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Temperature fluctuations and trends over the earth

By H. E. Landsberg and J. M. Mitchell, Jr.

One of us (Mitchell 1961) recently completed a study similar to that of Callendar. Inasmuch as this study was based on a rather different selection of data and method of analysis, due originally to Willett (1950), it is of some interest to compare Callendar's 30-yr changes of annual mean temperature in various latitude bands, shown in his Table 1, with the comparable changes derived from Willett and Mitchell's data.

Such a comparison is shown in the accompanying Table, and reveals good agreement in all latitude zones except the south temperate zone. There, Callendar's value is about twice as large as that of Willett and Mitchell. Without further information on Callendar's choice of stations in that zone, the reason for this discrepancy is obscure. At any rate, the differing trends found by Callendar for north temperate and south temperate regions is seen from Willett and Mitchell's data *not* to have followed solely from the fact that dissimilar increments of latitude were compared : a point that seemed to us to deserve verification.

The generally favourable outcome of the accompanying comparisons tends to justify Willett and Mitchell's earlier belief that little is gained by the use of more than the 120 to 180 stations they chose for their studies. This follows because additional stations are concentrated in relatively small geographical regions, whence the information they contribute to trend analysis is highly redundant.

Callendar's estimate of the magnitude of the ' urban effect ' in his data agrees rather well with an estimate Mitchell has made for his and Willett's data. We can therefore agree with Callendar that urban growth has probably had a relatively small effect on the computed global trends, although we share a hope that this matter can be investigated more thoroughly in the future.

We also agree with Callendar that the short-term fluctuations and the long-term trends of global climate should be attributed to different causes. On the other hand, Callendar's reasons for dismissing the solar and dust hypotheses of long-term global trends do not appear compelling. It seems to us that the *direct* influence on the global temperature field of factors that change the heating field of the atmosphere may become overwhelmed by the *indirect* influence on temperature governed by induced changes of the general circulation. Incidentally, the dust hypothesis may require further attention because of the remarkable variations of dust in interplanetary space detected by means of satellite observations (Dubin 1960).

Further, several facts conspire to challenge Callendar's conclusion that the lesser secular warming of the south temperate zone, as compared with the north temperate zone, may be attributable to greater industrial CO_2 production in the Northern Hemisphere. Firstly, a comparison of trends for larger but equal ranges of latitude in the two hemispheres reveals that the excess of warming in the Northern Hemisphere is relatively small (see comparison for 0-60°N and 0-60°S at the bottom of the accompanying table), and secondly, Vogel (1961) has recently determined from C^{14} measurements that the mean exchange time of air between the northern and southern tropospheres is of the order of one or two years. This represents a rate of mixing that is much too rapid to enable a substantially greater secular increase of industrial CO_2 in the Northern Hemisphere than in the Southern. Vogel has also assembled new data on the secularly increasing dilution of C^{14} in both hemispheres, due to fossil fuel combustion, that indicate approximately equal secular increases of CO_2 in both hemispheres. It thus appears that the solitary tree in the Southern Hemisphere analysed from this viewpoint by Arnold and Anderson (1957) was for some reason anomalous. Thirdly, the observations over the Atlantic from 60°N to 60°S published by Takahashi (1961) show no significant differences in atmospheric CO_2 concentration between the hemispheres.

A number of other questions come to mind when reading Callendar's paper. For example, how many stations contributed data for the various curves in his Figs. 1 and 2 ? What governed the unique choice of areas for comparison in Fig. 2 ? And why were available early records (before the 1890's) for Japan not used to extend the curve in Fig. 2 for that nation ?

30-YR CHANGE OF ANNUAL MEAN TEMPERATURE FOR VARIOUS LATITUDE ZONES*

	Callendar		Willett-Mitchell	
Zone	Latitude limits	T (C°)	Latitude limits	T (C°)
North temperate†	60-25°N	0·39	60-20°N	0·32
Tropical†	25°N-25°S	0·17	30°N-30°S	0·19
South temperate†	25-50°S	0·14	20-50°S	0·06
			20-60°S	0·10
N-T-S†	60°N-50°S	0·23	60°N-60°S	0·21
			80°N-60°S	0·27
Northern Hemisphere			80-0°N	0·36
			60-0°N	0·24
Southern Hemisphere			0-60°S	0·19

* 1921-1950 minus 1891-1920 (Callendar); 1920-1949 minus 1890-1919 (Willett and Mitchell). Willett-Mitchell data based on records for between 120 and 180 stations, not more than one station from each 10°-latitude-longitude square.

