IS MUCH PSYCHOTHERAPY STILL MISDIRECTED OR MISAPPLIED?

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Abstract

In the course of the past few decades, it has become increasingly evident that psychotherapy, generally speaking, is ineffective. Yet, for want of something better, practioners and patients keep this dubious enterprise going — because psychoneurotic problems stubbornly persist, and even proliferate. It is therefore axiomatic that psychotherapy is being misused, in any of the several possible ways. Why is this happening? The present paper suggests three major respects in which much (perhaps most) psychotherapy and counselling is being "misdirected or misapplied." As corollaries, alternative types of remedial endeavor are suggested.

Résumé

L'exérience des dernières décennies nous démontre assez clairement que dans l'ensemble, la psychothérapie est inefficace. Quoiqu'il en soit, faute de mieux, praticiens et patients continuent à s'y adonner car les problèmes psychoneurotiques ne démordent pas. Bien au contraire, ils deviennent plus nombreux. Il est donc évident que la psychothérapie est mal utilisée de plusieurs façons. Pourquoi en est-il ainsi? Cet article propose trois façons selon lesquelles presque toute la psychothérapie et la consultation sont mal dirigées ou utilisées. Enfin, on suggère d'autres types d'approches jugées plus efficaces.

The writing of this paper has been prompted by a letter from a Canadian psychologist which reads, in part, as follows:

> Following Donald Campbell's presidential address (1975) to the American Psychological Association, I have become increasingly interested in the "nature of man" as viewed from a sociobiological perspective. I have also become increasingly convinced that your model of the sociopath-, neurotic-, normal continuum is a better representation of reality than is the Freudian alternative so widely accepted in counselling psychology.
>
> I teach graduate courses in group process, and group counselling at the University of ——————. My personal experience and perusal of the literature in these fields has also led me to conclude that your model of "integrity therapy" merits greater respect than any other model of group therapy extant.

In two other recent papers, "The Growing Impact of Mutual-Help Groups on Professional Psychotherapy and Counselling" (Mowrer, 1978a) and "There May Indeed Be 'Another Way' — Reply to J.D. Smrtic" (Mowrer & Veszelovszky, 1978; Smrtic, 1978) the present writer has reviewed a variety of considerations and empirical research which seem to support the stance taken by the writer of the foregoing letter. This paper will draw upon these and other sources pertinent to the issue of an impending "paradigm shift."

THE FREUDIAN CONCEPTION OF PSYCHOPATHOLOGY AND PSYCHOTHERAPY

Since the ascendancy of Freudian psychoanalysis, early in this century, the prevailing assumption, in Europe, North America, and much of the rest of the world, has been that so-called "neurosis" has its origin in the oversocialization of children, which produces an excessively severe, repressive super-ego, or conscience. Sociopathy (psychopathy), according to the same theory, reflects under-socialization and a defective conscience. These two forms of deviance are shown, with respect to normality, in Figure 1. Here the two short arrows indicate the diametrically opposed "directions" of therapy for neurosis and for sociopathy, in relation to the continuum of socialization, represented by the long arrow, pointing to the right. However, psychoanalysis offered no technique for the treatment of

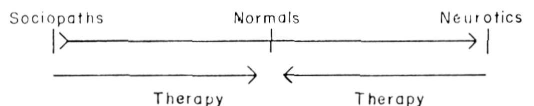

Figure 1. Diagramatic representation of the Freudian conception of two forms of psychopathology and their respective remedies.

sociopathy, so its application has been mainly restricted to neurotic difficulties.

From the outset, psychoanalytic theory has involved a logical inconsistency, and therapy with neurotic persons has revealed an empirical complication. It has long been clinical practice to refer to certain individuals as "sociopaths — mixed type". This meant that such persons manifest an admixture of sociopathic and neurotic tendencies. In the diagramatic representation of Freudian theory and practice shown in Figure 1, it is clear that an admixture of sociopathy and neurosis ought to result in a more or less *normal* individual person, not a "clinical type". *A priori,* this situation raises a serious question about the validity of psychoanalytic theory and, by implication, also treatment.

One of Freud's most famous "patients" was a certain "Dr. Schreber," whose autobiography was analyzed by him and whose paranoia he attributed to repressed homosexuality (Freud, 1933). Many years later, however, it came out that Schreber was guilty of a major crime that he never mentioned in his book. When Freud learned of this, his surprised comment was: "Here we evidently have a criminal with a conscience", i.e., a "mixed type". If Freud had used his standard technique for treating neurotics with this man, i.e., attempted to reduce the presumed over-severity of conscience, it would not have been surprising if he had been unable to help him. It apparently never occurred to Freud that many, but not necessarily all, neurotics are "criminals with a conscience", as we shall see presently.

