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Letter from the President

The Fragmentation of Comparative Politics

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The debate about what it is that unites – or divides – scholarship in comparative politics is as old as the sub field itself. Looking just at the debate within the section, as reflected in the letters from the presidents since the section's founding in 1989, we find concerns about regional, methodological, and theoretical fragmentation.¹ What has been notably absent from this debate is the concern with a new kind of fragmentation, a fragmentation into increasingly narrowly defined problem areas, without researchers making a great effort to link the insights from studies of these specialized areas to the big picture, that is, to the larger theoretical and substantive concerns of the field, or to the very core of comparative politics.²

Now, some would argue that comparative politics has never had a real core. I disagree. Lasswell's (1936) classic definition of politics as "who gets what, when, and how" has been widely absorbed by scholars of comparative politics across the ideological spectrum (see e.g. Huntington and Domínguez 1982).³ Studies of power relations, interests, domination, and their connection to regime forms and distributive outcomes have been central to the classic works in the field, from Weber's *Economy and Society* to Dahl's *Who Governs*, Lindblom's *Politics and Markets*, O'Donnell's *Bureaucratic-Authoritarianism*, and Skocpol's *States and Social Revolutions*.⁴ But we are in danger of losing sight of these core concerns of the field. For example, there is much research on

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power relations between and within different branches of government, without considering the larger power relations within which these institutions are embedded. A favorite type of exercise holds power relations along with other conditions constant and focuses on strategic interactions between players under a given set of rules. Frequently, the political system is analyzed as a unit disconnected from economy and society. Most importantly, these kinds of studies are crowding out studies that seek to understand power and privilege.

In the past 10 years, the *American Political Science Review* published 85 articles on comparative politics.⁵ Of these, only 11 articles, or 13 percent, attempted to explain a problem having to do with power and distribution among social actors. We tried to find articles dealing with power relations among social groups or classes, campaign financing, business or capital as major actors, outcomes of distributive processes by class, race, and gender, access to the system of justice by race and class, conceptions of distributive justice, and any other questions of power and distribution. (We did not count articles that dealt only with power distributions among different branches of government, or with the consequences of electoral rules, without linking them to larger societal power distributions or distributive outcomes.) Only eleven articles dealt with these kinds of issues. The same count for American politics, arguably a closely related field, was 18 out of 150 articles, or 12 percent.

Let me preemptively respond to two obvious objections to my enterprise here. The first objection is that the *APSR* is not offering a representative picture of research undertaken by comparativists or even political scientists in general. I certainly agree with this critique, but it does not render my argument irrelevant. The *APSR* gives us a good picture of the type of research that is valued in influential circles in the profession. Thus, we can take it as an indicator of a tendency with powerful support.

The second objection may be that I want to put comparative politics into a straitjacket that admits only some kinds of research as valuable. This is not my intention. Political science is a highly diverse discipline, with many sub fields and specializations, with a focus on micro and macro politics. This diversity of course is reflected in comparative politics, and no attempt is made here to elevate one kind of orientation over another. However, within all kinds of orientations and sub fields, there is a choice to be made by the researcher. She can pursue the big questions, or she can choose to formulate more narrow questions and/or impose restrictive assumptions in order to treat these questions with elegant and parsimonious models.

Neither is it my intention to argue that studies that are not by themselves directly investigating power and distribution are not worth doing. Specifically, I think that many rather narrowly focused studies, particularly in the empirically based institutionalist literature, have made valuable contributions to our understanding of political institutions and processes. Just to pick one important exam-

(Continued on page 24)

News & Notes

Call for Nominations

The committees for the Luebbert Book and Article Awards, the Sage Paper Award, and the Data Set Award solicit nominations for each of these prizes. Please send your nominations to the members of the relevant committee (see below).

Committee Members for the Comparative Politics Section

Nominations Committee:

- T.J. Pempel, *University of California, Berkeley* (chair)
- David Cameron, *Yale University*
- Valerie Bunce, *Cornell University*
- John Carey, *Washington University, St. Louis*
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Luebbert Book Award Committee:

The committee is being appointed by Peter Hall, our President-elect.

Luebbert Article Award:

- Jonathan Hartlyn, *University of North Carolina, Chapel Hill* (chair)
- Meredith Woo-Cumings, *University of Michigan, Ann Arbor*
- Gary Cox, *University of California, San Diego*

Sage Paper Award:

Duane Swank, *Marquette University* (chair)
Ken Roberts, *University of New Mexico*
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Data Set Award:

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Program Coordinators for 2003:

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Announcing the Reagan-Fascell Democracy Fellows Program

Call for Applicants

The National Endowment for Democracy is pleased to announce the establishment of the Reagan-Fascell Democracy Fellows Program to enable democracy activists, practitioners, scholars,

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and journalists from around the world to deepen their understanding of and enhance their ability to promote democracy. Reagan-Fascell Fellows will be in residence at the International Forum for Democratic Studies, the research and publications arm of the National Endowment for Democracy (NED), located in Washington, D.C.

The International Forum hosts 12-15 fellows per year for three to ten months each. Each fellow will receive a monthly stipend for living expenses plus health insurance and reimbursement for travel to and from Washington, D.C. Stipend levels range from a minimum of \$3500/month to a maximum of \$7500/month, taking into account the fellow's previous annual income, level of experience, and the cost of living in Washington, D.C. Limited funds may be available for travel within the United States.

Applicants for Reagan-Fascell fellowships must choose between two tracks: a practitioner track (typically three to five months) to improve strategies and techniques for building democracy and to exchange information with counterparts in the United States; and a research and writing track (typically five to ten months) to conduct original research for publication.

Eligibility: The Reagan-Fascell fellowship program is intended primarily to support practitioners and scholars from new and aspiring democracies. Distinguished scholars from the United States and other established democracies are also eligible to apply. Practitioners are expected to have substantial experience working to promote democracy. Applicants who will focus on research and writing are expected to have a Ph.D. or, for non-academics, to have published in an area of expertise. The program is not designed to support students working toward a degree.

Application: Applications should be sent by air mail as well as by email to the address below and should consist of the following materials:

- Eight copies of a 5-10 page description of the proposed project to be carried out while in Washington, DC.

Those choosing the practitioner track should: 1) describe the work that they have been doing to advance democracy; 2) explain what they hope to accomplish through the fellowship, identifying a feasible fellowship project that takes into account the specific resources that they would draw on and the activities they would undertake; 3) provide a preliminary outline of the fellowship product (short article, policy memorandum, etc.); and 4) discuss how the fellowship will strengthen their ability to conduct their work and contribute to the more effective promotion of democracy in their country or region.

Those choosing the research and writing track should: 1) discuss how their project will advance public understanding of the theory or practice of democracy; 2) briefly describe how the proposed research relates to or extends existing literature on the subject; 3) indicate how a fellowship at the International Forum in Washington, D.C., will facilitate this research; and 4) provide a preliminary description of the proposed written product (article, monograph, or book).

- An indication of the preferred starting date and desired duration of fellowship
- Eight copies of a detailed CV or resume
- Three letters of reference

The deadline for fellowships beginning in Fall 2002 is April 1, 2002, although applicants are encouraged to submit their materials earlier. Recipients will be notified in late June 2002

For more information, visit www.ned.org/forum/fellowship_program.html, or contact:
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Symposium

Dirty Politics

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Introduction

Politics is never entirely “clean”. As Ibsen’s Dr Stockmann, that early hero of environmental activism, warned: “You should never wear your best trousers when you go out to fight for freedom and truth.” Some kinds of politics, though, are undoubtedly dirtier than others. Recently, a number of scholars have turned their attention to the dirtier kind. The journals reveal a revival of interest in political corruption, influence-peddling, campaign finance abuses, and other morally dubious activities in which less scrupulous politicians and their associates engage.

The inspiration seems to come from the newspapers. In the former Eastern Bloc, criminal mafias have left their mark on the new, rickety democratic institutions that reformers tried to erect. Corruption has sparked public outcries in dozens of countries from Latin America to Asia. Even in usually staid Western Europe few elder statesmen now seem to be able to retire with their reputations intact.

With the revival of interest in dirty politics, new questions have emerged. Few now believe that corruption “greases the wheels” of commerce. But China—and some of the Asian tigers until 1997—managed to combine high levels of graft with rapid economic growth. How? Scandals in Italy and Japan have shown just how far politicians will go in order to finance their party machines. Does democracy—and vigorous electoral competition—reduce corruption or merely channel it in new directions?

These questions, today, arouse as much interest among economists as they do among political scientists. Economists have pioneered new data sources and methodologies for studying corruption in a cross-national way. So it seemed appropriate to ask how well this work is linking up with the more qualitative investigations of various comparative politics scholars. And how much have we learned in recent years that was not already familiar to the pathbreaking students of political development of the 1960s and 1970s?

We invited a range of scholars—from economics and sociology as well as political science—to contribute. Those that accepted have area interests that span the globe, and a variety of theoretical concerns. In the symposium below, Diego Gambetta considers how letting others glimpse the skeletons in one’s closet can create the commitment necessary for illicit cooperation. An intriguing corollary follows: the more a country’s regulations force citizens into minor violations, the more skeletons the closets will contain, and the more opportunities there will be for corrupt exchange. Next, Halvor Mehlum, Karl Moene, and Ragnar Torvik consider how the market for “protection” interacts with the market for predation, and illustrate with examples from post-communist Russia and contemporary Africa. The interested reader will learn from the piece—among other facts—how a “sobel” differs from a “pobber”.

