

UK Update

Half Term and Half-way There

"Nobody in psychiatry can do without a philosophical background..."

These words could well have been said by Paul McHugh, speaking at an early meeting of AAPP hosted by Michael Schwartz - the 1980s, Paul McHugh said, had been the decade of the brain; the 1990s would be the decade of the mind. In fact, this strong claim for our subject was made, not by Paul McHugh, nor by any other contemporary enthusiast for philosophy/psychiatry, nor even by Karl Jaspers, but by the man widely acknowledged as the father of modern *scientific* psychiatry in the UK, Sir Aubrey Lewis.

As Paul McHugh predicted, psychiatry is fast catching up with Aubrey Lewis' view. Like global warming, or the politician's elusive "feel good" factor, the signs are patchy and inconsistent. But the evidence of renaissance grows stronger by the day.

An early sign, in the States, was the willingness of Allen Frances, the chairman of the DSM Task Force, to write a foreword for a book on philosophical aspects of psychiatric diagnostic classification, timed to coincide with the publication of DSM-IV. To produce such a book in the face of the dogmatically empiricist stance of the DSM Task Force was a brave act of faith by John Sadler, Ossie Wiggins and Michael Schwartz. Well, faith certainly moved that mountain! Many of the most difficult problems faced by the task force turned out not to be empirical at all. They may not be philosophical problems, either. But philosophy, Allen Frances concluded, can at least help us to become more aware of the context in which our present system has developed.

A corresponding sign from this side of the Atlantic was an unsolicited lead editorial in the *British Journal of Psychiatry* last year by Michael Shepherd, enthusiastically endorsing PPP - "...there is a strong case in favour of acknowledging and re-examining the philosophical aspects of psychological medicine...", he wrote, for the "(Royal) College's Philosophy Group to be enlarged and fortified...", for "the in-

President's Column

"The trick is to get ahead of the pitch," my Little League coach told me. Not only did I not know what he meant then, I really didn't care very much. I have always found baseball (*pace* devotees) to be a rather boring game. Some would say the same about philosophy and psychiatry. After all, what is it? Unlike bioethics which has the penchant for attracting media attention—perhaps to its own detriment—philosophy and psychiatry seems remarkably diffuse and marginal subject. Its marginality may well be its chief virtue.

Last year I read an interview with a bioethicist colleague of mine about pharmacological developments involving the ability to alter moods and mental states. The question posed was whether there were any deep ethical issues involved. The bioethicist responded affirmatively, but then tied these developments to the general problem of access to health care by arguing that only in America could we worry about altering "bad feelings" when so many of our citizens have inadequate access to health care. The interviewer was apparently pleased with the response, because the rest of the interview focused on health care reform and managed care and not the topic originally defined. It left this reader to wonder whether the underlying issues were *not* ethically important.

The discomfort with questions about psychopharmacology, however, is understandable given that these questions are not only far more philosophical than they are ethical, but they do not make for easy sound bites. At least until we understand the meaning of term such as mind-altering drugs, the ethical analysis will remain not only controversial, but unfocused in a critically important way. For the time being, I doubt that we shall see an interview segue from managed care and health care reform to the problem of akasia or pathologies of the imagination. That is not all bad.

Working at the margins, philosophy and psychiatry has an enormous store of material from which to draw that seems too esoteric for bioethics to worry about. Unlike bioethics whose main agenda is at least partly determined by the whims of public policy and media interest, philosophy and psychiatry can, and indeed must, draw not only from the rich conjunction of its component disciplines, but from the sciences and humanities that affect and influence our understanding of psychopathology and the care of the mentally ill. We do not have a "hot topic of the month" to compel our attention. This Issue contains announcements for the January 1997 New England Regional Meeting, the May 1997 AAPP Annual Meeting, and the June 1997 Second International Conference on Philosophy and Mental Health—meetings which promise to be much more than temporary distractions.

An AAPP member recently complained to me, somewhat in jest, that AAPP is doing too much. He was having trouble deciding which meetings to attend during the next year and he complained that AAPP was complicating matters by affording him too many choices. My reply was that unlike other academic meetings, there are no professional disadvantages in missing a philosophy and psychiatry meeting. After all, many allegedly

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roduction of one or two compulsory questions in the Membership examination", including "Wittgenstein and Schopenhauer ... a challenge to examiners and candidates alike". These are not the words of a philosophical guru. Like Allen Frances, Michael Shepherd's work in psychiatry was mainly empirical. True, he was a widely read and scholarly man; he was indeed an acknowledged expert in his own right on the American Pragmatists. But throughout most of his life he was deeply sceptical of the *practical* value of philosophy in psychiatry.

I use the past tense of Michael Shepherd, because, sadly, he died recently. His editorial in the *British Journal of Psychiatry* has become known as Michael Shepherd's last word. We will miss him. Like many other leading figures in British scientific psychiatry, including our distinguished honorary chairman, Sir Martin Roth, he had become a good friend of the College's Philosophy Group.

What is changing our critics' minds? Why the conversions? It is not, I think, merely

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(Continued from page 1) *President's Column*

academic meetings are occasions for putting in professional appearances, for recruiting faculty, for finding jobs, and for networking. Fortunately, in the land of philosophy and psychiatry we have no such distractions. We have the luxury of dealing with ideas for their own sake.

The AAPP member concurred saying that his complaint was hardly for missing the *professional* opportunities that our meetings afforded, but rather for missing the intellectual stimulation that are the hallmark of our meetings. AAPP meetings are attended by people who genuinely care not only about the subject matter, but about what others have to say or think. There is an academic civility that is often absent when professional interests predominate. Indeed, this observation reminded me that my own involvement in AAPP has been partly stimulated by the genuinely collegial character of our meetings as anything else. We are fortunate to have such a variety of programs scheduled and such a diverse membership with which to share them.

George Agich

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the swelling tide of activity and interest. Though this is impressive enough. In the UK, our programme of workshops, post-graduate meetings and conferences has continued to expand, colonising new parts of the country, and drawing in an ever wider range of philosophers and philosophical disciplines; a growing number of post-graduate training schemes in psychiatry now include sessions on philosophical aspects, especially of classification and diagnosis; even CPD (Continuing Professional Development) programmes are targeting these areas; the Philosophy Group itself has many new members; and, more important still, new local sections continue to form, building on local skills and resources. These changes are of course strongly reflected throughout the rest of Europe and indeed world wide - witness the twenty-one countries, and richly interdisciplinary themes, of The First International Conference for Philosophy and Mental Health in Spain earlier this year.

Mere energy and activity, though, still less mere numbers of converts, are not enough to explain the change of heart of an

Allen Frances or a Michael Shepherd. To the contrary, such empiricists, the hard men and women of scientific psychiatry, share with John Locke (their philosophical progenitor) a proper mistrust of "enthusiasms".

The difference, the crucial shift or sea change, is that they now recognise, perhaps for the first time, that philosophy may have some real work to do in psychiatry. Not much work, perhaps. Allen Frances, although enthusiastically endorsing the Sadler, Wiggins and Schwartz book, went no further than hinting that philosophy might have a chance to prove itself in the preparation of DSM-V. Michael Shepherd identified with Jaspers in casting philosophy in an essentially negative role, as "...an unusually stubborn effort to think clearly". Michael Gelder, another hard man of scientific psychiatry, and shortly to become a further sad loss to us, on his retirement from the Chair of Psychiatry in Oxford, took this line in his keynote welcoming presentation at the St Catherine's conference back in 1991. Philosophy, he said, can help us to frame the right questions, to limit bias and presupposition, to avoid premature closure.

Well, this negative role is far from *de minimis*. Michael Shepherd complimented the contributors to the early issues of PPP on the wide range of practical topics in clinical work and research in psychiatry to which they had brought distinctively philosophical clarification, topics as diverse as needs assessment, thought insertion, psychoanalytic theory, phenomenology and connectionism.

But besides its negative role, philosophy also has a positive contribution to make to psychiatry. In research, in particular, there is a new dialogue between philosophers and practitioners. As recently as 1985, the British philosopher, Lord Quinton, anticipating the rebirth of philosophy and psychiatry, pointed to the remarkable neglect of madness by philosophers since Descartes. They could not be accused of neglect now. In the States, Jerry Kroll's conference on *Akrasia* this year attracted some of the brightest and best among younger philosophers. In the UK, the CIBA Foundation recently hosted an inter-disciplinary research day on AI models of schizophrenia; and Naomi Eilan, a Philosophy Research Fellow at Warwick University, plans to include work on disorders of self-consciousness in the programme of her new Centre for Consciousness Studies. In all these situations, it is true, there is still something of a communication gap to be bridged. But the model towards which everyone is now working is of philosophy, not merely as a ground clearing preliminary to scientific research, but as a full partner in the research process itself.

The communication gap is important,

of course, and it is here that new educational programmes, aimed at bridging the gap, are essential. Alec Jenner, now retired as Professor of Psychiatry in Sheffield, though still very active in the field, led the way here some years ago with an MA in *Philosophy, Psychiatry and Society*. This has been further developed with considerable flare by Tim Kendall, the Director of Sheffield's Centre for Psychotherapeutic Studies, and like Alec Jenner something of an expert on Continental philosophy, as part of an interlocking set of teaching and research programmes linking theory and practice in several areas of psychotherapy and psychoanalysis.

Our MA at Warwick University, in *The Philosophy and Ethics of Mental Health*, has a stronger Anglo-American bias than Tim Kendall's (though the Department of Philosophy at Warwick is unique in the UK in being equally representative of the Continental and Anglo-American philosophical traditions). In our pilot year (1995/6) we have completed forty 2-hour sessions covering "bridge topics" on concepts of disorder, the philosophical history of psychopathology, philosophy of science (psychoanalysis, research methods, diagnosis, etc), philosophy of mind (thought disorder, autism, etc), ethics and jurisprudence. Students have come from both sides (from practice and from philosophy), as have our lecturers, and the educational process has been essentially one of bridge building through shared learning. Two PhD students are well underway, also working on bridge topics, one in Continental philosophy (on Heideggerian phenomenology and the experience of trauma), the other in Anglo-American philosophy (on Wittgenstein and problems of meaning in dementia); and both are receiving bridge supervision, working partly with philosophers and partly with practitioners.

