

apsa-cp

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Comparative Politics of the
American Political Science Association

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Letter from the President

Bridging the Quantitative/Non-Quantitative Divide

Michael Wallerstein

Northwestern University
mwall@merle.acns.nwu.edu

The ability to read and converse in a language other than English is a fundamental component of most comparativists' expertise. Linguistic diversity does not interfere with our ability to communicate with each other, however, since we all share a common language that we use in our papers and seminars. A scholar may have spent years mastering Russian but, when he or she gives a talk, we expect the talk to be in English. So why are comparativists who have spent years mastering formal theory or statistical methods exempt from the general requirement to present their work in the common language? The *APSR* will not accept articles written in Russian. Why, many ask, does the *APSR* publish articles written in the language of mathematics, a language that is equally foreign to those without specialized training?

The answer that mathematically trained scholars give is that too much precision is lost in translating mathematics into English. While there are phrases in any foreign language that are impossible to translate precisely into English, mathematics is different. The content of social science, as opposed to the content of literature, should not depend on the (non-mathematical) language that is used to present the results. But, it is argued; essential content is lost in translating social science from mathematics into English.

Not surprisingly, the view of the technically trained that everyone should learn enough formal theory and statistics to follow

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Section Officers

President

Michael Wallerstein
Northwestern University
mwaller@merle.acs.nwu.edu

Vice-President, President-Elect

Evelyne Huber
University of North Carolina
ehuber@unc.edu

Secretary-Treasurer

Atul Kohli
Princeton University
kohli@wss.princeton.edu

2001 APSA Program Coordinator

Barbara Geddes
University of California, Los Angeles
geddes@ucla.edu

At-Large Committee Members

Nancy Bermeo
Princeton University
bermeo@ariel.princeton.edu

Kathryn Firmin-Sellers
Indiana University
kfirmin@indiana.edu

Robert Jackman
University of California, Davis
rwjackman@ucdavis.edu

Susan Pharr
Harvard University
susan_pharr@harvard.edu

quantitative work in the field is not well received by those without technical training. The problem is that life is finite and the number of things that could be studied is infinite. It is easy to say that everyone in political science should learn formal theory and statistics. It is difficult to say how students should find the time to master social choice, game theory, statistics, the general literature in comparative politics, the specific literature pertaining to one's area of specialization and a foreign language, all in two or three years of course work. There are always tradeoffs. More time spent learning formal theory and statistics means less time spent learning the history and culture of other societies.

It would help reduce the conflicts in comparative politics if partisans on both sides of the quantitative/non-quantitative divide could accept the validity of the arguments of their opponents. Consider, first, the case of empirical work. All empirical work consists of two steps. The first step is to gather the evidence or data. The second step is to apply the logic of scientific inference to identify the conclusions that can be drawn. A caricature of the non-quantitative social scientist is one who invests all of his resources in an effort to understand one particular case in all of its complexity, only to confront the logical impossibility of concluding anything about cause and effect from a single case. An opposing caricature of the quantitative social scientist is one who spends all her time performing intricate statistical tests using data that contain little information about anything we care about.

Caricatures aside, both sides have a point. On the one hand, quantitative researchers are correct to argue that all of the logical problems of inference that spur the development of statistical theory are equally present in non-quantitative work. In addition to the obvious problem of small sample size, the problems of identification and selection bias are, if anything, worse in non-quantitative work than in quantitative work since non-quantitative scholars are often not trained to recognize such errors in reasoning. In addition, learning to think in probabilistic terms is a great benefit in studying social phenomena, given the severe limits of what can be known with certainty. On the other hand, non-quantitative researchers are correct to say that the payoff from collecting better information often exceeds what can be gained from finding new ways to torture existing data. Moreover, by paying close attention to what people say in an open-ended setting, non-quantitative researchers may obtain insights into the causal mechanisms at work that exceed what can be learned from the study of quantitative measures of people's behavior (or people's responses to surveys).

The conflict between quantitative and non-quantitative empirical scholars is muted in comparison to the conflict between formal (i.e. mathematical) and verbal theory. Statistical work is widely accepted in comparative politics in the way that formal theory is not (yet). One reason for the greater hostility towards formal theory in comparative politics is that it is newer and, therefore, less familiar. Another reason, I think, is that the introduction of formal theory

(Continued on page 23)

News & Notes

Announcement of Nominations for New Officers for 2001-02

The nominations committee of the Comparative Politics Section has nominated Peter Hall, Harvard University, for the position of Vice-President and President-Elect of the Comparative Politics Section. Peter Hall would replace Evelyne Huber, who is leaving the position of vice-president to replace Michael Wallerstein as President of the section. In addition, the nominations committee has nominated Melanie Manion, University of Wisconsin at Madison, and Ian Lustick, University of Pennsylvania, to serve as at-large members of the Executive Committee. Melanie Manion and Ian Lustick would replace Kathryn Firmin-Sellers and Susan Pharr, whose terms are coming to an end. All positions are for two years. The nominations committee consisted of Francis Hagopian (chair), Jean Oi, Jonas Pontusson, Kaare Strom, and Crawford Young.

Any five members of the Comparative Politics Section may nominate alternatives, either at the business meeting of the Comparative Politics Section at the Annual Meetings of APSA or by petition sent to the president prior to the annual meetings. If additional nominations are received for any of the open positions, the position will be filled by secret ballot at the business meeting.

Call for Bids to Edit Newsletter

The newsletter needs a new editor. All who are interested in becoming the next editor of the newsletter of the Comparative Politics Section of the APSA are encouraged to submit a bid. At the last meeting of the executive committee, the following guidelines were adopted.

The editor of the newsletter will henceforth be a four-year term. The next four-year term will begin in the fall of 2002. The deadline for submitting a bid is Dec. 31, 2001. Bids should be sent to, Evelyne Huber, (University of North Carolina, Department of Political Science, Chapel Hill, NC 27599-3265, email: ehuber@unc.edu).

A three-person committee, to be appointed by the president of the Section, will select the winning bid. The selection committee's decision will be announced by April 1, 2002. While it is desirable for the Newsletter to locate in different universities, the incumbent editor or editorial team may submit a bid to continue to edit the newsletter for a second term.

The selection committee will use the following criteria to evaluate the bids:

- a. Bidding institutions should have a comparative politics faculty sufficiently large to support an editor, an associate editor, and have a pool of possible replacements. Responsibilities of the editorial team include identifying and developing themes, contacting potential contributors, selecting and editing submissions, and overall

Newsletter Staff

Editor

Daniel Treisman
University of California, Los Angeles
treisman@polisci.ucla.edu

Assistant Editor

Elizabeth Stein
University of California, Los Angeles
eastein@ucla.edu

Regional Editors -at-Large

Middle East

Leonard Binder
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oversight of the production and mailing process. The editor and associate editor must be able to commit an estimated working time of 2-3 weeks per issue, spread out over a longer period of time.

- b. Bidding institutions should have a pool from which to choose an assistant editor. Estimated time spent by the assistant editor is four weeks per issue. The assistant editor is expected to handle layouts, convert email submissions, arrange for printing and production, and manage a web site. Compensation for this position comes from the bidding institution.
- c. The bidding institution should provide office space, computer equipment, copying and phone support. Released time for faculty will also be taken into account but is not a requirement. Proposals should include a prospective budget and a statement of administrative support.
- d. Themes, directions, special topics and other ideas of the bidding editors will be taken into account.
- e. Section dues will pay for production and mailing expenses. The bidding university should be able to cover other expenses listed in (c).

Comparative Politics Section Awards

The winner of the 2001 Sage Award for Best Paper in Comparative Politics presented at the 2000 APSA Meetings goes to Alberto Diaz-Cayeros, Beatriz Magaloni and Barry

Weingast for their paper "Federalism and Democratization in Mexico."

For the Luebbert Award for Best Article in Comparative Politics Published in 1999/2000, the committee selected one winner and three runners-up. The winner is Nicholas Sambinas for his article "Partition as a Solution to Ethnic War: An Empirical Critique of the Theoretical Literature" in *World Politics* 52 (4). The three runners-up are Torsten Persson and Guido Tabellini, "The Size and Scope of Government: Comparative Politics with Rational Politicians" in *European Economic Review* 43; David Rueda and Jonas Pontusson, "Wage Inequality and Varieties of Capitalism" in *World Politics*, 52 (3); and Michael Wallerstein, "Wage-Setting Institutions and Pay Inequality in Advanced Industrial Societies" in the *American Journal of Political Science*, 43 (3).

