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Critical appraisal of the shock therapies in psychiatry

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A CRITICAL APPRAISAL
OF THE SHOCK THERAPIES
IN PSYCHIATRY

by

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Senior Thesis

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University of Nebraska,
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Preface

In the course of development of any chapter of medicine, there are periods when it becomes appropriate and even necessary to correlate the results of investigation on a particular problem, with the aim of creating, out of individual fragments of experience and research, a unified and meaningful whole.

This necessity is particularly great when the question under consideration happens to lie in the realm of therapeutics, for in this case it is only through a thorough analysis of the problem that rational guideposts for future clinical action can be erected. And especially when there enters upon the medical scene a new form of therapy bearing the possibility of revolutionizing an entire branch of medicine, a scientific assessment of its value becomes indeed a matter of utmost indispensability.

Such is the case today with the shock therapies in psychiatry. Born of chance and empiricism, subsequently treated, like many another new therapeutic agent, with all degrees of reaction ranging from exuberant enthusiasm to violent condemna-

tion, the shock therapies have survived a decade of stormy existence and have today reached the point where sufficient evidence has accumulated to warrant a rational evaluation of them.

Because of the controversy and confusion concerning the results obtained with the shock therapies -- a state of affairs which has plagued them ever since they became subjected to extensive clinical trial -- some observers have thrown up their hands in desperation and have discredited them on this basis alone. However, since every effect has its cause, there are good and ample reasons for this lack of consensus, and it has been my purpose in this thesis to sift out these causes of disagreement and to determine, as far as possible, the lines of future approach which will break the impasses existing today.

In this connection, I have attempted to keep these pages from becoming a mere review of the literature. Since the literature on any controversial question abounds with all shades of opinion and all degrees of conflicting evidence on the topic, a mere listing of such evidence and opinions, without any attempt to analyze and evaluate them, becomes a profitless venture both for the writer and for the reader. I have long felt that the most important

part of the learning process is not what one but how one's thinking reacts to what he learns. Consequently, I have included my own opinions and criticisms of the material presented, wherever I have felt them to be warranted. After all, when circumstances do not permit the possibility of doing original investigation on a medical problem, the next best thing is a critical analysis of the work of

I have not attempted to cover the entire literature on the subject. Such a task would be not only Herculean, because of the presence of literally thousands of contributions on the topic, but also profitless, because of the fact that the literature on the shock therapies has its share which creep into the literature of any medical topic. I have included a few specimens of what I consider to be worthless contributions, in order to demonstrate how the question under consideration has become unnecessarily confused by reports which possess neither scientific accuracy nor statistical completeness.

In the preparation of a work as extensive as a senior thesis, one is bound to carry away with him many ideas over and above the actual material which he has learned. In this regard, I have found

that the research I have done on my topic has impressed upon my mind, above all else, the eternal truth expressed by the father of our profession in the most famous of his aphorisms: "Experience is fallacious, and judgment difficult".

Chapter I

The History of the Shock Therapies

The heritage which we call modern medicine represents the summation of all the significant medical achievements of the past. It is merely the present status of an evolutionary process analogous to the biological course of development which has produced the current species of organisms; both phenomena represent a steady march of ever-increasing complexities stemming from rudimentary and relatively undifferentiated beginnings.

Throughout the countless centuries since the first physicians took on the task of alleviating the ills of their fellow-man and began to pass on their knowledge to others, the growth-processes of medicine have always been essentially the same. The principal difference between the medical progress of today and that of past generations is not one of fundamental nature but rather one of rate of growth. For, under the stimuli of rapid advances in the physical and biological sciences, an ever-increasing host of workers engaged in medical research, and a constant pooling

of the information obtained, modern medicine has advanced, and is continuing to advance, at a rate equivalent to a geometric progression.

Within the framework of this process, new therapeutic agents have been characterized by great variations in the amount of research and the degree of collective effort necessary for their development. Each method of shock therapy can be considered to be the fruit of individual investigation, rather than the result of group research, and each of them has enjoyed the privilege of widespread clinical use soon after its discovery.

Although the shock therapies as we know them today were inaugurated only within the past decade, still, as with many other discoveries in medicine, their arrival on the medical scene was not entirely unprophesied. For centuries, clinicians have observed that severe emotional and profound physical stimuli are able to bring some mentally ill patients back into contact with their environment. The herb hellebore was used in the treatment of mental disease several hundred years ago. In 1755, Auenbrugger observed four patients who suffered from "mania" with "periodic raving madness" and who were treated with hellebore (1). The reactions which he described were, among others,

a combination of coma and generalized convulsions, or, as we would describe it today, a combination of the reactions to insulin and to metrazol or electroshock.

Paracelsus, early in the sixteenth century, is credited with the use of camphor by mouth for the cure of mental disease by the production of convulsions (1). The drug was also used by Oliver in 1781, who administered it to relieve a manic patient (2). Camphor was generally used throughout Europe for the same purpose in the second half of the eighteenth century, but its use was gradually discontinued and forgotten without any obvious reason.

More recently, acute psychological changes had been observed to result from changes in oxygen tension, and experimentation with high concentrations of carbon dioxide had shown that temporary beneficial results were obtained in catatonic stupors (3a).

With this background, meager though it is, the medical mind was not entirely unprepared for giving sympathetic attention to the first reports of the present-day fathers of the shock therapies. Of the three methods of therapy in most general use during the past decade -- insulin, metrazol, and electroshock -- insulin takes precedence chronologically.

Prior to the introduction of hypoglycemic

shock therapy, insulin had proved its value in exerting a quieting effect on conditions of agitation and was used chiefly in cases of delirium tremens and catatonic excitement; Steck reported such use of the hormone in moderate doses since 1929 (3b). Other workers gave insulin to patients who refused to eat. Appel, Farr, and Marshall (4) in 1929 were some of the first workers to report the use of insulin in combatting the problem of undernutrition in psychotic patients. Bennett and Semrad (5), among others who used the drug for the same purpose, reported that, co-incident with the gain in weight of their patients, there appeared in many cases a striking improvement in behavior and mentation, and a return of normal affective tone. Also, practically all of their patients became more accessible for psychotherapy and other therapeutic measures.

In all such uses of insulin in this period of prelude to the hypoglycemic shock era, there was neither the ambition nor the hope of influencing the psychosis as such. Moderate doses were administered and shock symptoms avoided.

The use of insulin as a direct therapeutic attack on psychoses, known to us today as the hypoglycemic shock treatment, dates from 1928. Although

the treatment was discovered in that year, the method and the results in the first treated cases were not published until 1934, and the therapy did not begin to receive widespread trial until 1936.

Hypoglycemic shock therapy, like so many other discoveries in medicine, was come upon quite by accident. Interestingly enough, the discovery occurred almost simultaneously with another great epoch-making accident in medicine, for it took place only one year before Fleming's chance observation of the inhibition of bacterial cultures by the *Penicillium notatum* mold. Manfred Sakel, who had had a wide experience in the use of insulin in drug addiction, began in 1928 to give large enough doses to produce hypoglycemic states. Unintentionally, severe hypoglycemic shocks occurred. This event and its results are best expressed in Sakel's own words (6):

"The therapy is an outgrowth of observations made by me in the course of the attempted treatment of morphine addicts. I thought, first of all, that insulin abolished the phenomena of irritation during abstinence from morphine because the nerve cells were blocked and their function quantitatively affected. Starting from this observation and this idea I attempted to influence other states of excitation by means of insulin. At this

point, as often happens in such matters, I was helped by chance. By chance I produced deeper hypoglycemic reactions than I had intended. I was able then to observe that such reactions led to much quicker and more substantial alterations in mental states, and could even cause psychotic symptoms to vanish. I endeavored to evaluate these observations systematically and drew the practical conclusion that hypoglycemia was evidently responsible for these changes.

"I was led by the following observations to treat psychoses by hypoglycemia. The intense fixation seen in psychotic anomalies is toned down, the rigid pent-up personality is relaxed, the affect is reversed, and this reversal is maintained."

Thus the origin of hypoglycemic shock therapy, which is, as Sakel puts it, "an etiologically non-specific (though clinically specific) treatment for psychoses".

Sakel introduced the therapy in Vienna in 1933, and he began publishing his method and results in 1934. In 1935 the treatment was initiated at various centers throughout continental Europe, to be followed a year later by England and the United States. Since 1937, insulin shock therapy has been granted an extensive clinical trial throughout the world.

In contrast to insulin therapy, which was discovered by accident, the use of metrazol as a form of shock therapy was brought about through a means which is as common in the history of medicine as is the occurrence of chance discoveries; namely, through a misconception.

Ladislav von Meduna, the father of metrazol therapy in psychiatry, had made the observation that spontaneous convulsions occurring in catatonic schizophrenic patients were followed by a prompt remission. He also noted that an association between epilepsy and schizophrenia is extremely rare. From these observations, he made the following deduction (7):

"Between schizophrenia and epilepsy there exists a sort of biological antagonism which must be expressed in the pathological course of the two diseases. Without being able to characterize these pathological actions, I feel justified in asserting, a priori, that these courses are either mutually exclusive or they do at least to a great degree weaken each other in their mutual efforts."

From this working hypothesis, Meduna reached the conclusion that an epileptic seizure would alter the biochemical and hematologic substratum of the organism in such a way that a further development of the schizo-

phrenic process would be inhibited and a remission made possible. Of course, the biologic substratum is not the same in induced as in idiopathic convulsions. And, in the light of present evidence, to be presented in a later chapter, that metrazol therapy is much more effective against affective states than it is against schizophrenia, it would be necessary for Meduna, in order to be consistent, to postulate the existence of biological incompatibilities between affective psychoses on the one hand, and epilepsy on the other.

In 1933 Meduna started administering convulsive therapy on the basis of the conclusions from his working hypothesis. After preliminary animal experiments had indicated to him that induced convulsions did not cause major damage to the central nervous system, he began to use camphor, the same drug given four hundred years previously by Paracelsus, injected intramuscularly in a number of schizophrenics. Because of the definite disadvantages of camphor, such as the possible occurrence of abscesses and pain at the local site, and the difficulty in predicting the exact onset of the seizure, which may occur at any time from one-half to three hours following the injection, Meduna soon discontinued the use of the drug as a convulsant and substituted for it pentamethylene-tetrazol (metrazol or cardiazol), a synthetic product, which has the same pharmacologic effect

as camphor and in addition to low toxicity, possesses the advantage of being soluble in water, therefore becoming suitable for intravenous injection. The resorption of the drug is rapid and the seizure almost immediate, thus further increasing its advantages over camphor.

Meduna published his first article on the use of metrazol in 1934. Since 1935 the method has been given a clinical trial as widespread as that enjoyed by insulin. For reasons to be discussed later, metrazol shock therapy has now passed the peak of its utilization and is rapidly achieving the status of an outmoded method of treatment, its role in psychiatry having been taken over by the youngest of the shock therapies -- electroshock.

Shortly after the inauguration of the clinical use of metrazol, its marked disadvantages became apparent to those using it. Because of its untoward physical complications and mental anguish, which in some cases became so marked as to make the treatment very unpleasant for the patient and sometimes prevented continuance of the treatment (1), it became necessary to find a method that produced at least the same good results as metrazol, one that did not give rise to the complications of metrazol, one that could be carried out even more

easily than by intravenous injections, and one that could be applied with more uniformity as to the intensity and duration of the treatment.

With these aims in mind, Bini (8) in 1937 conducted convulsive experiments on animals, with electric currents. His investigations, although they led directly to the subsequent clinical use of electroshock, were far from being revolutionary, for electrical experiments in psychiatry have a history which stretches back more than forty years. Leduc in 1902 produced narcoses, states of stupor, and general anesthesia by the use of electricity (1). A year later, Zimmerman and Dimier succeeded in producing epileptic attacks in animals by using interrupted galvanic current (1). After them, numerous other workers confirmed the possibility of obtaining at will narcosis, epileptic attacks, catatonic states, and general anesthesia, by varying the intensity and character of the current, the form of the electrodes, and their point of application (1).

While many, as indicated above, have utilized electric current for the production of convulsions and unconsciousness in animals, it is to Bini and his co-worker Cerletti that the credit must go for applying the method clinically. In 1937, working on dogs, Bini produced epileptiform convulsions but also caused severe

damage to the central nervous system, as determined at autopsy (8). He carried on, however, until he found the current which produced convulsions in animals without causing damage to their nervous systems. In 1938 Bini collaborated with Cerletti in using this current on humans, by applying 90 to 125 volts of alternating current for $1/10$ to $3/10$ of a second to the head of the patient. They reported only a few cases, and it was their belief (1) that the therapeutic efficiency of their new method of therapy was at least as good as that of metrazol, but without the disagreeable subjective and objective complications of the latter.

There has recently been reported (116) the use of a modified form of electroshock therapy which has been given the name of electronarcosis. By this method a steady electric current, of a lower milliamperage than that used in electroshock, is maintained for approximately seven minutes. Although it has been claimed (116) that the results obtained in schizophrenia are superior to electroshock and similar to insulin, there has not as yet been enough confirmation of these claims to warrant conclusions as to the value of the method.

Electroshock therapy, in use in Italy since 1938, was rapidly adopted in England and Holland (1), and by the end of 1941 the method had been put to use

in more than 140 hospitals in the United States (41).
It is now generally conceded to be the method of choice
in the administration of convulsive shock therapy.

Chapter II

The Shock Therapies in Schizophrenia

Throughout the decade which has passed since the initial clinical use of the shock therapies, the strongest fortress against which this barrage of therapeutic weapons has been directed has been the disease which continues to justify its appellation of the mystery of psychiatry. In the entire realm of medicine there is no disease which rivals schizophrenia in regard to morbidity. In this respect it represents a more serious problem than either tuberculosis or carcinoma (9). There are more hospital beds occupied by its victims than by the sufferers of any other single disease. There are actually twice as many hospital cases of schizophrenia as of tuberculosis (9).

Such being the case, it is obvious that any therapeutic measure which would make it possible to directly attack and successfully eradicate this scourge could be rightfully regarded as one of the greatest medical victories of our time. It is not surprising, therefore, that when the hypoglycemic and convulsive shock therapies stepped onto the psychiatric scene,

hopes ran high and enthusiasm was unrestrained.

Fuel was added to the flame of general optimism by early reports of spectacular achievements by the originators of the methods. Sakel, in summarizing the results in the insulin treatment of his first one hundred cases of schizophrenia (10), found that where the duration of the disease did not exceed six months, 70% of the patients had full remissions and were able to return to their former work, and that an additional 18% had good improvements. In all cases of over six months' duration, the results varied in inverse relation to the length of the illness. These results Sakel compared with spontaneous remissions among untreated patients, varying from 5% to 20%.

Meduna, likewise, achieved results with metrazel which were as gratifying as those of Sakel, if not more so. The reports on his earliest cases (11) indicated, as did Sakel's findings, that the most spectacular results were obtained in schizophrenics whose duration of illness had been not longer than six months. Out of a series of 36 such cases, Meduna obtained remission in 33, a figure equivalent to 91%.

In the decade which has passed since the publication of these initial glowing reports, the question

of the efficacy of the shock therapies in schizophrenia has become a hotly-contested battle, with little unanimity of observation or opinion, the reasons for which we shall presently consider.