Much of this is still promissory. As an Allen Frances or a Michael Shepherd would be the first to point out, the bridge building between philosophy and psychiatry is far from complete. Its successful completion will depend on other bridges, inter-academic, inter-personal and international, being well maintained (remember the shameful splits in the early days of psychoanalysis). But half way through Paul McHugh's decade of the mind we are half-way there.

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K.W.M. Fulford
Professor of Philosophy and Mental Health, University of Warwick, and Honorary Consultant Psychiatrist, University of Oxford
22 June 1966

Letter from France

A Birth Announcement

The French group is born, and we hope that this letter from France will be the first of a long series of exchanges across the Channel and the Atlantic. In addition to our birth, this letter also announces the alluring proposition of another get-together under the Mediterranean sun.

In truth, "birth" is not the most appropriate word to describe the historic situation of a group in France bringing together psychiatrists and philosophers. The society out of which our group hopes to evolve is an old lady of more than 70 years!

The French group has existed in some fashion since the twenties, when the psychiatrists Henri Ey and Eugène Minkowski joined their efforts to create at once the journal and society, *L'Evolution Psychiatrique*. Minkowski was without doubt among the first French psychiatrists—after Esquirol, who learned to break their chains—to consider that the insane have a mind, a mind structured anthropologically like that of other humans. Like Binswanger he saw in philosophy the means to secure a rigorous grounding for psychiatric experience. And like Binswanger he was himself a philosopher, inspired by Bergson and Husserl, and author of works such as *Lived Time* whose influence went

well beyond the field of psychiatry.

Besides Henri Ey other psychiatrists and psychologists pursued this reciprocal questioning of psychiatry and philosophy. There was even a flourishing period when psychopathologists in France could be counted in the dozens—with occasional figures such as René Ebtinger, Jean Oury, Nicolas Abraham, or Pierre Fedida combining phenomenology and psychoanalysis, but with the majority, it must be said, adhering to a pure psychoanalysis. The question of the unconscious oriented the debates in psychopathology in France.

This direction reached its culminating point on the occasion of the Bonneval Colloquium, organized by Henri Ey, a conference in which the presence of Lacan and his early students was so powerful that only Merleau-Ponty (who began his notes for *The Visible and the Invisible* at that time) could confront them. The latter's death shortly thereafter put a halt to the evolution in France of the phenomenological alternative in psychiatry. The most intellectual psychiatrists continued to orient themselves rather unanimously toward the obscurity of Lacan's work. And while Lacan himself was admittedly inspired by the phenomenology of Heidegger—with the secret aid of the Belgian phenomenologist and psychoanalyst Jacques Schotte, only a few dinosaurs such as Arthur Tatossian at Marseilles, René Ebtinger at Strasbourg, or Georges Lantéri-Laura at Paris, from a perspective essentially epistemologic, seemed to have survived the cataclysm and continued to interrogate pure philosophy.

With respect to the philosophers, moreover, interest in the problems in philosophic anthropology posed by mental illness seemed to touch only a small group. Here we should mention, in addition to Michel Foucault, who translated and wrote a preface for a work of Binswanger, the Lyon philosopher Henri Maldiney, who produced an incontestably original philosophy and esthetics, inspired as much by Erwin Straus and Ludwig Binswanger as by Husserl and Heidegger. In his wake today we may locate the work of Marc Richir.

For our school of thought in psychiatry the contemporary period has been dominated by the immense work of the recently deceased Arthur Tatossian. Able to cite Wittgenstein or James as well as Husserl or Alfred Schutz, Arthur Tatossian clearly did not aspire to found a school. Affirming that the best way to teach a student to respect the freedom of the subjectivity of another was to begin by respecting that of the student, he did not look to impose any dogma on the latter. Nevertheless, despite himself, so to speak, he had students. It is his students who are at the origin of the French group which we hope to form.

This group has equally important roots in the French school of Daseinsanalysis, led at the Ecole Normale Supérieure in Paris by psychiatrists such as Jean-Michel Azorin and Dominique Pringuey, and philosophers such as Françoise Dastur, Eliane Escoubas, and Philippe Cabestan. Two monthly seminars are held there. This

APP NEW ENGLAND REGIONAL MEETING

Saint Joseph College

West Hartford, CT

Friday, January 17 -

Sunday, January 19, 1997

Theme:

Imagination and its Pathologies

Invited Speakers:

Edward Casey, Ph.D., Professor of Philosophy, SUNY, Stony Brook, NY

Amedeo Giorgi, Ph.D., Professor of Psychology, Saybrook Institute, San Francisco

Richard Kearney, Ph.D., Professor of Philosophy, University College Dublin

Ethel Person, M.D., Professor of Clinical Psychiatry, Columbia College of Physicians and Surgeons, Training and Supervising Analyst, Columbia University Center for Psychoanalytic Training and Research

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James Phillips, M.D.
(cf masthead)

year the first, entitled "Methodology and the Sciences of Life," dealt primarily with the question of biography, and the second, entitled "The Loss of Natural Evidence," dealt with the work of Wolfgang Blankenburg.

The idea of a French group affiliated with AAPP and developing out of the Society of l'Evolution Psychiatrique germinated at Benalmadena. In the enthusiasm of the conference and in a state of almost hypomanic enthusiasm I (Naudin) discussed this possibility late into the night with Michael Schwartz, Bill Fulford, and Catherine Wieder (psychoanalyst and the only French participant at the conference with me). This disinterest on the part of the French can be explained as follows. The Anglo-Saxon literature is in the mind of the French associated with a biological, empiricist/positivist psychiatry. No Frenchman preoccupied with psychopathology is at this time prepared to believe that there might be interesting things written in English. No one therefore was aware of the conference. I had by chance received a letter from Michael Schwartz.

Since the conference our project has been further refined. We, Jean-Michel Azorin and myself, are pleased to announce that the group which we lead is organizing an international conference at Marseilles, June 28-30, 1997, with the theme: "Vulnerability and Destiny: the Phenomenology of Schizophrenia." One of the challenges of this meeting will be to permit philosophers and psychiatrists of very different schools to exchange views on a theme that cannot a priori exclude either the empirical/biological or the psychopathological orientation in psychiatry.

The conference will be, after that of Benalmadena, the second international conference of psychiatry and philosophy and will be co-organized by AAPP, the Royal College of Psychiatrists, l'Evolution Psychiatrique, and the Association for the Research and Treatment of Schizophrenia. At this point numerous abstracts have reached us, and we hope to receive others soon in order to better organize the sessions in advance. The best articles from foreign authors will be published in advance in French in the journal *L'Evolution Psychiatrique*. The conference will take place in English and French, with simultaneous translation. A further publication in English in the journal *Philosophy, Psychiatry, Psychology* is also envisaged.

But it is time to conclude this brief letter from Marseilles. Perhaps better to conclude it as in a post card, with talk about the weather, always disagreeable for the Parisians as for the English. We are now in June: the sun is burning, the sky and sea the same blue, and the cicadas are singing under the pines. We await you im-

patiently, as we miss your conversation.
A très bientôt.

Jean Naudin, M.D.
Jean-Michel Azorin, M.D.
Marseilles

1996 Annual Meeting Report

The 8th Annual Meeting on of the Association for the Advancement of Philosophy and Psychiatry "Akasia or Weakness of Will" took place in New York, May 4-5. The rich program was organized by Jerry Kroll, with the assistance of Brad Lewis, Marilyn Nissim-Sabat, Greg Mahr, Donald Mender, and Jennifer Radden. After a brief business meeting with reports by President George Agich and Treasurer John Sadler, the audience of over 100 was treated to two excellent Keynote Addresses by psychiatrist Eric Hollander and philosopher Amelie Rorty. In his presentation on "Weakness of Will and Obsessive-Compulsive Disorders," Dr. Hollander described the shift in viewing akratic behaviors from the old perspective of "sins," depicted marvelously in his first slide showing Hieronymus Bosch's "Garden of Delights," to the modern psychiatric perspective as (in part) disorders of neurotransmitters. He reviewed data associating sexual obsession and pathological gambling with serotonin disturbances, and sketched the complexity of an evolving field of impulse disorders likely treatable with Prozac-like substances, suggesting akratic behavior is less "chosen" than it is "driven."

Dr. Rorty in her address on "Political Sources of Akasia," used two case studies of seemingly akratic individuals exemplifying hope and despair, to ask first what akasia is not. It is not, she pointed out, episodic, and can involve persistent character traits. It need not be self-centered, and may not involve any conscious beliefs at all. Akasia can, she suggested, have significant social and economic components that for some may be determinative. On Sunday, Dr. Alfred Mele presented the third Keynote address on "Weakness of Will: Strict and Akratic Action," in which he explored how different types of akratic action are possible. In the strict form, judgment functions well, but it and motivation conflict, with the latter outweighing the former. In cases where the individual changes his judgment, the analysis is more complex and judgment may fail. Dr. Mele

also drew on his three recent books, including *Irrationality: an Essay on Akasia, Self-deception, and Self-control; Springs of Action*; and *Autonomous Agents*, to fill in a detailed picture of ways that akasia may be further characterized, as well as overcome.

The Contributed Papers included some 18 presentations on both historical and modern themes. Aristotle, for whom akasia was particularly problematic given his practical reasoning approach, was discussed by Michael Stocker, Caleb Mason, and Robert Barry. Stocker distinguished two ways of interpreting Aristotle, and argued that moderns frequently misconceive the regressive aspects of Aristotelian akasia. For Mason, the proper understanding of Aristotelian akasia lies in his important motivational psychology. Charles Mathewes argued that akasia in the context of Augustine's psychology points the way to a the need for a serious revision of the modern concept of autonomy. Laura Smits juxtaposed the work of the thirteenth century poet Rumi with Aquinas, and urged that joint reading of both authors can shed synergistic light on conflicts between the will and intellect. Aquinas (and Aristotle) was also examined by Robert Barry for the value that virtue theory can have in explaining moral error, and Nancy Weiler defended Hume's theory of motivation as adequate to account for moral failures.