Comparative Politics at APSA Convention

The Comparative Politics Section will meet at 5:30 p.m. on Friday, August 31, 2001 at the APSA convention to be held in San Francisco, California. The particular room for the gathering will be announced at the event.

Call for Experts

Would you like to be a pundit? The APSA often receives press queries for experts in particular political fields. In response, the APSA is considering the establishment of a directory of names covering all subfields of the discipline. If you are interested in being in-

cluded in such a directory, you should send your name, full contact information including email address, plus a fairly detailed account of your area of expertise to Sue Davis. You can contact Sue Davis by telephone at (202) 483-2512 or by email at sdavis@apsanet.org. ❖

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Continuing Debate

From Philosophers' Stone to Unified Theory

Rein Taagepera

University of California, Irvine and Tartu University, Estonia

rtaagepe@uci.edu

Hopes of a unified “theory of everything” emerge mainly at dawn and high noon of a discipline. In its late morning, scientists are too busy packaging cumulative and interlocking knowledge into more limited theories of *something*.

It started with the alchemists' notion of a philosophers' stone. After the advent of not only Newton and Maxwell but also Einstein, Bohr and Schroedinger with their theories of *something*, physicists began to look for a unified theory, semi-jokingly dubbed the “theory of everything” (TOE). In contrast, biologists have little time left for a “theory of life,” busy as they are with exciting and cumulative studies of *something*, such as the human genome. Political scientists are tempted to latch on to the label of “TOE” when their *something* does not look cumulative, given that the competing term, “philosophers' stone,” has lost its glamour.

My high school philosophy of science course in Marrakech taught me that “theory” has an almost opposite meaning in everyday speech and in philosophy of science (see Figure). What I later encountered in physics agreed with philosophy of science. But what does “theory” mean in political science?

In Michael Wallerstein's lead article in *apsa-cp* (“Does Comparative Politics Need a TOE?” *apsa-cp* 12:1:1, winter 2001) “theory” seems to be synonymous with “paradigm” or “conceptual framework.” Comparisons with relativity and quantum mechanics overlook the respective stages of development. For understandable reasons (complexity and fluidity of the subject of study, plus limited ability to experiment) the achievements of political science, “rational choice paradigm” included, are more reminiscent of physics before Newton than after him. At this stage, dreams of a unified theory sound like “alpolitics” (cf. ‘alchemy’). To be more than a philosophers' stone, a unified theory presumes a basis in firmly established partial theories—and political science is as yet far from that stage.

A hallmark of science at the level of several partial theories is cumulateness. Successive paradigms build on each other rather than replacing the previous one. Relativistic mechanics adds specifications to the Newtonian rather than refuting it. Wave and particle approaches to light combine in a synthesis that defies everyday common sense. The last major notion fully dumped in physics was ether, two centuries ago. In contrast, contemporary political science offers a succession of fashions. Cumulateness is obtained chiefly at the data collection level. (Does it correspond to the Tycho Brahe stage in astronomy?) On the overall paradigm level the main difference with alchemy is that the identity of the hoped-for philosophers' stone changes every quarter-century.

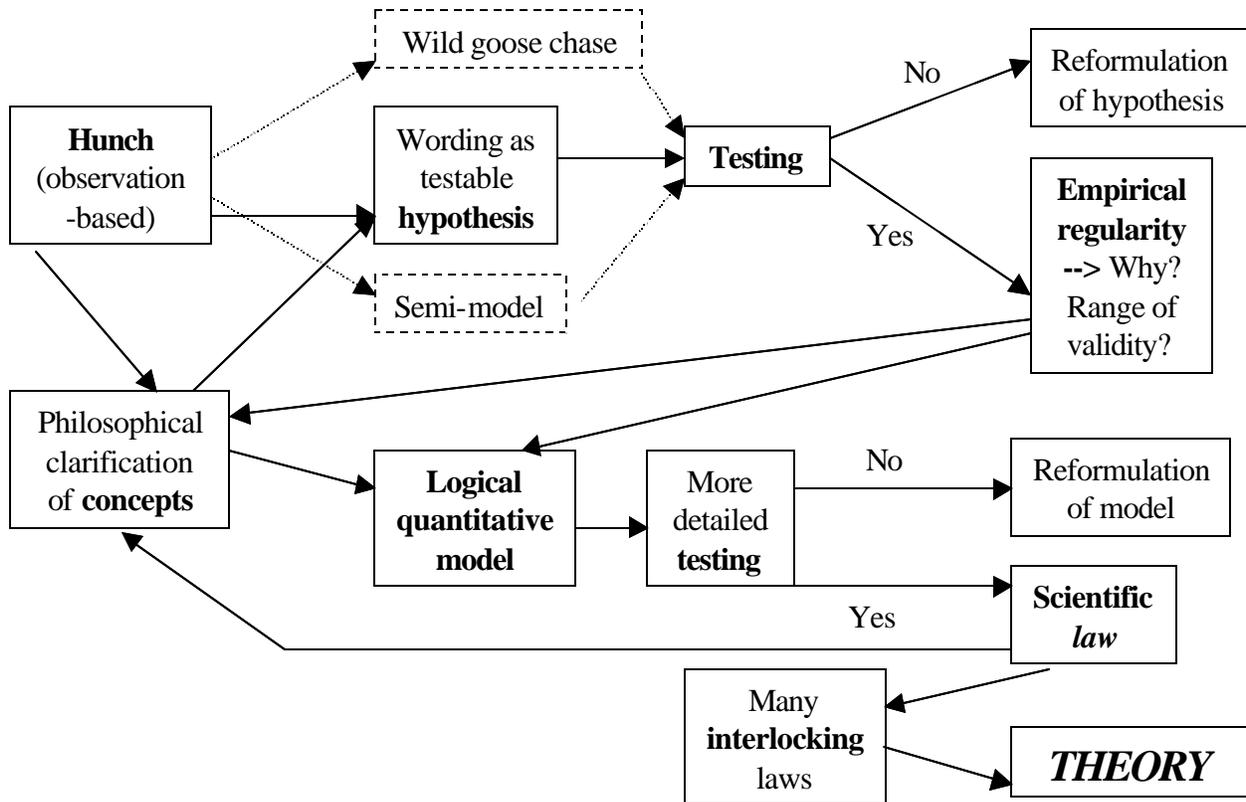
Analogies with more developed sciences may offer a useful road map when the location of political science on this map is fixed realistically. Borrowing the term “unified theory” from the physicists' current quest and pretending that political science, too, already has several proven and stable theories in need of unification is *mania grandiosa*. Let us first establish reasonably firm *laws* (in the scientific sense), and with a sufficiently interlocking set of such laws, some theories of *something* eventually will emerge.

As for conceptual frameworks, they are inevitable. Any empirical inquiry in any field is guided (or misguided) by such a framework, explicit or implicit. Empirical findings in turn affect the conceptual framework, but the latter may have considerable inertia. The Ptolemaic framework yielded to the Keplerian with considerable reluctance. This also means that major breakthroughs may come from researchers at the outskirts of the accepted paradigms. From this viewpoint the frequent shifts in dominant frameworks that

Wallerstein observes in political science may speed up the process. If one is out of fashion, one may only have to wait for 25 or twice 25 years to have one's findings seriously considered.

Actually, no framework has dominated completely and hopefully will not do so until fashionable promise is complemented with much firmer empirical results than presently is the case in political "science." At the present stage it would be stifling to restrict the conceptual frameworks to a single one at a time. And don't call them theories. ❖

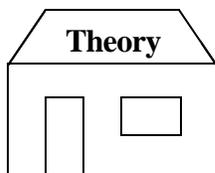
The meaning of "theory" in philosophy of science:



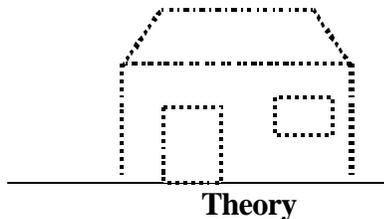
The meaning of "theory" in popular parlance:

"It's just a theory" = it's just a hunch (even less firm than a well-worded hypothesis).

Science: Theory is the ultimate roof



Popular: "Theory" is the ground under a non-existing house



Political science: ?

Finding the Good Theory: Explaining Comparative Politics

Herbert H. Werlin

Independent Consultant
werlin@crosslink.net

In his presidential letter, "Does Comparative Politics Need a TOE (Theory of Everything)?" in the Winter 2001 APSA-CP Newsletter, Michael Wallerstein makes a number of points that need to be examined: (1) we are all theorists inasmuch as we have explanations of what is happening; (2) we have no need for a unified theory; and (3) rational choice is the "only theory in comparative politics today that is sufficiently powerful and general to be a serious contender for the unified theory...." These assertions seem to me to require consideration of two questions: (1) what is politics? and (2) what is theory?

