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On the Physics of Incubation

A. P. Chattock

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VIII. *On the Physics of Incubation.*

By A. P. Chattock, *D.Sc., F.R.S., Emeritus Professor in the University of Bristol.*

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Contents.

Preliminary.

It is generally agreed that a considerable discrepancy still exists between the results of natural and artificial incubation; a larger percentage of eggs hatching out, on the average, under hens sitting in properly designed nests than in the best incubators. Exactly what this discrepancy may be is not certainly known—figures obtained a few years ago by the Board of Agriculture point to about 20 per cent.—but that it is worth taking into account in hatching on a large scale there is little doubt. Whether, if the physical conditions obtaining under the best sitting hens were known and could be exactly reproduced in incubators, incubators would hatch eggs as successfully as hens may be open to question; but it is reasonable to suppose that the quality of a hatch is at any rate very largely determined by these conditions, and their study is thus likely to lead to improvement in artificial hatching.

With this end in view the experiments, of which the following is an account, were begun in 1916 on a poultry farm at Crowcombe, Somerset, belonging to the writer. Their purpose was two-fold:

1. The quantitative determination of the physical conditions under sitting hens which are likely to affect the hatching; and

2. The comparison with these conditions of those that now obtain in the best artificial incubation.

When the work was half finished the writer became acquainted with two important papers, "Carbon Dioxide in Incubation" by G. H. Lamson, Jr., and H. D. Edmond. 'Bulletin No. 76,' March, 1914, and "Factors in Incubation" by Prof. W. F. Kirkpatrick, 'Bulletin No. 96,' February, 1918, published by the Storrs Agricultural Experiment Station, Conn. With the same object in view, of improving artificial incubation,

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the experiments described in these papers proved to be on such different lines to the writer's that, partly for the sake of what was new in it and partly as a check on results for which accuracy is most difficult to obtain, it was decided to carry on the present work to its conclusion. It may be added that where comparison has been possible the two sets of results have been found to be in satisfactory agreement.

As is well-known, the eggs under a hen, besides being kept warm, are rolled about from time to time by the motion of her body. They are also cooled each day while she is off the nest; and when she is on it evaporation from her body, from the nest if damp, and from the eggs themselves, renders the humidity of the air under her much greater than of that outside. Further, owing to the constant escape of water and carbon dioxide from the nest, the egg space is kept ventilated, and the eggs lose both water and carbon dioxide continuously.

These are the physical conditions that have been examined. They are tabulated in the following list :—

1. Motion of eggs.
2. Cooling of eggs.
3. Humidity of nest air.
4. Ventilation of nest.
5. Loss of weight of eggs.

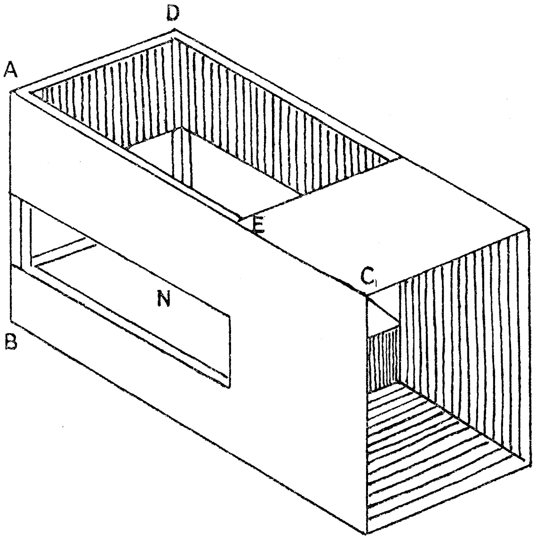

Fig. 1.

For the experiments on sitting hens nest boxes were used, of which fig. 1 gives a perspective view. AB was 14 inches, AC 20 inches, AD 12 inches, and AE 14 inches. The openings at top and sides were closed by moveable wooden lids and the front opening by a door of wire netting. The box contained a drawer 4 inches deep and filled with earth, of which the upper surface N formed the nest. N was sufficiently saucer-shaped to keep the eggs together near its centre, and was covered with a piece of thin canvas laid directly on the earth.

This arrangement is referred to below as the "earth-nest," to distinguish it from the "tin-nest," in which the earth and its canvas covering were separated by a sheet of thin tin—also saucer-shaped—to prevent interchange of moisture between the nest air and the earth.

The nest boxes were kept in a quiet shed which was well ventilated and without much light; dust bath, grit, maize and water were provided, and the hens allowed off their nests once a day. Except in one case, noted below, they appeared to be quite contented—an important condition for good sitting.

Quality of Hatch.—The usual criterion of the quality of a hatch is the percentage of the eggs that hatch out—reckoned, of course, on those that were fertile. This quantity is determined by the number of eggs that die during the incubation process; and when used as a criterion of that process is open to the objection that some of the eggs that died may have suffered from weakness or other predisposition to hatch badly, which is not the case with the rest of the eggs and may have no connection with the conditions of incubation. This defect may partly be avoided by arranging that the two sets of eggs to be compared "correspond," in the sense that for each egg in one set there is an egg in the other laid by the same hen within a few days of the first; and the condition of "correspondence" is always to be understood as present in what follows unless the contrary is specified.

Even when two eggs have been laid by the same hen within a few days of one another they will not necessarily correspond. An egg may accidentally miss fertilisation, and it is consequently possible for one of a pair to be fertile and the other not. When this occurs (and it is only seldom that it does so) both eggs must be treated as unfertile in calculating the percentages hatched.

