The Organized Section in Comparative Politics of the American Political Science Association

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Letter from the Editors

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On behalf of all of the members of the Organized Section in Comparative Politics the editors wish to extend their sincerest appreciation and warmest wishes to Sid Tarrow, our outgoing President, for his dedicated service to the section during the past two years. Following in the large footsteps of the section's past presidents – Peter Lange, Ron Rogowski, David Laitin, Bob Bates, David Collier, the late Michael Wallerstein, Evelyne Huber, and Peter Hall – Sid Tarrow inspired us in his annual Letters from the President with his insights into the spatial dimensions of politics and his challenge to renew the intellectual bonds between the disciplines of sociology and political science. Under his active and able leadership the section's paid membership has remained robust, even as the number of formal, organized sections has proliferated. Thanks to his entrepreneurship and particularly his successful efforts to engage the talents of others, our section continues as the largest organized section of the American Political Science Association.

With Sid Tarrow's departure, we welcome to the pages of this newsletter our incoming section president, Peter Gourevitch, professor at the Graduate School of International Relations and Pacific Studies at the University of California at San Diego. An intellectual whose work has uniquely bridged the divide between the subfields of international relations and comparative politics, Peter's scholarship is primarily rooted in the field of international political economy with an emphasis on international trade and economic globalization, trade disputes, and regulatory systems. Comparativists whose intellectual training began more than a few years ago more often than not first came across Peter's work when his path-breaking article, "The Second Image Reversed: The International Sources of Domestic Politics" appeared in 1978 in *International Organization* (IO) and his classic book, *Politics in Hard Times*: *Comparative Responses to International Crises*, was published in 1986. As a small measure of its global reach, *Politics in Hard Times* was subsequently translated into Korean, Italian, and Spanish. Both established and younger comparativists, of course, also know Peter well from his superb stewardship, with co-editor David Lake, of IO between 1997-2001. In addition to *Politics in Hard Times* Peter has produced nine books including, with James P. Shinn, *Political Power and Corporate Control: The New Global Politics of Corporate Governance* (2005). He has also authored or coauthored more than 30 articles. We are grateful to Peter for committing to serve as comparative politics section president and look forward to his contributions to this newsletter.

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Guest Letter
Bolivarian Democracy

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Despite Hugo Chávez's recent claims to be establishing Bolivarian democracy in Venezuela, Simón Bolívar himself was no democrat. During European tours between 1799 and 1807, he acquired broad familiarity with Enlightenment thought, but he favored the Enlightenment's emphases on rationality and freedom over its themes of equality and democracy. Starting in 1811, he led the military campaign for independence from Spain in Latin America's northern mainland. He eventually became famous as El Libertador, the liberator.

Chávez's evocation of Bolívar as his model calls attention especially to Chávez's expressed hope of liberating Venezuela and Latin America from North America's imperial influence. But his claim to be creating a distinctively Bolivarian form of democracy raises some important questions about his regime and about democratization in general. To what extent is Chávez's commandeering of national oil revenues to check his domestic opposition, pursue his populist programs, and support anti-American programs elsewhere actually promoting democracy in the long run?

Instead of answering that question fully, this brief provocation draws out two related implications of my recent book (Tilly 2007) for ways of addressing the question both in Venezuela and across the world. First, existing analyses of democratization and de-

democratization greatly underestimate the influence of the political arrangements by which rulers acquire the means of rule – resources supporting administration, political control, and patronage. Second, analysts of democratization have exaggerated the centrality of competitive elections as the site of negotiated consent to rule. They have slighted the day-to-day negotiation of compliance with state demands in such domains as taxation, military service, policing, and provision of information.

Joseph Schumpeter set the prevailing narrow view: "[T]he democratic method is that institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people's vote" (Schumpeter 1947: 269). This essay argues for a broader and different conception of consent. Grudging consent remains essential to democracy. But it concerns not only who is to rule, but also how they rule, with what resources, supplied by whom, and how.

"Grudging consent remains essential to democracy. But it concerns not only who is to rule, but also how they rule, with what resources, supplied by whom, and how."

Before getting to the heart of that argument, it will help to compare Chávez's liberation of Venezuela with

that of his claimed predecessor, Bolívar. It took Bolívar from 1813 to 1819 to become dictator of a permanently independent Gran Colombia including Venezuela and Colombia. In 1821, Gran Colombia annexed Ecuador as well. Soon Bolívar's troops and allies had also liberated a reluctant Peru. This newly liberated regime soon split into Peru and a separate country named for the liberator: Bolivia. As his political experience and power accumulated, Bolívar came to value order and authority far more than liberty, equality, and fraternity.

Within a few years, political rivalries undermined Bolívar's prestige and influence. Instead of giving him credit for national liberation, his rivals and successors blamed him for the arbitrary use of power. Bolívar spent his final days exiled from Bogotá and Caracas to Colombia's Caribbean coast. By that time he had lost whatever little faith in democratic constitutions he had expressed when launching the Republic of Gran Colombia in 1819 (Bushnell 2003: 31-53). On 9 November 1830, he wrote sadly from Barranquilla to General Juan José Flores:

> You know that I have ruled for twenty years, and I have derived from these only a few sure conclusions: (1) America is ungovernable, for us; (2) Those who serve revolution plough the sea; (3) The only thing one can do in America is emigrate; (4) This country will fall inevitably into the hands of the unrestrained multitudes and then into the hands of tyrants so insignificant they will be almost imperceptible, of all colors and races; (5) Once we've been eaten alive by every crime and extinguished by ferocity, the Europeans won't even bother to conquer us; (6) If it were possible for any part of the world to revert to primitive chaos, it would

> be America in her last hour (Bushnell 2003: 146).

Democracy, he concluded, inevitably gave way to demagoguery, anarchy, and tyranny. Five weeks later, Bolívar died on his way to exile in Europe.

"[...] Bolívar came to value order and authority far more than liberty, equality, and fraternity [...] Democracy, he concluded, inevitably gave way to demagoguery, anarchy, and tyranny.

Until the early 20th century Venezuela staged a familiar, dreary Latin American drama of military dictators, caudillos, coups, and occasional civilian rule. In 1908, however, a coup led by General Juan Vicente Gómez introduced a new era. He didn't rule exclusively by brute force. The opening of Venezuela's vast oil fields in 1918 provided him with revenues that allowed him to pay off his followers and hold off his enemies. From that point to the present, the regime oscillated uneasily, and sometimes abruptly, between outright authoritarian rule and partial democratization, both of them powered by oil revenues. De-democratization occurred whenever some caudillo, general, junta, or corrupt president used control over those revenues to withdraw from negotiating with representatives of the public will.

During the early 1980s, a group of nationalist army officers organized a secret network called the Revolutionary Bolivarian Movement. Paratroop officer Hugo Chávez

became their leader. In 1992, the Bolivarians almost seized power in a military coup whose failure sent Chávez to prison. In 1993, while Chávez languished behind bars, the Venezuelan congress impeached President Carlos Andrés Pérez's for corruption, and removed him from office. But Pérez' successor, Rafael Caldera, soon faced a collapse of the country's banks, a surge of violent crime, rumors of new military coups, and charges of his own corruption. As Chávez left prison and entered politics, popular demands for political housecleaning swelled. Ex-convict Chávez ran for president.

Chávez billed himself as a populist, and won by a large majority. As Chávez came to power in 1999, street confrontations between his supporters and his opponents accelerated. The new president's state visit to Fidel Castro's officially socialist Cuba later the same year dramatized his plan to transform the government and its place in the world at large. He renamed his country the Bolivarian Republic of Venezuela. He also began squeezing the state oil company, Petróleos de Venezuela, for more of its revenues, and chipped away at its fabled autonomy. Venezuela moved into a new stage of struggle over the country's future. Chávez defined that future as Bolivarian democracy.

When Chávez addressed a June 2000 meeting of the Andean Community in Lima, Peru, he denied that pre-Chávez Venezuela had been democratic. He announced his own version of democracy:

> We in Venezuela have had the idea – maybe a daring one – of referring to a Bolivarian democracy; we have taken up Bolivarian thinking to try to direct our people and to try to sow that awareness and to build that new model. Bolívar, for example, on

February 15, 1819 at the Congress of Angostura (there the idea of Colombia was born) stated in his speech that the most perfect system of government is that which gives the people the greatest amount of social security, the greatest amount of political stability, and the greatest amount of happiness possible (Chávez 2000: 3).

Chávez added that Bolivarian democracy promoted political equality and established a "happy balance" between extreme freedom, at one extreme, and autocracy, at the other. In short, he described a familiar Latin American program of top-down populism. In contrast to the western models of democracy that he vigorously rejected, the program offered plenty of elections, but made little provision for popular consultation and consent with respect to the means of rule. It followed Bolívar, furthermore, in featuring rebellion against foreign domination, especially that of the United States

Over the next seven years, Chávez used his control over oil revenues to consolidate his power, to cramp his opposition, to sponsor populism elsewhere in Latin America, and even to hold off an increasingly hostile United States. He survived a coup in 2002, concerted resistance from the national oil company in 2002-2003, a general strike during the same period, and a US-supported recall referendum in 2004. Step by step he responded with tightened repression. A Chávez-dominated legislature packed the Supreme Court, broadened prohibitions on insulting or showing disrespect for the president, and stepped up surveillance of mass media. Meanwhile, the courts prosecuted increasing numbers of regime opponents.

Chávez still enjoyed substantial support among Venezuela's numerous poor. But like heads of rentier states elsewhere he was relying on his country's oil-generated wealth to bypass resistance to his means of rule. Echoing his 2000 speech to the Andean Community, in 2006 he told a sympathetic interviewer that "We are building a true democracy, with human rights for everyone, social rights, education, health care, pensions, social security, and jobs" (Palast 2006: 2). He did not mention rights to opposition and consent.

> "[Chávez] described a familiar Latin American program of top-down populism. In contrast to the western models of democracy that he vigorously rejected, the program offered plenty of elections, but made little provision for popular consultation and consent with respect to the means of rule."

In this brief essay, I am less interested in joining the intense debate over Hugo Chávez's virtues and vices than in pointing out the long-term institutional implications of avoiding or engaging popular consent for state actions. In very general terms, democratization consists of increasing correspondence between a given regime's popular voice and its governmental performance. More precisely, we can think of democratization as occurring when popular voice is becoming broader, more equal, more binding on governmental agents, and better protected from arbitrary action by governmental agents. De-democratization (which continues to occur frequently in the contemporary world) then consists of narrowing, increasingly unequal, less binding, and more poorly protected popular voice.

> "[...] we can think of democratization as occurring when popular voice is becoming broader, more equal, more binding on governmental agents, and better protected from arbitrary action by governmental agents."

Serious democratization on a national scale has only occurred anywhere in the world during the last 250 years. Over that period, three main processes have promoted democratization: insulation of public politics from categorical inequalities such as by gender, race, or religion; contingent integration of interpersonal trust networks (e.g. lineages, trade diasporas, and craft fellowships) into public politics; and elimination of autonomous centers of coercive power (e.g. militias and mafias) from public politics (Tilly 2007). Reversal of any or all of the three processes promotes de-democratization.

