DISCUSSION.

DR. SIMPSON :—I would just like to make one or two remarks on Dr. Hankin's paper. I might say that he is an old friend of mine, and I have often had the pleasure of seeing him make these observations. I was therefore extremely sorry when I read in his abstract, and also heard to-night, his attack on scientists.

He accuses them of not being prepared to accept new ideas. How anyone can make such a statement passes my comprehension. During the last few years physicists have had all their preconceived ideas upset. Surely in such circumstances no one can accuse them of not being able to take to new ideas. May it not be the fault of the one who wishes to convert them? If any man is going to have his preconceived ideas changed, it must be by someone who appeals to his intelligence and shows that he has something better. If Dr. Hankin has met with controversy wherever he has gone, it is, I think, largely due to the fact that he has presented his new ideas in a manner which has rather rubbed the scientist up the wrong way.

For instance, he has talked about a new form of energy in the atmosphere, about the ridges on the wings of birds and fishes which helped them to fly and of soaring flight in descending currents. Now, these things do go against our preconceived ideas, but we should have been delighted to have changed them if his had been put forward in a way which carries conviction, but I think the trouble is that Dr. Hankin dearly loves a paradox which makes him stress these things beyond what he really means.

Dr. Hankin's work is very difficult for he is trying to observe the relative motion between a medium which he cannot see and birds and fishes in rapid motion and often in the far distance. Although I believe that he has the power of observation abnormally developed, I still think that we are not unfair to him in being reluctant to accept all the inferences he draws from his observations.

What we want is a number of men with Dr. Hankin's power of observation to repeat his work and give their results from other points of view. We shall then be able to sort out the wheat from the chaff and draw most valuable conclusions.

I do not wish to criticise Dr. Hankin's work to-night, the previous speaker has, however, shown that there is much to say on the opposite side. But I would like to say a few words about one matter that Dr. Hankin has dwelt upon—that soaring flight, especially with gulls, is most pronounced in descending currents. What does this mean? Descending currents are very abnormal, and only occur where there is some great disturbance taking place in the atmosphere. Thunderstorms make them and there is the same occurrence in dust-storms, and they are present in the wake of a moving ship.

Whenever you get this kind of disturbance you get a great amount of turbulence and it is only necessary to suppose that the energy is derived from the turbulence of the air to solve Dr. Hankin's paradox.

It must be remembered that the local vertical velocities in turbulent air are probably much greater than the average downward velocity of the current as a whole. Thus to a bird able to extract energy from turbulence, the downward component of the current may be insignificant. Further I am not entirely convinced that the birds were actually in the downward current, for it is difficult to locate the position of a bird relative to a cloud of smoke or even of bits of paper floating in the air.

Do not get the idea that I belittle Dr. Hankin's work. It has been fine work, and so far as I know he is the only man who has consistently made observations for a period of years to discover what birds do and how they do it and in the history of aeronautics that work will always be held in great reverence. I therefore hope that now he has returned from India and has come to a climate which is better than the one he has left, he will give us more of his wonderful discoveries and observations.

CAPTAIN SAYERS :—Dr. Hankin has given us rather a long explanation of some of the reasons why discoveries which appear to combat existing theories meet with opposition : he has not pointed out that there are sometimes strong reasons for refusing to throw over a theory which does work in many respects, merely because some facts do not fit into it. But existing aerodynamical theory does account for many facts. Curious as Dr. Hankin's discoveries seem to be, it is up to us to see if we can reconcile them with theory.

Perhaps I have neglected some of his facts or overlooked some parts of them but I think that none of the facts contained in the abstract of his paper are clearly consistent with existing theories.

To take three of the more mysterious of his observations—those relating to noise, to the " distortion " of certain white areas, and to the change of colour of similar areas. These seem to me to be probably purely secondary effects, for which reasonable explanations can be found.

Whatever we think the cause of soaring flight to be, we are all agreed that it is a matter of having more disposable energy than is available in ordinary flight. If a bird which has a flexible wing, finds an excess of power, he will alter the shape of his wing : Dr. Hankin's observations show this. It seems probable that a purely mechanical distortion of the surface, consequent on such a change of wing form, will account for the change in the white area.

As to noise, there are several cases on record of man-made aeroplanes, which, when fitted with larger engines than those for which they were originally intended, have developed violent oscillations of wings or control surfaces. These result from changes in the pressure distribution on these surfaces under the conditions of excessive power. There is no reason why a

bird flying at high speed and very fine angles of incidence should be immune from similar troubles—which, incidentally, are not particularly dangerous to his extremely flexible wing structure. And the small size of the bird's wings would account for any such oscillations being of a frequency sufficient to cause an audible note.