Sunday morning's presentation began with a Sartrean approach to akasia by Joseph Catalano, that construed akasia as a specific form of "bad faith," where the remedy is the justified choice of one from several competing narratives. This was followed by Craig DeLancey who identified a series of partly cognitive, partly affective action scripts for dealing with life that can "go suboptimal," and yield akratic actions. Michael Heyns compared Charles Taylor's and Foucault's theories of agency, in which Taylor was viewed as providing a useful corrective to Foucault's account. Marilyn Nissim-Sabat offered a Socratic/Platonic view based on the *Pratagoras*, in which addictive behavior is a cognitive failure (ignorance), and Emilio Moradini examined whether a correct approach to psychotherapy could be nondirective and avoid suggestion, in which he drew on the classical views of Gorgia of Leontini in the light of contemporary theorists Lacan in France and Gindro in Italy.

The final session of the meeting was begun by Peter Campbell, who proposed that akratic action and free action were mutually contradictory, sketched an account of the ideals of practical rational agency, and evaluated Davidson's and Mele's views on akasia in the light of this sketch. Peter Caws followed, contrasting a "deliberative" concept of will with an idea of will as a "drive," which he viewed as

more consistent with Nietzsche's and Freud's views, and suggested that akrasia is better conceived as a failure of inhibition, perhaps due to strong unconscious drives, rather than a failure of resolve. Paul Sturdee analyzed akrasia from the point of view of the psychoanalytic tradition of "object relations," and offered some suggestions for applying this perspective in clinical diagnosis and therapy, particularly in the area of personality disorders. Karen Jones looked at "framing" effects on incontinent deliberation and the powerful roles that self-conceptions can have on akratic actions, and James Sabin considered arguments for and against health insurance coverage of personality disorders. Nassir Ghaemi began from the proposal of Leston Havens that J. L. Austin's concept of performative statements might apply to psychoanalytic work, and suggested that akrasia might be conceived of in terms of "infelicities" of performative language. Joseph Loizzo presented the closing talk of this exciting and excellent meeting, arguing that dualistic and mechanistic biases can lead to therapies that undermine patients' weak wills, and that nondualistic interactionist approaches can assist in the development of patients' autonomy through new forms of Wittgensteinian language games.

Kenneth F. Schaffner

AAPP at the APA

Papers on weakness of will that were originally delivered at the annual AAPP meeting in New York last spring will reach a wider audience through panels scheduled at the Eastern and Pacific meetings of the American Philosophical Association next winter and spring. The AAPP has arranged a panel at the Eastern APA meetings in Atlanta at the end of December. That meeting is scheduled for 9-11 am on Saturday, December 28th in the Savoy Room in the Atlanta Mariott. The program for that session is as follows:

Topic: Akrasia: Weakness of Will
Alfred Mele, Davidson University:
"Weakness of Will: Strict and Socratic Akkratic Action"

Marilyn Nissim-Sabat, Lewis University: "Treatment of Addictive Disorders and the Rejection of Akrasia: An Alliance Between Philosophy and Psychiatry."

Commentator: Dr. Edwin Wallace, Institute of Bio-ethics, University of South Carolina

Chairman: Edward S. Casey, State University of New York at Stony Brook

Prof. Mele will also participate in the panel scheduled for the Pacific Division in March, which has been arranged by Prof. Elizabeth Radcliffe of Santa Clara University. The other two papers deriving from the AAPP sessions in New York last spring are:

Amelie Rorty, Brandeis University, "Political Sources of Akrasia"

Michael Stocker, New York City "Weakness of Will, Aristotle and Psychoanalysis"

The Pacific APA meetings will be held in the Claremont Hotel in Berkeley, CA between March 26th and 29th, 1997. The specific hour and room for this panel have still to be arranged.

Since our annual meetings in conjunction with the American Psychiatric Association occur just when many philosophers are caught up in examinations or commencement exercises, we have sought to reach more philosophers through sessions at their divisional meetings. We are especially pleased that these provocative papers from our meetings last spring will engage a wider audience through these panels and that Ned Wallace has agreed to join the discussion at the meeting in Atlanta. But we also welcome proposals for workshops and panels at philosophical meetings from Newsletter readers. Please send suggestions to the executive council member in charge of liaison with philosophical organizations:

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Epistemologic Issues in Psychoanalysis: A Bibliographical Guide

Psychoanalysis has come under increasing attack for failing to validate its beliefs with evidence meeting the rigorous standards of natural science. Some psychoanalysts retain the goal of testing psychoanalytic hypotheses by standard scientific methods while others seek alternative criteria. This split is reflected in the controversy over whether psychoanalysis is a natural science or a hermeneutic discipline. There is a question of whether the same methodological principles hold for all of science. Physics is the model for

natural science, but other principles might better suit the social sciences. The term *hermeneutics* also applies to a wide range of philosophical views. The concept of interpretation is central to all of them, but there is variation on the extent to which the objectivity of science is repudiated. Some hermeneuticists are radical relativists.

The split over natural science versus hermeneutics corresponds to a major division in philosophy between what is referred to as the Analytic (not in the psychoanalytic sense) or Anglo-American tradition versus the Continental tradition. The former emphasizes the clear use of language and close reasoning. The latter seeks profundity and tends to view Analytic philosophers as superficial and trivial. Analytic philosophers, on the other hand, tend to regard Continental philosophers as obscurantists who twist language for the purposes of creating the illusion of profundity. Passmore (1) underscores the interesting fact that philosophers belonging to each of these traditions have little interest in each other's work. The fact that highly intelligent people are drawn to both traditions suggests that each is attempting something different. Perhaps psychoanalysis can some day shed light on the motivations that lead an individual in one direction or another.

Since I am more sympathetic to the Analytic tradition, I have tried to be fair by including many Continental references. In his lucid presentation of epistemology, Bonjour (2) underscores the fact that there is no way to refute absolute skepticism. *Knowledge* is generally defined as "justified true belief." A belief unsupported by a justifying argument does not constitute knowledge. Since most contemporary epistemologists reject foundationalism in the sense of a set of beliefs that are

ADVANCE NOTICE CALL FOR ABSTRACTS

AAPP ANNUAL MEETING

May 17 and 18, 1997
San Diego, California

(in conjunction with the American Psychiatric Association Annual Meeting)

Theme:

Consciousness & Its Pathologies

Deadline for abstracts of 600 words is Dec. 6, 1997. For full information contact Jerome Kroll, M.D., Box 393 Mayo, U. Minn. Medical School, Minneapolis, MN 55455. Tel: 612-626-5574, Fax: 612-626-5591, E-mail: kroll001@maroon.tc.umn.edu

true with absolute certainty which can serve as the basis for all other beliefs, one can never be certain that any given belief is knowledge. It may be justified by currently accepted standards, yet later be proven false.

The justification for any belief requires assuming that the premises of the supporting argument are true. These premises may, in turn, be justified by additional supporting arguments. It is obvious that in order to avoid an infinite regress one must eventually arrive at supporting arguments or principles that are accepted without a justifying argument. These assumed principles are called a "justificatory framework." Hume showed that it is impossible to justify the principle of the uniformity of nature, which is the basis for inductive reasoning, without circular reasoning that presupposes the principle it attempts to justify.

Although there are important differences among contemporary epistemologists, a consensus exists on most fundamental justificatory principles such as the uniformity of nature. This point is ignored by psychoanalysts who believe that the rigorous standards of natural science can be evaded by classifying psychoanalysis as a humanistic discipline. For those who espouse the fundamental epistemological principles of Western philosophy, psychoanalysis and other social sciences must be regarded as differing from natural science in the degree of uncertainty and speculation required at this stage of knowledge. Reliance on mystical intuition or the authority of some guru provide the only escape from this uncertainty.

Bonjour's point is relevant to Barratt's (3) vehement attack on Analytic epistemology. Discussing what distinguishes a scientific way of knowing from one that is not, Barratt says, "A preliminary answer might be that science differs from other belief systems in its readiness to call every belief into question and most important, its willingness to call its own beliefs to account" (p. 28). Barratt seems unaware of the problem of infinite regress faced by his view of science. Assuming that every belief requires a justifying argument, the chain of justifications must stop somewhere. Barratt has two choices. One is to deny that we have any knowledge. This is the stance of total skepticism, which is irrefutable but not very interesting. The other choice is to invent a different justificatory framework. Justificatory frameworks are in a sense tested by their success in producing reliable knowledge, even though such testing presupposes the uniformity of nature. No radical alternative to prevailing methodological principles has yet been discovered, but Barratt is free to try.

Goldman (4) says, "The normative ar-

gument of this book thus far has established the need for our inductive principle at every epistemic level of validation of empirical beliefs. The only alternative to acceptance of the principle, then, is skepticism of the most radical sort, which denies to us not only theoretical knowledge but knowledge expressed in everyday perceptual beliefs and even knowledge of how things appear" (p. 293). The clarity of both Bonjour and Goldman presents a vivid contrast to Barratt's writing and exemplifies the differences between the Analytic and Continental traditions. Berger's (5) critique of "state process formalisms" and their detrimental influence on psychoanalytic theorizing is also in the Continental tradition.

Barratt and Berger are among those responding to criticisms of psychoanalysis as a science. One of the earliest and most influential discussions of the scientific credentials of psychoanalysis is the Hook Symposium (6). However, nothing compares to the debate evoked by Grünbaum (7-8) who is often mislabeled a "postivist." Grünbaum emphatically denies that he was ever a Logical Postivist, a movement once dominant in philosophy of science and now universally repudiated. It seems to me unfair to use this term because few are familiar with its precise meaning. Eagle (9) surveys recent developments in psychoanalysis from a critical perspective sympathetic to Grünbaum.