In its own way, each of the three processes contributes to the strengthening of popular consent. Insulation of public politics from categorical inequality facilitates political communication and coalition-formation across categorical boundaries. Contingent integration of trust networks increases both popular lever-

age and popular stakes in the outcomes of public politics. Elimination of autonomous power centers political activity on bargaining between rulers and ruled. All three processes become more likely when rulers must bargain with their subject populations for resources to support state activity.

"[...] during the last 250 years, three main processes have promoted democratization: insulation of public politics from categorical inequalities [...]; contingent integration of interpersonal trust networks [...] into public politics; and elimination of autonomous centers of coercive power [...] from public politics [...]."

How rulers acquire the means of rule therefore has profound consequences for the long-term development of relatively broad, equal, binding, and protected consultation of a regime's population concerning state actions – that is, for democratization. Here is the nucleus of my argument:

[1] No one can run a state without social arrangements that produce and reproduce resources supporting administration, political control, and patronage.

[2] Rulers have three main ways of acquiring such resources: a) produce them in their own enterprises; b)

seize them and exchange them for state-sustaining resources, e.g. oil for weapons; c) or extract them from subject populations that already hold and/or produce the resources.

[3] Options a and b reduce the reliance of rulers on citizen consent and incentives for bargaining with civilian populations, hence constitute impediments to democratization, conceived of as increased breadth, equality, protection, and bindingness of popular voice.

[4] Although c by no means guarantees democratization, it opens a possible path to democratization.

[5] Historically (with taxation being the most obvious process), most of the world's democratization over the last two hundred years has resulted in part from c. Alternatives a and b have mostly inhibited democratization.

[6] Nevertheless, if through a transfer of power (e.g. revolution or conquest) arrangement a or b becomes subject to popular collective control, that transfer likewise opens a path toward democratization.

Historically, many rulers supported themselves through arrangement a, for example by drawing revenues from their own lands or preying on adjacent populations. Twentieth-century communist regimes made efforts to institute self-sustaining arrangements. But nowhere today do rulers sustain their rule mainly from their own production of resources.

Arrangement b, on the other hand, became more common during the twentieth century, as rulers avoided consent by monopolizing and selling resources, most obviously fossil fuels. Today, Venezuela is far from alone. Bolivia, Chad, Kazakhstan, Libya, Russia, Sudan, Tunisia, Turkmenistan, Uzbekistan, and most

oil-producing Middle Eastern states avoid consent by relying on sales of monopolized energy supplies. (Yet energy production does not automatically forbid consent; for all their other democratic deficits, the oil-producing American, Mexican, and Nigerian states do actually bargain with their subject populations.)

"How rulers acquire the means of rule [...] has profound consequences for the long-term development of relatively broad, equal, binding, and protracted consultation of a regime's population concerning state actions – that is, for democratization."

Arrangement c — extraction of resources from a state's subject population — has historically prevailed, as rulers' demands for resources outran their ability to produce them and salable resources either ran short or slipped out of state control. In the forms of taxation, conscription, forced loans, and outright confiscation, extraction has marked the main historical path of state transformation. Indeed, struggle over extraction so prevailed in the European experience on which my work long concentrated that it took me years to recognize the distinct trajectories of historical regimes that adopted arrangements a and b.

When democratization has occurred, it has developed in part as an unintended by-product of extraction. Taxation, conscription, forced loans,

and even outright confiscation lead to bargaining, however asymmetrical, between rulers and their subject populations. Unlike Mongols who could simply strike and carry away wealth from alien peoples, extractive rulers always return to the sources of their supplies, and must at a minimum declare the conditions for future extraction. Moreover, populations that are producing, using, and storing the essential supplies – manpower, food, animals, money, information, and more – retain the capacity to conceal, divert, consume, or otherwise withhold them from rulers' agents. These circumstances promote bargaining. And bargaining causes grudging consent.

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Grudging consent is not a sufficient condition for democracy, but it is a necessary one. Grudging consent supports democracy more surely than happy assent because it allows political actors to withdraw consent from unacceptable state performance. In Albert Hirschman's famous terms, democratic citizens may provide loyalty when their governments falter temporarily, but in general they

shrewdly combine voice and the threat of exit. They offer grudging consent. Under most circumstances, however, rulers' successful pursuit of arrangement a or b bypasses consent, even grudging consent.

> "Grudging consent is not a sufficient condition for democracy, but it is a necessary one. Grudging consent supports democracy more surely than happy assent because it allows political actors to withdraw consent from unacceptable state performance."

We fans of democratization can read this analysis pessimistically or optimistically. In a pessimistic version, so long as rulers can produce their own essential supplies or sell off monopolized goods or service in exchange for the means of rule, attempts at democratization will fail. Either of these ruling strategies, if successful, baffles consent. As pursued by Hugo Chávez, Bolivarian democracy does just that in the name of top-down populism.

But two optimistic possibilities appear. First, arrangements a and b tend to exhaust themselves as supplies diminish or state demands increase. If world energy prices or demand were to fall precipitously, type b rulers' comfortable avoidance of consent would give way to type c extractive policies and/or splintering of ruling coalitions. Second, the very concen-

tration of executive power produced by both arrangements a and – especially – b facilitate the revolutionary seizure of that power. To be sure, the revolutionaries could turn out, as they often do, to be new tyrants who simply displace the old. But they could also speak for sufficiently broad popular coalitions to guarantee new openings toward democracy.

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Policy Implications of Research on Civil Wars

Introduction

Given the intense public debate about the war in Iraq, we thought it would be a good time to organize a policy-related symposium on the subject. Even those of us who do not specialize on civil war cannot resist analyzing and debating whether there is a civil war there, what is fueling it, whether it can be stopped, and what is likely to happen if the US withdraws. But some of our colleagues actually do specialize in the study of civil wars and have a well-informed comparative perspective on these questions, and some have offered their advice to policymakers. The purpose of this symposium is to have several experts share their thoughts in order to raise the level of our understanding, teaching, and discussion on one of the most salient issues of the day.

"[...] the best anyone can hope for in the near future is a situation similar to that in Lebanon from 1975 to 1991 [...]."

The lead essay is a condensed version of invited testimony that James Fearon gave to the House Subcommittee on National Security, Emerging Threats, and International Relations at its hearing on "Iraq: Democracy or Civil War?" in September 2006. We distributed this essay in advance to the other symposium contributors so that they could agree, disagree, or weigh in on a complementary theme. Fearon's

bleak and sobering testimony finds that Iraq has been engaged in a civil war for some time, that it is likely to end only with a decisive military victory rather than any power-sharing accord, and that the best anyone can hope for in the near future is a situation similar to that in Lebanon from 1975 to 1991: a long-lasting civil war among highly decentralized militias that ended up drawing in most of the neighboring states. The only bright note in Fearon's account – if it can be called "bright" – is that the US still has a small chance of preventing an even worse outcome if it resists the temptation to withdraw precipitously.

"[...] powerful incentives impel actors to challenge one another's authority."

Barbara Walter uses the example of the Iraq war to advance a general hypothesis about the causes of civil wars. She argues that such conflicts are more likely to break out in new states governed by a politically tenuous coalition that lacks effective control over the armed forces. In these situations it is nearly impossible for actors to make credible commitments to one another; instead, powerful incentives impel actors to challenge one another's authority. Her blow-by-blow recounting of the disintegration of order in Iraq supports both her hypothesis and Fearon's bleak prognosis.

Bethany Lacina and Jeremy Weinstein emphasize the limited usefulness of research for policy, echoing many of the sentiments expressed in the Summer 2003 symposium on "The Relevance of Comparative

Politics for Public Life." Researchers can help policymakers understand what is going on – for example, that combat deaths are only a fraction of the total casualties from internal conflict, especially in poor non-democracies. But they are not well-equipped to recommend solutions because they tend to identify causal factors – regime type, level of development, state strength – that are not easily manipulated.

"[...] combat deaths are only a fraction of the total casualties from internal conflict [...], especially in poor non-democracies.

Moreover, they are only beginning to study the effectiveness of interventions such as peacekeeping missions and demobilization that *are* in the policymakers' toolkits. And even if they expanded such focused research, Lacina and Weinstein note, it would not resolve complex and difficult tradeoffs among the various normative goals that conflicts create. They call on scholars to reach out beyond comparative politics, to political theory, to find answers to these questions.

Congressional Testimony (excerpts)

Editors' Note

This an edited and abbreviated version of Testimony to U.S. House of Representatives, Committee on Government Reform, Subcommittee on National Security, Emerging Threats, and International Relations on "Iraq: Democracy or Civil War?" September 15, 2006. The complete testimony can be found at http://www.stanford.edu/~jfearon/. See also James Fearon, "Iraq's Civil War," *Foreign Affairs* (March/April 2007).

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A *civil war* is a violent conflict within a country, fought by organized groups that aim to take power at the center or in a region, or to change government policies. How much violence is enough to qualify a conflict as a civil war as opposed to terrorism or low-level political violence is partly a matter of convention. The rate of killing in Iraq – easily more than 30,000 in three years – puts it in the company of many recent conflicts that few hesitate to call "civil wars" (e.g., Sri Lanka, Algeria, Guatemala, Peru, Colombia).

Civil wars typically last much longer than international wars. For civil wars beginning since 1945, the average duration has been greater than 10 years, with fully half ending in more than seven years (the median). The numbers are fairly similar whether we are talking about wars for control of a central government, or wars of ethnic separatism.

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When they finally do end, civil wars since 1945 have typically concluded with *a decisive military victory* for one side or the other. In contests for control of the central state, either the government crushes the rebels (at least 40% of 54 cases), or the rebels win control of the center (at least 35% of 54 cases). Thus, fully three quarters of civil wars fought for control of the state end with a decisive military victory.

Power-sharing agreements that divide up control of the central government among the combatants are far less common than decisive victories. I code at most 9 of 54 cases, or 17%, this way. Examples include El Salvador in 1992, South Africa in 1994, and Tajikistan in 1998.

In the rare cases where they have occurred, successful power-sharing agreements have usually been reached after an intense or long-running civil war reaches a stalemate. One of the main obstacles to power-sharing agreements seems to be political and military divisions *within* the main parties to the larger conflict.

Why Is Successful Power-Sharing to End Civil Wars So Rare?

If successful power-sharing agreements rarely end civil wars, this is not for lack of trying. Negotiations on power-sharing are common in the midst of civil war, as are failed attempts to implement such agreements, often with the help of outside intervention by states or international institutions. For example, the point of departure for the Rwandan genocide and the rebel attack that ended it was the failure of an extensive power-sharing agreement between the Rwandan government, Hutu opposition parties, and the RPF insurgents.

The main reason power-sharing agreements rarely work is that civil war causes the combatants to be organized in a way that makes them fear that the other side will try to use force to grab power, and at the same time be tempted to use force to grab power themselves. These fears and temptations are mutually reinforcing. If one militia fears that another will try to use force to grab control of the army, or a city, then it has a strong incentive to use force to prevent this. The other militia understands this incentive, which gives it a good reason to act exactly as the first militia feared. In the face of these mutual fears and temptations, agreements on paper about dividing up or sharing control of political offices or tax revenues are often just that – paper.

