

Comparative Political Corruption: Issues of Operationalization and Measurement*

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As with other areas of comparative political inquiry, analyses of political corruption must carefully negotiate around numerous methodological issues. In this article, we focus primarily on problems of operationalization and measurement of corruption. We evaluate the major examples of cross-country measures of corruption that have recently emerged and review research that has incorporated the new measures. We end with a discussion of an alternative method for the cross-national measurement and analysis of corruption, one that might also facilitate the goal of establishing universal principles and causal claims about political corruption.

Introduction

As with other areas of comparative political inquiry, analyses of political corruption must carefully negotiate around numerous methodological issues. In this article, we highlight some critical methodological obstacles in the comparative study of political corruption and suggest ways in which they may be addressed. We focus primarily on issues of operationalization and measurement.¹ First, we begin with a survey of the two generic approaches to comparative political inquiry: case-oriented and variable-oriented research. Second, we discuss problems regarding the operationalization of corruption. Third, we present the major recent examples of measures that are proving to be useful to researchers. Fourth, we consider two problems in the measurement of corruption: reduction of the phenomenon in a single country to a single measure, and

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the problem of averaging in such measures. Fifth, we review research that has incorporated recently available cross-national measures of political corruption. Finally, we consider an alternative method for the cross-national measurement and analysis of political corruption, one that might facilitate the goal of more comprehensive, comparative, and empirically based explanations of political corruption and its effects.

Variable versus Holistic Approaches

Many important methodological debates about the comparative study of political corruption can be reduced to disagreements over the appropriateness of variable-oriented and case-oriented approaches to political analysis. Case-oriented research in comparative politics emphasizes complexity, contextual richness, and in-depth analysis. Scholars working in this tradition often “share a suspicion of theories that generalize at too great a remove from specific cases” (Katznelson 1997: 92). As holistic undertakings, case studies help us understand the complexity of their cases by incorporating a great deal of detail in their explanations. Case studies emphasize whole systems, assuming that the whole is greater than the sum of its parts (Yin 1984). Case-oriented researchers implicitly assert “that relations among variables cannot be consistent from case to case” (Katznelson 1997: 92).

In contrast, variable-oriented research emphasizes parsimony and generalizability over complexity. It stresses the testing of causal hypotheses embedded in theoretical explanation. Such research focuses primarily on the identification of factors that help explain the causes and consequences of phenomena. Much of this research occurs at a macro-comparative level, with the nation-state as the unit of analysis. Macro-comparative research emphasizes relationships between variables rather than the full nature of the context within which they occur. Stated differently, variable-oriented researchers seek to understand the causal influence of some factors on others rather than a fuller analytical depiction of the whole that these parts comprise.

Macro-level comparative analysis of political corruption has many advantages over the case study method. First, case studies’ emphasis on contextual description tends to lead to the neglect of generalizable explanations. Variable-oriented analysis, with its emphasis on similarities rather than on the unique, pushes the researcher toward broader, theoretically focused questioning. Second, case study specialists tend to select unusual cases that are outliers. These cases provide less leverage in terms of generalizability to the general population (Peters 1998). Outliers are less of a problem in variable-oriented research given its focus on a larger number of cases; greater control can be achieved through case selection as well as through statistical methods. Third, and equally as important, whether utilizing single or multiple cases, small-N and holistic studies are generally plagued by over-determination—the problem of having more independent variables than cases (Liebersohn 1991; Lijphart 1971). This problem prevents a researcher from distinguishing the most likely determinants of a phenomenon under investigation from incorrectly specified causes. Researchers utilizing variable-oriented approaches typically have more

cases than explanatory variables; thus they are able to test competing hypotheses and rule out incorrectly specified causes. Fourth, case-oriented research tends to preclude evaluation of the relative importance of different independent variables. Statistical analysis provides a means to judge the independent impact of each causal variable, as well as the effect of more complex causal combinations represented by interaction terms. Fifth, concept validity is fundamentally problematic in variable-oriented analysis. Thus researchers engaged in macro-comparative cross-national analyses tend to be vigilant with respect to operationalization and measurement problems, for example, “conceptual stretching” (Sartori 1970; Collier and Mahon 1993). In contrast, case studies, which rely on descriptive analyses of particular nations, societies, and institutions, often lack sensitivity to questions of operationalization and measurement. Finally, while some case studies can be replicated, those based on ethnographic work generally cannot. Variable-oriented analyses, on the other hand, are typically replicable because they tend to be more explicit about the procedures used to collect and evaluate data (Jackman 1985).

Although variable-oriented analysis has many advantages over case studies, case-oriented research does have a place in the comparative analysis of political corruption (Yin 1984; Eckstein 1975; Lijphart 1975). The contribution of works within the latter approach depends, however, on the rigor of their research design. Case-oriented research is most useful when it is theoretically informed and when the systematic logic of comparative inquiry is used to select cases (Eckstein 1975; Geddes 1990).² Well-conducted case studies should engage an already existing literature rather than limit themselves to providing in-depth descriptive information regarding specific processes, institutions, and countries. When done well, they remain sensitive to issues of conceptualization and operationalization of measures of political corruption. Case studies, especially those with outcomes not predicted by existing theory, are best carried out when they generate new hypotheses, and thereby advance theory.

The study of political corruption must strive for meaningful generalizations and insightful theoretical explanation. Broad comparative studies provide a greater potential than case studies for understanding the sources and consequences of political corruption. While case studies often provide substantial, even if nomothetic and idiosyncratic, evidence of corruption and may occasionally be an appropriate technique for building and testing theory (Eckstein 1975), analysis of political corruption must move beyond the case-oriented approach. Unfortunately, until very recently, the number of truly comparative studies of political corruption has been severely restricted by the lack of plausible cross-country measures.

Problems of Variable Operationalizations

The operationalization of political corruption involves numerous issues that any rigorous comparative inquiry must confront. The most difficult steps involve defining the term itself and choosing indicators with which to measure variation in “level of corruption.”

As we discuss in a previous article (Lancaster and Montinola 1997), corruption has been defined in numerous ways, none without its problems. One set of definitions conceptualizes corruption as behavior by public officials that deviates from the public interest (Morris 1991), from accepted moral standards (Brooks 1970), or from principal-agent agreements between voters, politicians, and bureaucrats implicit in democracy (Rose-Ackerman 1978, 1999). Yet, who is to say what is in the public interest? Whose moral standard is the appropriate reference point when public opinion is divided? Whose preferences must be subverted in order for us to consider an act corrupt? Should it be the preferences of each politician's constituents? preferences of politicians' parties-in-the-electorate? or preferences of the general population? These definitions are problematic because they define the term "corruption" as deviation from some ideal state or natural condition about which scholars have different notions.³ Even the most narrow (and commonly used) definition of corruption, the misuse of public office for private benefits, suffers from this problem. The nature and size of personal benefit considered as corrupt gain is often dependent on accepted moral standards and/or laws and regulations. What are researchers to do if specific behavior is considered illegal but not immoral, or vice versa? At a minimum, researchers must be sensitive to the fact that diverse societies tend to have different norms, laws, and ideas of what constitutes democracy and the public interest when they make generalizations regarding the causes and consequences of corruption.

