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The Journal of Sociology & Social Welfare

Volume 10
Issue 4 November

Article 8

November 1983

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Recommended Citation

Haggstrom, Warren C. (1983) "A Note on Voethogenic Harm: The Politics of Science and the Professions," *The Journal of Sociology & Social Welfare*: Vol. 10 : Iss. 4 , Article 8.

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**A NOTE ON VOETHOGENIC HARM: THE POLITICS OF
SCIENCE AND THE PROFESSIONS**

by

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Abstract: "voethogenic": helper-engendered.

If one wants to check out how good a new dog is in herding cattle, one has to find some cattle, send the dog after them, and observe what happens. That works because what the dog does and its outcome are so striking as to rule out all but one rough interpretation concerning what has given rise to what.

It is different with the helping of people.

If a physician prescribes a medication for several patients and they all get better, the getting better may have little or nothing to do with the medication since most illnesses get better (or worse) over time regardless of the medication. In other words, there is an alternative explanation to that of the effectiveness of the medication: the interpretation that the disease is self-limiting. The most certain way to tell whether the medication was effective would be if there were two identical groups of sick people only one of which received the medication. If, after a period of time, it were to turn out that the members of the group receiving the medication were more nearly free of the illness than members of the other groups, the physician could then reasonably conclude that the medication was effective.

The situation becomes even more difficult when one evaluates the talk-therapies (counseling, psychoanalysis, dynamic psychiatry, the work of clinical psychologists, caseworkers, marriage counselors, etc.). The problem of evaluating a medication continues since many psychological problems also get better or worse over time regardless of therapy. Thus, when the therapist feels that she or he has been helpful, there is another reasonable interpretation: that the person in therapy would have been just as well off without having had it. The talk-therapist, therefore, almost always needs the experimental-control-random-assignment-group approach in order to determine his or her effectiveness.

However, evaluation of the talk-therapies encounters a further obstacle that is uncommon among physicians. The obstacle is that it is very difficult to estimate which of the identical groups is better or worse off after a period of being helped.*

For one thing, anxiety may have declined in the group being helped--but a realistic analysis of the situations of those helped may reveal that they should have remained as anxious as they initially were. A person in great danger is not helped by becoming complacent. Further, the problems of group members being helped may simply have been displaced by different problems. If a therapist helps clients to become less anxious and the anxiety is succeeded by depression, it is difficult to know whether the helping has left those helped better off or worse off. Finally, judgements concerning the problems of people who go to talk therapists are notoriously of doubtful validity.

Consider, for example, the study reported by Robert J. Stoller and Robert H. Geertsma ("The Consistency of Psychiatrists' Clinical Judgements," The Journal of Nervous and Mental Disease, Vol. 137, #1, July, 1963, pp. 58-66.)

This study had 27 psychiatrists watch a half hour film of a third year medical student talking with a patient. They were all psychiatric faculty in the UCLA School of Medicine and were trying to create a criterion for use in examinations within the School. The film was such that they could see the patient from the perspective of the interviewer.

In rating 565 statements concerning the patient on a scale of 0 to 5, their inter-rater agreement (correlation coefficient) was only 0.37. The authors summed up the results as follows:

*However, physicians are not completely without difficulties in this area. See e.g., Phillip L. Rossman, "Organic Diseases Simulating Functional Disorder," in GP (Published by the American Academy of General Practice), Vol. 28, #2, August, 1963, pp. 78-83. Dr. Rossman, (of Los Angeles), reported on 115 patients who had symptoms common to both organic and psychiatric illnesses and who were referred for psychiatric treatment although it later turned out that their diseases were organic.

"psychiatrist-experts were unable to agree as to a patient's diagnosis, prognosis, psychodynamics, causes of her problem, the feeling she was consciously experiencing, or the feelings that were latent (unconscious)." (P. 64)

A number of other studies have produced results consistent with this outcome.

For example, someone interested may examine Chapman, L. and Chapman, J., "The Genesis of Popular but Erroneous Psychodiagnostic Observations," Journal of Abnormal Psychology, 1967, 72, 193-204. Perhaps the classical discussion of how this comes about was published in 1949 by Gustav Leheiser ("Misunderstanding in Human Relations: A Study in False Social Perception", The Journal of Sociology, Vol. LV, #2, September, 1949; Part 2, pp. 1-70.) See, especially, page 25 and 46-47. A more recent summary of research concerning this criterion problem can be found in Michael J. Lambert, et. al., "Therapist-Induced Deterioration in Psychotherapy," pp. 452-481 in Alan S. Gurman and Andrew M. Razun, Effective Psychotherapy, N.Y.: Pergamon, 1977. In a section entitled "The Criterion Problem," the authors generalize as follows:

"Except for disorders of organic etiology and the distinction between psychosis and neurosis, the standard diagnostic classifications have provided low reliability." (P. 475)

If clinicians cannot agree on all these things, then a great proportion of their judgements must be invalid. How, then, can we use the judgement of clinicians or of clinically related researchers as to the status of those having been subjected to helping efforts by talk-therapists? This problem has rendered doubtful the interpretations of most outcomes of the hundreds of effectiveness studies of talk-therapies.

What, then, is left? Is there an outcome criterion which is valid? There remains, most convincingly, the criterion of relative mortality. If the helping helps people to live longer, then we can conclude (at least for most people, but not counting such exceptions as George Orwell and George Bernard Shaw) that the help has been effective.

It is for that reason that I have selected in evaluating talk therapies only and all random assignment-experimental-control studies in which the outcome criterion is mortality. Because very little competent medical research concerning effectiveness falls

outside mortality studies, except in such areas as the introduction of drugs, I have also used mortality as a criterion in evaluating medical practice. There are very few such studies. The following are representative of those few.

