DISCUSSION OF MEMOIRS

G. C. SIMPSON, C.B., F.R.S., ON SOME STUDIES IN TERRESTRIAL RADIATION

[Vol. 2, No. 16. Published March 1928—Read October 17, 1928.]

SUMMARY.

It is assumed that water vapour is the only constituent of the atmosphere which absorbs and emits long wave radiation. From the results of upper air observations approximate values of the temperature and water content of the atmosphere at all heights and in all latitudes are adopted. Knowing the amount of water vapour and the temperature, it is possible to calculate the outgoing radiation if the absorption coefficient of water vapour is known. From consideration of the temperature of the upper stratosphere it is found that one millimetre of precipitable water in vapour form absorbs 30 per cent of the incident long wave radiation. With this value for the absorption coefficient the total outgoing radiation is calculated for each latitude from the equator to the pole, and it is found that over three-quarters of the earth's surface—from the equator to latitude 50°—the outgoing radiation is uniform and independent of the temperature of the surface. This result is due to the fact that it is only the layers of the atmosphere whose temperature lies between 220d and 286d which contribute to the outgoing radiation. As these layers are well within the troposphere between the equator and latitude 50°, the outgoing radiation is uniform from 50°S to 50°N. At higher latitudes the radiation falls off slightly, and at the pole itself it is 20 per cent below its value at the equator.

As the outgoing radiation is practically independent of the temperature of the surface, the problem arises as to how the temperature of the atmosphere readjusts itself to changes in solar radiation. This problem is considered in detail, but no solution is found.

G. C. SIMPSON, C.B., F.R.S., ON FURTHER STUDIES IN TERRESTRIAL RADIATION

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SUMMARY.

By using the observed temperatures of the earth's surface and of the stratosphere, and observed values for the absorption coefficients for water vapour and carbon dioxide, approximate values have been obtained for the outgoing radiation from the earth and its atmosphere, and the laws governing nocturnal radiation have been indicated. Values have been found for the horizontal transfer of heat across the circles of latitude which is necessary to obtain radiative equilibrium of the atmosphere as a whole. The consequences of changes in the intensity of solar

radiation have been investigated, and the conclusions drawn that change in cloud amount would be the chief agency by which radiative equilibrium would be restored. An increase in solar radiation would result in increased cloud and precipitation, while a decrease in solar energy would lead to less cloud and less precipitation. The possibility of increased solar activity leading to an " ice age " is discussed.

DISCUSSION of both Memoirs, taken together.

Dr. L. F. RICHARDSON said that the atmosphere resembled London for in both there were always far more things going on than anyone could properly attend to. He who showed what could harmlessly be neglected had done a useful work. Dr. Simpson had shown that for radiation. By a set of bold simple approximations Dr. Simpson had obtained a number of brilliant agreements with observation. The author was to be congratulated on having produced, and the Society on having published, this excellent second *Memoir.* Nevertheless we might look critically at certain details. Was it not possible that some absorption band which was significant in the kilometres of the atmosphere might have escaped detection in the laboratory where tubes could only be a few metres long? Ångström's observations on Mount Whitney indicated that even dry air absorbed.

Again, according to classical electrodynamics, as Lindemann had remarked, wherever there was absorption there must also be scattering. [Added later: The new quantum theories of Schrœdinger and Dirac seem to lead to the same conclusion. See " The New Quantum Mechanics," by G. Birtwistle (Camb. Press, 1928) Ch. XXII., Ch. XXX]. Lastly there was the obliquity of the rays. He hoped that someone would examine the question further, using the more rigorous analysis of Gold and of L. V. King. If so, Dr. Simpson's paper would be a good starting point. [Added later: Obliquity can conveniently be treated by working separately with three zones and six zones and making a " deferred approach to the limit." *Phil. Trans A.*, **226,** 1927, p. 299.]

Mr. L. C. W. BONACINA thought that, in relation to the part which Dr. Simpson very forcibly shows can be attributed to clouds in maintaining radiative equilibrium, an assumption, bearing vitally upon the argument, is made that is at least debatable. This is the assumption that more cloud would develop over the whole globe in consequence of increased activity of the general circulation in response to increased radiation from the sun. Now it is important to remember that processes in the earth's atmosphere are complementary, and that any increase in the intensity of the rain and cloud-producing areas of the globe should necessitate also an increase in the drought-producing areas. Arguing from general principles it seemed to him (Mr. Bonacina) that any increase in the activity of the general circulation following upon increased solar activity would have the following effect:—

Cloudy Equatorial belts:	evaporation	+,	condensation	+.
Clear Trade belts:	,,	+,	,,	−.
Cloudy Westerly belts:	,,	−,	,,	+.
Clear Polar belts:	,,	−,	,,	−.

