

# Editorial

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Establishment Journal Looks at  
Orthomolecular Psychiatry

The British Medical Journal (1975) published a brief editorial entitled "Molecules and Mental Health." The writer concluded that "The subject ought to be dead, but it won't lie down, so what is the controversy about?" Finally the editorial concluded, "Sadly, Orthomolecular psychiatrists have generally not followed scientific methods of proof; when others have had poor results with scientific methods they have tended to reply by verbal arguments instead of with the argument of renewed experiment and fresh fact. The controversy is sterile."

To no one's surprise, the BMJ source for this opinion is the APA Task Force Report. This is another illustration of one establishment journal uncritically accepting the opinion of another without the slightest indication that the editorial writer has made the slightest attempt to go any deeper into the controversy.

Establishment journals rarely print

letters arguing against their position. However, Dr. Humphry Osmond prepared a critical examination of the editorial, excerpts of which we are presenting here, and I prepared a letter which could have appeared in the BMJ if I had submitted it to them. This is, therefore, an open letter of one journal to another which will likely never come to the attention of the editor of the BMJ, but which will provide additional information to the readers of this Journal.

## **Dr. Osmond's Memo, April 8, 1975**

It is understandable that members of the establishments, like all persecuted majorities, for so they feel themselves to be, prefer hanging together to hanging separately. I like the line, "The subject is dead but won't lie down, so what is the controversy about?" There is something rather pathetic and plaintive about the establishment on both sides of the Atlantic giving its verdict, and then,

oddly enough, not everybody believing in its infinite wisdom, knowledge, or even its honesty and integrity. It is well known that turbulent and vexatious minorities harbor many of these ill-conditioned even paranoid people who, for the most frivolous reasons, do not believe that establishments are usually virtuous and almost always right.

The BMJ editorial writer is short on research, short on references, and short on information. I imagine he is the kind of chap had he been in Washington in 1974 would have been sure that the misunderstandings and misinformation about Mr. Nixon and his friends would soon be cleared up. Oddly enough he would have been proved right by the end of the year with Mr. Nixon in disgrace and his henchmen en route for gaol.

He quotes the APA Task Force Report as if it were some sacred scientific writ. He apparently does not know about the unethical experiments by T. Ban, about Wittenborn's responders to niacin, or most importantly that in the Wittenborn study the double blind was broken. This catastrophe has so far been ignored by everyone involved on the establishment side. Not that this is particularly novel, but it tells us a good deal about the honesty, maturity, and even the common sense of those involved. This has to be answered some day. It won't go away.

Five hundred thousand dollars was virtually wasted due to poor design, and now simple dishonesty appears to be the motive for concealment. The BMJ article in ignoring or at least failing to make itself aware of this lays itself open to charges of dishonesty, too, although it seems more likely that sheer indolence would account for this just as well.

Perhaps the hallmark of intellectual dishonesty can be found in that piece beginning "If it were the case that 4 grams of niacinamide given daily to all patients diagnosed as schizophrenic abolished all the abnormalities in the majority in a few days—an effectiveness like penicillin in bacterial infections or insulin in diabetes—there would be no controversy. Unfortunately simple trials of megavitamin

therapy do not give results like this."

This implies that some such claim has been made which is false. It also implies that 4 g of nicotinamide is a standard dose and that a few days is the expected time to see results. Such results would be quite unlike the effect of most other treatments used in psychiatry (tranquilizers, antidepressants, ECT, deep insulin, psychotherapy, etc.) In fact, Orthomolecular psychiatrists have done a much better job of trying to disprove their own hypothesis than their critics. Ban and Lehmann have admitted to making little or no attempt to replicate the Saskatchewan findings.

A respected colleague and friend asked me why I thought that psychiatrists who usually agree upon so very little and whose specialty is riven by differences have managed to agree upon one thing—that they do not like or agree with the megavitamin approach. It is a strange phenomenon for here is a specialty some of whose members claim with apparent impunity that it does not exist, while others say that it is dead. These suggestions, which one might suppose would elicit alarm and resentment, are met with the blandness which many find make psychiatrists unusually irritating. A superior knowledge supposedly entitles the psychiatrist to be a non-responder to criticism; this clearly derives from Freud's view that only psychoanalysts can possibly criticize psychoanalysis.

As regards the megavitamins and Orthomolecular psychiatry, there is no evidence of this detached superiority, quite the contrary. A sustained assault has been going on for at least nine years. I have noted that there is ample evidence of bias, suppressing or repressing evidence favorable to megavitamins, and a failure to record errors in design such as breaking the double blind in the Marlboro study, even though this is in print. The psychiatrists who have done this are keen to state that they act more in sorrow than in anger because they are scientists, and as the BMJ leader writer has shown, science, that utterly impersonal goddess, has ruled against Orthomolecular psychiatry. I have noted that the less actual scientific achievement a person has the

more likely he is to wrap himself in the toga or winding sheet of an impersonal science. Indeed it is almost diagnostic of those who know little or nothing about the very difficult problems of scientific method and philosophy to pontificate about the nature of science.