First Effectiveness Studies

Beginning in October, 1966, and ending in October, 1969, a group of English physicians conducted a random-assignment-experimental-control-experimentation of the outcomes of homes as compared with hospital intensive care treatment for a group of men under 70 years of age who experienced acute myocardial infarction (heart attacks). See H. G. Mather, et al., "Acute Myocardial Infarction: Home and Hospital Treatment," British Medical Journal, August 7, 1971, pp. 334-338).

During those three years, 343 cases were allocated at random either to (a) home care with a family, or (b) hospital treatment initially in an intensive care unit. The patients given hospital treatment were usually assigned to intensive care units for a minimum of 48 hours and otherwise cared for in the adjacent medical ward.

Of 169 patients randomly assigned to hospital care, 12 (7.1%) died within seven days. Of 174 patients randomly assigned to home care, 5 (2.9%) died within seven days. During the time between eight and twenty-eight days after treatment began there were no differences between the two groups in number of deaths.

These figures probably substantially underestimate the advantage of home care since an unknown number of patients who transferred during the experiment from home to hospital care were counted as having received home care when the data were analyzed. Such a category decision presumably related to an assumption by the researchers that patients were transferred from home to hospital care because they had become disproportionately ill at home.

I have discovered only one empirical analysis which sheds light on whether that plausible assumption is, in fact, correct. In a similar experimental-control-group-random-assignment study comparing (a) only medical treatment with (b) coronary bypass surgery, Murphy, et al. found no survival advantage to the surgery. (See Marvin L. Murphy, et al., "Treatment of Chronic Stable Angina", New England Journal of Medicine, Vol. 297, #12, September 23, 1977, pp. 621-627.) In the Murphy, et al. study, as in the Mather, et al., one, there were cross-overs from medical treatment to the experimental one and initial assumption of the exceptional

illness of those who crossed-over. The authors, however, when they examined the cross-overs, were unable to discover that they differed from those who remained in the medical treatment group. (P 625) If that were also to hold for the Mather, et al., study, those who crossed-over to hospital care should not have been classified in data analysis as belonging to the home care category. (The cross-over group resembled the hospital group, not the home care group, in mortality rate.) If we take that Mather, et al., mistake into account, it becomes clear that, in the Mather, et al., study, a patient randomly assigned to hospital care was substantially more than twice as likely to die within one week than would have been the case had he stayed at home.*

The Mather, et al., study, gave rise to a vigorous discussion among English physicians of the relative merits of home versus hospital care. That study had been attacked as unethical at its inception and was not generally accepted by "the medical community" after being published. The most common criticism was that only a little over 28 percent of the population of the study was randomized. In the remaining cases, it was not possible for both physician and patient to agree to the randomized treatment.

A follow-up report was published by Mather, et al., in 1976. (Mather, H.G., et al., "Myocardial Infarction: A Comparison between Home and Hospital Care for Patients" British Medical Journal, 1976, 1, 925-929). This paper added that after 300 days 20 percent of those receiving home treatment and 27% of those receiving hospital treatment had died.

In order to get further data, David Hill and associates received a Department of Health and Social Security support grant to make a randomized study of home and hospital care. This effort began in 1973. The outcome was published in Lancet in 1978 (Hill, J.D., et al., "A Randomized trial of home-versus-hospital

*In 1971, Reuel A. Stallones reported that he and Robert Buechley "in analyzing mortality statistics from the states of the United States, found a strong positive correlation between the physician-population ratio and coronary disease." (Environment, Ecology, and Epidemiology, Pan American Health Organization Scientific Publication, No. 231, p. 9). Of course, no correlation demonstrates causation, but it may eventually turn out that part of such a relationship results from the greater prevalence of hospital care where physicians are concentrated.

management for patients with suspected myocardial infarctions", Lancet, 1, 837). In this study, 76 percent of the patients were randomized into the study, thus warding off in it the most common criticism of the Mather, et al., study.

Hill and his colleagues reported that, after six weeks, the morality from home care was 20%, that from hospital care, 18%. (I have not yet re-analyzed these data and do not know what the outcome of such a re-analysis would be.)

There have been no other home-versus-hospital studies done relating mortality to myocardial infarction which meet my criteria.

One indication of the orientation of the medical profession in England was the recommendation in 1975 that the number of coronary care units in hospitals be rapidly increased. (See Report of Joint Committee of British Cardiological Society and Royal College of Physicians, "The Care of the Patient with Coronary Heart Disease," Journal of the Royal College of Physicians, 1975, 10, 5).

The nature of the controversy in England may be reflected in the fact that many of the older physicians tended to prefer home care. Young physicians tended not to, perhaps because of possible recriminations if patients died unexpectedly. (See Aubrey Colling, Coronary Care in the Community, London: Croom Helm, 1977, p. 173).

Although the outcomes of these studies were available to physicians throughout the world, they appear to have had no other impact on publication, practice, or research. In the Soviet Union, organization to ensure early hospitalization was the ideal and, in the single recent book which has been translated into English, there are no references to home care or to the Mather, et al., and Hill, et al., studies in spite of a huge bibliography. (See E. I. Chazov, Ed., Myocardial Infarction: The Approach to Prevention, Diagnosis and Treatment in the Soviet Union, Littleton, Mass.: PSG, 1979).