Civil wars for control of a central government typically end with one-sided military victories rather than power-sharing agreements, because the parties are organized for combat and this makes trust in written agreements on the allocation of revenues or military force both dangerous and naïve. The US government and Iraqi politicians have attempted to put a power-sharing agreement in place in the context of a new, very weak central govern-

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ment and a violent insurgency and attendant militia conflicts. While the US military could easily destroy Saddam Hussein's formal army, militias and insurgents are "closer to the ground" and cannot by completely destroyed or reconfigured without many years of heavy occupation and counterinsurgency, if even then. This means that however long we stay, power-sharing is likely to fall apart into violence once we leave.

> "One of the main obstacles to power-sharing agreements seems to be political and military divisions *within* the main parties to the larger conflict."

Likely Consequences of US Withdrawal: Iraq versus Bosnia

What will that violence look like, on what scale and with what consequences? A central argument against rapid withdrawal of US troops is that this would lead to a quick descent into all-out civil war. The example of Bosnia in 1992 is sometimes invoked, when systematic campaigns of ethnic cleansing caused the deaths of tens of thousands in the space of months. Though there are some important differences, the analogy is a pretty good one. As argued above, US withdrawal, whether fast *or* slow, is indeed likely to cause higher levels of violence and political disintegration in Iraq. But rapid withdrawal would be particularly likely to lead to mass killing of civilians.

In Bosnia, massive and bloody ethnic cleansing was the result of systematic military campaigns directed by irredentist neighboring states and their local clients. For Milosevic and the

Bosnian Serb leadership, the whole point was to rid eastern Bosnia and Banja Luka in the west of Muslims. To my knowledge, no significant players on either the Sunni or Shiite side talk about wanting to break up Iraq by creating a homogenous Sunni or Shiite polity. Instead there remains a strong sense that "we are all Iraqis," even if they may strongly disagree about what this implies for politics.

To date, "ethnic" cleansing in Iraqi cities has been much less systematic, less centrally directed, and more individual than it was in Bosnia in 1992. The breakdown of policing plus insurgent attacks have led to the supply of local "protection" in the form of sectarian militias and gangs. Whether seeking generic revenge, suspected killers from the other side, or profit from extortion and theft, gangs make life extremely dangerous for members of the minority faith in their neighborhood. So Shiites exit Sunni-majority neighborhoods while Sunnis exit Shiite-majority neighborhoods. This is a "dirty war" in which gangs torture and kill suspected attackers or informants for the other side, along with people who just get in the way or have something they want.

Rapid reduction in US troop levels is not likely to cause a massive spasm of communal violence in which all Shiites start trying to kill all Sunnis and vice versa. But it *may* spur Moqtada al-Sadr to order his Mahdi army to undertake systematic campaigns of murder and, in effect, ethnic cleansing in neighborhoods in Baghdad and other cities where they are strong. Obviously a murky subject, some recent reports suggest that such plans exist.

Gradual redeployment and repositioning of US troops within the region is needed to allow populations to sort themselves out and form defensible lines that would lessen the odds of

sudden, systematic campaigns of sectarian terror in mixed neighborhoods. This is one of the strongest arguments against rapid US military withdrawal. Gradual redeployment – or, for that matter, "staying the course" – improves the chances of a less violent transition to a "Lebanon equilibrium" of low-level, intermittent violence across relatively homogeneous neighborhoods controlled by different militias.

> "[...] US withdrawal, whether fast *or* slow, is indeed likely to cause higher levels of violence and political disintegration in Iraq. But rapid withdrawal would be particularly likely to lead to mass killing of civilians."

Likely Consequences of US Withdrawal: Lebanon 1975-91

What happened in Lebanon in 1975-76 is a likely scenario for Iraq today. As violence between Christian militias and PLO factions started to take off in 1975, the army leadership in Lebanon initially stayed out, realizing that if they tried to intervene the national army would splinter. The violence escalated and eventually the army intervened, at which point it did break apart. Lebanon then entered a long period during which an array of Christian, Sunni, Shiite, and PLO militias fought each other off and on, probably as much *within* sectarian divides as across them. Syrian and Israeli military intervention sometimes reduced and sometimes escalated the violence. Alliances shifted, often

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in Byzantine ways. For example, the Syrians initially sided with the Christians against the PLO.
To some extent this scenario is already playing out in Iraq. US withdrawal – in my opinion whether this happens in the next year or in five years – will likely make Iraq (south of the Kurdish areas) look even more like Lebanon during its long civil war.

As in Lebanon, effective political authority will devolve to city, region, and often neighborhood levels, and after a period of fighting to draw lines, an equilibrium with low-level, intermittent violence will set in, punctuated by larger campaigns financed and aided by foreign powers. As in Lebanon, we can expect a good deal of intervention by neighboring states, and especially Iran, but this intervention will not necessarily bring them great strategic gains.

"Gradual redeployment and repositioning of US troops within the region is needed to allow populations to sort themselves out and form defensible lines that would lessen the odds of sudden, systematic campaigns of sectarian terror in mixed neighborhoods."

The Lebanese civil war required international intervention and involvement to bring to conclusion. If an Iraqi civil war post-US withdrawal does not cause the formal breakup of the country into three new states, which it

could, then ending it will almost surely require considerable involvement by regional states to make whatever power-sharing arrangements they ultimately agree on credible.

"Ramping Up" or "Staying the Course" Are Delay Tactics, Not a "Strategy for Victory"

In broad terms, the US has three options in Iraq: (1) ramp up, increasing our military presence and activity; (2) "stay the course" (aka "adapt to win"); and (3) gradual redeployment and repositioning our forces in the region, so as to limit our costs while remaining able to influence the conflict as it evolves.

The analysis above suggests that *none* of these options is likely to produce a peaceful, democratic Iraq that can stand on its own after US troops leave. While we are there in force we can act as the guarantor for the current or a renegotiated power-sharing agreement underlying the national government. But, in a context of many factions and locally strong militias, mutual fears and temptations will spiral into political disintegration and escalation of militia and insurgent-based conflict if and when we draw down.

"Ramping up" by adding more brigades could allow us temporarily to suppress the insurgency in the Sunni triangle with more success, and to prevent "Al Qaeda in Iraq" (AQI, which now consists overwhelmingly of Iraqi nationals) from controlling the larger towns in this area. Ramping up could also allow us to temporarily bring greater security to residents of Baghdad, by putting many more troops on the streets there.

But Congress and the Bush administration have to ask what the long-run point is. The militia structures may recede, but they are not going to go

away (absent some truly massive, many-decade effort to remake Iraqi society root and branch, which would almost surely fail). Given this, given myriad factions, and given the inability of Iraqi groups to credibly commit to any particular power- and oil-sharing agreement, *ramping up or staying the course amount to delay tactics*, not plausible recipes for success.

"[...] mutual fears and temptations will spiral into political disintegration and escalation of militia and insurgent-based conflict if and when we draw down."

Costs of Redeployment and Repositioning

Even if ramping up or staying the course are not "strategies for victory" as the administration has defined it, this does not imply that immediate withdrawal is the best course of action. Indeed, in principle it could be that the costs of withdrawal are so high at this point that the best option is to continue the status quo as long as possible. I seriously doubt this is the case. But I would agree that there are potential risks and costs to US national security from reducing our troop presence in Iraq, and that the question of "how to do it" to minimize these risks and costs is extremely complicated.

Rapid withdrawal of US forces would most likely cause rapid escalation of the sectarian and intra-sectarian dirty war, making for a sharp rise in civilian deaths well above the current rates. A more gradual reduction and repositioning of US forces within the region

would be far better, as it would allow mixed populations to sort themselves out in the larger cities, and to keep the rate of escalation of militia conflict as low as feasible. Gradual redeployment would also allow the US to prevent (through joint operations and other such mechanisms) the Iraqi army from rapidly becoming a full partisan in the dirty war.

"A more gradual reduction and repositioning of US forces within the region [...] would allow mixed populations to sort themselves out in the larger cities [and] keep the rate of escalation of militia conflict as low as feasible."

Al Qaeda in Iraq (AQI) is now the principle insurgent enemy of US forces in the Sunni-dominated provinces to the west of Baghdad, although it remains unclear how to interpret the nature and likely trajectory of this organization. Reduction of US troop presence in Al Anbar and the other Sunni-majority provinces would almost certainly lead to AQI and other Sunni insurgent forces taking fuller political and military control in these areas. The question for US policy is what sort of threat this would pose US interests, and what could be done about it. Though we are obviously in the realm of speculation here, I think the common assumption that Al Anbar would become like southern Afghanistan under the Taliban – a home for Al Qaeda training camps producing terrorists to

attack the US homeland – merits critical scrutiny.

The threat of increased Iranian influence seems to me the least persuasive argument about the costs of reducing the US military presence in Iraq. In the first place, it should be stressed that if the US were to *succeed* in helping to set up a peaceful, democratic Iraqi government that can stand on it own, there is no question but that Iran would have much more influence with this government and in the Middle East in general than it had under the Saddam Hussein regime.

"[...] there are potential risks and costs to US national security from reducing our troop presence in Iraq, and that the question of 'how to do it' to minimize these risks and costs is extremely complicated."

Compared to this scenario (which is the implausible object of current US policy), the scenario of a Lebanon-like civil war in Iraq that follows US redeployment probably implies *less* Iranian influence in the Middle East as a whole, and more costly Iranian influence in Iraq for Iran itself. Iran would be drawn in, much more than at present, to funding and arming Shiite factions against each other and against Sunni insurgents. Various Iranian leaders have said that they much prefer that the US continue to "stay the course" in Iraq, and that they are quite worried about the prospect of an escalated civil war on their doorstep.

Conclusion

"Staying the course" or "ramping up" in Iraq may put off political disintegration and major escalation of the civil war in progress, but are unlikely to produce a democratic government that can stand on its own and maintain peace after US troops are gone. The most likely scenario following reduction of US troop presence is the escalation of a Lebanon-like civil war. Unfortunately, the odds that this will occur are probably not much better if US troops stay for five (or even more) years as opposed to one.

The evidence supporting this assessment is drawn from the experience of other civil wars. Historically, civil wars tend to last a long time and usually end with decisive military victories. Successful power-sharing agreements to end civil wars are rare. When they have occurred, they have typically required that the combatants not be highly factionalized and that the balance of military power and prospects for victory be well established by years of fighting.

Iraq: Why Weak States Breed Violence

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Almost all the scholarly literature on Iraq has focused on how the civil war is likely to end, not on why it began. This heavy focus on termination is not surprising. When a country is at war, people want to know how the conflict can be resolved, not why violence emerged in the first place. Moreover, the reason civil war erupted in Iraq seems obvious. The insurgency began after the United States deposed Saddam Hussein and shut his supporters out of power. De-Baathification and the formal dissolution of the military created a heavily armed segment of society that had little or no stake in the new government and every reason to overthrow it.

I argue that the war in Iraq began, in part, because of two difficult-to-resolve bargaining problems that tend to afflict new and weak states. In states where the ruling coalition is fragile and where a state's military capacity is weak, enormous uncertainty exists. This uncertainty has two negative effects on the potential for political compromise. First, it creates strong incentives for political leaders to resist compromise in order to signal toughness, thus deterring other groups from challenging the state. Second, it creates strong incentives for opposition groups to reject any agreement, because incumbent elites cannot credibly guarantee that they will implement its terms.