Imperfection in the conditions of incubation will presumably affect all the eggs present more or less, and a criterion which depends on the behaviour of the eggs that do hatch forms a useful check upon that of the percentage hatched when incubation conditions are in question. To some extent the average time at which a hatch takes place affords such a criterion—unfavourable conditions usually, though not invariably, resulting in later hatchings—and in most of the experiments on incubators this quantity is given with the percentage, the time being reckoned in hours from the end of the nineteenth day.

The two tests of percentage and time are not, however, very intimately connected, the latter being apparently sensitive to disturbing causes which may hardly affect the former.

For instance, a comparison was made of the qualities of the hatching during the first and second halves of the 1919 season at Crowcombe.

Between February 2 and April 22, 1,507 eggs were set, 70 per cent. of which hatched at an average time of 53·8 hours. But while the percentage hatched of the 1,685 eggs set between May 6 and July 4 was about the same (68·9 per cent), the time of hatching was 31·6, or 22 hours earlier.

It is possible that this gain in time was caused by the starting of incubation in the eggs before they were set, owing to the warmer weather of the second period; but, whatever the discrepancy is due to, it shows that the time test is not dependable for comparing hatches that occur at different dates, and it is only in the case of single hatches, where part of the eggs have been treated differently to the rest, that it has proved useful in comparing the two treatments in question.

Even so, it must be applied with caution. If the hatch is a bad one there are often eggs for which the hatching extends over days, in which case the lateness of a single egg may swamp the early hatching of several others when the mean time is taken. Or, when only a small number of eggs has been set, it may happen that too few hatch for which the "corresponding" eggs also hatch; and as it is from corresponding pairs that it is most useful to calculate the times of hatching, there may not be enough of these to give dependable values. In the work recorded in Tables IV. and V., for instance, it is for both these reasons that the hatching times have not been given.

TABLE I.

Incubator set: June 26. 3 p.m.
End of 19th day: July 15. 3 p.m.

Egg-shells removed at :		H.	N.	N × H.
July 15. 11.0 p.m.	8·0		0	36
July 16. 7.0 a.m.	16·0	12·0	3	193
10.0 ,, ...	19·0	17·5	11	225
1.0 p.m.	22·0	20·5	11	347
3.2 ,, ...	24·2	23·1	15	205
6.2 ,, ...	27·2	25·7	8	143
9.0 ,, ...	30·0	28·6	5	0
11.0 ,, ...	32·0	31·0	0	67
July 17. 2.0 a.m.	35·0	33·5	2	0
7.0 ,, ...	40·0	37·5	0	
Totals			55	1216

$$\text{Mean time of hatch} = \frac{1216}{55} = 22 \cdot 1 \text{ hours.}$$

Table I. illustrates the manner in which the mean time of a hatch is obtained. In the first two columns are given the times at which the empty egg-shells were removed, the numbers in the second column being the times of the first in hours from the end of the nineteenth day, and the number of shells removed being given under N. Under H are the means of successive pairs of times from the second column; thus, 12·0 is the mean of 8·0 and 16·0, and so on. The rest of the table explains itself.

Quality of Hen Hatching actually obtained.—It is, of course, of primary importance that the hatching under hens shall have been good if it is to be taken as a guide to better hatching in incubators, and in Table II. are therefore collected detailed records of all the hatchings in the shed.

TABLE II.

Number.	Date of setting.	Type of nest.	Eggs set. A.	Eggs hatched. B.	Unfertiles and deaths at start. C.	Dead in shell.	$\frac{100\text{ B}}{\text{A}}$.	$\frac{100\text{ B}}{\text{A}-\text{C}}$.
[1]	16 June, 1917	Earth	38	30	2	6	79	83
[2]	28 June, 1917	,,	35	28	?	?	80	2
3	20 July, 1917	,,	16	13	2	1	81	93
4	6–7 Aug., 1917	,,	31	26	4	1	84	96
5	18 Aug., 1917	,,	39	36	3	0	92	100
[6]	19 Sept., 1917	,,	13	8	3	2	62	80
7	23 Sept., 1917	Tin	13	11	2	0	85	100
8	2 Aug., 1918	,,	12	12	0	0	100	100
9	26 Aug., 1918	,,	12	12	0	0	100	100
10	26 Aug., 1918	,,	12	9	3	0	75	100
Totals, excluding numbers in square brackets			135	119	14	2		

$$\frac{119 \times 100}{135} = 88 \text{ per cent.} \qquad \frac{119 \times 100}{135 - 14} = 98 \text{ per cent.}$$

Of the first five hatches (Nos. 1–5), each setting consisted initially of 39 eggs under three hens in three separate boxes, the remaining hatches (Nos. 6–10) being each for a single hen on 13 eggs. From these initial numbers have been subtracted the number of breakages during incubation to obtain those under "eggs set" in column 4, and in the case of Nos. 3 and 4 further subtractions were made for the following reason. There was a shortage of 29 eggs from the properly mated pens when Nos. 3 and 4 were set, which had to be made up from a large pen containing the refuse of the farm, 22 going to No. 3 and seven to No. 4. Of these 29 only three hatched, the rest having been either unfertile or having died within a day or so of being set; and as it was clearly the cock who was at fault, the whole of them were ruled out.

