Guest Editorial: Special Issue on Corruption at the Grassroots-level

What Can We Know About Corruption?

A Very Short History of Corruption Research and a List of What We Should Aim For

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1 Introduction

Corruption research has centered on three fundamental questions: What determines corruption? What effects does corruption have? And finally, what can we do to reduce corruption effectively and efficiently? Corruption has been, and continues to be researched so intensively that it is hard to keep abreast of all the latest developments in the various subfields of corruption research. Recent surveys of the vast literature include Aidt (2003), Svensson (2005), Lambsdorff (2007), Pande (2007), Olken and Pande (2011), and Kis-Katos and Schulze (2013). Instead of adding yet another survey, we focus on the main challenges of empirical corruption research and show by example how the literature has tried to address these challenges.[1]

The first and foremost challenge of empirical corruption research is that corruption is not directly observable, at least not in any systematic fashion as it is illegal and therefore clandestine. Thus, proxies for actual corruption levels need to be found or controlled situations need to be created in which corruption is observable. Proxies frequently used are corruption perceptions, stated corruption experiences, audit results of public expenditure tracking analyses or quantitative service delivery surveys, corruption incidents registered with law enforcement agencies, and corruption convictions. Controlled situations include laboratory and field experiments.[2] Moreover, there are some natural experiments as well.

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[1] We do not discuss the usual challenges of empirical research such as the search for a convincing strategy to identify causal relationships or sample representativeness but focus on those that are particular for corruption research.

[2] Closely related is the literature on politically connected firms that looks into the value of these political connections for the firms and their differential behavior (Faccio 2006) and on excessive spending of governments on themselves rather than on people they govern (Sjahrir et al. 2014). Both may not be illegal in the strict sense but fit the common definition of corruption as 'misuse of public office for private gain'.

Second, corruption is highly context-specific as it depends on the institutional setup, stage of development, but also norms and culture (*inter alia* Fisman/Miguel 2007; Kis-Katos/Schulze 2014). This begs the question to what extent results derived in one setting carry over to other settings as well. External validity is a major concern not only for experiments, both laboratory and real-world, but also for country-specific econometric studies.

Third, the type of corruption for which sufficiently good proxies exist may not be the most harmful. For instance, corruption in the allocation of driver's licenses (Bertrand et al. 2007) or in issuing identity cards (Kaiser et al. 2006) may be harmful, but corruption between the political elite and the business community in form of cronyism, preferential access to government contracts or freedom from prosecution may be fundamentally more important and much harder to measure. The former may be proxied sufficiently well by surveys of users of public services, whereas the latter, often referred to as *grand corruption*, is extremely hard to quantify in all of its consequences.

Corruption research in economics has a long history, seminal early articles include Rose-Ackerman (1975) and Shleifer and Vishny (1993). We identify two generations of corruption research. The first generation has focused on cross-country analyses, mostly using corruption perception indices as a proxy for real levels of corruption. The second generation has taken a micro-perspective using microeconometrics and experiments to analyze corruption at the individual, household or firm level. We discuss the generations with the help of one or two examples. The first generation included also grand corruption. Data on perceived levels of corruption include assessments by senior business leaders, respondents who are capable of judging corruption at the elite level. The second generation has advanced our knowledge on the causal processes that underlie corrupt transactions. But it tends to focus more on petty corruption because this is easier to measure. This is just one of many trade-offs that we discuss subsequently.

2 Macro studies

Macro studies have used the variation of corruption levels across countries in cross-section or panel analyses. One strand of literature has focused on the consequences of corruption pioneered by Mauro's (1995) article on the effect of corruption on growth and investment. This has initiated a wave of subsequent studies that largely corroborated Mauro's findings; i.e. a negative impact of corruption on levels of investments (measured as a ratio to GDP), which is the most important causal link to the impact of corruption on the growth of GDP (Lambsdorff 2007: 73–75; 100–102). A variety of further dismal effects of corruption have been found, for example relating to increased inequality of income or higher military spending.