Rafael di Tella, one of the first economists to study government quality using cross-national ratings of “perceived corruption”, discusses one of the counterintuitive findings that emerged from such studies: countries that pay bureaucrats higher wages do not seem to have lower corruption, controlling for other factors. He suggests three possible explanations. Melanie Manion reviews the main literature using such indexes and discusses the problems associated with reforming very corrupt political orders. Hong Kong, as she points out, is one of very few countries to have recently made such a change. Finally, David Samuels ponders the continuing “dirtiness” of politics in much of Latin America, even after almost all countries

have moved in the direction of democracy and defeated hyperinflation. He suggests that privatization and decentralization may not always be as benign as some of their advocates believe.

Displaying One's Skeleton's in One's Cupboard: Why Norms Breed Corruption

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Consider the case in which “both the kidnapper who likes to release his prisoner, and the prisoner, may search desperately for a way to commit the latter against informing on his captor, without finding one.” This situation represents an extreme case in which two agents would like to agree on an exchange, which they both prefer to all other outcomes, but one of them distrusts the other to comply with the terms of the exchange. The kidnapper knows that the victim will be tempted to inform on him once freed. The exchange will fall through, to both parties' detriment.

Not all is lost however. Thomas Schelling, who thought of the above example, also suggested the solution. “If the victim has committed an act whose disclosure could lead to blackmail, he may confess it [to the kidnapper]; if not, he might commit one in the presence of his captor, to create the bond that will ensure his silence” (1960: 43-44).

The kidnapper example is asymmetrical. It starts at a stage

in which the victim already has bad information on the kidnapper, which he can use against him. The kidnapper did not volunteer the bad information. That was the inevitable by-product of his crime. At that point, the problem is only for the victim to give the kidnapper bad information about himself to make the kidnapper trust the victim's promise not to inform on him. Often, however, these situations, common in criminal markets in which agents do not enjoy the protection of the law or of other enforcing agencies, come in a symmetrical form: all participants are worried about each other, and they may all chose to disclose bad information about themselves. Drawing from fiction, this is how it works: “The best part about this deal” [Edie Marsh] said, “is that nobody's in a position to screw anyone else. You've got shit on me, I've got shit on you, and we've both plenty of shit on Snapper. That's why it's going down so clean” (Hiaasen 1995: 234).

Schelling's solution pinpoints a counter-intuitive situation in which it is to our advantage to reveal bad things about ourselves of a kind that others can use against us. The parties have an interest in *volunteering* bad information about themselves to each other. Being bad, and displaying credible and transmittable evidence of it, can make one's promises credible. While common sense rationality inclines us to think that doing that would be irrational – our misdeeds are best kept secret – cases of this sort indicate that

selective revelation of our secrets can be to our advantage. It is precisely the fact that that information should remain secret that gives it its persuasive force. If we behaved too well in the past or have no transmittable evidence of the bad things we did, we even have the incentive to *do* something bad.

The idea is not entirely new. In “How to distinguish a flatterer from a friend”, Plutarch makes the point that the revelation of a secret about oneself, which he sees as a typical flatterer's strategy, binds others to oneself: “If someone has been trusted with a secret, he is more inclined to disclose a secret of his own; and once he has made such disclosure, a relationship has been formed and there is fear of loss of trust” (1992: 71). Plutarch adds an extra mechanism to this case, namely reciprocity: if A reveals some “shit” about oneself to B, B feels compelled to reciprocate.

If the prescriptions to solve this game are put into practice politics becomes *really* dirty. Not only do politicians have an interest in seeking the skeletons in their partners' cupboards – this is something we know that many politicians do. There are instances when there is an advantage to opening up one's own cupboard for one's partners to actually see the skeletons. There is an advantage to lifting the cost of finding out from the shoulders of one's partners and to bearing it oneself. Even worse, the politicians have an interest in filling their cupboards with some skeletons, which may come in handy. When corrupt exchanges are cast as games of

this sort, they may become safer to enter if the people involved have evidence of each other's misdeeds, like having been involved in prior corrupt deals. Not only does this indicate that they have played the game before successfully, but also that they are afraid to violate the pacts for fear that the knowledge of their past deeds may be used against them. In *Confessions of a corrupt mayor*, Agatino Licandro gives several examples of this kind. He makes adamantly clear that the corrupt world he so vividly describes held itself together by the mutual threat of revelation. (Licandro also shows some of the pitfalls of this strategy as information revealed in order to make oneself blackmailable for one's own purposes can end up in the wrong hands, see ch. 4 and pp. 77-83).

The solution to these games amounts to an exchange of hostages. The trustee gives a hostage to the truster, which the truster can harm if the trustee reneges on their contract. If the truster believes that the damage he could do to the hostage is a cost to the trustee greater than the benefit the trustee would gain by reneging, he may decide to trust the trustee. There is, however, an empirical difference that could have momentous implications.

In Schelling's version the hostage is the trustee himself, for it is on him that the bad information once revealed could unleash a punishment. The really striking, difference, however, is that while the truster can trigger the punishment if the other defects by revealing the damaging information he has on

him, the truster does not mete out the punishment himself. This solution parasites on the punishment inflicted by a third party. A promise becomes enforceable by exploiting the unwitting action of a third party who understands certain actions as deserving castigation. Evidence of bad behaviour attracts a response by the law or elicits feelings in others bound to cause shame or loss of reputation. Agents fear each other not because they are violent or in some way able to punish, but more subtly because they fear the effects which they themselves can unleash on each other at virtually no cost to themselves. The only cost they bear is that of revelation, rather than that of the actual punishment. Someone else will take care of that.

The dependence of this strategy on external enforcement yields an intriguing prediction. It has been said that the greater the number of norms, the greater the corruption. What economists mean by that is that more norms give people more reasons for violating them: red tape gives the incentive to find shortcuts and creates a market for those who can assist one in finding them. However, the positive correlation between norms and corruption could exist because of a subtler reason: the greater the number of norms one can violate, the greater the amount of potential information available for mutual blackmailing. (In this case, the correlation would not be between norms that are directly connected to corruption, but norms generally, and corruption. This difference is of interest for it provides the potential for testing

which of the two effects of norms is related to corruption.)

Italy is notoriously a country with a high level of corruption that has proved hard to explain. It pops up as an outlier in all the regressions that try to explain corruption rates. Italy is also a country of excessive legal norms and regulation. According to the Cassese Parliamentary Commission for the Reform of Public Administration, Italy has in excess of 100,000 (one hundred thousand!) laws and regulations, as compared to 7,000 in France and 6,000 in Germany. The probability of living a life without incurring at least one violation must be virtually zero for Italians. Everyone has secrets that he will keep mostly hidden; but he has both a greater number of violations that he can selectively reveal in case of need and a greater chance that at least some of his violations will become known to others, whether he likes it or not.

Now, on the one hand, one might think that the greater the number of violations, the lower the chances to get caught, since the law enforcement agencies will be extremely busy. One could conclude, therefore, that the potential blackmailing effect attached to any one violation is weakened. But this is not so. For, on the other hand, the probability of being caught and convicted for any one violation in countries where the police is overwhelmed depends to a great extent on whether someone will inform the authorities rather than on the latter's independently initiated investigations. The fear of sanctions is ancillary to the fear

of someone informing on one. It seems plausible therefore to hypothesise that the high levels of corruption in Italy could depend on the fact that everybody has “some shit” on everybody else – “we are all sinners”, as Catholic priests like to repeat, “and no one can be the first to throw a stone”. This giant web of dyadic secret sharing could sustain that strong implicit pact of mutual support against the law, which one finds amongst Italians, who display an uncanny predilection to privilege loyalty to their private friends over their public duties as law abiding citizens.

A final word for the analytically inclined. Schelling's case can be interpreted as two different games. Suppose the truster believes that no trustee is trustworthy. The truster believes that given half a chance the trustee would not keep his promises. The trustee who is believed to be a fundamentally dishonest type commits himself to behave as if he were honest by disclosing bad information about himself. He gives a new option to the truster and thereby alters his own payoffs. He binds himself to act honestly for he knows that if he did not the consequences unleashed by the other party would be worse for him. The kidnapper example is more easily interpretable as one such case, and this is how Schelling meant it. In that case, there is lack of type uncertainty. We can guess by introspection that all victims are of one type: namely if they could they would violate their promise and report the kidnapper to the police. We believe this because we believe that it is human nature when

wronged to try to retaliate.

However, we could interpret the game differently. Suppose the truster believes that there are trustees of two types, the honest ones—those who always keep their promises—and the dishonest ones. The truster's problem here is different. He does not know who is who. The bad information that one party discloses does not work in this case because it alters the payoffs of the trustee, but because it acts as a signal of trustworthiness in the sense of signalling theory. Only the trustworthy type *can afford* to disclose bad information about himself for he knows he will never breach the agreement. The transfer of private information has different effects depending on whether we think we are dealing with a world in which there are only bad types or one in which there are both good types and bad types. In the former the disclosing of information alters the choices of the only (bad) type, in the latter it reveals whether the type we are playing with is good or bad. In the former case, the truster is persuaded if he believes that the cost to the trustee if the private information is made public offsets the benefit to him of defection. In the latter, the truster is persuaded if the cost of disclosing the bad information can only or mostly be afforded by the good type and not by the bad type.

The test of which game one was playing comes ex-post: if the truster destroys or loses the evidence and the trustee, knowing that, defects, the truster will know that it was a case of binding a bad type. If in spite of

that the trustee continues to comply the truster will know that it was a case of signalling and that the trustee was a good type. If in doubt, keep the evidence!

¹ See for instance *Mondo Economico*, July 17, 1993; *La Stampa*, March 4, 1993 and *Il Sole 24 Ore*, January 5, 1996.

