i.e. radiation is due to the temperature of the system, (4) reduces to

$$\log \frac{x^2}{1-x^2}P = -\frac{U}{2 \cdot 3RT} + \frac{5}{2}\log T - 6 \cdot 5, \qquad (4a)$$

as is obtained directly from thermodynamical theories.

Equation (4) expresses equilibrium in a two-component system, equation (4*a*) in a one-component system.

Of course the ionising power is not limited to one single radiation, nor are all pulses of frequency $\nu > \nu_0$ (ν_0 = convergence frequency of the principal series) equally effective in causing ionisation. But these facts can be taken into consideration in the method used above.

MEGHNAD SAHA.
RAMANI KANTA SWE.

Allahabad, India,
January 21.

The Future of the Meteorological Office.

DR. G. C. SIMPSON writes (NATURE, February 14) that he is sure I should not wish my remarks on the future of the Meteorological Office to be taken as more than my own personal opinion; but on the contrary I believe that my views are shared by a large number of meteorologists. In 1920 the Royal Meteorological Society adopted a resolution regretting that the Meteorological Office had been placed under a Government department, created for another purpose, and urging that the Meteorological Committee should again have control; it was also pointed out that, when in the past changes had been made in the status of the Office, inquiries had been held. On this occasion, since no report has been issued, we must conclude that the transfer of the Meteorological Office to the Air Ministry took place with no public inquiry, and to an outsider it appeared to have been made in a hurried and even arbitrary manner.

The fear that the Meteorological Office may become a mere forecasting department of the Air Ministry, which is what I meant when I said that the future is uncertain, is not dispelled by Dr. Simpson's reference to the "complicated meteorological service for aviation with its thirteen stations on aerodromes." The cares of hourly reports for aviators are not conducive to the development of scientific ideas, and a Government department which has such claims on the Meteorological Office may, under pressure for economy, neglect those aspects of meteorology not directly connected with immediate requirements. The pressure for economy may very likely prove stronger than an advisory committee. It is with misgiving that one notices the giving up of the radio research station at Smallshot Hill (now fortunately taken over by the Radio Research Board), and the transfer of upper air research from Benson, originally chosen for its suitability for *ballons-sondes*, to Kew, where it is in charge of an assistant-superintendent in place of a director of experiments. These researches, of great promise for the study of the atmosphere, were at the time of the change of no immediate utility to the Flying Service. When one considers the real additions to our knowledge of the atmosphere that have come from the freedom and seclusion of Pyrton Hill and Benson, it is almost painful to contemplate the rigorous conditions of a ministry as a nursery of research. Such changes inspire the fear that other researches not directly connected with flying may be dealt with in like manner.

If the Meteorological Office is to be only a forecasting office the chief work of which is for the Flying Service, I agree that it could not be in a better position than at present; but if it is still to be an instrument for the advance of the more purely scientific aspects of meteorology I am forced, despite the larger financial grant, to agree with the resolution of the Royal Meteorological Society. I regret that I am constrained to disagree with Dr. Simpson in this matter, the more so as I am indebted to him for continued help in many meteorological matters ever since he became Director. I sincerely hope that he will be right and that I shall be wrong; but that the future only can decide. In the matter of the present staff of the Meteorological Office I am in absolute agreement with Dr. Simpson, and I used the following words in my address: "The staff of the Office is composed of men of the highest scientific calibre; probably never before in its history has it contained such a galaxy of talent."

C. J. P. CAVE.

Stoner Hill, Petersfield,
February 21.

The Michelson-Morley Experiment.

IN their scheme of their experiment, Michelson and Morley selected a single incident ray, showed how this would divide into a transmitted and a reflected moiety, traced out the path-lengths and found the difference δ. Then, comparing the results in two orientations, they computed the difference $\delta_2 - \delta_1$, which they estimated at $l \times 2\beta^2 = (0{\cdot}00000002)l$ when $\beta = v/c = 10^{-4}$ and the semi-translucent mirror is set at $\theta = 45°$. They applied this computed difference of path-differences to predict a shift of bands in an interference-field; but they did not go into the question how the interference-field (which is undoubtedly observed) is produced.

