Reply

Dear Sir,

There are two questions here: the first is the geophysical problem of the effect of a change in the CO_2 content on the temperature of the earth's atmosphere; and the second is the comparison of methods for performing the radiation calculations. Let us consider these two questions separately and see who and what is "misleading".

In my *Tellus* article (KAPLAN, 1960) it was the geophysical problem that was discussed; and the comparison with Plass' results in the conclusion and abstract was based on the assumption that Plass' results were presented as results for the real atmosphere. The comparison of our clear air fluxes was introduced only parenthetically and as an indication that part of Plass' overestimate of the temperature change is due to errors in his method. Since the accuracy of my method has been questioned by Dr. Plass, I will return to this later; but first we must clarify what it is that is being discussed.

In his *Tellus* article PLASS (1956 a) uses the value 3.8° C for the effect of a halving of the CO_2 in his discussion of the influence of CO_2 variations on past climate; and it is only this value and 3.6° C for the effect of doubling the CO_2 content that he gives in his conclusion and abstract.

At one point in the article he mentions that "the actual temperature change due to CO_2 variations is slightly smaller than the results given above for clear sky conditions. When clouds are present, the CO_2 is less effective ... the result is obtained that the average surface temperature must increase by 2.5° C or decrease by 2.7° C when the CO_2 amount in the atmosphere is doubled or halved". These "slightly smaller" numbers are never used in the subsequent discussion, and in later articles on the effect of CO_2 variations on the climate of the earth PLASS (1956 b, 1959) does not even bother to mention that the values of 3.8 and 3.6° C are for clear skies.

If Dr. Plass is not talking about the actual atmosphere, but about a cloudless one, he should make it clear. He has definitely given the impression that he was dealing with real geophysical problems rather than the formal problem of a cloudless atmosphere.

Plass' overestimate of the effect of CO_2 changes is mainly due to three causes, of which the tacit assumption of a cloudless atmosphere is one. A secound source of the overestimate is his underestimate of the shielding by the overlapping water-vapor band. In a paper given at the 1960 Helsinki meeting of the IUGG, Kondratiev found that this shielding was sufficient to cut down the CO_2 effect to a small fraction of its original value.

The third cause of Plass' overestimate is the error due to his method of calculating radiative flux. Dr. Plass claims that my calculations are not accurate enough for the comparison, and gives a long list of sources of error. Let us now consider what he feels are "the most serious defects" in my calculations.

Of these, he feels the worst is my truncation of the atmosphere at 100 millibars. He says "I have repeated my calculation, but with Kaplan's boundary condition that the downward flux is zero at 100 millibars. The result is that this boundary condition reduces the value for the change in the downward flux at the surface of the earth for a 50 percent decrease in CO_2 amount by 8.1 percent from the value obtained when this approximation is not made".

That this result is impossible is immediately evident from Plass' Figs. 7 and 5 in the article in which his radiation calculations are discussed (PLASS, 1956 c). In these figures he conveniently divides the fluxes into the following spectral regions: two almost transparent regions from 12 to 13 μ and 17 to 18 μ, an almost opaque region from 14 to 16 μ, and two intermediate regions from 13 to 14 μ and 16 to 17 μ. From his Fig. 7 it is seen that the downward flux in the transparent regions is zero at 100 mb and that none of the 100 mb change in the opaque region is propagated to the ground. The entire effect of the approximation must therefore come from the intermediate regions.

But according to this figure the total flux change in the intermediate regions at 100 mb is itself only 8 per cent of the total flux change at the ground. This means that the entire effect of the approximation at 100 mb must be propagated without diminution to the ground!

The curves in Plass' Fig. 5, showing the temperature dependence of the downward flux, prove that the entire effect *cannot* be propaga-

ted to the ground. The temperature dependence, which must have the same sign throughout the intermediate spectral regions, is seen to completely disappear at the ground.

This is in agreement with my estimate of the error due to the truncation at 100 mb, which I find to be less than one-half of one per cent. It is precisely because of the small propagation of stratospheric emission due to the shielding by the wider lines of the troposphere that I felt I could truncate the atmosphere at 100 mb and carry the constant lapse-rate to this level.

Dr. Plass feels that considerable, though "somewhat smaller errors have been introduced by the use of the Curtis-Godson approximation". Two of the advantages of the Curts-Godson approximation are that is it objective and, as Dr. Plass has learned, an upper limit can be placed on errors introduced by its use. The upper limit of 5.8 per cent cited by Dr. Plass is not applicable to the spectra under consideration, as η (see Plass' letter and KAPLAN, 1959) decreases with height for the lines with η near unity; and his "estimate that the Curtis-Godson approximation introduces an error of several percent in the total absorption of this band" is definitely wrong. A careful study of the error analysis contained in my discussion of the approximation (KAPLAN, 1959) should convince him that the error introduced by this approximation cannot possibly be as high as one per cent.

The other "serious defect" in my calculations seems to be that "the absolute accuracy of the theoretical calculations is limited by the experimental errors in the determination of the integrated band intensities which are at least of the order of 5 percent".

As the line-intensities enter the radiative transfer equations only as a factor into the CO_2 concentration, and *vice versa*, the small effect of change in concentration on the radiative flux shows that high accuracy in the values for the line-intensities is not required for accurate final results. Even accepting Plass' results for the cloudless atmosphere, it follows that an overestimate of all intensities by a factor of two will change the effect of a factor of two decrease in CO_2 content from 3.8° C to 3.6° C. To the extent that the intensity errors are less or of different signs, this small error in the results will be decreased.

I agree with Dr. Plass that my approximations were not adequately discussed in my *Tellus* article. To have done so would have made the article unreadable to the audience to which it was directed. However, details of the method and estimates of error were conscientiously given in the *Rossby Memorial Volume* article (KAPLAN, 1959) to which I referred and in the references in the latter to the basic data which I used. In view of reasonable space limitations, I must again refer Dr. Plass to these articles for answers to most of his points. In particular, my method is quite general and Dr. Plass cannot have read the *Rossby Memorial Volume* article carefully to have come to the conclusion that "Kaplan's method is only valid for a constant lapse rate".

I am sorry that Dr. Plass got the impression that I was claiming five figure accuracy. It would indeed have been better to round off the numbers in Tables 1 and 2 to two or three figures. The highest accuracy that I feel I can place on my results is an order-of-magnitude better than our differences. This, however, is enough to claim definitiveness, I would think.

Sincerely yours,

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