In calculating the totals entered at the foot of Table II., the hatches numbered 1, 2, 6 in square brackets have been omitted—1, because it contained so many dead in shell that it was useless as a standard of incubation conditions; 2, because the eggs which did not hatch were accidentally destroyed before being opened, so that the proportion of dead in shell is not known; and 6, because the hen tired of sitting at the end of a fortnight and the eggs had to be transferred to an incubator, bad sitting therefore probably accounting for the low percentage hatched in this case. Thus, of 135 eggs set, and which there was reason to hope would all hatch, 119, or 88 per cent., actually produced chicks. Of the remaining 16, two were dead in shell and 14 were either unfertile or died within, at most, a couple of days of being set.

Now, as a criterion of the effect on the hatching of the conditions present during the 21 days of incubation, the only eggs that are useful are the 119 that hatched and those that died some time after being set (in this case the two dead in shell). The other 14 that failed to develop presumably did so without reference to the conditions under consideration, and should therefore be subtracted from the 135 before the hatching percentage is calculated. When this is done, the latter rises to 98 per cent., from which it appears that the hatching conditions under the 13 hens concerned must have been so nearly perfect that they may be accepted as determining the standard of incubation sought.

PART I.

TURNING AND COOLING.

Motion of Eggs under Hens.—The movements of the eggs under fifteen of the hens experimented with were recorded by means of a wire grid consisting of two sets of straight wires at right angles to each other. This could be placed upon the eggs when the hen was off them, and it was then easy to register the position of each egg in the nest by its co-ordinates on the grid. In this way the movements of each egg were followed from day to day, with the result that not one of the fifteen hens failed to shift each of her eggs very thoroughly about to all parts of her nest. The same was also true of the turning of the eggs; but as they are probably moved about by rolling, this was to be expected.

Exactly how often a hen moves her eggs it is not easy to say. On returning to them after being off to feed she does so oftener than later on—perhaps once in 10 minutes; and she is apt to do it more frequently if she is disturbed by sounds, especially the lightest touch on her sitting box. The longest interval between successive movements that the writer has observed was 55 minutes, and the next longest 50 minutes. Perhaps half to three-quarters of an hour is a fair average, but it may be longer.

So far as the experiments with hens are concerned, they thus throw no light on any possible direct effect of turning upon hatching. They do not even afford evidence that such a direct effect exists, for the turning of the eggs is the result of their travelling about, and for the travelling there is ample reason in the fact that the temperature under a hen is not uniform, and unless the eggs are moved about some will have been incubated at higher temperatures than others.

Motion of Eggs in Incubators.—While collecting information as to the change of weight of eggs during incubation it was noticed that the weighed eggs frequently hatched better than the rest. The weighings were made every two days, and, as every egg in each incubator was turned twice and cooled once each day, the weighed eggs received most turning and cooling. Much of the difference between the hatching of weighed and unweighed eggs was subsequently found to have been accidental, and was due no doubt to the fact that the two sets of eggs did not " correspond "; but by that time a study had been begun of the separate effects on hatching of turning and cooling, and this has proved fruitful, as the following account shows: All the eggs in each incubator were turned twice and cooled once as usual, but at three other times during the day every alternate row of eggs was turned additionally. In this way the eggs were all cooled alike, but half of them were turned five times instead of twice. The results are given in Table III., where ½ under the heading " Eggs hatched " means that a chick was helped out, helping only being resorted to in cases where the chick was all but out already.

TABLE III.—Turning Experiments in Hot Air (Gloucester) Incubators.

A.—Five times turned.			B.—Twice turned.			Time of hatching.	
Eggs set.	Eggs hatched.	Percentage.	Eggs set.	Eggs hatched.	Percentage.	A.	B.
54	40	74	56	31	55	25·9	29·2
54	40	74	55	41	75	23·8	24·6
49	25	51	44	19	43	24·0	29·3
49	36	73	50	31	62	22·2	21·0
35	24	69	36	18½	51	43·7	42·8
34	28	82	34	23½	69	25·3	25·4
275	193		275	164		27·5	28·7
$\frac{193}{275} \times 100 = 70 \cdot 2$ per cent.			$\frac{164}{275} \times 100 = 59 \cdot 7$ per cent.			B − A = 1·2 hour.	

As the number of eggs set varied from hatch to hatch, and the dependability of each percentage hatched is roughly proportional to the number of eggs dealt with, the method set forth in the table of obtaining the final mean percentage seemed best for comparing A with B, since it results in each separate percentage being " weighted " in the final mean in proportion to the number of eggs set.

It is true that if the numbers of eggs set on each side of the table are not identical for each separate hatch a source of fallacy may be introduced, but in the present case the differences are so small that they have no appreciable influence on the result.

It thus appears that the eggs which were five times turned hatched better than the twice turned by 70·2–59·7, or 10·5 per cent.; and, as in five out of the six hatches the sign of the difference is the same, there cannot be much doubt of the significance of this result. The further fact that the five-times turned eggs hatched on the average 1·2 hour earlier than the others is, moreover, consistent with their better hatching, but an hour is not long enough to carry much weight.

As a hen moves her eggs night and day, and more frequently than five times in the 24 hours, it seemed desirable to try the effect of more frequent turning, and this was facilitated when, three years ago, the Crowcombe farm was given up and the work transferred to the Physical Department of the University of Bristol. Six small incubators were there constructed, of about 30 egg capacity each, and heated electrically by some of the ordinary units employed in electric fires, fastened under their lids above the egg trays, resistances being put in series with these units to keep their temperature low. Temperature regulation was by capsule, and the turning was effected by tapes which could be operated from the outside.