The classical meaning of politics has to do with the relationship of leadership to followership for the purpose of governance. Insofar as rational choice theory (as I understand it) refers only to the "selection of policies," rather than the "implementation of policies," it cannot help us with the most interesting questions in comparative politics, including: (1) why is it that highly authoritarian (even totalitarian governments, such as in North Korea or Cuba) are usually incapable of providing a high standard of living? (2) why is it that liberal democracy (as against classical democracy) may undermine, rather than promote, develop-

ment? (3) why is it that more developed countries are simultaneously more centralized and more decentralized than less developed countries? (4) why is corruption devastating for poor countries, but not rich countries? (5) why is economic globalization good for the development of some countries, but not for others? and (6) to what extent and how can political culture be changed?

In my 1998 book, *The Mysteries of Development: Studies Using Political Elasticity Theory* (University Press of America), I put forward political elasticity theory as my way of answering these questions and, as such, linking comparative politics to comparative administration and development studies. Since this book was summarized in the September 2000 issue of *PS: Political Science and Politics*, including some comparative case studies (The Netherlands and Egypt - decentralization; Singapore and Jamaica - democracy; Japan and Sierra Leone - corruption), I will here only invite readers to examine the relevance of the following propositions for their own theorizing:

1. The more governments or those in authority can integrate and alternate soft forms of political power (linking incentives to persuasion) with hard forms of power (including disincentives and coercion), the more effective they will be.
2. As leaders integrate and alternate soft and hard forms of power, their political power takes on "rubber band" and "balloon" characteristics, allowing them (a) to decentral-

ize or delegate power in various ways without losing control and (b) to expand their influence, reliably and predictably affecting the behavior of wider circles of citizens, participants and subordinates.

3. Political elasticity depends partly on the selection of appropriate political hardware but mostly on political software (which has to do with the "subjective" quality of relationships between leaders and followers).
4. Political software can be made more effective in various commonsensical ways, requiring establishing acceptable goals, hiring qualified personnel, encouraging training, delegating responsibility, stimulating motivation and competition, paying attention to morale, expanding two-way flows of communication, promoting legitimacy, maintaining supervision, cultivating contractors, protecting independent spheres of authority and developing conflict-resolution procedures.
5. The enhancement of political software requires a balancing of partisanship and statesmanship. These two meanings of politics (i.e., the struggle for competitive advantage and the struggle for consensus) can be considered as subdivisions (primary and secondary politics) of my earlier mentioned overarching definition of politics: the relationship of leadership to followership for the purpose of governance.

The theory presented here is a conceptual, rather than a formal theory, with nothing more than an explanatory capacity. There is no effort to quantify political words, separate facts from values or independent from dependent variables, develop testable hypotheses, or make predictions that can be verified. As such, my theory cannot altogether escape the tautological trap, particularly the circular argument. While I may appear to be overanxious to equate success with political

elasticity, I try to look for multi-dimensional evidence.

Yet, the pitfalls of formal theory seem to me far more dangerous. To ask for a social science theory that permits political terminology to be quantified and conditions to be rigorously or definitely predicted is as futile as asking for a perpetual motion machine. To pretend otherwise is to be either fraudulent or foolish (I am uncertain which), as do too many contemporary social scientists. Instead, what I have tried to do is qualify

or explicate such political words as decentralization, corruption and democracy. The justification for PE theory is a simple one – if it helps you, the reader, in your own theorizing, that is good enough! I would appreciate your communication in this regard. ❖

Announcing QUALMETH - a New Discussion List

The Consortium on Qualitative Research Methods is pleased to announce the creation of QUALMETH, an electronic discussion list for political scientists and other scholars to analyze, debate, develop, critique and apply qualitative methods, broadly defined. To subscribe to QUALMETH send an email to listserv@asu.edu with the subject line left blank, and with the message text: `subscribe QUALMETH yourfirstname yourlastname`. For more information see:

<http://www.asu.edu/clas/polisci/cqrm/qualmeth.html>

QUALMETH will post messages on a broad range of topics, including queries on particular methods, discussions of texts advocating or critiquing qualitative methods, analysis of work applying qualitative methods, and up-to-date information on opportunities for, or problems with, archival access. QUALMETH will also publish book reviews, paper abstracts, relevant job descriptions, and notices of the posting of syllabi and working papers on the CQRM electronic database. In addition to these regular activities, QUALMETH will host scheduled roundtables in the CQRM discussion series. The series posts recent or draft articles and book chapters in which scholars analyze, debate, develop, critique and apply a variety of qualitative methods, or discuss issues relevant to their application. For more information, see:

<http://www.asu.edu/clas/polisci/cqrm/discussion.series.html>

Any questions on QUALMETH or CQRM's other activities can be emailed to consortium@asu.edu, or mailed to Dr. Colin Elman, Executive Director, Consortium on Qualitative Research Methods, Box 872001, Arizona State University, Tempe, AZ 85287-2001.

Symposium

Academic Collaboration in Comparative Politics

Editor

Daniel Treisman

University of California, Los Angeles

treisman@polisci.ucla.edu

Introduction

Political science used to be almost entirely the preserve of soloists. In 1980, only one fifth of the articles that came out in the *American Political Science Review* had more than one author. Last year, there were as many co-authored as single-authored *APSR* pieces, including a number by comparativists. In *World Politics*, the share of co-authored articles rose from about 8 percent in 1980 to almost 16 percent in 2000.

Comparing just two years is a rough-and-ready way to gauge change that would make statisticians cringe, but the figures do seem to fit a general perception. (They also match the results of some more systematic counting reported by Ed Mansfield in his contribution to this symposium.) The apparent change afoot raises an important question: if collaboration is catching on, how will this reshape—how *is* it reshaping—our field?

Some practical issues are becoming familiar. Who, these days, has not sat in on a promotion case for a promising scholar whose work is copious but all or almost all of it co-authored? How should one parcel out credit and guess where the creativity or hard labor lay? What is the secret of great collaboration? What should one look for in a collaborator—besides, of course, brilliance, generosity and a Puritan work ethic? What are the advantages and difficulties of collaboration between junior and senior scholars? Across disciplines? Across nationalities? Across the breakfast table?

We asked a variety of scholars—not just in comparative politics but from a range of fields—to share their insights. Robert Kaufmann, a comparativist with a long history of collaboration, offers a few thoughts on the cooperative efforts that went into crafting his well-known work with Steph Haggard. Ed Mansfield surveys the growth of collaboration in both comparative politics and IR in the 1990s, and mines the major journals for hints about why people are doing more joint research.

How does collaboration work in other scientific disciplines? William Baumol, an economist with a record of distinguished collaborations that goes back more than four decades, reflects on the practice in economics. In the natural sciences, the titles of journal articles are often dwarfed by the list of authors, which sometimes seems to include everyone associated with the lab in question down to the janitor. To explain the norms that have developed to attribute credit for such collaborations, we picked at random an up-and-coming biochemist from the NYU Medical School whose (co-authored) articles have appeared in *Cell*, *Developmental Biology*, and *Nature*.

In psychology, a new way of using collaboration to advance the research process may be emerging. One senior scholar, Daniel Kahneman, when asked by a journal editor to write a response to an empirical attack upon his work, recently invited the attackers to join forces with him. Together, they designed experiments that could resolve their disagreement, and published a joint article laying out the results and their different interpretations. We publish here the protocol for “adversarial collaborations” that Kahneman developed.

Some Reflections on My Collaboration with Stephan Haggard*

Robert R. Kaufman

Rutgers University
kauftrutger@aol.com

For about the first 15 years of my professional life, my publications consisted primarily of single-authored works. Since the mid-1980s, on the other hand, a very large portion of my work has involved collaborative research and writing. During that time, I have been fortunate to team up with a variety of very able scholars, including Barbara Stallings, among others. The most sustained collaboration, however, has been with Stephan Haggard. We published our first co-authored article on the debt crisis in 1989, and since that time we have worked together on a book, two edited volumes, and at least seven other articles and book chapters, all related to the political economy of adjustment and democratization. I am proud of what Steph and I have accomplished together, but I will leave it to others to judge the contributions these works have made to scholarship. For the present purpose, it is a reasonable gauge of success that we have been able to complete so many projects and still remain good friends.