The empirical difficulty alluded to in the opening paragraph of this section has to do with the fact that, as experience with psychoanalytic therapy increased, it was discovered that this form of treatment, when applied to neurotic persons, commonly has one of two, equally undesirable outcomes: either it is ineffective, i.e., the therapist does not succeed in helping the client move down on the socialization continuum, toward normality, and as a result the client goes on being neurotic; or, in those instances where desocialization does occur, clients, instead of moving into the region of normality, began to "act out", i.e., became sociopathic. All this is highly anomalous. Since successful instances of psychoanalysis are comparatively rare, many analysts have become skeptical, or even cynical, concerning the validity of this type of therapy and the theory on which it rests (for example, Jurjevick, 1974; Mowrer, 1978b); but because it provides a comfortable source of income and many laymen continue to regard it as the treatment of choice, analysis, in more or less orthodox form, is still widely practiced.

Just as the author was starting work on this paper, on the morning of Thursday, July 13, 1978, he heard a woman's voice on television saying, categorically, "All adult hang-ups are due to childhood repression." His curiosity aroused, he continued to listen and discovered that the speaker was Althea Flynt, now publisher of the most notorious of all our innumerable "girlie" magazines, *Hustler.* Ms. Flynt was being interviewed on the Donahue Show somewhere in West Virginia; and from the context of the ensuing discussion, it was clear that the quoted remark had been made at the end of a defense of this and similar publications as sexually liberating, uninhibiting, *therapeutic.* The same argument, drawn knowingly or unknowingly from psychoanalytic sources, has been used repeatedly by the publishers, *and readers,* of others, similar magazines. Even though they sell by the millions, there is no evidence that this is what accounts for their popularity.

Milton Gross (1978), in a book entitled *The Psychological Society* is on much firmer ground when he says:

> Freud proved to be a poor sex prophet. Modern society has presented us with a living Freudian laboratory complete with birth control pills and respectable young women at the sexual ready. But neurosis is unabated, even increased, side by side with free and frequent copulation. As psychological counselors at universities can testify, the quest for emotional stability through orgasm has been a failure. (p. 319)

Gross's (1978) book will be belittled and, very probably, generally ignored; but his detractors will be hard pressed to show that he is wrong in his assessment of our situation.

A LESS PROBLEMATIC AND MORE COMMODIOUS ALTERNATIVE TO THE FREUDIAN MODEL

More than a quarter of a century ago, the writer ventured to suggest, on the basis of then rather meager but growing evidence, that neurotic anxiety and symptom formation occur, not because of repression by an intolerant and too severe superego, of the biologically given forces of the *id* (notably those of sex and aggression), but because the *ego* (or "executive" part of the personality) has repressed the superego in order to permit surreptious gratification of libidinal and aggressive impulses, with ensuing conflict, guilt, and neurotic manifestations (Mowrer, 1950). However, because repression is a difficult phenomenon to investigate emprically, this way of reformulating classical Freudian theory was hard to substantiate by means of suitable research and hardly anyone wished to take it on faith.

However, a number of rather well known and valid measures of socialization were available; and if the difference in views expressed above regarding the nature of pathogenic repression is

valid, then the three major personality types ought to lie along the socialization continuum as shown in Figure 2, and not as in Figure 1. This revised model of psychopathology and psychotherapy was fully developed in *The New Group Therapy* (Mowrer, 1964); and a well designed study by Peterson (1962), carried out with junior high school students, showed that students rated by teachers as having "no problems" scored high on the Gough-Peterson (1952) scale of social inadequacy-adequacy, those said to have "conduct problems" scored low on this scale, and those with "personality problems" clustered in intermediate areas, as the diagram shown in Figure 2 would suggest.

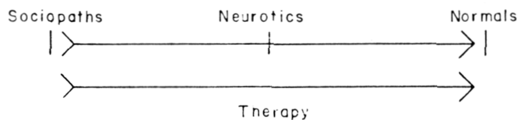

Figure 2. Revised model of psychopathology and its remediation.

