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Rosenthal Lecture

By Jim Lippard

On February 17, 1989, the psychology and communications departments of the University of Arizona co-sponsored a talk by Harvard social psychology professor Robert Rosenthal. Rosenthal is known for his decades of research into self-fulfilling prophecy, experimenter expectancy effects, nonverbal communication, and sources of artifact in data analysis. (Those who attended the 1987 CSICOP conference in Pasadena will remember his panel participation in the session on "Animal Language: Fact or Illusion?".)

Rosenthal's talk at UA was titled "Covert Communications in Classrooms, Clinics, and Courtrooms." He began by describing how he became interested in unintended social influence from nonverbal cues. He unconsciously influenced the results of his doctoral dissertation work (at UCLA in the 1950's) on the use of projection as a defense mechanism. (He did not give details, but joked that he would do so in the question and answer session if any audience members were insensitive enough to ask.)

The first early experiment into such unintended cues involved showing photographs of people to test subjects, who were asked to rate the individuals pictured for degree of personal success on a scale of -10 to +10. But the real test subjects were the experimenters themselves. Half of the experimenters were told to expect an average ranking of -5, and half were told to expect +5. The experimenters got the results they expected. Since all of the subjects had been read the same set of instructions, nonverbal cues were implicated.

This experimenter expectancy effect has been replicated many times, and one of the more interesting versions was done at the University of Manitoba by John Adair and Joyce Epstein. In their two-stage experiment, the first stage was as described above. In the second stage,

with new subjects, no experimenters were used. Instead, audio tapes of the experimenters' instructions from the first stage were used. Subjects were directed down a hall by signs, which told them to enter a room and press a button on a tape recorder. The expected result was that the expectancy effect would be eliminated or reduced. But in fact, the result was a *larger* effect. This is evidence that certain kinds of information (in this case auditory) are more easily gleaned than others. Rosenthal commented that people are more accurate at detecting liars by listening only to their voices rather than watching and listening to them. In our culture, he said, visual cues swamp the auditory ones (and apparently the auditory ones are a more reliable indicator of truth).

In experiments designed to determine what cues caused the expectancy effect, Rosenthal and colleagues made audio tapes of high-bias researchers (with a .60 correlation between experimenter expectation and photo rate). The tapes were of the experimenters' instructions to their subjects, which contained sentences like "The scale runs from -10 to +10. -10 means the person experienced extreme failure. +10 means the person experienced extreme success. -1 means the person experienced mild failure. +1 means the person experienced mild success." The sections of the tape with positive or negative connotations (+10, +1, success, -10, -1, failure) were examined by psycholinguists for differential vocal emphasis. A .72 correlation was found between emphasis and the photo ratings, but only a .24 correlation was found between the experimenter expectancy (the bias) and the emphasis.

At this point, Rosenthal digressed with a "footnote" about the practical meaning of a .24 correlation. He noted that while such a correlation appears small, it can have great practical significance. He noted that a recent study in which 22,000 doctors took an aspirin tablet every other day to prevent heart

attack was terminated early for ethical reasons. The aspirin was so obviously helpful that the control subjects were told to start taking aspirin. The observed correlation in this experiment between aspirin use and heart attack prevention was only .037--but this accounted for the extended lives of 4 out of 100 subjects. (A similarly small correlation was obtained in tests of the drug AZT on AIDS patients.)

Much of Rosenthal's work has initially been greeted with incredulity. One example he gave was his work with rats labeled as "maze bright" or "maze dull." At the end of a quarter, rats which had been (randomly) labeled "maze bright" ended up performing better in maze tests than those labeled "maze dull." One of Rosenthal's colleagues thought that this was perhaps plausible, but would certainly not occur if the rats were more isolated from their experimenters, as in a Skinner box. So he set about to do a replication with rats labeled as "Skinner box bright" and "Skinner box dull." By the end of the quarter, the rats labeled bright had become so, and those labeled dull had become so. As an interesting side note, Rosenthal said that this experiment was conducted in three lab sections, and the leader of each lab section had a different practice for dealing with students who came to complain about poor performance by "dull" rats. One section was led by a student of Rosenthal's, who responded by saying, "What do you expect from a dull rat?" One section was led by a clinical counseling student, who responded by saying, "And how does that make you feel?" And the third section was led by his colleague, who said, "What do you mean 'dull rat'? There's no such thing as a dull rat, just a dull experimenter." The surprising result was that all three sections had the same magnitude of expectancy effect.

