

# POLITICS AND THE LIFE SCIENCES

POLITICS AND THE LIFE SCIENCES

**Editorial Farewell:** Gary R. Johnson

**The Politics of a Scientific Meeting: The Origin-of-AIDS  
Debate at the Royal Society**

Brian Martin

**The Age of Transition to Sustainability: The End of the  
Exponential Growth Period**

John Cairns, Jr.

**Stuck in the Pleistocene: Rationality and Evolved Social Roles**

Derek Reiners

**Who Favors Legalizing Physician-Assisted Suicide? The Vote  
on Michigan's Proposal B**

John Strate, Timothy Kiska, and Marvin Zalman

**A Critique of the Naturalistic Fallacy Thesis**

Jan Tullberg and Birgitta S. Tullberg

**U.S. Chemical Program: Purpose, Challenges, and Evolution**

Odelia Funke

**Culture, Social Minds, and Governance in Evolution**

Claude S. Phillips

**The Homeopathy of Kin Selection: An Evaluation of van den  
Berghe's Sociobiological Approach to Ethnic Nepotism**

Ingo Brigandt

**Population and the Demise of Cheap Energy**

Andrew R.B. Ferguson

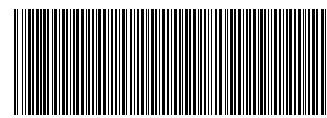
---

**Book reviews** on arguments against evolutionary psychology, the history of motherhood, marine law and ocean policy, the science of human emotions, a theory of everything, and the mismatch between our primal instincts and contemporary industrial societies

---

Abstracts, author affiliations:  
see back cover  
ISSN 0730-9384

Published by the Association for Politics and the Life Sciences



0730-9384(200109)20:2;1-2

# POLITICS AND THE LIFE SCIENCES

---

---

*Politics and the Life Sciences* (ISSN 0730-9384) is the journal of the Association for Politics and the Life Sciences. It is published semi-annually in March and September.

## EDITOR

---

### **Gary R. Johnson**

Department of Political Science  
Lake Superior State University  
650 W. Easterday Avenue  
Sault Ste. Marie, MI 49783-1699, USA

## BOOK REVIEW EDITORS

---

### **Andrea L. Bonnicksen**

Department of Political Science  
Northern Illinois University (USA)

### **Joseph Losco**

Department of Political Science  
Ball State University (USA)

## ASSISTANT TO THE EDITOR

---

### **Judy Bawks**

### **Suzette Olson**

### **Jeanne Shibley**

Lake Superior State University (USA)

## EDITORIAL ASSISTANTS

---

### **Loren Ayotte**

### **Kelly Bishop**

### **Charles Bopp IV**

### **Erik Fredericks**

### **Mark Heimonen**

### **Rebecca Hunt**

### **Jason King**

### **Winston Reeves II**

Lake Superior State University (USA)

---

*Politics and the Life Sciences* is indexed in ABC POL SCI; Article 1st; Biology Digest; BIOSIS/Biological Abstracts; Contents 1st; Current Biotechnology Abstracts; Current Contents: Social and Behavioral Sciences; Expanded Academic Index; Index to Periodical Articles Related to Law; International Bibliography of Sociology; International Political Science Abstracts; Social Science Citation Index; Social Sciences Abstracts; Sociological Abstracts; and Wilson Select Plus.

## EDITORIAL ADVISORY BOARD

---

### **George J. Annas**

Schools of Medicine and Public Health  
Boston University (USA)

### **Laura Betzig**

Museum of Zoology  
University of Michigan (USA)

### **William P. Brandon**

Policy House  
Univ of North Carolina, Charlotte (USA)

### **Arthur L. Caplan**

Center for Bioethics  
University of Pennsylvania (USA)

### **Marie Isabelle Chevrier**

School of Social Sciences  
University of Texas at Dallas (USA)

### **Peter A. Corning**

Institute for the Study of Complex  
Systems  
Palo Alto, California (USA)

### **Sherman Elias**

Department of Obstetrics and Gynecology  
University of Illinois at Chicago (USA)

### **Gary B. Ellis**

Office for Protection from Research Risks  
National Institutes of Health (USA)

### **Vincent S.E. Falger**

Department of International Relations  
University of Utrecht (The Netherlands)

### **John C. Fletcher**

Center for Biomedical Ethics  
University of Virginia (USA)

### **Heiner Flohr**

Institute of Social Science  
University of Dusseldorf (Germany)

### **Robin Fox**

Department of Anthropology  
Rutgers University (USA)

### **Henry R. Glick**

Department of Political Science  
Florida State University (USA)

### **Kathi E. Hanna**

Staff  
National Bioethics Advisory Commission  
(USA)

### **Robert A. Hinde**

St. John's College  
Cambridge University (UK)

### **Bartha Maria Knoppers**

Faculty of Law  
University of Montreal (Canada)

### **Deborah Mathieu**

Department of Political Science  
University of Arizona (USA)

### **Mary Maxwell**

Independent Scholar  
Adelaide (Australia)

### **Linda Mealey**

Department of Psychology  
College of St. Benedict (USA)

### **Ellen H. Moskowitz**

Associate  
Proskauer Rose LLP (USA)

### **Thomas H. Murray**

President  
The Hastings Center (USA)

### **John Orbell**

Department of Political Science  
University of Oregon (USA)

### **Graham S. Pearson**

Department of Peace Studies  
University of Bradford (UK)

### **Jerrold M. Post**

Political Psychology Program  
George Washington University (USA)

### **Mark E. Rushefsky**

Department of Political Science  
Southwest Missouri State University  
(USA)

### **David Shapiro**

Independent Scholar  
London (UK)

### **John Strate**

Department of Political Science  
Wayne State University (USA)

### **Jonathan B. Tucker**

Center for Nonproliferation Studies  
Monterey Inst. of Int'l. Studies (USA)

### **Frans B.M. de Waal**

Yerkes Regional Primate Research Center  
Emory University (USA)

### **John C. Wahlke**

Department of Political Science  
University of Arizona (USA)

### **Elliott S. White**

Department of Political Science  
Temple University (USA)

### **Susan Wright**

Residential College  
University of Michigan (USA)

### **Raymond A. Zilinskas**

Center for Nonproliferation Studies  
Monterey Inst. of Int'l. Studies (USA)

# POLITICS AND THE LIFE SCIENCES

SEPTEMBER 2001

CONTENTS

VOLUME 20, NUMBER 2

ISSN: 0730-9384

Appearing in Print September 2005

[Delayed for reasons explained in September 2000 (19, 2):211-245]

## EDITORIAL FAREWELL

Politics and the Life Sciences: A Second Decade and a Continuing Mission  
*Gary R. Johnson*..... 109

## ARTICLES

The Politics of a Scientific Meeting:  
The Origin-of-AIDS Debate at the Royal Society  
*Brian Martin* ..... 119

The Age of Transition to Sustainability:  
The End of the Exponential Growth Period  
*John Cairns, Jr.*..... 131

Stuck in the Pleistocene: Rationality and Evolved Social Roles  
*Derek Reiners*..... 139

Who Favors Legalizing Physician-Assisted Suicide?  
The Vote on Michigan's Proposal B  
*John Strate, Timothy Kiska, and Marvin Zalman* ..... 155

A Critique of the Naturalistic Fallacy Thesis  
*Jan Tullberg and Birgitta S. Tullberg*..... 165

U.S. Chemical Program: Purpose, Challenges, and Evolution  
*Odelia Funke*..... 175

Culture, Social Minds, and Governance in Evolution  
*Claude S. Phillips*..... 189

The Homeopathy of Kin Selection: An Evaluation of van den Berghe's Sociobiological Approach to Ethnic Nepotism  
*Ingo Brigandt*..... 203

## COMMENTARY

Population and the Demise of Cheap Energy  
*Andrew R.B. Ferguson* ..... 217

## CONTINUING DIALOGUE

Darwinism, Dominance, and Democracy:  
A Reaffirmation  
*Albert Somit and Steven A. Peterson* ..... 227

The Sociobiology of Democracy Revisited:  
A Reply and Reiteration  
*Peter A. Corning* ..... 231

## BOOK REVIEWS

Alas, Poor Darwin: Arguments Against Evolutionary Psychology  
(Hilary Rose and Steven P.R. Rose, eds.)  
*R. Elisabeth Cornwell, Craig T. Palmer, and Hasker P. Davis*..... 235

Listening to the Sea: The Politics of Improving Environmental Protection  
(Robert Jay Wilder)  
*Michael M. Gunter, Jr.* ..... 240

Why We Feel: The Science of Human Emotions  
(Victor S. Johnston)  
*Michael Hammond*..... 242

Mean Genes: From Sex to Money to Food:  
Taming Our Primal Instincts  
(J. Burnham and J. Phelan)  
*Nicole C. Hess and Edward H. Hagen*..... 244

Mother Nature: A History of Mothers, Infants, and Natural Selection  
(Sarah Blaffer Hrdy)  
*Laurette T. Liesen*..... 246

A Theory of Everything: An Integral Vision for Business, Politics, Science, and Spirituality  
(Ken Wilber)  
*Kay Phillips and Mostafa Rejai* ..... 248

# ASSOCIATION FOR POLITICS AND THE LIFE SCIENCES

## 2000-2001

---

---

### Executive Director

---

**Gary R. Johnson**  
Department of Political Science  
Lake Superior State University  
Sault Ste. Marie, MI 49783-1699  
USA

**Andrea L. Bonnicksen**  
Northern Illinois University (USA)

**Peter Corning**  
Institute for the Study of Complex Systems (USA)

**Vincent Falger**  
University of Utrecht (The Netherlands)

### Council

---

**Ira H. Carmen** (Chair)  
University of Illinois (USA)

**Samuel M. Hines**  
College of Charleston (USA)

**Robert H. Blank** (Vice-Chair)  
Brunel University (United Kingdom)

**Bruce Jennings**  
The Hastings Center (USA)

**Steven A. Peterson** (Secretary)  
Penn State University, Harrisburg (USA)

**Joseph Losco**  
Ball State University (USA)

**Larry Arnhart**  
Northern Illinois University (USA)

**Roger D. Masters**  
Dartmouth College(USA)

**Johann M.G. van der Dennen**  
University of Groningen (The Netherlands)

For more information about *this issue of PLS*, contact Gary R. Johnson, Department of Political Science, Lake Superior State University, Sault Ste. Marie, MI 49783-1699, USA; telephone +1-906-635-2763; fax +1-906-635-2111; e-mail GJOHNSON@LSSU.EDU.

## EDITORIAL FAREWELL

# Politics and the Life Sciences: A Second Decade and a Continuing Mission

Gary R. Johnson, Editor

Lake Superior State University, USA

With this issue of the journal, my ten-year term of service as editor of *Politics and the Life Sciences* comes to an end. As is customary for editors in their final issues, I want to review our work, express thanks, and offer some thoughts about the journal, the association, and the field.<sup>1</sup>

### The First Decade

My term of service covered the journal's second decade. The first decade began with the founding of the journal in 1981 as the scholarly publication of the Association for Politics and the Life Sciences (APLS). APLS itself had been established in 1980 in order to "advance interest in and encourage scholarship about biopolitics" (*Politics and the Life Sciences* 1:2).

The founding editor of the journal (and founding executive director of the association) was the late Thomas C. Wiegale. Many current members did not have the privilege of knowing Tom, but we continue to owe him an enormous debt for his ten years of visionary work as the journal's first editor and the association's first executive director.<sup>2</sup>

In his introduction to the first issue of *PLS*, Tom predicted that "the body of biopolitical scholarship will expand dramatically" (1982:3). He was right, and the journal under his leadership played a major role in making that happen. With the able assistance of Carol Barner-Barry as Book Review Editor (beginning with the second issue, in 1983), and Robert Blank as Associate Editor (beginning in 1986), Tom and the journal provided an invaluable outlet for biopolitical research and a forum that facilitated scholarly communication across disciplinary boundaries.<sup>3</sup>

Of course, establishment of the journal and the association was not a one-person effort. Tom may have been the "preeminent founder" (Caldwell, 1992:95), but many others also made important contributions. Tom recognized those individuals in his first editor's introduction (1982) and in his own editorial farewell (1991). We recently honored twelve of these pioneering individuals with APLS Founder

Awards: Carol Barner-Barry, Lynton K. Caldwell, Peter Corning, Samuel Hines, Fred Kort, Roger Masters, Steven Peterson, Glendon Schubert, James Schubert, Albert Somit, John Wahlke, and Meredith Watts.

Those of us who have benefited from the visionary and innovative efforts of Tom and these other founders cannot express our gratitude too many times. Therefore, on behalf of the association, the authors whose work has been published in the journal, and all of us whose professional lives have been enriched in one way or another by the journal and the association, we say, once again, thank you.

### The Second Decade

The journal made great strides in its first decade. Our aim when the editorial office moved here in 1991 was to build upon the great work that had already been done, adjust to some changed conditions, and take advantage of new opportunities (Johnson, 1992).

Our broadest goal, as might be expected, was to continue improving the journal's quality and reputation. Related to that, but also related to the broader mission of promoting the field, we hoped to publish more contributions. We also hoped to make the journal even more broadly interdisciplinary and more international.

### Staff and Board

A good editorial staff would be the first step toward reaching these goals. I was delighted that two excellent and creative scholars, Andrea Bonnicksen and Joseph Losco, agreed to join the journal as Book Review Editors. Andrea and Joe remained in those positions throughout the journal's second decade, and both did an outstanding job (a subject to which I will return). I also frequently relied on them for advice on other matters. I am deeply grateful to both for their wise counsel and their hard work.

Joining the journal as Bibliography Editors were Vincent Falger, Brian Gladue, Janna Merrick, and Raymond Zilins-

kas. Deborah Mathieu stepped in for Janna Merrick in 1998 when other obligations forced Janna to step down. These five performed a valuable service for the members of the association by sending contributions that collectively became our “Recent Books and Articles” feature. Because it was so difficult to keep track of the relevant new literature across diverse fields, this feature had been an important part of the journal since its first issue, and remained so until 2000.<sup>4</sup> I am grateful for the efforts of our Bibliography Editors in putting together their contributions, and I am also grateful for the advice they were always willing to share when I called about other matters.

We tried to take another step toward reaching our goals with the appointment of a high-quality Editorial Advisory Board that represented numerous disciplines and multiple countries. I was pleased that a distinguished group of scholars and scientists, representing ten different disciplines and eight different countries, accepted our invitations to join the board.

Those who served at least one term on the board during this period were George Annas, Larry Arnhart, Carol Barner-Barry, Laura Betzig, Robert Blank, William Brandon, Arthur Caplan, Ira Carmen, Marie Chevrier, Peter Corning, Sherman Elias, Gary Ellis, Vincent Falger, Martha Field, John Fletcher, Heiner Flohr, Norman Fost, Robin Fox, Odelia Funke, Henry Glick, Kathi Hanna, Robert Hinde, Bartha Knoppers, Fred Kort, Gordon Lake, Bobbi Low, S.J. Lundin, Douglas Madsen, Deborah Mathieu, Mary Maxwell, Linda Mealey, Ellen Moskowitz, Thomas Murray, John Orbell, Graham Pearson, Jerrold Post, William H. Rodgers, Mark Rushefsky, David Shapiro, Barbara Smuts, John Strate, Jonathan Tucker, K. Venkataraman, Frans de Waal, John Wahlke, Elliott White, Susan Wright, and Raymond Zilinskas.

Membership on the board varied somewhat over the decade (as reflected in the journal’s mastheads), and members played different roles. Whatever the role, however, and whether it was for one term or the entire decade, members of the board made important contributions to the journal’s success. I was pleased throughout to have such a distinguished and conscientious group to which to turn for advice. I thank all of them for their service.

### *Diversity*

Our efforts to become more international and even more interdisciplinary went beyond the appointment of an international and interdisciplinary Editorial Advisory Board. We also made special efforts to encourage manuscript submissions from other countries and disciplines, as well as to invite commentaries and book reviews from authors in a wide range of countries and disciplines. During this decade, 28 different countries and more than 70 disciplines were represented in the pages of the journal.

This national and disciplinary diversity was, in my view, a great strength. It brought perspectives from different parts

of the world and from a host of different disciplines. We also sought to bring diversity to the pages of the journal by being open to perspectives that are unpopular, either among our own members, or among those in the broader scholarly community. It was prudent to be cautious when we dealt with emotionally charged issues, but we sought to be open to diverse perspectives and critical commentary, even when it was directed at our own enterprise. Indeed, we often *invited* contributions from scholars and scientists who have been critical of efforts to use the life sciences in the study of human behavior.

### *Commentary Format*

The article/commentaries/response format that was a trademark of the journal in its first decade is especially well-suited to bringing forward diverse perspectives. Such diverse perspectives are valuable, as Tom Wiegele put it, because “criticism and dialogue are a necessary part of intellectual growth” (1982:3). Or, as John Stuart Mill put it—in a quote I always use as an epigraph on my political philosophy syllabi—“He who knows only his own side of the case knows little of that” (1974:98). By seeking out diverse perspectives and presenting them in one place, a journal may generate an intellectual synergy that could otherwise be achieved, if at all, only over a much longer period and across multiple publications.

In the journal’s first decade, the commentary format was used for most articles. We chose to use this format more selectively—reserving it for articles for which it was especially well suited—so that we could publish more articles and so that we could seek more commentaries and from a more diverse set of contributors. When we were able to publish an article, commentaries, and response in the same issue, we referred to the entire package as a “roundtable.” When practical considerations forced us to publish the commentaries and response in a succeeding issue, we referred to the collection in the succeeding issue as a “symposium.”

Several of our issues had no roundtables or follow-up symposia, but several had two, and one—the February 1994 issue—had three. With 20 issues and a total of 22 roundtables/follow-up symposia, we averaged slightly more than one per issue.<sup>5</sup> A few roundtables included only three commentaries, but our follow-up symposium on Lynton K. Caldwell’s “Is Humanity Destined to Self-Destruct?” (September 1999) included 28 commentaries. The roundtable on Jonathan Tucker’s “Chemical/Biological Terrorism” (September 1996) included 24. Together, our 22 roundtables/follow-up symposia included 252 commentaries, for an average of more than 11. I want to express my sincere thanks to the commentators, among whom are included some of the world’s most distinguished scientists and scholars.

The article/commentaries/response format is time-consuming and complicated. However, as I have indicated, I

believe there is great potential payoff from bringing diverse perspectives together. To the extent that time and finances permit, I hope the journal can continue using this format in the future.

#### *Other Symposia and Other Contributions*

In addition to roundtables and follow-up symposia, we also published collections of articles and essays that did not involve invited commentaries on an article. Thus, Raymond Zilinskas brought together an excellent symposium on biological weapons inspection in Iraq (August 1995). Susan Wright and Richard Falk organized a similarly excellent symposium on the challenges of biological warfare (March 1999). And, with generous advice from David Wasserman, who had organized a conference on the subject, I put together a symposium on “Genetics and Crime” (March 1996).

When it seemed appropriate, we also took the initiative and invited (or welcomed) other timely contributions. These took various forms, including stand-alone commentaries (such as the commentaries on cloning by Andrea Bonnickson and Susan Wright in the September 1997 issue), follow-up commentaries (“continuing dialogue”), subject “updates,” organizational and institutional “profiles,” conference reports, and special reports. We also tried to stimulate the development of new courses in the field by publishing “teaching foci” that reviewed interdisciplinary courses taught by our members.<sup>6</sup> We sought to continue and extend these efforts by inviting “teaching posters” at the annual meetings in 1999 and 2000.

#### *Book Reviews*

The journal’s standard book review format from its first issue in 1982 through our first two issues in 1992 was a multiple review format. A précis of a book was followed by multiple reviews, usually three. This was a stimulating format, and it was especially valuable in the journal’s early years. It brought diverse perspectives even to the matter of reviewing books.

It was with considerable reluctance, then, that we decided to discontinue multiple reviews as our standard format. By the time the editorial office moved here, many more books were being published in the field. If we had continued with multiple reviews for all books, we would either have been limited to reviewing a small proportion of the books being published, or we would have had to devote half or more of each issue to book reviews. Beginning with the February issue in 1993, then, we began publishing single reviews of a larger number of books.<sup>7</sup>

Over the course of the decade, we reviewed 260 books, with a total of 287 reviews (and two author responses to roundtable reviews). In other words, we averaged over 14 reviews per issue. As I looked back over our twenty issues, I was reminded of the extraordinary job that Andrea Bonnick-

sen and Joe Losco did as book review editors. They successfully reviewed many of the important books published during this period, and across all of the topic areas we wished to cover. They also brought us book review authors from many countries and from diverse disciplines. And finally, they also succeeded in getting book reviews both from scholars in the early stages of their careers and from scholars who are among the established leaders in their fields. They did a superb job. I hope *PLS* readers will express their personal thanks to Andrea and Joe for the wonderful work they did over the journal’s second decade.

#### *Topics*

From the very beginning, the field of politics and the life sciences was conceptualized as encompassing both basic science and policy analysis, as well as relevant philosophical analysis (Blank, 1982:38; Hines, 1982:6). As the scholarly journal for the field, then, the scope for *Politics and the Life Sciences* would be any problem or issue that involved politics or public policy and any one (or more) of the life sciences.

We attempted to follow this broad definition of the field in editing the journal. Thus, we tried to cover a broad range of appropriate scientific subjects—empirical, theoretical, and philosophical. Our subjects have included, among many others, Supreme Court oral argument, ethical naturalism, feminism and sociobiology, political cooperation, stress and political leadership, ethnic conflict, conservation attitudes, politics and reproduction, male age distribution and war, treatment of political prisoners, crime control strategies, the origins of monogamy, kin term usage in political rhetoric, threat and political tolerance, and ritual deception in politics.

We also sought to cover a broad range of appropriate policy subjects. Included here, among others, were contributions on cloning, fetal protection, prenatal diagnosis, donor insemination, controlling biological weapons, biodiversity, gene therapy, acid rain, transgenesis in farm animals, BWC verification, biotechnology policy, chemical and biological terrorism, physician-assisted suicide, crime control, reducing ethnic conflict, assisted reproduction, population control, the human genome diversity project, immigration policy, and xenotransplantation.<sup>8</sup>

I am especially proud of the role that *PLS* played during much of its second decade as one of the principal outlets in the world for scholarly work on biological weapons and biological warfare. Tom Wiegele, the journal, and the association recognized the importance of this subject long before it was recognized by many others, and we sought to play an active role in encouraging scholarship in this area. Since I had little background in the field myself, I want to especially thank Raymond Zilinskas, Marie Chevrier, and Jonathan Tucker for their advice and their hard work in facilitating our efforts (not to mention their own articles and essays).

### *Review Policies and Referees*

PLS is a refereed journal, which means that its main articles have to pass muster with peer reviewers before they are published. Referees perform an essential role, both in advising the editor and in advising manuscript authors. This is an especially important role for an interdisciplinary journal, because its authors are necessarily working at the boundaries of two or more disciplines. For that reason, it was not unusual for us to send an empirical or theoretical manuscript to a political scientist, a biologist, a psychologist, and an anthropologist or sociologist. A policy-oriented manuscript might go to a political scientist, a biologist, a physician, and a lawyer or philosopher (or perhaps both).

I offer my sincere thanks to the hundreds of referees, from many disciplines, who served the journal so well over the last ten years. They are largely unsung benefactors who made a profound difference in the quality of the journal. They served not only as assistant gatekeepers, but even more importantly, as coaches who helped not-yet-publishable manuscripts with potential become publishable, and helped already publishable manuscripts become even better. I salute all of you.

### *Copyediting*

An accepted manuscript, of course, must still be copyedited. I have been especially proud of our copyediting work during this decade. Authors regularly offered praise for the careful, extensive copyediting that they thought improved their manuscripts. Good copyediting was important not only for all of the obvious reasons that relate to journal quality, but also because we were attempting to facilitate communication across disciplinary and national boundaries. We wanted to make the journal as readable as possible, despite the diverse disciplines and diverse linguistic/cultural/national backgrounds from which our authors and readers came.

I discovered when I became editor that contract copyeditors are normally not listed on the masthead of a journal. I followed that custom. However, I want to acknowledge here the great work of Nancy Steinhaus, who did the initial round of copyediting on most of the manuscripts we published in ten years of issues. Nancy was careful, diligent, and creative in all of her work. I and the authors whose work we published owe her a great debt of gratitude. Thank you, Nancy.<sup>9</sup>

### *Additional Diversity and Total Contributions*

I have already mentioned the great national and disciplinary diversity among our authors. There was also, however, a great deal of career and institutional diversity. Thus, our authors included some of the world's best-known scholars and scientists, but they also included graduate students and others who were publishing for the first time. Our authors

also came from hundreds of different institutions of higher education, ranging from large research universities to small private colleges.

Our authors also came from a wide range of research, policy, and governmental organizations, including, among many others, the Australian Museum, Brookings Institution, Chemical and Biological Defense Establishment (UK), Chinese Academy of Social Sciences, Dana Farber Cancer Institute, Environmental Protection Agency, European Group on Ethics in Science and Technology, General Accounting Office, Hastings Center, Howard Hughes Medical Institute, Human Rights Watch, Institute of Medicine, Lawrence Livermore National Laboratory, Los Alamos National Laboratory, Maryland Biotechnology Institute, Max Delbrück Center for Molecular Medicine (Germany), National Advisory Board on Ethics in Reproduction, National Center for Human Genome Research, National Defense Headquarters (Canada), National Institutes of Health, Nehru Memorial Museum and Library (India), Neurosciences Institute, Nuffield Council on Bioethics (UK), Office of Management and Budget, Organization for the Prohibition of Chemical Weapons (Netherlands), Permanent Mission of the Russian Federation to the Conference on Disarmament, Population Reference Bureau, RAND Corporation, Sandia National Laboratories, Stockholm International Peace Research Institute, Tel Aviv Medical Center, U.S. Arms Control and Disarmament Agency, U.S. Army Medical Research Institute of Infectious Diseases (USAMRIID), United Nations Special Commission (UNSCOM), World Bank, World Conservation Union (Switzerland), and World Health Organization.

Altogether, over the course of the decade, we published a total of 768 authored contributions. When we add "Recent Books and Articles" and "New and Announcements" contributions, we published a total of 797 contributions, or an average of just under 40 per issue. It was my great pleasure to be able to work with so many different authors from so many different backgrounds. Almost all were congenial, conscientious, and cooperative. I offer my sincere thanks to these authors—and to everyone who submitted manuscripts for our consideration—for sharing their work with us and for being such a pleasure to work with.

### *One Regret*

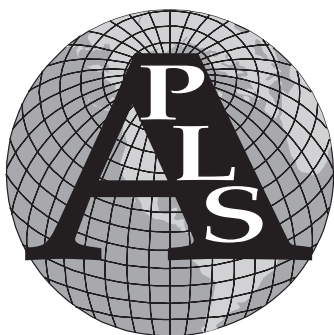
There are daunting challenges in producing a quality journal that is both broadly interdisciplinary and international. I think we met those challenges reasonably well. My only significant regret regarding this decade of effort concerns the indirect consequences that followed my decision to resist an effort to compromise our editorial independence—especially the great publication delays for this and the preceding two issues (see Johnson, 2000). I do not mean that I regret the decision itself; indeed, I am proud of the stand we took on behalf of open communication and criticism in science and scholarship. However, as often happens when individuals

or organizations take a stand on behalf of a public good, a variety of costs must be borne.

We have already received some generous donations and other assistance to help defray those costs. My hope is that those who believe in open communication in science and scholarship will provide additional assistance that will help the journal and association absorb more of those costs and recover ground that has been lost as a result of the principled position we took.<sup>10</sup>

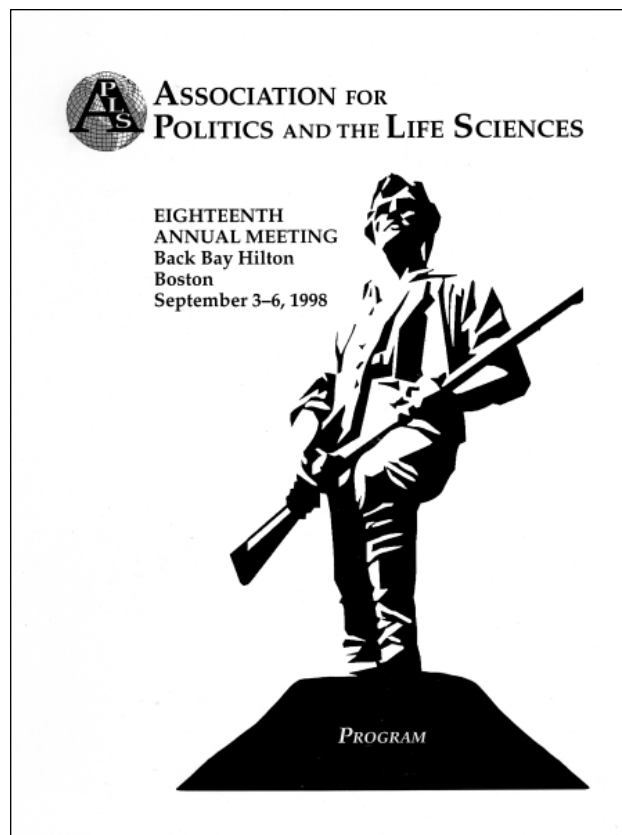
### Association

It was also my privilege during this decade to spend five years serving as APLS Executive Director (1996-2001). Our principal accomplishment during this period was the establishment of an independent annual meeting for the association. Those who helped in so many ways with these meetings were thanked in the programs for those conferences and/or at the conference banquets, but I wish to express again my great appreciation for the wonderful work performed by all of those who contributed in a variety of ways to the success of these meetings.



### Conferences

Since its founding in 1981, APLS had held its meetings as part of the meetings of the American Political Science Association (APSA), either as a “related group” or as a section. We are enduringly grateful to APSA for providing the organizational umbrella under which our fledgling organization could mature. However, the association would not be able reach its full potential under this arrangement. APSA could allow us only a limited number of events, and one needed to be a member of APSA to receive the hotel discount. This made it impractical for our many non-political scientist members to attend the meeting. Without a meeting that brought members together in one place to make presentations, have discussions, and share meals and coffee, the association could not achieve the cohesion it needed to mature.<sup>11</sup> It therefore became clear that we needed to establish our own, independent meetings. Given the organizational challenges, our office served as both organizer and program chair for the first three of these meetings, and organizer for the fourth.



Our inaugural independent meeting took place September 3-6, 1998 at the Back Bay Hilton in Boston. Edward O. Wilson, Pellegrino University Research Professor and Honorary Curator in Entomology at the Museum of Comparative Zoology at Harvard University, was the keynote speaker. As I noted in my conference report, this was a “landmark event” for APLS:

Had we continued to meet with APSA in 1998, we would have been allowed a total of 6 events [two panels, three meetings, and a reception]. These events would have attracted about 25-30 participants, most of whom would have been political scientists from the United States. By contrast, the independent meeting featured 70 events—a keynote address, seven plenary lectures or roundtables, 43 panels and roundtables, four meetings, seven coffee breaks, four breakfast buffets, three lunch buffets, and a banquet—and attracted 219 registered participants. The participants represented 11 countries and more than 30 different fields of study. (Johnson, 1998:209)

Our 1999 meeting was held September 2-5 at the Four Seasons Hotel in Atlanta, where Frans de Waal—C. H. Candler Professor of Primate Behavior at Emory University, and Director of the Living Links Center for the Advanced Study of Ape and Human Evolution at the Yerkes Regional Primate

Research Center—was the keynote speaker. In addition to a great program, highlights included pre-conference tours of the Field Station of the Yerkes Regional Primate Research Center and the bioterrorism facilities at the Centers for Disease Control (CDC).

The 2000 meeting was held August 31 to September 3 at the Washington Marriott Hotel in Washington, DC. The keynote speaker was Lionel Tiger, Charles Darwin Professor of Anthropology at Rutgers University. Pre-conference tours were available to the National Zoo's Think Tank and Orangutan Language project, as well as to the U.S. Army Medical Research Institute of Infectious Diseases (USAMRIID).

Our 2001 meeting was held October 18-21 rather than over the usual dates (the U.S. Labor Day weekend), and on the beautiful campus of the College of Charleston rather than in a large convention hotel. This was a delightful change of pace, although we had to face numerous practical challenges in the aftermath of 9/11 and the U.S. anthrax attacks. Despite the challenges, we had an excellent meeting. The keynote speaker was Francis Fukuyama, Bernard Schwartz Professor of International Political Economy at the Paul H. Nitze School of Advanced International Studies at Johns Hopkins University. I offer our thanks, once again, to Janna Merrick for serving as program chair and to Samuel Hines for serving as our local host. Incidentally, those who attended that conference may remember, with a smile, a fire alarm in the middle of the night that brought many of us together at an unexpected time, in an unexpected place, and in unexpected attire.

*Newsletter*

The new independent conferences seem to have played an important role in helping bind our members together across disciplinary (and national) boundaries. As another effort directed toward that goal, we established an expanded newsletter for the association. *APLS News* carried association announcements; association news; brief articles and editorials; announcements of conferences, workshops, and seminars; announcements of new resources in the field; announcements of grants and fellowships; conference reports; lists of degrees awarded in the field; lists of dissertations in progress; lists of new books by members; news about member activities; previews of the next issue of *PLS*; short quotes from the recently published work of members; noteworthy quotes from other recent publications; and web reviews. Like the independent conference, the newsletter was designed, ultimately, to help bind our diverse membership together and stimulate new forms of collaboration. We were proud of the design and content of the first two issues (December 1996 and June 1997)—which totaled forty pages—and looked forward to publishing additional issues. Unfortunately, the time required to plan and oversee our new independent conferences made it impossible to continue with the newsletter.



**APLS NEWS**  
Newsletter of the Association for Politics and the Life Sciences

Volume 1      Numbers 1&2      December 1996

**Launching APLS News**

Welcome to *APLS News*, the new newsletter of the Association for Politics and the Life Sciences. We are pleased to launch this new benefit and tool for the members of APLS. This is a successor to the association's first newsletter—edited initially by Steven Peterson and James Schubert, and then by James Schubert—which appeared seven times over four years between 1991 and 1994. We are grateful to Steve and Jim for breaking this ground for the association.

Association newsletters serve multiple functions. They disseminate organizational news and announcements; they circulate news about the professional activities and accomplishments of members; they draw the attention of members to important new opportunities, information, and resources. *APLS News* will do all of these things. Most broadly, however, *APLS News* will help bind together the members of our diverse, international, interdisciplinary organization. This is perhaps a newsletter's most important function—building a sense of community among those with common interests. A sense of community strengthens an organization and stimulates new forms of collaboration among its members.

Our association has been growing and maturing in recent years. At the beginning of the year, individual memberships were up more than 100% over the previous four years; library subscriptions to *Politics and the Life Sciences* were up 15%—despite a library serials crisis that has reduced subscriptions to many journals; and the journal had been added to eight more indexes. Over this same period, *PLS* published contributions

from thirty-seven different disciplines and twenty different countries. The journal's articles also attracted attention in a variety of important policy forums, including the United Nations, the World Health Organization, the U.S. Congress, and the U.S. Arms Control and Disarmament Agency.

These developments have been highly gratifying to all of us. Nevertheless, there is still much untapped potential. We think *APLS News* will help us realize more of that potential, but we need your help. Please share with us your comments about this issue and your suggestions regarding future issues. The newsletter may be edited and published by the association's administrative offices, but it must of necessity always be a product of the members' collective efforts.

Gary R. Johnson  
Editor

**“APLS News will help bind together the members of our diverse, international, interdisciplinary organization.”**

**U.S. Bioethics Commission Appointed**

**Kathi E. Hanna**

I t has been nearly fifteen years since the United States government sponsored a focused effort to address the multiple bioethical issues facing the nation. A failed attempt in Congress in the late 1980s to establish a deliberative body was the last such effort. In 1995, the newly elected Republican Congress closed down its own Office of Technology Assessment, one of the few federally funded bodies that routinely considered such complex issues. Since the early 1980s there has been a void at the national level for the systematic discussion of such issues as assisted suicide, use of fetal tissue for therapeutic transplantation, use of human embryos in research, and ethical issues surrounding the use and misuse of genetic information.

Two years in the planning, the National Bioethics Advisory Commission (NBAC) met for the first time in October of 1996. NBAC was appointed by President Clinton to provide guidance to federal agencies on the ethical conduct of current and future human biological and behavioral research. According to its charter, NBAC shall advise, consult with, and make recommendations to the National Science and Technology Council, federal agencies, and other appropriate entities, and also make available to the public the commission's advice and recommendations. The commission's purview includes the ap-

*—continued on page 7*

**In This Issue**

U.S. Bioethics Commission Appointed	1
On Presidents and King	4
BWC Fourth Review Conference	8
Evolutionary Psychology Meets the Press	14
Conference on Male Violence	17

I still believe that a newsletter performs an important function, and would therefore recommend for the council's and membership's consideration that a new position be created for editor of *APLS News* (which I assume would now be an on-line newsletter). Neither the association's executive director nor the journal's editor is likely to have sufficient time for this task.

**Other Debts of Gratitude**

In the course of reviewing our decade of work, I have been able to thank many people who made important contributions. There are many others, however, who also made crucially important contributions, and who remain to be thanked.

*Council*

First, I would like to thank the members of the APLS council. Those who served one or more terms over this period were Larry Arnhart, Denise Baer, Carol Barner-Barry, Robert Blank, Andrea Bonnicksen, Ira Carmen, Peter Corning, Vincent Falger, Odelia Funke, Richard Hartigan, Samuel Hines, Bruce Jennings, Joseph Losco, Roger Masters, Janna Merrick, Steven Peterson, Glendon Schubert, James

Schubert, Albert Somit, Lionel Tiger, Johan van der Dennen, and Meredith Watts. Special thanks are due to three of the council chairs with whom I worked closely — Joe Losco, Sam Hines, and Ira Carmen; to Bob Blank, Andrea Bonnicksen, and Roger Masters, on whom I often relied for advice; and to Jim Schubert, who served as Executive Director during my first five years as editor.

I am deeply grateful to the council for their confidence and support during our ten years of work together. This was one of the most cooperative and congenial groups with which I have ever worked. I will miss the camaraderie and the sense of shared venture. Ladies and gentlemen, it was a great pleasure working with you.

#### *Publisher*

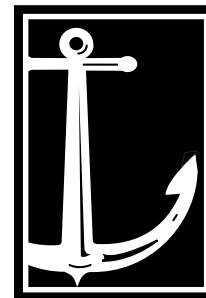
My first issue as editor of *PLS* was also the first issue for which the association itself would not be the publisher. Our new publisher, Beech Tree Publishing in England, is the company of William Page. I visited Beech Tree in the summer of 1991 while we were working on our first issue (February 1992). Bill, his family, and his staff were warmly hospitable, and we immediately established a good working relationship. It was wonderfully helpful to have an experienced publisher handle typesetting and layout, distribution of proofs, copyright management, processing of library subscriptions, printing, and journal distribution.

Beech Tree published 17 of our 20 issues. On behalf of the association, I thank Bill Page, Lynn Frances, and Trisha Dale (who did most of the typesetting and layout over this period) for an excellent job. The journal was the beneficiary of their expertise, their professionalism, and their hard work. And they were a pleasure to work with. I regret that the legal threats directed against the journal and the association forced us to part company with Beech Tree (see Johnson, 2000:223). Were it not for these threats, it is likely that Beech Tree would still be publishing the journal, and it might have continued doing so for many years. We will always be grateful to Beech Tree for the important role it played during these years, and extend our best wishes to Bill Page, his staff, and his family.

In assuming the work of publisher for our final three issues, we accumulated additional debts of gratitude. I thank Patti Kelly for the initial typesetting and layout work on the first part of the September 2000 issue. For completing the work on that issue (when Patti moved on to another position), and for handling all of the typesetting and layout for our final two issues, including this one, I thank Jeanne Shibley. Jeanne's experience and eye for excellence have been invaluable. For printing these final three issues, I thank Data Reproductions Corporation, Auburn Hills, Michigan, and especially Michael Seger, with whom we worked closely. For packaging and distributing these final three issues, we thank Unit Packaging Corporation, Ann Arbor, and especially Lynn Voegeding.

#### *University*

Our most general debt is to Lake Superior State University (LSSU) for ten years of support. The university provided office space, released time for me, and budgetary support. We are especially grateful to President H. Erik Shaar, who agreed to bring the journal to LSSU and support it, and President Robert Arbuckle, who maintained university support when he assumed office in the Fall of 1992. Dr. Arbuckle was also steadfast in his support of the journal, the principle of editorial independence, and freedom of expression during the ordeal we faced when legal threats began in 1999 (Johnson, 2000:212-223).



In addition to the university's provision of space, time, and budget, many LSSU staff, faculty, and administrators helped over the years in numerous ways. I want to specifically acknowledge Kathy Burrell, Georgiana Cox, Juliana Cox, Tracey MacQuarrie, and others in the Business Operations Office for their help with paying our bills, processing checks and credit card receipts, and keeping track of our budgets; Fred Michels, Scott Olson, and Aaron Weeks for their help with our special computer needs; Jennie Peterman and Colleen Rye for their help with purchasing and related matters; Ruth Gendzwill, Kristie Juda, Peggy Knuttila, Margaret Olson, and Beverly White for their help with payroll and personnel; and Cheri Hoonstra, Nancy LeGreve, and Ron Raffaele for their help with sending and receiving (in a variety of forms).

I offer special thanks to the staff of LSSU's Graphic Arts office for the great work they did with design, layout, and/or printing of a wide variety of materials, including *PLS* and *APLS* logos, *PLS* mastheads and tables of contents, *APLS News*, association directories, brochures, calls for abstracts, conference posters, conference programs, stationery, and a variety of other materials. Deb Cook, Patti Kelly, Jeff Harris, Jeff Oja, and Jeanne Shibley did a great deal of work for us, and sometimes on short notice. Their work was always superb, and they always showed great patience when I asked, having examined a draft, if they could adjust something "ever so slightly."<sup>12</sup>

#### *Office Staff*

Over forty LSSU undergraduate student assistants and interns worked for the journal and the association during our decade of work. They checked references, made copyediting suggestions, tracked down essential information in the library or on the internet, proofed, created and maintained our databases, created and maintained our web site, and helped in dozens of other ways. We benefited enormously from their labors, and they benefited from their experience in



**PLS/APLS Student Assistants and Interns, 1997-98**

working on the journal and for the association. Indeed, the experience played an important role for a number of them as they moved on in their education and careers.

Journal readers and association members did not see these students at work (with the exception of Deric Jones, who helped on-site at our conferences in 1998, 1999, and 2000). However, these student assistants and interns have been listed on the journal's masthead over the years. Some worked or interned for a semester or a summer, some for a year or two, and a few for three or four years. Deserving of special mention, since they served our office for multiple years, are Sophia Chandauka, Mark Heimonen, Rebecca Hunt, Deric Jones, Carolyn McCullough Powrozek, Rachel Olney Brakke, Carrie Clark Sharpe, Kerry Smith, Winston Reeves II, and Steven Wellington.

My greatest debt among those who did journal or association work over this decade is to Judy Bawks, who served as secretary and assistant to the editor from August 1991 to June 2000 (when she had to leave us for a full-time position). Judy supervised the student assistants and interns, prepared and sent out all correspondence, entered final copyediting changes in almost all manuscripts, proofread manuscripts, prepared payroll and all other university paperwork, and handled a wide variety of other tasks associated with running an editorial office.

As the association's administrative assistant from July 1996 to June 2000, Judy also handled all membership records and payments; prepared and sent out correspondence that related to membership, the council, and the conferences; prepared initial drafts of posters, conference materials, and conference programs; processed conference registrations and payments; worked the on-site registration desk and conference office at the 1998 and 1999 conferences (where many of you met her); and handled a variety of other tasks related to the business of the association.

Judy did an outstanding job in every way. She was talented, dependable, conscientious, and hard-working. She understood the goals of both the editorial and association offices, and she took the initiative in helping us reach those goals. She was also friendly and congenial, and yet professional. She developed wonderful working relationships with the student workers, other campus employees, authors, conference attendees, and association members. The entire association owes her a great debt for her contribution to the success of the journal and the association during these years.

When Judy was forced to leave us for a full-time position in June of 2000, we were fortunate to acquire the services of another highly talented and dedicated assistant, Patricia Smith. Patti assumed all of the duties of both positions, and did so enthusiastically. She learned quickly, and, like Judy, did an outstanding job. Many members met her at our 2000 conference in Washington. Thank you, Patti.

When Patti had to leave for a full-time position in April of 2001, Suzette Olson filled in over the summer of 2001 while she was on leave from her regular position. She also did a great job, as did Jeanne Shibley, who helped out for several months in late 2001 and early 2002. My thanks to both Suzette and Jeanne. Since that time, Judy Bawks has again helped out intermittently as time has permitted.<sup>13</sup>

### **A Continuing Mission**

The journal and the association are now in their third decade. Their mission, in my view, remains the same: "to advance knowledge of politics and promote better policymaking through multidisciplinary analysis that draws on the life sciences" (Johnson, 1992:3). That mission is as crucially important today as it was when the association and the journal were founded—perhaps more so.

I hope our work during the second decade helped advance that mission. However, there is still a great deal of additional, untapped potential. Reaching out to achieve more of that potential is inspiring, but the demands of the task must not be underestimated. Interdisciplinary associations face enormous challenges not faced by single-discipline organizations. Employment opportunities, educational degrees, academic structures, professional rewards, most publication outlets, most grant opportunities, and even our daily professional vocabularies are built, for the most part, around traditional, narrow disciplines.

For that reason, there are huge impediments to successfully establishing and maintaining interdisciplinary associations, especially those as broadly interdisciplinary as APLS. We must never underestimate the challenges. We should certainly seek out visionary university administrators, foundations, and philanthropists who see the potential and are willing to invest in helping overcome the obstacles. Nevertheless, the energy and the vision must come principally from us, the members.

To fulfill our mission, then, APLS members and officers must work harder and be more resolute than the members of single-discipline organizations. Fortunately, even if the work is harder, the potential payoff is greater. The function of APLS is to bring together scientists, scholars, and policymakers for the purpose of breaking down the disciplinary barriers that constrain the progress of knowledge and the formulation of better policy. The potential payoff for a successful APLS, then, is exactly the kind of “consilience” that Edward O. Wilson spoke of so insightfully in his book of that title (1998).

As we continue working to achieve more of our potential as an organization, I offer my best wishes to the journal’s new Editor, Robert Sprinkle, of the University of Maryland, and the association’s new Executive Director, David Goetze, of Utah State University. I hope Rob and David find the work as rewarding as I did, and that they receive the same kind of enthusiastic support and assistance that we received. Overcoming the impediments to interdisciplinary synergy *must* be a group effort.

I return now to my own research with the satisfaction of believing that we accomplished much of what we set out to accomplish. At the outset of my term as editor, I said that “involvement in [the journal’s] mission promises to be a truly exhilarating experience” (1992:4). It was, and I am grateful to all who helped make it so rewarding. It was a privilege to serve the association in this way. Thank you.

Gary R. Johnson  
Editor, 1991-2001  
Executive Director, APLS, 1996-2001

## Notes

1. I also hope that this review of our second decade may be useful to those who are new to the association and to those who are interested in its history. Scholarly and scientific organizations often do a poor job of keeping track of their own histories. For that reason, I thought it might be useful to the organization—while my memory is still reasonably fresh, and while our files still exist for checking—to provide a short account of this period in the journal’s and the association’s history.
2. For more information about Tom Wiegele and the crucial role he played in establishing the journal and the association, see Johnson et al., 1992. For an overview of the field of biopolitics in the years prior to the establishment of the association, see Wiegele, 1979. For a more recent overview of the field, see Blank and Hines, 2001.
3. For a brief review of the journal’s and the association’s first decade, see Wiegele, 1991.
4. With continuing growth in the field, and sophisticated computerized indexes for keeping track of that growth, it seemed clear by 2000 that this feature was no longer as useful to readers as it had been in earlier years. We therefore discontinued it.
5. It is likely that we would have had three or four additional roundtables/symposia in our last three issues had it not been for the special challenges we faced, as a result of legal threats, in having to become both editorial office and publisher for these issues (see Johnson, 2000). Indeed, I would like to have contributed a commentary of my own for two of those.
6. Those interested in curricular developments should also find Tom Wiegele’s August 1986 issue of the journal useful. This special fifth anniversary issue was devoted to “Teaching about Politics and the Life Sciences.”
7. While most of our reviews after 1993 were single reviews, Joe Losco arranged “roundtable reviews” of two books that lent themselves particularly well to discussion: James Q. Wilson’s *The Moral Sense* (August 1994, with four reviews and a response from Wilson) and Edward O. Wilson’s *Consilience* (September 1999, with three reviews and a response from Wilson).
8. Tables of contents for past issues may be reviewed on-line at <http://www.aplsnet.org> (click on “journal” and follow the links).
9. For copyediting assistance with a few to several manuscripts over the years, when Nancy was busy, I thank Beth Leech, Tammy Ditmore, Susan Finkelstein, Susan James, and Eric Gadzinski.
10. I want to again express my thanks to the various donors who have already made contributions, to the many people who offered encouragement and moral support, to the authors whose work was delayed in publication—they exhibited extraordinary patience—and to all of the libraries and individual members who were so amazingly patient.
11. APSA had also added restrictions that made it increasingly difficult to maintain a relationship (see Losco, 1997).
12. Many other LSSU administrators, staff, and faculty also helped in various ways during the years that the journal and the association’s administrative offices were here. So that they know that their help was genuinely appreciated, even if it was needed only once or a few times, I want to acknowledge them here: Kathy Albrough, Patty Allison, Bernard Arbic, Doug Atkinson, Kaye Batho, Bill Becker, Paul Bestman, James Blashill, Dean Bennette, John Burdett, Mary Cahill, Alden Campbell, Kevin Chamberlain, Richard Conboy, Dave Cryderman, Sheri Davie, Maureen Delaney-Lehman, Laurie DeNeve-Ewing, Jim Devaprasad, Pete Donofrio, Michael Donovan, Sharon Dorrity, Ted Eby, Wanda Eby, Dave Eitland, Susan Fitzpatrick, Kay Floyd, Brad Flood, Steve Forrest, Mary Gray, Dave Greengtski, Duane Gurnoe, Chuck Gustafson, Annette Hamel, Bruce Harger, Joe Herbig, Beth Hronek, Chris Johnson, Kari Jastorff, Mark Jastorff, Kristie Juda, Mary June, Pauline Killips, Jim McCall, Margaret Malmberg, Pat Manor, Suzie McAllister, Troy Lawson, Donald McCrimmon, Dave McDonald, Tom Mickewich, Glynis Moran, Roger Murphy, Ruth Neveu, Clark Noble, Suzette Olson, Sharyl Padgett, Rocco Paris, Danny Pavlat, Sherri Pavloski, Jackie Perron, Gene Pietrangelo, Tom Pink, Andrew Radford, Susan Ratwik, Chris Roll, Reg Rousseau, Tim Sawyer, Linda Schmitgal, Jay Schupp, John Shibley, Art Smart, Scott Smart, Jim Smith, Jerry Stephens, Mark Terwilliger, Gary Thesing, Bill Thompson, Earl Tomlinson, David Toppen, Rick Waligora, and David White. Thanks to all of you for your willingness to help in so many ways.
13. If there are others whom I have neglected to thank, I apologize for the oversight. Of course, my most profound debt is to my wife and children for the time spent away from them. Mere words are no recompense for that debt.

## References

- Blank, R.H. and S.M. Hines, Jr. (2001). *Biology and Political Science*. New York: Routledge.
- Blank, R.H. (1982). “Biopolitics: A Restatement of Its Role in Politics and the Life Sciences” [with commentaries and response]. *Politics and the Life Sciences* 1:38-51.
- Caldwell, L.K. (1992). “Tom Wiegele: Preeminent Founder.” *Politics and the Life Sciences* 11:95-96.
- Hines, S.M. (1982). “Politics and the Evolution of Inquiry in Political Science” [with commentaries and response]. *Politics and the Life Sciences* 1:5-37.
- Johnson, G.R. (1992). “Politics and the Life Sciences: A Journal, A Mission, A Vision” [Editor’s Introduction]. *Politics and the Life Sciences* 11:3-4.

- Johnson, G.R. (1998). "Association for Politics and the Life Sciences: Eighteenth Annual Meeting, Boston" [Conference Report]. *Politics and the Life Sciences* 17: 209-211.
- Johnson, G.R. (2000). "Science, Sulloway, and Birth Order: An Ordeal and an Assessment" [Editorial]. *Politics and the Life Sciences* 19:211-45.
- Johnson, G.R., C. Barner-Barry, R.H. Blank, L.K. Caldwell, K.L. Oots, and J.C. Wahlke (1992). "Thomas C. Wiegele: Founding Father." *Politics and the Life Sciences* 11:93-98.
- Losco, J.A. (1997). "Organized Sections and Intellectual Diversity within APSA." *APLS News* 2:8-9.
- Mill, J.S. (1974). *On Liberty*. Harmondsworth, England: Penguin; originally published 1859.
- Wiegele, T.C. (1979). *Biopolitics: Search for a More Human Political Science*. Boulder, CO: Westview Press.
- Wiegele, T.C. (1982). "Editor's Introduction." *Politics and the Life Sciences* 1:3-4.
- Wiegele, T.C. (1991). "A Note from the Editor." *Politics and the Life Sciences* 10:3-4.
- Wilson, E.O. (1998). *Consilience: The Unity of Knowledge*. New York: Alfred A. Knopf.

## ORIGIN OF AIDS

# The Politics of a Scientific Meeting: The Origin-of-AIDS Debate at the Royal Society

Brian Martin

University of Wollongong, Australia

**Abstract.** The Royal Society of London held a scientific meeting in September 2000 focusing on two theories of the origin of AIDS: one, that it occurred through “natural transfer” of immunodeficiency virus from monkeys or chimpanzees to humans; and the other, that it occurred through iatrogenic transfer via contaminated polio vaccines used in Africa in the late 1950s. This meeting was the culmination of years of public contention over the polio-vaccine theory. Several dimensions of the politics of science are revealed by analysis of this issue, including the power of scientific editors, the use of the mass media, decisions regarding selection of speakers and organization of the meeting, and epistemological assumptions made by participants.

---

**Brian Martin** is Associate Professor in Science, Technology and Society at the University of Wollongong, Australia. He has researched many scientific controversies, including fluoridation, nuclear winter, and pesticides, with special attention to suppression of dissent. He is the author of numerous publications in a number of fields, including nonviolent action, information issues, strategies for social movements, and participatory democracy. Recent books include *Information Liberation* (Freedom Press, 1998), *The Whistleblower's Handbook* (Jon Carpenter, 1999), *Random Selection in Politics* (coauthor, Lyn Carson; Praeger, 1999) and *Technology for Nonviolent Struggle* (War Resisters' International, 2001). Correspondence should be addressed to Science, Technology, and Society Program; University of Wollongong; Wollongong NSW 2522; Australia (email: brian\_martin@uow.edu.au; web: <http://www.uow.edu.au/arts/sts/bmartin>).

In London on September 11-12, 2000, the Royal Society—the independent scientific academy in Britain, the counterpart to the U.S. National Academy of Science—hosted a discussion meeting on the “Origins of HIV and the AIDS epidemic.” The brochure about the meeting gave this synopsis: “HIV-1 and HIV-2 causing AIDS are new human viruses of animal origin. When, how and why these cross-species infections occurred is the topic of this meeting. Discussion will focus on possible natural and iatrogenic routes of transmission in zoonosis and the subsequent epidemic spread of HIV.”

Since 1992, there had been fierce contention between advocates of two contrasting AIDS origin theories. Both sides agreed that AIDS arose when a simian immunodeficiency virus (SIV) from a monkey or chimpanzee was transmitted to and took hold in the human species, becoming the human immunodeficiency virus (HIV). The question of how this occurred divided the two camps.

One theory was that SIV jumped species when a hunter, while butchering a monkey, had gotten monkey blood in a cut or, alternatively, virus transfer occurred through a monkey bite or from eating undercooked monkey meat. This theory, commonly called “natural transfer” or “cut hunter,” was held by most researchers in the AIDS field (Hahn et al., 2000). The competing theory was that SIV entered humans through contaminated oral polio vaccines—cultured on monkey kidney cells—given to a million people in central Africa in the late 1950s. Called the polio-vaccine or OPV (oral polio vaccine) theory, it was advanced by a small group of journalists, independent scholars, and scientists (Hooper, 2000b).

Many other theories have been proposed for the origin of AIDS, including that it was due to smallpox vaccines or to a biological warfare experiment gone wrong (Hooper, 2000b:151-69; Lederer, 1987/1988). As well, the idea that AIDS is not a distinct disease at all and that it is not due to HIV has received considerable visibility, especially through the efforts of molecular biologist Peter Duesberg (Duesberg, 1996; Maggiore, 1999; for a critique see Harris, 1995). How-

ever, at the Royal Society meeting the two main contenders were the cut-hunter and polio-vaccine theories, and the focus here will be on these two theories.

The Royal Society meeting revealed, in concentrated form, the intense symbiosis of science and politics that had long characterized the origin-of-AIDS issue and which is found in many scientific controversies. The meeting provides a useful window into rhetorical and organizational strategies that can be used by partisans at a scientific meeting, and their limitations. In this article, considerable scrutiny will be given to the meeting itself as well as to the buildup to it.

In the next section, the earlier history of the treatment of the polio-vaccine theory is outlined; this is essential background for understanding the dynamics of the Royal Society meeting. In the following section, the Royal Society meeting is described, including politicking beforehand. In the final section, the numerous political dimensions of the issue are summarized.

### **The Origins Debate to 1999**

AIDS was first diagnosed as a distinct disease in 1981. Within a couple of years HIV was discovered, and since then scientific consensus has been that HIV infection is necessary to cause AIDS. In 1985, SIVs were discovered in African monkeys. Since the SIVs are the closest known relatives to HIVs, it seemed plausible that AIDS was a new disease caused by one or more SIVs entering and becoming transmissible in humans.

With no direct evidence for a specific transmission event, the next best thing is correlation in time and space. Although the first diagnosed cases of AIDS were in the United States, it soon transpired that most of the earliest cases of AIDS and HIV-positive blood were in central Africa, especially in what is now called the Congo (formerly Zaire). Monkeys and chimpanzees are found in this part of the continent, and both are hunted and kept as pets, leading to many opportunities for SIVs to enter humans. The natural-transfer theory was that this occurred through one of the many normal simian-human interactions; once in humans, the virus—now called HIV—was spread to other humans through sexual intercourse, shared needles, and other known routes.

There are complexities at the molecular level. There are various SIVs, with different ones found in different simian species and, since little testing had taken place, the likelihood of discovering further varieties. There are several genetically distinct types of HIV. HIV-1 Group M is responsible for most of the world's infections. HIV-2, a less virulent strain, is mainly found in western Africa. Years later, HIV-1 Groups O and N, both as genetically distinct from Group M as HIV-2, were discovered.

One problem with the cut-hunter theory is timing. Humans have been butchering monkeys for a couple of million years, so presumably there must have been repeated incidents in which humans were exposed to SIVs. Why did

this lead to a pandemic only in the twentieth century? One possible explanation is that AIDS had existed for a long time in remote villages, but only in the past century, with urbanization and improved transport, have the conditions existed for exponential growth. While this is a plausible argument, no evidence was available or was collected to back it up.

Although the natural-transfer theory has been accepted by most scientists, it has remained undeveloped, with few specifics given. No one has provided a definitive account. Natural transfer seems to have been accepted as the default option because all alternatives were rejected as implausible.

Compared to the theory of natural transfer, the polio-vaccine theory had a far more difficult time gaining a hearing. South African biomedical scientists Mike Lecatsas and Jennifer Alexander suggested in a brief communication that polio vaccines might be a route for the introduction of AIDS (Lecatsas and Alexander, 1989). This triggered a hostile response (Schoub, Dommann and Lyons, 1990).

Unbeknownst to Lecatsas and Alexander, the polio-vaccine theory had already been developed in far more detail by Louis Pascal, an independent scholar based in New York City. Pascal circulated his ideas to a range of biologists and AIDS researchers but obtained no more than a single acknowledgment. He also submitted a short paper to *Nature*, *Lancet*, and *New Scientist*, without success. More disturbingly to Pascal, he received no substantive critical comments (Martin, 1993). One of his correspondents passed his work to the *Journal of Medical Ethics*, whose editor asked Pascal to write a different sort of paper. He did so, but the result, at 19,000 words, was far too long for publication (Gillon, 1992). (As someone outside conventional scientific culture, Pascal was not accustomed to playing the publication game and, furthermore, was not willing to bend to editorial requirements.) Around this time a colleague passed Pascal's work to me, and I agreed to publish his manuscript in a working paper series if it was rejected by the *Journal of Medical Ethics*. Pascal's paper, the first major statement of the polio-vaccine theory, appeared in December 1991 (Pascal, 1991).

Pascal had combed through medical journals in the 1950s and early 1960s for evidence about polio vaccination campaigns in central Africa—especially in what are now the Congo, Rwanda, and Burundi—in the period 1957-1960. This was the region where most of the earliest cases of AIDS and HIV-positive blood samples had been found. The vaccine was manufactured using a culture of monkey kidney cells, thereby providing a route for SIVs to contaminate the vaccine. SIVs do not cause disease in their natural hosts, so monkeys with SIVs would not have been rejected as ill.

There are two main sorts of polio vaccine, using either killed virus or live virus. The killed-virus vaccine, associated with polio pioneer Jonas Salk, requires several injections. The live-virus vaccine, most commonly associated with Albert Sabin and most widely used worldwide from about 1960, requires just a single oral dose and is thus much cheap-

er and easier to administer. However, the late-1950s African polio vaccination campaigns were run by Hilary Koprowski who, though much less well known than Salk and Sabin, is considered the third great polio vaccine pioneer.

Koprowski's live-virus vaccine was squirted into people's mouths. Normally this procedure would not create a direct route to the vaccinee's blood, but this could occur through a sore or cut. Furthermore, SIV infection could occur directly through oral mucosa.

Pascal noted that the timing was right: when he wrote, HIV was thought to have originated just before 1960. Furthermore, Pascal noted that Koprowski's vaccine was given to many infants, whose immune systems are not developed, in doses 15 times as great as for adults (this was done to ensure immunization occurred).

Koprowski's vaccines had also been used elsewhere, such as in Poland. If the vaccine was contaminated, shouldn't AIDS have developed there at an early stage too? Pascal noted that different monkey kidneys would have been used to produce different batches of vaccine. Contamination need only have occurred in some batches. Pascal even identified the batch he thought responsible.

Another suggestive piece of evidence is that early polio vaccines were known to have been contaminated with a different monkey virus, SV40, given to millions of people worldwide (Shah and Nathanson, 1976). This showed that vaccine contamination was more than a hypothetical possibility.

Although Pascal provided no direct evidence that contaminated polio vaccines had led to AIDS, he adduced considerable circumstantial evidence, providing a detailed mechanism (vaccines grown on monkey kidneys), describing favorable conditions (vaccination of infants with undeveloped immune systems), explaining timing and location of early AIDS cases, and citing a precedent (SV40). Furthermore, his hypothesis was open for testing and falsification, for example by finding HIV-positive blood samples before 1957. Arguably, there was more evidence at the time to support the polio-vaccine theory than to support the natural-transfer theory. Yet natural transfer was widely and uncritically accepted, while Pascal could not find any scientists to explore the polio-vaccine theory. From a social science point of view, this suggested that the two theories were being treated differently, with an excessive burden of proof placed on the polio-vaccine theory.

Only a few months after Pascal's paper was published, the identical theory was published in the rock magazine *Rolling Stone* (Curtis, 1992c). AIDS activist Blaine Elsworth, independently of and more recently than Pascal, had developed the same theory. Elsworth alerted journalist Tom Curtis, who did further investigation and wrote a powerful story. Whereas Pascal's sober articles had been ignored by the scientific community, the engagingly written *Rolling Stone* story triggered a storm of comment in both scientific journals and the mass media.

The Wistar Institute, which was headed by Koprowski and which had manufactured his vaccines, set up a committee to look into the issue. It pronounced that the theory was extremely unlikely, since each stage—SIV contamination of polio vaccine, oral transmission, and evolution of any known monkey SIV into HIV-1—was unlikely. (The committee did not, however, assess the likelihood of natural transfer using a similar approach.) In spite of its skepticism, the committee recommended that as a precaution monkey kidneys no longer be used to produce polio vaccines ("Panel Nixes Congo Trials," 1992; Basilico et al., 1992). (However, most vaccines today are still produced on monkey kidneys.)

Curtis's article and, to a lesser extent, Pascal's paper, generated considerable interest in the polio-vaccine theory over the following years. There were some significant contributions, including by Elsworth and Stricker (1993, 1994) and by journalist Julian Cribb (1996), who pointed out that there had been massive population movements in central Africa for centuries due to the slave trade, making it harder to argue that AIDS had lain dormant in a remote village for decades before the 1960s. However, by the late 1990s, many commentators believed that the polio-vaccine theory had been discredited. There were two main reasons for this (Martin, 1998).

First, mainstream journals—especially the two most prestigious general science journals, *Nature* and *Science*—were resistant to submissions about the theory. Soon after his *Rolling Stone* article, Curtis was able to get a letter into *Science* (Curtis, 1992b). Koprowski (1992) replied, but *Science* refused to publish Curtis's response to Koprowski (Curtis, 1996). In 1994, eminent evolutionary biologist W. D. Hamilton—who had won two prizes equivalent to the Nobel Prize—submitted a letter to *Science* responding to Koprowski (Hamilton, 1996), but *Science* refused to publish it, revealing that rejections to submissions about the theory were not restricted to nonscientists. Since *Science* published no reply to Koprowski's letter, this gave the impression that Koprowski's arguments were definitive. Similarly, over the years *Nature* rejected submissions about the theory by half a dozen authors, publishing none (Hooper, 2000b:852).

The second reason why the polio-vaccine theory was perceived as discredited was legal action. In late 1992, Koprowski sued Tom Curtis and *Rolling Stone* for defamation ("Koprowski Sues," 1993). The case never reached court, being settled by *Rolling Stone's* payment of \$1 to Koprowski and publication of a "clarification" ("Origin of AIDS" Update," 1993). While the published statement made few concessions to Koprowski, its very existence superficially gave the impression of acquiescence to Koprowski's claims ("*Rolling Stone* Rolls Over," 1993). A better interpretation was that the statement was made under legal and financial duress: *Rolling Stone* had already spent half a million dollars in legal fees. Koprowski also sued the Associated Press over a different story; again, the case was eventually settled many years later. The impact of Koprowski's legal actions

was to discourage media discussion of the polio-vaccine theory. *Rolling Stone* declined to publish a follow-up article it had commissioned from Curtis. Furthermore, Curtis was badly burnt and was, in effect, silenced. If he had wanted to pursue investigation into the polio-vaccine theory, he would have felt obliged to warn every informant that his notes and recordings of their conversation could, in principle, be subpoenaed by Koprowski's lawyers. Michael K. Curtis (1995) has used this case to argue that defamation law should be modified to allow "heightened protection" for critical discussion of complex issues.

With the mainstream scientific journals leaving the impression that Koprowski's arguments were unanswered and with Koprowski's defamation actions having inhibited media discussion, many commentators treated the polio-vaccine theory as having been refuted (Garrett, 1994:381, 666; Karlen, 1995:245). This combination of editorial power and legal inhibition produced what can be called a political refutation of the polio-vaccine theory (Martin, 1998). Given this process, it is hard to judge what the outcome would have been if scientific arguments alone had been used to judge the theory.

If anything, the case for the theory was stronger than ever. The Wistar Committee (Basilico et al., 1992:6) had cited the case of a Manchester sailor who apparently contracted AIDS in 1959 (Corbitt, Bailey and Williams, 1990) as the most crucial piece of evidence against the theory. But a few years later, new testing found the previous findings to be incorrect (Bailey and Corbitt, 1996; Zhu and Ho, 1995). Meanwhile, no new evidence for natural transfer had been found—mainly because few were looking for any.

### ***The River and the Royal Society***

The polio-vaccine theory might well have gradually faded away without critical scrutiny except for the work of journalist and writer Edward Hooper. After nine years of investigation into the origin of AIDS, many of them focusing on the polio-vaccine theory, Hooper's mammoth book *The River* was published in September 1999 (Hooper, 2000b). Hooper had combed archives and interviewed hundreds of individuals in several continents, probing the earliest cases of alleged AIDS, tracking the spread of AIDS, exploring the early development of polio vaccines, and scrutinizing all aspects of Koprowski's African polio vaccination campaigns. He added a new claim to the polio-vaccine theory: that chimpanzee kidneys, from chimps held at Koprowski's Lindi Camp in the Congo, might have been used to produce polio vaccines. This was especially significant because a chimp SIV was the prime candidate as the precursor to HIV-1 Group M, responsible for most AIDS cases in the world.

*The River* was written as a scientific detective story, providing engrossing reading despite its great length. Its publication dramatically raised the profile of the polio-vaccine theory. There were dozens of reviews, including many

in major scientific journals (Gilks, 1999; Sharp, 1999; Wain-Hobson, 1999) and in the mass media, such as the *New York Times* (Altman, 1999; Epstein, 1999), as well as numerous raves by readers on Amazon.com. While only some reviewers professed to being convinced of the polio-vaccine theory, nearly all agreed that it was worthy of further investigation, with only a small minority being hostile (Moore, 1999).

Since 1991, calls had been made for the Wistar Institute to release any remaining polio vaccine samples from the 1950s for testing (Curtis, 1992a). If SIV or HIV were to be found, this would provide strong support for the polio-vaccine theory. However, for years the institute did not provide any samples for testing—at least not publicly (Hooper, 2000b:799). With the publication of *The River*, though, the Wistar announced that it did have some samples for testing, and arranged for this to be done at three independent labs. Publicity thus provided the necessary stimulus for scientific investigation.

One of Hooper's prime allies in his long search was Oxford University biologist W. D. Hamilton, the most prominent scientist supportive of the theory. Riding on the tremendous interest stimulated by *The River*, Hamilton used his position on Britain's prestigious Royal Society to obtain its sponsorship for a discussion meeting about the origin of AIDS, focusing on the polio-vaccine theory.

This was a controversial role for the society, traditionally seen as very much an establishment voice. Founded in 1660, the Royal Society is Britain's most elite scientific body, with some 1300 fellows and foreign members. Its core activities are publication of five scientific journals and the holding of numerous scientific meetings and lectures; as well, it funds hundreds of postdoctoral researchers, awards a number of medals and prizes, and produces reports and makes statements on issues in science and technology. Independent of the government, the bulk of its income is from donations and bequests. For many years it did not play a major role in public debates, but since the 1990s it has engaged more with contemporary issues, such as in running meetings and making statements on genetically modified organisms and nuclear power. Holding the meeting on the origin of AIDS was perhaps its most daring entry yet into controversial waters.

Hamilton's co-organizers for the meeting were Simon Wain-Hobson of the Pasteur Institute in France and Robin Weiss, Professor of Viral Oncology at University College London. The meeting was scheduled for May 11-12, 2000, at the Royal Society in London.

How was the meeting to be run? One model would have been a private roundtable discussion aimed at clarifying points of agreement and disagreement and pointing to areas for further investigation. This sort of approach would have aimed at open and honest examination of strengths and weaknesses of each theory, possibly with attention to other alternatives. Instead, the plan was for a more traditional conference format, with speakers, discussants, and questions from the floor. Given this format and given that the cut-hunter

and polio-vaccine theories were the main contenders to be discussed at the meeting, the balance of speakers was crucial. Being seen to be on one side were Hooper, Hamilton, and a few scientists such as Gerald M. Myers, head of the HIV Sequence Database at the Los Alamos National Laboratory, whose work added support to the polio-vaccine theory even though he was not committed to it. On the other side were Koprowski, his collaborator Stanley Plotkin, and several other scientists critical of the polio-vaccine theory, including phylogeneticist Bette Korber and microbiologist Beatrice Hahn. Speakers and discussants were invited, and the meeting was set to go ahead.

Early in 2000, Hamilton went to Africa to collect chimp feces that could be tested for the presence of SIVs, hoping to find evidence that might support the polio-vaccine theory. On the trip, he contracted malaria and, just after returning to Britain, collapsed in a coma, dying five weeks later.

Meanwhile, Hooper alleged that, behind the scenes, opponents of the polio-vaccine theory were putting pressure on the Royal Society to stop or delay the meeting by pulling out or threatening to do so (Meek, 2000). In late March, the Royal Society announced that the meeting had been postponed until September 11–12. There was an exchange of claims in letters to newspapers, with Hooper (2000a) alleging that the postponement was due to pressure tactics while the president of the Royal Society stated that there were perfectly good reasons for the decision, including waiting for the results of testing of Wistar samples (Klug, 2000).

Whatever the reasons, the format of the September meeting increased its visibility and raised the stakes for all parties. This time around, leading opponents of the polio-vaccine theory, including Koprowski, agreed to attend. The meeting was to be open to the public and a press conference was to be held. Behind the scenes, once again, the selection of speakers and discussants was the subject of much discussion, so Hooper informed me in many communications prior to the meeting. Table 1 gives the speakers as listed in the brochure about the meeting.

As well as these major speakers, there were 15 listed discussants, each of whom was given five or ten minutes, either squeezed at the end of one of the papers or in groups during hour-long slots at the ends of sessions II and III.

Prior to the meeting, it was possible to guess the positions of quite a number of the speakers. It could be anticipated that those opposed to the polio-vaccine theory and/or supportive of the cut-hunter theory included:

- Hilary Koprowski, developer of the CHAT vaccine;
- Stanley Plotkin, a key and vocal collaborator of Koprowski;
- Beatrice Hahn, whose recently published work (Hahn et al., 2000) dismissed the polio-vaccine theory and who had been quoted in the media as critical of the theory;
- Bette Korber, whose recently published work on dating of the origin of HIV was incompatible with the polio-

vaccine theory (Korber et al., 2000) and who had been quoted in the media as critical of the theory;

- Kevin De Cock and Paul Sharp, coauthors of Hahn.

In contrast, the only open supporter of the polio-vaccine theory was Edward Hooper. My own role, as a social scientist who had pointed out the way the polio-vaccine theory had been marginalized (Martin, 1993, 1998), could be interpreted as providing de facto support for the polio-vaccine theory (Martin, 1996). Of the main speakers, the only scientists whose work might be expected to give some comfort to the polio-vaccine theory were Tom Burr and Pascal Gagneux. Burr, a collaborator with Gerald Myers, stood in for Myers, who was ill. Myers had been quoted as critical of Korber's conclusions. Gagneux's work on chimp gene flow was compatible with the polio-vaccine theory. But neither Burr nor Gagneux could be expected to take a stand for any theory.

Some of the other speakers had not previously taken a partisan position and could be expected to discuss technical matters that had little direct bearing on the controversy — though it could be argued that these contributions provided important contextual material for assessing competing explanations of the origins of HIV. Preston Marx would be presenting the case for a different theory, namely that medical re-use of needles in Africa had allowed otherwise limited natural transfers of SIVs to explode into the AIDS epidemic.

Thus, the lineup of speakers seemed stacked against the polio-vaccine theory. On the one side were Koprowski, Plotkin, Hahn, Korber, De Cock and Sharp; on the other was Hooper. It was especially noticeable that this pitted several scientists against a nonscientist. The polio-vaccine theory's greatest scientist supporter, Bill Hamilton, had died earlier in the year. Myers, an important figure who was known to be open to the theory, was unable to attend. Jennifer Alexander and Mike Lecatsas, who had followed the theory since their early contributions in the late 1980s, were not invited to be speakers.

Of the 15 discussants, the most prominent supporter of the polio-vaccine theory was Julian Cribb, author of a book on the topic (Cribb, 1996); the most obvious opponent was Claudio Basilico, member of the Wistar Committee that had earlier dismissed the polio-vaccine theory (Basilico et al., 1992).

Another dimension to the meeting was a press conference scheduled for 3:45 p.m. on the first day. There certainly was plenty of media interest in the meeting. There were stories in the press in the days leading up to the meeting (e.g. Connor, 2000). As Hooper arrived in a taxi at the Royal Society, television cameras followed him into the building. However, television crews were not allowed into the meeting room — except for relaying the proceedings to an overflow room. Every one of the 350 seats in the main venue was taken.

The meeting proceeded as might have been predicted from the lineup of speakers. Hahn and Korber criticized the polio-vaccine theory, as anticipated, while Hooper came on

**Table 1. Speakers at the Royal Society Discussion Meeting on “Origins of HIV and the AIDS Epidemic,” Monday-Tuesday, September 11-12, 2000, as Listed in the Brochure Announcing the Meeting**

<b>Session I (Monday morning)</b>	<b>Chair: Professor Walter Finch</b>	
9:00	Dr. Simon Wain-Hobson	Opening Remarks
9:05	Sir Robert May	Bill Hamilton <i>In Memoriam</i>
		<b>Zoonosis and Epidemiology</b>
9:15	Professor Albert Osterhaus	Catastrophes after Crossing Species Barriers
9:50	Dr. Kevin M De Cock	Epidemiology and the HIV/AIDS Epidemic
10:25	Dr. Léopold Zekeng	Update on HIV/SIV Infection in Cameroon
		<b>Phylogenetics of HIV and Hosts 1</b>
11:30	Professor Beatrice Hahn	AIDS as a Zoonosis: Characterizing the Primate Reservoir
12:05	Dr. Bette Korber	Timing the Ancestor of the HIV-1 Pandemic Strains
<b>Session II (Monday afternoon)</b>	<b>Chair: Professor Neal Nathanson</b>	<b>Oral Polio Vaccines</b>
2:30	Mr. Edward Hooper	Experimental Oral Polio Vaccines and AIDS
3:05	Dr. Stanley Plotkin	Untruths and Consequences
4:40	Dr. John Beale	Polio Vaccine Development and Retroviruses
<b>Session III (Tuesday morning)</b>	<b>Chair: Sir John Skehel</b>	<b>Phylogenetics of HIV and Hosts 2</b>
9:00	Professor Paul Sharp	The Origins of AIDS Viruses: Where and When?
9:35	Dr. Tom Burr	The Origin of AIDS—Darwinian or Lamarckian?
10:10	Dr. Pascal Gagneux	What Do We Know about Gene Flow in Wild Chimpanzees?
<b>Session IV (Tuesday afternoon)</b>	<b>Chair: Dr Hilton Whittle</b>	<b>Epidemics and Society</b>
2:00	Sir Robert May	Why Epidemics Take Off
2:35	Dr. Preston Marx	Serial Human Passage of SIV: The Role of Unsterile Injecting Emergence of Epidemic Strains of HIV
3:10	Dr. Brian Martin	The Burden of Proof and the Origin of AIDS
4:15	Professor Hilary Koprowski	Hypotheses and Facts
4:45	Professor Robin Weiss	Closing Remarks

strongly, introducing new evidence suggesting that some of Koprowski’s polio vaccine might have been manufactured in Africa as well as at the Wistar Institute in Philadelphia and at labs in Belgium. Plotkin, following Hooper, also came on strongly—as suggested by the title of his talk, “Untruths and Consequences”—denying that chimp kidneys had been used to make any polio vaccine and claiming that numerous individuals linked to the 1950s polio-vaccine trials had signed statements denying that chimp kidneys were ever used, in apparent contradiction to quotes from these same individuals in *The River* used by Hooper to argue that this could have happened. In subsequent “discussion,” the exchange between Plotkin and Hooper became so heated, with

allegations of lying, that the chair of the session, Professor Neal Nathanson, threatened to shut down the meeting if civility was not restored (Cohen, 2000).