To illustrate the great variance of results obtained, let us examine a review (12) of three large surveys, in New York (1938-39), Ohio (1940), and Ontario (1941). The percentage of recoveries of schizophrenic patients treated with metrazol were 1.6, 15.2, and 31.6, respectively; the percentages for those treated with insulin were 12.9, 31.2, and 29.1, respectively. The following list of percentages of recoveries, which I have compiled from the reports of the workers listed below, demonstrates an even greater discordance in findings: Those investigators listed below under A treated their patients with hypoglycemic therapy, those under B with convulsive therapy, and those under C with a combination of the two methods:

	Investigator reporting	Year	Patients treated	Percentage of recoveries
A	Ross (13)	1937	286	32.0
	Malzberg (14)	1938	1,039	12.9
	Savitt (15)	1938	45	57.0
	Bateman and Michael (16)	1938	416	32.0
	Cheney and Clow (17)	1941	50	16.0
	Bond and Rivers (18)	1942	188	33.0
	Gottlieb and Fuston (19)	1943	66	35.0
	Weil and Moriarty (20)	1944	20	75.0

B	Ross (40)	1939	523	4.4
	Bateman and Michael (16)	1940	579	26.0
	Hemphill (21)	1942	114	3.5
	Smith et al. (22)	1942	16	0.0
	Neymann et al. (23)	1943	90	34.4
	Kalinowsky and Worthing (24)	1943	200	44.4
	Rennie (25)	1943	70	32.8
C	Notkin et al. (26)	1943	100	0.0
	Taylor (39)	1945	214	71.5

The great variations in the recovery rates listed above will give the reader an indication of the reason why there has been a lack of agreement on the value of the shock therapies in schizophrenia. By today the number of published reports dealing with the shock therapies has reached staggering proportions, running into the thousands. In view of this tremendous amount of investigation, one might expect some concordance in the results obtained. Such expectation is gratified in the question of the efficacy of the shock therapies in the affective psychoses, as will be demonstrated in the next chapter. Why, then, has there been a lack of consensus in the case of schizophrenia?

The reasons, as I see the problem, are complex and multiple. Discordances in results and findings in any scientific work indicate the necessity of additional investigation and suggest that there is some lack of

uniformity in the selection of material, in the technical procedure, or in the interpretation of the results. In addition to finding all these factors operating in the problem at hand, I have observed, in a distressingly large number of reports, evidences of a pitiable lack of knowledge of, or regard for, accurate statistical methods. In some instances, e. g. (15, 20, 22), the number of cases presented is so small that the statistical standard error becomes enormous and yet is not recognized as such by the investigator submitting the report. Worse yet, some investigators make little attempt to do much besides presenting raw data without benefit of statistical inference. In one such example (27), the number of cases studied is large -- more than one thousand; a study of a group of this size, if done with more care, would undoubtedly have resulted in a significant contribution, rather than a mere mass of data.

It is thus evident that an evaluation of the shock therapies must become, to some extent, an evaluation of the shock therapists. Over and above this factor, however, there are many aspects of the problem which constitute peculiarities inherent in the specific nature of schizophrenia, peculiarities which challenge one's critical judgment of even the

most painstakingly accurate statistical survey.

In the first place, in attempting to evaluate a particular form of therapy in any disease, it becomes a matter of fundamental importance to study the course of the disease in untreated cases. No competent laboratory worker would ever think of forming conclusions on an experiment without the presence of controls. In a clinical experiment, such as the introduction of a new method of treatment, how can one hope to arrive at a rational verdict without similar scientific safeguards?

It is obvious, therefore, that any attempt to arrive at a just evaluation of the shock therapies in schizophrenia without an adequate cognizance of the natural course of the disease if left untreated, is as unscientific as it is futile. Schizophrenia being the chronic disease that it is, with remissions and relapses, and with all degrees of exacerbations and improvements, it consequently becomes necessary to consider the course of the disease from the long-term viewpoint.

Many cases, apparently recovered or much improved, are dismissed from the hospital at the time of such recovery or improvement, only to relapse at a later date and be re-hospitalized. Others of this group maintain their recovered status over a period of

years. While those patients who have never shown any improvement constitute the group of chronic hospital cases. A true picture of the final outcome of the disease can therefore be constructed only by the use of extensive follow-up studies over a number of years.

There have been a number of such studies, emanating mainly from state hospitals. The duration of follow-up varies from two years in some up to ten years in others. Rupp and Fletcher (30) performed this type of investigation on a total of 641 schizophrenics, all of whom were hospitalized in the pre-shock therapy years between 1929 and 1934 and were followed for a period of five to ten years. The recovery rate for this group was found to be 16%.

Strecker (60) collected spontaneous-remission rates from eleven sources in the literature, with a total of 581 cases, and found an average figure of 24%. Bond and Rivers (35) noted a consistent rate of between 10% and 20% for spontaneous remissions over the years at their hospital. The figure noted by Bateman and Michael (16) for 325 patients was 15%. Guttman and his co-workers (61) followed up a group of 280 and noted a spontaneous recovery rate of 33%, the highest rate among all these reports. The lowest rate of all is to be found in Malzberg's series (14), where, out

of 1,039 patients, only 3.5% were considered recovered.

On totalling the above reports, I find that out of a grand total of 2,833 non-shock-treated patients, 364 spontaneous recoveries occurred. I compute this to be 12.8% of the total group.

The problem has recently been attacked, and quite forcefully so, from an entirely different angle, statistically speaking, by Penrose and Marr (12). Realizing that the percentage of recoveries of shock-treated patients varies so widely from one report to another that any attempt to evaluate the treatments is inadequate, they decided to attempt such appraisal by estimating the prospects of spontaneous recovery in the group of patients concerned. The method which they devised for this procedure has as its essence the comparison of the actual number of shock-treated cases remaining on the hospital books at a given time, with the expected number, calculated from a random sample of the mental hospital population. They constructed tables to show the chance of a patient's still being on the hospital books at a given point of time in the future, when the age on first admission, sex, and length of time since admission are all taken into account. The tables were calculated from a random sample, composed of 8,016 case histories of mental

hospital patients.

This method of determining controls by means of calculating expected figures was tested by Penrose and Marr. They took, at one hospital, all the patients who had been recommended for shock therapy but who, for one reason or another, did not receive it. Here, the observed and expected numbers of patients agreed very closely in this untreated group. We can assume from this experiment, therefore, that the total expectations, as calculated by Penrose and Marr, tend to be fairly accurate.

On the whole, the figures of Penrose and Marr suggest that shock therapy has definite value in keeping a certain proportion of treated patients out of the hospital for periods of time extending over one or more years, the proportion amounting to something between 6% to 11% of treated cases. The efficiency-record of the shock therapies was found, in this study, to be lowest for schizophrenics. According to these investigators, out of over 1,000 cases of schizophrenia only 34 could be supposed to have been discharged in response to treatment.

Invaluable though such studies are in helping one to arrive at an adequate standard of comparison be-

tween treated and untreated cases of schizophrenia in large groups of patients, still this method of analysis falls short of answering adequately the question which cannot fail to be uppermost in the mind of the psychiatrist as he considers each new case of schizophrenia; namely, what results can he expect from the use of shock therapy in the particular patient at hand?

In this connection, I have found that the more one studies of medicine, the more one realizes how specious are the rigid categories and the convenient pigeon-holes which pervade the entire field. Systems of detailed classification tend to confer on medicine the apparent status of an exact science, while in actuality Nature recognizes no such man-made designations but presents before us an infinite variety of subtly differing forms. This is as true of psychiatry as it is of any other branch of medicine, and it is particularly evident in the disease known to us as schizophrenia.

I say "disease", since ~~schizophrenia~~ schizophrenia is usually alluded to in the singular; however, when more closely studied it becomes apparent that we should with greater accuracy refer to "the schizophrenias", since the term "schizophrenia" is really more a description

of various reaction types than a single disease entity. In addition to the four typical varieties of the disease -- catatonic, paranoid, simple, and hebephrenic -- the diagnosis of schizophrenia is very frequently conferred collectively on a number of quite atypical clinical pictures.

Langfeldt (29) speaks of typical cases as those in whom the illness is clearly of endogenous origin and who present no atypical or unusual symptoms, and he considers the atypical patients as those whose illness resembles a manic-depressive psychosis or was precipitated or influenced by exogenous factors, and those where the diagnosis of schizophrenia is doubtful. According to Lewis (28), these atypical or pseudoschizophrenic forms compose the majority of schizophrenic patients treated in some hospitals. Lewis concludes, and rightfully so, that herein lies one of the major causes of the differences in the reported clinical results, which are certainly more favorable in the pseudoschizophrenic forms than in the genuine nuclear types, since the atypical forms cannot serve to test the value of any method of treatment to be interpreted in terms of the whole category of schizophrenia. Obviously, the inclusion of large numbers of atypicals in statistical data on therapy in schizophrenia tends to produce "loaded" resultant figures.

In addition to the presence of a large number of atypical forms, there are innumerable other variables all of which have distinct bearing on the course of the disease and the final outcome for any given patient. We must take into account such factors as precipitating causes, age, sex, race, physical constitution, duration of illness, social and economic status, family constellation, heredity, and psychological factors. As Lewis (28) analyzes this phase of the problem, studies of the individual over a number of years, which include the pre-psychotic character, the intellectual status, the social and physical disease factors, the bodily form, and the onset and course of the psychosis, are necessary in order to obtain pertinent information on what varieties of reaction tend generally toward recovery, remission, and improvement, and those which tend in the direction of a poor prognosis regardless of the type of treatment applied.

Realizing the importance of adequate criteria with which to assess the prognosis of any given case of schizophrenia, Chase and Silverman (31) conducted a survey of the literature on the subject. They found that the prognosis is most favorable when the duration of illness is short, the type of onset acute, exogenic precipitating factors obvious, an element of confusion

present, and atypical symptoms (especially manic-depressive symptoms) prominent. Concerning the relationship of constitutional type to prognosis, they noted that the pyknic build conferred a more favorable outcome than the asthenic. Also, extroversion and an adequate pre-psychotic life adjustment tended more favorably than introversion and inadequacy of reaction to life.

In regard to the relationship of schizophrenic type to prognosis, these investigators found that, as might be expected, the acute atypical cases, unclassifiable, have an especially favorable prognosis, and that of the four nuclear types, the catatonic offers the best prospects, the next best being the simple and hebephrenic, with the paranoid being apparently the least favorable type. Sex, education, abilities, and psycho-sexual history were found to have no prognostic significance. Age of onset was likewise noted as an insignificant factor, except that a relatively late age tends to offer an unfavorable prognosis.

Correlating their results of insulin and metrazol therapy with the prognosis as determined by these prognostic criteria, Chase and Silverman reached the conclusion that when the prognosis is favorable, shock therapy is beneficial, but when it is poor, the

treatment is of little value. They found that shock therapy shortens the duration of illness in patients with a good prognosis, and, even more significant, that it may be the deciding factor in patients with a doubtful prognosis.

Katzenelbogen's findings (38) are quite in agreement with these conclusions. Writing on the efficacy of insulin therapy, he noted that the treatment gives the best results in two types of schizophrenic reactions -- schizophrenia in psychoneurotic individuals in whom psychogenic factors appear to be the immediate provocative agents of the psychosis; and schizophrenia with a large question mark, where the diagnosis is the subject of controversial opinions and remains uncertain to all concerned. Katzenelbogen concluded, as had Chase and Silverman, that the therapy only accelerates the favorable outcome in those schizophrenics in whom the ordinary hospital care would accomplish similar results.

Cheney and Clow (17), in conducting a similar type of investigation, reached substantially the same conclusions. In addition to making observations similar to those listed above, they noted that patients who were to improve usually showed evidence of such improvement relatively early in the course of treat-

ment, and that if the results did not appear early, a prolonged course of treatment rarely produced recovery.

In this connection, there is one prognostic point on which a general agreement has long since been reached. This is the factor of duration of illness. Almost without exception, every worker who has investigated the problem has found that best therapeutic results are obtained in cases with a short duration of illness. Ever since the original reports by Meduna (7) and Sakel (6), practically all workers have noted an inverse proportion between effectiveness of therapy and length of psychosis.

If such be the case, that is, if the shock therapies have no specific curative effect in schizophrenia but merely accelerate or facilitate improvement in those who have the constitutional or innate capacity for improvement, then a highly significant factor in the production of wide discrepancies among various reported recovery-rates becomes at once apparent. For how can one reasonably expect to find the same results in a series of hand-picked patients with good pre-treatment prognosis as in a group representing a random sampling of the schizophrenic

population? With this factor in mind, among others to be analyzed presently, we begin to make some sense out of an apparently hopelessly confused state of affairs in which the range of published percentages of cures runs anywhere from seventy-five down to zero.

In this regard, it is important to note that the division of cases into various grades of improvement ranks high in the list of the myriad stumbling-blocks which beset the path of anyone who attempts to evaluate the results of shock therapy in schizophrenia. The placing of a patient within the category of "recovered", "much improved", "improved", or "unimproved" is at best only a subjective evaluation and therefore liable to large personal errors, unless objective criteria for such designations are accepted and used by all investigators. In an attempt to bring order out of a chaotic state of affairs in which a comparison between the results of one investigator and those of another is difficult if not impossible, Ross and his co-workers (34) in 1941 suggested a set of objective criteria to be used by all investigators submitting reports on results of shock therapy.

According to these suggested criteria, cases are designated as follows (34): "To be classified as 'recovered' the patient must have become entirely

symptom-free and must have developed insight regarding his illness. By 'insight' is meant that the patient must fully realize that he has suffered a mental illness and that his symptoms were in fact a part of this illness. He must also be able and willing to speak of his illness in detail and objectively, and with normal affect, and he must be able to adjust well in the community at his pre-psychotic level.

"The term 'much improved' means that the patient is entirely symptom-free but that insight as defined above is lacking or incomplete, although he is able to adjust well in the community at or near his pre-psychotic level.

"The patient is considered 'improved' if his symptoms are incompletely alleviated but less distressing, so that he is able to make a definitely better adjustment than before treatment.

"'Unimproved' is applied to those patients who derive no benefit from the treatment."

Shortly following the publication of these criteria in 1941, one report (3c) remarked that Ross's system of classification had already been put into general practice. One is led, however, to question on what basis this statement was made, since another writer, in a general review of the shock therapies (28),

came out two years later with a plea for general acceptance of a standard set of classificational criteria in order to end the anarchic conditions which still continued to exist. It is difficult to determine, from the results reported by various investigators, exactly to what extent a common set of criteria has been used, since in practically no cases, with rare exceptions, e. g. (14, 19, 34), is there any statement made by the investigator as to his standards for the various grades of improvement.

In addition to condemning the lack of usage of a common set of therapeutic criteria, Ross and his co-workers (34) have generalized their charge by stating that "since the introduction of insulin shock therapy there has been little uniformity in anything connected with it", a charge with which, I am sure, the reader by this point will find little cause for disagreement. Ross and his group further condemn the fact that "different terms are used to describe the same condition, and different meanings are given to the same term. "Moreover," they state, "the technique has about as many variations as there are clinics using it."

Here again, then, we find new additions to the innumerable variables which, as I have previously

emphasized, seriously impede the possibility of reaching a fair and just verdict in an appraisal of the shock therapies in schizophrenia. That the technique of administration of the shock therapies is a potent factor in determining their efficacy is demonstrated with crystal clarity in the results obtained by Bond and Rivers (35) with the use of two different techniques. During the years 1936-1938 these workers used a "mild" type of insulin therapy. The principle at that time was to keep the insulin dose as low as possible and still get hypoglycemic stupor. When the stupor dose was reached, a reduction was made on the subsequent days if stupor level could be maintained. Convulsions were considered to be a sign of overdosage. In 1939 they completely changed their technique. No longer was the stupor dose considered optimum. Rather, they produced deeper and longer stupor by increasing the dosage, and they no longer considered convulsions as a contraindication to increasing the amount of insulin.

There were approximately the same number of patients treated by each of the two techniques. Of those treated by the old method, 44% were recovered or much improved at the end of treatment, as contrasted with a rate of 63% for those treated with the later method. At the end of the first year following termi-

nation of therapy, the figures were 34% and 57%, respectively.

Comparative studies such as this thus give clear indication of the fact that the mode of administration of the therapy does make a difference in the effects on the patient. As in the question of criteria for measuring degrees of improvement, there is nothing in most reports on therapeutic results to indicate the method of administration by which those results were obtained. One has no means of even surmising, then, as to the extent of the role played by differences of technique in the production of discrepancies among various reported therapeutic results.

Regarding the effect of the duration of therapy on the prognosis of the treated disease, Malzberg (14) found that the rate of recovery and improvement decreased as the duration of treatment increased. In other words, patients who respond favorably to insulin therapy tend to do so early in the course of treatment. Similarly, Gralnick (37) is so convinced of diminishing returns in an extended course of treatment that he sees no reason for continuing the therapy in patients who do not show definite improvement by the time they have had 25 to 30 treatments. In his experience, such patients do not become suffi-

ciently well to be paroled no matter how many further treatments they receive, and any improvement that does occur is fleeting in nature, according to him.