In opposition to the "positivist" stance, various forms of constructivist relativism have been proposed. Constructivism has a natural affinity for relativism. If relativism is the view that the truth of a statement is relative to some perspective or framework, relativism seems more plausible if one also believes that each of us "constructs" his or her own reality. One of the most readable attacks on constructivist relativism is by Searle (10).

For a collection of papers both defending and attacking relativism, the edited volume by Krausz (11) is very instructive. So is one edited by Malachowski (12) on Rorty, a philosopher trained in the Analytic tradition who has been vigorously attacked for his relativism.

Harre and Krausz (13) distinguish four kinds of relativism: semantic, ontological, moral, and aesthetic. Ontological relativism is the kind most relevant to the controversy over psychoanalysis as a science. They explore the paradoxes of epistemic relativism arriving at the ultimate conclusion that "Different aspects of the world are available to different kinds of creatures, in so far as their sensory systems differ, and to different groups of human beings in so far as they are differently placed and differently equipped. In this sense knowledge of the world tends to the

relative. But all such aspects are aspects of one and the same world, and in that sense knowledge of the world tends to the absolute" (p. 224). Few would object to this version of relativism, and it is difficult to formulate a more controversial version that is defensible.

The commonsense relativistic stance described above holds that beliefs about reality are "constructed" and each person's knowledge is constrained by his or her location in history. Analytic philosophers

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are willing to embrace this version of constructivist relativism but strongly oppose more radical versions put forth by Continental philosophers. Analytic philosophers criticize proponents of more radical forms of relativism for equivocating on the meaning of *reality*. By defining *reality* as a person's experience or beliefs about reality, a plausible defense of constructivist relativism can be stated. This defense is no longer legitimate when the meaning of *reality* is shifted to refer to reality itself. This is the core of Devitt's (14) attack on constructivism.

For authors sympathetic to constructivist relativism and versions of hermeneutics that challenge the objectivity of science, I refer the reader to edited collections of papers by Dreyfus & Hall (15), Hiley, Bohman, and Shusterman (16). Madison's (17) attack on E. D. Hirsch (18) reveals the diversity of philosophical views among hermeneuticists. Hirsch defends a realist position in matters of interpretation whereas most hermeneuticists are antirealists. Other useful edited volumes are by Pickering (19) and Alexander & Seidman (20) dealing with various aspects of science and society.

Bernstein (21) has tried to bridge the gap between Analytic and Continental philosophers. He notes the dismissive response of Analytic philosophers to Continental philosophy: "It is a quagmire of confusion, obfuscation, and pretensions gesturing--flouting even the most minimal standards of clarity and rational argumentation" (p. 6). But he is also dissatisfied with those who have become so "enamored" with Continental philosophy that they never achieve critical distance. Other attacks on relativism include Laudan (22), Putnam (23), Hollis & Lukes (24), and Scheffler (25).

Other useful works dealing with epistemological issues relevant to psychoanalysis are listed in the references. Space to comment on each is insufficient. Since the core of Grünbaum's critique of psychoanalysis centers on the difficulty of validating causal hypotheses, I have included several works on the concept of causality such as Salmon (36). Since the concept of causality is central to scientific explanation, the edited volume by Knowles (37) is helpful. Wallace (38) has discussed causality specifically as it relates to psychoanalysis.

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Essay Review

What is DID?: A review of *Rewriting the Soul: Multiple Personality and the Sciences of Memory*, by Ian Hacking, Princeton University Press, 1995.

Between 1970 and 1990, multiple personality disorder (which was redefined to become dissociative identity disorder, or DID, in *DSM IV*) grew from being a rare condition which many experienced therapists had never seen, to one which, some claimed, afflicted 5% of the general population. This extraordinary increase in prevalence puzzled clinicians. Many wondered how such an apparently common disorder could have been overlooked for so long, while perhaps a larger number questioned whether, in fact, at least many of the recently reported cases had been produced by overly zealous therapists treating overly suggestible patients. Scientific controversy and widely publicized lawsuits ensued. Some of the furor has abated during the 1990s, as a more conservative consensus has emerged on how patients in this group should be treated. But there remain both clinical disagreement and conceptual discomfort with the diagnosis of DID.

Ian Hacking, University Professor of Philosophy at the University of Toronto, has now offered an important new perspective on this puzzling disorder, in his book, *Rewriting the Soul: Multiple Personality and the Sciences of Memory*. This is an unusual book in many respects. Hacking is a philosopher with an expert understanding not only with the work of modern British and American philosophers, including Wittgenstein, Elizabeth Anscombe, Donald Davidson and Daniel Dennett, but also with that of Michel Foucault whose work provides background for the central thesis of the book. Hacking further demonstrates an accurate and detailed familiarity with recent and historical psychiatric developments, and, despite the breadth of his knowledge and the weight of his subject matter, Hacking's style is clear and lively. The book is full of clinical and historical examples which are both entertaining and provocative. Thus, for anyone interested in the application of philosophical methods and conclusions to problems of modern clinical practice, *Rewriting the Soul* will be an exemplar and a treat.

Despite these virtues, however, the story which Hacking tells about DID will not be familiar to most clinicians, though it resembles certain dismissive views which are widely held—views that DID is a product of therapists' suggestion, hence an invalid or sham diagnosis. This dismissive attitude is not, I think, Hacking's. But his acutal positions resist our easy categorizations, so this book is likely both to be misunderstood and to provoke debate.

The story which Hacking tells about DID runs as follows. Our conventional model of DID is simple and clear: very traumatic events happening to children with a predisposition to dissociate leads them to develop alter personalities as a coping response. Later, in adulthood, at times of stress these alters "come out" and that is DID. But, Hacking suggests, this model is poorly supported by evidence and at odds with much of what is known (for example, why are there, as of 1994, no child multiples?)

Instead, to understand this disorder, we must consider how particular scientific developments in the late 19th century have come to be adopted by individuals and used by them to rethink and reexperience their own histories - thus developing DID.

During the late nineteenth century (in particular 1874-86, "the age of the Impressionists"), memory became an "object" of systematic scientific investigation. Broca localized specific memory functions in particular brain regions; Ebbinghaus applied statistics to the study of word recall; and others, of whom Hacking takes Theodule Ribot, a French professor of psychology who was Janet's immediate predecessor at the College de France, to be the prototype, began to study the "laws" of memory and, particularly, the laws of forgetting. During this period, which immediately preceded Freud, the notion developed that memories were *constitutive* of the person or soul, and that many of these constitutive memories were forgotten (later: repressed). Thus, by studying the "laws" of memory scientifically, one would develop scientific knowledge of what it was essential about being a person. "In a word [quoting Ribot] the *moi* can be considered in two ways: either in its actual form, and then it is the sum of actual states of consciousness; or in its continuity with the past, and then it is formed by memory." (p. 207)

Yet the development of the "sciences of memory" did more than provide information about brain functioning and psychological laws. It also provided us with models and pictures of who we are and how our minds work. It gave us concepts with which we define ourselves. As Gerald Holton has put it, "Science has always had ... a metaphoric function - that is, it generates an important part of a culture's symbolic vocabulary and provides some of the metaphysical bases and philosophical orientations of our ideology. As a consequence the methods of argument of science, its conceptions and its models, have permeated first the intellectual life of the time, then the tenets and usages of everyday life." (2, p. 41) The sciences of memory provided a model of persons as constituted by their memories (whether recalled or repressed).

Moreover, Hacking emphasizes,

these scientific developments did something further: they engendered the notion that what happens in the past is always a defined event, which existed, whether we are aware of it or not. Similarly, any effects of the past are exerted independently of our knowledge of them. The sciences of memory make the past and memory into what Foucault calls "objects of knowledge."

But this epistemological assumption, Hacking suggests, is incorrect for at least some classes of past events. For while it may be true that some events like bomb blasts and bus kidnappings - events which can be described independently of human intentions, events which, so to speak, just *were* - can be held to have objective existence, other events - those which involve human action and so require intentions - may not have a determinate existence. This is because whether something is an action depends upon how that "something" is described. An event is only an action, according to Hacking, basing his work on that of Anscombe, under a particular description. And since some descriptions may not have existed when the event originally happened (may not have existed within the culture at large, as in the case, perhaps, of trying to apply our current notions of "child abuse" to actions in prior eras, or may not have existed within the minds of the individual actors), the event could not have been described as an action of that type, at that time.

But it can be so described retrospectively. And that is how the sciences of memory have operated to help spawn the recent epidemic of DID: they have provided patients concepts and models with which they can retrospectively redescribe the events of their pasts - as abuse and as producing their current symptoms and life problems. Thus, Hacking is not simply saying that overzealous therapists "create multiples." He is saying that therapists and patients alike adopt a web of concepts which make understanding in terms of abuse, memory, repression and the causal influence of the past seem inescapable.

"We should not think of multiplicity as being strictly caused by child abuse. It is rather that the multiple finds or sees the cause of her condition in what she comes to remember about her childhood, and is thereby helped. This is passed off as a specific etiology, but what is happening is more extraordinary than that. It is a way of explaining oneself, not by recovering the past, but by redescribing it, rethinking it, refeeling it." (pp 93-4)

Hacking is not denying the reality of child abuse, its extent or its suppression. He is not denying that there may be some type of causal connection between child abuse and adult psychopathology, although he approvingly cites David Finkelhor's conclusion that we lack a

"theoretical underpinning" "about why the effects occur." (p. 64) He is not denying that "there are also lots of straightforward memories, suppressed or repressed, of perfectly determinate and thoroughly awful events." (p. 249). Rather he is "exploring memories that are on the fringe of these, memories that arise by mental mechanisms different from the more straightforward recollection, whatever that may be." (p. 249) Old actions under new descriptions may be reexperienced in memory, and if these are genuinely new descriptions, descriptions not available or perhaps nonexistent at the time of the episodes remembered, then something is experienced now, in memory, that in a certain sense did not exist before (p. 249).