A second group of studies has focused on the causes of corruption. One representative example is Treisman (2000) who tests twelve hypotheses on the determinants of perceived corruption levels. He runs on a sample of 34 to 64 countries four sets of cross-sectional regressions with Transparency international's Corruption Perception Indexes for the years 1996, 1997, 1998 and Business International's index for the early 1980s as endogenous variables. Explanatory variables comprise controls for the legal system, colonial status, percent age of Protestants, ethnolinguistic division, exports of fuel, minerals and metals, log GDP per capita, and federal state organization as well as democratic history, import penetration, state intervention, government salary levels and government turnover. Treisman finds, among other things, that countries with a higher share of Protestants,

histories of British rule and with a democratic history and higher import penetration are less corrupt, federal states more corrupt.

The challenge with cross-country studies, as it is often the case with macro data, is that causation often remains suggestive, even if statistics are carried out with utmost care. Two-way causation exists between corruption and many socioeconomic variables. For instance, corruption is likely to inhibit economic development and to increase poverty. Yet, corruption may also result from poverty partly because resources for anticorruption are scarce. More importantly, poverty and corruption may also be the simultaneous consequences of persistently bad institutions brought about by the colonial rulers (Acemoglu et al. 2001) – they are both endogenous to the same cause.

In order to identify causation, appropriate instruments have to be found for the endogenous regressors. This often proves difficult, especially if the endogenous regressors are time-variant. What might serve as an instrument for corruption that affects poverty only through corruption? Or, to identify the reverse causality, which variable might serve as an instrument for poverty without affecting corruption also through other channels?

There have been two strategies to dealing with these problems, none of them reaching perfection. The first is a pragmatic one: Do not investigate relationships where two-way causation is a major problem. This would for example be true of the relationship between poverty and corruption. Instead of embarking on the mission impossible of finding instruments for one of these variables, one would rather look for relationships where two-way causation is less of a problem. Rather than testing for poverty, one might investigate the impact of corruption on economic growth if researchers consider reverse causation in this case to be less likely. Rather than seeking the "right" statistical model, such a pragmatic approach assembles supportive statistical evidence. Testing for a varying battery of control variables allows researchers to assess the robustness of their findings. Any such approach should end with a discussion of potential omitted variables and derive plausible arguments on their relevance.[3] This might be complemented by suggesting instruments and testing their properties. But one needs to remain pragmatic even here – the exogeneity of instruments is nowhere iron-clad. The strength of the findings thus hinges on theoretical arguments that such a bias is implausible. In some cases one might need to be modest and settle for interesting correlations rather than for causation.

A second approach has been to exploit the dynamics that goes along with causation. A cause commonly precedes its effect, such that observations on the time-varying properties of certain variables may help identify causation. A simple example would be to use lagged data, stating the belief that these are exogenous and cannot be caused by more recent developments of the endogenous variable. This would allow for identifying within-country variation over time, for example by help of fixed effects panel regression.

Yet, we still know rather little about the dynamics of corruption. It might well be that levels of corruption are rather persistent, remaining almost unaltered for decades. In this case, they are highly correlated over time, such that lagged data may not be exogenous. The same might be true of some fundamental causes of corruption, such as limits to trade, low levels of openness, abundance of natural resources, or some cultural variables. If these

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[3] An inherent problem of cross-country analyses such as Treisman (2000) are unobservable or immeasurable variables influencing corruption levels such as culture or a permissive attitude towards corruption leading to unobserved heterogeneity. If these variables are correlated with included control variables an omitted variable bias will result.

variables change little over time, focusing on within-country variations may not lead to statistically significant results.[4]

The measure of corruption most often used in cross-country analyses is not beyond criticism. Corruption perceptions have three distinct advantages: They are easy to compile and readily available, different perception measures are in many instances highly correlated indicating that they measure "the same thing" and lastly perceived corruption (not necessarily actual corruption) is relevant for a number of economic decisions, in particular investment decisions. On the downside, perception-based measures may be prone to perception biases or interviewer biases, leading to a low correlation of perceptions and experiences of corruptions (Mocan 2008; Donchev/Ujhelyi 2013). Corruption experiences are more accurately measurable than corruption perceptions as they do not require a common yardstick of what is a high, medium or low level of corruption. Also, they are not prone to perception biases. Still, measures based on experienced corruption are not beyond criticism. Experience is easily measured in street-level situations, where the general public is subject to harassment by low-level bureaucrats, such as for avoiding speeding tickets or getting access to school and hospitals. But these levels of experienced corruption may not well capture the cases of grand corruption. This brings us back to our third challenge. Perceptions data might be measured with less precision as compared to data on experienced corruption, but they might relate to areas where corruption is more harmful.

