

WHAT HAVE WE LEARNED ABOUT THE CAUSES OF CORRUPTION FROM TEN YEARS OF CROSS-NATIONAL EMPIRICAL RESEARCH?

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■ **Abstract** I review recent efforts by political scientists and economists to explain cross-national variation in corruption using subjective ratings, and examine the robustness of reported findings. Quite strong evidence suggests that highly developed, long-established liberal democracies, with a free and widely read press, a high share of women in government, and a history of openness to trade, are perceived as less corrupt. Countries that depend on fuel exports or have intrusive business regulations and unpredictable inflation are judged more corrupt. Although the causal direction is usually unclear, instrumenting with income as of 1700 suggests higher development does cause lower perceived corruption. However, controlling for income, most factors that predict perceived corruption do not correlate with recently available measures of actual corruption experiences (based on surveys of business people and citizens that ask whether they have been expected to pay bribes recently). Reported corruption experiences correlate with lower development, and possibly with dependence on fuel exports, lower trade openness, and more intrusive regulations. The subjective data may reflect opinion rather than experience, and future research could usefully focus on experience-based indicators.

INTRODUCTION

In recent years, a growing community of political scientists and economists has sought to understand why in some countries governments and the bureaucrats they control are more corrupt than in others. Corruption is usually understood to mean the “misuse of public office for private gain,” where the “private gain” may accrue either to the individual official or to groups or parties to which he belongs (e.g., Bardhan 1997). The quintessential corrupt transaction envisioned is the gift of a bribe by a private citizen to a public official in return for some service that the official should either provide for free (e.g., registering a firm) or not provide at all (e.g., inside information). Scholars—like novelists, business people, and other global travelers—have long taken it as given that such exchanges occur more

frequently in some countries, such as Indonesia or Nigeria, than in others, such as Canada or Iceland.

About a decade ago, attempts to measure and explain such differences received a dramatic boost from two events. First, the Berlin-based organization Transparency International, which campaigns for honest government, began constructing a summary index of countries' "perceived corruption" in the hope of embarrassing their leaders into reform. Second, at around the same time, an economist reported evidence of a long-suspected link between higher corruption and slower economic growth, based on a similar cross-national rating of corruption compiled by a business consultancy (Mauro 1995). Soon others were using such data to investigate why some countries' governments were perceived to be more corrupt than others.¹

In this article, I survey the first decade or so of such work. There is already too much to cover comprehensively within the space limits; Lambsdorff (2005) provides a useful review. Instead, I focus on a few key questions—about the reliability of the data, methods of analysis, and the robustness of certain results. Where possible, I use the data themselves to resolve open issues. The goal is to provide a guide to readers of this literature who do not have the relevant data on their hard drive, and to sum up what is known and what is not at the end of this first phase of data exploration. Of course, there are various paths one could follow through this body of work, and different scholars might draw different conclusions on key points. I encourage interested readers to take their own look at the data, which are posted on my website at <http://www.polisci.ucla.edu/faculty/treisman/>.

Examination of these data reveals a puzzling dichotomy. The more subjective indexes of perceived corruption—based on evaluations of experts and opinions of business people and citizens—turn out to be highly correlated with a variety of factors that are commonly believed to cause corruption. Perceived corruption, as measured by such indexes, is lower in economically developed, long-established liberal democracies, with a free and widely read press, a high share of women in government, and a history of openness to trade. It is higher in countries that depend on fuel exports or have intrusive business regulations and unpredictable inflation. These factors explain up to 90% of the cross-national variation. However, measures of actual corruption experiences, based on surveys that ask business people and citizens in different countries whether they have been expected to pay bribes recently, correlate with hardly any of these factors once one controls for income. Reported experience with corruption correlates with lower development, and

¹For early examples, see Ades & Di Tella (1997), La Porta et al. (1999), and Treisman (2000). The growing attraction of corruption studies can be gauged from a quick search of the article database JSTOR. Searching its political science and economics journals for the word "corruption" resulted in 1945 hits from the 1970s, 2277 from the 1980s, and 3278 from the 1990s. Searching for uses of "corruption" and "statistics" in the same text yielded 419 cases from the 1970s, 481 from the 1980s, and 779 from the 1990s.

possibly with dependence on fuel exports, lower trade exposure, and more intrusive regulations. It is possible that the experience-based measures are noisier and less reliable or are measuring a different phenomenon (petty as opposed to grand corruption, perhaps). But it could also be that the widely used subjective indexes are capturing not observations of the frequency of corruption but inferences made by experts and survey respondents on the basis of conventional understandings of corruption's causes. I conclude that the challenge of the next wave of research will be to refine and gather more experience-based measures of corruption and to examine the patterns they reveal.

THE NEW DATA

Main Sources

Two indexes of perceived corruption have become the most commonly used in empirical work—the Corruption Perceptions Index (CPI) constructed by Transparency International (TI) and a rating of control of corruption published by a team led by Daniel Kaufmann at the World Bank (WB). (Details are available at http://www.transparency.org/policy_research/surveys_indices/cpi and <http://www.worldbank.org/wbi/governance/data>.) Both of these aggregate information from a number of sources that include country risk ratings produced by business consultancies, surveys of international or domestic business people, and polls of country inhabitants. Both groups aim to reduce measurement error by averaging different sources, and use similar (and overlapping) sets of inputs. Their methods for constructing indexes differ in two main regards. TI essentially averages the standardized values while adjusting to reduce the sensitivity of the index to changes in the surveys and countries included. The WB team treats the individual ratings as noisy measures of a common latent variable and estimates this using an unobserved components model (see, e.g., Kaufmann et al. 2006). Whereas the WB team includes all countries for which one component rating is available, TI only includes countries for which three ratings are available, which probably increases precision but leads to narrower country coverage.

TI has calculated ratings annually since 1995. The WB group produced its indexes biannually from 1996 to 2002 but now publishes new versions yearly. Coverage has increased over time—from 54 countries in 1996 to 159 in 2005 for TI, and from 152 to 204 for the WB group during the same period. Despite the different methodologies and sometimes different sources, the two ratings are extremely highly correlated. In the years for which both have been available, the correlation has varied between $r = 0.96$ in 2002 and $r = 0.98$ in 2004. Both groups also publish the number of sources on which each country's rating is based and estimates of the precision of each country's rating. In TI's case, these standard errors are (since 2001) calculated by a bootstrap method. In 2005 the standard error was just 0.15 for Liberia, suggesting a great deal of agreement about the

country's corruption level, but 1.97 for Laos, suggesting far more disagreement. WB publishes standard errors from its unobserved components estimation, which decrease with the number of sources available for the given country and with the extent of correlation between these sources and others across all countries.

Although the TI and WB perceived corruption indexes are the most frequently used, a number of studies have analyzed a third cross-national corruption rating produced by the firm Political Risk Services (PRS), based on evaluations by its network of experts and published in its *International Country Risk Guide* (ICRG). Unlike the TI and WB data, these are only available for purchase. The main attraction of the ICRG data is that ratings are available for all years since the early 1980s, which has tempted some to use them for cross-section time-series analyses. The ICRG data have been used in the construction of the WB index, and not surprisingly they are highly correlated with both TI and WB (the latest ICRG data I had were for 1995; these correlated at $r = 0.88$ with the 1996 TI index and at $r = 0.84$ with the 1996 WB rating).

All three of these measures are based on the subjective evaluations of experts or survey respondents of how widespread or costly corruption is in particular countries. More recently, certain other surveys have asked respondents about their own (or their family's or firm's) experience. TI conducted a "Global Corruption Barometer" (GCB) survey of inhabitants of 64 countries in 2004 and 69 countries in 2005. One survey question asked whether the respondent or anyone in his or her household had paid a bribe in any form during the previous 12 months. In the late 1990s, the United Nations Interregional Crime and Justice Research Institute (UNICRI) conducted an international survey of crime victims, which asked respondents whether during the previous year "any government official, for instance a customs officer, police officer or inspector" had asked or expected the respondent to pay a bribe for his services. UNICRI published the frequencies with which respondents answered positively in different countries. Some surveys have also asked business managers to estimate the proportion of annual revenues that "firms like theirs" typically pay in bribes or unofficial payments. The World Bank's World Business Environment Survey (WBES) interviewed managers in more than 10,000 firms in 80 countries during late 1999 and early 2000. Respondents were read the sentence: "It is common for firms in my line of business to have to pay some irregular 'additional payments' to get things done," and were asked whether this was true always, mostly, frequently, sometimes, seldom, or never. Similar questions have been included in the World Bank and European Bank for Reconstruction and Development's Business Environment and Enterprise Performance Survey on the transition economies. Wording is often chosen to reduce respondents' fears of self-incrimination—e.g., questions concern "firms like yours"—but it is hoped and assumed that respondents reply based on their own experience. (For details, see http://www.transparency.org/policy_research/surveys_indices/gcb, <http://www.unicri.it/wwd/analysis/icvs/index.php>, and <http://info.worldbank.org/governance/wbes/#wbes>.)

How Good or Bad are the Data?

Although research using these data has now been published in the leading journals in political science, economics, and sociology, their reliability has been questioned from the start. A number of issues have been raised (see, e.g., Knack 2006).