In what follows, I briefly describe the bargaining problems that are likely to make political settlements in weak states particularly difficult to reach and implement. I then apply the theory to the civil war in Iraq to see if it helps illuminate this important case. Could the war in Iraq be driven, in part, by private information Shi'a factions have about their capabilities, and the incentives they have to exaggerate this strength? Could it also be the result of long-term concerns about Shi'a dominance and the inability of this dominant group to credibly commit to any power-sharing arrangement? What we will see is that information and commitment problems help explain why war broke out when it did in Iraq, why it hasn't been resolved in a negotiated political settlement, and why it is unlikely to be resolved in any sort of power-sharing arrangement in the future.

"[...] the war in Iraq began, in part, because of two difficult-to-resolve bargaining problems that tend to afflict new and weak states."

Why Weak States Breed Violence

An interesting pattern emerges if one looks at all civil wars since 1945. Governments that are new or that have experienced significant institutional change are significantly more prone to experience civil war. Between 1945 and 1999, civil wars were five times more likely to break out in the first two years of a state's independence, and 67% more likely to occur if there was instability in any of the previous three years (Fearon and Laitin 2003).[1] At least two explanations have been offered in the

scholarly literature for why this is so. First, governments that experience a rapid change in political authority are more likely to be disorganized and weak, creating the opportunity for disaffected groups to challenge the state. Second, governments that are new or institutionally unstable have greater difficulty policing peripheral territory and repressing dissent, thus making mobilization easier (Tilly 1998, 443; Fearon and Laitin 2003). The weaker the government, the easier it will be for groups to organize, and the more likely they are to launch a successful rebellion. In both cases, it is the *opportunity* to rebel that drives and explains behavior.

On the surface, both of these explanations seem reasonable and persuasive. For a challenge to be made, insurgent groups must be able to avoid government repression, and they must reasonably expect that a rebellion will succeed. Times of upheaval, therefore, create an opening for groups to obtain desired political, economic or social goals. But being able to organize against the state doesn't explain why the government chooses to fight these nascent movements. If a group is able to threaten violence because the government is weak or unstable, why doesn't the government offer a deal that eliminates the possibility of war?

Existing explanations, therefore, cannot fully explain the puzzle of civil wars in new and unstable states such as Iraq. Why haven't the Arab Shi'a leaders been willing to make the concessions necessary to prevent and stop the insurgency in Iraq? This is especially puzzling since violence increased significantly after the elections in December 2005, making an already unstable regime even more vulnerable to collapse. One could argue that al-Maliki and his government are fighting because of U.S. pressure and support, and that in the

absence of such support they would compromise. But almost no one believes that a settlement will be reached once the U.S. withdraws.

Government weakness or instability, therefore, should not cause civil wars to break out, so much as they should cause weak governments to compensate those who have the capacity to rebel. A more satisfying explanation, therefore, needs to explain why Baghdad's leaders seem to prefer war to some type of compromise settlement.

> "Existing explanations [...] cannot fully explain the puzzle of civil wars in new and unstable states such as Iraq. [...] A more satisfying explanation, therefore, needs to explain why Baghdad's leaders seem to prefer war to some type of compromise settlement."

Argument

Why do leaders and their challengers fail to reach agreements that could halt spiraling violence? I argue that war serves two purposes. On the government side, fighting allows incumbent elites to signal strength, thus deterring other competing elites from challenging their power. In the case of Iraq, Prime Minister al-Maliki's government is vulnerable not only to challenges from Sunni insurgents, but from internal rivals and especially Moqtada al-Sadr (who

leads one of the largest voting blocs in Parliament) and Abdul Aziz al-Hakim (who heads Iraq's largest Shi'a party), each of whom controls his own militia.

On the insurgent side, fighting allows rebels to increase their group's bargaining power while the government is weak and unable to commit credibly to a deal. There is little Prime Minister al-Maliki could offer Al-Qaeda Iraq, for example, that could be enforced over time. Thus, one must consider that the main obstacle to peace is not limited state capacity per se, but private information and commitment problems that stand in the way of a settlement.

Information Problems Surrounding Weak States

The first problem facing weak states is an informational one. Whenever a state goes through a period of instability – e.g., as a result of a military coup, rapid leadership change, or radical economic and political reforms – uncertainty exists about how long the incumbent regime is likely to last and whether it will remain cohesive in the face of attack. It is unclear, for example, whether the current government of al-Maliki will be able to hold onto power if it were to crack down aggressively on competing Shi'a militias. It is also unclear just how weak or strong his government is in terms of military and police capacity. Incumbent leaders, therefore, know how strong the government is, but opposition groups do not, and this is the information problem that encourages war.

It is no surprise, therefore, that sectarian violence greatly increased at the same time the new government was established in early 2006. Violence would reveal whether al-Maliki's government had the unconditional support of Moqtada al-Sadr,

Abdul Aziz al-Hakim and the U.S., or was weak and vulnerable to internal collapse. Determining just how much control al-Maliki had over his government and military became critical in deciding if and when to challenge his government.[2]

> "Whenever a state goes through a period of instability [...] uncertainty exists about how long the incumbent regime is likely to last and whether it will remain cohesive in the face of attack."

Ideally, leaders of new and unstable governments should prefer to avoid civil war in order to focus on consolidating their own internal power. Leaders such as al-Maliki, however, face a strategic dilemma. On the one hand, it is in their interest to compromise with powerful opponents if this allows them to avoid the costs and risks of war. On the other hand, making these concessions suggests that the government is weak, encouraging other groups to launch their own attacks. Refusing to settle (and engaging in war), therefore, becomes an important long-term strategy for new regimes to pursue, even if this actually weakens them in the short-term.

Commitment Problems Surrounding Weak States

New and unstable states are also likely to face two types of commitment problems that make war more likely. The first has to do with incumbent elites and their tenuous hold on power. The second has to do with

demographics and the effect this has on treaty enforcement over time.

One of the challenges opposition groups face negotiating with leaders of new and unstable states is that these leaders cannot credibly commit to enforce a settlement over time. That is because incumbent elites such as al-Maliki cannot guarantee that they will be able to hold onto power long enough to execute the terms of an agreement. As long as the Prime Minister's control over his own ethnic group appears shaky, any offer to share power is likely to be meaningless.

Still, the fact that long-term commitments are difficult to make in fragile states does not explain why opposition groups resort to war as a result. For war to be chosen, it must serve some positive purpose for the groups utilizing it. I believe the Sunni insurgency is designed, in part, to increase their relative strength in anticipation of negotiating a better deal with a different, more stable government in the future. Thus, Al Qaeda Iraq is attempting to gain as much influence as possible before Shi'a groups are able consolidate power into a stronger government. The more members a group can recruit and the more land it can capture during the al-Maliki government, the better its bargaining position in the future.[3]

A second commitment problem is likely to arise in countries where a single dominant ethnic group represents a majority or plurality of the population. When a single, large ethnic group attempts to negotiate a power-sharing agreement with smaller ethnic groups it faces a dilemma. How does it credibly commit to share power with smaller factions when it is likely to dominate the electoral process over time? Everyone in Iraq, for example, knows that Shi'as represent about 65% of the population, with the remaining

populace split predominantly between Sunnis and Kurds. The main fear of the Sunnis is that they will not be able to prevent their opponent from eventually establishing an Iranian-style theocracy, even if the Shi'as agree to power sharing in the short term. The Sunni refusal to participate in the 2005 election was driven, in part, by this concern.

This, then, leads to a final line of reasoning. War may be more likely to occur in new and unstable states not because competing groups have no desire to share power or because they cannot work together, but because certain opposition groups understand that there is little they can do to prevent the dominant group from exploiting its demographically privileged position over time. In this case, it is in the interest of this smaller party (or parties) to increase power as soon as possible rather than negotiate a settlement it cannot enforce.

"War may be more likely to occur in new and unstable states [...] because certain opposition groups understand that there is little they can do to prevent the dominant group from exploiting its demographically privileged position over time."

What does this tell us about how the war in Iraq is likely to end? First, it tells us that any attempt to negotiate a settlement to this war will almost certainly fail. It will fail not because a perfectly crafted constitution cannot

be drawn up, or because a federal solution would not be acceptable to the main parties, or because an acceptable division of revenue cannot be reached. It will fail because negotiating a settlement is unacceptably dangerous for all the key players involved. Al-Maliki will not accept such a compromise because his faction must signal toughness to other Sunni and Shi'a factions seeking control of the government. Sunni and Kurd leaders will not accept it because in the absence of U.S. guarantees for their safety, any agreement will leave them vulnerable to exploitation in the future. The only player who would like to see such a settlement emerge is the United States, and it will leave Iraq long before these commitment problems disappear.

Notes

[1] Fearon and Laitin code "instability" as a three or greater change in the Polity IV regime index in any three years prior to the country-year in question.

[2] Post-election violence has revealed that al-Maliki's government is actually quite weak, thus encouraging the formation of additional militarized factions, as well as greater violence.

[3] This is not to say that al Qaeda's game is limited to Iraq, only that these may be the objectives they are pursuing in the Iraqi military theatre.

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Toward More Policy-Relevant Research on Violence in Civil War

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Figure 1. Duration and its Impact on Civil War Severity

Based on data from Lacina (2006)

The events of 9/11 precipitated a flurry of calls for political science to engage the pressing issues confronting the United States and the international community and to produce insights useful for policymakers (Diamond 2002). Partly in response, the American Political Science Association launched a set of high-profile task forces in an effort to translate the work of social scientists into accessible language and to put our best research "at the service of critical issues that have major public policy implications." James Fearon, in his Congressional testimony which leads off this symposium, offers a compelling example of how recent work on the duration and termination of civil wars might inform ongoing debates about the war in Iraq.

But inferring (useful) policy prescriptions from research in political science is not easy. Although scholars often use introductory and concluding chapters to emphasize the importance of their research and its implications for policymakers, such reflections often reveal only limited attention to whether such implications are justified by the evidence or merit recommending to policymakers in light of the tradeoffs they entail or the alternatives that might be pursued.

One might think that, among areas of research in political science, the literature on civil war would be the place to look for policy-relevant findings. Indeed, work on the causes of conflict (Collier and Hoeffler 2004; Fearon and Laitin 2003) often finds its way into the popular press and policy debate (Collier et al. 2003; Mack 2005). In this essay, we focus on an emerging part of this research agenda – why some civil wars are much more costly in terms of human life than others – which has not yet caught the attention of policymakers. How far has social science research on this critical topic moved toward generating insights that might be of value in the hands of activists, government officials, and leaders of international organizations?

What We Know About the Severity of Civil War

Civil wars vary dramatically in their severity. Lacina (2006) estimates the number of persons killed in civil wars taking place between 1946 and 2005 and finds a range that extends as high as two million combat deaths in the Vietnam War, but clusters closer to the threshold (usually 1,000 battle deaths) used to define a civil war. Not surprisingly, the total number of deaths is an increasing function of the duration of the conflict, but there is huge variance among wars of similar length to be explained (Figure 1).