The plus and minus signs simply symbolize increase or decrease with reference to standard conditions. There is a suggestive symmetry in this scheme that does not accord with the idea of a general increase of cloud in all circulation belts. If the scheme is correct it would mean that Dr. Simpson would have to find the proper excess of cloud entirely in the cloudy belts, the excess in those belts more than balancing the deficiency in the clear belts.

Colonel GOLD referred to the advance in our knowledge of radiation which had occurred in the past 20 years and expressed the opinion that it would not have been practicable to have a discussion such as they were having that evening 20 years ago. He thought that one of the most valuable features of the paper consisted in the tables, such as Table I. Such a table simplified greatly the application of theoretical results to practical problems. When one had to calculate the values which one desired to use from the fundamental formula

$$\frac{C}{\lambda^5\,(e^{c/\lambda T}-1)}$$

it made work on radiation a very arduous task.

The fact that the atmosphere was not a grey body was recognised by those who worked on atmospheric radiation many years ago. He himself had taken as one basis for computation that 25 per cent of black-body radiation at terrestrial temperatures was transmitted freely through the atmosphere and he had indicated that the variation in the height of the stratosphere with latitude could not exist if the atmosphere were a grey body (*London; Rep. Brit. Ass.*, 1910).

In connection with this it was an interesting fact that the higher the absorbing power of the atmosphere the lower the temperature and the greater the height at which the stratosphere begins. (*Proc. R. Soc.*, 1909, pp. 64-66; *London, Rep. Brit. Ass.*, 1910, p. 546).

There was another point which he thought needed consideration—the absorbing power of a gas depended not only on the total amount of gas in the path of a beam of radiation, but also on the density of the gas. Most of the experiments in which the absorbing coefficients had been found were made with gases at atmospheric pressure (about 1,000mb.). In the case of water vapour the experiments have usually been made on steam. The absorption, and therefore the radiation, of water vapour in a state so tenuous as the water vapour in the stratosphere might be very different in magnitude from the absorption of an equal quantity of water vapour whose density was 30,000 times as great. Langley's observations, taken in conjunction with those of Paschen and Rubens and Aschkinass supported this view. [What appears to be a continuous band of absorption in the case of steam may in fact consist of a series of broadened lines or strips, which would have between them lines or strips of little or no absorption in the more tenuous vapour. Actually most of the experimental results do indicate relatively sharp maxima in the absorption curves, which would accord with this view. Moreover it is by no means certain that the experimental results on the absorption in the region of longer wave lengths of 10 to 30 μ are free from considerable error; the difficulties of the experiments and the fact that radiation from the surroundings may be comparable in magnitude with the radiation under measurement would make detailed accuracy in all points most unlikely. If a source of high temperature is used for the emitted radiation, then it is difficult to exclude the effect, possibly indirect, of radiation of short wave lengths (which may be 10,000 times the intensity of the radiation under measurement); if a source of low temperature is used, then the radiation under measurement is of the same order of magnitude as the radiation of the same wave length from the surroundings of the apparatus, and these surroundings cannot be kept at an invariable temperature]. It might be argued that the agreement of the results of the computations with the observed facts or with the non-reflected balance of solar radiation was in itself sufficient justification for the assumption that there was no material difference between the absorption of water vapour in the stratosphere and the absorption of water vapour under a vapour pressure of 1,000mb. But these computations were all of an approximate character and it might be that the results agreed because errors of different signs cancelled out. For example, no computation of radiation conditions in an atmosphere in which temperature decreases with height could be entirely accurate, if it were assumed that the absorption and emission of radiation by any layer followed the same law. For example, the radiation of any wave length arriving in a vertical direction from below would on the whole arise from layers of higher temperature than if it came in an inclined direction. Consequently a smaller proportion of the total radiation would be absorbed in passing through the layer than if it were black-body radiation.

Clearly the paper was not, and did not aim at being, a final solution of radiation in the atmosphere; it did, none the less, constitute a valuable addition to the literature on the subject and it was written in a way in which it could be readily followed by any professional meteorologist even if he had no very advanced mathematical knowledge.

[*Note added subsequently*: He was struck by the close agreement of the absorption coefficients given in Fig. 1, page 4, with the values deduced from observations of Rubens and Aschkinass on page 49 of his paper (*Proc. R. Soc.*, **82**, 1909). It is an extraordinary coincidence that Rubens and Aschkinass should have made their experiments on an amount of water vapour very nearly exactly equal to the amount of water vapour in the stratosphere. Dr. Simpson states that the atmosphere is perfectly transparent to radiation between $8\frac{1}{2}\mu$ and 11μ, but he does not give his reasons for excluding the very strong absorption of ozone between 9 and 10μ found by Ångström, Ladenberg and Lehmann and Eva von Bahr. While the absorption of ozone may be of no great importance for the lower atmosphere he had always assumed that it was of material consequence in a case of radiation from or through the stratosphere.