A second set of definitions attempts to avoid these problems by defining the concept in terms of specific systems with particular attributes. The goal then becomes to explain the origins or persistence of the systems that elicit the apparently corrupt behavior. For example, corrupt behavior has been defined as a symptom of patrimonialism, a form of government distinct from democracy implemented through a rational legal bureaucracy (Theobald 1993); and similarly, as evidence of the coexistence of two notions of public which are often incompatible—the primordial and civic (Ekeh 1975). Corruption has also been defined as the result of the adoption of a market-pricing mechanism for government goods and services rather than a mandatory pricing model of allocation (Tilman 1970). Most recently, Hellman et al. (2000b) have "unbundled" the concept of corruption into three forms of interaction between firms and public officials: influence, state capture, and administrative corruption. They distinguish between these types of corrupt interaction based on the source and distribution of rents derived from such relationships. Influence occurs when firms are able to affect the formation of laws in order to derive rents without recourse to illicit private payments to public officials. State capture occurs when firms are able "to encode private advantages in the rules of the game as a result of bribes to public officials." Administrative corruption occurs when state actors use their discretionary capacity to regulate firms to derive rents for themselves.

Such definitions provide specific criteria with which to recognize corruption, regardless of whether or not the behavior under investigation is considered corrupt by those that practice it; thus, they appear to make comparative work more tractable. Moreover, disaggregating the concept of corruption, as

do Hellman et al. (2000b), is likely to yield significant insights. Different forms of corruption may have different causes and consequences.⁴ This second set of definitions, however, is equally subject to problems of operationalization, which we discuss further below.

That corruption is difficult to define does not mean it should not be studied. Our aim is simply to expose problems of existing definitions. If we are to deepen the state of knowledge on the phenomenon, researchers must be sensitive to other working definitions and their implications. Similarly, researchers hoping to engage in systematic comparative analysis must be sensitive to the different means of operationalizing and measuring corruption.

Recording the number of specific corrupt acts, the number of public officials involved in corrupt transactions, or the monetary value of the transactions are all possibilities for aggregating corruption. Choice of measurement instrument is inevitably linked to data availability as well as measurement validity and reliability. Written documents, such as press reports, judicial records, and records from anti-corruption agencies are important sources for scholarly analysis. Advantages of these empirical sources are numerous. They are non-reactive and likely to be less biased than interview data; they often cover longer periods of time; and they may involve less expense. Unfortunately, many disadvantages are also inherent in these documentary sources. They may be systematically biased due to the clandestine nature of specific acts of corruption, the political nature of the institutions and agencies that collect the data, and the different styles of data collection at different periods of time and in different countries. In contrast, survey data permit the researcher to control more of the potential for measurement bias. Surveys eliminate many record-keeping problems associated with written materials, and are more likely to apply conceptual definitions consistently across countries. In this way, surveys facilitate large-N comparative studies. They are, however, expensive and possess their own set of reliability and validity problems. Most importantly, they focus on *perceptions* of the incidence of corruption, as opposed to its actual occurrence.

Useful Operationalizations of Political Corruption

For rigorous cross-national studies of political corruption, the goal is to avoid collections of single cases and, instead, engage in truly cross-national comparative work. Toward this end, several organizations have collected cross-national data on political corruption. We highlight here such efforts by risk analysis firms, various international organizations, and Transparency International. These organizations, and especially Transparency International, which aggregates and standardizes the data collected by other institutions, have opened a door with a great deal of potential for research in political corruption.

At least since the early 1980s, private risk analysis firms have considered corruption a significant enough factor in the productivity of investment to warrant their attention. They incorporate evaluations of each country's level of corruption into their investment environment analyses across different countries. These evaluations are based on perceptions of local or expatriate business executives working in the different countries, or those of staff experts

who have spent significant amounts of time studying these countries. In annual surveys, respondents tend to be questioned on their perceptions of the extent to which they encounter corruption in the countries of interest. Risk analysis surveys recently used by academics include those by Business International and Political Risk Services (Mauro 1995; LaPorta, Lopez-de-Silanes, Shleifer, and Vishny 1999).⁵

The virtue of data from risk analysis firms is their consistency across countries and over time. Given the objective of these consulting firms, assessments focus on corruption that is likely to affect foreign business transactions. But, even within this domain, issues of validity and reliability arise. Many forms of corruption affect foreign business transactions. Yet some risk analysis firms do not specify the type of corruption that concerns them. Thus, respondents may have different forms of corruption in mind when they answer surveys. For example, the Political and Economic Risk Consultancy (PERC) simply asks their respondents: "To what extent does corruption exist in the country in which you are posted in a way that detracts from the business environment for foreign companies?" (cited in Lambsdorff 2000). The Institute of Management Development (IMD) asks their respondents the degree to which "improper practices (such as bribing or corruption) prevail in the public sphere" (IMD 1997:384). Moreover, assessments of extent of corruption are often dependent on one's comparative perspective, or, like definitions of corruption itself, on reference points that may vary among respondents. Respondents or raters with little experience in other countries may judge a country more or less harshly than those with broader comparative perspective.⁶

Other firms specify the type of corruption that concerns them. The World Economic Forum asks respondents whether "irregular, additional payments connected with import and export permits, business licenses, exchange controls, tax assessments, police protection or loan applications are common/not common" (cited in Lambsdorff 2000). Political Risk Services states that their experts are more concerned with "actual or potential corruption in the form of excessive patronage, nepotism, job reservations, 'favor-for-favors,' secret party funding, and suspiciously close ties between politics and business..." (PRS/ICRG 2000).⁷ These instruments may allow for more consistent ratings in terms of types of corruption, but they may still encounter the reliability problem mentioned above. In addition, because the corrupt encounters of foreign business are generally concentrated in specific areas within particular countries, such as capital cities and other large urban areas, the corruption scores derived from surveys that focus on the effects of corruption on foreign business may not fully reflect the overall level of corruption in any particular country.

More recently, a few international organizations have conducted surveys that avoid some of these biases. In 1997 Gallup International (www.gallupinternational.net), a market research agency which has as one of its tasks the goal of letting governments and industry know how satisfied people are with the goods and services they receive, surveyed around 30,000 individuals in 44 countries on their perceptions of corruption in face-to-face meetings or via telephone. The respondents, an average of 800 per country, were a more representative sample of the general public than those of risk analysis firms. Moreover, in contrast to

the risk analysis surveys, which tend not to distinguish between different forms and loci of corruption, the Gallup poll asked respondents to evaluate levels of corruption among specific groups of individuals, including politicians, policemen, judges, journalists, and clergy. This measurement strategy helped assure representation of different forms of corruption within the overall corruption score of each country. Such finer distinctions may prove extremely useful to researchers interested in specific forms of corruption. Unfortunately, these data are no longer available on Gallup's website, nor do they appear to be collected any longer in this disaggregated form.