There are at least five complex regularities that have emerged in the
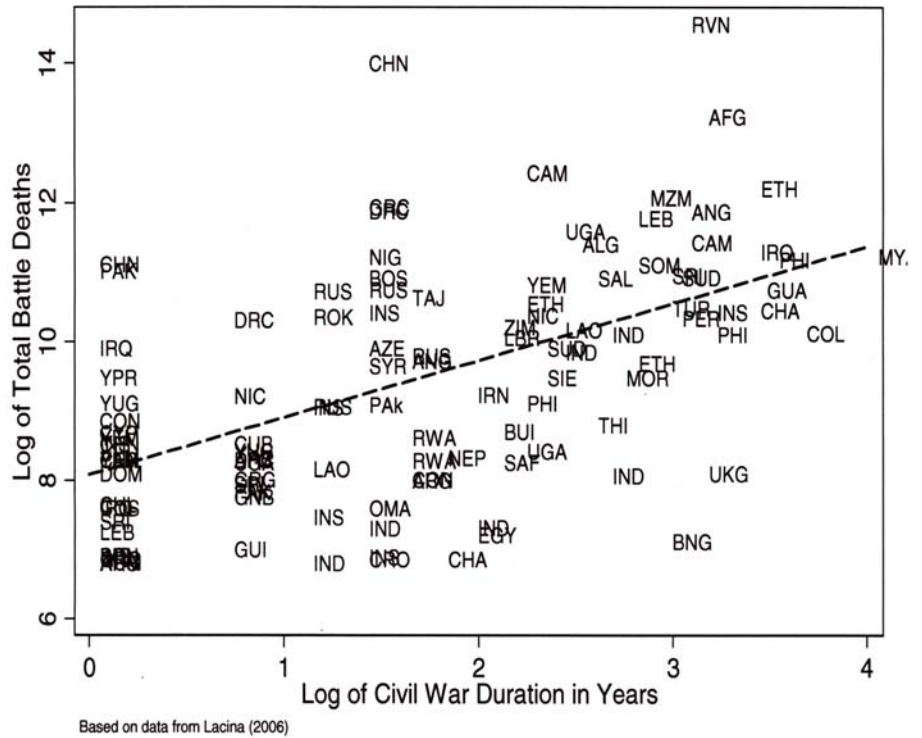
empirical work on the severity of civil conflict. First, in many civil wars the number of people killed by combat is orders of magnitude smaller than the total number of deaths that can be directly attributed to the war (Lacina and Gleditsch 2005; Murray et al. 2002). Epidemiologists have begun to document the true death toll due to civil war in a variety of environments (Burnham et al. 2006; Guha-Sapir et al. 2005; Kassa et al. 2005; Roberts et al. 2004; Roberts et al. 2003; Uganda Ministry of Health and World Health Organization 2005). Such indirect deaths are caused in some cases by acute humanitarian crises – such as massive population displacement or famine – but can also be the product of deterioration of public health over the course of a long conflict. Determinants of non-violent deaths due to war include pre-existing income and health levels; the degree of internal displacement; disruption to infrastructure and public services caused by war; and the extent of access of humanitarian aid workers to the population.

Second, in studies of a variety of different types of violence in civil war, democracies experience predictably different severities of internal conflict. Internal war tends to result in fewer battle deaths in democracies, controlling for income (Carey 2007; Lacina 2006). Democratic governments are also less likely to fight a rural insurgency by practicing collective punishment of civilians (but see: Downes 2006; Valentino et al. 2004; Valentino et al. 2005) or to perpetrate genocide or politicide (Harff 2003; Kiernan 2003; Rummel 1997; Wayman and Tago 2005).

Third, the internal coherence and discipline of rebel forces helps to determine the amount of indiscriminate violence perpetrated (Humphreys and Weinstein 2006). Group coherence and discipline are a function of the

rebels' material constraints, which are in turn determined by the availability of resources from patrons or lucrative black markets. High levels of indiscipline and subsequent indiscriminate violence are associated with rebel groups that can recruit in the manner of a mercenary army (Weinstein 2007). By contrast, factions with more limited means must thoroughly indoctrinate recruits and rely on civilians for a variety of resources. As a result, such groups tend to invest more in maintaining group discipline (Wood 2003).

Fourth, violence during irregular warfare is tied to the struggle of armed groups to control information flowing from civilians to rival groups (Kalyvas 2006). The resultant selective violence against civilians is most intense in those areas where there is neither direct confrontation between the sides nor completely consolidated control of one faction. Factions will also become increasingly indiscriminate if they cannot obtain the intelligence necessary to selectively target civilian informants. Some types of terrain – such as low hills – may be particularly likely to be oft-contested borderlands; case evidence also suggests that, because indiscriminate violence is a product of the inability to extract information, a faction's ideology, organization, and its historical ties to the population might be important causes of such killings.

Fifth and finally, biased military interventions are correlated with more combat deaths (Lacina 2006). However, in contrast to studies of conflict duration (Elbadawi and Sambanis 2002; Regan and Aydin 2006), there has yet to be a study of external intervention that takes into account the possibility that outside powers could be responding to especially severe wars rather than (or in addition to) causing them. An indirect indication that biased interventions

may have pernicious effects is that combat deaths were lower in conflicts that began after the end of the Cold War (Mack 2005).

What We Know vs. What Policymakers Need

Reviewing this literature, it would be reasonable to conclude that we are learning a great deal. But does this research agenda – one motivated in part by a normative concern with the tremendous cost in human life that conflict generates – offer any guidance to decision-makers concerned with the loss of life due to political violence?

"Scholars are identifying important empirical regularities that exist in the conduct of civil war. These insights can prove valuable in policymaking environments, helping decision-makers to see specific cases [...] in light of more systematic patterns."

With only modest aspirations, we can answer confidently in the affirmative. Scholars are identifying important empirical regularities that exist in the conduct of civil war. These insights can prove valuable in policymaking environments, helping decision-makers to see specific cases (which tend to dominate thinking in the short-term) in light of more systematic patterns.

But if our ambition is greater – to use research to empower policymakers with new tools or approaches to mini-

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mize the human toll of violence – the record is less impressive. This probably will not come as a surprise to many. Generating useful policy prescriptions is difficult and is rarely the central concern of academic researchers. Moreover, an enormous gap exists between the independent variables scholars identify as important and the tools policymakers have at their disposal. This gap is not insurmountable, as we will emphasize in the next section, but it cannot be overcome without a significant reshaping of the research agenda on violence.

"Generating useful policy prescriptions is difficult and is rarely the central concern of academic researchers. Moreover, an enormous gap exists between the independent variables scholars identify as important and the tools policymakers have at their disposal."

Consider some of the key findings we summarized earlier. The weight of the evidence suggests that most civilian deaths in wartime are a consequence not of violence, but of excess mortality attributable to natural causes. The critical factor that mediates the impact of political conflict on civilian deaths is the pre-existing infrastructure and wealth of a country, something beyond the control of policymakers in the short (and, perhaps, even long) term. Activists have seized on these findings to make the case for humani-

tarian assistance, but such policy proposals may entail tradeoffs, as humanitarian assistance, which undoubtedly saves lives, has been blamed for contributing to the persistence of some conflicts.

The difficulty of drawing clear inferences about appropriate policy interventions is also apparent in the literature on genocide and mass killing. Democratic governments seem to exhibit restraint in their treatment of non-combatants during war. But "more democracy" is far from a realistic short-term policy recommendation. Nor is it clear that long-term democracy promotion programs are effective and free of pernicious side-effects.

A search for reasonable policy recommendations, then, might lead one to hypothesize that democracy is important because it generates restraint, forcing politicians to consider the likely costs in electoral support of brutal counterinsurgency. Authoritarian regimes cannot be easily made accountable to their own constituents, but efforts have been made to increase the accountability of governments to outsiders: by empowering the International Criminal Court to issue indictments, by imposing economic sanctions, and by impugning the reputation of human rights violators through "naming and shaming." But again, we know little about the likely impacts of these "treatments." They are, at best, a step removed from the independent variable in which we have confidence.

Current research that draws attention to the internal discipline of rebel forces confronts a similar challenge. Yet, while some of the factors that facilitate the emergence of abusive rebel factions are difficult to change – such as weak central governments, the world price of small arms, or an abundance of natural resources – others are more amenable to policy

action. Outside actors can choke the sources of finance that facilitate rebel mobilization: contributions from diasporas, trade in illicit resources, and financing provided by neighboring governments. Regulatory regimes and sanctions appear promising as a response to the problem of abusive rebel factions, but we know little about their odds of success or about the unintended side-effects they may generate.

We thus find ourselves confident in the empirical regularities the literature identifies, but less confident in recommending the policy proposals they imply.

Making Research Relevant for Policy: an Empirical *and* Normative Agenda

Given this gap between what we know and what policymakers need, should we discard the ambition of conducting research that seeks to be relevant to the policy challenges governments confront today? We would not have chosen to invest years in the study of violence if we thought the answer was an unequivocal yes. But we do recognize that making research relevant for policy will require a more focused effort to study the impact of policies per se and the normative problems that policy choices raise.

Many core findings in the study of civil war severity emerge from projects designed to explain variation in dependent variables such as total mortality or civilian abuse. Much less work begins with the treatment itself – an independent variable – in an effort to identify the intended (and unintended) consequences of a particular intervention. This is not surprising. Methods courses in comparative politics increasingly push students toward the identification of meaningful variation to explain. And the empirical

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challenges of identifying the causal effect of a treatment are well known. The interventions discussed above – indictments, sanctions, naming and shaming, regulatory efforts, and so on – are rare events; they are non-randomly assigned; and they take myriad forms, making it difficult to observe the same treatment more than once. Randomized experiments, a popular method for studying the effectiveness of aid interventions, are of limited utility as many of these treatments are applied at the level of the nation-state or the international system as a whole.

Increasingly, though, researchers are focusing on interventions as an object of study and developing creative strategies for measuring their impact. Studies of the impact of third-party security guarantees (Walter 1997), cease-fires (Fortna 2004b), and UN peacekeeping missions (Doyle and Sambanis 2006; Doyle and Sambanis 2000; Fortna 2004a) are contributing useful lessons about how outside actors can facilitate post-conflict transition. Research on combatant demobilization (Blattman 2006; Humphreys and Weinstein 2007) is raising important questions about the utility of strategies that are in every policymaker's post-conflict toolkit. The treatments of interest in these studies were not randomly assigned, but powerful social science methods (e.g. selection models, propensity matching techniques, instrumental variables) are increasingly helping us to approximate an experimental design more closely.

If the literature on civil war severity is to provide more practical insights to policymakers, scholars will need to complement big-picture work with more focused studies of the impact of treatments that governments and activist groups actually employ to minimize the human toll of civil conflict. This is important to measure the

intended consequences of these initiatives and identify ways in which a particular intervention might have positive or negative (unintended) consequences for other outcomes that matter.

Yet even equipped with better knowledge about the efficacy of humanitarian aid, indictments, sanctions, and public pressure, we will still find ourselves unprepared to offer policy prescriptions unless we engage political theorists to help us interpret our findings within a framework of values. For a policy audience, it is not enough to report the causal effect of a treatment (e.g. each additional dollar of humanitarian aid saves ten lives). Policymakers want to know whether providing humanitarian assistance entails risks as well. Will humanitarian aid sustain the war? Will refugee camps provide rebels with safe havens? And if these effects occur, are they acceptable given the benefits of providing humanitarian aid? Empirical work can identify and measure such tradeoffs, exploring the likely impacts of alternative courses over different time frames, but it cannot help us answer a basic, but critical question: how do we weigh the lives we might save against the lives that could be lost, in the short- or the long-term, due to our efforts?

