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Intracranial Hemorrhage In Hemophilia

To the Editor.—Despite Hattersley's strong exception (216:2144, 1971) to my statement that "... intracranial hemorrhage in hemophilia is rare," I agree fully with his view and with his suggestion on the handling of head injuries. Fortunately, spontaneous intracranial bleeding is rare but head injuries will continue to occur. The following case illustrates how rewarding prompt and proper treatment can be.

Report of a Case.—The patient was born in 1949 and the diagnosis of hemophilia A was made at the age of 11 months on the basis of a clotting time of 60 minutes and a prothrombin consumption time of nine seconds. He had the typical episodes of bleeding which responded well to local treatment of cold and pressure and to transfusions of fresh-frozen plasma.

On July 4, 1965, he fell from his bicycle and bruised his forehead above the left eye. Two days later, he received 250 ml of fresh-frozen plasma, mainly because of the discoloration about the eye and after several days, he received three additional units of plasma. On July 19, he developed a severe headache followed two days later by nausea. A transfusion of 250 ml of fresh-frozen plasma was given on his arrival at the hospital. By this time he had a marked speech defect, word aphasia, and complete paralysis of the right arm. He was placed under the care of a neurosurgeon who decided against a decompression operation. He was given 250 ml of fresh-frozen plasma daily for four days. Gradual improvement was noted, and on Aug 8 he had completely recovered except for a slight

aphasia which disappeared by Aug 13 when he was discharged from the hospital.

It is from experiences such as this that I have been recommending that any head injury should be treated by transfusions of fresh-frozen plasma daily or every other day for a week to ten days. The patient should be given a transfusion as soon as possible after the head injury. I have added to this therapy the further recommendation that *under no circumstances* should aspirin be given. It is certain that in the past this analgesic taken for the headache resulting from a head injury probably caused the most harm.

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Marihuana "Study" Critique

To the Editor.—One can never adequately refute yesterday's headlines—the sensational is remembered, not the rebuttal. It is therefore frustrating to write regading the much-publicized communication entitled, "Effects of Marihuana on Adolescents and Young Adults" by Kolansky and Moore (216:486, 1971). Yet, though it will never arouse the attention already afforded the article, a critique should be done.

The report's first sentence causes concern. "The smoking of cannabis derivatives in the United States has now reached alarming proportions." Surely such activity is alarming only if known to be undesirable. Whether this is, in fact, true remains debatable. Given, at least, that harmful effects of marihuana are not well-established fact, the authors' choice of a word such as "alarming" immediately throws their objectivity into question.

Much of the communication is worded more cautiously: "It was our impression in these cases that the use of cannabis derivatives caused . . ." Unfortunately, the article is not entitled, "Our Impression of the Effects of Marihuana." Instead, it would appear that THE JOURNAL has published a communication about the effects of marihuana; it is only after study that we realize we are reading *impressions* of two psychiatrists of unknown bias and already (from their first sentence) questionable objectivity. But the headlines are already out: "Marihuana Perils Shown in Study."

The peruser of the morning newspaper does not know that each subject was diagnosed by only "one psychiatrist," that "most" (how many?) of the 38 patients in the study smoked

marihuana two or more times per week, that all patients, "except 11" (nearly one-third), were symptom-free prior to marihuana use, or that the "neurologic signs and symptoms" offered as evidence of marihuana's harm were not even documented by physical examination. Yet, he does know the authors reached conclusions such as, "Marihuana accentuates the inconsistencies of behavior, the lack of control of impulses, the vagueness of thinking, and the uncertainty of body identity which Anna Freud described." What evidence have the authors provided to support such a statement?

It is unfortunate that this communication was not subjected to the scrutiny advised in the paper published immediately following it: "Some Misuses of Health Statistics," by Ludwig and Collette (216:493-499, 1971). Particularly relevant is the discussion concerning errors in sampling. "The most obvious error in sampling lies in the use of a biased or non-representative sample to estimate incidence or prevalence in a population." When one selects cases from a sample limited to marihuana users referred for psychiatric help, he starts with a severely restricted group. The most obvious example of the type of patient excluded by such a sampling technique is the person who uses marihuana to the same extent as those studied but is either not in need of or not referred to the psychiatrist.

In short, the basic deficiency of this paper is its lack of a control group. Kolansky and Moore fail to consider persons of similar circumstances who do not smoke marihuana. Some may do better than those described, others may do worse; numerous adolescent psychotics have never used drugs of any kind, but the authors do not even broach the subject. In fact, they belittle the converse claim (that "large numbers of adolescents and young adults smoke marihuana regularly without developing symptoms . . .") because it is made without the "necessary accompaniment of thorough psychiatric study of each individual." The authors apparently realize the importance of rigorous controls to support contentions contrary to their opinions; it is thus ironic that the glaring absence of controls in their paper is its greatest fault.

That "conclusions" were formed from such data is unfortunate, especially in view of the publicity they generated.

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