In 1997, the World Bank also presented a survey including corruption measures. The survey was designed to help people identify obstacles to doing business (Brunetti, Kisunko, and Weder 1997). The Bank surveyed 3,600 entrepreneurs in 69 countries and recorded their perceptions on issues including the level of corruption in bureaucracies, the reliability of the judiciary, and the security of property rights. This project is notable for its effort to include respondents from firms of different sizes, types, and locations within the 69 countries. Such breadth in sampling adds greatly to the measurement validity of a country's overall level of corruption, particularly compared to surveys produced by private risk analysis firms. The World Bank's recording of this stratification information makes component and sub-sample analysis of the data set feasible, and consequently allows researchers to engage in within-country as well as cross-country analysis. The survey, however, is not without limitations. Most seriously, it underrepresents Asian and Latin American countries.⁸ The data include only nine of 26 Latin American countries. And, the only Asian countries in the survey are Fiji, India, and Malaysia.

Finally, the World Bank together with the European Bank of Reconstruction and Development (EBRD) has developed an even finer measure of corruption. In addition to the breadth in sampling similar to that of the 1997 World Bank survey, the 1999 Business Environment and Enterprise Performance Survey (BEEPS) distinguishes between three types of relationships between firms and the state: influence, state capture, and administrative corruption.⁹ Data on the incidence of these corrupt relationships is further broken down. Administrative corruption is divided according to particular "services," such as obtaining licenses and permits, dealing with taxes and tax collection, and dealing with customs and imports. Data for state capture include a "behavioral measure"—the number of firms that engage in the behavior, and an "impact measure"—the share of firms in each country that reports a direct impact on their behavior. The measures for state capture are further disaggregated by activity, such as the sale of parliamentary votes, presidential decrees, and court decisions. The current survey covers only 22 transition economies but is expected eventually to cover over 80 countries around the world. The availability of data with these finer distinctions in a relatively large sample is certain to generate significant insights into the causes and consequences of corruption, especially if, as suggested above, different types of corrupt activities have different sources.

The World Bank and WB/EBRD surveys have undoubtedly presented researchers with better measures of corruption than those of private risk firms;

however, the reliability, in contrast to validity, of data from any one survey cannot be taken for granted. This is in part why Transparency International (TI), an organization dedicated to fighting corruption around the world, created what is today known as the Corruption Perceptions Index (CPI) (www.transparency.de). The CPI is a "poll of polls" in that it combines survey results from private risk firms and other organizations. The 2000 CPI, for example, is composed of 16 surveys from eight different institutions, more specifically, one poll from each source that used country experts and more than one poll from sources that used elite or public surveys.

The CPI ranks a country's degree of corruption on an 11-point scale. A country perceived to have totally corrupt business practices is placed at the scale's end point of zero (0). Countries whose governments and public officials are seen as being honest in business dealings are assigned a score of ten (10). In TI's 2000 index, for example, Finland receives a 10, and Nigeria scores 1.2. Each country's CPI score is the standardized average of its corruption scores from the other indices. The surveys are aggregated by Johann Graf Lambsdorff at Goettingen University.¹⁰ The 2000 CPI includes 90 countries, all countries for which at least three surveys were available. The maximum number of surveys incorporated into the CPI is determined by the number of available surveys that TI deems sufficiently documented and reliable.

The CPI's greatest advantage is TI's commendable effort to maximize the reliability of its measures. Along with the annual corruption scores for each country, TI provides information on the reliability of each particular country's score—each score's standard deviation. A higher variance indicates greater disparity across individual surveys and highlights the problem of reliability of any single source. For example, a cursory inspection of one poll, the International Country Risk Guide (ICRG) of January 2000, which ranks countries from zero (0) to six (6), with six representing the least corrupt countries, might lead one to question scores for a few countries, including (but not necessarily limited to) that of Ireland which received a score of 2; Belgium, 3; the Philippines, 4; and Greece, 5 (See Appendix 1 for the complete ranking.) The data provided by TI suggest that we would be justified in questioning the ICRG scores' reliability for these countries. Each country had scores from at least eight polls, and once standardized, their scores from the individual surveys differed in range from an appreciable three points to a substantial six points on a scale of 0-10. The data provided by TI thus remind us of the potential reliability problems that can arise with reliance on any one poll, and show that reliability varies from country to country. Indeed, the goal of TI's aggregation process is to minimize this problem.

That said, researchers should bear in mind two caveats. First, concerns about the reliability of individual surveys should not be overstated. As shown in Table 1, the individual surveys are highly intercorrelated.¹¹ Second, researchers should bear in mind that the TI index is not necessarily more accurate than any single poll. Determining which poll is most accurate is not possible. Confidence in the CPI assumes each poll is subject to random error.

TI's corruption index, the research it has inspired, and other measures such as those by the World Bank and EBRD have begun to promote useful debate

Table 1
Correlation Matrix of Corruption Scores in CPI (2000)

	ACR 1998	ACR 2000	EIU	FH	GCR 1998	GCR 1999	GCR 2000	ICVS	IMD 1998	IMD 1999	IMD 2000	PERC 1998	PERC 1999	PERC 2000	PRS	WB
ACR 1998	1.00	0.87	0.73												0.69	
ACR2000		1.00	0.74												0.65	
EIU			1.00	0.85	0.90	0.85	0.87	0.45	0.86	0.87	0.88	0.91	0.89	0.88	0.77	0.70
FH				1.00	0.86	0.97	0.86								0.74	0.64
GCR 1998					1.00	0.96	0.96	0.78	0.87	0.92	0.93	0.90	0.86	0.92	0.70	0.95
GCR 1999						1.00	0.98	0.64	0.83	0.90	0.90	0.93	0.84	0.91	0.64	0.90
GCR 2000							1.00	0.76	0.84	0.91	0.91	0.91	0.83	0.91	0.67	0.82
ICVS								1.00	0.64	0.65	0.72				0.41	
IMD 1998									1.00	0.97	0.96	0.95	0.83	0.85	0.72	
IMD 1999										1.00	0.98	0.97	0.91	0.93	0.72	
IMD 2000											1.00	0.96	0.94	0.93	0.74	
PERC 1998												1.00	0.90	0.95	0.67	
PERC 1999													1.00	0.95	0.66	
PERC 2000														1.00	0.68	
PRS															1.00	0.69
WB																1.00

Source: Reproduced from Lambsdorff (2000).

Correlations which relate to less than 6 common countries are unreliable and not reported.

about the conceptualization, operationalization, and measurement of corruption. No measure is immune to debate, and the evaluation of such measures itself makes important contributions to our understanding of political corruption.

Reducing a Single Country to a Single Measure

Methodologically, operationalizations and measurements of political corruption are fundamentally similar to those in many other areas of comparative politics. They are likely to raise similar issues. One issue involves the reduction of complex phenomena into a single measure. Reducing corruption in a particular country to a single measure is no different, for example, from capturing the complex phenomenon of economic development in the measure “gross domestic product” (GDP).