This question is embedded in almost every policy prescription we have discussed so far. Are sanctions, market regulations, and asset freezes unjust if they hurt civilians in a manner disproportionate to the pinch put on leaders? Does the deterrence of future crimes outweigh the potential difficulties associated with ending a conflict once a key player is under indictment? If biased interventions lead to long-term peace, should we accept increased fighting in the short-term? James Fearon's Congressional testimony implicitly argues that the increased likelihood of a settlement

that would result if the U.S. departed Iraq is worth the cost in human lives of the short-term instability that might also ensue. Presumably, he makes this judgment on the basis that the cost in human lives of long-term instability outweighs the cost of an increase in violence in the short-term. Which time horizon should we use to make this calculation? How much should we discount lives in the future?

Generating policy prescriptions requires more than asking the right questions and employing high-tech methods for discerning causal effects. It requires a partnership with political theorists in understanding and grappling with tradeoffs among conflicting values as we make recommendations to policymakers. Tradeoffs such as whether to promote justice if it comes at the cost of peace; whether to aid civilians if doing so indirectly strengthens an oppressive government; and how to weigh the welfare of future generations against the lives we can protect today. Political theorists not only have more familiarity with the normative questions at stake, their research has the potential to point us toward additional empirical inquiries that could inform evaluation of the tradeoffs implied by policy prescriptions.

There is already evidence of what such collaboration can achieve. As the 1990s brought into view horrific instances of ethnic cleansing and genocide, normative theorists quickly turned their attention to the ethical dilemmas inherent in humanitarian intervention (Chatterjee and Scheid 2003). Such interventions have been advocated as consistent with just war theory and with a commitment to the universality of human rights, but these justifications face accusations of paternalism and difficult questions about their de-emphasis of self-determination and the fact that only a small

group of humanitarian tragedies may be singled out for intervention (Walzer 2006). At the same time, philosophers recognized that much theorizing took place "in a state of vincible ignorance" (Holzgrefe and Keohane 2003, 50) as ethical theories of war often rest on undocumented empirical claims. Political theorists understand that normative arguments are insufficient on their own to determine the conditions under which interventions are just. At the same time, resolving factual questions isn't sufficient either as, "ultimately what is involved is a choice of values and goals" (Hoffman 2003)

Isaiah Berlin (1992) highlighted (and struggled with) the fact that at times it simply isn't possible to maximize all of our values simultaneously, nor even to fit them together in a coherent whole. While recognizing that no simple solution to the problem of conflicting values exists, Berlin suggested that our first priority should be "to avoid the extremes of suffering" no matter the long-term costs. But Berlin's position is only the opening salvo in what should be a lively debate. Scholars of comparative politics: be prepared. Evidence, if unaccompanied by the input of our colleagues in political theory, will not be sufficient to answer the question of how policymakers can best reduce the suffering associated with civil war.

Social Science Concepts: A User's Guide.

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Gary Goertz's ambitious and admirable book attempts to redress the imbalance between the central analytic role played by concepts and their peripheral status in methodological discussions. By insisting that concept formation focus on the causal attributes of entities, he is equally critical of scholars, mostly quantitativists, who treat concepts only as measurement instruments, as well as those scholars, mostly qualitativists, who approach concept-formation as an exclusively semantic exercise. Goertz blends abstract reflection and concrete application in a concise and highly accessible volume that should be read by all users of social science concepts – that is, all of us.

The book has three themes. First, it reconceptualizes concepts and concept formation; second it diagnoses and treats the often-uneasy relationship between concept formation and measurement instruments; third, it addresses the relationship between concept formation and case selection. Goertz derives many important lessons from this material. One lesson is that scholars often employ measures whose mathematical structures are inconsistent with the theoretical structure of their concepts. Another lesson presents principles for selecting "negative" cases of relatively infrequent phenomena. Some of these lessons can be appreciated without considering Goertz's basic approach to con-

cepts: these no doubt will make an immediate impact on future scholarship. But other lessons require assessing the validity of Goertz's approach to the fundamentals of concepts and concept formation. Two main subjects comprise those fundamentals: how to think about concepts and how to construct concept structures.

Goertz draws on chemistry in his reflections on concepts and he repeatedly invokes the example of copper and its definition. Copper is defined in terms of its atomic structure, not its redness, because the underlying structure captures its essential characteristics and conveys its causal attributes that inform theoretical propositions. With this lesson in mind, Goertz crafts an approach to concepts that is explicitly causal, ontological, and realist. It is causal because it demands that concepts identify the causally-relevant properties that make them theoretically interesting. It is ontological because it focuses on the fundamental constituent elements of a phenomenon. And it is realist because is segregates superficial qualities from essential attributes that demarcate boundaries between natural kinds. Concept analysis thus involves far more than a semantic definition; it involves "ascertaining the constitutive characteristics of a phenomenon that have central causal powers" (5).

If we think of the references to chemistry as a heuristic device that encourages scholars to think theoretically about concepts, then I heartily endorse this discussion. But we need to quickly recognize the limits of any analogy to chemistry. As John Searle maintains, objects like mountains and copper are observer-independent, but institutions like money and democracy are observer-dependent: they cannot exist without the inter-subjective understandings of intentional agents.

Democracy is not a "thing" in the way that copper is a thing, and their causal properties are not equally predictable as a result. While there is plentiful evidence in favor of the intra-democratic peace hypothesis, it is unlikely that changing a regime from democratic to authoritarian changes its causal behavior with sufficient exceptionless regularity that we can analogize this shift to changing the number of electrons in an atom of copper to create "another element with different causal powers and liabilities" (55).

Consider next the discussion of concept structures. Traditional semantic approaches contain three components: terms (or signifiers), such as democracy or nation-state; the term's intension (or signified or connotation), such as competitive elections and other attributes that would qualify a political regime as a democracy; and its referents (or extension or denotation), the set of countries whose governance attributes qualify them as democracies. Goertz maintains a tripartite scheme but makes two significant modifications to the traditional approach. First, he proposes a rule for enumerating the intension (what he calls the secondary level): the term's intension should contain only causally-relevant definitions referring to the term's ontology. This rule exemplifies Goertz' preference for a more theoretical approach to concept formation. Second, he substitutes an indicator level for the extension. Operationalization, in other words, is an integral part of concept formation, not a discrete and secondary task. Goertz then identifies two main types of concept structure, by which he means the logical principle that links the three levels together and thus renders the concept coherent. Classic structures posit a set of necessary and sufficient conditions: the multiple elements of the secondary level are individually necessary and jointly suf-

ficient to constitute the basic term. The basic term is present if and only if n of n components are present. Family resemblance structures, alternatively, contain no necessary component and some subset of the set of possible components – m of n components – will be sufficient to constitute the basic term. These two structures lie on a continuum defined by the level of substitutability. On one end are necessary conditions for which no substitution exists; on the other end are sufficient conditions whose absence can be compensated for by other elements.

"[Goertz's] discussion of concept structure yields genuine value."

The discussion of concept structure yields genuine value. First and foremost is Goertz's insistence that scholars make explicit the structure of their concepts. He evinces impatience for formless concept structures that force readers to discover the structure through "textual exegesis, or at worst guesswork" (36). Chapter two is full of interesting guidelines for defining a concept's structure, including instructions on how to graphically illustrate concept structures. I suspect that future work will be full of "Goertzian concept diagrams" which will encourage scholars to formulate better concepts and communicate their content more effectively. A second payoff comes from his clear demonstration that the relationship between intension and extension is a function of the type of concept structure. In classic concept formation, an increase in intension implies a decrease in extension, so that more precise definitions containing more attributes refer to fewer cases. Violations of this rule result in what Giovanni Sartori called

"concept stretching." In family resemblance structures, on the other hand, increases in intension simply include more substitutable elements and hence increase extension. Substantively rich concepts therefore apply to more cases, not fewer cases.

"I suspect that future work will be full of 'Goertzian concept diagrams' which will encourage scholars to formulate better concepts and communicate their content more effectively."

Still, as an unrepentant devotee of the set-theoretic approach to concept formation, I see some possible problems with family-resemblance concept structures. Ludwig Wittgenstein advanced the idea of family resemblance, after all, to attack the idea of essential defining elements, and so family resemblances exist in deep philosophical tension with classic concept structures which are motivated by appeals to necessary – or essential – elements of concepts. That scholars have long been enamored of essential defining elements also explains why there is a parallel literature on "essentially contested concepts." To note, as Goertz does, that laypeople regularly categorize with family-resemblance structures does not, in my opinion, grant them regulatory authority.

I remain unconvinced as well by Goertz's main example of a family resemblance concept structure – Alexander Hicks' definition of a welfare state. Hicks argues that no single service defines the welfare state.

Welfare states exist if they supply *m of n* from among a range of possible services, such as unemployment insurance and healthcare. Goertz refers to this set of substitutable elements as the secondary-level dimension (on pages 38 and 60) but also as the operational definition (page 38). This conflation of two levels of the concept structure is disturbing: we should be better able to distinguish causal properties from instrumental measures. But it is also quite possible to subsume Hick's definition into the necessary and sufficient framework. The term welfare state refers to those states that (1) enact public policies that (2) decommodify the core elements of (3) the prevailing social understanding of what constitutes a just and dignified standard of living. Each of these three elements must be present to permit us to speak of a welfare state, and we operationally define welfare states via a range of (substitutable) policies, measured by data such as expenditures.

Goertz makes two other interesting claims about the relationship between the three levels of his concept structure. First, he argue that indicator-level variables *cause* secondary level variables. In this way, Goertz takes the unusual step of building causal hypotheses directly into concepts. He gives as an example Theda Skocpol's theory of social revolutions (see Figure 2.7 on page 64), claiming that the indicator-level elements dominant-class leverage, agrarian backwardness, and international pressure cause state breakdown (which is in turn one of the secondary-level elements of the basic term, social revolution). I disagree with this conceptual analysis. If you asked me to enumerate the indicators of state breakdown, I would propose a list that included the dissolution of military hierarchies, the crippling loss of authority as agents appropriated the resources of principals, and the subsequent pre-

cipitous decline in the state provision of goods and services. Dominant-class leverage and international pressure, on the other hand, are better understood as distinct concepts embedded in distinct variables in an extended causal chain. It might be fruitful to debate Goertz's basic claim, but first we need some criteria to better distinguish the boundaries within concept structures from the boundaries between distinct concepts. Goertz's second claim about the relationship between levels of a concept structure is that the secondary-level constitutes the basic level. I am unsure about either the validity or the value of this claim. The basic level, after all, is just a term, one that plays very little substantive role in his account. It might be better to think of the secondary level as regulating linguistic usage: observers of the properties of the secondary level are licensed to speak using particular vocabularies. Or, better yet, given the endless debates that normative-laded basic terms provoke, perhaps we should just prohibit basic terms and confine debate to the all-important secondary level.

These comments barely scratch the surface of this rich book. Political philosophers, quantitative scholars, and devotees of qualitative methods can all read it, benefit from it, and argue with it. The discipline will be better off for those discussions. I can think of no higher praise for this very impressive book.

Author's Response

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David Waldner has done an excellent job of summarizing the core elements of my approach to concepts and I have little to add to his summary.