The situation was similar in North America. For example, in a standard text, Emanuel Goldberger wrote, "All patients with an acute myocardial infarction or suspected myocardial infarction should be admitted to a coronary care unit (CCU)." (Treatment of Cardiac Emergencies, St. Louis: C.W. Mosby, 1977, 2nd Edition). There is also no mention of home care in the many other similar recent books which I have consulted. A recent bibliography on home care supported by the United States Department of Health,

Education and Welfare (The Franklin Research Center: Home Health Care Programs: A Selected Bibliography. Hyattsville, MD, U.S. Department of HEW, 1979) contains no references to heart attacks or myocardial infarctions--or to the English evaluation studies.

However, a group sponsored by the World Health Organization (A Working Group for the World Health Organization: Coronary Care Outside Big Centers, Copenhagen, Regional Office for Europe, 1975), has a short section entitled "Care of the patient at home" (p. 8) which considers the possible utility of home care, especially for people distant from hospitals.

One could anticipate the reactions to the Mather-Hill studies by extrapolating from current practice. In England, where there is substantial home care, the outcomes reported are nonjudgmental. Mather, et al., did not make note of differences within the first week after the attack, but drew conclusions based on a later time when the differences were less striking. Where there is no legitimated practice of home care (as in the Soviet Union and the United States) the evaluation research has been mostly ignored.

The criticisms by cardiologists of the Mather-Hill studies tend to be ignorant of the nature and uses of evaluative research. Some go further to mis-report data.

That these failures of solid research to be utilized are not unprecedented can be illustrated by reference to a famous occurrence from the history of medicine.

Second Effectiveness Study

In the mid-Nineteenth Century, the lying-in department of the General Hospital in Vienna was one of the greatest of its kind in Europe. It had been divided into two separate clinics in 1833. When women in labor came in to deliver their babies, if they came at one time of the week they were assigned to the first clinic, at another time they went to the second clinic. Thus, unintentionally, women were randomly assigned to one or another of the two clinics.

Beginning in 1840, the first clinic was made available for the instruction of medical students only; the second clinic was available to midwife students only. Thus, different treatments had been assigned for each of the two roughly identical groups of women.

With this arrangement, the mortality rate of the first clinic suddenly rose to more than double that of the second clinic and remained far higher in subsequent years. Women became aware of this difference and many sought, unsuccessfully, to be admitted, against the rules, to the second clinic.

The deaths were from puerperal fever (childbed fever), a disease which, in 1846, resulted in the deaths of 11.44% of the women in the first clinic, but only 2.7% of the women in the second clinic.

Ignac Semmelweis*, a physician and surgeon who was in charge of the first clinic, realized that the difference in mortality rates had something to do with the differences between the clinics and tried to figure out what was the cause.

Semmelweis noted that the doctors, some assistants, and some medical students in the first clinic dissected the cadavers of women who had died and then examined the patients who remained alive. This sequence of events did not occur in the second clinic. He concluded in 1847 that the difference between the two clinics in mortality rates was caused by those who first dissected cadavers and then conducted manual examinations of the women in labor.

Semmelweis, therefore, put into effect a rule that all who examined women would first thoroughly clean their hands in a chlorine wash. The death rate in the first clinic promptly dropped to approximately that in the second. Through analysis of statistical trends, clinical and anatomical, and even animal, studies, Semmelweis provided adequate evidence for his causal hypothesis and for the effectiveness of his treatment.

He thereby had become the first person to demonstrate the efficacy of antisepsis in medicine, a discovery which governs surgical practice to this day.

*Semmelweis had taken his medical training in what was then the leading medical school in the world, in Vienna. Among many sources, the reader can consult Frank Slaughter's Immortal Magyar: Semmelweis, Conqueror of Childbed Fever, New York: Schuman, 1950; or Gy. Gartvay and I. Zoltan, Semmelweis: His Life and Work, Budapest: Akademiai Kiado, 1968.

However, his work was either ignored or attacked. By 1859, more than a decade after his discovery had been demonstrated, his reports were largely forgotten or had excited very little interest in Europe and the United States, all places in which childbed fever was frequently epidemic. He could not even get his writings published in most of the important medical journals of his time. With few exceptions, all the great obstetrical authorities had rejected his ideas. When he published his major opus on the subject in October, 1861, the resulting reviews were mostly hostile. About three decades after Semmelweis's discovery, Pasteur announced the identity of the bacteria which caused puerperal fever. Only following that announcement, with Semmelweis dead and his work mostly unknown, dismissed, and distorted, did his method of averting childbed fever gradually win general acceptance. (He had also used antiseptics in surgery before its "discovery" by Lister.)

Semmelweis had suffered greatly from the rejection of his brilliant, tenacious, ingenious, and crucially important research. He died in 1865, mad.

These illustrations of the reception of effectiveness research are not generalizable only within the profession of medicine. Our next illustration is from a different helping profession.

Third Effectiveness Study

With the cooperation of thirteen selected social and health agencies, 164 older people in need of protective services were randomly assigned to receive, or not to receive, casework assistance during the year June, 1964, through May, 1965, at the Benjamin Rose Institute in Cleveland. The project director for this effectiveness study was Dr. Margaret Blenkner, professor and director of the Regional Institute of Social Welfare Research, the University of Georgia School of Social Work. Dr. Blenkner was nationally recognized in this area of research, having published eighteen studies prior to the final report on this one.

Four caseworkers provided help to those in the experimental group. Their average caseload was about 19 clients each. "All held master's degrees in social work and one had an additional third year of graduate social work training: all had more than fifteen years of experience." (See Blenkner, M.; Bloom, M. and Nielsen, M., "A Research and Demonstration Project of Protective Service", Social Casework, Vol. 52, Oct., 1971, pp. 483-499.