3 Microstudies

Cross-country (panel) analyses have provided valuable insights; their inherent drawbacks described above have led to a second generation of corruption studies that have complemented previous evidence. Their unit of observation is the firm, the household or the individual, mostly at the subnational or regional level. This reduces unobserved heterogeneity because the relevant institutional setup, culture, history and other factors that cannot be controlled for are rather similar. As a consequence the likelihood and the extent of a potential omitted variable bias is much smaller; at the same time external validity becomes a concern. To what extent are the results generalizable to other contexts?

The microstudies differ with respect to the methodological approach taken to measure corruption. They include econometric analyses of household and firm surveys on corruption experiences, public expenditure tracking, and quantitative service delivery surveys that measure financial leakage and lacking supply. Experimental evidence measures the extent of corruption in a controlled environment in the lab or in the field.

3.1 Econometric evidence I: household and firm surveys, expenditure tracking

Household or firm surveys ask respondents for their experiences with corruption or for what they consider a "typical level of corruption" in their line of business (Reinikka/Svensson 2006). This approach reduces perception biases as people refer to own experiences, yet it does not eliminate answering biases as corruption is illegal and in most settings

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[4] A good example for this is Van Rijckeghem and Weder (2001) who show that corruption is significantly negatively related to bureaucrats' relative pay – a result that is highly significant in pooled OLS cross-country regressions, but not significant at the usual levels in fixed effects panel regressions, even though it is very plausible and has been corroborated in other contexts (e.g. Schulze et al. 2013).

immoral and thus respondents are hesitant to implicate themselves. Consequently many respondents are reticent (e.g. Clausen et al. 2010). Examples for this approach are Rand and Tarp (2012), Henderson and Kuncoro (2011), and McCulloch et al. (2010).[5] These surveys mostly capture only frequent forms of corruption that many people experience, which may not be the most important ones. For instance corruption at the level of government, such as rigged public tenders in government procurement, may be experienced only by a few firms that may be very reluctant to make this public.

Public expenditure tracking surveys seek to evaluate the leakage of funds that occurred from the source of the funds, i.e. the central government or a donor organization, to its final destination such as schools or hospitals. This information is either based on special surveys or audit reports. Examples include Reinikka and Svensson (2004) for the former and Ferrez and Finan (2008) for the latter. If audits are not random – as they are in the case of Ferrez and Finan –, inference will be made on a biased sample. Yet, from an auditing agency's perspective profiling rather than random sampling may be the best strategy. Quantitative public service delivery surveys are particularly suited to measure corruption that seeks non-monetary favor as in the case of teacher absenteeism (Kremer et al. 2005).[6]

3.2 Econometric evidence II: All the rest

A relatively new and still small strand of literature has looked at law enforcement data as a measure for corruption (Glaeser/Saks 2006 and Alt/Lassen 2014 for the USA, and Schulze et al. 2013 for Russia). This is arguably a more objective measure of corruption, *if* law enforcement and institutional setup are the same across all units of observation. Thus this measure is useable only in subnational studies and only if there is no regional variation in the intensity of law enforcement or if a possible difference can be appropriately controlled for. For instance Glaeser and Saks use only FBI data, as state and local police may have different zeal, resources, mode of operation and legal environment. Alt and Lassen (2014) and Schulze et al. (2013) control for effectiveness and resource endowment of law enforcement agencies.

Another approach was employed by Escresa and Picci (2014) by focusing on cross-border corruption. They process 734 court cases on cross-border corruption (298 from the USA, 84 from Germany, 55 from UK, 49 from France ...), embracing convictions and ongoing cases between 1998 and 2012. They observe the frequencies of countries where the alleged transgression took place (74 in China, 40 in Nigeria and India, 22 in Kazakhstan, 21 in Brazil ...) and convert these figures into per-capita numbers. The resulting measure well represents the probability that a randomly chosen person in a country is investigated for cross-border corruption. The analysis builds on the assumption that a court in the USA or in Germany will apply identical standards of judgment and the respective law enforcement agencies investigate with homogeneous intensity, irrespective of whether corruption relates to business in Canada, China or Nigeria. The resulting measure may portray a country's overall level of corruption relatively well. Escresa and Picci's findings are interesting in so far as they largely replicate the TI Corruption Perceptions Index, indicating that perceptions data may in some contexts be a good proxy for real levels of corruption.