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Market-Based Extortion

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Charles Tilly, in his essay in Evans, Rueschemeyer, and Skocpol's *Bringing the State Back In* (Cambridge University Press, 1985), makes the following distinction: “Someone who produces both the danger and, at a price, the shield against it is a racketeer. Someone who provides a needed shield but

has little control over the danger's appearance qualifies as a legitimate protector” In this note, we discuss a third case in which all violent entrepreneurs are drawn from the same pool. There is a division of labor between those who use violence and those who protect against violence, and each entrepreneur chooses the activity that is most profitable. All entrepreneurs interact in a market-based extortion in which the violent entrepreneurs as a group produce both the danger and the protection. This market is particularly important when the state fails to supply basic security and protection.

One example is Russia. After the collapse of communism, Russia has experienced an economic downturn and a downsizing of the military forces. This implied that many young men, some with violence as part of their trade, did not have proper jobs to go to. Privatization and restructuring provided a large number of new targets to exploit as the state apparatus was in disrepair, unable to provide basic protection of property. As a consequence, crime rates in Russia increased by 70 percent from 1989 to 1993 (see the UNDP “s *Human Development Report for Central and Eastern Europe and the CIS 1999*).

Parallel to the growth in crime, there was an explosion in the number of private security companies. The number of such companies went from zero in 1989 to five thousand in 1993 and reached ten thousand in 1996 (Volkov 2000). While the protection business continued to grow, the crime rates stabilized

from 1993 and then declined somewhat from 1996. Who were the entrepreneurs of these security firms?

In some cases they were officers of the police, the Interior Ministry, and the KGB. In other cases, the criminal groups themselves established their own security enterprises. As Vadim Volkov observed in St Petersburg: “one of the oldest and most prominent protection companies ‘Scorpion’ was set up and headed by A. Efimov ..., one of the leaders of *tambovskaya* criminal group The private protection company Adris, providing protection to more than a dozen companies, ... is known to belong to the *malyshevskaya* criminal group” (Volkov 2000, p.494). These are examples of a general phenomenon in Russia in which criminal groups themselves are engaged in the provision of protection against similar groups.

Surely, we are not witnessing a transition towards secure property rights, efficiently guarded by private security firms. On the contrary we are experiencing the emergence of a *protection screw* where violent entrepreneurs exploit producers in an extortionist manner. In the protection screw a higher violent capacity increases both the supply and the demand for protection, increasing both the price of protection and the incomes to all the violent entrepreneurs.

The main sources of income for violent entrepreneurs are: *plundering* of unprotected areas or property, *trafficking* in drugs, weapons, precious stones etc., and *protection* of areas and

property from plundering. Plundering and trafficking require mobility and represent apparent law violations. The enterprises that are engaged in these activities are like roving bandits, to use Mancur Olson’s term. When engaged in the protection business violent enterprises protect targets against plunder by the roving bandits. With a limited capacity each violent enterprise has to specialize. An enterprise in the protection business has a varnish of respectability; after all it fights criminals. The enterprise can therefore do less of plundering and trafficking. Using again one of Mancur Olson’s metaphors, a protector is a stationary bandit (see Olson’s *Power and Prosperity: Outgrowing Communist and Capitalist Dictatorships*, New York: Basic Books, 2000).

The targets can be anything from households, shops and industries to communities, cities and regions; all are vulnerable to plundering and need protection. With protection a target is safe from plundering, but has to pay protection money. To sharpen our discussion of the entrepreneurial choice between plunder and protection we simply assume that the protection business consists of opportunistic enterprises that are willing to enter banditry if the price is right. Furthermore, an enterprise that moves into roving banditry must exit from the protection business to get incomes from plundering and trafficking. Vice versa, a violent enterprise must exit plundering and trafficking before entering the protection business.

Protection is therefore supplied at a price that is

determined by the protectors' outside option as roving bandits. The return to roving banditry goes down as the number of bandits rises, since a higher number of bandits reduces the trafficking rents for each of them. The willingness to pay for protection is equal to the expected loss of an unprotected target and is proportional to the probability of being approached by a roving bandit. This probability is equal to the number of roving bandits relative to the number of unprotected targets. A higher number of violent entrepreneurs unambiguously increases this probability. Obviously the probability goes up if all the new entrepreneurs enter as roving bandits. If the new entrepreneurs all enter as protectors, the number of roving bandits is unaffected; but as the number of unprotected targets declines, the probability that an unprotected target is approached also goes up.

Equilibrium in the market for protection requires that the supply price equals the demand price. This implies that the return to each roving bandit equals the return to each protector. Let us now consider how an increase in the capacity for violence, for instance due to demobilization of military forces, affects the equilibrium outcome of this market-based extortion. A higher number of violent entrepreneurs increases both the supply and the demand for protection. Hence, the extent of protection goes up. In the protection screw the price of protection goes up as well, and all violent entrepreneurs gain from an increase in their

number. A complete discussion of this logic is provided in our *Plunder & Protection Inc.* (Mimeo, University of Oslo, 2001)

Africa provides many examples of a protection screw following from an increase in the supply of violent capacity. The supply of violence reached new levels after the end of the Cold War. As military budgets began shrinking in the mid 1980s, many countries undertook considerable demobilizations. For instance, in the early 1990s Ethiopia reduced the army by 500,000 soldiers and Eritrea by 50,000. Also in the 1990s, Uganda reduced the army by 36,000 soldiers, Mozambique by 90,000, Namibia by 50,000, and Liberia by 20,000 (Kingma 2000). Demobilized personnel and low paid officers found new sources of income as violent entrepreneurs. Unfortunately, a substantial part of their entrepreneurial talent was used in extra-legal activities. As large numbers of military personnel were demobilized without appropriate civilian jobs to go to, many countries got a rising supply of people qualified for violent crime, local warfare, and private protection. As Lock (1998:1400) describes it: "[N]o matter whether large scale demobilization after conflict or the pervasive slow-motion demobilization are characterizing the scene, the emerging private security industry and the criminal sector of the economy alike...draw from a labor market oversupplied with a wide range of military experience and know-how to chose from." In

Liberia people talked about "sobels," soldiers who turned rebels at night and "pobbers," police who turned robbers at night.

The nature of military security, fighting and peacekeeping has also changed in a way that has generated a protection screw. Some observers talk about a new warrior class in a world of low-intensity conflicts. William Reno (1998) describes warlord capitalism in Africa, with special reference to the privatization of violence. Private groups and military corporations have stepped in to fill the vacuum after the withdrawal of superpower support. According to Kevin O'Brien (1999: 54), private organizations "are increasingly taking over the role of either (sometimes both) exploiter or peacemaker." One of the most prominent examples of the private security companies is Executive Outcomes, also called the world's first corporate army. This South Africa based company could at its height field a powerful force including a medium artillery unit and air support. It employed former members of the South African Defense Force and fought both against UNITA in Angola and against rebels in Sierra Leone.

The working of market-based extortion is different from both ordinary markets and extortion by organized crime. In ordinary markets, the entry of new enterprises hurts the profitability of the established ones. This is not the case in market-based extortion where the entry of new violent entrepreneurs enhances the

profitability of all. Of course, violent entrepreneurs provide beneficial services of protection that the state may fail to deliver. But the problems that such an entrepreneur solves are created by his competitors. Thus a rise in violent capacity creates a higher demand and a higher price for protection. In this way, a flow of new businessmen of crime into the pool of violent entrepreneurs boosts their profitability, but increases the costs to producers. What is a virtuous cycle for violent entrepreneurs is a vicious cycle for producers, a process that eventually may lead the economy into a violence-induced poverty trap.

As strongly emphasized by Thomas Schelling in his seminal paper from 1967 ("Economics and Criminal Enterprise," *The Public Interest*, 7: 61-78), organized crime is conventionally perceived as monopoly or exclusivity. While the bosses of organized crime have an interest in order and peace within the area under exclusive control, each violent enterprise in market-based extortion has an interest in disorder and violence. Disorder and violence increase the willingness to pay for protection and increase the rents to be collected by each warlord and roving bandit. Thus market-based extortion needs not to be dominated by a single enterprise stronger than the others. It is a competitive market driven by Say's law: the supply of violence creates its own demand. This is why market-based extortions are so harmful for development.

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The New Political Economy of Corruption and the Motivation of Politicians: Progress and Some Open Questions

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Most scientific inquiries start with a fact, and the study of corruption is no exception. Early work on the topic, mainly by political scientists, found the facts in a mixture of historical accounts, anecdotes, case studies and basic comparative analysis. With few exceptions, however, economists avoided corruption as a subject until Mauro (1995) introduced cross-country data in his study of the effects of corruption on investment. The main views on the topic at the time were in conflict. They ranged from the traditional accounts in which corruption acted as a form of confiscation and deterred

investment to accounts in which corruption was beneficial in providing an informal incentive or bonus scheme for bureaucrats to assist businesses. By showing that the cross-country correlation between corruption and investment was negative, Mauro more or less settled this debate and gave the field the kind of empirical discipline required for fast scientific progress. A mini-revolution followed, with economists and political scientists extending the field in different directions, often using data, but also revisiting old questions from new theoretical angles.

Clearly a first task was to provide some guidance on the relevance or otherwise of a series of forces that were argued to cause corruption. These ranged from cultural explanations for corruption to arguments based on the incentives faced by bureaucrats to take bribes, including the temptation implied by the level of rents in the economy (natural or policy-generated). (An example is Treisman (2000).) The more recent papers in this literature have studied the interesting question of the effect of electoral rules on corruption. The idea is that competition for office means that elected officials will be deterred from taking bribes for fear of losing their jobs. The empirical findings support the hypothesis that closed-list proportional representation systems, especially together with presidentialism, are associated with higher corruption levels (Persson, Tabellini and Trebbi (2001) and Kunicova and Rose-

Ackerman (2001)). These papers can be understood as part of a larger literature that investigates the role of democracy in controlling politicians. As emphasized by Wittman (1989), economists often bias their models against finding a positive role of democracy, assuming very imperfect political markets for votes that co-exist beside perfectly functioning markets for goods and services.