The research team followed up the clients in the two groups year by year for four years after their original registration in the project. They accumulated data on comparative survival.

At the end of the first year, they found that, in the experimental group, 19 out of 76 clients had died (25%). In the control group, 16 out of 88 had died (18%). Thus, those who received intensive casework help were much more likely to have died than those who did not receive such help. Analysis of the data demonstrated that caseworkers had tended to institutionalize their clients and that the difference in institutionalization accounted for the entire difference.

By the end of the fourth year, however, the picture had changed significantly. By then, 48 out of 76 in the experimental group had died (63%) and 46 of 88 in the control group (52%). In other words, the clients in the experimental group had continued to die at a greater rate than had the clients who had received no special intensive help. However, the difference at the end of the fourth year was maintained entirely by noninstitutionalized clients. Institutionalization made no difference at the end of that length of time. (The differences in absolute numbers reflect the fact that 43 clients had crossed over into institutions during the final three years of the analysis.) (For these and other data, see Margaret Blenkner, Martin Bloom, Margaret Nielson, and Ruth Weber, Final Report: Protective Services for Older People, Cleveland: The Benjamin Rose Institute, 1974.)

Although there had been several previous studies relating help to mortality in casework and allied professions, all consistent with it in outcome, the Blenkner, et al., study was followed by no further such research. There was no subsequent publicized discussion except for that aroused by later publications by Joel Fischer, and I have no evidence that this and previous similar studies affected casework practice.*

*I anticipate that Margaret Blenkner, who died in 1973, will become the greatest heroine of social workers since Jane Addams. There are no candidate heroes of social workers.

Discussion

The outcomes of these three studies reflect the limited population of effectiveness research in the helping professions which meets my criteria. No study demonstrated that helping increases life span, although a few allowed for the possibility that life span was unaffected.

The lack of utilization of the research was also typical. the studies have tended to be ignored and distorted.

A striking illustration of the latter can be found in Katherine M. Wood's comforting report, "Casework Effectiveness: a new look at the research evidence" (Social Work, Vol. 23, #6, November, 1978, pp. 437-458). Wood entirely ignored the Blenkner, et al., analysis of noninstitutionalized clients, although it had been prominently published in two places and reported in publications by Joel Fischer, all sources of which Wood was cognizant. Fischer is the only person to have published a fair (although-superficial) account of the Blenkner et al., research. Typically, Fischer's account led to an acrimonious, but unenlightened, set of reactions from social workers. Wood's distorted discussion in Social Work was followed by a single published letter applauding her "definitive" article and maintaining that "we have since moved past the question of whether or not direct social work is effective. The new and urgent question is how we can make this practice more effective." And, "It is a wonderful thing to be a social worker in this age..." (Social Work, September, 1979, p. 441.)

Given the reception of these studies, we may inquire next whether these effectiveness studies are significant enough to warrant attention. Let us consider them one by one.

The figures, of course, are small, and may not be widely generalizable. However, the significance results from the possibility that they are.

If we extrapolate from the Mather, et al., study to the United States, a change to home care, using the most conservative set of assumptions, would save more than eleven thousand lives annually during the first seven days following a heart attack. The saving in cost would be enormous.

There are no data which can lead us to estimate the number of lives which could have been saved had Semmelweis found a different

reception to his work. But the number of women dying during childbed fever epidemics sometimes reached 100 percent--and this in major hospitals throughout the western world.

The Blenkner, et al., study, generalized, would lead us to the conclusion that, of every 31 clients, five would be unnecessarily dead at the end of four years. That is, each worker with such a caseload would have killed, on an average, at least one client a year. Not even the average Sicilian Mafia member manages to affect mortality that much. If there were protective service workers adequate for the population in need of it (using Blenkner's estimate), the resulting loss of life through casework would exceed 40,000 a year. Of course, we do not know the population to which the Blenkner, et al., research can be extrapolated. Conceivably it could be the entire population of casework clients--there is no evidence to the contrary.

Assuming that these studies and the responses to them are representative and that they are significant, is it possible that they have not been given attention because they are simply inexplicable and to be regarded as anomalous phenomena unworthy of further attention?

Certainly the history of the Semmelweis discovery demonstrates its explicability and the value of taking it seriously, even though Semmelweis, himself, had been unable to demonstrate the underlying mechanisms of infection. Let us, therefore, next consider Mather, et al., and Blenkner, et al.

There has been substantial research supporting the conclusion that relocation of ill elderly people increases their mortality soon after relocation. (See, e.g., I. Wittels and J. Bilwinick, "Survival in Relocation," Journal of Gerontology, 29, 440-443, 1974). This research, contrary to the Blenkner, et al. opinion (but consistent with their data) demonstrate that it is not being in institutions, but becoming institutionalized, which results in higher mortality rates. Further, the patients taken to hospitals in the Mather, et al., study, were both ill and being relocated, and it was the older patients who disproportionately died in hospitals compared to homes.

Second, the studies are similar in that heart attack was the only cause of death in the one and allegedly the main cause of death (according to death certificates) in the other.*

Further, deaths in the Blenkner, et al., study occurred primarily in hospitals (47%) or other institutions (38%) in which iatrogenic illness is a considerable possibility. Let me briefly elaborate.