I suppose the main point to emphasize in the history of this collaboration was that it evolved "organically" and quite slowly out of a series of intellectual encounters which involved not only Steph and me, but a number of other people as well. Steph and I first met as participants in a series

of seminars on the debt crisis of the early 1980s, organized through the efforts of Tom Biersteker and Miles Kahler. In one of those seminars, we each presented individually authored papers, which were eventually published in a special edition of *IO* and in *The Politics of International Debt*, both edited by Kahler. I had not known Steph prior to our presentations, but remember being incredibly impressed by the quality of his paper.

In the course of these meetings, a number of participants began to engage in more intensive discussions about the political economy of stabilization and structural adjustment, and to coordinate their ideas into a relatively coherent research agenda. In addition to Steph and me, the other "core" participants included Joan Nelson, Barbara Stallings, Miles Kahler, and Tom Callaghy. Eventually, under Joan Nelson's leadership, we acquired research funding from Ford and Rockefeller for a collaborative project. An important first step consisted of case studies and small-N comparisons of adjustment experiences in thirteen poor and middle-income countries; these were published in *Economic Crisis and Policy Choice*, edited by Nelson. All of the chapters were individually authored, but the book as a whole was a joint effort that involved careful coordination of initial research questions, frequent meetings, and extensive discussion of the conclusions that could be derived from the individual reform stories.

Though the cast of characters changed somewhat, this group continued to work to-

gether to produce two other volumes: *Fragile Coalitions*, also edited by Nelson, and *The Politics of Economic Adjustment*, edited by Haggard and Kaufman. The first of these was addressed mainly to a "policy audience." The second, which also enlisted contributions from John Waterbury and Peter Evans, attempted to build on earlier case study work by focusing more explicitly on "cross-cutting" analytical issues: the influence of the IMF, the politics of inflation and stabilization, anti-poverty efforts, privatization and the role of the state.

The discussions, research, and learning that went on during this period provided the foundation for my co-authored book with Haggard, *The Political Economy of Democratic Transitions*. The ideas elaborated in that volume were initially sketched out in the introduction and conclusion of the co-edited book. Our objective was to explore the way our earlier analysis of the politics of market-oriented reform intersected with the strategy-oriented approaches that then characterized the democratization literature.

A number of factors helped to make this collaboration work. For example, we had complementary knowledge of different regions; Steph's comparative advantage was East Asia, whereas mine was Latin America. He also knew more about IPE, whereas I had done more on the issue of regime change and democratization. Moreover, since Steph and I had both established fairly visible records of individual scholarship prior to our collaboration, there were none of the issues that sometimes arise about

who contributed what; the "Haggard and Kaufman" brand name is based entirely on the alphabet (although I have at times considered dropping the letter "K" from my last name).

Perhaps the most important thing was that we approached problems of comparative research in similar ways. Although both of us were guided by theory, we also wanted to confront the specificities of individual cases, a commitment that usually pulled us in the direction of small-N comparisons. These instincts were reinforced by the numerous encounters I have just described. In part because we spoke a "common language," our way of working did not involve a clear division of labor. After we discussed a project or a chapter, one or the other of us would take his hand to a first draft. We would then pass the draft back and forth between us, sometimes as many as ten times, until we were both either satisfied or exhausted. There were plenty of arguments and mutual criticisms. But there was also a genuine synergy, and I strongly believe that the results were far better than either of us could have produced alone.

On the basis of my work with Steph as well as with other colleagues, I have become convinced that collaborative research should, and will, play an increasingly important role in our discipline. It is simply impossible to expect any single individual to master the increasingly specialized domains of knowledge that now characterize political science. On the other hand, collaborators must also be able to speak mutually comprehensible languages if their joint

work is to be integrated and coherent; a strict division of labor between narrow specialists will not do. So, opportunities for collaborative research do not obviate the need for individual scholars to acquire knowledge of a relatively wide range of skills and subjects.

There are, of course, many ways in which such problems can be overcome, and I am not sure that my own experience provides readily generalizable lessons. It is not easy to reproduce the circuit of seminars and informal encounters which allowed so many people from different academic institutions to come together and build on their common interests. Almost assuredly, there are many formulas for successful collaborations besides the one I have described. This one, however, evolved incrementally, beginning with efforts that involved relatively limited commitments and then grew from there.

**Thanks to Steph Haggard and Joan Nelson for comments and suggestions.*

The Growth of Collaborative Research: Comparative Politics and International Relations

Edward D. Mansfield

*University of Pennsylvania and Ohio State University
mansfield@polisci.sbs.ohio-state.edu*

Over the past few decades, there has been a noticeable increase in the amount of co-authored research being conducted by political scientists. In this article, I speculate on the

reasons for the growth of collaborative studies in comparative politics and international relations, the latter of which is my field of expertise. I then survey some leading journals to get a preliminary sense of how much jointly authored work is being published in these fields and who is conducting such work.

Collaborative research has always played an important role in comparative politics and international relations. In international relations, for example, some of the best-known studies, such as Robert O. Keohane and Joseph S. Nye's *Power and Interdependence* (Little, Brown, 1977) and Glenn H. Snyder and Paul Diesing's *Conflict Among Nations* (Princeton University Press, 1977), were co-authored. And the Correlates of War Project, which is led by J. David Singer at the University of Michigan, has given rise to a steady stream of collaborative work for almost four decades. But despite the prominence of certain collaborative enterprises, co-authored studies have traditionally comprised a relatively small portion of the research conducted in comparative politics and international relations. Even a casual glance at the literature suggests that this tendency has started to change: in recent years, there has been a marked rise in the amount of work involving investigative teams.

It seems to me that this development can be traced to at least three factors. First, work in comparative politics and international relations is becoming more technical and demands high levels of specific types of expertise.

Some research involves the use of formal models and/or the quantitative analysis of relatively large and complicated data sets. Other studies stand at the intersection of international relations and comparative politics, political science and economics, or political science and sociology. As well, comparative and international research often demands expertise in particular regions of the world. Given the time and diverse talents needed to conduct such work, there are often substantial gains to be realized from collaboration among two or more individuals with different skills.

Second, collaborative work has grown easier and less expensive to conduct, especially for individuals who are not located at the same institution. Central in this regard has been improvements in computer and communications technologies, which have allowed collaborators to exchange ideas, data, results and drafts of manuscripts quickly and at very low cost. Also noteworthy is the dip in the cost of travel over the past few decades and the willingness of some colleges and universities to bear these costs when collaborators need to meet in person.

Third, having a record of conference papers and published articles has become increasingly important for graduate students entering the job market. Faculty sometimes have tried to relieve these pressures by collaborating with their students. Clearly, faculty-student collaboration is nothing new, but my sense is that it is more common now than in the past.

Related to why co-authored research has become more common are the issues

of how pervasive such research has become and who is conducting it. To obtain some data that might help address these questions, I surveyed the articles in comparative politics and international relations (excluding review essays) published between 1990 and 2001 in the following journals: (1) *American Journal of Political Science*, (2) *American Political Science Review*, (3) *British Journal of Political Science*, (4) *Comparative Political Studies*, (5) *Comparative Politics*, (6) *International Organization*, (7) *International Studies Quarterly*, (8) *Journal of Politics*, and (9) *World Politics*. These periodicals differ on many dimensions, including the amount of collaborative research they publish, the frequency with which they publish articles on comparative politics and international relations, and whether they are geared to subfield specialists or political scientists in general. Each, however, is a high-quality outlet for research in comparative politics and/or international relations that does not discriminate against co-authored work.

I began by determining the percentage of comparative politics and international relations articles that were co-authored in each of these nine journals. In each of the general interest journals (that is, the *American Journal of Political Science*, *American Political Science Review*, *British Journal of Political Science*, and *Journal of Politics*), roughly 50 percent of the articles were co-authored. In contrast, the corresponding figure in the subfield journals is about 25 percent, although there is considerable variation among them,

ranging from a low of about 15 percent in *Comparative Politics* to a high of about 35 percent in *Comparative Political Studies*. Thus, while collaborative work has become more common, it is by no means the norm in comparative politics or international relations.

Interestingly, the growing use of quantitative methods in these subfields and the rise of collaborative research seem to be closely related. Regardless of which journal is surveyed, articles written by a team of authors are more likely to use quantitative methods than solely authored articles. This tendency is especially pronounced in the subfield journals, although it is also apparent in the general interest journals.