Another element in the politics of the meeting was an alteration to Monday’s Session II, notice of which was distributed to participants on arrival at the meeting that morning. The new arrangement is listed in Table 2.

Note that Hooper’s talk was moved half an hour earlier in order to squeeze in Basilico’s and Koprowski’s contributions just before the press conference at 3:45 p.m. The rearrangement had the effect of making the announcement of the testing of Wistar samples a prime news story without the opportunity for a studied response—or preparing a press

**Table 2. Speakers at the Royal Society Discussion Meeting on “Origins of HIV and the AIDS Epidemic,” Session II, Monday Afternoon, September 11, 2000, as Listed in an Addendum to the Brochure Announcing the Meeting**

Session II (Monday afternoon)	Chair: Professor Neal Nathanson	Oral Polio Vaccines
2:00	Mr. Edward Hooper	Experimental Oral Polio Vaccines and AIDS
2:35	Dr. Stanley Plotkin	Untruths and Consequences
3:10	Professor Claudio Basilico	Announcement of Results
3:25	Professor Hilary Koprowski	Hypotheses and Facts
4:40	Dr. John Beale	Polio Vaccine Development and Retroviruses

release—by those who might give a different interpretation than Basilico’s.

As expected by all parties, the results of the testing of samples released by the Wistar Institute showed no evidence of SIV or HIV. Furthermore, the cells on which the polio vaccine had been prepared were found to be Asian monkeys—though in one case the possibility of an African monkey—with no evidence of chimp cells. While this provided no support for the polio-vaccine theory, arguably it was not a serious blow to it. In *The River*, Hooper presented evidence that Koprowski’s polio vaccines used in Africa had been manufactured both at the Wistar Institute in Philadelphia and at labs in Belgium. Furthermore, in his paper at the Royal Society he provided new evidence that some polio vaccine may have been produced in Africa itself.

While the testing of the samples was carried out with a rigorous method, the provenance of the samples was far from clear. As pointed out by AIDS activist Billi Goldberg (personal communication, October 11, 2000) after the Royal Society meeting, Koprowski had claimed in 1992 that no samples of polio vaccine used in Africa remained at the Wistar (Koprowski, 1992). In summary, it might be said that whereas a Wistar sample revealing SIV or chimp cells would have been powerful evidence for the polio-vaccine theory, a negative finding in itself was not a significant blow against the theory. Yet the press statement put out by the Wistar Institute stated that “the findings provide strong evidence to refute the theory” (Wistar Institute, 2000).

The press conference was an event in itself, with dozens of journalists and half a dozen television cameras. The five invited participants from the meeting—Hahn, Korber, Hooper, Plotkin, and myself—were each given two minutes, in that order, to summarize our talks, and then questions were taken from the floor. Robin Weiss chaired. The questioning was vigorous, and there was a repeat of the heated exchange between Plotkin and Hooper, especially over Plotkin’s collection of statements from scientists saying they had not used chimp kidneys in vaccine preparation. The media interest was extraordinary for a scientific issue. As science communicator Julian Cribb remarked to me at the time, an announcement by the prime minister would hardly produce the same media enthusiasm. Part of the media interest can

be attributed to public relations efforts and the prospect of fierce controversy—fully realized—but, at a deeper level, the idea that the major killer AIDS might have arisen from a well-intentioned medical intervention against a previous killer disease, polio, provided journalists with an angle that was hard to resist.

As might have been expected from the timing of the press conference on the first day of the meeting and the repositioning of the results of the testing of Wistar samples just before the press conference, many news stories featured the first day’s events and the negative results on the samples in particular (e.g., Hawkes, 2000; Highfield, 2000). However, quite a few journalists looked more deeply, noting the lack of resolution of the debate (e.g., “AIDS Wars,” 2000; Cohen, 2000; Vidal, 2000).

Meanwhile, the Royal Society meeting continued, with a variety of contributions later on Monday and through the day Tuesday. Attacks on the polio-vaccine theory came from a number of different angles. As noted, Plotkin and Koprowski denied that any chimp kidneys were used in the manufacture of polio vaccines, while Korber dated the origin of HIV to about 1931. John Beale, one of the speakers, concluded that insufficient SIVs in simian kidney tissues would have survived the vaccine preparation process to cause infection. Hooper virtually single-handedly countered these scientific criticisms, displaying an amazing grasp of detail in a range of different fields.

What was striking to me was a systematic asymmetry in the discussion. The polio-vaccine theory was treated by hostile scientists as a fixed target to be shot down. Contrary findings, such as Korber’s 1931 dating and the testing of Wistar samples, were treated as refutations of the theory, which was accorded no flexibility. Hooper’s new evidence, such as that some polio vaccines may have been manufactured in Africa, was simply ignored. In contrast, the cut-hunter theory was not given much critical scrutiny and was allowed to remain quite vague and malleable, thus making it virtually impossible to refute. This asymmetry in treatment of the polio-vaccine and cut-hunter theories was in fact the subject of my paper at the meeting (Martin, 2001).

Robin Weiss, one of the meeting’s organizers, summed up at the end. Speaking as though on behalf of all the partici-

pants, he concluded that the polio-vaccine theory had been found wanting. In particular, he stated that chimp kidneys had not been used to produce polio vaccines. In effect, he presented his own views as if they were shared by nearly everyone. In reality, there had been no testing for consensus about any of the matters covered. Given that hundreds of people were present and most did not contribute to the discussion, it was impossible to know for sure what views prevailed. I personally spoke to quite a number in the audience who were not convinced that matters had been settled, some of whom saw Weiss's summing up as an attempt to prematurely close the debate.

### Politics of the Origin-of-AIDS Debate

The Royal Society meeting was a culmination of years of struggle over the polio-vaccine theory. Having described the lead-up to the meeting and the meeting itself, albeit in brief terms, it is now possible to summarize various political dimensions of the debate over the polio-vaccine theory of the origin of AIDS.

Let me first note that "political" is used here in a broad sense referring to the exercise of power. Analysis of political dimensions is not a commentary on conscious intentions or motives. My assumption in this analysis is that all participants have been well intentioned throughout, acting sincerely in accordance with their own interpretations of scientific evidence and the public good. This is quite compatible with an assessment of political dimensions, which reflect the role of interests—such as research funding, scientific status, and the image of science—in shaping the dynamics of the debate. The confluence of well-intentioned individuals, operating in systems of professional and economic power, gives rise to the politics of science. For example, Sir Aaron Klug, President of the Royal Society, in brief opening comments at the meeting, said there was no political agenda in postponement of the meeting. There may have been no conscious political motive in the decision, but that does not rule out influence from social and political factors.

First, consider the scientific marginalization of the polio-vaccine theory prior to 1999. One facet of this process was the blocking of submissions about the theory, including rejection of Pascal's paper (the short one) by several journals and rejection by *Science* of replies to Koprowski (1992) by Curtis and by Hamilton. A second facet of the marginalization process was refusal to investigate the theory, most prominently the refusal to test Wistar vaccine samples.

One explanation for this marginalization is that the theory is a threat to the image of medical research and especially to vaccination programs. If polio vaccinations were widely thought to have caused AIDS, then this might well stimulate much greater scrutiny of current medical inquiry, such as xenotransplantation, AIDS vaccines, and genetic engineering. Most of all, it would put a tremendous dent into the image

of medical research as a saver of lives. In his introduction to the Royal Society meeting, Simon Wain-Hobson stated that the probity of current vaccines would not be questioned and that if anyone said otherwise, then he and Robin Weiss would disabuse them. Koprowski claimed in his paper that the polio-vaccine theory was hindering polio vaccination efforts, sentiments that had been expressed by a number of other scientists (Hooper, 2000b:436, 783), though without any supporting evidence. This expression of worry about the way vaccination is perceived hints at the danger to the image of science posed by the polio-vaccine theory.

The most effective response to scientific marginalization was publication outside scientific journals. This included Pascal's 1991 paper, Curtis's 1992 *Rolling Stone* article, and Cribb's 1996 book *The White Death*. The *Rolling Stone* article in particular had a tremendous impact, cutting through the scientific marginalization and prompting a response from the scientific community, namely, the Wistar Committee report (Basilico et al., 1992).

Koprowski's lawsuits constituted another form of politics, serving to transfer the issue from the public domain to the legal system. After the lawsuits, there was much less media discussion of the theory, a correlation compatible with the documented "chilling" effect of defamation law on the media (Barendt et al., 1997). Arguably, then, the lawsuits contributed to marginalization of the theory.

So matters might have remained except for the appearance in 1999 of Hooper's book *The River*—another publication outside scientific journals. The book generated such wide interest among both scientists and the public that it could not be ignored. *The River* triggered two important responses from the scientific community: serious scientific investigations—among them the testing of Wistar samples—and the holding of the Royal Society meeting. These provided a visible signal that the theory was being addressed "scientifically." However, political marginalization of the polio-vaccine theory continued through these ostensibly scientific vehicles, mainly by the interpretations placed upon them.

At the organizational level, the Royal Society meeting had several political dimensions. First was the postponement of the meeting, enabling critics of the polio-vaccine theory to complete their investigations. Cancellation would have been more problematical given the visibility of the original meeting. Second was the choice of speakers, with numbers favoring opponents of the polio-vaccine theory. These numbers would have been seen by the organizers as a fair representation of the evidential support for each perspective; Hooper and Cribb told me before and during the meeting that they saw it as a form of stacking. Third was the last-minute rearrangement of the program to put the announcement of the results of testing of Wistar samples just before the press conference. Fourth was the press conference itself, presenting a take on the issue before the meeting was more than half completed. Fifth was Weiss's summing up, which

can be interpreted as rhetorically closing the debate, with the polio-vaccine theory refuted. These dimensions of the meeting worked in combination to make the meeting seem to be the scientific community's definitive rejection of the polio-vaccine theory, effectively communicated to the wider public through the mass media.

The Royal Society meeting also had several political dimensions at the epistemological level, namely, the struggle over knowledge claims. First was the emphasis on refuting the polio-vaccine theory without providing any convincing evidence for the cut-hunter theory, the main alternative. Second was the interpretation of the testing of Wistar samples as definitive evidence against the theory. Third was the treatment of the polio-vaccine theory as a fixed entity, without the capacity for modification or rebirth. Fourth was the assumption that scientific calculations or evidence—such as phylogenetic computer modeling—were sufficient to refute the polio-vaccine theory, without any need to address other bodies of evidence, such as interview material or historical archives.

It has long been recognized in the social studies of science that no evidence or calculation on its own is sufficient to refute a theory, since theories can be rescued by rejecting the evidence as incorrect or irrelevant or by modifying the theory, among other strategies (Barnes, 1974; Chalmers, 1976; Collins and Pinch, 1998; Hess, 1997). This occurred previously with the polio-vaccine theory when the case of the Manchester sailor was thrown out as incorrect.

It is certainly possible that calculations such as Korber's could be rejected or superseded, with different results obtained using modified assumptions or an entirely different model. The polio-vaccine theory could even survive a definitive finding that chimp kidneys were never used to produce polio vaccine, if a suitable monkey SIV were discovered. While Hooper has tied his argument to polio-vaccine manufacture using chimp kidneys and argues strongly for it against alternative routes of SIV contamination, in principle the theory could be resurrected or reformulated in other ways. For example, Goldberg and Stricker (2000) argue that human cell lines may have been used to produce the suspect polio vaccines, though this contention has been largely ignored.

Supporters of the cut-hunter theory have accorded it a remarkable degree of plasticity, with little done to pin down the proposed times and locations of infection and spread. Even some of its supporters admit that it is not easily falsifiable. In contrast, Hooper's version of OPV theory has been treated as a rigid, final structure that can be sunk by a single hit to any component. His evidence that polio vaccine may have been manufactured in Africa showed the potential modifiability of his picture. Perhaps because this evidence did not fit with the way the theory had been solidified in the minds of its opponents, and because it was outside the disciplinary scope of the scientists, it was simply ignored at the meeting.

Despite the organizational and epistemological hurdles put in the path of the polio-vaccine theory, it was not totally defeated at the Royal Society meeting. Hooper showed a remarkable capacity to counter the points made by opponents, and he had some degree of support within the meeting. More importantly, though, the politics of the issue could not be contained in a hermetically sealed meeting with a single definitive output. *The River* remains in print and ever more widely read. Many journalists have probed beneath and beyond the Wistar Institute's claim that testing of samples had refuted the polio-vaccine theory and have not accepted Weiss's portrayal of scientific closure at the meeting. The rise of the Web and the use of e-mail now mean that discussion of alternatives and new contributions can occur more readily without relying on publication in leading scientific journals.

Another political dimension to the issue is the issue of "undone science," namely, research that might have been done but wasn't due to social factors (Hess, 2001:64-69). Over the years, quite a number of scientists have been discouraged from investigating the polio-vaccine theory, or issues relating to it, due to their awareness that such research efforts would not be good for their careers. At the meeting, one participant told me that he had been given several warnings not to become involved with the OPV theory. Another told me that after circulating Pascal's paper years earlier, he had been shunned by the AIDS establishment. Yet another said that Koprowski's lawsuits had deterred an English translation of his work.

Disincentives for investigating risks of vaccines are not new. Bernice Eddy, the scientist who exposed contamination of early polio vaccines with monkey virus SV40, "was silenced, chastised and demoted" (Curtis and Manson, 1992: A-1; also O'Hern, 1985:150-59). Since then, the effects of SV40 have been understudied (Bookchin and Schumacher, 2000; Elswood and Stricker, 1994). Given the SV40 saga, as soon as SIVs were discovered in 1985 it should have been obvious that contaminated polio vaccines were a possible explanation for the origin of AIDS (Pascal, 1991:9-10). Curtis reported that "a senior AIDS researcher said it has been an open secret to many AIDS researchers for at least four years that polio vaccines might have been contaminated by HIV or a related retrovirus" (1992d:A-1).

But scientists did not go about exploring the possibility of contamination of early polio vaccines, with only Lecatsas and Alexander even publicly voicing the possibility. The task of investigation was left to nonscientists such as Pascal, Elswood, Curtis, and Hooper. The prime exception was Hamilton, a scientist who was sufficiently prominent and idiosyncratic to be able to resist peer pressure, but for others the cost may have loomed larger than the benefits. If the message wasn't clear enough already, the Royal Society meeting certainly sent a signal that pursuing the polio-vaccine theory is not a promising path for a mainstream scientist.

**Table 3. Some Political Dimensions and Associated Features in a Scientific Meeting**

Dimension	Features
Pre-meeting factors	Publication or rejection of papers Access to data or samples Access to funding Investigation or refusal to investigate Media coverage Legal action Undone science
Organizational	Decision to hold meeting Timing of meeting Cancellation or postponement Format of meeting Selection of speakers Arrangement of agenda Media coverage
Epistemological	Burden of proof Definitiveness accorded to evidence Flexibility accorded to theory Types of evidence accorded significance

Control of the media is another matter. Scientists, through public relations efforts, can influence media coverage but hardly control it. In 1992, Koprowski’s lawsuit discouraged further media coverage. Another lawsuit—Hooper (2000b:595-96, 808) reports that he has been threatened with an action for defamation—would signal to some that the Royal Society meeting on its own was not enough to remarginalize the polio-vaccine theory.

Every scientific meeting has politics. At the Royal Society meeting, the political dimensions were far more visible than usual, and it is for this reason that it is a useful vehicle for revealing what is otherwise much less obvious. Drawing on the discussion here of the Royal Society meeting, Table 3 gives some political dimensions of a scientific meeting, with a list of several features for each dimension.

Although the features listed here grow out of analysis of the Royal Society meeting, many of them will be relevant to other scientific meetings. Certainly some of the features, such as access to funding, undone science, format of meeting, selection of speakers, and burden of proof, will be of significance at most meetings even when political dimensions are submerged or downplayed.

The list of features in Table 3, derived from the examination of a single meeting, is far from complete or definitive. By studying other scientific meetings and noting political aspects, a more comprehensive list of features can be developed. However, a long list is not so useful as one giving features most frequently of significance, and for this an examination of other meetings is essential. With such a list, analysis of the political dimensions of scientific meetings—especially those that seem most apolitical—can be

facilitated. Of course, the politics of scientific meetings is simply one part of the wider politics of science. But meetings often play a special role in presenting and legitimating scientific ideas, so it is valuable to show that more goes on at scientific meetings than “just science.”

### Postscript

The politics of the origin-of-AIDS debate continued, predictably, after the Royal Society meeting. In April 2001, three studies were published in *Nature* and *Science* reporting tests of surviving vaccine samples, each modestly concluding only that their findings did not support the polio-vaccine theory (Berry et al., 2001; Blancou et al., 2001; Poinar, Kuch, and Pääbo, 2001). These were essentially the findings reported verbally at the Royal Society meeting. A fourth study reported a theoretical assessment of HIV-1 phylogeny, with findings seemingly incompatible with the polio-vaccine theory (Rambaut et al., 2001). Although these publications did not support the polio-vaccine theory, they were far from definitive refutations. Yet a typical media report stated that “Four new studies essentially refute the [OPV] theory” and quoted researcher Edward C. Holmes as saying “There is not one piece of hard evidence in favor of the polio vaccination theory” (Brown, 2001:A9). Robin Weiss, one of the organizers of the Royal Society meeting, wrote a commentary in *Nature* titled “Polio Vaccines Exonerated” (Weiss, 2001). Hooper sent a letter to *Nature* replying to the scientific points; it was rejected (personal communication, May 25, 2001). I sent a letter to *Nature* commenting on the exaggeration in media reports; it also was rejected.

In June 2001 the proceedings of the Royal Society meeting were published (Weiss and Wain-Hobson, 2001), with contributions by all the major speakers, some of the discussants, and a few additional contributions. A thorough analysis of these papers could reveal much about the epistemological politics of the issue, but here I comment only on a few matters relevant to the meeting. First, some of the published papers might be said to have been “sanitized” to some extent compared to what transpired at the Royal Society meeting (though Plotkin’s paper [2001] is remarkably forthright in criticism of Hooper). Just as scientific papers seldom reveal the passion and commitment that is involved in doing science (Mitroff, 1974), published papers seldom reveal the full dynamics of a scientific meeting.

Second, without inside information, it is difficult to examine the politics of selecting and editing scientific papers. Walter Nelson-Rees, a discussant at the meeting, wrote me on June 16, 2001, with an account and copies of correspondence with the Royal Society concerning his contribution. One aspect of this was that the Royal Society declined to publish certain passages because of the possibility of defamation. Without this information, a reader of the published article (Nelson-Rees, 2001) would have no inkling of the struggles that occurred over the text.

As well as the papers presented at the Royal Society meeting, the proceedings include an additional paper co-authored by Plotkin replying to Hooper’s talk (Plotkin et al., 2001). However, the proceedings do not include any additional paper by Hooper replying to Plotkin’s or any other talk. This would appear to represent a double standard by the editors in allowing parties from one side of the debate an opportunity not afforded to the other side. Hooper confirmed to me (personal communication, June 13, 2001) that he had not been given an opportunity to reply to Plotkin or other speakers. Again, knowledge from “behind the scenes” is essential for gaining a fuller understanding of the politics of a scientific meeting.

The Royal Society announced publication of the papers in a media release, and Hooper countered with his own comments (personal communication, June 12, 2001). However, there was little media coverage at the time, perhaps because the volume and complexity of scientific argumentation was too great. Even so, I will stick with my social scientist’s prediction made at the Royal Society meeting (Cohen, 2000:1851) that “Whatever happens at this conference, this controversy will continue.”

### Acknowledgments

I thank Edward Hooper, Gary R. Johnson, Allan Mazur, Michael Primero, Steve Wunsch, and several anonymous referees for helpful comments on earlier drafts of this paper, and many correspondents and participants at the Royal Society meeting for valuable insights.

### References

- “AIDS Wars” (2000). *Economist* (September 16):135-36.
- Altman, L.K. (1999) “New Book Challenges Theories of AIDS Origins.” *New York Times* (November 30):F1.
- Bailey, A. S. and G. Corbitt (1996). “Was HIV Present in 1959?” *Lancet* 347:189.
- Barendt, E., L. Lustgarten, K. Norrie, and H. Stephenson (1997). *Libel and the Media: The Chilling Effect*. Oxford: Oxford University Press.
- Barnes, B. (1974). *Scientific Knowledge and Sociological Theory*. London: Routledge and Kegan Paul.
- Basilico, C. et al. (1992). Report from the AIDS/Poliovirus Advisory Committee, September 18.
- Berry, N. et al. (2001). “Analysis of Oral Polio Vaccine CHAT Stocks.” *Nature* 410:1046-47.
- Blancou, P. et al. (2001). “Polio Vaccine Samples Not Linked to AIDS.” *Nature* 410:1045-46.
- Bookchin, D. and J. Schumacher (2000). “The Virus and the Vaccine.” *Atlantic Monthly* 285 (February):68-80.
- Brown, D. (2001). “Polio Vaccine-AIDS Theory Refuted.” *Washington Post* (April 26):A9.
- Chalmers, A.F. (1976). *What Is This Thing Called Science? An Assessment of the Nature and Status of Science and Its Methods*. Brisbane: University of Queensland Press.
- Cohen, J. (2000). “Forensic Epidemiology: Vaccine Theory of AIDS Origins Disputed at Royal Society.” *Science* 289 (September 15):1850-51.
- Collins, H.M. and T. Pinch (1998). *The Golem: What You Should Know about Science*. Cambridge: Cambridge University Press.
- Connor, S. (2000). “The Accused.” *Independent* (London) (September 8): Friday Review 1.
- Corbitt, G., A.S. Bailey, and G. Williams (1990). “HIV Infection in Manchester, 1959.” *Lancet* 336:51.
- Cribb, J. (1996). *The White Death*. Sydney: Angus and Robertson.
- Curtis, M.K. (1995). “Monkey Trials: Science, Defamation, and the Suppression of Dissent.” *William and Mary Bill of Rights Journal* 4:507-93.
- Curtis, T. (1992a). “‘Early Test’ for Theory on AIDS.” *Houston Post* (March 16):A-1, A-9.
- Curtis, T. (1992b). Letter. *Science* 256 (May 29):1260.
- Curtis, T. (1992c). “The Origin of AIDS.” *Rolling Stone* 626 (March 19):54-61, 106, 108.
- Curtis, T. (1992d). “Vaccines Not Tested for HIV?” *Houston Post* (March 18):A-1, A-12.
- Curtis, T. (1996). Unpublished letter to *Science*. In J. Cribb, *The White Death*. Sydney: Angus and Robertson.
- Curtis, T. and P. Manson (1992). “Scientist’s Polio Fears Unheeded.” *Houston Post* (April 17):A-1, A-12.
- Duesberg, P. (1996). *Inventing the AIDS Virus*. Washington, DC: Regnery.
- Elswood, B.F. and R.B. Stricker (1993). “Polio Vaccines and the Origin of AIDS.” *Research in Virology* 144:175-77.
- Elswood, B.F. and R.B. Stricker (1994). “Polio Vaccines and the Origins of AIDS.” *Medical Hypotheses* 42:347-54; erratum (1995) 44:226.
- Epstein, H. (1999). “Something Happened.” *New York Review of Books* (December 2):14, 16-18.
- Garrett, L. (1994). *The Coming Plague: Newly Emerging Diseases in a World Out of Balance*. New York: Farrar, Straus, and Giroux.
- Gilks, C. (1999). “Blame Me: Were Chimps the Source of HIV?” *New Scientist* 164 (November 13):54-55.
- Gillon, R. (1992). “A Startling 19,000-Word Thesis on the Origin of AIDS: Should the JME Have Published It?” *Journal of Medical Ethics* 18:3-4.
- Goldberg, B. and R.B. Stricker (2000). “Bridging the Gap: Human Diploid Cell Strains and the Origin of AIDS.” *Journal of Theoretical Biology* 204:497-503.
- Hahn, B.H., G.M. Shaw, K.M. De Cock, and P.M. Sharp. (2000). “AIDS as a Zoonosis: Scientific and Public Health Implications.” *Science* 287 (January 28):607-14.

- Hamilton, W.D. (1996). Unpublished letter to *Science*. In J. Cribb, *The White Death*. Sydney: Angus and Robertson.
- Harris, S.B. (1995). "The AIDS Heresies: A Case Study in Skepticism Taken Too Far." *Skeptic* 3 (2):42-79.
- Hawkes, N. (2000). "Tests Discredit Chimp Theory on HIV." *Times* (London) (September 12):6.
- Hess, D.J. (1997). *Science Studies: An Advanced Introduction*. New York University Press.
- Hess, D.J. (2001). *Selecting Technology, Science, and Medicine. Niskayuna, NY*: David J. Hess.
- Highfield, R. (2000). "Gene Tests 'Cast Doubt on Vaccine AIDS Claim.'" *Daily Telegraph* (UK) (September 12):10
- Hooper, E. (2000a). "No Shirking HIV Debate." *Guardian* (April 1):25.
- Hooper, E. (2000b). *The River: A Journey Back to the Source of HIV and AIDS*. Harmondsworth: Penguin; Boston: Little, Brown, revised edition.
- Karlen, A. (1995). *Plague's Progress: A Social History of Man and Disease*. London: Victor Gollancz.
- Klug, A. (2000). "No Shirking HIV Debate." *Guardian* (April 1):25.
- Koprowski, H. (1992). "AIDS and the Polio Vaccine." *Science* 257 (August 21): 1024-27; erratum, 257 (September 11):1463.
- "Koprowski Sues Rock Mag" (1993). *Science* 259 (January 8):180.
- Korber, B. et al. (2000). "Timing the Ancestor of the HIV-1 Pandemic Strains." *Science* 288 (June 9):1789-96.
- Lecatsas, G. and J.J. Alexander (1989). "Safe Testing of Poliovirus Vaccine and the Origin of HIV Infection in Man." *South African Medical Journal* 76 (October 21):451.
- Lederer, R. (1987/1988). "Origin and Spread of AIDS: Is the West Responsible?" *CovertAction Information Bulletin* 28:43-54; 29:52-65.
- Maggiore, C. (1999). *What If Everything You Thought About AIDS Was Wrong?* Fourth Edition. Studio City, CA: American Foundation for AIDS Alternatives.
- Martin, B. (1993). "Peer Review and the Origin of AIDS—A Case Study in Rejected Ideas." *BioScience* 43 (9):624-27.
- Martin, B. (1996). "Sticking a Needle into Science: The Case of Polio Vaccines and the Origin of AIDS." *Social Studies of Science* 26:245-76.
- Martin, B. (1998). "Political Refutation of a Scientific Theory: The Case of Polio Vaccines and the Origin of AIDS." *Health Care Analysis* 6:175-79.
- Martin, B. (2001). "The Burden of Proof and the Origin of Acquired Immune Deficiency Syndrome." *Philosophical Transactions of the Royal Society of London, Series B: Biological Sciences* 356:939-43.
- Meek, J. (2000). "Royal Society Accused in Row over Origins of HIV." *Guardian* (March 30):1.7.
- Mitroff, I.I. (1974). *The Subjective Side of Science: A Philosophical Inquiry into the Aims and Methods of Science*. Amsterdam: Elsevier.
- Moore, J.P. (1999). "Up the River without a Paddle?" *Nature* 401 (September 23):325-26.
- Nelson-Rees, W.A. (2001). "Responsibility for Truth in Research." *Philosophical Transactions of the Royal Society of London, Series B: Biological Sciences* 356:849-51.
- O'Hern, E.M. (1985). *Profiles of Pioneer Women Scientists*. Washington, DC: Acropolis Books.
- "Origin of AIDS' Update" (1993). *Rolling Stone* (December 9):39.
- "Panel Nixes Congo Trials as AIDS Source" (1992). *Science* 258 (October 30):738.
- Pascal, L. (1991). *What Happens When Science Goes Bad. The Corruption of Science and the Origin of AIDS: A Study in Spontaneous Generation*. Working Paper No. 9, Science and Technology Studies, University of Wollongong, NSW 2522, Australia.
- Plotkin, S.A. (2001). "Untruths and Consequences: The False Hypothesis Linking CHAT Type 1 Polio Vaccination to the Origin of Human Immunodeficiency Virus." *Philosophical Transactions of the Royal Society of London, Series B: Biological Sciences* 356:815-23.
- Plotkin, S.A., D.E. Teuwen, A. Prinzie, and J. Desmyter (2001). "Postscript Relating to New Allegations Made by Edward Hooper at the Royal Society Discussion Meeting on 11 September 2000." *Philosophical Transactions of the Royal Society of London, Series B: Biological Sciences* 356:825-29.
- Poinar, H., M. Kuch, and S. Pääbo (2001). "Molecular Analyses of Oral Polio Vaccine Samples." *Science* 292:743-44.
- Rambaut, A., D.L. Robertson, O.G. Pybus, M. Peeters, and E.C. Holmes (2001). "Phylogeny and the Origin of HIV-1." *Nature* 410:1047-48.
- "Rolling Stone Rolls Over for Koprowski" (1993). *Science* 262 (November 26):1369.
- Schoub, B.D., C.J. Dommann, and S.F. Lyons (1990). "Safety of Live Oral Poliovirus Vaccine and the Origin of HIV Infection in Man." *South African Medical Journal* 77:51-52.
- Shah, K. and N. Nathanson (1976). "Human Exposure to SV40: Review and Comment." *American Journal of Epidemiology* 103:1-12.
- Sharp, D. (1999) "A Controversial HIV/AIDS Hypothesis." *The Lancet* 354 (September 25):1129-30.
- Vidal, J. (2000). "'This Is Where Aids Started. I'm Quite Sure.'" *Guardian* (September 12):G2 2-4.
- Wain-Hobson, S. (1999). "The River: A Journey to the Source of HIV and AIDS." *Nature Medicine* 5 (10):1117-18.
- Weiss, R.A. (2001). "Polio Vaccines Exonerated." *Nature* 410:1035-36.
- Weiss, R.A. and S. Wain-Hobson, eds. (2001). "Origins of HIV and the AIDS epidemic." *Philosophical Transactions of the Royal Society of London, Series B: Biological Sciences* 356:777-977.
- Wistar Institute (2000). "No AIDS-Related Viruses or Chimpanzee DNA Found in 1950s-Era Polio Vaccine." Press statement, September 11.
- Zhu, T., and D.D. Ho (1995). "Was HIV Present in 1959?" *Nature* 374:503-4.

# PLANETARY SUSTAINABILITY

## The Age of Transition to Sustainability: The End of the Exponential Growth Period

John Cairns, Jr.

Virginia Polytechnic Institute and State University, USA

**Abstract.** Arguably, no curve in the world increases indefinitely—certainly no growth curve of human society. The quest for sustainable use of the planet involves estimating levels of activity, particularly resource use, that can continue indefinitely. Since the biosphere is dynamic, this continuation is not a stagnant, steady-state situation but rather a mutualistic interactive relationship between human society and the planet's ecological life support system. Human technology, creativity, and ingenuity may modify natural laws, but cannot be used to repeal them. Attempts to maintain the recent exponential growth of the human population, affluence, and artifacts cannot continue forever, or probably even for another century. At best, quality of life will be diminished, and, at worst, a substantial loss of human life will occur if both ethical and ecological issues are not freely and openly discussed. Sustainability may be visionary and unattainable, but it does offer an opportunity to improve the quality of life for future generations of humans and the other life forms, an opportunity that is far greater than possible with present unsustainable practices.

---

John Cairns, Jr. is University Distinguished Professor of Environmental Biology Emeritus in the Department of Biology at Virginia Polytechnic Institute and State University. Correspondence should be sent to Department of Biology, 1020 Derring Hall, Virginia Tech, Blacksburg, VA 24061, USA (E-mail: jcairns@vt.edu). Dr. Cairns was Curator of Limnology at the Academy of Natural Sciences of Philadelphia from 1948 to 1966, Professor of Zoology at the University of Kansas from 1966 to 1968, and has been at his present institution since then. Some of his honors include membership in the American Philosophical Society; National Academy of Sciences; Fellow, American Academy of Arts and Sciences; Foreign Member, Linnean Society of London; Fellow, Association for Women in Science; United Nations Environmental Program Medal for unique and significant contributions to environmental restoration and sustainability; Icko Iben Award for interdisciplinary research; Distinguished Service Award, American Institute of Biological Sciences, and Morrison Medal for outstanding accomplishments in the environmental sciences. He has written prolifically on restoration and sustainability.

*Death is knocking at the door.  
Let him knock, we have other things in mind.*  
—Stevenson (2001)

*Whoever thinks a faultless piece to see,  
Thinks what ne'er was, nor is, nor e'er shall be,  
In every work regard the writer's end,  
Since none can compass more than they intend;  
And if the means be just, the conduct true,  
Applause, in spite of trivial faults, is due.*  
—Alexander Pope, *Essay on Criticism*

The sine qua non of sustainable use of the planet is that one species, *Homo sapiens*, can persist for as long as the planet lasts. Some mammals have persisted for significant periods of time, but to postulate survival for an indefinite period of time would certainly be an unprecedented event. Theoretically, human creativity, technology, and ingenuity offer promise of achieving sustainability, but these attributes can also be used to promote unsustainable practices for the short-term benefit of a few individuals.

The use of the word *sustainable* as an adjective to modify the word *development* (e.g., World Commission on Environment and Development, 1987; Harris et al., 2001), which is usually associated with the word *growth*, is a cause for concern. Sustainable use without abuse seems a better descriptor, but would be less reassuring to those to whom growth and development are sacred. The phrase *smart growth* is even more appealing to advocates of perpetual growth, but there is no persuasive evidence that smart growth is indefinitely sustainable. Neither is there robust evidence that developing a more harmonious, mutualistic relationship between human society and the planet's ecological life support system will be sustainable. The biosphere is a dynamic system, especially over geological time, and to believe it will remain suitable for one species (humans) indefinitely may be the ultimate hubris. Still, achieving sustainability is a challenging undertaking that requires a reexamination of human society's relationship

with the biosphere and human society's responsibility to both its descendants and those of other life forms.

One major factor to consider during the transitional period is the degree to which sustainable practices are implemented globally. Those countries and areas not doing so are likely to experience emigration on a massive scale. This movement of population might temporarily alleviate carrying capacity problems in the country exporting people, but might easily destabilize a country aspiring to achieve sustainability. Sea level rise or exceptional hurricanes could easily result in numerous refugees, as could war, famine, or economic depression.

### The East Sea: A Glimpse of the Future?

Raspail's (1975) prophetic novel, *The Camp of the Saints*, tells of a flotilla of 100 rusty ships headed north from the Ganges carrying hundreds of thousands of impoverished people so desperate that they are willing to risk everything in hope of reaching the southern coast of France and a better life. Five more fleets from Africa and Asia join them, and the sheer numbers threaten to overwhelm France's resources and culture. The book describes the indecision and unwillingness of politicians to take action. They are torn between their humanitarian instincts and their desire to maintain their country's political stability and integrity. Raspail describes (in a review by Uhlich, 1994-1995) the vision that led to his book:

They were there! A million poor wretches, armed only with their weakness and their numbers, overwhelmed by misery, encumbered with starving brown and black children, ready to disembark on our soil, the vanguard of the multitudes pressing hard against every part of the tired and overfed West.

Raspail literally saw *them*, saw the major problem they presented—a problem absolutely insolvable by any present moral standard. To let them in would destroy France. To reject them would destroy them. So-called Christian charity would prove itself powerless. The times would be cruel.

More recently, Dickinson (personal communication, 2001) has called attention to the story of the *East Sea*, the rusty freighter that ran aground in mid-February 2001 near the French Riviera. Turkish smugglers had packed at least 910 Kurdish men, women, and children into the hull of the 90-foot ship, with a one-way destination in mind. The captain and crew fled by light boat after setting the ship facing land with the propellers turning so that it could not drift away.

Dickinson (personal communication, 2001) reports that one day after the *East Sea* grounding, 200 Africans in four boats landed illegally in Spain. If one boatload, planeload, or other conveyance of refugees were to be accepted, a torrent of vessels would head in the same direction. The countries of choice are unlikely to sink such vessels, certainly not in international waters or airspace, and the distance to the shoreline is not far, especially if a substantial number of ships or planes had to be tracked. As is the case for immigration into the United States, Britain, Germany, France, and a number of other countries, citizens are far from being of one mind in how to resolve this problem, and elected politicians are ever mindful of the effect of minority opinions on their chances for reelection. These worst case scenarios become increasingly probable daily as the world's population increases, the disparity in wealth becomes ever greater, resources become ever scarcer, and terrorists' weaponry becomes even more frightening.

This apocalyptic possible future scenario is presented here because the changes necessary for the transition to sustainable use of the planet requires behavior changes of both individuals and human society that would be unthink-

**Table 1. Factors Affecting Population (Bartlett, 1994)—Nature Chooses from the Right-hand Column, People Choose from the Left-hand Column**

Factors Increasing Population <sup>1</sup>	Factors Decreasing Population
Procreation	Abstinence <sup>2</sup>
Motherhood	Contraception/Abortion
Large families	Small families
Immigration	Halting immigration
Medicine/Public health/Sanitation	Disease
Peace	War
Law and order	Murder/Violence
Scientific agriculture	Famine
Accident prevention (55 mph speed limit)	Accidents
Clean air	Pollution (cigarette smoking)
Ignorance of the problem	Knowledge of the problem

Notes: 1. Many of the activities in the left-hand column are subsidized with taxpayer money (my comment, not Bartlett's). For details, see Myers with Kent (1998).  
 2. Added by Cairns, with Bartlett's approval.

**Table 2 Illustrative Choices that Will Hamper or Facilitate Sustainable Use of the Planet**

Hamper	Facilitate
1. Born to shop	1. Simple living
2. Exponential growth of resource use	2. Frugal use of resources
3. Flagrant individualism	3. Community spirit
4. Misery as the primary means of human population control (e.g., Boulding, 1971)	4. An enlightened social contract as the primary means of population control
5. Live for the moment	5. Compassion for future generations
6. Technology and ingenuity to free humans from natural laws	6. Acknowledgment of human dependence on ecological life support systems
7. Species extinction, if it actually occurs, does not bother me	7. Humans have an ethical and moral responsibility to cease anthropogenic extinction of other species
8. Economic development can and should raise all humans to the U.S. per capita level of affluence	8. The planet cannot support Earth's present population at the U.S. per capita level of affluence
9. Nobody can tell me what to do on my property	9. Property owners should be financially responsible for ecological damage resulting from their management practices
10. With low oil and coal prices, why spend money on alternative energy sources?	10. Solar and other alternative energy sources should be developed at an accelerated rate
11. No sharing of resources until human needs are fully satisfied	11. We should share resources equitably with other species—now
12. It is my right to drive wherever I please and own as many cars as I can afford	12. Environmentally, mass transit is essential for sustainable use of the planet

able for many people if the alternative were not so grim. A simple, ecological definition of sustainable use of the planet is “living on the services ecosystems provide without damaging the integrity of the systems that provide them.” Even under the best circumstances, the transition may be difficult, arguably impossible, because many people will not believe apocalyptic events are occurring until they personally experience them. The only hope is that reason guided by intelligence and evidence will prevail and, thus, thwart the catastrophes that exponential growth on a finite planet will otherwise bring.

### The End of Mass Migration

Humans have always moved over the land masses or even, as did the Polynesians, over vast uncharted expanses of the Pacific Ocean in the hopes of finding uninhabited areas with abundant resources. Some cultures, including the Polynesians, transported domesticated or semi-domesticated plants and animals with them to ensure that certain familiar types of food and fiber would be available in their new locations. Since other creatures preceded humans on the planet and since the evidence indicates that humans did not initially have a global distribution, human migrations have displaced other species and simultaneously competed with them for both space and resources. As human population size and levels of material affluence increased exponentially, subsequent migrations resulted in the displacement of members of the human species, and, since Earth is a planet of finite size, diminished average per capita resources. In the United States particularly, the view still

exists that the planet provides a vast cornucopia of infinite resources that needs only the technology to acquire them. The persistence of this belief is explained, but not justified, by the fact that the United States, which has less than 5% of the world's population, consumes nearly 25% of Earth's resources. In short, the size of the ecological footprint (e.g., Wackernagel and Rees, 1996) in the United States (and Canada) is substantially larger than most of the rest of the world and significantly larger than most other developed countries.

### The End of Exponential Growth on a Finite Planet

Bartlett (1994) has produced a superb video on exponential growth, which should be required viewing in a world where exponential growth is almost worshiped and its deficiencies ignored or denied. Even a “modest” growth rate in population of 1.7% results in a doubling time of just over forty years. Table 1 (Cairns, 1999) lists factors that would increase population and those that would decrease population. Most people would unhesitatingly choose activities that would primarily increase population and would choose sparingly, if at all, from activities that decrease population. However, if activities that decrease population are not chosen, nature will do so indiscriminately. Table 2 (Cairns, 1999) lists choices that will either hamper or facilitate sustainable use of the planet. The items listed that hamper sustainable use describe the behavior patterns in vogue today, and the items listed that facilitate sustainable use are choices that most affluent people would soundly reject. Durant and Durant note that “inequality is not only natural and inborn, it grows with the complexity of

civilization” (1968). In addition, they note: “We conclude that the concentration of wealth is natural and inevitable, and is periodically alleviated by violent or peaceable partial redistribution” (1968). The burning question of the twenty-first century is how this redistribution will occur, since the disparities now existing are arguably the greatest in human history. Add to this disparity the greatest array of mass technologies for destruction in human history, and the question becomes how to avoid catastrophic changes in both the environment and human society. Unquestionably, changes will not be attractive to most people who presently are enjoying the “good life.” A few illustrative examples of changes follow.

#### *Ceasing the Worship of Economic Growth*

As Douthwaite (1999) notes, economic growth has enriched the few, impoverished the many, and endangered the planet. Hardin (1993) has provided a superb analysis of why human society should live within limits. Ehrlich (2000) decisively destroys the notion that there is a single, genetically determined “human nature” and provides robust scientific evidence to both explain the human predicament and what must be done to escape it. McNeill (2000) has documented the degree to which the twentieth century witnessed environmental transformations of a scale and subtlety never before seen and the consequences of these transformations, which remain the most challenging problems of the human condition in the twenty-first century. As Myers with Kent (1998) document, much environmental damage is caused by perverse governmental subsidies, which politicians are encouraged to maintain because of a sizable army of well paid, special interest lobbyists. As a caveat, it is worth reaffirming that not all growth is environmentally damaging. Growth in spirituality, sense of community, sense of place, in human society’s relationship with the environment, etc. require neither enormous resources nor substantial funding and are attributes that most humans claim to desire.

#### *Reducing Ecological Footprint Size*

Flavin (2001) is persuasive in telling of the disparity between rich and poor nations and the ecological health of the so-called E-9 nations that account for 57% of the world’s population and 80% of the total economic output. An example of this disparity is that the per capita purchasing power in the United States was \$29,240 in 1998 and was \$2,060 in India (Flavin, 2001). Flavin also summarizes the myriad signs of human-induced global climate change and a number of other environmental challenges that are testing environmental limits. Both social and environmental reasons are compelling for reducing the size of any ecological footprint that is above the global norm, as well as reducing the global norm if this footprint damages the ecological life support system.

#### *Averting Disasters Stemming from Destructive Practices*

Abramovitz (2001) documents disasters caused by ecologically destructive practices and emphasizes fostering resilience in nature and communities. Some ecosystems are perturbation-dependent (e.g., floods, fires, hurricanes) and, arguably, all ecosystems may be, differing only in the size and periodicity of the perturbation and the temporal and spatial spans that they encompass. A recent trend in population modeling is to design models that are spatially explicit, thus capturing heterogeneity of natural world landscapes (e.g., Dunning et al., 1995). The relationship of this type of modeling to sustainability is quite clear in view of the number of people displaced by environmental disasters. Many of the displaced individuals appear in countries ill prepared to host them. Sustainable use of the planet requires that ecologically destructive practices be diminished to the extent feasible. In addition, increasing the resiliency of natural systems to these practices will increase natural capital, which is good for both human society and ecosystems.

#### *Adjusting to Nature’s Pulsing Paradigm*

The dominant global paradigm could be characterized as the perpetual exponential growth paradigm. When considering daily activities, from church membership to mutual fund performance, growth is usually the first descriptor used—with the caveat that the more exuberant the growth the better. Of course, some notable exceptions exist! For example, growth in small geographic areas may be a concern to policymakers because of a loss in the sense of community or ease in driving from one area to another. At the national or global economy, growth at a lower rate than that of the recent past poses many concerns.

Although not usually explicitly stated, some sustainable use models focus on either a steady state condition (rare) or “smart growth” or “sustainable growth.” However, sustainability, especially in the natural capitalism form, envisions a harmonious relationship with the planet’s ecological life support system, its natural capital, and its ecosystem services. Odum, Odum, and Odum note, “while the steady state is often seen as the final result of development in nature, a more realistic concept may be that nature pulses regularly to make a pulsing steady state—a new paradigm gaining acceptance in ecology and many other fields” (1995). They further state, “we suggest that if pulsing is general, then what is sustainable in ecosystems is a repeating oscillation that is often poised on the edge of chaos” (1995). Odum and colleagues make a persuasive case for the prevalence of pulsing at all ecological scales. The widespread energy transfer through the coupling of oscillators seems to be the general rule and steady states are the exception, although they can be induced by eliminating the outside pulses (Beyers and Odum, 1993). If the hypoth-

esis is correct (and evidence suggests it is), then the end of the exponential growth period for human society will not involve achieving a steady state of sustainable use of the planet, but rather the more difficult adjustment of coupling human society's behavior and practices to the pulsing system described by Odum and colleagues.

Persuasive reasons suggest the need to adjust to a pulsing system:

- (1) Natural capital is the basis of all forms of human society's capital (Hawken, Lovins, and Lovins, 1999);
- (2) Human society is dependent on the delivery of ecosystem services (Costanza et al., 1997) and, if natural systems pulse, then inevitably some pulsing would occur in the delivery of ecosystem services;
- (3) Damaging the integrity of natural systems by ignoring their pulsing nature will almost certainly, in the long run, require far more of an adjustment of human society than adjusting to the pulses that should be, as the science develops further, more and more predictable.

Some common situations in human society mitigate against living harmoniously with nature's pulses:

- (1) In democratic societies, elections that are at four-year or other intervals almost certainly will not correspond with the pulsing periods of nature. The pressures on elected politicians for reelection or on dictators to remain in power will tempt them to ignore nature's pulses;
- (2) Human economies also pulse, and the temptation to diminish environmental protection during an energy crisis (as is perceived to exist in the United States in 2001) or some other similar crisis will almost certainly ensure that the economic cycles will be given more attention than the natural cycles;
- (3) Arguably, the most intractable situation is the need to admit that humans are not as much in control of events as they have convinced themselves they are. Denial of dependence is more likely than admission of dependence on natural systems.

On the other hand, if human society is going to make a major paradigm shift, why not do it all at once—move from the perpetual exponential growth paradigm to the nature's pulsing paradigm in one leap, using the illusory steady state as a fleeting benchmark en route from one major paradigm to another.

### *Implementing the Precautionary Principle*

The precautionary principle (PP) asserts that, when an activity raises threats of harm to human health or the environment, precautionary measures should be taken even if some cause-and-effect relationships are not fully established scientifically (e.g., Raffensperger and Tickner, 1999). Arguably, this activity would be the most difficult

human behavioral change to achieve in the transition from exponential growth to sustainability. For example, the evidence for global warming is sufficiently documented for this phenomenon to appear on the cover of the April 9, 2001 issue of *Time* and be covered in a special report (Bjerklie et al., 2001).

Global climate change of even modest dimensions will definitely have a major effect on the quest for sustainable use of the planet. Greenhouse gases of anthropogenic origin are a major factor in global warming, and the United States is responsible for roughly 25% of these. Yet the United States has not signed the Kyoto Accord to limit anthropogenic production of greenhouse gases, and, as of early 2001, none of the nations that signed the accord has ratified it. If there was ever a case for precautionary action, this one is certainly a top contender; yet, the fact that some scientists disagree with the conclusions of mainstream science (e.g., Singer, 1998) is used as an excuse for procrastination. Ignored in this delay is the fact that disagreements will always exist among scientists, because scientists are responsible for challenging and testing every hypothesis. An absolute consensus, therefore, is not only extremely unlikely, but is also extremely undesirable. If the precautionary principle is not given serious attention in a matter of this magnitude, it is unlikely to affect any policy decision, however horrendous the consequences of not taking precautionary action.

### **A Hard or Soft Landing to Sustainability?**

Odum (2001) is hopeful that a harmonious relationship will develop between natural and techno-ecosystems. However, a mutualistic coexistence is only one of the possible outcomes, since one with severe consequences to human society is also quite possible (Cairns, 1994, 1997). Overshooting carrying capacities (Cairns, 1998, 2001) is a highly probable outcome (i.e., a hard landing), and a mutualistic coexistence (i.e., a soft landing) becomes increasingly less probable the longer present practices continue.

### **Seeking a Contract with Nature**

Humans are always seeking contracts. Children behave in certain ways to get adult approval. Teenagers behave in other ways to acquire the esteem of their peers. Adults use certain types of behavior to bring about community approval and may receive censure for other behaviors. In the educational system, grades above a certain level will gain an appropriate degree. Certain types of behavior in areas of employment will get promotion and monetary rewards; other types will perhaps even end in unemployment. However, even within human society, these social contracts are being increasingly ignored. For example, the conductor at a symphony concert raises his baton and the silence is con-

tagious—and then a cell phone rings and totally disrupts what would otherwise have been an enchanting moment.

Nature makes no contracts, even with a species (humans) able to bend but not abolish nature's laws. With appropriate technology, humans inhabit parts of the planet previously inaccessible to them and utilize amounts of energy available to no other species. However, nature can still exact penalties worldwide—floods, antibiotic resistant diseases, agricultural pests that become highly resistant to pesticides, and, of course, the ultimate control, death.

Nature does not negotiate or bargain. If certain laws are followed, human society will probably be rewarded, but nature is dynamic and constantly changing, so this situation is by no means assured. Patently, the most important factor is the lack of understanding humans have of how complex, multivariate natural systems work over large temporal and spatial spans. For example, the residence time of carbon in the atmosphere is approximately 100 years (Bjerklie et al., 2001). Given the pulsing nature of ecosystems, the response times of ecosystems may be on temporal spans not yet grasped by humans and in a form not readily recognized by present states of knowledge. Odum's (2001) fear that human society will overshoot carrying capacities and refuse to become proactive is a regrettable, but highly probable, scenario. Despite formidable evidence to the contrary, I continue to have faith that reason will prevail, and an environmental ethos, coupled with ethics and fairness, will emerge. Without in any way denigrating human technology, I believe that reason and ethos are the most admirable products of the human mind and that social evolution to reach a harmonious mutualistic coexistence with natural systems is possible. If such a coexistence is not possible, even a charitable person might conclude that intelligence, as humans define it, is an evolutionary failure.

### **Cherishing Natural Systems**

It is difficult to find precisely the right word to describe a sustainable, mutualistic relationship between human society and natural systems. Anthropogenic effects are so ubiquitous that natural systems, even as they were a century ago, are extremely rare today. By natural system I mean healthy, self-maintaining ecosystems with their integrity intact. *Respect* is an inadequate descriptor because, as viewed today, it is regarded as optional, not obligatory, and, in the present usage of the word, may well be a facade for those who wish a positive environmental image but are unwilling to commit to a level of personal involvement that would give the word substance. *Esteem* is a marginally better word, possibly because its meaning has been less degraded than the word *respect*. *Reverence* is arguably a satisfactory word because it implies a reverence for life even if it remains beyond society's control and is something that human society neither dominates nor destroys. *Sacred* is perhaps a better word than *rev-*

*erence* because it combines the components of inviolate and mysterious. *Sacred* requires a degree of reverence that cannot be set aside for economic development, job security, or personal exploitation of private property that damages adjacent ecosystems or the bioregional ecological fabric. *Sacred* also has a strong element of mystery which, among other things, indicates that violating the system for short-term gains (or even long-term gains) may well result in consequences to all of human society that are horrible to contemplate, yet increasingly probable as the environmental damage continues.

The word *cherish* carries the explicit requirement of active, tender, and nurturing care and seems a good way, if not the perfect way, to describe a mutualistic relationship between human society and natural systems. Cherishing does not permit endangering the planet's ecological life support system for perceived economic benefits. A superb alternative is the word *treasure* as a verb, which means "to hold or keep as precious" and also "to collect and store up (something of value) for future use." Furthermore, as Myers with Kent (1998) have shown, many economic enterprises that are so damaging to the environment are actually subsidized by tax dollars (or other currencies); if these enterprises paid for the damage they did to the environment and had no subsidies, they would probably not survive economically. The fact that human society uses government funding to subsidize damage (Myers with Kent, 1998) to the planet's ecological life support system shows how far society has to change in establishing a mutualistic relationship between human society and natural systems. Furthermore, Hawken, Lovins, and Lovins (1999) have shown that enterprises can be environmentally sensitive and profitable as well, even without government subsidies.

The central issue is, of course, whether human society will come to cherish or treasure the planet's ecological life support system before suffering serious consequences or after unmistakable catastrophic consequences are clearly evident to all of society. Kane (2001) describes how the lives of humans have become extremely complicated and documents the hidden social costs of a highly materialistic society. The literature calls for repairing (e.g., Hayes, 2000) and protecting (e.g., Caldwell, 1972) the biosphere, even though humans are increasingly attempting to fill non-material needs materially (Kane, 2001). In the quest for short-term gratification—which only seems to increase loneliness and decrease civility—humans may be irreparably damaging the lives of present and future generations. It does not seem possible for human society to drive more than 50 to 75% of the species on the planet to extinction without causing its own extinction. Life on Earth has survived some dramatic extinctions and recovered from them, although not to any semblance of the predisturbance condition. Cherishing the biospheric life support system is an act of enlightened self-interest

for human society, not a “triaging” activity for environmental weirdos.

I agree with Dubos that “humanized environments could be very different from what they are, but this does not mean that they can safely be anything” (1980:48). Few humans now alive could survive, let alone flourish, in a true wilderness. Recent television shows have displayed the difficulties of doing so effectively—sometimes in nauseating detail. Even these experiments were for relatively short periods of exposure from which one could always opt out. Dubos (1980) also comments that much of what humans think of as “nature” is merely human-altered landscapes with a harmonious combination of woodland, meadows, and cultivated fields. In short, from a human perspective, nature has been both embellished and despoiled. The quest for sustainability (at least as I view it) is not for a return to true wilderness over much of the planet, but, rather, an attempt to cease despoiling ecosystems for short-term material and economic gain. This position is between “nature knows best” and “economists and developers know best.”

## Conclusions

There is no robust evidence that sustainable use of the planet by a single species is possible. However, human suffering and biotic impoverishment (loss of both species and habitat) will be markedly reduced by a systematic and orderly transition from a series of unsustainable practices to practices less damaging to the integrity of the planet’s ecological life support system. Earth’s biosphere is a dynamic system, and a sustainable relationship with it will require establishing a mutualistic relationship between it and human society. This change will require precautionary measures to protect the ecological life support system when the consequences of misjudgment are severe and the outcome uncertain based on available evidence.

Many people who denigrate cherishing or treasuring natural systems attempt to mask esteem for the environment as putting them (other creatures) before us (human society) and as a period of intellectual stagnation. Achieving sustainable use of the planet by having compassion for other species, compassion for future generations, and compassion for those less fortunate members of the human species now present is arguably the greatest challenge human society has ever or will ever face. The transition must be guided by reason, ethics, and a sense of fairness and equity rather than by a profit motive, which in itself is not bad, unless it suppresses these other attributes.

Individuals are not defined by their material possessions, although many advertising agencies would have society believe otherwise. Rather, individuals are valued for their actions, or for their actions and practices, that benefit others, including other species. In the past, many extraordinarily wealthy people realized this fact and established organizations, funds, and the like (such as Alfred Nobel

and Andrew Carnegie). The dream of sustainable use of the planet can be realized if society takes action to implement this vision. The human species may not be destined to last forever, but it can make its existence more glorious than it now is. Sustainability requires the best from each individual so that, collectively, a heritage will be left that future generations will cherish. Persuasive evidence indicates that the time to realize this dream is growing ever shorter, but it is not yet irrevocably beyond human society’s grasp. Hope rests in the idea that ethics in environmental politics will emerge as one of the guiding forces of this paradigm shift!

## Acknowledgments

I am indebted to Eva Call for transcribing the dictation of the first draft of this manuscript and to my editorial assistant Darla Donald for assistance in writing and preparing manuscripts. The Cairns Foundation paid for the cost of transcribing and processing this manuscript.

## References

- Abramovitz, J. (2001). “Averting Natural Disasters.” In Worldwatch Institute (ed.), *State of the World*. New York: W.W. Norton.
- Bartlett, A.A. (1994). *Arithmetic, Population, and Energy*. A 65-minute videotape copyrighted by the Regents of the University of Colorado. Copies available from Kate Albers, Information Technology Services, University of Colorado, Boulder 80309-0379 (telephone: 303-492-1857).
- Beyers, R.J. and O.T. Odum (1993). *Ecological Microcosms*. New York: Springer-Verlag.
- Bjerklie, D., A. Dorfman, W. Dowell, R. Buchanan, M. Calabresi, J.F. Dickerson, D. Thompson, W. Kan, J. Kirwin, and J. Ressler (2001). “Feeling the Heat.” *Time* 157(14):22-36.
- Boulding, K.E. (1971). “Foreword to T. R. Matus *Population: The First Essay*.” In K.E. Boulding, *Collected Papers*, Vol. 11. Boulder, CO: Colorado Associated University Press.
- Cairns J., Jr. (1994). “Ecological Restoration: Re-examining Human Society’s Relationship with Natural Systems.” The Abel Wolman Distinguished Lecture. Washington, DC: National Academy of Sciences.
- Cairns J., Jr. (1997). “Global Coevolution of Natural Systems and Human Society.” *Revista de la Sociedad Mexicana Historia Natural* 47:217-28.
- Cairns J., Jr. (1998). “Consilience or Consequences: Alternative Scenarios for Societal Acceptance of Sustainability Initiatives.” *Renewable Resources Journal* 16(2):6-12.
- Cairns J., Jr. (1999). “An Epic Struggle: Sustainability and the Emergence of a New Social Contract.” *The Social Contract* IX(4):211-18.
- Cairns J., Jr. (2001). “Speculative Scenarios about Sustainable Use of the Planet.” *The Social Contract* XL(2):146-52.
- Caldwell, L.K. (1972). *In Defense of Earth: International Protection of the Biosphere*. Bloomington, IN: Indiana University Press.
- Costanza, R., R. d’Arge, R. de Groot, S. Farber, M. Grasso, B. Hannon, K. Limburg, S. Naeem, R. O’Neill, J. Paruelo, R. Raskin, P. Sutton, and M. van den Belt (1997). “The Value of the World’s Ecosystem Services and Natural Capital.” *Nature* 387:23-260.
- Douthwaite, R. (1999). *The Growth Illusion*. Gabriola Island, British Columbia: New Society Publishers.
- Dubos, R. (1980). *The Wooing of Earth*. New York: Charles Scribner’s Sons.
- Dunning, J.B., D.J. Stewart, B.J. Danielson, B.R. Noon, T.L. Root, R.J. Lamberson and E.E. Stevens (1995). “Spatially Explicit

- Population Models: Current Forms and Future Uses. *Ecological Applications* 5:3-11.
- Durant, W. and A. Durant (1968). *The Lessons of History*. New York: MJF Books.
- Ehrlich, P.R. (2000). *Human Natures*. Washington, DC: Island Press.
- Flavin, C. (2001). "Rich Planet, Poor Planet." In Worldwatch Institute (ed.), *State of the World*. New York: WW. Norton.
- Hardin, G. (1993). *Living Within Limits*. Oxford, UK: Oxford University Press.
- Harris, J.M., T.A. Wise, K.P. Gallagher, and N.R. Goodwin (2001). *A Survey of Sustainable Development*. New York: Island Press.
- Hawken, P., A. Lovins, and H. Lovins (1999). *Natural Capitalism: Creating the Next Industrial Revolution*. New York: Little, Brown and Company.
- Hayes, D. (2000). *The Official Earth Day Guide to Planet Repair*. Washington, DC: Island Press.
- Kane, H. (2001). *Triumph of the Mundane: The Unseen Trends that Shape Our Lives and Environment*. Washington, DC: Island Press.
- McNeill, J.R. (2000). *Something New Under the Sun: An Environmental History of the Twentieth-Century World*. London: W.W. Norton.
- Myers, N. with J. Kent (1998). *Perverse Subsidies: Tax \$s Undercutting Our Economies and Environments Alike*. Winnipeg, Manitoba: International Institute for Sustainable Development.
- Odum, E.P. (2001). "The 'Techno-Ecosystem.'" *Bulletin of the Ecological Society of America* 82(2):137-38.
- Odum, W.E., E.P. Odum, and H.T. Odum (1995). "Nature's Pulsing Paradigm." *Estuaries* 18(4):547-55.
- Raffensperger, C. and J. Tickner (1999). *Protecting Public Health and the Environment: Implementing the Precautionary Principle*. Washington, DC: Island Press.
- Raspail, J. (1975). *The Camp of the Saints*. English translation. Petoskey, MI: The Social Contract Press.
- Singer, S.F. (1998). *Hot Talks, Cold Science: Global Warming's Unfinished Debate*. Oakland, CA: Independent Institute.
- Stevenson, R.L. (2001) "Trilbies Are Fashionable Again." Quoted by A. Cook in BBC Letter to America, April 23.
- Uhlich, G. (1994-1995). "Out of Africa." *The Social Contract* V(2):102-3.
- Wackernagel, M. and W.E. Rees (1996). *Our Ecological Footprint*. Gabriola Island, British Columbia: New Society Publishers.
- World Commission on Environment and Development (1987). *Our Common Future*. Oxford: Oxford University Press.

## RATIONAL CHOICE THEORY

# Stuck in the Pleistocene: Rationality and Evolved Social Roles

Derek Reiners

Indiana University, USA

**Abstract:** This article argues that an evolutionary psychological perspective could be useful for developing second-generation models of rationality. The standard model of complete rationality is inadequate primarily on the grounds that it generates predictions inconsistent with empirical data. The model is extremely useful and should not, nor cannot, realistically be dismissed. It accurately predicts outcomes in markets and openly competitive situations. However, behavioral phenomena that the standard theory has trouble explaining—such as mass contribution to public goods, types of cooperation, and altruism—are usually treated as “anomalies.” These outcomes are too prevalent and important to be treated as such. I attempt to build upon Elinor Ostrom’s proposal to generate new models of rationality, which recognize the use of heuristics, norms, and rules, and are able to better account for empirical findings. I argue that second-generation models would benefit from the acknowledgment of social roles—but more specifically, evolved social roles. Evolved social roles are intuitive bundles of norms, expectations, and social strategies that shape an individual’s player type. If we extend rationality to incorporate underlying biopsychological mechanisms, such as evolved social strategies and evolved social roles, then we gain an understanding of a wider array of decision-making processes and social phenomena.

---

Derek Reiners is a Ph.D. candidate in Political Science at Indiana University—Bloomington. He is author or coauthor of several articles that apply evolutionary principles to the understanding of collective action and individual and collective strategy. His current research focuses on the impact of institutions on agency decision-making in the arena of wildfire policy in the Rocky Mountain West. Correspondence should be addressed to Department of Political Science, Indiana University, Woodburn Hall 210, Bloomington, IN 47405, USA (e-mail: dreiners@indiana.edu).

The privileged position that rational choice theory (RCT) has come to enjoy within political science has not gone unopposed. RCT has come under heavy fire for its theoretical and methodological weaknesses (Green and Shapiro, 1994). Perhaps the most serious charge, however, is that models built around an egoistic rational maximizer fail to predict some of the most significant social and political behavior we observe. Even practitioners have had to admit to certain disparities. And yet, despite well-founded criticism, RCT remains one of the most compelling and popular tools that social scientists have at their disposal. One of the reasons for its prevalence is that RCT creates the impression that we are closer to bringing social science to the same analytic plane as the natural sciences. RCT reinforces the conception that social science applies to a broad body of nested systems ultimately reducible to a basic unit of analysis. The basic unit (the individual actor) is defined by a set of properties, which can be used to predict outcomes within higher nested social and political systems. The predictive power of economic modeling enables the social scientist to emulate some of the most estimable capacities of the natural scientist. This makes RCT a powerful and useful tool. However, the fact remains that its predictions often fail to be supported by empirical findings.

The theory of norm-free complete rationality assumes (a) individuals have complete and transitive ordering of preferences, (b) all information generated by a situation is used in making decisions, (c) actors maximize their own expected payoffs, and (d) all needed resources to act are possessed (Ostrom, 1999:500). This theory has been found extremely useful in certain situations. Moreover, it has been supported by empirical evidence in many situations, such as in openly competitive contexts or where one-shot or finitely repeated games are played. But even in these experimental contexts, findings do not always support the theory (Ostrom, 1998).

---

*Editor’s Note:* An earlier version of this article won the 2000 Graduate Student Paper Award of the Association for Politics and the Life Sciences.

Laboratory and field experiments regularly contradict RCT-generated predictions by revealing human behavior that fails to match them. Those observations and phenomena that do controvert RCT-generated predictions are often dismissed as “anomalous.” I argue that many of these phenomena do not have to be treated in this manner. For my purposes I will discuss three especially salient phenomena of human behavior that consistently puzzle RCT. They are (1) contribution to public goods, (2) cooperation that requires risk or investment, and (3) altruism.

### Three Puzzling Phenomena

One thing that continually besets RCT’s critics and practitioners alike is RCT’s failure to predict certain “successful” collective action outcomes that have been clearly and repeatedly observed. For instance, the theory has trouble explaining why large numbers of individual actors contribute to the production and provision of public goods. This includes, perhaps most famously, the *voting paradox*. In the context of a popular election involving a large number of voters, the theory of complete rationality predicts that, since the effect that a voter has on the outcome of a large-scale election is literally infinitesimal (along with the benefit to the individual), the costs associated with making the effort to vote will convince the rational actor not to bother (Downs, 1957). Yet we see clearly that masses of people do, in fact, vote. Even more, many sacrifice a great deal to obtain the right to do so. There is little economic explanation for why the rational actor would do this except to say that it fulfills some need to express “civic virtue” (Riker and Ordeshook, 1968).

Political nonparticipation is also the predicted outcome for sufficiently large interest groups and social movements. Mancur Olson’s seminal work, *The Logic of Collective Action*, points out that, unless the group is small enough, or unless participants receive selective benefits, there is little reason to expect that a rational actor would participate in these activities (Olson, 1965). Olson’s “zero contribution thesis” is derived as the predicted equilibrium of an N-person Prisoner’s Dilemma game, and it is considered the standard theoretical model of collective action problems (Ostrom, 2000). In spite of this, a great many people do, in fact, participate in social movements; and they contribute to interest group activities even when their effect or their benefit is negligible.

Other phenomena that confound RCT are short-term cooperation and cooperation that requires risk or investment. Cooperation that does not involve immediate, direct exchange or payoffs implies some state of trust in the mind of the actor—essentially trust that the other actor(s) will reciprocate the favor sometime in the future. The closest RCT comes to incorporating trust is by way of the so-called “folk theorem.” In order to generate predictions other than noncooperation, theorists using standard RCT have found it

necessary to assume real uncertainty about the duration of a situation or to assume that some players are “irrational” in their willingness to reciprocate cooperation with cooperation (Ostrom, 1998:4). The folk theorem obliges certain conditions under which cooperation will occur—just as long as the payoffs are sufficiently high for cooperation, and as long as the iterations of the game are indefinable. However, RCT predicts that as soon as an actor ascertains the finitude of a game, the inclination to cooperate will disappear.<sup>1</sup>

Actors in public-good experiments are predicted to “learn” Nash equilibrium strategies. However, the fact is that cooperative behavior in finitely iterated games and indirect (or generalized) exchange strategies are consistently observed in laboratory experiments and in the field—in spite of a high number of iterations (Ostrom, 1998). Contrary to RCT predictions, laboratory experiments have yielded results indicating that players freely engage in costly monitoring and sanctioning behavior (Ostrom, 1999:505); they do not use backward induction in their decision-making; they do not learn to reach the Nash equilibrium in spite of scores of iterated rounds; they do cooperate in one-shot games; and Nash equilibrium strategies do not predict individual behavior in social dilemmas. The combined effect of these frequently replicated findings suggest rejection of the predictions derived from a complete model of rationality (Ostrom, 1998).

Cooperative behavior that is not contingent upon any past or future cooperation by others may simply be considered *altruism*. Altruism is any action done deliberately to benefit another without any expectation of reciprocity. It is a free gift. Altruism is yet another significant behavioral phenomenon that fails explanation by standard RCT. Rational self-interested actors simply do not squander their resources.

Altruism is one of those human expressions that are difficult to identify for certain. One is rarely sure whether someone acts for strictly altruistic reasons, or if there are some underlying expectations behind an action. Pure unadulterated altruism is somewhat of an empirical rarity, but the mere existence of altruism presents an important theoretical challenge for a theory of pure rationality (Monroe, 1994).

Some economic theorists have resisted the temptation to treat altruism as a statistically insignificant anomaly; however, most of these approaches have been unsatisfying. Political scientist Kristen Monroe observes that modern economists approach altruism theoretically in one of two ways (Monroe, 1994:866). First, they may treat altruism as a subtle variant of self-interest. That is, one receives an important psychic payoff for helping others. Although economists usually fail to explain *why* one would feel good about giving/sharing, economist Gary Becker (1976) adduces that people who are dependent upon specific others (e.g., friends, family members) will consciously see their own advantage reflected in the well-being of their benefactors. For example, a child may receive a larger benefit from

his association within his family than by individual efforts alone. A problem, however, with this conception of altruism is that people are observed to “altruistically” contribute to common resources (family or public) even after it becomes clear, in some cases, that one is not better off for doing so. Becker’s conception of altruism appears more like a conception of a clever strategy to elicit reciprocity rather than altruism *per se*.

The second way economists have incorporated altruism theoretically is essentially to impute a dual nature to humans with dual utilities. That is, an actor is both selfish and altruistic—both egoistic and public/other-spirited (Monroe, 1994:866). On this premise, an individual still seeks to maximize his own utility; it is just that his utility function mysteriously includes the desire to do well by others. The dual-nature hypothesis allows for a theoretical accounting of altruistic behavior found in empirical observation. But if individuals simply have both egoism and public-spiritedness in their utility function, then the theory of rationality seems to lose some of its usefulness. If our preferences are determined both by our own desires and by what we believe is good for others, there is no way of predicting the outcome of a simple prisoner’s dilemma. Does player A choose to defect because it offers her the highest payoffs? Or does player A choose to cooperate with player B because her public-spirited side was evoked?

Contribution to public goods, indirect exchange, and altruism are well-documented behavioral phenomena. The failure of the standard theory of rationality to satisfactorily account for these empirical data means that, in terms of collective action, there has not yet been developed a behavioral theory of collective action based on models of the individual that are consistent with empirical evidence about how individuals make decisions in social-dilemma situations (Ostrom, 1998).

This is not to say that collective action projects do not suffer from real-life rational egoists. On the contrary, RCT does offer important insight into collective action failures. Field research confirms that the temptation to free ride on the provision of collective benefits is a universal problem (Ostrom, 2000). It is also confirmed that many individuals do behave in a manner consistent with the standard theory—though this simply suggests that there are different types of players. However, the point is that most of us do not fit the rational egoist persona as much as the “thin” model of rationality (Ferejohn, 1991) says we should. The fact that we often manage to achieve collective action results that are “better than rational” (Cosmides and Tooby, 1994) has ushered in the call for development of second-generation models of rationality (Ostrom, 1998).

## Second-Generation Models of Rationality

The emphasis on complete rationality in first-generation models has been challenged by models of incomplete or

“bounded” rationality. Boundedly rational individuals do not necessarily calculate a complete set of strategies for every situation they face. Instead, they make use of learned heuristics, norms, and rules (Ostrom, 1998:9). The use of *heuristics* simplifies the decision-making process. It would be extremely difficult (or even disadvantageous considering the costs) to draw upon and consider all information before making every single decision in every single situation. Thus, efficiency is achieved by substituting heuristics, or rules of thumb, developed from experience. Individuals also learn to adopt and use *norms*. Shared norms are shared understandings about actions that are obligatory, permitted, or forbidden (Ostrom, 2000). A norm is an internal valuation that an individual attaches to a particular type of action. In a utility function, a norm is sometimes formalized as a delta parameter that is added or subtracted from the objective costs of an action (Crawford and Ostrom, 1995). Norms are learned in a social context, and the internalization of norms represents an actual change in preferences originating from the lessons and training of elders and peers (Ostrom, 1998:9). *Rules* are also shared understandings, but do not necessarily imply internalization. Rules are distinguished from norms by the fact that they are self-consciously adopted for use in particular situations—often under the fear of sanctioning for noncompliance.

One of the most important considerations for second-generation models of rationality is the norm of *reciprocity*. Individuals who make use of reciprocity norms generally treat other individuals the way those individuals treat them. Political scientist Elinor Ostrom explains that reciprocity refers to a family of strategies that can be used in social dilemmas involving (1) identification of other individuals, (2) an assessment of the likelihood other individuals will cooperate, (3) a decision to cooperate with other cooperators, (4) a refusal to cooperate with defectors, and (5) punishment of violation of trust (Ostrom, 1998). What these essentials amount to is that an actor will treat other individuals the way she feels she is being treated by them. For this to happen, however, an individual must recognize who these others are and record what they have done to/for her. Those who develop a record for favorable exchange earn a reputation, which, in turn, earns them trust. A reputation is a psychological perception on the part of other players about how a given player prefers to play a game (Miller, 1990). It is based on an assessment of another’s reputation that an individual decides whether or not to cooperate with her.

Reciprocal behavior has been observed in every society (Becker, 1990). Nevertheless, reciprocity in terms of both trust and punishment can be extremely costly to an individual, and therefore it is often considered “irrational” by economic standards. For example, it often costs more to enforce or punish an individual infraction than to just let it go. Yet, many of us bear the extra cost anyway. Moreover, one does not have to assume that others are irrational

in order for it to be rational to use reciprocity (Ostrom, 1998:11). This is because boundedly rational actors have reason to believe that other boundedly rational actors follow similar heuristics, norms, and rules.

### *Institutions*

Although the ascendance of RCT in political science has challenged many cherished beliefs about the effects of institutions, recent work conducted under the banner of “The New Institutionalism” has revived scholarship focused on institutions and the nature of political orders (Crawford and Ostrom, 1995:582). If individual choices are significantly affected by learned heuristics, norms, and rules, then it is worth studying the institutions that create, perpetuate, and destroy them.

Institutions are the rules of a game in society (North, 1990). They are guidelines that reduce uncertainty and (hopefully) lower transaction costs. William Riker defines institutions as “rules about behavior,” especially about making decisions (quoted in Ostrom, 1986:3). There have also been treatments of institutions-as-equilibria, institutions-as-norms, and institutions-as-rules (Ostrom, 1998). Institutions-as-norms and institutions-as-rules are once again becoming subject to a great amount of attention. Since institutional rules operate configurationally rather than separately (Ostrom, 1986), they create a complex array of considerations for an individual actor. There are multiple levels of rule analysis, and thus actor decisions and action situations will vary. Institutions cannot create cookie-cutter actors. Nevertheless, institutions do affect individual choice by influencing the evolution of values, tastes, and personalities. Because of this effect, Samuel Bowles calls institutions “...situations in the social psychology sense, which have framing and other situation construal effects” (Bowles, 1998:2).

Institutions influence individual preference sets by framing the particular goods, resources, and opportunities that are available. They also assign perceived costs and benefits to alternate pathways towards achieving one’s preferences. Rules and norms limit the number of ways that one can gain access to preferences. The existence of sanctions affects the ordering of preferences precisely because it artificially alters the total costs of obtaining them. Institutions also affect preferences through the socialization process. Social settings develop their own corporate cultures based on certain expectations of others within a setting (Kreps, 1990). Moreover, social leadership can be extremely important in establishing an ethos of mutual trust (Miller, 1990:346). We can neither accurately predict nor coherently evaluate the likely consequences of new policies or institutions without taking into account institutional preference endogeneity (Bowles, 1998). In this manner, institutionally endogenous preferences can help explain some of the individual variation *between* social groups. But what about *within* social groups?

### *Types of Players*

Another product of scholarship instigated by disparity between RCT predictions and empirical data is the acknowledgment of multiple *types of players*. Recent work has been conducted to create an alternative micro-theory of individual choice to help explain “anomalous” findings (Ostrom, 2000). Some scholars believe these anomalies can be explained when one assumes the existence of multiple types of players, including rational egoists and those who use social norms. For example, game theoretic models have incorporated such types of players as “conditional cooperators” and “willing punishers.” Conditional cooperators are those actors who use the norm of reciprocity and are willing to initiate cooperative action when they estimate that others will reciprocate, and they repeat these actions as long as a sufficient proportion of the others involved reciprocate (Ostrom, 2000).<sup>2</sup> Willing punishers are defined by their strategy of being willing to absorb the cost of punishing others when they defect.

Game theoretic models have formalized player type distinctions for some years now (Dixit and Nalebuff, 1991; Morrow, 1994). But conceptually, one may reasonably wonder how multiple types of players would be present in a situation. Ostrom suggests that recent developments in evolutionary theory, including the study of cultural evolution, can help provide some answers (2000). Evolutionary psychologists suggest humans evolved specific processes of learning. That is, *Homo sapiens* does not have general analytical skills that are then applied to a wide variety of specific problems. Instead, humans evolved specific types of reasoning for specific types of adaptive problems. For example, humans (unconsciously) use a different approach to reasoning about *deontic* relationships (i.e., what is forbidden, obligated, or permitted [Buss, 1999]) than they use for problems of truthness and falsity (Ostrom, 2000). This bifurcation of reasoning—which has been observed even in young children—results in natural cognitive processes. The implication is, just as the human being has a natural proclivity for learning verbal language and other skills, the human being has a natural proclivity to learn social norms and rules. Social norms in terms of deontic understanding are thus quite easily accommodated into the human psyche.