Not only is the Figure 2 paradigm thus supported, and the Figure 1 model disconfirmed, by empirical research findings, the previously mentioned paradoxes inherent in the latter are also smoothly resolved. Here the "sociopath, mixed type" falls neatly into place on the socialization scale, between sociopathy and neurosis, and we also see why a form of therapy designed to produce desocialization (superego weakening), if it is successful, pushes neurotic individuals over into sociopathy, instead of normality, which, in Figure 2, lies in precisely the opposite direction on the socialization scale. Similarly, it is clear why, at an earlier point, it was suggested that, in a manner of speaking, neurotics in general, may be referred to as "criminals with a conscience"; according to the foregoing analysis (but see also succeeding sections of this paper), they have fallen short of normal (at-the-norms) behaviour, but, unlike sociopaths, they have had enough conscience to be uncomfortable ("neurotic") as a result of their (often concealed) shortcomings.

Although *The New Group Therapy* is still in print and enjoys a modest sale, the author is aware of comparatively little influence which the reasoning and research findings just reviewed have had in the field of clinical psychology, psychiatry, and social work — but, as we shall now see, they did stimulate some further, very compelling research.

MORE POWERFUL AND MORE ABUNDANT RESEARCH SUPPORT FOR THE REVISED PARADIGM

Not only are there relatively valid measures of socialization (as suggested in the preceding section), but also of the phenomenon of anxiety. Since sociopaths, neurotics, and normal persons are known to vary, not only with respect to socialization but also anxiety, a new and quite powerful differential test of paradigms 1 and 2 becomes possible if data on both these variables are collected for the same population and plotted on a two-dimensional graph. The theoretical expectation concerning the nature of the resulting graphic function if paradigm 1 were valid is shown in Figure 3. Since, in this psychoanalytic context, the assumption is that sociopaths are least anxious, neurotics most anxious, and normal persons somewhat intermediate, then an approximately linear relationship, as here represented, would be predicted. If, on the other hand, the model depicted in Figure 2 is valid, an inverted "V" type of curve would be expected to result from data of this nature.

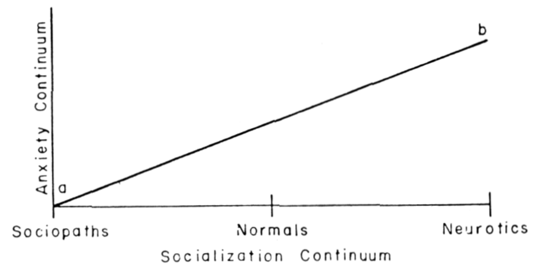

Figure 3. Graphic function generated by Freudian theory if anxiety as well as socialization is taken into account.

Within four years after the publication of *The New Group Therapy,* upwards of a dozen empirical tests were made of these theoretical deductions, all with results strongly favoring paradigm 2 (Mowrer, 1968). Here the findings of only one of these studies will be presented, as illustrative. In 1968, L.J. Rolls completed a doctoral dissertation, under the writer's supervision, in which he describes the following investigation. Rolls (1968) obtained access to the MMPI Master Files at the University of Minnesota, and thus secured scores on the *pd* (psychopathic deviate) scale, which is an inverse measure of socialization, and on the *pt* (psychasthenia) scale, which is a measure of anxiety, for several hundred persons diagnosed as sociopathic, neurotic, or normal. When the data were plotted the resulting function was very far from being linear as Freudian theory would predict. Instead the data conformed very nicely to the prediction of an inverted "V" made on the basis of the revised paradigm, as shown in Figure 4.

Again, this and the other related studies have received scarcely any attention, despite their high statistical reliability and revolutionary implications. Generally speaking, the continued

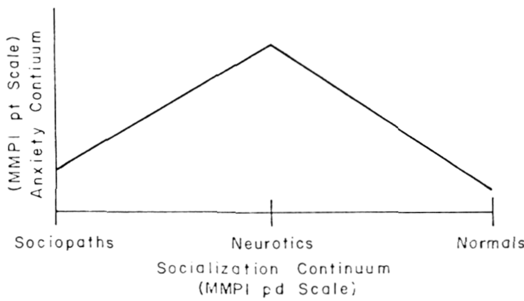

Figure 4. Empirical findings reported by Rolls (1968) which are inconsistent with Freudian theory.

tenor of events has been "business as usual", regardless of what should have been very disconcerting research findings.

ANOTHER TYPE OF MISDIRECTION, OR MISAPPLICATION, OF PSYCHOTHERAPY

Shortly before his death in 1964, Franz Alexander, a life-long devotee of psychoanalysis, wrote a paper entitled, "The Dynamics of Psychotherapy in the Light of Learning Theory" (Alexander, 1963). Although the exposition of learning theory in this paper was rudimentary and somewhat unsophisticated, the paper was nevertheless prophetic in that the author sensed the greater simplicity and propriety of this type of theory in comparison with that of psychoanalysis. Soon various techniques of "behaviour modification" or "learning therapy" were to be articulated, with a crescendo of books and journals devoted to this burgeoning field. Here there is neither need nor space to dwell upon these developments but two points are pertinent.