Who are the collaborators in comparative politics and international relations publications? Depending on the journal in question, between 20 percent and 30 percent of this research involves a faculty member and a graduate student. But the majority of collaborative work in these subfields is undertaken by faculty alone. Moreover, faculty members conducting such work tend to be located at research universities and think tanks. Perhaps not surprisingly, faculty at teaching colleges rarely publish in the journals being surveyed. When they do, however, the articles are almost never co-authored.

Especially pronounced is the tendency for collaborative research on comparative politics and international relations to involve a full professor. On average, about 60 percent of the collaborative articles that were surveyed included at least one

author of this rank. In rather striking contrast, only about one-quarter of the solely authored articles were written by full professors. On the other hand, roughly 40 percent of the single-authored articles were written by assistant professors and faculty of this rank tend to engage in less collaborative research than their more senior counterparts. Further, unlike both full professors and assistant professors, associate professors are about as likely to write single-authored articles as they are to write co-authored articles.

Finally, I made a preliminary attempt to address whether collaboration in comparative politics and international relations has become more or less pervasive over the past decade by comparing publication patterns from 1990-1995 to patterns from 1996-2001. I found that there has been a modest increase in the amount of collaborative research in these fields during the recent past and that this development stems primarily from the growth of jointly authored work published in sub-field journals. In general interest journals, the portion of articles on comparative politics and international relations written by teams of scholars has changed very little since 1990. There is also some evidence of an increase in the amount of jointly authored studies involving a faculty member and a student, although this varies quite a bit by journal. But the amount of collaborative research published by faculty at both universities and teaching colleges has been fairly stable. So too has the portion of co-authored articles written by assistant professors, associate

professors, and full professors, respectively, although it is noteworthy that there has been a marked jump in multiple-authored articles involving a full professor in certain journals (namely, *Comparative Politics* and *International Organization*).

Clearly, these figures should be interpreted with considerable caution. Particularly important is that they pertain to only a fraction of the research on comparative politics and international relations, most notably because no account is taken of research published as or in books.

Nonetheless, one of the most striking patterns emerging from this analysis is that faculty are more likely to engage in collaborative research as they become more senior. This tendency is not difficult to understand. After all, assistant professors start their careers with dissertation research that, except in very unusual circumstances, is conducted alone. Moreover, they have strong incentives to publish that research in high-quality outlets, like the ones surveyed here. Equally, it can be quite hazardous for assistant professors to become overly involved in co-authored work. One obvious problem in evaluating such work is determining each author's contribution to the final product. An assistant professor with a publication record composed solely of co-authored research who is being considered for tenure might find it difficult to convince a committee reviewing her work that she played enough of a role in this research to warrant promotion. Further, my sense is that there continues to be a

strong norm in most political science departments that junior faculty need to demonstrate the ability to publish alone in order to be promoted. That such a large portion of single-authored work in the journals I surveyed are single-authored pieces by assistant professors suggests that junior faculty are well aware of the potential pitfalls associated with becoming too involved in collaborative research at the beginning of their careers.

Freed from the need to impress promotion committees, however, full professors have much greater latitude to choose whether or not to do collaborative research. In addition, senior faculty have greater discretion to pursue projects that are interesting but out of the mainstream. Rather than make the (potentially substantial) investment needed to acquire the expertise necessary to complete such a project by herself, it may make more sense for a faculty member to team up with someone already having that skill. Finally, full professors are especially likely to mentor graduate students and we saw that a non-trivial portion of collaborative articles during the past decade have been jointly written by faculty members and students.

During my career, I have taken part in a fair amount of collaborative research, having worked with my students, my dissertation advisor, my departmental colleagues, and faculty at other institutions. These experiences have been remarkably enriching. I have learned a great deal from my co-authors and have enjoyed the process of collaborating with them. Moreover, many of the resulting papers sim-

ply could not have been written by any one of us individually. That, it seems to me, is the best reason to do collaborative research: it allows scholars to pursue a wider range of topics with greater expertise and insight than would be possible working alone.

On Collaborative Research and Writing in Economics*

William J. Baumol

*New York University and
Princeton University
baumolw@fasecon.econ.nyu.edu*

Substitute and complementary collaborators

The vital ingredient of many fruitful collaborations over the years has been my areas of ignorance. Recognition that much of what I was currently investigating was beyond my range of knowledge and analytic skills many times has led to partnership with someone who was able to provide what would otherwise have been missing. To the extent that I, too, could provide a contribution that would have been difficult for the other person, the result was superadditive – the whole was greater than the sum of the parts. More than that—this pattern helps to explain why my collaborators and I have, literally, never quarreled. We each knew that in relation to what the other was writing we had a good deal to be modest about.

Of course, the division of the territory in this way is not the only possible way of proceeding. Clearly, two analysts with similar abilities may nevertheless profit

from working together. They can stimulate one another's efforts; their discussions can lead to new ideas and approaches; and if they find writing to be an onerous task, each author will appreciate the opportunity to share the burden.

Why the growth in collaborative work?

But it is my hypothesis that such collaboration between investigators whose talents are substitutable rather than complementary is not the reason for the marked growth in frequency of such arrangements. The incentive of burden sharing and mutual stimulation has long been present. Rather, I believe, an explanation that is more persuasive is the marked increase in specialization and the difficulty of the technical analytic requirements that have characterized many academic disciplines in recent years. Today the general economist is virtually extinct. We divide ourselves not only into macro and micro practitioners, but within each of these there is a profusion of specializations: growth theorists, finance specialists, labor economists, pure theorists, applied econometricians, and so forth. Whereas forty years ago an economics department normally offered one or two seminars that were attended by most of the faculty and graduate students, nowadays these seminars have proliferated substantially, and each is usually attended by a handful of participants, the specialists in the sub-field. Moreover, a superficial acquaintance with such a field and its vast literature simply will not do. Hence, research and writing that

span several such sub-fields, as frequently occurs, urgently calls for collaboration.

Disputes among collaborators

Yet collaboration is not an easy thing. Working methods can differ. One participant may be neurotic about meeting deadlines, another may be chronically inclined to postponement until the deadline looms, and some may disregard deadlines altogether. Peace among collaborators is considerably facilitated if they select themselves as a group with homogeneous attitudes on such matters and on working procedures more generally. I understand that disputes among collaborating authors are quite frequent and can sometimes grow bitter, perhaps on the oft-noted principle that there is so little at stake. I remember how such a dispute arose many years ago, when a close friend of mine, a noted biologist, was writing an elementary textbook with a colleague who, I understand, was also eminent. The book contained a photograph of a sea turtle making its way across a beach as part of the egg-laying process. The one author had written, "The turtle comes ashore *to* lay its eggs." This aroused the ire of his partner who insisted that it be revised to read, "The turtle comes ashore *and* lays its eggs." The teleological battle was protracted and emotional. I remember one discussion that literally lasted the entire night, even enlisting the participation of a famous philosopher. My point is not that the distinction was unimportant. To specialists it may well be vital. But I suspect that among the many freshmen and sophomores

who read the book, there were few if any who would have noticed, let alone being brought up short, by what implicitly was being claimed.

Disentangling contributions: evidence for promotion and tenure

The growth in collaborative research and writing has evidently created severe impediments for evaluation of the work of the participants by their departments. It is clearly not easy to evaluate the contribution and abilities of an assistant professor who has taken part in the preparation of 20 articles, all of them having at least one coauthor.

The attempt to disentangle the evidence is surely undertaken in every university, but its success is plainly rather limited. Indeed, I am skeptical about its prospects. Experience in other fields leads me to take a dim view of attempts to divide up the indivisible. Unless an article is divided into sections — with each piece clearly emanating from a different author — how can one hope to disentangle not only the words contributed by each but also, more important and much more elusive, the relative values of those words? Attempts to find a systematic method for approaching the matter can easily contribute further confusion by appearing to provide some light, where they merely camouflage the difficulty and thereby only add to darkness. In governmental price regulation the attribution issue is a familiar problem and its dangers have been thoroughly documented. Price regulators customarily, and with good reason, seek to take costs into account.

But there are costs of which no portion can legitimately be attributed to any one of the regulated firm's products. An obvious example is railroad rate regulation, where a substantial proportion of the cost is made up of track, bridges, tunnels and so forth, all of which are used in common in transporting steel, coal, lumber, wheat and the many other products the railroad carries. Predictably, hoping to get a low rate for their clients, the attorneys who represent the shippers of feathers argue that weight is the proper basis for the apportionment of cost, while those who represent lead shippers argue for the use of bulk as the appropriate criterion. The result is extended litigation, heavy expenditure on attorneys and expert witnesses, and irrational decisions, but no illumination.