† Terminology after Callendar.

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U.S. Weather Bureau,
Washington, 25, D.C.
20 March 1961.

Reply

By G. S. Callendar

I was glad to see such good agreement between my zonal temperature changes and those computed from Willett's and Mitchell's data, apart from the south temperate zone, where, as I mentioned in my paper, the nineteenth century values are few and partially unreliable. Hence, for this zone, a more reliable change period is 1921-50 minus 1901-30, which is covered by about 40 stations in *World Weather Records*. This period gives a rise of 0·10°C, quite close to Mitchell's value for the longer period.

I fully agree with their conclusion that the indirect influence on temperature of induced changes in the general circulation may overwhelm primary effects. The recent big rise in the sub-Arctic zone is an outstanding example of this kind. On the other hand, if increased solar heating is assumed to be the cause of rising trends in world temperature, it is difficult to account for stable temperatures over the past 70 years in the arid basin of west central Asia, or sunny Australia. One would expect a primary response of temperature to solar heating changes in these areas.

As regards the distribution of excess CO_2, I think it best to compare the temperature trends in the westerly wind zones of north and south rather than the whole hemispheres, because almost all the industrial CO_2 has been released in 30°-60°N latitude, and would take longer to reach similar southern latitudes.

The new data quoted certainly indicate a more even distribution of CO_2 over the earth than I had assumed in my paper, and the mixing time for the whole atmosphere of 1 to 2 years seems remarkably rapid. No doubt the thermal inertia of the southern oceans and ice could account, in part at least, for the much smaller temperature rise there.

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An alternative derivation of the diabatic wind profile

By C. H. B. Priestley

For bridging the transition in the wind profile between free and forced convection there is now Ellison's interpolation formula (*J. Fluid Mech.*, 1957, p, 456) and my own smoothing hypothesis (*Q.J.*, 1960, p. 232), both of which fit existing data as well as could be hoped; the derivations of Businger (*J. Met.*, 1959, p. 516) and Swinbank (*Nature*, 1960, p. 463), more fundamental in intent; and Panofsky's recent elegant analysis (*Q.J.*, 1961, p. 109) which at first glance appears to combine both virtues. This, however, contains one vital assumption which is made but not stated, and some explanation is due before the contribution can be properly judged.

In the usual notation the open assumptions are that

$$K = f(z, \epsilon_1, \epsilon_2) \quad \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where the suffixes 1 and 2 distinguish the rates of creation of mechanical and convective turbulent energy respectively. Actually the vertical velocity is used as intermediary but this is unnecessary. From dimensional reasoning then, so Panofsky states,

$$K = C\, z^{4/3} (\epsilon_1 + \gamma\, \epsilon_2)^{1/3} \quad \quad . \quad . \quad . \quad . \quad (2)$$

where C and γ are constants. There would be little room for quarrel with this thesis if it were held that it was the *total* energy creation which was physically relevant, i.e. $\gamma = 1$. But this is clearly not the case, and it is found empirically elsewhere that (2) is most successful with γ about 15 : ϵ_1 and ϵ_2 therefore operate in quite different ways, as indeed is now well known from fine-structure studies.

All that dimensional reasoning from the stated premises really tells us is that

$$K = C\, z^{4/3}\, \epsilon_1{}^{1/3} f\left(\frac{\epsilon_1}{\epsilon_2}\right) \quad \quad . \quad . \quad . \quad . \quad (3)$$

where f is a function to be discovered by experiment or by further physical reasoning. What, one must therefore ask, is the underlying physical thinking which selects (2) from among the infinity of forms contained in (3) : or has Panofsky merely shifted the old empiricism, without changing its character, by writing down an interpolation formula for K instead of for $\partial V/\partial z$?

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