First, and most obviously, the data do not measure corruption itself but only opinions about its prevalence. Such opinions may not be based on any direct knowledge and could be biased. Cross-national differences could reflect differences in the socially encouraged level of cynicism, the degree of public identification with the government, and the perceived injustice of social or economic relations. This is particularly problematic because researchers have used the data to make arguments about the relationships between social trust, inequality, and democracy, on the one hand, and corruption on the other. Likewise, opinions about the extent of corruption might reflect the frequency of muck-raking media reports, of government anticorruption campaigns, or of politically motivated accusations by opposition politicians. Ratings by international business people and experts, disproportionately drawn from developed Western countries, might be influenced by Western preconceptions or by the raters' greater familiarity with certain cultures. Some of the organizations that prepare corruption ratings might also have ideological axes to grind. For all these reasons, "perceived corruption" may reflect many other things besides the phenomenon itself.

Other concerns focus on the aggregation of sources by TI and WB. Some technical issues relate to which methodology is more appropriate, but since the resulting indexes are so highly correlated I ignore them here. The main difference in early years was the far broader country coverage in the WB data—a plus if increasing coverage is important but a minus if precision is prioritized. However, some argue that the individual sources used in constructing each index are measuring different things, which makes their aggregation problematic. Some sources are evaluations by Western experts; others are polls of international business people or country inhabitants. Some surveys ask about the frequency of bribes, some about their size, others about the burden imposed on the economy or about the relative seriousness of the problem. Some focus on low-level administrative corruption; others also seem to include political dirty tricks. Some have narrow regional coverage, and others are worldwide in scope. As a result, skeptics wonder what exactly the average is measuring, and suggest that selecting specific components to match the particular problem at hand makes more sense than using an index. There is a tradeoff between reducing measurement error (if that is what causes surveys to differ) and reducing precision (if surveys are accurately describing distinct phenomena).

Even if precisely the same survey question were asked of the same target group in different countries, it would probably be interpreted differently. What respondents think of as corruption may vary substantially from place to place. Even if the word is defined for respondents, they may have different views about what constitutes "misuse" of public office. And the "seriousness" or "costliness" of corruption

will be evaluated relative to different baselines and beliefs about what is normal and what is “serious.” Having to pay officials 5% of profits might seem a huge imposition in the United States but a relatively minor inconvenience in a country with vast mineral wealth, unpredictable inflation, and rampant organized crime. So, besides worrying about comparability across surveys, critics worry about the comparability of responses across countries. (Local differences in the interpretation of “corruption” or evaluations of its “seriousness” are not a problem for the expert assessments, and some cross-national surveys phrase questions carefully to elicit more clearly calibrated answers.)

Researchers are aware of these concerns (for a good discussion, see Lambsdorff 2004) and have responded in several ways. First, some point—not unreasonably—to the lack of alternatives. Especially when this work began, there were few obviously more reliable sources of cross-national data about what all agree is a vitally important subject. One could instead study rates of prosecution or conviction for misuse of office. But these are as likely to reflect the zeal, competence, and integrity of the police and judiciary, or the political priority placed on fighting corruption, as they are to capture the true scale of the phenomenon. Some have used inventive proxies to measure corruption in particular contexts. Di Tella & Schargrodsky (2003) found that the prices paid by hospitals in Buenos Aires for homogeneous inputs such as hydrogen peroxide dropped by about 15% during an anticorruption campaign, suggesting the scale of previous price-padding or kickbacks. Golden & Picci (2005) constructed an index of corruption in Italy’s regions by comparing the value of existing infrastructure stocks to past infrastructure spending. Clearly, these approaches would be hard to extend cross-nationally.

More recently, the lack of alternatives has become a somewhat less compelling reason to focus on expert and public opinions about the prevalence of corruption. As noted in the previous section, TI’s GCB surveys, UNICRI’s crime victims survey, and some surveys of business managers such as the WBES now ask respondents about their (or family members’ or similar firms’) own experiences of corruption. Such data are also subject to problems—inaccurate or selective memory, fear of reprisals by authorities, etc. But they are less likely to pick up mere impressions based on gossip or popular prejudices than the more subjective survey questions or ratings.

A second justification for using perceived corruption data turns the point about the variety of component sources on its head. That different ratings produced by different organizations using different methodologies and even slightly different definitions of corruption turn out to be highly correlated among themselves suggests to some that these different spyglasses are aimed at a common target. At least the results are not purely idiosyncratic. TI reports that among the 16 sources used in its 2005 CPI, the average correlation was $r = 0.87$, and the lowest was $r = 0.43$ (Lambsdorff 2005). If ratings reflected Western biases, one would not expect the ratings of foreign experts to correlate so highly with surveys of country residents or of business people working in the country. For instance, the evaluations of the Economist Intelligence Unit’s experts in 2005 correlated at $r = 0.87$ with those of

domestic and international business executives surveyed by the World Economic Forum.

Kaufmann and his colleagues tried to evaluate the extent of ideological bias more systematically (Kaufmann et al. 2003). They compared the country ratings of different organizations' experts to the average responses on the WBES survey of business people from the country in question and studied the divergences. They were interested in whether any organization consistently rated countries more corrupt than indicated by the WBES survey when that country had a left-wing (or a right-wing) government. They found that the Heritage Foundation did rate countries with right-wing governments higher on regulatory quality and rule of law than the surveyed business people did, but they did not find any significant ideological biases on corruption ratings.

An obvious question in this context is how closely the subjective indexes correlate with experience-based indicators. In 2001–2003, Razafindrakoto & Roubaud (2005) surveyed households in eight African countries about their experiences with corruption. They then questioned a panel of experts, including governance specialists and country scholars, drawn from both developed and developing countries. The experts were asked what they thought the surveyed households would report about the frequency of corruption in their country. For each of the countries—from Burkina Faso to Togo—the experts grossly overestimated the level of corruption that households would report: “On average, 13 per cent of the population experienced acts of corruption in the past year, whereas experts expected a figure of 54 per cent. Moreover, only 5 per cent of the public believed bribery to be an acceptable practice, while experts expected a figure of 54 per cent” (Razafindrakoto & Roubaud 2005, p. 292). There was no correlation across the eight countries between the rates of corruption expected by the experts and the rates reported by the public. However, the experts' predictions *were* correlated with the WB corruption rating (at $r = 0.65$). This might be taken as a sign that experts have a quite coherent set of beliefs about the incidence of corruption that bears little resemblance to realities on the ground.

This particular study might, of course, have caught the experts on a bad day or picked up an isolated gap in expert knowledge. What about the correlations between the GCB, WBES, and UNICRI surveys and the WB and TI indexes? These turn out to be quite highly correlated, with correlation coefficients between about 0.6 and 0.8 (see Table 1). Indeed, with the exception of the two years of GCB surveys, which turn out to be highly correlated with each other, the experience-based surveys generally correlate more closely with the TI and WB indexes than with each other. (This may not be surprising in the case of WBES because it is a component in the construction of the WB corruption scores. However, the others do not seem to have been used for this.) A typical pattern is shown in Figure 1, which graphs the WB index for 2005 against the percentage of respondents to TI's 2005 GCB survey who said someone in their household had paid a bribe during the previous year. The correlation between the two indicators is quite strong, $r = -0.69$. However, what is striking is the far greater variance of reported bribe

TABLE 1 Perceived corruption ratings and survey measures of experienced corruption, correlations^a

	GCB ^b 2004	GCB 2005	UNICRI ^b	WBES ^b	WB ^b 2000	WB 2005	TI ^b 2000	TI 2005
GCB 2004	1	0.915 ^c	0.504 ^c	-0.655 ^c	-0.740 ^c	-0.728 ^c	-0.735 ^c	-0.699 ^c
n	62	56	33	44	61	62	54	61
GCB 2005	0.915 ^c	1	0.448 ^d	-0.549 ^c	-0.660 ^c	-0.693 ^c	-0.689 ^c	-0.644 ^c
n	56	68	32	48	67	68	55	66
UNICRI	0.504 ^c	0.448 ^d	1	-0.642 ^c	-0.790 ^c	-0.778 ^c	-0.793 ^c	-0.784 ^c
n	33	32	49	35	49	49	42	49
WBES	-0.655 ^c	-0.549 ^c	-0.642 ^c	1	0.744 ^c	0.665 ^c	0.791 ^c	0.688 ^c
n	44	48	35	80	80	80	62	80
WB 2000	-0.740 ^c	-0.660 ^c	-0.790 ^c	0.744 ^c	1	0.922 ^c	0.978 ^c	0.934 ^c
n	61	67	49	80	187	186	90	158
WB 2005	-0.728 ^c	-0.693 ^c	-0.778 ^c	0.665 ^c	0.922 ^c	1	0.955 ^c	0.970 ^c
n	62	68	49	80	186	204	90	159
TI 2000	-0.735 ^c	-0.689 ^c	-0.793 ^c	0.791 ^c	0.978 ^c	0.955 ^c	1	0.967 ^c
n	54	55	42	62	90	90	90	90
TI 2005	-0.699 ^c	-0.644 ^c	-0.784 ^c	0.688 ^c	0.934 ^c	0.970 ^c	0.967 ^c	1
n	61	66	49	80	158	159	90	159

^aWB and TI indexes measure *absence* of corruption. GCB measures percentage of respondents who said they or a family member had paid a bribe in last 12 months. UNICRI measures percentage of respondents who said they had been asked or expected to pay a bribe by a government official during the last year (late 1990s). WBES is country average answer to question "It is common for firms in my line of business to have to pay some irregular 'additional payments' to get things done." This is true: 1 = always, ..., 6 = never.

^bAbbreviations: GCB, Global Corruption Barometer (Transparency International); UNICRI, United Nations Interregional Crime and Justice Research Institute; WBES, World Business Environment Survey (World Bank); WB, World Bank; TI, Transparency International.

^cCorrelation significant at the 0.01 level (two-tailed).

^dCorrelation significant at the 0.05 level (two-tailed).