Rosenthal noted that the expectancy effect on animals had really already been mentioned in 1927 by Bertrand Russell, who stated that animals studied by Americans rush about, while those studied by Germans sit still and think.

In one of Rosenthal's more controversial experiments, teachers were told that some students of theirs (chosen at random) were gifted. The result was that those students claimed to be gifted performed better in school (correlation .08). In 1985, meta-analyses (not by Rosenthal) of about 150 of these experiments suggest that the best hypothesis about what happens when teachers are given high expectations about student performance involves two orthogonal factors--affect and effort. Affect is the teachers' treating students as being gifted, and effort is the teachers' working them harder (e.g., giving them more words to learn and more problems to solve).

In an as-yet-unpublished experiment by one of Rosenthal's students, various expectancy effects were found in elementary school classrooms. It is known that males perform better at quantitative and spatial tasks while females perform better at verbal tasks, but this experiment found that this generalization is not true in elementary school classrooms where teachers don't believe it to be true. Videotapes of male and female students and teachers teaching quantitative and verbal matters were examined for differential treatment. Among the discoveries were that teachers are more hostile in teaching cross-sexual materials (i.e., quantitative to girls and verbal to boys), and that the effect was less for female teachers and even less for androgynous-appearing teachers.

Rosenthal next described some further male/female differences. In observations of psychology experiments, it was found that experimenters smiled at male subjects 12% of the time, and at female subjects 70% of the time. When the experimenter and subject were of the opposite sex, standardized psychology experiments take longer than if they are of the same sex. (An audience member pointed out that the same effect may be observed in bank tellers serving customers.) Rosenthal also noted "channel discrepancies" between the sexes. Male experimenters were friendlier to female subjects both in body

movement and tone of voice, while female experimenters were observed to be friendly in movement but not in tone of voice with female subjects, and not friendly in movement but friendly in tone of voice with male subjects. Rosenthal also commented that females telling the truth and males lying were judged as being physically more attractive than females lying and males telling the truth.

Rosenthal next turned to expectancy effects in the courtroom. In an experiment conducted with the help of a Bay area judge and one of his former students, Peter Blank, five judges were videotaped giving instructions to juries in 34 trials. Subjects rated the "judicialness" and wisdom of each, some being shown videotape and some just audiotape. It turned out that the ratings of judge's wisdom could be used to accurately postdict the defendant's past criminal history. When the judge was rated as wise in terms of visual cues, the defendant was more likely to have had a past criminal history. When the judge was rated as less wise in audio, the defendant was also more likely to have had a past criminal history. In predicting the verdict, the ratings were not so good. When the judge's tone of voice was rated as honest, the jury was more likely to have found the defendant guilty. Rosenthal emphasized that no causal claim was being made here, but that further research will be looking at it.

Rosenthal reported several other experimental results in the same vein, and skipped over many others which he didn't have time to present. His work is obviously of great significance to parapsychology and other areas of the paranormal. Those interested in examining his work could begin with some of those listed below (from his 20 books and 250 articles). For a critical look, see the book review of T.X. Barber's *Pitfalls in Human Research*, elsewhere in this newsletter.

Rosenthal, R. (1968) *Pygmalion in the classroom: Teachers' expectations and pupils' intellectual development*. Holt, Rinehart & Winston.

- (1976) *Experimenter effects in behavioral research*. Irvington.
- (1984) *Meta-analytic procedures for social research*. Sage.
- and Rubin, D.B. (1978) Interpersonal expectancy effects: The first 345 studies. *Behavioral and Brain Sciences* 3:377-386.

