reason is not far to seek. The air motion known as wind is not even approximately steady and uniform until at least a height of 1000 feet is reached, and on some days there is no uniformity about it until much greater heights are attained. The supposition made, therefore, that the motion is uniform is incorrect, and if this assumption has been made by Prof. Bryan it limits the use of his equations to times of perfect calm.

He has probably also assumed that the flying machine, or at least the separate parts of it, may be treated as a rigid body. The small gliders made of thin mica, and described by Mr. Lanchester, may, no doubt, be treated as rigid, but cloth or canvas forms an important part of an aeroplane, and however tightly it may be stretched it certainly is not equivalent to a rigid sheet, but must yield under varying pressure.

My practical experience on this subject is with kites, and although there are fundamental differences between a kite and a flying machine, there are many points of resemblance, and it is probable that if the question of the stability of a kite could be completely elucidated, that of the stability of an aeroplane would follow. Almost any kite will fly in a steady wind of moderate velocity, but, so far as I know, there is no kite that will remain stable when the wind increases beyond a certain value. The limit with the kites used here is about forty-five to fifty miles an hour at a height of 1500 feet. The question is, Why do they become unstable? There are, perhaps, three reasons. The flying of a kite depends on gravity, for a weightless kite could not fly, as there would be no directive force. As the wind pressures increase, the necessary relationship between the magnitudes of the pressures and the weight is lost, and the kite becomes unstable. A cure, perhaps, lies in increasing the dead weight. Secondly, the stresses produced by the strong wind very likely deform the kite. It is not possible to meet this by increased strength, for increased strength means stronger, and therefore heavier, materials, and hence, unlike a dead weight at the centre of gravity, a greater moment of inertia, which, as I have long known from practical experience, and Mr. Lanchester has shown from the theoretical side, is inimical to stability. Thirdly, it may be that there is a certain critical velocity, like that of water in a pipe, beyond which the motion over the edges of the sails becomes turbulent, and the form of the stream lines suddenly changes.

These points can best be cleared up by extensive experimental work, and I much regret that no such work is being done.

W. H. DINES.

Pyrtton Hill.

It was difficult to prevent my article from running to ten times its present length, and a similar difficulty occurs in this letter. I can only state that the more I have examined the problem of stability the more I have been brought to conclusions exactly the opposite to those expressed in Mr. Dines's letter. My original view was to limit the mathematical investigation to the formation of the biquadratics and their discriminants, leaving their coefficients to be determined by experiment. In several popular articles I have advocated such experiments. Further experience has shown that there was little prospect of any conclusions of a practical character being obtained without a considerable further development of the problem from the mathematical side.

Moreover, experience has led me to believe that the English practical man is sometimes right when he says that if you cannot get a complete solution of a problem you ought to try to obtain the nearest approach to it that you can, and, further, that an admittedly approximate solution is often more useful than a more complicated exact one. In the present case the methods are perfectly general, and while it would have been futile to start, in the first instance, by making the problem too complicated, there is little difficulty in introducing any necessary modifications subsequently.

In regard to the assumption of uniform relative velocity, surely Mr. Dines is confusing the issues between stability and something else. The very notion of *stability* implies the existence of a state of equilibrium or of steady motion. The behaviour of an aeroplane or kite in a turbulent

atmosphere is a problem, not of stability, but of forced oscillations. The effect of these oscillations is to modify the motion of the kite; but it is necessary in the theory of forced oscillations to assume as a standard of comparison the state of equilibrium or steady motion which would exist if the disturbing forces were absent, and the character of the forced oscillations will depend vitally upon whether this state of equilibrium is stable or unstable. The first thing is to investigate the free oscillations, but there is nothing to prevent the investigation extending to forced oscillations as well. It is only a question of time and opportunity. Meanwhile, aviators are performing record flights on aeroplanes which do not satisfy the conditions of stability, and Mr. Dines's turbulent air currents are getting blamed for much that is not their fault. No solution of the problem can be final which does not completely separate the effects of free and forced oscillations. I do not see how this can be done by purely experimental methods of investigation, and surely the questions which Mr. Dines raises in connection with the behaviour of kites refer to this very point. He has, in fact, stated another problem for the mathematician.

Again, like Mr. Dines, I was at first of opinion that it might be possible to establish a simple connection between the stability of an aeroplane and that of a kite, and I suggested this problem to Mr. Harper. The investigation turned out to be more complicated and less likely to lead to any practical conclusions than was originally anticipated. With so many other problems on hand, it appeared desirable to give this one up for the present. I do not think Mr. Dines will find it possible to obtain an experimental solution. If he can, a great deal of trouble will be saved; but so many variables enter into the problem that it appears to me necessary to start with some idea as to what the connection is likely to be, *i.e.* with some kind of a mathematical theory as a starting point.