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[5] There are also cross-country studies using surveys on corruption experiences, see for instance Mocan (2008).

[6] Teacher absenteeism can also be analyzed by randomized control trials, cf. Duflo et al. (2012)

3.3 Experimental evidence from the lab

There are three major, closely related advantages of laboratory experiments as compared to other methods for research. First, the experimentor is by definition in control of the experiment, which allows for identification of causation. If findings differ significantly across treatments, they must be related to the differences that the experimenter implemented from one treatment to another. Second, incentives can be manipulated easily. Experiments can be designed in response to research questions. This is in contrast to macro studies and many field studies where the important data are unavailable, unretrievable, or too expensive to be obtained and where crucial questions must remain underresearched. A third advantage is that the laboratory allows for a closer look at the psychological determinants of corrupt behavior. What do people in a corrupt environment expect from each other, how guilty do they feel, and how might such feelings be affected by the beliefs they hold vis-a-vis each other?

This leeway in addressing important questions and the close scrutiny of human beliefs and incentives have led to a wave of experiments related to corruption. Do penalties impact behavior in ways that are predicted by theory? How do subjects value monetary versus non-monetary goals, such as donating to a charity or delivering decent work? In how far does trust among criminal actors promote corruption? Should the legal code provide leniency to those who confess their infractions? Do subjects feel responsible for their infractions and how might responsibility be affected by elections or monitoring? How might perpetrators justify their behavior or bias their self-image? Is transparency always helpful or might it backfire? These are just some of the many questions where substantial evidence has been collected recently (Lambsdorff 2012).

To illustrate this, many approaches have been made on whether women are less corruptible. Cross-country data revealed that corruption is less pronounced in countries with a high percentage of women in parliament and in the labor force. But causation is difficult to address. Evidence from the lab has revealed that women tend to be more pro-social and more risk-averse (Chaudhuri 2012). But whether and if so how this finding may carry over to issues of corruption remained to be clarified. Early evidence was provided by Frank and Schulze (2000) and Schulze and Frank (2003), who show that women are no less corrupt in non-risky situations, but reduce corruption more than men if risk of detection and punishment is introduced. Lambsdorff and Frank (2011) and Frank and Lambsdorff (2010) find that men reciprocate a bribe more often than women, while women tend to cheat the briber, taking money without giving the advantage. This suggests that men have a higher sense for positive reciprocity. Men are also seen to be more willing to engage in negative reciprocity. Bribers were given the option to exercise costly punishment. This option was exercised more often by cheated men. Similar results are reported by Rivas (2013) who runs a more complicated game across many periods. This finding has revealed how gender causes corruption, while at the same highlighting the deeper motivations that underlie this link.

Another interesting finding relates to delegation. Bribery often involves intermediaries who have the criminal expertise and resources to carry out the dirty work. Bribers may not only seek the expertise but alleviate the moral burden of their transgression. Hamman et al (2010) show for dictator games that contributions to recipients decrease almost to zero when dictators choose between competitive agents who announce upfront how much of the dictator's money they would transfer to the recipient. Acting through the intermediary allows dictators to distance themselves from the norm of fairness. Consequently,

they express little responsibility for the recipients' payoffs when having made use of intermediaries. This insight has been applied to corruption by Drugov et al. (2014), who find that officials expressed a higher willingness to take bribes from intermediaries and accepted lower bribes. Clients more frequently offered bribes when this was arranged by intermediaries. Intermediaries may thus enhance corruption by reducing the moral costs of bribery.