Book Review

Philosophical Essays in Pragmatic Naturalism

by Paul Kurtz

1990, Prometheus Books

Reviewed by Bill Green

My reason for reading Dr. Kurtz' *Philosophical Essays in Pragmatic Naturalism* is that I am searching for a philosophical foundation for methods of arguments that have the power to make me change my current set of advocated/practiced ethics. Being a skeptic, rational changes to my practiced ethical set come about for two reasons: accepted changes to my set of goals and recognition that a subset of my current set of ethics are inferior to a replacement subset with respect to their ability to achieve my current goals. (Being human, I don't live up to my ideal.)

Dr. Kurtz is one of my heroes. He is the current head philosophical guru of the skeptical and the humanist movements and a brilliant person. I loved the book as I feel it did outline the philosophical foundations upon which I could encourage the scientific evaluation of ethics with respect to their ability to support the proclaimed "ends" of the advocacy group.

Reading the book was a great education for me. Through serendipity, I had failed to return a paperback book club post card and received a copy of *The New York Public Library Desk Reference*. It has several pages of definitions of terms used by philosophers which was most useful to me in reading Dr. Kurtz's book as the majority of chapters are essays from various esoteric professional journals (mostly from the 50's and 60's) and brimming with terms used by professionals in the field.

The book is divided into three sections:

I. PRAGMATIC NATURALISM

"focuses on 'empirical metaphysics,' a theory of nature drawn based upon natural sciences and a theory of human nature drawing on behavioral science."

In this section I was most impressed with Chapter IV, "Co-duction: A Logic of Explanation in the Behavioral and Social Sciences." Dr. Kurtz's suggestion is that rather than lock yourself into either the reduction approach (combine all into one science) or the holistic approach (each must remain separate), use either or both as seems appropriate.

II. NATURALISTIC ETHICS

"defends a modified form of naturalistic ethics, i.e., the view that ethical problems can be resolved by empirical methods and value judgments tested by their consequences in conduct."

This section was a feast for my soul. I loved it. The following statements I consider beautiful:

A. "Religion stresses the existential dilemma of life conscious of itself: the soul of man cries in the wilderness of the universe for certainty, but it can find no compulsive guarantee for any way of life." (p. 126)

B. "But this is my point—if a person *refuses to believe*, there is little you can do. He may reject wholesale your definition and methodology. If so, you may argue *ad infinitum* and you still may not be able to *prove* to him the facts of nature and life and that he should believe them." (p. 131)

C. "Any successful solution of the problems of practice must be in terms of existing structures which sciences do not control. And it is time philosophers cease requiring of other philosophers absolute standards to solve practical problems. It just can't be done." (p. 139)

D. "Whatever contributes to the maintenance and expansion of life is valuable; ...Culture indicates that some restriction of life and some point may be essential for life..." (p. 162)

E. "Thus practical ethics in the last analysis cannot be abstracted from theoretical wisdom or scientific knowledge. True, Aristotle tells us that ethics is not merely concerned with

knowing good, but with making men good. Yet becoming good depends upon a knowledge of what the chief good is for man, and this in turn depends upon a teleological view of the human species." (pp. 174-175)

F. "There are three main factual tests of a 'good' rule: ...[that it] fulfill its purpose or ends and be consistent with existing long-term desires ... [be] framed in the light of the available and feasible means...be grounded in the laws of nature and be consistent with the demands of logic." (p. 183)

G. "If I were to bare my own normative position, the valuational base from which I operate, I would label it 'humanistic ethics' rather than simply 'naturalistic ethics.'" (p. 220)

H. "Ethical skepticism tends to liberate us from vain pretensions. It enables us to moderate and humanize intolerant doctrines. There is nothing as unprincipled as men of principles. Get out of their way, since they are all too prone to consume others in the name of their moral dogmas." (p. 237)

I. "The sad truth is that no person can live without some moral faith, not even the skeptic. The difference is that the skeptic is aware of the limits and pitfalls of his cherished principles and values." (p. 237)

III. NATURALISM VS. PHENOMENOLOGY AND EXISTENTIALISM

"Part Three contrasts pragmatic naturalism with the schools of phenomenology and existentialism. Kurtz maintains that the philosophy of pragmatic naturalism provides the foundations for a cosmic outlook and an authentic ethical humanism."

I thought he did a real good job showing the shortcomings of existentialism.