Like GDP and other aggregate measures, Transparency International’s CPI scores cannot reflect variation in different sectors or regions of each country. Yet respondents to the different surveys from which TI’s corruption index is constructed may have different sectors of the government or economy in mind when providing an assessment of corruption. Some sectors of a country’s economy or political system may be characterized by more corruption than

others. Consequently, high variance across surveys in a country's CPI score for any given year may not be due only to reliability problems, as suggested in the previous section. High variance may also signal a validity problem generated by TI's technique of averaging corruption scores from different surveys into a single measure. Researchers should thus bear in mind that a country's score on the CPI indicates the average level of corruption among the institutions within a country explicitly or implicitly considered by respondents of each of the different surveys. Since each survey may be measuring corruption in a different sector of a country, TI's use of varying numbers of surveys for different countries in any given year presents a potential methodological weakness. Researchers engaged in cross-country analyses using the CPI might consider replicating their work with other corruption indices to ensure robust findings. As mentioned earlier, single measures from other surveys are subject to their own respondent biases, but such respondents are more likely to have the same sector of the government or economy in mind when assessing level of corruption in a particular country.

The Problem of Averaging

Other issues that researchers using the TI index must confront fall under the rubric of the problem of averaging. These include issues of units of analysis, outliers, the combining of indices, and the problem of the averaging out of effects.

Units of analysis need to be explicitly addressed in all macro-level analyses in comparative politics. The central issue is a question of inference: can collective measures be utilized to infer individual level behavior? The use of aggregate measures of corruption such as the TI index to explain macro-economic outcomes may not be especially problematic. Although acts of corruption are committed by individuals, the determination of co-variation of such individual acts aggregated into a single measure and aggregated measures of economic performance is, in terms of explanation, parallel to theorizing about macro-economic performance by building on assumptions of basic micro-economic behavior.

In contrast, aggregated measures used to explain variation in CPI measures may be more problematic. Assumptions of individual behavior and the incentive structures underlying them are too readily *averaged* out in many indices of social, political, and economic behavior to make for robust explanation of aggregated measures of corruption such as the TI index. To be sure, we are not suggesting that research should not be undertaken to explain variation in the TI index. However, special attention must be given to delineation of the individual level incentive structures, often structurally and institutionally induced, that are embedded within aggregated measures of other types of social behavior if we are to achieve useful explanations.

As with any index, the issue of outlying cases requires special consideration. Outliers can significantly distort the meaning of aggregated measures and make meaningful generalizations difficult. These outlying cases pull "average" measurements significantly away from where they would otherwise be

situated. Nevertheless, outliers are part of the empirical reality researchers seek to understand. Simply dropping them from consideration for analytical convenience is inappropriate, and partially closes our analytical eyes to the very phenomenon under investigation. Thus, we believe that the unique features of certain cases must be incorporated into the analysis for a complete explanation of concepts such as political corruption.

How can this be accomplished? Cross-national indices, which lend themselves to the use of scatterplots and measures of deviation from the mean, facilitate the identification of outliers. Once identified, outlying cases can then be brought into the analysis. In quantitative analysis, the uniqueness of outlying cases can be empirically acknowledged through their incorporation as “dummy” variables. While analytically crude, this recognition controls for the special nature of these cases and highlights them for additional investigation. Such inquiry can include building upon previously conducted in-depth qualitative case study research. Or, it may serve as a call to instigate this work. The most important feature about such research is that it be theoretically motivated in a manner consistent with the macro-comparative analysis that initially highlighted its “outlier” status. Researchers’ sensitivity to the possible distortions created by outliers is an important first step in methodologically overcoming problems associated with outlying cases.

Combining several different measures to form an index raises another set of issues to which researchers should remain sensitive. TI’s earliest measures of corruption are not based on as broad a set of surveys as the most recent index. The 1980-85 measures, for example, come from only one or two years of polls performed by two risk analysis firms, Business International and Political Risk Services (www.transparency.de). Analyses using the earlier TI indices may thus be less robust than those using later scores. Moreover, TI’s procedure of averaging corruption scores from available surveys makes comparison across different periods (e.g., 1980-85 and 1990-95) more difficult, as the organization correctly observes.

Several caveats should be borne in mind in comparing a country’s score in one year to the previous year. Because TI includes as many different surveys as possible in determining each country’s score, sources change from year to year; some become outdated and are dropped while new and more reliable sources are often added. With different respondents and small variations in methodologies from year to year, a change in a country’s corruption index score may stem less from a change in its actual performance than from a change in sample and the methodology of the surveys used in any given year. Since TI combines different numbers of surveys (and more recently, surveys from different years) in creating its annual scores, the index is less well-suited to time-series analyses across short periods.

The individual component indices of the CPI, which are more consistent in their data collection procedures, may be more appropriate for pooled time-series cross-section analyses, although they may suffer from other methodological problems mentioned earlier. At the same time, researchers should be aware that in these data sets, variation in levels of corruption across countries dominates within-country temporal variation. The autoregressive characteris-

tics of these data have a number of implications. First, the pooled structure of data may lead researchers to overestimate the amount of information (i.e., the number of observations) they actually have. Second, since the observations are not independent of each other across time, little new information regarding causality will be gained by pooling the data, as opposed to analyzing data for a single year. Finally, analysis of the pooled data is likely to result in biased estimates of causal patterns due to autocorrelation of disturbances unless researchers design their statistical models to cope with this problem (Hannan and Young 1977: 59-69). Given the available cross-national empirical evidence, then, analyses of changes in level of corruption are likely to be less fruitful than explicitly cross-sectional analyses of corruption.

That said, we do not wish to overstate the problem. We are optimistic that the current measures can be used for temporal comparison, if performed between relatively long time spans. And, since most of the individual surveys that constitute the CPI scores demonstrate a high degree of correlation with each other, TI's averaging of scores from varying numbers of surveys through time would not necessarily undermine the use of the CPI in temporal comparison.

Finally, another potential methodological problem inherent in broad, cross-national indices of political corruption is generally referred to as an "averaging out of effects" (Ragin 1987). Measures such as the CPI are, at best, a proxy for individual level acts of corruption. Each corrupt act is unique. This uniqueness is lost when two or more acts of corruption are combined to create a single measure. Comparative analysis of such measures relegates the particularistic aspects of each individual act of corruption to the analysis's error term. Strategies that attempt to achieve generality sacrifice full comprehension or appreciation of the phenomenon's complexity. The tradeoff between generality and complete description is unavoidable, but sensitivity to the issue improves inferences (Jackman 1985).

Early Advances

Bearing the above discussion in mind, research on corruption has made great strides using the CPI and the indices that compose it. Indeed, the potential for even more work in this area is just beginning to be realized. Recent works that have used the CPI or its component indices have focused on a range of topics. Among others, these include (1) the consequences of corruption for economic growth; (2) the causes of corruption; and (3) the effect of corruption on trade, investment and foreign aid.