Laboratory evidence has been instrumental in getting deeper insights into the motivations that underlie corrupt transactions and getting clearer guidance for reform. But the external validity of these findings continue to be the biggest challenge. This is already true for experiments on standard human behavior (Levitt/List 2007a,b) and holds even more for those related to corruption. Data are derived mostly from a pool of undergraduate students in an artificial environment where incentives are designed that are only loosely linked to the corrupt incentives one may find in reality. In particular, the moral implications of a lab situation may be significantly different from a real world situation as are the consequences of being detected and punished. Researchers thus often have a hard time stating that their findings from the laboratory may also hold in real-world circumstances.

3.4 Experimental evidence from the field

External validity appears to be stronger for field experiments as they can involve actors and environments that are the direct targets of reform. They can robustly reveal actual behavior- for instance they may show whether a reform method works in a specific environment. For the latter two important requirements have to be met: First, data must be obtained from an environment where the reform was implemented and compared to a control treatment with no such reform or a different type of reform. Second, treatment group and control group should not be endogenously determined. This would be the case if the reform was implemented in areas where it was needed more or believed to be more successful.

One example for a field experiment relates to driver's licenses in India (Bertrand et al. 2007). The authors investigate the effect of bonus payments on the behavior of Indian applicants who wanted to obtain a driver's license. Individuals were randomly assigned to three different groups. One group was given free driving lessons, another a bonus payment for obtaining the license within 32 days and a control group where none of these measures were implemented. Data were obtained on whether the applicants were successful in obtaining the driving license, how many days this required, whether they participated in the official test, whether they engaged an agent to facilitate the application (which may involve bribery) and how they performed in an independent test of their driving qualifications. Those who were given a bonus were less qualified in driving, less often participated in the official test and more often engaged local agents to arrange things. The willingness to pay bribes is thus related to need (due to lack of driving capabilities) and advanced further by monetary incentives. The study highlights how important it is to randomly assign individuals to the groups to avoid that they might sort themselves into their preferred group. Differences between groups would no longer be caused by differences in treatments but by those of the underlying self-selecting sample.

Another method for arriving at valid comparisons is when nature randomly selects individuals into treatments or when nature changed the environment such that differences can be observed over time. One example of such a natural experiment is Olken and Barron (2009), who study extortions payments paid by truck drivers along two main roads

in Aceh, an Indonesian province where separatist guerrillas had long been active. Over nine months in 2005 and 2006, data gatherers accompanied the truckers on 304 trips to and from Aceh, recording more than 6,000 illegal pay-offs at military roadblocks, police checkpoints, and weigh stations. This allowed some tests on the validity of economic theory. First, as the trucks neared their destination checkpoint officials demanded increasingly larger sums. This shows that drivers found themselves with a progressively stronger incentive to avoid hassle and safeguard their cargo, which gave the extortionists greater power over them. Midway through the study, after the Indonesian government had signed a cease-fire with the rebels, a phased withdrawal of 30,000 troops began, leading to a fall in the number of checkpoints. The amount lost to extortion decreased, but only by 36%. Fewer stops meant less frequent extortion, but this was offset by a rise in the amounts demanded at the remaining checkpoints, whose operators captured part of the newly liberated surplus. This reveals the economic logic that corrupt officials behave like monopolists, setting their prices so as to maximize their own revenue, without considering the response of the fellow at the next checkpoint, or whether their activities would deter truckers.

Another example of a natural experiment is reported by Vicente (2010). He shows that the announcement of oil discoveries in Sao Tome and Principe in the late 1990s led to an increase in corruption as compared to Cape Verde, the control economy that had previously experienced a similar decline in corruption and that had a very similar colonial past and economic performance in the present. Households in São Tomé and Príncipe reported increased perceived levels of vote buying and corruption in customs.

3.5 The value of political connections

Closely linked to field experiments on bribery and corruption is the analysis of the value of political connections. Such studies use cross-section and panel econometrics as well as event studies. This literature investigates whether firms that have politicians or their relatives on the board or as owners perform differently. Since the seminal paper by Faccio (2006) a substantial literature has developed. The measurement of the effect of political connections may indicate corrupt behavior if, for instance, politically connected firms have preferential access to government contracts or finance by state-owned banks (Khwaja/Mian 2005). Yet, such correlations do not necessarily prove the existence of corrupt behavior; in particular, they do not prove causality: If politically connected firms were observed to be more productive, politicians could join the boards of more productive firms or firms could become more productive because politicians joined their boards. Event studies can explain changes in firm values by unexpected, exogenous political events. Such events lead to an exogenous change in the value of political connections and cause a change in the firm value (e.g. Fisman 2001 and Ferguson/Voth 2008). Such events are rare; moreover to establish the relevant political connections is far from straightforward.