The change of colour certainly seems mysterious. But many colour effects depend on what are known as diffraction and interference effects, and not on any pigment. The colours of a soap-bubble, or a thin film of oil, depend on the absolute thickness of that film. The colours of a butterfly's wing are the result of very fine striations on the scales of that wing. The exact colour effect in either case is altered if the thickness of the film, or the pitch of the striations are altered—which can be done by mechanical distortion. Without being ready to offer any definite explanation, I suggest that there are quite rational explanations of this change of colour which do not markedly affect the problem of soaring.

Regarding the upward glide, Dr. Hankin says that in known ascending currents birds fly apparently with a negative angle of incidence, and "appear to be borne aloft as on a rising tide." Those are the conditions of flight in an inclined up-wind which has a smaller vertical than horizontal velocity.

Dr. Hankin's steep upward glide has all the characteristics of what would happen to a good glider in a vertical current with very little horizontal velocity. If the vertical velocity is big enough, the bird's angle will be tilted, and his apparent flight-path will nearly correspond to his body-line.

Dr. Hankin says that this occurs only in a descending current; therefore, our only chance of squaring the upward glide with aerodynamical theory is to prove that he is in error.

Dr. Hankin has had opportunities of observing how birds behave in many conditions, and it is quite possible that there is some evidence that cannot be gainsaid that some of these occurrences have taken place in descending currents, but none of the evidence I have yet come across is anything like conclusive on this point.

Regarding the observations of Mr. J. D. North with experiments with paper at the stern of a steamer, Dr. Hankin says that this is evidence of some ascending currents in a generally descending current. This involves some curious selective quality whereby the ascending currents alone take hold of the paper and the descending one takes hold of the smoke. I believe it was Dr. Hankin who pointed to the fact that feathers from certain birds were falling through the air while those birds were soaring, as proof that they were soaring in a descending current.

I have made a few experiments in this matter. I collected feathers from the inside of an eiderdown quilt and dropped them in as still air as I could find. The rate of descent of the eiderdown was over 5 ft. per second.

The evidence of bodies falling through the air can only be accepted as evidence that those bodies have a higher rate of descent than the upward velocity of the air.

Dr. Hankin refers to an " instability " exhibited by birds when they move from an ascending current into what he believes to be a descending one. It should be pointed out that " instability " or rather unsteadiness of either a bird or any normal type of aeroplane may result from more than one cause. It may be due to a near approach to stalling speed or stalling angle. A glider leaving a horizontal current for an ascending one, may momentarily reach stalling angle.

Unsteadiness may result from flying at an angle too near to the " no-lift " angle of incidence. This is closely akin to the condition likely to lead to wing oscillation and noise—and the lateral instability of Dr. Hankin may be the initial stages of the development of such oscillations.

Another factor in the case is that no sudden transition from one air current to another having a markedly different velocity can occur without passing through a region of turbulence. This may quite conceivably cause unsteadiness of various types.

Regarding the curious facts about dust-storms, Dr. Hankin looks upon the raising of dust as proof that the wind comes down to get at the dust; but in fact it is even stronger evidence that it goes up again. How does he discover exactly where it starts going up or where it stops coming down? The only evidence on this point is the dragon-fly's " steep glide " after leaving the dust curtain. The conditions at the advancing front of such a disturbance must involve a very complicated form of turbulence. It would certainly not be safe to conclude that rising or descending currents could be distinguished merely by the presence or absence of dust, and I think it is probable that the glide occurred in an up current.

Dr. Hankin refers to the Köller-Betz effect as a possible explanation of some of his observations of soaring in a descending current. It should be pointed out that this effect was not discovered experimentally by Dr. Betz, but was predicted from standard theory by Dr. Köller, and demonstrated in the wind channel by Dr. Betz. This effect is simply that in a wind having alternate up and down components the resultant force on an aerofoil is inclined somewhat forward of the resultant force produced on the same aerofoil by a steady wind of the same average velocity.

If the aerofoil is sufficiently good, and the variation of velocity about the average value sufficiently great, the resultant force may be inclined forward of a right angle to the average wind direction, and the aerofoil then has apparently negative drag.