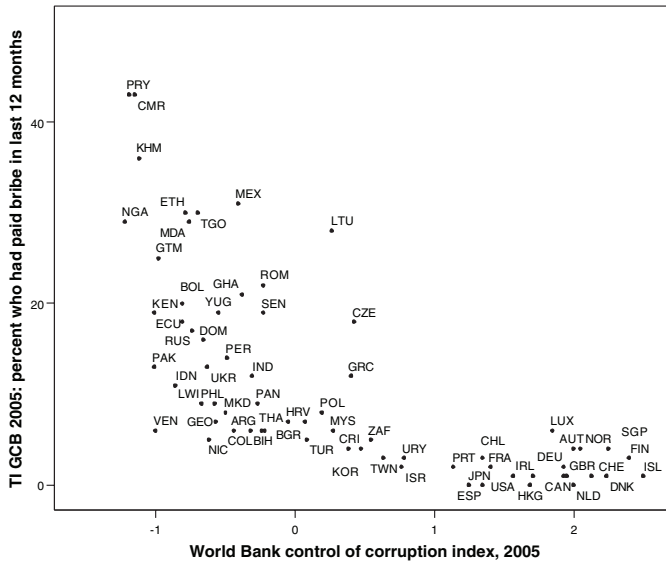


Figure 1 Perceived and reported corruption, 2005. Sources: World Bank, Transparency International.

frequency among the countries that rate low on the WB corruption index. In the developed democracies of Europe and North America, reported bribery is rare and the corruption level is consistently perceived to be low. But among the countries perceived to have the highest corruption, there are some (e.g., Paraguay and the Camerons) where a large proportion of residents report paying bribes, whereas in others (e.g., Venezuela and Nicaragua) very few do so. Although reported bribery may be correlated with the aggregations of expert judgments and surveys, the correlation is not as strong as one might expect, especially among the less developed countries. It is possible that the two groups are measuring different things, or that the survey respondents are not being frank. Or it could be that, on some countries, the experts are way off.

Even among the residents of a country, opinions may not match reported experience (see Weber Abramo 2006). The GCB 2005 survey asked respondents to what extent corruption in their country affected politics, the business environment, and their personal and family life. Answers to these questions correlated quite highly with each other ($r = 0.77, 0.54, \text{ and } 0.49$), but much less ($r = 0.16, 0.18, \text{ and } 0.30$) with responses to the question whether anyone in the respondent's household had paid a bribe during the previous 12 months. In fact, the respondents' evaluations of the effect of corruption on politics, business, and their personal and family lives correlated more closely with the WB index than with the individuals' own reported experience. It is not just external experts whose opinions seem at times disconnected from concrete experience.

This must at least raise questions about the use of perceived corruption data as one of many criteria for establishing countries' eligibility for U.S. aid under the Millennium Challenge Account. It also raises questions about the interpretation of research findings. Even if one concluded that perceived corruption indexes do not reliably measure the actual frequency of corrupt transactions, there would still be reason to study them. Perceptions—even if not matched by reality—can have powerful effects. Mauro (1995) found that perceived corruption reduced growth via its effect on investment but found much weaker evidence of effects going via other paths, such as increased costs or reduced productivity. Other studies have confirmed that higher perceived corruption reduces foreign direct investment. Soares (2004) found that high perceived corruption discourages victims from reporting crimes to the police. So perceptions clearly matter. Nevertheless, the inferences drawn—and the policy implications derived—would be different if perceptions were believed to be false.

A final question about the data is whether they can appropriately be used for longitudinal analysis. A number of studies have stacked annual perceived corruption scores into a time-series cross-section panel for analysis. In the cases of WB and TI data, this seems ill-advised for several reasons. First, TI has changed the methodology for constructing its CPI over the years, so changes in the index may have nothing to do with changes in corruption perceptions. Second, both TI and WB have altered the set of sources used in successive years, so changes in the index might reflect the inclusion of data from new organizations rather than changes in corruption perceptions. Conversely, TI has sometimes reused the same surveys in successive years, automatically reducing interyear variation. Both the TI and WB teams caution against reading much into interyear changes unless they are very large. Kaufmann & Kraay (2002, pp. 13–14) estimate that about half the variance over time in the WB index results from changes in the sources used and the weights assigned to different sources.

Although annual changes in the index should not be taken seriously, Kaufmann et al. (2006) argue that some changes over longer periods are large enough to be significant. Having compared their results from 1996 and 2004, they state that the changes in 5%–7% of countries are large enough to be considered reliable. Perceived corruption increased during these years in Zimbabwe, Cyprus, Israel, and Moldova, while it fell in Latvia, Estonia, the United Arab Emirates, and Bahrain. This finding could motivate useful case studies but not the use of panel methods on the whole set of countries.

Various analysts have run panel regressions on the data from the *International Country Risk Guide* (ICRG) to assess the determinants of annual changes in corruption. Examination of these data casts doubt on their use for such purposes. Political Risk Services (PRS) does not explain how its raters ensure that a rating of 3 in one year or in one country means the same as a 3 in another. Moreover, PRS appears to have recalibrated the index several times, causing unexplained jumps in country ratings in particular months (Knack 2006). Casual inspection of the data also turns up a number of abrupt changes that seem puzzling. In 1995,

France suddenly drops from a perfect score of 6 to a score of 4, the same level as Mozambique and China. In 1985, Nicaragua jumps from a 3, on a par with India, to a 5, the level of Austria. It is hard to find any explanation for these sudden changes. Examining longer periods might seem more reliable, but here, too, there are odd changes. Between 1982 and 1995, Egypt, Syria, Iran, Libya, and Tanzania undergo dramatic improvements, rising from the level of Nigeria, 1, to that of Belgium, 4. By 1995, these four countries have surpassed Spain, which falls during this period from a perfect score of 6 to a 3, on a par with Congo and Malawi. It is troubling to reject data merely because they do not fit one's preconceptions; challenging preconceptions is surely what data are useful for. But these sudden changes seem unrelated to anything in the history of these countries. [Another concern is that the ICRG data actually purport to measure not perceived corruption but the risk of political instability caused by corruption, which may be somewhat different (see Lambsdorff 2005b).]

Even if there were not such doubts about the data, it is uncertain what sort of lags one should expect before political or economic changes influence perceived corruption. It is hard to believe changes in political institutions would affect corruption levels, and then the perceptions of raters, within a single year—the assumption made by most who have analyzed panels. It is much more likely that changes in institutions would cause experts to *expect* changes in corruption, although they do not observe them directly. Relationships in the data would then reveal something about the experts' theories about corruption rather than their observations of it.

For all these reasons, I treat such panel regressions with much skepticism and do not consider them in this article.

Methodological Questions

A few issues arise in analyzing these data. To start with the purely technical, the data are truncated at both ends by the scales used. Raters are usually required to score countries on a scale from, say, 0 to 6; highly corrupt countries cannot fall below 0 and highly clean ones cannot rise above 6. Although there rarely appears to be enough bunching at the top or bottom to suggest a major problem, the use of a finite scale does imply that the impact of, for instance, economic development will seem to taper off as countries reach the maximum value for clean government. Therefore, one should beware of claims that factors such as development have diminishing returns for corruption control.

Second, the data for the aggregate TI and WB ratings are more or less continuous, but ratings such as the ICRG's assign only whole-number scores to countries, so ordered probit is more appropriate than ordinary least squares (OLS) regressions. Still, the OLS estimates tend to be close to those obtained through probit.

Third, You & Khagram (2005) advocate averaging the values of dependent and independent variables over time to reduce measurement error. Although this may indeed generate more accurate estimates for some variables, it also has some significant costs because not all intertemporal variation is mere noise. If, for instance,

per capita income changes over time, and there is a strong relationship between income in a given year and the level of corruption in the following two years, then regressing average corruption on income averaged over a long period will *create* measurement error and underestimate the strength of the relationship. Such averaging will not affect variables that are relatively constant over time, and it may inflate their estimated impact if they are correlated with the meaningful variation in income that has been averaged out.

Fourth, some studies have used such subjective variables as perceived “rule of law” or “bureaucratic efficiency” to explain perceived corruption. Sometimes the dependent and independent variables are even taken from the same survey or rating agency. The chance that respondents have formed distinct opinions about, on the one hand, the effectiveness of bureaucratic or judicial institutions, and on the other, the integrity of bureaucrats and judges seems slim. More likely, these variables are measuring the same underlying perception and correlations between them are meaningless.

A fifth issue concerns the appropriate significance level. Given the large amount of exploratory analysis of these datasets that has been done in a loosely theorized environment, and the danger of publication bias, it probably makes sense for readers to protect themselves by focusing on results that hold at higher levels of significance or that are unusually robust. On the other hand, given the significant problems of measurement and imperfections of the data, it is possible that results that do not seem significant or robust are actually correct. So conclusions must be provisional and tentative.

The central methodological issues are related, and very hard to solve. Most importantly, there is no widely accepted theory on which to base an empirical model. What theory there is relates to the micro level—an exchange is imagined between an individual citizen and an official—and the terms of this exchange are traced by sometimes tortuous logic to characteristics of countries on which data are available. As a result, some variables are included in regressions with only rather flimsy notions of how they might cause cross-national variation in corruption. At the same time, some plausible determinants are highly correlated among themselves, rendering it difficult to disentangle their separate effects. To make things worse, many of the supposed causes of corruption may also be caused by it or by some third factor that causes both. Problems of endogeneity are severe, and there are almost no valid instruments that can be assumed to influence corruption by only one pathway.