The advantage of this literature is that it captures indirectly a particular type of *grand* corruption for which in most cases sufficient data are unavailable. The abuse of political office for private gain can be brought about by direct payments such as kickbacks in public procurement (which is unobservable in large numbers) or through various channels that increase the firm value of the firms that are politically connected (and may have paid for the political connection one way or the other).

4 Research desiderata: What is next?

Laboratory experiments provide experimenters with the highest level of control. Variations across treatments are in the hand of the experimenter. Data is collected not only on behavior, but also on attitudes, beliefs, and perceptions of the respective environment, for example the salience of social norms. Studies allow a deep understanding of human behavior and how it relates to beliefs about the (potentially corrupt) context. This depth of control and understanding will continue to make laboratory experiments a workhorse for research. But the contexts tend to be rather artificial and the subject pools are often not the ones that might be relevant for reform. External validity thus tends to be the biggest challenge. What is more, until now there has not been an idea of how to bring grand corruption to the lab.

Field studies have the advantage of being more reliable with respect to external validity, but they are arduous to implement. Often they are implemented as part of a given government project (for example related to infrastructure in Indonesia, Olken 2007). Experimenters that are well connected to the authorities in charge of a project are in need of some control, for example allowing them to randomly assign recipients to treatment and control groups. But the respective government project may qualify primarily for reducing poverty, less for understanding anticorruption and advancing research. This may imply that field studies are the result of random opportunities rather than responding to identifiable gaps in research and the interests of the academic community. Likewise, natural experiments depend on a manipulation that was chosen by nature, not an experimenter. While this can advance our knowledge considerably, nature does not manipulate for the purpose of best helping research. Our first research desideratum is thus that laboratory and field experiments should not be seen as substitutes, but as complements. Each addresses questions that the other will find difficult to tackle.

The ample evidence on the determinants of corruption has shown that corruption is context- and culture-specific and that results do not easily carry over to contexts other than those in which they have been derived. This holds true also for the effectiveness of anticorruption strategies. While information campaigns may work very well for government transfers in Uganda (Reinikka/Svensson 2005) central auditing may be the measure of choice in community-based self-managed projects in Indonesia (Olken 2007). What makes a certain anti-corruption strategy successful in one context but not in another? The policy-relevance of this topic is obvious, yet the question has not been sufficiently addressed. With our second research desiderata we thus reemphasize the first one. Only joint efforts that involve a variety of methods can advance our understanding of corruption and reform and generate findings that are valid for a given context and culture. Field studies and laboratory studies must complement each other. The lab entails the advantage of allowing for replication. Conditions for running an experiment can be held (largely) comparable in different countries while operating only with different samples (see for example Banuri/Eckel 2012; Armantier/Boly 2013; Alatas et al. 2009; Cameron et al. 2009). But laboratory experiments suffer from the uncertainty whether the behavior of laboratory subjects comes anywhere close to potential corruptors, officials or politicians. They should thus inspire field studies to heal this caveat and investigate behavior of those who are the target of reform. Field studies might not lend themselves easily to replication. Identical projects in a different country might not be available for experimentation. Results from the field should thus inspire experiments in the lab for detecting differences across contexts and cultures. Furthermore, possible differences between lab

and field data – if they existed – would be informative on the type of behavioral bias that the lab situation creates, which would inform future lab experiments.

Ideal empirical evidence that identified context-specificity of corruption determinants and consequences would entail studies that were identical save for identifiable dimensions such as culture, regime type, religion etc. There are cases in which similar field and econometric data are available across countries, at least in principle: for instance standardized enterprise surveys, surveys of public services, or opinion surveys. These data could be used to identify country differences and their possible determinants, which might then feed into the design of cross-country lab experiments. But, even if empirical cross-country evidence is not ideal (as is often the case) new evidence on countries that have been largely neglected is desirable as it allows contrasting existing empirical evidence with evidence derived in different settings. Prime candidates are (semi) authoritarian regimes or regimes in transition such as China or Russia as reliable data are only becoming publicly available.

