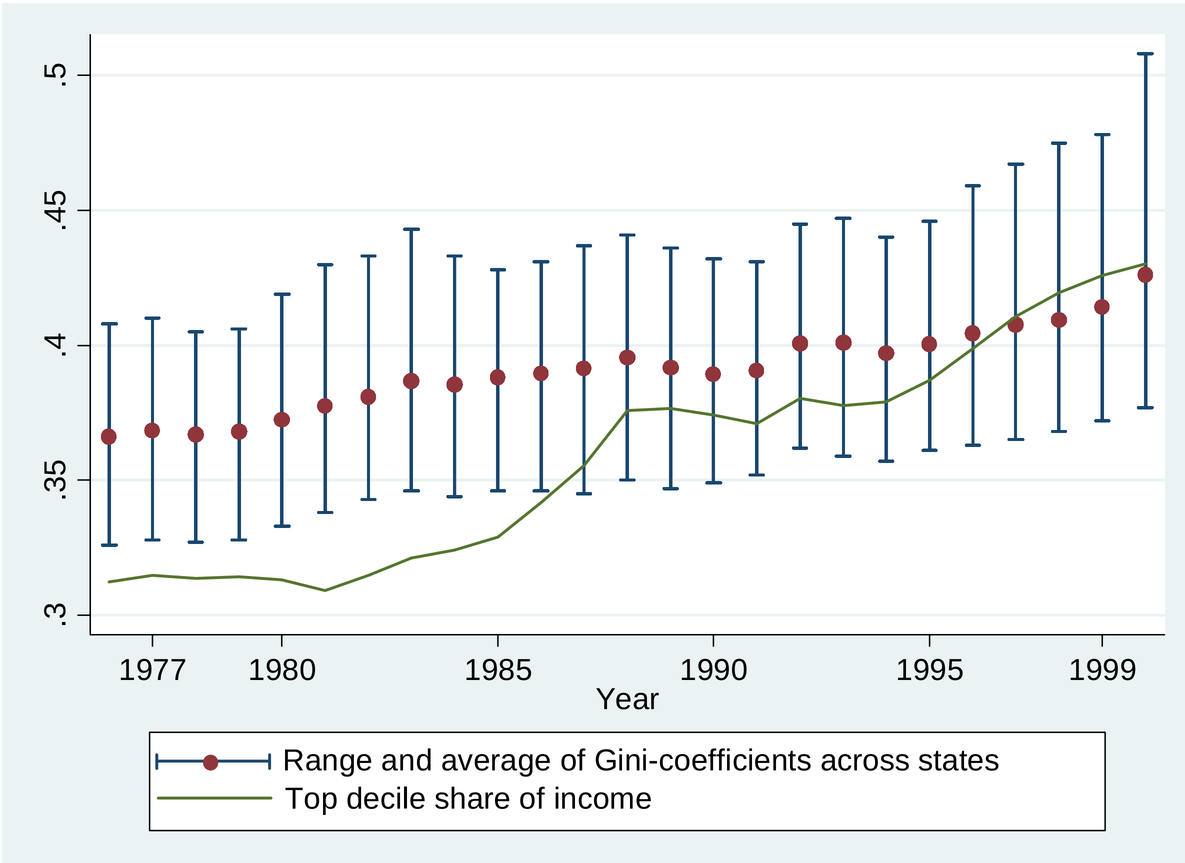

Figure 3: Ratio of average state and local government wage to the average wage and lowest decile total income, by year