Although these philosophical concerns are at the heart of Hacking's interest, most of the book - 200 of its 250 pages - is actually devoted to history. Hacking describes and analyzes a number of developments within psychiatry and psychology pertaining to DID. Yet, despite the intrinsic interest of this historical material (and it is often fascinating), its true functions in the book are, first, to lead us to question what we now assume about DID and, second, to introduce us to a period of psychiatric history in which the concepts we now use to describe (and, if Hacking is right, create) DID first emerged.

Hacking's discussions frequently have the effect of weakening conceptual connections which, for us, have come to seem self-evident: connections between multiplicity and memories, childhood trauma and a particular developmental model of adult psychopathology, acknowledgment of victimization and empowerment, dissociative symptoms in adults and in children, the consequences of actions and their moral worth. They disturb our preconceptions and make us skeptical of obvious-sounding, mechanistic models, as well as the evidence upon which they are supposedly based. All this will be helpful to clinicians who must act on a body of literature and opinion which they frequently cannot evaluate or fully comprehend. One will wish, though, that these various discussions were tied earlier on and more explicitly to the book's central themes and conclusions. Although individually clear, the chapters are disconnected from one another and it is not until late in the book that their import and the philosophical themes which they illustrate are fully revealed. To adapt an idea from Kierkegaard, although this book must be read forward, it must be understood backwards. (3) It also seems that Hacking is more interested in asserting and illustrating his historical-developmental conclusions (e.g., that scientific events in the late 19th century are actually causal in the development of DID), than in proving them by careful, detailed historical analysis.

To return, though, to the philosophically - and clinically - crucial question: Is it possible that *retrospective redescription* of events previously unrecognized as traumatic produced new ways of experiencing the past, and thus DID? A process of this type, as Hacking observes, was postulated to occur by Freud, who believed that sexual events in childhood were not experienced as sexual by the child, but came to be experienced that way - and to *exert influence* as such, by producing symptoms - only later in life, when an awareness of sexuality was present. Freud said that these experiences operated by "deferred action" (*Nachträglichkeit*) (4, p. 25). Is it possible, then, that there are traumas, unrecognized as such at the time they occurred (whose traumatic nature was, therefore, "indeterminate" at that time) which are later redescended and, in the process of being redescended, produce symptoms of DID? It seems that we can distinguish several sorts of case which illustrate differing relationships among events in childhood, subjects' awareness of them at the time as abusive, the actual (determinate) harmfulness of the events, and DID.

First, there may be what Hacking calls "fringe" cases - his example is of a man showering with his nine year-old daughter - in which the adult may not have intended to do harm, nor realize he was doing harm, and in which the child was not aware of the intention or fact of harm being done. In such a case, it may be useful to think that it was indeterminate at the time whether child abuse actually occurred. Only a description using concepts available later, to the adult, will permit understanding or experiencing the event as abusive.

Second, there may be cases in which either the adult or the child or both were unaware of something abusive happening (and, therefore, not intending it, since, per Anscombe, intention implies such awareness of consequences) and yet something abusive was actually happening. Freud's example of a childhood sexual or sexualized experience producing symptoms by deferred action (sexual or other relationship problems, for instance) would illustrate this. We assume that this type of case is common, since children, sadly, often need to represent their childhoods and their parents as much better than they actually were. It may be only during a course of psychotherapy that patients realize something different. Hacking might question what we really know about this type of causality and that is a useful point to make, I think. Yet the effects in this type of case do happen before the patient is aware of their causes (certainly before she is aware of them as causes): they happen independently of retrospective redescription.

Neither of these two cases, though, involves the sort of history given by pa-

tients with DID. These individuals describe childhoods filled with unbelievable torture. Here, the question of whether abuse has occurred does not seem to arise: it is obvious, although suppressed and denied. Not only is it obvious that abuse has occurred, it seems very implausible to suggest that either the child or the adult was unaware of it as abuse. We will not, I think wish to say that it was only later, armed with the languages of symptoms which the sciences of memory have provided, that such patients have reexperienced or redescended their lives as having been abusive.

What Hacking might say about this type of case, though, is this: Yes, those events were traumas - we can't meaningfully question that. Here neither the adult's intention nor the child's experience, nor the probable damaging effects make indeterminacy a real issue. Yet there are still a number of ways in which describing what happened as trauma and using scientific concepts of memory can influence patients who go on to develop DID.

First, childhood trauma *plus* the conceptual ability to describe and think about that trauma in certain ways might be jointly necessary to produce symptoms of DID. Again, this would build upon Freud's notion of *Nachträglichkeit*, but it would supplement the notion of "deferred action" by adding the element of retrospective redescription - both necessary, perhaps, for DID to develop. For DID, hypothetically, the memory of trauma *and* having a concept of the self as composed of memories could lead the individual to dwell on the past, come to interpret her life fully in its terms, etc. And this sequence, somehow - perhaps by changing her perceptions of current situations, or heightening certain affective responses - could promote dissociation. This type of model fits the data Hacking analyzes, as well as much clinical experience (the appearance of new alters during treatment, the rarity of childhood multiples, etc.).

Second, patients who have experienced trauma may learn to appeal to those experiences, use them, in ways which they defend by reference to allegedly scientific models and results. For example, they may claim to know that traumas have "made them who they are": they define themselves in its terms. They could claim to know this because, as Ribot put it, the self is composed of its past experiences. Individuals might also claim and believe in the validity of the "repressed memory theory" - thus in the truth of their own "recovered" memories. For a brief time, therapists believed that the use of hypnosis could provide valid access to previously repressed memories - thus apparently relying on justification by reference to supposedly established scientific fact (this approach is now largely abandoned).

In these ways, more or less explicit appeals to scientific concepts of memory can serve important personal and interpersonal functions - of self definition, self assertion, the formation of common bonds with others. But appeals to science may be used for more suspect purposes, as well. They may be used to avoid personal "ownership" of behavior, avoid meaningful, but frightening interpersonal relationships, or provide mechanisms for the exercise of power and aggression. Hacking suggests, moreover, that, to the extent that these self definitions are based on *falsely* believing that we know certain "facts" about memory and its effects on later life, these dynamics are a kind of "false consciousness." They are, he maintains, to be avoided on that basis alone.

It may seem as if Hacking wants it both ways: he wants both to accept (or at least not deny) the reality of trauma and of its effects, and also to propose a model in which trauma does not, directly, produce DID. But don't we want to have it both ways? Don't we want to accept the essence of what these patients tell us, while we also want to question their certainty, and the impersonal, mechanistic understanding they can have of themselves? Don't we need to accept and account for the fact that multiples do seem to develop or emerge as we know patients longer, even when we clinicians try to steer clear of encouraging them? Hacking's account can help us to accept these difficult and seemingly contradictory realities.

How clinicians might work with a model such as Hacking's or how patients will respond to it are different matters. Clinicians may be led, again, to believe that DID is "merely" a product of "suggestion" (another poorly understood term), while patients may fear, again, that they are being disbelieved or accused of "making it up." These tend to be terms of criticism, intended or not. If clinicians reading Hacking's book then wish to help patients with DID, it will be a challenge to avoid these pejorative attitudes. How to do this is a question which Hacking does not take up. In other disorders, for example schizophrenia or major depression, it has seemed helpful to patients and their families to provide scientific, mechanical models of their disorders to minimize the blaming and shame which are so often present, and to justify helpful "self management" skills (e.g., improved communication, stress reduction, medication adherence). To some extent, this has seemed to work in borderline personality disorder, which often co-occurs with DID. Yet, if Hacking is right, in DID, this scientizing trap is to be avoided. But if it is, what alternative understanding can patients be offered which will acknowledge their past and current suffering (including experiencing themselves as if "fragmented," powerless and

under "foreign" domination), without encouraging false certainty, mechanistic self-representation or splitting into alters? In practice, this seems to require disagreeing with patients and opposing their own self-representations as multiples. How can this be done without antagonism or retraumatization? The task will be to find common ground with these patients which we, as clinicians who, per Hacking, do not have to accept an unproven causal theory, can honestly occupy. This may require that, despite this disbelief, we also come to appreciate what is correct in the DID patient's fragmented, "multiple" self-presentation - how, for example, such a presentation may be an accurate portrayal of her own experience and her "best effort" at finding a way to live within it.

It may seem that Hacking's arguments are a skeptical attack on scientific work in this area. It may look as if Hacking is accusing therapists of an error which makes them and their patients seem foolish. But this is wrong. Hacking is not criticizing genuine scientific research into the effects of trauma or DID. He is saying that that research is not yet conclusive, though it is taken, outside of the scientific community, as being so. And he is not saying anything about foolishness, or wishing to deny the reality of patients' pain and disability. Rather, he is broadening our perspectives on these disorders and asking us, patients and clinicians, not to deny the many factors which determine clinical facts. We may not easily recognize or readily accept the fact that even what we think of as "inner" (such as our memories, self-representations, and "experiences") have external, even political causes. But hasn't it always been the role of psychiatry to say what others have wished to overlook? To humble and empower us at the same time?

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The Freud Exhibit: an Exchange

To the Editor:

Your "From the Editor" column in Volume 4, Number 1, propagates a slanderous allegation that has by now become conventional wisdom in Freudian circles: that the real, hidden purpose of last summer's petition calling for a more broadly based Freud exhibit by the Library of Congress was to get the entire project stopped. To my knowledge—and Morris Eagle, current president of Section 39 of the American Psychological Association, corroborates my impression in the Spring 1996 number of *Psychologist Psychoanalyst*—not one signer saw the petition in that light. As Eagle points out, the initial planning of the exhibit was kept within a tight circle of the analytic faithful—a likely sign of propagandistic intent. The petition that Eagle among others signed meant just what it said: not that the show should be canceled but that a historical presentation by a unit of the United States government should not be exclusively and privately directed by people with a stake in perpetuating a contested biographical legend.