This suggests that human beings are *not* rational in the sense that they assess every situation from the same cognitive, calculative, analytical standpoint. Some learning processes are more or less “instinctive,” even though the content of one’s learning varies across cultures, families, and other social environments. Ostrom uses an indirect evolutionary approach to explain how a mixture of norm-users and rational egoists would preside in the same settings where standard RCT predicts the presence of rational egoists only (2000). In market situations, the rational egoist strategy might be dominant, but outside of market set-

tings, it should initially be assumed that multiple players survive. These other types of players will utilize social norm strategies, such as reciprocity, trust, and fairness (Ostrom, 2000). Ostrom exhorts that future scholarship in this area should “understand [how] institutional, cultural, and biophysical contexts affect the types of individuals who are recruited into and leave particular types of collective action situations” (Ostrom, 2000:154).

## Roles

Institutions and social norms are both types of constraints that affect an actor’s payoff structure. In doing so, they can account for individual choice outcomes. A useful way of describing these processes is in reference to social roles. A social role is a bundle of expectations, habits, and strategies concerning social conduct within a particular social situation. When a specific social context presents itself, this bundle is evoked and implemented. The result is a particular type of behavior—or a type of strategy denoting a type of player. Role assumption confers a degree of efficiency to the decision-making process; thus, a role in these terms is a heuristic, albeit a complex one. Although individuals may continually adjust a role, by definition a role produces a coherent pattern of behavior which is evoked repeatedly for each appropriate social context.

The concept of *institutional roles* (also called *structural roles*) is useful in discussing the ways that institutions shape an individual’s conduct within those institutional contexts. Institutions confer certain norms and expectations that an individual may build into her utility function (Searing, 1991:1243). This can happen overtly by setting certain paths (rules) by which it is acceptable to pursue one’s preferences. But as discussed above, institutions also provide norms that may be internalized, precluding the need for external monitoring. Roles incorporate institutional and social constraints into a viable strategy bundle designed for achieving one’s goals or preferences. Roles may even be said to fundamentally define the goals themselves (Coleman, 1990:11).

Political scientist Donald Searing calls for a revival of the study of roles—as “patterns, as configurations of goals, attitudes, and behaviors that are characteristic of people in particular positions” (1991:1253)—in order to show how behavior of politicians is affected by the goals and incentives associated with their perceived roles. He seeks to build on the project defined by James March and Johan Olsen (1989), who call for the blending of “rules, roles, and reasons” as the primary ingredients of the New Institutionalism. Although “The New Institutionalism offers fertile ground for the study of ‘rational norms’ and ‘rational roles’” (Searing, 1991:1241), Searing encourages a focus on informal rules and norms as they provide a significant part of the story of choice. For this purpose, institutions, norms, and roles form a pedagogically useful core relationship.

Although conscious goals are of primary importance to the formulation of a role, an individual’s incentives are not always cognitive (Searing, 1991:1253). David Hume’s demonstration that reason by itself can never be a motive for action means that desire supplies the motivation and reason seeks the path for its satisfaction. In other words, although our plans to achieve what we desire may come from reason, the desires themselves do not. Searing argues that “the links between a role’s objectives and its attitudes and behaviors cannot be understood properly without understanding how these relationships are driven not just by career goals but by emotional incentives” (1991:1254).

## Multiple Roles

In the language of psychology, different people have different “personalities.” It is largely on the basis of others’ personalities that we evaluate each other and determine whether or not we like them or dislike them—or whether we trust them or distrust them. However, humans also have the incredible capacity to manipulate their personalities to accommodate the social context. In other words, an individual acts differently around different people.

Each human being has a cache of social roles that they may draw upon (Stryker 1980, 1987). The reason that individuals call upon different roles at different times is because their strategic payoffs vary within different social contexts. The payoff for acting aggressively in a market situation or in a sporting context will often be different than the payoff for acting aggressively among peers in the workplace or at home with one’s family. Role switching is a strategy that most individuals employ to some degree every day. For example, a bureaucrat will often act differently with a superior than he will with a subordinate; a senator will act differently with a colleague than she will with a constituent; a father will act differently toward a daughter than he will toward a friend. The decision to assume a particular role is often a conscious choice, but it is sometimes unconscious. Nevertheless, role switching is based on some internal assessment of the change in the payoff structure. If the bureaucrat acts too aggressively toward his superior, he may pay a price. On the other hand, if the bureaucrat allows subordinates to take advantage of him in the workplace, he may lose respect from his peers.

In summary, role “choice” is a function of an assessment of the social context, and strategy is a function of role. I argue that an analysis of actors’ roles (as heuristics) should be considered in terms of the development of second-generation models of rationality. Role heuristics enable an actor to deal with those factors that frame her options and order her preferences. That is, an individual absorbs the norms, rules, and expectations presented by institutions and particular social contexts, and then is able to negotiate these types of social relational contexts with a set of ready strategies (i.e., role).



social roles may contribute to an understanding of values and the refinement of RCT (1994).<sup>3</sup>

Any successful integration of EP and RCT will probably have to establish that a number of preferences are endogenous qua nature. This does not mean all or even most are a result of “human nature,” but that some significant ones are. There is considerable variation among scholars in terms of how important biological factors are to this issue. However, there is a growing interest in academic cross-fertilization for the purpose of improving existing theoretical models. Ostrom argues that “the implications of developing second-generation models of empirically grounded, boundedly rational, and moral decision making are substantial ... [w]e need to expand the type of research methods regularly used in political science. We need to increase the level of understanding among those engaged in formal theory, experimental research, and field research across the social and biological sciences” (1998:14). It is in the spirit of this project that the argument here is developed.

## Evolutionary Psychology

In this section, I offer a brief digression in order to introduce the field of evolutionary psychology. The corpus of EP rests on three basic defining aspects: (1) the theory of evolution, (2) psychological mechanisms, and (3) domain-specificity. *Evolutionary theory* describes the observable physical characteristics of an organism as the result of the generations of natural selection that form an organism’s genotypic structure. It rests on a functionalist epistemology that defines endemic aspects of an organism by the function these parts serve for the survival or reproductive fitness of the organism. For example, livers evolved in order to filter out toxins from the blood stream. Evolutionary theory asserts that all organisms acquired these aspects, such as hearts and livers, through a process of natural selection.

It is not really accurate to describe the development of organs as purposeful in any sense; it simply makes it easier to describe the process. It is actually more accurate to say that organisms that had the benefit of adaptations like working livers had an advantage over other organisms that had less effective ways of dealing with certain adaptive problems. Thus, individuals that possessed these adaptations had better opportunities to pass their genes along to succeeding generations. Sometimes viewed as the study of “human nature,” evolutionary psychology simply asserts that the forces that were at work in forming animals by natural selection were also at work in forming human beings.

It is essential to point out that evolutionary explanations only serve as ultimate causes of behavior. They do not explain proximate causes. The proximate causes of people’s decisions and choices are a matter of personal

tastes and values. However, as biologist Richard Alexander puts it, “proximate causes are evolved because of ultimate causes” (1987:161). Whereas proximate explanations can be considered to answer labeled questions of “how,” the ultimate (evolutionary) can be considered the “why” (Mayr, 1961).

Another important defining aspect of evolutionary psychology is *psychological mechanisms*. Just as human beings evolved special *physical* attributes to deal with specific adaptive problems posed by their environment, humans also evolved special *psychological* features in order to deal with other important adaptive problems—especially those problems resulting from social existence. Like many physical mechanisms (e.g., sweating, adrenaline boosts, tanning, production of T-cells, etc.), many psychological mechanisms are temporary in that they are invoked in response to particular environmental circumstances. Yet, these physical and psychological mechanisms are no less the result of natural selection simply because they are triggered by environmental factors.

To evolutionary psychologists, the debate between nature and nurture represents a false dichotomy. It makes no sense to talk about behavior without the social context. Nor does it make sense to speak of environmental influences on behavior, without the physical mechanisms involved. Evolutionary psychologists Leda Cosmides and John Tooby aver that the idea that the phenotype can be partitioned dichotomously into genetically determined and environmentally determined traits is deeply ill-formed (1992). A basic principle of EP is that it is not enough for an organism to be able to formulate mental associations. Instead, behavior requires the existence of a sort of “program” that directs organisms regarding expedient avenues of response.

Evolutionary psychologists gainsay behaviorism’s *equipotentiality assumption*—that is, the assumption that all causal associations are developed in a learning organism with equal ease. Experiments by John Garcia and others have demonstrated that there are some relatively simple cause and effect connections that animals cannot associate very easily, or at all (Garcia and Koelling, 1966). Associations that do come easily may suggest an underlying psychological mechanism that evolved in response to a prevalent adaptive problem.

The third defining aspect of evolutionary psychology is *domain-specificity*. Domain-specificity refers to the concept that each psychological mechanism has evolved in response to relatively specific and discrete adaptive problems. For example, in the Pleistocene (the era in which humans spent the huge majority of their existence), males who naturally responded jealously and sometimes violently to mate infidelity may have improved their chances for producing offspring over other males who did not use such strategies. The mechanism of male sexual jealousy is domain-specific because it evolved as a solution to that

particular dilemma. The mechanism's usefulness probably does not extend beyond the circumstances that made it useful—hence, it is specific to that domain.

Evolutionary psychologists stress that some of the challenges that faced human beings in the Pleistocene may no longer pose much of a threat to us in modern society. However, because we are, in a limited sense, hardwired to respond in certain ways in certain situations, we may possess behavioral drives that no longer provide the same benefits that they did in the Pleistocene. Today, if a male behaves violently in response to his feelings of sexual jealousy, his fitness will probably not be increased because of it. In fact, it may be diminished since he may have to face costly modern sanctions.

Another concept that evolutionary psychologists find particularly useful is that of the *environment of evolutionary adaptedness* (EEA) (Bowlby, 1969) or *ancestral environment* (Ridley, 1996). This is the environment in which human beings spent 2 million years evolving into their present-day form (the Pleistocene: about 2 million - 10,000 B.C.E.).<sup>4</sup> The time that humans spent in large agricultural societies is only a small fraction of the total time that humans have spent on Earth. Therefore, evolutionary psychologists reason that if there were environmental or social contingencies that *Homo sapiens* developed psychological mechanisms to solve, the vast majority of them were found within this ancestral environment. The example of male sexual jealousy illustrates how a behavior that might have provided an advantage to one's fitness in the ancestral environment may not provide any advantage whatsoever in today's world.

Above are the major components of evolutionary psychology. Together, they form a theoretical framework that improves our understanding of the dynamics of human behavior. Buss expresses this point in a list of five basic premises that he argues summarizes the foundations for evolutionary social psychology:

1. All behavior is a product of mechanisms internal to the person, combined with input (external and internal) that activates those mechanisms;
2. Evolution by natural selection is the only known causal process by which species-typical mechanisms can be created, at some fundamental level of description;
3. Evolved social psychological mechanisms are likely to be large in number and complex in nature, and many will be domain-specific;
4. *Many of the most important problems for humans were social in nature;* and
5. Many of the important social adaptive problems humans had to solve entailed the successful

formation of enduring social relationships such as mateships, friendships, kinships, and coalitions. (Buss, 1990:268-272, emphasis added)

## Evolved Social Roles

The main thrust of the latter portion of this article argues that human biology has a hand in shaping social roles. Just as roles are created by learning and the adoption of social norms, some very fundamental roles are heavily influenced by nature. I call the roles influenced by nature, *evolved social roles*. These are still *social roles* because their evocation depends on the type of relationship situation (social context). That is, the internal valuation of the situation is dependent upon the type of social relationship in which one is engaged (i.e., with whom am I playing?).

Biological factors have a hand in shaping social roles by providing some of the basic behavioral strategies that comprise some of our fundamental social roles.<sup>5</sup> In game theoretic terms, the type of game presented to an actor largely depends on what sort of relationship the actor has with the other player. To avoid circular reasoning, I argue from the standpoint that the type of relationship is determined before any iteration is played—this is the effect of biological factors. The type of relationship that the actors have with each other determines their positions in the game. Positions germane to this discussion may include father, daughter, friend, mate, etc. Such a position provides an actor with a set of likely strategies, which together can be thought of as part of a social role. Inasmuch as a learned social role affects player type, an evolved social role also affects player type.

Figure 2 shows how these relationships can be conceptualized. Ultimately, social context leads to social role, which leads to strategy (Figure 2A). But more precisely, the nature of the social relationship determines the position that an actor assumes. Positions evoke social roles, which includes both learned and institutionalized social roles (as engendered by rules, norms, and institutions) and evolved social roles (as engendered by human biological factors) (Figure 2B).

Acknowledgment of evolved social strategies and evolved social roles offers an especially important contribution to theories of decision-making. That is, these concepts can help predict the emergence of altruism and important kinds of cooperation. The likelihood of altruistic and cooperative behavior is conditioned upon the basic type of relationship an actor has with another actor. There is absolutely no doubt that different cultures and institutions around the world give us an immense range of specific types of social relationships. However, within all of these cultures there are several general types explicable by the basic social functions they serve. These universal types of social relationships are what I call *fundamental relation-*

(Figure 2A)

Social Context → Social Role → Strategy

(Figure 2B)

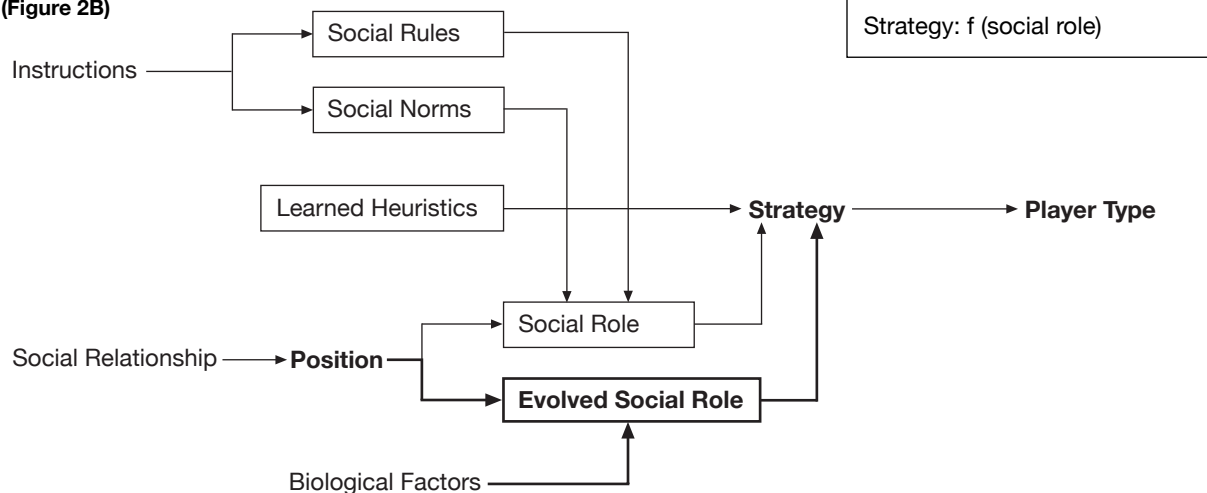


Figure 2: Player Type Function

ships. They are *fundamental* in the sense that no culture or community is without some variation of them; and, if defined in general enough terms, they can probably be considered products of our “human nature.” A fundamental relationship context evokes corresponding evolved social roles in the actors.

Although much more complex and plastic, evolved social roles in humans can be thought of as having analogues in other animal species. Some animal roles serve similar functions as some human roles—for example, leaders in wolf packs, matriarchs in elephant herds, second- and third-order coalitions in dolphins, and long-term mates in puffins. Some evolved social roles in the animal kingdom do not have natural analogues in human societies, but can illustrate the concept of the evolved social role quite clearly. One pronounced example of this can be found among ant, bee, and termite colonies. These organisms are able to maintain extremely large and complex societies by breaking down the colony into different roles (e.g., “workers,” “scouts,” “raiders,” “queen,” etc.). Evolved social roles are identifiable by the function they serve (i.e., on a teleological basis). In biological terms, function does not fulfill a metaphysical purpose. It simply means that, during a species’ evolution, those individuals that developed “solutions” to adaptive social problems ultimately improved their chances for survival and reproductive success. In this capacity, an evolved mechanism that serves as a solution to an adaptive problem serves a function. An evolved social role can be seen as one of these mechanisms—and each role has a distinct function.

In order to acknowledge evolved social roles in humans, one must identify the social adaptive problems that gener-

ate them (i.e., fundamental relationships). To begin, I outline three fundamental relationships whose distinctiveness is illustrated best when presented together in contrast to one another. They are *non-kin coalitions* (*alliances, friendships*), *kin relationships*, and *dyadic mate relationships*.<sup>6</sup> Evolutionary psychologist David Buss presents these three relationships as specific examples “in which evolutionary psychological thinking has been applied to the analysis of social relationships” (1990:276; see also 1995). He uses these examples to show how humans form intense, long-duration relationships as solutions to different adaptive problems. These social problems were omnipresent in our species’ history, and those individuals that evolved mechanisms to deal with these problems had distinct advantages over those who did not. Thus, those whose advantages translated into reproductive benefits (i.e., more reproductive opportunities, etc.) were literally able to outreproduce others. EP assumes that modern-day humans inherited evolved solutions to social problems and that they continue to play a significant part in our social interaction.

I argue that Buss’s three examples in which evolutionary thinking applies to the analysis of social relationships are three examples of fundamental relationships. That is, these relationships are a part of the human experience everywhere, and that our engagement in them evokes a set of expectations and behavioral strategies (i.e., a role) appropriate to their unique relational contexts.

The point of comparing each of these fundamental relationships is to suggest that each of them presents a *different* set of problems for those engaged. They naturally invoke *different* sets of expectations for those involved and instigate shifts in strategy. Those who behave counter to

expectations may be considered to be “violating” the conditions of the relationship. But the conditions are different for each. For example, a violation of the non-kin relationship may not necessarily be considered a violation of a parent-offspring (kin) relationship, and vice versa. Whereas a she-wolf may expect repayment for some favor tendered to an ally, the same she-wolf would not expect the same sort of reciprocity from her pup—who drains her milk, monopolizes her time and energy, and yet could not possibly “pay” her back *per se*. These different expectations suggest that there are inherent differences in the natures of these two relationships. It is probably not too far of a stretch to imagine that, like other social species, humans may naturally distinguish between certain types of social relationships; and they are able to gauge behavioral propriety within different relational contexts.

### Conceptualizing Evolved Social Roles

In terms of conceptualizing evolved social roles, we can look to evolutionary psychology or evolutionary social psychology for help (Reiners, 2000). We can refer to the three different types of social relationships outlined above that Buss contends evolutionary psychology helps explicate—namely, friendships, kinships, and mateships. This gives us nothing less than a short list of roles (friend, kin, mate), social identities evoked by the contingency of specific kinds of interaction. As mentioned above, these fundamental relationships are identifiable and discrete because the conditions of these relationships are violable, and because a violation of one of these relationships would not necessarily be a violation of another. For example, the same individual could act as a mate in one situation, and act as a friend in a different situation, but likely would not act quite the same way in both contexts. Acting simply as a friend in the mate situation might very well be taken as an offense. Evolved social roles can be said to be bundles of expectations and strategies appropriate<sup>7</sup> to a fundamental relationship, and those strategies (evolved social strategies) can be said to be the result of evolved *mental algorithms*.

#### *Coalitions—Friends and Allies*

It is precisely mental algorithms that some evolutionary psychologists hypothesize enable humans to create coalitions and maintain them. Cosmides and Tooby suggest that humans have internal mechanisms for detecting very subtle forms of cheating (1992). Cosmides’ use of the “Wason selection task” examined if there is any difference in the ability of people to find a “solution” to certain problems posed in the abstract and then in a social contractual context (Cosmides, 1989). A significant majority of subjects were found to be able to solve the problem in the context of a social contract, but not in the abstract form.<sup>8</sup> Cosmides’

experiments suggest that deontic reasoning is separate from reasoning about what is true or false. Deontic reasoning has been repeatedly detected in children as young as three years old, and is not associated with overall intelligence or educational level of the subject (Ostrom, 2000:15).

From the Wason selection task and other experiments, Cosmides and Tooby deduce that (1) psychological mechanisms are not general-purpose (domain-general)—they evolved because they met specific adaptational problems, and (2) humans have probably evolved psychological mechanisms “designed” to adapt to social dilemmas—a social dilemma being any situation where it would be more “rational” for an individual to pursue his own self-interest than share, cooperate, or sacrifice for others (Cosmides and Tooby, 1992). The domain-specific psychological mechanism that enables us to detect cheaters developed because our ancestors, who had these mechanisms, were better able to determine whom and whom not to trust, thus making their social existence more profitable—ultimately in terms of fitness and reproduction.

Cheater-detection enables individuals to detect who their friends are by identifying those with whom it is profitable to engage in social exchange. One’s status as friend or ally is determined by one’s reputation. In short, reputation is the requisite of iterated transaction, where reputation, reciprocity, and trust are the foundation of social exchange (Ostrom, 1998).

Social exchange evolved long before formal markets. All highly social animals engage in some form of reciprocity-based cooperation. *Reciprocal altruism*, coined by biologist Robert Trivers (1971), refers to the biological phenomenon of non-kin cooperation. Ever since Darwin’s theory emerged, a nagging question for biologists was, If animals are self-seeking in the sense that they take every advantage presented to them to increase their individual fitness and reproductive success over that of others, then why do we observe them doing favors for each other? Surely animals are not altruists in the moral sense. Moreover, we know that if an animal that is genetically programmed for self-sacrifice actually gets sacrificed in the process of doing good, then those self-sacrificing genes no longer get passed along. It is an evolutionary dead-end (Ridley, 1996), and a mathematically dominated strategy (Axelrod, 1984). In other words, without extremely specialized conditions,<sup>9</sup> truly selfless individuals will not prosper as a matter of course.

Trivers (1971) explains non-kin cooperation (reciprocal altruism) as a process maintained by the “expectation” of reciprocity. That is, if one animal does a favor for another at one point in time, then that animal expects to be paid back or privileged in some way afterwards. In the animal kingdom, what this requires, however, is a brain sophisticated enough to be able to (1) identify and differentiate other individuals, (2) record (remember) information about other individuals’ past dealings, and (3) distribute favors

discriminately. Trivers's theory predicts that animals that do favors for others "expect" favors in the future, and if they do not receive them, their responsive strategy will usually be to refuse further aid.

Exactly this has been observed in many species of animal, although some dealings are more sophisticated than others. Dolphins and the great apes are unique in that they are able to develop second- and third-order coalitions—that is, they have circles of friends within a circle of friends. Essentially, they differentiate between friends, acquaintances, and other subtle forms of coalition relationships. But even vampire bats have been shown to discriminate allies from non-allies. For example, nightly forays for blood provide individual bats with variable success. Sometimes a bat will receive the favor of regurgitated blood from another if the former's own effort during the night proved unsuccessful. It has been observed that some bats have been refused this favor if they had failed to pay back for an earlier favor (Wilkinson, 1984). Thus, they are able to identify individuals, remember specific favors, and discriminate appropriately in granting favors of their own. These abilities belong to the "family of strategies" that are also involved in human reciprocity (Ostrom, 1998:10).

#### *Kinships—Parents, Children, Siblings, Relatives*

Kinships are fundamentally different from non-kin relationships. In kin relationships, altruism is not necessarily employed with the expectation of reciprocity. Biologists have also observed that members of many species make tremendous relative sacrifices ministering to their offspring's survival. In other words, parents express a great deal of altruism towards their young—without necessarily expecting "repayment."

The biological definition of altruism is any risk or sacrifice that one animal makes that has the result of decreasing its own fitness for the benefit of another. Parental altruism has consistently been observed in many varieties of laboratory and field experiments. Evolutionary psychologists view this in terms of specialized evolved psychological mechanisms that are attendant upon the unique adaptive problems that parents have confronted when interacting with their children (Buss, 1990:277). Put another way, when we find ourselves in parenthood, we find ourselves taking on parental roles.

Siblings and other kin are also observed to exhibit mutually supportive behavior. This behavior comes much closer to true altruism than non-kin reciprocity. Like with non-kin altruism (reciprocal altruism), biologists puzzled over this phenomenon for many years, wondering why a supposedly self-serving individual would make such sacrifices for others.

The reigning theory concerning kin altruism did not fully develop until the 1960s, when William Hamilton (1964), George Williams (1966), and Richard Dawkins (1976) all

contributed, in varying degrees, to what is now known as the theory of *kin selection*. Simply put, kin selection is a process in which kin assist each other because their genes make them do it. What made this theory possible was Hamilton's "inclusive fitness theory" (1964). With the help of advances in genetic science, Hamilton constructed a mathematical thesis of which the primary thrust was that genes are the driving factor behind kinship sociality. By taking the perspective of the gene (instead of the individual), biologists were able to give an explanation for why individual animals appear to make "irrational" personal sacrifices for others. To the individual, these sacrifices can be very costly. But to the family gene, which exists in each member, individual sacrifice for the good of others can be a shrewd investment. If the same genes are contained within two different individuals, as far as the genes are concerned, one individual is worth as much as another. The fact that parents are driven to aid and sacrifice for their young is the doing of the genes, which "care" only about reproducing themselves, not about some temporary, organic, individual vehicle for reproduction, whose evolved sense of self-awareness is, nevertheless, in service of the genes.

Inclusive fitness theory predicts that individual organisms have mechanisms that prompt them to aid each other in degrees corresponding to their level of genetic relatedness. Parents and children share exactly half of each other's genes; consequently, parents are predicted to be highly protective of their offspring. On average, 50% of genes are shared between siblings, 25% between grandparents and grandchildren, 25% between uncles and nieces, 12.5% between first cousins, and so on. The inclusive fitness revolution marshaled in a new era of what has been called "gene's eye" thinking (Dawkins, 1976; Hamilton, 1964; Ridley, 1997). In essence, the unit of analysis, which used to be the individual, became the gene. At the level of genetic relatedness, Hamilton's theory was able to explain biological altruism in a new light, which before had been, at best, a foggy intuition. Now when we observe the incredible coordination and sacrifice found in some social insect colonies, we can understand it within the context that female siblings within a colony share 3/4 of their genes. Such conditions of relatedness must certainly be a factor in the appearance of eusociality (Wilson, 1971).

Inclusive fitness has proven a very useful tool for evolutionary social psychologists. Buss notes that the tremendous surge of love that parents feel towards their children seems to be mitigated only by a highly predictable set of conditions that is difficult to account for on any grounds but evolutionary ones (1995). For example, parental "love" is mitigated (1) when a child's caregiver is not the biological parent, (2) when paternity is uncertain, (3) when the child is deformed or otherwise of poor phenotypic quality, and (4) when circumstances such as poverty, lack of food, or material overburdening from too many children render prospects for surviving and flourishing poor (Daly, 1989).

It is precisely under these circumstances that infanticide instigated by the parents is carried out, at least in “traditional” societies (Daly and Wilson, 1988; Buss, 1990). According to studies by psychologists Martin Daly and Margo Wilson, North American children living with stepparents were many times more likely to be fatally abused than children living with both biological parents (1988).<sup>10</sup> Similar results have been found in diverse cultures and countries over the globe (Daly and Wilson, 1998).

Inclusive fitness theory can generate accurate predictions of altruistic behavior among human individuals. Moreover, this has been shown to be true cross-culturally. The “function” of the kin relationship is different from the function of the non-kin coalition relationship (i.e., reciprocal social exchange). The “function” of a kin relationship (e.g., parent-offspring, sibling, etc.) is to ensure the survival of genes. Therefore, this relationship is not “rational” insofar as rationality is expressed only at the level of the individual. In this case, rationality is also expressed at the level of the gene.

A kin-based fundamental relationship involves different norms than relationships among non-kin. A child who “cheats” a parent by taking resources and not reciprocating may not be considered to be cheating or have committed any particular violation of that parent-offspring relationship. But a child who does not reciprocate with a non-kin friend will most likely be considered to be abusing the norms of that relationship. Cheating, as a violation of social norms (Cummins, 1999:231), means different things in these social contexts.

#### *Mateships—Spouses, Boy/Girl Friends*

In the EEA, the need for mates provided humans with yet more social adaptation problems. These problems are serious because, in biological terms, having at least one mate is key to genetic fitness. Human offspring are born into the world in an extremely vulnerable state, and they are vulnerable for a long period of time. They must have direct attention from caregivers for many years before they are actually able to become somewhat self-sufficient (in the sense that they can create their own sophisticated social ties, which usually corresponds with a child’s own sexual development). Thus, successful reproduction is not as simple as conception. It also requires a weighty investment for some time afterwards, which may include some paternal investment.

Desmond Morris’s “pair-bond” hypothesis suggests that potential human mates naturally couple off (1967). Although pair-bonding is not a universal rule—there are certainly variations of mating relationships for various reasons—evolutionary psychologists recognize that pair-bonding is at least a culturally pervasive phenomenon. For example, more than 90% of the world’s population gets married at some point in life. The degree of personal com-

mitment and the dynamics of extra sexual relationships is not entirely consistent among cultures and individuals, so it is hard to draw a conclusion as to the “natural” sexual behavior of humans. Even evolutionary psychologists admit that it is probably not the case that most of our human ancestors found one partner and stayed with that individual forever. Successful child rearing does not require that kind of commitment. Instead, natural selection probably “equipped” our species with a proclivity to share intense emotional love bonds for a duration that was statistically “long-enough” to ensure that any children produced by this relationship would reach a minimum level of viability through the emotional (and material) investments of their parents.<sup>11</sup> In any case, evolutionary psychologists maintain that the emotion of *sexual love* is an evolved adaptational mechanism that had the result of committing sexual partners to each other for periods of time long enough to produce and raise children. Both human males and females have this adaptation.

Love is not the only psychological adaptation produced by the necessity of finding and keeping mates. Because of the undeniably different levels and methods of contribution that men and women make to the perpetuation of our species, different adaptations evolved in each to deal with their respective reproductive dilemmas. One obvious difference that men and women face is found in how they are able to establish parental certainty. A man has no sure-fire way of knowing if a child is his or not; and men certainly had no way of ensuring this before the twentieth century. The possibility of sexual infidelity, at least historically, posed distinctive adaptive problems for men and women. Men whose mates were unfaithful risked investing in children who were not their own. Women whose mates were unfaithful, although not risking analogous maternal uncertainty, risked the diversion of a man’s resources to other women and their children. There is compelling evidence that “sexual jealousy” evolved as a complex psychological solution to these problems (Buss, 1995; Daly, 1982; Symons, 1979).

EP’s theory of sexual jealousy also predicts that men and women will experience jealousy differently. It predicts that men will be more upset by the idea of sexual infidelity of their mates, while women will probably be more upset by the prospect of emotional infidelity on the part of their mates. Evolutionary psychologists cite several field and laboratory experiments that support these predictions. A cross-cultural study conducted by Buss showed that in Germany, the Netherlands, the United States, Korea, Japan, and other countries, men reported significantly greater distress than women in response to a scenario of sexual infidelity. On the other hand, women across all culture samples reported greater distress than men in response to emotional or love infidelity (1999).

Under the principle of domain-specificity, evolutionary psychologists explain that male sexual jealousy may not

have any adaptational advantages in today's modern societies. In fact, it probably does nothing but cause emotional stress. The act of punishing an unfaithful partner or her lover might result in jail time instead of parental benefits. Therefore, it may be considered "irrational." Yet this "irrational" behavior is common even today. There may have been some drawbacks, but overall the benefits probably statistically outweighed the drawbacks. It is not too difficult to imagine that, in an ancient setting, those people (especially the highly ranked) who acted on their feelings of sexual jealousy probably increased their biological fitness because of it. But as discussed above, evolutionary psychology does not claim that all of our evolved psychological mechanisms provide any advantage for us here in this highly specialized, large, and complex modern society. We can think of them as outdated evolved social strategies that developed originally because they ultimately contributed to fitness.

In this way, mateships also present individuals with an evolved social role. The "function" of the mate relationship, which is usually avoided between kin, is similar to kin, in that the rationality of it is largely from the "gene's-eye" point of view. But the norms and corresponding roles are different. One does not normally treat a child or sibling the same way that one treats a mate. Love and jealousy are fundamentally "irrational" from the point of view of the rational egoist. They are both incredibly costly; yet one can hardly imagine a society without them.

### Evolved Social Roles and Theoretic Modeling

Above, I argue how three specific examples of evolved social roles can be conceptualized. Many social strategies that have roots in evolved social roles are conspicuous because they can be considered empirically universal and yet "irrational" in RCT terms. After all, what is more universal to societies than parenthood? And what could be more costly and self-defeating to a rational egoist than having children and taking care of them? The same goes for mates. The only fundamental relationship within these three examples that does not require economic irrationality is the non-kin coalition type. Yet there are irrational levels of initial trust and generosity involved here too. Nevertheless, those engaged in non-kin coalition relationships are much quicker to detect and punish cheating in social exchange. If non-kin have a number of other actors to choose from, they will eschew the noncooperators in favor of individuals they can trust. After all, you can choose your friends, but...

Non-kin coalition, kin, and mate-based relationships are not the only fundamental relationships for which there is evidence that people assume "natural" roles. To be sure, the three outlined above might not be the most important, nor even articulated most usefully. As alluded above, there is also strong evidence that sex also generates particular roles. In fact, evolutionary psychologists probably have

the strongest claim for differentiated gender roles (Wright, 1994). Both sexes have slightly different sensitivities for learning different social norms. There is also evidence that humans are particularly sensitive to learning *hierarchy norms*, thus suggesting that hierarchical social systems were a large component of human societies during human evolution (Buss, 1999). Thus, we should not be surprised to find roles shaped in terms of social hierarchies.

One study of 59 three-person groups composed of heretofore strangers, noted that a "clear" hierarchy emerged within only one minute in 50% of the groups (Fiske and Ofshe, 1970). A clear hierarchy emerged in the other 50% within the first five minutes. Another study found that cheater-detection is actually modified by social rank (Cummins, 1999). Evolutionary psychologist Denise Dellarosa Cummins observed that subordinates do not tend to look for cheating in higher-ranked individuals, whereas higher-ranked individuals actively seek out cheaters among the lower ranks. Cummins's "Dominance Theory" posits that even though cheater-detection is crucial to attaining a high-ranking position, subordinates detect cheating in superiors much less than superiors detect cheating in subordinates—presumably because there are often great costs involved in doing so (Cummins, 1999).

In addition to sex and hierarchy, another potential area in which one might find evolved social strategies and evolved social roles is that of clan or ethnicity. Although it is highly improbable that individuals have a natural propensity to define each other in terms of ethnicity *per se*, there is at least theoretical evidence that it is evolutionarily rational to aid probable coprogenitors of their own descendants (Whitmeyer, 1997). For example, in the context of a small, relatively closed community (recall EEA), helping those who might marry your daughter or grandchildren, in the form of contributing nonrival goods, could have positive genetic payoffs. This requires, however, that an individual is able to distinguish between likely candidates and unlikely ones. In this context, aid to others would tend to be directed at a nearly endogamous set of people to which the contributor belongs (Whitmeyer, 1997:175). Moreover, physical indications of endogamy—today characterized in terms of "ethnicity"—would likely be a heuristic for identifying individuals belonging to one's "group." This could also be a partial answer to why individuals have the habit of "irrationally" contributing to the provision of public goods at all.

The point of all of this is to suggest that determining the evolved social role that is evoked in an actor can help us predict what type of strategies the actor will use. Using the chart in Figure 2B, one may determine the player type from the initial position of the player. For example, non-kin dyadic peer friendship will likely generate "conditional cooperator" player type.

Ultimately, this can lead to a theory for predicting under what social conditions we can expect to see the important but "puzzling" phenomena of social interaction (e.g., altru-

ism, trust, generalized exchange, restricted exchange, costly punishment, etc.). Another way to think about modeling is in terms of each fundamental relationship as having its own threshold, within which each region has different but possibly overlapping norms. Peter Singer's book on evolutionary ethics adumbrates potential models by providing a theoretical conceptualization of how human moral horizons might be configured (Singer, 1981). Singer's metaphor expresses how kin selection, reciprocal altruism, and group selection might be traced through an expansion of human moral horizons.

Game theorist Ken Binmore adapts Singer's metaphor to explain the advance of human moral horizons in terms of the evolution of progressively more elaborate equilibrium selection devices (Figure 3). Binmore submits that in the center, the evolution of human ethics began as a consequence of equilibrium selection operating in family games. The closer our relation to kin, the more we sympathize with their needs and concerns. However, when we get to distantly related kin, the degree to which we sympathize with them diminishes. But even as we move out away from the center, Binmore suggests, "we still retain more than enough capacity to sympathize even with absolute strangers to explain the 'warm glow' feelings that lead us to leave tips in restaurants we never expect to visit again ... [but] such vestigial warm-glow feelings provide an inadequate foundation on which to build a modern state" (1998:210). Binmore explains:

A strategy profile that is an equilibrium for an inner-circle game will not normally be an equilibrium for an outer-circle game. The use of the inner-circle equilibrium will therefore be selected against. However, playing the outer-circle game as if it were an inner circle game will sometimes result in the players coordinating on an equilibrium of the outer-circle game. The group will then have stumbled upon an equilibrium selection device for the outer-circle game. (1998:211)

In other words, we may fool ourselves into thinking that we are playing a more intimate game than we really are. This is exactly what institutions and social norms do, but obviously they are not foolproof in this capacity. From the perspective of the individual, the nation-state is one of the most irrational institutions imaginable. Olson (1965) was right to point out that massive contribution to social goods theoretically should not happen when you are dealing with a bunch of rational utility-maximizers.

### Conclusion

The fact is we often are playing outer-circle games as if they were inner-circle. Fortunately for modern societies, part of us is stuck in the Pleistocene. We are still using

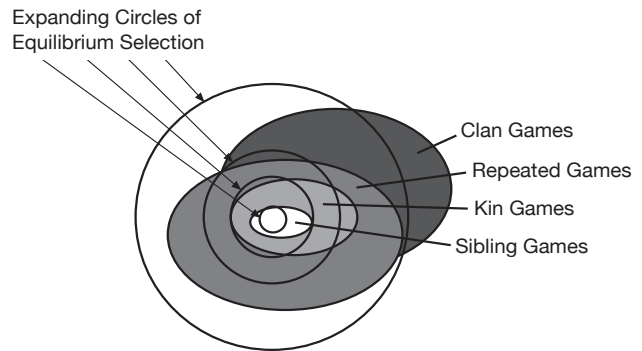


Figure 3: Singer's "Expanding the Circle"

Source: Binmore (1998:184)

many strategies and heuristics that were designed in an era when our utility was decided for us. One of the conditions of that era was that we were not trying to coordinate with millions and millions of complete strangers. The upside is that altruism, trust, and cooperation with people we do not know come easy for us. The downside is that jealousy, revenge, and discrimination toward people we do not really know come easy for us as well. Just because we understand how costly it is to personally wreak revenge on others does not mean that we stop doing it. On the other hand, just because we know that a child is not biologically ours does not keep us from loving it. And just because we understand that it is irrational to vote does not prevent many of us from dying for the right to do so.

The important implications for the study of politics and political behavior lie with understanding what conditions "fool" us into drawing on certain evolved social roles and strategies, or "fool" us into playing certain circle games. For example, child adoption is a mechanism that draws parents into the innermost-circle equilibrium. Firms, through corporate reputation, are mechanisms that fool us into the repeated games equilibrium. In addition, ethnic nationalism is a political way of fooling us nearly into the clan or even the kin equilibrium. Genetically, Nazis were not kin; they were not even clan, but some level of their consciousness was convinced otherwise.

Institutions appeal not only to our pure rationality, but also to our sense of roles. They function largely by triggering role behavior, not simply by presenting individuals with a complete set of incentives for each of which an individual must rationally weigh the costs and benefits. If human beings can be thought of as having deep-seated evolved social roles, then those interested in political behavior should be interested in what they are and how they function. Hence, I add to the growing call for interdisciplinary research, especially between the life sciences and social sciences.

Rationality is not lost. However, individuals can be viewed as "rational" on more than one level. *Emotions* provide a link between evolved preference, strategies,

and action. Economist Robert Frank's "strategic theory of emotions" proffers the argument that emotions evolved precisely in order to *solve social and collective action dilemmas* (Frank, 1988). Most of these dilemmas are based on what Frank calls "commitment problems." Certain emotions enable people to commit to actions that are in their long-term interest instead of trying to maximize utility in each individual situation. For example, a gentleman waits for a flight at the airport with an elegant suitcase. A thief, estimating that the gentleman is a purely rational self-interested individual, would correctly calculate that the cost of retaliation would be prohibitive. If the gentleman wished to retrieve his suitcase, he would have to miss his flight, call the police, and return later in order to testify (Frank, 1993). In this scenario, the cost of getting angry and pursuing "justice" would literally be more costly than simply buying another suitcase. But in these situations, the emotion of anger provides us with a *credible commitment*. One can imagine that in a more intimate community, like a hunter-gatherer band in the Pleistocene, punishment of even petty infractions would prevent further abuse by creating a credible threat.

Other emotions also function to provide credible commitment in order to serve our long-term interest—at least this is the case in a social context similar to the Pleistocene. But emotions can and do help us solve collective action dilemmas in modern society. They may be even more important in today's mass society. For example, guilt commits us to be honest and trustworthy, thereby providing ourselves with a reputation that will serve as a foundation for future cooperative endeavors. Love commits us to partnership and to the rearing of children. In short, emotions help us avoid becoming Amartya Sen's "rational fool" (Sen, 1977).

RCT is a tremendous tool. It offers us a powerful account of individual decision-making, and it accurately predicts many collective action outcomes. However, there is a real need to develop second-generation models of rationality, especially ones that can better account for the important "anomalous" or "irrational" phenomena of cooperation, altruism, and other observed behaviors that currently can only be treated in a dismissive way. Looking at the perspective of the gene as it manifests itself through evolved social roles can help our theories account for the empirical evidence.

## Notes

1. This does not jibe with empirical evidence. Laboratory experiments indicate that actors *do not* use backward induction as RCT predicts (Ostrom, 1998:5). However, there is evidence of end-game effects. Cooperation tends to break down as actors near the final iteration.
2. See also Axelrod's discussion of Tit-For-Tat strategy (1984).
3. Specifically, Hechter focuses on role conflict.
4. Although we cannot be positive what the EEA was like, several anthropological hypotheses are now conventional. One

assumption is that our human ancestors most likely lived in small groups of 50-200 members, hunting and foraging out an existence (Wright, 1994). Another assumption is that most of this time was spent on the East African savanna. Interestingly, some EP studies even suggest an innate human preference for savanna-like environments (Orians and Heerwagen, 1992).

5. In conjunction, human biology has most likely also had a hand in shaping our *preferences*—and as such, this also ultimately has implications for social roles. However, this is a related but separate project that will have to be the focus of another paper.
6. These do not necessarily exhaust the range of fundamental relationships, but they are three salient examples of highly generalized but distinct types of universal human relationships.
7. By "appropriate" I mean adaptive within the EEA, where it was appropriate because it was ultimately advantageous to the individual engaged in it—or at least advantageous to his/her genes.
8. *Modus ponens*, or deontic reasoning, was found to come easily to subjects regardless of the familiarity of the situational context. Conversely, *modus tollens*, or abstract reasoning, was much more difficult.
9. Specialized conditions may include those that allow for *group selection*. This is a highly controversial theory that may have application for later projects, but is not addressed here. For discussion, see Sober and Wilson, 1998.
10. It is also worth noting that, unlike single-parent families, step-parent families are not especially likely to live in poverty.
11. Of course, this emotionally provisioned commitment would be shortened or extended based on individual predilections and circumstances.

## References

- Alexander, R. (1987). *The Biology of Moral Systems*. Hawthorne, NY: Gruyter.
- Axelrod, R. (1984). *The Evolution of Cooperation*. New York: HarperCollins Publishers.
- Becker, G. (1976). "Altruism, Egoism, and Genetic Fitness: Economics and Sociobiology." In G. Becker (ed.), *An Economic Approach to Human Behavior*. Chicago: University of Chicago Press.
- Becker, G. (1996). *Accounting for Tastes*. Cambridge: Harvard University Press.
- Becker, L. (1990). *Reciprocity*. Chicago: University of Chicago Press.
- Binmore, K. (1998). *Game Theory and the Social Contract*. Volume 2. Cambridge, MA: MIT Press.
- Bowlby, J. (1969). *Attachment and Loss*. Volume 1: *Attachment*. New York: Basic Books.
- Bowles, S. (1998). "Endogenous Preferences: The Cultural Consequences of Markets and Other Economic Institutions." *Journal of Economic Literature* 36 (1):73-112.
- Buss, D.M. (1990). "Evolutionary Social Psychology: Prospects and Pitfalls." *Motivations and Emotion* 14 (4):265-86.
- Buss, D.M. (1995). "Evolutionary Psychology: A New Paradigm for Psychological Science." *Psychological Inquiry* 6 (1):1-49.
- Buss, D.M. (1999). *Evolutionary Psychology: The New Science of the Mind*. Needham Heights, MA; Allyn and Bacon.
- Coleman, J.S. (1990). *Foundations of Social Theory*. Cambridge, MA: Harvard University Press.
- Cosmides, L. (1989). "The Logic of Social Exchange: Has Natural Selection Shaped How Humans Reason? Studies with the Wason Selection Task." *Cognition* 31:187-276.
- Cosmides, L. and J. Tooby (1989). "Evolutionary Psychology and the Generation of Culture, Part II. Case Study: A Computational Theory of Social Exchange." *Ethology and Sociobiology* 10:51-97.
- Cosmides, L. and J. Tooby (1992). "Evolutionary Psychology and the Generation of Culture: Part II. A Computational Theory of Social Exchange." *Ethology and Sociobiology* 10:51-97.

- Cosmides, L. and J. Tooby (1994). "Better Than Rational: Evolutionary Psychology and the Invisible Hand." *American Economic Review* 84:327-332.
- Crawford, S.E.S. and E. Ostrom (1995). "A Grammar of Institutions." *The American Political Science Review* 89:582-600
- Cummins, D.D. (1999). "Cheater Detection Is Modified by Social Rank: The Impact of Dominance on the Evolution of Cognitive Functions." *Evolution and Human Behavior* 20:229-48.
- Daly, M. (1982). "Some Caveats about Cultural Transmission Models." *Human Ecology* 10:401-8.
- Daly, M. (1989). "Parent-Offspring Conflict and Violence in Evolutionary Perspective." In R.W. Bell and N.J. Bell (eds.), *Sociobiology and the Social Sciences*. Lubbock: Texas Tech University Press.
- Daly, M. and M. Wilson (1988). *Homicide*. Hawthorne, NY: Aldine.
- Daly, M. and M. Wilson (1998). *The Truth about Cinderella: A Darwinian View of Parental Love*. New Haven, CT: Yale University Press.
- Dawkins, R. (1976). *The Selfish Gene*. Oxford: Oxford University Press.
- Dixit, A.K. and B.J. Nalebuff (1991). *Thinking Strategically*. New York: W.W. Norton.
- Downs, A. (1957). *An Economic Theory of Democracy*. New York: Harper.
- Ferejohn, J.A. (1991). "Rationality and Interpretation: Parliamentary Elections in Early Stuart England." In K. Monroe (ed.), *The Economic Approach to Politics: A Critical Reassessment of the Theory of Rational Action*. New York: Harper Collins.
- Fisek, M.H. and R. Ofshe (1970). "The Process of Status Evolution." *Sociometry* 33:327-46.
- Frank, R.H. (1988). *Passions within Reason: The Strategic Role of the Emotions*. New York: W.W. Norton.
- Frank, R.H. (1993). "The Strategic Role of Emotions: Reconciling Over- and Undersocialized Accounts of Behavior." *Rationality and Society* 5 (2):160-84.
- Garcia, J. and R.A. Koelling (1966). "Relation of Cue to Consequence in Avoidance Learning." *Psychonomic Science* 4:123-24.
- Green, D. and I. Shapiro (1994). *Pathologies of Rational Choice Theory*. New Haven, CT: Yale University Press.
- Hamilton, W.D. (1964). "The Genetical Evolution of Social Behavior." Parts I and II. *Journal of Theoretical Biology* 7:1-52.
- Hechter, M. (1994). "The Role of Values in Rational Choice Theory." *Rationality and Society* 6 (3):318-33.
- Kanazawa, S. (2001). "De Gustibus Est Disputandum." *Social Forces* 79:1131-63.
- Kreps, D.M. (1990). "Corporate Culture and Economic Theory." In J. Alt and K. Shepsle (eds.), *Perspectives on Positive Political Economy*. New York: Cambridge University Press.
- Lindenberg, S. (1992). "The Explanation of Preferences." In Van Goor (ed.) *Empirische Sociologie als Oprach*. Groningen: MB-Boek.
- March, J.G. and J.P. Olsen (1989). *Rediscovering Institutions: The Organizational Basis of Politics*. New York: Free Press.
- Mayr, E. (1961). "Cause and Effect in Biology." *Science* 134:1501-6.
- Miller, G. (1990). "Managerial Dilemmas: Political Leadership in Hierarchies." In K. Cook and M. Levi (eds.), *The Limits of Rationality*. Chicago: University of Chicago Press.
- Monroe, K. (1994). "A Fat Lady in a Corset: Altruism and Social Theory." *American Journal of Political Science* 38:861-93.
- Morris, D. (1967). *The Naked Ape*. New York: McGraw-Hill.
- Morrow, J.D. (1994). *Game Theory for Political Scientists*. Princeton, NJ: Princeton University.
- North, D. (1990). *Institutions, Institutional Change and Economic Performance*. New York: Cambridge University Press.
- Olson, M. (1965). *The Logic of Collective Action: Public Goods and the Theory of Groups*. Cambridge, MA: Harvard University Press.
- Orians, G.H. and J.H. Heerwagen (1992). "Evolved Responses to Landscapes." In J. Barkow et al. (eds.), *The Adapted Mind*. Oxford, UK: Oxford University Press.
- Ostrom, E. (1986). "An Agenda for the Study of Institutions." *Public Choice* 48:3-25.
- Ostrom, E. (1998). "A Behavioral Approach to the Rational Choice Theory of Collective Action." *Presidential Address, American Political Science Association, 1997. The American Political Science Review* 92:1-22.
- Ostrom, E. (1999). "Coping with Tragedies of the Commons." *Annual Review of Political Science* 2:493-535.
- Ostrom, E. (2000). "Collective Action and the Evolution of Social Norms." *Journal of Economic Perspectives* 14:137-58.
- Reiners, D. (2000). "Evolutionary Social Psychology of the Self." Paper presented at the annual conference of the American Sociological Association, Washington DC, August 12-14.
- Ridley, M. (1997). *The Origins of Virtue: Human Instincts and the Evolution of Cooperation*. New York: Penguin.
- Riker, W. and R. Ordeshook (1968). "A Theory of the Calculus of Voting." *American Sociological Review* 62:25-42.
- Searing, D.D. (1991). "Roles, Rules, and Rationality in the New Institutionalism." *The American Political Science Review* 85:1239-60.
- Sen, A. (1977). "Rational Fools: A Critique of the Behavioral Foundations of Economic Theory." *Philosophy and Public Affairs* 6:317-44.
- Singer, P. (1981). *The Expanding Circle: Ethics and Sociobiology*. New York: Farrar, Strauss, and Giroux.
- Sober, E. and D.S. Wilson (1998). *Unto Others: The Evolution and Psychology of Unselfish Behavior*. Cambridge, MA: Harvard University Press.
- Stryker, S. (1980). *Symbolic Interactionism: A Social Structural Version*. Menlo Park, CA: Benjamin/Cummings.
- Stryker, S. (1987). "Identity Theory: Developments and Extensions." In K. Yardley and T. Honess (eds.), *Self and Identity: Psychosocial Perspectives*. New York: John Wiley.
- Symons, D. (1979). *The Evolution of Human Sexuality*. New York: Oxford University Press.
- Trivers, R. (1971). "The Evolution of Reciprocal Altruism." *Quarterly Review of Biology* 46:35-57.
- Whitmeyer, J. (1997). "Endogamy as a Basis for Ethnic Behavior." *Sociological Theory* 15:162-78.
- Wilkinson, G.W. (1984). "Reciprocal Food Sharing in Vampire Bats." *Nature* 308:181-84.
- Williams, G. (1966). *Adaptation and Natural Selection*. Princeton, NJ: Princeton University Press.
- Wilson, E.O. (1971). *The Insect Societies*. Cambridge, MA: Harvard University Press.
- Wright, R. (1994). *The Moral Animal: Evolutionary Psychology and Everyday Life*. New York: Vintage.

## PHYSICIAN-ASSISTED SUICIDE

# Who Favors Legalizing Physician-Assisted Suicide? The Vote on Michigan's Proposal B

**John Strate** Wayne State University, USA  
**Timothy Kiska** University of Michigan—Dearborn, USA  
**Marvin Zalman** Wayne State University, USA

**Abstract.** At the November 1998 general election, Michigan citizens were given the opportunity to vote on Proposal B, an initiative that would have legalized physician-assisted suicide (PAS). PAS initiatives also have been held in Washington State, California, Oregon, and Maine, with only Oregon's passing. We use exit poll data to analyze the vote on Proposal B. Attributes associated with social liberalism—Democratic Party identification, less frequent church attendance, more education, and greater household income—led to increased odds of a “yes” vote. Attributes associated with social conservatism—Republican Party identification and frequent church attendance—led to decreased odds of a “yes” vote. Similar to the abortion issue, PAS's supporters strongly value personal autonomy, whereas its opponents strongly value the sanctity of life. Voter alignments like those in Michigan will likely appear in other states with the initiative process if PAS reaches their ballots.

---

**John Strate** is Associate Professor of Political Science at Wayne State University, where he also directs the Master of Public Administration program. He teaches American government, introduction to political science, biopolitics, elementary and advanced statistics, and policy analysis. Current research interests include the politics of physician-assisted suicide, legislative term limits, and Good Samaritan law. Correspondence should be addressed to Department of Political Science, Wayne State University, Detroit, MI 48202, USA (e-mail: jstrate@wayne.edu). **Timothy Kiska** is an Assistant Professor in the Communications Department of the University of Michigan—Dearborn. Previously, he was a journalist employed in the Features Department of *The Detroit News*, where he wrote feature stories about television shows, the television industry, and Hollywood. He also organized and directed exit polls for the newspaper. **Marvin Zalman** is Professor of Criminal Justice at Wayne State University. He teaches constitutional criminal procedure and the politics of criminal justice. He is coauthor of a textbook entitled *Criminal Procedure*. A current research interest is the jury selection process.

We analyze the correlates of citizen voting on the end-of-life policy option of physician-assisted suicide<sup>1</sup> (PAS) using data from an exit poll of Michigan voters in the November 1998 general election. On the ballot at that election was Proposal B, an initiative measure that, if passed, would have made it legal for Michigan physicians following specific procedures to write a prescription that terminally ill patients could take to end their lives. The measure reached the ballot because of the efforts of Merian's Friends,<sup>2</sup> an Ann Arbor-based citizens group that collected the necessary 261,000 signatures. The opponents of Proposal B, including Catholic organizations, Right to Life of Michigan, and more than 30 other groups, joined together into an umbrella group named Citizens for Compassionate Care.

Merian's Friends used up most of its money collecting signatures and had only about \$75,000 to spend for the campaign, while Citizens for Compassionate Care managed to raise and spend about \$5 million. Most of the \$5 million was spent on television advertising. Eight ads were produced and aired that attacked PAS and that alleged defects in Proposal B. The negative advertising was effective, and Proposal B was defeated by a huge margin, with only 28.9% voting “yes” and 71.1% “no.” The margin of defeat was remarkable, because public opinion polls in earlier years had routinely shown that a majority of Michigan citizens favored PAS.<sup>3</sup>

Before the Proposal B vote, the issue of PAS had been on the state's governmental agenda for eight and a half years, since Jack Kevorkian's assistance in the suicide of Janet Adkins in June 1990 (e.g., Fino, Strate, and Zalman, 1997). The bizarre nature of the death—which involved Kevorkian's suicide machine and took place in his Volkswagen van—attracted international media attention and placed the issue of PAS at the front of the right-to-die movement.

In part due to Kevorkian's notoriety, a national debate ensued on PAS. The medical community nationally began to take the issue seriously after Dr. Timothy Quill

(1991, 1993) admitted publicly to writing a prescription that enabled one of his terminally ill patients to end her life. There were large increases in coverage of the issue in the popular, medical, and ethical and religious literatures (Glick and Hutchinson, 2001). A number of state legislatures, including Michigan's, considered PAS legislation, but none passed a bill. Right-to-die advocates, seeing little or no chance for PAS bills in state legislatures, turned to the courts, and in states where the initiative process exists, to the citizens (Clark, 1997).

In the courts, the issue of PAS eventually reached the U.S. Supreme Court, based upon appeals of cases that originated in the states of New York and Washington. The U.S. Court of Appeals for the Second Circuit struck down a New York statute imposing criminal penalties for assisted suicide,<sup>4</sup> and the U.S. Court of Appeals for the Ninth Circuit struck down a similar Washington statute.<sup>5</sup> The U.S. Supreme Court reversed these decisions, arguing that there was no federal constitutional right to die.<sup>6</sup> The net result of these rulings has been that PAS has become an issue for the states to deal with.

Using the initiative process, right-to-die advocacy groups have put the issue of PAS before the voters in five states. PAS measures failed in Washington,<sup>7</sup> California,<sup>8</sup> Michigan, and Maine.<sup>9</sup> In November 1994, however, Oregon voters approved Measure 16 by a narrow 51% to 49% margin. Its implementation was stopped when a district court judge issued a restraining order and later ruled that it was unconstitutional. In 1997, the Catholic Church and Right to Life of Oregon successfully lobbied the state legislature to put Measure 51 before the voters. If approved, it would have repealed Measure 16. Although PAS's opponents spent \$4 million, Measure 51 lost 60% to 40%. Analysis of voting patterns on Proposal B in Michigan thus should be informative about voting that may occur in other states where the initiative process exists and PAS reaches the ballot.

In longer historical perspective, end-of-life issues became increasingly salient due to the growing availability and use of life-sustaining medical technologies, including especially antibiotics, artificial nutrition and hydration, cardiopulmonary resuscitation, dialysis, pain medications, respirators, and surgery (Hoeffler, 1997). While medical technology has saved and added years to many patients' lives, for some terminally ill patients such technology only prolongs the suffering that accompanies an inevitable dying process. Indeed, physicians have sometimes ignored the wishes of terminally ill patients to stop aggressive treatment.

The use of such technology has been especially problematic for patients who are in a persistent vegetative state, and thus not sentient. In a landmark legal decision in 1976, the New Jersey Supreme Court ruled in the case of Karen Ann Quinlan<sup>10</sup> that a guardian had the right to make medical care decisions for an incompetent patient. This case,

and many others that followed, established and extended the rights of patients and surrogates to make medical care decisions at the end of life. A growing number of states enacted legislation on advance directives, such as the living will (Glick, 1992). These allow persons to specify in advance what they would want done medically in the event they became unable to make their own decisions; however, only about a third of adults actually go to the trouble of preparing one (Yates and Glick, 1997).

PAS is a moral issue (Glick and Hutchinson, 2001). Positions on moral issues tend to be non-negotiable, because they are derived from basic values that cannot be compromised (Mooney, 2001). The supporters of PAS, typically social liberals, put foremost value upon personal autonomy and choice. They argue that some categories of patients, especially those who are terminally ill, have a right to choose the timing and manner of their own deaths. There are patients dying from AIDS, ALS, cancer, and other diseases who experience unbearable pain and suffering, as well as losses of dignity, that are not relieved by palliative care. Such patients may value the quality of life more than just its length. They may see PAS as an acceptable and even desirable end-of-life option that government should not prohibit.

The opponents of PAS, typically social conservatives, who are also religious, put foremost value upon what they view as the sanctity of life. Life is a gift of God, they believe, and it is only God who should decide when we should die. There is intrinsic dignity to natural death, they aver, even when accompanied by pain and suffering. PAS, according to opponents, devalues and even threatens the lives of those who are severely disabled, sick, and senile. Society should therefore affirm and support people's decisions to live, not to die. PAS is unnecessary, they believe, when hospice care and pain management are made available.

We hypothesize that citizens' views on PAS—as is true of moral issues generally—have been shaped to a significant extent by the culture war that in the United States has helped to redefine what it means to be a liberal or a conservative (cf. Lakoff, 1997; Nolan, 1996). Two generations ago, until the mid-1960s, apart from racial issues, it was primarily economic issues that divided liberals and conservatives. In recent decades, however, it is increasingly social issues that have divided them. Social issues—including abortion, alcohol and drug abuse, animal rights, biomedical issues, the death penalty, gambling, gay rights, censorship and pornography, gun control, social welfare, women's rights, and school prayer—have assumed prominent places on public and governmental agendas. Advocacy coalitions, often on both the liberal and conservative sides of the issue, have formed to pressure governmental policymakers to enact policies that validate their basic values but repudiate those held by their opponents (Mooney, 2001).

In the United States, for many social issues, the foremost value of social liberals—personal autonomy vis-à-vis government—directly conflicts with the foremost value of many social conservatives—control of human lives and destiny by God. The different sides on the PAS issue reflect this value conflict. In Michigan and elsewhere, it has been civil liberties organizations (such as Hemlock, Choice in Dying, Compassion in Dying, and the ACLU) that have been the strongest advocates of the right to die; it has been conservative organized interests (such as the Catholic Church and right-to-life anti-abortion groups) that have been the most vigorous opponents.

## Methods

We conduct secondary analysis of exit poll data to analyze the relationships between various demographic characteristics and the vote on Proposal B. Appendix A provides details on the survey design. The exit poll included the following question: “How did you vote on Proposal B, the proposal that would make it legal for terminally ill patients to get a lethal dose of medication to commit suicide?” Voters could check a “yes” box, a “no” box, or a box indicating that they did not vote on Proposal B. Only 2.6% checked that they did not vote on Proposal B. Marginal percentages to the exit poll are within 3% of the actual statewide vote: 31.7% indicated that they voted “yes”; 68.3% that they voted “no.” Given the short time interval between the vote and the voter’s report of the vote, exit poll data are likely more reliable than post-election interview data.

Due to space limitations, the exit poll did not include questions on social liberalism/conservatism. Accordingly, we cannot use these data to construct a model in which social liberalism/conservatism intervenes between demographic characteristics and the vote. The exit poll also did not include questions of religious affiliation and occupation.

## Hypotheses

We hypothesize that the demographics of voting on Proposal B will reflect the divisions created by culture war issues. Votes should be affected by the following factors:

### *Party Identification*

The foremost marker that divides citizens on culture war issues is party identification. Social liberals have found a home in the Democratic Party; social conservatives, a home in the Republican Party. The activities of advocacy coalitions have much to do with this. For example, on the abortion issue, pro-choice groups acquired influence in the Democratic Party, whereas pro-life Christian fundamentalist groups acquired influence in the Republi-

can Party (Watson, 1997). The Democratic Party became the party of abortion rights; the Republican Party became the party of the rights of the unborn. These differences between the parties at the elite level have not gone unnoticed by ordinary citizens,<sup>11</sup> who increasingly align themselves with parties<sup>12</sup> (Adams, 1997; DiMaggio, Evans, and Bryson, 1996) and even vote (Abramowitz, 1995) on the basis of the abortion issue rather than on issues such as spending and taxes. More generally, the Democratic Party was widely viewed as taking positions on social issues that were sympathetic to the concerns of casual drug users, feminists, gays, and welfare recipients. The Republican Party was widely viewed as taking positions on social issues that were unsympathetic to these groups but favored the concerns of traditional families and fundamentalists (Bolce and de Maio, 1999).

Although on the face of it PAS is a nonpartisan issue, it can quickly become linked with party, since it divides social liberals from social conservatives. Citizens often use shortcuts to form opinions on issues, such as specific information they get from the media or other sources about the positions that are held by elites and interest groups (Karp, 1998). On the PAS issue, elites and interest groups supporting PAS generally have links with the Democratic Party; those opposing PAS, with the Republican Party.

This was true in Michigan. Kevorkian’s flamboyant attorney, Geoffrey Fieger, was a Democrat and social liberal. Most of his verbal assaults were directed at Republican officials, including Oakland County prosecutor Richard Thompson and Governor John Engler. In the state legislature, Republicans soon became united in opposition to PAS, voting overwhelmingly in favor of a bill to ban assisted suicide. In 1998, toward the end of the conflict, Fieger used his notoriety to defeat a handpicked United Auto Workers candidate in a primary election, winning the Democratic Party nomination for governor. Although Fieger had nothing directly to do with Proposal B, his presence at the top of the Democratic Party ticket and his long-standing association with Kevorkian further strengthened the linkage of the PAS issue to party.<sup>13</sup>

### *Religiosity*

Another marker that distinguishes social liberals from social conservatives is religiosity, which may be gauged by the frequency of church attendance. Particularly for fundamentalists, religiosity is associated with greater support for conservative positions on a host of social issues (Miller and Wattenberg, 1984). For those who attend church frequently, opposition to abortion and PAS stems from the belief that life is sacred. It is God who has the authority to give and take life, not those he created. God has a plan for each of us, and conception, birth, life, and a natural death are the unfolding of that plan. To Catholics who accept Church teachings, suicide is a mortal sin and bars entry

into heaven (see Childress, 1998). In Michigan, not surprisingly, those for whom religion is very important tend to be against both abortion and PAS (*Detroit News and Free Press*, January 1, 1994:F1).

### Socioeconomic Status

Socioeconomic status (SES) has three indicators: education, household income, and occupation. The relationships between each of these indicators and opinions on social issues are complicated and can vary with the specific issue.

SES may affect attitudes toward the acceptability of suicide, which may in turn affect opinions on PAS. Individuals of lower socioeconomic status may demand less of life, be more resigned to hardships like pain, physical disability, and terminal illness, and think of PAS as an unnecessary, easy way out. Individuals of higher socioeconomic status may be more fearful of the losses of status and dignity that can accompany the dying process, and thus be more receptive to PAS.

Greater education and household income are associated with more frequent use of the media to follow politics and higher levels of political knowledge. Since PAS has only received attention from the popular media in the last decade (Glick and Hutchinson, 2001), those with greater education are more likely to have read about it, be aware of the arguments for it, and on that account perhaps be more receptive to it. In general, citizens tend to be risk averse and will vote "no" on initiative measures they have heard little or nothing about (Donovan et al., 1998).

Greater education is also associated with higher levels of tolerance. With regard to PAS, such tolerance may be reflected in greater acceptance and sympathy for persons choosing PAS as an end-of-life option, and thus greater support for the option itself.

Survey research (Clark, 1997) and polling data (*Detroit News and Free Press*, January 1, 1994:F5) show greater support for PAS among those with higher levels of education.

### Race

Although blacks tend to be liberal, this is most noteworthy on racial, social welfare, and economic issues (Kinder, 1996).<sup>14</sup> For several reasons, support for PAS should be lower among blacks. Rates of suicide are lower among blacks. They also display higher levels of religiosity and lower levels of acceptance of suicide generally (Stack, 1998). We speculate that members of minority groups who have been subjected to discrimination may show greater resiliency in confronting pain, physical disability, and terminal illness. They also may fear PAS because they put less trust in physicians and the health care system generally.

### Age

Older age tends to be associated with more conservative positions on a wide array of social issues, including abortion, marijuana use, school prayer, and women's rights (Campbell and Strate, 1981). It is likely that older cohorts acquired values that are conservative by today's standards, and they have not changed as much as have younger cohorts (Cutler and Kaufman, 1975). Older cohorts also acquired more conscientious habits of church attendance (Putnam, 2000).

With regard to PAS, and consistent with the greater conservatism of older persons on social issues, public opinion data show higher levels of opposition among older persons (e.g., *Detroit News and Free Press*, January 30, 1994:F4).

Distinctive among social issues, however, PAS is more relevant to the old than to middle-aged or young adults. Older persons are more likely to suffer from a serious disability and become terminally ill. They are more likely to have observed the suffering of a spouse, a relative, or a friend that occurred because of a prolonged dying process. Accordingly, they may see greater need for PAS as an end-of-life option, and be more likely to support it for that reason.

Countering this, however, is that some older persons may fear that if PAS is legalized, society will be more inclined to disregard their decisions to continue holding on to life in situations where their quality of life is deemed by others to be low.<sup>15</sup> Thus, while some factors appear to predispose older persons to support PAS, other factors appear to predispose them to oppose PAS. These effects may cancel each other out.

To many young adults, PAS may seem remote, just as old age and death are remote. Opinions on PAS are not likely based on memories of the terminal illnesses and deaths of family members and friends. Young adults have a tendency to exaggerate the plight of the aged and may fail to appreciate their resiliency in adjusting to limitations due to poor health and disability. Thus, they may be more likely to view PAS as a reasonable end-of-life option.

### Sex

It is unclear how sex should affect support for or opposition to PAS. Obviously, both sexes confront problems of dying, and so both may see the need for PAS as an additional end-of-life option.

Women and men are found on both sides of social issues. On the issue of abortion, for example, women express greater opposition than do men (De Boer, 1977-78), but the differences are not large, and appear to be due to women's greater religiosity. On the issue of PAS, similar to the abortion issue, slightly higher percentages of women than men are opposed (Clark, 1997; *Detroit News and Free Press*, January 30, 1994:F5).

Is gender important? Do women's attitudes towards PAS differ from men's because of women's role as caregivers? Traditional women who value this role to the exclusion of other roles may think of PAS as the antithesis of caregiving. This may be especially true of black women, who historically were employed as child care and household workers, and who now are often employed in assisted-living facilities, nursing homes, and hospitals.

### Bivariate Analysis

We first use the exit poll data to examine the bivariate relationships that exist between various demographic characteristics and the Proposal B vote. We can confirm and extend the findings of public opinion data based on newspaper polls. Analysis of polling data has found positive correlations between Democratic identification, income, education, and support for PAS. It has found negative cor-

relations between Republican identification, age, being black, being a woman, and support for PAS (e.g., *Detroit News and Free Press*, January 30, 1994:F1, F4, F5).

Table 1 shows the results. Consistent with findings regarding public opinion and PAS, a higher percentage of Democrats voted for Proposal B (39.9%) than Republicans (20.5%). Also consistent, the vote for Proposal B was highest among those with no religious affiliation (53.2%) or who almost never attend church (51.4%); it was lowest among those who attend church every week (13.4%). There was a positive relationship between household income and a vote for Proposal B, with support highest in the \$100,000 plus category (43.0%). Education was positively correlated with a vote for Proposal B, although those with "some high school" showed an unusually high percentage (39.5%) voting "yes."

Unexpectedly, the percentage of blacks voting for Proposal B (31.4%) was only slightly less than for whites

**Table 1. Personal Characteristics and Percentage Voting "Yes" on Proposal B**

Characteristic		% Yes	N	Correlations with Vote
Party Identification:	Democrat	39.9	736	Kendall's tau-c = -.173** Gamma = -.295**
	Independent, Other, None	34.3	670	
	Republican	20.5	683	
Church Attendance:	No affiliation	53.2	126	Kendall's tau-c = -.332** Gamma = -.460**
	Almost never	51.4	278	
	Few times a year	43.8	413	
	Once or twice a month	31.6	285	
	Almost every week	25.8	326	
	Every week	13.4	640	
Education:	Eight grades or less	21.2	33	Kendall's tau-c = .087** Gamma = .122**
	Some high school	39.5	86	
	Some high school +	20.6	107	
	High school graduate	24.0	313	
	High school graduate +	34.9	229	
	Some College	31.5	476	
	College Graduate	31.7	524	
Household Income:	Advanced Degree	41.1	326	Kendall's tau-c = .117** Gamma = .166**
	\$0-\$19,999	22.6	186	
	\$20,000-\$39,999	29.6	450	
	\$40,000-\$59,999	29.0	503	
	\$60,000-\$79,999	32.9	401	
	\$80,000-\$99,999	39.5	185	
Race:	\$100,000+	43.0	244	Lambda = .000, n.s. Phi = .014, n.s.
	White	32.4	1573	
	Black	31.4	328	
Age:	Other	29.9	127	Kendall's tau-c = -.004, n.s. Gamma = -.006, n.s.
	Under 20	35.5	76	
	20-29	32.1	224	
	30-39	32.3	430	
	40-49	28.7	616	
	50-59	38.0	434	
Sex:	60-69	30.9	223	Lambda = .000, n.s. Phi = -.031, n.s.
	70+	21.3	94	
	Women	30.4	1021	
	Men	33.2	1065	

Source: Analysis of *The Detroit News*-WWJ Radio exit poll data, November 3, 1998

Notes: Vote is coded 1 = "no", 2 = "yes." Race "other" category includes American Indian, Asian, and other. \*\* p < .01; n.s. = not statistically significant.

(32.4%). There was almost no relationship between age and a vote for Proposal B, with the exception that only a small percentage of those age 70 and older (21.3%) voted “yes.” Finally, as expected, a smaller percentage of women (30.4%) voted “yes” on Proposal B than men (33.2%), but the difference was not large.

In summary, the direction of the bivariate relationships found with respect to the vote on Proposal B were the same as those generally found in public opinion polls on PAS. For several relationships, however, such as for race, age, and sex, correlations were very small.

**Multivariate Analysis**

Do these bivariate relationships persist when controlling for the effects of other personal characteristics? We hypothesize that the largest effects upon the Proposal B vote are attributable to party identification and religiosity. Identification with the Democratic Party and infrequent church attendance are the most important markers of social liberalism; identification with the Republican Party and frequent church attendance are the most important markers of social conservatism. Controlling for these personal characteristics, the effects of other personal characteristics are likely to be attenuated. That is, the unexpectedly small “yes” vote among women, among those 70 and older, and among blacks may reflect their higher levels of identification with the Democratic Party and their more frequent church attendance.

To examine the separate effects of different personal characteristics upon the Proposal B vote, we conducted logistic regression. This method was used because a dichotomous dependent variable (0 = voted “no” on Proposal B; 1 = voted “yes”) is best reconceptualized in terms of odds or probability (i.e., the odds or probability of voting yes on Proposal B). In the initial regression specification, we included a large number of independent variables: separate dummy variables for Democratic identification and Republican identification,<sup>16</sup> church attendance,<sup>17</sup> education,<sup>18</sup> household income, a dummy variable for black race, age, and a dummy variable for woman. Two of these variables, including age and the dummy variable for woman, had small and statistically insignificant coefficients, and were dropped from the equation. We also added a dummy variable for black woman, a demographic group that in theory should be opposed to PAS. The results of the reduced logistic regression are shown in Table 2.

Variables with a negative coefficient included Republican identification, church attendance, and black woman. Variables with a positive coefficient included Democratic identification, education, and household income. The negative coefficient means smaller odds of voting yes on Proposal B; a positive coefficient, greater odds. The fit of the overall model is statistically significant, and the model correctly predicted 71.2% of the cases.

For the dummy independent variables, the odds ratio or exp (b) can be used to interpret the effects on the odds of possessing a particular characteristic when other indepen-

**Table 2. Logistic Regression Analysis of Personal Characteristics Associated with “Yes” Vote on Proposal B**

Independent Variable	B	S <sub>b</sub>	Prob	Exp(b)
Constant	-.153	.242	.528	.858
Republican ID	-.710	.141	.000	.492
Democrat ID	.253	.124	.041	1.288
Church attendance	-.389	.033	.000	.678
Education	.072	.034	.032	1.075
Household income	.183	.039	.000	1.200
Black woman	-.372	.190	.050	.689

N = 1,924  
 -2LL = 2139.2  
 Cox & Snell R-Square = .131  
 Nagelkerke R-Square = .182  
 Chi-square (6) = 267.4  
 Correctly predicted = 71.2%

Source: Analysis of The Detroit News and WWJ Radio exit poll data.

Notes: Variable coding

Republican ID: 0 = Other, None, Independent, Democrat; 1 = Republican

Democrat ID: 0 = Other, None, Independent, Republican; 1 = Democrat

Church attendance: 1 = No affiliation; 2 = Almost never; 3 = Few times a year; 4 = Once or twice a month;

5 = Almost every week; 6 = Every week

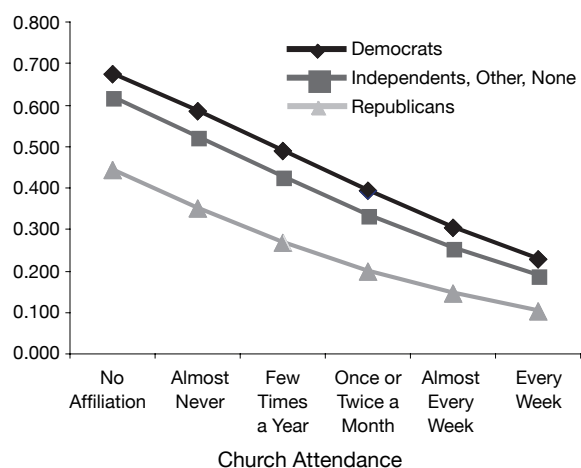
Education: 1 = Eight grades or less; 2 = Some high school; 3 = Some high school plus; 4 = High school graduate;

5 = High school graduate plus; 6 = Some college; 7 = College graduate; 8 = Advanced degree

Household income: 1 = \$0-\$19,999; 2 = \$20,000-\$39,999; 3 = \$40,000-\$59,999; 4 = \$60,000-\$79,999; 5 = \$80,000-\$99,999;

6 = \$100,000+

Black woman: 0 = Other; 1 = Black woman



**Figure 1. Predicted Probability of Voting “Yes” on Proposal B, by Party Identification and Church Attendance**

dent variables are held at their means. Being a Republican reduced the odds of voting “yes” on Proposal B by a multiplier of .492. Being a Democrat increased the odds of voting “yes” on Proposal B by a multiplier of 1.288. Being a black woman reduced the odds of voting “yes” on Proposal B by a multiplier of .689.

The person with the highest probability (.80) of voting “yes” on Proposal B was a Democrat who had no religious affiliation, held an advanced degree, and had a household income of \$100,000 or greater.<sup>19</sup> The person with the lowest probability (.05) of voting “yes” on Proposal B was a Republican who attended church every week, had eight grades or less of education, and had a household income less than \$20,000.