When Newton published his "Principia" it would have been an easy matter for a forerunner of Mr. Dines to raise objections to this attempt to solve problems relating to moving bodies by applications of the laws of motion on the ground that the motions must largely depend on air resistance, friction, the earth's rotation, and other causes, and that a solution which did not take all these effects simultaneously into account could be of no practical value. Similarly, it might have been objected that the solution of the problem of stability of ships could not be approached by means of the theory of the metacentre on the ground that it failed to take account of wave motion, the velocity of the ship, the effects of the wind, and such resistances as viscosity. I venture to suggest that if the above problems had been attacked by purely experimental methods alone, we might still have been in the dark as to their solutions.

In my article I alluded to difficulties caused by "want of opportunity," but out of fairness reference ought to have been made to the similar difficulties referred to in Mr. Lanchester's book. Had his investigations been published when they were made, a large amount of work might now have been accomplished which is still undone.

It is only fair also to add that since the appearance of the note I have had some correspondence with the editor of the *Aëronautical Journal*, and I hope he will succeed in maintaining the same high standard in its published papers of a mathematical character as in its illustrated articles on general aeronautics.

G. H. BRYAN.

Colour-Blindness.

IN the issue of NATURE of January 27 there is a discussion of colour-blindness, with particular reference to the case of Mr. John Trattles, whose colour-vision seems to have been difficult to determine. It is not my purpose to discuss the peculiarities of this case, or the methods of examination used by the Board of Trade. As a worker in this branch of science, however, I may be permitted to say that in any case of supposed defective colour-vision there need be no appeal from the opinion of Sir William Abney, who is so justly celebrated on both sides of the Atlantic for his researches on the perception of colour.

Since, however, in the case of Mr. Trattles, and possibly in others as well, the evidence was not regarded as con-

clusive by all the examiners, it is most desirable that some method should be used about which there could be no doubt whatever. Such a method, I believe, is one founded on the exact measurement of the persistence of vision of colours throughout the spectrum, when the frequency of flicker is at its critical value. The only thing the person under examination has to do is to adjust the speed of a sector disc so that the flickering of the part of the spectrum under observation just ceases. The speed of the disc may be automatically registered on a chronograph by electrical means. A series of measurements throughout the spectrum when plotted gives a "persistence curve," which can readily be compared with a similar curve for a normal eye. The great advantage of this test is that it is purely mechanical, and does not involve the handling of coloured materials or the judging of colours in any way by the candidate.

A careful study by this method of twenty-six cases of colour-blindness which I made some years ago ("Persistence of Vision in Colour-blind Subjects," *Phys. Rev.*, vol. xv., 1902) showed such systematic differences between eyes with normal and those with abnormal colour-vision that I am convinced it is the most exact method yet used for the study, not only of colour-blindness, but also of normal colour-perception. I can the more readily express such an opinion inasmuch as this method, though used very extensively by myself, was originally devised by Prof. E. L. Nichols.

In the article in NATURE there is the statement that in the study of colour-blindness "there is none of that accuracy of definition in the scientific picture which rejoices the heart of the physicist." To some extent this may, perhaps, be true, especially for very strong or weak intensities of stimulus. On the basis of the cases I have referred to above, I venture to assert that, for ordinary daylight, there is far more "accuracy of definition" than is usually supposed to exist. I do not know how many cases of colour-blindness must be studied to render the conclusions drawn therefrom reasonably sure, but those to which I refer can readily be divided into distinct classes on the basis of three fundamental colour-sensations, red, green, and violet.

Holmgren, I believe, first pointed out the possibility of the following varieties of colour-blindness:—

Red-blindness	Defective in <i>one</i> colour sensation.
Green-blindness	
Violet-blindness	
Red-green blindness	Defective in <i>two</i> colour sensations.
Red-violet blindness	
Green-violet blindness	
Red-green-violet blindness	Defective in <i>three</i> colour sensations.

In the paper on colour-blindness referred to above, there is, I believe, a demonstration of the existence of each of the above types with the single exception of violet-blindness. This classification, which was arrived at by means of persistence curves, seems to me to be as definite as could almost be desired.

I would therefore suggest this method for the examination of at least doubtful cases of defective colour-vision. The only objection is that the measurements, while simple and easily performed, are somewhat tedious if the spectrum is to be thoroughly examined.