In the first place, behaviour therapists have addressed themselves, not to desocialization of their clients but to making them more "acceptable", both to themselves and others, in respect to a range of behaviours extending from nail-biting to alcoholism. The attempt was and is to help the individual "shape-up" by means of a type of training known as behaviour "shaping" (Skinner, 1938) — or "learning through successive approximation" Thorndike (1898). Thus, the argument directed in the preceding pages against psychoanalysis is not relevant to the behaviour modification movement. But there is another respect in which a challenge to this movement is long overdue.

Until about the middle of the 1960's, the present writer was, himself, a "behaviourist" in that he, too, embraced the view that all psychopathology (except in cases of demonstrable neurological impairment) was experiential in origin and remedy. He had often heard the assertion made, in both lay and medical circles, that "mental illness is inheritable"; but he chose not to believe it — and didn't. The supposition that all *psychopathology* is psychologically caused provides a more optimistic outlook, and besides, gives the burgeoning profession of clinical psychology more scope and independence from psychiatry than if one admitted the existence and operation of genetic variables. This would be especially embarassing to "radical behaviourism".

However, somewhat inadvertantly, the writer began to examine the evidence for genetic components in psychopathology which largely derives from "twin-studies". It is common knowledge that there are two types of human twins: *identical* (monozygotic, one-egg) twins, who not only look almost exactly alike, but who, in respect to their genetic make-up, are also virtual duplicates; and ordinary, or *fraternal* (dyzygotic, two-egg) twins, who are no more alike, morphologically and genetically, then individual siblings. Nature has thus provided a convenient and tidy opportunity to study the role of heredity in respect to any of a variety of personal and physical characteristics and defects.

Thus, the presumption would be that if a given type of genetically determined personality disorder occurs in one identical twin, this disorder will be more likely to appear in the other twin than if the twins were not identical. This type of coincidence is commonly called "concordance", and its presence or absence can usually be established with considerable reliability. In a paper entitled, "Is 'Behaviorism' Clinically Adequate?" (Mowrer, 1978b), the writer has recently assembled, in tabular form the results of well-replicated studies concerning the role of heredity in determining any of several personality predispositions.

The first document that happened to come under scrutiny in this connection was an English symposium on *Recent Developments in the Affective Disorders,* edited by Coppen and Walk, in which there is an article by John Price entitled, "The Genetics of Depressive Behavior" (1972). Here, as shown in Table 1, the results of seven independent twin studies of manic-depressive psychosis are presented, which indicate that in 97 sets of identical twins, 68% were concordant with respect to this disorder, whereas in 119 sets of fraternal twins, only 23% were concordant. As indicated in the legend for Table 1, this difference is reliable at a high level of statistical confidence. Tabulations of the results of twin studies of various other "psychological" debilities, which are reproduced in the paper cited in the preceding paragraph, decisively indicates that the consistently positive results here reported can no longer be dismissed or ignored, as due to artifacts or sloppy research methodology, as has often been done in the past.

TABLE 1

Concordance for Manic-Depressive Psychosis in Seven Twin Investigations (Price, 1968). "Monozygotic" and "dizygotic" pairs are used here to denote, respectively, what are perhaps more commonly known as "identical" as opposed to "fraternal" twins. The term "concordance" means that if the manic-depressive syndrome is found in one twin, the other also manifests this disorder. Although not computed by Price, the P-value for the difference in percentage of concordance for the monozygotic and the dizgotic twins is well beyond the .001 level of significance.

Author	Monozygotic Pairs		Same-sexed Dizygotic Pairs	
	Concordant	Total	Concordant	Total
Luxenburger (1928)	3	4	0	5
Rosanoff (et al., 1935)	16	23	8	35
Kallman (1950)	22	23	Not	stated
Slater (1953)	4	8	3	15
da Fonseca (1959)	15	21	15	39
Essen-Moller (1963)	1	8	0	3
Harvald and Hauge (1965)	5	10	1	22
Total	66	97	27	119
% Concordant		68		23

The almost universal position taken in "radical (blind?) behaviourism" is typified by the first sentence in *A Psychological Approach to Abnormal Behavior* (Ullmann & Krasner, 1975): "The central idea of this book is that the behaviors traditionally called abnormal, are no different, either quantitatively or qualitatively, in their development and maintenance from other behaviors" (p. 2). For anyone who is willing to examine the empirical evidence cited, this is manifestly a highly doctrinaire and dogmatic position, prompted more by "the sociology of professions" than by facts.[1] To the extent that "learning therapy" is applied to behaviour that has a constitutional, rather than learned, basis, such therapy is patently inappropriate, misdirected, misapplied — or, at the very least, seriously incomplete.