For the continuous independent variables, because of nonlinearity, interpretation of effects is not straightforward, and is best depicted graphically. Figure 1 shows the probability of voting “yes” on Proposal B for Democrats, Republicans, and the residual category that includes mostly Independents, at different levels of church attendance. We assigned values for education and household income equal to the sample means. We assumed that the voter was not a black woman, that is, its dummy variable was set equal to zero. The probability of voting “yes” declined steadily with each increment in church attendance for all three groups. There was a sizable gap between Democrats and Republicans at all levels, although the gap narrowed for those attending church every week.

## Summary

In Michigan, social liberals lined up to support PAS; social conservatives lined up to oppose it. Voters in Michigan, due to media coverage of the PAS issue over an extended period, picked up on the linkages between support for PAS, social liberalism, and the Democratic Party; and the link-

ages between opposition to PAS, social conservatism, and the Republican Party. When voting on PAS, those who were social liberals—generally those holding a Democratic Party identification, those with no religious affiliation or low rates of church attendance, and those with high levels of education and household income were more likely to vote “yes” on Proposal B. Those with the opposite characteristics tended to vote “no.” Similar alignments are likely to occur in other states that have the initiative process if PAS should reach their ballot.

The conflict over PAS is only a small battle in the culture war, and far less important than the conflict over abortion, a moral issue that has held a privileged position on the public and governmental agendas for three decades. For social liberals, the high priority accorded to the value of personal autonomy guides views on both abortion and PAS—it is a woman’s right to choose whether or not to gestate a fetus, just as it is a terminally ill person’s right to choose PAS as a means to end suffering, and government has no right to interfere with these personal decisions. For social conservatives, belief in the sanctity of life establishes both the right of the unborn child and the need for the state to prohibit PAS. Thus, moral reasoning about PAS often parallels moral reasoning about abortion.

## Postscript

In Michigan, the landslide defeat of Proposal B in November 1998, and more significantly, the conviction and imprisonment of Jack Kevorkian in March 1999 on a charge of second degree murder in the euthanasia of Thomas Houk, led to the disappearance of PAS from the state’s public agenda.

PAS is public policy in Oregon. Policymakers in other states now have a basis to judge its consequences. Annual reports (Oregon Health Division, 2000) indicate limited use of PAS. In 1998, 24 prescriptions for lethal doses of medication were written; in 1999, it was 33; and in 2000, it was 39. Through 2000, 70 persons had died after ingesting lethal doses of medication. Patients using PAS differed little demographically from other citizens in Oregon dying of similar diseases, except that they were better educated. Oregon’s experience with PAS appears to refute several arguments made by opponents against it. It has not led to a rush by terminally ill patients to die, and it has not been used mostly by the disadvantaged. In addition to Oregon, the Netherlands has legalized euthanasia, providing yet another case for state policymakers to observe.

Will PAS remain on the national public agenda? Several trends favor increased public interest in PAS as an end-of-life option, including the aging population, declining church attendance, growing liberalism on end-of-life issues, and the continuing use of life-extending medical technologies. Other trends, however, favor diminished public interest in PAS. There appears to be increasing

emphasis in medical education on pain management and end-of-life care. Hospice is becoming more widely available. Terminally ill patients may also learn about and turn to end-of-life options, such as voluntary dehydration, that require little medical assistance (Hoefler, 1997).

## Appendix A.

### Exit Poll Sampling Information

The number of registered voters in each of Michigan's 83 counties was obtained. The counties were then put in alphabetical order. The counties were assigned, sequentially, a starting and ending number, the range or width of the class interval of each county being equal to the number of their registered voters. A random sample of counties was then selected. This was done by selecting numbers, after a random start, using a fixed interval, and determining the county to which the number applied (i.e., the probability of selection of a county would be proportional to the number of its registered voters). A random sample of precincts was then chosen from those counties. The precincts were exit-pollled between 7 a.m. and 10 a.m. on election day, and then again between 2 p.m. and 5 p.m., or between 3 p.m. and 6 p.m.

There was oversampling of voters in Wayne County. About 2/5 of Wayne County's population is black, since it is home to Detroit, where 81% to 83% of the population is black. Thus, blacks constituted a larger portion of the sample than they do of voters statewide (about 9%). Our analysis is based upon unweighted data, since it generally yields smaller standard errors. We note, however, that assigning a weight to voters from Wayne County yields results that are very similar (for cross-tabulation percentages, within less than a percent or two) to those obtained from unweighted data.

There were a total of 2,140 respondents who indicated their vote on Proposal B in the exit poll: 31.7% indicated a "yes" vote, 68.3% indicated a "no" vote, percentages within the range of sampling error of the actual vote. Standard errors for estimated proportions based upon the assumption of a simple random sampling may be calculated using the formula  $s_p = \sqrt{[(p/q)/n]}$ , where  $q = p-1$ . Since exit polls approximate a cluster sample, actual standard errors would be higher than these.

### Acknowledgements

This research was supported in part by a grant from the Barber Fund, Center for Legal Studies, Wayne State University. We thank *The Detroit News* and WWJ Radio for use of their exit poll data, and four anonymous reviewers for their helpful and constructive comments.

### Notes

1. The meanings attributed to words are important in the PAS debate, as is true of policy debates generally. The American Medical Association (AMA) makes the following distinction: Euthanasia and assisted suicide differ in the degree of physician participation. Euthanasia entails a physician performing the immediate life-ending action (e.g., administering a lethal injection). Assisted suicide occurs when a physician facilitates a patient's death by providing the means and/or information to enable the patient to perform the life-ending act (e.g., the physician provides sleeping pills and information about the lethal dose, while aware that the patient is intending to commit suicide) (American Medical Association, 1991). Some advocates of PAS dislike use of the word "suicide" because it commonly implies (except for rare cases of self-sac-

rificial altruism) the existence of a diagnosable mental health problem. They argue that such an implication is wrong when competent, terminally ill adults decide to take actions that bring a quick end to suffering caused by an irreversible dying process.

2. The group took this name in order to honor Merian Frederick, who suffered from ALS, and was Jack Kevorkian's nineteenth assisted suicide.
3. *January 15-20 1997*; Question: Do you favor or oppose the idea of allowing physician-assisted suicide for people who are physically suffering, or terminally ill, but mentally capable of requesting help to die? Results: favor, 63%; oppose, 33%; undecided, 4%; Sample Size: 600; Method: telephone poll; Polling Organization: EPIC/MRA, Lansing, MI; Source: *Detroit Free Press*, March 8, 1997 (<http://freepress.com/suicide/qkevine8.htm>).
- April 1996*; Question: How would you vote on a ballot proposal to give mentally competent adults the option to request physician-assisted suicide under certain conditions? Results: yes, 60%; no, 36%; undecided/don't know, 4%; Source: Statewide telephone survey of 600 registered voters by EPIC/MRA, Lansing, Michigan; *Detroit Free Press*, June 29, 1996:3A.
- May 1995*; Question: Should assisted suicides be legal? Results: legal, 9%; strict guidelines, 71%; banned, 22%; undecided, 3%; Source: Statewide telephone survey of 600 adults by EPIC/MRA, Lansing, Michigan; *The Detroit News*, May 30, 1995:D1.
- December 1994*; Question: Please try to imagine that doctors discovered that you have a terminal illness that is certain to involve a great deal of pain and suffering. If physician-assisted death were legally available, do you think you might request it for yourself? Results: yes, definitely, 33%; probably, 25%; uncertain, can't say, 20%; probably not, 8%; definitely not, 15%; Source: *Detroit News and Free Press*, December 4, 1994:A21.
- January 1994*; Question: Can you think of any situations where you would consider assisted suicide as an option for yourself? Results: yes, 51%; no, 44%; undecided/don't know, 4%; Question: Can you think of any situations where assisted suicide might be appropriate for someone else? Results: yes, 64%; no, 31%; undecided/don't know, 5%; Source: *Detroit News and Free Press*, January 30, 1994:F4.
4. *Quill v. Vacco*, 80 F.3d 716 (2nd Cir. 1996) cited the equal protection clause of the Fourteenth Amendment.
5. *Compassion in Dying v. Washington*, 79 F.3d 790 (9th Cir. 1996) cited a constitutionally based liberty interest.
6. *Vacco v. Quill*, 521 U.S. 793 (1997); *Washington v. Glucksberg*, 521 U.S. 702 (1997).
7. In November 1991, Washington voters had the first opportunity to vote on a PAS initiative, Proposition 119, and turned it down by a margin of 54% to 46%.
8. In November 1992, California voters rejected Proposition 161, turning it down by a margin of 54% to 46%.
9. In November 2000, Maine voters rejected a PAS initiative, Question 1, by 51% to 49%.
10. *In Re Quinlan*, 355 A.2d 647 (N.J. 1976). Ms. Quinlan was a young woman who collapsed at a party and fell into a coma, probably as a result of the synergistic effects of alcohol and drugs. She suffered irreversible brain damage, with no hope of recovery.
11. More than one-third of the public is able to correctly locate the positions of the Democratic and Republican parties on the issue of abortion. When asked to locate the position of the Democratic Party on abortion, 35.6% said "always as personal choice"; when asked to locate the position of the Republican Party, 17.8% said "never permitted" and another 34.5% said "rape, incest, and danger only" (source: analysis of 1998 National Election Study [NES] data).
12. Democrats tend to hold more liberal positions on the issue of abortion. Whereas 46.0% of Democrats hold the opinion that abortion should be permitted "always as personal choice," only 32.0% of Republicans do. Whereas 13.4% of Republicans hold the opinion that abortion should "never be permit-

ted," and another 35.4% hold the opinion that it should be permitted only in cases of "rape, incest, and danger," smaller percentages (10.4% and 28.0%) of Democrats hold these views (source: analysis of 1998 NES data).

13. William Milliken, a popular former *Republican* governor, endorsed Proposal B in a television advertisement; however, the ad ran for only a few days. Also, since Milliken last served as governor in 1982, he was a virtual unknown to many of Michigan's current citizens.
14. We exclude civil rights issues (affirmative action, fair housing, equal employment opportunity, voting rights) from the category of social issues.
15. Citizens for Compassionate Care ran a television advertisement, "Just Like Me," that portrayed an avuncular African-American elder who calmly and persuasively expressed his fears of what would happen to people "just like me" if legislation like Proposal B passed.
16. Party identification was measured using one of the NES questions: "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an independent, or what?" Voters were given five choices: Republican; Independent; Democrat; Other party; No preference. Apart from the dummy variables for Democratic and Republican identification, we were left with a residual group that included voters who checked Independent, Other party, and No preference. They were used as the baseline group in the logistic regression.
17. We include church attendance, a variable measured at the ordinal level, even though it is assumed in regression analysis that the independent variables are interval level or dichotomous and measured without error. The effect in regression analysis of using an independent variable that has some measurement error is to generate bias in the estimated coefficient.
18. Like church attendance, education was measured at the ordinal level (see Note 17).
19. Probabilities may be calculated from the following formula:  $P(y=1) = 1/(1 + e^{-L^{\wedge}})$ , where  $e$  is the exponential constant.  $L^{\wedge}$  is the predicted log odds of voting "yes" ( $y=1$ ) and may be calculated from the logistic regression equation  $L^{\wedge} = b_0 + b_1 x_1 + \dots + b_n x_n$ .

## References

- Abramowitz, A.I. (1995). "It's Abortion, Stupid: Policy Voting in the 1992 Presidential Election." *The Journal of Politics* 57:176-86.
- Adams, G.D. (1997). "Abortion: Evidence of an Issue Evolution." *American Journal of Political Science* 41:718-37.
- American Medical Association, Council on Ethical and Judicial Affairs (1991). *Report: D (A-19), Decisions Near the End of Life 3*.
- Bolce, L. and G. de Maio (1999). "The Anti-Christian Fundamentalist Factor in Contemporary Politics." *Public Opinion Quarterly* 63:508-42.
- Campbell, J.C. and J. Strate (1981). "Are Old People Conservative?" *The Gerontologist* 21:580-91.
- Childress, J.F. (1998). "Religious Viewpoints." In L.L. Emanuel (ed.), *Regulating How We Die: The Ethical, Medical, and Legal Issues Surrounding Physician-Assisted Suicide*. Cambridge, MA: Harvard University Press.
- Clark, N. (1997). *The Politics of Physician Assisted Suicide*. New York: Garland.
- Cutler, S.J. and R.L. Kaufman (1975). "Cohort Changes in Political Attitudes: Tolerance of Ideological Nonconformity." *Public Opinion Quarterly* 39:69-81.
- De Boer, C. (1977-78). "The Polls: Abortion." *Public Opinion Quarterly* 41:553-64.
- DiMaggio, P., J. Evans, and B. Bryson (1996). "Have American's Social Attitudes Become More Polarized?" *American Journal of Sociology* 102:690-755.
- Donovan, T., S. Bowler, D. McCuan, and K. Fernandez (1998). "Contending Players and Strategies: Opposition Advantages in Initiative Campaigns." In S. Bowler, T. Donovan, and C.J. Tolbert (eds.), *Citizens as Legislators: Direct Democracy in the United States*. Columbus, OH: Ohio State University Press.
- Fino, S.P., J.M. Strate, and M. Zalman (1997). "Paging Dr. Death: The Political Theater of Assisted Suicide in Michigan." *Politics and the Life Sciences* 16:87-103.
- Glick, H.R. (1992). *The Right to Die: Policy Innovation and Its Consequences*. New York: Columbia University Press.
- Glick, H.R. and A. Hutchinson (2001). "Physician-Assisted Suicide: Agenda Setting and the Elements of Morality Policy." In C.Z. Mooney (ed.), *The Public Clash of Private Values: The Politics of Morality Policy*. New York: Chatham House.
- Hoefler, J.M. (1997). *Managing Death*. Boulder, CO: Westview Press.
- Karp, J.A. (1998). "The Influence of Elite Endorsements in Initiative Campaigns." In S. Bowler, T. Donovan, and C.J. Tolbert (eds.), *Citizens as Legislators: Direct Democracy in the United States*. Columbus, OH: Ohio State University Press.
- Kinder, D.R. (1996). *Divided by Color: Racial Politics and Democratic Ideals*. Chicago: University of Chicago Press.
- Lakoff, G. (1997). *Moral Politics: What Conservatives Know that Liberals Don't*. Chicago: University of Chicago Press.
- Miller, W.E. and M.P. Wattenberg (1984). "Politics from the Pulpit: Religiosity and the 1980 Elections." *Public Opinion Quarterly* 48:301-17.
- Mooney, C. (2001). *The Public Clash of Private Values: The Politics of Morality Policy*. New York: Chatham House.
- Nolan, J.L., Jr. (1996). *The American Culture Wars: Current Contests and Future Prospects*. Charlottesville, VA: University Press of Virginia.
- Oregon Health Division, Center for Health Statistics (and Vital Records) (2000). *Oregon's Death with Dignity Act Annual Report 2000*. Oregon Health Division, Center for Health Statistics and Vital Records.
- Putnam, R.D. (2000). *Bowling Alone. The Collapse and Revival of American Community*. New York: Simon and Schuster.
- Quill, T.E. (1991). "Death and Dignity: A Case of Individualized Decision Making." *New England Journal of Medicine* 324 (10):691-94.
- Quill, T.E. (1993). *Death and Dignity: Making Choices and Taking Charge*. New York: W.W. Norton.
- Stack, S. (1998). "The Relationships between Culture and Suicide: An Analysis of African Americans." *Transcultural Psychiatry* 35 (2):253-69.
- Watson, J. (1997). *The Christian Coalition: Dreams of Restoration, Demands for Recognition*. New York: St. Martin's Press.
- Yates, J.L. and H.R. Glick (1997). "The Failed Patient Self Determination Act and Policy Alternatives for the Right to Die." *Journal of Aging and Social Policy* 9:29-50.

## FACTS AND VALUES

# A Critique of the Naturalistic Fallacy Thesis

Jan Tullberg  
Birgitta S. Tullberg

Stockholm School of Economics, Sweden  
University of Stockholm, Sweden

**Abstract.** If the prescriptive “ought” is separated from the factual “is,” an intellectual analysis of the real world is by definition without normative value. The naturalistic fallacy thesis—maintaining that normative and descriptive spheres must remain separated—is often presented in a weak sense that seems reasonable. However, only in a strong sense—by strictly separating facts and values—are fallacy accusations supported. We claim that this naturalistic fallacy thesis is unsound and that normative statements instead should be based on rational understanding as found in the Darwinian and social sciences. The Cartesian compromise should be abandoned, since only naturalism can provide a cogent framework for better understanding and support ethics with a solid foundation. Many people nurture values based on tradition, whim, subgroup identification etc., and they demand respect for those values. However, we can demand respect for values only when they have a rational foundation. The common belief in the thesis of naturalistic fallacy is an anti-intellectual device that shields values from rational inquiry.

---

**Jan Tullberg** is Assistant Professor at the Stockholm School of Economics, Box 6501, S-11350 Stockholm, Sweden (E-mail: jan.tullberg@hhs.se). His main interest is the relationship between ethics and behavior, specifically the effect of normative values on the ability to cooperate. **Birgitta S. Tullberg** is Professor at the Department of Zoology, University of Stockholm, S-10691 Stockholm, Sweden (E-mail: birgitta.tullberg@zoologi.su.se). Her research includes the evolution of defenses in insects, phylogenetic ecology, and human behavior. Together, they have published a book on naturalistic ethics based on an evolutionary perspective, *Natural Ethics, a Confrontation with Altruism* (printed in Swedish), that has caused a major debate in Sweden. In 1997 they published an article in *PLS* (September issue), suggesting principles for solving ethnic conflict.

This article is a critical evaluation of the widely accepted “thesis of the naturalistic fallacy” (NF-thesis), an idea claiming that the normative sphere is strictly separated from the descriptive, and that a serious mistake is made in deducing an “ought” from an “is.” Considering its popularity, to question the NF-thesis in a blunt manner may be construed as a provocative or unfruitful exercise, but we share Jonathan Barrett’s view of the issue at stake: “if we feel that we must take Darwin really seriously, then we must realise that we can do this only by showing how the NF-thesis should not be taken seriously at all” (Barrett, 1991:436-37). In this article we examine the conventional acceptance of this thesis and ask whether there exists any appropriate support for its validity. Further, we try to shed light on the popularity of the thesis and ask what lies behind its attraction. We conclude that the naturalistic fallacy is a popular but unfounded thesis that can be used to support nonrational ideas.

### Origin

Are there any good reasons to question the conventional acceptance and popularity of the naturalistic fallacy thesis? From a wider than the current philosophical perspective, important thinkers, such as Aristotle and Adam Smith, formed their ethical ideas under strong influences from observations of reality. Did they blunder? Current support for the NF-thesis can be traced to two influential philosophers, namely David Hume and G. E. Moore. We start with a historical, albeit sketchy, account of the origin of the NF-thesis to reopen the question for serious evaluation. Simply because the current opinion favors the NF-thesis does not make it right, or by default provide it with immunity from deliberation.

G. E. Moore coined the label, but David Hume is widely recognized as its originator—even if Moore makes no reference to Hume. When Moore introduced the term, he was focused on another aspect, as indicated in this quote:

And it is a fact, that Ethics aims at discovering what are those other properties belonging to all things which are good. But far too many philosophers have thought that when they named those other properties they were actually defining good; that these properties, in fact, were simply not ‘other,’ but absolutely and entirely the same with goodness. This view I propose to call the “naturalistic fallacy” and of it I shall now endeavour to dispose. (Moore, [1903]1948: section 10:10). [As a semantic faux pas, it might be noted that the independence of good from facts is emphasized with “is a fact” as well as “in fact.”]

This position launched the thesis, but its content has changed profoundly from what Moore had in mind. As Bernard Williams notes: “the doctrine of the naturalistic fallacy is not or at least rapidly ceased to be, a ban merely on defining good. Rather it was taken as setting up two classes of expression. . . . The ban prohibits any attempt to deduce an evaluative conclusion from premises that are entirely non-evaluative” (Williams, 1985:122).

Hume’s central lines are often quoted and perhaps provide a better impression for what the thesis now conveys than the quote from Moore. Despite being repetitive, these lines are so central to the thesis that we dare not omit them at the risk of overlooking exceptional readers unfamiliar with this quote.

In every system of morality, which I have hitherto met with, I have always remark’d, that the author proceeds for some time in the ordinary way of reasoning, and establishes the being of a God, or makes observations concerning human affairs; when of a sudden I am surpriz’d to find, that instead of the usual copulations of propositions, is, and is not, I meet with no proposition that is not connected with an ought, or an ought not. This change is imperceptible; but is, however, of the last consequence. For as this ought, or ought not, expresses some new relation or affirmation, ‘tis necessary that it shou’d be observ’d and explain’d; and at the same time that a reason should be given, for what seems altogether inconceivable, how this new relation can be a deduction from others, which are entirely different from it. (Hume, [1740]1973:469)

Is this really a strong argument and has anyone ever said that “is” and “ought” are synonymous? But are they “entirely different” and without connection? A dichotomy often creates an illusionary division, fundamentally non-existent. Light and dark are opposites, but they are held together by shades of grey; dark is fundamentally just very little light, nothing entirely different at all.

Some phenomena make a strict dichotomy between facts and values questionable. Terms like *rude*, *treason*, *promise*, and *brutality* have a descriptive content and are

loaded with value. If viewed from the opposite angle, they are value statements strongly affiliated to some factual conditions. John Mackie (1977), Philippa Foot, and Bernard Williams are philosophers pointing at such “thick” concepts as a demonstration that facts and values are not entirely separate (Thomson, 1990). In evaluating many problems, an acceptance of a common dichotomy in the premise of analysis sets the scientist on the wrong track. Rather than simplify the analysis and constructively focus the problem, dichotomies oversimplify and often distort the issue at stake (e.g., Rutherford, 1992).

### Different Interpretations of the NF-Thesis

Like most ideas, the NF-thesis can be understood in a strong sense, in which the thesis has a message of substance, right or wrong. But it can also be considered in a weak sense, or have associated weak-sense arguments, that normally have little or nothing to say. Although being advantageous in being difficult or impossible to attack, weak-sense arguments often carry a limited message and are not worth the trouble. Therefore, we place emphasis on discussing the NF-thesis in a strong sense.

If values and facts are “entirely different,” then the person bringing facts into a value discussion commits a fallacy. If facts and values are like apples and pears, they should be split into two different discussions. In science, such a split has been made, and values have been extracted from scientific discourse. What Copernicus thought was desirable has no relevance to whether the heliocentric explanation is true or false. The significance of values is limited to what is often called the context of discovery. Certainly, affections and values had an influence in motivating Copernicus in his task, but in the context of justification, they have no proper place. Giving them an influence, consciously or unconsciously, could justly be labeled committing “the ideological fallacy”: turning an “I wish” to an “it is.” With the pears dismissed from the apple-discussion of science, it seems reasonable to suppose that there should also be a discussion of pears free from apples, that is, a value discussion based only on values. This view is the aim of the thesis in its strong sense.

No strict separation exists in the weak sense, since both values and facts can contribute to a value judgment. Using the apple-and-pear metaphor, a fruit salad is concocted with only one stipulation—that at least one pear be found among the apples. The difference between the naturalistic fallacy in its strong and weak senses is illustrated in Table 1.

If the NF-thesis is right in the strong sense, a person using facts to motivate a normative conclusion is making a logical mistake. Values and value judgments are generated by value premises, and there is no real opening for other influences. To strong-sense supporters, values are caused by core values, and factual statements are peripheral. In a similar way, controversial ideas can be discarded as being

**Table 1. Strong and Weak Senses of the Naturalistic Fallacy**

	<b>Strong Sense</b>	<b>Weak Sense</b>
Premises:	Value 1 Value 2 Value 3 <u>Fact 1 (IRRELEVANT)</u>	Fact 1 Fact 2 Fact 3 <u>Value 1 (NECESSARY)</u>
Conclusion:	Ought	Ought

a function of negative values. At the core of the ideas is usually something labeled (and personally defined) as “humanistic values,” which are seen as something some people have and others lack. The acquisition of these values might be a matter of personality or socialization, but hardly reasoning. Basically, values are generated by values. Values can be hierarchical, so more important ones might override those less important. Prevailing facts in a situation can determine a behavior in contradiction to a value, but this is a temporal adjustment, because fundamentally only a value can offset another value.

This interpretation raises doubts about facts really being so unimportant, and one may ask which quality distinguishes values. Before penetrating the question about values, we will look into less radical versions of the thesis. The weak sense of the NF-thesis is that an ought-conclusion can be drawn from is-premises, if at least one ought-premise exists as well (Sober, 1993:204). The “is” is relevant, but an “ought” is also needed. From being limited to 0 percent of the premises for a value judgment, facts can now expand to 10, 50, or 90 percent, the only restriction being that facts cannot constitute 100 percent, since at least one value is needed. If one eliminates the last value and tries to get to an ought-conclusion on facts alone, the fallacy is committed. Such a fallacy statement might be of importance when somebody makes the claim that there are no values, only facts, behind his recommendation.

With regard to the intensity with which the NF-thesis is defended, it could be assumed that it generally has a strong influence on the consistency of a line of reasoning—that people committing the naturalistic fallacy must change their opinion if convinced they committed the fallacy. If, on the other hand, a line of argument arrives at the same conclusion, regardless of opposition or acceptance of the NF-thesis, it might be considered a minor technicality of some theoretical, but of little practical, importance.

For the sake of illustration, let us take a biologist who argues that genetic diversity in a species is valuable because it increases the likelihood of survival for that species should the environment be drastically altered. A philosopher refers to the NF objection and says that there is a naturalistic assumption that can be questioned: is it really right to secure life?—without life there is no pain. The philosopher advises the biologist in Kantian terms to exchange such a categorical imperative with a hypothetical imperative (Kant, 1983): “If it is desired that the chance of

survival be enhanced, there is an advantage with genetic diversity.” The goal is less self-evident than the biologist first thought and more dependent on values. He could also explicitly tackle the question of whether life is desirable or not. Should our biologist conclude he must reject or reexamine the values behind his assumptions?

For most questions the difficult task is to establish the causal links between various factors. A second set of problems are the associated side effects and consequences that follow from a certain solution. The simplest problem is often the value part. There is a general agreement that health is better than sickness, happiness than sorrow, and life than death. These values can be questioned, but doubts have to be supported by strong reasons to motivate a change of mind.

A revision of the opinion that a judgment is not only a matter of fact, but also of values, does not imply a change of this judgment. For either a change of heart or mind, there is a need for arguments. If the biologist becomes convinced that his opinions include the value that life is intrinsically good, he might just say, “OK, so I must have supported that value.” A discovery of hidden or needed values is no argument at all for a person to change his opinion. The reaction is, of course, to stand by the “unconscious” value judgment until presented with strong arguments for a change. Charging him with the naturalistic fallacy does not ruin his line of reasoning; it just adds a subordinate clause to the same judgment.

Often the NF in the weak sense is seen as so reasonable a condition that it even becomes impossible to commit a fallacy. If there is always a value premise behind a value judgment, the only question is whether the value premise should be explicit or not. Often it is claimed that it is helpful for a reader if the writer declares his values rather than let them be cryptic. An implicit consequence of that advice is that potential readers might avoid a line of reasoning that is inconsistent with their values. To avoid such a preselection, voluntary declarations are often not very informative, but more of a marketing character, stressing common values. However, there will not only be positive marketing, but opponents will suggest that unpopular values are the prime force behind uncomfortable results and reasoning. No doubt there are situations in which explicit values are helpful for understanding a line of reasoning, but we do not think those situations arise often enough to motivate a general demand for explicit values. There are some good reasons for the present situation as characterized by Sjöberg and Montgomery: “Argumentation is normally not about values but about the state of the world. This is a common finding in studies of political controversies. . . . This may be because in debates values are typically either all positive to all parties or some of them are not suitable for public disclosure. . . . *De gustibus non est disputandum*” (Sjöberg and Montgomery, 1999:618).

In the weak sense, the place for values seems to be a

pragmatic decision, and talk of fallacy seems a bit overblown. In any discussion, arguments from one side can be refuted on the grounds that they seem wrong or irrelevant. But such objections can be raised against values as well as facts. In the weak sense of the thesis, there is no basis for claims that a fact is inappropriate per se—that it is an apple in a discussion strictly about pears. Our conclusion is that the value condition of the NF-thesis in the weak sense is reasonable to such a high degree that it is unavoidable, but that the contribution of implicit or explicit values is often of little importance. A common tactic is to establish the NF-thesis in the weak sense, but it is crucial to understand that there is no rational reason to accept this version as a leverage to the strong sense. To support more substantial versions of the thesis, it is necessary to justify a strict separation of facts and values, indicating an independence for values.

A less philosophical but very common interpretation of the NF-thesis is that it is not really an objection against the theoretical “is” of science but rather an anti-conservative objection against the practical “is.” This version is an objection to some kind of “Panglossian Paralysis.” If we live in the best of all possible worlds, there is no reason for change. To many non-philosophers, the rather trivial idea that we do *not* live in the best of all possible worlds is the message of the NF-thesis. In a longer historical perspective, the NF-thesis has been understood as a doctrine for secular reformists against religious conservatives that see the *de facto* situation as created and morally authorized by God. However, we do not think this is the line of conflict. Religious fundamentalists will generally support the NF-thesis in order to strengthen the independence of “ought”—an “ought” instead supported by the supernatural, the holy scriptures. Even before science became the strongest authority on facts in theory and in social practice, most religions granted a moral superiority of God’s visions over the actual conditions of the world. The discrepancy was at first caused, not by modern science, but by the revolt of Satan and Adam eating the apple of knowledge. Jesus, Mohammed, and Buddha were all moral reformers challenging the present order. Few, if any, religious fundamentalists are satisfied with the way things are, so we think the only proponent of Panglossian Paralysis is a straw man.

A fourth version of the NF-thesis is the criticism against environmentalists and naturalists who dogmatically prefer the natural or evolved to the artificial. Certainly, there are a number of people to whom epithets such as *ecological* and *natural* are equivalent to good. Others may see this as some kind of simplistic fallacy—a *naturalistic* naturalistic fallacy. However, we find such a preference for the natural not that different from other core values, such as freedom and equality. Opponents will claim that the proponents overrate them at the expense of other values. Further, these core values have in common that the proponents see them as undermined by social forces rather than being

an automatic outcome of the “is” in society. The values are all preferences for a potential state rather than being “Panglossian.”

Both the “Panglossian Paralysis” interpretation and the “naturalistic naturalistic fallacy” interpretation are common. In the following, we will, however, concentrate on the strong and, to some degree, the weak versions of the thesis. If it is important to distinguish values from facts, it is central to penetrate what is the basis for values.

## Naturalism, Ethics, and Objectivity

Edward O. Wilson pinpointed the biological foundation of ethical intuitions:

These centres [the hypothalamus and limbic system] flood our consciousness with all the emotions—hate, love, guilt, fear, and others—that are consulted by ethical philosophers who wish to intuit the standards of good and evil. What, we are then compelled to ask, made the hypothalamus and limbic system? They evolved by natural selection. That simple biological statement must be pursued to explain ethics and ethical philosophers. . . . (1975:1)

We can observe other species, and we can study cultural variation in anthropology and history. We can imagine or extrapolate alternatives generated by imagination or religious tradition, but the evaluator is nonetheless always human. What, then, justifies claiming a fixed point outside the biological system? The philosopher trying to eliminate all anthropocentric influence is still a member of *Homo sapiens*, and the analysis is always a thought experiment.

Our ability to care for others is much less the result of ethical analysis and teaching than a salient feature of being a species of mammal. The difference in sociality between a group of dolphins and a school of fish says something about our starting point. Hume expressed an understanding that what we feel is dependent on who we are: “there is some benevolence, however small, . . . some particle of the dove kneaded into our frame, along with the elements of the wolf and serpent” (Hume, [1777]1992). It is difficult not to see this statement as contradicting the NF-thesis. The sentiments and the values of the subject are linked to Hume’s highly regarded passions, and these passions are products of evolution. In the following quote, he takes one more step in sounding like a modern sociobiologist: “A man naturally loves his children better than his nephews, his nephews better than his cousins, his cousins better than strangers, where every thing else is equal. Hence arise our common measures of duty, in preferring one to the other. Our sense of duty always follows the common and natural course of our passions” (Hume [1740]1973:3.2.1). This reads like an abstract on the genetic rationality of kin selection.

In light of these examples we feel motivated to question Hume's position as a devoted critic of naturalistic ethics. To understand Hume's point of view regarding "is" and "ought," it seems reasonable to view it as a part of his general skepticism. The NF-thesis might be seen as a reasonable consequence of his questioning of causality as such. Hume claimed that we had no valid reason to consider one thing causing something else. The timing between events, for example, never proves that there is a causal connection; empiricism and rationalism cannot establish a connection between phenomena. The fire burning is one thing, the radiation of heat is another, and we cannot say that the blazing fire causes heat. Such a theory is hard for most people to take seriously, but philosopher Bertrand Russell (1961) comments on Hume's critique, suggesting that to date it has not been effectively refuted. At the same time, Russell notices Hume's problems by pointing out that Hume makes causal statements in the very sentences by which he questions causality. It is impossible to take any practical action without strong assumptions about causality. It is also difficult to say or write anything consistent and intelligible without implying causality. The objection against causality is a statement of principle, after which the philosopher continues to act and argue as if there is causality. But for a man who holds that there can be no connection between one "is" and another "is," the thesis that there can be no connection between an "is" and an "ought" is much more limited.

This reasoning is an attempt to find a link between the naturalistic fallacy and Hume's philosophy. It seems, however, a protracted and indirect argument, and Hume has written more directly on the subject of naturalistic justification; his general line favors a naturalistic position. It is strange that this is seldom acknowledged, and that the NF-thesis is even called "Hume's law." Kant's philosophy and the thesis are much more compatible. Here, rationality is not the slave to natural passions, but a tool for a higher purpose, to find out what is really right. Here, morality is not a part of the natural fabric, but is to be found by distancing the thought from lower determinators such as emotions and interest.

A confusion of Kant and Hume with regard to a central issue seems too elementary a fault for being generally accepted. However, we are not alone in making this judgment. For example, Larry Arnhart (1998), Richard Taylor (1979), and Alasdair MacIntyre (1981) also see the NF-thesis as a strange attempt to link Hume's name to a Kantian view and then use it against Hume's intellectual position. This stance is dubious, at best. With this statement, we do not claim to make a contribution to an historical investigation, but to point out that the NF-thesis not only has consistency problems, but also has some problems with the endorsement of the authority most often referred to.

One critique against naturalistic ethics is that it cannot provide the objective status desired by moral philosophy.

When evolutionary theory abandoned teleology, it questioned the basis for universal moral rules. Evolution itself makes no anthropocentric preference for man over bacteria or wolves, and may therefore be regarded as opening the door to relativism or indifference. Evolution *per se* gives no moral guidance.

The pure idea has always attracted attention, and mathematics is the language of science. The laws of mathematics are certainly not culturally relative and are not restricted to humans, but are a reality for all rational beings. Even without the existence of any agent, it might still be argued that  $1 + 1$  makes two. This high universality is attractive as such, but to desire that ethics resemble mathematical objectivity is overambitious and misguided.

An analogy with nutrition may provide a more accurate picture. What constitutes good food for man is not necessarily good for all rational beings. Substantial variations in nutritional habits are found among human cultures, but underlying the surface are common elements and behaviors: the fork and the chopstick may not be fundamental. Many feeding habits are linked to environmental resources, but much variation is purely cultural. But this does not make nutrition a subjective field, where any statement or habit is as good as another. This analogy is not a surrender to emotivism. Some foods are poisonous and others are beneficial in limited amounts. The way our bodies react to different food components—and our understanding of these reactions—are of fundamental importance for what we ought to eat. The ambition to make ethics as objective as mathematics has influenced thinkers to search in wrong directions under the assumption that this type of objectivity is possible, desirable, or even necessary. Our position is that only in the nutritional sense, and not in the mathematical sense, can ethics be viewed as objective.

This criticism of a mathematical ambition undercuts the common assumption in the discourse that moral judgments are logically necessary conclusions following the premises. A set of premises is a priority rather than an exhaustive list of relevant axioms. Moreover, the conclusion—with or without a value premise—is hopefully reasonable, not a logical necessity.

From this medium universalistic view, it is not unjustified to take an anthropocentric view, asking classical questions like "what is a good life for man?" and "how do we construct a fair society?" Contributions from Darwinism and the social sciences seem to be at least as suitable for such quests as religious and philosophical contemplation.

Taste and aesthetics are two other phenomena linked to evolution and objectivity in much the same manner. Symmetry is admired and may reflect health, and many other signs of beauty are indicative of well-being and reproductive capacity. Beauty may lie in the eyes of the beholder, but the fact that the eyes are in the head of a human tells us about its perception and judgment. In many animals, there is a habitat preference that tells what is to be desired.

There are two other perspectives. According to the emotive view, all statements are personal judgments expressing nothing but subjective preferences. According to the perspective of Objectivity, value statements should be more or less accurate estimates of the Right, the Beautiful, and the Sublime.

One reasoning about the objectivity of ethics is Michael Ruse's (1986) thesis that ethics is subjective in reality, although there exists a genetic predisposition to see it as objective. This theory may underestimate ordinary people. Not only clever intellectuals, starting with Herodotus and Xenophanes, but people in general are aware that many of their norms are shaped by socialization. They are aware of the relativism of norms, and many complaints about lack of ethics mostly point at loose social norms. It is essential to make a distinction between subjectivism and cultural relativism, and the second kind can be seen as valuable without being objective. The common view is probably that some norms are better than others, but that some norms of behavior might be no better or worse in themselves, their value being derived from dominant norms of a certain culture. Driving on the right-hand side of the road (or the left-hand side in other societies) is no virtue in itself, but society's insistence that we do so and the punishment of those who disobey is justified.

The proponents of the NF-thesis seldom deny cultural diversity, but see differences and similarities as of minor importance. The aim of ethics is to construct a model according to which descriptive ethics is to be judged, and therefore there is little to be learned from descriptive ethics. We think this is a misjudgment.

On occasion, the NF-thesis is used as a wedge between normative and descriptive ethics. Antony Flew states that the heart of the doctrine is "the logical Grand Canyon" between being valuable and being considered valuable. He writes: "Again, after our earlier stress on the enormous difference between saying that something is desired and saying that it is desirable, we are bound to notice the tendency to equate the valuable with what is in fact valued" (Flew, 1967:39, 42). When the "is" expands from distant facts to descriptive ethics, the step to normative conclusion becomes even less distinguishable. A philosopher pursuing the issue might conclude with these two statements: "Action A ought to be done" and "My normative judgment is that action A ought to be done." Without a microscope with high resolution, no canyon or any other enormous difference can be seen.

### **Values and Facts as Separate Worlds**

One effect of the NF-thesis is that it indirectly promotes two emotionally attractive bases for ought-statements. One is to succumb to the familiarity of traditional attitudes; the other is to adjust to uncompromising new ideas that emphasize far-reaching potential rather than realism. These old

and new visions, religious or secular, should not, according to the NF-thesis, be valued according to facts and reason, since these variables are too heavily influenced by the conditions in an amoral world. These factors may have a function when the "oughts" are to be implemented, but not when deciding these ultimate goals. At the normative level, goals should be evaluated for their intrinsic value. The following quote is representative of this opinion:

Proposed naturalistic justification of morality have appeared to suffer from a number of problems that seem to make them intrinsically unfit for playing a justificatory role. First, appeals to causal factors seem to be incompatible with the freedom and intentional activity that is essential to moral action. And, second, the facts of causal connection appear to be inappropriate for establishing normative justifications or prescriptions. (Rottschaefer, 1991:342)

In more practical terms, Beckstrom argues for limiting the facts to a secondary phase of implementation: "As I said, evolutionary science cannot be used 'normatively'—it cannot be used to determine what social goals ought to be pursued. But once social planners, public or private, have used their values and tastes to select social goals, modern evolutionary science may then step in to provide factual guides toward achieving those goals—facilitative guides" (Beckstrom, 1993:2).

Any impression of logical reasoning in the citations above will be dismissed when the same idea is expressed using another example. George Stein discusses the foundation for criticism of National Socialism:

But then, if we are to argue that they made a naturalistic fallacy in developing a racist "ought" from their putative "is," by what epistemological standard do we derive our "ought" of the unity of humankind from our putative "is" of the unity of humankind? If science cannot support racist nationalism, neither can it support liberal humanitarianism or any other normative, ethical construct. We are forced to reassert that science is totally irrelevant in the choice of public policies. (Stein, 1987:267)

However, not all political scientists accept such a declaration of impotence, that "science is totally irrelevant" when considering politics and ethics. Roger Masters makes a point that we think hits the bull's-eye:

I am, of course, aware of the so-called naturalistic fallacy which has so often been condemned by logicians and methodologists. But when the doctor prescribes a treatment, we don't normally object that this practice bridges the logical distinction between the facts of diagnosis and the value of health. Biology, like medicine, has much to teach us about our species and

the risks we confront by ignoring the natural basis and consequences of our habits of life. (1989:xv)

Later he adds one more sentence to this line of argument:

When a naturalistic approach is used in political science, it does not follow that we can say nothing about the relative desirability of different regimes. Such a conclusion would make as little sense as the assertion that the science of medicine makes it impossible to define health and prefer it to illness. (Masters, 1989:227; see also Masters, 1993 for a more extensive critique of the fact-value dichotomy)

One explanation for the popularity of the NF-thesis is that it connects to an important philosophical question—that of the free will. Most people do not agree with Spinozan dismissals, like this statement by Ken Binmore: “We are not unpredictable because we have a free will. We say we have a free will because we are not always predictable” (Binmore, 1998:513). Economists and philosophers are not always in agreement, but most will disregard random events and trembling hands as manifestations of free will. Economists will ask for “consistent preferences” and philosophers for “an authentic person” before paying homage to an individual’s choices.

The case for determinism is pushed by many different paradigms. Biology is often viewed as arguing for fixed programming by instincts. Behaviorism proposes a more flexible programming, while psychodynamic theory argues for the influence of early childhood. Economic man is more openly programmed insofar as he chooses the means for rationally obtaining his preferences, but economists say little about these preferences. Neo-Darwinism also suggests a more open programming, but ultimately there are natural passions, not an “unmoved mover.” Values may be regarded as a means of escape from the Scylla of determinism and the Charybdis of happenstance. Some ethical intuition, subjective belief, or Kantian rationality can be regarded as both sufficiently consistent and genuinely independent to be an expression of the free will. The naturalistic fallacy thesis might be seen as a guardian of free will.

Religion has long attempted to motivate values. Through time it has become successively more difficult for the proponents of religion to use reason and knowledge as positive support. The following reflection by Martin Luther illustrates the conflict between faith and knowledge: “Reason is the greatest enemy faith has: it never comes to the aid of spiritual things, but—more frequently than not—struggles against the divine Word, treating with contempt all that emanates from God” (Luther, 1569:353).

When trying to pass the court of reason, the best option religion may have is the ignorance defense. God’s existence is seldom proven anymore, but the line of defense has regrouped around the questions “Can we really know for sure that God does not exist? Is it not wiser to say we

do not know?” Pascal argued that the uncertainty should result in an acceptance of the religious proposition even if it seems very improbable (because the potential rewards are very high for a believer). If religion can get through the court of reason with a hung jury, it will do just fine. If we do not know for certain, we will believe, and in the judgment of hearts, faith has a lot of attractions.

If science cannot prove the existence of God, this failure can be understood in two ways. That God does not exist is one of the alternatives, but the devout will claim that the lack of proof illustrates the shortcomings of science. In much the same manner, the mission of moral philosophy is to prove the virtue of altruism. The crucial measure of whether naturalistic realities provide a sound basis for ethics is whether a naturalistic perspective can support an altruistic philosophy. If it does not provide such support, it is dismissed as unsuitable for ethics.

B. Mayo is one of many philosophers arguing for naturalistic support of conventional ethics: “Naturalistic theories offer to rescue ethics from perpetual uncertainty, or even meaninglessness, by offering substitute statements which we know how to verify, and claiming that they are equivalent to the original moral judgments” (Mayo, 1986:39).

Robert Richards is a philosopher who argues that naturalism can support altruism. His thesis runs as follows: “the evidence shows that evolution has, as a matter of fact, constructed human beings to act for the community good; but to act for the community good is what we mean by being moral. Since, therefore, human beings are moral beings . . . each ought to act for the community good” (Richards, 1987:623-24). Whether the author commits the NF or not is a minor question; the argumentation has more severe shortcomings (Neo-Darwinism has repeatedly shown that evolution is not driven by what is good for the species or even the community, but can generally be explained by what is good for the gene). This ecumenical agenda seems to hold that the antagonism between Darwinism and traditional values in religion and philosophy will evaporate if Darwinism can be interpreted as another path to the same destination. Richards seems to support the project suggested by Mayo. We think such a project is against the spirit of science. In pre-Copernican times, the mission of science was to confirm religious truths, but since then the result of research can be a firm critique.

The ethical discussion needs some bases. In our time, when religion has lost much weight, we are left with a void that has partly been filled with a semi-secular altruistic philosophy that hangs in the air while the pillars of religion disintegrate. The problem is how successful we can be in finding normative goals without using facts. Similarly, moral philosophy has a problematic relationship with reason and science. Long ago, Francis Bacon made the statement, which still rings true, that moral philosophy is but a handmaiden to religion.

It is hardly controversial to consider the evaluation of nature by Huxley as the prevailing opinion among humanists of our time: "Let us understand, once for all, that the ethical progress of society depends, not on imitating the cosmic process, still less in running away from it, but in combating it" (Huxley, 1894:83). With such condemnation of nature as the enemy, the gateway between ethics and nature should be replaced with a Berlin Wall. This is the context in which to see the popularity of the NF-thesis.

### **The Cartesian Compromise**

Over a century ago, Josiah Royce made an observation that was partly a forecast. He wrote: "Once man himself was accepted as a natural product of the evolutionary process, the rest of the Cartesian compromise could hardly be maintained" (Royce, 1892:viii). That forecast seems reasonable considering the potential of evolutionary theory, but the outcome has been wide off the mark. Rather, the Cartesian compromise stands stronger today.

This development can be understood in light of the reasoning behind the naturalistic fallacy. Few would disagree if we claim that Max Weber is a very influential social scientist. When writing about the objectivity of the social sciences ([1904]1968), he makes a distinction between "social science" and "social politics." The former is about means, and it can be studied objectively by scientists. The latter is about goals for which science has little or nothing to say. Unfortunately, it is unclear what is to be said in favor of social politics. Weber makes the distinction between "instrumental rationality" and "value rationality," but one is left with similar indecisiveness since instrumental rationality can be treated scientifically but value rationality cannot. The message is again to proclaim the futility of passing the boundary and saying something substantial about goals and values. Questions arise demanding deliberation. What wisdom will science damage by intruding, and can something be said about the manner in which these goals and values are decided in the absence of science? But Weber provides no appropriate answers. Some combination of will/meaning/belief influences goal-setting, but no theory is provided for how a comparison between candidates is made to improve quality. It is further difficult to understand why interference from science should damage this process.

The conclusion is a familiar one—keep the two apart. Science should stick to analysis of facts, means, and effects—but what constitutes good and what should be done are left to others. The ground for these other decision-makers is unclear, and the mystery of values prevails. The demarcation line has been set and is seen as practical by many—even though the intellectual founding eroded with the theory of evolution. The Cartesian split between the soul and the body is still untouched, even if few see the body as pure mechanics and the soul as pure spirit connect-

ed by the pineal gland. The long life of this dichotomous relationship is remarkable and puzzling.

However, the impression that values are of major importance has some possible explanations. Values are often shorthand for a complex of facts and values. Such a complex is labeled with terms like freedom, equality, and socialist values. Even Christian values are not simply values, but a myriad of ideas about how the world works, human reactions, causes and effects. A value judgment is often motivated by referring to general values which themselves are based on some combination of facts and values. After a more penetrating discussion, the differences in "pure" values are often best explained by the differences in perceived self-interest; justice has a tendency to sympathize with general rules in line with our specific needs. In other cases, value is the label used for assumptions about facts, expectations about reality, or vague generalizations and prejudices.

One advantage of the term *value* is that it asks for some respect. It is not just a statement about something in the world but about your relation to it. Such a personal relationship is not so much a topic for critique or general discussion but rather a position to be acknowledged. In any discussion a disadvantage due to lack of knowledge or to some contradiction may be repaired by referring to a value or some special weight associated with a value. Referring to value becomes a saving device for ending the discussion with an air of parity rather than defeat.

If the NF-thesis is such a powerless weapon, why bother with whether it is considered false or true? The reason why we believe it is important is that it has some serious consequences for the general debate. The thesis tilts the debate towards declarations of goals and values of very limited intellectual importance. The serious discussion lies in how the world works and what can be done to change it in a beneficial way (i.e., intellectual reformism). Comparatively, the NF-thesis, by ignoring important aspects of reality, supports fundamentalistic or unstructured attitudes.

The NF-thesis plays a role, to say the least, in the sociobiological discussion. In one way or another, sociobiologists will be accused of committing the fallacy, and assurances to honor it can rightly be seen as inconsistencies. Richard Alexander charged E. O. Wilson with supporting the NF-thesis and at the same time breaking it (Alexander, 1987:167-68). A few pages after his support for the thesis, Alexander himself breaks it (p. 220, last paragraph). Recently Frans de Waal (1996) expressed support for the thesis while formulating normative prescriptions from his knowledge of nonhuman primates.

Too many writers take the fallacy's claims at face value and try to pass the unclear line of how the thesis is to interpreted. Even if not committing the fallacy in the weak sense, accusations will be levied. Inevitably there will be some feeling of too much reliance being placed on observations and knowledge but not sufficiently on values. In

more substantial interpretations of the thesis, all sociobiologists are committing the fallacy. We think it is unwise and unnecessary to give opponents a blank check for this accusation.

We have referred to several skeptics of the thesis, and no doubt the reader will think of further examples. Some skeptics, like Williams and MacIntyre, are well-respected and influential philosophers, but so far their criticism has not threatened a belief in the thesis, which continues to be presented to new students as a firm piece of theory, tried and true. It is not problematic that some people hold this belief; we can all think of much more bizarre ideas that have some following. What is problematic, however, is that the naturalistic fallacy is considered an uncontested thesis, and something that is agreed upon. It can even be seen as a trivial truth, which is why most proponents limit themselves in proclaiming the essence rather than indulging themselves in argumentation. Few philosophical ideas are marketed so widely, massively, and shallowly.

The reason for both marketing and acceptance is that many academics politically believe in some kind of Cartesian compromise. As scientists, we attempt to stay away from far-reaching normative conclusions. If I want less interference in my area, it might be smart to follow Weber's line. Others set the goal, and I investigate the means and effects. If the basis for setting the goal is arbitrary and weak, the normative guidelines might become some diffuse platitudes posing no major obstacle.

The next question is seldom addressed. From what substance do these value-judgments emerge? There is a need for seriously evaluating goals and establishing priorities. An elected person has not become a breed apart, and resolutions by party congresses or the United Nations do not generate judgments of a special intellectual dignity. Goals and decisions need to be founded on facts and rational reasoning.

Most politicians understand the importance of facts and attempt to be rational, but they are also practical enough to see the utility of an escape clause if they run into trouble. Poor proposals can be supported by the dubious defense of being in line with important values or in conveying important value messages. Politicians are given mandates from the people to make decisions. Good advice in making these decisions, however, is not always welcome, and some politicians will reject unsolicited recommendations. Scientists, on the other hand, have not only a privilege, but an obligation to draw conclusions from their knowledge and press their point rather than making a halt at some demarcation line. Of course, the present position of science may be proven wrong, or at least shown not to contain the whole truth, but it seems reasonable that it provides a better foundation for good decisions than ignorance. Efforts by scientists to draw normative conclusions will not hinder politicians in making similar efforts. On the contrary, it might support a more enlightened discussion. Academia is

doing everyone a disservice by voluntarily disconnecting their "is" from the "ought" of the discussion.

## Conclusions

A preoccupation with the naturalistic fallacy makes us vulnerable to the real fallacy—the ideological fallacy: to think that something exists because it is wished. When the "ought" is disconnected from the profane "is," the road is opened for illusions of positive thinking.

The Delphi had an exhortation: *Gnothi seauton*—understand yourself. Darwinists, psychologists, economists, political scientists are all making progress toward an understanding of *Homo sapiens*. The fractional split between disciplines makes it important to remind ourselves that we are analyzing the same species and that different answers need to be compatible. Of course, religion may still claim to have good reasons for its recommendations, but to many people these reasons seem insufficient. However, this weakness does not make philosophy the heir by default, and philosophers who think they can make ethics their separate domain will probably be mistaken. The Cartesian separation of the soul from the body will be hard to maintain. Skeptics of the NF-thesis hold that the "is" of science is highly relevant for the "ought" of ethics.

A common approach is to establish the NF-thesis in a weak sense and then later, discreetly, expand its consequences into claims based on the thesis in a strong sense. This intellectually confused and ethically dubious transformation is how the thesis gets its bite. There are many convenient reasons for the popularity of the NF-thesis, but we find its justification most incoherent and ultimately unproductive.

## Acknowledgments

We thank Roger Masters, Paul Needham, Tore Nilstun, Jan Edman, and two anonymous referees for valuable comments on the manuscript, and Bohdan Sklepkovych for improvements in the text.

## References

- Alexander, R. (1987). *The Biology of Moral Systems*. New York: Aldine de Gruyter.
- Arnhart, L. (1998). *Darwinian Natural Right—The Biological Ethics of Human Nature*. Albany, NY: State University of New York Press.
- Bacon, F. ([1600]1975). *Advancement of Learning II*. London: Athlone.
- Barrett, J. (1991). "Really Taking Darwin and the Naturalistic Fallacy Seriously." *Biology and Philosophy* 6:433-37.
- Beckstrom, J. (1993). *Darwinism Applied: Evolutionary Paths to Social Goals*. London: Praeger.
- Binmore, K. (1998). *Game Theory and the Social Contract. Volume 2*. Cambridge, MA: MIT Press.
- Darwin, C. (1981). *The Descent of Man, and Selection in Relation to Sex*. Princeton, NJ: Princeton University Press.

- Flew, A. (1967). *Evolutionary Ethics*. London: MacMillan.
- Hume, D. ([1740]1973). *A Treatise of Human Nature*. Oxford: Oxford University Press.
- Hume, D. ([1777]1992). *Enquiries Concerning Human Understanding*. Oxford: Oxford University Press.
- Huxley, T.H. (1894). *Evolution and Ethics*. London: MacMillan.
- Kant, I. (1983). *Ethical Philosophy*. Indianapolis: Hackett.
- Luther, M. (1569). *Table Talk*. Edited by T. Tappert. Philadelphia: Fortress.
- MacIntyre, A. (1981). *After Virtue*. Notre Dame, IN: University of Notre Dame Press.
- Mackie, J. (1977). *Ethics: Inventing Right and Wrong*. Harmondsworth: Penguin Books.
- Masters, R. (1989). *The Nature of Politics*. New Haven, CT: Yale University Press.
- Masters, R. (1993). *Beyond Relativism: Science and Human Values*. Hanover, NH: University Press of New England.
- Mayo, B. (1986). *The Philosophy of Right and Wrong*. London: Routledge.
- Moore, G.E. ([1903]1948). *Principia Ethica*. Cambridge: Cambridge University Press.
- Richards, R. (1987). *Darwin and the Emergence of Evolutionary Theories of Mind and Behaviour*. Chicago: University of Chicago Press.
- Rottschaefer, W.A. (1991). "Evolutionary Naturalistic Justifications of Morality: A Matter of Faith and Works." *Biology and Philosophy* 6:341-49.
- Royce, J. (1892). *The Spirit of Modern Philosophy*. New York: Houghton Mifflin.
- Ruse, M. (1986). *Taking Darwin Seriously*. Oxford: Blackwell.
- Russell, B. (1961). *History of Western Philosophy*. London: George Allen and Unwin.
- Rutherford, J.H. (1992). *The Moral Foundation of United States Constitutional Democracy*. Pittsburg, PA: Dorrance Publishing.
- Sjöberg, L. and H. Montgomery (1999). "Double Denial in Attitude Formation." *Journal of Applied Social Psychology* 29:606-21.
- Sober, E. (1993). *Philosophy of Biology*. Oxford: Oxford University Press.
- Stein, G.J. (1987). "The Biological Bases of Ethnocentrism, Racism and Nationalism in National Socialism." In V. Reynolds, V. Falger, and I. Vine (eds.), *The Sociobiology of Ethnocentrism: Evolutionary Dimensions of Xenophobia, Discrimination, Racism and Nationalism*. London: Croom Helm.
- Taylor, R. (1979). *Good and Evil*. London: Collier Macmillan.
- Thomson, J. J. (1990). "The No Reason Thesis." In P. Frankel et al. (eds.), *Foundation of Moral and Political Philosophy*. Oxford: Basil Blackwell.
- de Waal, F. (1996). *Good Natured—The Origins of Right and Wrong in Humans and Other Animals*. Cambridge, MA: Harvard University Press.
- Weber, M. ([1904]1968). "Die Objektivität Sozialwissenschaftlicher und Sozialpolitischer Erkenntnis." In J. Winkelmann (ed.), *Gesammelte Aufsätze zur Wissenschaftslehre*. Tübingen: Mohr.
- Wilson, E.O. (1975). *Sociobiology*. Cambridge MA: Belknap Press.
- Williams, B. (1985). *Ethics and the Limits of Philosophy*. London: Fontana Press.

## CHEMICAL TESTING

# U.S. Chemical Program: Purpose, Challenges, and Evolution

Odelia Funke

Environmental Protection Agency, USA

**Abstract.** This article explores long-term issues and problems that have seriously undermined the U.S. Chemical Testing Program established by the Environmental Protection Agency (EPA) under the Toxic Substances Control Act. This program is meant to gather information needed to protect human health and the environment from damaging exposure to toxic chemicals. Despite seemingly broad and impressive authority under the statute, there are a number of inherent difficulties, as well as substantial political constraints, that impede comprehensive oversight of chemicals in U.S. commerce. The article discusses several approaches that EPA has adopted to overcome statutory and political limitations and increase chemical testing information. The most recent and promising of these efforts has involved international negotiations to harmonize testing approaches with OECD nations and to cooperate on an agenda that will better share the testing burden on an international level.

---

**Odelia Funke**, a charter member of APLS, has a Ph.D. in political theory from the University of Virginia. She is a manager at the Environmental Protection Agency (EPA), responsible for developing EPA policy for public access, developing measures for public feedback, and identifying methods to improve access. She has over twenty years of experience with EPA, including positions as senior analyst, special assistant, and manager, and an assignment as a visiting Senior Fellow at the Army Environmental Policy Institute. Before joining EPA, she taught as an assistant professor at the University of Missouri. She has continued to teach as an adjunct professor at several local universities, and has lectured at other universities. She continues to both write and peer review articles and book chapters on environmental policy and political theory. The views expressed here are the responsibility of the author and do not represent official EPA policy. Correspondence should be addressed to 5308 Reno Road NW, Washington, DC 20015, USA (e-mail: funke.odelia@epa.gov).

Chemical testing has rarely been a headline issue, either domestically or internationally. But among those responsible for controlling risks from chemicals in commerce, unanswered questions about the toxicity of these chemicals have been an ongoing concern. Agencies charged with protecting public health and the environment are often without sufficient information to judge the safety of chemicals, and they have even less information about subtle long-term effects and synergistic effects. Periodically an episode brings public attention to this issue. For example, great concern, and new legislation,<sup>1</sup> followed the release of deadly chemicals in Bhopal, India.

Despite negative publicity over the Bhopal tragedy, the chemical industry's share of foreign investments in developing countries increased dramatically, from 18 percent in 1990 to 31 percent in 1996. The manufacture and sale of chemicals is a huge international business, which has raised concerns in some circles (French, 1998:27). The formidable size of the industry in the United States can be seen in the existence of about 200,000 chemical facilities by 1995 (INFORM, 1995:4). The potential for effects from chemicals in the environment has therefore grown tremendously, but the data available for risk assessment are comparatively sparse. In 1998, there was a flurry of media interest, and open scientific debate, regarding popularly published research on unexplained reproductive abnormalities and statistical changes in fertility in industrialized nations (Colburn, Dumanoski, and Myers, 1996). Researchers are drawing attention to evidence of disrupted hormone functions as the cause of abnormalities in humans, and dramatic deformities in some animal species, such as frogs. Many suspect that industrial chemicals are the source of these hormone malfunctions. This issue put the adequacy of chemical testing in the spotlight, not only in the United States, but in other nations. In response, government agencies in the U.S. and Europe have initiated inquiries into the causes of endocrine disruption, and have outlined a host of chemical tests required to address the issue. In this context of increased potential risk and growing public attention, efforts to fashion an international approach to chemical test-

ing could have very important implications for chemicals in international commerce.

The EPA has responsibility, under the Toxic Substances Control Act (TSCA), for protecting human health and the environment from unreasonable risk from chemicals in commerce. TSCA establishes oversight for both new chemicals (those not already in U.S. commerce) and existing chemicals (those already in commerce as defined by inclusion on the TSCA Inventory). To identify unreasonable risks, TSCA gives broad authority to both collect and require chemical testing. Under this statute, EPA gathers health and safety as well as exposure information on chemicals:

It is the policy of the United States that adequate data be developed with respect to the effect of chemical substances and mixtures on health and the environment and that development of such data be the responsibility of those who manufacture and those who process such chemicals and mixtures. (TSCA 2(b)(1)/§2601)

EPA can control, even ban, chemicals in commerce based on findings of unreasonable risk. Its authority extends to the manufacture, processing, marketing, and distribution of chemical substances and mixtures. TSCA does not, however, cover all chemicals in commerce. TSCA specifically excludes pesticides, tobacco, foods, food additives, drugs, cosmetics, and nuclear materials (TSCA 3(2)(B)/§2602), which are regulated under other statutes.

A second gap in comprehensive oversight is the Section 9 provision that EPA should refer action to other agencies or EPA offices if they have appropriate authority. Further, the seemingly broad authority to control industrial chemicals under TSCA is in practice quite constrained. The findings that must support any regulation place severe limitations upon EPA's authority. A final set of limitations arises from the highly politicized context in which EPA operates (U.S. Congress, 1989; Davies, 1990:55; Roe et al., 1997; Funke, 1997:173-200; Ginsburg, 1986; INFORM, 1995; Rosenbaum, 1994:131-32; Rosenberg and Wheeler, 1993).

The chemical industry is very well organized for resisting costly EPA controls. EPA's ability to exert regulatory control is also constrained by the oversight power of the Office of Management and Budget (OMB) put into place in the early 1980s.<sup>2</sup> Under the rubric of cost-benefit analysis, OMB has exercised this power to slow as well as constrain regulation by forcing upon EPA expensive and time-consuming analytic requirements for meeting the already difficult-to-satisfy statutory findings. Demonstrating that benefits clearly exceed costs, and that reporting requirements are not burdensome, is difficult—particularly as many benefits of regulation are qualitative. In this atmosphere, actual regulation of chemicals in commerce has been made nearly unworkable.

This article considers issues surrounding the safety of chemicals in commerce by looking at the framework for screening/assessing these chemicals: What are the limitations of the chemical testing program under TSCA? Why does EPA have so little data on widely distributed and used chemicals? What are the prospects for overcoming obstacles to create a better—which is to say more complete—chemical assessment program? The article looks at the evolution of EPA's chemical testing program as a key to EPA's ability to oversee the safety of industrial chemicals. It considers how this authority has been exercised since TSCA's 1976 enactment. It also examines differences in the current handling of existing as compared to new chemicals, shortcomings of the legal framework (including equity concerns), and creative attempts to address the problems associated with implementing the chemical testing program. The discussion focuses particular attention on gathering greater domestic support and moving toward an international testing regime to address inherent weaknesses in the current framework. That is, during the 1990s, EPA used publicity and negotiations to promote an international testing program to enhance its ability to protect human health and the environment domestically without creating a disproportionate burden for U.S. chemical companies, either domestically or in the international marketplace.

The move toward an international effort is part of a decade-long evolution in the testing program, which had already started a significant realignment away from command-and-control toward voluntary or negotiated testing agreements. Several factors seem particularly important in this evolution: (1) frustration with the inherent difficulties of meeting TSCA findings and OMB requirements, and the resulting inefficiency and ineffectiveness of regulation; (2) the spread of the right-to-know (RTK) ethos, which leverages the power of public information about pollution to create pressure for action; (3) the fact that the program responsible for TSCA also administered the successful 1986 Emergency Planning and Community Right-to-Know Act RTK program; (4) the pursuit of voluntary programs as a way to address environmental problems; and (5) the growth of the global economy, emphasizing the cross-boundary impact of pollution and the need for international cooperation to mitigate costs as well as risks.

## EPA Chemical Testing Program

Congress recognized the tremendous financial and societal benefits realized through the development and use of industrial chemicals. On the other hand, because these chemicals are widely manufactured, distributed, and used, Congress understood the need for mechanisms to identify and limit risks to public health and the environment. The difficult practical and political question was, and remains, how to balance the benefits and the risks to maximize social gain. When the profitability of huge and powerful enterprises

such as the chemical industry are at stake, such balances are forged in a very heated debate, and compromises are inevitable. TSCA was the result of such a process, and its compromises created practical difficulties for implementing the balance sought under the statute. The shortcomings of TSCA implementation have been the target of periodic reviews by the General Accounting Office (GAO, 1991; Guerrero, 1994; GAO, 1994). But Congress has not seen fit to fix the statutory problems with which EPA struggles.

Neither TSCA nor EPA has established any automatic or minimal testing requirements for chemicals. Many chemicals in commerce have little or no testing available for EPA, or the public, to conduct even basic screening of their toxicity. While TSCA gives the EPA broad authority to collect existing information, requiring the generation of new data requires EPA to make findings that demonstrate a need for such data. Chemical testing is quite expensive, and EPA weighs costs seriously in deciding whether to require *de novo* chemical testing. Making these findings for any chemical requires a substantial effort, which can in turn be legally challenged. Thus, EPA has targeted its resources to address chemicals of known concern. Key sections of TSCA related to screening and testing are Sections 4, 5, and 8 (see Table 1).

For *new chemicals*—those not on EPA's inventory of existing chemicals and regulated under Section 5 of TSCA—the agency has set its review requirements taking into account that such reviews should not stifle innovation. The new chemicals review process, called the Premanufacture Notification or PMN Program, therefore does not

place demands of up-front testing prior to any guarantee of marketability. Aside from the cost, testing can also delay introduction of potentially beneficial, profitable chemicals in a highly competitive market. The industry has resisted any substantial testing requirements for new chemicals based not only on uncertain marketability but also on the rationale that chemicals already in commerce, and grandfathered when TSCA was first implemented, have not been similarly scrutinized and might in fact pose greater risks than new chemical substitutes. Some argue that the current review process for new chemicals, even though it requires submittal only of available testing, constitutes a disproportionate burden on new chemicals compared to existing chemicals, most of which have undergone no scrutiny.<sup>3</sup>

To oversee *existing chemicals* under TSCA, EPA has the authority to require, through rulemaking, that companies submit test information for chemicals, which in turn provides the basis for judging the relative safety of a given chemical's manufacture, distribution, and use. Control of any of these aspects of commerce requires a separate finding, analyzing risks and economic tradeoffs, and a lengthy regulatory process.

EPA has the authority to both collect already existing data and require new testing. The agency reviews existing data under (1) TSCA 8(c), which requires facilities to keep records of "significant adverse effects" that EPA can inspect on site; (2) TSCA 8(d), which requires companies to submit unpublished health and safety studies formally requested by EPA for specified chemicals; and (3) TSCA 8(e), which requires industry to submit information (i.e.,

**Table 1. TSCA's Key Chemical Information Clauses**

TSCA Section	Topic	Coverage	Requirement
Section 4 §2603	Testing Requirements	Manufacture, distribution, processing, use, or disposal that may present "unreasonable risk"	If data are insufficient, thru rule-making, EPA may require tests on health/env'l effects to aid risk determination; EPA must consider costs.
Section 5 §2604	Premanufacture Notification (PMN) Review	Manufacture of new chemicals +/- or manufacture or processing of significant new uses for chemicals	Submit EPA a Notice at least 90 days prior to manufacturing or processing a new chemical; submit any testing. EPA may: thru Section 4, require test data; limit entirely or in part the manufacture, processing, distribution, use, disposal to prevent "unreasonable risk".
Section 8(c) §2607	Records	Manufacturers, processors, distributors of chemical w/ known or reported info of significant adverse effects	EPA may define "significant adverse reactions". Manufacturers, processors, distributors must keep records of known or alleged reactions. EPA may inspect records.
Section 8(d) §2607	Health & Safety Studies	Manufacturers, processors, distributors of chemical for which there are known studies wanted by EPA	EPA may require submission of lists of health & safety studies manufacturers, processors, distributors have or know of. EPA may require copies of studies.
Section 8(e) §2607	Notice of Substantial Risk	Manufacturers, processors, distributors of chemical w/ info of substantial risk of injury	Immediately inform EPA of evidence of risk.

Source: TSCA, 1976

tests and incident reports) to EPA in cases where the company concludes that there is evidence of "substantial risk" from a chemical substance or mixture. Industry sometimes submits studies on a "for your information" basis, when they recognize that the results are of interest under TSCA, but they do not wish to declare that they are under any clear statutory or regulatory requirement to submit. Finally, EPA can require companies to conduct additional chemical testing under TSCA Section 4. This testing authority is very broad, covering a wide variety of possible endpoints and effects. In addition to incidents reported under 8(e), EPA has collected and catalogued over 100,000 chemical tests under these various TSCA authorities, covering over 8100 individual chemicals.

EPA does not know how many chemicals have actually been tested, however. The chemical industry conducts tests for TSCA-covered chemicals for its own purposes that are never submitted to, or reviewed by, the government. Industry conducts testing for both efficacy and toxicity. If EPA has not specifically requested information on a chemical, and industry has not seen evidence of "substantial risk," tests are not submitted.

The universe of testing concerns under TSCA is the over 70,000 chemicals in U.S. commerce that are on the TSCA Inventory. As EPA considers this large universe, it must set some priority for concern and investigation. EPA separates chemicals into three categories: low volume chemicals, which are defined as those which have less than 10,000 pounds-per-year produced or imported; polymers, which are judged to have low toxicity (because their large molecules make absorption into the body unlikely); and non-polymer chemicals produced or imported in quantities exceeding 10,000 pounds-per-year.<sup>4</sup> The low volume category accounts for about 25,000 chemicals on the inventory; polymers account for about 30,000 chemicals on the inventory, and commercial chemicals in quantities over 10,000 pounds constitute the other 15,000 or so chemicals. These 15,000 chemicals are the ones most likely to pose risks. Of these 15,000, there are 3,000 to 4,000 chemicals defined as high production volume (HPV) chemicals, which means they are produced or imported in quantities over 1,000,000 pounds per year. These HPV chemicals have the greatest potential for widespread exposure and are of greatest concern.

Since the costs of testing are significant, some argue that if experience with actual use has not demonstrated cause for concern, testing is not warranted. Others argue that lack of proven association is not persuasive, as cause-and-effect from chemicals may be hard to recognize and is even harder to prove, particularly if exposures are small and over a long period of time. Some worry about an inherent pressure not to test; companies do not want to develop information that could increase their liability. In any case, individual chemical companies understandably do not wish to take on such expenses unilaterally for chemicals

marketed by multiple companies, which would result in carrying the burden for their competitors. Companies want to guarantee burden sharing among manufacturers. Under TSCA Section 4, testing costs are often shared by domestic manufacturers for required testing. However, international chemical companies are considered to be "free riders" on U.S. testing. While countries in the European Community (EC) have more stringent requirements for new chemicals testing than does the United States, which has no minimal requirements, EC members do not have minimal requirements for testing existing chemicals.

To require new testing of existing chemicals, EPA has to make certain findings under Section 4,<sup>5</sup> and this process can be a long and even contentious undertaking. Over the first decade of the TSCA testing program, EPA found writing test rules to be a very slow and noncooperative process. The agency was subject to considerable congressional criticism for its lack of progress. EPA then explored other approaches, based more on negotiated requirements and schedules. Over the second decade of TSCA, and after legal challenges and consequent modifications, EPA developed a workable negotiation process. With Enforceable Consent Agreements and voluntary agreement approaches, the testing program has matured into a less contentious, more cost-effective, and quicker means of generating new testing.

Starting in about 1990, EPA initiated new efforts to develop its chemical testing program, including a Master Testing List. In 1996, EPA published the list (U.S. EPA, 1996a) to give notice to industry and the public of EPA's judgment regarding which chemicals, which information gaps, were most important to fill, and to invite public input. The agency also sought to encourage more voluntary testing by industry. The notice indicated an intention to take more aggressive action to collect data. Providing information to the public was another aspect of the drive to revitalize the program. The list contained more than 500 chemicals, as well as thirteen specific categories of chemicals.

In addition to setting more explicit priority for chemicals, the agency had started to define chemical categories to streamline the regulatory and testing processes and minimize costs. EPA's objective was to gather information more efficiently by collecting information on numerous chemicals for each categorical rule. Defining testing by categories can be very useful for assessing types of chemicals, chemical uses, and substitutes, such as paint strippers and respirable fibers. EPA has struggled with broad categories, such as endocrine disruptors and persistent bioaccumulative toxics (PBTs). These rules have proven quite difficult to develop, because of the complexity of reaching scientific agreement on category definitions and establishing test protocols across a category. Constructing categorical rules has proven to be a formidable undertaking, and progress has not been as rapid as originally anticipated.<sup>6</sup>

Under EPA's TSCA chemical testing program, the

agency has taken approximately 550 final testing actions; more than 50% of those test requirements have been developed in the mid-1990s. About 150 of the actions have been through traditional rulemaking; another fifty or so have been developed as Enforceable Consent Agreements, a few have been done through a Memorandum of Understanding, and the other approximately 300 are results of the international Screening Information Data Sets (SIDS) program (see below).

### International Collaboration

During the second decade of the testing program, EPA began to explore other strategies for significantly expanding available test data. The testing program considered building stronger links to the international community to design a broad-based, equitable approach to data collection. It was a natural evolution, given the increased awareness of international commerce, the concern about free riders on U.S. testing, and the increasingly porous nature of national borders in the economic and environmental realms. U.S. participation in the Organization for Economic Cooperation and Development (OECD) was a natural channel for international discussions. The OECD had taken some initial actions regarding chemicals in commerce. Thus, in 1981 the OECD passed a Council Act on the Mutual Acceptance of Data. The act recommended that countries require manufacturers/importers to supply certain test data, following OECD test guidelines, before marketing new chemicals. This premarketing data set (MPD) is known as the "base set." Although this preliminary work was done to build a framework within the broader group of OECD nations, no fully developed system for mutual acceptance of data came into being in the 1980s.

The European Community (EC) Commission<sup>7</sup> also took action in 1981 on new chemicals. It established a chemicals notification scheme in accord with EC Directive 79/831/EEC. This directive amended the existing general EC Directive on classification, packaging, and labelling of dangerous substances (Directive 67/548/EEC), which covers toxic chemicals. Under the amendment, manufacturers or importers must submit standardized data for new chemicals before they go on the market. The data set and the guidelines for performing the testing were both roughly equivalent to the OECD MPD and guidelines. So, although the OECD did not implement the scheme for the mutual acceptance of data, the EC nations did (U.S. EPA, 1994).

The EC directive applies to its twelve member states. The EC central administration coordinates among the member countries for the notification process for new chemicals. Among member states, standard notification dossiers, harmonized at the EC level, are required of manufacturers/importers who plan to market a new chemical in quantities greater than one ton per year. "New chemicals" are those that are not on the European Inventory of Existing Com-

mercial Chemical Substances. In 1981 there were already over 100,000 chemicals on the European Inventory.

When an EC member receives a notification, it prepares a summary dossier of the basic data set, which the Brussels EC Commission circulates to other member states. Either the commission or individual states may ask for changes to the dossier or additional information from the notifier. This is a decentralized system whereby the state that receives the data set decides on the acceptability of the notification dossier on behalf of the entire community. Twelve different national authorities act independently, with the commission coordinating decisions. This system makes it imperative to have fixed data requirements and decision criteria for classification and labelling (U.S. EPA, 1994:8). Further, EC countries not only receive a dossier of the base set tests for new chemicals, but they also receive supplemental testing if marketing exceeds certain triggers. When marketing levels reach ten tons per year (per notifier), state authorities may decide they want additional testing. When marketing levels reach 100 and 1000 tons per year, the notifier must provide additional supplemental testing. Test methods are standardized across the member states, so results, and related decisions, can be shared.

As a followup to its 1981 act, and after eight years of experience with the EC notification scheme, in 1989 the OECD sponsored a workshop for its members (the U.S., Japan, Canada, Mexico, Korea and Australia in addition to the EC nations) on notification schemes for new chemicals. Most member countries at that time already had notification schemes requiring some kind of pre-marketing data set, though not necessarily conforming exactly to the testing regime recommended in the OECD Council Act.

The United States, as noted earlier, is an exception compared to EC countries. Both the European Community and the United States define "new" chemicals as those not already on a domestic inventory, which is the comprehensive listing of chemicals in commerce. One difference is that the U.S. (TSCA) notification scheme for new chemicals requires that they be reviewed prior to manufacture, not marketing. A more important difference is that in the U.S. program for new chemicals only available data must be provided by the company. As noted earlier, TSCA has no positive requirement for submitting test data. More than half of the submittals to the U.S. PMN Program under TSCA have contained *no* toxicological data (Schutz, 1998:15).

In the late 1980s, EPA undertook an effort to negotiate a testing program within the OECD framework. The purpose was to share the costs and results of toxicological testing for chemicals. To establish a cooperative testing program, OECD nations had to agree that gathering a minimal set of screening data on existing chemicals is needed, and that it is a high priority. A cooperative effort to share the burden of producing mutually acceptable testing information brings significant benefits to participating states, including reducing burden on the regulated industry, increasing ef-

iciency in data collection and in risk assessment, avoiding duplication of testing efforts, saving animals (as well as money), and reducing barriers to trade. But burden-sharing requires more than an agreement to share testing responsibilities. Data must be transferrable and useful to all parties to the agreement. European states within the European Community already had agreed to work together for screening new chemicals. Within the OECD framework, major new players, whose cooperation and participation are very important for an international testing program, had to be integrated into the scheme. The focus differed in that the U.S. interest was in pushing an agenda for existing, not new, chemicals.

For those outside the EC to reach agreement, OECD had to address basic issues of alignment on acceptable testing protocols for toxicity testing; a [minimum] set of endpoints to test; priority chemicals for testing—which includes criteria for concern; which country is responsible for which chemicals; what would be reported, what made public; how to recognize and protect confidential business information (CBI); and whether to share raw test data (and, if so, when, how, and with whom).