That megavitamins don't work, cannot work, and must not be allowed to work is something about which psychoanalysts, Laingians, Szaszians, eclectic psychiatrists, biological psychiatrists, social psychiatrists, indeed the whole catalogue of psychiatrists can agree. They have not had such agreement for most of this century. We provide what I believe is technically speaking a displacement object against which they can vent their frustrations in comparative safety. They can also enjoy a spurious sense of unity for here at long last they have a common if rather limited basis for temporary amity. Such displacement is well known psychologically. It is seen best in kicking the cat. It tells us much about the state of psychiatry today, but little or nothing about Orthomolecular psychiatry and the megavitamin approach.

There are many curious ironies about this. Until megavitamins came on the scene it was difficult to get much real interest in schizophrenia. Now NIMH publishes a journal of that name. We are always being told about the need for community interest and the need to get patients and their families involved, but this is usually very difficult to achieve. I think that the American Schizophrenia Association has been unusually successful in this regard. This, however, has not been in the least bit pleasing to the APA and is presumably ascribed to our perverse salesmanship rather than the goodness of our wares.

I don't doubt that the establishment psychiatrists feel very reassured that they can muddle together in friendly amity for once. Their affairs have gone much worse in the last five years than they had expected, and although this can hardly be blamed upon megavitamins, denouncing them diverts everybody's attention from the growing

evidence of public disillusionment, resentment, and suspicion.

Establishments, especially medical establishments, are much the same the world over. They are concerned with Tightness which is the general consensus of the profession rather than goodness which is innovative. They don't like the boat being rocked, and the more waterlogged the boat the keener they are to insist that all is well. As I have noted here, in my view the establishment has resorted to threats, lies, damaging innuendos, deliberate omissions, and intimidations. It should be emphasized that this is not in the least bit unusual. It is simply the way of establishments. It does not imply that its members are especially crooked or self seeking. However, it equally does not imply that the establishment is correct. If one goes by the record, highly entrenched establishments have a very poor track record for being supported by history. Their members are too deeply buried in their defenses to be able to assess the situation of the opponents.

There has always been a substantial amount of fairly well-recorded scientific data supporting our work. It is very unfortunate that the establishment, as so frequently happens, did not bother to replicate our work. Once again there is nothing new or strange about this. It is the way of the beast. If one knows one is right, why bother to exert oneself to inquire into something better? Who cares if replicating experiments don't replicate? I have heard Dr. Morris Lipton attack megavitamin treatments as used today on the bizarre grounds that they were not easy to replicate and that techniques had changed over a period of 20 years. In surgery, for instance, surgeons go to the skilled surgeon's clinic to learn his technique. A clinician is under no obligation whatever to employ techniques which can be learned by any casual reader of clinical papers. He is merely obliged to state what he does and how he does it. Establishments scratch each other's backs because it is convenient to do so. However, since at the

moment, according to their own accounts, the psychiatric establishments in the U.S.A. and Britain too are in trouble, they have an even stronger motive to hang together.

**Humphry Osmond, M.R.C.P.,  
F.R.C.Psych.**

**Dr. A. Hoffer's letter not submitted to BMJ because it would be an exercise in futility**

May 14, 1975

Sirs:

Your editorial writer, presumably a psychiatrist, concludes that Orthomolecular psychiatry "ought to be dead but it won't lie down, so what is the controversy about?" Surely this is the key issue. His conclusion is that we have not followed scientific methods and that when our critics have run "controlled" experiments, meaning double-blind experiments, we have replied with verbal argument instead of with renewed experiment and fresh fact. This conclusion is very satisfactory for psychiatrists who on the basis of a priori reasoning know Orthomolecular psychiatry could not work, for those who are too slothful to read carefully the literature and therefore depend upon hearsay, upon the conclusion of other experts who also have not studied Orthomolecular psychiatry.

The main reason Orthomolecular psychiatry will not lie down although it has been declared dead for 10 years is that there are over 1,000 physicians in North America who have examined the methods, have used them as described, and who have treated over 50,000 schizophrenics. Their main supporters are the same patients most of whom had not responded to standard therapy of present psychiatry—tranquillizers. We have as yet not run across any group of schizophrenic patients who have become equally vociferous in favor of tranquilizer therapy.

The controversy is not a scientific controversy contrary to your editor's point of

view. It is a political controversy.

