A CRITICAL NOTICE OF BROWN ON "THE AGE AT MENARCHE"

BY

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Dr P. E. Brown's opinions on "The Age at Menarche" (*Brit. J. prev. soc. Med.*, 20, 9-14, 1966) and on the meaning of the word biology are unconventional, but at least they are his own. His opinions of what I believe, and his reports of what I have written, are so erroneous that in the interest of your readers they demand correction.

Writing of the causes of the trend towards earlier menarche, Brown says:

"Tanner . . . discounts explanations which rely on ordinary environmental agents, such as improved nutrition, poor social conditions, and increased psycho-sexual stimulation. He seems to prefer semi-cosmic causes: alterations in world temperature, the breaking-down of genetical isolates, and the selective effect of the reduction of bacterial infections".

In fact, when discussing causes of the trend, I wrote (1962):

"Nobody knows for certain why the secular trend has occurred. Better nutrition and generally improved environmental circumstances are usually given the credit, and with considerable reason".

In a later paper (1965), I wrote more explicitly:

"Probably better nutrition is the factor chiefly responsible, though there are certain facts, mentioned above, which at least make us hesitate before accepting this explanation too readily. It seems likely to be more than a matter of simple calories; and it seems likely to be due to an influence which starts early in life. The best guess might be that better infant feeding, and particularly giving infants more protein early in life, is more responsible than any other aspect of nutrition".

As for discounting poor social conditions, let your readers see the 1962 reference, pp. 139, 152, 154, and the section on the relation of menarche and number of children in the family.

I am supposed to be *for* "alterations in world temperature", but in 1961 I wrote:

"Again the trend has been ascribed to a rise in world temperature, although we have little evidence of the effect

of environmental temperature on rate of growth; what there is suggests that heat may either accelerate or retard, depending on humidity and other circumstances",

and in 1965:

"Hot climates used to be cited in the older literature as a potent cause of early menarche. But the evidence for this seems chiefly anecdotal, and nobody now supposes climate exerts more than a very minor influence, if any at all".

As to the breaking down of genetical isolates, I have explained (1962) that this might account for some of the trend in *adult* height, which is quite different from the trend towards achieving that height at a younger age. So far as I am aware nobody has ever suggested outbreeding as a possible cause of more rapid growth or earlier menarche, nor have they invoked "the selective effect of the reduction of bacterial infections". I cannot easily think where Brown got this idea from (was it 1961, top of p. 118, where I reject with scorn the notion that such a selection could have any effect on the *adult* height trend?).

There remains psycho-sexual stimulation, an increase in which Brown regards "as the most promising" explanation of the general trend in menarche. In this he shows good acquaintance with the work of Freud and Nabakov, for the earlier maturation is well established by the time of entry to nursery school. (There really is no evidence of any change in the position of menarche on children's height growth curves; on the contrary, individual longitudinal records show exactly similar relationships over at least the last 50 years.) I do not assert there is any really good evidence against this theory; but I do assert there is no evidence in favour of it.

Finally, I must comment on Brown's suggestion (p. 12, col. 1, lines 27-30 and on) that I selected data not on the basis that I said I did, but to accord with some hypothesis. This suggestion is quite unjustified. I can assure your readers that before choosing the

most reliable body of data I could find I read Backman's (1948) review with care and consulted every one of the several hundred references cited that I could find. Many of the data Backman refers to are statistically very poor and some reports include previous series. None can be used without a critical look at the originals. It appears that Brown did not spot Backman's miscalculation of the mean in one of the earlier English references, and why did he count Robertson's cases in twice? Why, too, did he in his Fig. 3 plot data relating to approximately 1913 at 1933 for graphic comparison with data relating to 1949 plotted at 1949? When I first plotted the results from the data I assembled, I certainly had no particular view of the matter.

It seems that Brown's distortion of my views may come at base from his curious concept of biology. He writes:

"If Tanner (1961) believes 'the secular trend . . . is one of the most considerable phenomena of human biology at present . . . and has a host of medical, educational, and sociological consequences', then it must be admitted that no comparable phenomenon readily comes to mind that is not the outcome of a prolonged evolutionary process".

May I suggest a few thoughts to fill the blank? Family planning for instance, and the trend toward smaller families; or migration both within and without countries: other examples may be found in a textbook of "Human Biology" (Harrison, Weiner, Tanner, and Barnicot, 1964). To Brown, biology seems to exclude the environment, nutrition, disease, and so forth—in other words the warp of the subject. This is an old-fashioned view and one from which I most emphatically dissociate myself.

REFERENCES

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CORRECTION

In the article by A. E. Philip and J. W. McCulloch (*Brit. J. prev. Soc. Med.*, 1966, **20**, 122) in Table I, the Rank Order for Variable 16, Ward 7, should read 11 (not 1 as printed).