He raises the issue of the reliance of social science concepts on inter-subjective understandings and states that "they cannot exist without the inter-subjective understandings of intentional agents." At some philosophical level I would not disagree with this, but my book was intended to be a *practical* guide. With very few exceptions social scientists who are interested in using concepts in causal explanations (my target audience) treat social things as things. This is the realist part of my approach. I can easily imagine settings where the understandings of intentional agents might be crucial, for example, when the concept of democracy is new and a lot depends on what its creators think it is.

Waldner classifies himself as an "unrepentant devotee" of the set-theoretic approach to concept formation. I would be happy to include myself in that set as well since in many ways the book is motivated and informed by fuzzy sets (Ragin 2000). Jim Mahoney and I have explored at some length the differences between "qualitative" and "quantitative" research (Mahoney and Goertz 2006). One sign of the depth of the divide is that much qualitative work relies explicitly or implicitly on set theoretic notions. Math boot camps are a

booming phenomena, but I know of none that teaches logic and set theory. The family resemblance structure falls within fuzzy set theory. What I try to do is make some links from this unfamiliar logic to the kinds of mathematics familiar to everyone, e.g., maximum, minimum, sum, etc.

I like the way Waldner reformulates the concept of the welfare state (my standard example of the family resemblance concept) in terms of necessary and sufficient conditions. The book's goal is to provide tools for constructing concepts but I do not generally have strong preferences *a priori* between concept structures. Actually, even more important as a motivation for using the welfare state example is that typically, in quantitative analyses at least, the dependent variable is measured in terms of total expenditures on various social services. Money is a perfect example of a substitutable, fungible measure.

> "The book's goal is to provide tools for constructing concepts but I do not generally have strong preferences *a priori* between concept structures."

While Waldner is correct that the book stresses the importance of causal issues in concept development, not all concepts work that way. In particular, dependent variables – and often concepts used in case selection – do not usually involve causal factors. To include them would in general – some exceptions can arise – not be wise because we would have causal factors within the dependent variable along with causal factors on the independent variable

side of the equation. Skocpol's concept of social revolution is somewhat problematic for exactly this reason when she requires class conflicts to have a role: "And these changes [social revolutions] occur through intense sociopolitical conflicts in which class struggles play a key role" (Skocpol 1979, 5). This could be interpreted as saying that class struggles are part of the cause of social revolutions. This concept of social revolutions could work however if it is interpreted more like a scope condition.

Skocpol's *States and Social Revolution* is an example that appears in a number of chapters. Waldner is correct that the concept framework of secondary-level and indicator-level is a bit misleading in this example. The term "indicators" here is misleading since, as he notes, it is really another level of a causal chain.

Finally, Waldner correctly notes that in general the basic level does not play that big a role, though quite a few of the guidelines in chapter 2 focus on it. One must remember that the book is mostly about constructing concepts. However, these concepts – the basic level – play a central role in theories and research design. As independent, dependent, case selection or scope variables we use concepts at the basic level. While Waldner suggests we might be better off confining debate to secondary-level factors, I think he knows that this is unlikely to happen.

What is Civil War and How Should We Measure It?

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Many studies of civil war use statistics to explain when, where, and why civil wars occur, how long they last, or how they end. These studies rely on quantitative measures of civil war. But it makes a difference how civil war is measured.[1]

Civil wars are usually defined as armed conflicts between the government of a sovereign state and domestic political groups mounting effective resistance in relatively continuous fighting that causes a high number of deaths. This broad definition cannot easily distinguish civil war from other forms of political violence, so we resort to *ad hoc* coding criteria, such as the death threshold: some code a civil war when 1,000 people are killed overall;[2] others require 1,000 battle deaths per year.[3]

These arbitrary coding rules allow quantification, but may create conceptual confusion about what civil war is and what causes it.

Three sources of coding uncertainty should be singled out: start- and end-dates of civil wars; distinguishing civil war from other forms of political violence; and estimating the magnitude of violence.

Consider the magnitude problem first. It is always hard to dig up deaths data from historical sources. But the problem is even harder because governments lie about how many of their citizens they kill; and reporters often avoid the worst places during the

worst violence.[4] Thus, for example, Ball (2005) finds severe under-reporting of violence in Guatemala and that has consequences for analyses of state terror.[5] Even with good reporting, it can be difficult to get reliable estimates of deaths. A well-known mortality study in the ongoing civil war in Iraq estimated 601,027 violent deaths (confidence interval 426,369-793,663) in the post-invasion period.[6] In an unpublished paper, a team of researchers from Oxford argue that the statistical methods used in that study over-estimate deaths.[7]

"[...] arbitrary coding rules allow quantification, but may create conceptual confusion about what civil war is and what causes it."

This "magnitude" problem (call it "reporting bias") is sometimes systematically related to characteristics of the country. Drakos and Gofas show that bias in deaths reporting in terrorism datasets is related to regime type and they provide an estimate of the impact of reporting bias on inferences about the causes of terrorism.[8]

Reporting bias is likely to affect studies of violence levels more than studies of war onset/termination, where researchers typically want to know if deaths crossed the "civil war" threshold. A common problem in such studies is how to date the war.

It is often hard to tell when "civil war" starts or ends, especially when other types of political violence accompany civil war. Is a coup that kills some soldiers followed by state-sponsored pogroms a civil war? Or must killing be reciprocal throughout the duration

of the war (and who has such data)? Does a long insurgency that causes a hundred deaths per year and suddenly spikes into thousands of deaths become a civil war at the spike, or was it always a civil war?

Coding an end to civil wars presents similar difficulties. If a "civil war" turns into low-level insurgency (as in the conflict between Indonesia and the Free Papua Movement) when does "civil war" start and "insurgency" begin? Is this a distinction that we really care about? What if violence continues after the civil war between political parties or militias, but not the government? In South Africa, for example, the violence of the 1980s and early 1990s, which some datasets code as a civil war, was followed by inter-communal violence mainly between supporters of the ANC and the Inkatha Freedom Party in KwaZulu-Natal that killed more than 2,000 people from 1997-2000.[9] When did South Africa's civil war end?

Some say "civil war" ends when fewer than 1,000 deaths occur in one year.[10] In Burundi since 1990, for example, Collier, Hoeffler, and Rohner (2007) code civil wars from 1991-92, 1993-98, and 2000-2002, with temporary "ends" due to this coding rule. What about other criteria, such as coding an end if the rebels take over? If rebels win, but violence picks up again within a few months, most datasets would code one war, not two. But this turns a blind eye to failed victories. Without coding an end on every occasion when there is a war-related regime change, the data will lead us to find a strong association between victory and peace (since only victories that last are coded).

What if a cease-fire or peace treaty is signed? Do all the parties have to sign? Are cease-fires endpoints that are as good as treaties that outline a

political solution? Cease-fires are much more likely to be under-reported since they are more informal and much more frequent. In Burundi, several small groups signed cease-fire agreements with the government at several points from 2000 to 2002, but fighting between the government and the FNL lasted until a September 2006 cease-fire with deaths in the last three years surpassing 500 (Uppsala Conflict Database).[11] According to the 1,000 deaths/year criterion, or a coding rule that does not require all groups to sign cease-fires, that war would be coded as ending in 2002. Other criteria might support coding the war as ongoing until the end of 2006.

The uncertainty surrounding start and end dates is exacerbated when researchers measure war duration in days rather than years.[12] Coding war duration or peace duration in days helps minimize the problem of having too many "ties" (i.e. cases with the same event duration) since it is more likely to have twenty cases with peace duration of one year rather than one day. But when is a cease-fire broken and the civil war back on? At the first shot, or when a certain number of deaths occur in a single day (most likely the 1,000 deaths threshold cannot apply to days as easily as it does to years).

So, one important question is whether we should code one "big" period of civil war or several "episodes." There is disagreement on this question. Some scholars code countries as being at war for decades, although it is hard to find evidence that violence levels or effective resistance criteria are satisfied for the whole period.[13] Others separate periods of war, marked by peace settlements or military victories.[14] But the country may not be at peace in the intervening periods. If what we care about most is the causal effect of certain variables

(like democracy or income) on civil war onset, duration, or termination, then such coding rule differences will have substantive implications. Since coding rules are likely to continue to differ across datasets, we should at least have an appreciation of what authors of each dataset mean by "civil war" onset or termination so we can put their empirical results in perspective.[15]

Advances in the study of civil war are likely to come from taking these issues seriously and from taking the data in different directions. More and more, scholars are looking away from country-years, and toward administrative subdivisions of countries;[16] different civil war typologies;[17] geo-coded locations of violent events;[18] and micro-level surveys that explore why people join rebellions.[19] These new datasets promise to allow greater specificity about the measures of civil war.

For now, an important point to keep in mind is that most civil war datasets are works-in-progress rather than definitive measures of "civil war." This is not just because good data are hard to come by, but also because, even if there is agreement on some "core" cases of civil war, there is disagreement on some other cases at the margins that can inform (or transform) how we understand civil war. It is worth considering if what we now call "civil war" is a coherent category of violence that is causally distinct from other forms of political violence and if it should be analyzed in isolation, or if our focus should shift away from "civil war" and toward political violence more generally.

Notes

[1] Nicholas Sambanis, 2004, "What is Civil War? Conceptual and Empirical Complexities of an Operational Definition," *Journal of Conflict Resolution* 48 (6): 814–58.

[2] James D. Fearon and David D. Laitin, 2003, "Ethnicity, Insurgency, and Civil War," *American Political Science Review* 97 (1): 75-90.

[3] Nils Petter Gleditsch, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand, 2002, "Armed Conflict 1946-2001: A New Dataset," *Journal of Peace Research* 39 (5): 615–37.

[4] Reporting biases are common in studies of state repression. An insightful review is Christian Davenport, 2007, "State Repression and Political Order," *Annual Review of Political Science* 10: 1-23.

[5] Patrick Ball, 2005, "On the Quantification of Horror: Field Notes on Statistical Analysis of Human Rights Violations," in *Repression and Mobilization,* edited by Christian Davenport, Hank Johnston, and Carol Mueller (Minneapolis: University of Minnesota Press); and Christian Davenport and Patrick Ball, 2002, "Views to a Kill: Exploring the Implications of Source Selection in the Case of Guatemalan State Terror, 1977-1996," *Journal of Conflict Resolution* 46 (3): 427-450.

[6] Gilbert Burnham, Riyadh Lafta, Shannon Doocy, Les Roberts, 2006, "Mortality After the 2003 Invasion of Iraq: A Cross-Sectional Cluster Sample Survey," *Lancet* 368: 1421-1428.

[7] Neil F. Johnson, Michael Spagat, Sean Gourley, Jukka-Pekka Onnela, Gesine Reinert, 2006, "Bias in Epidemiological Studies of Conflict,"

Households in Conflict Network, Research Design Note 2.

[8] Konstantinos Drakos and Andreas Gofas, 2006, "The Devil You Know but Are Afraid to Face: Underreporting Bias and Its Distorting Effects on the Study of Terrorism," *Journal of Conflict Resolution* 50 (5) : 714-735.

