

LETTERS TO THE EDITOR

"The Influence of Carbon Dioxide Variations on the Atmospheric Heat Balance", by L. D. Kaplan, *Tellus* 12, pp. 204—208 (1960).

Dear Sir,

In a recent article with the above title, Kaplan (1960) describes the results of his carbon dioxide calculations, which according to his own evaluation can "be accepted as definitive". On the other hand his article criticizes my earlier treatment of this problem which Kaplan describes as "inadequate", "approximate", and "too subjective to be reproducible". In addition in his abstract Kaplan writes that "Plass' estimate of a temperature drop of 3.8°C due to a halving of the carbon dioxide concentration appears to be too high by a factor of two or three." This statement would seem to be somewhat misleading since Kaplan compares two entirely different quantities: my result for clear skies with his result for cloudy skies! Actually, when our results for the temperature drop are compared under approximately the same conditions, there is only a 22 % difference between them. This question is discussed in more detail later in this letter.

Kaplan has made a number of approximations in his calculations, none of which are adequately discussed in his article in *Tellus* nor in the Rossby Memorial Volume (KAPLAN, 1959). Some of these approximations introduce significant errors in his final results which are tabulated to five significant figures. Thus the reader naturally presumes that these results are also accurate to five significant figures. Unfortunately this is not the case since errors in the first two significant figures have been introduced into his calculations by the truncation of the atmosphere at 100 millibars. Somewhat smaller errors have been introduced by the use of the Curtis-Godson approximation. Furthermore the absolute accuracy of 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.

Before I discuss the differences between our calculations, I should like to emphasize that climatic changes which occur over time intervals of geological significance can be explained by the carbon dioxide theory regardless of whether the temperature decreases by 3.8°C (PLASS, 1956b) or by 1.8°C (KAPLAN, 1960) when the CO_2 amount is halved. Our present knowledge of the relative importance and the magnitude of the various factors which have entered into the carbon dioxide cycle during past geologic eras is very limited. Many of the climatic changes which have occurred over the past several billion years of the earth's history can readily be explained by variations of the atmospheric CO_2 amount. Sufficiently large variations are entirely consistent with the known facts even if Kaplan's value is adopted for the temperature change. On the other hand, the difference between our two values for the temperature change is significant for the explanation of the worldwide temperature increase which has occurred in the twentieth century. If Kaplan's value is correct, this recent temperature increase cannot be explained entirely by the CO_2 theory. If my value is correct, the CO_2 theory offers a possible explanation.

I would like to discuss the following points about Kaplan's calculation and his comparison of our results:

(1) In my opinion the most serious defect of Kaplan's calculations is the truncation of the atmosphere at 100 mb. Kaplan must have assumed (although he does not state this) that the downward flux is zero at 100 mb since the flux above this level cannot be obtained by the method as described.

KAPLAN (1959) correctly states that the equation for the net upward flux at a given height must be integrated to some

arbitrary level chosen so that virtually no radiation from above this arbitrary level can reach the level at which the flux is being evaluated. In the present article Kaplan takes this arbitrary level to be at 100 millibars. This is equivalent to the assumption that the downward flux is zero at 100 millibars. A glance at Fig. 7 of my article (PLASS, 1956b) shows that the value of the downward flux over the entire band at the 100 millibar level is already 14 percent of its value at the ground. Since this emission from the stratosphere is entirely omitted from Kaplan's calculations, this downward flux should be added to his results which are given in Table 1 for the net radiation at 100 millibars. This correction becomes relatively more and more important the nearer the cloud top is to the 100 millibar level. A smaller error is introduced into the results of Table 2 for the net radiation at the ground since his results do not include the fraction of the radiation emitted above 100 mb which reaches the ground.

The emission by the molecules in the stratosphere in the wavelength band from 12 to 14 microns and from 16 to 18 microns has an important influence on the change in the infrared flux with carbon dioxide concentration. The physical explanation of this fact is that the emission in a given frequency interval of this radiation by the molecules in the stratosphere increases more rapidly with the carbon dioxide concentration than does the absorption of this same radiation by the lower atmospheric layers. In order to check this point quantitatively I have repeated my calculation (PLASS, 1956b), but with Kaplan's boundary condition that the downward flux is zero at 100 millibars. Consider the difference ΔI between the downward flux when the CO_2 has its present value and when it is 50 percent of this value. The result is that this approximative boundary condition reduces the value of ΔI by 8.1 percent. Thus Kaplan's values for ΔI the change in flux with carbon dioxide amount should be increased by approximately 8.1 percent of this amount because of his incorrect boundary condition at 100 millibars.