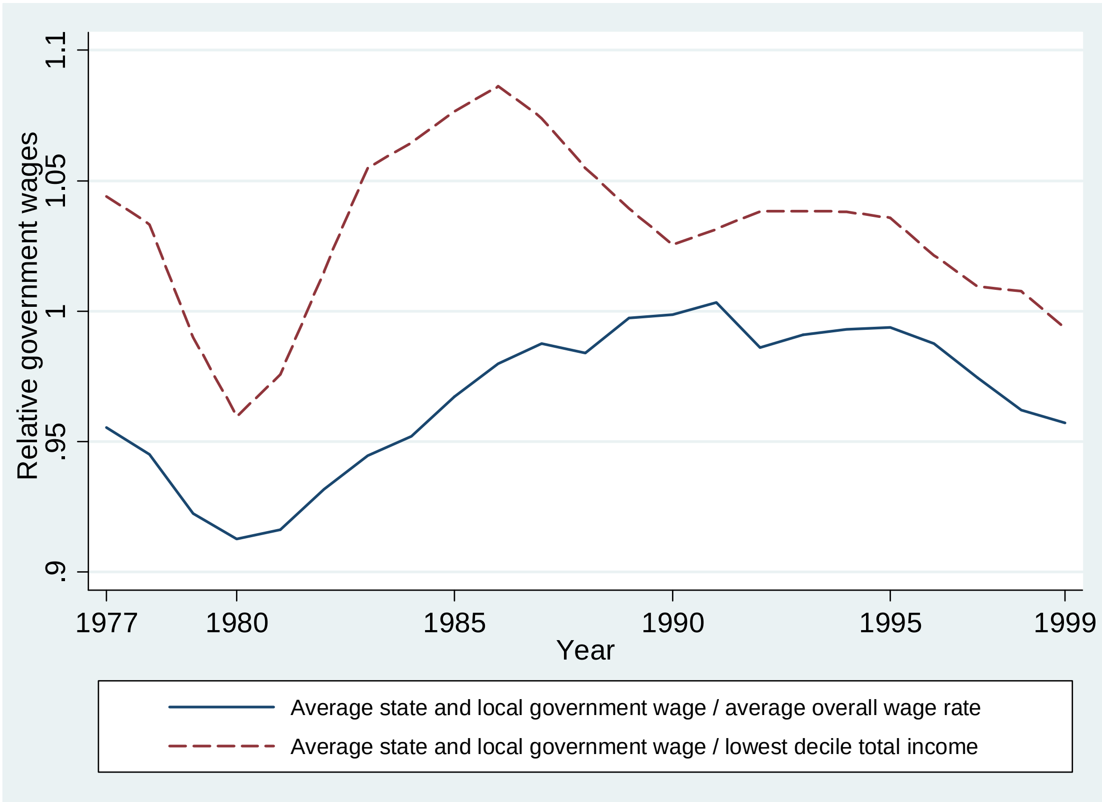

Figure 4 Convictions and Inequality, annual averages

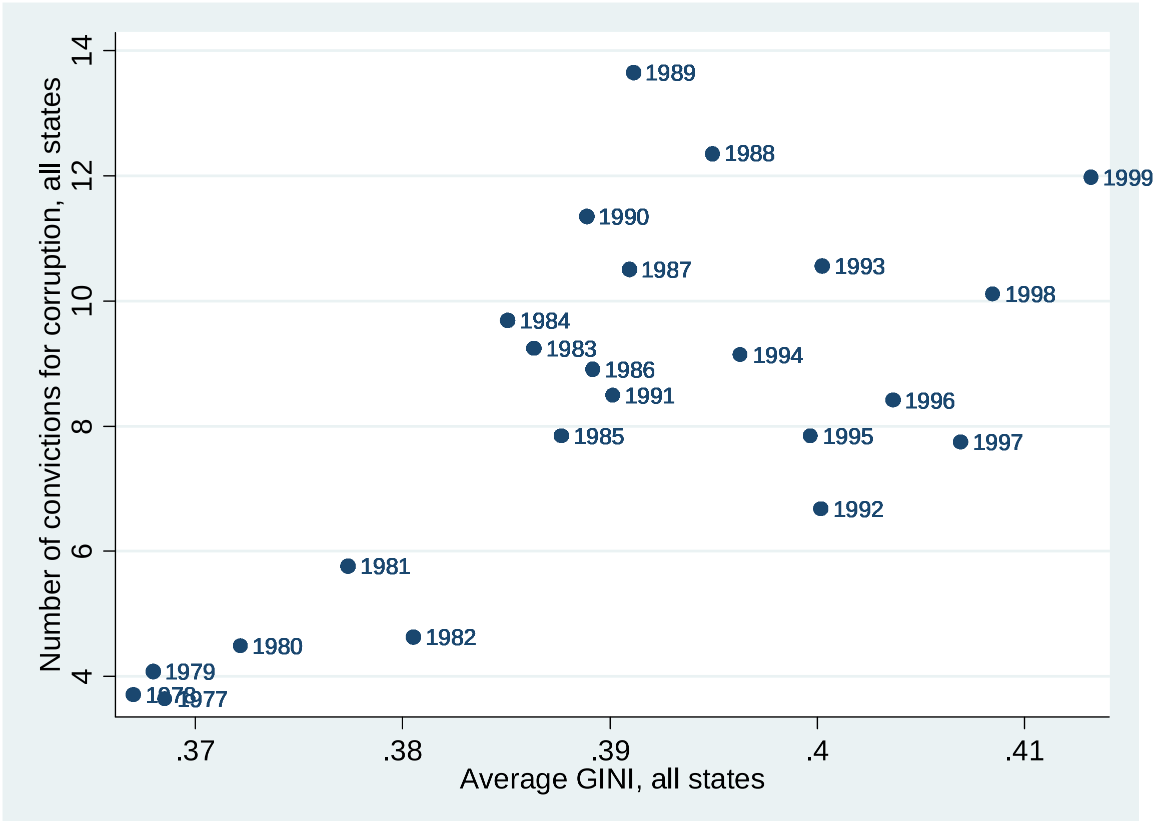

Figure 5: Within-state regressions of detrended convictions on inequality