In addition to resolving technical and scientific differences, harmonization must address concerns about trade secrets. Under both the EC and U.S. notification schemes, confidential business information is a critical concept. Information deemed confidential by manufacturers/importers, which may include test data, is not distributed to the public. It is only shared internally by those few individuals who are authorized for access to it. Extensive measures are typically taken to protect such information from disclosure. This need for secrecy further complicates any attempt to gather and share toxicity information about chemicals.<sup>8</sup> Overall, with the exception of CBI submitted from industry, information is generally available in the United States. For example, the over 100,000 industry-sponsored studies on existing chemicals submitted under TSCA are available to the public. Tests are not generally so easily available in the EC process.

OECD nations agreed on a program (known as Screening Information Data Set, or SIDS) to set an agenda for international testing (OECD Secretariat, 1996). The SIDS program was to produce a base set of testing information on high production volume (HPV) chemicals in international commerce that were not well characterized. High production chemicals are defined as those imported in excess of 10,000 metric tons-per-year in any one member country, and in excess of 1,000 metric tons-per-year in two or more countries. This base set was adopted from the base set defined in 1981, which includes tests for six endpoints: acute toxicity, repeated dose toxicity, reproductive toxicity, mutagenicity, eco-toxicity, and environmental fate. The purpose of SIDS data is to provide enough information to screen chemicals for potential risk and to set priority for additional testing or for risk management actions. The ba-

sic tests should answer screening questions. For example, environmental fate tests should provide information on whether a chemical will degrade quickly in the environment and how it will disperse.

The initial priority list was assembled by consulting member states for candidate chemicals and setting testing priority by focusing primarily on those chemicals for which there was little or no information. The resulting “working list” contained 648 chemicals in 1992, from which nations could select chemicals for sponsorship. A sponsor country collects available information on a chemical and judges whether additional testing is needed in order to complete the base set. Since 1993, selections for sponsorship may be from an expanded HPV list, rather than the “working list.”

Data sharing in the OECD requires harmonized guidelines for testing. In the early 1990s, an effort to harmonize requirements got underway in conjunction with the SIDS program. Harmonization is not just a problem between OECD member states. It is a problem within the United States, and even within EPA. EPA has been working to harmonize both internally and internationally. The EPA testing programs (for chemicals and pesticides) have published about 215 guidelines in four areas—human health, ecotoxicity, environmental fate, and physical chemistry. The OECD has published 55 in these same four areas. In 1998, EPA and OECD had harmonized 30 health effects guidelines and six ecotoxicity test guidelines. This is an ongoing task, with additional guidelines to be harmonized over the coming years (Cimino, 1998:12).

Under the SIDS program, and unlike the U.S. testing program for existing chemicals, individual tests are not the focus. Each country collects effects and exposure information and coordinates studies on a given chemical, then uses these materials to develop a summary report, referencing but not actually providing the underlying tests. Industry is asked to provide data it has developed as a foundation for the dossier. In the United States, industry is responsible for providing the initial analysis. A full SIDS dossier on a chemical leads to an initial assessment document, which presents conclusions about potential risks and recommendations for additional work to fill gaps as needed. There is extensive peer review at two stages. Documents undergo review and revision in the sponsoring nation and are then submitted to the OECD for discussion, and possible revisions at that level, including addition of any new testing data. An initial assessment report is prepared and reviewed by OECD, which decides whether the chemical has low priority or needs further work. When the dossier and initial assessment reports are accepted, they are made available worldwide through UN databases, such as the International Registry of Potentially Toxic Chemicals (IRPTC).

Since 1989, the SIDS program has undertaken an effort to develop this shared information for approximately six hundred HPV chemicals. U.S. industry has agreed to take responsibility for about 25 percent of the almost 300

chemicals now active in the program. Other participants, including Japan, have undertaken the other 75 percent of the work. Progress has been quite slow. By 2000, about 166 assessments had completed OECD review (about 30 from the United States). Approximately 45 more were underway and expected during 2000, of which 15 or 16 would be from the United States. The EPA estimated that there might be completed assessments for an additional 75 chemicals, about one third from the United States.

In late 1998, the chemical industry announced a new initiative for global testing (ICCA, 1998), in keeping with the industry's international Responsible Care program. The International Council of Chemical Associations (ICCA) represents chemical manufacturers' trade associations from North and South America, Europe, Japan, Australia, and South Africa. Their announcement emphasized principles of product stewardship and continuous improvement, including systematic data gathering and hazard assessment of HPV chemicals within a reasonable time, global burdensharing, cooperation between chemical companies and public authorities, and working with stakeholders.

The ICCA is leading a voluntary effort to speed up the process of data collection and hazard assessment for existing chemicals. To this end, it intends to establish a priority list of approximately 1000 HPV chemicals of high concern (i.e., high dispersal or potential for extensive human exposure) in consultation with OECD, and encourage member companies to complete SIDS data packages for these chemicals by the end of 2004. These tests, like the OECD SIDS profiles, would provide a baseline or framework to help in chemical management and in targeting risk assessment needs for selected chemicals. The ICCA has been working with EPA and OECD to reach agreement among government ministers and industry CEOs on chemical testing, including the scope of the effort and how to make the results public. In addition, the American, Japanese, and European industries are committing to increase their investments in a long-range research initiative, to provide financial support for independent research for the purpose of improving understanding of the mechanisms through which chemicals affect people and the environment. If ICCA plans come to fruition, it will significantly increase the number and speed of chemical assessments.

International coordination has obvious benefits. Sharing data can dramatically increase the number of tests and chemicals available for review. The United States has been particularly anxious to establish greater confidence that HPV chemicals do not pose substantial risks, or to gather the information necessary to identify and control those risks. But the international program has implications beyond increasing the basic information about HPVs. It marks a movement toward a harmonized international testing regime that could affect all chemical management.

## Harmonizing Techniques Internationally

Lacking sufficient test data on new substances, EPA has increasingly relied on a set of techniques developed by its scientific staff to judge the hazard/risk potential of new chemicals by analogy to known chemicals. These methods are predictive tools that rely on comparisons of chemical structures to estimate the properties/activity of a new chemical. The techniques are known as Structure Activity Relationships (SAR). EPA first formed a team to use SAR techniques for new chemicals in 1979. Since then, the team has evaluated over 30,000 chemicals. The flow as of 1999 was about 2500 *new* chemicals per year (Jones, 1996:14).

As SAR matured, it gained international attention and recognition. Other nations were interested in its accuracy, given harmonization attempts, and in its potential for significant test savings if they could establish confidence in its adequacy as compared to testing. Therefore, another aspect of the cooperative effort under OECD was to compare approaches and results of new chemical program reviews in the EC and the United States. As noted earlier, the European Union generally has tighter requirements for new chemicals than does the United States. The 1989 OECD workshop recommended that the results of the SAR methodology be compared to the eight years of data collected under the EC scheme. The purpose was to evaluate the predictive power of SAR for new chemical notifications. Considering the strengths and weaknesses of SAR as compared to actual test data would show possible opportunities for reduced testing costs for new chemicals in the EC. Conversely, areas where the predictive capabilities of SAR techniques compared to actual test data appear weak, suggest the need to rely less on SAR and establish minimum data requirements in the United States in order to have a sufficient basis for judging risk potential for new chemicals. In either case, the study stimulated greater collaboration between the EC and the United States regarding chemical testing.

Comparing SAR results to test data results proved quite complicated, as the two approaches measure different things, or use classifications that are not entirely commensurate. One major difference is that the EC requires testing of the actual substance to be put into commerce, to which humans and the environment will be exposed. This means that any impurities or additives are included in the testing results. SAR, however, predicts the properties/activities of the pure chemical substance, not taking impurities or additives into account, unless the submitter is aware of them and reports the information to EPA. Second, SAR estimates are not as precise as the data the EC states use in their classification schemes. Third, greater precision has been needed in the EC approach, because mutually accepted categories and decisions across national borders demand clear definitions and criteria (U.S. EPA, 1994:7-14).

For new chemicals, greater harmonization could lead to savings for the industry. If more nations, particularly major trade nations, would accept a similar set of data and information for new chemicals, testing and/or administrative costs might be significantly reduced. In the chemicals marketplace, saving months in multiple submittal processes could provide an important advantage. In the United States, both government and industry are interested in greater harmonization of testing regimes for existing chemicals. Through such harmonization, the United States has sought to achieve greater knowledge of chemicals of concern while at the same time equalizing the testing burden among nations. Additional chemical data can in turn help scientists further develop the data underlying SAR techniques. It is unclear whether the EC will move toward less required testing data for new chemicals, substituting SAR in its areas of demonstrated strength and reliability. It is also unclear whether the United States will move to constitute data requirements for new chemicals in areas where SAR techniques are less reliable. Whereas the EC might reduce its burden based on the study, in the United States the adjustments based on the joint study would increase the burden. Since the new chemicals program is generally thought to be quite successful, there is little incentive to increase the burden. This might change if a significant problem were to arise from a new chemical. Or, if the minimum data set is a requirement of international trade, opposition to up-front testing for new chemicals in the United States might not be strong in companies with large international markets, especially if it is not required prior to proving it in the market. If additional requirements were part of international harmonization, the clear economic advantages of harmonization for industry might outweigh the costs.

The future of SAR might have implications for existing chemicals as well. EPA has the goal of enlarging its overall data collection and its ability to assess chemicals with both better data for many individual chemicals and also with a richer set of data to identify analogues for conducting SAR. U.S. industry looks for a potentially lower burden internationally, and for a burden equalization with OECD-based industries. The U.S. government, led by EPA, can perhaps support U.S. industry in this goal through a two-pronged approach: enriching its SAR predictive capabilities, thus minimizing the need for new testing, on the one hand, and harmonizing criteria and testing requirements/guidelines internationally on the other hand, so companies in other nations share responsibility for testing. All would save the expense of overlapping tests and the years it takes to conduct them.

### Increased Attention to Chemical Testing Issues

While cooperation on chemical testing has continued to grow within the OECD since 1989, progress has been slow. In the interim, other developments are generating public

interest and thereby increasing visibility and pressure on the issue of chemical safety. Publicity about genetically engineered products, particularly in food, has raised the public's awareness of the need to insure the safety of chemicals in consumer products. Another element is the emerging evidence of chemicals in the environment that mimic or aggravate hormones. Malfunctions in human and animal endocrine systems (causing sexual, pituitary, or thyroid abnormalities, for example) are being linked to chemical exposures. The push for more test data and greater public dissemination of this information has been aided by media attention to issues of developmental deformities and potential intergenerational effects from endocrine disruptors.

These concerns have triggered a reaction among European countries as well as in the United States. In November 1998, European researchers met in Great Britain to discuss endocrine activity discovered in some pesticides, herbicides, and fungicides. The evidence raised the question of whether laboratory results to date warrant a ban on any chemical (*P&TCN*, 1998d: 3, 7-9). Current research is insufficient to draw clear conclusions about suspected chemicals, and therefore governments lack the information needed to identify the causes and set safe limits. Some judge that a ban is the only rational path when there are links to reproductive disorders, immune dysfunction, and other severe impairments.

In the Fall of 1998, the European Parliament voted on the phaseout and subsequent ban of endocrine disruptors in the European Union. The results were remarkable. The European Parliament voted by an overwhelming majority to phase out hormone disrupting synthetic chemicals from the European market. Only four of 501 members opposed the phaseout. The Europeans have yet to establish an inventory of chemicals that interfere with the human endocrine system, however. This job was referred to the European Commission, which will develop a strategy to deal with endocrine disruptors. The Parliament wants products with synthetic chemicals to be labelled with an indication of how risky they are. The burden of safety would in this way fall increasingly on manufacturers (*C&IM*, 1998:868).

An EU chemical industry group has set up a group to fund up to seven million dollars in research on endocrine disruptors (*P&TCN*, 1998c:6). Seven million dollars sounds like just a beginning compared to thinking in the United States. The Endocrine Disruption Screening and Testing Advisory Committee (EDSTAC) created by EPA met for months before delivering a final report in September 1998. The report recommended a screening and testing program to identify chemicals with a potential to disrupt human or wildlife hormone functions. Testing needs are great. One scientist commented that the testing goals would cost \$40 to \$60 million in the first year for validation and standardization work (*P&TCN*, 1998a:3, 21). Globally, the program was funded at \$20 million by late 1998 (*P&TCN*, 1998b:4). Several times in 1998, an OECD working group

on Endocrine Disruptor Testing Assessment met to discuss validation of screening and testing protocols. Reaching definitional agreement on categories and test methodologies is difficult. Both European and U.S. sources are pursuing the harmonization of international test guidelines and cooperative research efforts for these chemicals.

Another important element of EDSTAC's recommendation to EPA was to set up a tracking and communication system for quick public access to check a chemical's status. This recommendation is in keeping with a general trend toward wider dissemination of environmental information. The idea of a public "right-to-know" about chemicals in commerce has been gaining support during the past decade. As noted earlier, the accidental chemical release in Bhopal engendered a movement supporting public disclosure in the United States, which has continued to grow. Support for right-to-know in other nations has not been as strong, but has also been growing in the past few years. In June, 1998, the Fourth Ministerial Conference of the UN's Economic Commission for Europe, Committee on Environmental Policy, issued a "Convention on Access to Information, Public Participation in Decision-Making and Access to Justice on Environmental Matters." This document includes a recognition of the right of each individual "to live in an environment adequate to his or her health and well being" and the duty to protect and improve the environment. Further, it acknowledges that this right and duty require access to information and to justice in environmental matters (UN Economic Commission for Europe: Article 4).

To raise public attention and spur government action in the United States, the Environmental Defense Fund (EDF) launched a major effort to focus attention on chemicals in the environment, including publishing an Internet "scorecard" highlighting chemical information and lack of information. EDF also published a report, *Toxic Ignorance*, that asserted: "for most of the important chemicals in American commerce, the simplest safety facts still cannot be found... even the most basic toxicity testing results cannot be found in the public record for nearly 75% of the top-volume chemicals in commercial use" (Roe et al., 1997:7).

A victory for U.S. champions of a more aggressive testing program occurred in April 1998 (the eve of Earth Day) when Vice President Al Gore gave a speech formally announcing a Chemical Right-to-Know (CRTK) Initiative that would establish an HPV chemical testing program and a children's health testing program for the United States. EPA, prodded by EDF's action, is challenging the chemical industry to provide health and ecological testing data for almost 3,000 of the most widely-used chemicals in the next several years. Under the same initiative, EPA is proposing to require new, lower reporting thresholds for releases of persistent bioaccumulators (PBTs) in the Toxics Release Inventory (a RTK program under EPCRA; see Note 1). The third element of the CRTK Program is a test rule to re-

quire additional testing of the HPV chemicals of particular concern for children.

This challenge, announced by the Vice President, was a direct result of analysis EPA conducted that looked at the availability of basic screening information, as defined in the OECD SIDS program, for chemicals produced or imported at more than 1,000,000 pounds per year. EPA analysis demonstrated that, of the approximately 3,000 HPVs in the United States, a full set of SIDS testing (i.e., the basic data set covering six endpoints) was available for only seven percent of the chemicals, and that 43 percent have *no* available data on SIDS endpoints. Of chemicals used in consumer products, many (though far less than half) appear to have basic testing data. Of the chemicals already found to be toxic and regulated by EPA in the Toxics Release Inventory Program, which receive relatively more attention, many do not have all six of the basic tests available. The lack of adequate test data is also notable in that 148 of the 830 companies EPA has identified as making HPV chemicals in the United States, have no SIDS data available for any of their products (Auer, 1998). In October 1998, EPA issued its challenge, based on the Vice President's announcement. The agency also established a website to provide an ongoing source of information about the progress of the program ([www.epa.gov/opptintr/chemrtk](http://www.epa.gov/opptintr/chemrtk)).

This constitutes a call for a massive testing program, with substantial cost to industry. Underlying this call is the realization that government cannot provide minimal safeguards or assurances about chemicals widespread in commerce and the environment if it lacks even basic screening information about them. A second theme is that the public has a right to basic health and safety information, to know about potential risks from these chemicals. Industry realizes that it has to acknowledge possible concerns about the huge volumes of chemicals in commerce, and related potential human and environmental exposures. Manufacturers associations, which represent major producers and importers, as well as individual chemical companies, have been discussing how they might respond to this massive challenge. In January 1999, in the midst of the discussion about how to implement the CRTK Program, the Chemical Manufacturers Association announced that its members will pay \$67 million to fund research on health and environmental effects. Manufacturers also approved a \$1.2 billion initiative for essential research in product testing and establishing health and environmental safeguards. They expect funding to grow to \$25 million per year within five years (*DER*, 1999: 1, AA1-2, E1-4).

### Progress Toward Cooperative Chemical Testing

EPA has found that cooperative efforts will yield larger, faster results in generating testing than is possible through using regulatory authority. The agency held discussions with representatives of the chemical industry and the en-

vironmental interest community, particularly EDF, about a possible CRTK Initiative prior to launching it.

One concern expressed by industry was that the United States should promote burden-sharing among producers for this formidable, and expensive, effort. In so far as the initiative remains a voluntary effort, public and industry pressure will be the main tool for minimizing "free riders." EPA does retain its regulatory "club" to encourage companies to accept the more flexible voluntary arrangements, and the agency has announced its intention to write test rules under TSCA Section 4 for chemicals not voluntarily tested. But the principle of burden sharing extends to international markets, where rulemaking cannot be employed to enforce equity. Thus, there is significant interest in gaining OECD support for, and assistance with, the goal of gathering SIDS data for HPV chemicals on an accelerated timetable. EPA has been discussing this issue within OECD and determining how to bring the issue to the council.

Between the Vice President's announcement in April 1998 and EPA's announcement in October, much effort went to defining the shape of the program, checking the accuracy of EPA data to identify HPV chemicals and the companies who manufacture them, and working to build a cooperative foundation for the initiative domestically. The U.S. Government has not formally brought the initiative to the OECD, though informal discussions have occurred. To pursue an international agreement, EPA will most likely use the normal OECD environmental channels, though it might seek to elevate the issue so that the U.S. Government would go directly to embassies for support in promoting the program within the OECD.

Several sets of issues related to testing have been particularly knotty and potentially important for a right-to-know program. One issue is whether, and how, to accommodate companies whose voluntary cooperation can only be gained if there are provisions for confidentiality claims. EPA's growing commitment to public disclosure has to be balanced with legitimate confidentiality concerns. Under TSCA, companies are allowed broad confidentiality claims, though these claims are restricted when it comes to health and safety information. It is difficult to define a proper balance; risks are to some extent a matter of judgment. If EPA wants a voluntary program, it might have to curtail the public access aspects of the program. To the extent that EPA accepts confidentiality concerns, it will support limiting the availability of some data to the public. Such limitations fly in the face of citizen right-to-know principles the agency has been promoting, and which provide political support for the effort.

Companies are allowed to claim confidentiality for the identity of their new chemicals. This means that the chemical name/identity is not on the publicly available TSCA Inventory (there is a parallel confidential inventory), and EPA cannot disclose the name. It is not clear how—or whether—EPA will include the confidential HPV chemi-

cals in the voluntary initiative, when the chemicals cannot be named. Yet, if they are produced in high volume, they should not be excluded, for these chemicals pose the same kinds of potential risks. Handling these confidential chemicals separately, following procedures for maintaining confidentiality and hence no public access, is one alternative. Another confidentiality issue for the initiative that might reduce either disclosure or voluntary participation is the claim some companies make that even divulging that they manufacture certain chemicals might give their competitors clues about important business secrets. Skeptics note that the real concern might be to avoid public accountability. It is very difficult to demonstrate whether disclosing this kind of information would be harmful to a company's trade secrets or marketing strategy. But if companies believe that disclosing that they produce a chemical would create a potential competitive disadvantage, they are unlikely to voluntarily join the program if that is a mandatory component. One possibility is that companies could use cooperative consortia arrangements through their trade associations to mask which company is testing which chemicals. Under such an arrangement, the consortia rather than individual companies would provide testing data to the EPA, or otherwise make it public. EPA would receive the needed data, and could acknowledge companies who participate without linking them to specific chemical tests.

A newer confidentiality claim to emerge is that terrorists might use chemical information to target attacks on chemical plants. This issue has recently been offered as a reason to block dissemination of plant-specific emergency plans in another environmental right-to-know program under the Clean Air Act. But since EPA is seeking toxicity data and not locational information, it is not clear how this issue relates to public disclosure within this program, unless the company has a single plant. These various confidentiality concerns must be resolved before some companies will be willing to submit data voluntarily under the initiative.

Another disclosure issue, not related to trade secrets, is whether there should be public release of toxicity tests, or just summaries of the tests. The argument here centers on "free rider" concerns. These tests represent a large investment, and some companies do not wish to provide free data to competitors and potential competitors. If EPA negotiates an international aspect of the program, this would only partially satisfy the concern about free riders. The free rider issue covers both domestic and international companies that do not volunteer for a fair share, and it cannot be easily addressed. Health and safety studies currently submitted for existing chemicals under TSCA authority are available to the public. Providing only summaries would be a departure from twenty years of TSCA practice. This voluntary program differs somewhat in that the number of chemicals, and thus the information at stake, is large and, perhaps more importantly, EPA wants industry to submit the information electronically. TSCA studies have not been avail-

able electronically, which is to say they are not available quickly and easily. EPA has not in the past been efficient in its handling of test studies, nor has there been significant public interest in these data. Having these data easily accessible might raise new concerns about free riders. But it is noteworthy that these tests, though expensive, would represent only a tiny fraction of the chemical industry's commercial trade.

If only summaries are provided, it is not clear whether the tests would be generally available upon request, or whether access would be restricted. As a practical matter, most people have not wanted to see actual studies, because the information is highly technical and copies must be ordered and purchased. The CRTK program will have greater public visibility, with attention from the public interest community. EPA plans to make the information quickly available to the public in electronic format. Industry concern with providing the full studies might reflect their desire to minimize any adverse impacts of making these data widely available. Misuse and misunderstanding are issues inherent in any right-to-know program, and industry has resisted such programs in the past. From EPA's perspective, misuse of data has not been the significant problem opponents predicted in existing right-to-know programs. EPA will seek to help minimize "free riders," but it has in the past tried to resist pressures to withhold data that are based on fear of public disclosure.

If EPA seeks to take this program into the international arena, parallel confidentiality and disclosure issues would undoubtedly arise. EPA must find an acceptable balance between holding to its right-to-know principles and reaching a cooperative agreement with industry on a voluntary program. If EPA can resolve these issues with U.S. industry, they might not be impediments in international negotiations.

A second set of issues raised by the CRTK Initiative involves data collection and management. Establishing a cooperative testing effort on this scale might be a catalyst to move the United States and EC countries to adopt similar data systems. The possibility of adopting similar data systems has been discussed for several years, but there has not been a sufficient motivation to overcome technical difficulties and organizational inertia. EPA is already tying its objectives in the initiative to the OECD-defined basic data set (SIDS). In Europe, SIDS data are shared in a common data format. If the OECD agrees to participate in the CRTK Initiative, industry and governments will have a strong motivation to facilitate data sharing internationally. EPA or industry might decide to adopt the European database software for CRTK information. Industry is anxious for the data to be seen as a voluntary industry effort; they propose to have summary information housed at a "neutral" location, accessible to government and the public. Adopting the European standards would provide this kind of platform, and industry might subsidize necessary

software modifications to adapt the European database to TSCA needs, and pay a third party data manager to support this approach. For EPA, adopting the European data system would require significant systems modifications, but having an international dataset upon which to draw for toxicity screening would be a substantial gain. EPA is considering adopting the European database, with certain modifications to meet TSCA needs, but no decisions have been made. Clearly, pressures for systems harmonization would increase if OECD nations agree to share the testing burden for the HPV chemicals.

Another thorny issue raised by the HPV challenge is the implication of large-scale animal testing raised by the Animal Legal Defense Fund (ALDF), People for the Ethical Treatment of Animals (PETA), and other animal rights advocates. Based on current methodologies that call for hazard and risk assessments, many animals would be needed for testing. Animal rights groups therefore attack the current initiative as an alarming program that would result in the suffering and death of many animals. These groups are calling for alternatives, including more careful review of existing studies to avoid duplication, and a thorough exploration of alternative test methods to take animal welfare into consideration. In early 1999, EPA organized a large internal document search and retrieval to respond to a broad Freedom of Information Act (FOIA) request for records on eleven broad categories involved in planning the CRTK Program (Nelson, 1999). Whether and how EPA and industry might modify program plans to respond to this opposition is not entirely clear. They might, for example, dedicate more effort to defining categories of chemicals in order to reduce the number of individual tests, or perhaps rely more heavily on SAR to minimize testing. But, in keeping with current policy, it is likely that animal testing will for the near future continue to be judged the most reliable means for gaining needed information about the safety of chemicals to which animals as well as humans are exposed.

## Prospects for the Future

What impact does party affiliation or ideology appear to have on international negotiations to harmonize chemical testing? Is the current U.S. move toward greater harmonization likely to fade or change with the election of a different party to the White House? U.S. administrations differ in their approach to international relations, and how they define the U.S. role in dealing with trade and commerce, as well as environmental, issues. Has the change in U.S. party leadership had any noticeable effect on the push for greater international cooperation and coordination?

The process, begun in earnest in the late 1980s, to foster greater coordination on chemical testing issues, does not appear to have been significantly affected by partisan politics. One sign of the nonpartisan nature of this effort is that EPA has led it, rather than the State Department. EPA has

focused primarily on the technical and scientific issues to be resolved, which naturally involve political and legislative differences among participating nations. But the effort has been carried on through a long series of discussions within OECD, not through embassies. The locus of the discussions has helped to insulate the issues from political changes. Promoting business internationally is a bipartisan goal, and burden sharing has been a strong motive in this effort. Saving money for U.S. business, particularly in the name of environmental protection and with no net loss in social benefit, is a position that either party can readily endorse.

Building more of an international approach to chemicals in commerce probably has more benefits for industry than disadvantages. Both testing and administrative costs could be diminished. Data sharing could save U.S. industry the costs of testing, because toxicity information would in some cases come from non-U.S. companies. If the OECD harmonizes standards, it might move toward accepting new chemical notifications across national boundaries, which would represent a significant savings for companies that currently do business in EC nations, and give them quicker access to international markets.

Harmonization of chemical testing can be seen as a way to collect more data and enhance government's ability to screen chemicals for potential risks. Greater government involvement is not typically an industry goal, but harmonization can be the way to share the information burden. In the United States, this should please liberals in the sense of having more information available for the purpose of protecting human health and the environment; the conservatives have emphasized the need to rely on risk assessment before taking any action, and should support data collection for this purpose. Thus, this effort has the efficacious character of being pro-business and pro-environment. Different administrations can emphasize different objectives.

Whether EPA will be successful in its attempt to collect information on thousands of chemicals over the next few years remains to be seen. The motivation for finding an efficient testing regime depends upon at least one significant assumption—namely, that there is continued domestic political pressure on industry to embrace the need for, or inevitability of, testing. This pressure might be sustained through several means—through the continued spread of the right-to-know ethos or the reaction to a chemical catastrophe, creating sufficient political support for EPA test rules, and/or through fear of liability from damage claims. If industry accepts that testing is a necessary cost of business, there is wisdom in expanding the effort to the international realm to spread the financial burden and increase lab capacity. But an international testing regime would also increase the difficulty of overseeing timetables and the chances of having the program bog down. The next year or two will be important in determining whether the 1998-1999 initiative continues apace, and particularly how

changes in the executive branch affect EPA policy. The next few years could be a defining phase in the future of international harmonization of chemical testing and screening activities.

## Notes

1. The Emergency Planning and Community Right-to-Know Act (EPCRA) of 1986 was a direct consequence of the Bhopal tragedy. It established a minimum requirement for emergency planning and public disclosure at industrial facilities, including the first major right-to-know program. The program requires manufacturers to estimate facility releases of specified chemicals to EPA in an annual report. These estimated releases, known collectively as the Toxics Release Inventory, are made publicly available; the reports do not, however, provide information about the toxicity of the listed chemicals.
2. Under both the Paperwork Reduction Act and Executive Orders first put in place early in the Reagan Administration (Executive Order 12291 and its successors), OMB reviews and clears draft regulations before agencies propose them for public comment in the Federal Register.
3. In 1989, existing chemicals accounted for 99.9% of production. Under TSCA PMN review, between 1979 (when TSCA monitoring began) and 1993, of the 21,100 new chemicals reviewed, only 4% were restricted; 5% were withdrawn, and 45% were introduced into commerce by 1995 (INFORM, 1995:76-77).
4. Some are not entirely comfortable with this triage. INFORM noted that most polymer-containing products "also contain large amounts of additives, some of which may migrate from the product and which are highly toxic." Further, polymers resulting from incomplete reactions might contain toxic monomers (INFORM, 1995:114). The triage goes back many years in the TSCA program, and had some critics from the beginning (for example, see GAO, 1985).
5. Testing may be required based on risk and/or exposure findings. EPA must find that (1) the chemical "may present an unreasonable risk of injury to health or the environment," and that exposure is probable, and/or (2) substantial quantities are produced/imported and enter the environment or there is significant human exposure, and (3) EPA needs more data to conduct a risk assessment, and (4) testing is necessary to develop the data.
6. Respirable fibers, for example, is a priority category for testing because EPA has virtually no information about health effects from these chemicals. There was no agreement, however, about how to test for chronic inhalation toxicity and carcinogenicity. Before EPA can write a test rule or negotiate a testing agreement or voluntary action, the agency has to know what tests it seeks. EPA gathered a panel of experts to recommend a test guideline; the recommended guideline must be further reviewed, modified, and approved before EPA can develop the testing requirements.
7. The commission refers to a body of seventeen EC commissioners, and their staff, who head directorates, including the environment, and are the source of legislation sent to the European Parliament for action. They also oversee implementation of the laws by member states.
8. The concern for CBI in the United States was very evident as TSCA was being debated. As Congress considered the legislation, the record shows that the committee recognized "that some information which the Administrator may obtain will be of tremendous competitive advantage to the person providing it. Accordingly, section 14 contains specific prohibitions against release of such information so that the competitive position of those supplying information will be protected" (see U.S. House of Representatives, 1976, cited in U.S. EPA, 1996b:23).

## References

- Auer, C. (1998). "Chemical Hazard Data Availability Study." *Chemicals in Our Community* (Fall). EPA 747-N-98-001.
- C&IM (1998). "Europe to Ban Hormone Mimics," *Chemical and Industry Magazine*. Issue 21 (November 2).
- Cimino, M.D. (1998). "OECD Test Guidelines in Health and Environmental Effects." *Chemicals in Our Community* (Fall). EPA 747-N-98-001.
- Colburn, T., D. Dumanoski, and J.P. Myers (1996). *Our Stolen Future*. New York: Dutton.
- DER (1999). *Daily Environment Report*, No. 18 (January 28). Washington, DC: BNA.
- Davies, J.C. (1990). "The United States: Experiment and Fragmentation." In N. Haigh and F. Irwin (eds.), *Integrated Pollution Control in Europe and North America*. Washington, DC: Conservation Foundation and Institute for European Environmental Policy.
- French, H.F. (1998). "Investing in the Future: Harnessing Private Capital Flows for Environmentally Sustainable Development," *Worldwatch Paper 139* (February).
- Funke, O. (1997). "Prospects for Integrated Environmental Policy." In L.K. Caldwell and R.V. Bartlett (eds.), *Environmental Policy*. Westport, CT: Quorum Books.
- General Accounting Office (1985). "Chemical Inventory: Environmental Protection Agency's Proposed Inventory Update." December. GAO/RCED-86-47FS.
- General Accounting Office (1991). "Toxic Substances: EPA's Chemical Testing Program Has Not Resolved Safety Concerns." June. GAO/RCED-91-136.
- General Accounting Office (1994). "Toxic Substances Control Act: Legislative Changes Could Make the Act More Effective." September. GAO/RCED-94-103.
- Ginsburg, R.A. (1986). "TSCA's Unfulfilled Mandate for Comprehensive Regulation of Toxic Substances—The Potential of TSCA §21 Petitions." *Environmental Law Reporter* (November): 16ELR10330-10337.
- Guerrero, P.F. (May 17, 1994). "Toxic Substances Control Act: EPA's Limited Progress in Regulating Toxic Chemicals." May 17. Testimony before the Subcommittee on Toxic Substances, Research and Development, Committee on Public Works. U.S. Senate, GAO/T-RCED-94-212.
- INFORM (1995). *Toxics Watch 1995*. New York: INFORM, Inc. Press.
- International Council of Chemical Associations (1998). "Major Global Chemical Industry Initiatives Launched in Prague." Press Release (October 12).
- Jones, B. (1996). "The OPPT Structure Activity Team." *Chemicals in the Environment* (Fall). EPA 749-R-96-001c.
- Nelson, L. (1999). ALDF Letter to EPA's FOIA Officer (dated January 12, 1998[sic]).
- OECD Secretariat (1996). *SIDS Manual*, Second Revision (May). Paris: OECD.
- P&TCN (1998a). "EDSTAC Delivers Final Report to EPA with Recommendations for Screening, Testing nearly 87,000 Chemicals." *Pesticide and Toxic Chemical News* (September 3).
- P&TCN (1998b). "European Chemical Industry Urges Global Approach to Endocrine Disruptor Research." *Pesticide and Toxic Chemical News* (November 19).
- P&TCN (1998c). "European Parliament to Call for EU Ban on Endocrine Disruptors." *Pesticide and Toxic Chemical News* (September 3).
- P&TCN (1998d). "Explosion in Toxicity Studies Leaves Key Questions about Potential Endocrine Disruptors Unanswered, Researchers Say." *Pesticide and Toxic Chemical News* (November 19).
- Roe, D., W. Pease, K. Florini, and E. Silbergeld (1997). *Toxic Ignorance: The Continuing Absence of Basic Health Testing for Top-Selling Chemicals in the United States*. Washington, DC: Environmental Defense Fund. Available at [www.environmentaldefense.org/pdf.cfm?ContentID=243&FileName=toxicignorance.pdf](http://www.environmentaldefense.org/pdf.cfm?ContentID=243&FileName=toxicignorance.pdf).
- Rosenbaum, W.A. (1994). "The Clenched Fist and the Open Hand: Into the 1990s at EPA" In N.J. Vig and M.E. Kraft (eds.), *Environmental Policy in the 1990s*, Second Edition. Washington, DC: Congressional Quarterly Press.
- Rosenberg, E. and J. Wheeler (1993). "Unreasonably at Risk." *The Environmental Forum* 10 (4): unpaginated reprint.
- Ruggerio, C. (Fall, 1989). "Referral of Toxic Chemical Regulation Under the Toxic Substances Control Act: EPA's Administrative Dumping Ground." *Boston College Environmental Affairs Law Review* 17 (1).
- Schutz, D. (1998). "PMN (Premanufacture Notice) Chemical Testing Issues." *Chemicals in Our Community* (Fall). EPA 747-N-98-001.
- Toxic Substances Control Act of 1976, 15 U.S.C.A. §§ 2601-2629. Referred to as TSCA.
- UN Economic Commission for Europe (1998). "Convention on Access to Information, Public Participation in Decision-Making and Access to Justice in Environmental Matters." June. ECE/CEP/43.
- U.S. Congress (1989). "Whatever Happened to the Toxic Substances Control Act?" Hearing before the House Environment, Energy, and Natural Resources Subcommittee of the Committee on Government Operations. October 3, 1988. Washington, DC: U.S. Government Printing Office.
- U.S. EPA, Office of Pollution Prevention and Toxics (1994). "U.S. EPA/EC Project on the Evaluation of the (Quantitative) Structure Activity Relationships." Final Report (March). EPA 743-R-94-001.
- U.S. EPA, Office of Pollution Prevention and Toxics (1996a). "1996 Master Testing List: Notice of Availability." *Federal Register* 61FR65936 (December 13).
- U.S. EPA (1996b). *Chemicals in the Environment*. Fall. 749-R-96-EPA 001c.
- U.S. House of Representatives (1976). Committee Report, *Legislative History of the Toxic Substances Control Act (1976)*. Chapter 3. H.R. 14032, 457.

## CULTURE

# Culture, Social Minds, and Governance in Evolution

Claude S. Phillips

Western Michigan University, USA

**Abstract.** In the past quarter century, the concept of culture has undergone change as evolutionary scientists have come to include social behavior in their purview. Evolutionary psychology is the newest field to concern itself with culture by claiming that most *specific* human behaviors are generated by minds specifically designed for these behaviors—and not from a general-purpose mind—as a result of adaptations made during the Pleistocene. Thus, mental behaviors are explained as having formed independently of cultural learning. In defending the concept, however, the leading proponents practically slough off culture as significant in human affairs. I argue that they have neglected the powerful explanatory statement of Darwin regarding at least one general-purpose adaptation of social animals, namely, the instinct for sociability, a position supported by recent neurological studies. Expanding the Darwinian concept, modern research shows that (1) the human brain was selected for sociability, which explains the origin and strength of culture, as well as its variability; (2) the development of complex culture in a pre-human primate initiated the two-and-one-half million-year evolution to modern humans; and (3) there are political contributions to cultural evolution that rest on the nature of groups (competitive and cooperative).

---

Claude S. Phillips is Emeritus Professor of Political Science at Western Michigan University, Kalamazoo, Michigan. He received his PhD from Duke University, where he was inducted into Phi Beta Kappa. His early research efforts were concerned with developing areas and involved a Fulbright grant to India and university grants to Nigeria. He was founding director of the Institute of International and Area Studies at Western Michigan University, and earned the Distinguished Service Award there. He has written two books and six original chapters in books on Africa, as well as a number of articles. In the 1970s, he transferred his interests to politics and the life sciences, and has authored a number of papers on cultural evolution. Correspondence should be addressed to 518 Phoenix Street, South Haven, MI 49090, USA (e-mail: csphil@btc-bci.com).

*Remember, boy, that behind all these men you have to do with, behind officers, and government, and people even, there is the Country Herself, your Country, and that you belong to Her as you belong to your own mother.*

—Edward Everett Hale, *The Man Without A Country*

When one notes all that has changed recently in regard to our understanding of culture, it is sobering to recall that only a little over forty years ago social scientists were making dogmatic statements about culture and society that today are challenged by most biologists and a growing number of social scientists. Consider the following by two scholars in a famous text designed to introduce students to the social sciences: “Needing society, we are nevertheless born asocial, if not antisocial” (Ross and Van Den Haag, 1957:11). They do concede that “biological endowments ... must not be underestimated,” and then proceed to underestimate them, as would be required if humans are really born “asocial.” Only three years later, C. Wright Mills declared, just as strongly: “Some thinkers, Spencer for example, have a central theme—Evolution—that we [social scientists] do not now find at all valuable; in fact, it has been said, with scholarly authority, that ‘Spencer is dead’” (Mills, 1960:13).

My purpose is not to demean our forefathers but to emphasize how far, considering our understanding of culture, the social sciences—or at least some social sciences, coupled with the biological sciences—have come in the last quarter century. In that time, a few social scientists, held aloof by most of their peers, have embraced evolutionary concepts as the dominant paradigm for their work, some of whom will be noted below. Parallel to this trend, a great many biologists (to the dismay and resentment of many social scientists) have incorporated culture into their paradigm. Here one need only note that the most dramatic move to bring biology and social sciences together came from the work of biologist E. O. Wilson, who coined the term *sociobiology* to cover both fields (1975). Shortly thereafter, biologist Richard Dawkins coined the term *memes* to do for culture what genes do for biology, changes in each being basic, re-

spectively, for cultural and biological evolution (Dawkins, 1976:205-15; see also Brodie, 1996). Lumsden and Wilson followed with the concept that culture and biology co-evolve (1981). Biologist William Hamilton contributed much to our understanding of cooperation in biological systems (Axelrod and Hamilton, 1984: chapter 5). Anthropologist Laura Betzig related differential reproduction to early despotisms (1986). Biologist Richard Alexander made a strong case for a biology of moral systems (1987). Biologist Bobbi Low forcefully defended the thesis that “conflicts of interest, if not open aggression, are universal among living things” (1993:13). Biologist David Wilson and philosopher Elliott Sober set forth the thesis that group selection exists for culture, just as they maintain that it does for biology (1994). And biologist Michael Ghiselin goes so far as to claim that biological species are individuals, with all that implies about group behavior (1997).

The purpose of this article is not to spell out the contributions of biologists to the fields formerly dominated by social scientists. Rather, the objective is to re-examine the concept of culture after the innovations of a quarter century. This will be done by examining and criticizing the treatment of culture by the recently emerged field of evolutionary psychology, whose champions have defined the human mind as an adaptation to specific challenges faced by pre-*Homo sapiens* in simple environments. They have earned much attention and deserved respect, and are probably justified in claiming, as one scholar’s book jacket does, that a “paradigm shift” is in the making (Wright, 1994). The most persistent scholars, however, have been those connected with *The Adapted Mind* (Barkow, Cosmides, and Tooby, eds., 1992), and their work constitutes the introduction to this article. The thesis here is that, in their enthusiasm to defend their new perspective, the evolutionary psychologists have failed to appreciate the importance of culture in human mental adaptations.

### Evolutionary Psychology’s “Adapted Mind”

The major theme of evolutionary psychology is that the human mind today, anywhere on earth, is the result of adaptations made during the Pleistocene by *Homo erectus* hunters and gatherers. These hominids, as they developed, faced countless challenges to which they responded piecemeal, including such things as identifying faces, learning languages, cooperating, deciphering motivations, dealing with non-kin, finding mates, getting and giving assistance, planning, signaling desires and intentions, foraging, and forming coalitions. Slowly adaptations were made, i.e., selection favored certain mental responses over others, and gradually, over many thousands of years, a “mental architecture” for coping with the particular challenge came to characterize the hominid mind (Tooby and Cosmides, 1992:5, 89).

When *Homo sapiens* appeared, hundreds, perhaps thousands, of these mental designs were already part of

what it meant to be human. Thus, modern humans possess what Tooby and Cosmides (1992:91) call “a single human metaculture,” a common adapted mind that allows them to communicate, learn their culture, adopt cultural practices of others, imitate others, and even understand the meaning of other people’s acts. They conclude that “humans have psychological adaptations that contain contentful structure specifically ‘about’ their mothers, ‘about’ their children, ‘about’ the sexual behavior of their mates, ‘about’ those identified by cues as kin, ‘about’ how much to care for a sick child, and so on, and these contents are not derived exclusively from either a short list of drives or from culturally variable, socially learned ‘values’” (1992:99-100).

Specifically, what Tooby and Cosmides are rejecting is the notion that culture is outside of individual human minds and can affect, condition, or impact human lives. Culture is inside the heads of individuals: “One might say that what mostly remains, once you have removed from the human world everything internal to individuals, is the air between them” (1992:47). Individual humans make up the parts of a social system, and as in any system, the social system operates on relationships “structured by the dynamic properties of its component parts” (1992:47). Inside each head is a computational mind that selects “from ‘out there’” and interacts with other such minds to create population dynamics (1992:47). In understanding human behavior, one need not look at the culture for causes; rather, all humans share a highly organized mental architecture that allows them to express “a variety of organized within-group similarities that are not caused by social learning or transmission” (1992:116; cf. Pinker, 1997:210).

One might conclude from the above that for Tooby and Cosmides, while there is metaculture, there cannot be culture in its traditional usage. In fact, it is clear that they would like to get rid of the word *culture* but are unable to find a substitute (1992:117). On the other hand, they claim that they are not abandoning the classic concept of culture, which they say are the ideas “or regulatory elements that reappear in chains from individual to individual” (1992:118). Nevertheless, their theory requires that “the traditional concept of culture must be completely rethought” (1992:115), although they also insist that their theory must not be mistaken “for a substantive discussion of what a new theory of culture ... would look like” (1992:114). In fact, “by themselves, psychological theories do not and cannot constitute theories of culture” (1992:115).

To start the rethinking about culture, they posit three aspects of culture: (1) metaculture, the universal mental architectures that interact with the structure of the social world; (2) evoked culture, the “similarities triggered by local circumstances” (Cosmides and Tooby, 1992:210); and (3) transmitted culture, that part of culture that is learned from other humans or earlier generations. (This third aspect, they say, is the restrictive and flawed definition of

culture that results from the Standard Social Science Model, which their theory is designed to overthrow.)

What then can they mean by the word culture? “We will use *culture*,” they say, “to refer to any mental, behavioral, or material commonalities shared across individuals, from those that are shared across the entire species down to the limiting case of those shared only by a dyad, regardless of why these commonalities exist” (Tooby and Cosmides, 1992:117). This definition, I believe, greatly weakens *culture* as a meaningful term for the social sciences.

The definition as given means that all Catholics in the world constitute a culture since they share a common mental characteristic. Men who court only obese women five feet tall presumably share a behavioral culture. All people who use a computer anywhere on earth share a material culture. Two humans who share an obsessive fear of houseflies share a mental culture. All minds that get pleasure out of watching a sunset share a mental culture. A hundred strangers flying together in an airplane share a material culture. The more traditional definition of a culture as a group of humans sharing in common certain ideas, behaviors, and technologies in a more or less circumscribed area also fits their definition. Where two or more humans are involved, it is difficult to imagine, from their usage, what is not culture.

While there are, they say, “phenomena and processes operating at the population level,” these must not be understood as having “autonomy, teleology, functionality, organism-like integration, intelligence, intentionality, emotions, need-responsiveness, ... [or] consciousness” (Tooby and Cosmides, 1992:47). Whatever culture is, apparently it does not do anything to individuals: it does not constrain them, it does not contribute to their survival, it does not cause anything. Rather, individuals in their adapted minds observe the social world, infer some of the ideas existing in the minds of others, and incorporate some of the ideas in their own mind, thus creating culture (see especially 1992:118). It is these claims, I believe, that cannot be supported by contemporary research.

Difficulties arise when Tooby and Cosmides assert that “mental states, such as behavioral intentions and emotions, cannot be directly observed,” but can only be inferred (1992:69, 90). They say “the term *mind* refers to an information-processing description of the functioning of an organism’s brain” (1992:65) which I take to mean that the term *mind* refers to an information-processing function of the brain. They would, however (if I read them correctly), reject a definition of culture as “an information-processing function of a group of humans” on the grounds that a group of humans is not real in the sense that individual brains are real. They could not, I think, accept Durham’s reasonable observation that “humans are possessed of *two* major information systems, one genetic, and one cultural” (Durham, 1991:9). In fact, in a truly startling statement, they say that rather than see humanity as divided into separate

cultures, “it is probably more accurate to think of humanity as a single interacting population tied together by sequences of reconstructive inferences than as a collection of discrete groups with separate bounded ‘cultures’” (Tooby and Cosmides, 1992:121). Presumably, then, only individuals exist, in material environments, all possessive of a common mind adapted for coping with challenges of the Pleistocene; cultures are simply vague concepts.

The editors of *The Adapted Mind*, furthermore, maintain that their theory entails “no Procrustean burden to show how the present consequences of behavior are functional in the modern context” (Barkow, Cosmides, and Tooby, 1992:626). To escape the charge of saying little that is useful about the contemporary world, Barkow maintains in his chapter that “there is little human that is really ‘evolutionarily unanticipated’” (Barkow, 1992:627). Barkow illustrates that point by asserting that social stratification is very recent in human existence and could not have been directly genetically selected for in the time since it appeared. How then could it have been evolutionarily anticipated? He thinks it was by three psychological traits that must have been selected for in earlier eras, namely, the pursuit of high social rank, nepotism, and the capacity for social exchange (Barkow, 1992:632). He then concludes that since psychological explanations are not yet clear for most of modern cultural behavior, we still need the social sciences to help find explanations for other evolutionarily anticipated behaviors, and asks all social scientists to jump in and help (1992:635).

Barkow, however, seems to have created and then solved by a “just so” story a false assumption, namely, that social stratification is recent. Primatologist Sarah Blaffer Hrdy has offered overwhelming evidence that non-human and pre-human primates evolved as competitive, hierarchical, rank organized, and unequal, and there is no reason to assume that the ancestors of *Homo sapiens* avoided these characteristics only to acquire them in recent times (Hrdy, 1981: chapters 5 and 6; see also de Waal and Lanting, 1997:72).

## Evaluation

My purpose is not to reject all of the impressive concepts of the evolutionary psychologists. I readily concede that many specific adaptations to a variety of challenges were probably made by our ancestors in the Pleistocene and have now become innate. Many of those adaptations probably explain how modern humans have coped with some of their present challenges, i.e., those challenges faced in the last ten thousand years. But Tooby and Cosmides do not claim that *every* single act of humans is possible only because a specific adaptation made during the Pleistocene now makes it doable. What they do claim is that “many of these mechanisms [evolved mental architectures] are functionally specialized to produce behavior that solves par-

ticular adaptive problems, such as mate selection, language acquisition, family relations, and cooperation” (1992:24). However, they then drop the modifier “many” and say that “general-purpose, content-free psychology” is not a viable model of human psychology (1992:49), although later they grant that some “general-purpose” mechanisms exist (1992:94), which they then basically ignore.

Dennett, who lauds the work of Tooby and Cosmides for showing that “such rationality as we human beings have is the product of the activities of a host of special-purpose gadgets designed by natural selection,” nevertheless asserts that it does not follow that these gadgets “cannot have been used, time and time again, to reinvent the wheel.” It still has to be shown, he adds, “that any particular adaptation is *not* a cultural product responding quite directly (and rationally) to quite recent conditions” (Dennett, 1995:490).

When Tooby and Cosmides illustrate the kinds of present-day conditions with which minds adapted in the Pleistocene can cope, these seem mainly to deal with language acquisition and interpersonal relations, particularly among kin. Their arguments in this respect are weighty. Supporting their position, Pinker and Bloom (1992:451-94) argue that language ability developed by natural selection and all human minds are adapted to learn a language. Wilson and Daly make a powerful case for the argument that male and female minds evolved to be qualitatively different (1992:292). Symons finds that humans have a universal mind adapted for defining sexual attractiveness (1992:141-45).

Nevertheless, if all behavior is innate, then it seems impossible to account for variation. Barkow, however, admits that it would be difficult to detect the Pleistocene conditions that would evolutionarily anticipate such things as literacy and libraries, mass media, ease of travel, money, contraception, and institutions that substitute for family responsibilities (Barkow, 1992:627). But these concessions are mild compared to the many recent innovations of humans that could not have been made by minds adapted to specific Pleistocene conditions, as I think the following observations demonstrate.

- What in the adapted psychology of the Pleistocene prepared humans in most of the world’s cultures to create gods, while 20% of the world, living in a circumscribed part of East Asia, remained atheistic (see “World Population Data Sheet,” 1997)? Were some minds more adapted to superstition than others? What panhuman mind simultaneously creates and does not create deities?
- What permitted humans adapted to a hunting and gathering existence to abandon that way of life to live in coercive political states with powers to control their freedom, their property, and even their lives? Specifically, what conditions selected some hominid minds to make political decisions that bound millions of other

hominids to obey, and simultaneously selected the willingness to obey in those millions?

- What in the Pleistocene prepared humans to live in giant societies among millions of strangers and only a relatively few kin? Did kin selection cease to be important in recent times?
- What prepared modern humans to live in systems of laws rather than customs, to grant some people fabulous wealth while others live in abject poverty, to live in systems of coerced monogamy, to create social castes where rewards and punishments are given according to genetic inheritance, to turn massively to agriculture and its radical departure from the hundreds of thousands of years of nonagricultural existence, and to live in cities, amidst filth and squalor, harsh labor, high-rise buildings, overcrowding, loss of leisure, and reliance on banks, markets, and interest rates?
- What Pleistocene challenges selected modern human minds willing to engage in mass warfare, fighting alongside strangers against strangers, or for such self-sacrificial behavior as volunteering for military service, or practicing celibacy, fasting, self-flagellation, scarification, sexual mutilation, or being willing to pay taxes, or give a kidney to a stranger?
- What Pleistocene conditions caused an adaptation for smoking, drinking too much, engaging in dangerous sports like mountain climbing and skydiving, or working deep in mines for coal, copper, or diamonds?
- What in the Pleistocene prepared humans to live in many different, often antagonistic, organizations at the same time—churches, clubs, cities, districts, jobs, schools, factories, farms, armies, political parties, committees, gangs, and so on?

The point is that minds individually and specifically adapted to live in Pleistocene conditions surely were challenged when modern conditions emerged involving huge societies and non-kin, and when new knowledge and inventions required, not individual, but collective responses. It seems to me that a general mentality of long standing related to sociability must have been used to allow the modern reactions. (Note that I do not use the word *adaptation* because it is not clear that humans have yet adapted to the modern period. While their numbers have indeed increased remarkably in ten thousand years, humans have also chosen solutions that threaten to destroy them.)

By claiming that almost every specific behavior today must have been adapted for in bygone eras, Barkow et al. have created unnecessary problems for themselves. I believe that they have neglected an important element of the adapted mind that allows us to reintroduce culture as something that is real, that many contemporary challenges (such as those mentioned above) have been met by general-purpose mental mechanisms related to sociability, and that culture and biology are co-evolutionary processes. It

does not belittle the main, but not exclusive, role of the individual in genetic adaptation to acknowledge (1) that culture and cultural evolution were essential and necessary processes in the development of modern humans, (2) that society can induce or compel individuals (always by rewards and punishments, but even more often by habits of compliance) to behave in certain ways, (3) that the willingness to observe social norms and cooperate in social purposes results from a mental adaptation of many millions of years ago and was thus pre-Pleistocene, and (4) that by observing social norms, cooperating in social organizations, and utilizing available technologies, individuals contribute to their own survival, either directly or by inclusive fitness, thus continuing their own genes in the gene pool. Further, the acknowledgment by the psychologists that in culture we are dealing with *social systems*, of which individuals are constituent parts, permits us to invoke systems analysis. What follows is an attempt to defend the reality of culture as well as its essential role in human development, without which humanity would not have arisen.

### General-Purpose Mental Adaptation to Sociality

Darwin made a powerful observation about the adapted human mind that extends it back much farther than the Pleistocene, and involved many more species than the human. He was, of course, referring to something that shaped the mind of all individuals in species that are social. Of all social animals in general, he said, “[S]ympathy is directed solely toward the members of the same community, and therefore toward known and more-or-less beloved members, but not to all the individuals of the same species” (1874:121-22).

Of humans as social animals, Darwin claimed that man has few special instincts but nevertheless has “some degree of instinctive love and sympathy for his fellows.” Man, he added, has an “inherited tendency” to be faithful to his comrades and obedient to the leader of his tribe. However, “the social instincts never extend to all the individuals of the same species.” The moral virtues “are practiced almost exclusively in relation to the men of the same tribe; and their opposites are not regarded as crimes in relation to men of other tribes” (1874:124, 132). From this, it might be concluded that Darwin was merely proposing another innate specific characteristic—ethnocentrism. What his statement also recognizes, however, is that sociability is what accounts for the great variability in cultures.

Psychologist David C. Geary has demonstrated that two domains are involved in cultural evolution. The first domain is individual: “The function of individual-level modules is to control the dynamics of one-on-one social interactions, to develop and maintain long-term relationships with kin and friends, and to support attempts to obtain social and material resources from other people” (Geary, 1998:182). How individuals learned to accomplish these

activities could very well have resulted from Pleistocene experiences.

The other domain is social:

In addition to cognitive systems that are specialized for maintaining and regulating dyadic relationships and interactions, there are almost certainly complementary systems . . . that are designed to divide and organize the social universe. The categorization of people into social groups appears to reflect the above-described categorical significance of kin, the formation of in-groups and out-groups, and ideologically based social identification, as exemplified by gender roles (Geary, 1998:184).

*The very essence of cultures is that memberships never include the entire human species.* Treating all six billion humans on earth as an undifferentiated mass cannot explain cultural variation. Each culture determines who belongs, not only by rules, laws, customs, and definitions, but also by mental percepts among fellows that reinforce them. This was demonstrated by innovative research done by Warnecke, Masters, and Kempter. They showed adult American subjects television images of the heads of people speaking, with the sound turned off. The speakers were American, French, or German, but the subjects did not know that. All speakers, as far as phenotype and dress, could have been American. The only cues were head and facial movements. The test subjects felt “more negatively when seeing the foreigners” and judged them “more negatively than their fellow citizens” (Warnecke, Masters, and Kempter, 1992:267; see also Geary, 1998:185; Wilson, 1978:70). Again, as Darwin noted, the social instinct never extends to all the members of a species. In fact, Geary argues, “one important condition for effective competition against an out-group is the disengagement of the emotional and moral mechanisms that appear to be designed to reduce conflict and to foster cooperation within in-groups” (1998:184). Furthermore, such disengagement gave those groups a competitive advantage (1998:185).

Symons illustrates the problems associated with the idea that every single cultural behavior can be explained by assumptions of a common, universal, specialized human mind that is adapted specifically in each case for that particular behavior. He makes a strong case that there may be some “universal, specialized psychological mechanisms” that are “responsible for human perceptions of sexual attractiveness,” which he says are common to all males in relation to females, and to all females in relation to males (1992:142-43). Nevertheless, in his study of the evolution of human sexuality, he demonstrates variable, group-defined, culturally selected minds. For example, all women are biologically capable of multiple orgasms, and in some societies *all* women have them, and in other societies few or no women have them (1979:84-86). What in the

Pleistocene specifically adapted modern women in some societies to have multiple orgasms but not those in other societies? Note that Symons could not explain this difference among women by reference to a universal, specialized mental mechanism that caused a specific physiological response in millions of women and a different one in other millions. When a huge proportion of humans in one place behave one way physiologically, and in other places humans with the same anatomy behave another way, and when it is conceded that the difference rests on what each group has learned, then it is not unreasonable to argue that culture is the causative factor. What I am suggesting is that a collection of individual adapted minds does not explain the social mind.

### Whole Groups as Organisms

Contrary to the thesis presented here, anthropologist Marvin Harris declares categorically that “collective minds don’t exist.” When huge populations of humans do and believe the same thing, it is nothing more than “the cumulative result of the individual decisions of millions and millions of” persons (Harris, 1977:147). Yet Harris calls himself a “cultural determinist” and defends the proposition that “cultural evolution has hitherto been shaped by unconscious impersonal forces” carried out “in countless acts of individual obedience to cultural rule and pattern.” In fact, “individual wills seldom prevail in matters requiring radical alterations of deeply conditioned beliefs and practices” (Harris, 1977:194-95). These statements appear to me to acknowledge collectively induced behavior, parallel with the thinking of scientists who speak of the beehive as a thinking machine (Seeley and Levien, 1987) and of army ants as a collective intelligence (Franks, 1989).

Humans, seemingly, have a mind that is innate in some behaviors and also receptive to novel, socially proffered ideas, behaviors, and relations with the material world; and the character of these acquisitions in their youth separates them from other humans who are imbued with different ideas, behaviors, and relations with the material world (Peres and Hopp, 1990:126). Lumsden and Wilson raise the right issues when they marvel at “the capacity of individuals to perceive the patterns of usage by whole groups and to use the result in decision making,” a process they attribute to the human ability to reify “complex patterns and processes” (1981:107; cf. Wilson, 1998:153).

It is not necessary for us as social scientists to get involved in the hot debate among biologists concerning the matter of group selection for genetic adaptation (see Wilson and Sober, 1994 and the 32 commentaries that follow). Many, perhaps most, biologists, apparently reject the argument that genetic evolution can occur by group selection, and instead accept what psychologist Donald T. Campbell calls “the dogma of the dominance of individual selection” (1994:28). Campbell, however, states that he now “tentatively”

accepts the dogma for most vertebrates, in reference to *genetic* selection, but insists that group selection of *cultural* traits surely occurs. His argument is worth noting:

It is the internal group homogeneity and intergroup variability which set the stage for group selection, were any of the traits involved to provide a group level advantage. This is a central concept for the cultural evolution of group attributes, ideologies, organizational traditions, and so on.... It is important to emphasize that this is a selection of culturally transmitted, not biological, attributes.... Groups (social organizations) can “die,” with all of their biological individuals joining other groups, becoming converted to other ideologies and organizational structures. Defeated groups can retain continuity of biological personnel but adopt a new religion or political structure. The selective process could be pure emulation by unsuccessful groups of successful. Or it could be the forcible imposition of the victor’s culture upon the vanquished.... The “group selection” posited is a selection of culturally transmitted beliefs, social-organizational structures, religious ideologies. It is not a “group selection” of genes. This cultural organized-social-group selection would make possible social norms and behaviors that lead individuals to override their own *individual* maximization of “inclusive fitness.” (Such overriding might lead to the maximization of the *average* individual inclusive fitness of the members of the group, but the only-individual-selection dogma precludes a biologically innate tendency to achieve this.) (Campbell, 1994:28)

While Wilson and Sober were primarily concerned with proving that group selection occurs in *genetic* evolution, their arguments are useful for an analysis of *cultural* evolution. The source of selection, they maintain, rests on identifying the *vehicle* of selection, which can be genes, individuals, or groups. They claim to avoid “naive” group selection by conceding that individuals compete in most groups, so that the unit of natural selection in within-group competition is the individual, not the group (Wilson and Sober, 1994:596). It is only when social groups are competing with other social groups that we can treat them as organisms (“any biological entity whose parts have evolved to function in a harmonious and coordinated fashion” [1994:606, note 1]). While individuals may engage in intense within-group competition, “intense between-group competition will favor *psychological mechanisms* that blur the distinction between group and individual welfare” (1994:601, emphasis added; cf. Monroe, 1994:876-78). Social scientists would be hard-pressed to deny that “members of the same group often share a feeling of high regard, friendship and trust that is based not on any prior experience but merely by the fact that they are members of

the same group” (Wilson and Sober, 1994:601).

Of special interest for political analysis is the following observation:

Since decision-making has occurred in small groups throughout human history, it is reasonable to expect “Darwinian algorithms” that cause individuals to relinquish their capacity to act as autonomous intentional agents and adopt a more limited role in a group-level cognitive structure. The architecture of group-level cognition might simply take the form of “leaders” who act as self-contained intentional agents and “followers” who abide by the decisions of others. (Wilson and Sober, 1994:602)

This can be demonstrated in those cases where people cooperate in their own deaths and that of their kin. A recent notorious example involved the people of Jonestown in Guyana in 1978. Almost one thousand people, prodded by a religious leader, not only committed suicide but also poisoned their children. A similar event occurred in Waco, Texas on April 19, 1993. Mass suicides involving hundreds of families are not unusual in history. To escape capture at Masada in 74 AD, Jews killed themselves and their children by the hundreds. They did the same in Europe in 1096 and 1190, to escape forced baptism.

In the seventeenth century, Russian Christians, almost 2,000 strong, did likewise to avoid a changed liturgy (see *The Encyclopedia of Religion and Ethics*, 1958, vol. 12:23, 29, 38; and *The Encyclopedia of Religion*, 1987, 12:26-27). Less well known, but involving many thousands of people, was the slaughter of Ugandans by Idi Amin in the early 1970s. Former president Milton Obote described how victims cooperated in their own deaths. One method of killing was to line up the victims “in single file and then the victim at the head of the line is ordered to lie down. The next victim to the one on the ground is then given an axe to crush the head of the person on the ground; knowing full well that it is his turn next. The last victim on the line is then either bayoneted or shot” (Mazrui, 1975:298).

Despite the fact that biologist John Tyler Bonner (1980:197) argued that memes “that are suicidal are summarily rejected,” the above examples suggest otherwise. Group suicidal behavior cannot be explained by individual self-interest, nor even by “herd interest.” How can a herd of suicidal humans be regarded as selfish, unless it be argued that they killed themselves to punish the survivors? A social mind seems to exist, one that is adapted to the purposes of the group, for the creation of leaders and followers, and for a high degree of cooperation (even mass suicide) within a group that faces out-group competition, as Darwin implied. As Matt Ridley argues, “conformist transmission of culture is one way of ensuring that you do what works—you inherit a disposition to copy your neighbors” (1997:182; cf. Boyd and Richerson, 1985:283).

Individual competition occurs within a group, but individuals cooperate when the group is competing with another group. Psychologically, each individual appears to believe that the group or subgroup identification that he/she has inherited is more important than life itself (certainly a nonbiological position and one that is not explained by the evolutionary psychologists). One can only wonder what Pleistocene challenge could have led to this uncommon and unadapted mind. It seems more reasonable to recognize the general mechanism of the adapted *social mind*—a mind that can respond to novel situations—and to acknowledge that rather than being “very weak,” as Tooby and Cosmides claim (1992:39), it is indeed powerful.

All human individuals live in a number of groups as well as in groups-of-groups, all overarched by culture, the most inclusive group with which humans identify. At any level where individuals are competing, including the cultural level, natural selection operates to promote the inclusive fitness of those who outcompete others (which is the argument of those who pursue the dogma of individual selection only). Where the competition is not genetic, it will be *memetic*, the term coined by Dawkins as the cultural parallel of the gene and which by natural selection spreads certain competing ideas, social institutions, and technologies (Dawkins, 1976:206; Brodie, 1996). Because *genetic* selection leading to adaptations requires many generations, Dennett (1995:473) is correct in charging that Wilson and Sober (1994), in referring to the recent Hutterite characteristics, demonstrate cultural (memetic) evolution, not genetic. Dennett also argues that memetics cannot be as scientific as genetics because of convergent cultural evolution (1995:487). As Boyd and Richerson maintain, “cultural transmission differs from genetic transmission because it includes inheritance of acquired variation” (1985:283).

At any level involving a group, when the group is in competition with other groups, individuals will often cease to compete among themselves and join to cooperate in defending the group. It is this condition that challenges rational choice theory, which assumes that individuals always act on the basis of self-interest even when their group is being challenged by another group (see especially Cory, 1999:55-57, 73-78; and Monroe, 1994:864-876). (This is not to say that *every* selfish individual will be completely cooperative. Even in warfare, when the survival of the group is at stake, often some individuals counter the war efforts by open or secret opposition, by black-market activities, or by selling out to the enemy.) Thus, at the cultural level, individuals will compete to define the contents of the culture, but when their culture is competing with another culture, the individuals usually cooperate in defending the culture. Were it otherwise, cultures would easily collapse. When a group of cooperative individuals competes with other groups, group selection is occurring, and the group that survives a particular competition will have its peculiar traits (ideologies, social organizations, technologies) rein-

forced, which is of course a process of natural selection. Losing groups will have their traits reduced, often, as Donald Campbell said (1994:28), to the point of extinction. As Matt Ridley noted, group extinction does not necessarily mean the death of its members, but rather their assimilation into other groups: “because the defeated individuals drop their culture and absorb that of the victors, cultural group selection can work” (1997:187). Since groups were the primary means by which humans survived and reproduced, groups and their expansion must be recognized as “forces of selection” (Shaw and Wong, 1989:45; cf. Corning, 1997:368; Geary, 1998:185). “Culture is in one sense the group mind of *Homo sapiens*” (Campbell, 1985:399).

The above arguments have now been reinforced by the heuristic researches of neuroscientist and medical doctor Leslie Brothers, and by brain scientist and biological anthropologist Terrence W. Deacon. Brothers has produced a volume that suggests how society shapes the human mind. She uses the metaphor of Robinson Crusoe to show that when Crusoe saw Friday’s footprint he was able to draw inferences because of his earlier enculturation, despite many years of isolation (Brothers, 1997).

Brothers rejects the paradigm of most neurologists and psychologists who, she maintains, try to study the brain as if it were an isolated mind or an “isolated automata, like little Robinson Crusoes” (1997:110, 127). She argues that if one treated Crusoe as asocial, then it would be impossible to explain “his diaries, his prayers, and all his ideas.” Yet, she maintains, that is what happened in cognitive neuroscience: “Because asocial brains are actually mindless, it was logically impossible to make the leap from the isolated brain to the mind” (1997:145). This, then, becomes her main theme:

By excluding from consideration the essential sociality of human beings, a purely artificial conundrum was perpetuated—for the human brain, stripped of its intrinsic sociality, is in fact mindless.... Similarly, considering an individual’s behavior as the product of his or her isolated “mind” depends on ignoring the extensive socialization that produces organized thought and behavior in every human being, socialization for which our brains are innately prepared (1997:68).

Brothers cites her own research and that of numerous other neuroscientists to demonstrate that “the existence of *socially* specialized single neurons indicates that primates have entire neural ensembles that are specialized for registering social features of the environment” (1997:42). She maintains that “during our species’ history, through the usual evolutionary processes of mutation and selection, there arose neural mechanisms especially good at registering social features—faces, for example” (1997:71). In fact, she demonstrates that experiments on single neurons as well as neural ensembles show that even infants are able to re-

spond to facial expressions, eyes, open mouths, and voices (1997:14-15).

She marshals evidence that “the network of meanings we call culture arises from the *joint* activities of human brains” and that “the mind is communal in its very nature: It cannot be derived from any single brain in isolation” (1997:xii).

Brothers supports Darwin’s claim that social animals are divided into *we/they* groups as follows: “Individuals who are born into a culture or who are fully acculturated gear their actions effortlessly to those of the group, whereas novices or outsiders who may be trying to ‘pass’ as members struggle to acquire embedded rules, and thus can only interact successfully with some effort” (1997:107).

Terrence Deacon, in a work that complements the work of Brothers, has provided a panorama of evolution from australopithecines (two and one-half million years ago) to modern humans that incorporates both cultural and anatomical selection. In fact, it was not the australopithecine brain (comparable to that of modern chimpanzees) that resulted in the co-evolution of culture and biology but *behavioral* (cultural) modifications involving symboling. Furthermore, anatomical evolution *alone* would not have produced the genus *Homo* line that evolved from australopithecines (Deacon, 1997:347); responses to *social* problems required symbolic thinking. The radical innovations of hunting, supplementing the diet regularly with meat, instituting routine sharing, involving the entire community in the enforcement of a social contract, and making sounds that represent abstractions, repeated over thousands of generations, coincided with changes in the prefrontal lobes of the brain that came to control laryngeal and tongue development as well as language and abstraction.

The near synchrony in human prehistory of the first increase in brain size, the first appearance of stone tools for hunting and butchery, and a considerable reduction in sexual dimorphism is not a coincidence. These changes are interdependent. All are symptoms of a fundamental restructuring of the hominid adaptation, which resulted in a significant change in feeding ecology, a radical change in social structure, and an unprecedented (indeed, revolutionary) change in representational abilities. The very first symbols ever thought, or acted out, or uttered on the face of the earth grew out of this socio-ecological dilemma, and so they may not have been very much like speech. They also probably required considerable complexity of social organization to bring the unprepared brains of these apes to comprehend fully what they meant. (Deacon, 1997: 401)

Deacon’s stated purpose is to define the conditions that required symbolic reference in the first place, and not to spell out all the developments of language from the beginning to the present. Putting evolutionary causes and effects in

proper order, he maintains, and “precisely identifying the anatomical correlates of this transition,” allows him to go beyond “just so” versions of the process: “The key to this is the co-evolutionary perspective which recognizes that the evolution of language took place neither inside nor outside brains, but at the interface where cultural evolutionary processes affect biological evolutionary processes” (Deacon, 1997:409).

In his research on fossil remains, evolutionary biology, symboling, representational speech, logic, and brain analysis (even involving inferences about symboling and reasoning abilities from the inner surface of the cranium), Deacon marshals evidence that “more than any other group of species, hominids’ behavioral adaptations have determined the course of their physical evolution, rather than vice versa” (Deacon, 1997:345). Again: “The evolutionary dynamic between social and biological processes was the architect of modern human brains, and it is the key to understanding the subsequent evolution of an array of unprecedented adaptations for language” (1997:349-50). In fact, he maintains that the principle involved was first enunciated a century ago by the psychologist James Mark Baldwin: “Baldwin suggested that learning and behavioral flexibility can play a role in amplifying and biasing natural selection because these abilities enable individuals to modify the context of natural selection that affects their future kin” (1997:322; cf. Corning, 1983:38-40; Leakey and Lewin, 1992:292-94).

Deacon has, I believe, captured the significance of cultural evolution as it effloresced from the innate sociability of pre-human ancestors and, faced with social problems, led to the co-evolution of both cultural and biological processes, each affecting the other. Deacon’s work provides an additional defense of co-evolutionary theories involving both culture and genes as propounded by biologist E. O. Wilson (1998:157), anthropologist William Durham (1991), and evolutionary historian Carl Degler (1991:312-17). More than that (to be noted below), he shows that late pre-humans used their inherited general-purpose minds related to their sociabilities to solve a number of cultural, particularly political, problems associated with behavioral variation.

The research of Brothers and Deacon justify an emphasis on cultural conditions as the basic source of the primate evolution to humanness. Together, I think they destroy the position of Tooby and Cosmides that emphasizes the centrality of isolated brains to explain culture. The adapted mind, in other words, has to respond to something given in its social environment. The infant, born with the tendency to cooperate with its fellows, does not then create a culture and observe those parts it happens to like, as I believe one might (wrongly, I am sure) infer from Tooby and Cosmides.

## Culture as Virtual Governor

Culture is often thought of as an abstraction and therefore unreal. Abstract wholes, however, can affect concrete parts, as physicist E. M. Dewan has shown. He examines the phenomenon by which separate parts of an interrelated system engage in a “mutual entrainment” of behavior out of which emerges a “virtual governor” that controls the entire system even though the virtual governor has no locatable, physical existence. Among other examples, Dewan describes one by Norbert Wiener concerning a number of interconnected generators in a power grid. Each generator is set up independently, and alone its output will be somewhat unsteady. When interconnected, however, all the generators gradually mutually entrain, so that soon all are operating synchronically as one: a virtual governor emerges that controls the entire system. “This virtual governor is not located in one spot in the system, but rather it pervades the system as a whole, so that it does not have a ‘physical existence’ in the usual sense. It is an *emergent property of the entire system* which goes far beyond what any single unit can accomplish in accuracy and power” (Dewan, 1976:185; cf. Corning, 1997:382).

Dewan then illustrates the existence of virtual governors in a system of two clocks mounted on a common support that adjust themselves to tick in unison. The same principle applies also in biology; for example, in the cells of the heart that beat as one (fibrillation resulting when the virtual governor fails), or in the synchronous flashing of fireflies in Southeast Asia (1976:184). He is not hesitant to note that the virtual governor has a “holistic property of control which has causal potency” and “*supervenes* in the behavior of the individual units” (1976:185). Dewan makes the obvious connection between virtual governor and culture by noting that the self-organization property that arises from mutual control as seen in entrainment “has many analogies in life and living systems” (1976:186n).

Each person has individual needs, and by mutual interactions between many people there emerges a community or social system. Such a system organizes itself so that the needs of the individuals are satisfied. *Somehow the individuals in the system have a way of controlling each other for their own goal satisfactions (manipulation, cooperation, financial arrangements, etc.) and in the end an overall control system comes into being.* In this example, we have an illustration of generalized entrainment; a common government, economic system, language, culture, and law system are all part of it. Anyone “getting out of line” in terms of behavior gets “feedback” to pull him “in” again just as, by analogy, the generators “pulled together in frequency.” (Dewan, 1976:188, emphasis added)

A work that parallels Dewan is the seminal thesis of political scientist Peter Corning. Although he does not speak

of virtual governor, he does speak of *synergism*, which he defines as “combinatorial effects that are produced by the joint action of two or more discrete elements, components, or individuals” (Corning, 1983:76). He emphasizes that functional synergism is the “key to understanding our biological evolution as a species,” and devotes many pages to illustrating that wholes are made up of parts but the whole is always more than the sum of its parts (1983:228-237).

The concept of virtual governor sharpens a problem that has long plagued cultural theory, namely, how an abstraction like culture could perform real acts, like controlling the behavior of individuals. In fact, the concept of virtual governor makes culture as real as consciousness or mind and probably just as mysterious (cf. Douglas, 1986:99-103; Deacon, 1997:449). Even E. O. Wilson refers to “virtual reality” to describe scenarios in the mind that create consciousness (1998:109). Therefore, the notion that interconnected parts (individuals who share a culture) naturally act to produce a virtual governor should stop further efforts to deny the reality of culture, as I believe Tooby and Cosmides inadvertently do. Here we need only add to the Tooby and Cosmides concept of the adapted mind the Darwinian adapted mind designed for social living to realize that humans live in systems of social control.

Although each mind has an instinct for social affiliation, as James Q. Wilson called it (1993:127), cultures are variable and they cause humans to behave in variable ways. Furthermore, while the socially adapted mind allows for enculturation, that mind, as Darwin asserted, has no inclination to identify its life interests with the entire species, certainly as true of humans as other animals. There is no culture that embraces the entire species in its identification.

## Culture and Ideas

Although virtual governor (or culture) appears to be simply an abstraction, it must be recognized that cultures have a material and organizational base as well as an ideational one. That is, the difference between a spear-throwing individual in Australia and a taxi-driving individual in New York is not fully captured by looking at their ideas, which in any case cannot be seen, only inferred. To capture their peculiar way of living requires that we understand how they are organized (not all spear-throwers organize the same way), and how they use the earth’s resources for survival (not all taxi-drivers use automobiles).

Much current literature restricts the term *culture* to values, or ideology, or to individualistic concepts, or ideas. Thus, as noted above, Tooby and Cosmides maintain that culture is in the heads of individuals (1992:47). William Durham says that “culture consists of shared ideational phenomena (values, ideas, beliefs, and the like) in the minds of human beings” (1991:3, 154n). E. O. Wilson maintains that “culture is created by the communal mind, and each mind in turn is the product of the genetically structured hu-

man brain” and “culture is reconstructed each generation collectively in the minds of individuals” (1998:127; italics removed). If we stopped here, we might be tempted to conclude that, like anger, fear, disgust, pleasure, and pain, culture is restricted to individual human minds, which could easily lead to solipsism—the claim that there is no reality outside an individual’s perception.

While ideas and values clearly generate and constrain behavior, they are not enough to grasp the full explanation of how and why humans behave in certain ways. The difficulty of treating culture as only ideas is revealed by Durham’s work. He claims that there is a new consensus in anthropology that defends the ideational theory of culture, and that “culture should be thought of not as behavior but as part of the information that specifies its form” (Durham, 1991:4), thus apparently agreeing with Tooby and Cosmides. Despite his persistent usage of this meaning, he cannot follow it in explaining the *sources* of the ideas of specific cultures, which presumably must be other ideas. Instead, he refers to property and animals, scarcity of land and water, optimal labor force, resource constraints (1991:59-63); to domestic economic patterns (1991:63-64, 474); to salt trading and ownership of valuables (1991:81); to “land, wealth, and agricultural production” ((1991:174); to inheritance of property (1991: chapter 2); and to sources of food and the means for acquiring sustenance (1991:393). Material conditions accompanied by social arrangements thus explain the source of many ideas and consequently much of culture. As Brothers said, “I take mind to be irreducibly transactional. Rather than something packed inside a solitary skull, it is a dynamic entity defined by its transactions with the rest of the world” (1997:146).