These are not the only perils that beset the search for systematic methods to assign the unassignable. One may, for example, be tempted to use a "common element" criterion associated with J.S. Mill — the view that if one author is a collaborator common to a number of particularly successful articles, she, as the one common contributor, can be presumed to bear the primary responsibility for their success. But that, as Jacob Viner was fond of pointing out, is like observing that since bourbon and soda, scotch and soda, and brandy and soda are equally inebriating, it is the soda that must be responsible.

My conclusion is that the problem has no systematic answer. We are forced to stumble toward an evaluation on the basis of casual observation of the

person's performance, intuitive evaluation based on discussions with the person, and other bits and pieces of evidence that can be presumed to supplement the limited inferences that can be derived from the evidence of collaborative publications alone.

Conclusion

Collaboration is not all benefit. The process of working together can, for example, tend to discourage heterodox approaches and conclusions, or unconventional modes of expression. Still, it does tend to add to the range of knowledge taken into account in the work, and gives each participant the opportunity to benefit from the stimulation of the ideas, suggestions and criticisms of the others. There is plainly room for both collaborative work and work that is individually produced. By and large I feel that the trend toward more frequent co-authorship is a beneficial development; but, that is a conclusion for which no firm and objective foundation can be claimed.

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Collaboration and Credit in the Biological Sciences

Jessica Treisman

New York University

School of Medicine

treisman@saturn.med.nyu.edu

Experimental research in the natural sciences is almost exclusively a collaborative effort. The most common type of collaboration is between one or more junior members of a laboratory and the head of that lab; however, collaborations among two or more labs are becoming increasingly frequent. The first type of collaboration is a result of the extended training period required to gain experience in complex experimental techniques, as well as the significant amount of time that must be devoted to fund-raising to support research that requires expensive equipment and materials. The second is due to the realization that many problems require a multidisciplinary approach beyond the expertise that can be assembled in one lab, and also to the increasing convergence of interests between scientists working on apparently different questions that turn out to be related. Although such collaborations often result in publications that are deeper and more intellectually satisfying than what a single group could have achieved, they complicate the process of awarding credit to the individuals involved. In the biological sciences, a system of rules and conventions has been developed to deal with this, and in general contributions are evaluated appropriately; however, the exceptions that arise

can cause problems in deciding both credit and responsibility. Other disciplines use alternative conventions; for example, many high-energy physics papers list their numerous authors alphabetically.¹

Division of labor within a lab

A biology lab is normally headed by a faculty member or principal investigator, and may also contain one or more research technicians, graduate students and postdoctoral fellows. Each of these individuals has different goals and responsibilities. In general, the responsibilities of the lab head are to supervise the other lab members, provide intellectual input to their research, assist in writing papers, write grants to raise money for salaries and supplies, publicize the research of the lab through seminars and conferences, and perhaps teach undergraduates or graduate or medical students. Research technicians are often recent graduates seeking lab experience before applying to graduate or medical school, although they can also be older individuals who have chosen to make their career in developing a particular technical expertise. They may have independent projects, but also are usually expected to assist others in the lab with the less intellectually stimulating aspects of their work, such as ordering supplies or making solutions. Graduate students are expected to spend an average of five years on an integrated research project that will introduce them to both the practical and intellectual aspects of science, as well as leading to significant findings and publications. Postdocs, who already

have a Ph.D., are continuing their training in another area of research, and also must establish a foundation of accomplishments on which to build their own labs. They are generally more independent than graduate students, in part because of their greater experience, and in part because they need to find a direction to follow in the future that will not directly compete with the lab in which they trained.

Although this collaborative structure allows faster progress than individuals could achieve, at times it can be frustrating for all concerned. Junior members of the lab may feel they are not given enough freedom to pursue their own ideas, perhaps because of commitments made to funding agencies. The lab head, who has been trained as an experimentalist, may find it difficult to adjust to management and watch less experienced people bungle his or her ideas. A few lab heads do manage to continue their own experimental work, but in most institutions promotion depends on growth in lab size and grant support, making this difficult.

Recognizing contributions in print

The division of labor between lab members results in multi-author publications in which the order of authors reflects the differences between their contributions. Conventions for ordering authors in the credits seem to have evolved in a decentralized way, but they are widely understood by scholars in the field. Typically, the lab head is the last author, and this position is taken to mean that the person had a guiding intellectual role in the project rather than making a

“hands-on” contribution to the research. The first author is the person, usually a student or postdoc, who did the bulk of the experimental work. The different meanings of the first and last positions mean that conflicts between these two authors are rare. However, conflicts frequently arise between the first author and other contributing “middle” authors. A position as second, third, fourth, etc. author means that the person has made a contribution to the work; however, this contribution may vary greatly, from simply donating a reagent to carrying out a large proportion of the experimental work. Because of this variability, the credit assigned to a second or third author is significantly less than that assigned to the first author. Postdocs applying for faculty positions are judged predominantly on their first-author papers, and there is also pressure on students to publish as the first author. Thus individuals in a lab normally prefer to publish their more time-consuming projects as first-author papers. Counteracting this is the need to have a complete story, often more than one person can accomplish in a reasonable period of time, in order to publish in the top journals. This may result in the lab head prevailing on other lab members to contribute their work to a paper on which they will be the second or third author.

No completely satisfactory strategy to recognize the contributions of such authors has been found. Many journals allow an asterisk to appear after several of the authors’ names, followed by a note to say that those individuals contributed

equally to the paper. Although this is the most common way to appease an unhappy second author, credit will not in fact be equally perceived by those who simply see the paper listed on a c.v. without accompanying asterisks. In general, conflicts can best be avoided by deciding the order of authors as early in the project as possible. This allows those who know they will be middle authors to put in only the amount of effort justified by such a position. This approach may run into problems if one person has to leave the lab before the project has been completed. This person may have done a large amount of work and have assumed that he or she would be the first author of the resulting publication. However, completing the project may take significantly more work than anticipated, due to technical difficulties or reviewers’ criticisms, giving the person who completes it an equally legitimate claim to first authorship. The latter claimant then has the advantage of proximity to the lab head.

Multi-lab collaborations

Collaborations between labs raise a different set of issues. Such collaborations can arise in a number of different ways. The members of one lab may realize that a set of techniques beyond their expertise would be necessary for a full analysis of the problem on which they are working, and turn to another lab for assistance. In such cases it is often understood that the second lab will make an important but minority contribution to the finished work, which is normally recognized by making the student or postdoc from that lab

the second author and the lab head the second to last author. If a third lab becomes involved, its members will take up authorial positions correspondingly closer to the middle of the list. A more difficult situation can arise if two groups discover that they have been working on the same topic. One option is to publish back-to-back papers repeating much of the same data, but another possibility is to pool the results in a single paper to avoid duplication of effort. However, if the contributions of both labs have been fairly equal, it can be difficult to decide both the first-author and last-author positions. There is no good mechanism yet to acknowledge equal contributions of the two lab heads.

Awarding credit

Variations in the relative contributions of authors arising for the reasons listed above can lead to difficulties in evaluating candidates for hiring or promotion. In general, postdocs applying for a faculty position are judged predominantly on the first-author papers on their c.v. This probably means that their contributions to papers on which there are several other authors are over weighted, while their contributions to papers on which they were the second author are under weighted. However, collaboration as a second author can still be beneficial to such candidates, as it enables them to discuss the collaborative work in their seminar. Faculty members applying for promotion are judged predominantly on their last-author papers, although additional middle-author papers can be helpful as evidence of the

ability to collaborate. Thus an over-enthusiasm for collaboration may be penalized if it reduces the time available for more independent research. Paradoxically, single-author papers, written by rare faculty members who continue their own experimental research, may be valued less than multiple-author papers, which demonstrate that the last author has trained additional scientists in his or her lab.

Although the mechanisms for evaluating an individual's contributions to collaborative papers are imperfect, foreknowledge of these mechanisms allows some adjustment of effort. The rewards of collaboration are the satisfaction of contributing to a study that is more complete and of greater interest than the part of it that one could have accomplished alone, as well as the intellectual stimulation and insights gained from discussion with one's collaborators.

1. *Nature* (1997) **387**, 831.

Adversarial Collaboration

Must collaborations occur between friends? Not, according to one leading psychologist. In a recent article in the journal *Psychological Science*, Daniel Kahneman and several colleagues introduced the concept of "adversarial collaboration."¹ Ralph Hertwig of the Max Planck Institute for Human Development in Berlin had challenged a finding of Kahneman about common biases in probabilistic reasoning.