In 1981, Steel, et al., published the latest in a long series of studies of iatrogenic illness which have been conducted in many places by many research groups. (See "Iatrogenic Illness on a General Medical Service at a University Hospital," New England Journal of Medicine, March 12, 1981, 304: 638-642.) In this study, 36 percent of 815 consecutive hospital patients on a general medical service were conservatively estimated to have an iatrogenic (hospital-acquired) illness. In two percent of the 815 patients, the iatrogenic illness was believed to contribute to the death of the patient. That great an incidence of iatrogenic-related death would wipe out about half of the mortality difference in the Mather, et al., study (although one should regard such comparisons with caution.) However, it would account for less of the difference observed in the Blenkner, et al., study even if caseworker intervention made it more likely that clients would go to such places (as seems evident from the data).

Further, it is likely that the Steel, et al., study underreported iatrogenic deaths since it apparently did not take into account the possibility of psychogenic deaths. For example, Jarvinen ("Can Ward Rounds Be a Danger to Patients with Myocardial Infarction?", British Medical Journal, February 5, 1955, pp. 318-320) reported five people who died during ward rounds in a short time at a Helsinki University Hospital and an additional patient who died shortly after an attack which began during ward rounds. If deaths had occurred at an expected rate, given the time spent

*One must, however, regard such death certificate figures as very uncertain in view of diagnostic problems. For example, Marizama, Danber and Kennel (in National Cancer Institute Monographs, 19: 405, 1966) looked at 1,362 death certificates coded as cardiovascular. They found that in only 12% of cases was the diagnosis "well-established," in an additional 40% was the diagnosis "reasonable," and in 48% of the cases the diagnosis was "probably incorrect" or "better replaced by another diagnosis." The debate continues in the medical journals.

on ward rounds, only one, or at most two, deaths would have occurred then. ("Two deaths occurred when the patients were told that the time for their discharge, so eagerly awaited, had come.")

A recent study indicates that even nonaffective speech, very likely disproportionately to occur in the experimental groups of the Mather-Blenkner-et al., studies, might increase the possibility of death from heart attacks.

James J. Lynch, et al., ("The Effects of Talking on the Blood Pressure of Hypertensive Individuals," Psychomatic Medicine, Vol.43, #1, February, 1981, pp. 24-33) studied the blood pressure of 30 hypertensive and 15 normotensive people before, during, and after they talked for two minutes in response to the suggestion, "tell me about your work." All subjects showed increased blood pressure while talking. "The patients with the higher pressure tended to have the greater increase in blood pressure during talking." One patient went "from a pressure of 157/86 while sitting quiet up to 200/120 while speaking and immediately down to 174/88 in the first resting quiet minute after talking."

It is certainly likely that more intensive help is accompanied by more talk.

Possible explanations could be multiplied. For example, it may be that, in the two studies, experimental treatment resulted in an imposition of standards and norms on those being helped which were alien and degrading to them, more so than in instances of less intensive help.

But one mystery remains. How is it possible that the Blenkner, et al., clients began to succumb so greatly in the experimental group years after the experiment was over? Is that not inconceivable? Not at all. We can suppose that various subsequent helpers became aware of whether those helped had or had not been in the experimental group and differentially treated them (possibly with more intensively mobilized help for those who had been members of the experimental group) on that basis.

Although these are only possible explanations for the outcomes of the effectiveness studies, they do illustrate the fact that the study outcomes cannot be considered worth rejection or being ignored as beyond comprehension.

If the studies are representative, significant and comprehensible, how then can we explain the contrast between the studies and their utilization?

What May be Going On

A long time period between a discovery or invention and its adoption is not unknown. The cotton picker was invented in 1889, yet it was fifty-nine years before it became an innovation in 1948, long after the death of the inventor and during a time of labor shortage. (Keith Norris and John Vaizey, The Economics of Research and Technology, London: Geo. Allen & Unwin, 1973, p. 77). In the field of geology, the continental drift theory was proposed more than fifty years ago and generally accepted just recently. (See Seymour H. Mauskopf, Ed., The Reception of Unconventional Science, Washington, D.C.: 1979.) In this instance, acceptance awaited an adequate explanation for the phenomenon (as was also the case with Semmelweis).

The literature does not account in any detail for variations in acceptance, although numerous cases of resistance to discovery have been published. (Anthony M. Graziano, "Clinical Innovation and the Mental Health Power Structure: A Social Case History," American Psychologist, Vol. 24, #1, January, 1969, pp. 10-18; Genevieve Miller, The Adoption of Innoculation for Smallpox in England and France, Philadelphia: University of Pennsylvania Press, 1957; Bernard Barber, "Resistance to Scientific Discovery," Science, Vol. 134, No. 3479, September, 1961, pp. 596-602; A.J. Dessler, "Swedish Iconoclast Recognized After Many Years of Rejection and Obscurity," Science, Vol. 170, #3958, 6 November, 1970, pp. 604-606); Ludwik Fleck, Genesis and Development of a Scientific Fact, Chicago: University of Chicago Press, 1979; Thomas S. Kuhn, The Structure of Scientific Revolutions, 2nd Edition, Chicago, University of Chicago Press, 1970; Levine, Why Innovation Fails, Albany, New York: State University of New York Press, 1980; and innumerable accounts of the resistance to technological innovation in traditional societies.)

My impression, based on examining the huge relevant literature, is that its main thrust concerning our question can be indicated by observations of three famous people and one who continues not to be well known:

1. Walter Bagehot: "One of the greatest pains to human nature is the pain of a new idea."
2. Niccolo Machiavelli: "The innovator makes enemies of all those who prospered under the old order, and only lukewarm support is forthcoming from those who would prosper under the new."

3. Oliver Heaviside (when his important contributions to mathematical physics had been ignored for 25 years):
"Even men who are not Cambridge mathematicians deserve justice."
4. T.H. Huxley: "Authorities, 'disciples,' and 'schools' are the curse of science and do more to interfere with the work of the scientific spirit than all its enemies."