Those same people—the very ones who have been content for decades to keep the bulk of Freud's papers locked away from scholarly view—have subsequently turned the exhibit controversy into a phony censorship issue. Their dishonesty was most recently epitomized in a *Tikkun* article (March/April 1996) by Michael Roth himself, who mock-indignantly quotes me as having declared, "Scrap the whole exhibit." What I actually said (to *Lingua Franca*), after it became apparent that an impasse had developed, was this: "The only helpful thing that Freud scholars can do right now is to ask the Library of Congress to scrap everything and start from scratch." The disinformation campaign in which your editorial perhaps unwittingly participates has been the Freudian insiders' means of forestalling such a reorganization by portraying themselves as innocent victims of an assault on free speech.

Your column goes on to speculate de-meaningly about the unconscious motivation of "Freud bashers" like me, who supposedly can't acknowledge Freud's impact on modern culture. (On the contrary, absolutely no one denies that impact.) As you note, such ad hominem gossip is a classic Freudian technique of blunting criticism and ducking substantive issues. Yet while halfheartedly deploring the practice as it has been recently exemplified by Peter Gay and Jonathan Lear, you seize the occasion to endorse their trite armchair diagnosis of me and other critics. It must be

"the kid in all of us" (Gay) who resists the truth of psychoanalysis, and the anti-Freudian voices convey "echoes of Oedipus's attack on Tiresias" (Lear). Hmm, you say—sounds right to me! Thus those who raise fundamental questions about the cogency of an embattled and waning set of dogmas must really be acting out conflicts they have nurtured since infancy. Can the tactics of evasion get much shabbier than this?

Your own editorial, by alluding to the fact that Freudian tenets are "interwoven into the very fabric of everyday psychiatric practice," reveals why the members of your organization ought to welcome and engage, not pathologize, uncompromising interrogation of psychoanalytic claims. In hour after daily hour of consultation, you are relying on received notions that may well be wrong, inefficient, and even injurious to your patients. But rather than contemplate that possibility, you portray dissenters as Freudian "cases" and pretend that the debate is really about the non-issue of Freud's influence on our age.

This ploy of yours is so familiar that I wrote a whole chapter about it in a book, *The Memory Wars*, that was published before your editorial appeared. There I showed how analytic advocates have self-servingly treated "Freud bashing" as a psychological disorder that can be identified without benefit of acquaintance with the afflicted party. And I added, "What passes today for Freud bashing is simply the long-postponed exposure of Freudian ideas to the same standards of noncontradiction, clarity, testability, cogency, and parsimonious explanatory power that prevail in empirical discourse at large."

Members of AAP who care about those standards will find it difficult to reconcile them with any portion of the psychoanalytic legacy—but they shouldn't be discouraged from trying. In major works of scholarship and critique by Adolf Grünbaum, Frank Cioffi, Malcolm Macmillan, Frank Sulloway, Edward Erwin, Mikkel Borch-Jacobsen, and others, they will come to grips with formidable, invigorating challenges that they may or may not be able to turn back. Surely only a nervous Freudian with his head firmly planted in the sand would want to trade the prospect of such an adventure for Dr. Phillips's limp proposal that we "grant Freud his rightful place among the classics of our culture and be done with it"—meanwhile continuing, illogically but with a good store of inertial momentum, to "trade on psychoanalytic notions in our daily practice."

Frederick Crews
Berkeley, CA

To the Editor:

In his comments on "the brouhaha" over the Freud exhibit planned by the Library of Congress (AAP&P Newsletter, 1996, vol. 4, no. 1), James Phillips demonstrates the difficulties that some people have in responding to the situation in a fair-minded way. (I should mention, though, that given his emotional tone, Phillips may very well have been making no attempt to respond in a fair-minded way.) Rather than offering a reasoned discussion of the issues, he appears to follow those who would label anyone who criticizes Freud in a strong, serious, and fundamental way as a Freud-basher.

Phillips lumps together people (such as Crews, Grünbaum, Masson, and Swales) simply on the basis of their having made strong criticisms of Freud. On the basis of what he regards as the "ferocity" of the criticism of Freud by certain authors, he agrees that such criticism "invites interpretation" along such lines as those offered by such authors as Peter Gay ("Freud's message is really hard to take") and Jonathan Lear ("The real object of attack is ... the very idea of humans having unconscious motivation"). He acknowledges that such interpretations "perpetuate a long and dubious tradition in psychoanalysis of disarming the enemy through interpretation," but suggests that the fact that such interpretations "are self-serving does not make them wrong." Then, it would seem to follow, the fact that criticism of Freud might be (for some) characterized by ferocity does not make it wrong. And does Phillips really want to characterize someone like Adolph Grünbaum as ferocious in his criticism of Freud? Grünbaum may be harsh with Freud on occasion, but he also demonstrates a great deal of respect for Freud and much more ferocity towards certain critics of Freud than towards Freud himself. But whether or not one regards Grünbaum's criticism as harsh and inviting interpretation, his criticisms of Freud and psychoanalysis, as well as the related criticisms of others such as Edwin Erwin in his recent book, *A Final Accounting*, deserve to be taken seriously and have been taken seriously by many scholars and clinicians, including those who are psychoanalytically oriented in their outlook. The issues are there to be dealt with regardless of the emotional tone of the presentations.

Phillips is alarmed about the "rather virulent anti-Freudian and anti-psychoanalytic animus that has taken hold in some quarters of our culture." I wonder if he is concerned about the rather virulent pro-Freudian and pro-psychoanalytic animus that has taken hold in some quarters of our culture? There can be virulence on either, or any, side of

an issue. That there exists such virulence settles nothing.

If one were to accept that emotional tone and use of language might alert one to what invites interpretation, one might consider that Phillips' own remarks invite interpretation. While Peter Gay is an "eminent" historian, Peter Swales is characterized as "the historian who has built a research career on the question of whether Freud had an affair with his sister-in-law...." I believe that even Peter Gay would agree that this is a very unfair characterization of Swales' work. In any event, even psychoanalysts thought enough of Swales' work to invite him to speak last year at an important conference that celebrated the hundredth anniversary of the publication of *Studies on Hysteria*. Swales has also personally helped analysts with their historical research. Four of his papers were included in the first volume of Spurling's four volume collection, *Sigmund Freud: Critical Assessments*. And Grünbaum, who is appreciated even by many of his critics as having done some of the most important philosophical work on psychoanalysis, is referred to merely as "a historian of science."

According to Phillips, the "critics" are engaged in "misguided" "anti-Freudian barrages." They are "purveyors of anti-Freudian invective," "wrong-headed in their attitude." They responded to an article by Jonathan Lear not with thoughtful comments, but "with the predictable knee-jerk rejoinders." Does not the use of such language by Phillips invite interpretation along lines similar to those he would apply to those he refers to as "the critics"? He also wonders out loud if a majority of the signatories (to the petition requesting representation at the conference of the diversity of scholarly opinion on Freud and the origins and significance of psychoanalysis) are really "in agreement with Swales" [the organizer of the petition] design or have been hoodwinked by him into signing the petition." All he had to do was ask. I signed the petition because I thought a balanced exhibit, reflecting diversity of opinion on Freud, was a good idea. Given my interest in Freud's relationship to Nietzsche, I was hoping, among other things, that this very important relationship (as well as others) in the history of depth psychology would receive attention in the exhibit. I believed that this was a greater likelihood if the organizers of the exhibit included scholars reflecting a wide range of opinion on the origins of psychoanalysis.

Regarding the, so-called, attack on unconscious motivation, Phillips might have mentioned that the signatories in-

clude psychoanalysts and psychotherapists who draw on depth psychology in their work. He might have mentioned that John Kihlstrom, a highly regarded psychologist with a strong interest in unconscious mental processes (although not within a Freudian perspective on unconscious motivation), has made very favorable comments on the work of Frederick Crews. Furthermore, it has been evident that Crews and other critics would be quite willing to agree with Phillips on the point of Freud's "enormous and enduring effect on twentieth-century culture" (although there would be disagreement on the nature and value of the effect). It is on matters of scientific credibility that some believe Freud's theories and therapy have fallen short. Also, some authors have argued not that Freud's ideas are without value or originality, but that some of the most fruitful ideas that are typically associated with him and thought to be original with him, were, in fact, not original with Freud at all. Notions of unconscious motivation, unconscious psychic conflict, repression or inhibition, unconscious guilt and envy, psychological defense, the importance of the drives, drives turned against the self, sublimation, the meaningfulness of dreams, and much more were not original with Freud. In his response to Crews, Mortimer Ostow (*The New York Review of Books*, February 3, 1994, p. 37) writes of "the basic dynamic mechanisms recognized by Freud ... conflict, repression, transference, defense, sublimation, unconscious guilt" Ostow does mention some ideas that are original with Freud, and it is true that he writes not that Freud discovered such things but "recognized" them. But, as is often the case in writings of many Freudian-oriented Freud scholars, there is no, or only minimal, indication that a number of psychiatrists, psychologists and philosophers before Freud explored, in a serious way, many of the important concepts associated with psychoanalysis. We still can read Peter Gay, in his book *Freud: A Life for Our Time*, refer to "the memorable epigrams of Schopenhauer and Nietzsche," and state that from such sources Freud was able to take "a shadowy, as it were poetic, notion, lend it precision, and make it into the foundation of a psychology..." (p. 128). It was Freud himself who first began to characterize the psychological insights of the likes of Schopenhauer and Nietzsche as flashes of intuitive brilliance to be contrasted with the hard, tough, laborious accumulation of knowledge that characterizes psychoanalysis as a science. Nietzsche, however, clearly and repeatedly referred to himself as a psychologist and developed a psychology that had a prominent place for dynamic unconscious mental processes.