The effect of corruption on economic growth has been an issue of intense scholarly and policy debate since the 1960s. These debates over the economic consequences of corruption started when a number of scholars questioned the conventional and seemingly moralistic view that corruption was harmful for development.¹² The revisionists, as they came to be known, argued that bribery might actually enhance efficiency at particular stages of a country's development. Many newly independent developing countries were rife with corruption at that time. The revisionists theorized that the movement from tra-

ditional to modern society required substantial investment, and that corruption emerged as a means to fulfill that need. They argued that developing countries tended to be politically unstable, and consequently, investment was unlikely unless some mechanism existed to minimize risk. Bribes played this role. They helped win over government officials who may initially have been indifferent or hostile to particular entrepreneurs. They also helped ensure policy stability in an environment of frequent changes in government personnel. In addition, bribes arguably enhanced efficiency because they provided incentives for bureaucrats to accomplish their tasks more quickly. The increases in investment and productivity from policy stability and more efficient bureaucrats were hypothesized to accelerate economic development. Once developed, the need for massive levels of investment would diminish. The need for corruption, the logic of this argument concluded, would dissipate. Corruption would thus sow the seeds of its own demise.

Thirty years later, it is rarely if ever argued that corruption will die a natural death. The notion that corruption may enhance efficiency, however, retains some currency.¹³ This aspect of the argument continues to be advanced in part because, at least until the 1997 Asian financial crises, most East Asian countries were experiencing phenomenal growth rates despite apparently high levels of corruption. Indonesia serves as an excellent example. Considered extremely corrupt by a number of political risk analysis firms, Indonesia had an average growth rate of 7 percent during 1970-1995.¹⁴ Moreover, the key to this growth may have been the ability of Chinese entrepreneurs to bribe officials. The Chinese in Indonesia have been considered “pariah entrepreneurs” for many decades.¹⁵ Their status as Indonesian citizens was seriously questioned at independence in 1949. Since then, laws were passed restricting their behavior in cultural and economic affairs (Mackie 1976). Yet Chinese investment has clearly been instrumental in the high growth rates that Indonesia has experienced. By 1989, 82 percent of the 200 top business groups in Indonesia were controlled by Chinese interests, and Chinese entrepreneurs were estimated to be responsible for as much as 70 percent of all private industry (Schwarz 1994: 99, 109). How was this possible? As one veteran observer of Indonesia has argued, Chinese dominance of the economy was due not only to their “access to well-established networks of credit, market information, and domestic and overseas trading contacts” but also to their “political connections, bribes, and payoffs to ensure immunity from arbitrary imposts” (Mackie 1992: 165).

An important advantage of the TI index and other cross-national measures is that it allows researchers to determine whether corruption facilitates investment and economic growth, as suggested by the case of Indonesia and the revisionist thesis, or whether the relationship between corruption and growth in Indonesia is in fact spurious. This is possible with a large-N study because the effect of corruption on growth can be examined, controlling for a host of other variables that have also been posited to lead to growth. Four recent studies, each of which uses a different corruption survey and each comparing a minimum of 40 countries, cast serious doubt on the revisionist thesis. They show that corruption is negatively associated with investment (Mauro 1995; Brunetti,

Kisunko, and Weder 1997), expenditure on education (Mauro 1998), and expenditure on the maintenance of infrastructure (Tanzi and Davoodi 1997); and these variables have been shown to significantly affect growth. In addition, Mauro (1995) and Brunetti et al. (1997) show that corruption has no statistically significant effect on growth when controlling for investment. What are the implications of these studies for a country like Indonesia? They suggest that Indonesia secured sufficient investment to develop as rapidly as it did, but it grew *despite* its corruption and not *because* of it. This conclusion would not have been possible without the comparative perspective afforded by cross-country studies.

Cross-national measures are clearly valuable contributions to the comparative study of corruption. Much light has been shed on the debate over the economic consequences of corruption. However, potential problems in the use of cross-country indices are already evident. One such issue in three of the four studies mentioned above is the use of measures for corruption that supercede, rather than precede, the consequences they are hypothesized to generate. Mauro (1998), for example, uses data on corruption averaged from 1982-1995 to explain data on government expenditure averaged from 1970-1985. A related issue to consider more carefully is the appropriate number of years to use when averaging the corruption measures across time. As previously mentioned, reliability of data for a single year, especially those from single sources, may be a problem. A country may score highly in a specific year due to an unusually large corruption scandal. On the other hand, averaging scores over too long of a time frame may obscure important information. The availability of cross-national indices provides enormous possibilities. It would nevertheless behoove researchers to use them judiciously.

A second much-debated issue touched upon by studies using the CPI and other cross-national measures is the question of causes of corruption. The major divide between theories of corruption pertains to the primacy of macro- versus micro-level variables. Macro-level theories tend to attribute the incidence of corruption to particular cultural values or norms, or the gap between norms and values disseminated by older and newer institutions (Bakker and Schulte Nordholt 1996: 10). Variation in levels of political corruption across societies is due to differences in conceptions of public office and the distinction between public and private roles. For example, it has been argued that in Indonesia, using public office to enrich oneself is accepted as "the natural order of things." As a former Indonesian government minister asked: "What is wrong if among...those who fought to uphold the New Order there are those who get the chance to succeed in business?" (Schwarz 1994: 133, 137). From this perspective, until societies internalize the notion of public office as a position held in the interest of the public, government officials will continue to plunder the state treasury, and societal pressures for reform will be minimal. Micro-level theories, on the other hand, focus on the structure of incentives and opportunities faced by individuals.¹⁶ The decision to engage in corruption is considered a function of the costs and benefits to individuals associated with the behavior. Psychological costs in contravening societal values exist, but values are assumed to be violable, even in the short run. Thus, micro-level theories tend to

focus on political and economic institutions that affect material incentives and opportunities associated with corruption.

Since the introduction of the CPI and other cross-country measures of corruption, a handful of studies have investigated the causes of corruption, but the debate over the phenomenon's sources is far from resolved. This is largely due to the difficulty of disentangling macro- and micro-level variables, which has resulted in studies that do not directly address the debate. Ades and Di Tella (1997a, 1997b), for example, focus mainly on the micro-level incentives of entrepreneurs. In one study, they find that corruption is higher in countries that promote national champions through subsidies. In another, they show that corruption is associated with less competitive market structures. Their studies are notable as the first attempts at cross-national statistical analyses of the causes of corruption. Their work, however, suffers from a serious measurement weakness. Both studies include measures of the causes and the effects of corruption from surveys answered *by the same informants*. Use of common sources of measurement means that critical variables may be subject to correlated errors stemming from respondents' biases.¹⁷

Other works on the causes of corruption discuss both macro- and micro-level theories, but their empirical analyses use indicators that arguably measure both types of variables. For example, using different aggregate-level indicators, Sandholtz and Koetzle (2000), and Montinola and Jackman (forthcoming), find that a country's political and economic openness influences its level of corruption. The effects of their aggregate-level indicators on corruption, however, can be attributed to either democratic norms and a transnational business culture, as the former two authors suggest, or to the structure of incentives and opportunities created by political and economic competition, as the latter two argue. A similar ambiguity occurs in the works of La Porta et al. (1999) and Rauch and Evans (1997). LaPorta et al. (1999: 224) examine the relationship between corruption and a macro-level variable, legal tradition, but they acknowledge that legal tradition may be a proxy for a particular pattern of state intervention that could affect incentives and opportunities of individual actors. Rauch and Evans (1997) investigate the relationship between corruption and bureaucratic structure, which they posit produces norms as well as incentives and opportunities that influence behavior. Thus, considerable debate remains over the sources of corruption. For better tests distinguishing the effects of macro- versus micro-level variables on corruption, future research might theorize about, and develop hypotheses regarding, the appropriate lag period associated with each type of cause and corruption.