Rather than publish a series of critiques and rejoinders, the two set out to design, conduct and write up joint experiments that promised to resolve their disagreement. The two brought in a third psychologist, Barbara Mellers, to act as "arbiter," resolving disputes about procedures. To cut a long story short, the collaborators did not end up agreeing in all respects. But the differences narrowed on the basis of the new results.

As the authors write in the resulting article: "Collaborating with an adversary is not easy... Despite our mishaps, we hope the approach catches on. In an ideal world, scholars would feel obliged to accept an offer of adversarial collaboration. Editors would require adversaries to collaborate prior to, or instead of, writing independent exchanges. Scientific meetings would allot time for scholars engaged in adversarial collaboration to present their joint findings. In short, adversarial collaboration would become the norm, not the exception." A second type of adversarial collaboration, pioneered by Kahneman with different collaborators,² is the joint commentary on a critique: rather than just responding in print, the scholar whose conclusions have been challenged joins with his critic to co-author a commentary sharply defining their areas of agreement and difference.

Experiments are rare in political science. Nevertheless, there may be ways in which such a practice could be adapted. Scholars might agree on what would constitute a "critical case," and then look for examples. In

empirical analysis, they might agree on different ways to reanalyze the same data. Kahneman proposes the following protocol for those inspired to try an adversarial collaboration. We are grateful to *Psychological Science* and its editor, Sam Glucksberg, for permission to reprint it.

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Suggestions for Adversarial Collaboration

Daniel Kahneman

Princeton University

kahneman@princeton.edu

1. When tempted to write a critique or to run an experimental refutation of a recent publication, consider the possibility of proposing joint research under an agreed protocol. We call the scholars engaged in such an effort participants. If theoretical differences are deep and/or if there are large differences in experimental routines between the laboratories, consider the possibility of asking a trusted colleague to coordinate the effort, referee disagreements and collect the data. We call that person an arbiter.
2. Agree on the details of an initial study, designed to subject the opposing claims to an informative empirical test. The participants should seek to identify results that would change their mind, at least to some extent, and should explicitly anticipate their interpretations of outcomes that would be inconsistent with their theoretical expectations. These predictions should be

recorded by the arbiter to prevent future disagreements about remembered interpretations.

3. If there are disagreements about unpublished data, a replication that is agreed by both participants should be included in the initial study.
4. Accept in advance that the initial study will be inconclusive. Allow each side to propose an additional experiment to exploit the fount of hindsight wisdom that commonly becomes available when disliked results are obtained. Additional studies should be planned jointly, with the arbiter resolving disagreements as they occur.
5. Participants should agree in advance to produce an article with all participants as authors. The arbiter can take responsibility for several parts of the paper: an introduction to the debate, the report of experimental

results, and a statement of agreed conclusions. If significant disagreements remain, the participants should write individual discussions. The length of these discussions should be determined in advance and monitored by the arbiter. An author who has more to say than the arbiter allows should indicate this fact in a footnote and provide readers with a way to obtain the added material.

6. The data should be under the control of the arbiter, who should be free to publish with only one of the original participants if the other refuses to cooperate. Naturally, the circumstances of such an event should be part of the report.
7. All experimentation and writing should be done quickly, within deadlines agreed in advance. Delay is likely to breed discord.

8. The arbiter should have the casting vote in selecting a venue for publication, and editors should be informed that requests for major revisions are likely to create impossible problems for the participants in the exercise.

¹ Barbara Mellers, Ralph Hertwig and Daniel Kahneman, "Do Frequency Representations Eliminate Conjunction Effects? An Exercise in Adversarial Collaboration," *Psychological Science*, Vol. 12, July 2001, pp. 269-275.

² Dan Ariely, Daniel Kahneman and George Loewenstein, "Joint Commentary on 'The Importance of Duration in Ratings of, and Choices Between, Sequences of Outcomes,'" *Journal of Experimental Psychology*, December 2000, 129, pp. 524-529. ❖

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APSA Annual Meeting

Comparative Politics in San Francisco

The preliminary schedule for this year's APSA Convention in San Francisco includes the following panels:

11-1	Institutional Arrangements and Their Policy Implications in Emerging Democracies	Fri 1:30 pm	11-24	Taxation and Public Sector Spending and Capacity	Thu 8:45 am
11-2	Evaluating Success and Failure in Post-Communist Economic Reform	Thu 1:30 pm	11-25	The Politics of Economic Reform	Thu 10:45 am
11-3	Political Foundations of Economic Development	Sun 10:45 am	11-26	Regionalism, Political Institutions and Voting Behavior	Sun 8:45 am
11-4	Processes of Diffusion in Political and Economic Liberalization	Thu 3:30 pm	11-27	Establishing Justice and the Rule of Law in New Democracies	Fri 1:30 pm
11-5	The Politics and Economics of Post-Communist Reform: A Theoretical and Methodological Discussion	Fri 8:45 am	11-28	How Institutions Shape Politics	Sat 8:45 am
11-6	The Institutional Determinants of Governmental Performance	Sat 8:45 am	11-29	Central Bank Independence, Labor Institutions, Inflation and Unemployment	Fri 10:45 am
11-7	The Comparative National Elections Project	Fri 10:45 am	11-30	Party Systems in New Democracies	Sun 8:45 am
11-8	The International Diffusion of Policy Innovation	Thu 8:45 am	11-31	The Effect of Federalism on Economic Institutions and Performance	Sat 3:30 pm
11-9	Rethinking Democracy and Dictatorship in Interwar Europe	Sun 8:45 am	11-32	Democracy and Growth	Thu 1:30 pm
11-10	What We Know and How We Know It in Comparative Politics	Fri 8:45 am	11-33	The Effect of the Economy on Public Opinion and Voting in New Democracies	Fri 1:30 pm
11-11	Rules and Social Norms: Formal and Informal Institutional Dynamics in the Politics of Late Developers	Sun 10:45 am	11-34	The Politics of Decentralization	Thu 3:30 pm
11-12	Government Capacities and Policy-Making in Latin America	Thu 10:45 am	11-35	The Politics of Welfare Spending and Income Inequality	Fri 10:45 am
11-13	Southeast Asian Industrialism on Trial	Fri 8:45 am	11-36	Contested Transitions to Democracy	Thu 8:45 am
11-14	Market Reform, Institution Building and Democratization	Sun 10:45 am	11-37	The Politics of Environmentalism	Sat 10:45 am
11-15	Comparative Politics, Rational Choice and the Hermeneutics of Strategic Action	Fri 3:30 pm	11-38	Women, Politics and Family Welfare in Comparative Perspective	Sat 1:30 pm
11-16	Politics in Japan: Regime Shift or Incremental Change?	Sat 10:45 am	11-39	New Research on the European Union	Fri 3:30 pm
11-17	When Politicians Switch Parties or Parties Switch Positions: A Comparative	Thu 3:30 pm	11-40	Comparative Electoral Systems	Fri 1:30 pm
11-18	New Political Economy of Latin America	Sat 1:30 pm	11-41	Veto Players and Political Analysis	Thu 10:45 am
11-19	The Role of Mass Protest during Transitions	Sat 3:30 pm	11-42	Comparative Analyses of Human Rights Violations and Protective Mechanisms	Sun 10:45 am
11-20	Strategic Behavior during the Mexican Transition	Sat 8:45 am	11-43	Putting the Politics back into Privatization: Cross-National Studies of Transitions in Socialist, Post-Socialist and Developing Countries	Thu 3:30 pm
11-21	Popular Attitudes toward Economic Reform	Sat 10:45 am	11-44	State-Market Relations in Post-Communist Settings	Sun 10:45 am
11-22	Models of Bureaucratic Accountability and Reform	Thu 1:30 pm	11-45	Economic Change and Political Response: The Politics of Market Liberalization	Fri 1:30 pm
11-23	New Approaches to Understanding Ethnicity	Sat 1:30 pm	11-46	Globalization, Domestic Politics and Political Causes: Tracing the Causal Pathways	Fri 8:45 am
			11-47	Globalization, Democratization and the Accumulation of Human Capital	Fri 3:30 pm
			11-48	Informal Institutions and the State: A Cross-Regional Perspective	Sat 3:30 pm
			11-49	Taking Democracy Seriously: The Role of Political Parties in Southeast Asia	Fri 1:30 pm
			11-50	Regime Transition and the Radical Right in Central and Eastern Europe	Thu 1:30 pm
			11-51	Scaling in Comparative Research	Fri 8:45 am
			11-52	Race, Ethnicity and the Power of Cross-National Comparison	Sun 10:45 am