Sometimes the argument is advanced: If genetic factors are involved in such a disorder as manic-depressive psychosis, how does it happen that, as in the Price (1972) study, some identical twins are *not* concordant with respect to this disorder? The answer given by specialists in this field is that traits such as gender and eye-color, which are determined by a very simple genetic mechanism and thus follow the Mendelian Laws of inheritance, whereas personality disorders are determined "polygenetically", i.e., complexly and may or may not be manifested. A perhaps more satisfactory way of accounting for this type of variability is the *diathesis-stress hypothesis,* which is widely held in England but not well known this side of the Atlantic. "Diathesis" simply means constitution or predisposition; so the meaning of the diathesis-stress hypothesis is clearer if it is written as "diathesis/stress" or diathesis/stress = ? Thus the presence or absence of a disorder such as manic-depressive psychosis is readily seen to be a function of *two,* rather than just one variable. For purposes of further clarification, let us suppose that "diathesis" refers specifically to *stress* tolerance. There is all manner of evidence for genetic differences in stress tolerance with respect to both mental and physical diseases; hence, the occurrence of such diseases is a function of the amount of stress *experienced,* as well as the "diathesis" (hereditary) element.

When the word, neurosis, was first used in this paper it was put in quotation marks and prefaced by "so-called", as a means of indicating a need for qualification, to be made later — now that qualification is in order. "Neurosis", as it is conventionally used, is a very inappropriate and misleading term. It implies an "osis" or disorder of the nerves, when just the reverse, namely something quite functional, is what is usually implied. In some ways it would have been more appropriate for the term "psychosis" to have been used in this connection and "neurosis" reserved for those disorders for which there is now clear and compelling evidence of subtle neuro-encrindological variations, produced by the interaction of genetic and environmental factors. Freud, himself, made at least a hypothetical distinction between psychoneuroses and *actual* neuroses. Skinner as well has likened psychosis to something wrong with the circuitry in a television set. Learning therapy may be able to alter the amount of stress which a person experiences, but it can hardly be expected to influence stress tolerance, at the genetic level. Here our best hope would seem to be genetic counselling — or what

1. The ulterior use of "behaviourism" in clinical psychology is, from time to time, quite explicitly acknowledged. See, for example, the following quotation: "By challenging and questioning the heuristic value of disease analogies and various medical models, behaviour therapy has placed psychological change and intervention in its proper context — within education rather than medicine" (Lazarus, 1977, p. 553).

used to be called "eugenics" until the Nazis sullied the term. (But see also the discussion in the following section.)

In any case, it is clear that behaviour therapists have, for "political" reasons, chosen to make themselves oblivious to the obvious, if one but examines the relevant evidence.

HYPOGLYCEMIA: PSYCHOTHERAPY OR CORRECTIVE NUTRITION AND EXERCISE?

During the early 1940's, it was found that pellagra psychosis—characterized by "nervousness, insomnia, loss of memory, confusion, suspiciousness, hallucinations, apprehensive, and depression" (Williams, 1973) — could be quickly (usually within a week) and permanently cured by means of victim B_3 (niacinimide). Although this discovery was made before behaviour therapy was much in vogue, it seems most unlikely that the latter, or any other form of psychotherapy, would have been effective with pellagra, since not only the cure but also the cause (a diet consisting largely of cornbread, grits, and "fat-back") have no relation to "behaviour". The dramatic success in using vitamin B_3 with pellagrins, led, quite understandably, to its pervasive administration to thousands of hospitalized persons and many "outpatients" of virtually every known diagnosis, but with essentially negative results. Apparently vitamin B_3 was *specific* to pellagra psychosis.