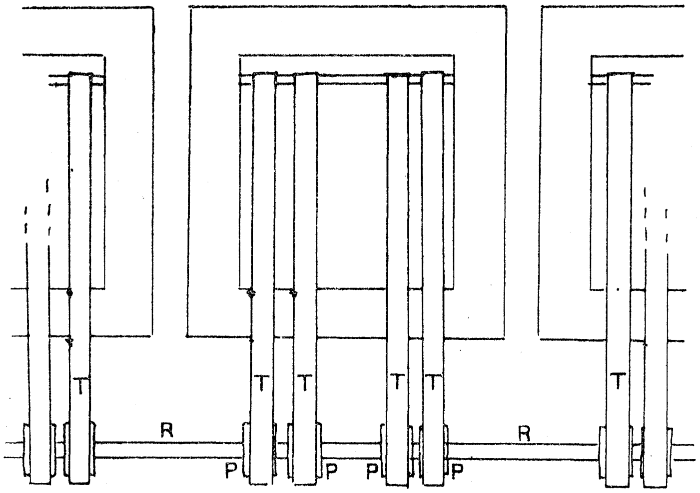

FIG. 2.

In fig. 2 is given a horizontal section through one of these incubators in the plane of the tapes. The six stood side by side in a row, and in front of them ran a brass rod, RR, carrying wooden pulleys, PP, covered with sandpaper, over which the tapes were led. Each pulley could be either clamped to R or left free, so that the corresponding tape could be driven by R or not as desired. R ended in a large worm wheel, of which the worm was coupled direct to a direct-current motor mounted on a separate table to the incubators upon a very solid floor (the details are important), the gearing being such that the velocity of the tapes was of the order of an inch per second when R was being driven. The driving circuit of the motor was closed by a platinum piece on one of the hands of a clock making contact with a platinum stop, and this contact was broken a second or so after having been made by simple mechanism actuated by the motion of R.

When the contact piece was carried by the minute hand of the clock the tapes moved twice in the hour, and travelled about $1\frac{1}{4}$ inch each time. When it was carried by the hour hand their travel was about $\frac{5}{8}$ inch every three hours. A light wooden grid (not shown) rested on the tapes which kept the eggs separate from one another and free to turn. Each tape carried six eggs, and of each pair of tapes one was moved by hand twice a day in the ordinary way (" normal " turning), and the other by motor. In the first hatch a fifth tape was used in the centre space of each incubator, carrying six more eggs. It was discarded owing to doubt as to its temperature, and is not shown in the figure.

Four hatches were made with this apparatus (Table IV., 1, 2, 3, 4), the eggs being turned twice every hour, day and night. Unfertiles have been allowed for in the data given, and brackets connect the settings that " correspond."

TABLE IV.—Experiments on Turning Eggs by Motor.

Hatch.	Eggs set.	Eggs hatched.	Percentage.	Change of percentage due to extra turning.	Description of turning.
1	71	39	55	33 loss	Normal hand [2].
	71	15½	22		Motor [48].
6 Incubators	36	8	22		Normal first week then motor [48].
2	64	34	53	35 loss	Normal hand [2].
6 Incubators	67	12	18		Motor [48].
3	33	25	76	50 loss	Normal from start [2].
	35	9	26		Motor from start [48].
6 Incubators	34	26½	78	49 loss	Normal after 2nd day [2].
	34	10	29		Normal 2nd to 5th day [2] then motor [48].
4	34	21	62	20 loss	Normal hand [2].
	36	15	42		Motor day time only [24].
6 Incubators	36	18	50		Motor 8 days [48] then normal [2].
	36	17	47		Normal 8 days [2] then motor [48].
5	57	35½	62	34 loss	Normal hand [2].
5 Incubators	58	16½	28		Motor [8].
6	48	35	73		Hand—slow [2].
4 Incubators	48	35	73		Hand—normal [2].
7					
3 Incubators	36	17½	49		Normal hand [2].
(protected)	35	12	34		Motor [8].
2 Incubators	23	11½	50		Normal hand [2].
(not protected)	23	6	26		Motor [8].
8					
3 Incubators	35	12	34		Normal hand [2].
(protected)	36	5½	15		Motor [8].
2 Incubators	24	4½	19		Normal hand [2].
(not protected)	23	0½	2		Motor [8].
9	12	9	75	8 gain	Hand [2].
1 Incubator	12	10	83		Hand [8].

The figures in square brackets give the numbers of times the eggs were turned in 24 hours.

In addition to being much increased, the effect of turning the eggs 48 times a day was in the opposite direction to that of turning them five times, the slight gain in hatching percentage due to the latter having thus changed to a considerable loss. It looked, in fact, as if the turning had been overdone.

To see whether the change was gradual the clock was next set for eight turnings a day (Table IV., 5), with the rather unexpected result that the loss was about the same as before—unexpected, because eight is so much nearer to five than to 48 that a tendency to pass from loss to gain might well have shown itself.