Culture, furthermore, can be seen as consisting also of the behaviors and artifacts of humans and thus of a reality that can be at least partially detected by the senses. One detects by sights and sounds and smells and tastes the differences between Chicago, London, New Delhi, and Lagos. I cannot imagine anyone who has been to these cities having doubts as to which one he/she is in. The culture is reflected in the dress and behavior of the people one meets, in the technology one encounters, in the social organizations one has to deal with, and, of course, in the language, dialect, or localism that one hears spoken. That is why one would be surprised to hear the call of the muezzin in Kalamazoo but not in Timbuktu. It seems to me that to put a culture exclusively in the minds of individuals would make it invisible to people of another culture. A culture is real because the mind can infer the mind of others and the senses can detect the material and social contexts of it. I am forced to agree with biologist Michael Ghiselin, who criticizes Durham for treating “living organisms and their activities as mere products of culture rather than as its components” (Ghiselin, 1993:124). Treating culture as information, Ghiselin adds, is “a wonderful way to get rid

of the organisms and what they do!" (1993:124). And, as Brothers said, "It makes all the difference in the world that our brains have a special sensitivity to the sights and sounds of our fellow beings" (1997: 72).

This is not to deny the importance of mind to culture, but to suggest that those who restrict culture to ideas ignore much of what it means to be a cultured animal, which includes also social institutions, behaviors, and technologies. Furthermore, while ideas can only be inferred, social organization can be detected, often vaguely, in relationships between and among humans and their behaviors, and technologies can be known by direct sensory perception.

## Political Implications

Political scientists were among the earliest of contemporary social scientists to develop an interest in the relationship of biology to politics and governance. Beginning in 1964, when L. K. Caldwell coined the contemporary term *biopolitics* (Caldwell, 1964), followed in 1968 by Albert Somit, who related politics to ethology and psychopharmacology (Somit, 1968), and in 1970 by Thomas Thorson, who related politics to scientific evolutionary development, there has followed a steady growth in the number of political scientists who have turned to biopolitics for their paradigm of the discipline. The founders are identified for the years up to 1978 in Wiegele, 1979. So great was the interest among political scientists after the early 1970s that they founded the Association for Politics and the Life Sciences, and in 1981 they founded a journal, *Politics and the Life Sciences*, now in its twentieth volume.

Political scientists, therefore, should find special significance in the concepts treated in this article by noting the way government has contributed to human evolution. Government itself, of course, is an obvious way that humans have learned to control themselves. In fact, political scientist Peter Corning has argued that government refers to something that is generic and functional rather than institutional, and as such was associated

with the most rudimentary socioeconomic behaviors in our most protohominid ancestors: hunting and gathering activities, care of the young, the making of primitive tools, the construction of rude shelters, the management of fire, the domestication of animals, primitive exchange patterns, migrations, and defense against predators of all kinds. Thus "government" is associated with any co-operative, survival-related social activity in human beings or animals, and it probably dates back several million years (Corning, 1983:318; cf. Deacon, 1997; Geary, 1998:180).

Recognizing the coercive aspect of government, Corning shows that a policing function exists in many species (Corning, 1997:372). Since humans, like other animals,

tend to be self-serving, the question of why humans cooperate must be faced. If, however, cooperation "offers sufficient benefits it may be in the interest of some people to invest in coercing the cooperation of others," group selection being one mechanism (1997:373). In such a context, one can see how a government could be imposed, one purpose of which is to control the dangers of unrestrained self-interest (1997:378).

Political scientist Gary Johnson also argues that government and politics both exist not only in humans, but in all social animals. He offers the creative thesis that government and politics exist also in the regulatory mechanisms of molecules, cells, and organs. Government he defines as centralized coordination of a social system (Johnson, 1995:247), and politics as competitive efforts to influence government (1995:268), both stemming from a "predisposition to engage in centralized coordinating behavior" (1995:247, 268), involving both leaders and followers (1995:264). Darwin, as noted above, specifically saw obedience to leaders, a governmental behavior, as a characteristic of the socially adapted mind.

Deacon has now supplied significant evidence that it was a *governmental*, i.e., political, problem faced by australopithecines two and one-half million years ago that initiated the slow movement toward modern humanness. The problems rested on reproductive success in a hunting society where men and women are often apart, involving questions of paternity uncertainty, adequate provisions for females and infants, the establishment of strong systems of reciprocity, sexual jealousy, and how to involve the whole society in the creation and maintenance of new social rules. Such a complex system could have come about only by symbolic communication. In fact, "without symbols that refer publicly and unambiguously to certain abstract social relationships and their future extension, including reciprocal obligations and prohibitions, hominids could not have taken advantage of the critical resource available to habitual hunters" (Deacon, 1997:401). "Symbolic culture," Deacon concluded, "was a response to a reproductive problem that only symbols could solve: *the imperative of representing a social contract*" (1997:401, emphasis added)!

Deacon has basically claimed that it was the problems of governance (not random genetic mutation) that created modern humans. What a marvelous tool he has handed political scientists for their explanations of political behavior! The need for a social contract, of course, was a need for procedures to cope with the communication of ideas related to the changes produced by stone tools and hunting (cultural characteristics). And in making countless attempts over thousands of generations to express ideas, formulate rules, and communicate abstractions, pre-humans became human, and today all humans find meaning in their lives by the social contracts they have inherited. The complexities of government surely rest on general-purpose social minds, augmented by specific-purpose minds for some situations.

Deacon's position contrasts with that of Tooby and Cosmides. He has a whole section titled and devoted to the idea that "No Mind Is an Island" (1997:423–32), while they propose that "the component parts of the population are individual humans, so any social dynamics must be anchored in models of the human psychological architecture" (1992:47).

Political scientist David Easton, dealing with his long-standing concern with political systems, has provided a view of modern society that augments Deacon. He notes that all systems have structures. The political system is a set of structures arranged hierarchically, and consists of political authorities, regime, and community. We can pick one part, say the regime, and analyze it further into parties, legislatures, executive, etc. These in turn can be analyzed in terms of individual political roles. Each level has its own structure and can be analyzed at that level. However, "each lower level can be understood only by taking into account its position in the next higher level of which it serves as a constituent element. Each higher level helps to organize the next lower level of the system and thereby creates new properties in the system" (1990:270).

One may begin at any level and by analysis break it into its parts. However, for Easton, analysis alone provides little or no explanation, only description. To understand, one must examine the level above the one being studied, and then the one above it, and so on, until the structure of the whole system is understood: "The whole so created has properties that cannot be inferred from knowledge about the parts and does not represent some kind of summation of such parts" (1990:284). From this sort of analysis, we can see that individuals in social systems are just as likely to be the object of the socially prescribed behavior as the creator of it, a position parallel to that of Wilson and Sober, and in contrast to Tooby and Cosmides.

Easton's concept that higher-order structures are necessary to explain the behavior of lower-order structures, even down to the individual, can be seen as parallel to the operation of a virtual governor. Of course, he says, individuals are the actors in the social system; but it must be recognized that "many of the properties of political relationships—often called emergent ones—may be accessible only if dealt with at the collective level" (1990:257). Nevertheless, he says we must avoid the individualistic fallacy, that is, the belief that we can "derive higher-level properties from the properties of constituent individuals," just as studiously as we avoid the ecological fallacy, namely, "assuming that properties of aggregates of individuals necessarily enable us to predict characteristics of single individuals" (1990:251). Here, he is reminding us that something about individuals in groups is different from individuals in isolation, and challenges the notion of Tooby and Cosmides that societies can be known by focusing on individuals.

On the first point, we need only note the astute observation of Wilson and Sober to the effect that "intelligent in-

dividuals do not automatically combine to form intelligent groups" (1994:602). There is simply no way, I submit, that we can examine the beliefs, interpersonal relations, and means of livelihood of individuals and their adapted minds and predict and explain the doctrine of the Trinity, the creation of baseball, judicial review, or the American presidency. The whole is simply different from and greater than its parts, having a history, social memory, standards, and rules that exist both before and after particular individuals are identified. While it may be possible to predict that humans anywhere will have the same standards for sexual attractiveness, we cannot predict that they will have the same standards for good government, or the best god, or what is prestigious. Some things are socially learned, and the mind adapted for sociability makes such learning possible.

But what about the reverse fallacy? May structures permit us to predict the characteristics of constituent single individuals? Clearly, in any structure, including cultures, many individuals will behave more or less the same way, else the structure will collapse. Yet no single individual behavior can be predicted. This is so because the total environment of any individual has many structures in it and he/she is forced to react selectively. An individual may react in an unpredictable way because of immediate self-interest, conflicting structural stimuli, weak enculturation about some structures, biologically defective sociability, incomplete knowledge, superstition, prejudice, fear, anger, and perhaps other characteristics. If all minds are basically adapted for specific responses to specific environments, then I would think that human cultures would be more alike than they are today.

When Easton asserts that "the structures of systems have properties which cannot be inferred from the properties of their constituent individuals," he said that what he meant is "that the behavior of individuals in relationship to each other generate such properties. Hence we need to look to these relationships to discover them" (Easton, 1990:257), a position that agrees with that of Tooby and Cosmides in their reference to individuals as component parts of a social system (1992:47). Because we cannot comprehend all of the possible human relationships, "we are compelled through the sheer enormity of the task of unraveling these ties to turn, instead, to the collective results of their actions, as group properties, so called" (Easton, 1990:258), which I believe Tooby and Cosmides would deny.

While we think of a culture as having specific characteristics, we also think of it as having individual and subgroup variations. The virtual governor that is culture allows for variation among members. Easton, then, is correct in emphasizing that "the individual only reacts to one or another part of this whole. It is this fact that enables us, in the end, to account for individual responses to what appears to be the same context" (Easton, 1990:251).

## Conclusions

If the evolutionary psychologists, in their strongly stated arguments, mean to assert that whatever a human does he/she has a mind adapted to do it, then we are merely dealing with a tautology. If, on the other hand, they mean to argue that every human has *some* evolved mental architectures for dealing with a great number of specific matters, but also some general-purpose architectures to deal with novelty, then these latter deserve to be studied too.

The human mind evolved out of the confluence of behavioral and genetic selection. In the process, a general-purpose mind inclined to socialization was utilized, and explains the complex behaviors of humans to a myriad of social situations. Culture, the virtual governor, is not simply a series of discrete stimuli among which individuals pick and choose what they like, but an extremely complex array of rules, values, social arrangements, and technologies to which individuals with socially inclined minds must adjust to give their lives meaning.

We might agree with Dennett (1995:485) that we should recognize the *nonstupidity* of our species and acknowledge that some behavior is cultural, and not assume that there must be a genetic base for every act, which must be assumed if each act is generated by a mind specifically “adapted” for that purpose. For example, there is no need to assume a genetic base for territoriality since humans (and other animals) have to be located some place, so they tend to protect where they are (Dennett, 1995:487).

The interests of the cultural group are often different from the interests of the individuals that compose it, yet individuals cooperate in a group because of the general-purpose mechanisms associated with the adapted group mind. As Geary has argued,

The sociality of humans, and probably of many other primates ... involves developing and maintaining preferred relationships with other individuals and organizing conspecifics into social categories. These features of sociality, in turn, appear to reflect the evolution of individual-level and group-level modules (1998:182).

In recapitulation, I have argued that primate sociability began long before the Pleistocene. A social instinct inclines primates to behavior that includes fellows, and in humans inclines them not only to be aware of the feelings and interests of others but to the formation of a social contract among identifiable groups. Sociability explains how australopithecines (faced with questions of governance in a context of stone tools, hunting, and mate protection) could begin incipient symbolic language and, ipso facto, complex culture. Political problems faced our ancestors even before the beginning of the Pleistocene. In their struggle to resolve their challenges, they learned to symbolize and, of special importance, developed a social contract, the

cauldron for politics to this day. The search for political solutions led the evolution from australopithecine to *Homo sapiens sapiens*, behavioral (cultural) modifications co-evolving with anatomical modifications.

Culture is real and exists outside human minds, but it can be detected by human minds through the senses as well as the ability to infer what is in other people’s minds. Behavior that is not replicated by, or understood by, or communicated to, others is probably strictly individual but rare. Culture is the virtual governor that, because of a social mind, makes it possible for humans to know who belongs to the group and who does not, to channel behavior into acceptable patterns, and to institute social contracts.

Over forty years ago, biologist Sewall Wright grappled with the question of individuals and their choices. He said that logically it would appear that individuals function in a “creative process,” based on will or choice. But he was forced to add (1953:16-17):

There are, however, an enormous number of largely separate minds and what happens anywhere at any time is not the choice of any one participant but a resolution of the choices of all, at all levels of organization from the lowest to the highest.

## References

- Alexander, R. (1987). *The Biology of Moral Systems*. New York: Aldine de Gruyter.
- Axelrod, R. and W.D. Hamilton (1984). *The Evolution of Cooperation*. New York: Basic Books.
- Barkow, J.H. (1992). “Beneath New Culture is Old Psychology: Gossip and Social Stratification.” In J.H. Barkow, L. Cosmides, and J. Tooby (eds.), *The Adapted Mind*. New York: Oxford University Press.
- Barkow, J.H., L. Cosmides, and J. Tooby, eds. (1992). *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*. New York: Oxford University Press.
- Betzig, L.L. (1986). *Despotism and Differential Reproduction: A Darwinian View of History*. New York: Aldine.
- Bonner, J.T. (1980). *The Evolution of Culture in Animals*. Princeton: Princeton University Press.
- Boyd, R. and P. Richerson (1985). *Culture and the Evolutionary Process*. Chicago: University of Chicago Press.
- Brodie, R. (1996). *Virus of the Mind*. Seattle: Integral Press.
- Brothers, L. (1997). *Friday’s Footprint: How Society Shapes the Human Mind*. New York: Oxford University Press.
- Caldwell, L. K. (1964). “Biopolitics: Science, Ethics, and Public Policy.” *The Yale Review* 54, 1:1-16.
- Campbell, B.G. (1985). *Human Evolution: An Introduction to Man’s Adaptations*. New York: Aldine.
- Campbell, D.T. (1994). “How Individual and Face-to-Face-Group Selection Undermine Firm Selection in Organizational Evolution.” In J.A.C. Baum and J.V. Singh (eds.), *Evolutionary Dynamics of Organizations*. New York: Oxford University Press.
- Corning, P.A. (1983). *The Synergism Hypothesis*. New York: McGraw-Hill.
- Corning, P.A. (1997). “Holistic Darwinism: ‘Synergistic Selection’ and the Evolutionary Process.” *Journal of Social and Evolutionary Systems* 20:363-400.

- Cory, G.A., Jr. (1999). *The Reciprocal Modular Brain in Economics and Politics*. New York: Kluwer Academic/Plenum.
- Cosmides, L. and J. Tooby (1992). "Cognitive Adaptations for Social Change." In J.H. Barkow, L. Cosmides, and J. Tooby (eds.), *The Adapted Mind*. New York: Oxford University Press.
- Darwin, C. (1874). *The Descent of Man and Selection in Relation to Sex*. Second Edition. New York: Hurst.
- Dawkins, R. (1976). *The Selfish Gene*. New York: Oxford University Press.
- Deacon, T.W. (1997). *The Symbolic Species*. New York: W.W. Norton.
- Degler, C.N. (1991). *In Search of Human Nature*. New York: Oxford University Press.
- Dennett, D.C. (1995). *Darwin's Dangerous Idea: Evolution and the Meanings of Life*. New York: Touchstone.
- Dewan, E.M. (1976). "Consciousness as an Emergent Causal Agent in the Context of Control System Theory." In G.G. Globus, G. Maxwell, and I. Savodnik (eds.), *Consciousness and the Brain*. New York: Plenum.
- Douglas, M. (1986). *How Institutions Think*. Syracuse, NY: Syracuse University Press.
- Durham, W.H. (1991). *Coevolution: Genes, Culture, and Human Diversity*. Stanford, CA: Stanford University Press.
- Easton, D. (1990). *The Analysis of Political Structure*. New York: Routledge.
- Franks, N. R. (1989). "Army Ants: A Collective Intelligence." *American Scientist* 77:139-145.
- Geary, D.C. (1998). *Male, Female: The Evolution of Human Sex Differences*. Washington, DC: American Psychological Association.
- Ghiselin, M.T. (1993). Review of W. Durham, *Coevolution: Genes, Culture, and Human Diversity*. *Politics and the Life Sciences* 12:123-24.
- Ghiselin, M.T. (1997). *Metaphysics and the Origin of Species*. Albany, NY: State University of New York Press.
- Harris, M. (1977). *Cannibals and Kings: The Origins of Cultures*. New York: Random House.
- Hrdy, S.B. (1981). *The Woman That Never Evolved*. Cambridge, MA: Harvard University Press.
- Johnson, G.R. (1995). "The Evolutionary Origins of Government and Politics." In A. Somit and J. Losco (eds.), *Human Nature and Politics*. Greenwich, CT: JAI Press.
- Leakey, R.E. and R. Lewin (1992). *Origins Reconsidered: In Search of What Makes Us Human*. New York: Doubleday.
- Low, B.S. (1993). "An Evolutionary Perspective on War." In W. Zimmerman and H.K. Jacobson (eds.), *Behavior, Culture, and Conflict in World Politics*. Ann Arbor, MI: University of Michigan Press.
- Lumsden, C. J. and E.O. Wilson (1981). *Genes, Mind, and Culture*. Cambridge, MA: Harvard University Press.
- Mazrui, A. (1975). *Soldiers and Kinsmen in Uganda*. Beverly Hills, CA: Sage Publications.
- Mills, C.W. (1960). *Images of Man: The Classic Tradition in Sociological Thinking*. New York: George Braziller.
- Monroe, K.R. (1994). "A Fat Lady in a Corset: Altruism and Social Theory." *American Journal of Political Science* 38:861-93.
- Peres, Y. and M. Hopp (1990). "Loyalty and Aggression in Human Groups." In J. van der Dennen and V. Falger (eds.), *Sociobiology and Conflict*. London: Chapman and Hall.
- Pinker, S. (1997). *How the Mind Works*. New York: W.W. Norton.
- Pinker, S. and P. Bloom (1992). "Natural Language and Natural Selection." In J.H. Barkow, L. Cosmides, and J. Tooby (eds.), *The Adapted Mind*. New York: Oxford University Press.
- Ridley, M. (1997). *The Origins of Virtue: Human Instincts and the Evolution of Culture*. New York: Viking.
- Ross, R. and E. Van Den Haag (1957). *The Fabric of Society: An Introduction to the Social Sciences*. New York: Harcourt, Brace.
- Seeley, T.D., and R.A. Levien (1987). "A Colony of Mind: The Beehive as Thinking Machine." *The Sciences* 27, 4:38-43.
- Shaw, R.P. and Y. Wong (1989). *The Genetic Seeds of Warfare*. Boston: Unwin Hyman.
- Somit, A. (1968). "Toward a More Biologically-Oriented Political Science: Ethology and Psychopharmacology." *Midwest Journal of Political Science* 12:550-67.
- Symons, D. (1979). *The Evolution of Human Sexuality*. New York: Oxford University Press.
- Symons, D. (1992). "On the Use and Misuse of Darwinism in the Study of Human Behavior." In J.H. Barkow, L. Cosmides, and J. Tooby (eds.), *The Adapted Mind*. New York: Oxford University Press.
- Thorson, T.L. (1970). *Biopolitics*. New York: Holt, Rinehart and Winston.
- Tooby, J. and L. Cosmides (1992). "The Psychological Foundations of Culture." In J.H. Barkow, L. Cosmides, and J. Tooby (eds.), *The Adapted Mind*. New York: Oxford University Press.
- de Waal, F. and F. Lanting (1997). *Bonobo: The Forgotten Ape*. Berkeley: University of California Press.
- Warnecke, A.M., R. Masters, and G. Kempter (1992). "The Roots of Nationalism: Nonverbal Behavior and Xenophobia." *Ethology and Sociobiology* 13:267-82.
- Wiegele, T.C. (1979). *Biopolitics: Search for a More Human Political Science*. Boulder, CO: Westview.
- Wilson, D.S. and E. Sober (1994). "Re-Introducing Group Selection to the Human Behavioral Sciences." *Behavioral and Brain Sciences* 17:585-654.
- Wilson, E.O. (1975). *Sociobiology*. Cambridge, MA: Harvard University Press.
- Wilson, E.O. (1978). *On Human Nature*. Cambridge, MA: Harvard University Press.
- Wilson, E.O. (1998). *Consilience: The Unity of Knowledge*. New York: Alfred A. Knopf.
- Wilson, J.Q. (1993). *The Moral Sense*. New York: The Free Press.
- Wilson, M. and M. Daly (1992). "The Man Who Mistook His Wife for a Chattel." In J.H. Barkow, L. Cosmides, and J. Tooby (eds.), *The Adapted Mind*. New York: Oxford University Press.
- "World Population Data Sheet" (1997). Washington, DC: Population Reference Bureau.
- Wright, R. (1994). *The Moral Animal: Evolutionary Psychology and Everyday Life*. New York: Vintage Books.
- Wright, S. (1953). "Genes and Organism." *American Naturalist* 87:5-18.

## ETHNICITY

# The Homeopathy of Kin Selection: An Evaluation of van den Berghe's Sociobiological Approach to Ethnic Nepotism

Ingo Brigandt

University of Pittsburgh, USA

**Abstract.** The present discussion of sociobiological approaches to ethnic nepotism takes Pierre van den Berghe's theory as a starting point. Two points, which have not been addressed in former analyses, are considered to be of particular importance. It is argued that the behavioral mechanism of ethnic nepotism—as understood by van den Berghe—cannot explain ethnic boundaries and attitudes. In addition, I show that van den Berghe's central premise concerning ethnic nepotism is in contradiction to Hamilton's formula, the essential principle of kin selection theory. It is further discussed how other approaches that make reference to ethnic nepotism are related to van den Berghe's account and its problems. I conclude with remarks on the evolutionary explanation of ethnic phenomena.

While some aspects relevant to the study of ethnicity (warfare, aggression) have already been addressed sociobiologically, it was Pierre van den Berghe who offered the first attempt to integrate ethnicity as such in a biosocial framework (see van den Berghe, 1978b). His central idea is that the behavioral disposition of ethnic nepotism, derived from kin selection theory, explains the central features of ethnicity. This theory of ethnic nepotism has been of particular influence for some other authors who want to include evolutionary biology in their account, including Shaw and Wong, 1989; Rushton, 1995; Vanhanen, 1999b; Salter, 2001. For this reason, van den Berghe's theory is taken as a starting point for my discussion.

In addition, two essential steps in van den Berghe's argument that have not yet been clearly addressed are here argued to be unsound, thus posing fundamental problems for his position as it is stated. In particular, the central premise of ethnic nepotism is shown to be based on a gross misunderstanding of Hamilton's rule; this tenet cannot be salvaged because it contradicts kin selection theory. In the second part, other authors' evolutionary approaches to ethnocentrism are discussed with respect to how they relate to van den Berghe's theory and the mentioned problems, where the focus will be on the offered evolutionary explanation of ethnic attitudes. I conclude the analysis with remarks on the methodology and explanation of ethnic phenomena using evolutionary theory.

---

**Ingo Brigandt** is a graduate student in the Department of History and Philosophy of Science, University of Pittsburgh, 1017 Cathedral of Learning, Pittsburgh, PA 15260, USA (e-mail: inb1@pitt.edu). He has a master's degree in mathematics from the University of Constance (Germany), where he studied mathematics, philosophy, and biology. His research includes biological approaches to race and ethnicity, the concept of homology in biology, and probabilistic causality and explanation. He is currently working on a Ph.D. thesis that examines the origin and role of concepts and conceptual change in the new field of evo-devo (evolutionary developmental biology).

### Van den Berghe's Account

A review of van den Berghe's theory is necessary in order to highlight the main points of his sociobiological account and to situate my discussion. The perspective taken by van den Berghe is one that regards humans as organisms striving to maximize their inclusive fitness. Three main mechanisms that characterize both animal and human behavior are postulated: *kin selection*, *reciprocity*, and *coercion*.

*Kin selection*, on this account, is a genetically founded behavioral disposition. In fact, it is the oldest mechanism of sociality to have developed. Kin selection is altruistic behavior directed to relatives. Since it is a propensity to favor kin over non-kin and close kin over distant kin, van den Berghe also calls it *nepotism*. Indeed, the intensity of kin selection is proportional to the coefficient of relatedness (of the donor and the recipient of the altruistic act). Relatives, to the extent that they are related, can be expected to help increase each other's fitness even at some cost to their own fitness. This behavioral disposition could evolve because each individual reproduces its genes not only directly through its own reproduction, but also indirectly through the reproduction of its relatives to the extent that it shares genes with them. Therefore, behaving nepotistically increases one's inclusive fitness. We share kin selection with countless other species (e.g., insects). Consciousness of kin relatedness is not necessary for kin selection to operate, although it is a partially conscious process in our species.

Van den Berghe's definition of kin selection may surprise those who are acquainted with sociobiology. In evolutionary biology, kin selection refers to a special mechanism of natural selection, that is, to an evolutionary process.<sup>1</sup> By considering kin selection a behavioral mechanism, van den Berghe conflates the evolutionary process with its product. In what follows, I try to keep these separate by using the term *kin selection* for an evolutionary process and denoting the behavioral disposition which is so central for van den Berghe's work by the terms *kin nepotism*, *ethnic nepotism*, or simply *nepotism*.

*Reciprocity* is van den Berghe's name for reciprocal altruism. It is altruism that is furnished with the expectation of return in the future. This sort of cooperation for mutual benefit operates also between non-kin. As it presupposes memory and elementary forms of cognitive capacities, it evolved much later than kin selection and is more developed in humans than in other animals. While reciprocity originated via natural selection, it has been enormously elaborated on by human cultures. It is a means of cooperation and coalition formation within and between human societies. Of course, reciprocity may invite cheating or free-loading and may motivate some humans to deceive others.

*Coercion* is the use of force for one-sided benefit, that is, for purposes of intraspecific parasitism or predation. The development of collective means of coercion for the purpose of exploitation is mainly a cultural development. It operates between and within societies. Coercion in human societies differs from that in animal populations, because humans not only form individual dominance hierarchies, but also establish group hierarchies. Further, physical strength or other biological properties play a much smaller part in human competition. Finally, ideology may be used to justify hierarchies and roles.

Van den Berghe (1979) tried to show that human family systems and kin groups are organized according to sociobi-

ological predictions. The main mechanism for kinship organization is kin nepotism, and van den Berghe applies it to the question of ethnic relations as follows: Ethnic groups are extended kin groups, since in-group members are more genetically related to each other than they are to out-group members. Though kin and ethnic terms do not denote biological kinship, they are correlated with it. Common ethnic descent is a belief, but to be effective it must coincide with biological descent to a large degree.<sup>2</sup> Ethnic sentiments are nothing but extended kin sentiments. Intraethnic relations are determined by kin nepotism, insofar as they can be called ethnic and do not belong to other types of social organization. While nepotism is a strong mechanism only with respect to kinsmen, it does have some influence on the relations of other members of the ethnic group, because the members of an ethnic group are to a certain extent related to each other. Nepotism means to favor close relatives over distant relatives. Ethnic attitudes of nepotism and ethnocentrism were selected for because they increased the inclusive fitness of the members of an ethnic group.

The relation between ethnic groups is largely determined by reciprocity or coercion, where both are dependent on cost-benefit considerations. The social organization within an ethnic group is also influenced—as is every type of human behavior—by these two mechanisms. Ethnicity is a special basis of sociality, irreducible to others, though often overlapping with other principles of sociality. Its essence is nepotism, a mechanism of evolutionary origin with a genetic basis, although it is transformed by culture. In contrast, other social groups, such as classes, are formed according to common material interest, that is, by means of reciprocity or coercion. The change in common material interest also explains the dynamics of ethnicity, as in changes of ethnic identification.

Why do ethnic groups use cultural markers of ethnicity instead of physical or phenotypic properties? Because during the last ten thousand years, the members of those ethnic groups that came in contact were not very phenotypically different. That is, cultural criteria are applied since they are much more reliable markers of extended kinship. Humans, for that reason, were selected to favor kin, not to favor those that look alike. When there is, however, migration across a phenotypic gradient, "race" is a good test of kinship. Indeed, racism and such migrations are both very recent phenomena. Racism is a cultural invention, since the employment of certain phenotypic criteria is a cultural choice. But the inborn mechanism of ethnic nepotism makes racism possible and probable in modern societies.

## The Homeopathy of Kin Selection

An important contribution by van den Berghe is the focus on the similarities of kinship and ethnicity. Ethnic groups as well as kin groups are defined by common descent. Eth-

nic categories are often derived from kin terms (van den Berghe, 1979). These aspects have been emphasized only by some scholars in the field of ethnic relations (see, e.g., Horowitz, 1985). There is no doubt that kinship terminology is important for ethnic identity. Van den Berghe thinks that kinship and kin nepotism are also important for ethnic phenomena such as ethnocentrism. There are strong, emotional family attachments that provide a stable cohesion of the family. A similar situation applies to ethnicity. Van den Berghe's idea is to regard ethnic groups as extended kin groups—ethnicity is extended kinship (1978b; 1986; 1987; 1995). The basic mechanism of ethnic solidarity is nepotism (see, e.g., 1986:250). The latter—van den Berghe also calls it kin selection—is the propensity to favor kin over non-kin and close kin over distant kin. It has a genetic basis and evolved according to kin selection theory (1978a; 1978b; 1979; 1987; 1995).

“Kin selection” (i.e., nepotistic altruism) may reduce the fitness of the individual acting altruistically, but it increases, conversely, the fitness of the recipient. The intensity of kin selection depends on the coefficient of relatedness, which declines exponentially when considering more distant kin. I call this basic principle of van den Berghe's the *homeopathy of kin selection*. For this reason, nepotism is not simply active among kinsmen, but also to a certain extent among fellow ethnics:

But the principle of nepotism, however diluted, suffuses all levels, and there is no a priori reason why nepotistic discrimination should stop at any particular point, unless it can be displaced by a superior strategy of fitness maximization. (van den Berghe, 1995:362)

Ethnic sentiments are characterized as *extended* kin sentiments or as “kin selection.” The central idea of ethnicity as extended kinship implies in particular that the basis for ethnocentrism is “kin selection,” that is, ethnic nepotism (1979:212; 1987:18).

But what does the metaphor of “extension” actually mean? “Extension” could refer to the historical or phylogenetic situation that large ethnic groups evolved out of small kin groups. Van den Berghe states this fact, but not in connection with his “extension” metaphor. The idea of ethnic sentiments as extended kin sentiments can serve as a useful starting point for psychological studies. This could, in particular, include developmental studies, since family identities are formed before ethnic identities. Maybe ethnic identity originates by means of socialization from kinship identities. This does not seem to be what van den Berghe has in mind, because it is important for him that during the course of human evolution there has been selection for extended kin nepotism: “What I am suggesting is that ethnocentrism evolved during millions, or at least hundreds of thousands of years as an extension of kin

selection” (1978b:404).

Ethnicity is characterized as extended kinship. This apparently means that all basic forms of ethnic social organization are extensions of kinship organization. Nevertheless, with respect to each specific aspect of organization, “extension” might mean something different. Obviously, van den Berghe considers an ethnic group a sort of kin group writ large and ethnic sentiments as weakened kin sentiments toward members of the same ethnic group. But is there a strong phenomenon of kin-centrism, the extension of which is its diluted form ethnocentrism? Is ethnic competition an extension of competition among kin groups? Is warfare among ethnic groups a weakened form of warfare among kin groups? The idea of “ethnicity as extended kinship” is in my view a very powerful perspective. Unfortunately, even in his book-length treatment (1987), van den Berghe gives us no explication of his central starting point.

The sociobiological explanation of ethnocentrism given by Pierre van den Berghe proceeds basically in two steps. The pivotal point is the homeopathy of kin selection, that is, the claim that people behave altruistically towards other persons according to their degree of relatedness. The first step of the argument is a claim about ultimate mechanisms, namely, the tenet that ethnic nepotism evolved by kin selection. The second point concerns proximate causes, namely, the idea that homeopathic nepotism explains ethnic phenomena such as ethnocentrism, due to the fact that the members of an ethnic group are more closely related to each other than to out-group members. I would like to discuss these two essential points, beginning with the second one.

Ethnic discrimination, according to van den Berghe, is the result of kin nepotism, the intensity of which is proportional to the kinship coefficient. The decline of the latter from close to distant kin is exponential ( $1/2$  for my full-siblings,  $1/4$  for the children of my siblings,  $1/8$  for my cousins, etc.). Since kinship and ethnicity are both claimed to be formed by means of differential granting of altruism (though in the case of ethnicity with respect to more distant relatives), the distinction between kin group and ethnic group calls for an explanation. Why is there usually a clear social boundary between the kin group and the ethnic group to which an individual belongs? Van den Berghe says nothing about this. Nepotism, or differential altruism, in this account, does not include an option that predicts the existence of relevant intraethnic boundaries. Some fellow ethnics are simply more closely related to a given individual than others.

As far as the existence of ethnic attitudes is concerned, the homeopathic theory of kin selection requires that ethnic—that is, cultural—boundaries correlate with genetic boundaries. In fact, it is claimed that ethnic discrimination is caused by nepotism operating on differently related individuals:

Ethnic groups, for nearly all of human history, were what geneticists call breeding populations, in-breeding superfamilies, in fact, which not only were much closer related to each other than to even their closest neighbors, but which, almost without exception, explicitly recognized this fact, and maintained clear territorial and social boundaries with other such ethnic groups. (van den Berghe, 1978b:404)

Reynolds (1980) argued that because of gene flow, different ethnic groups (or their precursors) were not significantly genetically separated. Salter (2001) replies that a genetic gradient is inevitable given the structure of groups such as hunter-gatherers. There are surely some differences in relatedness, and empirical studies are needed to assess how large they might be. The question remains whether the genetic gap is significant enough to account for differential behavior towards in-group and out-group members. My point, however, is somewhat different, because it does not depend on the extent of the genetic gradient.

One may grant that members of an in-group are more closely related to each other than they are to members of an out-group. But can this explain ethnic discrimination using a homeopathic framework? The problem is that van den Berghe claims nepotism to be proportional to the coefficient of relatedness, which declines exponentially. The average kinship coefficient of a hypothetical ethnic group might be about  $1/_{64}$ , while the one of the surrounding population might be  $1/_{256}$ . These hypothetical values might apply for ethnic groups of a certain size, but my argument is independent of this. The point is that the above difference—however large it might be—is much *smaller* than differences between close relatives. As the intensity of altruism is claimed to be proportional to the coefficient of biological relatedness, relevant differences in the spending of altruism are between an individual and his parents and full-siblings (1 compared to  $1/_{2}$ ) or between an individual and very close kin (1 compared to  $1/_{4}$ ), but surely not between distant and very distant kin. The boundaries of homeopathic nepotism are within families, but not between families or ethnic groups. For this reason, ethnic discrimination is a miracle for a homeopathic theory of nepotism.

Van den Berghe contents himself with declaring that there are differences of relatedness among different populations. He does not give an estimation of kinship or in-breeding coefficients, so that the mentioned problem due to the exponential decline of the coefficient of relatedness does not become apparent. Ethnic phenomena such as ethnocentrism often have not only rather clear, but also distinctive boundaries. Either you are accepted as a fellow ethnic, or you are not. This does not conform to homeopathic altruism. In addition, the theory predicts that a very small proportion of altruism is granted even towards out-group members. Van den Berghe does not explain the “sign change” of ethnicity: rather friendly attitudes towards in-

group members, but often mistrust or hostility towards strangers. Ethnicity involves qualitative differences with respect to in-group and out-group members. Homeopathic nepotism, on the contrary, simply declines in a continuous way. This behavioral disposition—in the manner it is postulated by van den Berghe—is not correlated with ethnic attitudes, so that it cannot be the essential mechanism that accounts for ethnicity.

I now turn to the other step in van den Berghe’s argument, the tenet that ethnic nepotism evolved by kin selection. It is of great importance to discuss this point, because this is the place where evolutionary biology enters the theory, that is, where ultimate mechanisms are included in the account. The central premise of the homeopathic theory is that the intensity of “kin selection”—that is, the propensity to act altruistically—is proportional to the coefficient of relatedness (van den Berghe, 1978a:45; 1978b:402; 1987:7, 19; 1995:360). Van den Berghe justifies this with reference to inclusive fitness theory, namely, as an implication of Hamilton’s formula:

The propensity to be “altruistic,” i.e. to contribute to alter’s fitness at the expense of ego’s fitness, is directly proportional not only to the coefficient of relatedness between ego and alter, but also to the benefit/cost ratio of the altruistic act. (1978b:402)

But wait a minute—is this true at all? Let  $r$  be the kinship coefficient of two individuals, let  $c$  be the cost of a specific altruistic act and  $b$  its benefit to the other individual. Other things being equal, Hamilton’s rule states that behaving altruistically is a better strategy than refraining from doing so whenever  $r > c/b$  (the above statement of van den Berghe is apparently due to this equation), or equivalently, if  $rb - c > 0$ , as van den Berghe correctly states (1978a:45; 1978b:402, 1979:14; 1987:20).<sup>3</sup> The sign of  $rb - c$  tells us whether the altruistic act under consideration will be selected for or against. The quantity  $rb - c$  might be used as a measure of the selection pressure. In this sense, the intensity of kin selection, an evolutionary mechanism, is dependent on  $r$  (though not in a proportional manner). But this does not mean at all that “kin selection” in van den Berghe’s sense, as a propensity to act altruistically, is proportional to  $r$ . This fundamental misinterpretation of Hamilton’s rule may be due to van den Berghe’s conflation of kin selection as an evolutionary mechanism with nepotism as a behavioral mechanism. Hamilton’s formula is not an *equality* that gives the degree of altruism with respect to the coefficient of relatedness (and the cost-benefit ratio). Instead, it is an *inequality* that states in which situation altruism (rather than selfishness) will evolve.

Why van den Berghe’s tenet is a fallacy, and why it is inconsistent with Hamilton’s formula, can be seen as follows. Whenever Hamilton’s rule is not satisfied, that is, if  $rb - c < 0$ , acting altruistically reduces inclusive fitness.

Thus, in this case, the best strategy is to refrain from being altruistic. Given constant cost and benefit, this applies for any individual that is distantly enough related (namely,  $r < c/b$ ). Due to the fact that the coefficient of relatedness declines exponentially, and thus very fast, this holds for nearly all individuals. As a simple example, consider sacrificing one's life for four other beings. This is adaptive when you save four brothers ( $r = 1/2$ ) and neutral when you save four nephews ( $r = 1/4$ ). But it is simply maladaptive to show this kind of behavior towards more distantly related individuals. Van den Berghe, on the contrary, claims that an individual should always invest a certain proportion of fitness in altruism (or act altruistically with a certain probability, namely  $r$ ). The homeopathic theory of nepotism maintains that a certain amount of altruism is in any case adaptive, no matter how distantly related the beneficiary of the altruistic act. This contradicts the theory of kin selection. Given a certain benefit for the recipient of an altruistic act, the loss of individual (classical Darwinian) fitness of the organism acting altruistically can only be compensated when the behavior is directed towards individuals that are closely enough related (or if we deal with reciprocal altruism).

In addition, when an individual has the possibility to act altruistically towards several other individuals of different relationship, the homeopathic theory entails that the best strategy consists in distributing altruistic actions according to the coefficients of relatedness (van den Berghe, 1978b:402; 1995:360). Or, as van den Berghe summarizes: "The biological golden rule is 'give unto others as they are related unto you'" (1987:20). This suggests that if you have a brother (kinship coefficient  $1/2$ ) and a nephew ( $r = 1/4$ ), the ratio of altruism spent towards your brother and nephew should be 2 : 1. However, the best altruistic strategy (given constant cost and benefit independent for different relatives) is to concentrate altruism on the closest relative, which is your brother in this case.<sup>4</sup> As other things are usually not equal, further relatives may profit by altruism as well. For example, an iterated altruistic act towards a brother might eventually bring no additional benefit for him. In this case, it might be recommended to spend altruism towards other relatives. Nevertheless, this holds only under specified circumstances. Furthermore, kin selection theory can account for altruism only towards close kin, and this empirical fact is included in Maynard Smith's definition (see Maynard Smith, 1964). Van den Berghe, on the contrary, postulates a mechanism of altruism that works also for extremely distant relatives. For these reasons, the homeopathic theory of kin selection cannot be salvaged.<sup>5</sup>

The above-mentioned two main points of van den Berghe's argument face fundamental difficulties. The homeopathic theory of nepotism gave van den Berghe's position some initial plausibility, because it would allow a kind of altruistic behavior that is not only restricted to close kin, but

to fellow ethnics as well. However, the starting point that kin selection accounts for homeopathic nepotism is untenable because it contradicts kin selection theory. Therefore, van den Berghe's theory completely breaks down. Any account that circumvents the mentioned problems can only be called a completely different theory.

Is it possible that the above-stated reconstruction of van den Berghe's argument is wrong, so that my criticism does not concern van den Berghe's theory at all? In my view, the discussion is not based on a misunderstanding, and there can be no doubt that the following points are essential for van den Berghe, as the above-given citations of and references to his repeated statements show. Van den Berghe offers a homeopathic theory of nepotism: kin selection (using Hamilton's formula) is claimed to be the evolutionary origin of this behavioral disposition, and nepotism explains ethnic behavior. This is exactly what I have focused on.

### Kin Selection and Genetic Similarity

Some other authors endorse van den Berghe's theory and use it as a starting point for the further development of an evolutionary account of ethnic attitudes. The political scientist Tatu Vanhanen (1999a; 1999b) derives from the sociobiological approach to ethnic nepotism two political hypotheses and tests them using data from several contemporary states. With respect to the evolutionary explanation of ethnic nepotism, Vanhanen simply restates van den Berghe's claims, namely, that ethnic nepotism depends on the genetic relatedness of individuals and could evolve because in-group members are more closely related to each other than they are to those in other groups. This is basically the homeopathy of kin selection:

Evolutionary theories of inclusive fitness and kin selection explain the evolutionary origin and universality of ethnic nepotism. The members of an ethnic group tend to favor their group members over non-members because they are more closely related to their group members than to outsiders. (Vanhanen 1999b:xiii, see also 1999a:57)

Unfortunately, Vanhanen does not offer more justification for this assertion. The problem of how homeopathic nepotism can evolve at all is not addressed. Although it is surely a better strategy to spend altruism towards a fellow ethnic than an outsider, altruism towards a member of one's ethnic group may be maladaptive as well. According to Hamilton's formula, the evolution of altruism (not reciprocal altruism) can usually occur only with respect to close relatives.

Political scientist and ethologist Frank Salter (2001) also defends van den Berghe's theory of ethnic nepotism. He replies successfully to several criticisms leveled against van den Berghe; however, he offers no analysis or discus-

sion of the evolutionary account of van den Berghe. The counterarguments that he assesses concern only the behavioral disposition of ethnic nepotism. For this reason, Salter does not address an essential aspect of van den Berghe's position. At the beginning of his discussion, Salter restates the main points of the defended theory, but the homeopathy of nepotism and its alleged evolutionary origin (namely kin selection), on which I focused, remain rather vague. Salter seems to accept van den Berghe's postulate that altruism and genetic distance are inversely proportional, that is, the homeopathy of kin selection.<sup>6</sup> Although he explicitly states in his conclusion that van den Berghe offers an evolutionary explanation of the proximate mechanisms involved in this account, he does not offer a discussion of this point, besides mentioning inclusive fitness. It is a pity that some authors content themselves with reference to some intuitions about the maximization of inclusive fitness in order to claim that a sociobiological explanation has been given. In addition, Salter and Vanhanen do not address the point that the boundaries of homeopathic nepotism are within families rather than between ethnic groups.

Up to now, the concept of genetic relatedness to which altruism and nepotism are proportional, according to Pierre van den Berghe, has been used without close attention to its definition. *Genetic relatedness* here means the kinship coefficient, that is, the probability with which two individuals share an allele by common descent. But consider two parents that are both homozygous for the same allele. Any two of their children must have this allele (in fact, be homozygous for it) and thus the same genotype (provided that no mutation occurs). That is, the genes of the two siblings at this locus are *identical in state*. But this does not mean that any two alleles will be *identical by descent*. In fact, the probability that an allele of the first sibling and a given allele of the second sib are a copy of a parental gene is  $1/2$ . Hamilton's model using the concept of inclusive fitness is about genes identical by descent. This is the reason why the kinship coefficients ( $1/2$ ,  $1/4$ , etc.) enter van den Berghe's theory of the degree of nepotism.

The *genetic similarity* of individuals in the sense of common genes identical in state is much higher among individuals. Even across species, many genes are shared. One approach that focuses on this property is the genetic similarity theory defended in particular by J. Philippe Rushton (see, for example, Rushton, 1995). This theory holds a core tenet in common with van den Berghe's position. Both authors argue that nepotism and the differential granting of altruistic acts is correlated with genetic relatedness and that this behavior, which has an adaptive evolutionary origin, explains ethnocentrism (due to the fact that in-group members are more closely related). The difference between the approaches is that in van den Berghe's theory genetic relatedness means kinship (the probability that two genes at a locus of two individuals are identical by descent), while Rushton's theory focuses on the genetic similarity of in-

dividuals (the overall amount of genes identical in state).<sup>7</sup> Despite the difference, some remarks by van den Berghe belong to genetic similarity theory rather than to an account based on kinship coefficients: "It [van den Berghe's framework] identifies nepotism based on proportion of shared genes as the basic mechanism of ethnic solidarity" (van den Berghe, 1986:250).

The core of genetic similarity theory is that an individual is able to detect its degree of genetic similarity to other individuals (using phenotypic clues) and then prefers more similar individuals over less similar ones. Genetic similarity is intended as a generalization of kin selection theory (Rushton, Russell, and Wells, 1984), and in this sense, it tries to circumvent the limitations of kin selection with respect to close kin. For instance, the general behavioral disposition postulated by genetic similarity theory is used to account for ethnocentrism (Rushton, 1995). The general claim of genetic similarity theory about a proximate mechanism (preferential behavior in accordance with similarity) is supposedly justified using empirical data (e.g., on the selection of spouses and friends). Several commentaries in the intensive discussion of Rushton (1989) criticize the intended interpretation of the given data, but I am not concerned with the question of whether individuals behave in correspondence with genetic similarity. Instead, I want to briefly discuss the evolutionary explanation of this alleged behavior, which the proponents of genetic similarity theory advance.

Rushton states the evolutionary explanation of preference according to genetic similarity theory as follows:

Rushton et al. (1984) proposed that, if a gene can better ensure its own survival by acting so as to bring about the reproduction of family members with whom it shares copies, then it can also do so by benefiting an organism in which copies of itself are to be found. This would be an alternative way for genes to propagate themselves. Rather than merely protecting kin at the expense of strangers, if organisms could identify genetically similar organisms, they could exhibit altruism toward these "strangers" as well as toward kin. Kin recognition would be just one form of genetic similarity detection. (Rushton, 1995:74)

There is a theoretically possible way for a gene to ensure that copies of itself (identical in state) spread by causing altruism that is directed not only towards kin. This could happen if this gene caused both a specific phenotypic trait and preferential behavior towards all individuals with that trait. However, while this is a possible evolutionary mechanism, it is usually considered highly unlikely, and no examples of it are known. Richard Dawkins (1976) named it the "green-beard effect" (a gene that causes a green beard and preferential behavior towards carriers of green beards), and has stated that "the green-beard effect is a kind of academic biological joke" (1987:206).

The proponents of genetic similarity theory favor, in particular, another mechanism that should give a gene the possibility to propagate copies of itself without restricting altruism to close kin. The above-cited argument appears to be sound if you only have a superficial understanding of the selfish-gene approach (according to which organisms are vehicles that are programmed to increase the number of copies of genes that are in them). On this reading, behaving preferentially towards a genetically similar individual would yield more copies of one's genes. However, the question is *how* a gene that causes such a behavior can evolve. This is clear from a correct understanding of the selfish-gene perspective, and exactly here lies the problem for genetic similarity theory. Note that it makes reference to *overall* genetic similarity; what Rushton and his colleagues tried to show is that humans treat other individuals preferentially according to their overall genetic similarity (based on measurements on several genetic markers).

Evolution by natural selection concerns change in the frequency of a specific allele at a given locus. In this sense, an allele competes with other alleles at this locus. It does not matter whether the effects of an allele increase the frequency of some alleles at other loci; a gene simply has to augment its own frequency to be evolutionarily successful. For this reason, an allele that influences an organism in a manner that causes this individual to behave altruistically towards other individuals that are genetically similar to it *with respect to other loci* is neither selected for nor against. But genetic similarity theory focuses on overall genetic similarity, which basically includes all these irrelevant genes or loci. Instead, the question should be whether a gene is able to detect (based on phenotypic effects) whether another organism also has this allele at the same locus. Preferential behavior toward an organism that has this allele is a better strategy than preferential treatment toward other organisms. But this scenario is simply the green-beard effect, which, as noted, is usually excluded as a real possibility. This criticism has already been put forward by other authors (e.g., Mealey, 1985). Rushton, however, has not been able to give an answer to this critical question. From remarks he has made in response to his critics, it is not clear to me whether he understands the problem.<sup>8</sup> Standard kin selection theory, however, is able to give an explanation for the evolution of altruism. When a gene causes altruism toward a relative, this relative has—with a determined probability—the same gene identical by descent, and a fortiori identical in state. This is a clear way in which an allele can benefit the same allele in another organism (at least with a certain probability). It does not invoke a green-beard effect or the irrelevant genetic similarity at other loci.

The second problem for the alleged evolutionary explanation of preferential behavior towards genetically similar individuals stems from the fact that the account does not include cost/benefit considerations. Even if preferring genetically *more* similar individuals (or *closer* relatives in the

case of van den Berghe) is a *better* strategy (other things being equal), it might nevertheless be maladaptive. Since altruistic behavior involves a cost for the individual, the cost must be compensated for the behavior to evolve (in the presence of egoistic rivals). The evolution of altruistic behavior necessarily depends on the cost, the benefit towards the other organism, and the relatedness to this organism. These critical points reveal a general drawback for genetic similarity theory. Its evolutionary scenario is not based on a quantitative model, but only on qualitative remarks about genes. In a quantitative model, the discussed point would become apparent; in particular, it would be clear whether the intended behavior can evolve. This is the strength of Hamilton's model based on the concept of inclusive fitness: the formula includes cost/benefit considerations.

The basic intuition of van den Berghe, Rushton, and their comrades is that an individual is genetically more related to its fellow ethnics than to outsiders, and that it therefore—according to sociobiological considerations—prefers the in-group members over out-group members. Richard Dawkins felt compelled to make a public statement because a group of organized racists used his name and similar sociobiological claims to try to justify their political position: “The equating of ‘kinship,’ in the sense of kin selection, with ‘ties of race’ appears to result from an interesting variant of what I have called the fifth misunderstanding of kin selection” (Dawkins, 1981:528). The fifth misunderstanding consists in failing to notice that kin selection is about the coefficient of relatedness, not about the total number of shared genes (Dawkins, 1979:190-92).

Van den Berghe and Rushton certainly do not want to further a racist view, but what they state as facts (an alleged evolutionary explanation) shares the same intuition that these racists used. This inference is simply—as it is put forward—a fallacy. In this approach, all behavior is basically adaptive, in particular the type of altruistic behavior that constitutes ethnic nepotism. I have tried to explain why altruism based on kin selection can usually be expected only towards close kin. True enough, it is an evolutionarily better strategy to spend beneficial behavior towards fellow ethnics than towards outsiders, because you are more closely related to them. But this fact as such does not indicate that this kind of behavior will evolve (rather than egoism or other behavioral patterns) independent of cost/benefit considerations. There is no reason to accept a version of van den Berghe's or Rushton's evolutionary scenario without a model that shows how such a behavior can evolve. Whether such a model reflects the real course of evolution, how big the average genetic difference between ethnic groups is, and how the postulated gradual differences in behavior towards fellow ethnics and outsiders can explain ethnocentrism are additional points that would need to be answered as well.

## **Ethnicity beyond Homeopathy**

There are approaches that include kin selection as an important mechanism in the evolution of human sociality, but which do not follow van den Berghe in suggesting that ethnic relationships evolved by the same mechanism, namely, that ethnicity is based on a commonality of genetic interests and that ethnocentrism is therefore adaptive. Gary Johnson (1986) develops a theory of patriotism that uses kin recognition mechanisms as links between genetic evolution and the socialization process. On this account, kin selection is an important ultimate mechanism that shaped the behavioral repertoire of hominid ancestors. The operation of kin altruism requires that individuals be able to recognize kin using available information. The mechanisms presented as probably the most important ones in humans are familiarity and phenotypic matching. These proximate mechanisms of kin recognition are still present in more recent societies. Altruistic acts towards non-kin can be elicited by using the cues for these behavioral dispositions. Patriotism originates by the exploitation of the mechanisms during the socialization process. In particular, patriotism need not be an adaptive behavior; there was no selection for it in the recent past. Instead, behavioral dispositions that were adaptive when they originated are now transformed during socialization.

Another account is the one by Irwin Silverman and Danielle Case (1998). They disagree with van den Berghe and Rushton in “maintaining that the influences of genetic relatedness in interpersonal relations are limited for the most part to direct kin,” since “ethnic nepotism would have been a maladaptive characteristic” (1998:390). Instead, it is proposed that selection would have favored behavioral dispositions that enable individuals to form the most effective alliances. Ethnic prejudices are seen as rationalizations, “means of preserving self-images of fidelity and fairness in the face of the perennial pursuit of situationally optimal affiliations” (1998:390). The authors agree with the claim that members of ethnic groups are often more closely related to each other than they are to those outside the group. However, they believe that group and alliance formation need not follow lines of genetic relatedness, but can change according to pragmatic considerations. Silverman and Case offer some empirical material that is intended to show that ethnocentrism is not limited to ethnic nepotism but rather reflects the pragmatic considerations of individuals (a discussion of their interpretation is beyond the scope of this article).

Although the two presented approaches advance different explanations, they restrict the evolution of altruism to kin and need not invoke a homeopathic theory of ethnic nepotism proportional to the kinship coefficient. For this reason, their selection scenario has much more plausibility than the ones by van den Berghe or Rushton. The account by Silverman and Case is especially interesting, because it tries to explain ethnic attitudes basically without reference

to altruism. Instead, it points to the importance of group structure and group formation in humans.

As the existence of altruism had been a serious problem for evolutionary theories relying on natural selection, it is no wonder that sociobiology stressed evolutionary mechanisms that can explain altruism, in particular the paradigmatic process of kin selection. Van den Berghe, who is inspired by the rise of sociobiology, regards kin selection as the main mechanism of animal and human sociality (see, e.g., 1987:239). In particular, his theory of ethnic nepotism is based on kin selection. As his second evolutionary mechanism is reciprocal altruism, van den Berghe offers a pan-altruistic image of sociality. This, however, ignores the traditional Darwinian mechanism of individual selection that explains all kinds of adaptive egoistic behavior. As in most social species, an individual is more closely related to its own offspring than to a relative's offspring, and there is usually a disposition towards an individual's own reproduction at the expense of that of its relatives. Being selfish is a good means to maximize one's inclusive fitness. This is the reason why Robin Dunbar (1997) states that mutualism is probably more important as a driving force behind the evolution of social groups in animals, and certainly in primates, than kin selection. Mutualism is a situation in which all individuals benefit from cooperation. Understood as symbiosis, it works even across species boundaries. Another important example of mutualism is group formation. Every animal in a group profits from this social structure because it yields better protection against predators. For this reason, mutualism—that is sheer egoism—is the main force in group formation, and it explains why many organisms live in groups at all. Van den Berghe endorses a mistaken picture of the evolutionary mechanisms of primate sociality.

Mutualism not only explains the mere origin of groups, but it is also probably the cause of several other important adaptations to social life. Not every kind of social behavior must be altruistic (in the sociobiological sense). Possible examples include adaptations that enable hominids of a group to cooperate in collective hunting or tasks that can only be performed if different individuals do not do the same thing at the same time but adapt their actions in accordance with what their fellows do. Predators were a threat to early human groups, but, at a later period, individual groups might have also competed with each other to a relevant extent. This suggests a possibility important for the theory of Silverman and Case. They postulate adaptations to form the most effective alliances according to perceived circumstances. The evolution of such behavior would be due to mutualism benefiting each member of the alliance to a certain degree. In addition, reciprocal altruism makes the evolution of altruistic behavior directed towards non-kin possible. As humans obtained high cognitive capacities, this mechanism was probably a source of the capacity to engage in various social agreements that

involve temporary costs for one individual. In my view, there is some plausibility that mutualism and reciprocal altruism had a greater influence on human sociality than kin selection.

During the period when hominids usually lived in kin groups, the evolution of adaptations for social living was enhanced by kin selection, because altruistic social behavior was directed to kin members, increasing one's inclusive fitness. Nonetheless, the possibility cannot be excluded that several of these behavioral dispositions would have evolved if the groups had not been composed of relatives, implicating kin selection as a factor, but not always the important one. At any rate, when bigger groups emerged that consequently were not composed of close relatives, behavior involving all members of the group could still evolve by mutualism or reciprocal altruism.

Independent of the concrete explanation of ethnicity, several authors acknowledge that it is important to include human group structure into one's account (see, e.g., Shaw and Wong, 1989; Salter, 2001). David Goetze (1998) argues that contemporary ethnic groups—characterized by a high degree of mobility, thereby undermining traditional kin groups—are basically the consequence of kinship as well as functionality considerations. Both factors influence a human being's decision about which ethnic groups to join. Goetze points to the strong human tendency to form groups and compete with other groups, which is often independent of the similarity of the group members. Group formation based on kin recognition is only one factor. In addition, many resources important for humans can only be obtained and defended through cooperation in large groups.

Jan and Birgitta Tullberg (1997) also emphasize the disposition to form in-group alliances, even when splitting into groups is arbitrary. However, ethnocentrism is not considered to be based on kin selection. Instead, it is a special case of group egoism, which is based on individual advantage. The function of group egoism is to form alliances that are able to compete with other groups. A group identity is formed because individuals of larger groups do not know each other well enough. Group egoism, and particularly ethnocentrism, is regarded as collectivistic and dichotomous, whereas kin selection, which is individualistic and differentiated, is a separate phenomenon. The idea that ethnocentrism is simply extended kin interest is criticized on the ground that humans are able to distinguish whether a person is actually a close relative or whether kin terms are utilized to appear related.

Robert Boyd and Peter Richerson (1985) offer their own approach to the question of why cooperation among large groups of unrelated individuals has not been observed except in humans (see also Richerson and Boyd, 1998). These authors, who do not even mention inclusive fitness theory, point to the fact that the explanation of ethnocentrism as rational self-interest is especially difficult

when group size increases. Their solution is a quantitative model that combines evolution and cultural transmission. It is an attempt to model phenomena like ethnocentrism by means of a sort of "cultural group selection."

## Discussion

An important lesson clearly emerges from van den Berghe's work: one must keep proximate and ultimate causes distinct. It is one thing to detect a behavioral disposition in a population of individuals, but quite another to determine whether a particular evolutionary mechanism produced this behavior. For a complete evolutionary approach to ethnocentrism, there are two explanatory issues at stake: how to explain this ethnic phenomenon successfully by existing proximate mechanisms (e.g., ethnic nepotism), and how to account for the adaptive origin of these behavioral dispositions. Only facts can be explained. It is not necessary to explain why the coelacanth became extinct; in fact, it cannot be explained, because this alleged fact does not obtain. That is, before being able to correctly explain the evolutionary origin of a behavioral disposition, it must be shown that this proximate mechanism actually exists, which is an empirical task. Van den Berghe, on the contrary, conflates ultimate and proximate mechanisms by using the term *kin selection* for the behavioral disposition of ethnic nepotism. This indeed relieved him of the necessity to give an evolutionary explanation for ethnic nepotism.

As I have shown, what van den Berghe calls "kin selection," homeopathic nepotism, cannot be explained by kin selection. Moreover, he simply postulated (homeopathic) ethnic nepotism based on an alleged prediction from sociobiology. He did not empirically verify that this behavioral disposition actually exists. It remains to be shown that people behave according to homeopathic nepotism. Van den Berghe believes that he has given an evolutionary explanation for something that we do not know conclusively even exists. In addition, he claims that he has explained ethnic attitudes by this behavioral disposition. But a situation can be explained only by invoking causes that really exist. Thus, it is the wrong strategy to fill the important gaps in one's account of proximate mechanisms by referring to behavioral dispositions that are to be expected from evolutionary theory. Instead, one must empirically detect the proximate mechanisms, the probable existence of which is motivated by evolutionary theory.

In addition, there is a fundamental difference between prediction and explanation. Assume, for instance, that in tossing an (unfair) coin one thousand times, heads always obtained. This astonishing correlation yields a justified prediction that the next time heads will also obtain. But when heads is actually obtained on the one-thousand-and-first toss, the explanation for that phenomenon cannot be the fact that heads obtained on the first one thousand tosses. Correlations can be used to make predictions, but one

must find the relevant causal mechanism in order to give an explanation. When a prediction derived from a theory is empirically verified, support is lent to the theory. But this does not mean that there is sufficient evidence that the causal mechanisms of that theory (provided that the theory is about causes instead of correlations) are the right ones. It would be true only to the extent that rival explanatory attempts are excluded by the account.

These considerations are the reason why I do not accept Vanhanen's (1999b) claim that he has explained ethnic conflicts by ethnic nepotism. He simply made predictions about ethnic conflict based on his understanding of ethnic nepotism, and then verified his prediction. But this does not—as he maintains—provide an explanation. He has not yet given an account of how ethnic nepotism and other proximate mechanisms give rise to specific ethnic conflicts.

Similarly, Rushton offers data that might show that preferential behavior is *correlated* with genetic similarity. But he claims, in addition, that there is a *specific causal relationship* between two variables: "people detect genetic similarity in others in order to give preferential treatment to those who are most similar to themselves" (Rushton, 1989:503). However, Rushton does not offer any evidence for this interpretation. Instead, this is taken for granted based on a probably flawed prediction from evolutionary theory. For instance, assortative behavior according to genetic similarity might as well be the consequence of cooperation and competition in social groups. Nevertheless, making predictions is an important task. It may be a heuristic tool for finding plausible hypotheses. In fact, considerations about inclusive fitness motivated a lot of interesting hypotheses that might have been undiscovered without this approach. Although confirming predictions reveals that one is probably on the right track, an explanation must finally be provided, as it was rigorously done in many cases of animal altruism.

One and the same phenomenon may be explained by both proximate mechanisms and ultimate mechanisms. These are actually two different types or levels of explanation. For instance, to explain why individuals of a species show a specific kind of behavior in a certain situation, a researcher might, on the one hand, find mechanisms that trigger this behavior given certain environmental stimuli. This kind of explanation is complete in itself, for the relevant proximate causes have been stated. Another question is why this type of behavior evolved, which calls for ultimate causes and a different account.

These two types of explanations are in principle independent of each other. Providing a proximate explanation does not require that one also provide an ultimate explanation. The fact that an explanation by proximate mechanisms is possible does not mean that an adaptive evolutionary one is possible. Proximate mechanisms (e.g., physiological ones) may be specific for a single individual, but an explanation by selective mechanism requires that a certain proportion

of the whole population exhibit this type of behavior. In addition, it is even possible to separate different types of explanations by proximate mechanisms. First, there are explanations that make reference to triggering causes (for example, physiological or behavioral ones). Second, there are ontogenetic accounts that focus on the developmental origin of behavioral patterns. Finally, there are teleological explanations that reference the function of a structure. Although there is certainly overlap between these levels, they can be distinguished for theoretical purposes. Knowing what stimuli triggered a certain behavior does not predispose one to a specific answer about how this constellation originated in the ontogeny of an organism.

With respect to the explanation of human social behavior, there emerge even more types of explanation. Some of them might be called "psychological," and others "social." Some might explain human conduct with reference to desires and intentions. Such explanations involve neither ultimate nor genetic causes. As well, it is possible to give an adequate cultural explanation of a specific social feature without reference to evolutionary explanations, namely, by taking biological and psychological properties as given (as is done in the case of a physiological biological explanation). Different types of explanation simply address different theoretical levels or different theoretical aspects and focus on one set of causes. For this reason, van den Berghe is wrong in claiming that a cultural explanation without an evolutionary one amounts to creating separate realms of nature and nurture (1986:257). He also states that explaining universal human traits by invoking culture begs the question, because culture is only a proximate cause (1978a:405).

I have already explained why a proximate explanation is a complete explanation, which can be complemented—but need not be completed—by an evolutionary account. A universal feature of human sociality might also admit of a historical explanation that makes no reference to evolutionary causes. This type of explanation would use mechanisms that operated long before the emergence of the situation, similar to an evolutionary explanation. Van den Berghe's insistence on a selection explanation amounts to the tenet that it is the only possible or admissible type of explanation. In biology, proximate and ultimate causes simply refer to different types of explanation, and there is no contention that only evolutionary explanations have ultimate truth, while other explanations do not.

The statement that a trait evolved because of selection may have two meanings. It may refer to the fact that there was *selection for* having this trait, i.e., the property of having this trait was a selective advantage, so that the trait was a cause of selection. But the statement could also mean that there was *selection of* this trait in the sense that it is the product of a selection process. For instance, there was selection for having a thick and thus warm coat in polar bears (and also selection of thick coats). Since a thick coat

is also a heavy one (for developmental reasons), there was selection of heavy coats (but, of course, no selection for being heavy). Only in the first case are we dealing with an adaptation. The second case is about the consequences of adaptive processes.<sup>9</sup> Of course, not every trait must have evolved because of selection, for there are other evolutionary processes, such as mutation and genetic drift. An evolutionary explanation is not restricted to a selective one. In addition, a behavioral trait may have been adaptive in the past but is no longer so in its present environment. Certain human behaviors may simply be the epiphenomena of psychological dispositions (that may have an adaptive history) according to environmental circumstances.<sup>10</sup>

It is often necessary to distinguish between the origin of a trait and its maintenance or further modification. For instance, the purpose of insect wings is the ability to fly. However, for reasons of developmental genetics, the wings had to develop from small appendages that surely did not confer any advantage with respect to flight. But these appendages were adaptive with respect to thermoregulation. Only later on did they have a size that conferred an advantage for flying or gliding to their possessors.

The maintenance of a trait is often easily explained by stabilizing selection. Speech in humans is an example. In a group in which people make use of speech, it is selectively disadvantageous to have reduced language capacities. Many features of life in social groups can be selectively maintained, because the individual gains benefit from them (individual selection). But it is another question how this trait emerged. Why did the first person with the beginnings of some language capacities have a selective advantage?

In van den Berghe's approach, individuals are programmed to maximize their (inclusive) fitness. Several behavioral dispositions discussed by van den Berghe, including ethnocentrism, are seen as the expression of the general tendency to maximize one's fitness. However, organisms are not programmed to behave in a fitness-maximizing way whatever the environment may be. Instead, a certain number of *distinct* behavioral dispositions evolved because they contributed to fitness at that time. If one behavioral disposition has been sociobiologically explained, the fact that another disposition is found that today probably contributes to the fitness of an individual does not entail that this disposition is an adaptation as well. Instead, a new evolutionary explanation for this trait has to be given as in the first case.

Johnson states in his account: "Thus, we have good reason to believe that kin selection has operated on our forebears (both distant and near), and that we therefore retain a genetically-based capacity for altruism" (1986:129). The two mentioned items are, however, two different points. Because of kin selection, we may expect some altruistic behavioral dispositions. But the question is *what* altruistic behavioral patterns. General altruistic behavior surely

did not evolve. The task is to identify and explain these dispositions one-by-one. Moreover, the term *altruism* as used in sociobiology has a specific meaning. It does not refer to what in social contexts is called altruism. Instead, it is the increase in another's fitness *at the expense of one's own fitness*. Evolutionary approaches to ethnicity that rely on altruism have to show that the disposition they want to explain was actually altruistic at the time it evolved. For instance, in times of peace it does not cost anything to be a patriot who declares that he would sacrifice his life for his country. That is, it is not self-evident that the broad behavioral pattern of patriotism is altruistic.

A further general point is that explanatory attempts have to meet scientific criteria before one is justified in accepting them. For instance, the claim that a certain type of behavior increases the fitness of an individual has to be substantiated. To take an example that does not involve social behavior, the above-mentioned explanation of how insect wings evolved was experimentally verified. Using engineering methods, the thermoregulatory as well as aerodynamic properties of artificial wings of different shapes and sizes were studied. It is these data that make the given explanation a scientific one that can be clearly kept apart from mere plausible stories of the evolutionary origin of morphological structures. Philip Kitcher (1985) has elaborated this topic in a masterful way. Based on examples, he distinguishes rigorous sociobiology from what he calls pop sociobiology, which does not meet the scientific standards of the former.

Most behavioral and evolutionary studies in biology are cases of serious sociobiology, which employ evidence from the field or laboratory, usually with respect to animal behavior. Pop sociobiology can often be found among accounts of human nature, where the relevant evidence is not supplied. Elisabeth Lloyd (1999) discusses the approach of the evolutionary psychologists Leda Cosmides and John Tooby, which tries to substantiate evolutionary "social contract theory" by empirical data on how people perform the Wason selection task. Lloyd argues that the experimental data are not able to rule out the rival "pragmatic reasoning schemas theory" about psychological mechanisms. Instead, evolutionary considerations are invoked by Cosmides and Tooby to convince (or rather persuade) the reader that their version of which proximate mechanisms actually obtain is the right one. In addition, no evidence is given for their evolutionary account. This is a pity, because this study is important for several evolutionary psychologists, and because evolutionary psychologists base their claims about proximate mechanisms on experimental data, usually trying to avoid standard pitfalls for evolutionary approaches, knowing what must be shown for a putative adaptive claim.

My own view about the evolutionary origin of ethnocentrism is that highly plausible reasons have not yet been advanced to show that ethnocentrism is an adaptation, as

the strong accounts of van den Berghe or Rushton maintain. Instead, I believe that ethnocentrism was not and is not adaptive. My position is based on the recognition that early groups of hominids probably did not interact with each other to a relevant extent. While this lack of contact between groups is used by the proponents of ethnic nepotism accounts to justify that hominid groups are genetically separated, the limited interaction makes it unlikely that ethnocentric attitudes were of selective advantage. Since groups did not often meet, there was no need for ethnocentrism; this behavioral disposition would not, therefore, confer a higher fitness on ethnocentric individuals. From this reasoning, I cannot accept that ethnocentrism and related phenomena are adaptive behavioral strategies.

In conclusion, evidence on this subject still needs to be elaborated. This is also true for the proximate mechanisms involved in the formation of ethnicity. Several questions need to be answered in more detail. For instance, which psychological properties make up ethnocentrism? How do emotive aspects and cognitive or language-based ones intergrade? How do they link to action, and how do ethnic identity and ethnic mobilization influence each other? What is the relationship of social structures and psychological dispositions? What phenomena are the causes of ethnocentrism, and what are, rather, its effects? Is there basically one type of behavioral mechanism—for example, nepotism—that generates ethnicity, or are ethnic attitudes constituted by the interaction of several psychological mechanisms of ethnicity? Are those mechanisms rather specific ones relating to ethnicity alone, or are they attitudes that influence several social phenomena?

My suggestion is that these questions must be answered before a real evolutionary explanation of the corresponding mechanisms can be given.

## Acknowledgments

I am indebted to Alan Love, Peter McLaughlin, Gereon Wolters, Bernhard Kleeberg, and Michael Kempe for helpful discussions on my work.

## Notes

1. The term *kin selection* was introduced by John Maynard Smith (1964) for the evolution of altruistic behavior towards close kin (based on the model of Hamilton [1964] introducing the idea of inclusive fitness). Interestingly enough, this is the source to which van den Berghe refers. The meaning of kin selection as an evolutionary mechanism has been maintained since then. See, for example, Ridley, 1997.
2. That there is no contradiction between the fact that an evolutionary account must deal with biological kinship and the fact that kinship as well as ethnic membership is often fictive or constructed was explicitly pointed out by van den Berghe (1995), as well as by Salter (2001). Biological and perceived kinship simply have to overlap to a high degree.

3. In what follows, the notion of a “better strategy” is to be understood in the sense of evolutionary game theory, that is, one strategy is better than another if it will be favored by natural selection, which does not mean that the behavior is better for the individual itself. My discussion of “a better strategy,” and that it is “better to behave” in a certain manner, also has nothing to do with ethical or political evaluation of how an individual ought to behave.
4. This is reminiscent of the following notorious fallacy. Assume that you have a certain amount of money that you can stake on different persons (you may distribute it over several persons) and that you obtain a fixed sum if your bet was right. If you may choose among a lot of people, the person with the highest chances might win with a probability of only 10%, whereas the chances of other candidates might be 5% to 8%. If you choose this person, you will lose your money with a probability of 90%. This might tempt you to distribute your money and stake on other persons as well, because one of them might win. However, the best strategy is simply to stake your money on the candidate with the best chances. The homeopathic theory of altruism exactly parallels this fallacy.
5. Van den Berghe’s article (1978a), in which it is again stated that kin selection is a behavioral mechanism and that its intensity is proportional to the kinship coefficient, was reviewed before publication by Edward O. Wilson. Seemingly, Wilson had no serious problems with van den Berghe’s statement, which is—as just shown—inconsistent with kin selection theory. As I learned during the revision of this paper, van den Berghe is not the first one to commit the discussed fallacy. Mealey (1985), who discusses Rushton’s genetic similarity theory, drew my attention to Dawkins (1979). In this extremely useful article, several misunderstandings of kin selection are discussed. Dawkins included the misunderstanding “An animal is expected to dole out to each relative an amount of altruism proportional to the coefficient of relatedness,” because Altmann (1979) criticized some assumptions by sociobiologists that amount to this fallacy. These biologists did not base an explanation or a theory on this misunderstanding, but they measured frequencies of altruistic behavior, due to the alleged prediction from kin selection theory that these frequencies are correlated with the kinship coefficients. (Like me, Altmann compared this line of thought with the fallacy of distributing stakes according to odds, presented in the foregoing note. So I am not the first one to detect this kind of fallacy in the application of Hamilton’s formula.)
6. See the discussion of the criticism of emphasis on violent emotion and instinct in Salter (2001).
7. Because of this notable difference, I do not agree with Vanhanen (1999b), who states that Rushton’s genetic similarity theory complements van den Berghe’s theory of ethnic nepotism.
8. For instance, Rushton replied to his critics that he does not claim that a single gene brings about a kind of green-beard effect, but that there are groups of genes that cause phenotypic traits as well as preferential behavior towards these traits. But the latter is exactly what his critics called into question. Several alleles at different loci (e.g., new mutations) might be separated during meiosis. For this reason, the question remains: How does a gene, or a group of genetically linked genes that cause a trait (as well as the corresponding preferential behavior), evolve?
9. The distinction between *selection for* and *selection of* was introduced by Sober (1984). Although this terminology is not used among evolutionary biologists, I consider it to be very useful.
10. According to van den Berghe, every universal trait can be presumed to have a genetic basis (1979:6). That this is simply a fallacy is made clear by an example of Daniel Dennett (1995:486). He points to the fact that in every culture hunters throw their spears pointy-end-first, though there is no pointy-end-first gene. Instead, this behavioral pattern results from general human problem-solving capacities, which Dennett takes to be of adaptive origin.

## References

- Altmann, S.A. (1979). "Altruistic Behavior: The Fallacy of Kin Deployment." *Animal Behavior* 27:958–59.
- Boyd, R. and P.J. Richerson (1985). *Culture and the Evolutionary Process*. Chicago: University of Chicago Press.
- Dawkins, R. (1976). *The Selfish Gene*. New York: Oxford University Press.
- Dawkins, R. (1979). "Twelve Misunderstandings of Kin Selection." *Zeitschrift fuer Tierpsychologie* 51:184–200.
- Dawkins, R. (1981). "Selfish Genes in Race or Politics." *Nature* 289:528.
- Dawkins, R. (1987). *The Blind Watchmaker: Why the Evidence of Evolution Reveals a Universe Without Design*. New York: W.W. Norton.
- Dennett, D.C. (1995). *Darwin's Dangerous Idea: Evolution and the Meanings of Life*. London: Penguin.
- Dunbar, R.I.M. (1997). "Sociality among Human and Non-Human Animals." In T. Ingold (ed.), *Companion Encyclopedia of Anthropology*. London: Routledge.
- Goetze, D. (1998). "Evolution, Mobility, and Ethnic Group Formation." *Politics and the Life Sciences* 17:59–72.
- Hamilton, W.D. (1964). "The Genetical Evolution of Social Behavior. I." *Journal of Theoretical Biology* 7:1–16.
- Horowitz, D.L. (1985). *Ethnic Groups in Conflict*. Berkeley: University of California Press.
- Johnson, G.R. (1986). "Kin Selection, Socialization, and Patriotism: An Integrating Theory" [with commentaries and response]. *Politics and the Life Sciences* 4:127–54.
- Kitcher, P. (1985). *Vaulting Ambition: Sociobiology and the Quest for Human Nature*. Cambridge, MA: MIT Press.
- Lloyd, E.A. (1999). "Evolutionary Psychology: The Burdens of Proof." *Biology & Philosophy* 14:211–33.
- Maynard Smith, J. (1964). "Group Selection and Kin Selection." *Nature* 201:1145–47.
- Mealey, L. (1985). "Comment on Genetic Similarity Theory." *Behavior Genetics* 15:571–74.
- Reynolds, V. (1980). "Sociobiology and the Idea of Primordial Discrimination." *Ethnic and Racial Studies* 3:303–15.
- Richerson, P.J. and R. Boyd (1998) "The Evolution of Human Ultrasociality." In I. Eibl-Eibesfeldt and F.K. Salter (eds.), *Indoctrinability, Ideology, and Warfare: Evolutionary Perspectives*. New York: Berghahn Books.
- Ridley, M. (1997). *Evolution*. Boston: Blackwell Science.
- Rushton, J.P. (1989). "Genetic Similarity, Human Altruism, and Group Selection" [with commentaries and response]. *Behavioral and Brain Sciences* 12:503–59.
- Rushton, J.P. (1995). *Race, Evolution, and Behavior: A Life History Perspective*. New Brunswick: Transaction Publishers.
- Rushton, J.P., R.J.H. Russell, and P.A. Wells (1984). "Genetic Similarity Theory: Beyond Kin Selection." *Behavior Genetics* 14:179–97.
- Salter, F.K. (2001). "A Defense and Extension of Pierre van den Berghe's Theory of Ethnic Nepotism." In P. James and D. Goetze (eds.), *Evolutionary Theory and Ethnic Conflict*. Westport, CT: Praeger.
- Shaw, R.P. and Y. Wong (1989). *The Genetic Seeds of Warfare*. Boston: Unwin Hyman.
- Silverman, I. and D. Case (1998). "Ethnocentrism vs. Pragmatism in the Conduct of Human Affairs." In I. Eibl-Eibesfeldt and F.K. Salter (eds.), *Indoctrinability, Ideology, and Warfare: Evolutionary Perspectives*. New York: Berghahn Books.
- Sober, E. (1984). *The Nature of Selection: Evolutionary Theory in Philosophical Focus*. Cambridge, MA: MIT Press.
- Tullberg, J. and B. Tullberg (1997). "Separation or Unity? A Model for Solving Ethnic Conflicts." *Politics and the Life Sciences* 16:237–48.
- van den Berghe, P.L. (1978a). "Bridging the Paradigms. Biology and the Social Sciences." In M.S. Gregory, A. Silvers, and D. Sutch (eds.), *Sociobiology and Human Nature: An Interdisciplinary Critique and Defense*. San Francisco: Jossey-Bass.
- van den Berghe, P.L. (1978b). "Race and Ethnicity: A Sociobiological Perspective." *Ethnic and Racial Studies* 1:401–11.
- van den Berghe, P.L. (1979). *Human Family Systems: An Evolutionary View*. New York: Elsevier.
- van den Berghe, P.L. (1986). "Ethnicity and the Sociobiology Debate." In J. Rex and D. Mason (eds.), *Theories of Race and Ethnic Relations*. Cambridge: Cambridge University Press.
- van den Berghe, P.L. (1987). *The Ethnic Phenomenon*. New York: Praeger.
- van den Berghe, P.L. (1995). "Does Race Matter?" *Nations and Nationalism* 1:359–68.
- Vanhanen, T. (1999a). "Domestic Ethnic Conflict and Ethnic Nepotism: A Comparative Analysis." *Journal of Peace Research* 36:55–73.
- Vanhanen, T. (1999b). *Ethnic Conflicts Explained by Ethnic Nepotism*. Stamford: JAI Press.