A. "To claim that man has no nature but only an 'existence' is to make inexplicable the facts of science that we already know about him, and these are considerable." (p. 261)

B. "The ideals of reason and science have not been effectively refuted, yet they are in constant danger of being

undermined by an Existentialist type of reaction." (p. 264)

I have only one problem with Dr. Kurtz. He has claimed in *Free Inquiry* that it should be possible to develop a set of ethics that would be acceptable to believers and skeptics, and I am skeptical of the claim as our goals are different. He did not make any such statements in the book.

The thing that pains me most is that for centuries scientifically oriented philosophers have advocated using scientific methods to evaluate our ethics for their ability to promote our declared ends. However, I have not seen a published article in a popular scientific journal such as *Nature*, *Science*, *Scientific American*, etc. that described the results of such a study or that covered methodology. I would not only cheer, but financially support (on a modest scale, maybe several K/year), any organization that produced refereed articles in popular technical publications that covered either results of actual ethics evaluations studies or methodologies. I don't think I will need to make any contributions in the near future.

I highly recommend reading/studying Dr. Kurtz's book.

Book Review

Pitfalls in Human Research: Ten Pivotal Points

by Theodore X. Barber

1976, Pergamon Press, 117 pp.

Reviewed by Jim Lippard

I came across this short volume while writing a paper on the subject of fraud and error in science. While this book was written primarily to aid experimental psychologists, it is definitely useful for those with an interest in the paranormal. Barber looks at ten different pitfalls in human behavioral research: investigator paradigm effect, investigator experimental design effect, investigator loose procedure effect, investigator data analysis effect, investigator fudging effect, experimenter personal attributes effect, experimenter failure to follow the procedure effect, experimenter misrecording effect, experimenter fudging effect, and experimenter

unintentional expectancy effect. The first noteworthy point here is Barber's making a distinction between the investigator, who designs the experiment and analyzes its results, and the experimenter, who carries it out. Different types of pitfalls can arise during the performance of these two different functions.

The investigator paradigm effect is when an investigator sees nonexistent events or effects in the data (e.g., N-rays) or fails to recognize events or patterns in data, due to the investigator's accepted paradigm or set of general beliefs about the area of study.

The investigator experimental design effect is when an investigator fails to take account of certain factors in experimental design, such as accounting for sex differences, overly complex experiments, incorrect choice of measurement scales, and so on.

The investigator loose procedure effect is when the experimental protocol is imprecisely specified, which can lead to failure to replicate experimental results.

The investigator data analysis effect occurs when an investigator misapplies statistical methods, fails to report data that do not support conclusions, or engages in unreasonable post-hoc analysis.

The investigator fudging effect is the altering or fabrication of data, either outright or unconsciously. In Barber's discussion of this pitfall, he specifically discusses parapsychology.

The experimenter personal attributes effect is that when the same experiment is carried out by experimenters who are different in race, sex, age, prestige, friendliness, and so on, experiments may produce different results.

The experimenter failure to follow the procedure effect occurs when the experimenter fails to precisely follow the investigator's standardized procedures.

The experimenter misrecording effect is when the experimenter unconsciously misrecords subjects' responses, which is usually done in the direction of the desired conclusions.

The experimenter fudging effect is fabrication or alteration of data by the

experimenter (similar to the investigator fudging effect).

Finally, the experimenter unintentional expectancy effect is the effect popularized by Robert Rosenthal (see "Rosenthal Lecture" in this newsletter). This is perhaps the most interesting chapter of the book, because Barber thinks that Rosenthal has overstated his case and that most effects attributed to this pitfall are really the result of investigator data analysis effect, experimenter failure to follow the procedure effect, experimenter misrecording effect, or experimenter fudging effect. He reports on numerous studies which have failed to replicate Rosenthal's findings.

Each section of Barber's book gives examples of the pitfall under discussion, usually from the psychological literature, as well as recommendations for avoiding the pitfalls. The book concludes with some general recommendations for conducting behavioral research.

Book Review

They Call It Hypnosis by Robert A. Baker
1990, Prometheus Books, 313pp.