The fact that the eight turns were made by motor and the five by hand suggested that the explanation probably lay in some essential difference between motor and hand turning *as such*. Now the motor turning was smooth and gradual, while that by hand (normal) was relatively quick and jerky. A test was therefore arranged in which all the eggs were turned by hand, but half of them jerkily as in "normal" turning and the rest slowly, as if by motor. The results are given in Table IV., 6. They are the same—exactly the same as it happens—for both types of turning.

There was apparently no connection between the beneficial effect of ordinary turning and the detrimental effect of motor turning. As is shown below (p. 411, Tests I to IV), the former is probably limited to the first week or so of incubation, but the motor turning is equally harmful all through the incubation period (Table IV., 3 and 4).

Vibration is known to be bad for hatching, and another possible explanation seemed to be the transmission of some vibration from the motor to the motor-turned eggs, but not to those turned by hand. For this it would have to be of very special type. Any disturbance reaching the eggs through the incubator from the table, the transmitting mechanism, or the air would affect the hand-turned eggs equally with the rest. To discriminate between them it would have to pass along those tapes that were transmitting motion from the motor, and not along those that were disconnected. The disturbance would thus have to be of a circular or rotatory type superposed on the driving couple transmitted from the motor to the wooden pulleys, and might be due to the smallness of the number of the motor's commutator parts. Its frequency would in that case be high, and a substance like soft rubber might be used to absorb it. The rod R was therefore cut, and its cut ends joined by a block of soft rubber, two of the incubators being on the motor side and three on the other side of the rubber. The eggs in the three should thus be less affected by the hypothetical vibrations than those in the two. The results are given in Table IV., 7 and 8, and also in Table V., where they have been rendered comparable by altering the percentages hatched from motor-turned eggs to what they would have been if 60 per cent. of the hand-turned eggs had hatched in each case. Thus $42 = 34 \times \frac{60}{49}$ and so on. In both hatches the motor-turned eggs beyond the rubber have hatched much better than those that were not protected in this way, and the conclusion seems to be justified that the rubber block did, in fact, partly absorb some high-frequency vibration and prevent its reaching the eggs.

TABLE V.—Experiments with Rubber Transmission.

	Percentages hatched from motor-turned eggs for 60 per cent. hatches from normally turned eggs.	
	Hatch IV. 7.	Hatch IV. 8.
Rubber protected	42	26
Unprotected	31	6
Gain due to protection	11	20

One other possibility remains to be referred to. The experiments showing the improvement in the hatching of five times turned eggs were made with hot air (Gloucester) incubators; and, as appears below, eggs turned eight times in Gloucester incubators show a similar improvement. Now the motor-turned eggs were warmed by radiation from above, and there was, therefore, just the possibility that the bad hatching obtained from motor-turned eggs was due to this difference in the type of heating. A final test was, therefore, arranged (Table IV., 9), in which all the eggs set in one of these radiation incubators were turned by hand—half every three hours and the rest twice a day; and of this the hatching indicates that when the more frequently turned eggs are turned by hand instead of by motor the drop in the hatching percentage met with in every other instance with these incubators becomes a rise; and though the number of eggs set was too small for the result to possess much value numerically, it shows that when the effect of the motor is eliminated the top-warmed eggs fall into line with those hatched in hot-air machines.

As the experiments with motor-turned eggs have thus all to be ruled out, it would perhaps have been sufficient to dismiss them with less discussion. But the work of searching for the cause of the discrepancy had to be done, and for the sake of other possible experimenters on the same lines, it seemed more useful to enter into this much detail in describing it. Moreover, though the explanation given does fit the facts, it remains a surprising thing that a vibration of which so little direct evidence is available, and which is only applied for a few seconds at a time, should be capable of doing so much damage to a hatch.

Motor-turning having failed, the experiments had to be completed by hand-turning. Two new 70-egg Gloucester incubators were therefore set up close to the writer's bedroom, so that the turning might be continued through the night by the help of an alarum. The egg trays of this machine are fitted with seven turning tapes, each carrying ten eggs, which are normally operated together by a frame connecting them. These were removed, and seven separate tapes, each arranged as an endless band, put in their place. Each tape could thus be operated independently of the rest; and from below, so that the eggs were not cooled in the process. The hanging thermometers were kept at 103° F. for the first fortnight, and then raised to 104° F. The egg trays were interchanged every two days (see section on "Cooling," below). The eggs on six of the tapes in each machine were made to "correspond" in pairs, one of each pair being turned twice and the other at eight equal-time intervals in 24 hours. At the end of the hatch the machines were well disinfected and re-set, the tapes which were turned twice being now turned eight times and *vice versa*. In this way symmetry was obtained in the hatching so far as the incubators were concerned, the double hatch being regarded as one complete measurement.

TABLE VI.

Turning conditions.	Eggs set.	Percentage hatched.	Change of percentage due to extra turning.	Time of hatching.	Time difference.
1. Twice turned	120	53·0	9·0 gain	32·1	1·9
Eight times turned	120	62·0		30·2	
2. Twice turned	120	53·0	13·5 gain	27·1	2·7
Eight times turned	120	66·5		24·4	
Total . .	480	Mean 58·6.	Mean 11·2		Mean 2·3

The number of eggs for each type of turning was thus 240, and two complete measurements were made (Table VI., 1, 2). Both showed a marked gain in percentage hatched for the eight-times over the twice-turned eggs; and in both the twice-turned eggs hatched later than the others. The effect of the eight-times turning was thus comparable with that of the five-times turning of Table III.