Katzenelbogen (38), however, could find no consistent relationship between the therapeutic results, on the one hand, and the number of treatments, on the other. He concluded therefore that "aside from the pharmacodynamic effect of insulin there must be other influences at work". He is of the opinion that insulin makes the patient more receptive to other types of therapy by establishing a better patient-physician rapport.

This view has been expressed by enough other workers to make one seriously question the specificity of the shock therapies in schizophrenia. Gralnick (37) is convinced that it is not merely the insulin-hypoglycemia that counts, but the specific insulin-treatment-situation which does. He considers that the fact that principally cases of short duration do so well is understandable only from this point of view. According to his view, these patients are close enough to reality to respond to the treatment-situation, whereas the others are so withdrawn and psychologically fixed that the drug can have little or no effect. One is led to doubt, however, whether this stand is entirely justified, in view of Ross's observation (40) that beneficial results from

the insulin treatment of all cases of schizophrenia, regardless of duration of the psychosis, are greater than the results in untreated groups. Ross found that although the recovery and improvement rates are inversely proportional to the duration of illness, still there are enough good results obtained in cases where the duration is over two years that it would be an error to neglect such cases.

It is evident, nonetheless, that the shock therapies are more effective if used as an adjunct to other psychiatric techniques than if relied upon as the sole therapeutic weapon. The importance of the use of the well-established conservative psychiatric techniques, particularly psychotherapy, is stressed by many different investigators. The general acceptance of this viewpoint is apparent in the findings of an excellent survey on the use of the shock therapies, prepared in 1942 by Kolb and Vogel (41). These investigators conducted a poll of 305 mental hospitals in the United States and reported that the majority of the hospitals polled placed great emphasis on the importance of the combination of shock therapy with psychotherapy.

This consensus is echoed in the words of one observer (42) who states that "shock therapy is

a means of temporarily improving the patient's mental state so that he is accessible, and by means of psychotherapy can be carried on to a wholesome mental state." The survey of Kolb and Vogel indicates that many hospitals are using the shock therapies as an adjunct not only to psychotherapeutic interviews with patients but also to the total psychotherapeutic approach, including programs of physical education, occupational therapy, hydrotherapy, physiotherapy, and the socialization program which is practiced in the better-managed psychiatric units.

One is thus led to the view that the shock therapies cannot be considered specific curative treatment in schizophrenia, that they are best used in conjunction with, and not to the exclusion of, other methods, and, as discussed previously in this chapter, that they merely accelerate or facilitate improvement in those who have the constitutional or innate capacity for improvement.

Granting all this, can one then conclude that shock treatment of schizophrenia has little more to offer than the use of more conservative measures? There have been certain observations which tend to indicate that this is far from being the case.

In the first place, regardless of the question of permanency of cure, any method of treatment which can bring about a significant reduction in the hospital population of schizophrenics is to be regarded as a therapeutic tool of no mean usefulness. That this has been accomplished is conclusively shown in a recent survey of 2,004 insulin-treated schizophrenics, conducted by Folks (43). According to this investigator's analysis, the average hospital stay of a schizophrenic patient with a duration of illness less than 18 months, is approximately 7 months without insulin-shock therapy, whereas with the therapy this is reduced to approximately 3 months.

In addition to this quantitative decrease in the length of hospital stay, there appear to be qualitative differences between shock-treated and non-shock-treated patients which tend to modify any mood of pessimism generated by such reports as that of Gottlieb and Fuston (19), who found no difference in recovery-rate between their insulin-treated and their control patients. Bond and Rivers, for example, in a long-term survey (36) published in 1944, noted that the quality of the remissions in their insulin-treated cases was of a much higher standard than in their control cases. Of course subjectivity necessarily plays

a large role in the assessment of qualitative differences in any medical question; however, Bond and Rivers formed their impressions not only from personal interviews with their patients but also from information contained in follow-up letters from the patients' physicians and relatives. That this difference in the quality of remissions is present from the onset of remission is evidenced by such reports as that of Strecker (44), who noted the phenomenon as early as 1938.

Thus far we have considered mainly the effectiveness of insulin shock therapy in producing recovery and improvement in schizophrenia. A question of even greater significance is, how well-sustained are the favorable results brought about by the therapy? In other words, what is its long-term value?

I have found that one of the most significant factors operating in the production of discordant estimates of the end results of shock therapy is the highly variable time allowed to elapse between the end of the therapy and the final examination of the patient. Unfortunately, relapses are not uncommon among shock-treated schizophrenics, and therefore short-term estimates of final quantitative results become misleading.

Horwitz, Blalock, and Harris (32), for example, found that 25% of their improved patients relapsed within several months to a year after the completion of insulin treatment, and at the time of their report (1938) they felt justified in assuming that still more relapses would subsequently be added to this already quite large percentage.

Such facts tend to lead one to discount analyses such as that of Malzberg (14), in which, reporting in 1938 on the outcome of insulin treatment of 1,039 patients, he gives a figure of 65.3% as the total of all insulin-treated cases classified as recovered, much improved, or improved, as contrasted with a figure of 23.4% in a similarly large group of controls. The difference, at first glance, is overwhelming; however, after one notes that the period of observation for the insulin patients was approximately one month while that of the controls was between one and two years, the difference obtained loses its significance.

I should add, however, in defense of Malzberg, that he admitted the impossibility of giving a final verdict on his insulin cases until after following them for perhaps five years after the close of treatment. Indeed, reporting on the same group of cases one year later, Ross and Malzberg (33) noted that 20%

of the original paroled group of insulin-treated patients had already relapsed.

Nevertheless, taking Malzberg's original data at face-value (and I should state that on analyzing his procedures I find him to be an extremely able and competent statistician), one can feel justified in assigning a high degree of immediate effectiveness to the treatment.

In view of the deceptiveness of results obtained, as reported immediately at the end of treatment, the need for adequate follow-up studies becomes at once apparent. I have mentioned above that an analysis of Malzberg's original cases one year after the completion of treatment showed a relapse rate of 20% within that year, thereby reducing to 45% the original figure of 65% for cases considered recovered, much improved, or improved. After these cases had been followed for two more years, thus making three years since the close of therapy, Ross, Malzberg, and their co-workers (34) found that this figure of 45%, for those showing some degree of improvement, remained unchanged. The conclusion was made, therefore, that the condition of the patients had become stabilized with respect to the possibility of any further deteri-

oration, and that about 45% showed some degree of lasting improvement. Of this number, 12.9% were considered full recoveries.

A similar much-needed follow-up study has been conducted by Bond and Rivers. These investigators noted (35) that in all such studies conducted at their hospital during the years before shock therapy, the recovery and much improved rate seemed to run consistently between 10% and 20% at the end of five years after admission. In the same report, Bond and Rivers noted an immediate recovery-much improved rate of 54% (3 to 5 times that of the controls) in a group of 251 cases treated with insulin, but they found that this figure had dropped, at the end of one year, to 43%. At that time, in 1941, they felt that if similar decreases were to occur in further follow-up periods, the rate at the end of five years might not be far different from the control cases. However, in a later report (18) they noted that the recovery rate tended to level off at about 33% in the second, third, and fourth years.

The latest study of these investigators (36) included a five-year follow-up on 49 cases; of these, 22, or 41%, had maintained their status. Bond and Rivers contrasted this figure with an approximate rate

of 15% on five-year cures among their controls. I find, however, quite a large fly in this ointment, for it is misleading to consider that any percentage of a group of 49 accurately indicates the status of an equal percentage of a group of 138 (the original number of patients constituting the immediate recovery rate of 54%). To contrast a rate of 41% (22 out of 49) with a rate of 15% on hundreds of controls is as unwarranted as would be the conclusion that since only 22 out of the original total of 251 cases showed a five-year cure, the percentage of such cure is only 22 out of 251, or 9%. This illustrates, of course, the great difficulty involved in conducting long-term follow-up studies and in interpreting the results obtained; namely, that the number of former patients available for inclusion within data becomes progressively smaller with the passing of each year, thereby causing a progressively increasing standard error and, consequently, a steadily decreasing statistical reliability.

Gralnick, in a recent seven-year survey (37), found that although 268 (50%) of a total of 554 insulin-treated patients were paroled, 317 (60%) were in the hospital at the time of the survey six years after the treatment. One can assume from these figures that the

other 40% represents sustained results.

Gottlieb and Huston (19), in a follow-up period of four years on 66 insulin-treated cases, found a four-year recovery rate of 35%. Similarly, Rennie (25), in a one to three year follow-up study on 70 schizophrenics, noted that the final percentage of favorable responses after this length of time (starting with a 55% immediate recovery rate) was 32.8. Also, Bateman and Michael, in a two to three year follow-up analysis (16) of 416 insulin-treated patients, recorded a final recovery rate of 31.2%.

The most comprehensive, detailed, and carefully prepared of all the follow-up studies on insulin shock therapy which I have been able to find, is an analysis published in 1944 by the New York Temporary Commission on State Hospital Problems (120). This is a one to five year follow-up report on 1,128 schizophrenics treated at the Brooklyn State Hospital. These patients were contrasted with 876 controls who did not receive any form of shock therapy but who were otherwise comparable, as to significant factors, to the insulin-treated group.

In gratifying agreement with the points stressed earlier in this chapter, the Commission found that not only did the insulin-treated patients have a consistently larger proportion able to leave the hospital

in each diagnostic group than the non-treated patients, but also that the hospitalization period prior to release was 3.8 months shorter per patient than among the non-treated. In addition, the insulin-treated group had a consistently larger proportion of patients who were at home the entire period from date of release to date of study, than the non-treated.

The Commission found, as had Ross, Malzberg, and their co-workers (34), that the majority of all patients who returned for further hospitalization, did so within a year after release, the number decreasing with the passage of time. At the end of the period of study, 58.9% of all the insulin-treated patients were at home, as against 44.0% of the non-treated group.

In contrast to Ross's system (34) for grading patients into the four categories of recovered, much improved, improved, and unimproved, the Commission judged their results by dividing their patients into seven "levels of usefulness". It was noted that there was a consistently larger proportion of insulin-treated patients in the higher levels of usefulness and non-treated patients in the lower levels.

According to the designation of the Commission, "Level 1" is described as "those patients who developed

beyond their pre-psychotic level and who at the time of study were getting along well and better than before their illness in their social and familial relationships. "Level 2" indicates "those patients who were doing well and at least as well as they did before their illness". These two categories together, then, correspond to "recoveries" in Ross's (34) system of classification. On combining the numbers of insulin-treated patients in these two levels, as listed by the Commission, I find that they represent 363 cases, or 32.2% of the total group treated.

The following table represents a summary of the long-term recoveries following insulin shock therapy, as noted in the follow-up studies which I have discussed above:

Report	Number of patients treated	Recoveries (Percent)	Recoveries (Number)
Gottlieb and Huston (19)	66	35.0%	23.1
Rennie (25)	70	32.8%	22.9
Bateman and Michael (16)	416	31.2%	129.8
Bond and Rivers (18)	251	33.0%	83.7
Gralnick (37)	554	40.0%	221.6
Ross et al. (34)	1,039	12.9%	134.0
New York Commission (120)	1,128	32.2%	363.0
Totals:	3,524		978.1

The total obtained, 978.1 long-term recoveries out of 3,524 insulin-treated patients, corresponds to 27.7%. It will be recalled that earlier in this chapter a figure of 12.8% was computed for the rate of long-term spontaneous recoveries (364 out of 2,833 non-shock-treated patients). Even adding to these figures the relatively more favorable number of spontaneous recoveries in the New York Commission report (193 out of 824 controls), the rate is increased to only 15.2% (557 out of 3,657).

In summary, then, the long-term recovery rate in a group of 3,524 insulin-treated patients is 27.7%, while this rate in a group of 3,657 controls is 15.2%. Is this difference significant, or is it one which can be ascribed to randomness? The answer to this question can be determined only by computing the standard error of the difference. The standard error of two proportions is equal to the square root of the sum of the squares of the standard errors of the two proportions. The standard error for the proportion of recoveries in the insulin-treated group is the square root of $P \frac{(1-P)}{N}$, where P equals the quotient of the total recoveries (both treated and controls) divided by the total number of patients subjected to analysis (both treated and controls), and where N equals the number of insulin-treated

cases. P then equals $\frac{978.1 + 557}{3524 + 3657} = \frac{1535.1}{7181.0} = 0.214$.

One minus P is then 0.786, and the standard error for the treated group becomes equal to the square root of (0.214 X 0.786 divided by 3,524); this is equal to the square root of 0.000048. Similar computations for the standard error of the untreated group yields, by dividing the product of 0.214 and 0.786 by 3,657, a figure equal to the square root of 0.000045. The standard error of the difference between two proportions, being equal to the square root of the sum of the squares of the standard errors of the two proportions, becomes the square root of (0.000048 + 0.000045). This square root comes out 0.0096, or 0.96%.

The observed difference between the insulin-treated group and the controls was 27.7% minus 15.2%, or 12.5%. In terms of a standard error of 0.96%, this difference of 12.5% represents 13.0 standard errors (12.5% divided by 0.96%). The upper limit of randomness of statistical data being generally considered to be 2.3 standard errors, it becomes immediately obvious that the difference observed between the two groups is highly significant. The chances of a difference equivalent to 13.0 standard errors being due to randomness are only one in ten to the thirtieth power.

On the basis of my calculations, I feel thoroughly justified in concluding that, observed from the long-term viewpoint, the probability is extremely high that the results of the insulin treatment of schizophrenia are quantitatively almost twice as good as those in non-shock-treated cases.

Of course I realize that the figures I have used represent only a fraction of the total number of schizophrenics who have been treated with insulin (the survey of Kolb and Vogel (41) showed that 23,651 patients had received the treatment up to the end of 1941). However, in statistical analyses one must necessarily derive clues from samples, since the total supply is practically never available for consideration. And in the data which I have presented, the number of patients considered is certainly large enough to nullify sampling errors. I feel that the method I have used is certainly more reliable for the formation of quantitative conclusions than is the custom, in some reports I have read, of discrediting insulin shock therapy merely on the basis of finding isolated instances in which the final percentages of cures, in a handful of treated cases, are equal to the recovery rates observed in an equally small number of controls.

On the basis of the evidence which I have presented in this chapter, the case for and against insulin shock therapy in schizophrenia can be summarized as follows:

Although the treatment has not lived up to the high expectations generated by the over-enthusiastic initial reports of its value, still it has proved itself a therapeutic weapon capable of producing nearly twice as many sustained recoveries as occur in non-shock-treated cases. The benefits to be derived vary from one patient to another and can be expected to accrue, in any given case, in direct proportion to the prognosis of that case if left untreated, and to the duration of the psychosis previous to the commencement of therapy. Insulin-shock therapy is valuable in facilitating and accelerating recovery, thereby shortening the period of hospitalization. The treatment, when effective, is only one step in the process of rehabilitating the schizophrenic; the best final results are produced when insulin is used in conjunction with other forms of treatment, particularly psychotherapy.

Having analyzed the record of the hypoglycemic treatment, let us now proceed to a consideration of the role of convulsive shock therapy in schizophrenia.

At the outset, it should be stated that though the term "convulsive shock therapy" applies both metrazol and electroshock, the former is fast becoming a matter of historical interest only, its place in the field having now been taken over, in the great majority of hospitals, by the latter. (Incidentally, it is interesting to note the similarity of the trend away from chemical and toward physical methods both in shock therapy and in fever therapy, the latter being now accomplished by the electrically-controlled fever cabinet rather than, as formerly, by the injection of foreign proteins.)

There is good reason for the rapid replacement of metrazol by electroshock, in view of the marked advantages of the latter over the former. The advantages have been summarized (53, 55) as follows: Electroshock produces an unconsciousness and amnesia for the treatment, thereby lessening fear of treatment and assuring the patient a greater degree of mental and physical comfort; there is a much lessened degree of psychomotor agitation following the treatment, patients being usually

quiet and drowsy at that time; the difficulty with inaccessible and thrombosed veins that occurs with metrazol does not exist with electroshock; and, finally, a larger number of treatments can be given with a minimum of time and personnel.