The effect is to be explained by the fact that air forces are proportional to the square of the velocity. In a variable wind the average force is proportional to the mean square of the velocity—and not to the square of the mean velocity. The actual magnitude of the Köller-Betz effect is small, and cannot account for an upward component of force in a wind whose average velocity has more than a very small downward component.

If the downward trend of the air current is very marked, Köller-Betz effects cannot account for soaring.

Then Dr. Hankin refers to cases of soaring in places where the contour

currents were obviously downward. Although he admits that convection currents were also present he seems to assume that the contour current is necessarily predominant.

A thirty-mile-an-hour wind, blowing up a slope of 1 in 10 gives an upward component of 4.4 ft. per sec. These conditions can be found along the South Downs with great frequency—and British birds soar comfortably in them. On a sunny summer day on the southern face of these same Downs, the same birds can be found soaring with equal facility in a wind of 4 m.p.h., and the cause can be found in convection currents. Thus even in England there is evidence that convection currents may be as strong as quite powerful contour currents. In India one would expect them to be much stronger, and so quite capable of annulling a descending contour current.

There seems to be an impression that convection currents must be extremely local—that there cannot be an ascending warm current without an equal and opposite descending cool current. This is partly true. For every pound of hot air that goes up there must be a pound of cold air that comes down—but the pound of hot air occupies an increased volume, so must have a greater velocity. Thus the ascending hot column contains more energy than the corresponding descending cold column, and there is no reason why the ascending and descending particles should not be very closely intermingled. The net effect of such intermingled up and down currents would be that of an average up current, and this condition may obtain over square miles of surface.

Then there is the case of the steamer. Dr. Hankin does not conceal the fact that it is a steamer, but he does not refer to one essential feature of a steamer—its funnels—which must produce a very strong up-current. Close to the funnel any self-respecting bird could soar—but birds not being made of asbestos have to keep away from the funnel itself and use the ascending current which it produces after the funnel gases have been cooled and diluted by fresh air—that is some way astern of the funnel.

Here the up-current from the funnels is mixed up with the contour current over the stern. The question whether the down-contour current, or the up-convection current will prevail at any particular position relative to the ship is obviously a very complicated one, affected by wind conditions, steamer speed, etc., and not to be settled by considering the contour current alone.

On the whole I cannot see the evidence at present which should lead us to suppose that accepted aerodynamical theories do not fit the facts of soaring. There are a number of very interesting, and, I think, obvious, conclusions to be drawn from Dr. Hankin's observations, and among these are :—

(1) That birds can soar quite comfortably in air which provides only a very small amount of energy.

(2) That much more violent disturbances than are necessary for soaring are of quite frequent occurrence.

(3) That birds do not care for flying in unnecessarily violent dis-

turbances such as would provide excessive amounts of energy. A pilot sent out on patrol duties on a machine so overpowered that he had either to work hard to keep his machine level, or else allow it to climb far above the necessary altitude would not consider that his machine was unsuitable and objectionable. The bird flying on too energetic an up-current is in the like position.

(4) Birds prefer to soar in convection rather than in contour currents. This is perfectly natural—as contour currents are normally accompanied by smaller horizontal velocities.

(5) Birds do not like flying in dust or smoke.

In his lecture Dr. Hankin refers to one or two cases not included in the advance copy. One of these is the case of two species of dragonflies, one of which always flaps when another species soars. This would seem to indicate that the flapping species is a less efficient aeroplane than the soaring species.

In conclusion it should be pointed out that so far Dr. Hankin has always used his observational equipment as a basis on which to attack standard theories of soaring flight, but his attack rests essentially not on his admittedly accurate observations of the motions of the birds themselves, but on his deductions as to the air movements which accompany the birds' movements. He has given no hint of any constructive theory which fits in with his deductions.

The correct answer to his case is clearly to produce a constructive theory which accounts for his observations—and to do this it is necessary to distinguish very carefully between the observations—which I am sure are extremely reliable, and certain of his assumptions—which I am sure are extremely shaky.

MR. BRYANT :—I have for several years past been associated at the National Physical Laboratory with problems connected with the flow of air round obstructions, among which cases of ships of various types have arisen. The experimental data for the most part applied to models, but some work on the full-scale obstacles has shown that the flow is seldom substantially different from that observed on the corresponding model, provided all conditions are similar in the cases compared. Regarding experiments in natural winds, I may add that pieces of paper, cotton-wool, threads and straw have to be used with great caution or they are bound to lead to wrong conclusions. We never trust evidence derived from such indicators without some more trustworthy confirmation.