This is no place for a comprehensive survey, so let me focus on what, in my mind at least, is one of the most interesting findings of this first wave of papers, namely that the idea that corruption originates in the decision to pay bureaucrats very low wages has largely failed to find empirical support. The idea of raising wages to curb corruption originates in a justly celebrated paper by Becker and Stigler (1974) and is the backbone of most policy proposals to fight corruption, including those of the World Bank. Explaining the empirical failure of the Becker-Stigler hypothesis and, more generally, gaining a better understanding of the motivation of politicians and bureaucrats, seems to be one of the most important challenges in the field.

The explanations that I know can be ordered into three different categories. The first argues that testing the Becker-Stigler hypothesis requires information on the amount of auditing carried out, something not usually available in cross-country studies. The reason why this matters is that both when auditing is low and when it is very intense, the original

Becker-Stigler model predicts no correlation between wages and an official's propensity to be corrupt. In this, and related models, the act of taking a bribe is very much like playing a lottery. There is some probability that the agent gets lucky and escapes detection with the prize, and some chance that he gets caught in the act of taking bribes and loses his job. The higher the probability of detection and the higher the penalty (the difference between the official wage and the alternative wage) the less attractive corruption is. The crucial point is that when there is no risk of getting caught, everybody will be corrupt regardless of the wage they receive. If corrupt agents are certain to be caught, there will be no corruption. This means that the typical cross-country empirical work that mixes observations with different degrees of auditing will have a bias towards zero in the coefficient on wages in a corruption regression. (In other words, the effect involves an interaction between auditing and wages that is measured with noise.) Di Tella and Schargrodsky (1999) make this argument and find evidence consistent with the Becker-Stigler hypothesis by looking at the prices paid by procurement officers before, during, and after an anti-corruption crackdown in the hospitals of the City of Buenos Aires.

Some might argue, however, that these effects are observed only because we are dealing with low-level procurement officers, who can steal relatively small amounts of money. When we

focus on political corruption, things might be different because the sums involved are often so large. In order to prevent corruption of high-level public officials, who have discretion over large amounts of money and who are difficult to audit, wages have to be unrealistically high. An example of a country that seems to be following this high-wage strategy is Singapore. Interestingly, even though it imposes extremely harsh penalties on those who break the law, and has a very professional auditing office, wages tend to be set at a level that seems too high to be feasible in more democratic countries. In the year 2000, Singapore's prime minister earned a wage equal to US\$1.1m.

When our theories end up justifying something that would normally be socially condemned, such as exorbitant levels of wages for politicians, we should be especially careful about the motivating theories. In particular, I wonder if there is not something fundamentally wrong about our understanding of the motivation of public officials and politicians. Remember that politicians and bureaucrats are supposed to be socially motivated. At the very least we would expect them to give less weight to monetary incentives, particularly when compared with their private sector counterparts. One possibility is that high levels of wages may change the *nature* of the motivation of agents working in the public sector. There is a growing literature on managerial compensation that makes a related distinction between "intrinsic" and "extrinsic" motivation. Part of it argues that

rewards undermine the very processes they are designed to reinforce. Another literature makes the point that civic virtues in the general population may be crowded out by money (see Frey and Oberholzer-Gee (1996)). This suggests the possibility that a related process operates here. When socially motivated politicians are offered monetary incentives to do the right thing, they may become less inclined to do so. For a start, decent people often get offended if you want to compensate them for their honesty. If I find a wallet with money on the street and return it to the owner, I probably would not accept a tip for doing it. If anything, that would tend to put me off. And if I knew beforehand that such an offer was forthcoming, chances are that I would not even bother returning it. The reason is probably that there is an implicit assumption that good will is the currency of trade. When money is offered, the potential trade has to be defined in a new currency—money rather than good will—and it is far from clear that the money involved will produce a similar level of motivation.

A third potential explanation for the empirical failure of the Becker-Stigler hypothesis is that the compensation of politicians includes non-pecuniary components. It could be reputation, so some of the effects outlined above are again relevant. Or it could be that pressure groups use some form of threats to influence them. These could include threats of legal harassment, smear campaigns in the media, or even

threats of physical violence. A priori, there is no reason why we should expect that interest groups restrict their actions to just offering carrots (bribes) and not including sticks (threats). And there is ample anecdotal evidence from developing countries that various groups are in the business of threatening policymakers. In Colombia, for example, judges and policymakers involved in the war against drug trafficking receive messages asking "*Plata o plomo?*" (i.e. silver—as in money—or lead—as in a bullet). The dismal record of political violence in the country is partly a reflection of the fact that not all Colombians have a price. Another example is Russia, where a recent article in *The Economist* reported that seven of the largest eight business groups owned media interests. The interpretation given was that they are a way of influencing politicians through intimidation. In this view of motivation in the public sector, wages and corruption are not correlated because the state is captured using threats, not bribes.

Finally, it seems dear that real progress will also involve work in at least two related areas. The first is the general structure of pay to top elected officials. A plausible case can be made that a key component of pay to politicians is prestige and votes. This means that their incentive scheme is state contingent: as for the CEO of a big company, if things go well, pay (or vote intention) is high, otherwise it is low. The extent to which voters deliberately subject politicians to such high-powered

incentive schemes and are able to discern aggregate shocks from idiosyncratic shocks (that should be "blamed" on the politician) is the topic of intense debate (see Wolfers (2001) for a recent contribution).

The second area where more work is needed is on the determinants of the quality of politicians. The political institutions in each country determine the costs and benefits of entering political life. These obviously include the wages and perks derived from office, but also less traditional forms of rewards such as the influence of the collective reputation of the political class on any one individual and the likelihood that politicians will be harassed by pressure groups. The potential for this line of research is illustrated by a recent paper by Caselli and Morrelli (2000), who build a model of entry into political life by individuals of different ability/honesty types and are able to deliver multiple equilibria and path dependence, two features that seem to be consistent with the available evidence.

¹ I thank Ernesto Scharfrodsky and Lynn Paine for discussions and suggestions.

² But see Rose-Ackerman (1978) and Becker and Stigler (1974).

³ See Kohn (1993) for a discussion and presentation of the relevant evidence. See also the discussion in the *Harvard Business Review* (November-December 1993).

⁴ For a start on this see Dal Bo and Di Tella (1999) and Baron (2001).

⁵ Using data on the salaries of US governors, Di Tella and Fisman (2001) argue that the official wages of politicians are also structured in this way.

The references cited in this piece are listed in full on the newsletter's website, www.shelley.polisci.ucla.edu/apsacp

Anticorruption Reform at the “Dirty” End of the New Corruption Continuum

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In recent years, the increasing availability (and acceptability) of subjective measures of corruption has made possible an exciting new empirical literature. It uses the evaluations of corruption by risk analysts, country experts, domestic and expatriate business executives, and (more rarely) the general public across a large number of countries to study the causes and consequences of corruption. These statistical analyses have substantially confirmed conclusions from case study research about the effects of corruption. *Ceteris paribus*, higher levels of subjective corruption are associated with less investment, lower growth, and weaker political system support. Higher subjective corruption is also associated with less government spending on education (and perhaps public health too), presumably in favor of projects that yield higher rents (although empirical support for this latter association is weak). Progress has also been made using subjective measures to examine causes of corruption. The new studies seem to concur that, *ceteris paribus*, British colonial heritage and Protestantism are associated with lower levels of subjective corruption, as are economic liberalization and higher per capita income. There is also

support for a relationship between political freedoms and lower subjective corruption, but the new scholarship yields conflicting findings about the strength of the relationship and whether or not long experience of democracy is required for any significant impact. There is disagreement too about the relationship between subjective corruption and trade openness, administrative centralization, and country size. Certainly, our degree of unease about the implicit assumption of a single dimension to corruption and the typical source bias toward multinational businesses will influence how much stock we put in analytical findings that use the subjective measures of corruption. This aside, the promise of the new literature is to pose and answer a number of questions about corruption as tractable empirical problems.

Corruption in the new literature is a continuous variable with a wide range of variation across a large number of countries. At one end of the continuum are countries such as Finland and Denmark, where corruption is judged to be very unusual; at the other end are countries such as Nigeria, which has anchored the Transparency International Corruption Perceptions Index year after year with a score that suggests ubiquitous corruption; in between are countries such as Italy, South Korea, and a number of post-communist countries. By contrast, traditional studies of corruption of the 1960s and 1970s tended to be richly descriptive case studies of countries in which corruption was really very

widespread, countries located only at one end of the continuum newly defined by the subjective measures. This focus was not because traditional studies assumed that advanced industrial democracies were exemplars of clean government, but because politics in countries at that other end of the continuum could be described reasonably well without assigning prominent place to the “informal political system” of corruption. Put another way, in contexts where corruption was really the mainstream of politics, it was also properly the mainstream of a literature describing these politics.