Of course, the main criterion with which to decide to supplant one method of treatment by another is not the administrative advantages of the new method but its therapeutic efficiency. For any new therapeutic technique must be at least as successful as the one whose use it displaces before it can be considered as a substitute. Fortunately, this has been found to be the case with electroshock. In 1939, shortly after the method had first been put to clinical use, Kalinowsky (45), although admitting that the number of cases treated at that time was too small to allow definite conclusions to be drawn, reported that the number of recovered and improved cases of schizophrenia, treated by electroshock, corresponded at least to the number of similar cases whom he had treated with metrazol.

In an attempt to determine whether there are any long-term differences between the results obtained by the two methods, Pacella and Barrera recently conducted a follow-up study (46) on two groups of patients, one group treated with metrazol, the other with electro-

shock. They found that the therapeutic effects of both methods are essentially the same. These findings were confirmed in a similar and simultaneous study by Reznikoff (47).

Shortly after the demonstration of the distinct advantages of electroshock over metrazol, the former became recognized as the treatment of choice in convulsive shock therapy and has therefore enjoyed a rapid general acceptance, as is evidenced in the large hospital-poll conducted by Kolb and Vogel (41).

Although metrazol is almost a dead issue today, the similarity of its results to those of electroshock justify its consideration in an appraisal of convulsive shock therapy, since for approximately five years it was the only generally used method of administration of this type of treatment.

In attempting to evaluate the role of convulsive shock therapy in schizophrenia, one finds, as might well be expected, the same host of variables which contribute to the difficulty and complexity of the task of formulating a just assessment of hypoglycemic therapy. Also, as in the case of the latter method, it is clearly evident that convulsive shock therapy cannot be regarded as a substitute for con-

servative methods of treatment but must be thought of as a useful synergistic adjunct to such methods.

This fact has long been recognized by the better quality of investigators. Meduna himself, even in the early days of enthusiastic reception of his metrazol treatment, called attention to the fallacy of relying solely on any pharmacologic method.

"I should like to mention", he warned (7), "that this treatment of schizophrenia cannot effect a complete cure. For schizophrenia presents a psychic disorder based on a patho-physiological foundation, and, therefore, we must not only influence the biological patterns but must also seek to help the patient along psychological lines. It seems to me superfluous to emphasize that the treatment of schizophrenia can never be successful with mere medical treatment like an internal ailment, much as I am convinced of the purely material nature of this disease.

"The significance of the psychiatric treatment seems to me to be so far beyond doubt that I consider it unnecessary to look for justification. The fact is that those who have tested my treatment have not achieved my 50-60% success, but have had only 36-60%. The same authors, however, in testing Sakel's method, had an even lower percentage of re-

missions; indeed, with both methods there was the same difference of 20-30% between them and the original authors. This difference I ascribe to their neglect of the psychiatric treatment."

Whether Meduna was correct in assuming this factor to be the only one operating in the production of the discrepancies between his results and those of subsequent workers, it is difficult to surmise, in view of the many other variables which must be taken into account when comparing one case of schizophrenia with another, as I have previously emphasized. The fact remains, however, that Meduna did produce a spectacularly high degree of success in his early cases. In a series (11) of 36 patients with duration of illness not longer than six months, he obtained remissions in 33 -- over 91%. His remission rate in cases with duration of illness up to one and a half years was 84%. Up to two years it was still quite high -- 78%. It was considerably lower but still remained on a relatively high level in cases in which the length of illness was between two and five years 35%.

In contrast to this shining record, one can cite examples which show the opposite side of the picture. Ross (40), for instance, in contrasting his

results with metrazol on 523 patients with results on 1,039 untreated controls, found an equal recovery-much improved rate in both groups -- 14%. It is obvious then that matching one report against another will get us nowhere in our search for an evaluation of convulsive shock therapy in schizophrenia. Because of the great variety of assignable causes for discordance in results, which I have analyzed above, the ideal method of attacking the problem would be to compare individual cases with each other. The closest that most investigators come in this regard is to break down their data into groups corresponding to duration of illness. Here, at least, we can note a gratifying degree of uniformity; for, almost without exception, there is general agreement that, as in the case of hypoglycemic therapy, the results obtainable by convulsive treatment are inversely proportional to the duration of illness at the beginning of treatment.

The consensus on this point is evidenced in a statistical compilation of results from metrazol in 3,000 cases, prepared in 1939 by Meduna and Friedman (48). They calculated a mean remission-rate of 52% for patients whose illness had lasted less than one and one-half years, with great improvement noted in an additional 20%. In cases lasting longer than this length of time, the mean

remission-rate was found to be 10%, with much improvement obtained in an additional 37%. In performing a similar analysis on 2,000 metrazol-treated cases, Reitman (49), reporting in the same year, found an average rate of remission, for cases of less than eighteen months' duration, exactly equal to that noted by Meduna and Friedman; namely, 52%.

These figures, taken at face-value, would lead one to conclude that the chances of effecting a cure in any recent case of schizophrenia are approximately one out of two. Although they do indicate that results obtainable are inversely proportional to the duration of illness, on closer examination, however, I find good reason to take reports such as the ones just listed with a grain of salt, because of the wide range of percentages entering into the calculation of the above means.

It is common to hear the unthinking remark made that "statistics can prove anything", whereas in reality statistics "prove" nothing by themselves; neither are they a substitute for common sense. What statistics do accomplish is to describe a selection of individuals from a large supply, and to present the type, spread, skew, proportions, and relationships of these individuals. It is only by taking into considera-

tion all of these descriptive details that one can justifiably make inferences from the data collected.

In the case under discussion, a mean recovery rate of 52% means little, standing alone. This can easily be illustrated by the following example: Let us suppose that there were a total of three hospitals reporting recovery rates of, let us say, 50%, 52%, and 54%, respectively. The mean rate would be 52%. On the other hand, let us assume that the reported rates were 12%, 54%, and 90%, respectively. The mean rate would still be 52%. In the first case, we could infer, assuming that a large enough number of other hospitals gave similarly concordant figures, that any schizophrenic whose illness is of a duration less than eighteen months stands a 52% chance of being cured by convulsive shock therapy. In the second case, we could infer nothing as to the effectiveness of the therapy. Rather, we would be led to conclude that there is a serious lack of uniformity in the selection of cases, the criteria of recovery, the amount of psychotherapy practiced, the technique used, the number of treatments given, or all of these factors combined.

That such lack of uniformity does exist is obvious in the wide range of recovery rates reported for convulsive shock therapy. In Reitmann's report (49),

(the one in which he gave a mean recovery rate of 52%), the percentages of remissions as reported from different hospitals varied from 7% to 100%. Similarly, Kennedy (50) noted, in 1939, that not only were there great variations in the remission rate from year to year, but that the total rates from different countries ranged from 79% in Italy to 39% in Germany.

Selection of cases is undoubtedly one of the most significant of the factors causing these statistical discrepancies. The data available in which the case-material is classified into types, are scanty indeed. Because of this fact, about the only way of forming an opinion as to the relative efficacy of convulsive shock therapy among the various types of schizophrenia is to take a poll of a large number of hospitals as to their indications for the use of the therapy. This was done, as I have mentioned above, by Kolb and Vogel (41), who noted that most of the institutions reporting placed the catatonic type high on their lists of indications for electroshock and metrazol, with the paranoid and hebephrenic types at the bottom of the list. This is in distinct contrast to the indications for insulin, as noted by these hospitals; here, all these three types stood at the top of the list of indications, with only

the simple type at the bottom.

This being the case, it is possible to single out the cause for almost consistently low percentages of results with convulsive shock therapy, emanating from state institutions (12, 16, 33, 40, 51), as contrasted with the generally better results reported by private psychiatric units and general hospitals. For, according to Kennedy (50), paranoid cases, for legal reasons, are more often sent to mental hospitals than to psychiatric clinics. Thus there appears to be one definitely assignable cause for wide discrepancies in results obtained.

While on the subject of evaluating convulsive shock therapy as to types of schizophrenia benefited therefrom, it should be stressed at this point that there seems to be general agreement that where the manifestations of the disease are essentially psychomotor or where there are well-marked affective components, the response to convulsive shock therapy is definitely favorable (2, 47, 50, 52, 53).

As might well be anticipated, the experience of electroshock in the hands of various investigators has repeated that of metrazol; namely, spectacular results by the originators of the method, followed by

an enormous range of recovery rates obtained by later workers. Cerletti, one of the originators of electroshock, reported 80% complete recoveries in his early cases, where the duration of illness was less than six months (54). A sampling of other reports gives an indication of the disconcertingly wide range of results obtained:

Impastato and Almansi (55)	70%
Kalinowsky and Worthing (24)	67%
Gonda (1)	59%
Neymann et al. (23)	47%
Hemphill (21)	4%
Smith et al. (22)	0%

The causes for these discrepancies can be considered to be the same as those outlined under the discussion of metrazol, since, as I have already pointed out, the two methods can be grouped together when considering the question of convulsive shock therapy. Some of the workers who have obtained high percentages of good responses with electroshock are convinced that the reason why the method has not shown equal effectiveness in other hands is that many workers have used an insufficient number of treatments on their patients. Emphasis on this point was early laid by Cerletti. "In the discovery of new therapies", he remarked (cited in (56)), "the salient point is not the type of treatment applied but the courage of the

therapist in repeating the application again and again."

This view has been stressed more recently by Impastato and Almansi (55) and by Kalinowsky (56), who obtained excellent results in the electroshock treatment of schizophrenia. Kalinowsky's stand is that since there is agreement on the necessity of production of a long series of comas in insulin therapy, the same need holds good for electric convulsive treatment. He admits that the temptation toward abbreviation of the treatment is greater in the case of electroshock, since most schizophrenic patients with a reasonable chance of improvement become temporarily free from symptoms after a few electroshocks. Such a procedure, according to Kalinowsky, accounts for the frequent statement that schizophrenic patients treated by means of convulsions usually relapse.

That the number of shocks given does represent a significant factor is indicated by a study of the literature on metrazol, conducted in 1941 by Zeifert (57). His analysis revealed that results comparable to those in favorable reports on insulin therapy were obtained only by workers who gave twenty to thirty metrazol treatments, even in those cases in which early improvement was obtained.

Kalinowsky and Worthing (24) gave ten extra treatments to patients who did not maintain improvement after twenty electroshocks and found lasting remissions to be the result in many of these cases. Because of the beneficial effects of the added number of shocks, Kalinowsky's present practice is as follows (56): After twenty convulsions he keeps the patient under observation for at least three weeks. An unsatisfactory result is usually apparent within two weeks, when the confusion clears up and residual symptoms become recognizable; in this event, ten more treatments are given. If no definite improvement is noticeable at any time during the period of the first twenty convulsions, he considers that further treatment will not change the situation, and he therefore discontinues the course. It is Kalinowsky's belief that prolonged application of electroshock therapy is useless for patients who do not give an early response, but he is convinced that it is imperative for patients who have shown the possibility of a remission by favorable initial improvement.

Neymann and his co-workers (23) have not only echoed Kalinowsky's views but have gone even farther, recommending up to 45 or 50 shocks. They suggest that the patient should be treated until he is thoroughly confused; then he should be rested and examined as to

insight, after orientation is re-established. If there is then no insight, they suggest giving the patient another series of treatments. If necessary, they give a third series.

Whether such heroic therapy is always justifiable, it is difficult to say, since at the present time we do not know to what extent the post-treatment state of confusion is concerned in the mechanism of recovery. The fact remains, however, that 47% of the patients in Neymann's series of 90 electroshock-treated schizophrenics were maintaining recovery twenty months after treatment, when a follow-up study was done (23). Even more remarkable, Neymann and his group found that the recovery rate was greatest in their paranoid patients; the majority of other investigators, as I have mentioned previously, have noted a very poor response among the paranoids. A high rate of recovery for patients of this diagnostic type, similar to the findings of Neymann and his group, was obtained by Impastato and Almansi (55), who, like Neymann and like Kalinowsky, also used a large number of treatments.

The excellent results obtained by the use of this intensive type of therapy appear to me as a highly significant clue for further investigation.

If other workers in the future will be able to confirm the above findings by using intensive therapy on all patients except those incapable of responding to any shock therapy, then it is not too unreasonable to assume that electroshock may one day become the treatment of choice for all forms of treatable schizophrenia, just as it is today, as we shall see in the next chapter, for the affective psychoses.

At the present time there are hardly any means of determining how extensively intensive electroshock therapy is being applied. Kalinowsky (56) discounts the significance of the average number of treatments indicated in some statistical reports because, according to him, they usually include high values for patients with hopeless, chronic illness who received a long course of treatments, so that the average number of applications for the whole series was raised. On the other hand, he charges, treatment of patients with a good response, who should have had a long course of therapy, was discontinued after a few sessions, when they were temporarily free of symptoms, and these patients generally had a relapse.

In addition to these points, I have found that some workers become too easily discouraged in the electroshock treatment of recoverable cases,

Hemphill (21), for example, expected his patients to show some improvement before the fifth convulsion in order to be classified as recoverable. There is little cause for wonder, then, that he succeeded in producing only four recoveries out of a total of 114 schizophrenics.

While on the subject of intensive therapy, it should be mentioned that a large factor in the production of discrepancies in results can probably be assigned to the differences between investigators in the technique of the individual convulsion; i. e., the question of grand mal versus petit mal seizures. Because of the fear of damage to the patient by grand mal convulsions (a question which will be taken up in a later chapter), some workers have used a subconvulsive dose of current in electroshock therapy. By now, however, most writers are agreed that grand mal convulsions are essential in order to produce good results. Androp (78), for example, in contrasting the results obtained in schizophrenia on two different groups of patients, one group treated with convulsive doses, the other with subconvulsive, found a 14% higher improvement rate with the former technique. It is quite possible, then, that the use of the subconvulsive method by some workers has contributed in part to the

reporting of unfavorable results.

In view of the fact that the amount of perseverance exhibited by most investigators in the convulsive shock treatment of schizophrenia is a matter for surmial, not to mention the fact that many workers do not even take the trouble to classify their cases by types in their reports, there is little value in accepting data on cases treated by both convulsive shock and insulin shock simultaneously or in series.

A combination of the two methods, or the use of one method after failure with the other, has been practiced by various workers since the early days of insulin and metrazol. Meduna, in 1938, expressed his opinion on this point (7):

"Special attention is due to those cases who do not respond to one method of treatment but are cured by the other. This very important finding seems to me to show clearly not only that the mechanism of the cure involved in the two methods is different but also that at least two forms of schizophrenia, with different pathological mechanisms, must exist. Hence the importance of the fact that two different therapeutic methods are at our disposal. Cases that show resistance to one of the two methods but later

respond to the other, I call 'crossed cases'. I am fully convinced that endocrine studies of these crossed forms will lift the veil that still hangs over the nature of schizophrenia. For both methods, a knowledge of the nature of the biological events going on in the organism during the treatment, and their chronological sequence, is of the greatest importance."

There have been many reports of such "crossed cases", and many different methods of combining or alternating the two methods have been used. Taylor (27), for example, has recently reported that he gives insulin to all types of functional psychoses and neuroses. If after twenty insulin treatments the patient does not show decided improvement, then electroshock is added to the treatment. Weil and Moriarty (20), on the other hand, prefer to start with electroshock in all cases of schizophrenia, since this method alone may be sufficient, and, according to them, it seems to have a time-saving effect if a subsequent course of insulin treatment is necessary. Goldstein, Dombrowski, and Edlin (58) reversed this procedure by administering metrazol to patients who remained unbenefited with insulin. Ewen (59) combined electroshock and insulin from the start on a group of his patients.

Notkin and his co-workers (26), in reporting

on combined treatment, indicated that their rate of improvement was considerably greater than they had previously obtained with convulsive shock alone. They also analyzed their cases according to type and found that, in the improved group treated with combined hypoglycemic and convulsive therapy, the paranoid and the catatonic types were equally represented. Ewen (59) found that his paranoids fared better than either his catatonics or his hebephrenics.

These observations are interesting in view of the general experience that paranoids as a group are resistant to convulsive shock therapy alone. Taylor's experiences in this regard are more in line with this general observation. In conducting a five to seven year follow-up study (39) on a group of his patients who had all been treated by the combined method, he noted that not only did the simple, catatonic, hebephrenic, and mixed types respond better to the treatment than did the paranoids but also that the paranoids had the highest rate of relapse.