To generalize, a discovery is more likely to be accepted if it does not violate basic assumptions and/or interests of those who become acquainted with it, if they perceive there to be an advantage for them from accepting or adopting it, and if it is explainable to those who become acquainted with it.

The effectiveness studies in the population meeting my criteria can be regarded, therefore, as discoveries which are unlikely quickly to be utilized in spite of their relevance to the life spans of millions of people and to annual expenditures of hundreds of billions of dollars.

Can anything be done about this paradox?

Some Things Which Might Be Considered for Eventual Trial

I make two recommendations as follows:

1. One might be able to affect the perceptions, and thereby eventually the practices, of helpers.

It is characteristic that helpers and those who make helping policy are seldom informed by the perception of other groups relevant to the helping.

Consider Semmelweis. When he discovered that the death rate in the first clinic was several times that in the second, he need not have looked for the cause of childbed fever. He could instead, have tried to have the hospital closed down (since he knew that women giving birth in the gutters were safer than were women giving birth in his clinic.) He could have changed the hospital so that the two clinics were the same--most easily by beginning also to teach only midwives in the first clinic. This change would immediately have resolved the problem. He could have deployed midwives and midwife students throughout the neighborhoods, taking into his clinic only women with complications so grave that their chance of living would be greater in his clinic than outside it.

He apparently conceived of none of these solutions although we can suppose that the women patients wanted some such alternative and that midwives would have been receptive to it. Truly, every way of seeing is also a way of not seeing! And professions and disciplines provide ways of seeing.*

If, today, we wish not to inform the public of the implications of presently known and possible future effectiveness research concerning the helping professions, an alternative which may yet become necessary, then we might begin by providing that "outside" perspectives become vividly and interminably available to those who are important in the formation of helping policy and related research, education, and funding. For example, it may be possible to ensure that representatives of organizations of consumers of health and other helping services be included in the decision-making processes of the helping professions. Others who should be "counted in" include chairpersons of legislative committees which are central to funding helping enterprises and key members of their staffs, directors of relevant foundations, leading epidemiologists, public health professionals, public health nurses, sociologists of the helping professions, philosophers of science, historians of organized helping, and so forth.

My second and final recommendation is so complex that I will only sketch it here, leaving a systematic presentation for some future time. It is as follows:

The unfortunate fate of effectiveness research concerning the helping professions is increasingly likely as time goes on as it is a reflection of broad and accelerating tendencies within U.S. society. We should, therefore, look outside the helping professions if we wish to ensure their increasing cost-effectiveness.

*A fact abundantly illustrated, but inadequately researched. That socialization and the resulting perceptions are helped along by supporting institutional arrangements may be indicated by the fact that the UCLA Biomedical Library possesses no copy of Ivan Illich's famous critique of medicine, Medical Nemesis (New York: Pantheon, 1976) but two copies of David F. Horrobin's Medical Hubris: A Reply to Ivan Illich (Montreal: Edan, 1977).

Let me unpack this notion a little.

Especially in this century there has been the gathering momentum of large bureaucratic organizations and the consequent incorporation of people into them from early years until the end of life. As David Riesman argued in The Lonely Crowd (The Lonely Crowd: A Study of the Changing American Character, New Haven, Conn.: Yale University Press, 1950; Abridged Edition, 1965), (although with an explanation different from mine) this has resulted in a transformation of human nature in the United States. As compared to now, a higher proportion of Americans in 1900 were value-oriented: directed by fundamental assumptions of what was right, good, true, and so forth. Now, a higher proportion of Americans are situation-oriented: not directed primarily by standards arising independently of the circumstances of their adult lives.

Students in colleges and universities have spent nearly their entire lives moving from structured situation to different structured situation within a single general context, in each situation changing to meet the expectations of those around them. After graduation, they usually continue to be situation-oriented for the remainder of their lives. The structures of science, research, and higher education have likewise shifted from entrepreneurship alone or in small groups and organizations in the direction of large organizations. The general drift of social change, resulting in a concomitant shift in human nature, is also reflected in science, research, technology, higher education, and so forth. This change has also shifted the incentive system for intellectual and scientific work. Publication in intellectual and scientific sources is almost entirely for advancement in large organizations (universities included), and not an outgrowth of a search for truth or understanding. Within the helping professions, publications has also been for career advancement rather than for most cost-effective help for those in need.

As publication has gone, so has gone the rest of the lives of researchers and helpers. One needs only to display presumed brilliance to those who can affect one's career, one need not be brilliant at all. (Being brilliant would get embarrassing within organizations which tolerate only mediocrity.) People who may be described as 'fits' move up; misfits move out and down or remain where they are. And value-oriented people are always misfits.

The history of fundamental discoveries and innovations is instructive in exhibiting the kinds of person who are likely to make such contributions:

1. Persons with an understanding of reasoning (usually as it occurs in science);
2. Persons in marginal roles;
3. Persons engaged in some kind of practice; and
4. Value-oriented people

The best single introduction to that history has been provided by Joseph Ben-David ("Roles and Innovations in Medicine," The American Journal of Sociology, Vol. LXIV, #6, May 1960, pp. 557-568.)

Considerable marginality. Marginal people tend disproportionately to develop tuberculosis, schizophrenia, alcoholism, to become accident victims, to commit suicide, to die early, to be poor, but, also, marginal people disproportionately are creative, initiators of new ideas and practices, become eminent men and women of science and philosophy, and understand (without necessarily prospering from) the social world which they inhabit.