A third area of research addressed with the CPI and other cross-national measures focuses on the consequences of corruption for external relations and the variation in the inclination of governments and individuals from one country to transact with their counterparts in countries rife with corruption. Lambsdorff (1998), for example, finds that the *export structures* of five countries—Belgium/Luxembourg, France, Italy, the Netherlands, and South Korea—are positively related to the corruption level of importing countries. Alesina and Weder (1999) find *foreign aid* donors behave differently toward corrupt countries. In particular, foreign aid from the United States tends to go to more

corrupt countries, while aid from Scandinavian countries goes to less corrupt ones.¹⁸ Finally, Wei (1997) finds that corruption discourages *foreign direct investment*, and that entrepreneurs in OECD countries react similarly to corruption when deciding where to invest. These innovative lines of inquiry would not have been possible without cross-national measures of corruption.

We are encouraged by many scholars' use of cross-national measures of political corruption. The incorporation of cross-national measures has already begun to provide broader and more robust explanations of corruption. The availability of these measures should challenge other researchers to move beyond mere verbal description of corruption and the inevitable context-specific and unique definitions of the term. Such cross-national measures allow us to take a giant step away from the rather imprecise conceptual demarcation between corrupt and non-corrupt behavior, and to tackle questions that would not otherwise have been possible.

An Alternative Methodology

The view that research on political corruption should be framed within the broader debate on the logic of comparative political inquiry has motivated our discussion of measurement and operationalization. We are encouraged by the recent contributions to the measurement and operationalization of political corruption, but we believe that variable-oriented research on political corruption remains underdeveloped. There remain epistemologically grounded disagreements regarding such macro-level comparative inquiry. One central concern, voiced most strongly by researchers committed to case study and holistic methodologies, is the charge that variable-oriented analysis removes political corruption from its rich context and thus limits explanation. Consistent with methodological debates in other areas of comparative politics, many researchers believe that quantitative macro-comparative measures of corruption fall short of capturing the embedded nature of the phenomena. We therefore believe that students of corruption should not be content with the addition of macro-level measures, such as the CPI. In the spirit of triangulation, we believe other methodologies, especially those incorporating alternative procedures for measurement and operationalization of corruption, should be employed to yield more complete analyses.

Qualitative comparative analysis (QCA) is one procedure researchers might consider to keep variable-oriented analysis of political corruption more deeply embedded in its political, economic, and social context. Most visibly promoted by Charles Ragin, QCA relies on the creation of binary variables, generally measured as the presence or absence of a condition, to create a data set referred to as a "truth table." QCA uses Boolean algebraic methods to reduce patterns of variables to basic expressions of causation (Ragin 1987). It strives to identify overlapping factors associated with particular outcomes. An inherently inductive technique, QCA offers a unique manner by which to make general statements about multiple cases while keeping the variables embedded in their context. Through its qualitative yet focused concern for causal explanation, QCA incorporates the strengths of both case- and variable-oriented ap-

proaches in the assessment of rival explanations of political phenomena. In doing so, it remains loyal to the comparative method.

Applied to the study of political corruption, QCA might help overcome some of the problems associated with the quantitative reduction of political corruption to a single measure. Unlike other techniques, QCA encourages the analysis and measurement of macrosocial units and encompasses the complexity of interrelationships that Tilly warns comparativists not to lose (Tilly 1984). Important to analysts (oriented to) case studies, QCA allows cases to be viewed holistically and thus in the context of their specific conditions. Configurations of variables serve both to describe and explain cases, and to facilitate comparisons between them. Equally important, QCA helps to establish equivalence in the comparison of cases. Going beyond Yin's (1984) "multiple case studies," QCA is best used on a small to medium number of cases.

Case-oriented researchers should also find appealing QCA's requirement that all contradictory cases be accounted for in the analysis. Large-N studies rarely, if ever, attempt to explain all variance in their data. Researchers who favor large-N studies assume that some deviation from predicted outcomes will occur due to random error in measurement and the inherently probabilistic nature of the phenomena we seek to understand. They also assume that models identifying all possible explanatory variables and interactions among variables will be rare (Lieberson 1991: 106-7). In regression models, an error term absorbs this unexplained variance. Since QCA has no error (or residual) term, all contradictory cases must be explained. When contradictions arise, the researcher using QCA must either add an additional variable to separate the configurations or recategorize the dependent variable. Contradictory cases that arise in QCA thus redirect our attention to particular cases, hopefully producing better models.

As a multivariate technique, QCA should also appeal to variable-oriented researchers of political corruption. Its advantages over techniques such as regression analysis add to this appeal. For example, some multivariate statistical techniques provide only for the statistical significance of relevant variables. They permit the researcher to infer that a certain variable may have a larger impact on all the cases as a whole. But they are not as clear in determining the relative impact of each variable in specific cases. QCA, in contrast, requires that the researcher directly implicate the number of cases for which a variable is important. QCA demands definitive statements about either the presence or absence of a case's particular characteristics. In this regard, it helps establish necessary and sufficient conditions for a certain outcome. Necessary conditions are satisfied when a specified variable is present in all possible configurations leading to either the presence or absence of the dependent variable. Sufficient conditions are satisfied when one variable can, on its own, supply explanatory power.

To the best of our knowledge, no study of political corruption has utilized QCA. Since operationalization and measurement issues about political corruption should be viewed within the broader context of the logic of comparative analysis, the field might learn from other areas of research that have utilized QCA. The technique has been used to explain: Latin American revolutions (Wickham-Crowley 1991; 1992); ethnic political mobilization in 15 Soviet

successor states (Taras 1993); labor management practices in the textile industry (Coverdill, Finlay, and Martin 1995a; 1995b); interracial solidarity in labor disputes (Brown and Boswell 1995); social welfare programs (Hicks 1994; Ragin 1994; Ameta and Poulsen 1996; Kangas 1994); and investments and mortality rates in Britain, France, Sweden, and the United States (Hollingsworth, Hanneman, Hage, and Ragin 1996). In many of these and other studies, QCA is directly compared to other analytical techniques.