Context specificity could also be analyzed in a national context, analyzing sector differences or subnational regional variation using the same data type (e.g. Glaeser/Saks 2006; Alt/Lasson 2014; Schulze et al. 2013 for law enforcement data). Such approaches have the distinct advantage that a number of determinants are almost identical across the units of observation such as legal environment, law enforcement, culture, tradition so that the focus can be placed on the remaining differences.[7]

The third area which we consider especially fruitful for future research is methodology. Clandestine and illegal in nature corruption is typically not directly observable. As a consequence data are notoriously inaccurate and selection issues are abundant. For instance, how reliable are perception-based surveys in international or interregional comparison? Which respondents are reticent and under which circumstances (inter alia Azfar/Murrell 2009; Clausen et al. 2010; Friesenbichler et al. 2014), what do people mean when they state that firms 'like theirs' pay bribes (Clarke 2012) and who is entering into transactions in which bribes may be required? These and related issues are underresearched; yet they are very important in understanding the empirical results on corruption research.

The fourth research desideratum is the analysis of grand and systemic corruption. It can neither sensibly be simulated in the lab nor analyzed via randomized control trials due to the sheer scale, nor does it lend itself to econometric analyses as the number of observations is small (often one). Yet, as systemic corruption affects the entire political-economic system, its understanding is crucial for designing successful anti-corruption policies. This includes a comprehension of factors that have led to corruption being systemic (and not only frequent) and of the way the incentive structures were designed to keep the system corrupt.[8] Likewise the design of anti-corruption policies, both success stories and failures, need to be analyzed. Indonesia after Suharto's demise is a good example for a fairly successful development towards lower, non-systemic corruption that has suffered from a lot of setbacks (e.g. Butt 2011). One of the reasons why these highly relevant grand stories have not received the attention in the economics literature that they deserve may be that systemic political-economic corruption cannot be analyzed with the same methodological elegance as more small-scale, but more frequent corrup-

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[7] In particular sectors should be studied that have received relatively little attention so far. The higher education sector is a case in point – even though there is ample anecdotal evidence that grades and degrees are purchased in many countries, there are hardly any empirical studies that show the determinants of such a corrupt behavior.

[8] An example for this is the description of systemic corruption in Suharto's Indonesia by McLeod (2008).

tion.[9] As a consequence, corruption in the allocation of driver's licenses in India may be better analyzed (Bertrand et al. 2007) than the systemic corruption in Indonesia or China. We suggest that *if* there is a trade-off between relevance and elegance (often times there is none), relevance should take precedence over elegance. Macro studies that are based on perceptions of grand corruption and also qualitative studies do have their role.

5   This volume

The papers in this volume – two from the lab and four from the field – contribute to closing the gap for the first three research desiderata for empirical corruption research. They venture into unchartered waters as they study corrupt behavior in novel and relevant situations of strategic interactions (Li et al. and Khachatryan et al.) and analyze cross-cultural differences (Li et al.), evaluate corruption attitudes across religions (Gouda/Park), use cross-country firm-level data to study the interaction of micro- and macro-determinants on the effects of corruption on productivity (de Rosa et al.), analyze for the first time corruption in college admission in China (Liu/Peng) and lastly advance our methodological understanding by looking at selection issues in corruption research (Ivlevs/Hinks). These studies enhance our understanding of corruption in various ways by analyzing corrupt behavior at the grassroots level.

Donato De Rosa, Nishaal Gooroochurn, and Holger Görg analyze the effect of bribery on firm level productivity using the EBRD/World Bank 2009 Business Environment and Enterprise Performance Survey (BEEPS) of firms in 28 countries of Central and Eastern Europe and Central Asia. This cross-country firm-level data set allows investigating macro (country level) and micro (firm level) influences on individual firm productivity. Applying an augmented production function approach that controls for a wide variety of firm characteristics, firms' perceptions of institutional quality, and competition intensity, the authors measure the effect of corruption on total factor productivity (TFP) in two dimensions: whether firm officials pay bribes frequently (monetary costs) and what percentage of senior management's time is spent on dealings with public officials (time or opportunity costs). As corruption is endogenous they apply also an instrumental variable approach and find that productivity is reduced by monetary bribes, but not by the time that management spends on government and bureaucracy. The productivity reducing effect of corruption is stronger in high corruption environments. They find no evidence for the "greasing the wheels" hypothesis which posits that bribing helps to 'get things done'.

