sible to reproduce, by means of some drugs such as phenothiazines of the Stimetil type, actual hysteroid conditions. We have heard this before. Dr. Deniker told us that, after administration of prochlorperazine, the patients manifested all the characteristics of hysteria, such as very spectacular psychomotor phenomena, nervous fits, contortions—in short, a clinical picture resembling strongly the ones described after the great incidence of encephalitis which occurred after the First World War. These psychomotor manifestations are totally different from the Parkinsonian symptoms which may be observed after treatment with phenothiazine and reserpine. It is remarkable, indeed, that we are able to reproduce acute psychoses and to induce Parkinsonian extrapyramidal syndromes by the administration of toxins; the most important finding, however, is the drug-induced reproduction of psychomotor manifestations with all the characteristics of hysteria.

I am sorry that I am unable to show you a film recording the behavior of patients with hysteria manifestations induced by the administration of phenothiazine. I believe this film would document the hysterical manifestations which can be modified by persuasion and which are actually started by the administration of phenothiazine.

Zubin: I want to take only a few minutes to address my remarks to the topic which Professor Reiss had discussed in the use and abuse of statistics in psychiatry. That there have been abuses no one will deny, but whenever one raises the question whether statistics are useful in a given field it is always well to look at the record.

A good example of the usefulness of statistics in the field of psychiatry is the case history afforded by the discovery of the etiology of general paresis. This disease goes far back in history and many glimpses about its origin are recorded. A Danish physician, for example, noted about 1854 that all of the cases of general paresis which he saw in a neighboring state hospital had previously come to him to be treated for syphilis. This observation, as well as observations made by others, led to the statistical inference that there might be a connection between syphilis and general paresis. The earlier guesses about the etiology of general paresis had covered the entire gamut from psychogenesis to somatogenesis. Some regarded it as a disease that resulted from excessive mental activity, which was the reason given for men being more frequently attacked than women. Others attributed it to a variety of other causes such as frequent congestions to the head; masturbation; alcoholic and venereal excesses; diseases of the
heart; abdominal diseases; hemorrhoids; faulty menstruation; misuse of mercury; the patient's conviction of his own greatness, power and wealth, which was in contrast with the sordid reality in which he lived; the night air, because most of the individuals who were afflicted with it were night workers; the emotional upheaval in actors produced by shifting emotional roles from night to night in their acting, and thus disturbing their mental equilibrium; and a variety of other theories and hypotheses which remind one of the plethora of hypotheses that abound today in our attempts at explaining mental illness.

Careful statistical studies of general paretics, however, soon began to bring the rewards of good observation. Esnarch and Jessen were the first to report in 1857 that in nine cases of supposed dementia paralytica which were observed, a previous syphilitic infection was demonstrated through case history methods in all except one case, and in the latter a visit to a brothel could be ascertained. Thus, by indicating that eight out of the nine cases were actually previously infected with syphilis, is was demonstrated for the first time that there was likely to be a connection between syphilis and general paresis. A present-day statistician would have stated the probability of obtaining eight out of nine cases on the basis of sheer chance as somewhere between the .04 and .02 level.

In 1897, Krafft-Ebing reported to a congress in Moscow that some thirty years earlier a student of his, one Hirschel, had pointed out that approximately eighty percent of the patients in the ward who were general paretics had given a previous history of syphilis and proposed, as proof that the other twenty percent also had a previous history of syphilis, to vaccinate them with matter from syphilitic sores and thus demonstrate their immunity to such infection. They finally selected nine moribund cases and all of them proved to be immune. This was the first experimental evidence that syphilis and general paresis were related. The statistical nature of this proof, namely, the concurrence of syphilis and general paresis in nine out of nine instances, which by chance alone would have occurred only once in 256 times, is another evidence of the fruitfulness of the statistical approach applied to this field. It is true that it wasn't until Noguchi and Moore found the spirochete in the brain of the general paretic that the issue was finally settled, but without the previous statistical evidence there might have been no search for the presence of the spirochete in the brain.