To help clarify the logic behind QCA, we present a hypothetical example, one adapted to the issue of political corruption from actual data and analysis on another subject entirely.¹⁹ We emphasize that this particular article is not the appropriate place for full instructions regarding QCA.²⁰ The following imaginary example should nevertheless give the reader a “taste” of the advantages and disadvantages of utilizing QCA to study comparative political corruption.

We start our analysis with two countries: *Alpha*, reputed to be relatively corrupt-free, and *Omega*, widely perceived to be rife with graft and corruption in its public affairs. What causal factors might explain the difference in levels of political corruption between countries such as Alpha and Omega? Since we seek to generalize about political corruption beyond specific case studies, we include in our study as many cases for which we have data, which in this hypothetical example is 17.

With QCA, the “data collection and management” stage involves the construction of a “truth table” that includes information about our population (See Table 2.). The first column in the truth table lists the names of the countries (cases) to be analyzed. The second column contains the dichotomized value of the “dependent” variable for each respective case: coded as “0” if the case is

Table 2
A Truth Table: Hypothetical Data

Case	Clean Government (C)	Open Economy (B)	Universal Literacy (E)	Dictatorship (D)	Social Heterogeneity (G)	Independent Judiciary (I)
A	0	0	0	0	0	1
O	1	1	0	1	0	1
H	1	0	1	0	1	1
J	0	0	0	0	0	0
K	1	1	1	0	1	1
L	1	1	0	0	1	1
M	0	0	0	0	1	0
N	1	1	0	1	0	0
P	0	0	0	0	0	0
Q	0	0	0	1	0	0
R	0	0	0	1	0	0
S	1	1	1	0	1	1
T	1	1	1	0	0	1
U	1	1	1	0	1	1
V	1	1	1	0	1	0
W	0	1	0	0	0	1
Z	0	0	0	0	0	0

Source: Data fabricated from actual example in Hicks (1999), p. 143.

closer to the non-corrupt status of Alpha; and “1” if it is more like Omega in terms of political corruption. The remainder of the table contains a similar dichotomy of values for the explanatory variables—derived from hypotheses elaborated in the literature. In this example, these are the absence or presence of (1) an open economy; (2) a generally literate society; (3) a dictatorial legacy; (4) substantial social heterogeneity; and (5) an institutionally and de facto independent judiciary. A value of “1” is assigned in the truth table to the respective cell for that particular case and variable if the country can be characterized as having the conditions mentioned above. Otherwise, the cell is assigned a score of “0.”

Once completed, Boolean methods fully explained in Ragin (1987) can be utilized to solve this truth table. The solution to the truth table for this hypothetical example on political corruption produces the following result:

$$\text{Eq. 1: } C = EdI + eDgB + dGI + EdGiB$$

This can in turn be reduced to:

$$\text{Eq. 2: } C = Id(E + G) + B(eDg + EdGi)$$

Where C = clean government and c = high political corruption
 D = dictatorial legacy and d = democratic legacy
 G = socially heterogeneous and g = socially homogeneous
 I = an independent judiciary and i = a non-independent judiciary
 B = an open economy and b = a closed economy
 E = characterized by universal literacy and e = not universally literate

In this artificially constructed example, QCA permits the researcher to summarize a great deal of information—data from 17 cases and 6 variables for a total of 102 pieces of information—to present succinct causally focused explanations of cross-national variation in political corruption. From Equation 2, we derive two conclusions. First, an independent judiciary in democratic countries suffices to produce clean government when such countries are characterized either by a high literacy rate (countries H, K, S, T, and U) or by social heterogeneity (country L and again country H). Second, an open economy suffices to maintain clean government when countries are characterized by the combination of either (i) non-universal literacy, dictatorship and social homogeneity (countries O and N); or (ii) universal literacy, social heterogeneity and non-independent judiciaries (country V). In effect, QCA’s emphasis on pattern identification through a Boolean reduction of information highlights the association of clean government and democracies with independent judiciaries (6 out of 9 cases) and the importance of an open economy in accounting for the other three cases in this hypothetical example.

Several points should be emphasized about the potential to apply QCA to the study of political corruption. First, QCA is a viable alternative to macro-

level quantitative studies. It can add rigor to the construction of small-N studies. Second, while adhering to the importance of variable-oriented and causal analysis that characterizes the comparative method, QCA incorporates context into the analysis. It permits consideration of various historical paths, conditions, and configurations into the study of complex phenomena in a manner that might otherwise be restricted due to the lack of precise data. Third, QCA utilizes causal analysis to examine configurations of conditions for the presence or absence of a phenomenon. It thus enables alternative causal configurations to emerge analytically. For these reasons, we believe the comparative study of political corruption might greatly benefit from the utilization of QCA.

As with all research methodologies, QCA is not immune to criticism. We briefly mention a few of these. First, the binary nature of its data is simplistic; its measurements indicate only the presence or absence of a characteristic or outcome. While its sensitivity to both the presence and absence of variables in particular cases is beneficial, QCA reduces all presumably continuous measures into dichotomous categories. Such a binary operationalization of all variables often entails a loss of information.²¹ With regard to political corruption, QCA would force a binary classification of both corruption and the variables potentially causally associated with it. On a positive note, this would facilitate the scope of societal, cultural, and historical variables that a researcher might wish to consider in the explanation of political corruption, or its impact. Negatively, a binary measure of corruption would sacrifice the large amount of variation suggested in other measures, such as the CPI, so that shades of corruption would be ignored. Second, QCA cannot assess the relative significance of causal factors. It cannot say which of the conjunctionally linked variables is more or less important in terms of explanation. It can only account for the presence or absence of a variable in these causal configurations (Liebersohn 1991). Third, QCA's binary measurements are quite sensitive. A change of a single value of any variable may produce significant shifts in causal conjunctures. This sensitivity forces researchers to pay close attention to their data to ensure that each value on each variable is correctly coded.

Despite its shortcomings, QCA has considerable potential for the comparative study of political corruption. Its greatest strength, an ability to bridge case- and variable-oriented research methods and philosophies, has already been demonstrated in other areas of comparative political analysis. Future research into political corruption should take advantage of QCA's analytical technique to draw from, and build upon, the vast qualitative literature already produced. Using previous descriptive and theoretically driven case studies as data sources for QCA's binary measures could open the doors for significant work on corruption. QCA's ability to handle more cases than traditional case studies would greatly enhance the *comparative* study of corruption. Similarly, QCA's variable orientation, with its use of Boolean algebra, keeps causal analysis at the forefront of our work. As a potential bridge across various research traditions, it should at least be utilized to supplement other methods in the investigation of political corruption.²²

Conclusion

This article has been framed by wider debates about the appropriateness of different methodological approaches to comparative research. Epistemological questions about research objectives, methodological tools, the nature of evidence, and the type and number of cases to utilize are present, whether acknowledged or not, in any research on political corruption. Recognizing that some scholars have other epistemological goals and make different methodological assumptions, we are explicitly concerned about empirically based comparative explanations of political corruption. Thus, we have chosen to highlight questions about operationalization and measurement as topics for debate.