Artjoms Ivlevs and Timothy Hinks address the issue of sample selection in corruption analyses, arguing that in order to face the decision whether to bribe people need to interact with public officials. Those that do interact are a non-random sample of the entire population and thus a selection bias may result. To remedy this selection bias they apply a Heckman procedure. Ivlevs and Hinks use the 2010 'Life in Transition 2' survey conducted in 30 Central and Eastern European and Central Asian countries (and also five Western European countries), which asks for actual bribing experiences in dealings with eight different types of public officials, and find evidence for a significant selection bias. Their findings show some common features across the countries studied, but also distinctive differences between country groups providing additional evidence for the context and culture-specific nature of corruption (Kis-Katos/Schulze 2014).

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[9]   An exception is the phenomenon of politically connected firms as stock market valuations can be easily measured over time and politically connected firms constitute a substantial share of firms in the national stock markets.

Qijun Liu and Yaping Peng provide evidence of corruption in art college admissions in China. They have compiled a new data set on art students that have been admitted and find that a large share of them have bribed public officials in order to facilitate acceptance. They show that the likelihood of bribing depends on the perceived corruption level and the personal tolerance for corruption, but not on gender. Corruption is higher for middle income families than for poor or rich families and it is more frequent for the admission of lower tier art colleges. More capable (i.e. more experienced) candidates are less likely to bribe.

Moamen Gouda and Sang-Min Park employ data from the World Values Survey to investigate the link between attitudes towards corruption and religion. They find that people who consider religion and God to be important and who attend religious services are less likly to state that paying a bribe is justified. Interestingly, this finding does not depend on the religious denomination. Relevant to levels of corruption is thus not the actual content of a religious belief but more its intensity. At the risk of overinterpreting these findings, the religious denomination does not provide a clear candidate for understanding behavior in a corrupt context. This would indeed be an interesting conjecture for experimental researchers, who may have to care about one context less.

Sha Li, Christoph Bühren, Björn Frank, and Haiying Qin take a deep look at human behavior in a context that is highly relevant to reform. Anticorruption programs in the public sector often involve the rigorous implementation of the "four eyes principle". This mechanism is often employed as a safeguard against misuse. It has also been repeatedly involved in efforts to reduce bribe-taking and nowadays belongs to the standard arsenal in the public sector. Bribing two public servants, so goes the argument, is more demanding than bribing just one. If only one rejects the offer a corrupt relationship cannot be established. But in laboratory experiments in China and Germany the authors report the opposite. As compared to individual players, groups of three players were offered higher bribes and they more often reciprocated. The authors relate this finding to a diffusion of responsibility. Groups decrease their members' moral costs, making it more likely to engage in malpractice.

Elina Khachatryan, Sebastian Kube, and Björn Vollan investigate citizens' reactions to extortive officials. Depending on the treatment, citizens can either reward officials for good behavior or punish them for bad behavior. Which of these methods is superior for reducing extortion? The authors find that this largely depends on the mode of interaction, in particular, whether officials and citizens are in a one-shot or a repeated exchange. Reporting is superior in reducing extortion in one-shot interactions, where laboratory subjects are randomly re-matched with a different partner. But in repeated exchange there is an advantage to rewarding: Citizens are less likely to pay the requested bribe. The findings might be explained by feelings of reciprocity. The chance to recommend an official may frame an environment of positive reciprocity. Being extorted in such an environment may stir higher levels of resistance. Certainly, whether these findings provide direction for reform, particularly in areas where also collusive bribery might arise, is something that will require further research.

We believe this volume advances research on corruption in the four key area. The contributions not only look at the micro side but also the macro causes of corruption, the ones that tend to be understudied otherwise. They address robust differences across countries and identify where to look for context-specificity. They seek to join experiences in the lab with those in the field and, finally, take a critical look at methodological issues. The

contributions bring about original findings that should inspire reform. At the same time we believe they can reveal how research on corruption should continue in years to come.

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