The new subjective measures of corruption are important to students of comparative politics because they permit us to pursue, with the tools of inferential statistics, a variety of analytical research questions. The measures are also useful, however, for the broader descriptive patterns they illuminate. Consider the following three patterns in subjective corruption scores compiled by Transparency International. First, the countries and regions featured in traditional studies of corruption are also among the countries ranked most corrupt two or three decades later. Countries of South and Southeast Asia and Sub-Saharan Africa figure prominently. Second, this persistence of corruption emerges as well from an examination of the subjective measures over the shorter period for which they are available. The scores span more than two decades, from 1980 through 2001. If, for example,

we collapse the index to form three categories defined by thirds of the ten-point scale, few scores change enough to cross categories: more than 90 percent of the 54 countries for which scores are available for the entire period have basically consistent high, low, or intermediate scores.¹ Corruption persists, but so (it seems) does clean government. Third, only 11 countries have consistently intermediate scores over the period, significantly fewer than either high-scoring or low-scoring countries. Of course, this is largely driven by inclusion of the rankings from the 1980s: in rankings produced after the mid-1990s, the number of countries in the intermediate range grows – practically all post-communist countries of Eastern Europe appear here (just as practically all countries that constituted the former Soviet Union appear in the low-scoring, most corrupt category). It will be interesting to follow these intermediate countries over the next decade to see whether or not this is a status that persists or one largely associated with transitions of one sort or another. Overall, then, looking across time at the subjective measures as rough categories, it seems that countries can mostly be categorized in terms of their widespread corruption or their clean government, and that corruption and clean government tend to persist.

One way to think about the patterns described above is to consider widespread corruption and clean government as fairly robust equilibria in some underlying game (or set of

linked games) played in a society. Traditional case studies and newer cross-national statistical analyses are in basic agreement about the consequences of corruption: they concur that widespread corruption is inferior to clean government in many aspects for most societies. That it persists is an interesting generic research puzzle and difficult practical policy problem. What sustains the equilibrium of widespread corruption? What are the prospects for an equilibrium shift to clean government? How (if at all) can such a shift be hurried along?

Hong Kong offers an example, probably the best in the world, of successful transformation from extensive, institutionalized corruption in the 1960s to clean government in the 1970s. Since then, new opportunities, new pressures, and new players challenged clean government in Hong Kong, but it has sustained a consistent ranking as one of the “cleanest” countries in the world since 1980. In Asia, only Singapore ranks higher.

Many have attributed success to the independence and draconian powers of the anticorruption agency in Hong Kong. More important, however, is the three-pronged strategy adopted by this agency: enforcement, education, and institutional design. In other countries, anticorruption efforts, sincere and insincere, have focused mostly on enforcement. A growing literature on anticorruption reform has recognized that enforcement, while necessary, is insufficient to bring about a major and lasting

shift to clean government. Changing corrupt payoffs through institutional design is also necessary. This involves interventions to restructure work procedures to reduce incentives and opportunities for corrupt activities. In Hong Kong, institutional design is also preemptive: corruption prevention analysts are full participants in the initial design of procedures, policies, and legislation. They bring to that process specialized knowledge and a single concern, the implications for corruption. Requiring their involvement at the early stages of institutional design (legislative drafting, for example) is one of the more remarkable features of the Hong Kong anticorruption strategy.

The literature has been less attentive to the crucial role of beliefs—about the prevalence of corruption and about the reliability of the government—as an enforcer of rules ostensibly constraining official venality. In a setting of widespread corruption, however, anticorruption reform is a problem not only of reducing corrupt payoffs, but also of changing the shared expectations that define a “folklore of corruption.” Even if anticorruption interventions change corrupt payoffs significantly, the illegality of corruption delays adjustments of choices to transact corruptly because of the information problem it poses. “Bad guesses” about corrupt payoffs and the corruption rate can persist long after adoption of anticorruption reforms, especially in settings where corruption is long

entrenched. As described in traditional studies and a new survey-based study of post-communist countries, widely shared beliefs about the ubiquity of corrupt practices and the unreliability of the government as anticorruption enforcer may be highly exaggerated, but nonetheless affect choices. To be sure, if government interventions reduce corrupt payoffs, then bad guesses should be self-correcting over time. In Hong Kong, rather than waiting for players to discover, in the course of experience, new evidence about reduced corrupt payoffs, the government intervened massively to provide information to prevent players from choosing “incorrectly” to transact corruptly. This is the third prong of anticorruption reform: education, which was not only about reducing the psychic costs of corruption, but also about publicizing enforcement successes and anticorruption agency reliability with the underlying message that the environment for corruption had already changed. These interventions, aimed at changing expectations about the prevalence of corruption and about the government as anticorruption enforcer, independently accelerated the process of building clean government.

If Hong Kong demonstrates the possibility of an equilibrium shift from corruption to clean government, examples of the difficulty of such a shift are far more numerous. Interventions to change corrupt payoffs and shared beliefs in a setting of widespread corruption encounter serious obstacles that

do not figure prominently in anticorruption efforts of governments closer to the other end of the corruption continuum.

When corruption is widespread, changing corrupt payoffs through institutional design is inherently problematic. What most fundamentally distinguishes countries with widespread corruption from those where it is unusual is not the content of laws or regulations relating to abuses of official power, but their relevance. The general irrelevance of rules practically defines situations of widespread corruption. The problem here is not that some officials are bound to discover loopholes in redesigned rules and then take advantage. Rather, in a setting of widespread corruption, officials do not need to search for loopholes, as it is taken for granted that rules are no real encumbrance to practice. Put differently, institutional design solutions mostly assume that formal institutions matter in guiding actions, but this correspondence is itself an “informal institution”—and the relevance of rules cannot simply be mandated.

Interventions aimed at changing beliefs are also problematic in a setting of widespread corruption. The government is not a player outside the setting: indeed, its unreliability in anticorruption enforcement is generally an integral part of the setting. From a principal-agent perspective, widespread corruption suggests, at best, a monitoring failure of massive proportions. At worst, it suggests tacit acquiescence (or

active connivance) by the principal in supplying a “public bad.” By implication, even when many (or most) in society prefer clean to corrupt equilibria, the crux of anticorruption reform in a setting of widespread corruption is government credibility where the problem of credibility is chronic. It is in this context that we should understand the usefulness of an independent anticorruption agency, such as the one created in the early 1970s in Hong Kong. Properly designed, such an agency can signal the credibility of government commitment to anticorruption reform. Badly designed, it can easily fail to serve this function. Nor are such agencies, even well-designed, the only (or necessarily the best) way to communicate to ordinary citizens and corrupt officials that the environment for corruption has changed.

Some recent work on corruption has elaborated the fairly obvious point that corruption control is not costless, that the optimum level of corruption is not zero. Dysfunctional anticorruption excess is hardly the problem in most countries, however. Nor, despite the importance of more precise measures of corruption for analytical inquiry, are the problems of anticorruption reform at the “dirty” end of the corruption continuum essentially similar to those at the other.

¹ The Transparency International Corruption Perceptions Index (CPI) is calculated using surveys for up to the past three years, which means that sources often overlap from one year to the next. Thus consistency of scores in different (especially adjacent) years can

result simply from use of the same sources and surveys. I looked at five rankings that cover the 1980-2001 period, but overlap little: the 1980-85 CPI (based on four surveys, from 1982 and 1984-85), the 1988-92 CPI (based on four surveys, from 1988 and 1992), the 1996 CPI (based on ten surveys, from 1993-96), the 1999 CPI (based on 17 surveys from 1996-99, of which 16 are from 1997-99), and the 2001 CPI (based on 14 surveys from 1999-2001, of which 11 are from 2000-2001). Of the 49 countries I characterize as basically consistent across the period, 35 retain scores in a single category in all five rankings, and 14 have one score outside their category; the discrepant score usually differs by very little and is usually from the 1980s rankings, which were based on fewer sources. The rankings can be accessed at <http://www.gwdg.de/~uwvw/>.

Dirty Politics in Latin America

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Politics is rarely pretty. Yet scholars have never agreed on what the “dirtiest” aspect of politics is: the fact that in many countries the poor have little or no equality before the law? That the rich are rarely prosecuted for tax evasion or worse crimes? That the highest authorities of numerous countries have been implicated in electoral manipulation, illegal arms sales, campaign finance scandals, money laundering, and even drug trafficking? In this brief piece I would like to touch on several broad changes in Latin America that might help us understand the relative dirtiness of politics. The problems for comparative research are

unfortunately very clear, as corruption confounds both operationalization and measurement. Yet scholars should take up that challenge, as what we don’t see about politics may be as important as, if not more important than, what we consider to be “clean” politics.

Let me first note a few positive developments. First and foremost is the advent of democracy throughout Latin America (with with several important exceptions, of course, and all the usual caveats). As your editor Professor Treisman demonstrated recently in *The Journal of Public Economics*, although newer democracies are not perceived to be more or less corrupt than dictatorships, over the long haul democracies are indeed seen as less corrupt. Whether longstanding democracies are *in fact* less corrupt should be an object of investigation. We should like to believe that there is some relationship between perception and reality in this regard, so comparativists ought to attempt to determine what mechanisms link democracy to a perceived decline in corruption. For example, is increased electoral competition associated with increased electoral accountability for certain outcomes (e.g. budget deficits, economic performance or other indicators of governance)? Is electoral competition a sufficient factor to reduce corruption? If not, what is it about democracy that reduces the perception of corruption? The development of judicial independence or a freer press?

The development of a freer press and of alternative media sources is also worthy of note.

Newspapers and newsmagazines in different countries around the region now compete aggressively to expose government corruption. And as in the US, the spread of the internet also helps disseminate news more rapidly and widely, making it relatively more difficult to “keep the lid on” emerging scandals. An important question for research is whether increased press attention to corruption contributes to a decrease or increase in *perceived* corruption (and in real corruption). For example, we would like to think that the scandals involving the presidents of Argentina, Brazil, Ecuador, Venezuela and other countries would demonstrate to politicians that they can no longer act with impunity. On the other hand we might also expect politicians to continue their timeless game of cat and mouse and simply seek more clever ways to hide their dirty linen. Perhaps content analysis of major newspapers could contribute to understanding this question, which would help us understand the link between democracy and corruption.