Division chair: Barbara Geddes, University of California, Los Angeles

Book Review

From Voting to Violence: Democratization and Nationalist Conflict

By Jack Snyder

New York: W.W. Norton and Company. 2000. 382p. \$29.95

Robert A. Dowd

University of California, Los Angeles
rdowd1@nd.edu

Jack Snyder claims that all good things do not go together and that while democratization may be a good thing in the long run for stability and economic growth, in the short run it produces nationalist conflict and therefore makes war within and between states more likely. While both peace and democratization are good and desirable goals, Snyder suggests that there is reason to expect that the conditions that prevail in many countries, especially poverty, make having a great deal of both peace and democratization very unlikely if not impossible. Snyder argues that too much democracy, meaning universal or widespread suffrage, too fast leads to the most violent and destructive types of nationalist conflict. The author traces causal processes within and between historical cases of democratization, such as Britain, France, Germany, the Balkans, Malaysia and Sri Lanka, and contemporary cases of democratization, such as India, Russia, Yugoslavia, Rwanda, and Burundi to test the plausibility of the argument. While Snyder raises many important points that serve as a caution to those

who would accept democratic peace theory and helps the reader understand why in theory democratization may cause nationalist conflict, in this reader's view Snyder's test lacks the empirical rigor that would allow the reader to join the author in dismissing the possibility of having both democratization and peace in relatively poor countries.

Snyder does a fine job of clearly presenting his argument, which he calls the elite persuasion argument, and explaining its underlying logic (pp. 31-32). Snyder notes that nationalist conflict is not necessarily violent and argues that whether nationalist conflict is rather subdued or results in violence depends largely on the level of economic development in a country, the extent to which the careers of political elites who rule under an authoritarian regime are threatened by democratization, and the strength of political and social institutions (pp. 38-39). In short, Snyder argues, à la Seymour Martin Lipset (1959) that it is very unlikely that poor countries that long have been under authoritarian rule will democratize without violent nationalist conflict. As pressure to democratize mounts, suffrage is extended widely or universally, and elections are convoked, political elites who have benefited from authoritarian rule become threatened and are more likely to use nationalist appeals and scare tactics to drum up electoral support. In a poorly developed country, where educational attainment is low, political

elites are more likely to use nationalist appeals than in more developed settings because these citizens are more susceptible to such nationalist appeals than those in more developed political settings. Further, the scare tactics based on ethnic or national identity used by political elites are more likely to result in violence than where there is a higher level of economic development and the citizenry is generally better educated.

Snyder does a very good job of comparing and contrasting several country cases in order to show that there is a correlation between the pace of democratization, largely defined as the rapidity with which suffrage was expanded, and the type and intensity of nationalist conflict that manifested itself. For example, where suffrage, political rights and civil liberties expanded gradually, as in Britain, Snyder convincingly argues that nationalism was less belligerent, less violent and less destructive than in France and Germany, where suffrage, political rights and civil liberties expanded more suddenly (pp. 76-79). Although Snyder's portrayal of events in Rwanda and Burundi is generally less convincing, he does show that there is a correlation between the pressure for power sharing in those countries and the intensity of ethnic nationalist conflict in Rwanda and Burundi.

However, as the old adage goes, correlation is not causation, and Snyder does not provide the kind of evidence to confirm that the relationship

between the pace of democratization and nationalist conflict is in fact a causal one or that it is causal in the direction that he proposes. It was as if Snyder noted that where there is nationalist conflict there is democratization and therefore concluded that democratization must cause nationalist conflict without adequately recognizing that there also has been nationalist conflict and aggressive international behavior where there has been little in the way of democratization, such as in Western Europe before Britain and France democratized and, more recently in the Sudan since its independence and Iraq since the coming to power of Saddam Hussein. In

other words, democratizing countries do not have a monopoly on nationalist conflict. While this need not destroy Snyder's argument, Snyder needs to explain more adequately the cases that do not seem to fit well with the elite persuasion argument.

It also should be noted that it is possible that, rather than democratization causing nationalist conflict as Snyder proposes, democratization may in fact be the result of protracted and destructive nationalist conflict over control of the state. This at first may seem like an odd argument. However, it is important to note that the world's oldest and most consolidated democracies were plural societies and experienced nationalist conflict within their borders and wars with nations beyond their borders long before democratization. Minorities almost always resisted majorities or vice versa, depending on which was in power, and occupied nations almost always sought to throw off occupying nations. It is possible that democratization was set in motion because political elites of various nations and/or ethnic groups gradually recognized that the costs of the nationalist violence were outweighing the benefits and that in order to promote peace and the wealth that comes with it there would need to be some kind of power sharing arrangement among them. Such an arrangement, in order to last, would need some kind of popular approval. Snyder would have done well at least to address this possibility.

Snyder's argument would be strengthened if he had more thoroughly tested the elite persuasion

argument against rival explanations for the rise of nationalist conflict. Snyder uses his very thorough case studies to show that the elite persuasion argument is plausible, but the reader is left wondering: plausible compared to what? Some serious treatment of rival arguments about the rise of nationalist conflict would have helped.

In spite of the shortcomings, Snyder's work stimulates thought and should serve as a caution to those who uncritically accept the democratic peace theory. The book is a must read for those interested in critically assessing the democratic peace theory and those interested in explaining the variation in nationalist conflict within and between states. It is reasonable to think that nationalist conflict is likely to accompany democratization. When the vast majority of the people who long have been disenfranchised suddenly become enfranchised, we can expect there to be a great deal of tumult and instability. However, it is important to ask oneself whether the tumult and instability attributed to expanded suffrage and democratization by Snyder would have been even greater without the expansion of suffrage and democratization. ❖

Robert A. Dowd is an advanced graduate student in the Department of Political Science at UCLA and currently a Visiting Scholar at the Helen Kellogg Institute for International Studies, University of Notre Dame.

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has changed the questions that are asked. Comparativists who might welcome new ways to think about the questions they have spent their careers studying have less reason to adopt new conceptual tools that only appear to be helpful in studying topics they find uninteresting.

Again, both sides have a point. There is no substitute for the precision of mathematics in determining the conclusions that follow logically from a set of assumptions. The work of the formal theorist is to eliminate ambiguity and to simplify until anyone can check the validity of the deductive argument. If doing so leads to new research questions, so much the better. The introduction of new questions is an important way in which science advances. It is a gain for comparative politics that there is much more research today on the comparative organization of legislatures or on national differences in how votes are aggregated than there was ten years ago.

On the other side of the ledger is the stubborn fact that few applications of formal theory have yielded new insights sufficiently powerful to have a significant impact on the study of the questions that concern most non-mathematical comparativists. Of course, new applications of formal theory are continually being developed and no one can predict where the next advance will occur. But the fact remains that applications of formal theory are far less pervasive in comparative politics than in American politics. In part, this is due to the natural inclination of mathematically trained scholars

to focus first on the country they know best, which is usually the United States. But in part, the limited use of formal theory in comparative politics reflects the nature of the questions comparativists have asked. The big questions in comparative politics, such as the origins and survival of democratic and authoritarian regimes or the role of the state in promoting (or retarding) economic growth or income equality, are complex and difficult to specify with the precision that mathematical models require. One reasonable response is to limit one's research to narrower topics that can be studied in a rigorous fashion. Another, equally reasonable response is to study the big questions, doing the best one can, on the grounds that the questions are too important for human welfare to ignore.

The recognition that quantitative and non-quantitative work have different advantages and disadvantages has led some to argue that the best approach is to do both. Such advice, however, ignores the potential benefits of a division of labor. While I believe that the field will advance most rapidly with a combination of quantitative and non-quantitative work, it doesn't follow that each individual in the field should try to be a master of both trades. What is most important for the future of the field is for each of us to make the effort to read articles and attend seminars by scholars working on the other side of the divide. The potential benefits of the division of labor cannot be realized if producers resist trading with one another. This implies that producers of

non-quantitative work learn enough formal theory and statistics to understand the general principles that underlie the work of their more quantitative colleagues. It also implies that producers of quantitative work read enough of the non-quantitative work to understand the questions that motivate their less quantitative colleagues. In my vision of a healthy discipline, some specialize in quantitative work, some specialize in non-quantitative work and some do both, but all learn enough of each other's language to be able to participate in a common conversation. ❖

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Professor Daniel Treisman, Editor
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University of California, Los Angeles
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