(2) Some of the temperature distributions used by Kaplan are quite unrealistic. He assumes a constant lapse rate from the ground to the 100 mb level. Consider first the case when $\kappa = \frac{1}{3}$. If this corresponds to a lapse

rate of 11.2°C per km as stated in his text (however 11.4°C per km is used in his tables!), then, when the surface temperatures are 273°K and 313°K , the 100 millibar level is at 13.0 and 14.7 km respectively and the temperatures at this level are 127°K and 146°K respectively. These values clearly do not correspond to any temperatures that have ever been observed in our atmosphere. The values are somewhat more reasonable when $\kappa = \frac{1}{8}$. When the surface temperatures are 273°K and 313°K , the 100 millibar level is at 15.5 and 17.9 km respectively and the temperatures at that level are 186°K and 213°K . However, neither of these distributions represents the average temperature variation with height in our actual atmosphere with much accuracy. In contrast to these unrealistic temperature distributions my calculations were made with a distribution which is very close to the actual observed values. In these calculations the temperature decreased with a constant lapse rate of 6°K/km up to 13 km, had the constant value of 210°K from 13 to 22 km, and then increased again.

(3) In his calculation Kaplan uses the Curtis-Godson approximation. For η near unity this can cause errors as large as 5.8 percent in the absorption between surfaces at one and zero atmospheres (KAPLAN, 1959). This error decreases as η becomes either very small or very large compared to unity. Thus it is not easy to estimate the error introduced by this approximation for the total flux from an actual band which contains a wide range of values of η . Nevertheless, the fact remains that the spectral lines with η near unity are the most important for the effects under discussion here. When η is very small the lines may be too weak to contribute to the total unless they are quite numerous. There are only a few spectral lines with large η . There is little change in the absorption from these strong lines as the CO_2 concentration changes because they overlap the neighboring lines. When all of these effects are taken into account, I estimate that the Curtis-Godson approximation introduces an error of several percent in the total absorption of this band.

(4) It would seem that Kaplan should have presented sufficient details of his calculations so that the interested reader could recalculate any of his results. However, he has not given

us the most fundamental data which enters into his calculation: the total band intensities. In a private communication, he has stated that they are nearly the same as those given by YAMAMOTO and SASAMORI (1958). But exactly what values did Kaplan use?

Kaplan states that he used "better laboratory data" than was available when my calculation was performed. The reader must presume that this is the data referred to by KAPLAN (1959, p. 176). He does not tell us what criteria were used to decide that this data is better than the careful measurements of Cloud. Furthermore, the measurements which he uses were not made for the primary purpose of obtaining band intensities. Actually the most comprehensive and possibly the most accurate CO_2 measurements have just been completed at Ohio State and have not been used by either of us. How well do Kaplan's calculations actually agree with laboratory data over a range of path lengths and pressures? It would seem that he should give us some detailed comparisons or sufficient data so that the interested reader can make his own comparisons.

(5) Kaplan does not state what isotopes of CO_2 were included in his calculation. Even some of the rarer isotopes can make an appreciable contribution over a path length through the entire atmosphere. For example, even though only 0.2 percent of the molecules are $\text{C}^{12}\text{O}^{16}\text{O}^{18}$, the contribution of this molecule to the total absorption can be appreciably more than this amount in a frequency interval where a maximum of one of its absorption bands corresponds with a region of small absorption for the most common isotope. If the isotopes were taken into account in the calculation, how were the intensities and frequencies of their spectral lines obtained?

(6) Kaplan's method uses the random Elsasser model to represent the absorption of the band. In each frequency interval a small number of sub-bands are found such that the spectral lines of each sub-band have approximately equal spacing and equal intensity. How many sub-bands did Kaplan use in typical frequency intervals? What intensity did Kaplan choose for each of these sub-bands? The reader can only guess that the actual intensities of all the spectral lines of this sub-band for the given frequency interval were averaged and that this average intensity was used in the

calculation. This is probably a good approximation although the intensities of a single sub-band may vary by a considerable amount in an interval of 10 cm^{-1} .

(7) The effect of the wings of strong spectral lines which are one or more frequency intervals away from the one under consideration may contribute appreciably to the absorption in some spectral regions. This effect may materially increase the variation of the absorption with CO_2 concentration in some of the regions of weaker absorption since the wing absorption increases at first in direct proportion to the CO_2 concentration. In order to calculate this effect a wing line shape must be assumed. Unfortunately the line shape far from the line center is not well known so that some particular shape must be assumed. The effect of the wings of strong spectral lines whose center is outside of the spectral interval being considered does not seem to have been included in Kaplan's calculation since he does not mention it.

(8) The half-width of the spectral lines varies with the rotational quantum number. Kaplan apparently uses an average value in his calculation. This is probably necessary for this type of calculation, since it would be very difficult to take account of the actual variation.

(9) What were the weakest lines which Kaplan included in his calculation? He does not tell us the criterion which was used to terminate the calculation of the rotational lines which belong to a given vibrational transition. The numerous weak lines may be important in some spectral intervals over the long path lengths which occur in the atmosphere.

(10) Kaplan's method is only valid for a constant lapse rate. Thus the flux cannot be calculated for the actual temperature variation obtained from a given sounding.