[9] The violence in the 1980s and early 1990s is coded as a civil war in some datasets, but not others. It is coded in Fearon and Laitin (2003) and Sambanis (2004), but not in J. David Singer and Melvin Small, 1994, "Correlates of War Project: International and Civil War Data, 1816-1992" [Computer file (April)] (Study #9905), Ann Arbor, MI: Inter-University Consortium for Political and Social Research [distributor]; or Meredith Reid Sarkees, 2000, "The Correlates of War Data on War: An Update to 1997," *Conflict Management and Peace Science* 18 (1): 123-144.

[10] Gleditsch et al (2002); Paul, Collier, Anke Hoeffler, and Dominic Rohner, 2007, "Beyond Greed and Grievance: Feasibility and Civil War," Working Paper, University of Oxford and University of Cambridge (February).

[11] Uppsala Conflict Data Project (UCDP). http://www.pcr.uu.se/research/UCDP/.

[12] See, for example, Page Fortna, 2004, "Does Peacekeeping Keep Peace? International Intervention and the Duration of Peace After Civil War" *International Studies Quarterly* 48 (2): 269-292.

[13] Thus, for example, Fearon and Laitin (2003) code one continuous civil war in Myanmar (Burma) from 1948 until the end of their analysis time in 1999; and in the United Kingdom (Northern Ireland) from 1969 to 1999.

Datasets

[14] Sambanis (2004), for example, codes three "episodes" of civil war in Angola since 1975, not including the war in the Cabinda enclave, which he codes as separate from the war between UNITA and the government.

[15] Supporting documentation in datasets would help readers figure out for themselves which datasets are more reliable and more in line with their own concept of civil war. Two datasets offer such information: the Gleditsch et al (2002) dataset (regularly updated) and Sambanis (2004), which includes civil wars from 1945 to 1999.

[16] Nicholas Sambanis and Branko Milanovic, 2007, "Explaining the Demand for Sovereignty," unpublished manuscript, Yale University and World Bank.

[17] Nicholas Sambanis, 2006, "What is an 'Ethnic' War? Organization and Interests in Insurgency," unpublished manuscript, Yale University.

[18] Clionadh Raleigh and Håvard Hegre, 2006, "Introducing ACLED: An Armed Conflict Location and Event Dataset," unpublished paper, PRIO. Lars-Erik Cederman, Jan Ketil Rød, and Nils Weidmann, 2006, "Geo-referencing of Ethnic Groups: Creating a New Dataset," unpublished manuscript, International Conflict Research, ETH Zurich; and Halvard Buhaug, Lars-Erik Cederman, Jan Ketil Rød, 2007, Modeling Ethnic Conflict in Center-Periphery Dyads, unpublished manuscript, International Conflict Research, ETH Zurich.

[19] Ana Arjona and Stathis Kalyvas, 2006, "Survey of Demobilized Combatants in Colombia," unpublished paper, Yale University.

RNGS Dataset on State Feminism

The Research Network on Gender Politics and the State (RNGS) announces the release of its dataset. The result of a ten-year study, the RNGS dataset contains quantitative and qualitative data about 130 policy debates/observations in 13 countries coded on 28 concepts and over 110 variables. It provides extensive information on women's movements, feminism and gender politics, women's policy offices, policy making processes and policy debates that occurred between the late 1960s and the early 2000s in 13 post-industrial democracies (Austria, Belgium, Canada, Finland, France, Germany, Great Britain, Ireland, Italy, Netherlands, Spain, Sweden and the US).

The RNGS team of international scholars designed and carried out this large-scale comparative research project to examine whether, how, and why women's policy offices, through their relations with women's movements, make post-industrial democracies more democratic and the state more feminist. The network's study encompasses the momentous years of women's movements from the emergence of autonomous protests in Europe and North America for the liberation of women in the 1960s and 1970s through the successful integration of movement activists into conventional politics in the 1990s and 2000s. The unit of analysis in the study and the dataset is a policy debate that takes place in a political/government arena and ends with an official decision or non-decision.

The dataset suite includes a PDF codebook/users guide and two data files – one of the numerically based data set, in SPSS, and a second file

with text appendices of supplemental descriptive information for 22 variables in PDF. After October 1, 2007 the RNGS dataset will become part of the ICPSR data archive and also available to download from the RNGS website – http:/libarts.wsu.edu/polisci/rngs. The website also has documents that provide detail on all aspects of the project.

For more information contact, Amy G. Mazur (Washington State University) mazur@wsu.edu.

Editors' Notes

The editors welcome suggestions of other relatively new and potentially useful datasets that should be announced or reviewed in *APSA-CP*. Anyone interested in reviewing a dataset for the newsletter should contact Michael Coppedge at coppedge.1@nd.edu.

We invite our readers to request hard copies of back issues (beginning with the winter 2003 newsletter issue) at the cost of $1.50 per issue. They should send their request(s) by email to kschuenk@nd.edu.

Comparative Politics Section Prize Winners, 2007 APSA Meeting

Gregory Luebbert Award for the Best Book in Comparative Politics

Jonathan Rodden, MIT
Hamilton's Paradox: The Promise and Perils of Fiscal Federalism,
Cambridge University Press, 2006

Gregory Luebbert Award for the Best Article in Comparative Politics

Macartan Humphreys, Columbia University and Jeremy Weinstein, Stanford University
"Handling and Manhandling Civilians in Civil War,"
APSR 99, 1: 61-74.

and

Torven Iversen, Harvard University and David Soskice, Duke University

"Electoral Institutions and the Politics of Coalitions: Why Some Democracies Distribute More than Others."
APSR 100, 2: 154-81.

Dataset Award

Ronald A. Francisco, University of Kansas

For his events dataset on European protest and coercion

Sage Publication Award for the Best Paper in Comparative Politics at the 2006 APSA meeting

Lily L. Tsai, MIT

"Informal Institutions, Accountability,
and Public Goods Provision in Rural China."

Britain after Blair: The Legacy and the Future

A Conference Sponsored by the British Politics Group of APSA Wednesday, August 29, 2007, The Gleacher Center, University of Chicago Business School, Chicago, IL

Much like Margaret Thatcher before him, Tony Blair has dominated the British political scene for nearly a decade. What has been the legacy of the Blair government, both in terms of politics and public policy? What are the likely directions for the future, either under the leadership of Gordon Brown or beyond? The British Politics Group is organizing a special one-day conference to explore these issues <http://www.uc.edu/bpg/panels.htm> in addition to our regular APSA panels. Panel topics included in the program include:

* Blair's Constitutional Revolution
* Anglo-American Relations and the Bush-Blair Axis
* The EU in British Politics and Policy-Making
* Democracy post-Blair: Parties or Groups?
* The Ideological Legacy of Tony Blair
* The Vision and Reality of Devolution
* Party Leaders after Blair
* The State of the Parties After Blair
* New Labour and Public Policy
* UK Foreign Policy and the Iraq War

The conference is being held on the Wednesday prior to APSA and convened at the Gleacher Center of the

University of Chicago Business School. Located on the Chicago River, the Gleacher Center is a very short walk from both of the main APSA conference hotels. Registration for the conference is $30 and includes a one-year membership in the BPG.

Registration forms and further information can be found on the conference website (http://www.rose-hulman.edu/~casey1/BAB.htm). A preliminary program will be posted in early June. If you have any questions or need any further information, please contact Terrence Casey Executive Director, BPG.
(casey1@rose-hulman.edu)

Erratum

In the Winter 2007 Issue of this newsletter, the Luebbert Article award committee should have been listed as:

Robert Rohrschneider, Indiana University, chair
(rrorsch@indiana.edu),
Mitchell Seligson, Vanderbilt University, chair (m.seligson@vanderbilt.edu),
Devra Moehler, Cornell University
(dcm37@cornell.edu).

Call for Papers: Workshop "The Numbers We Use, the World We See Evaluating Cross-National Datasets in Comparative Politics"

This workshop will be part of the 2008 Joint Sessions of Workshops of the European Consortium for Political Research (ECPR), the main European organization of (comparative) political scientists, which will be hosted in Rennes, France. Unlike panel conferences such as APSA and IPSA, participants at this conference attend only one workshop but for the full period of 11-16 April 2006. This unique setting provides for the most stimulating academic environment in which everyone discusses each other's work.

The prime aim of this workshop is to evaluate critically, as well as constructively, some of the major cross-national datasets that are used in the field of comparative political science. Authors shall assess the strengths and weaknesses of these datasets – their conceptual foundations, measurement decisions, documentary bases, technical procedures, and publication practices. In addition, they shall discuss the empirical and theoretical implications these strengths and limitations carry for the study of politics. How do the numbers we use affect the world we see? Some of these datasets have been genuinely constitutive of their field of research. Their critical assessment shall provide vital insights to both younger scholars, who are still looking for which datasets to use in their work,

and more seasoned colleagues, who might not have full oversight of the qualities and implications of the datasets available in the field.

The workshop is aimed at discussing a wide range of datasets that are used frequently in the field, including those used for primary and secondary analysis. Among the datasets we are interested in are cross-national surveys of mass attitudes (like the Eurobarometer, European Social Survey or the World Values Studies), collaborative research projects (like the ECPR Manifesto Project or the Freedom House dataset), so-called expert studies (such as those organized by Michael Laver and his collaborators or the Euroskepticism dataset from UNC), cross-national collections of "objective" data (like the OECD or UN datasets), and combined meta datasets (such as that of Transparency International).

We are looking for neither strong protagonists not vehement critics of these datasets. The project can only be successful if the contributors are critical but constructive in their attitude towards the dataset they assess. The key aim is not to reject the use of cross-national datasets, but to systematically examine their strengths and weaknesses as well as, most importantly, the substantive implications these strengths and weaknesses carry for comparative empirical research. Datasets are not an end in themselves. By improving them we aspire to improve the comparative research based on them. Proposals for papers should be submitted to both conveners: Cas Mudde (cas.mudde@ua.ac.be) and Andreas Schedler (andreas.schedler@cide.edu). For more information on the 2008 Joint Sessions of Workshops, or on this workshop, see http://www.essex.ac.uk/ecpr/events/jointsessions/rennes/index.aspx.

2008-2009 APSA Call for Papers and Panels

Ben Ross Schneider, Northwestern University (brs@northwestern.edu), chair; Steven Wilkinson, University of Chicago (swilkinson@uchicago.edu), chair

The Comparative Politics section, using this year's APSA theme of Categories and the Politics of Global Inequalities as our inspiration, especially invites panel and paper proposals that reexamine the analytic categories we use in comparative politics. Proposals, for instance, might wish to examine how categories such as class, ethnicity, race, developing world, institution, globalization, or corruption were formed, whether these categories are valid, and also how the use of these categories shapes our interpretations of outcomes (and categories) such as conflict, economic development, democratic consolidation, party competition, or economic, ethnic, and gender inequalities. In addition, inequality has long been a central concern in comparative politics, both across countries and within. We welcome proposals that interrogate conventional wisdom on inequality, as well as the impact of global economic integration and democratization on it. Because APSA meetings provide rare opportunities for debate among scholars working on different regions, we welcome cross-regional panels on topics such as immigration, rentier and failed states, party and electoral systems, and varieties of capitalism, as well as the categories and inequalities mentioned above.In addition to these thematic panels, we also as usual encourage panels from the full range of diversity of areas, topics, and theoretical and methodological perspectives that together constitute comparative politics.

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