Of Taylor's original group of 214 treated by insulin and metrazol combined, there was a recovery rate of 71.5% at the end of treatment. Today, following relapses during the five to seven year period, this figure has been reduced to 58.6%.

One is led to wonder, judging from the favorable results obtained by the proponents of intensive convulsive therapy, if Notkin's results, outlined on the previous page, might not have been equally as good had he used a prolonged course of convulsive shock alone. Although he found improvement in 37% of his combined-treatment cases, he reported none of them as recovered. Is one to assume from these results that the combined-treatment is impossible of producing recoveries? Taylor's series indicates otherwise. Since 83% of Notkin's treated patients had had a psychosis for over eighteen months, is there the possibility that these really are mere improvements and not full recoveries? Or, would a case whom Notkin considers improved be classified by other workers as recovered? Here again, one sees the crying need for the general adoption and use of a definite set of criteria for grades of recovery.

In summary, the role of convulsive shock therapy in the treatment of schizophrenia can be stated as follows:

Thus far convulsive shock therapy has shown itself to be of greatest value in the treatment of the catatonic type of schizophrenia and in cases where there

is an affective component in the clinical picture. In all other types, more favorable results have been produced by hypoglycemic shock therapy. This division of indications is to be regarded, however, as only a very rough guide. Many patients respond to a combination of both methods where the use of one or the other had previously failed. It is possible that in the future further investigation will substantiate the claims of some workers that failures with convulsive shock therapy are due not to the therapy itself but to ineffective, half-hearted application of it, since all those who have subjected their schizophrenics to prolonged, intensive courses of treatments, have obtained, in recoverable cases, more gratifying results than those who have not used this technique.

In the absence of an adequate number of follow-up studies covering significantly large groups of patients, it is impossible to determine statistically the long-term effectiveness of convulsive shock therapy in schizophrenia. This being the case, possibly the wise thing to do is to accept the consoling thought expressed by Bond and Rivers (36): "Perhaps in the final analysis the years of health given to these cases are more important for the individual patients involved, the family groups in which they are functioning, and therefore for society in general,

than are final statistics concerning the number recovered or relapsed at the end of an arbitrarily selected time period."

Chapter III

The Shock Therapies in the Affective Psychoses

In the early years of the shock therapy era, the focus of attention was directed practically entirely on schizophrenia, since this was the disease which had occupied the efforts of the originators of the hypoglycemic and convulsive methods. It was not until 1938 that evidence began to appear concerning the use of shock therapy in the affective psychoses.

In that year Cook and Ogden (62) were impressed with the favorable results in cases of schizophrenia which showed depressive features, as well as in a small series of cases of clear-cut affective disorders. In the same year, Low and his co-workers (63) reported the effective use of metrazol in a series of manic-depressive patients. They noted that despite a duration of illness of more than two years in six of their patients, five of these recovered. The most gratifying of these early reports was that of Bennett (64), who, also in 1938, reported the termination of severe depressions in 21 patients, by the use of an

average of 5 metrazol convulsions.

Since the publication of these initial reports, impressive confirmation of the efficiency of convulsive shock therapy in the affective psychoses has come from a host of investigators. The high degree of uniformity of good results is evident in the following examples of reported recovery rates:

Young and Young (65)	95%
Cottington and Gavigan	85%
(66) Bennett (67)	90%
Ebaugh and Johnson (68)	87%
Wilson (69)	76%
Cronick (70)	93%
Cummins (71)	91%
Ziskind et al. (72)	89%

Because of the success of convulsive shock therapy in the affective psychoses, and since this method is a much simpler procedure than is insulin shock therapy, convulsive shock has long been regarded as the method of choice in treating the affective psychoses.

In the pre-shock era, all forms of chemical and endocrine methods had been tried in the treatment of the affective psychoses, including hematoporphyrin, estrogenic, testicular, or pituitary hormones, and narcosis; even fever therapy had been used. In none of these had any consistent effect in shortening the course of the psychosis been observed (64).

A clear-cut indication of the value of shock therapy, as contrasted with the ineffectiveness of these methods of the pre-shock years, is obvious in a study conducted by Bennett and Wilbur (74), in which 64 patients with involuntional psychoses, who had previously received varying amounts of estrogenic hormones without benefit, were treated with convulsive shock therapy and psychotherapy; 90% of this group showed social or full recovery in 4 to 6 weeks. Since some patients in this group had been ill for more than a decade, it could be said that they served as their own controls.

There have been some, however, who have minimized the value of the therapy on the grounds that the affective psychoses represent essentially benign conditions which have a generally favorable prognosis regardless of treatment. Kennedy, for instance, states (50), "Convulsive therapy is unlikely to have any specific effect on the affective psychoses but is able sometimes to determine a favorable change of mood in cases where such a change may be expected to occur sooner or later".

That such a viewpoint is ill-considered is indicated by the results of studies in which patients having affective psychoses and treated with shock have

been matched against a similar group of non-treated controls. Tillotson and Sulzbach (73), for example, in conducting this type of study in 1945, found that shock treatment not only accelerates remissions but has an independent value. They performed an 18 to 45 month follow-up observation on a group of 70 electroshock-treated patients and 68 controls and found that 80% improved under shock therapy, as against only 50% for the control group. Furthermore, four of their patients who had been hospitalized with unrelenting depressions for five to fifteen years recovered fully in three weeks to four months after the beginning of shock treatment and had maintained their recovery at optimal level for at least two years and three months. As in the report of Bennett and Wilbur (74), these patients served as their own controls.

In addition, speaking qualitatively rather than quantitatively, Tillotson and Sulzbach claim that some of their shock-treated patients display a far more efficient intellectual as well as emotional adaptability to their environment than ever before in their lives.

By using the statistical technique of computing the standard error of the difference between two proportions (see page 49), I calculate that the chances of the quantitative difference reported by Tillotson and Sulzbach being due to randomness are of the order of 1 in 5,000. Over and above this significant increase of recoveries among shock-treated affective psychotics as contrasted with controls, the efficiency of the therapy in shortening the length of hospitalization is, as we found it to be in the case of schizophrenia, a matter which can not be lightly dismissed in considering the value of the treatment. Tillotson and Sulzbach found that the average length of hospitalization in their control group was 21 months; in their treated group, the average length of hospital residence as computed from the time of the first treatment was only 1 month and 26 days. This difference is, of course, tremendous, amounting to a 75% to 90% shortening of hospitalization by the use of shock therapy.

Similarly, Bennett (53), in reviewing cases of recurrent types of manic-depressive psychoses, found that the average duration of illness, including hospitalization, in the years before the onset of the shock therapy era, was six months, as contrasted with an ave-

rage duration of two months in cases treated with shock therapy.

After the introduction of electroshock, this form of therapy, because of its obvious advantages over metrazol, as pointed out in the previous chapter, began to be regarded as the method of choice in the treatment of the affective disorders. The rapid acceptance of electroshock therapy by the profession is indicated by the fact that whereas at the end of 1939, only 3 of the 305 hospitals in the survey of Kolb and Vogel (41) were using this method, it was reported as being used two years later by 256 hospitals in the treatment of manic-depressive psychoses and by 216 in the treatment of the involuntional states.

A survey (75) of the results of electroshock therapy appearing in the literature up to 1942 gives evidence of the effectiveness of the treatment in the affective psychoses. A combination of the findings of 28 different authors showed that out of 158 involuntionals treated, 69% recovered and 20.2% improved and that out of a total of 596 manic-depressives, these rates were 59.5% and 27.8%, respectively.

Although surveys such as this one are useful in giving an over-all picture of the affective psychoses

as treated by shock therapy, one must look farther to find variations in effectiveness of the treatment among the various forms of these psychoses.

In the survey of Kolb and Vogel (41), it was found that, in the majority of hospitals reporting on their specific diagnostic indications for the use of electroshock therapy, involuntional psychoses topped the list; the other affective disorders followed closely in their list of indications, the manic-depressive de-pressed state being second, and manic-depressive manic fourth (third place was held by catatonic schizophrenia).

In line with this order of indications, many investigators have found that manics usually require a more intensive therapy than do depressives. Kalinowsky (56), for example, (who, it will be recalled, is one of the leading advocates of intensive electroshock therapy for schizophrenia), noted that the usual number of 8 or 10 treatments given for depressions is not sufficient to maintain improvement in manic patients. The necessity for the induction of 20, or even more, convulsions in some manic patients led him to apply more intensive treatment by means of two or three convulsions daily. By the use of this technique he found it possible to break up severe manic excitements by a course of five or six convulsions, instead of the

large number which he had used previously. He also found that patients with cyclic states, with constant shifting from one phase to the other, did not benefit from the therapy.

A difference between subtypes of psychosis even more pronounced than between the manic and the depressed manic-depressives is noticeable when analyzing the subdivisions of involuntional psychoses. Kalinowsky found that although the recovery rate for patients with involuntional melancholia was 86.9%, it was only 43.7% for those with the paranoid type of involuntional psychosis. The difference in the number of treatments required was similar to the difference between depressives and manics. Involuntional melancholiacs usually received 8 treatments, whereas patients with the paranoid type usually required at least 20 convulsions.

Another point of difference is notable in considering the effect of the duration of the disease on the final therapeutic outcome. It will be recalled that in the early report of Low and his coworkers (63), recovery was obtained in manic-depressives whose duration of illness had been greater than two years. Similarly, Tillotson and Sulzbach (73) found, in analyzing the prognostic factors in the treatment, that the chronicity of illness had no bearing on the recovery

of their cases; also, that the role played by previous attacks was negligible. As previously mentioned, four of their patients, who had been hospitalized with depressions lasting five to fifteen years, made a complete and apparently permanent recovery in three weeks to four months after the first shock treatment. In Bennett's

original series of affective psychotics treated by convulsive shock (67), the range of duration of illness for recovered patients was 1 week to 6 years for the depressed manic-depressives and 1 week to 3 years for the involuntional melancholiacs. Even more noteworthy, recoveries have taken place in involuntional melancholiacs whose duration of illness was as long as 12 years, as reported in the series of Bennett and Wilbur (74).

Thus we find that, in gratifying contrast with the question of treating schizophrenia, the factor of duration of illness in the manic-depressive psychoses and in involuntional melancholia has no bearing on the outcome of the treated disease. It was noted by Kalinowsky (56), however, that the outcome in involuntional paranoids is, on the other hand, largely dependent on this factor. It will be recalled that in the discussion of schizophrenia the point was noted that those schizophrenics in whom there is an affective component in the clinical picture respond better to

therapy than do those who are largely devoid of affect. It is interesting to compare this with the observation that involuntional psychotics of the paranoid type are comparatively unresponsive to treatment and, in contrast to involuntional melancholiacs, depend for their prognosis upon the duration of illness.

These facts tend to indicate that the prognosis of a given psychosis, treated with shock therapy, is directly proportional to the ratio of affective to schizophrenic components in the particular patient in question.

Concerning other prognostic factors in the shock treatment of the affective disorders, Tillotson and Sulzbach (73) found that the balance of assets and liabilities in the make-up of the pre-psychotic personality is usually a favorable one in cases with good response to therapy. However, according to them, the correlation coefficient between favorable pre-psychotic personality and favorable response to therapy, while being on the positive side of zero, is not great enough to be statistically significant.

Another prognostic factor to be taken into consideration is the number of treatments required. Tillotson and Sulzbach note that this number does show

an inverse relationship to the eventual degree of improvement. Thus, improvement in their cases usually became noticeable after as few as two treatments in patients with eventual recovery. If no beneficial results were evident with 8 or 10 treatments, invariably no therapeutic gain was achieved by an additional number of treatments, and relapses from transitory improvement failed to show better results on resumption of treatments; in fact, according to them, subsequent improvements were usually of less degree.

Although the observations given by these authors relate to the affective psychoses in general, it will be recalled that, despite the fact that Kahlinsky (56) found it necessary to administer more treatments to his manics than to his depressives, and more to his involuntional paranoids than to his involuntional melancholiacs, still he noted the possibility of obtaining recoveries in these more resistant types after the application of approximately 20 shocks. All of which tends to indicate that a therapeutic method which results in failure in some hands is successful in others, and that one can not rely too heavily on the results obtained by any one group of investigators alone.

As in the case of schizophrenia, the majority

of investigators lay great stress on the desirability, or rather the necessity, of using shock therapy as an adjunct to psychotherapy in the treatment of the affective psychoses. As one writer has stated (77), "A grave injustice is done to any patient who does not receive intensive psychological assistance when-ever possible. By intensive, I do not mean one or two superficial interviews but a persistent investigation of those factors in the functioning of the personality which led to the mental breakdown. This therapeutic co-ordination assures a healthier individual, less likely to break down again in a critical situation". As another author (1) puts it, "It is not overstating the case to say that intensive psycho-therapy is almost as important as the elicitation of the convulsion itself."

There are some workers, e. g. (42), who go even farther in this regard and consider the therapeutic function of shock therapy as being only a means of rendering the patient responsive to psychotherapy, which, according to them, accomplishes the real cure.

The therapeutic relationship between shock therapy and psychotherapy appears to me, from these reports, to be analogous to the relationship between opsonins and leucocytes in the body-defenses. The

manner in which the two forms of therapy are inter-related has been analyzed by Levy and Grinker (81). In discussing depressions in particular, they state, "The major obstacle to psychotherapy of depressed patients is the strong repression of all but the self-punishing tendencies. The ego, which customarily cooperates with the psychiatrist in the task of under-stating and modifying the emotional conflicts, is usually overwhelmed and subjugated by the punishing super-ego. The inhibited, retarded, depressed or the depressed, agitated patient, completely pre-occupied with self-punishing fantasies, presents almost impenetrable resistance against psychotherapy. Experience with shock therapy shows that these resistances and repressions can be sufficiently influenced to permit release of repressed feeling and impulses into motor activity, verbally expressed thoughts and feelings, or fantasies and dreams. In some cases this may even lead to conscious insight into deeply repressed emotional conflicts, a discussion of which may bring about significant changes in certain basic emotional attitudes."

Not all are agreed, however, that psychotherapy is indispensable. Kalinowsky, for example, omitted psychotherapy in 200 patients with affective psychoses and yet obtained an 86.6% recovery-much improved rate

with electroshock therapy. Nevertheless, regardless of these results, the consensus is that psychotherapy following a course of shock treatment is the most effective means of maintaining recovery and of assuring the patient a better adjustment than can be attained by the use of shock alone.

Another problem concerning technique of treatment which has been largely settled by now is the question of grand mal versus petit mal reactions. As in the case of schizophrenia, noted in the previous chapter, it has been shown that grand mal seizures are essential for the production of good results in treating the affective psychoses. Gottesfeld and his co-workers (79), for example, obtained an improvement rate of zero on using subconvulsive electroshock on a series of such patients. When repeating the treatment with convulsive doses of current, they secured a recovery and improvement rate of 28.7%.

Similarly, Ziskind (80) found that subconvulsive reactions are not only useless but harmful, even though followed shortly thereafter by a convulsion. His results on affective psychoses treated with metrazol showed a near 100% response in the cases having no petit mal reactions. Not only were there therapeutic

failures in his group with petit mal reactions, but these failures were increasingly greater in proportion to the percentage of such reactions. Findings such as these have convinced most investigators that nothing is to be gained from the use of subconvulsive methods, and so today the production of grand mal convulsions is, by general agreement, considered imperative.

We have seen that the immediate results of shock therapy in the affective psychoses, when the treatment is correctly applied, are uniformly excellent. However, since the crucial test of any therapy is the permanence of its beneficial effects, we must, as in the case of schizophrenia, resort to an analysis of long-term follow-up studies to determine the ultimate value of the treatment.

