# Comments on "Quasi-Biennial and Long-Term Fluctuations in Total Ozone"

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Angell and Korshover's discussion of the world ozone data (such as there are) is an interesting and indeed stimulating attempt at a global picture. There are several points on which I would like to comment.

1) The separation and grouping of the total ozone stations into latitude belts and other sub-groupings is a worthwhile attempt to demonstrate the representativeness of the data. Unfortunately, the major question regarding representativeness, at least in the Northern Hemisphere, concerns possible systematic differences between predominantly oceanic and continental longitude sectors (London, 1963). The possibility that the observed trends in total ozone over North America are primarily the result of the deepening of a long-wave trough was put forward by Komhyr *et al.* (1971) and gains support from Namias (1972) and others.

2) The procedure by which "total ozone is held constant at its endpoint value while the shorter records are extrapolated to the lengths of the longer records" does not necessarily lead "to the minimum (most conservative) variation in total ozone." Rather to the contrary, such a procedure places greater statistical weight on short-period fluctuations which may or may not have been reduced by a longer length of record.

3) The use of 12-month running averages in the discussion of the quasi-biennial fluctuations, while not uncommon [e.g., Pittock (1968) did the same], is liable to be misleading since it enhances the random variance in the spectral region centered on a period of about two to five times the span of the running mean, relative to other parts of the spectrum (Deacon, 1966). Additional confusion arises from the use of 30-month running averages which will contain an appreciable component of the annual cycle. By the time these effects are propagated through lagged correlations between different series of 12-month minus 30-month running averages,

the significance of the derived phase differences become rather dubious. I for one would need considerable reassurance before taking such results at face value.

4) Despite the preceding comment, there is much support for the possibility of an inverse relationship between stratospheric temperature anomalies in the tropics and those at higher latitudes, both on a short time scale (Fritz and Soules, 1970) and on a year-to-year basis (Pittock, 1973). This would appear to be related to fluctuations in the strength of the Hadley circulation, with the change of phase situated between about 10° and 30° N or S of the equator. A similar relationship should be expected for total ozone, but unfortunately the only station within 10° of the equator is Gan (0.41S, 73.09E) which has an insufficient length of record.

5) Regarding the possible influence of the Mt. Agung eruption, an examination of ozone data for all available years at Brisbane (28S), Aspendale (38S) and Macquarie Is. (53S) lends little or no support to the idea that 1963 was anomalous with respect to ozone, even though 1963 is apparently anomalous with respect to stratospheric temperatures. This is discussed more fully elsewhere (Pittock, 1974). The variation shown by Angell and Korshover in Fig. 9 of about 2% deviation from mean monthly values must be compared with the standard deviations of the corresponding monthly means. For 16 years of data at Brisbane these are 3.5% in June and 3.4% in July. It is thus clear that deviations at least as large as those observed in 1963 are observed at Brisbane in most years.

Contrary to Angell and Korshover's statements, total ozone fluctuations at Aspendale and Brisbane were not discussed in Pittock (1968), while Pittock (1966) concluded that "Significant destruction of ozone by the volcanic material is not indicated."

6) The reported difference in estimates of the significance level of long-term trends as determined by Angell and Korshover using the Mann-Kendall rank statistic and myself using the two-sided Student's $t$-test may well be due in part to the fact that Angell and Korshover used mean data from groups of stations whereas my estimates (Pittock, 1972) were based on the statistics of a single mid-latitude station. Assuming similar variability at $M$ stations whose variations are spatially independent, the uncertainty of an estimated trend over $N$ years will be reduced over that from a single station by a factor $M^{\frac{1}{2}}$. Neither assumption is correct, although Aspendale is at about the middle of the range of variabilities and therefore not altogether unrepresentative of other stations in the global network. It is probable (see e.g., Grimmer, 1963; Borisenkov, 1966) that an upper limit of about 8 or 10 independent pieces of information per month might be available from an ideal global network. This suggests that a maximum reduction in the uncertainty of an estimated trend from a global network compared to that from a single station would be by a factor of about three. There are of course other subtleties of statistical argument, but in my view the question of representativeness is dominant. I doubt whether the present global network is distributed well enough to provide more than half the number of independent pieces of information per month which would be available from an ideally spaced network containing fewer stations.

7) Regarding the possible effects of nuclear testing, which it is argued may have produced globally significant quantities of $NOx$ which might affect the ozone content (see also Foley and Ruderman, 1973; Johnston *et al.*, 1973; and Goldsmith *et al.*, 1973) all we can conclude from the world ozone data is that there has been *no large net change* in global ozone content (greater than say 5%) over the last decade due to a whole combination of possible effects. These include possible effects due to volcanic debris, changes in the quasi-biennial cycle, and the solar cycle, as well as nuclear testing. Until we quantify the other possible effects we cannot place observational limits on the effect of nuclear testing alone.

The discussion over the possible effects of nuclear testing is based on the theoretical production of a globally significant amount of $NOx$. It would be greatly strengthened by direct observational evidence of a widespread and significant increase in oxides of nitrogen.

Finally, on this point, Angell and Korshover's Fig. 15 is subject to much the same criticism as their Fig. 9 (see above), and remains totally unconvincing in the absence of statistical evaluation.

8) Kulkarni (1973) has pointed to a serious uncertainty concerning the effect of aerosols on the apparent total ozone amount. Given that aerosols due to human activity may well be increasing more rapidly in the Northern Hemisphere than in the Southern, this effect deserves careful evaluation in assessing long-term trends in ozone content. Volcanic debris might also be relevant at this point.

To sum up: while Angell and Korshover's paper is an interesting and stimulating contribution to the evaluation of global ozone data, I find a number of their conclusions less than convincing. Perhaps of most importance, the representativeness of the existing total ozone network must be seriously questioned as it is incapable of distinguishing between a truly global or hemispheric trend and one due to a deepening or shifting of the long-wave ridge-trough standing wave system. It is doubtful whether satellite-derived total ozone data as presently obtainable are sufficiently accurate to resolve this question.

Despite obvious logistical problems, these comments serve to underline the importance of setting up additional total ozone observing stations in the mid-oceanic areas of middle latitudes, using instruments fully compatible with the existing total ozone network.

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