Since, moreover, the standard error of the difference between the percentages observed in two samples of 120 each, with an expectation of 58·6 per cent. is 6·4 per cent., the agreement between the two percentage gains is not only as close as can be expected, but the small difference between their mean and the gain for five-times turning (Table III.) may well be due to pure accident.

On the whole, Tables III. and VI. may be said to establish the fact that when the amount of egg turning is raised from twice to five-times in 24 hours there is a real improvement in the hatching of the order of 10 per cent., and that for a further rise from five- to eight-times the corresponding change in the hatching—if any—is negligibly small.

One of the "Factors in Incubation" studied by Prof. KIRKPATRICK (*loc. cit.*) was the effect of turning. Using about seven times as many eggs as the writer, his average hatch percentages were 61·1 and 55·4 for five-times and twice-turned eggs respectively, making his gain in favour of the five times turned 5·7 per cent. It is, however, probable that the extra weight attaching to the experiments made on the larger number of eggs must be considerably scaled down because the influence of the incubator conditions was not so completely eliminated as in the present work.

TABLE VII.

Turning conditions.	Percentage hatched.		Percentage gain of A over B.
A. B.	A.	B.	
5 times *v.* twice (K)	61·1	55·4	5·7
5 times *v.* twice (C)	70·2	59·7	10·5
8 times *v.* twice (C)	64·2	53·0	11·2

In Table VII. are collected the results of all the turning experiments. K = KIRKPATRICK, C = CHATTOCK.

Origin of the Influence of Turning on Hatching.—There appear to be two ways, from the physical point of view, in which the influence may be exerted.

(*a*) The usual explanation is that the turning prevents the germ from sticking to the shell. If this is the only function its effect should vanish after the first ten days or so of incubation.

(*b*) But when an egg is turned over the warmer upper surface goes below and the cooler under surface comes up. The temperature of the egg is consequently more uniform, while the initial difference of temperature between these surfaces is re-establishing itself; and this implies that the average value of the highest temperature in the egg is lowered. It seemed possible that this redistribution of temperature might also be responsible for some of the effect.

[There is a certain amount of evidence available for *a* and against *b*, which, though not conclusive (it would require at least a couple more seasons' hatching to make it that), seems sufficiently definite to be worth collecting here, under the following six tests.

Tests I to IV.—To test hypothesis *a*, comparison was made of the relative effectiveness of turning during the earlier and later parts of the incubation period. Four experiments were made for this purpose, of which the results are given in Table VIII, each experiment (except 1) being the mean of two in which the conditions were reversed to obtain symmetry, as already explained.

In VIII, 1, two 160-egg Gloucesters were set with 107 eggs each. The eggs in every other row were turned five times after the seventh day; those in the remaining rows twice; while before the seventh day all the eggs were turned twice.

The test was not thought of until after the incubators were set, so that the eggs did not correspond; but the number of them was considerable, and it may, at least, be said of the result that the extra turning has not *improved* the hatching.

TABLE VIII.

Turning conditions.			Eggs set.	Percentage hatched.	Percentage gain of A over B.
1. A. Turned twice in first week, five times in last fortnight.			214	73	− 9
B. Turned twice throughout				82	
Turning conditions during half the incubation period.	First half.	Second half.			
2. A.	Twice	None	40	40	+ 15
B.	None	Twice		25	
3. A.	Twice	Once	172	56	− 4
B.	Once	Twice		60	
4. A.	Eight times	Twice	78	78	+ 6
B.	Twice	Eight times		72	

VIII, 2, affords stronger support to the germ-sticking theory; the percentage hatched when the turning occurred in the first half-period being well above that for the second.

The absolute values of both percentages have also been considerably reduced—presumably by the absence of turning—as the hatchings of 2 and 3 occurred together in the same incubator, and the percentages of 3 are much nearer normal (4 was hatched in a different incubator). Absence of turning in the second half-period is thus only a degree less harmful than in the first.

Considering how serious the results are known to be of omitting altogether the turning of incubating eggs, it is, perhaps, surprising that more harm was not done to the hatching in this case. The writer is inclined to explain it by the fact that the egg trays were turned end for end each day; so that, though the eggs were not turned over in the ordinary way, they did partake of a certain amount of gentle turning about a vertical axis.

The remaining two tests, VIII, 3 and 4, show changes in percentage of 4 and 6; the 6 for and the 4 against theory *a*. VIII, 3, is thus the only one of the four tests that is not in accord with that theory.—*Added January*, 1925.]

Test V. (*Table IX.*).—The egg tray of a 130-egg Gloucester was replaced by five narrow trays, each tray capable of holding two rows of "corresponding" eggs—9 eggs in each—one row being turned five times and the other twice daily. The trays stood at different levels so that their initial temperatures (each the mean of three values obtained from three clinical thermometers among the eggs) varied from 104°·3 F.

to 99°·5 F. The "initial" temperatures were taken on the second and the "finals" on the nineteenth day. The hanging thermometer was kept at 103° F. and did not vary by more than 0°·1 F. between these two days. In this way when the chicks came out a tray was found for which the temperature had been on the verge of being too high for hatching; and if the better hatching of the more frequently turned eggs were attributable to redistribution of the temperature (*b*) the extra turning for this particular tray should have an especially marked effect in raising the hatch percentage. Tray 2 appears to have been this particular tray, but there is no sign that the five-times turned eggs had any advantage over the others—rather it was the other way. The trays which do show the advantage are Nos. 3 and 5, lying on either side of the tray for which (No. 4) the conditions seem to have been practically perfect; and this is borne out by the times of hatching, which for Trays 3, 4 and 5 are all some hours earlier for the five-times than for the twice-turned eggs, while for both turnings Tray 4 was the earliest to hatch of the three. That Tray 5 was so many hours later than the other two was to be expected from its low temperature. The times of hatching for Tray 2 are, of course, not dependable with so few eggs to reckon from. It is interesting to note the effect on the final egg temperatures of the living eggs in the trays, and how the lack of these in 1 and 2 results in low final temperatures.