Reviewed by Jim Lippard

Robert Baker has written an entertaining and useful book for those interested in the facts about hypnosis. While he argues for a particular interpretation of hypnosis (the social-psychological interpretation favored by researchers such as Spanos, Barber, and others), he also presents numerous other interpretations which have been offered. On the question of whether hypnosis is a special state of consciousness or not, Baker comes down firmly (and rightly, in my opinion) on the side of the non-state theorists.

This is a position which contradicts popular culture's view of hypnosis, which is how Baker begins his book. He gives examples from literature and the mass media of what hypnosis is, and then shows how and why they are mistaken. Baker's book then gives a history of the concept of hypnosis and a summary of contemporary views. An entire chapter is devoted to hypnosis and pain.

The book deals with nearly every major issue in hypnotic behavior, though there were a number of subjects which I thought could have been dealt with in more detail. For example, Baker maintains that "hypnotized" individuals will not do anything they would not ordinarily do. To explain such cases as experiments in which subjects threw acid at the face of an experimenter (who was actually shielded by glass), Baker maintains that in such cases the subject knows that the experimenter is taking responsibility for his behavior and assumes that nothing will really go wrong (pp. 49, 154). This explanation, however, doesn't work for cases such as two legal cases from Germany described in Leo Katz's book *Bad Acts and Guilty Minds* (1987, University of Chicago Press, pp. 128-133). Katz describes cases where unethical hypnotists induced patients to give them large sums of money, commit crimes, and attempt murder and suicide. It is perhaps because of cases like this that the Model Penal Code (MPC) lists "conduct during hypnosis or resulting from hypnotic suggestion" as behaviors which are "not voluntary acts." When I asked Baker about these cases, he found the MPC definition unreasonable and stated that if the descriptions in Katz's book were correct, the people were effectively using the hypnosis as an excuse for behavior in which they would have engaged anyway. (It is worth noting that the alleged hypnosis-induced murder attempts were stopped by the subject at the last minute rather than failing for chance reasons, and that Katz himself (p. 133) warns that the accounts are questionable for the same reason Baker gave me.) This explanation, however, does not seem to be subject to scientific examination.

Another story that appears to lend credence to the idea that hypnosis can result in loss of voluntary control is found in Richard Feynman's autobiography, *Surely You're Joking, Mr. Feynman* (1985, W.W. Norton, pp. 53-55). Feynman describes volunteering to be hypnotized by a stage hypnotist while a graduate student at Princeton. He

mentions doing things he "couldn't normally do" (a statement Baker does a good job of falsifying) and being given the suggestion to walk all the way around the room rather than returning to his seat directly. Feynman decided to try to resist the suggestion, without success. The social-psychological interpretation would state that this is simply due to social pressures, not to any magical power of hypnosis.

Baker maintains in his book that there are no differences in the EEGs of hypnotized versus non-hypnotized individuals, however David Spiegel of Stanford University, a hypnosis researcher, maintains otherwise (e.g., Spiegel, Cutcomb, Ren, and Pribram 1985). (Nicholas Spanos, in his author's response to commentary by Spiegel citing this research (1986, p. 492), argues that Spiegel has misinterpreted his data given the nature of the control subjects used.) It would have been nice to at least have seen some acknowledgment of disagreement on this subject, but Spiegel is not even mentioned.

Another peculiarity of Baker's position on hypnosis was pointed out by Stanford hypnosis researcher Ernest Hilgard at the session on hypnosis at the 1991 CSICOP conference in Berkeley, California. Hilgard noted that Baker rejects the usefulness of hypnotic susceptibility scales ("To my dismay I soon discovered this sort of screening was of no value ... Neither I nor my fellow researchers found the tests to be discriminatory. ... we found that nearly all of our subjects scored almost exactly alike—near the top—on the Stanford Hypnotic Susceptibility Scales, forms A, B, and C," p. 35). Hilgard stated that Baker's position is contrary to that of not only state theorists, but to non-state theorists like Nicholas Spanos. (Indeed, Spanos presented data at the same conference which made use of differential results on hypnotic susceptibility scales.)