A scientific debate arises when two independent scientists conduct comparable experiments and come up with different conclusions. These controversies usually are resolved by a recognition by the scientists that there were differences in technique or design. A political controversy arises when a group which attempts to confirm (or to destroy) another's claims uses a different design or technique, draws different conclusions, but then assumes the conclusions have relevance to the original report. The following facts will clarify this situation.

The first double-blind experiments in psychiatry (at least we have never seen any previously published reports) were conducted under our direction in Saskatchewan beginning in 1952. Over the next 10 years our group conducted four double-blind experiments. We concluded:

(a) That the addition of vitamin B3 (nicotinic acid or nicotinamide) in at least 3 g doses per day to electroconvulsive therapy (ECT) which was then our only treatment improved the recovery rate from a natural recovery rate of 35 percent to about 70 percent. The double-blind design we used has become the present classic method. It uses randomized groups, blind evaluators, and patients unaware of what they are on. Our patients were carefully described, and many case histories were given. In the book labeled "uncritical" regarding Orthomolecular psychiatry by Hawkins and Pauling are listed all the pertinent references.

(b) On the basis of other work, O'Reilly (1955), one of our colleagues, on our urging reported that nicotinic acid alone did not help chronic schizophrenics.

These original claims were made before the tranquilizers became established. Since then Orthomolecular therapy has become considerably more complex as we have recognized that the schizophrenic syndrome arises from a variety of causes each of which will require its own treatment regimen. ECT

Original Studies (double blind) Studies Accepted as Proof by APA Task Force

Patients First illness or relapse  
after having once recovered

Location General Hospital

Treatment Nicotinic acid,  
nicotinamide, or placebo ECT where necessary  
Chronically ill for many years

Chronic - Mental Hospital

Nicotinic acid, placebo,  
tranquilizers  
No ECT

has remained a treatment for a small proportion of patients who have failed to respond to any other treatment.

So far the main studies referred to in the APA Task Force Report made no attempt to repeat any of the four original double-blind experiments. They have, however, confirmed our claims that nicotinic acid alone will not help most chronic patients.

The above comparison table shows the differences in design and treatment.

Our critics have adopted the following rules of logic:

(1) Do not consider any experiments completed in Saskatchewan since these are contaminated by bias, enthusiasm, etc., even though they were double blinds.

(2) Do not consider any medical report as evidence unless it is double blind, but not one from Saskatchewan.

(3) Any double blind, not matter how poor, is evidence.

(4) If any author reports both positive and negative findings, bury the positive and highlight the negative."

(5) If patients originally diagnosed as schizophrenic recover on vitamins, immediately doubt the diagnosis.

Wittenborn apparently was unable to accept the conclusion inherent in his own data that less sick schizophrenics responded better to nicotinic acid and tranquilizers than to placebo and tranquilizers. He instead suggested perhaps this entire group of 24 were really not schizophrenic, i.e., he obeyed the rule that nicotinic acid could not be therapeutic, that a response indicated the patient had not been schizophrenic. If the diagnosis of one-third of his group was invalid, it suggests his entire study is equally invalid and his earlier conclusions equally invalid. Wittenborn provided no proof the double-blind code had not been broken, while De Liz, a psychiatrist on his staff, reported that a few patients were aware they were on placebo and purchased their own vitamins while out of hospital. This is not referred to by the APA committee.

So far the four double blinds completed in Saskatchewan were corroborative, but then they need the same basic design. So far every physician who uses the published procedure observes similar improved results. All the negative controlled experiments used different kinds of patients (chronic) without following any of the published treatment regimen.

Our question to your editorial writer about our critics is, "Have they the capacity to be scientists obeying the first rule of corroborative science, i.e., when you try to corroborate published work repeat the experiment the way it was first done, i.e., the same kind of patients, the same design, the same treatment, and

<sup>1</sup> As an example, Wittenborn, Weber, and Brown (Archives of General Psychiatry 28, 308-315, 1973) reported that they found no significant difference between two groups given placebo plus tranquilizer and nicotinic acid and tranquilizer. The APA committee accepted this as an important study which invalidated our claims. However, they were then made aware by Wittenborn of a second study on the same groups (Wittenborn, Archives of General Psychiatry 31, 547-552, 1974) where he reported that he had applied pretreatment criteria and was able to select 24 (one-third of the total original group) who had a premorbid history of relatively strong inter-personally oriented commitments. From this group, 10 of the 12 on nicotinic acid were much improved compared to five out of 12 on placebo, a difference of 83 percent improvement compared to 41 percent on tranquilizer and placebo. The APA committee ignored this finding.

## **EDITORIAL**

the same method of evaluation?" When physicians decide to follow this first rule of science, they will soon join the ranks of Orthomolecular psychiatry.

Sincerely,  
**A. Hoffer, M.D.,  
Ph.D.**