In connection with the reference to himself on page 2 he might mention that he first stated his reasons for considering that the atmosphere at the base of the stratosphere must be saturated with water vapour in the report by Dr. Harwood and himself to the British Association at Winnipeg (*London, Rep. Brit. Ass.*, 1909, p. 108.]

Mr. L. H. G. DINES said that he was chiefly interested in the question of nocturnal radiation. The observations made at Benson which have been summarised in *Memoirs of the R. Meteor. Soc.*, No. 11, and are referred to by Dr. Simpson in Table VI. of *Memoir* No. 21, provide a means of testing the correctness of the assumptions made by him in regard to the extent to which wave lengths between 7 and 14μ are absorbed by the atmosphere. The observations consisted of values of long wave radiation received from various elevations ranging between the vertical and an elevation of $7\frac{1}{2}°$. In the latter case the path of the entering ray through any definite horizontal layer is about eight times the length of the vertical ray, so that if a certain wave length suffers absorption of anything over, say, 10 per cent in the case of the vertical ray very little indeed will get through in the case of that at an elevation of $7\frac{1}{2}°$.

In Fig. 5 of *Memoir* No. 21 the bounding curve refers to a temperature of 280a, and in Table VI. it will be seen that the mean temperature of the surface black body at Benson in March was also 280, hence in discussing the observations made in March we may use Fig. 5 without modification. Now referring to Table I. of *Memoir* No. 11, it will be found that the ratio between the radiation in March received from the zenith and from the surface black body was 65 per cent, and between that from an elevation of $7\frac{1}{2}°$ and the black body it was 91 per cent. The point is that on Dr. Simpson's assumptions it is difficult to arrive at a value as great as 91 per cent. Referring to Fig. 5 we see that at the elevation $7\frac{1}{2}°$ the radiation received between wave lengths 14 and 50μ must have been represented by the area HCH on the right of the diagram; between 14 and 11μ it must have been nearly, but not quite, equal to the area AKCH. Between 11 and $8\frac{1}{2}\mu$ is the debatable region; between $8\frac{1}{2}$ and 7μ the radiation received must have been nearly equal to the area GFJD, while below 7μ, save for a very small correction, we may take the whole area below the curve LF. On adding up these areas by a planimeter it appears that even if we fill up the whole area between 7 and $8\frac{1}{2}$ and 11 and 14 we cannot get more than about 83 per cent. The only way in which the area can be increased is by filling up part of the region between $8\frac{1}{2}$ and 11, and as it is impossible to conceive of the whole of the regions on each side of it being full it will be necessary to fill up about 2/3 of it before a reasonable curve can be drawn and at the same time the ratio of 91 per cent be arrived at.

Such a large amount of radiation between $8\frac{1}{2}$ and 11μ in the case of the ray at elevation $7\frac{1}{2}°$ implies that an appreciable amount, say 9 per cent, of the vertical ray between these wave lengths must be absorbed by the atmosphere. It is to be observed that since the temperature of the air decreases with height this figure is too small rather than too large.

A reason for this absorption may be found in the presence of material particles suspended in the air, but this does not alter the conclusion that the atmosphere as it exists, by reason of dust or otherwise, absorbs an appreciable percentage of radiation between $8\frac{1}{2}$ and 11μ. It can be argued that the air at Benson is on the average more contaminated by material particles than the air over the globe as a whole, and in such case the results obtained there do not apply to Dr. Simpson's conclusions as regards the globe as a whole.

Dr. L. F. RICHARDSON said that, knowing the apparatus, he found Mr. L. H. G. Dines's argument about imperfect transparency convincing.

During the course of the discussion Dr. SIMPSON made the following replies, and he has added further replies to the addenda.

With regard to Dr. Richardson's suggestion that there may be some absorption band due to oxygen or nitrogen in the long-wave region which had not been taken into account in the paper and might be significant when kilometres of atmosphere were involved, it should be pointed out that the opinion of Burmeister quoted in the paper, was based on theoretical considerations and not on direct experimental observations. These theoretical considerations clearly show that none of the elementary gases of the atmosphere have absorption bands of wave lengths greater than 1μ. This theoretical reasoning is supported so far as the observations go, and it makes it very probable that Burmeister's calculation is correct and absolutely certain that there can be no absorption bands of sufficient intensity to affect the general reasoning of the paper.

Ångström's observations on Mount Whitney do not indicate that "even dry air absorbed." What Ångström showed was that his observations on Mount Whitney indicated considerable radiation of long waves from the atmosphere above. There can now be no doubt that this radiation comes from the water vapour in the upper atmosphere, and in the paper I remark that Ångström's observations agree with the amount of radiation from the stratosphere which the theory in the paper indicates. I also should be glad if somebody would "examine the question further, using the more rigorous analyses of Gold and L. V. King," but as my method was developed because these more rigorous analyses are so difficult as to be almost impracticable, I see no reason why we should wait for this fuller investigation before accepting the conclusions of my paper.