TABLE IX.

Tray.	Temperature.		Eggs hatched out of 9.		Time of hatch.	
	Initial.	Final.	Five times.	Twice.	Five times.	Twice.
1	104·3	103·0	0	0	?	?
2	103·4	103·9	1	2	21 ?	11 ?
3	102·1	104·3	7	4	21	28
4	101·0	104·2	8	9	19	23
5	99·5	102·7	8	4	32	36

It is, of course, true that the number of eggs on each side of Table IX. is too small for stress to be laid on the numerical value of any single comparison, but it is here less a question of individual percentages than of the table *as a whole*. Considering the five cases together, it might have been hoped that if increased turning had had any considerable favourable effect in lessening the deadliness of the higher temperatures, this would have been exhibited by a *tendency* for the numbers in the five-times column to fall at a rapidly diminishing rate as the death temperature was approached. No effect whatever of this kind is apparent. If we omit Tray 4, because the hatching is there so perfect that the turning has not scope to show its full effect, the ratios of the "five-times" to the "twice" hatchings for Trays 5, 3, and 2 are 2·0, 1·7, and 0·5 respectively—a result which would require the hatching in Tray 2 of several eggs (say four or five) in place of the one egg actually hatched to be consistent with the redistribution of temperature theory.

Test VI. (*Table X.*).—The remaining test depended on the fact that if change of temperature distribution in the egg is to account for the effect of extra turning, this effect should vanish if the extra turning occurs in a region where the temperature is uniform.

TABLE X.

	Percentages hatched.			Times of hatch.		Difference of temperature between top and bottom of the eggs.
	A.	B.	Difference in favour of A.	A.	B.	
Upper incubator . .	69	51	18	43·7	42·8	0°·4 F.
Lower incubator . .	82	69	13	25·3	25·4	6°·2 F.

" A " means five times turned ; " B " twice turned.

The Gloucester Incubator Company make a type of double incubator consisting of one of their ordinary machines with a second egg chamber above it, both chambers being heated by the same lamp. In the lower incubator there is the usual temperature difference of several degrees between the top and bottom of the eggs, but in the upper the egg temperature is almost uniform. The company had kindly lent me one of these double incubators, and I was glad to use it for the present test.

Four sets of 35 eggs each were taken, which very nearly all " corresponded." Two of them were arranged in alternate rows in the lower and the other two in the upper half of the incubator; but though there was practically no temperature gradient in the upper eggs, as the last column of Table X. shows, the extra turning was effective in both parts of the machine, and, allowing for the smallness of the number of eggs dealt with, to about the same extent in both. The hatches in the lower incubator will be seen to have been much earlier than in the upper, therein agreeing with the fact that the percentage hatched in the lower was a good deal larger than the upper. This is, no doubt, because the eggs in the upper had to be kept there longer than the makers advise for the purpose of the test. No certain difference appears between the times of hatching of the differently turned eggs.

Though the numbers of eggs dealt with were not large enough for the conclusion to be quite beyond doubt, when these six tests are considered together they furnish a strong case for the germ-sticking theory (a) and against that of temperature redistribution (b).

Cooling of the Eggs.—Table XI. contains the record of all the experiments made to determine whether the cooling of incubated eggs, in the way in which they are cooled

TABLE XI.

	Eggs set.	Percentage hatched.		Increase of percentage due to cooling.	Time of hatch.		Time lag of cooled behind not cooled.	Time of 18 coolings.
		Cooled.	Not cooled.		Cooled.	Not cooled.		
1*	72	94	45	+19	23·6	20·9	2·7	3·5
2	72	59	74	−15	31·8	30·6	1·2	3·1
3	72	74	67	+ 7	27·2	25·2	2·0	2·9
4	140	64·3	57·9	+ 6·4	35·8	31·6	4·2	6·0
5	140	57·9	56·7	+ 1·2	35·3	31·4	3·9	6·0
6	140	53·6	58·0	− 4·4	31·6	22·8	8·8	6·0
7	140	52·3	59·0	− 6·7	27·8	21·4	6·4	6·0
Weighted means :				+ 0·3			4·8	5·2

* The temperature of this experiment may have been too high.

when the hen leaves the nest, is of benefit to the hatching or not. Experiments 1 to 3 were made at Crowcombe with a 160-egg Gloucester, of which the egg tray was replaced by eight narrow trays holding nine eggs each, with their floors of the same curvature as the original tray to obtain uniform temperature. By a simple device the eggs could be turned in the incubator without cooling. All the eggs were thus turned twice a day, and after the first of these turnings the four alternate trays were put outside to cool. Thirty-six eggs were thus cooled daily and 36 were not. The time of cooling was about 11 minutes each day.