The rise of people who are situationally oriented has, in the arenas of intellect and science, resulted in increasing numbers of "gesture people"--people who intimate enough to succeed without ever having to accomplish anything worthwhile. Such people produce ideas or knowledge only when under surveillance--and the surveillance has to be omnipresent and searching and competent if they are ever to amount to much. They move from situation to situation chameleon-like. Lacking continuity of concern with any basic problem, they lack the promise of such concern and, hence, cannot accomplish things which require a long time perspective and tenacity. Despite the enormous increase in the number of scientists, researchers, academics, and members of related disciplines, professions, and authors of associated publications, there has emerged what must be described as pervasive pathological science. (See, for example, Felix Franks' Polywater, Cambridge, Mass.: The MIT Press, 1981.)

That the situation in science was not perfect even in Max Weber's time can be inferred from his Science as a Vocation, addressed to students, in which he wrote: "Do you all conscience believe that you can stand seeing mediocrity

after mediocrity, year after year, climb beyond you, without becoming embittered and without coming to grief?" (Quoted in Warren O. Haggstrom, The Scientific Community, N.Y.: Basic Books, 1965). For a contrary contented view of the state of affairs in science, one might consult "The Sociology of Science: An Episodic Memoir" by Robert K. Merton (pp. 3-141 in Merton and Jerry Gaston, Eds., The Sociology of Science in Europe, Carbondale, Ill.: Southern Illinois University Press, 1977.) Merton reports a world which is belied by the relevant data and which I have never observed, although if one remembers very selectively and from the perspective of the science establishment one may be able to write such a utopian memoir. The appearance of knowledge races on. Knowledge limps far behind. And virtually no one is engaged in trying to distinguish the two. Intellectual production has come to mean throwing a few diamonds into a great pile of paste imitations with no jeweler available subsequently to sort things out. Since large organizations are usually stable, the perspectives attached to them also tend to remain stable. Situationally oriented people may remain in a single organization-related assumptive world for all their lives. As Felix Franks suggested in a different context, had such an approach been taken "in the very early days of technical development, we would now possess the most superior stone axes but little else." (ibid., p. 188).

Thus, situation oriented people change from context to context within stable limits--they lose both the benefit of continuity in working on a single set of problems and they also lost the benefits of being likely to acquire original ways of seeing.

Those who move up tend to be situation-oriented people who have disproportionately acquired interpersonal skills, who proclaim, but shudder at realizing, such values as creativity, since realizing them would interfere with their plausibility and safety in the minds of those above them. And these plausible people themselves end up as gatekeepers to decisions concerning resource and incentive allocation. Value-oriented people, typically implausible, are ignored or punished. The fits make good entertainers, may tell interesting anecdotes lifted from others, are skilled at the character assassination of rivals in such a way that their tactics don't bounce back on them, and are basically always conforming and conventional except, maybe, in superficial symbolic matters.

These situation-oriented successful people scorn such value-oriented questions as:

What do you mean by that?
How do you know that?
Why is that important?
What fundamentally new is to be done?

in favor of such questions as:

Where will that get you?
What's in it for me?
What can I get away with?
Why not be successful?

Within the helping professions, value-oriented people are concerned primarily effectively to meet needs; situation-oriented people are primarily self-concerned. This difference is reflected in the history of the helping professions as it has developed since the beginning of this century.

The preponderance of types today (and the view of that in the minority) may be indicated by Graham Hughes' cynical statement that: "One cannot throw off moral obligations by entering a profession," and his observations concerning people whose only talent is a talent for suffocating talent.

With this brief introduction in mind concerning what I conceive to be the problem, I will now venture some modest specific recommendations:

1. Develop a marginal discipline for a certain kind of marginal person: selection, education, and support for value-oriented people who want to work on some important problem over long periods of time and who choose to do so in relating to all available relevant knowledge from all fields, experiments, and experiences. They would be complete generalists. They would follow Norbert Wiener's advice: "If a problem leads us into a new field in which we have no knowledge, we should acquire such knowledge. It is no excuse, when working on a problem, to say 'but that's not my field'." (Reported in J.C. Barker, Scared to Death, London: Muller, 1968.)
2. Build on the fact that competent researchers and scientists increasingly circulate papers before publication. Move to the next step of not publishing until there is some reason to suppose that important published work can be separated out from the rest for the use of people who need to base their intellectual struggles on actual ideas and/or knowledge. Help "vanity publications" to emerge in which

people can pay to get their work in print. (Most publications would become vanity publications.)

3. Systematically fund efforts to sift out from what has been published that tiny portion which is important, including that which can adequately be substantiated by reasons and evidence. Reward those who originate important work, even if it remains obscure, controversial, or baffling.
4. Require that university and other teachers label what they teach as important knowledge or non-knowledge which is important--and that they be required frequently to defend their labels.
5. Drastically alter the criteria in accordance with which research is now evaluated. It is rare in the history of science that any important advance has been made by way of a single application of a methodology. The question should not be one of methodology application in one study, but one of emerging important knowledge by way of a variety of studies involving careful thinking and rigorous reasoning carried out over a long period of time within a research tradition. Thus, one should pay no attention to whether a thousand ordinary studies lean in one direction. Ordinary studies are invalid and unimportant and completely worthless except for career promotion. The recruitment of rigor should be made both within and among research studies--they are thinking and reasoning chains, they are never merely additive.
6. Thomas Kuhn, having examined the history of science, has concluded that the social sciences are not cumulative. It is similarly doubtful that the helping professions have made an overall progress for many decades. Even medicine, the most prestigious of the lot, has, for more than a decade, given rise to comments from its leaders such as that of Franz J. Ingelburger, Editor Emeritus of the New England Journal of Medicine:

"If the whole spectrum of medical care is included, ranging from a pat on the back to transplantation of the heart, it is doubtful that the benefit-harm ratio of personalized medical care has changed appreciably over the last 100 years." (Science, Vol. 200, 16 May, 1978, p. 946.)