The comparative study of political corruption has many methodological avenues open to it. Until very recently, however, case studies dominated this area of research. Case studies have an important role in research on political corruption, and in comparative political inquiry more generally. Case studies should be part of a strategy of triangulation, or multiple methods. Variable-oriented scholars should not dismiss outliers of their studies as irrelevant. The use of case study methods to explore outlying cases makes such research more persuasive and expands the theory upon which their hypotheses are built (Peters 1998: 21-2). Similarly, case study scholars may not be fully cognizant that they are studying outliers unless they compare their case to the larger population. Scholars of each of these general research strategies should, as suggested by our emphasis on questions of methodology, remain open to the advantages, and weaknesses, of the other generic approach. Nevertheless, throughout this essay we have been motivated by the belief that comparativists can establish universal principles and causal claims about corruption. Recent developments in the measurement of corruption have moved us considerably closer to that goal.

Notes

- * The authors would like to thank Richard Doner, Robert Jackman, and the editor and referees of *SCID* for their helpful comments.
- 1. In a previous article (Lancaster and Montinola 1997), we focused on the difficulty of defining corruption and a few problems of measurement.
- 2. For recent systematic case studies on corruption, see Geddes and Ribeiro Neto (1992); Manion (1996); and Manzetti and Blake (1996).
- 3. This argument is also made by Mark Philp (1994).
- 4. Note that “influence” would not be considered corruption by the narrow definition—misuse of public power for private gain, although it may fall under most other definitions. Furthermore, the emphasis on difference in distribution of rents between administrative corruption and state capture can be misleading. Private actors may gain substantial amounts through administrative corruption, as a consequence, for example, of tax evasion or the procurement of a license. Alternatively, public officials may gain little especially compared to the substantial rents private actors can reap due to state capture. The more insightful distinction implicit in Hellman et al. (2000b) is that regarding the arena of corruption: legislative versus the implementation stage. Other terms used to convey this distinction based on arena are: political vs. bureaucratic corruption; and grand vs. petty corruption. See Lancaster and Montinola (1997: 191–2) for a few of the other means of classifying different forms of corruption.

5. Other surveys that have been included in Transparency International's Corruption Perception Index include surveys by Hong Kong-based Political and Economic Risk Consultancy and the Institute for Management Development in Lausanne, Switzerland.
6. It might be useful to require respondents to be familiar with a specific country considered a reference point and then to specify a score for other countries relative to the reference point.
7. International Country Risk Guide, "Brief Guide to the Ratings System," February 2000, p.A-7. The guide notes that because corruption is often hidden until a scandal emerges, raters also consider, as an indicator of corruption, the duration of governments in power, assuming that long-standing governments are likely to be more corrupt than those in power for shorter periods.
8. The World Bank appears to be extending their survey to cover some of the omitted countries (Hellman et al., 2000b: 2).
9. See Hellman et al. (2000a) for a detailed description of the survey.
10. See Lambsdorff (2000) for a more detailed description of the aggregation process.
11. Only one survey, the International Crime Victim Survey (ICVS) stands apart from the others, displaying relatively low correlations ($r = 0.41$ and $r = 0.45$) with surveys by Political Risk Services (PRS) and the Economist Intelligence Unit (EIU). These lower correlations likely reflect validity issues, which we highlighted earlier. The ICVS index, a recent addition to the CPI, is the only one incorporated in the 2000 CPI that elicits perceptions of corruption from the general public, and asks whether respondents *themselves* were asked to pay a bribe in exchange for a government service. In contrast, the PRS and EIU indices are two of three in the 2000 CPI composed of expert staff assessments on the general level of corruption in particular countries.
12. See, for example, Leff (1964); Bayley (1970).
13. See, for example, Lien (1964); Liu (1985); Braguinsky (1996).
14. Calculated from data in World Bank's 1997 *World Development Indicators* (CD-ROM version).
15. The term is from Riggs (1966).
16. For a comprehensive review of works that focus on incentives and opportunities as causes of corruption, see Susan Rose-Ackerman (1999).
17. Ades and Di Tella (1997a) are aware of this problem and attempt to address it in one study by using harder indicators for their key variables. Data constraints, however, leave them with as few as sixteen cases, so that their results are affected by both small-N and sample composition issues.
18. Alesina and Weder (1999) find that countries characterized by high levels of corruption receive more foreign aid.
19. Hicks (1999). The authors warmly thank Professor Alex Hicks for suggesting these data and for his advice and consent in working through them.
20. For this type of instruction, we suggest the reader consult Ragin (1987) and Hicks (1999).
21. For example, a relationship between level of democracy and corruption would likely be evident if a binary measure of democracy and corruption is utilized, but the particular curvilinear relationship that Montinola and Jackman (forthcoming) find would likely have been masked.
22. Other techniques should also be considered. For example, the use of counterfactual analysis might shed insight into our understanding of political corruption. On counterfactuals, see King et al. (1994).

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Appendix 1
ICRG Corruption Score, January 2000

1	2	3	4	5	6		
Congo, D	Albania	Moldova	Argentina	Namibia	Austria	Australia	Canada
Gabon	Algeria	North Korea	Bahrain	Oman	Bahamas	Costa Rica	Denmark
Iraq	Angola	Pakistan	Belgium	PNG	Belarus	Estonia	Finland
Lebanon	Armenia	Panama	Bolivia	Peru	Bulgaria	Greece	Iceland
Myanmar	Azerbaijan	Paraguay	Botswana	Poland	Chile	Hungary	Netherlands
Niger	Bangladesh	Qatar	Brazil	Romania	Congo, R	Luxembourg	Sweden
Nigeria	Burkina Faso	Saudi Arabia	Brunei	Senegal	Cyprus	New Zealand	
Russia	Cameroon	Tanzania	Ecuador	Sierra Leone	Czech Republic	Norway	
Somalia	China	Togo	France	South Africa	Dominican Rep	Portugal	
Sudan	Colombia	Turkey	The Gambia	South Korea	El Salvador	Switzerland	
Yugoslavia	Côte d'Ivoire	Uganda	Guinea	Suriname	Germany	United Kingdom	
	Croatia	UAE	Guyana	Taiwan	Guatemala		
	Cuba	Vietnam	Hong Kong	Thailand	Iran		
	Egypt	Zimbabwe	India	Trinidad	Libya		
	Ethiopia		Israel	Tunisia	Madagascar		
	Ghana		Italy	Ukraine	Malta		
	Guinea-Bissau		Jamaica	Uruguay	Mozambique		
	Haiti		Japan	Venezuela	Nicaragua		
	Honduras		Jordan	Yemen, R	Philippines		
	Indonesia		Kazakstan	Zambia	Singapore		
	Ireland		Latvia		Slovakia		
	Kenya		Lithuania		Slovenia		
	Kuwait		Malawi		Spain		
	Liberia		Malaysia		Sri Lanka		
	Mali		Mongolia		Syria		
	Mexico		Morocco		United States		

Source: Political Risk Services, "International Country Risk Guide," January 2000.