Consequently, the most important question to ask about published results is how robust they are. In the next section, I review the main findings of some published studies of perceived corruption, reproduce them in recent data, and examine how they stand up to changes in the year of data used and the controls included. One technique sometimes used to test robustness is Edward Leamer’s extreme bounds analysis, which at its simplest involves including all combinations of three variables from a set of controls. Although this is often valuable, the method’s “judgments” can seem somewhat arbitrary. One variable may “fail” because it is

highly correlated with others so that its standard errors are high, while another—perhaps relatively weakly related to perceived corruption—may still pass because it is not correlated with any of the other potential determinants. At the same time, the results obviously depend on which variables are included in the initial set of controls, a choice that can itself be quite subjective. This is particularly problematic when combined with the lack of compelling theory to guide selection of the candidate controls. As a result, some papers that have used extreme bounds analysis to assess which determinants of perceived corruption are robust have reported quite different results.

I proceed in a less comprehensive manner. I test whether a reported finding holds in cross-sectional data for the WB and TI perceived corruption indexes for the years 2000, 2002, and 2005, with independent variables adjusted as appropriate (usually lagged by a year or two). I report heteroskedasticity-corrected standard errors and weight countries by the inverse of their standard errors where these are available, in order to place less emphasis on cases where perceived corruption is measured with less precision. I then test whether the results change when I include a battery of historical and cultural variables (dummies for colonial history, prevalence of different religions, type of legal system, ethnic division, recent experience of internal or external war). Where possible—and usually it is not—I instrument to control for reverse causation. By the end, I can also check that variables found to be significant are robust to inclusion of other significant correlates of perceived corruption. In addition, I explore whether factors that predict variation in perceived corruption also predict variation in the experience-based indicators.

ECONOMIC DEVELOPMENT

By far the strongest and most consistent finding of the new empirical work is that lower perceived corruption correlates closely with higher economic development (La Porta et al. 1999, Ales & Di Tella 1999, Treisman 2000). For instance, the correlation between the natural log of per capita GDP at purchasing power parity in 2005 and the 2005 TI index is $r = 0.79$, and the correlation with the WB index is $r = 0.81$ (recall that the perceived corruption indexes assign higher numbers to less corrupt governments). There is also a strong correlation with the experience-based indicators of corruption. The log of per capita GDP correlated at $r = -0.74$ with the frequency with which business executives told WBES it was necessary to make irregular payments to get things done; correlated at $r = -0.72$ with the percentage of respondents who told UNICRI they had been asked or expected to pay a bribe in the previous year; and correlated at $r = -0.78$ and $r = -0.69$, respectively, with the 2004 and 2005 GCB survey questions on paying a bribe in the last 12 months.

Indeed, there is a strong, negative correlation between current levels of perceived and experienced corruption and estimates of countries' levels of development even as of hundreds of years ago. Maddison (2003) has estimated per capita GDP for a number of countries from 1500 on. As Table 2 shows, current levels of perceived

TABLE 2 Correlations of 2005 perceived and experienced corruption with Maddison’s (2003) estimates of per capita GDP, 1500–1950^a

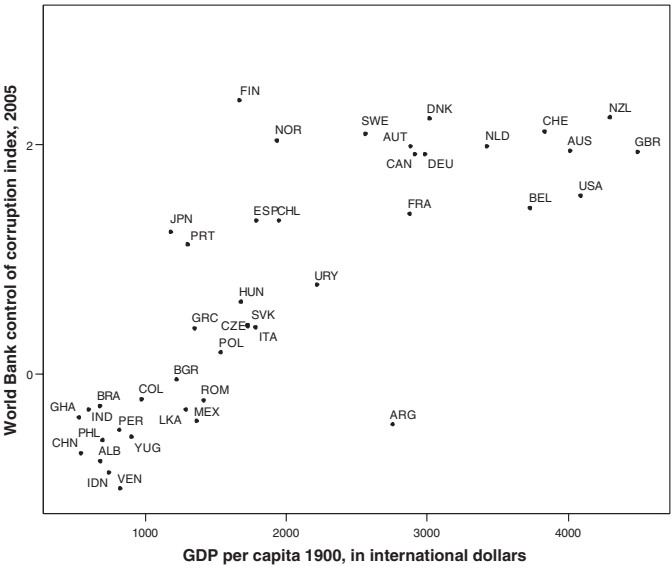
	1500	1600	1700	1820	1900	1950
WB 2005	−0.047	0.145	0.228	0.599 ^b	0.791 ^b	0.441 ^b
p	0.835	0.520	0.308	0.000	0.000	0.000
n	22	22	22	46	42	146
GCB 2005	−0.375	−0.360	−0.290	−0.409 ^c	−0.559 ^b	−0.514 ^b
p	0.152	0.170	0.276	0.025	0.001	0.000
n	16	16	16	30	31	50

^aFor definitions of variables and abbreviations, see footnotes to Table 1.

^bCorrelation is significant at the 0.01 level (two-tailed).

^cCorrelation is significant at the 0.05 level (two-tailed).

and experienced corruption (as in the WB index and the GCB survey) correlate highly with estimated per capita income from as far back as 1820. (Comparisons of the correlations across years should, of course, take into account the great changes in the number of countries for which data are available. Such changes probably explain the drop in correlations in 1950.) A considerable part of current cross-national variation in corruption levels could be predicted by countries’ levels of development almost 200 years ago. By 1900, the relationship is even more pronounced (see Figure 2). Argentina, whose twentieth-century relative income



decline has puzzled political economists, is an outlier. This is not to say recent changes in national income are unimportant, but the correlation between per capita GDP today and that in 1900 or even 1820 is high. For the 41 countries with data, the correlation between per capita GDP rank in 1900 and rank in 2002 was 0.84.

The correlation between economic development and perceived corruption is extremely robust. It survives the inclusion of a variety of controls (for ethnolinguistic fractionalization, latitude, region, religion, culture, democracy, trade, inequality, inflation, and various policy variables), and it can be found in each region of the world. Part of the relationship seems to operate via the association of democratic institutions and free media with both richer countries and lower corruption (the estimated coefficient falls as measures of these are added to regressions—see “Political Institutions” below). But I did not find evidence that the relationship with development was picking up effects of greater average educational attainment or urbanization in the developed countries.

If the correlation is strong and robust, the question remains what—if anything—it means. Does economic development reduce perceived corruption? Does corruption slow economic development? Are both caused by some third factor? A similar debate has pitted those who believe good institutions explain why some countries developed faster than others (Acemoglu et al. 2001, Rodrik et al. 2004) against those who think economic development explains why some countries acquired good institutions (e.g., Boix & Stokes 2003) and others who believe the accumulation of human capital caused both economic development and superior political institutions (Glaeser et al. 2004).

To establish a direction of causation, one needs good instruments, which are unfortunately in short supply. To be valid, an instrument should both correlate reasonably highly with the corresponding explanatory variable and not affect the dependent variable by any other pathway. To assess the effect of perceived corruption on investment and growth, Mauro (1995) instruments for perceived corruption with an index of ethnolinguistic fractionalization compiled in the early 1960s. His results are not significant for either investment or growth once he introduces basic controls (see his tables 4 and 6, coefficients on corruption index, columns for 2SLS). In any case, ethnolinguistic fractionalization is a dubious instrument because ethnic divisions might reduce growth in many other ways—for instance, by prompting political instability, civil wars, and populist distributive politics (Easterly & Levine 1997), or underinvestment in public goods (Alesina et al. 1999).

Another possibility would be to use the historical mortality rates of European settlers in their colonies as an instrument for corruption today (following Acemoglu et al. 2001). These settler mortality data are correlated with higher current perceived corruption as measured by either the WB or TI indexes ($r = 0.52$ and 0.56 , respectively). However, because the settler mortality measure is correlated with many possible proximate determinants of development, it is hard to say what it is instrumenting for. Acemoglu et al. used it as an instrument for current constraints on the executive and expropriation risk, with which it correlates at about the same

rate as with perceived corruption. But, as Glaeser et al. (2004) note, the settler mortality data are even more strongly correlated with the current risk of malaria ($r = 0.67$ in my data) and the current level of human capital as measured by schooling ($r = -0.71$), both of which would also affect the pace of development. So, does the correlation between historical settler mortality and current development reveal that institutional constraints on the executive, traditions of official corruption, low human capital accumulation, or vulnerability to malaria impede development? One cannot be sure.

Looking at the opposite direction of causation, Treisman (2000) uses countries' distance from the Equator as an instrument for economic development and reports evidence that higher development causes lower corruption. However, this is open to objections based on the Acemoglu et al. (2001) arguments already cited. If settler mortality led European colonists to create more exploitative, unaccountable governments in countries with more tropical climates, closeness to the Equator might slow economic development by encouraging corruption.

Using Maddison's (2003) laboriously reconstructed historical GDP data, more evidence can now be adduced. If one is willing to assume that a country's per capita income in 1700 affects current corruption perceptions only via the effect on subsequent economic development, then one can use Maddison's estimates of income 300 years ago as an instrument for current development. (This reduces the number of countries available to 22.) Per capita GDP in 1700 correlates with the natural log of the subsequent change in GDP per capita up to 2002 at $r = 0.29$. As the OLS regression in column 1 of Table 3 shows, the change in income since 1700 is much more closely related to current perceived corruption than is the 1700 level. In column 2, I show a regression with current income per capita instrumented by the 1700 level. Current income instrumented in this way shows up as a significant determinant of perceived corruption in 2005. It is still significant, with a larger coefficient, if I control for colonial history, religion, and legal family (column 3). In columns 4 and 5, in order to increase the number of available countries, I run similar regressions using Maddison's estimate of 1820 GDP per capita as an instrument for current GDP per capita. The coefficient remains significant. Columns 6–8 run regressions using the experience-based GCB and UNICRI data, and instrumenting using Maddison's historical estimates. Although the 1700 GDP per capita instrument leads to a less than significant result for the GCB 2005 data, the 1820 income yields a highly significant result, which suggests that the reported frequency of bribery is lower in countries that grew faster since 1820. Residents of countries that grew faster since 1700 were also less likely to say they had paid a bribe recently on the UNICRI survey.