Phillips suggests that "the detractors"

have "entirely missed" the fact that Freudian psychoanalysis continues to significantly inform psychiatric work. But how could anyone with an interest in the field not be aware of the fact that psychoanalytic theory and therapy continue to inform psychiatric work, even though that influence is waning in certain quarters? After all, some of "the detractors" write for psychoanalytic clinical journals and speak at conferences sponsored by psychoanalysts, some of whom are prominent psychiatrists. The important issues include, I believe, whether or not the psychoanalytically informed aspects of such theory have been validated and the specifically psychoanalytic ingredients of the therapy have been found to make a difference in regard to treatment effectiveness. Phillips concedes that behaviorists could deny the psychoanalytic influence, but that since "most behaviorists are *cognitive-behavioral* therapists," the door is open "to psychodynamic notions." (The idea here is that the critics fail to see the influence of Freud and psychoanalysis even upon those who do not regard themselves as psychoanalytically oriented.) Yes, the door may be open, but many cognitive therapists (and cognitive psychologists) have chosen not to step inside, or, after having been inside, to leave. Then Phillips remarks that he "might also cite the elegant empirical research" that has demonstrated the equivalent efficacy of psychotherapy as compared to psychopharmacology in the treatment of depression. (Does he think that those who are critical of Freud and psychoanalysis are enthusiasts of psychopharmacology or believe that psychotherapy is useless?) Many readers may be aware of the research, but Phillips does not cite it. He only states that while the therapies were structured and short term, "they are clearly informed by concepts carried over from the psychoanalytic tradition." But even to the limited extent that this may be true, an important issue concerns which aspects of the therapy are the most significant therapeutic ingredients. As is well-known, one might be quite willing to argue that the evidence demonstrates that psychoanalytically oriented or informed therapy may be of significant help to many persons, but not that there is anything particularly psychoanalytic about what are the most active therapeutic ingredients.

Ultimately, however, Phillips seems to be willing to bypass matters of validity and effectiveness when it comes to the work of Freud himself. He believes that Freud can and should be read as a classic, like Plato. Richard Webster's important book, *Why Freud was Wrong*, is dismissed as Phillips states that "it would be as unproductive to dismiss Freud as 'wrong' ... as it would be to dismiss Plato as 'wrong'."

But while whether or not Freud was wrong on any number of matters may not be the most important issue in evaluating his contributions, surely an understanding of where Freud was right and where he went wrong is of some importance. In any case, the issue of whether or not to read Freud as a classic is not what "the brouhaha" has been about, and reading Freud only as a classic is not something that most analytically oriented therapists would be willing to settle for. And while in certain ways Freud thought of himself as a philosopher, conquistador, and prophet, he also regarded himself as a scientist offering accurate observations and testable theories.

Phillips is probably right that some critics of Freud and psychoanalysis are offended by the "certainty" with which someone like Jonathan Lear proclaims psychoanalysis' special attunement to the deeper currents of life. Some critics may believe that while psychoanalysis has made valuable contributions to the fields of mental health and the general culture, it also has often not been attuned to the deeper currents of life; that, in fact, it has too often offered answers and certainty where the questions (along with lived experience) were insufficiently attended to.

Phillips suggests that "there would be nothing inherently 'wrong' in the Library of Congress mounting an exhibit that offered a favorable and acknowledged one-sided portrait of a historical figure" Is he suggesting that the organizers of the exhibit were going to acknowledge that they were presenting just such an exhibit? Not likely. It would not have been seen and understood as such by many visitors. And can one imagine at this time an exhibit (by such a prominent public institution) on other figures who have had a profound impact on the intellectual world of the twentieth century that would not include diversity of opinion on the achievements and legacy of that person? Can we imagine a Library of Congress exhibit on, for example, Nietzsche, who has been described as the father of the post-modern world, that did not include critical perspectives on his work? If a public exhibit was being planned that did not include such critical perspectives, I would be quite willing to sign a petition calling for the presence of a diversity of opinion.

Ronald Lehrer, M.S., Ph.D.
New York

Editor's Reply

First, since in my view the discussion of the Freud exhibit has been more shrill than productive, I will begin by regretting my contribution to that tone. While still mystified over the intensity of some of the Freudian revisionists' language, I think it serves no useful purpose to impugn their motives or discount their various arguments on the basis of the tone in which the arguments are delivered. They don't require psychoanalytic interpretations of their intentions, and Professors Crews and Grünbaum's responses to Jonathan Lear's article don't need to be labeled "knee-jerk." In this regard I agree with Dr. Lehrer that one could equally question the motivations of the true-believing Freudians. Indeed I find the Freud-adulators just as mystifying as the Freud-bashers. I am not an analyst and don't count myself among the former. In my column I was simply taking the occasion of the Freud exhibit to write about the latter.

The issues raised by the two letters divide into three topics: the exhibit, the status of psychoanalysis as theory and treatment, and the place of Freud in twentieth century culture. Let me address each in turn.

About the exhibit we are left with a number of conflicting stories or narratives. Let me relate what I take to be the three main ones. (1) Official psychoanalysis, as embodied in the figure of Harold Blum, in an effort to buoy up the sinking ship of psychoanalysis, conned the Library of Congress, along with the guest curator, Michael Roth, into a hagiographic spectacle of their hero, Freud. A committee of the psychoanalytic faithful such as Peter Gay were gathered to organize the exhibit in a safe and predictable manner, and none of the prominent psychoanalytic detractors or revisionists was invited into the committee. This piece of outrageous mendacity might have been accomplished but for the watchful eyes of Peter Swales and others. A petition was organized demanding a more balanced exhibit. Michael Roth's feckless efforts to accommodate the petitioners were rejected as inadequate. The exhibit was canceled for 1996 but then rescheduled for 1998, now with the participation of some of the petitioners such as Adolph Grünbaum and promising to be a balanced exhibit. (2) In view of the fact that the Library of Congress has the largest collection of Freud archival materials in the world, a decision was made to mount an exhibit that would both display the materials and present Freud's towering place in twentieth century culture. Although the intent was to offer an evenhanded exhibit that included the controversies surrounding psychoanalysis, a petition organized by Peter Swales was developed in the con-

cern that the exhibit not be too biased toward the pro-psychoanalytic position. Roth contacted all the signatories requesting their advice, but this exchange was thwarted by Swales and Crews, who challenged the whole enterprise and succeeded in scuttling it. (3) An exhibit dealing with Freud, psychoanalysis, and their impact on twentieth century culture was scheduled by the Library of Congress for the Spring of 1996. A petition was organized by a group of scholars who were concerned that the exhibit as planned would not accurately represent the controversies within the scholarly community concerning Freud and psychoanalysis. In the midst of negotiations around the petition the exhibit was canceled/postponed for budgetary reasons and then rescheduled for the Autumn of 1998. In the midst of this process the media jumped on the story and made it out that the petition had brought about the cancellation of the exhibit. (Writers such as myself were unwitting victims of the media disinformation campaign.) Current planning involves a broader representation of the diverse scholarly community in the committee of consultants than was originally planned.

How does one judge among these narratives? Each is coherent, as good narratives tend to be. And each has its supporting evidence. Both Crews and Lehrer insist that the goal of the petitioners was *not* cancellation of the exhibit. Morris Eagle, who signed the petition and whose credentials as a friend and scholar of psychoanalysis are beyond question, writes to the same effect in the Spring 1966 issue of *Psychologist Psychoanalyst*. He also references letters to the *New York Times* by several other signatories, including Adolph Grünbaum and Oliver Sacks, all arguing the same point. Michael Roth, on the other hand, writes in his *Tikkun* article: "The legitimate concerns expressed by the petition were soon overshadowed by the overheated rhetoric of two stars of Freud criticism, Frederick Crews and Peter Swales. 'Scrap the whole exhibit,' Crews said; the exhibit should be aborted, Swales insisted. Neither knew for sure, they admitted, what would be in the exhibit--how could they? But they felt justified in trying to stop it in any case, because they distrusted some of the people and institutions associated with its planning. In a letter to the *New York Times*, Crews made the nasty charge that the organizers and the writers of the catalogue were partisan and intellectually mediocre. In fact, the basis for the accusation was simply that the famously non-partisan and original Crews imagined that he would disagree with several of them" (Vol. 11, No. 2, March/April 1996). Now, I assume that Crews, Lehrer, and Eagle would each lean toward some combination of Narratives One and Three, and Roth toward a combination of Narratives Two and

Three. But how is the impartial witness to decide which of these narratives is closest to historical truth? Lehrer advises that if I want to know the intentions of the signatories, all I have to do is ask. Good, but can I then also ask Roth about his intentions? Crews accuses the latter of dishonesty and disinformation, so somehow I can believe the signatories but not Roth!

In fact, an objective assessment of which narrative is most accurate would be extremely difficult. The various parties would have to be questioned, their previous communications regarding the matter examined, and--most critically--their responses evaluated for credibility. The last point is not a minor one. In a contentious matter like this, why take anyone's statements at face value? After all, if Frederick Crews and Michael Roth are each calling the other a blackguard and insisting on his own probity, someone is surely fibbing. In the end, one's attitude toward the controversy and the conflict of narratives will probably depend on one's previous attitudes toward the parties to the dispute. For myself, I can see merit in each of the narratives (after all, I made them up). On the one hand, the committee did seem stacked with card-carrying Freudian club members; on the other, some of the provocative statements attributed to the detractors seem anything but conciliatory. Regarding the reasons for the the cancellation/postponement, Morris Eagle assures us that "there is little or no evidence that the letter had anything at all to do with the postponement of the exhibition..." but Roth's remarks (and he, I assume, is in a position to know) in the *Tikkun* article are in that regard quite ambiguous. As for me I will continue to waver unsatisfyingly from one narrative to the other. And if I am perceived to have moved from Narrative two to a more neutral position, that is correct. All credit to my critics!

In the end the squabble over the exhibit is small change compared with the larger issue of the status of Freud and psychoanalysis at the end of the twentieth century. This discussion is a large one, with many voices and many points of view. Much of the revisionist criticism, including much of Professor Crews', deals with the early history of psychoanalysis. Although this research is often of great interest (while I can't get too exercised over Freud's possible affair with his sister-in-law, I certainly find other of Peter Swales' research interesting), I agree with several of the respondents to "The Unknown Freud" that the relation of the research to the contemporary evaluation of psychoanalytic theory and treatment is only marginal, and I will not address those findings here. The same must be said of Lehrer's questions regarding Freud's originality--again, certainly interesting, but mostly irrelevant to the contemporary

evaluation.