Finally, the book is somewhat marred by a large number of typographical errors which should have been caught by an editor. These include not only misspellings (like "Hilgard" for

"Hilgard" on p. 107), but disagreement in number between verb and subject ("Barber's own personal experiences with pain has led him to be able to control it," p. 100) and other mistakes. I must say, however, that despite these minor flaws, this remains one of the best books on the subject of hypnosis I have read. I recommend it highly.

References

- Spanos, N. (1986) "Hypnotic Behavior: A Social-Psychological Interpretation of Amnesia, Analgesia, and 'Trance Logic'." *Behavioral and Brain Sciences* 9:449-502.
- Spiegel, D., Cutcomb, S., Ren, C., and Pribram, K. (1985) "Hypnotic Hallucination Alters Evoked Potentials," *Journal of Abnormal Psychology* 94:249-255.

Editor's Column

By Jim Lippard

With this issue, the editorship of *The Arizona Skeptic* moves from Phoenix to Tucson, at least on a provisional basis. I hope to return to at least a regular quarterly publishing schedule if not a bimonthly one. I presently have enough material for another entire newsletter (and more if I reprint articles from other groups' newsletters), but more material is always needed. Submissions may still be sent to the Phoenix Skeptics address; they may also be sent to me more directly in care of the Department of Philosophy, University of Arizona, Tucson, AZ 85721.

A word about the volume and issue numbers. The first issue of *The Phoenix Skeptics News* was the July/August 1987 issue. A regular bimonthly publishing schedule was kept up for ten issues, ending with the January/February 1989 issue (vol. 2, no. 4). The next two issues, January and February/March 1990, carried no volume number but should be considered issues 1 and 2 of volume 3. The most recent two issues, July 1990 and December 1990/January 1991, make up volume 4. And so this issue, coming four years after the first, is now the first issue of volume 5.

CORRECTION: In the article "Dissension in the Ranks of the Institute for Creation Research" in the February/March 1990 issue of *The Arizona Skeptic*, I stated that the myth of live freshwater clams radiocarbon dated in excess of 1600 years is put forth in Duane Gish's booklet, *Have You Been Brainwashed?*. I based this claim on my reading of Hugh Young's article, "The Case of the Living Fossil," which appeared on pp. 10-11 of the March 1988 issue of the *New Zealand Skeptic*. In that article, Young attributes the claim to an unnamed booklet by Gish which I have not found. The claim does not appear in *Have You Been Brainwashed?*, though it does appear on p. 162 of Henry Morris'

Scientific Creationism. (Morris does not misrepresent the facts as does the Jack Chick tract *Big Daddy?*, but he does draw unwarranted conclusions from them.) I regret the error, and thank Allan Lang of the Australian Skeptics for discovering it.

Upcoming Meetings

The Phoenix Skeptics will meet at the Jerry's Restaurant on Rural/Scottsdale Road between McKellips and the river bottom, with lunch at 12:30, on September 7, October 5, November 2, and December 7 (at which predictions for 1992 will be made). Meetings are on the first Saturday of each month except where it conflicts with a holiday.

The *Arizona Skeptic* is the official publication of the Phoenix Skeptics and the Tucson Skeptical Society (TUSKS). The Phoenix Skeptics is a non-profit scientific and educational organization with the following goals: 1. to subject claims of the paranormal, occult, and fringe sciences to the test of science, logic, and common sense; 2. to act as clearinghouse for factual and scientific information about the paranormal; and 3. to promote critical thinking and the scientific method. The contents of *The Arizona Skeptic* are copyright © 1991 by the Phoenix Skeptics unless otherwise noted. Reprinting of material in this publication with Phoenix Skeptics copyright may be reprinted provided that *The Arizona Skeptic* and the author are provided copies of the publication in which their work is reprinted. Address all correspondence to the Phoenix Skeptics, P.O. Box 62792, Phoenix, AZ 85082-2792. All manuscripts become the property of the Phoenix Skeptics, which retains the right to edit them. Subscription rate is \$12.50 per year.

Our Speaker: Gary Mechler
Topic: Why People Believe In the Paranor
Sept. 7th at 12:30 pm at the Jerry's in
Tempe, AZ

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