Looking more casually at international experience, one sees that some countries have grown extremely rapidly in recent decades despite a perception that their states were highly corrupt. Among the fastest were China, South Korea, Thailand, India, and Indonesia. There is also evidence that countries are more likely to become democratic as they develop economically, although the odds do not necessarily change at the same rate in all eras (Boix & Stokes 2003). In line with this, South

TABLE 3 Economic development and perceived corruption^a

	1	2	3	4	5	6	7	8
	WB 2005	WB 2005	WB 2005	WB 2005	WB 2005	GCB 2005	GCB 2005	UNICRI
GDP per cap. 1700	−0.00 (0.00)							
Ln change GDP per cap. 1700–recent year	1.05 ^b (0.20)							
Ln GDP per cap. recent year		1.03 ^c (0.40)	1.61 ^b (0.51)	1.05 ^b (0.11)	1.00 ^b (0.25)	−11.50 (6.80)	−6.40 ^b (1.21)	−7.79 ^b (1.28)
Constant	−9.03 ^b (2.01)	−8.89 ^c (4.03)	−14.64 ^b (4.51)	−9.20 ^b (1.06)	−9.27 ^b (2.07)	119.5 (69.8)	68.2 ^b (12.5)	78.20 ^b (12.72)
R ²	0.706	0.708	0.841	0.781	0.884	0.293	0.423	0.945
n	22	22	21	44	40	16	29	16
Method	OLS	IV	IV	IV	IV	IV	IV	IV
Controls	no	no	yes	no	yes	no	no	no

^aWhite-corrected standard errors in brackets, cases weighted by inverse of the standard error. For definitions of variables and abbreviations, see footnotes to Table 1. “Recent year” is 2002 for WB 2005, and GCB 2005; 1995 for UNICRI. Instrument in instrumental variables (IV) regressions columns 2–3, 6, and 8 is Maddison’s (2003) estimate of GDP per capita in 1700; in columns 4, 5, and 7, Maddison’s estimate of GDP per capita in 1820. Controls for history: percent Protestant 1980, percent Catholic 1980, percent Muslim 1980, British law, French law, German law, Scandinavian law, former British colony, former French colony, former colony of other power except Spain or Portugal.

^bp < 0.01.

^cp < 0.05.

Korea, Indonesia, and Thailand all improved their Freedom House (FH) political rights score by 2–3 points (on a seven-point scale) between 1980 and 2005, while India and China remained unchanged. (The median change for this period—which included the fall of communism in Eastern Europe—was an improvement of one point.) As discussed below, democracy—especially if long-lived and liberal in form—is associated with lower perceived corruption. These considerations would seem to suggest a causal pathway from development to perceptions of cleaner government.

POLITICAL INSTITUTIONS

A number of papers have explored the effect of democratic institutions on perceived corruption. Some have argued that democracy's effects are gradual, and so a long experience of popular government is necessary to lower perceived or actual corruption. Similarly, others have suggested the relationship is nonlinear: Democratization may increase corruption in the short run, even if graft is reduced as democracy deepens. Finally, scholars have examined the effects of particular institutions within democracies—the type of electoral rules, whether the regime is parliamentary or presidential, and the degree of freedom of the press—as well as the effect of decentralized government.

Simply regressing perceived corruption indexes on FH political rights scores (lagged to reduce endogeneity), while controlling for economic development, I find that greater political rights are significantly related to lower perceived corruption in the WB ratings. Political rights remain significant when one controls for a variety of other possible determinants, including colonial heritage and religion (see Table 4, columns 1–2). However, the results are somewhat less significant using the TI perceived corruption data, and they are not always significant for the WB data if the Polity IV democracy rating is substituted for the FH score.

Montinola & Jackman (2002) note a nonlinear relationship between democracy and perceived corruption. They model this by including a term for the square of their democracy indicator. I also found this significant in simple models. However, examining the mean values of perceived corruption for different values of the FH political rights scores suggests a more complicated relationship. Perceived corruption always decreases as democracy increases from 3 to 1 on the FH scale or as authoritarianism softens from 7 to 6, but the effects of movements between 6 and 3 are more erratic. The slight softening of an extreme autocracy (as occurred in Afghanistan in 2003 or Tajikistan in 2007, according to FH) or improvement to a high level of political freedom (as occurred in Hungary and Poland in 1990) tend to coincide with a fall in perceived corruption. But among imperfect democracies or soft authoritarian states, small increases in freedom do not have a consistent impact on corruption perceptions.

Others have suggested it may take decades for democratic institutions to translate into lower perceived corruption (Treisman 2000). If I include a variable

TABLE 4 Political institutions and perceived corruption^a

	1	2	3	4	5	6	7	8	9	10	11
Dependent Variable: WB 2005											
Ln GDP per cap. PPP 2002	0.62 ^b (0.05)	0.57 ^b (0.05)	0.56 ^b (0.07)	0.52 ^b (0.07)	0.40 ^b (0.06)	0.48 ^b (0.06)	0.37 ^b (0.06)	0.45 ^b (0.06)	0.28 ^c (0.14)	0.40 ^b (0.11)	0.46 ^b (0.12)
Political rights (low = freer)	-0.11 ^b (0.03)	-0.13 ^b (0.03)	-0.42 ^b (0.13)	-0.29 ^c (0.13)							
Political rights squared			0.04 ^b (0.02)	0.03 ^d (0.02)							
Democratic ^e since 1950				0.57 ^b (0.15)	0.52 ^b (0.16)	0.26 (0.18)	0.49 ^b (0.15)	0.24 (0.17)	0.45 ^b (0.15)	0.34 ^d (0.17)	0.28 ^d (0.17)
FH press freedom ^f					0.008 ^b (0.003)	0.012 ^b (0.003)	0.010 ^b (0.003)	0.013 ^b (0.003)	0.019 ^b (0.004)	0.020 ^b (0.005)	0.017 ^b (0.005)
Newspaper circulation 1996					0.0018 ^b (0.0006)	0.0012 (0.0009)	0.0017 ^b (0.0006)	0.0012 (0.0009)	0.0013 ^c (0.0005)	0.0012 ^c (0.0006)	0.0005 (0.0007)
Presidential democracy							-0.11 ^c (0.04)	-0.06 (0.05)	-0.18 ^b (0.06)	-0.19 ^b (0.06)	-0.18 ^c (0.07)
Pure plurality system									0.38 ^d (0.19)		
Open-list system									0.22 (0.17)		

(Continued)

TABLE 4 (Continued)

	1	2	3	4	5	6	7	8	9	10	11
Dependent Variable: WB 2005											
District magnitude									0.000 (0.001)		
Open-list X district magnitude									-0.02 (0.01)		
Fiscal decentralization										0.009 ^c (0.004)	0.006 (0.005)
Protestant % adherents											0.0054 ^c (0.0025)
Including controls for history ^g	no	yes	no	no	no	yes	no	yes	no	no	only Prot.
Constant	-4.98 ^b (0.50)	-4.87 ^b (0.55)	-4.06 ^b (0.72)	-4.00 ^b (0.77)	-3.35 ^b (0.59)	-4.01 ^b (0.56)	-2.91 ^b (0.62)	-3.68 ^b (0.60)	-1.80 (1.27)	-3.02 ^b (1.08)	-3.55 ^b (1.10)
R ²	0.719	0.815	0.732	0.768	0.801	0.844	0.810	0.848	0.855	0.885	0.891
n	162	151	162	156	131	122	129	121	68	55	54

^aOLS regressions. White-corrected standard errors in brackets, cases weighted by the inverse of the standard error. Dependent variable measures absence of corruption. PPP, purchasing power parity.

^bp < 0.01.

^cp < 0.05.

^dp < 0.10.

^eDemocratic by classification of Beck et al. (2001).

^fPress freedom scale adjusted so high value indicates freer press.

^gControls for history are as in footnotes to Table 3.

measuring the number of consecutive years the country has been an electoral democracy since 1930 (as classified by Beck et al. 2001), this is highly significant and it reduces the coefficient on the FH score, although the latter remains significant. On examination, this duration-of-democracy variable turns out to be picking up only the division between those that have been in the democratic club since before 1950 and those that have not. When I run the same regression only for countries that have not been democracies in all years since 1950, the coefficient on the duration measure is indistinguishable from zero. So I include a dummy for countries that have been electoral democracies since 1950, which is more significant than the quadratic term in political liberties.