To engage the discussion of the status of psychoanalysis one needs to know from what common ground the discussion may begin. With Professor Crews this is not clear. Does he maintain that *all* psychological intervention is bogus, in which case the discussion would more productively focus on that rather than on psychoanalysis. Or does he accept that psychological intervention as such (or at least *some* psychological intervention) is valid, and only the psychoanalytic therapies bogus? I will assume the latter in order to continue the dialogue. (Lehrer states clearly that while there may be evidence for the efficacy of some—including psychoanalytically informed—therapy, the issue remains to demonstrate whether the “active ingredient” has anything to do with psychoanalytic principles.) Crews writes in his letter: “In hour after hour of consultation, you are relying on received notions that may well be wrong, inefficient, and even injurious to your patients. But rather than contemplate that possibility, you portray dissenters as Freudian ‘cases’ and pretend that the debate is really about the non-issue of Freud’s influence on our age.” This is not a very flattering portrait of contemporary psychiatrists like myself. I certainly prefer to see myself, as do I think many of my colleagues, not as an unthinking robot mouthing psychoanalytic dogmas, but as an eclectic practitioner using *and evaluating* psychoanalytic and other therapeutic modalities on an ongoing basis. I can’t imagine on what basis Professor Crews could insist that I am *not* doing that.

How is such evaluation to be carried out? Professor Crews is fond of pointing out that “no uniquely psychoanalytic notion has received independent experimental or epidemiological support...” While efforts to confirm or disconfirm empirically the efficacy of analytically based therapies are clearly important, they will only partially determine the fate of those therapies. As practices that belong more to the tradition of practical knowledge than positive science, they will ultimately be tested out more in the consulting room—and by the ongoing consensus of practitioners—than in the psychology laboratory. Therapeutic notions that are found useful will continue to be employed; those that are not will be tossed aside. If some psychoanalytic principles continue to survive this winnowing process, this may not be, as Crews suggests, merely because the practitioners are blind to their unproven status.

Crews and others may certainly lament our reliance on this kind of evaluation. I can only respond that, in my opinion, it belongs to the nature of therapeutic practice. This is of course the point of my disagreement with Adolph Grünbaum, whose critique of the scientific claims of

psychoanalysis is applicable to its present as well as its past incarnations. I have no quarrel with his conclusions regarding psychoanalysis’ failure to make good on its scientific claims. I would argue rather with his premise that it needs to. If it is primarily an interpretative discipline, it will be judged by other standards (see my “Grünbaum on Hermeneutics,” *Psychoanalysis and Contemporary Thought*, Vol. 10, 1987). At this point I would expect Crews (and perhaps Lehrer) to respond (1) that psychoanalytic principles as I am defending them are so removed from their original, concretized Freudian formulations as to be vague and fuzzy beyond the possibility of empirical validation; and (2) that interpretative, hermeneutic approaches to psychoanalysis are such relativistic, anything-goes, enterprises as again to remove them from the arena of objective validation. I plead partial guilt on both counts, arguing again that these problems inhere in the phenomena under discussion. Here, I would argue, is where I at least would try to engage the discussion.

Finally, a brief word about Freud’s place in our culture. Crews insists that he *does* acknowledge Freud’s impact on modern culture. He doesn’t add that he presumably considers it a pretty baleful impact. Writing in his Freud Basher article, he calls Freud “the most overrated figure in the entire history of science and medicine”—thus paying him the underhanded compliment of being the biggest con-man, if not a hero, of twentieth century culture. The most Crews seems willing to grant to Freud as a figure of modern culture is the regurgitation of commonplaces that could equally be attributed to any number of historical figures. For his part Lehrer seems willing to grant the significance of the Freudian message, while questioning Freud’s originality. The grand themes of Freudian discourse—the presence of an other side to rational consciousness, the persistence of the infantile in adult life, the inevitability of psychological conflict, along with the consequences of facing or not facing it, the limits to self-awareness and the inevitability of self-alienation—such themes, which continue to inform modern consciousness, are neither totally original nor banal commonplaces. To Dr. Lehrer I would propose that we question not only their sources and originality but also why it is in their Freudian formulation that they have had their impact. To Professor Crews I would question on what basis he assumes that these ideas continue “to be accepted largely on faith—namely, a faith in Freud’s self-description as a fearless explorer, a solver of deep mysteries, a rigorously objective thinker” (“The Unknown Freud”). As in the case of clinicians working with psychoanalytic concepts, the only reason he can imagine

for adhering to such ideas is that one is a dupe. That critics in the arts and humanities may continue to employ Freudian ideas because they find them useful and valuable doesn’t seem to merit consideration.

James Phillips

Professor Crews Replies:

I am grateful to Dr. Phillips for granting such an ample forum to his critics and for acknowledging that his account of the Library of Congress controversy was not based on established facts. But I am afraid that his current withdrawal to a “Rashomon” neutrality with regard to the motives and actions of last summer’s petitioners—he now intends to “waver unsatisfyingly from one narrative to the other”—typifies the antiinvestigative stance that I (along with Dr. Ronald Lehrer) found characteristic of his approach to a quite different topic, the scientific standing of Freudian tenets. If one highly injurious “narrative” about the petitioners is ascertainably contradicted both by their unanimous recollections and by the documentary record, there can be no virtue in portraying it as bearing the same degree of plausibility as what the record actually shows.

How, Phillips asks, can he possibly find out whether Michael Roth was telling the truth when he quoted me as having said “Scrap the whole exhibit”? The answer is that he can check Roth’s “quotation” against the correct version in *Lingua Franca* and can thus perceive that, indeed, Roth inverted the meaning of my sentence. Likewise, when Roth later declared in *The Chronicle of Higher Education* (May 24, 1996) that “a few critics urged the library to cancel the exhibit,” he was making an allegation that can be readily checked: just ask James Billington, the library’s director, whether or not any such requests were ever received. If not, then Roth was (to put it mildly) gravely mistaken in his claim, and his version of events should be emphatically laid to rest. Can I count on Dr. Phillips to pick up the phone and make this minimal forensic effort, or must I and the other petitioners remain under a cloud of slander?

So long as Dr. Phillips continues to refer to principled critics of psychoanaly-

sis as "Freud bashers" and to phrase their arguments in straw-man terms, I despair of conducting a useful intellectual exchange with him. Of course I don't maintain that he and his colleagues are "unthinking robot[s]" mouthing psychoanalytic dogmas," or that *all* psychological intervention is bogus," or even that "only the psychoanalytic therapies" are "bogus." Compare the crudity of these formulations with the actual issue as Ronald Lehrer correctly states it: "whether or not the psychoanalytically informed aspects of [contemporary psychiatric work] have been validated and the specifically psychoanalytic ingredients of the therapy have been found to make a difference in regard to treatment effectiveness." I myself have repeatedly raised that question in books and articles dating from 1980 to the present—but rather than address it, Phillips retreats once again into his circular appeal to the "practical knowledge" of therapists. Abusing his hospitality, let me go over his head and ask readers of this newsletter: don't *you* believe that elements of discredited folklore get entangled with "practical knowledge," and don't you think it worthwhile to try to disentangle them?

Frederick Crews

Dr. Lehrer Replies:

There are a few points that I will attempt to clarify. Regarding the petition, I signed and supported it, but I was not involved in organizing it. My suggestion that Phillips might have asked about the intentions of the signatories was a specific re-

sponse to his concerns that those of us who were not involved in organizing the petition might have been duped by the organizers. Regarding Freud's originality, I did not, and do not, question that Freud created and developed many important ideas and hypotheses. I suggested, though, that some of the most important ideas typically associated with Freud were not original with him. Phillips suggests that this matter, while interesting, is "mostly [note: not "entirely"] irrelevant" to the contemporary evaluation of these ideas. That may or may not be the case, but I did not make any related statements or claims on this matter. The exhibit, as I understand it, is to be, among other things, a historical presentation. It would seem to follow that the matter of origins is essential to such an exhibit. In this regard, Schopenhauer, Herbart, Griesinger, Fechner, Hartmann, Nietzsche, Breuer, Meynert, Charcot, Janet, Benedikt, Bernheim, and a number of other important figures would seem to require attention. (On Nietzsche, see my *Nietzsche's Presence in Freud's Life and Thought*, SSUNY Press, 1995, and "Freud's Relationship to Nietzsche: Some Preliminary Considerations," *The Psychoanalytic Review*, Vol. 83, June, 1996.) Phillips' question as to why it is that many ideas that have emerged in the field of what we might refer to as depth psychology have made their impact in their Freudian formulation would be a very good question for the exhibit to consider. But the answers found will not necessarily be related to the validity, or even the plausibility, of the relevant aspects of Freud's work.

Like Phillips, I regard myself as "an eclectic practitioner using and evaluating psychoanalytic and other therapeutic modalities on an ongoing basis." But while

I attempt to attend to what can be "tested out...in the consulting room," I also attempt, as I imagine Phillips does, to integrate what I can that derives from empirical, including experimental, research on human development, the development of what we refer to as psychopathology, the nature and function of dreams, process and outcome studies, etc., as may be applicable in my work with unique individuals. Careful attention to the unique perspectives or narrative constructions of an individual does not exclude contributions to our work that can be derived from the more general conclusions of relevant empirical research. And I believe that a good deal of the relevant and compelling research that has appeared over the last twenty-five years or so points in directions that run counter to many of the foundations upon which much that characterizes psychoanalytic therapies are built. It will not do, as has been done by some prominent analysts, to proclaim, for example, that the findings of empirical research on human development are of little relevance to psychoanalytic theory and therapy when psychoanalytic theory and therapy are, in part, founded on certain views of human development. In a related vein, it is not an encouraging development when the decision-makers at a prominent psychoanalytic journal like *Psychoanalytic Dialogue* make an exception to their usual invitation to the subjects of symposia and do not invite Grünbaum to reply to the papers in their recent "Symposium on the Grünbaum Debate."

Ronald Lehrer

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