

Letters to the Editor

Symptoms of stress predict musculoskeletal disorders

SIR—I refer to the paper by Päivi Leino in the September issue (vol 43, p. 293). Our studies started with the assumption that the stress symptoms scores in Leino's paper were expressions of a state of "stress" in or experienced by the respondents to our enquiry. The abundance and/or frequency of appearance of these symptoms would be a measure of the degree of the "stress" that our respondents experienced, consciously or not.¹² I now think that the concept of "stress" is too meagre in content to describe the phenomena under study. The concepts of "human need" and "deprivation" seem more proper for the purpose—they seem to make the object more understandable.³⁴ The richness and complexity of the concepts of human need and deprivation seem to correspond more accurately to the object of study, the human way of life.⁵

There is apparent in Leino's material an intimate association between the frequency/abundance of the stress symptom scores and the symptoms located by the respondents in the locomotor system. One might ask to what extent both kinds of symptoms reflect the same state of the subject. We have to remember that the latter symptoms were "fixed" by us into different parts of the locomotor system. The association between the stress symptom scores and chronic disease was not as intimate. I think that we have to keep in mind that the concept of "disease", as well as that of "cause of death", has been constructed by the medical community for administrative and professional purposes. If we accept this fact, it becomes easier to understand that the prevalence and incidence of "disease" may fluctuate for reasons that may have little to do with processes in the organisms of the human beings.⁶

In conclusion, I think that the stress symptoms scores in Leino's paper mirror, more or less quantitatively, states of deprivation, threats to the integrity of the human being within his way of life. If this proposal is accepted, it opens up one way to study the notoriously difficult subject of human needs and their satisfaction.⁷ True, the symptoms reflect nothing of the kinds of need that remain unmet, but something about this can be deduced from the specific features of the ways of life in which the persons to be studied live.⁴

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JEDDI HASAN
Institute of Public Health,
University of Tampere,
Box 607, 33101 Tampere, Finland,
and the Social Statistics Research unit,
The City University,
Northampton Square,
London EC1 0HB,

Adult body height and childhood socioeconomic group in the Swedish population

SIR—I should like to comment on the article "Adult body height and childhood socioeconomic group in the Swedish population" by A M Nyström-Peck and D H Vågerö, published in this journal in 1987 (41: 333-7). These comments mainly concern the authors' method of measuring height, their so called socioeconomic differences in height, and their conclusions.

Method of measuring height—Just asking a person about his/her height is regarded as one of the "cheapest and dirtiest" methods of estimating height. In addition we were not told whether the interview was made over the telephone. There is also reason to believe that overestimation/underestimation of height and weight is related to sex as well as to socioeconomic group. These factors were not controlled for in any way in the Nyström-Peck and Vågerö study. Reference to English and American data in this regard does not seem appropriate. Since the study covers the age groups from 16-74 years there is also reason to believe that this tendency to overestimate and underestimate has been changing over time.

So called socioeconomic differences—there is confusion over what the authors mean by

“differences”. In Results they refer to their figure showing the “association between body height and age” but they do not say whether these differences were tested. In the Discussion, however, they state that “The numbers are small and the differences are not statistically significant but they are concordant with the trend in the figure and with the mean values in table 3” (p. 336). In this sense they are referring to my study concerning urban children mainly born in 1955.¹ If I understand this correctly their sample of persons born in 1955 would at the time of the interview have been about 25 years of age. In their figure the difference between the mean height of 25-year-old women from senior salaried occupations and unskilled workers seems to be nil. For the 25-year-old men there seems however to be a difference between these extreme groups of about 3.9 cm. Now, if I am interpreting their figure and their tables correctly, they have actually tested two groups consisting of eight persons from senior salaried employees’ homes and 35 persons from unskilled workers’ homes. If so I can understand why the authors did not report the sample sizes and why they found no significant differences. Thus I question some of their conclusions.

The authors’ conclusions—I do not agree with the statement that adult height in Sweden today is associated with socioeconomic status in childhood (it could however, be a working hypothesis for a new study). Of course there is also the matter of the definition of an *adult*. So far as the sample born in 1955 is concerned, I think it was about that time the differences levelled out.

There is also another study confirming this. In 1976 Otto,² studying a total annual population of conscripts born in 1953, reported a correlation coefficient of $r=0.044$ between height for 18.5-year-old conscripts and social group (so the Nyströms-Peck and Vågerö study is not the first study on adult height and socioeconomic group). This coefficient was significant because of the large number of conscripts ($n=51\ 897$). However, it is not *relevant* since if you multiply r with itself you get 1.9 per thousand, in other words 0.2 per cent of the variance is common to height and social group, which is practically nothing. However, I am glad that they agree with my suggestion that “the height differences between social groups diminish with time due to a greater height gain in the lower classes” (p. 337).

What is interesting in this whole area of research, however, is that bad conditions in society or in subgroups within society affect stature as well as the tempo of physical growth. Tanner³ coined the phrase “Growth as a mirror of conditions in society”; stature can generally be used as a proxy for health and living conditions in the society. In developed countries,

however, socioeconomic groupings may not be so relevant today—not only because of the difficulties in defining meaningful groupings, but also because there are probably other factors in the environment that affect people more (pollution, stress, and so on). In these respects all socioeconomic groups are in the same boat. For example in a Swedish nationwide sample of schoolchildren born in 1967 (where the only socioeconomic differences found were in weight) we found that the birthweight for children in the city of Malmö was significantly (100g) lower than in the rest of the country.⁴ The area around Malmö is highly polluted, so it is tempting to infer that living in a polluted area is more of a danger today than being born in the home of an unskilled worker. I do not think that babies are any more resistant than fish and seals in this respect.

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GUINILLA W-LINDGREN
Department of Educational Research,
Stockholm Institute of Education,
Box 34 103,
S-10026 Stockholm,
Sweden

The authors reply as follows:

SIR—Gunilla Lindgren has commented on our previous article in your journal.¹ There we concluded that differences in body height by childhood socioeconomic group existed in the adult Swedish population of today, and, at the same time, that these differences were diminishing.

Lindgren has three objections to our study, namely: (1) our method of establishing body height is not valid; (2) the differences between socioeconomic groups are not statistically significant when analysing 1 year age groups; and (3) our interpretation of results is questioned.