What about institutions within more or less democratic systems? Brunetti & Weder (2003) argue that freedom of the press is particularly important for exposing corrupt officials and deterring the misuse of office. They regress several perceived corruption indicators on an index of press freedom constructed by FH and get significant results. Along similar lines, Adsera et al. (2003) include a variable measuring the “free circulation of daily newspapers,” constructed by interacting a measure of democratic liberties with a measure of newspaper circulation. [They use the interaction on the grounds that “newspaper readership can only generate real political accountability under conditions of democratic freedom” (p. 455).] I tried including both these variables and found both to be significant in regressions of perceived corruption, although the simple measure of newspaper circulation was more significant and robust than the measure interacted with democracy. (Press freedom was not significant in the TI 2005 and 2002 data but was marginally so in the 2000 index.) The press freedom index is correlated extremely closely with the FH political rights score ($r = 0.94$), so it does not make sense to include both at once—doing so causes the coefficient on political liberties to paradoxically reverse sign. Because press freedom is more significant than political rights, I include it. These variables were quite—but not universally—robust to the inclusion of controls (see Table 4, columns 4–6). As more controls are added, some aspects of democracy become insignificant, which may of course be due to the moderately high correlations among them. The coefficient on “democratic since 1950” falls as countries’ colonial history is controlled for (among colonies, former British ones appeared more likely to have been durably democratic), and that on newspaper circulation falls as one controls for religion (Protestant countries have particularly high, and Muslim countries low, newspaper circulation).

Perhaps the most significant constitutional distinction among democratic countries is between those that have a directly elected president with significant powers and those where the executive is elected from within the parliament. Panizza (2001) found that presidential systems were associated with significantly higher perceived corruption (see also Gerring & Thacker 2004 and Lederman et al. 2005). Kunicová & Rose-Ackerman (2005) confirm this and find that presidentialism is particularly harmful when combined with closed-list proportional representation (PR) in legislative elections.

I tried two commonly used measures of presidentialism. Beck et al. (2001) code countries as 2 if the president is directly elected, 1 if a strong president is elected by the legislature, and 0 if there is no president. By contrast, Kunicová & Rose-Ackerman (2005) use a simple dummy for whether or not there is a directly elected president. They also restrict their sample to countries with a FH political rights score below 5.5 to eliminate dictatorships. Both indicators of presidentialism turned out to be marginally significant in the WB 2005 regressions and more highly significant in TI 2005 regressions. Since the Kunicová & Rose-Ackerman dummy required dropping a large number of cases, I instead constructed a variable taking the values of the Beck et al. indicator for countries with FH scores below 5.5, and zero in countries that were not democratic by this criterion. Presidentialism, by this measure, was significantly associated with higher corruption across different years of data, and was robust to the inclusion of most cultural and historical controls. It became insignificant controlling for Catholicism, apparently because of the corrupt presidential regimes of South America—when a South America dummy was included, presidentialism lost significance. It also lost significance when I controlled for how early the country opened its markets to trade (most democracies that opened early were parliamentary) or for the variability of inflation (see Table 5, discussed in the next section).

Scholars have also sought relationships between the type of electoral system and perceived corruption. Persson et al. (2003) and Kunicová & Rose Ackerman (2005) both argue that accountability of legislators to their constituents should be greatest in plurality systems, followed by open-list PR systems, with closed-list PR systems generating the greatest corruption. In addition, Persson et al. expect corruption to decrease with the size of electoral districts because barriers to entry should be lower and competition for office more intense. Golden & Chang (2006) hypothesize that corruption will increase with district magnitude in open-list PR systems. Finally, as already noted, Kunicová & Rose-Ackerman expect the corruption-inducing effect of presidentialism to be greater under closed-list PR.

Here, it seems to me, we are getting close to the end of the information that can be squeezed out of these data. I did manage to construct regressions that reproduced most of these results (although not the interaction of closed-list PR with presidentialism or any general effect of district magnitude). However, the results often fell to insignificance if I tried other years or started introducing controls. Although the effects might exist, the evidence for them is fragile. The strongest evidence was that pure plurality systems had less perceived corruption than pure closed-list PR ones (the excluded category in Table 4, column 9). It is possible also that the effect of open-list PR changes with district magnitude, as suggested by Golden & Chang (the coefficients are of the right signs, although insignificant in column 9). However, these data seem to lack the variation necessary to answer this question reliably. A general problem with using these data to test hypotheses about electoral systems is that the arguments are concerned with the accountability of legislators, but the perceived corruption indexes are more focused on the venality of bureaucrats, over whom legislators may have little control.

TABLE 5 Rents and competition, gender, and inflation^a

	1	2	3	4	5	6	7	8
Dependent Variable: WB 2005								
Ln GDP per cap. PPP	0.37 ^b (0.07)	0.26 ^b (0.06)	0.38 ^b (0.08)	0.24 ^b (0.08)	0.26 ^c (0.11)	0.28 ^b (0.10)	0.25 ^d (0.13)	0.18 (0.12)
Democratic since 1950	0.55 ^b (0.16)	0.43 ^c (0.17)	0.16 (0.19)	0.27 (0.21)	0.10 (0.27)	0.10 (0.21)	-0.20 (0.27)	0.18 (0.24)
FH press freedom	0.011 ^b (0.003)	0.010 ^c (0.004)	0.011 ^b (0.004)	0.008 ^d (0.004)	0.008 (0.005)	0.005 (0.006)	0.010 ^d (0.006)	0.005 (0.007)
Newspaper circ. 1996	0.0013 ^c (0.0005)	0.0016 ^c (0.0006)	0.0017 ^d (0.0009)	0.0014 ^c (0.0006)	0.0018 ^d (0.0009)	0.0012 ^d (0.0006)	0.003 ^b (0.001)	0.0015 ^c (0.0007)
Presidential democracy	-0.10 ^c (0.05)	-0.08 (0.05)	0.00 (0.06)	-0.07 (0.07)	-0.03 (0.08)	-0.05 (0.08)	-0.03 (0.11)	-0.06 (0.09)
Fuel exports ^e	-0.001 (0.002)	-0.003 ^c (0.001)	-0.004 ^b (0.001)	-0.005 ^b (0.002)	-0.006 ^c (0.002)	-0.006 ^c (0.002)	-0.007 ^b (0.002)	-0.005 ^c (0.002)
Imports % GDP	0.001 (0.002)							
Year opened to trade		-0.013 ^b (0.004)	-0.013 ^c (0.005)	-0.012 ^c (0.006)	-0.016 ^c (0.006)	-0.014 ^c (0.007)	-0.018 ^c (0.008)	-0.011 (0.007)

(Continued)

TABLE 5 (Continued)

	1	2	3	4	5	6	7	8
Dependent Variable: WB 2005								
Time required to open firm				-0.21 ^b (0.07)	-0.21 ^c (0.08)	-0.19 ^c (0.07)	-0.15 (0.09)	-0.09 (0.08)
Women in govt. %						0.011 ^c (0.005)	0.013 ^c (0.005)	0.006 (0.007)
Ln SD of inflation								-0.30 ^b (0.07)
Includes controls for history ^f	no	no	yes	no	yes	no	yes	no
Constant	-2.86 ^b (0.69)	-0.89 (0.70)	-1.99 ^c (0.98)	0.07 (0.92)	0.01 (1.31)	-0.42 (1.21)	-0.81 (1.54)	0.16 (1.54)
R ²	0.818	0.836	0.882	0.849	0.894	0.852	0.918	0.881
n	111	101	93	72	66	61	56	48

^aOLS regressions. White-corrected standard errors in brackets, cases weighted by the inverse of the standard error. Dependent variable measures absence of corruption.

^b $p < 0.01$.

^c $p < 0.05$.

^d $p < 0.10$.

^ePercentage of merchandise exports.

^fControls for history are as in footnotes to Table 3.

I also regressed the experience-based corruption indicators (the GCB for 2004 and 2005, UNICRI, and WBES) on the democracy, presidentialism, and pure plurality system variables. Nothing was significant in the GCB regressions. Presidentialism and press freedom were sometimes marginally significant for the UNICRI survey but fell to insignificance if I included a variable for dependence on fuel exports (which was significant). A plurality electoral system was associated with lower reported bribery, but this also became insignificant if I controlled for dependence on fuel exports and the time required to register a business. In the WBES data, presidentialism was marginally significant with an unexpected positive sign (suggesting presidential regimes had less corruption), but this became insignificant once any of several controls was introduced.

Another question concerns the degree of political decentralization. Treisman (2000) finds federalism significantly related to higher perceived corruption using TI data from the mid-1990s. This result was also found by Goldsmith (1999), Kunicová & Rose-Ackerman (2005), and Gerring & Thacker (2004). Fisman & Gatti (2002) examined fiscal decentralization, measured by the proportion of state spending that occurred at regional and local levels (as recorded in the International Monetary Fund's *Government Finance Statistics*), and they found a negative relationship between fiscal decentralization and perceived corruption.

Exploring the data now available, I find the link between perceived corruption and federal structure is not at all robust; it disappears as the number of countries included in the WB and TI indexes approaches 100. Federalism's strong significance in regressions using TI's 1996 and 1997 data probably reflected the limited number of countries available for those years. The fiscal decentralization variable is significant at $p < 0.05$ in the WB 2005 regression, but it is not significant in the WB 2002 or 2000 data or in any of the TI indexes. It becomes insignificant in the WB 2005 regressions as well if one controls for religion—in particular, the share of the population that is Protestant. Protestantism is more significant in the same regression, suggesting lower perceived corruption in traditionally Protestant countries (as found by La Porta et al. 1999 and Treisman 2000). The share of Protestants correlates with fiscal decentralization at $r = 0.35$. Fiscal decentralization was not significant in regressions for the experience-based indicators.

