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CORRESPONDENCE

Comments on the Draft paper for a New Technology Policy

[*Curr. Sci.*, 1993, 65, 374]

From the proposed New Technology Policy one would have expected clear guidelines for the technological and industrial development of the country. However, I find that the draft paper is unduly verbose, is unclear, fails to identify real issues and discusses material which is common knowledge. To me the following questions appear to be paramount as far as the technology policy is concerned:

(i) A clear statement on what technologies should be imported and those which cannot be. In the interest of the country, it is essential that we do not encourage import of technologies which can be easily developed in the country or for which there is no real need. Several examples can be cited, from Barbie doll to fast foods and garments. What we really need to import are the so-called high-technologies which will help us catch up with the rest of the world in crucial areas or those which will assist in upgrading our productivity.

(ii) There should be a clear policy decision on repetitive import of technology. This should not be permitted and even the first import of technology should be with the proviso that the technology will be adapted and improved upon and then resold if not globally, at least within the country.

(iii) A clear policy on intellectual property rights is called for. The present patent laws have cut both ways. It is essential that any new product/process developed within the country receives appropriate protection so that R&D investment becomes economically attractive.

(iv) Bulk of the technology development abroad is carried out by industrial houses. Ways and means must be spelt out so that the industry is made to invest in R&D in a big way.

(v) The role of the various government funding agencies should be clearly demarcated, as at present there is undue overlap. As a matter of fact, the need is not to draw up a new Technology Policy statement, but to see that the earlier Technology Policy is diligently implemented.

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SCIENTIFIC CORRESPONDENCE

Inconsistencies in the long term Indian total ozone data

Thanks for bringing out a comprehensive and interesting special section on atmospheric ozone in the 10 March 1993 issue. In this section, Rao[1] has drawn our attention to the doubts expressed by a number of scientific organizations on the quality of Indian total ozone data. In his concluding remarks, the author pointed out "The above results broadly support the view that the Indian data considered for this analysis are consistent, though there seems to be need for a further examination of data of Varanasi and Ahmedabad/Mount Abu."

I take this opportunity to bring to the attention of the scientific community some major sources of error involved in the total column ozone data along with a reanalysis of revised data over Ahmedabad/Mount Abu in an attempt to resolve the debate surrounding the quality of the recorded data. Basher[2] mentioned seven sources of error in the standard AD direct sun method of ozone observation with a Dobson spectrophotometer, out of which two most important sources of error, viz. optical adjustments and their historical improvements and extra terrestrial constant determination, could alone conceivably contribute trend-like error to archived data up to about 10 per cent per decade (1973–83) for some instruments. This sets a significant limit to the trend-detection capability of the data. Bojkov *et al*[3] mentioned that direct use of published data without quality assessment can result in erroneous statement and major disagreements in apparent trends even between stations located in the same

macro circulation region. The accuracy of Dobson instrument is strongly dependent on the quality of the instruments calibration and operation. Unfortunately, this quality can vary widely during an instruments history and only through the availability of periodic recalibration can the ozone data be revaluated Bojkov *et al.*[3] also mentioned that stations do not include corrections for periodic instrument calibration or for inter-comparisons with the world primary or regional standards despite repeated recommendations by WMO. The data from ground-based observing stations are also contaminated by ultraviolet absorption from atmospheric sulphur dioxide[4]. Dobson, the inventor of ozone spectrophotometer, pointed out in 1963 that very large amounts of SO_2 pollution could lead to interpretation as incrementally higher values of ozone[5].

The general technique for data revaluation begins with the identification of periods in which calibration shifts have occurred, e g. at Varanasi, calibration work was done towards the end of 1965 and 'N' values related to extra terrestrial constants were first revised with effect from 1 January 1966. This immediately showed up as an apparent decrease in ozone by 8 DU[6]. 'N' values at Varanasi were again revised with effect from 1 January 1967. The result was an apparent decrease in ozone amount again by 5 DU[7]. Obviously, such changes which are instrumental in origin and nothing to do with natural trends, will exhibit erroneous trend-like behaviour if the data are uncorrected for the apparent change in ozone caused by instruments periodic calibration. In fact, Basher also mentioned that 'It is particularly ironic that considerable efforts which have been made over the past decade to upgrade the adjustment and calibration of instruments, are themselves sources of some of the largest potential erroneous trends.' Similarly, 'N' values at Ahmedabad were revised several times, e g. on 1 September 1965 and then again on 1 November 1967. These revisions showed up as an apparent decrease in ozone amount by 5 DU[6] in the first case and an increase of 11 DU[8] in the second case. Such changes have taken place from time to time and at New Delhi also. I am tempted to mention this because of the potential danger involved in trend analysis from such ozone data without making corrections for the instrument-related trends as mentioned above and drawing hasty conclusions.