Ziskind and his co-workers (72) conducted this type of study for a period ranging from 4 to 40 months, averaging 22 months. Fifty-nine treated cases and 74 untreated controls were considered. The immediate recovery rate for the treated group was 89%, as contrasted with a rate of 46% recoveries among the controls. Relapses occurred in 11% of the patients discharged as full recoveries; however, new attacks were twice as frequent in the untreated as in the

treated group. I note that 37 of the 74 controls had mild symptoms and were therefore given no shock therapy. In view of this fact, the significance of the difference in relapse rate between the treated group and the controls becomes even greater, since the control group, half of whom were only mildly psychotic, produced twice as many relapses as the treated group, all of whom had symptoms severe enough to require shock therapy. This would tend to indicate that shock therapy in the affective disorders has an independent value over and above a mere acceleration of recovery.

Another well-controlled study has been done by Tillotson and Sulzbach (73), who observed a group of 70 treated patients and 68 controls over a period of 18 to 45 months. The immediate recovery rate in their treated group was 80%, as contrasted with 50% spontaneous recoveries among the untreated group. At the end of the first year after discharge, the rate of failures and relapses was 17% for the treated cases and 40% for the controls. Of all the recoveries in the shock-treated group, the manic-depressive depressions had the highest absolute rates, but relatively higher gains were accomplished with regard to the cases of involutional melancholia. The difference from the controls was 38% for the manic-depressives, whereas it was 52% for the involuntions, thus

indicating a higher relative effectiveness of shock therapy over non-treatment in involuntional melancholia.

As in the consideration of schizophrenia, I find it hazardous to regard many figures obtained in follow-up studies on the affective psychoses as being statistically significant, the reason being the same as it was in schizophrenia; namely, that in many cases not enough of the original group of discharged patients are available at the time of the follow-up study to be able to draw justified conclusions concerning the entire original group. (This is particularly evident in the report of Smith, Hastings, and Hughes (76).) Because of the small size of the total number of affective psychotics who have been observed for long periods of time following treatment, it is difficult to derive any more than a roughly accurate idea as to the ultimate value of shock therapy in the affective disorders.

Significant factors in the production of sustained results seem to be the amount of time that the shock-treated patient is kept in the hospital and the number of shocks received. Bennett (53), in contrasting a group of relapsed patients with a control group who had remained well, after treatment, for from 2 to 5 years, found that the former were held in the

hospital on an average of ten days less than the latter. Also, the relapsed patients averaged 5.5 treatments, whereas the controls, who had not relapsed, were given a mean number of 7.2. From this, it may be concluded that adequate treatment and a sufficiently long period of hospitalization (Bennett considers six weeks as the optimum time) provide reliable assurance for the maintenance of the patient's recovered status.

In conclusion, the role of shock therapy in the affective psychoses can be summarized as follows:

The therapy has consistently brought about uniformly gratifying recovery and improvement rates, even in cases of long duration. Electroshock is the method of choice today, and good results are obtained only by producing grand mal seizures. More intensive treatment is required for manics than for depressives and for involuntional paranoids than for involuntional melancholiacs. However, with adequate therapy these more resistant psychoses are capable of giving a good percentage of responses. As evidenced by comparison with non-treated controls, the therapy produces not only better immediate results but also more sustained improvements. As in the consideration of schizophrenia,

shock treatment of the affective psychoses produces best results when used in combination with psychotherapy.

Chapter IV

The Shock Therapies in the Psychoneuroses

Although the shock therapies have found their most extensive field of application in the major psychoses, they have also been used (albeit grudgingly, it appears) in the treatment of the psychoneuroses. Unfortunately, most of the results of shock treatment of the neuroses appear usually as reports on mere handfuls of cases, appended to articles dealing mainly with the major psychoses. One regrets to note that the trial given the therapies in the neuroses has been on such a small scale that a quantitative statistical analysis of the results obtained is not warranted. Nevertheless, on the basis of the findings in a limited number of cases, one is justified in forming a qualitative estimate of the sphere of usefulness of the treatment in these conditions.

Myerson (82) reports success in dealing with a number of borderline mental states which,

according to him, yielded not in the least to other forms of therapy, including long periods of psychotherapy. His cases included patients in whom there appeared profound alteration in conduct, marked disturbance in mood, failure of energy, anhedonia, actual and severe depression, and industrial and social incapacity. The majority of these cases benefited markedly after a few electroshock treatments. Since this group had remained unbenefited by other forms of treatment, one is led to agree with Myerson's conclusion that in such borderline conditions the physical therapy involved in the shock treatment is superior to any other form of therapeutics, including psychotherapy.

Concerning the use of shock therapy in clearly-defined psychoneuroses, one of the earliest reports on the question was that of Shapiro and Freeman (83) in 1939. These investigators noted that neurotic patients usually require fewer doses of metrazol than do psychotic patients in order to cause subsidence of their symptoms. Some of their neurotics were completely relieved of their preoccupations after one or two convulsions. However, they usually continued

treating them until a series of 6 to 12 shocks had been given, or whatever number was found necessary to produce forgetfulness and confusion. It was found by these writers that chronic cases often proved amenable to treatment, although prolonged fixations, either psychasthenic or hysterical, reacted unfavorably.

The patients treated by Shapiro and Freeman included cases of obsessive-compulsive neurosis, anxiety neurosis, reactive depressions, conversion hysteria, chronic tension states, post-traumatic neurosis, and chronic alcoholism. Their best success was obtained in the obsessive-compulsive and conversion hysteria groups. Out of 7 patients in the former group, 5 recovered, 1 improved, and 1 failure was noted. In a series of 5 cases of conversion hysteria, the treatment resulted in 4 recoveries, 1 improvement, and no failures. Their equally small number of cases of anxiety neurosis, reactive depressions, and chronic tension states reacted somewhat less favorably. The only groups in which complete failure was obtained were post-traumatic neurosis and chronic alcoholism (only one case of each of these two types was treated). Since the number of patients in each of the above diagnostic categories is so small, conclusions can not be drawn from this one study alone.

The findings of Kalinowsky (56) and of Sagebiel (118) agree with the above in regard to the obsessive-compulsive neuroses. Kalinowsky feels that shock-therapy is justified for severe and chronic cases in this group, when psychotherapeutic methods have failed. He notes, however, that although obsessive thoughts or compulsions may disappear or become less troublesome during the confusional state of a long course of electroshock treatments, they usually return shortly. Although Kalinowsky's report was published in 1943, he noted that the usefulness of shock therapy in the psychoneuroses had not yet been established. He had treated 50 neurotics and found that favorable responses occasionally occurred but realized that they were too rare to warrant routine recommendation of the therapy for the neuroses except, as mentioned above, for chronic and severe obsessive-compulsive neuroses unyielding to psychotherapeutic methods, and also for psychoneurotic depressions. Good results in the latter condition have also been reported by Myerson (117).

The most favorable report of any I have found is that of Moriarty and Weil (119), who obtained recoveries in 10 out of 20 electroshock-treated neurotics, with an additional 9 being listed as much improved.

Their cases were composed mainly of anxiety hysterias and psychasthenias. Moriarty and Weil contrasted these results with those observed in 79 controls, who had received no form of shock therapy. In this group, only 7% were considered to be recovered on discharge, while 71% were pronounced much improved or improved.

That the good results obtained by these workers in their small series of cases are the exception rather than the rule is evidenced in a review, contained in their report (119), of the findings of 14 authors reporting on the use of shock therapy in the psychoneuroses. Here, out of a total of 130 shock-treated neurotics of all types, only 36 cases (28%) were described as cured.

In the regrettable absence of a sufficiently large number of cases on which to form quantitative conclusions, one has little to go on except general impressions, such as given by Proctor (42), who, in surveying the indications for shock therapy, has recently expressed full agreement with the findings outlined previously; namely, that shock therapy is occasionally indicated in cases of the obsessive-compulsive type of behavior and in the hysterias, when these cases have not responded to at least three months of energetic psycho-

therapy. He adds, with apparently justified pessimism, that one should realize, on attempting the treatment, that recovery is unusual.

Concerning the relationship between shock therapy and psychotherapy, it is interesting to note, in contrast to this aspect of the problem in the case of the major psychoses, how little faith is put by some workers in the use of shock treatment as a therapeutic adjunct in the neuroses. Kalinowsky (56), although he grants that the patient's increased accessibility can be used to advantage for a better psychotherapeutic approach, feels that the occasional helpfulness of shock treatment in the neuroses as such an adjunct cannot be considered comparable in type or degree to the improvement which can be achieved by its application in the psychoses.

Shapiro and Freeman (83), on the other hand, assume an almost nihilistic attitude in regard to post-shock treatment psychotherapy. Their stand is that any suggestion by the physician of the psycho-pathologic sources of the original conflict is distasteful to psychoneurotics and serves to encourage introversion and pre-occupation. They actually dissuaded their patients from exploring the causes of their illness and

persuaded them to indulge in physical activity in a further attempt to dissipate emotional tensions. For intensive psychotherapy they substituted "encouragement and gentle re-education". To my knowledge, Shapiro and Freeman have not conducted any follow-up study on their patients, and it is therefore impossible to assess the permanent gain to be derived from a program such as this.

A more orthodox view, and one which is more in line with the majority-opinion presented in previous chapters, has been expressed by Moriarty and Weil (119), who feel that shock therapy prepares the ground for psychotherapy by improving the affective tone, fostering active co-operation and tending to overcome the "repetition compulsion". They advocate the subsequent use of psychotherapy to permit the patient to gain understanding and inner fortitude, as a guard against relapse.

Even Shapiro and Freeman (83), whose results in the neuroses, it will be recalled, were not unfavorable, are conservative in their estimates of the total good accomplished in these disorders by the use of shock therapy. After stating their results, they hasten to add that they do not claim that the personalities of their patients are altered or that their difficulties have been forever done away with. They do feel, though,

that the majority of the patients treated by them no longer suffered from the complaints that caused them to seek psychiatric help.

In contrast to the limited usefulness of shock therapy in the neuroses of civilians, the treatment has become a valuable weapon in dealing with the neuroses encountered in military life, caused by battle-conditions. Evidence for this is to be found in the survey (84) by Grinker and Spiegel of the handling of war neuroses in the Tunisian campaign of 1943. Their indications for the use of shock therapy are best expressed in their own words:

"We have been slow in using shock therapy in this theater of operations, because we believed that persistence in 'uncovering' technics, which aim to bring repressed emotions to the surface, would accomplish much more in the way of permanent good to the patient. We also wished to avoid others being exposed to the temptation of non-psychologic short-cuts in treatment.

"It soon became apparent that many cases suffering from clinical depression, or other mani-

festations of repressed hostility, did not respond well to pentothal treatment (narcosynthesis) or psychotherapy in the time available to an individual patient. Yet we wanted to do something for these patients promptly, since they are the ones who quickly attain a state of chronicity and rigidity, and become so difficult to treat after return home. Therefore we began the cautious use of convulsive shock treatment, after pentothal interviews and psychotherapy had accomplished all that was possible. We followed the shock treatment with adequate psychotherapy before transferring or evacuating the patient."

Thus it is evident that, in the handling of war neuroses, convulsive shock therapy is valuable as a last resort when other, more conservative, methods fail. Grinker and Spiegel present two cases as examples of the good results obtained with the therapy in such instances.

To summarize the role of shock therapy in the psychoneuroses, it may be said that, despite the absence of any large series of reported cases, one may conclude that the therapy has little value, except in war neuroses, psychoneurotic depressions, obsessive-compulsive and hysterical states, and in

borderline states between the neuroses and the psychoses. In all of these conditions shock therapy is indicated if the patient has proved resistant to more conservative methods. For the majority of psychoneurotics, psychotherapy continues to be the treatment of choice.

Chapter V

The Hazards of the Shock Therapies

An evaluation of any new form of therapy is dangerously incomplete if it fails to take into consideration not only its assets but also its liabilities. There are numerous methods of treatment used in every field of medicine which can be employed wisely only if due regard is given to the dangers involved in their use. There are others whose disadvantages far outweigh any therapeutic benefit to be derived from their administration. And certainly, in a form of treatment as drastic as shock therapy, it would indeed be folly to endorse the method before investigating the price paid in bodily damage by its use.

The recognition of the risks involved in the shock therapies has led to their unmerited discreditment in some quarters, and the terms "brutal" and "sadistic" have not infrequently been used in descriptions of them.

Complications involving either life or limb were noted very early after the inauguration of the

treatment methods. Kinsey (85), in 1941, surveyed all the published cases on deaths resulting from the shock therapies and found the incidence to be 73 per 10,000 patients treated with insulin, and 23 per 10,000 treated with metrazol. The death-rate of electroshock-treated cases has been estimated by Kolb and Vogel (41) to be 5 per 10,000 patients.

In his report, Kinsey analyzed 43 metrazol deaths. Of these, 21 were attributed to pulmonary complications. Twelve of these were the result of pulmonary tuberculosis activated during treatment. Three deaths resulted from pulmonary infarction. There were three cerebral deaths, two due to hemorrhage and edema. Status epilepticus was also noted as a cause of death. In considering insulin treatment, it was found that hypoglycemic encephalitis was the cause of 38 deaths.

Since the shock methods are therapeutically directed against the central nervous system, one might expect the greatest amount of post-treatment damage to be found there. Numerous experimental and human autopsy studies have been undertaken in attempts to study this phase of the question. The pathological effects of insulin treatment on the human brain were

studied by Ferraro and Jervis (86), who noted fatty degeneration of the neurons and an increase in intracellular lipoids, particularly in the temporal lobes. They also found chromatolysis and, in the cells in the region of the blood vessels, ischemic changes.

In a later report (87) on additional insulin cases, Ferraro found zones of rarefaction in various cortical areas, due apparently to the gradual disappearance of affected nerve cells, as well as the absence of cells in patchy, focal areas where they had undergone degeneration and disintegration. Also, there were areas of cortical devastation involving various layers, with disturbed cortical cytoarchitecture, particularly in the frontal and temporal areas and in the purkinjian layer of the cerebellum. Blood vessel changes in these cases were productive in nature, consisting of proliferating changes of the intima, and hyperplasia and swelling of the intimal endothelial cells. It was Ferraro's opinion that this vascular proliferation might be related directly to insulin intoxication, as the changes noted were similar to those reported by other investigators as being due to various other toxins such as lead, organismal toxemias, and infections.

In studying the pathologic effects of metrazol,

Weil and Liebert (88) examined the brains from six patients who died two to ten months after treatment with this form of shock therapy. The outstanding features noted by them were marked hypertrophy and hyperplasia of astrocytes and, to a lesser degree, of the microglia. It is interesting to note that they found the severity of reaction to be inversely proportional to the duration of each patient's psychosis. It has been found experimentally, however, that the amount of pathological change has a positive correlation with the sum total of metrazol given throughout the course of treatment, the duration of the treatment, and the survival period after the last injection.

This correlation, noted by Strecker and his co-workers (89), is apparently not always consistent, since Arieti (90), who also experimented with metrazol on monkeys, found that the pathological changes in this series were not proportional to the number of convulsions, dosage of the drug, or the duration of the seizures. Both groups of investigators are agreed, however, that in some cases no changes at all are produced in the brain by metrazol. Thus, the question is left open as to what amount of brain damage can be expected after any given course of metrazol treatments.

The evidence for central nervous system damage

after electroshock treatment indicates that it is mainly vascular in nature. Experimental electroshock has been done on rabbits and rats by Weilbrunn and Weil (91). Although they observed no generalized ganglion-cell lesions or generalized proliferative glial reactions, they noted changes in the venous system in 25 out of their 28 cases. Hemorrhages were present in the meninges and in the substance of the brain and spinal cord. It was found that these hemorrhages were confined to the perivascular regions of the capillaries, being caused by rupture of the capillary walls. Similarly, sub-arachnoid and punctate hemorrhages in the brains of electroshocked cats were observed by Alpers and Hughes (92).

These investigators corroborated their findings in an autopsy study (93) of two patients treated by electroshock. One patient dying after 62 convulsive treatments showed fresh hemorrhages in the cerebral cortex and white matter. The brain of the other patient, who died five months after the final treatment, showed old areas of perivascular damage mainly in the white matter. Apparently then, the lesions produced by electroshock are less severe in nature than those produced by insulin and metrazol, since the latter two methods, it will be recalled, caused neuron degeneration and glial

proliferation.

