and many others. Also the great strides in behavioural evaluation resulting from the work of Lorr, Wittenborn, Glueck, and Burdock cannot be overlooked. The model for a good number of basic studies, carried out in many laboratories, was set by the interdisciplinary laboratory at N.I.H. formerly headed by Seymour Kety. In this laboratory the application of basic science techniques to the study of brain behaviour relationships has reached a high level.

Perhaps more important than any single discovery has been a shift away from the breakthrough approach in the study of mental illness with the increasing maturity of the field of biological psychiatry. Frontal assaults on crucial disorders, such as schizophrenia, have been replaced by systematic and orderly basic studies of underlying biological systems. Unfortunately, the potential significance of these advances has not been widely discussed in the psychiatric press. This fault can, in large measure, be traced to the investigators themselves, some of whom do not work directly in the mental health areas. It seems to me unreasonable to expect a breakthrough from the spontaneous generation of brilliant light which will in one flash clarify the nature, the pathogenesis and the treatment of mental ills. No doubt an ultimate synthesis of many findings from various investigations will someday be made, but not until all of the essential groundwork has been completed.

ARNOLD J. FRIEDHOFF.

Department of Psychiatry and Neurology,
School of Medicine,
New York University, Medical Center,
550 First Avenue,
New York, N.Y. 10016, U.S.A.

THE LOGICAL REQUIREMENTS FOR WRITING A PAPER ON THE LOGICAL REQUIREMENTS OF RESEARCH INTO SCHIZOPHRENIA

DEAR SIR,

As one of the researchers felled by Bannister (*Journal*, February 1968, pp. 181-188), I wish to defend myself slightly and make some additional general comments. He used my report (Rubin, 1965) as an illustration of the failure to link conceptual and operational definitions ("... it is quite certain that the vast majority of studies in the biochemistry of schizophrenia do not attempt to explicate such relationships between the alleged causal agent and the omnibus abstraction of schizophrenia"). First, I did not study "schizophrenia", but acutely ill patients who were floridly psychotic on admission to the hospital, and all but one of whom manifested hallucinations, delusions, or extreme hyper-activity. Most were diagnosed as various subtypes of schizophrenic reaction, but the presence of acute psychosis, not the diagnosis of schizophrenia, was the criterion for inclusion in the study. Second, the linking of conceptual to operational definitions had been done primarily by other workers cited in the introduction of my report. Had Bannister familiarized himself with some this work published in other journals, he might have chosen to indict me for other (and perhaps more serious) methodologic crimes.

The most outstanding methodologic flaw in Bannister's paper is his restricting his literature survey to papers on schizophrenia published in the *British Journal of Psychiatry* since the beginning of 1964. Each paper cited is categorized as an example of failure to fulfil at least one of his five tenets for a valid research effort. Whether use was made of all papers appearing in the *Journal* in this time period or only of selected examples, and if the latter what percentage of the total they constitute, is not specified. At any rate, the conclusion is that "in a near literal sense a great deal of extant research in schizophrenia is half-baked".

In his tenets, Bannister carefully specifies the need for a clear definition of the population studied and inclusion of control groups to delineate population parameters. Since he surveyed no other psychiatric journal, the only logical conclusion he can offer is that a great deal of extant research in schizophrenia *published in the British Journal of Psychiatry* is half-baked. Perhaps Bannister is suggesting that the Editorial Board should use his five tenets as criteria for acceptance of manuscripts.

I would also like to comment on the wishful-thinking nature of his fifth tenet, the need for long-term research. If strictly adhered to, as Bannister suggests, all research into "schizophrenia" would be placed in the hands of the senior "grantsmen" for whom administrating long-term research is a full-time job. Those of us who remain the "littlest astronomers", whether by choice or because we are new to the game, would never be allowed near the telescope, even if only to quickly peek at the far-away stars.

ROBERT T. RUBIN.

Department of the Navy,
Navy Medical Neuropsychiatric Research Unit
San Diego,
California 95152, U.S.A.