Whatever faint interest in the dietary and vitamin approach to psychiatric problems may have lingered on, it was very much over-shadowed in the 1950's by the discovery, in that one decade, of potent synthetic antidepressant drugs and major tranquilizers (phenothiazines). These new pharmaceutical agents had an unmistakable calming effect upon both the patients and staff in mental hospitals, permitting many otherwise "hopeless cases" to be discharged and general practitioners as well as psychiatrists in private practice to use these drugs preventively, in the sense of enabling many patients to avoid hospitalization. Psychotherapy continued to be used as a not uncommon adjunct to drug treatment, with indeterminate effects. Although the new drug therapies were not without certain disadvantages (noxious "side-effects," patient discontinuation after discharge, etc.), there is no question that they were a great and, on the whole, valuable, innovation.

In the 1960's, interest in corrective nutrition and vitamins, used in unprecedented volume and variety, re-appeared, with applications ranging from the common cold to schizophrenia. The results were inconclusive, or so at least "established" psychiatry tried to make them seem; and the field is still blazing with controversy. However, two developments are noteworthy:

1). An association known as The Academy of Orthomolecular Psychiatry had come into existence, with membership open to persons other than conventionally trained psychiatrists. This organization is rapidly growing in numbers and apparent influence see, for example, its official publication, *Journal of Orthomolecular Psychiatry* (publication office, 2231 Broad Street, Regina, Saskatchewan, Canada S4P 1Y7). An instructive editorial appears in the Second Quarter, 1978, issue of this Journal by A. Hoffer; and articles on the history and vicissitudes of Orthomolecular Psychiatry will be found in earlier issues.

2). Although the dispute over the relative value of synthetic psychotropic drugs versus vitamins and nutritional regimes continues unabated, there is a growing area of convergence with respect to the biochemistry of "mental illness". It is not known (Prastka, 1978) that there are at least nine subtle substances called "neurotransmitters" which are essential to the effective and normal functioning of the human nervous system; and there is growing agreement that anomalies in these substances constitute the biochemical substructure of much disturbed behaviour, affect, and perception. The question is: precisely what are these anomalies and how best to correct them, pharmaceutically or orthomolecularly?

We come now to the subject of hypoglycemia, not because its cause and treatment were discovered more recently than the developments just discussed, but because they considerably antedate them and are still receiving comparatively little attention. Banting made insulin available for the treatment of diabetes in 1922; and two years later, in 1924, a physician by the name of Seale Harris reported the observation that if a diabetic patient is given a slight overdose of this substance, he or she shows a congeries of symptoms of which nondiabetic persons not uncommonly complain, including "cold sweats, fainting or blackout spells, 'going crazy' sensation, emotional upsets, unstable temper, moodiness, fatigue, confusion, anxiety, shakiness, fearfulness, claustrophobia, irritability, indecision, abnormal sexual behaviour, and alcoholism" (Nittler, 1972, pp. 59-60). Diabetes involves excessive blood sugar, and insulin metabolizes it, thus, in the right amount, bringing the blood-sugar level down within the normal range. A little too much insulin, however, abnormally lowers the blood-sugar level, and a more serious overdose produces convulsions or comma. The brain is nourished exclusively by glucose (blood sugar), so it is not surprising that glucose deprivation or "undernourishment," episodic or chronic, produces various psychopathological effects. Many persons who are today diagnosed (by means of both subjective and biochemical changes during a glucose tolerance

test) as having hypoglycemia (or hyperinsulinism) report a history of varied and protracted psychotherapy. They have gone to physicians who could find nothing organically wrong with them and therefore referred them for psychotherapy, or they have gone directly to psychotherapists or counsellors and been accepted for treatment, with no exploration of the possibility that their strange feelings and behaviour may be coming from hypoglycemia.

Here, then, is another instance of the misapplication of psychotherapy. How often it occurs is related to the prevalence of so-called "functional" (as opposed to "essential," i.e., organically caused) hypoglycemia, or low blood sugar. Conservative, conventionally trained physicians maintain that this is actually a "very rare and greaterly overdiagnosed disease", whereas other physicians and nutritionists estimate that twenty to fifty million persons (Americans) have this disorder and that, due to our increasing use of sugar, white flour, chocolate, nicotine, caffeine, and alcohol, its incidence is rapidly growing. Because there is at present no effective form of "medical" treatment (only diet, avoidance of the substances indicated, and exercise), physicians tend to "dump" hypoclycemics on psychotherapists who generally accept them as psychoneurotics and do nothing to help them reorder their lives in the only way which will reestablish their physical and mental health. Thus, both physicians and psychotherapists are guilty of a special form of "malpractice". A few physicians and psychotherapists are becoming more sophisticated in this area, but the great majority of hypoglycemics are still either going untreated or are mistreated. A paper devoted exclusively to hypoglycemia and its neglect and mismanagement is now in preparation.

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