The principal feature of the air flow over a long obstacle like a ship, is the formation of " cliff eddies " with axes roughly parallel to the windward edge of the deck, whenever the relative wind is not approximately fore and aft. These eddies are continuously forming over the deck, and are carried aft by the wind over a long distance before being finally dissipated. On the windward side of the eddies there is an up-current, and on the leeward side a down-current. The magnitude of the up-current in a wind of 30 m.p.h.

is often as much as 5 m.p.h. at two or three ship's lengths abaft the stern, and at about deck-level or a little below it.

With regard to the regions of up-currents, the accompanying diagrams (Figs. 1-4) sketching the limits of ascending currents in plan at deck-level, indicate the soarable areas for a ship having a fair-sized flat deck aft. It need hardly be said that the reason why soarability does not exist in calm air, is simply because there cannot be any side wind components in a calm, and therefore no "cliff eddies" can in general be formed.

Turning to the question of the effect of funnel gases, there is no doubt that when smoke falls low, it is the heavier particles that are descending; and some of these may even fall through an ascending current. At any rate, it is inconceivable that hot gases could produce a down-current where there would otherwise be an up-current near an obstacle like the stern of a ship; and one must conclude that our model evidence holds good as an indication of ascending currents, and, further, that hot gases may possibly increase them or at most slightly displace them.

I have explained my case thus at length in the hope that it will help towards the solution of one at least of Dr. Hankin's mysteries. The position of Dr. Hankin's long trial of soarable air below the funnel smoke, in his case of a strong wind, is an excellent confirmation of what we have found in the rear of models; and his other cases are scarcely less conclusive.

It is a great pity that Dr. Hankin should persist in his idea that ascending currents are not the usual cause of soarability. He has never produced any convincing proof of the existence of a descending current combined with soarability, simply because such proof, if it exist, is very difficult to secure. The vertical currents involved, especially in the case of dragon-flies, are often quite small and only detectable by the use of accurate instruments.

MR. HOWARD FLANDERS :—I should like to say that I have observed the behaviour of the lesser albatross in the "Roaring Forties," and you can observe them for about a fortnight after leaving Cape Town. They glide low down near the water, and, of course, one is at a great height above them.

When one considers, as Captain Sayers has pointed out, the large amount of coal used on the ship, there must be much heat radiated. The whole ship is heated and the hot air rises : you therefore get turbulence.

I should like to say that I certainly had preconceived opinions before I started observing, and am afraid I still have them.

DR. HANKIN'S REPLY TO THE DISCUSSION.

The course of the discussion illustrates my contention that it is a universal attribute of the human mind for it to remember and to lay stress on facts that agree with its theories rather than on facts that tend to disprove

https://doi.org/10.1017/S2976690700000413 Published online by Cambridge University Press

them. For instance not a single speaker has referred to the remarkable fact about the flight of the flying-fish.

Some years ago Wegener described smoke from factory chimneys as drifting in a thin layer for several kilometres to leeward. This he asserted only happened in a very light wind in the absence of turbulent movement. Similarly, on some occasions in the red sea, steamer smoke has been seen by me making a trial extending right across the horizon. That is to say smoke particles in the absence of turbulent motion may take hours to settle to the sea surface. But, if I have not misunderstood Captain Sayers, his view is that when in an ascending current at the stern of a steamer they fall through it like lead shot. I fear that my description must be at fault and that if Captain Sayers had observed the behaviour of smoke at the stern for himself, he would not have made this suggestion.

Although smoke from the funnel descends, till some of it, in a very diluted form, reaches sea-level, Captain Sayers thinks that heat from this smoke causes an ascending current of sufficient strength to explain the soaring of gulls at the stern. If this idea were true one might expect gulls to soar better in a very light wind at the stern than in a gale, for in the latter case any hot air from the funnel would be rapidly diluted. Also if the wind direction was such that the smoke from the funnels went directly over the beam instead of over the stern, then, if Captain Sayers were right, gulls in such a case would soar on the beam of the ship under the smoke instead of at the stern. Before finally concluding that his suggested ascending current exists, Captain Sayers would be well advised to go to sea and discover whether the above eventualities correspond with the facts of the case.

None of the highly ingenious arguments put forward by Captain Sayers meet the case of the flying-fish, which appears to me to be the most remarkable and important form of soaring flight yet observed.

A hearty vote of thanks to Dr. Hankin for his interesting lecture brought the meeting to a close.