Spontaneous Locomotor Activity and Life Span

A Reply to R.S. Sohal

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In a letter to the editor of Gerontology [p. 332–334, this issue], Sohal claims that the experimental design or, to be more precise, the technique that we used to test the rate of living theory [Lints et al., 1984] was inadequate. We do not agree with that statement.

Sohal acknowledges the fact that we found highly significant differences both in the rhythm of activity and in the mean activity scores of the flies under observation. Yet he severely criticizes the fact that in our apparatus the flies were not able to fly. His conclusion is that ‘in physically restricted flies, the actual differences in the level of physical activity among different individuals would be minimal’. Do we have to understand that, in Sohal’s opinion, the rate of living theory can only be tested when differences are maximal or, as suggested by Sohal, in ‘conditions which will maximize rather than minimize the differences in metabolic expenditure among different test animals’? Do we have to understand that, in Sohal’s opinion, the rate of living theory only applies to abnormally active flies (or living beings) and not to organisms displaying a peaceful and normal life? We feel that the answers to these questions are positive. Indeed, Sohal further ar-

gues that ‘it is likely that the deleterious effects of physical activity on life span, if any, would occur above a certain threshold of activity. This threshold may not have been exceeded by the voluntary walking activity of a fly confined within a small vial’. These ideas about the need for deleterious (!) effects of physical activity and about a threshold activity are new ones in the literature about the rate of living theory. In 1976, Sohal simply wrote, ‘it therefore seems that life span in the adult housefly is inversely related to physical activity’ without pointing to any threshold of activity. In 1928, when Pearl introduced his hypothesis, he said that ‘the length of life depends inversely on the rate of living’.

We acknowledge the fact that Sohal [Bu-chan and Sohal, 1981; Sohal, 1982] did try to relate the physical activity, the oxygen consumption and the life span of groups of flies (*Musca domestica*) maintained in various environmental conditions which altered the level of physical activity. Yet his results are open to criticism [Lints, 1983; Lints et al., 1984; Lints and Bourgois, 1985] and pertain mainly to a simple phenomenon of exhaustion – ‘the deleterious effects of physical ac-

tivity'!, as recognized by Sohal himself- as shown as early as 1924 by Pearl in a detailed study of human mortality in relation to the amount and intensity of physical labour.

In contrast with Sohal's works the study under discussion was aimed at finding a relation between measured individual scores of activity and individual longevities of approximately 600 flies (*Drosophila melanogaster*) of both sexes and of three different genotypes. The existence of such a relation among populations and among members of these populations, submitted to identical environmental conditions, would, of course, considerably strengthen the rate of living theory and, furthermore, endow it with a genetical foundation. Such relation could, however, not be disclosed neither at the populational, nor at the individual level [see also Le Bourg et al., 1984]. Sohal himself [Sohal and Buchan, 1981] measured the levels of activity and the life span of a modest sample of 11 males observed during 3 consecutive days. He found that the correlation between scores of activity and longevities was equal to -0.415 ($n = 11$; $p = 0.095$). In spite of the non-significance of that correlation Sohal and Buchan concluded that 'the levels of physical activity of flies did, in general, correspond to the life span of the flies'!

A last word should be said about the criticism of Sohal pointing to the fact that our observations were made during a single day. This was mainly done because of ergonomic constraints.

Considering the size of the sample measured by Sohal, we are sure that he will understand that point. Yet he is totally right when he says that 'life time estimates of spontaneous locomotor activity cannot be reliably made on the basis of observations on a single day'. We are perfectly aware of that limitation in our work and are currently

working on an experimental protocol which will allow us to measure locomotor activity throughout the life of our flies. In all fairness we feel that it may be said that in the study of the relations between measured physical activity and life span our data are the most extensive ever published. Yet we never contended that our results – which are what they are – were the last word in the experimental study of the rate of living theory. On the contrary, reviewing a relatively large, and sometimes contradictory, set of data – including our own ones – related to Pearl's hypothesis we concluded that it was urgent for gerontologists to investigate the problem on a new basis.

We are sure that Sohal's letter conveys a high interest in the theories of ageing. Many theories of ageing have been elaborated and discussed during the last few decades, yet too often on the basis of weak data. Theories of ageing actually need more hard data than strong convictions. We are modestly trying to provide such data.

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Spontaneous Locomotor Activity and Life Span

337

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