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AVERSION THERAPY

Dear Sir,

I have read with interest the article by Marks and Gelder (*Journal*, July, 1967, p. 711). They treated five transvestites and fetishists by faradic aversion. I wonder why.

Kinsey noted that there are many magazines for men featuring nude women, none for women featuring nude men. There is a great difference between the sexes in their capacity for psychic sexual stimulation. Women are aroused by physical contact with an acceptable man, and show little interest in representations of sexual material or in looking at male genitals. They lack the imagination to fantasy vividly. Men on the other hand may be aroused entirely by imagined objects; by direct contact with female genitals; or by any combination of reality and fantasy.

Women have used calculated exposure from time immemorial to arouse the male, for all manner of reasons. It is normal for a man to be aroused by female underclothes because they strongly suggest the sex organs they cover. It is only abnormal if he is not also roused by the female body itself, and this can only be determined if he has willing women at his disposal. Otherwise his "deviation" is only for want of something better. In a society which severely limits sex opportunity by taboos and economic sanctions, no wonder "deviants" occur.

Of the cases reported, only one, "A", had ever had an opportunity for anything like normal sexual outlet. "A" would probably never have sought treatment but for a transient episode of impotence which cleared up almost at once. Unlike many non-fetishists, his marriage was happy. Why the fuss over his foible? "B" had been completely without normal sex outlet. Given a female sex therapist with whom he could practice and perfect the art of love, would he have bothered to cross-dress?

"C", poor man, had only a frigid wife. How could he help thinking about his daughter? How pathetic that he should have been "tempted by her clothes"? "D" was sexually quite uneducated and deprived. "E" was also deprived, but had shown himself capable enough in his brief marriage.

The behaviour of all these people was simply an adaptation to the unnatural difficulties and restrictions which society places on normal sexual outlet. By means of their imagination they were able to make this adjustment. To set about callously to destroy it, and put nothing in its place, is like criticizing a one-legged man for walking with a limp, and then taking away his crutch.

B. Bignold.

Heathcote Hospital,
Canning Bridge,
Western Australia, 6153

Dear Sir,

We read with interest Dr. A. B. Goorney's excellent article on the treatment of a compulsive horse race gambler by aversion therapy (*Journal*, March, 1968, pp. 329–333). In reference to our earlier work (Barker and Miller, 1966a and b), Dr. Goorney claims that "aversion of imagery was not included in the techniques employed". We consider, however, that this point requires further elaboration. McGuire and Vallance (1964) and others have recommended the use of "conceptual deconditioning" alone for treating cases of sexual deviation. While this offers many practical advantages we are not yet convinced whether it is as effective as aversion therapy directed towards the maladaptive behaviour itself or to reproductions of such behaviour, using films, coloured transparencies, tape-recordings or video-tape. Furthermore, in our experience, perverse fantasies would appear to be more important to the sexual deviant than are fantasies of gambling to the inveterate compulsive gambler.

We have now had the opportunity of treating several compulsive gamblers, including "one-armed bandit", "pin-table" and "betting-shop" addicts (Barker and Miller, 1968). They have all denied that they are able to produce realistic fantasies of their particular gambling habits in the clinical atmosphere of the treatment room and particularly when fearfully anticipating the next shock. We have therefore resorted to treating the gambling behaviour itself, or have reproduced the patient's gambling before him, using films and photographs. In some cases we have supplemented this by pre-recording the patient's own account of his gambling on tape, which is then conveniently replayed during aversive sessions contemporaneously with the visual cues, coupled with "betting-shop" sounds, etc., where applicable.

Dr. Goorney seems to have been fortunate in finding a gambler whose compulsive behaviour had a precise initiation and stereotyped pattern which favoured treatment largely on an imaginal level. In our experience such gamblers are rare. The majority of our gamblers have been quite unable to reproduce realistic fantasies of the "betting-shop" or "dog-track" atmosphere during treatment sessions, since they seem to experience much difficulty in imagining