The only way in which the scheme would work is to assume that the two virtual images of a point on the incident wave-front, formed by the two moieties from the single incident ray, themselves act as two virtual point-sources, capable of producing an interference-field. Also, there must be some definite observation point within the interference-field.

It does not seem to me to be reasonable to assume that the virtual images would act as luminous point-sources, because there would only be one ray to each virtual image, derived from the single incident ray.

I have worked out the precise positions of the final virtual images and the precise single reflected rays passing through these; all as definite functions of c, v and the constants of the apparatus including θ; and I have found a reasonable observation-point, to be reached by the observing eye in a time t.

Then, waiving objections to it for the moment, I have followed up the above implied assumption and calculated out the values of $(\delta_2 - \delta_1)$ for various values of v and various settings θ of the semi-translucent mirror. The results are remarkable and unexpected.

Let the apparatus be ideally constructed; true right angles, equal arms, etc. Let the semi-translucent mirror be set at an exact 45°; and let v be the full $c \times 10^{-4}$. Then (*a*) in both orientations the virtual images, assumed radiant, would be too close together (less than $\frac{1}{4}\lambda$ for yellow light) to produce any interference-field at all; and (*b*) $\delta_2 - \delta_1 = (0{\cdot}0000{,}0000{,}0003{,}0002)l$.

Therefore, on the single-ray scheme, when $\theta = 45°$ and $v = c \times 10^{-4}$, there ought to be no observable effect, not even an interference-field. With θ still at 45°, but $v > c \times 10^{-4}$, as v increases the virtual images are farther apart, and if they acted as virtual point-sources, an interference-field would begin to be possible until, at a certain large value of v, the breadth of bands would correspond to that actually observed. The immediate neighbourhood of $\theta=45^\circ$ is a region of extraordinary sensitiveness, in which $(\delta_2-\delta_1)$ passes twice through a zero value. Very minute changes in θ make very great changes in the value of $(\delta_2-\delta_1)$.

The numerical data do not lend themselves to any general statement as to the value of v; but they point towards an actual value of v much greater than $c\times10^{-4}$, however this may be accounted for. So far for the single-ray scheme, with the assumption required by it.

I prefer to deal not with a single incident ray but with an incident plane wave-front, and to study the kind of interference-field necessarily formed where the two reflected moiety-wave-fronts cross one another. Each virtual "image" of the previous working now appears as a point on a virtual plane wave-front, which is at right angles to the corresponding "single reflected ray" of the previous working. The working out is straightforward and unforced; and it again leads to remarkable and unexpected results.

Assuming θ to be an exact 45°, and $l=1100$ cm. in an apparatus of ideal construction as above; then with yellow light ($\lambda=0{\cdot}00005892$ cm.) we have in the first orientation a band-breadth of 11784 cm. if $v=c\times10^{-4}$; 117·84 cm. if $v=c\times10^{-3}$; 1·1784 cm. if $v=c\times10^{-2}$; and 0·011784 cm. if $v=c\times10^{-1}$.

Assuming v to be $c\times10^{-4}$, we similarly have band-breadths 0·0059 cm. if $\theta=45^\circ+1024''$; 0·59 cm. if $\theta=45^\circ+10''{\cdot}3$; 3928 cm. if $\theta=45^\circ+0''{\cdot}001$; 11784 cm. if $\theta=45^\circ$; ∞ if $\theta=45^\circ-0''{\cdot}0005$; 11784 cm. if $\theta=45^\circ-0''{\cdot}001$; 59·22 cm. if $\theta=45^\circ-0''{\cdot}103$; 0·59 cm. if $\theta=45^\circ-10''{\cdot}3$; 0·0059 cm. if $\theta=45^\circ-1036''{\cdot}43$.

Working out and tabulating combinations of various θ's and v's and orientations we might hope, if we had an extraordinarily accurate knowledge of the lengths and angles involved, to be able to reach a conclusion as to the operative value of v from the band-breadths alone. The comparative shift of bands as between two orientations is not helpful in this respect; it depends upon a remainder in decimal places only, where we do not know either l or λ to a sufficient number of working figures. ALFRED DANIELL.