As mentioned above, it is difficult to judge the extent of brain damage caused by shock therapy in any given case. This view has been stressed by Pacella, Barrera, and Kalinowsky (94), who state that "An insufficient number of cases have been examined pathologically, and as far as the human material is concerned, any examination of the brains from those dying during or after the treatment must differentiate as to which lesions, if found, are due directly to the action of the therapeutic measure, which are caused indirectly by various somatic-physiochemical processes released by the therapy, and finally, which are produced by other processes and bodily lesions playing a major role in the lethal event. The brain must be examined in the light of all other post-mortem findings and conditions in the body before any final evaluation can be made in a given case, as the brain participates in many lethal conditions the foci of which lie in the other organs of the body."

Because of the lack of use of controls in animal experimentation, because some of these investigators have employed doses higher than those used clinically, and because of the variables noted by Pacella and his co-workers, in their analysis of human material,

one is left with the impression that the evidence for permanent brain damage caused by shock therapy methods is far from conclusive.

That changes in brain physiology occur due to the shock therapies is evident from electroencephalographic studies. The changes have been noted particularly in the case of electroshock and have been reported ever since the earliest clinical use of this method. Fleming, Golla, and Walter (95) noted, in 1939, that no electroencephalographic record is obtainable for 20 seconds after the administration of the shock, and that the electrical accompaniments of the convulsion itself are the same as those seen during a spontaneous seizure. They found that for about ten seconds after the convulsion is ended, there are few cortical potentials detectable, but then large slow waves appear over the whole surface of the head. The potential rises in about 30 seconds to several hundred microvolts, and the waves have a frequency of one or two per second, with an irregular form, resembling the electroencephalogram during deep natural sleep.

They observed that this generalized, slow discharge becomes progressively more regular, more rapid, and smaller, and that about 30 minutes after the end of

the seizure, the electroencephalogram shows only waves resembling the normal alpha rhythm in frequency and size, although the area from which these arise is larger than in the normal person.

It has been found (55) that despite bursts of abnormal activity in the electroencephalograms of shock-treated patients, the pattern returns to normal within 3 to 4 weeks after cessation of the treatment. It has been stressed (80), nevertheless, that the reversibility of these electroencephalographic changes does not necessarily mean that brain pathology, if present, is reversible.

Some have raised the question of whether electroshocks might initiate epilepsy. According to Bennett (53), it has been shown that, where electroencephalographic studies have been made before and after treatment, later spontaneous convulsions are not likely to develop unless the individual is constitutionally predisposed (already dysrhythmic).

Intellectual changes produced by the shock therapies have been noted mainly in regard to memory defects. This is usually temporary and minor in degree. While recovery is the rule, it has been found in some cases to be incomplete. Ziskind (96) has re-

ported persistent amnesia resulting from metrazol therapy, resembling the memory impairment noted in organic psychoses. In the milder cases there is a disturbance for isolated events of recent origin; in the more severe cases remote memory is also affected. The consensus of most investigators, however, is that in the huge majority of shock-treated cases, memory defects are temporary.

The effect of the shock therapies on mentation and personality seems to be a highly inconstant phenomenon. Fumbert and Friedmann (97), in discussing insulin, stated that some patients who showed a good social recovery appeared more euphoric, but at the same time somewhat dull, passive, and in-different than they were before the onset of their illness. Bennett (67) noted personality changes suggestive of mild sensorium defects. Levy, Serota, and Grinker (98), in a clinical and neurological study of 23 patients, found evidence of disturbed cerebral function in 50% of the patients, as indicated in intellectual function. Although they noted recovery in most cases within a few weeks, some severe cases lasted as long as six months.

The long-term effects on intellectual function seem to be either slight or nil in most cases. Some

workers have found (80) that in many cases patients recovering from psychoses by means of the shock therapies are now successfully carrying out severely intellectual positions and tasks. Indeed, one group of investigators (73) have gone so far as to state that some patients, subsequent to shock therapy, display a far more efficient intellectual as well as emotional adaptability to their environment than ever before in their lives.

Although the central nervous system changes noted above have been used by many as a cause for attacking the shock therapies, the hazard which has aroused the most vociferous condemnations is the matter of traumatic complications ensuing as a result of convulsions.

In 1939 Polatin and his co-workers (99) shocked the medical profession by announcing a vertebral fracture rate of 43% in a series of 51 metrazol-treated cases, with an average age of 28.7. In the same year, Carp (105) estimated the incidence of serious extremity fractures of humerus or femur to have been 1.5% to 2%, and dislocations 17.2%.

These reports were particularly significant in view of the neglect of the fracture issue in previous

surveys of the results of convulsive therapy. In 1936 Meduna had claimed (100) that there were no serious complications with the use of metrazol. A year later, Kennedy (101), in reviewing results in the treatment of 1,000 patients, made no mention of fractures. And in 1939, the same year as the publication of Polatin's disturbing re-port, Meduna and Friedman (48) listed mechanical complications of convulsive shock therapy without referring at all to vertebral fractures.

There is evidence that unsoftened convulsions produce not only skeletal complications but also trauma to viscera. A group of veterinarians (113) reported, in 1930, on findings encountered in the viscera of cattle and hogs in slaughter-houses, which had experimentally been given convulsive doses of electric current. It was found on autopsy that, in addition to vertebral fractures and ruptures of muscles, there were pulmonary congestion and petechial hemorrhages throughout the lungs and other viscera, indistinguishable from hemorrhagic septicemic states. In all likelihood, similar lesions are produced by unsoftened convulsive shocks in the human subject and are considered (53) to account for the flareup of latent tuberculosis, which has been reported (85, 114) as a danger involved in straight convulsive shock therapy.

The serious problem of vertebral fractures and, in extremities, fractures, dislocations, and ligamentous ruptures, soon became generally recognized as one of the greatest, if not the greatest, drawback in the use of convulsive shock methods. Indeed, in some cases these traumatic complications were so frequent that, according to Bennett (102), many condemned the treatment as inhumane and subsequently abandoned it.

The recognition of the high incidence of traumatic complications precipitated a general search for methods of reducing or abolishing such complications while maintaining the beneficial therapeutic effects derived from grand mal convulsions. Polatin and his co-workers, in their original article (99), recommended that the patient be held in strong antero-flexion during the convulsion. Famsa and Bennett (103), in 1939, advocated spinal anesthesia before administering each shock. However, neither of these methods gave any constant assurance that fractures could be prevented.

In 1940, Bennett (102) introduced curare; in the same year Rosen, Cameron, and Ziegler (104) reported the use of beta-erythroidine hydrochloride. In 1941, Yaskin (106) advocated the use of intravenous magnesium sulfate. Two years later, Impastato and his co-workers (107) came out in favor of sodium amytal. All

of these drugs were introduced in an attempt to block motor impulses to muscles and thereby to lessen the severity of the convulsion. Although any of these chemicals are employable, the one which has gained the widest acceptance at the present time is Bennett's pre-convulsive curarization method. It has the advantages (108) of being specific in its action, and non-toxic, with no side effects. It is available in a well-standardized preparation (Intocostrin, E. R. Squibb and Sons) and requires only a few cubic centimeters per injection. Moreover, there is no question of a central action, as is the case in the use of the other drugs.

In 1941, Bennett (109) reported on the use of curare in 74 patients receiving 466 metrazol shocks. In none of these did any complications occur. Similarly, Cash and Hoekstra (108), in 1943, reported encountering no traumatic complications in a total of 139 patients receiving 995 combined curare-electroshock treatments. In the same year, however, Cummins (110) noted a fracture rate of 3.9% in his series of 232 patients treated with 3,057 curare-metrazol shocks. In his 9 cases of compression fracture occurring when curare was used, seven showed changes in one vertebra only; in the eighth case, two vertebrae were involved,

and in the ninth case, three vertebrae. In view of the fracture rate of zero as reported by Bennett and by Cash and Hoekstra, one is led to question Cummins' technique of curarization.

Although curarization has certainly shown itself to be the most effective method of dealing with the problem of traumatic complications, it has met objections in some quarters, e. g. (56, 111), because of supposed dangers, the usual criticism being that curare allegedly causes prolonged post-treatment apnea. Bennett (53) regards these objections as being unwarranted, since curare has no effect upon the central respiratory mechanism; the only respiratory embarrassment which can occur, according to him, is that due to shallow intercostal breathing or pharyngeal muscular relaxation, factors which can be overcome with artificial respiration. Also, curare possesses the advantage of having a specific antidote -- prostigmine -- which adequately counteracts any excessive curare effect on the respiratory muscles (53, 108). Indeed, Cash and Hoekstra (108) made successful use of curare in treating a patient whose intercostal muscles had been markedly paralyzed by poliomyelitis.

The safety of the method is evidenced by the fact that by 1945 over 100,000 curare-metrazol and

curare-electroshock treatments had been given with only one reported fatality, and in that case neither proper artificial respiration nor prostigmine was used (53). According to Bennett and Cash (112), the only contraindication to the use of curare is myasthenia gravis.

There have been some workers who have been fearful of using shock therapy on the aged. There is evidence, however, to indicate that this fear is unwarranted. Evans (115) has reported the use of convulsive shock therapy in 50 patients who were older than 50 years. Of these, 40 were either recovered or improved enough to be discharged, and the incidence of complications was found to be no greater than in younger groups.

Contraindications to the shock therapies are not as many as might be supposed in view of the drastic nature of the treatment. Cardiac patients have been treated successfully and so have those with hypertension, as well as those who have reached the 70-year level or above, with circulatory systems showing evidence of senility and arteriosclerosis (55). Indeed, successful treatment in cardiac patients has led to the concept that immediate treatment is imperative when

the psychotic excitement causes constant strain on an already damaged heart (56). On the other hand, some patients have been reported as having died suddenly from cardiovascular accidents within a few weeks after full recovery from depressive psychoses, with the possibility that the treatment may have been the causative factor (53).

The shock therapies have been administered successfully in the presence of diabetes, pernicious anemia, spastic paralysis, pregnancy, hyperthyroidism, carcinoma, coronary disease, hypertension, and cerebral thrombosis, without organic complications following the course of treatment (53). It has been noted (55) that the anxiety and tension due to the mental disorder may be worse physically for the patient than some of these supposed contraindications. Apparently the only true contraindications are pulmonary or systemic infections and cardiac decompensation (53).

In summary, it may be said that the most important hazards of shock therapy -- namely, skeletal and visceral trauma -- can now be successfully eliminated by preliminary curarization of the patient, that there is no convincing proof of significant damage to the nervous system, and that definite contraindications

are surprisingly few in number. One may therefore feel justified in concluding that the assets of the shock therapies far outweigh their liabilities.

Chapter VI

The Mechanisms of the Shock Therapies

This thesis being an evaluation of a particular type of therapy, the reader may question the necessity of including within it a chapter dealing with its modus operandi. To me, the study of therapeutic mechanisms has distinct bearing on the problem, since it is only by an analysis of the manner in which a form of treatment acts that one can ascertain the reasons for therapeutic successes and failures. The question of mechanisms is therefore one not only of fascinating theoretic interest but also of direct practical significance.

The disconcerting but true fact of the matter is that the mechanisms of the shock therapies are at present unknown. During the decade since the inauguration of the shock methods, the theories which have been advanced to explain their action have been legion, yet none of them has given a completely acceptable answer to the question.

This is particularly interesting in the question of the use of the shock therapies in schizophrenia, for here we behold the odd spectacle of a mystery treated with a mystery, with successful results.

Sakel, the originator of insulin shock therapy, was quick to recognize this bizarre state of affairs. "I should be glad", he stated (6) in 1937, "if it were possible to follow in this special field the method of procedure customary in medicine, i. e., first to investigate the cause of the malady, and afterwards to look about for a corresponding mode of treatment. But once a path is accidentally found which begins at the point that should be the end of the journey, shall we have the temerity to abandon it, especially in working with a disease as difficult as schizophrenia? And if the hypoglycemic therapy fulfills even part of its promise, it will be possible, I think, to reverse the usual course of inferences and in going backwards to learn about the nature and prime cause of schizophrenia."

Of course, as Barrera (80) puts it, "In medicine we do not wait for a scientific explanation if we have something of therapeutic value". The fact remains, however, that if we did have a scientific

explanation, it might be possible to modify and improve the existing methods of shock therapy. In the words of Himwich (30), "Perhaps we are doing the right thing but in a very crude way, just as if one were trying to right a watch with a hammer. If we could find out the essential step in the amelioration process, then it might be possible to accomplish it in a more direct and less brutal fashion".

This need has been felt ever since the early days of the shock therapies. Referring particularly to the use of shock methods in schizophrenia, Meduna (7) expressed his attitude on the problem in 1937. "Beyond all doubt," said he, "from biological and therapeutic points of view, we are undertaking a violent onslaught with either insulin or metrazol, because at present nothing less than such a shock to the organism is powerful enough to break the chain of noxious chemical processes that leads to schizophrenia. I hope that in the future we shall reach a point where it will not be necessary to break the series of events leading to schizophrenia with brutal force. Then we shall be able to discard the shock and coma of the insulin treatment and the epileptic seizure of the metrazol treatment. Instead we shall provoke directly the slow chemical processes that can now be stimulated only by the explosive

mechanisms of the two methods of treatment."

The bulk of this thesis has consisted of an analysis of facts -- statistical data. When considering a problem such as the mechanisms of the shock therapies, it must be realized that one is dealing in the realm of theory. Consequently, an analysis of this question becomes a matter of contrasting one opinion against another, rather than combining and correlating clinical data.

The theories which have been advanced in explanation of the mode of action of the shock therapies can be divided into two main groups -- the somatic and the psychic. Let us first take up a consideration of the former.

Sakel's original assumption (6) was that insulin exerts its therapeutic effects by a diminution of the function of nerve cells. Looking upon a reaction of the nervous system as a response to stimuli, traversing certain pathways, he explained the processes in hypoglycemia by a blocking of pathways previously active so that reactions to the same stimuli run their course over pathways previously inactive. According to Sakel's theory, there are nerve cell path-

ways higher and lower in the evolutionary scale. The higher ones are newer, more complex, and more sensitive. In the course of the schizophrenic process these are crippled, and the phylogenetically older ones become dominant. Under the influence of insulin shock these older ones are progressively weakened so that the cell has an opportunity to recuperate.

As pointed out in Chapter I, Meduna began using convulsive shock therapy with the idea that schizophrenia and epilepsy represent two mutually antagonistic diseases and that therefore a schizophrenic could be cured if subjected to epileptiform convulsions. Of course his hypothesis of a fundamental antagonism between the two diseases never had any basis of proof. In fact, on reviewing the literature on this point, Kennedy (50) found that spontaneous convulsions are fairly frequent in cases of catatonia and are by no means always followed by a remission of symptoms. Even if there were this antagonism between schizophrenia and epilepsy, how would this explain the excellent results obtained with convulsive therapy in non-schizophrenic conditions such as the affective psychoses?

By 1939 Meduna had realized the error of

his hypothesis (121), and he has since become one of the leading investigators of the mechanisms involved in the shock therapies. The crux of his present theory on the question is that the shock therapies possess a common factor of interference with carbohydrate metabolism. It is his belief (122) that since only a certain group of patients benefit from hypoglycemic treatment, despite the fact that insulin produces the same biochemical effects in every patient, the factor responsible for cure must be not the primary effect of the insulin but the patient's response to the biochemical effects of the hormone. This response occurs about 30 minutes after the coma dose of insulin has been injected and, according to Meduna, is characterized by the following changes in the blood: increased sugar, increased lactic acid, a slight ketosis, an acid shift of the pH, an increase of total fat, and a leukocytosis. In other words, the response comprises biochemical changes diametrically opposite to the biological effects of insulin.

Interestingly enough, Meduna has found the same blood changes to occur as an immediate response to convulsive shock methods (122). He points out that these changes, produced by the shock therapies, are germane to diabetes and to stimulation of the

sympathico-adrenal system. In both of these conditions there is a shift of the equilibrium from the vago-insulin to the sympathico-adrenal system. In view of the fact that a certain proportion of schizophrenics behave like diabetics in their glucose and insulin tolerance (122, 134), Meduna regards this observation as the mobilization of an unsuccessful defense mechanism against the disease itself, and that if this reaction is furthered by shock therapy the patient's chances of recovery are greatly enhanced. According to his findings, only those patients recover, as a result of insulin treatment, who produce an increased amount of adrenalin during the hypoglycemic shock.