FRANK ALLEN.

Physics Department, University of Manitoba,
Winnipeg, February 11.

Practice and Knowledge.

MR. AND MRS. HOWARD in NATURE of February 17 show that "the past history of agricultural science furnishes several examples of belated explanations of the utility of practices the value of which has long been a tradition among practical men." In other departments of life practice in advance of knowledge is frequent, and there is one which struck me recently, and may have been observed by others, which is the practice of blowing hot and cold with the mouth which Æsop makes use of in his fable of the "Satyr and the Traveller," and has given rise to the common disparaging saying of "blowing hot and cold."

Some may suppose that the whole explanation of blow-

ing hot and cold may be in the difference in temperature of the two bodies blown on; the Traveller's hands in Æsop's fable were colder than his breath, while the heated mulled wine was warmer. This, however, is only a partial explanation. We have unconsciously acquired the practice of blowing at different temperatures. If we wish to warm our hands we open the mouth wide and direct a slow moving current of air to them, or, more properly expressed, we breathe on our hands. This current has nearly the body temperature; but when we wish to blow cold we purse in the lips until there is only a small opening, as in whistling, and discharge a fine jet of air under pressure. This jet entangles a large amount of air with it, and when it arrives at the hot surface its temperature is much lower than that of the breath. Should the hot surface be also moist, the current of air quickens the evaporation, and so hastens the cooling.

In passing, it may be noticed here that Æsop and the modern use of the expression "blowing hot and cold" seem to have missed the mark. The objects blown on are not the same, but different, and require and receive different treatment. It is no disparagement to say of a man that he blew hot on a scheme which seemed to him to require encouragement, while he blew cold on another he thought ought to be suppressed. Æsop putting the words into the mouth of a being of the type of a Satyr seems to suggest he was not quite sure he had given the highest interpretation of the incident recorded in the fable.

JOHN AITKEN.

Accelerated Velocity of Jupiter's Red Spot Hollow.

THE longitude of the middle of the Hollow has shown a comparatively rapid diminution since the beginning of the present apparition, as the figures below clearly indicate:—

Month	Mean longitude	Diminution	Month	Mean longitude	Diminution
1909 October ...	15°3'	...	1910 January ...	8°8'	3°0'
1909 November...	13°3'	2°0'	1910 February ...	7°4'	1°4'
1909 December...	11°8'	1°5'			

From its estimated position on October 25 and February 25 (the first and last dates of observation in the above table), a rotation period of 9h. 55m. 37.9s. (287 rotations) has been deduced. This is 2.7 seconds shorter than the adopted period.

If the present accelerated rate of motion continues until June next, the centre of the Hollow will arrive at λ 0°, or, rather, its longitude will coincide with the zero meridian of system ii.

Now this quicker velocity became evident towards the end of the last apparition. An examination of my transit-chart of the Red Spot region for that epoch shows that, up to the beginning of May, 1909, the Hollow exhibited a normal monthly increase in longitude of 1.0°. Subsequently it began to move decidedly quicker, and this acceleration has been well maintained up to the present, as will thus be seen:—

Date	λ	Elapsed rotations	Rotation period h. m. s.	Remarks
1908 Dec. 20 ...	13°6'			{ May 2 chosen as approximate date of change in velocity
1909 May 2 ...	18°1'	321 ...	9 55 42.1	
1909 May 2 ...	18°1'			{ The last transit of the apparition was taken on June 12
1909 June 12 ...	18°4'	99 ...	9 55 40.9	
1909 June 12 ...	18°4'			{ Planet invisible for greater part of time between the two dates
1909 Oct. 25 ...	15°3'	327 ...	9 55 40.5	
1909 Oct. 25 ...	15°3'			{ Hollow in conjunction with S. Tropical Dark Area, since January
1910 Feb. 25 ...	7°0'	287 ...	9 55 37.9	

The approximate date when the present accelerated velocity first commenced may be regarded as May 2, 1909.

The recent behaviour of the Hollow, or the Red Spot, is attributed to one well-known phenomenon. Once about every twenty-three months the Great South Tropical Dark Area passes rapidly by the Spot, and on each occasion the latter temporarily participates in its movements. As it is now two years since we witnessed the last of these periodical occurrences, the dark matter, having swept round the planet, is once again involving the region of the Red Spot. The recent accelerated motion of the Hollow, therefore, was fully anticipated.

March 4.

SCRIVEN BOLTON.