P.S.—By the courtesy of the Editor I have seen Sir Oliver Lodge's comment on the above letter. May I explain further that no bands would or ought to appear unless the instrument be in sufficiently rapid motion *when the semi-translucent mirror is set at an exact* $\theta=45^\circ$; if at any other angle, there will always be a certain amount of separation of the virtual images which may not be sufficient to produce an interference-field until aided by a sufficient velocity of movement (smaller than in the former case) producing a farther separation of the virtual images. The breadth of bands is a function of θ and v.

THE Michelson-Morley experiment looked for a shift of well-known interference-bands, about the formation of which there was no doubt or controversy. Ordinary wave theory explains the appearance of these bands with ease. Dr. Daniell, however, claims that no bands would or ought to appear unless the instrument was in motion, and that the width of the bands is itself an indication of the rapidity of motion, which is thereby proven to have a high value. This view is so hopelessly unorthodox that it is difficult to regard it with equanimity. Probably he is attending to one single precise ray—whatever that may be—and not to a small portion of a wave-front, with its inevitable slight obliquities. OLIVER LODGE.

The Theory of Hearing.

IN his letter in NATURE of February 14, p. 228, Prof. Scripture directs attention to the valuable work on the theory of hearing done at the New York research laboratories of the American Telephone and Telegraph Company. He refers to the papers of Fletcher, and of Wegel and Lane. The results obtained by these experimenters, in his opinion, completely confute the resonance theory, though he considers that "The simple facts of the accelerated toothed wheel and of portamento speech . . . ought to have been enough to convince any one."

All minds do not function alike, and those propositions which appear self-evident to one are by no means so to another. As an illustration of this truism one finds that Prof. Scripture, though he avails himself readily of the experimental results in question, rejects at once as unworthy of serious consideration the interpretation of those results given by the experimenters themselves. To him it appears self-evident that the results are wholly inconsistent with the resonance theory, though the experimenters state their conclusions in terms of that modification of the resonance theory to which they give the name, the "dynamic theory."

The cochlea as conceived by Wegel and Lane is a highly damped resonating organ giving more or less localised responses to simple tones conveyed to it. The pitch of the tones heard is determined by the maximum points of the disturbances in the basilar membrane. By the term "non-linear" response they imply (as seems to the writer) that the relation at various pitch levels between the intensity of the impulse and the loudness of the tone heard cannot be expressed graphically by a straight line. From this they deduce the generation of combination tones and subjective harmonics in the cochlea. Their theoretical deductions from the results of their experiments are perhaps vitiated by reason of their having left out of consideration the progressive graduation in tension of the basilar fibres by the spiral ligament. In any case there is nothing in them inconsistent with the resonance theory.

Fletcher's results are indeed startling at first sight. The elimination of the fundamental and the first four upper partials from a clarinet tone produced no alteration of the pitch of the tone, the fundamental still appearing as the characteristic pitch. He explains this as being due to the difference tone generated by the remaining partials. To Prof. Scripture this explanation appears so surprising that he can only express his feelings by a note of exclamation. To the writer the suggestion appears rational, and indeed inevitable. Are we to understand that Prof. Scripture does not believe in the existence of the subjective difference tone? Prof. D. C. Miller has analysed the clarinet tone. He states that it may have twenty or more partials, with the seventh to the tenth predominating. This latter group of partials are even stronger than the fundamental, and it is they which are chiefly concerned with giving the characteristic quality to the tone of the instrument. The difference tone of each successive pair of partials would, of course, have the same pitch as the fundamental. Even after the elimination of the five lowest partials, there would still remain fourteen pairs of generators to supply this difference tone. Possibly not all the partials would have sufficient intensity to act as generators, but the four predominating partials probably would. All these experimenters ascribe the generation of the difference tone to the cochlea, and not to the middle ear, as Helmholtz suggested. The writer has advocated the same view elsewhere, though not on the same grounds.