A meeting of experts was held in Toronto in 1982 and in its report a methodology based on local and international calibration aimed at thoroughly revising the past records of stations with long years of observations was outlined. WMO World ozone data centre recently published completely revised data set for many stations. In 1991 it published revised Dobson ozone data sets for Mount Abu/Ahmedabad for the period October 1951 to May 1985. The revised ozone data sets for other Indian stations have not been published so far by World ozone data centre. This data set over Ahmedabad/Mount Abu has been reanalysed here to make a fresh appraisal of the long term trend of total ozone over Ahmedabad/Mount Abu.

Figure 1 shows the plots of the 12-month running means (square marks) spaced three months apart, of total ozone deviations from monthly means at Ahmedabad/Mount Abu. For a study of long term trends and to minimize the effect of QBO (Quasi Biennial Oscillation), further 3 years running means were also evaluated as shown by plus marks. From the 3 years smoothed curve, the long term trend at Ahmedabad/Mount Abu seems to be as follows. The years 1952 to 1957 show abnormal low ozone values with a slow decreasing trend of about 9 DU

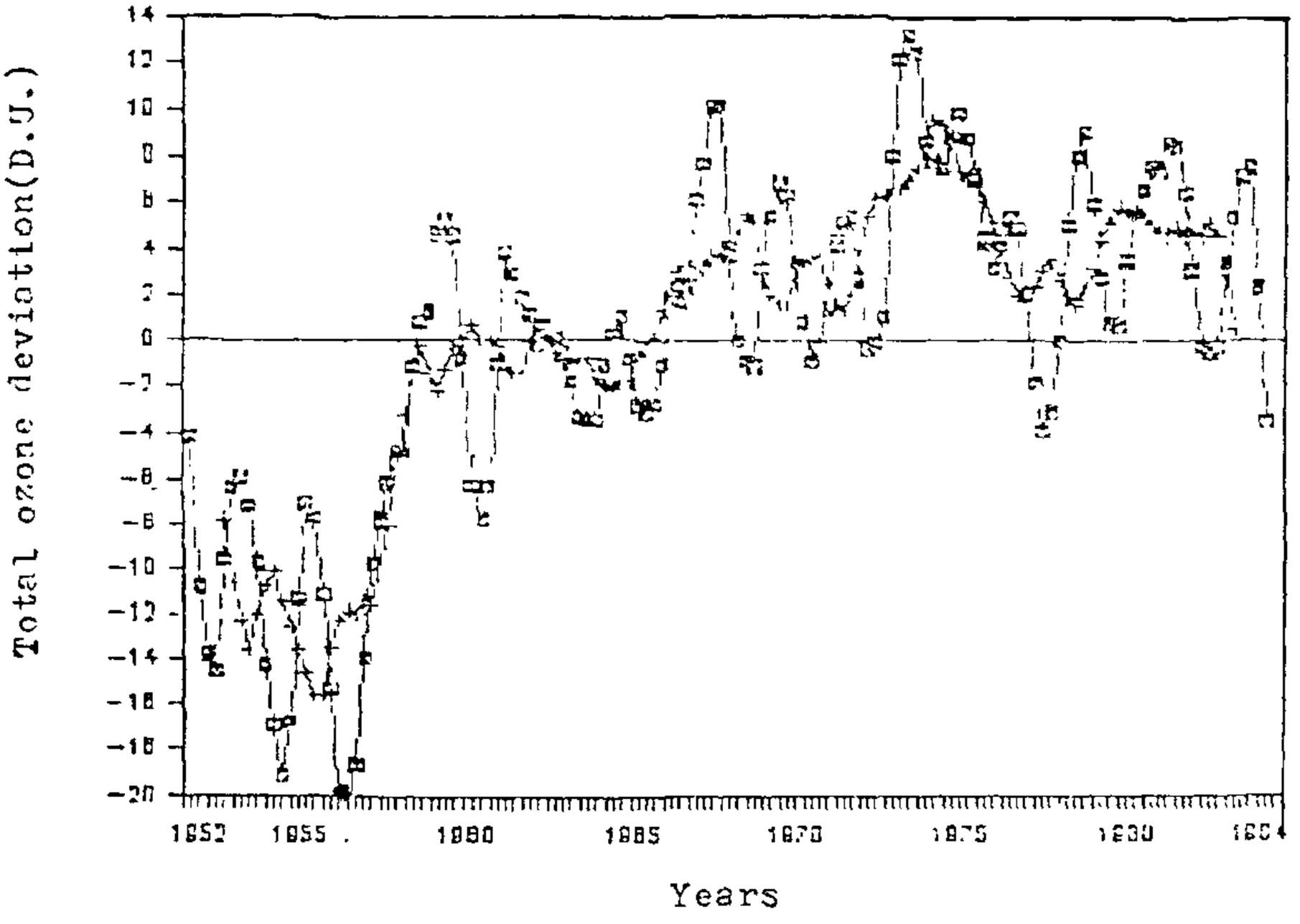

Figure 1. Twelve-month (square marks) and 3-year (plus marks) running means of total ozone deviations at Ahmedabad/Mount Abu.

during this period. Between 1957 and 1960 there is a rapid rise of total column ozone by about 13 DU. Both this abnormal low and the rapid rise thereafter seems to be a unique feature of column ozone over Mount Abu. No significant trend was observed between 1960 and 1965, a rise again of about 10 DU from 1965 to 1974, and a slow decrease thereafter by about 9 DU from 1974 to 1983. Before revision, Ahmedabad/Mount Abu total ozone data showed a marked rise in the year 1976–84 (ref. 1) which was very different from the decreasing trends of other Indian stations, but after the revision, it became in unison with the others. This is also in agreement with the overall down trend in total ozone observed over the latitude zone 26–60° N during 1970 to 1986 using revised Dobson total ozone data[3].

The analysis presented here indicates the presence of inconsistency in the unrevised Ahmedabad/Mount Abu column ozone data. It is hoped that this paper will serve as a reference for the analysis of other Indian ozone data sets in future.

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Comments on 'A computer simulation model for action potential in an excitable membrane'

[M. P. Mujumdar and C. K. Mitra, *Current Science*, 1990, **59**, 920–925]

We would like to raise the following points about the above paper:

1. On page 3 it is mentioned that the authors 'have found GHK equation to represent satisfactorily the potential of the membrane' under non-steady conditions'. But the basic assumption in deriving the GHK equation is that the membrane is in the *steady state*, since in the derivation of the equation total transmembrane ionic flux is set to zero[1]. This is a very basic tenet of electrophysiology, and in view of this, the authors should have at least given a more detailed justification for the use of their modified equation on page 3, which they have failed to do.

2. Figure 2, on page 3, shows that the permeability of potassium (P_K) *decreases* during the action potential. But it is well known that P_K also increases during the action potential, although with a time lag compared to P_{Na} (ref 2). We are not aware of reports in the literature of nerve action potentials in which P_K *falls* during the depolarization phase (although this may occur in cardiac action potentials; see ref. 3).

3. In Figure 3, on page 4, it seems that the rates of depolarization and repolarization of the action potential are almost identical. However, it is experimentally found that the rate of depolarization is considerably faster than that of repolarization. The authors have not commented on this important lack of agreement between experiment and theory.

4. On page 4, at the end of para 1, the authors state that 'the threshold range for this particular system is 55–59 mV'. However the thresholds of experimentally observed action potentials lie in the range – 50 to – 35 mV. If one needs to depolarize a membrane to + 55 to + 59 mV to generate an action potential peaking at + 50 mV (as apparent from Figure 3), then it is unclear as to how this 'threshold' is defined, since the stimulus itself is bringing the membrane potential to its value at the peak of the action potential!

5. The most glaring error in the paper, which seems to contradict all known physiological facts, seems to be the following. In the authors' simulation the action potential, and the ionic permeability changes underlying it, last for only about 5 nanoseconds. This does not seem to be a printing error as it appears in no less than three figures (Figures 2, 3 & 4). However, it is well known from repeated experimental observations that the nerve action potential lasts for at least a *millisecond*, if not more (for original observations see refs. 2 and 4). The discrepancy between the simulated time course and the observed time course is therefore no less than 1,000-fold, and it makes us wonder how far we can trust the validity of the simulation.

The authors have considered the ionic concentrations of squid axon, on which a lot of research was done. Therefore we have also referred to the above observations on the same axon. It is clear that the simulation is at odds with several physiological features of the action potential of the unmyelinated neuron. But in the Discussion the authors have ignored these issues, and have paid addressed any of the discrepancies.

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