## COMMENTARY

# Population and the Demise of Cheap Energy

Andrew R. B. Ferguson

Optimum Population Trust, United Kingdom

**Abstract.** Standard of living is determined in part by the availability of ecological resources, and in part by the availability of “cheap energy.” The demise of cheap energy is likely to occur during the twenty-first century. Gradually, over this time, humans will be restricted to using renewable natural capital. Reliance on renewable energy sources will require a reduction in population to within the range of 1.5 to 3 billion people in order to maintain what most of us would regard as an adequate lifestyle. Attention is drawn to the need for nations to exercise sovereign responsibility.

In September 1997, *Politics and the Life Sciences* published a target essay, by J. Kenneth Smail, titled “Beyond Population Stabilization: The Case for Dramatically Reducing Global Human Numbers.” In accordance with the usual roundtable format, this was accompanied by commentaries, with a final response by Smail, which he titled, “Population Growth Seems to Affect Everything But Is Seldom Held Responsible for Anything.” It was a premise of Smail’s papers, that a *sustainable* world population would be around 2 billion. Although the discussion papers only occasionally challenged this premise, its veracity is of fundamental importance. A main purpose of this essay is to establish the truth of the premise beyond reasonable doubt.

“Cheap energy,” I should first point out, is merely a shorthand for easily available energy. It is energy that can be acquired for little effort. For example, many third world women spend hours a day collecting enough firewood to provide for their small daily energy usage. In the sense intended here, they do not have “cheap energy,” even though they may pay nothing for the wood. On the other hand, consider a typical British lawyer who, say, throughout an eight-hour working day wishes to keep alight a two-bar electric fire. Forty-five seconds of work is sufficient to pay for that privilege. While the power of lawyers to extort money is a partial explanation for the relative cheapness of this energy, the main explanation is the ease with which fossil energy can be acquired.

During the twenty-first century, as I shall show, cheap energy will likely disappear. In planning for this, there is no time to waste; and since adjusting population size to stay within carrying capacity is a long-term project, we particularly need to study the effect that the demise of cheap energy will have on carrying capacity. An important determinant of carrying capacity is food production, so let us start by considering the benefits we have enjoyed in that regard while cheap energy has been available.

Cheap energy

- facilitates the production, transport, and application of fertilizers;

---

Andrew R.B. Ferguson is Research Coordinator at Optimum Population Trust, 11 Harcourt Close, Henley-on-Thames, RG9 1UZ, United Kingdom (e-mail: andrewrbferguson@hotmail.com). He is also editor of the biannual *OPT Journal*. His special interests include applying eco-footprinting to estimating national carrying capacities, assessing the net energy capture of renewable energy sources, and raising awareness of the existence of Clive Ponting’s seminal book, *A Green History of the World* (1991). In an earlier stage of his career, he was an airline pilot (1957-1983).

- allows fresh food to be transported quickly to consumers, and other food to be processed and preserved, thus minimizing wastage;
- can be used to build dams and pump water to provide irrigation;
- allows the use of tractors so that plowing, sowing, and harvesting can be carried out at the most appropriate times;
- facilitates the rescue of livestock from floods and droughts by transporting them to safer areas;
- allows crops to be grown over extensive areas where rainfall is uncertain and failure is a not infrequent occurrence. That is to say, because manpower is not a limiting factor, and because food can be transported to drought areas, *intermittent* large scale crop failure can be tolerated;
- frees up land for crops and pasture that otherwise would be needed to grow trees to provide fuel;
- allows the use of tractors, which permits land that would otherwise be used for maintaining draft animals to be used for other purposes;
- allows the use of trucks and motorized transport to collect fuel-wood, paper, building materials, general commodities, and to provide personal transport, all of which would otherwise need land to maintain draft animals.

Perhaps it helps to know that in 1900 about a quarter of U.S. cropland was used to provide horse feed (Ponting, 1992:274), and that in 1920, according to USDA figures, there was one draft animal per five persons. So does that list bring a clear message that we need to reduce population to the level that could be supported without cheap energy? It would, except that most people are under the impression that there are other ways out of the difficulty, namely: (a) when fossil fuels are exhausted we can turn to nuclear energy in the form of fission or fusion reactors; (b) fossil fuels will be available for so far into the future that there is no cause for immediate action; or (c) renewable energy sources will replace fossil fuels.

### The Nuclear Energy “Solution”

Despite the accidents at Windscale, Three Mile Island, and Chernobyl, nuclear power might be considered a possible energy source. There are several problems, however, with this potential solution. *The Ecologist* produced a two volume, 1,300-page report, called *Nuclear Power, Shut it Down!* that covers most of the ground (Aubrey, Grünberg, and Hildyard, 1991-93). Also helpful is a well-balanced picture from a long-serving scientist in the nuclear industry, Fred Roberts, *Sixty Years of Nuclear History* (1999). The book serves as a reminder that we need to judge not what *might* be done to ensure safety, but what is *likely* to be done, taking human frailty into account. Uncertainty

about the prerequisite of maintaining adequate civil order in society, over the long term, is well covered by Hardin (1993:156-9).

Apart from the dangers of widespread radioactive pollution, cost may be a limiting factor. In the United Kingdom and United States, at least, insurance is virtually impossible.<sup>1</sup> The full cost of nuclear power, including the storing of waste and the decommissioning of the power stations, has yet to be established. When this full energy cost is taken into account, the input/output ratio for nuclear power is likely to be even worse than the 1:5 figure quoted in Pimentel and Pimentel (1996:206); and without fossil fuels to provide the energy input, dollar costs may well become prohibitive.

Nuclear power is not a renewable source. In his book *The Decline of Oil*, Brian Fleay (1995:65), citing Gever et al. (1991), gives 1:4 as the input/output ratio for nuclear energy (lightwater reactor), pointing out that this figure does not include decommissioning or safe disposal of the nuclear waste. He continues, “U235 as the active component in uranium is too dilute to fission, being only 0.7 per cent of the metal. The supply of high-grade uranium ores is limited and any substantial increase in the use of conventional nuclear power will quickly consume these.”

For a brief appraisal of the future of *fusion* power, one cannot do better than consult the three-page paper, “Fusion, An Illusion or a Practical Source of Energy” by nuclear physicist Albert A. Bartlett (1994). Key points, in addition to the uncertainty of economic viability, are first that fusion power is not the “clean” energy source that it is sometimes made out to be. Bartlett says that because of the neutrons emitted during the fusion process, “The vessels and associated structures will become radioactive and this radioactivity will persist for several generations.” Secondly, the amount of heat released during the process may be an insuperable problem (it is a problem for fission power).

### The More Fossil Fuels “Solution”

Leaving the reader to pursue deeper enquiries into nuclear energy, let us turn to the possibility that we can continue to rely on fossil fuels. In a yet-to-be-published paper, petroleum geologist Jean Laherrère produces graphs for combined oil and gas production, which show that by 2060 production will have halved, and by 2100 it will be only 20% of the current rate. However, in such an important matter, we need to get to grips with the details, so let us examine nine reasons why we should expect that fossil fuels will not be freely available through the twenty-first century.

1. Ivanhoe, 1996, says: “It is concluded that the critical date, per USGS discovery records, when global oil demand will exceed world production, will fall sometime between 2000 and 2010.” More recently, in 1998, the International Energy Agency finally took the plunge

and made a forecast that the likely peak would occur between 2010 and 2020. However, precise timing is not important. The oil company Conoco estimates that within a decade crude oil demand will rise to about 30 billion barrels a year (Youngquist, 1997:200). Total remaining world oil as of 1995, including both that discovered *and that which it is estimated remains to be discovered*, was estimated at 1,512 billion barrels (Youngquist, 1997:170). Even if we make no allowance for further increases in consumption beyond 30 billion barrels a year (although such increases are likely), total oil would last  $1,512 / 30 = 50$  years. More realistically, it would drop to about half after 50 years.<sup>2</sup> Gradually, energy demands will be transferred to natural gas and coal. We need, therefore, to consider the prospects for gas and coal.

Total world gas remaining is estimated at 10,340 trillion cubic feet. Note that is the likely *absolute total*, not just the proved reserves, which amount to only 4,710 trillion cubic feet. 5,600 cubic feet of gas has the energy equivalent of a barrel of oil. (Youngquist, 1997:193-95). The energy available as gas can therefore be estimated as 1,850 billion barrels of oil equivalent. Worldwide, natural gas consumption is expected to continue to grow at the rate of 2.5 percent annually, compared with oil's 1.5 to 2 percent growth (Youngquist, 1997:195). At a 2.5% growth rate, consumption would double in 28 years, from its present level of 14 billion barrels of oil equivalent a year (Youngquist, 1997:266). Making allowance for such growth, and considering the situation when gas has to largely take over for oil, an annual consumption of gas can be guesstimated at 44 billion barrels of oil equivalents per year, double the 1995 rate of oil consumption (22 billion barrels a year). Thus, all the gas that it is anticipated that we *may discover*, containing the energy equivalent of 1,850 billion barrels of oil, would last  $1,850 / 44 = 42$  years, from that time. Scarcity will appear long before that.

When oil and gas are virtually used up, the burden will fall on coal. Coal presently satisfies about 22% of world energy demand, whereas oil contributes 30% and gas 23% (Brown, Flavin, and French, 1999). Thus, without oil or gas, coal supply will be called upon to increase by 240% in order to provide the same thermal energy. But much more than that will be needed because the oil needed to mine and deliver the coal will have to be produced synthetically (see item 6). It seems fair to conclude that amidst the thunderous clamour for easily transportable forms of concentrated energy, the era of cheap energy will effectively be over when we have to rely mainly on coal.

Even if the cheap energy era is not over when gas and oil become scarce, it soon will be as the more easily accessed coal seams are used up and energy profit

ratios fall. For instance, Gever et al. (1986) reported that the mine-mouth energy profit ratio for U.S. coal fell from 80:1 in the 1940s to 30:1 in the 1970s (see also item 8).<sup>3</sup>

2. Despite heroic efforts over many years, China's population is still expanding at about 13 million a year (Youngquist, 1997:470), and many other nations are not doing as well as China in controlling their populations. Expanding populations will demand more energy, even while supply is declining.
3. The remaining oil resources are largely in the Middle East, and population there is likely to continue to expand exuberantly according to what has succinctly been termed the *fertility opportunity model* of demographic change (Abernethy, 1993). Current annual growth rates are Kuwait 6%, Qatar 6.5%, United Arab Emirates 7.3% (Youngquist, 1997:404). At a 6.5% annual growth rate, population doubles in 11 years; and in 100 years a population of 5 million becomes a population of  $5 \times 1.065^{100} = 2.7$  billion. It is not necessary to accept the full assumptions to see the danger.
4. Many nations, such as China, have a drive to improve their physical standard of living, and there certainly is much room for improvement. In terms of energy equivalents of kilograms of coal per capita, United Nations statistics give annual consumption in the United States as 10,127 kg, compared to 810 kg for China and 307 kg for India (Youngquist, 1997:458).
5. It makes no sense, of course, to assess environmental impact by merely counting heads. It is necessary to take consumption into account; use of energy is the best indicator. Thus, taking the figures of the previous paragraph, the population of the United States, about 270 million in 1997, was equal to  $270 \times (10,127 / 810) = 3.375$  billion "Chinese equivalents." If the United States of America continues with its largely immigration-driven expansion, at say the 1970-2000 rate of about 1% a year, then, in only a generation, the population of the United States will amount to 4.3 billion "Chinese equivalents," and in 100 years it would amount to  $3,375 \times 1.01^{100} = 9.1$  billion "Chinese equivalents." These figures are projections, not predictions. It is surely not *impossible* that Americans will belatedly follow the advice, given in 1972 by the John D. Rockefeller Commission, to stabilize population at the then-current 210 million.
6. When oil becomes scarce, somewhat wider use will be made of the process that is currently used in South Africa to satisfy 46% of the home market (Youngquist, 1997:224), and which was used in Germany during the last world war, namely production of synthetic oil from coal. As well as the fact that the energy profit ratio for coal is lower than for oil (and falling), the energy losses that occur during this conversion from

coal to oil will mean a much greater use of energy to achieve the same *net* energy. Indeed, to make 1 ton of gasoline requires 3.65 tons of coal, including the coal needed to carry out the conversion process (Durrant, 1953:309). The process is such that by time synthetic gasoline has been burnt, 76% more carbon has been released than would be by burning gasoline made from refined oil. The excess carbon effect is similar when producing synthetic gas from coal: 80% more carbon dioxide is released per unit of energy than fuel oil (Youngquist, 1997:361).

7. Since the best mineral ores are now exhausted, we are steadily having to use poorer ones that need more energy for processing. For instance, in 1910 the United States was able to use ore with a 2% copper content, but now it has to be produced from a fine-grained igneous rock that contains only 0.4% of copper. Thus, a ton of rock has to be processed to produce 8 pounds (3.6 kg) of copper (Youngquist, 1997:414). The U.S. Bureau of Mines calculates that the annual consumption of each U.S. citizen amounts to about 25 pounds of copper (Youngquist, 1997:23).
8. As the energy resources that are easy to recover are exhausted, more energy is required to extract energy. In the United States, about 25% of energy is currently used to produce energy; it is estimated that within the next decade this energy cost will increase to 33% (Youngquist, 1997:452).
9. The United States, with less than three percent of total world oil reserves, and with about five percent of the world's population, uses about 27 percent of the world's oil, and a similar percentage of the world's total energy (Youngquist, 1997:172, 458). Suppose that the United States makes Herculean political efforts to reduce its per capita energy use to a "modest" third of its present level, which would be below European levels. Suppose also that only two-thirds, say, of the present world population are aspiring to be significant fossil fuel users, and all of these would be content with the aforesaid "modest" level of energy use, then total world energy consumption would *increase* by  $(27 / 3) \times (100 / 5) \times (2 / 3) - 100 = 20\%$ . So even with these assumptions, which might be termed "heroically optimistic," it seems likely that the rate at which the world is consuming fuel is going to increase. *And the increase would be even more obvious were we to allow also for the probable population increase and for the problems in items 6 and 8.*

Note, incidentally, that if the United States fails to change its current per capita use of energy, then, at its present rate of population growth, in 70 years time it would be using  $27\% \times 2 = 54\%$  of present world energy consumption—which, by itself, would produce carbon beyond the Earth's absorption capacity.<sup>4</sup> Thus, the United States, having long

ago swept serenely past the fabled "demographic transition" point, can, by dint of immigration and by simply sailing blithely on, sink the good ship Earth single-handedly!

### The Renewable Energy Sources "Solution"

All but the most die-hard optimists should by now be convinced that fossil fuels will not remain plentiful throughout this century. Can renewable energy sources come to the rescue? The Optimum Population Trust has made a start by analysing energy from biomass. Using data from Pimentel and Pimentel (1996:236), and with assistance from the Mauritius Sugar Industry Research Institute, we produced a short paper, *Sugarcane and Energy* (Ferguson, 1999a). It came to the firm conclusion that mean *net* energy-capture (amount of *useful* energy that can be captured per hectare) as ethanol is about 2.1 kW/ha (or kWyr/ha/yr if you prefer).<sup>5</sup> However, the sugarcane figure is based on the assumption that the by-product bagasse could be used for the ethanol conversion process. But removing all the biomass results in soil degradation; so, at 2.1 kW/ha, the process cannot be regarded as sustainable.

The only reason that our "Biomass and Energy" paper remains in draft is because we are still hoping to get firm data on the thorny subject of pyrolysis (heating in the absence of oxygen). At present, little definite can be said about the efficacy of the pyrolysis process in producing usable oil, but otherwise the overall conclusion of the paper stands: *net* yields from biomass, significantly higher than from sugarcane, should be regarded as a hope rather than a probability. Our analysis entirely supports Giampietro, Ulgiati, and Pimentel (1997), as well as Youngquist (1997:246), who say: "Replacing any very appreciable amount of conventional oil now used with liquid fuel from biomass seems quite unrealistic. Biomass cannot supply even a modest proportion of the current oil consumption in the United States nor anywhere else." A back-of-the-envelope calculation confirms his point.

During 2000, world consumption of conventional oil was about 23 billion barrels a year (more easily visualized as being nearly 4 cubic kilometers). A barrel of crude oil has an energy content of about 1,700 kWh = 0.194 kWyr (Youngquist, 1997:206). Thus, oil provides us with a gross 4.46 billion kWyr of energy per year. Current output/input ratio is probably better than 20:1, so that equals a *net* use of at least  $4.46 \times 19/20 = 4.24$  billion kWyr per year. Sugarcane, ignoring its links to soil erosion, is probably the most effective way of turning solar energy into a substance that can substitute for oil. At the aforesaid *net* energy-capture of 2.1 kWyr/ha/yr, using sugarcane to supply *a fifth*, say, of the energy that we currently enjoy from oil would require 400 million ha. Ignoring the improbability of finding such a large area suitable for growing sugarcane, this "demand" must be set against the global arable land figure of 1.4 billion ha (the 1990 figure given in Engelman and LeRoy,

1995), while also bearing in mind the following significant pointers to the future availability of cropland: (a) in the 1980s, about 80 percent of cleared forest land was being used to provide additional cropland and pastureland for agriculture because of pressure for more land for crops (Pimentel and Pimentel, 1996:151); and (b) "During the past 40 years, about 30 percent of total world arable land has been abandoned because it was no longer productive" (Pimentel and Pimentel, 1996:293).

So much for biomass, but surveying the whole field of renewable energy is not easy. Considerable work has been done on it because determining a realistic energy/land ratio is a vital part of eco-footprinting. The wider perspective has been covered more fully in *The Logical Foundations of Ecological Footprints* (Ferguson 1999b). While it has to be said that not much has been published on this quagmire of a subject, nevertheless there are many working papers that, taken together, indicate that the energy/land ratio most commonly used in eco-footprinting, 3.2 kW/ha, is the best available estimate of *net* energy-capture when using an optimum mix of renewable energy sources.

I admit that what is really required is for several independent experts to address the matter. But of course it is important to have the right experts. We have, in draft, a paper called *Renewable Energy and Commonsense Checklists*. It emphasizes the need to avoid those experts whose arguments have too tenuous a grip on common sense, or who are over-imbued with optimism. This would include such experts as those who thought we could get rid of atomic waste by firing it into space, or if not that some suitable solution was sure to turn up! Similar experts are just as common in the renewable energy field; for instance, they quote the rated output of photovoltaic cells without considering either latitude, or season, or the related problem of cloudiness. Some even ignore the great problem of storage. Something that is almost never mentioned is that the capacity factor (amount of energy produced as a fraction of the rated value) of photovoltaic cells is in the range 10-20%, depending on insolation. Almost as rarely mentioned is the fact that even wind turbines, which are by far the best source of renewable energy, have a capacity factor, on land, of about 25%.

Hydrogen is the current subject of wishful thinking. Even journals that aspire to some scientific balance, such as *Worldwatch* and *New Scientist*, have, in recent articles on hydrogen, omitted to mention any of the following: (a) the energy needed to store liquid hydrogen below its boiling point of  $-253^{\circ}\text{C}$ ; (b) the pressure, of 930 atmospheres, that would build up in a container of liquid hydrogen, were it allowed to warm up to  $50^{\circ}\text{C}$ ;<sup>6</sup> (c) that "the absorption of hydrogen in steel may cause 'hydrogen embrittlement' which sometimes leads to the failure of chemical processing equipment" (*McGraw Hill Encyclopedia*, 8<sup>th</sup> Edition, 8:659); (d) the amount of electricity, 12 kilowatt hours,

required to produce 3 liters of liquid hydrogen, an amount that, in an internal combustion engine, has the same motive power as 1 liter of gasoline, (e) the quantity of energy that would be needed to transport the hydrogen, and (f) the sources of *renewable* energy that might be available to produce the hydrogen.

It is not difficult to write glowingly optimistic articles, if one is prepared to be so economical with the truth as to gloss over all those issues.

In broad perspective, the key question is this: what mean *net* energy-capture from renewable energy sources can be achieved? Our fairly comprehensive studies point to 3.2 kW/ha being a probable mean value. It is certainly a good precautionary value until someone can produce clear evidence that the figure should be higher.

The most recent source of data on ecological footprints is the *Living Planet Report 2000* (WWF, 2000). Allowing 10% of ecologically productive land to preserve biodiversity, it shows that the world has overshoot its carrying capacity by about 45%. However, it makes no attempt to take account of, for instance, the 830 million people who get insufficient calories in their diet, the 3 billion people who are malnourished, or rapidly degrading agricultural land and falling water tables (for a comprehensive list, see Pimentel et al., 1999). When these factors are taken into account, any competent eco-footprinting study will agree with the select band of eminent scientists who have made estimates to the effect that, for a reasonably comfortable lifestyle (e.g., modest European), world population needs to be in the range 1.5 to 3 billion.<sup>7</sup>

### The Vital Importance of Cultural Factors and Sovereignty

At first sight, there may appear to be a contradiction between the previously mentioned projections of massive population increases, and the figures just given for world carrying capacity: moving from the early figures to the required destination may seem impossible. However, the world figures provide only a general signpost, showing where the world needs to be heading. Responsibility must rest with sovereign states.

The importance of cultural factors in controlling population growth has been described by Virginia Abernethy (1993), and also by Garrett Hardin in *Living Within Limits* (1993) and in his classic essay "The Tragedy of the Commons" (1968), where he argued that "mutual coercion mutually agreed upon" is the only way to control that tendency to overbreed that holds humans, as much as other species, in thrall. Anyhow, it is a matter of common sense that only cohesive tribal groups or sovereign nations can impose sufficiently effective social penalties on their members to significantly affect rates of procreation and, of equal importance, can prevent people from the more

Table 1. Ecologically Productive Land Available to Nations in 1993

Nation	Ecologically Productive Land ha/cap	Nation	Ecologically Productive Land ha/cap
New Zealand	15.57	U.K.	1.39
Finland	10.07	Iceland	1.35
Canada	8.92	South Africa	1.29
Ireland	8.60	Turkey	1.20
Sweden	7.99	Belgium	1.16
Chile	4.94	Peru	1.14
Russian Federation	3.91	World (comprising these 52 nations)	1.12
Argentina	3.77	Venezuela	1.06
France	3.67	Mexico	1.00
Austria	3.59	Italy	0.96
Australia (see footnote)	3.47	Greece	0.91
USA (see footnote)	3.23	Thailand	0.90
Norway	3.23	Japan	0.72
Czechoslovakia:	2.57	Pakistan	0.51
Netherlands	2.52	China	0.50
Spain	2.30	Indonesia	0.48
Europe	2.27	Ethiopia	0.48
Switzerland	2.26	Nigeria	0.35
Brazil	2.22	Israel	0.34
Denmark	1.97	India	0.30
Poland	1.96	Philippines	0.26
Portugal	1.82	Egypt	0.24
Germany	1.77	South Korea	0.21
Costa Rica	1.71	Bangladesh	0.14
Hungary	1.67	Jordan	0.09
Malaysia	1.62	Hong Kong	0.01
Colombia	1.58	Singapore	0.00

Note: Ecologically productive land (excluding built-up land) per capita, expressed in terms of worldwide productivity, derived from Wackernagel, 1997, with some agreed errors corrected. Sustainability buffers have been built into Australia and USA, as described in Ferguson (1998).

Source: Optimum Population Trust (UK). March 3, 1999.

over-populated nations, such as Mexico, flooding into *less* over-populated ones, such as the USA. Thus, the responsibility must fall to individual nations for acting upon the moral imperative that each nation should keep within its own ecological resources. In that task, each nation will need to guide its actions according to its ecological circumstances. The per capita availability of resources is shown in Table 1.

Table 1 expresses areas in terms of worldwide productivity. That makes it possible to compare one nation's ecological resources with another. Moreover, it makes it easy to see the implications of any particular "standard" size of ecological footprint needed to sustain a specified lifestyle. For instance, in order to *sustainably* maintain a current European lifestyle, an area of about 3.5 ha per person is needed (as established by eco-footprinting). Thus, all 42 nations below Austria in the table, *with their present populations*, have insufficient land to *sustain* a European lifestyle. As already mentioned, the level of population that could be supported at a modest European lifestyle is a relatively simple calculation, using eco-footprinting. It has been addressed in various papers, the earliest being Ferguson, 1998. Here I will sketch in a few elements.

One component of that 3.5 ha/cap is the land needed to supply energy; but often energy consumption is higher than it really has to be. Vaclav Smil (1993) provides evidence that an *average* energy use of 64 gigajoules/cap/yr, 2 kW/cap, would be adequate to preserve the more important aspects of an acceptable lifestyle.<sup>8</sup> That energy use is about two fifths of the present European level (equal to a fifth of the current US level). This would reduce the ecologically productive area needed to provide for each person sustainably from 3.5 ha to 3 ha. Using that figure, the corollary is that, in Table 1, the 39 nations below Norway would, *with their present populations*, have insufficient land to sustain even this more frugal European lifestyle. Of course, none of this is apparent while we have cheap energy.

The above assertions are simplifications. A closer look at Table 1 reveals some of the weaknesses of assessing ecologically productive land as a single unit. Finland and Sweden appear to have so much land that they could about double their populations. However, detailed investigation reveals that most of their ecologically productive land is forest (and little of their forest land can be converted to arable land or pasture). Taking arable land and pasture as the measure, the population of each nation has already

risen to the level of its carrying capacity. Also, it may seem odd that the Netherlands has 2.5 ha/cap of ecologically productivity land, *at worldwide productivity*, compared to the UK's 1.4 ha/cap. The explanation is that eco-footprinting does not, at present, take sustainability into account, and hence overlooks the serious pollution problems in the Netherlands (Prugh et al., 1995:155-158). Sometimes nations need to be studied individually, and in detail.

But while—at least at a glance—some messages from Table 1 may be misleading, others are not. The very low figures for the last 12 nations, with less than 0.5 ha/cap available, spell out a *clear and correct message* that, with their present populations, these nations have no possibility of living *sustainably* in a European lifestyle, unless they

are sufficiently small to be able to import their ecological resources from elsewhere. *Whether it is morally right for them to do so is another question.*

### The September 1997 Roundtable Discussion

Despite the extensiveness of the above argument, it could be said that this essay contains only three propositions:

1. For everyone to *sustain* an adequate, comfortable (i.e., modest European) standard of living, world population would need to be about 2 billion;
2. History, anthropology, and politics tell us that nations will never agree about how to share out the pain

**Table 2. Agreement of Contributors to the September 1997 *Politics and the Life Sciences* Roundtable with Propositions 1, 2, and 3 (see text)**

	Proposition 1	Proposition 2	Proposition 3	Notes
J. Kenneth Smail	Y	N	Y	
Virginia Deane Abernethy	Y	Y	Y	
Tim Dyson	N	N	N	
Timothy F. Flannery	?	?	?	a
Lindsey Grant	Y	?	Y	b
Betsy Hartmann	N	N	N	
Carl Haub	?	?	?	c
Richard D Lamm	?	?	?	d
Wolfgang Lutz	N	N	N	e
Norman Myers	Y	?	?	
Jack Parsons	Y	Y	Y	f
David and Marcia Pimentel	Y	Y	Y	g
M.S. Swaminathan	Y	?	?	h
Tang Re-Feng	?	Y	Y	i
Bruce Wallace	Y	?	?	j
Charles F. Westoff	Y	?	?	k
David Willey	Y	Y	Y	l

Note: Y = Yes, agreement; N = No, disagreement; ? = uncertainty of position

- a. Flannery refers only to Australia, hence the uncertainty.
- b. Grant says he agrees with Smail, so perhaps his answer to Proposition 2 is N.
- c. Haub intentionally limits his comments to demography.
- d. Lamm generally agrees with Smail, but thinks that one must proceed in small steps.
- e. Lutz thinks that education will solve everything.
- f. Perhaps Parsons provided the most succinct support for Proposition 2, when he says, "When I look at the savagery of the infighting within the EEC about the sharing out of our rapidly diminishing fish stocks, I sadly shake my head and wonder how we could ever begin to agree about massively reduced 'quotas' for our national human numbers" (1997:215). He was equally to the point with respect to Proposition 3, saying, "While we must both hope and work for a more just, rational, and ecologically sustainable world, we must also face the prospect that it may be essential for those countries that can manage it in this 'politically fragmented world,' to batten down the hatches, keep a tight ship, and go it either alone or in the company of those few others that see the writing on the wall and do likewise" (1997:216). Additionally, the most succinct support for both propositions is surely contained merely in the title of his two volume book: *Human Population Competition: A Study of the Pursuit of Power Through Numbers* (1998).
- g. In their contribution, the Pimentels do not actually make it clear that they agree with Propositions 2 and 3, but papers such as *An Optimum Population for North and Latin America* (Pimentel et al., 1998) leave no doubt as to their position.
- h. Swaminathan does not express direct support for Propositions 2 and 3, but his focus on India suggests it.
- i. A '?' is appropriate to Proposition 1 because Re-Feng says she is not sure the Earth's carrying capacity is what Smail suggests. Her unequivocal support for Proposition 3—from a Chinese perspective—is worth quoting: "nor do we think about how other countries, especially developed countries, can contribute to the population control program. We feel that we have sowed the wind, so we must reap the whirlwind by ourselves" (1997:222).
- j. The queries by 2 and 3 are doubtful, since Wallace quoted the vital point made by C.G. Darwin, that self-restraint in fertility, by some, will be negated through evolution selecting for the most fecund.
- k. Westoff does not show overt support for Propositions 2 and 3, but his piece is so eminently sound that it is implicit that he would if asked!
- l. The late David Willey helped to introduce the standard of a "Modest" European footprint.

involved in making the dramatic changes required by Proposition 1; and

3. Only nations can exercise some control over the two key variables—consumption and population. Therefore, nations must assume responsibility for living within their biocapacity.

Proposition 1 made use of the disciplines of geology (particularly petroleum geology), horticulture (for biomass), and physics (for renewable energy). Perhaps those subject matters are outside the usual scope of *Politics and the Life Sciences*; nevertheless, the correctness of the premise is essential to the discussion. Table 2 attempts to tabulate the extent to which the contributors to the roundtable discussion expressed agreement with the three propositions. It is manifest, from viewing the table, both that there is disagreement and that in some cases it is hard to tease out any clear viewpoint.

Since, as mentioned, Proposition 1 is almost outside the scope of this journal, perhaps the most productive discussion could continue by accepting the *hypothesis* that Proposition 1 is correct (at least it is plausible). The ensuing discussion would surely be valuable. The scientists of the Intergovernmental Panel on Climate Change have made a clear statement about the dangers of global warming. Elite petroleum geologists, such as Campbell, Ivanhoe, Hubbert, Laherrère, and Perrodon, have produced a clear consensus about our fossil fuel future. That governments have failed to respond to both these bodies of knowledge does not entirely vitiate the academic effort. As Colin Campbell says by way of justification for his book, *The Coming Oil Crisis*, even if governments “cannot bring themselves to prepare, perhaps this will at least help them know what hits them when the crisis strikes, as it surely must” (1997:1). For similar reasons, those engaged in politics and the life sciences need to strive to reach a consensus view of the political direction in which the human race should proceed when the reality of the crisis can no longer be denied.

## Conclusion

Throughout recorded history, humans have struggled to obtain adequate food, with famine being a recurrent experience in North Africa, Europe, and the Far East (Ponting, 1992). Over the last two centuries, the situation has been transformed by the availability of fossil fuels. However, the peak of oil production will come within the next two decades (Campbell, 1997), and our cheap energy bonanza will come to an end during the twenty-first century, during which period nations will become steadily more reliant on renewable resources. Some people, and organizations such as Friends of the Earth (McClaren, Bullock, and Nusrat, 1997), fantasize that a sort of worldwide welfare state will develop, but there is already ample evidence for the unreality of that, and such ideas will become more fantastic as energy for transport gets more expensive.

Some nations, or perhaps politically-linked groups of nations such as the European Community, may have the foresight to bring their populations down to levels that can be comfortably sustained from renewable resources (we estimate that the 19 main nations of western Europe would need to reduce from 445 million to 300 million, and the USA from the 1997 figure of 270 million to 200 million). For nations that fail to do so, the outcome will depend on the per capita availability of ecologically productive land. For some nations, it will be possible to sustain existing populations, but at a reduced physical standard of living. Those nations which, without the benefits of cheap energy, do not have enough ecologically productive land to even feed their populations will suffer a harsher penalty, one that will be exacted by Nature’s fell sergeant, Death!

It is customary to finish off papers of this kind with a few optimistic thoughts, and I will follow that admirable precept. I read not long ago in *New Scientist* of an organization with exactly the right ideas. Its location in cyberspace is [www.whemt.org](http://www.whemt.org), and its full name is The Voluntary Human Extinction Movement, VHEMT, pronounced “vehement.” It has as its creed “phasing out the human race by voluntarily ceasing to breed,” which, it claims, will allow the Earth’s atmosphere to return to good health. VHEMT says that it wants to offer a positive and caring alternative to the involuntary, violent extinction that seems to be the direction in which humans are heading at present. Surely the very existence of such an organization is cause for optimism.

## Acknowledgments

The spark for this endeavour was Jill Curnow, of *Sustainable Population Australia*; she also helped to shape it. Our chairman, the late David Willey, contributed valuable information. Thanks, too, for considerable help from Colin Campbell and David Pimentel, and some good points came from two anonymous referees; also, my thanks to David Gosden for smoothing out many infelicities. My indebtedness to other sources is apparent from the references.

## Notes

1. When attempting to privatize the nuclear industry, the United Kingdom discovered the impossibility of getting commercial insurance cover. In the United States, the industry was sheltered with the Price-Anderson Act, which limits liability to \$8.9 billion for U.S. domestic damage, and about 1/90th of that for damage to all foreign nations (Grossman, 2001:38).
2. It is obvious nonsense to divide the amount of resource available by a *static* amount, the peak supply. But, since we are close to peak supply, and since supply must eventually fall to effective zero, a straight line decline in supply would, after the calculated 50 years have elapsed, give half present supply. Thus, the calculated 50 years is a rough indicator of “half supply.” Probably a more accurate figure is the one presented earlier, with the indication of combined oil and gas supply falling to half by 2060 and 20% by 2100.
3. Energy profit ratio is the ratio between the *gross* output of the energy captured (e.g. by mining) divided by the energy needed to capture it. It is also called the output/input ratio. There

are many different energy-profit ratios, depending on how far down the line one wishes to go. For instance, the energy-profit ratio to “capture” electricity using coal, which is about 8:1 (Pimentel and Pimentel, 1996:206), is much lower than the mine-mouth ratio of 30:1 in the 1970s.

4. The Intergovernmental Panel on Climate Change recommended, early in the 1990s, that fossil fuel emissions needed to be cut to 20-40% of 1990 rates of emission (Engelman, 1994:27). The 40% figure is a simple calculation based on the rate at which carbon is accumulating in the atmosphere. It is clear, therefore, that if the USA were to emit 54% of current global emissions, it would exceed the Earth's absorption capacity.
5. 1 kWyr, a flow of 1 kW (a small electric fire) for a year, =  $1 \times 24 \times 365 = 8,760$  kWh. The natural cycle of biomass is a year, so it is natural to express energy yields as an amount of energy per year. But 1 kWyr per year can conveniently be expressed as a steady flow of 1 kW. In reality, the flow is not constant, but that is obvious.
6. The pressure of 930 atmospheres, about 13,700 psi or 960 kg/cm<sup>2</sup>, is calculated on the basis of hydrogen behaving like an ideal gas. In a real gas the pressure is likely to be higher.
7. Relatively few scientists give a number for sustainable population at a specified lifestyle. Pre-eminent among those few is David Pimentel of Cornell University. Together with some colleagues, he has given figures for particular areas—about 200 million for North America and the same for Latin America (Pimentel, Giampietro, and Bukkens, 1998). In 1994, with another group of associates, he estimated a *global* figure of 1 to 2 billion based on an assumption of 5.3 kW/cap (about half U.S. energy use). With less precise specification of energy use, in their superb book, *Food, Energy, and Society*, the Pimentels gave a figure of about 2 billion (Pimentel and Pimentel, 1996:xvi). Paul Ehrlich mentions that “we and others” estimate a global figure of around 1.5 to 2 billion for maintaining a European lifestyle (Ehrlich, Ehrlich, and Daily, 1998:112). Any figures are, of course, highly dependent on the lifestyle that is assumed; the top figure of 3 billion is broadly compatible with the conclusions reached in *World Carrying Capacity: An Interim Report* (Ferguson, 1998), where it is concluded that 1.5 billion could live at a modest European lifestyle, while taking note of about 2 billion who remain largely untouched by modern technology. Fleay (1995) suggests that, “The world may only be able to support a population of 3 billion without this [fossil fuel] input.”
8. Based on infant mortality and access to education, Vaclav Smil (1993) provides evidence of a minimum energy use. Further considerations supporting the 64 GJ/ha/yr figure appear in Ferguson, 2001. Nevertheless, the exact choice for a minimum average figure for energy use must remain to some extent a matter of judgement. Note that the figure can only be an *average*: for example, the actual per capita primary energy delivered to *households* in Canada, in the 1980s, was 48 GJ/yr (Smil, 1993:143). Taking into account that modern life aims to provide all people with an acceptable physical standard of living, the following extract from Smil (1993:49) provides supporting evidence for the choice of 64 GJ/cap: “A pre-industrial European consumed annually mostly between 20-40 GJ of wood and charcoal for cooking, heating and manufacturing, and the maxima in North America of the nineteenth century went as high as 70-100 GJ/capita. . . . in 1990 the world's rich nations consumed about 150 GJ of primary energy per capita.” 150 GJ/yr = 4.8 kW (the U.S. uses about 11 kW/cap).

## References

- Abernethy, V. (1993). *Population Politics: The Choices that Shape Our Future*. New York: Plenum.
- Aubrey, C., D. Grünberg, and N. Hildyard, eds. (1991-93). *Nuclear Power, Shut It Down!: An Information Pack on Nuclear Power and the Alternatives*. Two volumes. New Madden, England: The Ecologist.
- Bartlett A.A. (1994). “Fusion: An Illusion or a Practical Source of Energy?” Carrying Capacity Network, Clearinghouse Bulletin, January.
- Brown, L.R., C. Flavin, H. French, et al. (1999). *State of the World, 1999*. New York: W.W. Norton.
- Campbell, C.J. (1997). *The Coming Oil Crisis*. Essex, UK: Multi-Science Publishing Company & Petroconsultants S.A.
- Durrant, P.J. (1953). *General and Inorganic Chemistry*. London and New York: Longmans, Green.
- Engelman, R. (1994). *Stabilizing the Atmosphere: Population, Consumption and Greenhouse Gases*. Washington, DC: Population Action International.
- Engelman, R. and P. LeRoy (1995). *Conserving Land: Population and Sustainable Food Production*. Washington, DC: Population Action International.
- Ehrlich, P.R., A.H. Ehrlich, and G.C. Daily (1998). *The Stork and the Plow: The Equity Answer to the Human Dilemma*. New York: G.P. Putnam's Sons.
- Ferguson, A.R.B. (1998). *World Carrying Capacity: An Interim Report*. Manchester, UK: Optimum Population Trust. Archived at [www.members.aol.com/optjournal/cc3.doc](http://www.members.aol.com/optjournal/cc3.doc).
- Ferguson, A.R.B. (1999a). *Sugarcane and Energy*. Manchester, UK: Optimum Population Trust.
- Ferguson, A.R.B. (1999b). “The Logical Foundations of Ecological Footprints.” *Environment, Development and Sustainability* 2:149-56.
- Ferguson, A.R.B. (2001). “Perceiving the Population Bomb.” *WorldWatch* (July/August):36-39.
- Fleay, B. (1995). *The Decline of the Age of Oil*. Annandale, NSW: Pluto Press Australia.
- Gever, J., R. Kaufman, D. Skole, and C. Vörösmarty (1986). *Beyond Oil: The Threat to Food and Fuel in the Coming Decades*. Niwot, CO: University Press of Colorado.
- Gever, J., R. Kaufman, D. Skole, and C. Vörösmarty (1991). *Beyond Oil: The Threat to Food and Fuel in the Coming Decades*. Niwot, CO: University Press of Colorado.
- Giampietro, M., S. Ulgiati, and D. Pimentel (1997). “Feasibility of Large-Scale Biofuel Production: Does an Enlargement of Scale Change the Picture?” *Bioscience* 47:587-600.
- Grossman, K. (2001). “Disgrace into Space.” *The Ecologist* 31 (2):34-38.
- Hardin, G. (1968). “The Tragedy of the Commons.” *Science* 162:1243-48.
- Hardin, G. (1993). *Living within Limits: Ecology, Economics, and Population Taboos*. Oxford: Oxford University Press.
- International Energy Agency (1998). “World Energy Propects to 2020.” Paper prepared for the G8 energy ministers' meeting, Moscow, March 31-April 1.
- Ivanhoe, L.F. (1996). “Updated Hubbert Curves Analyse World Oil Supply.” *World Oil* (November):91-94.
- McLaren, D., S. Bullock, and Y. Nusrat (1997). *Tomorrow's World*. London: Earthscan.
- Parsons, J. (1997) “Reflections on the Downsizing of Humanity.” *Politics and the Life Sciences* 16:213-16.
- Parsons, J. (1998). *Human Population Competition: A Study of the Pursuit of Power through Numbers*. Lewiston, NY: Edwin Mellen Press.
- Pimentel, D. and M. Pimentel (1996). *Food, Energy, and Society*. Revised edition. Niwot, CO: University Press of Colorado.
- Pimentel, D., M. Giampietro, and S.G.F. Bukkens (1998). “An Optimum Population for North and Latin America.” *Population and Environment* 20 (2):125-48.
- Pimentel, D., O. Bailey, P. Kim, E. Mullaney, J. Calabrese, L. Walman, F. Nelson, and X. Yao (1999). “Will Limits of the Earth's Resources Control Human Numbers?” *Environment, Development and Sustainability* 1:19-39.
- Ponting, C. (1992). *A Green History of the World*. London: Penquin.
- Prugh, T., R. Costanza, J.H. Cumberland, H. Daly, R. Goodland, and R.B. Norgaard (1995). *Natural Capital and Human Economic Survival*. Solomons, MD: ISEE Press.
- Re-Feng, T. (1997) “Birth Control as an International Program.” *Politics and the Life Sciences* 16:222-23.

- Roberts, F. (1999). *Sixty Years of Nuclear History*. Oxfordshire: Jon Carpenter Publishing.
- Smail J.K. (1997). "Beyond Population Stabilization: The Case for Dramatically Reducing Global Human Numbers." *Politics and the Life Sciences* 16:183-92.
- Smail J.K. (1997). "Population Growth Seems to Affect Everything But Is Seldom Held Responsible for Anything." *Politics and the Life Sciences* 16:231-36.
- Smil, V. (1993). *Global Ecology: Environmental Change and Social Flexibility*. London: Routledge.
- Wackernagel, M. (1997). *Ecological Footprints of Nations: How Much Nature Do They Use? How Much Nature Do They Have?*

- [Computer File]. Xalapa, Mexico: Center for Sustainability Studies.
- World Wildlife Fund (2000). *Living Planet Report 2000*. Gland, Switzerland: WWF-WorldWide Fund for Nature. This 32 page booklet was produced by the World Wide Fund for Nature International (Switzerland) together with the UNEP World Conservation Monitoring Center (UK), the Center for Sustainability Studies (Mexico), and Redefining Progress (USA). It can be downloaded from <<http://panda.org/livingplanet/lpr00/>>.
- Youngquist, W. (1997). *GeoDestinies*. Portland, OR: National Book Company.

## CONTINUING DIALOGUE

# Darwinism, Dominance, and Democracy: A Reaffirmation

**Albert Somit**

Southern Illinois University, Carbondale, USA

**Steven A. Peterson**

Penn State Harrisburg, USA

So that the reader can better adjudge possible disagreements, we would like to begin by stating, at the very outset, the thesis argued in our book, *Darwinism, Dominance, and Democracy* (Somit and Peterson, 1997). As Peter

---

**Albert Somit** is Distinguished Service Professor Emeritus at Southern Illinois University. He has taught at New York University, the State University of New York at Buffalo, and the Naval War College, and has held Senior Fellowships at the Netherlands Institute for Advanced Study and Hebrew University in Jerusalem. He has served as Executive Vice President at SUNY/Buffalo and as President at Southern Illinois University. Since the early 1960s, his major interest has been in the possible influence of biological factors (genetic and other) on political and social behavior. He was a founding member of the Association for Politics and the Life Sciences and founder (and Chair) of the International Political Science Association's Research Committee #12 (Biology and Politics). Among his publications are *The Development of American Political Science* (Allyn and Bacon, 1967), *Biology and Politics* (Mouton, 1976), *Hierarchy and Democracy* (Southern Illinois University Press, 1991), *The Victorious Incumbent* (Dartmouth, 1994), and, with Steven A. Peterson, *The Dynamics of Evolution* (Cornell University Press, 1992), *Birth Order and Political Behavior* (University Press of America, 1996), *Darwinism, Dominance, and Democracy* (Praeger, 1997) and, most recently, *Human Nature and Public Policy: An Evolutionary Approach* (Palgrave Macmillan, 2003). **Steven A. Peterson** is Director of the School of Public Affairs at Penn State Harrisburg and Professor of Politics and Public Affairs. Before that, he taught at Alfred University. He is Chair of the Council of the Association for Politics and the Life Sciences and Vice President and Secretary of Research Committee #12 of the International Political Science Association. He has served as President of the New York State Political Science Association and the Northeastern Political Science Association. His research interests include biopolitics, American politics, public opinion and voting behavior, and public policy (AIDS policy and education policy). Among his authored or coauthored books (in addition to those noted above) are *Political Behavior: Patterns in Everyday Life* (Sage, 1990), *The World of the Policy Analyst* (Chatham House, 1997), and *State and Local Politics* (McGraw-Hill, 1994). Correspondence should be addressed to School of Public Affairs, Penn State Harrisburg, 777 W. Harrisburg Pike, Middletown, PA 17057, USA (e-mail: sap12@psu.edu).

Corning correctly reports in his commentary (2000), we contend that “the most important reason for the rarity of democracy is that evolution has endowed our species, as it has other primates, with a predisposition for hierarchically structured social and political systems” (1997:1). In short, we argue that *Homo sapiens* has a “genetic bias” toward authoritarian political societies characterized by hierarchy, dominance, and submission.

But that is not our thesis in full. Obviously, despite this inherent bias, democracies *do* occasionally arise. To explain that happy reality, we immediately go on to say that two things make this possible. First (and here we trod familiar ground), there must be a “unique concatenation of economic, social, historical and political ‘facilitating’ factors” (Somit and Peterson, 1997:4).

For the second “necessary, though not sufficient condition,” we turn to evolutionary theory—and given Corning’s opening criticism, we think it important to cite the statement in full:

Although it shares the proclivity of its fellow social primates for hierarchical social organizations, *Homo sapiens* is the only species capable of creating and, under some circumstances, acting in accordance with cultural beliefs that actually *run counter* to its innate behavioral tendencies. The generally accepted, if lamentably awkward, term for this truly unique capacity is ‘indoctrinability.’ Celibacy and the (presumably) less demanding ideal of faithful monogamy are obvious examples of indoctrinability at work. Democracy, an ideal almost as alien to our social primate nature, is another. It is indoctrinability, then, that makes it possible, given some conjunction of the aforementioned facilitating social, economic, and other, conditions, for democracies occasionally to emerge and to have some chance to survive. (Somit and Peterson, 1997:4)

That done, we can now better address Corning’s concerns. To summarize them in the order of their expression, they are

1. that we are guilty, as was Edward O. Wilson in the “famous/infamous” concluding chapter of his 1975 *Sociobiology: The New Synthesis*, of “biologizing human behaviors” (Corning, 2000:104). Later restated and considerably expanded, this becomes the allegation that missing from our discussion is a “more adequate conception of the role of ‘nurture’ in political life” (Corning, 2000:104 and passim);
2. that we “see ourselves as engaging in an act of political ‘consciousness raising’” (Corning, 2000:104);
3. that although “we express a personal preference for democracy, [we] don’t spell out exactly why” (Corning, 2000:104);
4. that our “prescriptions [for preserving democracy] are not very compelling because they cannot also tell us why it is important to foster democracy” (Corning, 2000:104);
5. that we seem to be, our statement of preference notwithstanding, insufficiently enthusiastic about democracy (adumbrated on p. 4, the idea is made explicit in his final two sentences—Corning, 2000:108);
6. that we pay insufficient attention to other than genetic factors (i.e., self-interest, the role of coercive power, etc.) in explaining obedience to authority and the frequency and durability of authoritarian modes of governance (Corning, 2000:105, 106);
7. that we seem not to recognize the fact that “hierarchy and democracy are not incompatible opposites” (Corning, 2000:105, 106);
8. that, to support our thesis, we “discount” the establishment and spread of democracy in the past few millennia (Corning, 2000:106). (We assume Corning means “centuries” rather than millennia.)

These, we think, are the objections he raises. We apologize to Corning (a long and valued friend) if, with characteristic authorial over-sensitivity, we see criticism where none was intended; or, with equally characteristic authorial insensitivity, we overlooked some that were.

Now to respond. Let us begin with #3 above, our failure to justify our personal preference for democracy. The answer is quite simple: the task we (rashly) set ourselves was to explain, in neo-Darwinian terms, why democracy is historically so rare a phenomenon. We really do not think that, in carrying out that task, we are also obligated to argue quite a different case, that is, that democracy *should be* the favored form of polity. We do not see any necessary logical connection between the two ideas; plainly, Corning does. Perhaps he will deal with the issue in his reply.

Closely related is his criticism (#4 above) that our suggestions for preserving democracy are not very compelling “because they cannot also tell us why it is important to foster democracy.” This strikes us as a curious leap from a single premise to a conclusion. His conclusion is that our “prescriptions” may not work—and we can hardly take

issue. In fact, in the final pages of the book we repeat our fear that the “basic requisites” for preserving democracy are now so attenuated (the heading is “Going, Going. . .”) that it may be too late. Further reflecting our doubts, the first sentence of our closing paragraph asks whether it is now possible “to carry out a national policy of effective civic education.” The answer is, “In all candor, we do not know.”

But that is not Corning’s reasoning. It will not work, he writes, because we (the authors) “do not tell us why it is important to foster democracy.” Somewhere, we suspect, either a major or a minor premise has disappeared along the way. Again, we can hope for clarification in his reply.

That said, with the clairvoyance of hindsight, we concur in his conclusion that our prescription is no longer—if ever it was—compelling. Our plan at that time was to follow *Darwinism, Dominance, and Democracy* with a book spelling out how the proposed national program of democratic education/indoctrination might be carried out. After almost a year of work, we sadly and reluctantly came to the conclusion that it had very little chance of being enacted or, if by some miracle enacted, even less chance of successful implementation.

There were two basic reasons for that judgment: first, mounting evidence that the “basic requisites” for democracy were continuing to deteriorate; second, a growing conviction, after intensively studying the literature, that the American school system was no longer adequate for the task.

We came to that conclusion sadly, we should add, because of profoundly disturbing implications for the long-term health of the American democracy; and reluctantly because we very much would have preferred to believe otherwise.

That brings us to item #5 on the above list—the implication, though perhaps not so intended by Corning, that we seem to be insufficiently enthusiastic about democracy. As he first puts it, we are “even somewhat cynical and disparaging about what the term democracy means, since it has been used and misused in so many ways” (2000:104). So stated, the hastening reader might mistakenly see cynicism and disparagement as describing our attitude toward democracy per se, rather than to the manner in which the term “democracy” has been abused.

The same implication surfaces at the end of Corning’s review when he chides us for having “buried” Winston Churchill’s famous dictum (“democracy is the worst form of government—except for all the others”) in a footnote. We should, he insists, “have made it the frontispiece for [our] book” (Corning, 2000:108). We resist the temptation to respond that our intention was to honor Mr. Churchill, not to bury him. We cannot, however, refrain from observing that this is a near-classic example of the “reviewer’s Catch 22”: we are damned if we did—and equally damned if we didn’t.

We turn now to #2, that we “see ourselves as engaging in an act of political ‘consciousness raising.’” To tell the truth, we are not sure whether this is intended as a criticism or not. Still, since that places us (according to Corning) in the same company as Thomas Henry Huxley, George Williams, Richard Dawkins, and Richard Alexander, we hasten to claim “guilty as charged.” But if such was our objective, we were notably unsuccessful. If book sales are taken as the criterion, deplorably few readers have had their “consciousness” raised or lowered. Toward that end our publisher cooperated nobly, first pricing the volume at a preposterous figure near \$70 and, clearly reluctant to part with even a single copy, being extraordinarily secretive about its publication review copies—with *Politics and the Life Sciences* a happy exception—which seem to have fallen upon stony soil. The “standard” professional political science journals, consistently chilly toward “biopolitics” generally, were surely not going to alter their stance in behalf of a volume arguing (and Corning has it right) that “democracy is not in our genes.”

Corning says that we seem to be unaware (#7 above) that hierarchy and democracy are not “incompatible opposites.” Approaching the matter from a “cybernetic perspective,” he spends two full pages on what he concedes is a “subtle and complicated, argument” to establish his case (Corning, 2000:105). Here he is quite correct. All political systems, democratic as well as authoritarian, are hierarchically structured. To be sure, the authoritarian form of hierarchy has been overwhelmingly predominant both historically and, although we are loath to admit it, today as well. But democracies are also hierarchical, however they may differ in form and in mode of functioning. It is a point we, too, should have made, and we are indebted to Corning for calling the omission to our attention.

Undoubtedly, Corning’s most serious criticism substantively, and the one with which he begins his review, is that we are guilty of “biologizing human behaviors” (2000:104). More directly, he laments that he did not find a “more adequate conception of the role of ‘nurture’ in political life.” The term “nurture” refers, of course, to the role of culture—and, we confess, it is a criticism that we find quite surprising.

We say this for several reasons. First, as the reader may recall, and perhaps Corning forgot, the initial presentation of our thesis (see above) explicitly states that “*Homo sapiens* is the only species capable of creating and, under some circumstances, acting in accordance with *cultural* (our emphasis added here) beliefs that actually run counter to its innate behavioral tendencies.” We then go on to say that it is this idea of democracy, “an ideal . . . alien to our social primate nature,” which is a necessary condition (but not sufficient condition) “for democracies occasionally to emerge and to have some chance to survive.”

We could hardly put it more plainly. It is culture that, by (sometimes) leading us to overcome our inherent au-

thoritarian inclinations, makes democracy possible. This insistence that, if democracy is to be successful, the aforementioned requisite enabling conditions must go hand-in-hand with support for (or at least acceptance of) democratic ideology is subsequently stated so often that we worried it might become annoying.

Second, we devote an entire chapter to a discussion of the manner in which mankind creates culture and then, through the evolved trait that we call “indoctrinability,” culture frequently “triumphs over nature, even over some of our most basic drives” (Somit and Peterson, 1997:77). And, to cite only one of the many passages developing this theme, we later write: “There is almost no limit to the range and variety, or eccentricity, of the values humans are capable of accepting and acting upon. This is true in religion, in philosophy, in ethics, in art—and politics” (1997:78).

Third, successful democracy, we assert, requires not only favorable enabling conditions but belief in, and support for, the *idea* of democracy. Human political behavior is influenced both by our evolutionary legacy and by our political beliefs. Then, to further illustrate the manner in which culture can shape political behavior, we devote another chapter to describing how the growing acceptance of democratic doctrines opened the way for that then-rare polity, democratic government.

As most students of political philosophy know, the idea of majority rule (or popular democracy) has been a very late bloomer. Until the end of the eighteenth century, H. B. Mayo remarked, “democracy was not thought well of by most people, and certainly not by most of the educated or ruling classes” (quoted in Somit and Peterson, 1997:99). The turning point in the United States is usually dated from the “Age of Jackson”; in Europe, a bit later. After that, if we may quote ourselves, “the pendulum swung swiftly to the opposite end. Barely a century and a half later, mass attitudes had changed to where, as Sartori ironically remarked, the term had become so sacred that now almost no one dares openly to say that he is antidemocratic” (1997:99).

Of course, culture (transmitted through nurture) significantly influences political behavior. We emphasize that at the very outset; it is the central subject of two chapters; it is the assumption on which we base our closing chapter’s “Prescriptions” for preserving democracy; it is a theme sounded repeatedly in other chapters.

How, then, to account for Corning’s accusation that, by “biologizing human behaviors,” we fail to give adequate weight to the role of culture in political life? This is truly a puzzlement.

The answer may be—and we realized this only after a careful rereading of his commentary—that we have here a grave terminological misunderstanding. We used the term “indoctrinability” to describe that unique human capability of creating culture and then of having our actions profoundly influenced by those cultural creations, even when

the resulting behavior runs counter to our inherent inclinations. That is a phenomenon whereby humans become, in Jonathan Kingdon's succinct phrase, "artifacts of their own artifacts" (Kingdon, 1993:3).

Coined by Irenaus Eibl-Eibesfeldt, probably the leading ethologist of his day, *indoctrinability* is surely an infelicitous term. Nonetheless, we could think of no better way of describing the trait which makes it possible for culture to shape our behavior. We used the term, consequently, as a shorthand synonym encompassing three logically distinct, but closely related, ideas—(1) culture, (2) nurture (i.e., the process whereby a society transmits its culture), and (3) the human trait that makes that transmission possible.

Corning objects to the term, calling it "lamentably constricted and insufficient" (2000:104). That may well be where the difficulty arises. If anything, we have used "indoctrinability" in a much broader (we might say "unconstricted") sense than he takes it to mean. For us, it automatically entailed a reference to nurture; for him, it seems that it did not. Hence his criticism—and our resulting puzzlement. We thought that our usage was quite clear; at least for him, apparently, it was not. If so, we are guilty of not being sufficiently precise. But of "biologizing human behavior"? No way.

Corning says that we have "discounted" the rise of democracy and of democratic institutions. Somehow, this brings to mind former President Clinton's boast that he had "restored democracy to Haiti." When did the unfortunate Haitians *ever* have a democracy—before, during, or after Clinton's presidency? We devote a chapter ("Will The Real Democracies Please Stand Up") to this issue, providing the reader literally a country-by-country count (not discount). For that purpose, we use the two broadly accepted criteria—majority rule and the rule of law. The result (as of 1997) was the same as that reported by Robert Dahl and other scholars. Even in what is popularly called the "Age of Democracy," governments satisfying those criteria remain very much in the minority.

Since then, sad to say, the situation has, if anything, worsened. Consider what has happened these past few years in Africa, in the Balkans, in Asia, and in the former Soviet states. We would very much like to share Corning's optimism, but the data run otherwise.

Corning also says that we give insufficient attention to self-interest and the fear of coercive power in explaining political obedience and the undeniable predominance of authoritarian, rather than democratic, polities. Since there are very few persons as knowledgeable about evolutionary theory as Peter Corning, we find this is a rather paradoxical objection. He is surely familiar with the standard neo-Darwinian explanation for the evolution of hierarchical social

structures and dominance and submission behaviors among social animals generally, and among the social primates in particular. Explicated in considerable detail in our book, that theory holds that these behavioral tendencies evolved because they would minimize the actual use of force and thus serve the self-interest of both those who are dominant and those whose lot it is to obey.

The actual resort to force, the reasoning goes, is potentially dangerous for dominants as well as for subordinates, since for both it entails the possibility of physical injury or even death. For both, accordingly, it carries the risk of lessening (or terminating) that ultimate metric of self-interest, reproductive (now usually stated as "inclusive") fitness. By lessening the need to resort to force, then, obedience and submission work, on balance, to benefit both dominants and subordinates.

There can be no question that self-interest and fear of force are factors in submission to authority. They are part of our genetic legacy. Still, if Corning prefers to explain obedience behaviors solely in terms of calculated decisions, he is surely free to do so. But, as we noted earlier, it is a remarkable position for him to take.

One final comment: Corning's thoughtful review forced us to reread our book with care and, we would like to believe, a dispassionately critical eye. On a number of points, as discussed above, we agree with him: there are some things we could have done better—or perhaps better not done at all. All in all, though, we think our thesis stands up pretty well. To put it charitably, our species is not genetically predisposed to democracy. And, on one point, we were also dead right: it has been "predictably unpopular."

Corning began his commentary by talking about Edward O. Wilson. The views expressed in the concluding chapter of his *Sociobiology* made Wilson an intellectual pariah among right-thinking individuals. Twenty-five years later, though, he is a richly honored elder statesman, and those views, somewhat but not greatly softened, are becoming conceptual coin-of-the-realm in every one of the social and behavioral sciences.

The moral? A quarter century is a terribly long time.

## References

- Corning, P.A. (2000). "The Sociobiology of Democracy: Is Authoritarianism in Our Genes?" *Politics and the Life Sciences* 19:103-8.
- Kingdon, J. (1993). *Self-Made Man: Evolution from Eden*. New York: Wiley.
- Somit, A. and S.A. Peterson (1997). *Darwinism, Dominance, and Democracy: The Biological Bases of Authoritarianism*. Westport, CT: Praeger.
- Wilson, E.O. (1998) *Consilience: The Unity of Knowledge*. New York: Alfred A. Knopf.

# The Sociobiology of Democracy Revisited: A Reply and a Reiteration

**Peter A. Corning** Institute for the Study of Complex Systems, USA

The response by Somit and Peterson to my earlier commentary in this journal (Corning, 2000), which sharply criticized their little-noticed 1997 book (by their own admission), must seem like old news that should be relegated to the obituary pages. On the contrary, it concerns one of the central political issues of our time—what sustains modern democracy, and what are its future prospects. Somit and Peterson in their modest book offered both a diagnosis and a prescription. I concluded in my essay that the result was disappointing—more for what was omitted than for any commissions. My critique involved a rather subtle argument, and, judging by the authors' response (Somit and Peterson, 2001), it seems to have been lost on them as well.

To be specific, Somit and Peterson made a set of claims that I did not fundamentally disagree with (I characterized

it as perhaps half of the truth), but I also found their thesis to be seriously oversimplified and deeply insufficient. I argued, in effect, that the causal dynamics associated both with consensual democratic processes and with coercive authoritarianism of various kinds is significantly more complex (and multifactorial) than Somit and Peterson portrayed it to be, and that any “genetic bias” (as they called it) for “hierarchy, dominance and submission”—a common primate pattern—has been “overwritten” by perhaps five million years of human evolution in the context of socially organized, interdependent cooperating groups whose relationships do not begin and end with “dominance.” As Darwin himself suggested, “family selection” (a.k.a. kin selection) and “group selection” may well have had a moderating/socializing influence on the “selfish gene” in human evolution. (Dominance is a term of art borrowed from ethology that looks to be increasingly murky in the context of modern primate research showing that many primates—especially the Great Apes—have very complex social systems. Indeed, even chickens don't behave quite the way Schjelderup-Ebbe—the originator of the pecking-order concept—assumed was the case.)

In other words, “human nature” has many facets; genetic influences are at once multi-dimensional and, more important, deeply entangled with developmental, cultural, economic and political influences—call it nurture (see the extended discussions in Corning, 2003a, c; 2004; 2005). I also pointed out in my essay that hierarchies in modern societies often have a functional basis that may actually be consensual (democratic) and have nothing to do with any supposed genetic bias; a reductionist explanation may be gratuitous and unnecessary. I'll elaborate on all this below.

I am saddened that Somit and Peterson did not really respond to these criticisms. When academicians take to referring to an adversary as “a long and valued friend”—a commonplace and often insincere practice in partisan politics—the reader should beware. However, this is not a personal matter. I would have preferred not to confront these two distinguished and respected colleagues. I was concerned (and I still am) that an inadequate diagnosis of the causes and the cure for what ails democracy will ill serve the objective that the authors themselves say they wish to advance—not to mention the cause of the so-called biopolitics movement.

Before addressing some of the specific points in their response, let me briefly paraphrase some (not all) of the key

---

**Peter A. Corning** is Director of the Institute for the Study of Complex Systems, 3501 Beaverton Valley Road, Friday Harbor, WA 98250, USA (e-mail: pacorning@complexsystems.org). His academic background includes a B.A. from Brown University and a Ph.D. from New York University, as well as post-doctoral training under an NIMH fellowship at the Institute for Behavioral Genetics at the University of Colorado and nine years of teaching and research at Stanford University's Human Biology Program, Institute for Political Studies, Behavior Genetics Laboratory and Engineering Economic Systems Department. Dr. Corning was also a recent senior fellow at the Collegium Budapest (Institute for Advanced Study) in Hungary. His other professional affiliations include the International Society for the Systems Sciences, where he is a recent past-president, and the International Society for Bioeconomics, where he has served as treasurer. In addition, he is a member of the Council of the Association for Politics and the Life Sciences and is on the Board of the Epic of Evolution Society, as well as an actively participating member of the International Association for Cybernetics, the Human Behavior and Evolution Society, the International Society for Endocytobiology, and the International Society for Human Ethology. He is also on the editorial boards of four scientific journals and is the author of four previous books and more than 150 scientific papers and book chapters. Another book, *Holistic Darwinism: Synergy, Cybernetics and the Bioeconomics of Evolution* will be published by the University of Chicago Press in 2005. Currently in preparation are two additional books. One is an edited volume (with Johan van der Dennen) on *The Evolution of War: An Interdisciplinary Synthesis*. The other volume is tentatively titled, *Fair Shares: The Biological Basis of Social Justice*.

points in my original commentary. (I would urge the reader to go back and read my full 2000 essay, if not the book itself.) Of central importance is the authors' claim that there is a "genetic bias" toward both dominance and submission in humans. This is, of course, a "hypothesis," not an established experimental finding. Nevertheless, it leads Somit and Peterson to the conclusion that democratic (i.e., consensual) processes are "alien" to our "primate nature" and can only be sustained by "indoctrination."

There are several reasons for doubting that this is in fact the case. One is that democracy is not the "rarity" that Somit and Peterson say it is. There is extensive ethnographic evidence—apparently overlooked by the authors—that informal democratic processes are widespread (the norm) in the small face-to-face groups and societies which, most likely, resemble those of our hunter-gatherer ancestors. Anthropologist Christopher Boehm (1999) calls it the "egalitarian syndrome." The appearance of highly authoritarian societies may in fact be a recent, pathological condition that is associated with the evolution of larger, more complex societies during the past 10,000 years. There may be distinct vulnerabilities to, and opportunities for, the rise of coercive, exploitative hierarchies in large-scale societies.

Indeed, there is evidence of various kinds—from child psychology to the management sciences and behavioral economics (which I cited in my essay)—that resistance to dominance, a need for autonomy (call it "freedom") and a desire to participate in group decisions/actions are equally "natural" in humans and provide a countervailing aspect of human nature, along with such social/psychological forces as group identification (patriotism), susceptibilities to social and peer pressures, "reputational" concerns, altruistic impulses, a sense of "fairness" (there is growing experimental evidence of this), and even moralistic aggression (see Corning 2003c). In other words, any bias toward dominance must be viewed as part of a far more complex behavioral mosaic. There is even some evidence for participatory group decision making in other social animals (Conradt and Roper 2003; Corning 2003c). The implication that "indoctrination" is an artifice, and the only thing that stands in the way of authoritarianism, is flatly wrong; culture (our responsiveness to social norms, rules, even ethics) is also a part of human nature (for an extended treatment of this crucial issue, see Ridley, 2003). To repeat what I said in the original essay, it is evident that Somit and Peterson became the captives of their categories.

A third point is that a fundamental distinction must be drawn between the many hierarchies in large, complex organizations (and societies) that are both functionally based and consensually acceptable to the participants (call it a democratic hierarchy) and the coercive, self-serving dominance hierarchies that mimic the classic zero-sum paradigm. Indeed, Somit and Peterson blurred an all-important distinction between "leadership" and "dominance" in their book, and said little about socially-defined "roles." The

former can be explained without recourse to the "whisperings within" (as David Barash characterized it), and the latter can also be explained (often more parsimoniously) in terms of the calculus of economic and political self-interest (which, needless to say, entails a great many of our genes; there is no gene for self-interest). By the same token, the dominance of a leader may derive from the power of the office, not from his or her genes. As Somerset Maugham observed in the opening lines of his classic novel, *The Moon and Sixpence*, the "greatness" achieved by a leader may be "a quality which belongs to the place he occupies rather than to the man... The Prime Minister out of office is seen, too often, as a pompous rhetorician" (for more on this, see Rubin, 2003; Corning, 2004).

Another problem with the genetic bias hypothesis is that it is based on less than air-tight "circumstantial evidence"—e.g., ethological studies in other species, the political history of recent centuries, Stanley Milgram's famous experiments on obedience to authority, and the like. (We still do not know how much of what Milgram found was a reflection of previously learned responses; obedience to authority is also something we start learning—the hard way—in childhood, after all, and the prisons are filled with people who didn't get the message.)

Somit and Peterson also take me to task for criticizing them for "biologizing human behaviors." In fact, I was paraphrasing a direct quote from Edward O. Wilson only two paragraphs earlier in my essay; Wilson also warned us (recently) that human behavior genetics is "still in its infancy." What I actually said was that we should therefore be "extremely cautious about biologizing human behaviors, especially when it involves something as complex and overlain with cultural influences as democracy." I did not say that it should not be done at all.

However, I can second Wilson's motion on this point. I was formally trained in behavior genetics under a two-year post-doctoral NIMH fellowship at the Institute for Behavioral Genetics (University of Colorado) under Gerald McClearn and did some pioneering experiments in the 1970s on the genetics of different kinds of aggression in inbred strains of mice (see especially St. John and Corning, 1973; Corning, Kessler, and Kakihana, 1977). Even in laboratory mice, the genetic substrate for a given behavior—say intermale aggression—can only be inferred (at this point) from performance differences (the variance) between strains. The precise genetic substrate remains obscure. Moreover, "developmental plasticity" (call it nurture) is hardly unique to humans. It is ubiquitous in nature (including even in laboratory mice), and it is the focus of the rising new science of Evolutionary Developmental Systems (or "Evo-Devo").

Finally, and not least, biological variation is a fundamental property of the natural world (it's the basis for natural selection, after all). Thus, it may be seriously misleading to make a blanket assertion that humankind has a

“genetic bias” for something without taking account of the variance. It may well be the case that only some humans have a strong bias for dominance and submission (I will name no names), while others have a more “democratic” bias. Human nature comes in many different flavors (see the empirical evidence cited in Corning, 2003c).

If Somit and Peterson’s diagnosis for what ails democracy was deficient, their prescription for “saving” democracy was even more disappointing. Instead of an expansive vision that emphasized the full panoply of shaping influences—from a democratic political culture to institutional inventions like legal systems and the supremacy of law, elections, representative government, a free press and even public opinion polls—Somit and Peterson placed their bets on “indoctrination,” an unfortunate choice of terms. Surely these scholars know how labels can influence our perceptions, even when something more elaborate is intended (as they claim). But *was* something more elaborate intended? In fact, their prescription focused on making a more concerted effort to provide “civic education” in our schools (sounds like indoctrination to me). This will hardly suffice to solve the problem if our elites are systematically manipulating and corrupting our institutions in pursuit of various partisan and “special interest” objectives. As I put it in my essay, democracies are often “murdered” for proximate economic and political objectives rather than in pursuit of an “ultimate” genetic agenda; to survive, democracy must also be valued and supported by those who have the power to destroy it. We also need to “indoctrinate” our elites. I also find it necessary, with genuine sadness, to reply to some of the specific comments in Somit and Peterson’s response:

- On their point No. 1, well, not exactly; what was *also* missing from their discussion was a more adequate conception of human nature, no small oversight.
- On point No. 2, this was a throw-away line—hardly worthy of being cited as a criticism, which it was not.
- On points No. 3-5, these are all really facets of the same basic issue, not three different issues. There were two distinct parts to the Somit and Peterson book. One part purported to be descriptive (“social science”); the other part was pointedly “prescriptive.” I argued in my essay, and still believe, that their prescription might have carried more weight if they had prefaced it with an argument for why we *should* resist our (supposed) genetic biases, rather than simply going with the evolutionary flow as they see it. Later on the authors charge me with creating a “near-classic” Catch-22 situation, when I suggested that they could have made Winston Churchill’s famous defense of democracy the frontispiece for their book, rather than burying it in a footnote. I fail to see how this represented a Catch-22 (damned if you do; damned if you don’t). If Somit and Peterson had indeed made this quintessential bit of Churchillian word-play the frontispiece for their book, I would have been a bit

more sanguine about their sometimes skeptical, often laconic and curiously detached posture.

- On point No. 6, self-interest is a capacious “umbrella category” that characterizes all living organisms; it does not single out any particular behavior, and it can hardly be described as a “genetic factor.”
- On point No. 7, the point is that social hierarchies in humankind often do not arise from any (postulated) genetic bias toward dominance. Nor does acquiescence necessarily serve those at the bottom, as Somit and Peterson suggest. There may be more compelling proximate (economic, social, political, even military) explanations in both cases.
- On point No. 8, what Somit and Peterson discount are the many cultural/political inventions (customs, practices and institutions) that undergird and foster democracy. Formal “indoctrination” is a very small part of the overall picture.

Another corrective has to do with their helpful “reminder” that democracy dates back only a few centuries (not millennia, as I said) and that “students of political philosophy” know better. Actually, formal democratic institutions date back at least 2500 years—to the reforms instituted by the Athenian lawgiver, Solon, and it was elaborated by many of his Athenian successors—from Cleisthenes to Pericles and Demosthenes. The Greeks, after all, coined the term “*dēmokratia*” (rule by the people), and democracy was practiced in Athens and elsewhere in Greece for over two centuries during its heyday as a congeries of independent city states. (Athens was swallowed up in the Macedonian Empire after the battle of Charonea in 338 B.C.) Likewise, the early Roman republic evolved a model of “mixed government” over time (including an assembly of “*plebeians*” and elected “*tribunes*”), which conformed in spirit to what Plato and Aristotle had advocated in their writings, and which Cicero among others updated. The Roman republic lasted for several hundred years. Moreover, it is also likely that less formal democratic practices go back at least 100,000 years, and maybe more, among the small hunter-gatherer groups that migrated out of Africa and, over time, populated the rest of the globe. (Again, see Boehm, 1999; Corning, 2003a, c; also Sober and Wilson, 1998.)

A further corrective has to do with Somit and Peterson’s conclusion that my naïve “optimism” about democracy is contradicted by the data. This seriously misrepresents my position. I’m quite familiar with the data, and I too am troubled about the future of democracy, certainly in our own sadly dysfunctional country. But I also see some hope in the “learning curve” of political evolution over the past few centuries and believe that the kind of enervating negativism that Somit and Peterson purvey not only undercuts their own prescription but runs the risk of becoming a self-fulfilling prophesy. If democracy is worth saving, why not say so with conviction?

Finally, I too respect and honor Edward O. Wilson's many contributions to biology and the social sciences. (For the record, Somit and Peterson misquoted me on Wilson. I did not refer to Wilson's "famous/infamous" volume. What I actually referred to was the "famous (some would say infamous) final chapter" of *Sociobiology: The New Synthesis*, 1975, an allusion to the controversy surrounding his treatment of human nature.) However, my esteem for Wilson does not require me to accept the theoretical position that Wilson himself has softened in the years since his pioneering work was published. Early on, his "genetic leash" metaphor was drawn a bit too tightly; he underrated the rich and inextricable shaping influences of the social environment in humankind. (He is, after all, an entomologist by training and experience). Many biologically-oriented social scientists have "moved on" to a more balanced "gene-culture co-evolution" paradigm (among others, see Corning, 1983, 2003a, 2005; Boyd and Richerson, 1985; Durham, 1991; Weingart et al., 1997; Dunbar et al., 1999; Ehrlich, 2000; Ridley, 2003; Richerson and Boyd, 2004). One hopes that Somit and Peterson will ultimately be moved to move on as well.