One must remember that statistics cannot be used as a substitute for
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thinking or even as a substitute for experimentation. However, statistics can help in validating or in arriving at sufficient conclusions from available data. In order to make progress in science, one has to separate the realm of discovery, which is a purely subjective and intuitive field, from the realm of verification, which is primarily dependent upon the utilization of good statistical methods. You cannot discover with statistics but you can verify with them!

HIPPIUS: I would like to make a few remarks about the excitomotor syndrome ("syndrome excitomoteur") described by Professor Delay and Dr. Deniker, on the basis of my investigations made in collaboration with Kanig and Hartmann.

1. We observed the syndrome in treating psychoses with various phenothiazine derivatives; it occurred frequently in patients treated with phenothiazine compounds which have a piperazine ring on the aliphatic side chain. Chlorpromazine (a phenothiazine derivative with a pure aliphatic side chain), on the contrary, produces this syndrome very rarely.

2. Clinical observations invite comparison with hysteriform reactions; especially the fact that strong suggestibility of the patients plays an important role in the development of the excitomotor syndrome emphasized by Delay and Deniker, and also observed by us, made us think of hysterical mechanisms.

3. In an attempt to explain the pathogenetic mechanisms of this syndrome it is not necessary, in our opinion, to think of hysteria or to go even so far as to denote the excitomotor syndrome as a "model hysteria." We believe that a biochemical explanation is possible. We observed, for instance, that some patients did not show this syndrome at rest, but it appeared when they moved. This finding seemed to point to an interrelationship with metabolic processes. It can probably be shown that treatment with phenothiazine derivatives produces disturbances of the energy-rich phosphate metabolism in the nuclei of the brain stem. Clinically this assumption could be confirmed because in some cases it was possible to cancel out the excitomotor syndrome by the administration of adenosintriphosphoric acid.

HEFFER: Mr. Chairman, it is impossible to review in a few minutes the immense quantity of material which has been presented to us. I will therefore assume the privilege of telling you my reaction to this meeting. I am usually optimistic and this will govern my remarks.
The history of modern science is rather brief since Galileo. It is therefore interesting to review two apparently conflicting hypotheses of mental disease, the environmental and the chemical. One precedes Galileo, the other comes after.

The environmental hypothesis has been with us in one form or another for at least the past two thousand years. The endogenous chemical hypothesis is just beginning to walk, although on a rather shaky basis. Only fifty years ago, Dr. Carl Jung on the basis of his psychological studies concluded he must postulate toxin X to account for schizophrenia.

Dr. Osmond, my colleague and friend, has beautifully described the story of the toxic theories of psychosis and he has raised a question as to why progress has been slow. I think the answer is clear. Scientific progress does not advance simply; it advances in geometric progression. During the initial phases, progress is slow. In the past fifty years since Jung suggested toxin X, we have been in such a phase. During this period, periodic biochemical discoveries have been reported, only to fall back into obscurity or to be buried. However, after a time the tempo of discovery accelerates until the rate of discovery goes on so rapidly it is impossible to keep up.

If, therefore, we have been in the lag phase, what is the evidence that we are entering a critical period which heralds the age of new discovery? Or will we for many years continue to lag? I predict we are in a phase of striking development. Evidence for this prediction has been presented by this symposium.

I think it is appropriate at this meeting to honor by naming them, some of the pioneers in the field of experimental chemical psychiatry. These pioneers have periodically jolted us with their reports—Dr. DeJong, Dr. Baruk, Dr. Buscaino. Unfortunately, Dr. DeJong, a brilliant pioneer and colleague of Dr. Baruk, died before this meeting. We have also here today Dr. Hofmann with his LSD, Drs. Hoagland and Reiss with the endocrines, Dr. Heath with his taraxein, Dr. Sherwood with his cannula, and Dr. Rinkel who introduced LSD to North America.

It has been frequently claimed that LSD produces a toxic confusional state. This, I think, arises from some misconceptions regarding the function of a model. It is not the purpose of a model to reproduce the original but to illustrate and make available for study certain facets of the original condition. The theoretical model does not re-