Another important notch in the plus column is the taming of inflation in many countries. Inflation is a terrific smoke screen for budgetary shenanigans, fiscal profligacy, and financial-services industry scams. For example, under inflation the government can deflate the real value of disbursements for contracts it must pay—for example for employees’ salaries or for construction of roads or dams—by delaying payment for a week or a month. Inflation thus contributes to corruption by

allowing the government to manipulate contracts it has entered into. Government leaders may desire to use inflation simply to cover revenue shortfalls, i.e. to rob Peter to pay Paul; but they may also enjoy personal gain from the confusion that inflation causes with public accounts. The end of inflation, coupled with the increasing importance that the IMF places on "fiscal responsibility," has made these kinds of budgetary manipulation less likely. The explicit hypothesis here is that the end of inflation permits voters to perceive corrupt practices. The implicit hypothesis is that voters care about corrupt practices and will punish them "appropriately." This hypothesis demands operationalization and testing, both within Latin America and across regions, given the suggestions that some cultures are relatively more or less tolerant of corruption.

Democratization, the development of a freer press and the taming of inflation are potentially important checks on dirty politics. Unfortunately, several recent region-wide developments may lead to an increase in perceived and possibly real corruption. First, a region-wide process of decentralization of money and policy responsibility may contribute to corruption. The effort to decentralize has been linked to democratization, as part of a desire to bring government "closer to the people" and sweep away the vestiges of centralizing authoritarian governments. However, the process of decentralization is driven more by political rather than technical or

normative criteria. Partly as a result (for example), given that budgetary oversight is scant at the national level, fiscal decentralization may result in greater absolute levels of corruption, as subnational officials face far less public and media scrutiny. Decentralization and democratization may be associated, but decentralization and democratic accountability may not be. Perceptions of corruption (or what some call the "capture" of subnational governments) might be obtained through survey research, for example through the Latinobarometer survey results, which have repeatedly asked citizens in Latin America about whether corruption is increasing or decreasing. Perhaps the cross-national responses are correlated with broader policy developments such as decentralization. Scholars should also consider employing survey research for within-country comparisons across disparate regions that could unpack the relationship between decentralization and the relative efficacy of service provision.

A second potential development that may contribute to dirty politics is the relationship between what we might call the "decline of the developmentalist state" and corruption. Although many hoped that privatization, deregulation and the general retreat of the state from involvement in the economy might decrease corruption, the opposite may be just as likely. For example, privatization does not mean that the state is relinquishing its responsibility to regulate a given policy area. Yet in Brazil, for example, privatization has sometimes

proceeded without the construction of an adequate regulatory framework. This may contribute to an increase in both perceived and real corruption. Moreover, as scholars such as Richard Snyder (*World Politics*, 1999) have shown for Mexico, deregulation at the national level does not end government involvement but opens the possibility for government re-regulation of numerous policy arenas by subnational political actors. As mentioned, accountability mechanisms are relatively more fragile at the subnational level around Latin America than their already fragile national-level counterparts. These two points suggest that the end of the developmentalist state may be associated with increases in corruption, not decreases, but that the nature (for example, suborning of regulatory agencies versus old-fashioned government "inefficiency" in state-owned enterprises) and "location" (e.g. national versus subnational) of the corruption may change as the nature of state involvement in the economy also changes.

A final important factor to mention is the relationship between democratization, campaign finance and corruption. As I argued recently in *Comparative Politics*, campaign finance gains increasing importance as countries democratize, because both the demand for and supply of campaign finance is likely to increase as the intensity of electoral competition increases. For example, we know that in the US and Japan, campaign spending increases when the

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Book Review

Politics After Neoliberalism: Regulation in Mexico

By Richard Snyder
New York: Cambridge University Press. 2001.

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Neoliberal reforms have been promoted as a means to reduce the role of the state, resulting in de facto market re-regulation. In *Politics After Neoliberalism*, Richard Snyder demonstrates that neoliberal reforms do promote a re-regulation, but rather than a re-regulation by market forces, the process has been commandeered by subnational actors seeking to create new support bases. Developing a framework to explain the new institutional outcomes for market governance, Snyder emphasizes the interaction between structures and political agency to explain the re-regulation of the Mexican economy.

Snyder's analysis focuses on the re-regulation of the coffee sector after the dismantling of the Mexican Coffee Institute (INMECAFE), the state-owned enterprise created during a period of heavy federal intervention. The empirical analysis focuses on four states: Oaxaca, Guerrero, Chiapas, and Puebla. Snyder utilizes an innovative approach, the subnational comparative method, making intranational comparisons while holding the sector constant. The design allows Snyder to control

for nonpolitical factors that would complicate the analysis, such as cultural, historical, and socioeconomic variables. By selecting four southern states with similar characteristics, he is able to overcome some of the control issues inherently problematic in conventional small-N analyses.

Snyder frames the re-regulation phenomena with a politician-centered analysis; in countries like Mexico that lack insulated bureaucracies, politicians are likely to use regulatory policy as a means to develop support and vie for power. In the analysis, politicians' choices of re-regulation strategies are affected by three factors: societal forces, regime institutions, and policy repertoires. Societal forces delimit the range of policy options, while regime institutions affect policymaking by defining the structure of the policy process and the responsiveness of politicians to societal groups. The last factor affecting politicians' choices is policy repertoires, preferences governing decision-making that are conditioned on beliefs, values and ideas. Snyder's framework then integrates the subsequent bargaining between politicians and societal groups over the terms of re-regulation. Here, the strategies of societal groups are affected by politicians' re-regulation strategies, and relative group power. The result is the creation of new institutions for market governance, not the dissolution of institutions in favor of unregulated markets.

In the 1970s, in an attempt to increase its political control of the countryside, Mexico's federal government became increasingly involved in the coffee sector. INMECAFE was the main instrument of the government, controlling export quotas, working with small producers to increase production, and purchasing and marketing small-holder coffee production. Throughout the 1980s, often in response to dissatisfaction with INMECAFE, small farmers formed cooperatives and sought control over the marketing of their coffee. At the end of the 1980s, the federal government implemented neoliberal reforms that included a massive deregulation of agriculture, including the dismantling of INMECAFE. Snyder's analysis examines the resulting re-regulation of the industry by comparing the re-regulation projects sought by state governments and how they were affected by the strength and response of small producer organizations.

Oaxaca and Chiapas shared similar configurations of societal forces during this period. Both states had high numbers of small producers who were well organized and politically powerful. Both states had powerful oligarchies that influenced the strategies of the states' governors. Despite societal similarities, however, their policy outcomes differed significantly. In these cases, the differences between the politicians resulted in different institutional outcomes. In Oaxaca, Governor Ramírez

pursued policies in contrast to national neoliberal policies, resulting in a neocorporatist re-regulation project. This neocorporatist project was coupled with an engaged productivist strategy from producers. Deemphasizing partisan politics, producers sought to modify the governor's regulation project through inclusion in the policy arena. The result was a participatory framework for re-regulation.

In contrast, Governor González of Chiapas sought to align his government with the

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powerful oligarchy, developing a crony-capitalist re-regulation project that marginalized the small coffee producers. Unlike Ramírez in Oaxaca, González repressed opposition and befriended the traditional oligarchy. The response of producers was a disengaged productivism that sought to prevent the exploitation of small producers by forming their own statewide cooperative. In 1991, González was offered a cabinet position, and state leadership changed hands three times within three years. The armed Zapatista rebellion changed the political landscape and an interim government accountable only to President Zedillo was put in place. This led to a participatory outcome thanks to the alliance between federal reformist officials and small producers. However, Chiapas seems anomalous in the study due the influence of the federal government and the concern for stability in the state. Therefore, Snyder's emphasis on the three factors affecting politicians' re-regulation strategies seems less compelling.

Guerrero also enjoyed a powerful small-producer movement, but had an almost non-existent agrarian oligarchy. Yet the outcome in Guerrero was neither participatory nor inclusive. Governor Massieu's administration sought to implement the neoliberal doctrine at the state level while repressing opposition. Like Ramírez in Oaxaca, Massieu sought to strengthen the existing corporatist confederations in an environment with strong small-producer organizations. Unlike in Oaxaca, the small-producer organizations chose a partisan

strategy opposed to Governor Massieu's neocorporatist strategy. Electoral conflicts in Guerrero resulted in a partisan strategy that put the producer organizations at odds with PRI elites at both the state and local levels. The result was an exclusionary policy framework despite strong producers and a weak oligarchy.

Juxtaposed with Guerrero, Puebla's societal configuration consisted of a strong oligarchy coupled with weak coffee producers; however, the outcome in Puebla was also an exclusionary policy framework that benefited elites. Unlike in Guerrero, the governors sought crony capitalist re-regulation projects and exclusionary policy environments in response to the dismantling of INMECAFE. And rather than decentralization strengthening small producers and promoting participatory policy frameworks, Puebla emphasizes the likelihood that in states where grassroots organizations are weak and elites strong, crony capitalism and exclusionary policy environments are likely to flourish.

