Letters to the Editor

Classification of Diabetes

We would agree with Drs. O'Sullivan and Fajans[1] that neither the National Diabetes Data Group nor any other proposals should be accepted uncritically; indeed we have ourselves already entered into the polemic on the subject and we welcome it.[2] The proposals will no doubt require modification if and when relevant new data appear. However, the Group's report was the result of a critical review of the unsatisfactory situation concerning, at a certain level, the diagnostic criteria and, at another, the concept of diabetes itself. It took its direction from the availability of new data. Contrary to O'Sullivan and Fajans' statement, the blood glucose levels adopted were based largely on three prospective studies in the United Kingdom, and not on the Pima Indian data, which was regarded as confirming, in a different and special population group studied by different methods, the findings in the United Kingdom.

The major outcome of the Bedford and Whitehall prospective studies was the identification of a "more glucose-intolerant" group at risk of clinically significant retinopathy within a few years of identification, contrasting with a "less glucose-intolerant" group of persons who did not develop significant retinopathy even after 10 yr of observation. The risk of retinopathy, together with the proven value of treatment with photocoagulation, justifies labeling the first group as diabetic, with all that the term implies in medical care.

The absence of retinopathy and the different natural course of the condition justified the separation of the less hyperglycemic group into a separate category of Impaired Glucose Tolerance (IGT). It is also the case that in our studies (although not in all others) IGT also indicates an increased risk of coronary heart disease. However, while diabetic hyperglycemia is closely linked with, perhaps even directly causal of retinopathy, the association of hyperglycemia with coronary disease is indirect and varies widely between populations.[3] There is no proven therapeutic relationship between arterial disease and IGT, or indeed, diabetes itself.

The category of IGT is decidedly heterogeneous; in many persons it is a transient state reverting spontaneously to normal. It includes some people who are destined to become diabetic, but who cannot be considered as diabetic without extending the terminology beyond presently accepted limits. Would O'Sullivan and Fajans really have us label someone as diabetic because he or she did not have an acute insulin response to intravenous glucose?

Finally, O'Sullivan and Fajans misquote us—perhaps their worst sin! We did not suggest a modification of the Data Group criteria.[4] We simply reiterated the Group's proposal that when the fasting plasma glucose level was greater than 140 mg/dl, an OGTT was unnecessary. There are certainly individuals with fasting values less than this who would qualify for diabetes diagnosis on the basis of an OGTT.

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REFERENCES

[1] O'Sullivan, J. B., and Fajans, S. S.: Selecting criteria for the diagnosis of diabetes. Diabetes Care 3: 565–66, 1980.

[2] Keen, H., Jarrett, R. J., and Fuller, J. H.: Discipline and dissidence in science. Lancet 2: 315, 1980.

[3] Stamler, R., and Stamler, J. (Eds.): Asymptomatic hyperglycemia and coronary heart disease. J. Chronic Dis. 32: 683–87, 1979.

[4] Keen, H., Jarrett, R. J., and Alberti, K. G. M. M.: Diabetes mellitus: a new look at diagnostic criteria. Diabetologia 16: 283–85, 1979.

Selecting Criteria for the Diagnosis of "Diabetes Mellitus"

We thank Drs. Jarrett and Keen for their communication; we have read with interest their numerous Letters to the Editor in the recent past. We agree that the report of the National Diabetes Data Group was based "largely" on the prospective studies from the United Kingdom. However, they were also based "in large measure on the characteristic blood

glucose distribution in the Pima Indians"; this is not "contrary" but in addition.

Drs. Jarrett and Keen present again one rationale for choosing diagnostic criteria and in doing so emphasize a major point in our editorial: any such selection will depend on the choice of one of several reasonable objectives for diagnostic standards. We agree that the presence of diabetic retinopathy can be one basis for validating diagnostic standards. However, as pointed out in our editorial, this is not as clear-cut as presented by Drs. Jarrett and Keen. We would not agree that the diagnostic criteria should be based primarily on the availability of effective therapy in any branch of medicine, including diabetes mellitus. We do not suggest that anyone be labeled as diabetic solely because of the absence of an acute insulin response to intravenous glucose. We did indicate that such a response characterizes a biologic event (rather than a predictive or prognostic one), which could be another definition of diabetes exemplifying the range of objectives that criteria can have.

We are not aware that we have misquoted Drs. Jarrett and Keen, nor that we have sinned! We recommend that they reread their own editorial "Diabetes Mellitus: A New Look at Diagnostic Criteria" (Diabetologia *16:* 283, 1979). In Table 2 a definition of diabetes mellitus by OGTT included a fasting plasma glucose of >140 mg/dl. This view is upheld in the footnote to Table 2 and is contrary to their present statement that "There are certain individuals with fasting values *less* than this who would qualify for diabetes diagnosis on the basis of an OGTT."

Leaving "polemic" and "sin" aside, we are gratified to know that Drs. Jarrett and Keen are keeping an open mind also about the report of the National Diabetes Data Group and their above-mentioned editorial of a year ago, as appears from their opening statements.

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