I quite agree with Colonel Gold's remarks that there is a difference between the absorption of water vapour with low density as compared with water vapour at high density, and as a matter of fact I used this difference in my paper to explain why Hettner found considerable absorption between $8\frac{1}{2}\mu$ and 11μ, while Abbot and Aldrich found practically no absorption in this region. At the same time I do not think that this effect, the magnitude of which is now fairly well known, can necessitate any material change in the method of the paper. It might shift to some small extent the wave lengths adopted as the limits of the regions of total absorption by the stratosphere, but it cannot affect materially the absorption in the stratosphere set out in my Fig. 2. Quite appreciable changes in the wave lengths which limit the five regions which I consider can be made without altering the character of the results. As stated by Colonel Gold my method is admittedly an approximate method; the only justification for which is that it does give a balance between incoming and outgoing radiation accurate to a few per cent. Ultimately this is the only justification for this or any other approximate method.

In the note which he subsequently added Colonel Gold raises a question which I purposely did not consider in my paper, namely, the effect of the ozone in the upper regions of the atmosphere. There are several reasons which confirm me in my belief that the radiation from this upper layer may be neglected. One, however, must suffice here. The ozone layer can only radiate as much energy as it absorbs from the incoming and outgoing radiation. Now we know very approximately the extent and magnitude of the ozone bands and although I have not made an actual numerical calculation, it will be generally agreed that the small amount of ozone in the upper atmosphere can only absorb a very small fraction of the total incoming and outgoing radiation, probably at the most one or two per cent. As the amount absorbed is radiated in both directions only half of it can enter into our circulation, and I feel justified in leaving this small amount of radiation out of account.

I find considerable difficulty in understanding the point of Mr. Bonacina's criticisms. I distinctly said in my paper that the "increase in cloud amount would naturally occur chiefly in the three main cloud belts, one over central regions and two in high latitudes. In between these belts the skies would probably remain relatively clear." The only difference between us so far as I can see is that Mr. Bonacina wishes to state definitely that in four regions, namely, the two trade belts and the two polar belts, the cloud amount would actually decrease. I am not prepared to be as dogmatic as that. I think that all that we can say in the present conditions is that in these belts the skies would probably remain relatively clear. In any case there could be little doubt that the total cloud amount for the world as a whole would definitely increase with an increased general circulation, and that is all that I was out to demonstrate.

The remarks made by Mr. L. H. G. Dines are based on a misapprehension. My Fig. 5 was not supposed to represent the radiation through an indefinitely long column of air near the ground. It represents the energy which falls on a flat surface from all regions of the sky. In many conditions of the atmosphere the flux of radiation in a horizontal direction near the ground must be very nearly 100 per cent of the radiation from a black body at the temperature of the air. I am therefore not surprised that he has found that the radiation received from rays at an elevation of $7\frac{1}{2}°$ is 91 per cent of black-body radiation. This high radiation from rays near the surface is not due entirely to the length of the column traversed, but is also due to the fact that in this region the atmosphere is heavily charged with particles, partly solid and partly liquid, which radiate as true black bodies. I would refer Mr. Dines to the paper by Fowle referred to on page 3 of my paper, in which he shows, as stated by me, that he and Abbot could "detect no certain absorption by the aqueous vapour in the atmosphere from wave length 9μ to 12μ even when there was vapour in the path equivalent to 2cm. of precipitable water." This is a wider region of wave length than that which I take for a transparent atmosphere, namely, between wave lengths $8\frac{1}{2}\mu$ and 11μ. If Mr. Dines's contention that there is appreciable radiation in this clear band were correct, then the Benson observations which I have plotted on Fig. 6 should fall very much nearer the minimum line than any of them do in reality, for this line can only be reached when the atmosphere is acting as a full radiator at the temperature of the apparatus in all wave lengths except the narrow clear band between $8\frac{1}{2}\mu$ and 11μ. If there were appreciable radiation in this band, the points would lie below the minimum line.

Meteorology at the University of Salonika

A Chair of Meteorology and Climatology has been created at the University of Salonika and in connection with it a Meteorological and Climatological Institute has been formed. Dr. E. G. Mariolopoulos, F.R.Met.Soc., has been nominated to the Chair and the direction of the Institute will also be in his hands. Dr. Mariolopoulos was a student of the School of Meteorology in the Imperial College of Science under Sir Napier Shaw. Prior to taking up his new appointment Dr. Mariolopoulos was head of the Meteorological Section of the National Observatory at Athens; he has published several works on the climate of Greece.