Of equal interest are the biochemical changes in the brain occurring as a result of the shock therapies. According to Meduna (122), studies of the cerebral cortex reveal that alkaline strikes occur in both the hypoglycemic and convulsive treatments. Also, there is an increase in lactic acid and inorganic phosphorus, and a decrease in phosphocreatin.

The similarity of these findings to those noted in cerebral anoxia, and the relationship between anoxia and hypoglycemia, are matters which appear to be of great significance in determining the

mechanisms of the shock therapies. It has long been known that cerebral metabolism depends upon the direct oxidation of glucose, and many different experiments have shown that the oxidative processes in the brain can be diminished equally as well by either hypoxia or hypoglycemia (123, 124, 125).

Gerard (123) contends that interference with oxidation in the brain leads to a secondary "overshooting" with increased activity and increased stimulation of neurons. Similarly, Halvorsen (124) has found that a sudden production of anoxia has a stimulating rebound effect on the medullary centers. Gerard takes the view that a relatively long period of increased respiration and activity may follow one of interference with metabolism. He feels that the cause for this effect may lie in a leakage of potassium ions from the interior of the neuron during anoxia. At any rate, it is his belief that the shock therapies obtain their therapeutic effect not by the primary decrease in brain metabolism, but by an increase in cerebral activity caused secondarily as a rebound phenomenon following the period of cerebral depression. This theory appears to me to be the cerebral equivalent of the "rebound" blood-chemistry phenomena noted by Meduna, as listed above.

Whether the beneficial effects of the shock therapies are directly due to the primary depression of cerebral metabolism through hypoglycemia or hypoxia (either of which accomplishes the same results), or whether it is a secondary hyperactivity of cerebral metabolism which is the real therapeutic mechanism, the observations noted above tend to indicate that the shock therapies possess a common factor of causing a primary interference with brain function.

Following the recognition of anoxia as the outstanding change caused by convulsive shock therapy (126), attempts were made to produce this effect directly, by means other than the use of metrazol or electroshock. Both nitrogen inhalation (127, 128) and the breathing of air containing a low percentage of oxygen (129) have been tried. The former method was experimented on in the days of unsoftened metrazol metrazol convulsions, when it was desired to achieve a method of convulsive therapy lacking the undesirable traumatic effects of metrazol. As was pointed out in the preceding chapter, the objections to metrazol or electroshock therapy on the basis of traumatic complications are no longer warranted since the introduction of the use of preliminary curarization. Not enough investigation has yet been carried out along the line

of inducing anoxia by the inhalation of low-tension oxygen to warrant an opinion as to the efficacy of this method. It may be noted, however, that its use in ten patients (129) produced no significant changes, either beneficial or detrimental.

If one accepts the theory that the therapeutic effects of the shock therapies are due entirely to the production of changes in metabolism, one must immediately realize that this viewpoint leaves un-answered the question of the psychological mechanisms involved. That somatic factors are far from being the only ones operating in producing cures is evidenced by the observation, noted in previous chapters, that patients who have had a course of shock treatment supplemented with psychotherapy have consistently enjoyed better and longer-lasting recoveries than those who have had shock therapy alone.

It should be realized at the outset of our discussion of the psychic mechanisms involved in the shock therapies that here we must necessarily enter into the realm of speculation. Many of the views on this phase of the question are filled with much dramatic content, the reliability of which is a matter for individual surmial.

Although Sakel, it will be recalled, proposed an explanation of the effects of insulin therapy on a basis of changes in neural physiology, he

did not neglect to include a psychic component in his hypothesis. He assumed (6) that hypoglycemia abolishes and subdues principally the parts of the psychic life which have been most active. Thus, according to his theory, it permits the other antagonistic part to reach the surface and to attain a dominance.

"In cases which run a favorable course," he stated (6), "hypoglycemia, frequently and correctly induced, leads to a permanent restoration of previously suppressed psychic components. At the same time those that were previously active are weakened and submerged. Hypoglycemia apparently breaks the pathological mental pathways and cuts off the 'short circuits'. In this way, the passing of impulses across to an inappropriate pathway is prevented; with further use of hypoglycemia, the short circuits are more and more fenced off and isolated. Eventually every stimulus starts an impulse over the proper pathway only."

Meduna likewise, although he is convinced of a large biochemical factor in the mechanisms of the shock therapies, as pointed out above, does not neglect the admission of psychological forces. His theory is psycho-

analytic in nature, for he believes (134) that the slowly progressive physiological decerebration of the patient during insulin treatment produces a regression of the personality to so low a level that the psychiatrist is invested with qualities peculiar to the father of the patient. According to his theory, this slow regression permits a transference, which is later utilized in subsequent psychotherapy.

Jessner and Ryan (3d) feel that hypoglycemia changes the organism in such a way that the patient becomes able to turn his affection and his interest to persons and objects of the outside world and so to give up his narcissistic isolation. They believe that whether this altered attitude is merely temporary or becomes permanent depends greatly upon his capacity "to endure reality, with its alluring and threatening qualities."

There have been many workers who, in contrast to Sakel and Meduna, have discarded any physical explanation of the shock therapies and have built up hypotheses in which the effect of therapy on the patient's psyche is regarded as the sole factor in operation. Such a viewpoint has been taken, for example, by Gottlieb and Fuston (19), who, as noted in

Chapter II, found no difference in recovery rate between an insulin-treated group of schizophrenics and a control group who had had no shock therapy but who had been subjected to intensive psychotherapy, including re-conditioning through socialization programs.

The similarity of results obtained suggested to Gottlieb and Fuston a common factor in the two methods of treatment. This factor they assumed to be the pressure toward socialization which was being exerted constantly on the patients from several directions. They felt that the insulin treatment could be thought of as one method of exerting such pressure, especially through the attention the patient receives in experiencing the coma. Gottlieb and Fuston therefore concluded that shock treatment can certainly not be considered a specific therapy for schizophrenia.

Of course this conclusion is not unique, since, as will be recalled from previous chapters, it has repeatedly been observed that patients who have been subjected to a therapeutic program in which shock therapy is only one of many items, fare much better than those who receive shock alone. One must therefore search for the mechanisms whereby shock therapy makes the patient a more favorable subject for psychotherapy.

Lowenbach and Stainbrook (133) have stressed

the view that each shock treatment leaves a human being in a state in which all that is called the personality has been extinguished. Following the convulsion, according to them, the return of the higher functions is accompanied by a re-integration of the personality. These writers neglect, however, to consider the psychological mechanisms whereby the re-integration takes place.

Shapiro and Freeman (83) feel that the temporary impairment of cerebration, caused by the anoxia which is produced by the shock therapies, is the significant factor in operation. Concerning the effects of this phenomenon, particularly in dealing with the neuroses, they contend that when consecutive thought is impossible, individuals are no longer capable of cogitation and rumination, and the perplexity and anxiety induced by failure to find a solution to their problems tend to disappear. In other words, a relief of tension is produced.

In regard to the affective psychoses, the consensus seems to be that the shock therapies operate by producing a release of affective energy. Flescher (130) holds that shock represents a means of conveying huge amounts of energy inherent to the death and destructive drives, thereby unloading them in an indivi-

dually and socially harmless manner.

According to Levy and Grinker (81), the physiologic disturbances in the brain produced by convulsive shock treatment affect the dynamic relations between the inhibiting, repressing functions and the inhibited, repressed, aggressive drives, resulting in a freer expression of these affects more directly in dream, fantasy, or verbal or motor activity. These authors note that the liberation of aggressive drives need not be accompanied by conscious intellectual awareness of the process, since they may appear in rationalized or projected form or as increased self-assertiveness or aggressiveness. Apparently, then, the effect of subsequent psychotherapy is to add emotional insight and intellectual understanding of the psychic conflict, after the release of feeling obtained by shock.

What is the meaning of shock treatment for the patient? The only direct means of determining this is by noting the thoughts expressed by patients after regaining consciousness following a shock treatment. One said (77), "What have you done to me? I feel as though I'm reborn!" Others have made statements such as the following (64): "I've come to life";

"Where am I? Things are all changed, more natural";
"I am younger again"; and so on.

It has been noted (77) that some patients react to shock treatment as a form of physical punishment, just as they may react to psychotherapeutic sessions as scoldings. This appears to be the case with electroshock just as it was with metrazol, for although the former does not produce the horrifying feeling of dissolution and impending death which is one of the main disadvantages of metrazol (55), still it appears that electroshock does cause a vague fear of the treatment, at either an unconscious or conscious level (77). Selinski (77) has proposed an interesting hypothesis as to the nature of this sense of fear.

"In my opinion," he states, "shock treatment affects the psyche of the patient as a profound threat to his very existence. It reaches down to something primitive we can call it the instinct for self-preservation or ego instincts -- or what you will -- in the human organism. Certainly it is a fact that they regard the loss of consciousness resulting from the treatment with dread; they feel that they must completely surrender themselves to the mercy of others as one undergoing general anesthesia. The same doubt as to whether they will emerge alive is operative in

shock therapy. Significantly, a common reaction is to find early disappearance of suicidal impulses among the psycho-pathologic phenomena of the patient's mental disorder. One may speak of a re-integration of ego structure made possible by a violent shock to the personality. The benefit derived from shock therapy seems to stem from psycho-physiological alterations which stimulate the individual's will to live and reduces the withdrawal tendency."

That shock treatment represents a punishment, a threat to existence, and, following the regaining of consciousness, a rebirth, has been emphasized by a number of different writers (64, 81, 119, 131, 132). This is particularly significant in the depressive psychoses, since here the demand of the conscience for punishment and death represents the paramount aspect of the psychosis. As Bennett (64) puts it, "Having undergone the painful convulsive therapy, the patient has approached death psychologically, has suffered punishment, and has, as it were, proved himself willing to take punishment. His conscience is then freed; and he can allow himself to start life over again free from the compulsive pangs of conscience."

If the psychological factors noted above

represent the true psychic mechanisms whereby convulsive shock therapy acts, then possibly we have an explanation of the more favorable effect of the treatment on the affective disorders than on schizo-phrenia, except in those schizophrenics who exhibit affective components in their psychosis. In other schizophrenics, apparently, improvement occurs only in so far as their thought disorders are the consequence of inhibition. This latter view (50) has been substantiated by the use of the Rorschach test(11), in which it was found that those schizophrenic patients who benefited from convulsive treatment proved to be more emotionally inhibited, more psych-ically constricted, and more socially withdrawn than those who did not.

Even with all these interesting observations, there are still some significant questions, regarding the mechanisms of the shock therapies, which remain unanswered. Since, in the somatic sphere, it has been shown that the same physiological disturbances are produced by both the hypoglycemic and the convulsive methods, why should not all cases respond equally as well to one form of treatment as to the other? Or, on the other hand, if improvement and re-

covery are brought about by psychic mechanisms rather than by metabolic changes, what is there about the psychological response to hypoglycemic therapy which makes some forms of schizophrenia yield to it more easily than to convulsive methods?

The status of our present understanding of the mechanisms of the shock therapies is best summarized by Müller's statement (135): "Neither the endocrine phase of the treatment, nor brain pathology, nor the question of convulsions, nor the soothing, quieting effect, nor falling consciousness, nor the potent psychic shock is alone sufficient to solve our problem."

My own personal feeling concerning these heuristic questions is that future investigation will probably not only reveal the answers but will also lead to a greater understanding of the nature of mental diseases. There is already much evidence (134) to indicate that there is a large somatic factor in schizophrenia, in the form of a dysfunctioning of the endocrine system, and that there is a great degree of inter-action between mental and physical forces in mental diseases in general.

With this in mind, and with the observation, in other branches of medicine, that an increasing understanding of the nature of so-called "organic" diseases has led to an emphasis on their "functional" aspects, one realizes how closely psychiatry and the non-psychiatric fields of medicine are now approaching each other.

We are slowly beginning to recover from the damage wrought to our conception of disease by Galen and by Virchow, and we are now returning to the wisdom of the Hippocratic concept of the human being, a two-thousand-year old concept which has recently been dubbed with the misnomer "Psychosomatic Medicine". I call this appellation a misnomer since it implies a dualism which does not exist, and therefore belies the very idea which it seeks to describe. To me, the Hippocratic concept represents the study of the individual not as a combination of psychic and somatic elements, but as one dynamic unit in which psyche and soma are fused. If I were to coin a name for this neo-Hippocratism, I should call it "Unitary Medicine".

In investigating the role of the shock therapies in psychiatry, I have been amazed at the enormous number of ramifications of the problems

raised by their use. Over and above the fact that they have revitalized the whole field of psychiatry with a renewed interest and have dispelled the defeatism of former days, they have provided an impetus to many fields of research, including biochemistry, pharmacology, physiology, pathology, psychology, and clinical medicine. All of which is an example of the fact that the full significance of any new advance in medicine cannot be foreseen at the time of its discovery.

Medical progress is a cumulative phenomenon, and no one generation can hope to reach the solution of all of medicine's problems. What each medical generation can accomplish however, it seems to me, is to increase and to enrich the heritage which it has received from the past. Unanswered problems thus become not a matter for regret, but a stimulus for continuous and diligent investigation.

As Foster Kennedy (136) has put it, "We are now only picking at the locks of doors behind which lie the answers to these mysteries. Many keys will be needed for the opening, but it surely will not be beyond man's wit to make them."

Summary

1. The shock therapies, inaugurated a decade ago as innovations in psychiatric treatment, have now become major weapons in the armamentarium of the psychiatrist and have been used widely enough and long enough to permit a rational appraisal of their merits and shortcomings.

2. Because of lack of uniformity in the reporting of results and because of the many and complex variables to be taken into account within the schizophrenic disease-process, the value of the shock therapies in this disease is difficult to assess with certainty or finality. At the present time it can be said that the therapies have proved useful in facilitating and accelerating the recoveries of schizophrenics and that the benefits to be derived from them vary from one patient to another and are in direct proportion to the prognosis of each case if left untreated and to the duration of the psychosis.

3. Of the two forms of therapy, the hypoglycemic method has proved itself to possess more general

usefulness in schizophrenia than has the convulsive type of treatment. The latter has shown itself to be of greatest value in catatonic schizophrenia and in schizophrenic cases where there is an affective component in the clinical picture. There is evidence to indicate that convulsive therapy, when used intensively, is possible of effecting responses as favorable as those which have been accomplished with the use of insulin. Enough statistical data has been accumulated to indicate a high probability that the latter method has produced almost twice as many sustained recoveries as occur in non-shock-treated cases.

4. In the affective psychoses, the shock therapies have brought about uniformly gratifying rates of recovery and improvement, irrespective of the duration of illness. Depressives respond better than manics, and involuntional melancholiacs better than involuntional paranoids. Convulsive shock therapy has found its greatest sphere of usefulness in these psychoses, the method of choice today being electroshock, because of its marked advantages over metrazol.

5. The shock therapies have proved of little value in the psychoneuroses, except in war neuroses, psychoneurotic depressions, obsessive-compulsive and

hysterical states, and in borderline states between the neuroses and the psychoses.

6. Best results from the shock therapies are obtained when they are supplemented with other forms of treatment, particularly psychotherapy.

7. Bodily damage produced by the use of the shock therapies is now negligible, since the introduction of methods, notably curarization, by which convulsions are softened. The treatments can be given in the presence of a host of organic diseases and are con-~~tra~~indicated only in the existence of pulmonary or systemic infections or of cardiac decompensation.

8. The modus operandi of the shock therapies is not definitely known at the present time. There is evidence to show that the therapies possess a common factor of interfering with cerebral metabolism either through anoxia or hypoglycemia. Many theories have been advanced to explain the mode of action on the basis of subconscious psychic alterations. It is quite likely that future research concerning the mechanisms of the shock therapies may bring forth findings which will lead to a greater understanding of the diseases treated by them.

* * * * *

Before terminating this analysis of the shock therapies in psychiatry, I should like to conclude with a statement by Katzenelbogen (38). It is a thought which in my opinion contains a truth profound enough to be used as a guiding motto in the practice of any field of medicine:

"The success of shock therapy, as that of any other therapeutic procedure, depends much more upon enlightened individualization than upon hard and fast rules supposedly applicable to all cases."

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