In assessing Snyder's work, a number of issues can be raised. First, the politician-centered analysis emphasizes the ability of subnational politicians to seek re-regulation strategies as a means to develop support and vie for power. This is a logical assumption in federal systems and in states without insulated bureaucracies, although re-regulation experiences within different systems would likely differ.² However, emphasizing the need for politicians to develop support seems to undermine the inclusion of "policy repertoires." These pre-

dispositions are argued to lead politicians to support policies that “do not enhance their career fortunes” (p. 10). In the conclusion, Snyder states that his “study has shown repeatedly how imperatives of political survival give politicians powerful incentives to intervene in markets” regardless of their ideological persuasions (p. 199). As a result, policy repertoires appear explanatory only when they coincide with market intervention, as in Oaxaca under Ramírez. Political realities and rational politicians seem better explicans of policy strategy in the remaining cases.

Second, Snyder deemphasizes international economics in his analysis. While admitting that the international coffee market had a significant influence on re-regulation, he favors domestic factors as determinants. However, he poses an interesting counterfactual scenario: if the re-regulation process had occurred during a price boom rather than a bust, it is likely that small-producer organizations would have been far weaker, leading to exclusionary outcomes that would favor the coffee oligarchy. If international economics affects grassroots mobilizing, which in turn affects societal pressures, then international economics affect the parameters imposed on governors’ policy options and should be discussed more overtly.

Third, the discussion of the role of partisan politics in this political understanding of policy development seems understated. Snyder discusses the effects of political competition, highlighting the differences between Oaxaca and Guerrero, both states with semi-competitive two-party systems. Both states shared neocorporatist re-regulation projects with strong small-producer organizations. However in Oaxaca, the producers sought an engaged productivist strategy that deemphasized politics, while in Guerrero the producers responded with a partisan strategy. While Snyder emphasizes that there is no “right” strategy in the re-regulation process, his conclusion is that there are two plausible re-regulation patterns after neoliberalism: “innovation along corporatist lines and, alternatively, hegemony by oligarchs” (196). If these are the only two plausible re-regulation patterns, then apolitical involvement in corporatist structures appears to be the recommended strategy for grassroots organizations, and Guerrero’s producers appear to have chosen the “wrong” strategy. Snyder points out the tensions between political and economic democratization, but limits the discussion of the restrictions on contestation inherent in corporatist structures to a footnote in the conclusion. Given Mexico’s slow pace

with political liberalization, and the relationship between democracy and participation, a more explicit discussion of what is meant by corporatism in a democratizing country is warranted.³

These issues aside, Snyder’s book marks an important change of course for comparative political economy from the analysis of neoliberal reforms to an analysis of the outcomes of these reforms. It does so with a well-formulated empirical analysis utilizing an innovative technique, the subnational comparative method. And perhaps most importantly, it emphasizes the political nature of economic reform, highlighting the continued importance of political actors in this era of free markets. *Politics After Neoliberalism* sparks a wide range of research questions concerning the continued role of the state in this neoliberal era.

¹Richard Snyder. “Scaling Down: The Subnational Comparative Method” in *Studies in Comparative International Development* 36:1 (Spring 2001).

² See Peter Evans. 1995. *Embedded Autonomy: States and Industrial Transformation*. Princeton, N.J.: Princeton University Press.

³See Philip D. Oshorn and Graciela Ducatenzeiler. 1998. *What Kind of Democracy? What Kind of Market? Latin America in the Age of Neoliberalism*. University Park, P.A.: The Pennsylvania State University Press. ❖

Good Read

Clausewitz in Afghanistan

By Carl von Clausewitz
On War, ed. Anatol Rapoport,
New York: Penguin, 1968 [1832].

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We were at war, so I read Clausewitz. It was not obvious that the Prussian military theorist would have insights to offer into our current preoccupations. When he wrote, terror still meant “La Terreur,” the Middle East had yet to be colonized and decolonized by the West, and anthrax was a disease of cattle (although, oddly enough, the French diplomatist Talleyrand was to die of it a few years later in 1838). War was still fought mostly by men on horses and marching ranks of infantry with muskets, notwithstanding the recent innovations of a certain brilliant, Corsican usurper.

Nevertheless, Clausewitz is reputed to have looked into the heart of War and glimpsed some of its essential features. He remains a source of inspiration in the military academies. Colin Powell read *On War* at the National War College. In his autobiography, he recalls that the book struck him “like a beam of light from the past, illuminating present-day military quandaries.”

So I turned to my disheveled, second-hand Pelican Classics edition of *On War*, with several quandaries of my own. The Bush Administration

decided early on to label the attacks on New York and Washington an act of “war”. Since then, we have responded with familiar military tactics—airial bombardment and commando raids. Yet our enemy is not so much a state as a concept, and a war against terrorism—like one against drugs or forest fires—can only be metaphorical. Would Clausewitz recognize this struggle as a war at all? Are his principles of strategy shaping the way we are fighting?

The Nature of War

A Clausewitzian war is one without limits—“an act of violence pushed to its utmost bounds.” It is “the shock of two opposing forces in collision with each other, from which it follows as a matter of course that the stronger not only destroys the other, but carries it forward with it in its movement.” No room here for sentimentality or the delicate battlefield ballet of the pre-Napoleonic great powers. The key to victory is to use overwhelming force, for “he who uses force unsparingly, without reference to the bloodshed involved, must obtain a superiority if his adversary uses less vigour in its application.” As a result, “[t]he best Strategy is *always to be very strong*, first generally then at the decisive point.” Generals should “keep the forces concentrated in an overpowering mass,” rather than dividing them up into separate armies and strategic reserves. Furthermore, force

should be concentrated in time: there is nothing to be gained by stretching conflicts out rather than betting everything on a single engagement.

Second, war is instrumental. It is “an act of violence intended to compel our opponent to fulfill our will.” Or, in Clausewitz’s most famous dictum, war is “a mere continuation of policy by other means.” Two points follow from this. First, political goals must govern the military decisions, and one must continually evaluate whether the two remain consistent. Military tactics can undermine the goals for which force is used. Second, military goals and tactics must enjoy domestic political support.

These features of Clausewitzian war hint at a rationalistic, almost deterministic, conception. War is fought by rational leaders who reconcile ends and means, and the outcome is determined by the balance of forces. But Clausewitz goes on to undermine this simplistic image in what is the book’s greatest contribution—an inspired description of the uncertainty in which wars are fought and the elements of military genius that such uncertainty brings to the fore. Here we truly see Clausewitz the philosopher-scientist, who, as Anatol Rapoport puts it in his introduction “sought simplicity and... distrusted it.”

War is a game, but—game theorists, take note—a game of roulette rather than chess. “[F]

rom the outset there is a play of possibilities, probabilities, good and bad luck, which spreads about with all the coarse and fine threads of its web, and makes War of all branches of human activity the most like a gambling game.” He mocks those who turn to a treatise on war expecting to find geometry or proofs. Mathematical theorems are about as much use here as a physics that does not recognize the effects of friction. Danger, the physical exhaustion of the troops, inaccurate information, weather, and an “infinity of petty circumstances” combine to complicate the implementation of simple plans, and render the actual fighting of war akin to walking underwater. The “correct theorists” are like those who try to teach swimming by demonstrating the necessary movements on dry land. Not only must soldiers march underwater, the lack of accurate information means that their generals must command them in the dark. In war, “all action must, to a certain extent, be planned in a mere twilight, which in addition not infrequently—like the effect of a fog or moonshine—gives to things exaggerated dimensions and an unnatural appearance.”

As a result: “Everything is very simple in war, but the simplest thing is difficult.” Military victories are determined as much by the genius—or lack thereof—of the generals as by the balance of forces. He describes with vivid precision the mental qualities that make up such genius—*coup d’oeil* (the ability to rapidly discover a truth “which to the ordinary mind is either not visible at all or only

becomes so after long examination and reflection”), resolution, and presence of mind, among others. From the arithmetic of force and the instrumentalities of war, we are drawn into a portrait of the psychology of greatness, built up, one senses, from observation of the master general to whom Clausewitz had himself devoted such long examination and reflection.

Fighting Terror

So what light can Clausewitz shed on our current military struggle? He warns us to expect friction and bad luck as well as good, and perhaps to reserve judgment on various aspects of the campaign until after the “fog and moonshine” recede. He reminds us to keep our political aims in view at all times.

The main reason Clausewitz makes interesting reading now, though, is because of his evident influence on some of those shaping our strategy. He remains popular in at least one camp within the US foreign policy establishment. Colin Powell’s contribution to strategic thinking—the so-called Powell Doctrine, which requires that the US only go to war in pursuit of clear goals with strong political support and that it use overwhelming force—comes straight out of *On War*. Caspar Weinberger, Powell’s former boss, articulated a similarly Clausewitzian doctrine in the early 1980s.

Are we following such an approach? It is hard to see how. Strong political support exists, but it seems secondary at this point for *what*. The stated mission—to destroy terrorist

organizations “with global reach” and the governments that assist them—is vague and elastic. No one seems to know exactly which states will end up on the outlaw list, and just what constitutes “global reach”. Our apparent readiness to expand objectives when things go well (to spread military action to Iraq or elsewhere) might well strike Clausewitz as reminiscent of Napoleon’s fatal blunder of invading Russia. As for using overwhelming force, there is a Clausewitzian echo in the use of “daisy-cutter” bombs to demoralize the Taliban. But when the enemy hides behind civilians or underground, the principle is harder to apply. The aerial force we have employed, while overwhelming to a poorly equipped, Third World army, represents a small fraction of the military resources the US could muster. We appear not so much to be using extreme force as using force against the extremely weak.

For neo-Clausewitzians in Washington, it must be frustrating how poorly the strategist’s insights seem to fit the current struggle against terror. This is not accidental. In fact, it could be argued that modern terrorism evolved precisely to target the chinks in the Clausewitzian armor.