## References

- Boehm, C. (1999). *Hierarchy in the Forest: The Evolution of Egalitarian Behavior*. Cambridge, MA: Harvard University Press.
- Boyd, R. and P.J. Richerson (1985). *Culture and the Evolutionary Process*. Chicago: University of Chicago Press.
- Conradt, L. and T.J. Roper (2003). "Group Decision-making in Animals." *Nature* 421:155-58.
- Corning, P.A. (1983). *The Synergism Hypothesis: A Theory of Progressive Evolution*. New York: McGraw-Hill
- Corning, P.A. (2000). "The Sociobiology of Democracy: Is Authoritarianism in Our Genes?" *Politics and the Life Sciences* 19:103-8.
- Corning, P.A. (2003a). *Nature's Magic: Synergy in Evolution and the Fate of Humankind*. New York: Cambridge University Press.
- Corning, P.A. (2003b). "Evolutionary Ethics: An Idea Whose Time Has Come? An Overview and Affirmation." *Politics and the Life Sciences* 22 (1):50-58.
- Corning, P.A. (2003c). "Fair Shares: Beyond Capitalism and Socialism, Or the Biological Basis of Social Justice." *Politics and the Life Sciences* 22 (2):12-32.
- Corning, P.A. (2004). "The Evolution of Politics." In F.M. Wuketits and C. Antweiler (eds.), *Handbook of Evolution*, Vol. I. Weinheim, Germany: Wiley-VCH Verlag GmbH.
- Corning, P.A. (2005). *Holistic Darwinism: Synergy, Cybernetics and the Bioeconomics of Evolution*. Chicago: University of Chicago Press (in press).
- Corning, P.A., S. Kessler, and R. Kakihana (1977). "Three Kinds of Aggressive Behavior in Laboratory Mice." *Behavior Genetics* 7(1):51-52.
- Dunbar, R., C. Knight, and C. Power (1999). *The Evolution of Culture: An Interdisciplinary View*. New Brunswick, NJ: Rutgers University Press.
- Durham, W.H. (1991). *Coevolution: Genes, Culture and Human Diversity*. Stanford, CA: Stanford University Press.
- Ehrlich, P.R. (2000). *Human Natures: Genes, Cultures and the Human Prospect*. Washington, DC: Island Press/Shearwater Books.
- Hammerstein, P. ed. (2002). *Genetic and Cultural Evolution of Cooperation*. Cambridge, MA: The MIT Press.
- Richerson, P.J. and R. Boyd (2004). *The Nature of Cultures*. Chicago: University of Chicago Press.
- Ridley, M. (2003). *Nature Via Nurture: Genes, Experience and What Makes Us Human*. New York: HarperCollins.
- Rubin, P.H. (2002). *Darwinian Politics: The Evolutionary Origins of Freedom*. New Brunswick, NJ: Rutgers University Press.
- St. John, R.D. and P.A. Corning (1973). "Maternal Aggression in Mice." *Behavioral Biology* 9:635-39.
- Sober, E. and D.S. Wilson (1998). *Unto Others: The Evolution and Psychology of Unselfish Behavior*. Cambridge, MA: Harvard University Press.
- Somit, A. and S. Peterson (2001). "Darwinism, Dominance, and Democracy: A Reaffirmation." *Politics and the Life Sciences* 20 (2):[this issue].
- Weingart, P. et al., eds. (1997). *Human by Nature: Between Biology and the Social Sciences*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Wilson, E.O. (1975). *Sociobiology: The New Synthesis*. Cambridge, MA: Harvard University Press.

## BOOK REVIEWS

# Alas, Poor Darwin: Arguments Against Evolutionary Psychology

Hilary Rose and Steven P.R. Rose (eds.)

New York: Harmony Books, 2000, 400 pp. US\$20.00 cloth. ISBN 0-609-60-513-5. Harmony Books, 202 E. 50<sup>th</sup> St., New York, NY 10022-7702, USA.

**R. Elisabeth Cornwell** University of Colorado at Colorado Springs, USA  
**Craig T. Palmer** University of Colorado at Colorado Springs, USA  
**Hasker P. Davis** University of Colorado at Colorado Springs, USA

A book reviewer is generally charged to present a balanced view of both the strengths and weaknesses of a book, but *Alas, Poor Darwin* placed us in a quandary. It was genuinely hard to find enough positive things to say to sustain even a semblance of balance. Thus, we begin by forewarning the reader that this book did not impress us.

We consider *Alas, Poor Darwin* important because it is a dangerous book, and it is dangerous for the following reasons. First, it has the potential to mislead individuals who are unfamiliar with what evolutionary psychologists actually say. Second, this book is another in a series of attacks on scientific work based more on political ideology than objective analysis (e.g., Tierney, 2000). Our strategy is to give examples of the misinterpretations and inaccuracies,

the reactionary rhetoric, the previously rebutted criticisms, and, where applicable, positive contributions.

### Introduction

The introduction, by Hilary Rose and Steven Rose, portends much of what is to follow, and it is filled with inaccuracies. Some errors, such as referring to David Buss as a sociobiologist, are trivial (p. 4). However, other errors are serious misrepresentations of other scientists' work. For example, one work that we are particularly familiar with, *A Natural History of Rape*, by Randy Thornhill and Craig Palmer, is touted by the Roses as "the nadir of evolutionary psychology's speculative fantasies" (p. 3). Instead of discussing what is actually in the Thornhill and Palmer book—an evaluation of no fewer than ten alternative evolutionary explanations and the conclusion that existing evidence does *not* warrant the claim that rape is an adaptive reproductive strategy—Rose and Rose write, "In characteristic EP style, Thornhill and Palmer argue that rape is an adaptive strategy by which otherwise sexually unsuccessful men propagate their genes by mating with fertile women" (p. 3). But this is not the end of their misrepresentation of Thornhill and Palmer's work. They go on to state that Thornhill and Palmer insult women and victims of rape "by suggesting, for example, that a tight blouse is in itself an automatic invitation to sex" (p. 3). This is very different than what the authors actually say: "he may *mistake* a woman's friendly comment or her tight blouse as an invitation to have sex when in fact sex is practically *the last thing on her mind*" (Thornhill and Palmer, 2000:179, emphasis added).

---

**R. Elisabeth Cornwell** is now completing a Ph.D. in psychology at the University of St. Andrews, Scotland. Her current research takes a Darwinian view of human development, including its influence on social behavior during adolescence and how it shapes adult mate choice preferences. Correspondence should be addressed to School of Psychology, University of St. Andrews, St. Andrews, Fife, KY169AJ, Scotland. **Craig T. Palmer** is now Assistant Professor in the Department of Anthropology at the University of Missouri-Columbia. He does research on evolutionary explanations of altruism, religion, sexual aggression, and the ecology of fishing communities. He is coauthor, with Randy Thornhill, of *A Natural History of Rape* (MIT Press, 2000). **Hasker P. Davis** received his doctoral degree in 1980 from the University of California at Berkeley, with a specialization in biological psychology. He is Professor of Psychology at the University of Colorado at Colorado Springs. His current research focus is on changes in cognition across the life span.

In addition to the obvious misquotes and misrepresentations of many well-respected scientists, the Roses intertwine a litany of social sins with sociobiology and evolutionary psychology. We are not suggesting that the Roses actually believe that scientists such as E.O. Wilson, Richard Dawkins, and David Barash are racists, misogynists, or eugenicists; however, Rose and Rose make no attempt to differentiate Nazism, eugenics, racism, and sexism from sociobiology or evolutionary psychology. The novice reading their prose might very well believe that such noxious social agendas are at the core of an evolutionary perspective of behavior (one must consider the possibility that such tactics are in part driven by a politically motivated agenda, rather than by objective scientific criticism).

### “Less Selfish than Sacred?”

Dorothy Nelkin’s chapter, which is subtitled “Genes and the Religious Impulse in Evolutionary Psychology,” provides no insight into evolutionary theory or its application to human (or nonhuman) behavior. Nelkin attempts to portray evolutionary psychologists as acolytes of a religion rather than as scientists, and she confuses ultimate explanations for particular adaptations with ultimate explanations about the religious meaning of life. Nelkin repeats many of the mistakes commonly espoused by the critics of evolutionary psychology, including the outdated and inaccurate criticism that “evolutionary principles imply genetic destiny” (p. 27). The author also commits the naturalistic fallacy, an error often made by naïve critics of sociobiology, in claiming that by defining behavior as natural, evolutionary explanations convey a message about appropriate social policies (p. 27). As an aside, Nelkin states that evolutionary psychologists dismiss their critics: “Evolutionary psychologists reject all postmodern thought, a category in which they include Afrocentrism, constructive social anthropology, eco-feminism, deep ecology, Neomarxism and New Age holism” (p. 24). While one might be tempted to declare, “That seems about right,” the truth is that criticisms from these areas have been repeatedly addressed, not merely ignored.

### “EP, Phone Home”

The chapter by Charles Jencks, with its overly clever title, provides no useful criticisms or comments regarding evolutionary psychology, or indeed anything else of a scientific nature.

### “Anti-Dawkins”

Gabriel Dover’s bombastic attack is yet another failed attempt at derailing the perspective of “the selfish gene.” He asserts that “Dawkins’ *vulgar* misappropriation of the

theory of natural selection, as embodied in the selfish gene *delusion*,” has “*infected* the studies of human psychology, animal behavior, philosophy, sociology, and medicine” (p. 56, emphasis added). The reasons why Dover should employ such nasty rhetoric in his “Anti-Dawkins” attack are not hard to find: he simply has no other ammunition, since his theory of molecular drive was long ago dismissed (Dawkins, 1987:312-16).

In his attack on Dawkins, Dover makes the rather confusing argument that since genes don’t reproduce but phenotypes do, the level of selection cannot possibly be the gene (p. 61). As Dawkins has repeatedly argued, all the paraphernalia of the living cell (whether in single- or multi-celled organisms) is indeed necessary for reproduction. This does not, in any way, negate the concept of the gene as the level of selection. The phenotype is clearly the unit of *something*, but it isn’t selection. In *The Selfish Gene*, Dawkins writes:

Some people object to what they see as an excessively gene-centered view of evolution. After all, they argue, it is whole individuals with all their genes who actually live or die. ...Just as whole boats win or lose races, it is indeed individuals who live or die, and the immediate manifestation of natural selection is nearly always at the individual level. (1989:45)

Dover is also critical of what he projects to be Dawkins’s view of adaptations. He writes, “For Dawkins all structures are adaptations (defined as having arisen through the process of natural selection) and all are ‘improbable perfections’ that could only have arisen by natural selection” (p. 58). He misleads the reader into believing that Dawkins sees all aspects of phenotypes as adaptations, and all adaptations as perfection. This assertion that evolutionary psychologists hold that all parts of all living organisms are adaptations is probably the single most often repeated misrepresentation in the book as a whole.

Dover never divulges any theory or empirical data that contradicts the underlying premise of selection at the level of the gene, and his meanderings about molecular drive, jumping genes, and phenotypes simply entangle the reader in a web of incoherent nonsense.

### “Why Memes?”

This is not Mary Midgley’s first attack on Dawkins (see Midgley, 1979a, b), and her essay is enough to take one’s breath away. We were left to wonder just why she seems so obsessed with the man, particularly when Dawkins has previously replied to her complaints (Dawkins, 1981).

Midgley’s attack on memes and evolutionary psychology conveniently overlooks what Dawkins has actually said concerning memes. For example, she writes about how Dawkins came up with the term *memes*, saying

that it is “evidently modeled on *phoneme*,” and then gives an analysis of phonemes (pp. 80-81). Dawkins’s own telling of how he came up with the term *meme* had not the remotest connection with phonemes (Dawkins, 1989:192). Rather than take up a great deal of space highlighting the numerous misunderstandings by Midgely concerning memes and evolutionary theory, we suggest that interested readers review Dawkins’s reply to her previous attacks, “In Defense of Selfish Genes” (1981).

### “More Things in Heaven and Earth”

Stephen Jay Gould’s chapter is recycled from an essay in the *New York Review of Books*. He begins here by accusing evolutionary psychologists of being Darwinian *fundamentalists* or *ultras*, and then proceeds to a critique of Daniel Dennett’s book, *Darwin’s Dangerous Idea*. Gould argues that there are three major claims at the core of evolutionary psychology that contain both strengths and weaknesses. For Gould, they turn out to be severe flaws for the development of evolutionary psychology as a science. These core claims, according to Gould, are modularity of the brain, human universals, and an adaptationist view of human behavior. We share some of Gould’s concerns about the modularity of the brain, and currently the extent and level of modularity is an open debate within evolutionary psychology. In the case of human universals, Gould and others (e.g., Steven Rose, pp. 306-307) criticize evolutionary psychologists for not seriously considering the possibility of evolutionary changes in *Homo sapiens* since the Pleistocene. But, this is a hotly debated issue among evolutionary theorists themselves, and there are a number of scientists who share Gould’s and Rose’s view on this issue (e.g., Rushton, 2000; MacDonald, 1994).

For Gould, the fatal flaw of evolutionary psychology is its continuation of a version of sociobiology’s adaptationist program. This has been Gould’s main criticism of sociobiology/evolutionary psychology for the last 25 years, and evolutionary biologists and psychologists have repeatedly replied to it (Alcock, 2000; Buss et al., 1998; Mayr, 1983). Yet, critiques of evolutionary psychology continue to focus on alleged adaptationism. Evolutionary psychologists are well aware that not all behaviors are adaptations, and they rigorously test alternative hypotheses. Scientists have responded to the criticisms of Gould and then moved on to successful programs investigating evolutionary explanations of behavior, resulting in remarkable successes in studies of both animals (Alcock, 1997) and humans (Buss, 1999).

### “Colonizing the Social Sciences?”

Hilary Rose’s essay surpasses all others in its use of ideological rhetoric, misleading inferences, and inexcusable

misquotations. We shall, for brevity’s sake, overlook Rose’s political ideology, and instead discuss her disregard for academic honesty.

She refers at one point to “the sociobiologist David Barash’s rhetorical appeal in defense of his misogynist claims that men are naturally predisposed to rape, ‘If Nature is sexist don’t blame her sons’” (p. 139). Having read Barash, we were unaware of any statement, implied or explicit, that could lead anyone to assume Barash was a misogynist. Not only was the quote she noted not on the page listed in her citation, it was not in the entire chapter! Here is some of what Barash actually writes in *The Whisperings Within*:

There is probably a risk that the sociobiological understanding of male-female differences will be used to justify sexist attitudes, to defend the view that it is only “natural” for men to be aggressive and for women to be more passive, and all the rest. But, as we’ve said before, what is natural is not necessarily what is good. (1979:90)

The quote to which we believe Rose was referring is as follows: “If male-female differences are sexist, we should put the blame where it really belongs, on the greatest sexist of all: ‘Mother’ Nature!” (1979:90).

Perhaps Rose is overzealous and does not mean to misrepresent a highly respected scientist, but one must seriously question her intent or scholarship when such misquotations are repeatedly employed. For example, she writes of the “debate” concerning the “cultural status of both science and its critics,” and criticizes Paul Gross and Norman Levitt’s book *Higher Superstition* (1994). What is of particular interest is one of her footnotes:

The most arrogant and ill-informed moment in the *Higher Superstition* is surely when Gross and Levitt propose that their colleagues who teach English and who evidently lack respect for science should be fired. Their certainty that as natural scientists they could then lash up an entirely satisfactory English course takes the biscuit! (p. 152, n. 18)

What Gross and Levitt actually say is that when an individual from an English department, for example, critiques a topic such as quantum mechanics in his or her role as an academic, it is not unreasonable to have someone from physics evaluate the scholarship. Similarly, they say the reverse is true, and that if a mathematician’s scholarship were a rhetorical and stylistic analysis of mathematics papers, it would be appropriate to have a literary expert evaluate such work (1994:255-56). If we had not been familiar with the writings of these scientists, perhaps we could have been misled by Rose’s unique interpretation of their works.

### “Sewing Up the Mind”

Barbara Herrnstein-Smith begins her essay by suggesting that the mind as defined by Steven Pinker is not the sort of thing scientists should seek to explain, or even could explain (p. 156). One wonders what the alternative is—simply giving up? She then incorrectly cites Pinker, takes writings out of context, and misconstrues meaning. For example, she writes: “He [Pinker] is especially perturbed by the idea of *learning* (which he identifies, improperly and tendentiously, with mind-as-blank-slate empiricism) and goes on to great and often tortuous lengths to avoid acknowledging that our perceptual and behavioral tendencies ...” (p. 164).

In fact Pinker goes to great lengths to support the notion of learning, and has never identified learning with “mind-as-blank-slate,” but rather proposes that learning is bounded by evolved capacities within the organism (1997:33-34). Herrnstein-Smith’s attacks are peppered with acerbic comments, and she provides no arguments that might sway a serious scientist.

### “Why Babies’ Brains Are Not Swiss Army Knives”

Annette Karmiloff-Smith’s writing is clear and concise, and she does not engage in *ad hominem* attacks. The only feature of this chapter with which we take issue is her painting of all evolutionary psychologists with the same broad brush.

Karmiloff-Smith points to the importance of understanding the ontogenetic development of the human brain, and she is critical of extreme forms of modularity as proposed by some evolutionary psychologists (e.g., Tooby and Cosmides, 1992). Her suggestion that scientists studying the development of the brain should take into consideration multiple perspectives is certainly worth repeating. We encourage readers to seek out a more detailed description of the author’s work described in this chapter (see Paterson et al., 1999).

### “Taking the Stink Out of Instinct”

Patrick Bateson argues that a recurring theme of evolutionary psychology is that “human activities are organized into modular systems, each of which serves a specific use” (p. 189). He links this way of thinking about human behavior to the misconceptions of instinct by ethologists that led to the mid-twentieth century debate with American comparative psychologists. Bateson provides a nice historical sketch of the issues dividing American comparative psychologists and European ethologists. There is, however, no effective argument raised against evolutionary psychology. This is, for the most part, a reasonable chapter, although we do rather wonder what it is doing in this book.

### “Beyond Difference”

After a brief history of feminist thought concerning Darwinism, Anne Fausto-Sterling begins with the assertion that “scientifically sound theories about the evolution of human behavioral patterns and their relationship to contemporary behavior could emerge from collaboration between social scientists, evolutionists and behavioral biologists” (p. 216). To facilitate the generation of these sound theories, she recommends a model developed by Latour and Strum (1986) to evaluate their quality (p. 216). To our surprise, the purpose of Latour and Strum’s “Nine Questions” was not to evaluate the validity of an evolutionary hypothesis; rather, its intent was to evaluate the *textual* analysis of creation stories. Latour and Strum wanted to find a “starting point to give us *comparable* accounts” among “scientific stories (falsifiable) and mythical stories (unfalsifiable)” (1986:171). It is incomprehensible to us why Fausto-Sterling would “urge anyone devising theories about evolution and human behavior to use Latour and Strum’s nine questions” (p. 216), interesting as Latour and Strum’s work is in its proper context. This is another example of either an error or misrepresentation of the original article.

Although Fausto-Sterling attacks “fundamental Darwinists,” she focuses a great deal of her attention on David Buss, and most specifically on his classic 37-culture study (1989). In her attack, she cites Wallen (1989) concerning four “essential criteria” for “the acceptance of conjectures about the evolution of human reproductive behaviors” (p. 217). However, these criticisms have been repeatedly addressed and dismissed by Buss (1989) and others. The arguments presented by Fausto-Sterling contribute nothing new or enduring to the intellectual debate concerning human nature.

### “Different Strokes”/“Evolving Skills”

We consider two of the contributions together here because they so clearly illustrate the variation in value of different chapters in this book. “Different Strokes: Beyond Biological Determinism and Social Constructionism” was contributed by Tom Shakespeare and Mark Erickson; “Evolving Skills” was contributed by Tim Ingold.

Both chapters contrast evolutionary psychology, which they misportray as biological or genetic determinism, with social (Shakespeare and Erickson) or cultural (Ingold) constructionism. Then they both call for an alternative synthesis that “would seek to transcend the dichotomy, moving beyond the inadequacies of dualism” (Shakespeare and Erickson, p. 230). The difference is that Shakespeare and Erickson fail to provide such a synthesis, devoting most of their time to their unfounded attacks on their view of evolutionary psychology. Indeed, their claim that both the physical disability of a person and the social stigmatism

it provokes must be considered in studies of disabled people seems to be only a fairly simplistic continuation of the dichotomy between biological and social factors. This lack of substance means there is little else in their chapter beyond their misrepresentations of evolutionary psychology. In contrast, Ingold spends relatively little time attacking evolutionary psychology and devotes the bulk of his article to a sincere attempt at explaining just how intricately genetic and environmental factors are interwoven during ontogeny. Of particular importance is his argument that psychological mechanisms are not “innate” givens encoded by genes, but are themselves the product of gene/environment interactions. This is a point most evolutionary psychologists would probably agree with, but often fail to emphasize.

### “Social Causes and Natural Relations”

The contribution by Ted Benton offers little beyond the typical anti-evolutionary psychology accusations. In this case, the author not only accuses evolutionary psychology of “biological determinism” (p. 256) but “extreme biological determinism” (p. 263). He refers to “the reemergence, as popularized secular religion, of new forms of genetic deterministic ideology” (p. 256). However, he does bring up an important subject when he mentions paleoarchaeology. Unfortunately, instead of suggesting that evolutionary psychologists pay more attention to this form of evidence, he claims that it is too “fragmentary” to be of any use.

### “Escaping Evolutionary Psychology”

One can extract a long list of criticisms of evolutionary psychology from Steven Rose’s chapter. The list includes the following: (1) “like its predecessor, sociobiology, it offers a false unification, pursued with ideological zeal”; (2) it provides a “reductionist account in which presumed biological explanations imperialize and attempt to replace all others”; (3) it “misspeaks and impoverishes ... in three key areas: the processes of evolution, of development and of neural function”; (4) it presents a “misunderstanding of the relationship between enabling and causal mechanisms”; and (5) it attempts to “privilege distal over proximal causes” (pp. 299-300). Rose also implies that evolutionary psychologists believe that all features of phenotypes are adaptations (pp. 302-03), or, “if not all then most observed characters must be adaptive” (p. 313). Rose’s last major criticism is of evolutionary psychologists’ proposed modularity of the brain (pp. 314-16).

These complaints have all been made in other contributions to this book, and all have been repeatedly addressed and dismissed by us, or previously addressed and dismissed by others. We shall consider two blatant errors or misrepresentations from this chapter. First, Rose

implies that evolutionary explanations of violence in the United States make no contribution to our appreciation or prevention of crime (p. 305). Instead, he suggests that we should look to the more proximate cause of handgun availability in the United States. This implies a competition between ultimate and proximate explanations. Actually, of course, they are complementary. Steven Rose must ask, “Does the availability of handguns explain why there are so few gun-toting grannies in the United States in comparison to gun-toting young males?” The answer to this question would give us an ultimate explanation, while the availability of handguns provides a proximate explanation.

Second, Rose states that evolutionary psychologists see virtually all phenotypic characteristics as adaptations (p. 313). This assertion is followed by a retelling of Thayer’s idea that “the flamingo’s pink coloration is an adaptation to make them less visible to predators against the pink evening sky” (p. 313). It is not a stretch to imagine that a naïve reader, given the context of this statement, might conclude that this is a contemporary idea posited by a modern evolutionary psychologist. Rose fails to make clear to the reader that Thayer is a nineteenth-century artist. This type of academic dishonesty must be noted and confronted.

### Conclusion

Our review of *Alas, Poor Darwin* has focused on numerous misrepresentations of the ideas and statements of sociobiologists and evolutionary psychologists. The two senior authors of this review are gravely concerned with the tactics of this book. There are misquotes, misrepresentations, and a thinly veiled political agenda. We have documented a few of the misrepresentations, misquotations, and acerbic comments that occur throughout *Alas, Poor Darwin*. Scientists of considerable reputation, such as Richard C. Lewontin, who lend their name and scientific status to a dust jacket endorsement of this type of academic dishonesty, owe an answer to the scientific community. The scientific community as a whole owes to itself and the public an obligation to speak out against such tactics.

At the time this review was written, R. Elisabeth Cornwell was an undergraduate student, and she raised another serious question: what do the misleading statements, inaccurate citations, and out-and-out misrepresentations of other scientists’ work found throughout *Alas, Poor Darwin* imply to her and her peers? If the scientists whose work students study is so filled with egregious errors and poor scholarship, is it license for students to follow in their footsteps?

### Acknowledgments

Special thanks to Gerald Broce, Richard Dawkins, and David Perrett for their comments and invaluable insights.

## References

- Alcock, J. (1997). *Animal Behavior: An Evolutionary Perspective*. Sixth edition. Sunderland, MA: Sinauer.
- Alcock, J. (2000). "Misbehavior: How Stephen Jay Gould Is Wrong about Evolution." *Boston Review* 24:1-7. Available at <http://bostonreview.mit.edu/BR25.2/alcock.html>.
- Barash, D. (1979) *The Whisperings Within: Evolution and the Origin of Human Nature*. New York: Harper and Row.
- Buss, D.M. (1989). "Sex Differences in Human Mate Preferences: Evolutionary Hypotheses Tested in 37 Cultures." *Behavioral and Brain Sciences* 12:1-49.
- Buss, D.M. (1999). *Evolutionary Psychology: The New Science of the Mind*. Needham Heights, MA: Allyn and Bacon.
- Buss, D.M., M.G. Haselton, T.K. Shackelford, A.L. Bleske, and J.C. Wakefield (1998). "Adaptations, Exaptations, and Spandrels." *American Psychologist* 53:533-48.
- Dawkins, R. (1981). "In Defense of Selfish Genes." *Philosophy* 56:556-73.
- Dawkins, R. (1987). *The Blind Watchmaker: Why the Evidence of Evolution Reveals a Universe Without Design*. New York: Norton.
- Dawkins, R. (1989). *The Selfish Gene*. Second edition. New York: Oxford University Press.
- Gross, P.R. and N. Levitt (1994). *Higher Superstition: The Academic Left and Its Quarrels with Science*. Baltimore: Johns Hopkins University Press.
- Latour, B. and S. Strum (1986). "Human Social Origins: Oh Please, Tell Us Another Story." *Journal of Social and Biological Structures* 9:169-87.
- MacDonald, K. (1994). *A People that Shall Dwell Alone: Judaism as an Evolutionary Group Strategy*. Second edition. Westport: Greenwood.
- Mayr, E. (1983). "How to Carry Out the Adaptationist Program." *American Naturalist* 121:324-33.
- Midgley, M. (1979a). *Beast and Man*. Hassocks: Harvester Press.
- Midgley, M. (1979b). "Gene-Juggling." *Philosophy* 54:439-58.
- Paterson, S.J., J.H. Brown, M.K. Gsoedl, M.H. Johnson, and A. Karmiloff-Smith (1999). "Cognitive Modularity and Genetic Disorders." *Science* 28:2355-58.
- Pinker, S. (1997). *How the Mind Works*. New York: Norton.
- Rushton, J.P. (2000). *Race, Evolution, and Behavior: A Life History Perspective*. Third edition. Port Huron: Charles Darwin Research Institute Press.
- Thornhill, R., and C.T. Palmer (2000). *A Natural History of Rape: Biological Bases of Sexual Coercion*. Cambridge: MIT Press.
- Tierney, P. (2000). *Darkness in El Dorado: How Scientists and Journalists Devastated the Amazon*. New York: Norton.
- Tooby, J. and L. Cosmides (1992). "The Psychological Foundations of Culture." In J. Barkow, L. Cosmides, and J. Tooby (eds.), *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*. New York: Oxford University Press.
- Wallen, K. (1989). "Mate Selection, Economics and Selection." *Behavioral and Brain Sciences* 12:37-38.

# Listening to the Sea: The Politics of Improving Environmental Protection

Robert Jay Wilder

Pittsburgh, PA: University of Pittsburgh Press, 1998, 269 pp. US\$19.95 paper. ISBN 0-8229-5663-2. University of Pittsburgh Press, 3347 Forbes Avenue, Pittsburgh, PA 15261, USA.

Michael M. Gunter, Jr. Rollins College, USA

Robert Wilder offers a "complex, holistic, and organic view" of marine law (p. 183) in *Listening to the Sea: The Politics of Improving Environmental Protection*. Making a passionate plea for greater integration of the precautionary principle into ocean policy, Wilder writes from a po-

litical scientist's perspective in both describing marine law historically and outlining a blueprint for how ocean policy may be improved in the United States. Wilder emphasizes the interdependent nature of oceans and employs a "cross pollination" (p. 187) approach that is attractive to an interdisciplinary or multidisciplinary readership. Too often, as Wilder states, marine law "policies continue to be divisive or focus exclusively on one resource, and in that way are quite unlike the highly interconnected life of the sea" (p. xiii). To be truly effective, marine policy must be more like the resources it seeks to protect. It must exude interdependence.

Wilder uses an array of primary data, especially official governmental correspondence, to achieve this end. His analysis begins in the first chapter with a discussion of the origins of the 3-mile territorial sea, including antecedents

---

**Michael M. Gunter, Jr.** is Assistant Professor in the Politics Department, Rollins College, Box 2762, 1000 Holt Avenue, Winter Park, FL 32789 (e-mail: [mgunter@rollins.edu](mailto:mgunter@rollins.edu)). He holds a Ph.D. in Political Science and an interdisciplinary certificate in Environmental Systems from the University of Kentucky. Gunter recently completed his dissertation, entitled "Building the Next Ark: NGO Strategies in Protecting Transnational Biodiversity," thanks to a Sidney Ulmer Fellowship and a Dissertation Enhancement Grant from the University of Kentucky.

such as the cannon-shot rule and line-of-sight doctrine. From British naval superiority and pirate smuggling to U.S.-Japanese economic disputes over salmon to military conflicts amongst the English, French, and Spanish, Wilder details how politics has consistently trumped science when it comes to formulating policy.

Subsequent chapters build on this base. Chapter 2 examines divisions in domestic jurisdiction, with a focus on the “Tidelands Debate” and oil extraction. It is a solid description of the federalism fight, although probably too detailed for the general reader. The third chapter concentrates on the ecology of the sea and biodiversity, offering a wealth of information to support the precautionary principle. Chapter 4 describes the management of offshore oil and gas, pointing out that benefits tend to be distributed nationally while costs are concentrated locally (p. 113). Chapter 5 serves as the heart of Wilder’s argument by outlining how to better achieve energy efficiency by becoming more ecologically aware. Chapter 6 describes the basic obstacles to integrating science and policy, especially bureaucratic compartmentalization that appears as a “mass of unrelated domains” (p. xiv). Chapter 7 offers six general steps to improving protection measures, such as linking activities on land to the impact they have on the sea (as land-based sources cause most contamination of the sea) and expressly integrating the precautionary principle into these interrelationships (p. 210).

Wilder’s strength throughout these chapters is his ability to string together changes in policy as a coherent story about the sea. For example, in describing the evolution of the United Nations Convention on the Law of the Sea, from UNCLOS I to UNCLOS II to UNCLOS III, Wilder deftly notes a number of political variables, including the impact of Cold War power struggles like the controversy that swirled around submarines, specifically whether subs would be required to surface as they passed through territorial waters (p. 83).<sup>1</sup> Examples such as this demonstrate the extent to which politics permeates marine law and provide still another testament of the extent to which policymaking is a messy world where “poorly defined problems and incomplete information” dictate the agenda (p. 98).

Another component of Wilder’s story worth noting, particularly given the ongoing debate about energy supply and demand in the United States, positions efficiency and conservation versus increasing supply. President Bush’s current “energy plan” emphasizes the latter at the expense of the former, an approach that clashes with Wilder’s recommendations.

Wilder’s discussion is intriguing to those who would like to learn more about this debate, particularly in the context of competing federal and state interests in Florida, where varying combinations of tourism and quality of life are seen as nonnegotiable when it comes to leases for offshore oil or natural gas drilling by an overwhelming number of citizens and political representatives alike. Wilder provides an in-

teresting history to this debate that in the summer of 2001 literally pitted brother against brother (Governor Jeb Bush of Florida versus President George W. Bush).

This also leads to one suggestion Wilder may want to incorporate in future editions of this text. States’ preferences are trumpeted throughout the text (ironically, this is typically a conservative theme), but Wilder fails to address potential conflicts between states adjacent to one another when radically different policies are implemented. This is currently the case with Florida as opposed to Alabama and Mississippi (and Louisiana and Texas for that matter), states that are more receptive to offshore leases in the Panhandle area and Gulf generally (pp. 121-124). Another minor critique of Wilder’s work involves his discussion of the conservative versus liberal debate over gas. Wilder says liberals may argue that a gas tax is regressive because gas is inelastic, especially for the poor who can least afford any increases (p. 117). This is partly true—but not completely. People can drive less if they make major changes, such as living closer to where they work. The fact that the poor often cannot afford to relocate does help support Wilder’s general contention, of course. But certainly poor and rich alike travel less when gas prices rise, so this commodity is not entirely inelastic.

These secondary points aside, Wilder makes a valuable contribution to the literature in the marine law and policy arena. For one, his work is another valued voice sounding the attributes of the precautionary principle, specifically supplementing this chorus by providing a political analysis of U.S. policies at the close of the twentieth century. Wilder also offers an “ecological imperative” (p. 205) as a new yardstick for assessing marine law and policy. He cautions against overreliance upon the age-old canary-in-the-mine-shaft approach as our warning that oceans are in distress, and convincingly argues that twenty-first-century policies must move beyond this archaic tactic. Perhaps most notable among Wilder’s contributions, though, are the extent to which he highlights the deficiencies in thought that permeate U.S. oceans policy today as well as the specific suggestions he offers on how to correct these deficiencies. Examples include the aforementioned six general steps listed in the concluding chapter and thirty specific recommendations that offer even greater detail (pp. 211-212).

These suggestions range from educational measures similar to those proposed by David Orr (1992) to the green taxes prominent in works by Paul Hawken (1993). Wilder also emphasizes the need to foster continued development of local knowledge, noting the role of transparency and public participation. Indeed, greater wisdom among ordinary people is a critical component of effective ocean protection. Wilder notes that more must follow in the footsteps of Jacques Cousteau<sup>2</sup> and dedicate themselves to teaching the mass public. Wilder cites the simple power of aquariums (p. 196) in achieving this objective to understand the sea around us in ways that call to mind another pathbreak-

ing author, Rachel Carson (1951). Two praiseworthy efforts along these lines are the Baltimore and Chattanooga aquariums, but both need to attract even more local interest by instituting such programs as Baltimore's dollar-day special offered each year to honor the anniversary of the aquarium's opening.

Truly effective ocean protection policies are only possible if governments better integrate their regulatory attempts—a level of integration that demands recognition of the complex interdependence that permeates the oceans from the high seas to the continental shelf to the tidelands and the terrestrial sources of pollution. Policymakers must stop their endless shell games that position the sea as the “ultimate receiving medium” (p. 208). Sector-by-sector approaches produce nothing more than piecemeal legislation. The public role here is to demand consistent application of the precautionary principle so that threats from bioaccumulation to by-catch (which can end up hurting the target fish as well, due to interdependence) are minimized. Wilder provides a well-researched and passionate plea for precisely this level of consideration. Policymakers and laypersons alike should *listen* to him.

## Notes

1. As Wilder explains, this was a controversy because coming to the surface is required by innocent passage, and, of course, coming to the surface makes submarines an easy target. Not as well explained, though, is the term innocent passage itself. The next edition would benefit from a glossary of international legal terms such as this one. A time line or flow chart of laws discussed, particularly during the extensive chapter 5 discussion, would also be helpful.
2. Cousteau's contributions range from publications such as *The Silent World* (1953), *The Living Sea* (1962), and *World Without Sun* (1964), to his 1968 to 1976 television series—“*The Undersea World of Jacques Cousteau*”—which utilized the research vessel *Calypso* to stress the importance of ocean conservation, to The Cousteau Society, an organization that raises funds for ocean exploration, research, and conservation.

## References

- Carson, R. (1951). *The Sea Around Us*. New York: Oxford University Press.
- Hawken, P. (1993). *The Ecology of Commerce: A Declaration of Sustainability*. New York: HarperCollins.
- Orr, D.W. (1992). *Ecological Literacy: Education and the Transition to a Postmodern World*. Albany: State University of New York Press.

# Why We Feel: The Science of Human Emotions

Victor S. Johnston

Cambridge, MA: Perseus Books, 1999, 210 pp. US\$16 paper. ISBN 0-7382-0316-5. Perseus Publishing, 11 Cambridge Center, Cambridge, MA 02142, USA.

**Michael Hammond** University of Toronto, Canada

*Why We Feel* is a good news and bad news book. First, the good news. This book is actually two mini-books combined. There is an excellent account of why we perceive in

**Michael Hammond** teaches sociology at the University of Toronto. His research interests focus on the social history of humans studying their own evolution, and on theoretical modeling rooted in evolutionary principles. The latest publication in his model on the consequences of the uneven neurophysiological changes in the evolution of our cognitive capacities, affective capabilities, and arousal-arousal conversion rules is “Arouser Depreciation and the Expansion of Social Inequality” (pp. 339-358 of David Franks and Thomas Smith [eds.], *Mind, Brain, and Society: Towards a Neurosociology of Emotion*, Stamford, CT: JAI Press, 1999). Correspondence should be addressed to Department of Sociology, University of Toronto, 725 Spadina Avenue, Toronto, ON, M5S 2J4 Canada (E-mail: mhammond@attcanada.net).

certain patterns, especially in regard to the human face and to the concept of beauty. In particular, Johnston's description of his Face Prints program is utterly fascinating. It is one of the outstanding practical applications of Darwinian algorithms to increase the capacity of individuals to recreate faces they have seen for even the shortest of periods. Perhaps a future episode of *Law and Order* will feature an evolutionary psychologist whose revolutionary new technology plays a key role in solving a crime. There is also a fine summary of why we express emotions in certain patterns. The text reviews much of what we know at present about the evolutionary origins of emotions and the role of neurophysiology in emotions. Each section is well worth the price of the book.

However, the bad news is that the author tries to bundle these two sections together with a most unfortunate con-

cept. In claiming to offer a “radical new view of reality,” the argument is presented that our perceptions and feelings are illusions, because they are not perfectly accurate representations of the physical and social worlds around us. To be sure, these illusions are not random or arbitrary, because they have been shaped by natural selection. But they are still illusions. I wish the author well. A term like *illusion* is much more cognitively sexy than a more accurate term like *biased perception*. It could be that the book’s reproductive success in terms of total sales will be increased. But I believe that any such success will be short-lived. A notion like “illusion” might catch some people’s attention, but it will not withstand much close analysis.

It is true that scientists must constantly stress that our perceptions and feelings only capture a part of the total reality surrounding us. There is no real “red.” Red is only a convenient label we use to highlight one small part of one aspect of the world. However, it is something else altogether to set up a false dichotomy between something we might label as immaculate perception and illusion. Our perceptions of the world are not pure and free from fault, but that does not mean that therefore we should use the label of illusions. Instead, they are partial perceptions based on parts whose utility of being perceived has proven to be useful over time in terms of Darwinian principles of natural selection. Beauty is a partial perception whose biases are not random or arbitrary. Some of these biases are a product of selection and are wired into our brains. Of course, if Johnston uses a term like *biased perception*, then there cannot be a claim for a “radical new view of reality,” since this concept was widespread in the late nineteenth century and onwards.

*Why We Feel* tries to conflate three categories of existence into a single term. There are things that clearly have an existence outside our perception of them. A red apple really is something, even if our labeling terms for it are incomplete. There are also things that do not have an existence outside our perception of them. Indeed, Johnston begins the book with a tragic description of a young schizophrenic with a head full of monsters. Unable to distinguish what is and is not illusory, the fate of this individual is precarious. Finally, there are things that may or may not have an existence outside our perception of them. To use a single term such as *illusion* to cover all these possibilities is more confusing than enlightening.

Things become quickly confused with the sweeping use of this terminology. Take the following examples. I have a feeling that my brother-in-law really dislikes me. How do I get that feeling? Because he attacked me with the carving knife over dinner last Thanksgiving. In the face of this very real physical threat, my feelings will certainly not capture the entire spectrum of the situation. Their expression will be biased in terms of a rather narrow set of parameters established over countless generations by natural selection. But is it really useful to label my reactions as illusions? Now take an alternative scenario. I have a feeling that my brother-in-law really dislikes me. How do I get that feeling? Because he didn’t talk with me during dinner last Thanksgiving, even though he did talk with me during previous Thanksgiving dinners. My perception of intense dislike may or may not be an illusion in this second situation. It is important that we have a scientific vocabulary that can make a distinction between such cases. There is a spectrum of biased perception, with illusion at one end. We cannot use one end of an ideal-type construction to describe the entire construction.

Furthermore, Johnston’s use of the term *illusion* makes it more difficult to consider certain important aspects of the natural selection of our favorite new primate, because there might be situations in which real illusions have selective advantages. For instance, take the case of religion and strong feelings. Many evolutionary psychologists have made the argument that it is a most useful illusion to believe that the world is populated with extraordinary spirits and beings having capacities humans can only dream of. This illusion appears to increase psychological well-being in a great many different circumstances. It also appears to have a positive impact on the recovery from a vast variety of diseases, both physiological and psychological. As a result, the illusion can have very real selective advantages. But how to distinguish this possibility if everything involving strong feelings is an illusion? What is gained scientifically by equating these illusions with the illusions experienced when—alone, unarmed, and weaponless—we have to face a hungry-looking carnivore or an enraged brother-in-law?

Of course, every weakness has a strength. Even though flawed, the illusion paradigm would be very interesting for an undergraduate class to chew over. Combined with the really excellent sections on biased perception and the evolution of emotions, this book would be ideal for such a class, especially since it is so reasonably priced.

# Mean Genes: From Sex to Money to Food: Taming Our Primal Instincts

J. Burnham and J. Phelan

Cambridge, MA: Perseus Publishing, 2000, 263 pp. ISBN 0-738-20230-4. Perseus Publishing, 11 Cambridge Center, Cambridge, MA 02142, USA.

**Nicole C. Hess**  
**Edward H. Hagen**

University of California—Santa Barbara, USA  
Humboldt University of Berlin, Germany

Combining Richard Dawkins with *Dear Abby*, Harvard economist Terry Burnham and UCLA biologist Jay Phelan are two of the first to publish what will no doubt be a flood of self-help books based on evolutionary psychology. In *Mean Genes: From Sex to Money to Food: Taming our Primal Instincts*, the authors argue that we are the unwitting henchmen of genes inherited from distant ancestors, genes that expect dinner to come only after a nasty row with a hyena, but that instead find themselves with a plate full of pork chops at the local diner. This mismatch between the dangerous and uncertain world of our ancestors, and the comparatively safe cities and suburbs enjoyed by inhabitants of the First World is, according to the authors, the source of the inner conflicts that trouble us all. That is, genes that were beneficial to reproduction in ancestral environments can nowadays have harmful effects on our physical and social well-being, engendering painful intrapsychic conflicts. In this short, easy-to-read book intended for a popular audience, Burnham and Phelan present an impressive number of findings from evolutionary psychological and sociobiological research on human and nonhuman behavior in an attempt to explain, from their mean genes

perspective, why people frequently feel driven to do things they *know* are bad. The human data come from a variety of sources, but often focus on those non-Western peoples living in conditions similar to those our ancestors evolved in. At the end of each breezy and often entertaining chapter, Burnham and Phelan offer practical self-help advice for *overcoming* our mean genes and living up to our rational and moral senses, with the general advice being: anticipate what your genes are going to make you do, and then prevent them from making you do it. Interspersed throughout the book are amusing illustrations of the authors' experiences with overcoming their "mean genes."

The mean genes Burnham and Phelan describe include, for example, a lack of motivation to save money, an urge to eat to excess, a desire to take drugs, the seemingly superficial importance we attach to beauty, and extramarital attraction. In the section on beauty, the authors first play Dawkins, clearly explaining a variety of results that do not make sense except in light of evolutionary theory. For example, they review findings that symmetrical individuals are more attractive as mates (Bill Clinton is apparently about as symmetrical as a male model), and explain that this may be because symmetry indicates high quality, quality you want in a mate. Because a 0.7 waist-to-hip ratio is reliably correlated with higher fertility, men from many cultures prefer a 0.7 ratio in women, regardless of overall body mass (both Twiggy and Marilyn Monroe had 0.7 ratios). Men also prefer women with younger-looking features (e.g., large eyes) because, in women, youth is correlated with fertility.

In *Dear Abby* mode, the authors wisely recommend that we anticipate our often unconscious tendencies to prefer attractive people to unattractive people, and try to prevent ourselves from conferring unfair advantages based on beauty. They then blandly suggest that, while we can't do much to change our looks, looks alone do not determine attractiveness, so work on your personality if you're not good-looking. How about some real advice: get that face-lift if you think appearing younger will help you get what

---

**Nicole Hess** is a doctoral candidate in biological anthropology at the University of California, Santa Barbara. She is researching the evolutionary psychology of female friendship and gossip, and is currently a visiting scientist at the Institute for Theoretical Biology at Humboldt University in Berlin. **Edward Hagen** received his Ph.D. in biological anthropology at the University of California, Santa Barbara, and is currently a postdoctoral fellow at the Institute for Theoretical Biology in Berlin. He is investigating the evolutionary psychology of postpartum depression and parental investment, and conducts fieldwork with Amazonian hunter-horticulturists. Correspondence may be addressed to Ms. Hess at Department of Anthropology, University of California, Santa Barbara, CA 93106 (E-mail: n.hess@biologie.hu-berlin.de) and Dr. Hagen at the Institute for Theoretical Biology, Humboldt-Universität zu Berlin, Invalidenstrasse 43, 10115 Berlin, Germany.

you want in this unfair world; wear sunscreen every day; pay for braces so your kids get the benefits of looking more symmetrical; and if you're female, wear clothes that help you fake a 0.7 ratio.

There is a more fundamental problem with Burnham and Phelan's presentation, however, than the occasional piece of bland advice. Their conceptualization of the relationship between genes and behavior leaves out a key concept—psychological adaptations—an omission that undermines much of the book. Evolutionary psychologists look at adaptations, *not* genes, and there is an excellent reason for this. As critics of sociobiology never tire of pointing out, we understand very little about the relationship between genes and behavior. This truth would seem to cut evolutionary psychology off at the knees. The key insight, however, is that one infers adaptation not from genes but from *evidence of design*. We know without doubt that the uterus is an adaptation, not because we know which genes build uteruses (we don't), but because the features of the uterus show overwhelming evidence that they have been designed to solve the critical reproductive problem of containing the developing embryo for nine months, and then expelling it at birth.

Although they are sometimes identified by comparing traits across species, adaptations are most readily identified by a fit between the reproductive problems presented by the environments they evolved in, and the features they possess that are well designed to have solved those problems. Evolutionary psychologists study the fit between the features of psychological phenomena (which are evident in behavior and, for humans, in the thoughts/feelings/desires they report) and specific problems of reproduction in ancestral environments that those features would have solved. The better the fit, the more likely it is that a psychological phenomenon may be an adaptation produced by natural selection. In failing to discuss psychological adaptations, Burnham and Phelan cannot compellingly argue that the behaviors they discuss are a product of natural selection. They cannot provide the evidence of design that is central to identifying adaptations, and thus must resort to a genes-for-behavior shorthand that is unconvincing to almost everyone.

Focusing on genes instead of adaptations limits Burnham and Phelan in another way. The genes-for-behavior shorthand does not provide an adequate evolutionary framework for understanding environmentally contingent responses to the kinds of problems that are the focus of this book. If we all have genes for bad behaviors like overeating, we should all suffer from those genes. Yet most readers of self-help books, we suspect, want to know why they and not others must endure a particular hardship like obesity, drug addiction, depression, or high levels of jealousy. The concept of facultative psychological adaptations is helpful here; these are adaptations that monitor environmental inputs and activate only when they receive the right cue. Male sexual

jealousy, for example, should only activate when a man fears losing his mate to another man, and this might be particularly likely when he has a markedly lower status than other men, and when his mate is attractive enough to obtain these men. This problem is not caused by a mismatch between ancestral and modern environments. Rather, social conditions like status varied in ancestral environments and vary today in the *same* ways. People with low status faced (and continue to face) problems that people with high status didn't (and don't). It is likely that we have sophisticated psychological adaptations that were designed to respond to such variable social conditions. Consequently, only some of us may suffer from severe jealousy, even though we all have the jealousy adaptation. *Mean Genes* just won't help us identify and understand these kinds of adaptations.

The *Mean Genes* strategy of anticipating intrapsychic conflicts caused by the mismatch between ancestral and current environments, and then preventing those conflicts, also cannot solve genuine conflicts among individuals. Take the following example: a husband wants additional sexual partners. His wife, who feels that she should be the sole recipient of his affection and resources, wants to prevent her husband from seeking additional sexual partners. Which member of the partnership should win this conflict? This is a battle between one person's adaptations and another's, and the outcome will ultimately rely on the level of intensity with which each person experiences his or her adaptation (e.g., he may only feel a slight desire to cheat, whereas she feels a strong urge to prevent him from doing so); the norms of their surrounding group (e.g., a society that condones polygyny vs. one that prohibits it); power relations (e.g., the wife cannot effectively deter his actions); and other specific circumstances (e.g., the wife may wish to seek alternative mates herself). Knowledge of the psychological adaptations underlying these kinds of conflicts can inform dialogue between conflicting parties, but only if we recognize that there is a genuine conflict of interests.

There are other limits worth mentioning. First, Burnham and Phelan are successful, young, North American male professionals. The problems they address in the book are problems posed by their current life circumstances: maintaining physical fitness, saving income, finding and maintaining long-term mates, seeking the thrills of past youth, etc. Their personal perspective gives the book character. Research on other key life endeavors, such as fathering, grandparenting, menopause, child nutrition and development, female competition, and psychological problems like depression and anxiety, however, is largely ignored. Second, many evolutionary psychologists would caution against offering advice based on what we currently know about psychological adaptations because much of our knowledge is fairly rudimentary. Finally, the superficial treatment of numerous studies prevents readers from being able to critically evaluate the research for themselves; pro-

fessional audiences will not be able to form their own interpretations of data presented, and they will not be able to examine evolutionary explanations presented (references must be obtained online at [www.meangenes.org](http://www.meangenes.org)).

Burnham and Phelan have ventured onto fertile grounds for using an understanding of evolution to inform better living. *Mean Genes* explains, to a popular audience, how be-

haviors that were adaptive in ancestral environments are ill fitted to promoting individual well-being in many of today's environments. Though this approach is a welcome first attempt to use evolution-minded research in improving the qualities of our lives, there remains a need for a thorough and precise analysis of the relationship between the goals of self-help literature and evolutionary psychology.

## Mother Nature: A History of Mothers, Infants, and Natural Selection

**Sarah Blaffer Hrdy**

New York: Pantheon Books, 1999, 723 pages. US \$35.00 cloth. ISBN 0679442650. Ballantine Books, 1540 Broadway, 11<sup>th</sup> Floor, New York, NY 10036, USA.

**Laurette T. Liesen** Lewis University, USA

Sarah Blaffer Hrdy's *Mother Nature* is an evolutionary, comparative, cross-cultural, and historical analysis of mothers and their relationships with their infants and children. This is a tremendous book, integrating over twenty years of Hrdy's research on nonhuman and human female reproductive behavior. It reflects her intellectual as well as personal journey into motherhood, examining the joys, choices, and compromises that many women make in their lives regarding their children. In addition to relying on sociobiology and primatology, Hrdy demonstrates that her research supports feminists' arguments that women desire control over their reproductive lives and ongoing material and personal support in raising children.

*Mother Nature* is divided into three sections, which examine evolutionary theory and animal behavior; the development of mothers, motherhood, and alloparents; and, finally, the physical development and evolved behaviors of infants. Not only is this book well researched and written, it engages readers and shows how Hrdy's own experiences with motherhood and questions about maternal instincts

have led her on this intellectual journey. While *Mother Nature* is accessible to a general audience, the notes and extensive bibliography of nearly 150 pages make this book an outstanding resource for scholars.

The first section of *Mother Nature* presents a history of evolutionary thinking on female behavior across species. Hrdy begins with Darwin and early evolutionists, showing how these earlier approaches neglected female reproductive behavior. Hrdy demonstrates how early evolutionists perceived female reproductive behavior as passive, and how evolutionary feminists have challenged and changed contemporary evolutionary science. These feminists have shown how females across species are active and strategic participants in reproductive decision-making. Hrdy presents her own research on langurs, other primates, and other species to show the wide range of behaviors that females use in reproduction, as well as the contingent nature of motherhood.

The second section of *Mother Nature* discusses what infants need from mothers, and how mothers have devised a variety of means to meet these needs. For example, Hrdy discusses the development of lactation in mammals and human females, and the corresponding physical, neurological, emotional, and social benefits infants receive from the milk, as well as the bonds formed with the mother. Not only does lactation enhance the survival chances of the infant and further its emotional development, lactation also enhances the long-term reproductive success of the mother. While mammals—especially human females—have evolved physical and emotional responses to pregnancy

---

**Laurette T. Liesen** is Assistant Professor of Political Science at Lewis University, One University Parkway, Romeoville, IL 60446, USA. She has taught various political philosophy, biopolitics, and public policy courses. She is author of a review essay, "The Legacy of Woman the Gatherer: The Emergence of Evolutionary Feminism," *Evolutionary Anthropology* (November 1998) and "Feminism and the Politics of Reproductive Strategies," *Politics and the Life Sciences* (August 1995).

and motherhood, Hrdy argues that women are not “naturally” or “essentially” mothers. Instead of being instinctual, motherhood for women depends on the environment and the individual circumstances in which each woman finds herself.

Indeed, Hrdy’s work goes well beyond the old “nature versus nurture” debate. She convincingly demonstrates that there are both evolved physical processes and environmental triggers or cues that play a role in each woman’s experience of pregnancy and motherhood. Not only does she address the role of hormones in pregnancy and lactation, she also uses a cross-cultural approach in examining women’s birthing experiences. Instead of a universal “maternal instinct,” there is variation across cultures and over time in how women approach birth and motherhood. Therefore, mothering is not just a “natural” response; it is sensitive to both nature and the environment. Hrdy explains that

attention needs to be focused on the complicated interactions among genes, tissue, glands, past experiences, and environmental cues, including sensory cues provided by infants themselves and by the individuals in the vicinity. (p. 174)

One of the most interesting discussions in *Mother Nature* is about women’s efforts to secure assistance in rearing their children. Hrdy demolishes the stereotype of the all-compassionate, heroic mother who selflessly sacrifices everything for her child. The evolutionary and historical reality is that women have always needed help in raising their children, and they have elicited help from men and other women in acquiring resources and parenting. For example, women throughout history have sought ways to mitigate the physical costs of caring for their children. Hrdy describes the practice of hiring wet nurses to feed infants. Wet-nursing especially benefited the elites in eighteenth and nineteenth century European societies, because it enhanced their fertility and it enhanced the probability that their infants would survive. Even today, Hrdy suggests that daycare can be considered a type of modern wet-nursing.

Like the choice of motherhood itself, women face many options in securing help with their infants, and fathers are not the only ones who can supply help and resources. Both nonhuman and human primate females create a network of female allies (both kin and non-kin) who can help care for their offspring. Among humans, female alloparents—pre-teen females, grandmothers, aunts, and girl friends—all can provide invaluable help to a mother and her children.

As much as women may be physically prepared to be mothers, and as much as perhaps most cultures set the expectation, some women will go to great lengths to avoid pregnancy or evade the responsibility of rearing their children. Using both her research on langurs and various historical and cross-cultural studies, Hrdy maintains that under certain conditions, infanticide may be an adaptive re-

productive strategy for the long-term reproductive success of some females. Among humans, mothers and fathers have chosen various means of terminating reproductive investment in some of their children—through infanticide, through giving the child to kin to raise, through abandonment, or through leaving the child at founding hospitals or orphanages.

In one of the most fascinating parts of *Mother Nature*, Hrdy presents historical accounts of how common it was in ancient Rome and medieval Europe for children to be abandoned. Under difficult circumstances—bad timing, too many children, poverty—mothers throughout history and across cultures have chosen a variety of ways to disinvest in their children. The conditional responses women have to their infants and children indicate that although women are biologically prepared for pregnancy and lactation, these responses are neither automatic nor instinctual:

Mothers have always had to make the most of resources at hand while coping with the sliding scale of paternal and alloparental help available. Mothers make tradeoffs compatible with their own subsistence, the needs of different children, and their own future reproductive prospects. These tradeoffs are made in a world of constantly shifting constraints and options. (p. 376)

With a human mother’s commitment potentially tenuous and contingent, a human infant has many traits and behaviors that can elicit caring and loving responses from its mother. This is the focus of the third part of *Mother Nature*. According to Hrdy, at some point in human evolution, human mothers became much more discriminating about their infants. Human infants are incredibly dependent and vulnerable compared to the infants of other primates. As a result, they have evolved a number of traits and behaviors that help strengthen the bond with their mothers. Those infants who looked too different, or were ill-timed, faced an increased chance of abandonment and death. Consequently, Hrdy suggests that babies have evolved certain traits humans consider cute—rounded heads, big eyes, and plump cheeks—that further entice mothers to commit to their care.

Not only do infants need secure attachments with their mothers (or any adult) for their basic survival, these attachments are necessary for their emotional and cognitive development. All babies need to feel loved by the adults in their lives, making them feel safe, clean, fed, and stimulated. The biggest challenge for women, especially those who work and have children, is finding a reliable alloparent to make this type of commitment to their children.

*Mother Nature* is a wonderful synthesis of Sarah Hrdy’s work from the past twenty years. She convincingly argues that early scientific and cultural biases that perpetuated the myth that women must be the primary parent do not re-

flect the evolutionary history of primates, human history, or the experiences of most women across cultures. At the same time, Hrdy presents much evidence that babies need to be attached securely to the adults in their lives in order to survive and to develop. *Mother Nature* provides great insights into the variation in women's approaches to, and experiences with, motherhood, and it explains why mother-

hood—which many think to be “natural” for women—does not always feel “natural.” Hrdy shows that when women have children within loving relationships, and with help and support from family and community, motherhood can become one of the most rewarding and joyful experiences a woman can have.

## A Theory of Everything: An Integral Vision for Business, Politics, Science, and Spirituality

Ken Wilber

Boston: Shambala Publications, 2000, 208 pp. US \$13.95 paper. ISBN: 1570628556. Shambala Publications, 300 Massachusetts Ave., Boston, MA 02115, USA.

**Kay Phillips**      Miami University, USA  
**Mostafa Rejai**    Miami University, USA

The quest for a unified or general theory of knowledge has preoccupied philosophers and scientists since time immemorial. Plato thought that, given the opportunity, his philosopher-king would establish order throughout the universe. Aristotle's doctrine of the Golden Mean was conceived as a panacea for all that ails humankind. Writers as diverse as Democritus, Hobbes, and Newton believed that the universe was governed by invariant laws of motion. The list is interminable. And, unhappily, the results are nil.

In more recent times advances in science and technology provided a new impetus to identify a unified theory of knowledge. It is in this context that Ken Wilber writes, unabashedly calling his book *A Theory of Everything: An Integral Vision for Business, Politics, Science, and Spirituality* (Boston: Shambhala Publications, 2000).

Wilber borrows from the physicists in using the term “theory of everything.” For them, such a theory “would

unite all the known laws of the universe into one all-embracing theory that would literally explain everything in existence” (p. x). Thinking along the lines of social theorists from a variety of disciplines who wish to be on the cutting edge of social thought, Wilber suggests that his approach to the world (both natural and social, including the human psyche and culture) goes beyond the contemporary wave of enthusiasm for evolutionary psychology. That perspective attempts to explain human behavior and mind in terms of Darwinian principles of natural selection for human behavior and the creations of human thought, which result in human societies and cultures. Wilber offers a model that he maintains can be used to overcome the ethnocentric, selfish view of the world presented by postmodernism, and can provide a worldcentric view that transcends the limitations of postmodernists whose pluralistic relativism coincides with an open disdain for a universal value system.

*A Theory of Everything* attempts to integrate long-standing models of development in four seemingly unrelated and unrelatable areas mentioned in the paragraph above, which include as two parts the universal dichotomies of science and religion (as part of culture). When the reader grasps and accepts Wilber's unusual way of bringing together these four disparate areas, his model does provide some feeling that perhaps he is “on to something.” While his specific understanding of the natural (other than human) world appears limited, his proposals do cause the uninitiated reader to think “outside the box,” so to speak, and

---

**Kay Phillips** is Professor in the Department of Sociology, Gerontology, and Anthropology, Upham Hall, Miami University, Oxford, OH 45056-1879, USA (e-mail: PHILLIKP@muohio.edu). **Mostafa Rejai** is Distinguished Professor Emeritus of Political Science at Miami University, where he has also been the recipient of an Outstanding Teaching Award. Professors Phillips and Rejai are coauthors of numerous books and articles on comparative revolutions, comparative leadership, and comparative ideologies.

expand the possibility that he may really be thinking in the right direction.

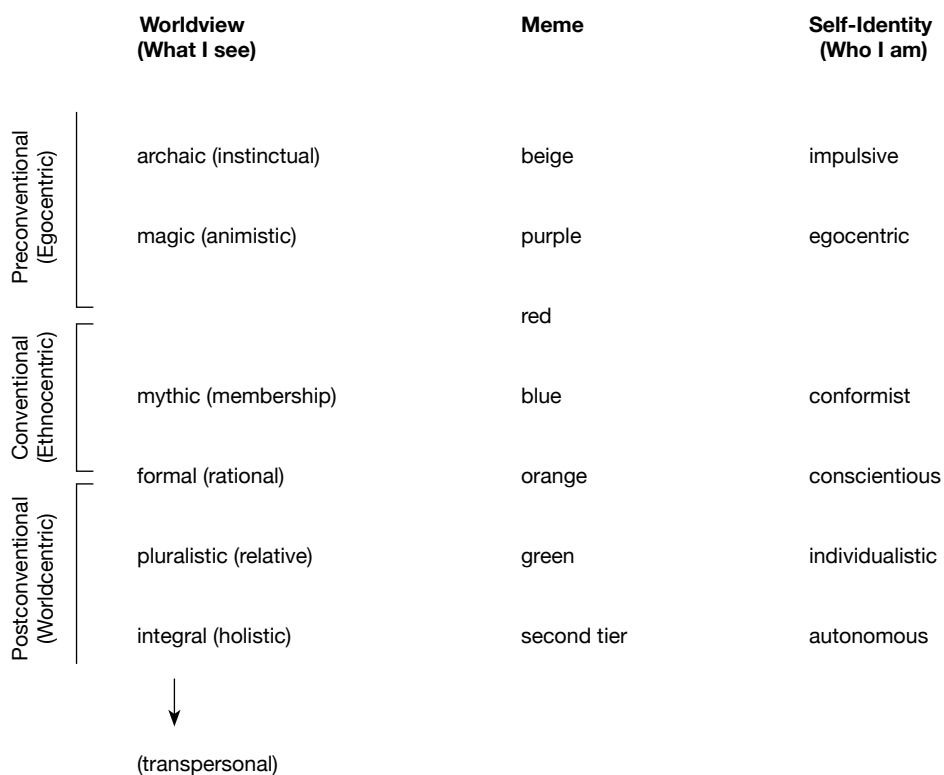
The principles of development from simple to complex, or those principles borrowed from evolutionary theory which posit development along linear axes from small to large or simple to complex, are employed by Wilber in four different arenas that he argues are related. The first area has to do with worldview and selfhood, as borrowed from developmental psychologists (see Don Beck and Christopher Cowan, *Spiral Dynamics: Mastering Values, Leadership, and Change*, Blackwell, 1995). Wilber uses colors to identify the evolving/developing worldviews, “memes” (waves of existence), and self-identity that progress from the simplest and most personally selfish to those that he identifies as second tier and beyond ego- or ethnocentric. Figure 1 (Wilber’s 2.I, p. 21) identifies the various stages of human psychological development and accompanying worldview, starting with the simplest and earliest form at the top of the figure to the most highly developed and complex at the bottom.

Starting with the archaic (instinctual level of worldview characteristic of early stages of human psychological development in which self-identity is characteristically impulsive), Wilber takes us through the stages from highly

egocentric (beige/archaic-instinctual) to red, which is the pinnacle of the highly egocentric and pre-conventional ways of perceiving the world. The subsequent level of blue/conformist marks the stage in which original narcissism is placed onto the group, allowing for and demanding intense ethnocentrism.

This stage persists until a level of egoic-rational or orange, which indicates the start of the post-conventional stage of green, to be followed by yellow, turquoise, and coral (see p. 8, Figure I.I). Green is the stage identified as that in which communitarian, human bonding, ecological sensitivity, and networking exist. From this level, Wilber argues that “human consciousness is poised for a quantum jump into ‘second tier thinking’...[in which] one can think both vertically and horizontally...(both ranking and linking)...[and] vividly grasp the entire spectrum of interior development, and thus see that each level, each meme, each wave is crucially important for the health of the overall Spiral” (p. 11). In other words, in order to understand and interact successfully, one must grasp and interrelate the necessary existence of each stage.

At this point, Wilber attempts to relate the nature of hierarchies from any realm of existence: natural world, psychological development, cultural, and social. Each succes-



**Figure 1. Worldviews and Selfhood**

Source: Wilber, 2000: Figure 2.I, 21

sively higher stage of the hierarchy must envelop/include its predecessors. This point becomes clear at the end of his treatise, when he argues that all levels of the hierarchies that he identifies may coexist in the world at the current time, and must be dealt with as such.

Important to understanding Wilber's argument is that he attempts to relate that which appears unrelatable. He posits a way to understand Interior-Individual (human psychological development) with Exterior-Individual (human behavioral development from the traditional realm of natural science) with Interior-Collective (cultural) development with Exterior-Collective (social development). In Figure 2 (Wilber's 4.4, p. 70), The Four Quadrants, one can see that starting from the zero point of the axes (middle) identified as the simplest form of the four developmental hierarchies identified, Wilber argues that development proceeds toward the distal corners of the chart as complexity/sophistication proceeds. For example, in the Upper Left Quadrant (I), psychological development proceeds from prehension through perception to symbols to rules to concepts to formal to vision-logic. In the Upper Right Quadrant (IT), behavioral [or natural scientific identification of the complex organism] proceeds from the atom through to molecule to the neural cord to the

complex neocortex. Accordingly, the Lower Left Quadrant (WE), or the Interior-Collective, proceeds from the physical-pleromatic [we have no idea what that means] to the protoplasmic to the locomotive to the archaic to the magic to the mythic to the rational to the centauric stages. Here, Wilber seems to be identifying that which others may refer to as collective conscience or cultural worldview. The Lower Right Quadrant seems contradictory in nature, in that it starts with galaxies, then planets, then heterotrophic ecosystems, and only then moves to social/societal elaborations of human evolutionary stages, which include groups/families to tribes, early states/empires to nation/states, and then, if we read Wilber correctly, the planetary/integral stage of human social development to which he aspires.

Again, if we read Wilber correctly, the rest of his book attempts to argue that as human psychological development proceeds (Quadrant I), human behavioral development (IT) proceeds apace in a wave-like fashion from its central axis. Likewise, cultural development (WE) must proceed from its central axis, and human social (ITS) development from its central axis as well. Arguably, these developmental or hierarchical sequences do not take place at the same time. In other words, the behavioral develop-

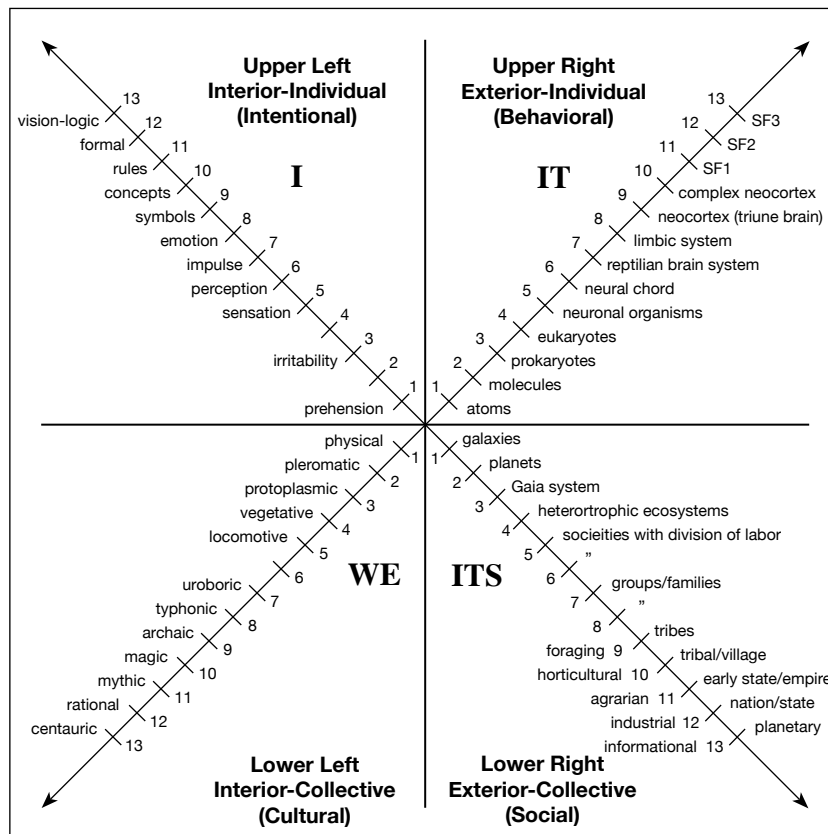


Figure 2. The Four Quadrants

Source: Wilber, 2000: Figure 4.4, 70

ment of the species must have taken place in that quadrant (upper right) long before the individual stages of human development could take place in the upper left quadrant. In the realms of culture and social development, Wilber may have a more secure position. Sociocultural evolution does proceed in tandem, with the states of the WE and ITS of the lower left and lower right quadrants somewhat correlated in time and appearance.

As a method of resolving the age-old question of science versus religion, Wilber's integral approach to developmental sequences may offer some understanding. Wilber argues that evolving in different quadrants, and dealing with different phenomena, science from the Upper Right (behavioral) quadrant and the Lower Right (social) quadrant are [sic] different types of developmental sequences than the individual/self-identity quadrant (Upper Left) and the cultural quadrant of the Lower Left, describing exterior and interior understandings, respectively. At first reading, Wilber's arguments make sense, but only the test of time and further analysis will prove them valid.

We question the independence of evolution, however, in the four spheres or quadrants that Wilber identifies, particularly the independence of the social and cultural quadrants. Wilber seems to ignore the important and complex interrelationships that exist between the interior and exterior quadrants, especially that which exists between the individual (I) and social (ITS) as they are related to the cultural (WE). It would seem that the limitations placed on individual and social development by the cultural is ignored. While the social and cultural are certainly related, permission to develop beyond a particular social stage in the hierarchy must exist within the cultural values that are extant in the cultural realm. For example, if cultural values do not allow change, and demand negative sanction for innovation, social development is not likely to occur. Also, if cultural permission to think individually beyond the limits placed by a magic or mythic cultural stage do not exist, the individual is not likely to move beyond the stage of rules and formal thinking (blue or conformist). Alternatively, scientific investigation must be allowed by the cultural rules or norms if development in the scientific realms of behavior and society is to take place.

Wilber may be correct in his assertion that we need to move beyond ethnocentric and egocentric ways of thinking in that we "need genuinely second tier [integral vision-logic] forms of political philosophy and governance [for global systems and integral networks]" (p. 90).

Using his integral linkages, Wilber demonstrates their use in business, medicine, politics, and education, but the

clarity of his discussions does not fully support his integral position (Ch. 5). What the reader is left with is his hope that integral thinking and policies will solve problems in those areas. Throughout his manuscript, Wilber argues that even though only a small percentage of the world's population, found primarily in industrial and informational stage societies, has reached the second tier of worldview, this level of understanding is necessary for a truly integrated world/global system that can peacefully coexist and cooperate in the future.

In proposing *A Theory of Everything*, Wilber is among those post postmodernists who are attempting to provide a way of integrating the realms of the individual, cultural, natural scientific, and social in such a fashion that we might move to a level of thinking that recognizes the integration between the realms, and act within that understanding. Rather than believe that all things are relative, Wilber believes that there are universals, if only one knows how to understand them.

On the whole, Ken Wilber gets high marks for focusing on a topic of immense complexity. By the same token, the scope of the project dooms it from the outset.

For one thing, if by "theory" we mean a set of interrelated propositions having explanatory power, one finds very little of it in *A Theory of Everything*. There are thoughts, to be sure, lofty and otherwise, but there is no general or unified theory.

For another, the "everything" in Wilber's title does not include everything in the literal sense. The four topics in discussion—business, politics, science, and spirituality—are covered in a fragmentary fashion in a mere 141 pages. The fine arts are barely treated, though conceivably—and erroneously—they may be covered under "spirituality."

Moreover, Wilber is highly redundant throughout, repeating the same point over and over again, and relying heavily on his own previous writings. The 40 pages of footnotes at the end of the book reiterate what has been said in the text. Wilber's citations are incomplete; where he bothers to cite other writers, place, publisher, and date are almost uniformly missing.

Finally, *A Theory of Everything* teems with jargon, as the foregoing review may suggest. Now, to be sure, a certain amount of jargon is necessary to cover technical topics, but Wilber subjects the reader's mind to sustained vertigo.

In a few words, Wilber's objective is commendable, but the execution is wanting and incomplete. Perhaps the contemporary universes of knowledge do not lend themselves to a general or unified theory. This problem persists *within* disciplines, let alone *between* them.

# POLITICS AND THE LIFE SCIENCES

## Main Articles in This Issue (most abstracts abridged)

---

**The Politics of a Scientific Meeting: The Origin-of-AIDS Debate at the Royal Society**

Brian Martin (*University of Wollongong, Australia*)

Pages 119-130

**The Age of Transition to Sustainability: The End of the Exponential Growth Period**

John Cairns, Jr. (*Virginia Polytechnic Institute and State University, USA*)

Pages 131-138

**Stuck in the Pleistocene: Rationality and Evolved Social Roles**

Derek Reiners (*Indiana University, USA*)

Pages 139-154

**Who Favors Legalizing Physician-Assisted Suicide? The Vote on Michigan's Proposal B**

John Strate (*Wayne State University, USA*)

Tiimothy Kiska (*University of Michigan—Dearborn, USA*)

Marvin Zalman (*Wayne State University, USA*)

Pages 155-163

**A Critique of the Naturalistic Fallacy Thesis**

Jan Tullberg (*Stockholm School of Economics, Sweden*)

Birgitta S. Tullberg (*University of Stockholm, Sweden*)

Pages 165-174

**U.S. Chemical Program: Purpose, Challenges, and Evolution**

Odelia Funke (*Environmental Protection Agency, USA*)

Pages 175-187

**Culture, Social Minds, and Governance in Evolution**

Claude S. Phillips (*Western Michigan University, USA*)

Pages 189-202

**The Homeopathy of Kin Selection: An Evaluation of van den Berghe's Sociobiological Approach to Ethnic Nepotism**

Ingo Brigandt (*University of Pittsburgh, USA*)

Pages 203-215

The Royal Society of London held a scientific meeting in September 2000 focusing on two theories of the origin of AIDS: one, that it occurred through "natural transfer" of immunodeficiency virus from monkeys or chimpanzees to humans; and the other, that it occurred through iatrogenic transfer via contaminated polio vaccines used in Africa in the late 1950s. This meeting was the culmination of years of public contention over the polio-vaccine theory. Several dimensions of the politics of science are revealed by analysis of this issue, including the power of scientific editors, the use of the mass media, decisions regarding selection of speakers and organization of the meeting, and epistemological assumptions made by participants.

Arguably, no curve in the world increases indefinitely—certainly no growth curve of human society. The quest for sustainable use of the planet involves estimating levels of activity, particularly resource use, that can continue indefinitely. Since the biosphere is dynamic, this continuation is not a stagnant, steady-state situation but rather a mutualistic interactive relationship between human society and the planet's ecological life support system. Human technology, creativity, and ingenuity may modify natural laws, but cannot be used to repeal them. Attempts to maintain the recent exponential growth of the human population, affluence, and artifacts cannot continue forever.

This article argues that an evolutionary psychological perspective could be useful for developing second-generation models of rationality. The standard model of complete rationality is inadequate primarily on the grounds that it generates predictions inconsistent with empirical data. The model is extremely useful and should not, nor cannot, realistically be dismissed. However, behavioral phenomena that the standard theory has trouble explaining—such as mass contribution to public goods, types of cooperation, and altruism—are usually treated as "anomalies." These outcomes are too prevalent and important to be treated as such. I attempt to build upon Elinor Ostrom's proposal to generate new models of rationality.

At the November 1998 general election, Michigan citizens were given the opportunity to vote on Proposal B, an initiative that would have legalized physician-assisted suicide (PAS). PAS initiatives also have been held in Washington State, California, Oregon, and Maine, with only Oregon's passing. We use exit poll data to analyze the vote on Proposal B. Attributes associated with social liberalism—Democratic Party identification, less frequent church attendance, more education, and greater household income—led to increased odds of a "yes" vote. Attributes associated with social conservatism—Republican Party identification and frequent church attendance—led to decreased odds of a "yes" vote. Similar to the abortion issue, PAS's supporters strongly value personal autonomy, whereas its opponents strongly value the sanctity of life.

If the prescriptive "ought" is separated from the factual "is," an intellectual analysis of the real world is by definition without normative value. The naturalistic fallacy thesis—maintaining that normative and descriptive spheres must remain separated—is often presented in a weak sense that seems reasonable. However, only in a strong sense—by strictly separating facts and values—are fallacy accusations supported. We claim that this naturalistic fallacy thesis is unsound and that normative statements instead should be based on rational understanding as found in the Darwinian and social sciences. The Cartesian compromise should be abandoned, since only naturalism can provide a cogent framework for better understanding and support ethics with a solid foundation.

This article explores long-term issues and problems that have seriously undermined the U.S. Chemical Testing Program established by the Environmental Protection Agency (EPA) under the Toxic Substances Control Act. This program is meant to gather information needed to protect human health and the environment from damaging exposure to toxic chemicals. Despite seemingly broad and impressive authority under the statute, there are a number of inherent difficulties, as well as substantial political constraints, that impede comprehensive oversight of chemicals in U.S. commerce. The article discusses several approaches that EPA has adopted to overcome statutory and political limitations and increase chemical testing information.

In the past quarter century, the concept of culture has undergone change as evolutionary scientists have come to include social behavior in their purview. Evolutionary psychology is the newest field to concern itself with culture by claiming that most specific human behaviors are generated by minds specifically designed for these behaviors—and not from a general-purpose mind—as a result of adaptations made during the Pleistocene. Thus, mental behaviors are explained as having formed independently of cultural learning. In defending the concept, however, the leading proponents practically slough off culture as significant in human affairs. I argue that they have neglected the powerful explanatory statement of Darwin regarding at least one general-purpose adaptation of social animals, namely, the instinct for sociability, a position supported by recent neurological studies.

The present discussion of sociobiological approaches to ethnic nepotism takes Pierre van den Berghe's theory as a starting point. Two points, which have not been addressed in former analyses, are considered to be of particular importance. It is argued that the behavioral mechanism of ethnic nepotism—as understood by van den Berghe—cannot explain ethnic boundaries and attitudes. In addition, I show that van den Berghe's central premise concerning ethnic nepotism is in contradiction to Hamilton's formula, the essential principle of kin selection theory. It is further discussed how other approaches that make reference to ethnic nepotism are related to van den Berghe's account and its problems. I conclude with remarks on the evolutionary explanation of ethnic phenomena.