In short, fairly robust evidence exists that countries with high current levels and long histories of liberal democracy—especially those with a vigorous free press—are perceived to be less corrupt. Parliamentary systems and those with plurality electoral rules for the legislature probably share in the reputation for lower corruption. There is no robust relationship with political or fiscal decentralization. At the same time, there is no solid evidence that the actual frequency of corrupt interactions, as judged by the experience-based data, is related to democratic institutions. And with regard to the perceptions data, endogeneity is as great a concern as in the case of economic development. Corruption could itself weaken democratic institutions or could be caused by factors that also undermine democracy. Corrupt rulers are likely to limit press freedom to avoid embarrassing exposure and to limit democratic accountability mechanisms. Transitions to democracy might give

observers the impression that corruption had been reduced even if it had not. Researchers have not found any convincing instruments for democratic institutions, so these questions remain unresolved.²

RENTS AND MARKET COMPETITION

Ades & Di Tella (1999) argue that corruption tends to be greater where there are larger economic rents available for bureaucrats to capture. They hypothesize that where the economy is more oriented toward natural resources—and especially the export of these resources—rents and corruption will be higher. At the same time, openness to international trade will intensify market competition and reduce the monopoly power of domestic producers, shrinking the profits available for corrupt officials to extract. They present evidence that the share of imports in GDP and the share of fuels and minerals in exports are both significantly related to perceived corruption. Treisman (2000) finds the share of imports in GDP is associated with lower corruption in the TI data from 1996–1998, although not always significantly in controlled regressions. Sandholtz & Koetzle (2000), Sandholtz & Gray (2003), and Gerring & Thacker (2005) also find a relationship with trade openness. Treisman (2000) investigates the relationship with exports of fuels, metals, and minerals, the variable Ades & Di Tella used, but reports that in 1990s data (unlike the 1980s data examined by Ades & Di Tella), this becomes insignificant once income and democracy are controlled for.

Table 5, column 1, shows a regression including the share of imports in GDP. The WB's *World Development Indicators* no longer publishes figures on exports of metals and minerals, so I used a measure of the share of fuels in merchandise exports. (I control for economic development and the political variables already found to be significant.) These variables do not appear significant at all in regressions using the WB and TI data from recent years. However, the effect of trade openness is likely to be felt only gradually as competition erodes profit margins and officials are driven out of the market for bribes. I therefore included instead a dummy indicating the year in which the country opened its markets to international trade (as classified by Sachs & Warner 1995). (I coded those countries still closed as of 1995 as if they had first opened in 2000.) This variable turned out to be highly significant, and the measure of fuels exports also became significant in the expected direction. These two effects seem quite robust to adding controls and using other years of WB and TI data, although significance levels do sometimes fall below 0.05.

To capture the effect of state regulation on perceived corruption, I tried using several measures of intervention created by Djankov et al. (2002). Djankov et al.

²Brunetti & Weder (2003) use the FH political rights score to instrument for the FH press freedom rating, but clearly political rights can affect corruption by channels other than press freedom.

find that the number of procedures and the time required to register a business in different countries correlate strongly with the TI index of perceived corruption. I examined how this stood up to a range of controls and uses of different corruption data. I also tried using two indexes, constructed by the same team, of the extent of "formalism" in the legal procedures necessary to collect on a bounced check and evict a tenant for nonpayment of rent (Botero et al. 2004). Greater formalism is associated with greater regulatory interventions and less reliance on an independent judiciary to consider the facts of specific cases. The four indicators were highly correlated. Each was significant in regressions with the WB 2005 corruption data. The time necessary to register a business was the most significant, and it was very robust to the use of different years' data and the inclusion of controls. In fact, the only variable I found that rendered it insignificant was the variability of inflation, and this might have been just because of the associated sharp drop in the number of cases to 48.

Again, however, openness to trade and the extent of bureaucratic regulation are highly endogenous and there are few plausible instruments. To instrument trade, some have used a measure of the average distance of a country's capital from the world's 20 major exporters, weighted by the value of bilateral imports (available in the Barro and Lee dataset). However, as Treisman (2000) points out, the Barro-Lee distance variable is much more strongly correlated with uninterrupted democracy since 1945 (at $r = -0.47$) and with the natural log of per capita GDP ($r = -0.49$) than it is with imports as a share of GDP ($r = -0.12$), rendering it a dubious instrument for trade. I tried instrumenting with the country's area and a dummy for whether the country was landlocked. The relationship with the year of opening disappeared in these regressions, which might be because of a relatively low correlation between instruments and the year of trade opening. The degree of state regulatory intervention is so endogenous that it is hard to know what to make of this correlation. Djankov et al. suggest the extent of regulatory intervention is largely determined by the origin of the country's legal order, and it is true that the time necessary to register a business correlates positively with a French-type legal system ($r = 0.43$) and negatively with a British-type system ($r = -0.42$). However, instrumenting with legal origin yields a result of no significant relationship between regulatory obstacles and the WB corruption index. In other words, high corruption may cause a high level of red tape, but the data do not allow one to conclude that regulatory interventionism leads to higher perceived corruption or that perceived corruption could be reduced merely by limiting regulatory intervention.

OTHER FACTORS

Two recent papers have argued that countries in which a larger share of legislators or government ministers are female tend to have lower perceived corruption (Swamy et al. 2001, Dollar et al. 2001). The logic is somewhat unclear and seems to run

into problems of ecological inference. The arguments depend on the finding that women, when individually surveyed, appear more public-spirited or less tolerant of corruption. But those women who get elected to parliament or appointed ministers are unlikely to be typical of those surveyed. In any case, politicians of either gender are likely to act in office in ways not fully consistent with their answers to abstract questions about professional ethics; and even if most female politicians preferred lower corruption, how a marginal increase in the female share in either the legislature or government could be expected to produce lower corruption at ground level is unclear. The median share of women in the lower house of parliament as of 2000 was about 9% and in the government about 12%. Sung (2003) argues that the correlation is spurious, caused by other aspects of liberal democracy that go along with the protection of women's political rights.

Including variables for the share of women in the lower house of parliament as of 2000 and the share of women in the government as of 2001, I found a significant and quite robust relationship with both WB and TI perceived corruption. It was strongest using the measure of women in the government. Nor was it easy to eliminate this with variables that measure other aspects of liberal democracy. Besides press freedom and long duration of democracy, which were already included, I tried controlling for the government party's margin of victory and the degree of fractionalization of parties.³ Looking at the data, I do not think this is just picking up an effect concentrated in Western Europe or Scandinavia, as initially seemed likely. Besides high scores for such countries as Sweden (55% of the government female) and Norway (42%), the share of women in government was high in Colombia (47%), Zimbabwe (36%), and Cape Verde (35%). Controlling for Western Europe and Scandinavia does reduce the effect but does not eliminate it. And the effect was increased, rather than reduced, by controlling for religion, colonial history, and legal family (Table 5, column 7). The female minister effect did fall sharply when I controlled for inflation variability, which was itself highly significant (see below). The two are weakly correlated ($r = -0.22$). But the drop in gender's significance could merely result from a substantial drop in the number of cases available for this regression. Including gender lowers the coefficients and significance on newspaper circulation and press freedom, so it remains possible that the women-in-government measure is picking up the effects of liberal democracy and a vigorous press more precisely than the measures of these.

Some have examined the relationship between perceived corruption and inflation. Braun & Di Tella (2004) argue that high and variable inflation makes monitoring public spending and contracts harder and so increases corruption. They demonstrate a statistical relationship with the ICRG rating. Like them, I included

³Sung's (2003) results may depend on his inclusion of a subjective index of "rule-of-law." The danger that subjective impressions of rule-of-law will be conflated with subjective impressions of the extent of corruption seems to me too great to make including such variables advisable.

in regressions the log of inflation, a dummy for countries that experienced hyperinflation (which I defined as inflation above 100% in a year), and the log of the annual variance of monthly inflation rates (I averaged this over three years of data). In my dataset, the standard deviation of inflation turned out to be significant and quite robust across corruption measures and controls, although its significance sometimes fell when many controls were included simultaneously. Unpredictable inflation, thus, does seem to be associated with higher perceived corruption.

Finally, I looked for evidence of a link between corruption perceptions and both income inequality (measured in various ways) and the relative wage in the public sector (as discussed by Van Rijckeghem & Weder 2001). These were not generally significant in regressions that included basic controls. This does not mean such relationships do not exist, but I did not manage to find them in any of the data at my disposal.

EXPERIENCE-BASED MEASURES OF CORRUPTION

In Economic Development, above, I showed that the experience-based UNICRI and GCB corruption measures correlated with per capita income and that the relationship remained significant using income as of 1700 or 1820 as an instrument for current income. I experimented including the other variables discussed in the previous sections in regressions of experience-based corruption indicators. There were few significant correlations, and given the small number of cases available, few that were robust besides income.

It might be that respondents in some countries are more reluctant to admit paying bribes and so would reply “don’t know” or “no answer” rather than “yes” to survey questions about this. If so, a better measure of the cross-national variation might be the proportion that answer the question with a simple “no” (I am grateful to Johann Lambsdorff for suggesting this). Using the percent answering “no” on the GCB 2005 survey yields very similar results. The only variables found to be significant were per capita income and (marginally) the time necessary to open a firm.

The models in Table 6 show variables that did survive exposure to controls relatively well. The time required to register a business correlated with higher reported corruption, although this was not always significant. Greater exposure to imports was associated with lower reported corruption in the WBES data, although the effect diminished when the number of cases dropped to 23 in the instrumental variables (IV) regression. (The imports variable also fell to insignificance when I controlled for population.) Fuel exports were robustly associated with higher reported frequency of bribery in the United Nations’ crime survey. Other variables—associated with democracy, women in government, or inflation—were not robustly significant, although a plurality electoral system was often correlated with lower bribes in the UNICRI survey.