Clausewitzian war—the use of overwhelming force to impose one’s will on an adversary—is an instrument of the strong. Modern terror is a mutant strain of resistance by the weak. How to withstand the application of overwhelming force, concentrated in time and place? Deny the enemy a target. Disperse your soldiers in global

networks. Strike at his weak points. Spread out attacks over time, exploiting moments of absent-mindedness when the powerful sleep. How to defeat a military leader who seeks to impose his will, who fights to achieve concrete political goals, who relies on civilian support? Strengthen your own will with religious or ideological fervor, lure the adversary into military tactics that undermine his goals, kill civilians. How to defeat the eagle who strikes from above? Become a snake who burrows in caves. Then, to instill fear, strike at the eagle's nest high up in its towering sanctuary.

Different soldiers learned different Clausewitzian lessons from the US failure in Vietnam. General Westmoreland learned that force must be overwhelming and that military strategy is sometimes shaped by political expediency. Colin Powell learned that it is futile to fight wars the American public will not support and that every use of force should have a clear goal. But the most useful lesson from that war may have been another: that the weak were developing instruments of their own—guerrilla warfare, terrorism—that exploited the weaknesses of the strong. To appreciate the significance of this—and adapt strategy accordingly—would have required real *coup d'oeil*.

¹ Clausewitz's distinction between resolution—"courage in the face of moral danger"—and boldness is particularly nice: "If a young man to show his skill in horsemanship leaps across a deep cleft, then he is bold; if he makes the same leap pursued by a troop of head-chopping Janissaries he is only resolute." Boldness is incompatible with necessity. ❖

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ple, the consequences of electoral rules are much better understood now than they were a decade ago. What I am arguing against are professional norms that privilege the narrow and elegant over the broad and less elegant, or over the important and less tidy. What I am arguing for is to recover space for the core concerns of our discipline by asking the "so what" question about the relevance of our research, and by thinking about its relationship to power, domination, and distribution. Or, to put it in different words, we need to interface research on political institutions and processes with the question of their ultimate effects on power and privilege.

A further clarification is in order: Saying that we need to address the important questions of political life, even at the expense of tidiness and elegance, does not at all mean that methodological discipline can be relaxed. I believe strongly that we need to adhere to the methodological canons of the "disciplined, rigorous center" of comparative politics identified by David Collier (APSA-CP Newsletter Vol. 10, Issue 2, Summer 1999). As he argued, this center needs to be constructed through a dialogue between quantitative and qualitative methods, innovations in comparative-historical analysis, and interaction between theory-driven research and inductive learning from cases. This certainly is the way to make progress and produce cumulative knowledge. Furthermore, the very pursuit of these kinds of investigative strategies enables us to put different weights on the

costs and benefits of parsimony and elegance vis-à-vis relevance for the core concerns of comparative politics than have seemingly become the norm in the profession.

Of course, research with a focus on the fundamental questions of power, domination, and distribution has not disappeared. It has been carried on, for instance, in the study of social movements, or contentious politics (e.g. McAdam, Tarrow, and Tilly 2001), of welfare states, poverty, and inequality (e.g. Glyn 2001, Hicks 1999, Huber and Stephens 2001, Pierson 1994, 2001, Tokman and O'Donnell 1998), of regime formation and transformation (e.g. Bunce 1999, Collier and Collier 1992, Hartlyn 1998, Hooghe and Marks 2001, Linz and Stepan 1996), and of the effects of market-oriented reforms (e.g. Snyder 2001). However, if we take the *APSR* as a barometer, this type of research is losing centrality and prestige in the discipline, and we need to restore it to prominence in order to channel the energies of our best and brightest graduate students in these directions.

How can we do that? Obviously, by pushing the frontiers of research in the areas just mentioned. In addition, I would like to indicate just a couple of research areas that seem to have been particularly neglected and promise to shed much-needed light on crucial questions. First, we should focus on economic elites as central political actors. This entails investigating networks of ownership and membership in organizations among corporate leaders, organizations of an economic, political, and

social nature, both at the national and the transnational level. We need to understand corporate elites both as managers of crucial economic organizations and as members of a social class, both as economic and political actors. Their preferences and strategies are crucial to an understanding of the formation and functioning of political regimes, such as the quality of democracy, and of distributive outcomes, such as welfare states, access to the judicial system, skill distributions in the society, etc. Unfortunately, this type of research is extremely difficult to do. Whereas public officials are constrained to be accessible to some extent and to provide public information, leaders in the private sector are not subject to such constraints. The dearth of information available, though, makes serious research in this area all the more important. There are some examples of this type of research, of both a historical (Swenson forthcoming) and contemporary nature (Conaghan and Malloy 1994; Domhoff 1998; Martin 2000), and some strands in the rapidly growing literature on varieties of capitalism address these issues as well (e.g. Hall and Soskice 2001).

Second, we should reinvestigate research on political parties as major organizers of political interests and channels for the attainment and exercise of power. There is wide agreement that parties are essential for the functioning of democratic regimes, but little established knowledge about the development of parties and party systems outside of Western Europe and North America. In

some areas of the world, notably Latin America, parties are an endangered species, which is alarming because it further weakens already low quality democracies. Yet, whereas we understand a lot now about the consequences of different systems of control over nominations within parties, we have little systematic comparative knowledge about sources of party financing, conditions that shape links between parties and social groups, the role of parties versus other social and political actors in shaping public opinion, and the conditions for adaptation and survival of political parties. There is important recent work to build on (e.g. Kitschelt et al. 1999, Mainwaring and Scully 1995) but much remains to be done until we understand the conditions under which parties shape different patterns of power and distribution.

If we want to stem excessive fragmentation of our field, then, and have research on a wide variety of topics and with different approaches contribute to building cumulative knowledge, we need to keep our eyes on the core concerns of comparative politics. By adopting the analytic approaches of the "disciplined, rigorous center" of comparative politics, researchers are able to address important and complex questions about power and distribution without sacrificing the scientific canons of investigation. What the profession needs to think about is the relative esteem accorded to elegant and parsimonious models making restrictive assumptions about reality and explaining narrow issues compared to more complex explanations of issues that have been central to the study

of comparative politics since Max Weber.

¹Peter Lange, in his first Letter from the Chair, specified several goals that the section was to realize (Newsletter Vol. 1, No. 1, 1989). One set of goals was to lessen the fragmentation of the comparative politics field and to prompt discussion of approaches and their merits and limitations. This discussion of approaches has been both very necessary and highly fruitful, and it has spurred important advances in writing about methodology, such as King, Keohane and Verba (1994) and the forthcoming response by Brady and Collier, as well as Bates et al. (1998), and the forthcoming volume by Mahoney and Rueschemeyer on comparative historical research. There has also been progress in lessening regional fragmentation through more discussion of how concepts and mid-range theories do or do not travel across regions and through more cross-regional comparisons.

²I was tempted to call this core the "soul" of comparative politics, playing on Michael Wallerstein's question whether comparative politics needs a TOE - a Theory of Everything (APSA-CP Newsletter Vol. 12, Issue 1, Winter 2001). I agree with Michael that comparative politics does not need a toe, and that efforts to impose one have not been particularly successful in the past and seem not very promising in the near future. However, I would like to suggest that it does need a soul, or a set of common core concerns, from which our thousands of diverse research enterprises derive inspiration and purpose. To spin the organic analogy further - loss of a toe can be tolerated, loss of the soul means loss of the essence of being.

³In order to save space in the newsletter, there is no list of references provided here; the list is available at <http://www.shelley.polisci.ucla.edu/apsacp>

⁴With my apologies to the authors of other classic works, this cannot possibly be an exhaustive list, but has to remain an unsystematic sample for illustrative purposes. The same holds for all the following references to contemporary works.

⁵I would like to thank Gabriel Ondetti for research assistance. The data come from a coding of articles from Volume 85, No. 1 (1991), through Volume 94, No. 4 (2000).

References for this article will be available on the APSA-CP website at www.shelley.polisci.ucla.edu/apsacp. ❖

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degree of electoral competition also increases. Around the region, Brazil is the only country that requires candidates to report contributions to their campaigns. The campaign finance data that the government releases have flaws, but Brazilian newspapers have successfully employed these data to expose the relationships between particular business interests and political campaigns. Perhaps not surprisingly, the data also reveal that campaign spending in Brazil increases when electoral competition increases. The confirmation of this finding from other countries implies not simply that electoral competition is thriving in Brazil, but also that democratization and the concomitant increase in the intensity of electoral competition may be associated with increases in corruption,

because there is much going on behind the scenes that the data cannot reveal.

Of course, this is true for any country. Even the US, with the strongest campaign-finance laws on the planet, suffers from the problems of "soft money," as scandals repeatedly reveal. The important point is that around Latin America, enhanced electoral competition may have increased the "price" that private interests must pay to assure themselves of continued privileged access to government services. This dynamic is illustrated by the fact that in the US, business interests hedge their bets by contributing to both parties. In short, in the absence of strong laws or oversight agencies, electoral competition may be associated with increases in corruption, not decreases. Problems in the data should not preclude empirical research on the connections

between money and politics.

The study of corruption has always been relevant, but empirical research about "dirty politics" is notoriously difficult, and this has perhaps also discouraged theoretical investigation. However, especially in the area of political economy, scholars are developing new methodological tools to address these long-standing issues. The increased availability of economic and fiscal data in a number of countries ought to permit at least indirect testing of our theories about the relative increase or decrease in corruption given different political and social contexts.

¹See for example "Relative Capture of Local and Central Governments: An Essay on the Political Economy of Decentralization," by Pranab Bardhan and Dilip Mookherjee. UC Berkely Dept. of Economics, 1999. ❖

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