But how can there be progress without first paying attention to what that is actually known is important and relevant? Concerning most matters, given where we are, we now have to legitimate the statement "we don't know," and reward those who frequently use it. With the resulting release from the heavy pseudo-facticity of pseudo-knowledge, the imagination will be unfettered. As people become drawn into working on important problems, one can try to design situations which they can enter and which are likely to advance their work. I suggest the creation and institutionalization of carefully designed non-competitive small discussion groups which meet at least weekly, and in which all members who honestly try hard to contribute are carefully respected. Progress, of course, will be messy, disorderly, will veer back and forth, will leap forward at some times and lag at others. But the participation of serious, disciplined value-oriented people in such surroundings will provide the best chance for important progress to emerge. This suggestion indicates the distance we have now gone from Kurt Lewin's important early study, "Group Decision and Social Change," in Theodore M. Newcomb and Eugene L. Hartley, Eds., Readings in Social Psychology, New York: Henry Holt, 1947.

7. Select for advanced education value-oriented people characterized by wonder, imagination, tenacity, intellectual courage, a willingness to look carefully at data which violate his or her assumptions, etc. Do not select on the basis of grades, scores on the Graduate Record or other examinations, reference letters which do not describe and illustrate the above mentioned personal characteristics. Advanced education, as anyone who has recently examined the works mentioned in Dissertation Abstracts can attest, should be reduced to a tiny proportion of the students now involved in it.
8. The review process in journals, by funding sources, in employing organizations, now provides little more than legitimation for fashionable prejudice. Particularly, initial plausibility unrelated to careful analysis should be eschewed.

For example, suppose someone were to propose to study and compare two groups in relation to the intelligence of their members. One group would be composed of stupid people who had recently received advanced degrees from Harvard, Yale, Princeton, Cambridge University and Oxford in England. The

other group would be composed of brilliant nonliterate people from England and the United States. I anticipate that such a study would initially appear implausible enough not to be well received by funding sources. Yet, since intelligence and formal education are not associated (although IQ and formal education are), and since formal education is associated with training people not to see, I imagine that sustained attention to the possibility of such research might well improve its chances.

9. Advertise in the public media (including television and radio) for deviant young people willing to contribute with hard work and thinking to the future of the people of our society without the prospect of getting rich and/or famous. Help promising residents into appropriate courses of action.
10. Reduce reliance on measurement and counting as major sources of decisions.

Abraham Maslow commented: "If the only tool you have is a hammer, you tend to see every problem as a nail." Operationalization has become a ready source of hammers which get in the way of adequate perception. Counting and measuring have been driving out thinking, have distracted attention from significance, and have disadvantaged millions of people who have thereby become unfortunately labelled.

11. The teaching of research in advanced programs should de-emphasize design and methodology. These are trivia which anyone can learn at any time. Instead, advanced students should be taught how to become detectives for the resolutions of lacks of understanding and of fuzzynesses and the evaluation of their significances. At present, students get tools (tractor, cultivator, fertilizer, combine, milking machine, etc.)--but they are not being taught how to farm.
12. Effectiveness research should, for the most part, involve random assignment. Such studies should be supported at far higher levels than is now done. It is true that helpers often suppose that randomization is unethical. But consider. The National Association of Social Workers published a summary of its Code of Ethics which includes the statement: "The social worker's primary responsibility is to clients." Similar norms have been proclaimed in the other helping professions. But, human suffering cannot be

reduced by helping unless we know its outcome. Research presently available gives poignancy to Thomas' statement that: "Suffering is only marginally more tolerable when inflicted with the best intentions." (The Role of Medicine: Dream, Mirage, or Nemesis?, London: The Nuffield Provincial Hospitals Trust, 1976, p. 158.) Instead of a few sporadic ad hoc studies, evaluation research should be systematic, thoughtful, continuously supported, with knowledge from outcomes fed into the helping processes.

13. We should examine certain kinds of nonevents with tenacious care: things of consequence which reasonably could be expected to happen but didn't.
14. We should take a scientific approach to the evaluation of these recommendations and assumptions related to them.

Conclusion

The term, 'iatrogenic,' came to be used in medicine by virtue of the fact that 'iatros' is the Greek word for physician and 'genesis' means origin. Thus, iatrogenic illness is taken to be physician-originated illness. (This usage has been expanded somewhat in recent years.)

In this note, we have been concerned with helper-originated harm. Since the modern Greek transliteration of 'helper' is 'voethos', I suggest that we refer to helper-originated harm as voethogenic harm (of which iatrogenic disease is one variety).

The notion of voethogenic harm has not here been spelled out, although it has been illustrated. In particular, the above examples lead to an emphasis of sins of commission, not those of omission--even though the latter should make up a substantial part of the whole. We will elsewhere explicate the range of applications of this term.*

*This note has incorporated some reflections accompanying my study of the thinking involved in helping.

This note, of course, is very preliminary, and will be extensively revised at a later time, after my work has further progressed. The last few pages are especially problematic. The note only reflects where I happened to be on one afternoon of August, 1981. For contributing to my research, I wish to express my appreciation to my wife, Raquel, who has taught me so much, and to my daughter, Marni, who has been my sole relentless critic.