TABLE 6 Experience-based corruption measures^a

	1	2	3	4	5	6	7	8	9
	GCB 2005	GCB 2005	GCB 2005	WBES	WBES	WBES	UNICRI	UNICRI	UNICRI
Ln GDP per capita PPP	-4.72 ^b (0.89)	-5.42 ^b (1.07)	-5.57 ^b (1.63)	0.54 ^b (0.07)	0.44 ^b (0.11)	0.57 ^c (0.22)	-6.27 ^b (1.03)	-7.79 ^b (2.38)	-6.69 ^c (2.61)
Time required to open firm	1.65 ^d (0.97)	1.77 (1.09)	1.00 (1.22)	-0.17 ^c (0.08)	-0.26 ^d (0.14)	-0.17 (0.13)	1.53 ^c (0.74)	-0.01 (1.98)	1.51 (1.05)
Imports, % of GDP				0.007 ^c (0.003)	0.008 ^d (0.005)	0.005 (0.005)			
Fuel exports ^e							0.48 ^b (0.13)	0.44 ^c (0.20)	0.44 ^c (0.21)
Constant	46.3 ^b (10.8)	55.6 ^b (12.4)	56.9 ^b (18.6)	-0.04 (0.79)	1.35 (1.30)	-0.39 (2.33)	57.5 ^b (11.6)	75.2 ^c (27.9)	61.8 ^c (28.8)
R ²	0.478	0.593	0.445	0.592	0.700	0.551	0.727	0.751	0.808
n	52	47	29	55	49	23	35	29	22
Method	OLS	OLS	IV	OLS	OLS	IV	OLS	OLS	IV
Controls ^f	no	yes	no	no	yes	no	no	yes	no

^aOLS regressions. White-corrected standard errors in brackets. For definitions of dependent variables, see footnotes to Table 1. Instrument for Ln GDP per capita in IV regressions is per capita GDP PPP in 1820 (Maddison 2003).

^bp < 0.01.

^cp < 0.05.

^dp < 0.10.

^ePercentage of merchandise exports.

^fControls for history are as in footnotes to Table 3.

CONCLUSION

The newly available data about corruption perceptions have stimulated an exciting burst of empirical work on what had been a frustratingly difficult topic for social scientists to study. We now know that states are perceived by business people and their citizens to be less corrupt if they are highly developed, long-established liberal democracies, with a free and widely read press, a high share of women in government, and a long record of openness to international trade. Countries are perceived to be more corrupt if they depend on fuel exports, have intrusive business regulations, and suffer from unpredictable inflation. We cannot reliably say that most of these factors *cause* corruption perceptions to be high or low. Evidence of this is strongest for economic development. How corrupt a country is perceived to be today turns out to correlate with how rich it was in 1700—a time when China's income per capita was higher than Canada's, and the Netherlands was four times as developed as the United States. If one assumes that current corruption perceptions are affected by a country's level of economic development 300 years ago only via its effect on subsequent growth, then one can be fairly sure that countries' levels of development affect how corrupt they are perceived to be.

Thus, the good news is that we are getting better and better at constructing indexes of perceived corruption and fitting explanatory variables to the cross-national variation in them. The R-squareds now achieved are quite remarkable. The less good news is that these indexes do not correlate as highly as one might expect with citizens' actual experiences with corruption as measured by surveys of business managers and other victims. And most of the factors that predict perceived corruption do not correlate with measures based on direct experience. These facts together raise a nagging suspicion. It is possible that the ratings we have been analyzing measure not corruption itself but guesses about its extent in particular countries that experts or survey respondents have derived by applying conventional theories about corruption's causes. These same conventional theories inform the hypotheses of researchers, which turn out—surprise!—to fit the data well. Believing democracy reduces corruption, the experts give high grades to democracies; researchers then discover that democracy predicts a low perceived corruption rating.

It might be that the experience-based measures are noisy and that the corruption perceptions indexes are accurately capturing the true prevalence of corruption. How frequently individuals in their daily life, or company executives in their business activities, have been targeted by venal bureaucrats may not capture what the expert analysts know about the extent of corruption at the state's highest levels. Or survey respondents may not answer as frankly about their own experience. Still, the question is rarely asked just how the experts of PRS and other business consultancies are believed to know about the frequency of bribery, and to be able to assess its change from year to year. What can their sources be? Press reports? Data on criminal convictions? Rumors? Scholars usually treat all these

types of information with skepticism on the grounds that they are likely to be biased. Are there other sources available to the risk consultancies, and if so, why could researchers not study them directly? The conclusions of the expert raters are attributed an authority proportional to the mystery with which their ratings are constructed.

Based on existing experience-based surveys (Table 6), it appears that economic development does reduce the frequency of demands for bribes. This is confirmed by instrumenting for current income per capita with the level as of 1820, and controlling for countries' colonial history, religion, and type of legal system. A doubling of GDP per capita—roughly the difference between Algeria and Estonia—is associated with a drop of 3–4 percentage points on the GCB and about 4–5 points on the UNICRI survey in the share of respondents saying they or a family member paid a bribe during the last year. Given that the median responses on these survey questions were about 6% and 7.5% respectively, this is substantial. Increasing the share of fuels in exports by 25%—the difference between Iceland and Indonesia—is associated with an approximately 11-point higher reported frequency of bribery on the UNICRI survey. Greater openness to imports may be associated with a lower reported frequency of bribery in the WBES, but the effect is very small. And all surveys suggested more intrusive business regulations may lead to greater reported corruption, but the size and robustness of this effect were less clear.

To question that perceived corruption indexes—and other subjective ratings of “rule of law,” “security of property rights,” or “bureaucratic effectiveness”—are accurately measuring the target concept is not to deny the value of studying them. As Mauro (1995) and others show, perceptions of corruption or rule of law are important in their own right. They affect investors' decisions, and—at least since the launch of the Millenium Challenge Account—the allocation of foreign aid. If governments are persuaded to democratize and increase female representation in the hope of reducing corruption, that would not be a tragedy. But insofar as research aims to explain corruption itself, there now seem to be alternatives available. The United Nations Crime Victims' survey, the World Business Environment Survey and others like it, and several years of TI's Global Corruption Barometer all offer measures of how frequently citizens or business people encounter demands for bribes in different countries. They deserve closer study. (For one early analysis of the UNICRI data, see Mocan 2004.) Resources of international organizations could usefully be redirected from further analyzing perceived corruption to repeating and expanding country coverage of experience-based surveys, while ensuring questions are designed to elicit factual answers about the respondents' own interactions with bribe-soliciting officials. The techniques developed for constructing aggregate indexes of perceived corruption could, as the number of such surveys increases, be used to provide summary indexes of experienced corruption. Such a focus would invigorate the empirical study of corruption for the next ten years.

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CONTENTS

STATE REPRESSION AND POLITICAL ORDER, <i>Christian Davenport</i>	1
BRINGING THE COURTS BACK IN: INTERBRANCH PERSPECTIVES ON THE ROLE OF COURTS IN AMERICAN POLITICS AND POLICY MAKING, <i>Jeb Barnes</i>	25
NEOPLURALISM, <i>Andrew S. McFarland</i>	45
IMMIGRANT INTEGRATION IN EUROPE: EMPIRICAL RESEARCH, <i>Terri E. Givens</i>	67
THE LIBERAL TRADITION AND THE POLITICS OF EXCLUSION, <i>Marc Stears</i>	85
FRAMING THEORY, <i>Dennis Chong and James N. Druckman</i>	103
POLITICAL PLACES AND INSTITUTIONAL SPACES: THE INTERSECTION OF POLITICAL SCIENCE AND POLITICAL GEOGRAPHY, <i>Philip J. Ethington and Jason A. McDaniel</i>	127
ELECTORAL LAWS, PARTIES, AND PARTY SYSTEMS IN LATIN AMERICA, <i>Scott Morgenstern and Javier Vázquez-D'Elía</i>	143
BE CAREFUL WHAT YOU WISH FOR: THE RISE OF RESPONSIBLE PARTIES IN AMERICAN NATIONAL POLITICS, <i>Nicol C. Rae</i>	169
POLITICAL FINANCE IN COMPARATIVE PERSPECTIVE, <i>Susan E. Scarrow</i>	193
WHAT HAVE WE LEARNED ABOUT THE CAUSES OF CORRUPTION FROM TEN YEARS OF CROSS-NATIONAL EMPIRICAL RESEARCH? <i>Daniel Treisman</i>	211
DIVIDED POLITICS: BICAMERALISM, PARTIES, AND POLICY IN DEMOCRATIC LEGISLATURES, <i>William B. Heller</i>	245
THE END OF ECONOMIC VOTING? CONTINGENCY DILEMMAS AND THE LIMITS OF DEMOCRATIC ACCOUNTABILITY, <i>Christopher J. Anderson</i>	271
PUBLIC POLICY ANALYSIS: IDEAS AND IMPACTS, <i>William T. Gormley, Jr.</i>	297
IRRATIONAL CHOICE AND MORTAL COMBAT AS POLITICAL DESTINY: THE ESSENTIAL CARL SCHMITT, <i>John P. McCormick</i>	315
HOW DOMESTIC IS DOMESTIC POLITICS? GLOBALIZATION AND ELECTIONS, <i>Mark Andreas Kayser</i>	341

ELECTORAL LAWS AS POLITICAL CONSEQUENCES: EXPLAINING THE ORIGINS AND CHANGE OF ELECTORAL INSTITUTIONS, <i>Kenneth Benoit</i>	363
---	-----

INDEXES

Cumulative Index of Contributing Authors, Volumes 1–10	391
Cumulative Index of Chapter Titles, Volumes 1–10	394

ERRATA

An online log of corrections *Annual Review of Political Science* chapters
(if any, 1997 to the present) may be found at <http://polisci.annualreviews.org/>