DISCUSSION Samuel H. Preston, United Nations Population Division

For various reasons I must confine my remarks to the paper by Stockwell, et.al.. This paper provides a useful review and updating of studies of census tract differentials in mortality. Such studies provide one of the cheapest means of documenting the existence of social differences in mortality. The authors are reasonably careful not to push their inferences beyond those which the data can support. They note many of the very serious limitations to which such studies are subject, particularly their inability to provide much detail on the sources of revealed mortality differences. They fail to note one of the advantages of this type of study, namely that its geographic specificity provides a valuable guide to structuring local governmental programmes of health care that are usually implemented on a geographic basis.

There is one very serious disadvantage of such studies, which is partially remediable by improved techniques of analysis. The size of differentials uncovered for one area during one period is not strictly comparable to the size of the differential derived for another population. Stockwell, et.al., attempt to draw inferences about whether differentials are contracting or expanding and whether they are larger in one city than in another. But I doubt that any such inference would be justified without much greater attention to issues of measurement. Take the case of one city in which tract differentials are being compared in 1960 and 1970. If the same tracts form the high and low group in both years, then there is obviously a problem that the social composition of one or both sets of tracts is likely to have changed during the period. If a different set of tracts is used. there is still the problem that the "high" or "low" group may have a quite different mixture of social groups in one year than in another. A tendency toward greater residential intermixture of social groups would obviously tend to produce a contraction of measured differentials between high and low areas, without involving any change in death prospects for individuals. Furthermore, changing the set of tracts can introduce exogenous factors associated with ethnicity, density, access to health care, etc. that will affect measured mortality differentials, without implying any necessary change in underlying relations. In this respect it is wise to remember that census tracts do not have mortality rates, only people do. Tract differentials are valuable only insofar as they are suggestive of individual differences in mortality. As I have suggested, the macro-micro translation problem is acute under present procedures.

It seems to me that a much better way to measure the tract differential would be to use the regression coefficient expressing the relationship between tract death rates and mean tract status on the indicator in question. Such a coefficient would, for example, express the effect of a one-year gain in mean adult educational attainment on the death rate. This effect could then be compared over space and time. No grouping of tracts whatsoever is required. Furthermore, the mixture of groups within tracts would have no effect on the measured differential so long as mortality is linearly related to the characteristic in question. This proposition is easily demonstrated algebraically. Non-linearities will continue to disturb measured relations, as will the occasional need to rely upon medians rather than means. But in general much more confidence could be placed in statements about the relative size of differentials. Such a treatment is readily generalized to one that recognizes various causes of death, since the cause-specific regression coefficients must sum to the regression coefficient for all causes combined. This aggregation property is absent when ratios are employed.

In his earlier review of social differentials in mortality, Antonovsky speculated that after a long period of contraction, differentials may again expand as a result of the development and slow social diffusion of methods of preventing the chronic diseases. Since we seem to have entered at last a period of persistently declining mortality from chronic diseases, it would be interesting to reexamine this proposition. Census tracts are a clumsy vehicle in this regard, but as Stockwell, et.al. quite rightly point out, they are an important stopgap until larger and more expensive studies of individuals are conducted. Census tract studies have served a valuable role in pointing out that social status is still a major dimension of variation in American mortality. When comparably-sized differentials were discovered for a personal habit such as cigarette smoking, there was an enormous outpouring of funds for research to discover the causes and mechanisms of effect. It is unfortunate that there has been no such movement in regard to class differentials. There are some obvious differences related to the specifiability of cause and effect relations. But it is probably also true that the biomedical establishment in the National Institutes of Health is by training and inclination more comfortable supporting studies of physical than of social factors. Demographers using "found" data must continue to call attention to the existence of major social inequalities in the length of life and hope that someone eventually pays attention and supports studies designed to uncover the causes.