(11) I cannot agree with Kaplan's comparison of our results since he uses the wrong lapse rate for my calculation. The result of Kaplan's calculation is two values for the change in the downward flux when the carbon dioxide concentration is halved: 8,020 and 4,890 c.g.s. units for surface temperatures of 40°C and 0°C respectively. In order to compare his results with mine (surface temperature of 15°C), he makes a linear interpolation. Now a linear interpolation is certainly not justified between two values that differ as widely as

these when the quantity is known to vary non-linearly with the surface temperature. However, since only these two values are available, let us for the moment allow him to make a comparison in this manner. The value obtained by linear interpolation of Kaplan's results is 6,060 c.g.s. units. This is calculated for a lapse rate of 5.6°C per km. My calculation assumed a lapse rate of 6.0° per km (PLASS, 1956b, p. 311) rather than 5.5°C per km which Kaplan states that I used. The quantity 220 c.g.s. units must be added to Kaplan's result to correct it to a lapse rate of 6.0° per km. Furthermore, as discussed above, 490 c.g.s. units must be added to his value because of the incorrect boundary condition at 100 millibars.

Thus, as nearly as can be estimated, Kaplan's result is 6,770 c.g.s. units for somewhat similar conditions to those which I assumed. My result is 8,720 c.g.s. units. Thus Kaplan's result is 22 % lower than mine by this rather approximate method of comparison. Actually this would seem to be excellent agreement when it is considered that: (1) entirely different methods of calculation are used; (2) different laboratory data is used; (3) the distribution of temperature with height is not the same in the two calculations; (4) approximations were made in both methods of calculation. The comparison of these results certainly offers no basis for Kaplan's claim that this shows that my method is "inadequate for the problem of determining radiative temperature changes in the atmosphere".

(12) In his abstract Kaplan compares his result for the temperature change when the average cloud amount is taken into consideration (1.8°C) with my result for clear skies (3.8°C). This seems rather misleading to the reader, especially since the question of cloudy skies was thoroughly discussed in my papers (PLASS, 1956b, p. 320; PLASS, 1956c, p. 148). The argument given there showed that increased precipitation and cloud amount were likely to occur when the CO_2 amount was decreased. This in turn increases the earth's albedo and further decreases the surface temperature. As the temperature drops the maximum possible water vapor content of the atmosphere decreases very rapidly. The infrared bands of H_2O are thus less effective in holding the heat energy near the surface of the earth and the temperature further decreases. In addition

the infrared energy loss from the cloud tops to space increases. This in turn increases the convection currents in the cloud and allows it to grow. An increased cloud amount causes more sunlight to be reflected to space and further reduces the temperature. Thus the effect of water vapor is to amplify the initial temperature changes caused by CO_2 . Because of the many complicated interactions this effect cannot be calculated quantitatively. However, I believe that the actual temperature change for the earth with an average cloud distribution when the CO_2 amount is halved is at least 3.8°C and probably considerably more. Even in the tropics H_2O cannot effectively shield the CO_2 , but merely slightly raises the height at which the CO_2 begins to influence the radiation. This occurs because the variation of the H_2O and CO_2 concentrations with height is very different. Kaplan implies on the contrary that H_2O will reduce the temperature changes due to CO_2 rather than reinforcing them. He offers no arguments to support this view.

There are several different ways to calculate the radiative flux in the atmosphere. The method based on the direct use of laboratory data (PLASS, 1952, 1956a, 1956b, 1958, 1960) is limited by the accuracy of the original experiments. The absolute accuracy of Kaplan's method is also limited by the same factor. The advantages of the method which is based directly on the laboratory data are: (1) even the weakest spectral lines are included which can contribute to the absorption for the range of variables measured in the laboratory; (2) all isotopes are included in the correct proportion; (3) the variable half-width of the spectral lines is automatically taken into account; (4) all of the spectral lines are included with their correct line intensities and frequencies; (5) the correct wing absorption for distant strong lines is included for not only the Lorentz line shape, but for any line shape whose absorption coefficient is proportional to the pressure in the wings of the line; (6) the effect of the overlapping is correctly taken into account; (7) the method can be used with any arbitrary variation of pressure and temperature with height. The disadvantages include: (1) a discontinuity in the slope (but not in the value) occurs where any two of the three different approximations for the absorption curve are joined together; (2) until laboratory measure-

ments are available at lower temperatures the results must be corrected theoretically (PLASS, 1956b) for their variation with temperature; (3) in order to extrapolate the results over a wide range of pressures and path lengths the laboratory measurements must have been made over a sufficiently wide range of the variables in order to define the "strong line", "weak line", and "nonoverlapping line" regions. The effect of variations in the absorption curve chosen to represent the laboratory data has been considered in detail in one case (PLASS, 1956a). The absorption curves are closely defined by the laboratory data. The total band intensities can only be determined in Kaplan's method from curves which are obtained from experimental results. The line intensities calculated by Kaplan are directly proportional to the band intensity obtained by experiment.

The method which involves the theoretical calculation of the intensities and positions of all the spectral lines is certainly another valid approach to this problem. However, numerous approximations which have been outlined above have been made by Kaplan in applying this method in practice. The accuracy of the final results depends on how judiciously the investigator has approximated the actual physical situation. Kaplan should have pointed out some of these limitations in his article.

Sincerely yours

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