Experiments 4 to 7 were made in the two 70-egg incubators, at the same time as the experiments on turning already referred to, by arranging that each egg in one of the incubators "corresponded" with the similarly situated egg in the other, and that one of the egg trays was cooled each day and the other not. By interchanging the egg trays every two days any possible difference in the effect of the two incubators on the difference of the hatchings was eliminated, and a second hatch, in which the rôles of the trays in the first hatch as to cooling were reversed, eliminated any effects of the trays themselves. The two hatches forming one of these pairs are indicated by brackets. The cooling for experiments 4 to 7 was for 20 minutes each day, and was effected by leaving one of the trays outside its incubator; while, except during its rapid transference from one to the other incubator, the remaining tray was not cooled.

In the fifth column of the table are collected the changes in percentage of eggs hatched that have accompanied the various coolings. It will be seen that these changes, while often considerable, take place roughly as much in the positive as in the negative direction, their mean being no more than 0·3 per cent. in favour of the cooled eggs. KIRKPATRICK, on the other hand, working with nearly five times as many eggs, found that the cooled eggs hatched on the average about 3 per cent. less well than the non-cooled. This figure he obtained as the mean of five hatches, and although the number of eggs set for each hatch ranged from about 600 to 900, his individual percentage changes (viz.: − 16, + 2, + 8, − 1, − 8) are scarcely less erratic than those of Table XI.

It is, of course, obvious that the means of results which differ so widely among themselves and are so few in number cannot possess much numerical significance. At the same time the two sets of experiments agree in showing that, whatever may be the cause of the fluctuations observed, the mean percentage change accompanying cooling is probably negligible when measured on a large enough number of separate hatches; and so far as the economics of poultry farming are concerned, this information is sufficient. For it is the average of the incubation results for the year's working that is economically important, and it appears that, although individual hatches may fluctuate, this average will remain sensibly the same whether the eggs are cooled or not.

Time-Lag due to Cooling.—It will be seen that the times of hatching in Table XI. are far less erratic than the changes of percentage. In every case the cooled eggs hatch out later than the non-cooled. Now the eggs were cooled from the second to the nineteenth day, *i.e.*, 18 times. If these 18 daily coolings are added together and expressed in hours the numbers in the last column are obtained; and if these numbers are compared with those in the last column but one a rough, but unmistakable, agreement between them appears, the weighted means for the two columns being, in fact, practically equal.

The retardation in the time of hatching of cooled eggs is thus, in all probability, satisfactorily accounted for by the temporary suspension of growth while the eggs are out of the incubator, and it does not seem necessary to invoke any other cause, such as a weakening of the cooled embryos, to explain it.

Fluctuations in Change of Percentage with Cooling.—These are so much larger than the fluctuations which accompany changes in other physical conditions of incubation that they seem to require special examination.

When the eggs in an incubator are divided into the two equal groups which are to be treated differently during incubation, and of which the hatchings are subsequently to be compared, it is impossible to obtain exact initial equality between them even by arranging that they "correspond." Differences in the influence of the incubator upon the two groups may be to a great extent eliminated as already explained, but those between the two eggs of a "corresponding" pair cannot be allowed for; and it therefore follows that when a series of such hatches is carried out in each of which the difference of treatment of the two groups is the same, it is mainly to the eggs themselves that we have to look for the origin of any large accidental fluctuations that may occur in the difference, D, between the two percentages hatched.

A convenient measure of the violence of such fluctuations is the root-mean-square (R.M.S.) of the differences between each individual D and the mean of all the values of D in the series, and this quantity is given in Table XII. for both sets of experiments on cooling. The only other data for which it can be satisfactorily calculated are those for turning of the eggs, and its values in this case are also given for comparison. An increase in the number of eggs per hatch should, of course, reduce the R.M.S., and in both the cooling and the turning figures there is, in fact, a drop shown of about the same amount in each, when we pass from the small to the large numbers of eggs.

The drop is, however, only from 10·3 to 8·3, or 24 per cent. in the case of the cooling experiments, and from 6·7 to 5·2, or 29 per cent., for those on turning; while, as the corresponding changes in the numbers of eggs are respectively seven- and nine-fold, much larger reductions in the R.M.S. might have been expected. That these did not occur may have been for the following reason.

So long as the R.M.S. only measures fluctuations which have their origin in the eggs themselves its value must fall as the number of eggs per hatch increases, but if it includes effects due to the imperfect elimination of fluctuations in the incubation conditions it will be to that extent less affected by a given increase in the number of eggs. Now in the experiments recorded in Table XI. the effect of the incubators on the percentage differences observed was very completely eliminated by one or other of the methods described on p. 413; and as this precaution was not taken in Prof. KIRKPATRICK's work it is probable that the fluctuations found in his observations are too large for comparison with those of Table XI., so that the expected reduction in the R.M.S. due to his use of larger numbers of eggs is greater than that actually found.

The chief interest in Table XII., however, lies in the high values of the R.M.S. obtained from the cooling experiments when compared with those from the experiments on turning.

TABLE XII.

		Average of eggs set per hatch.	No. of hatches.	R.M.S.
Cooling	Table XI.	110	7	10·3
	KIRKPATRICK	730	5	8·3
Turning	Table III.	90	6	6·7
	KIRKPATRICK	820	9	5·2

10·3/6·7 = 1·54, and 8·3/5·2 = 1·60. The mean value of the R.M.S. for cooling is thus 57 per cent. higher than that for turning, a number which implies large fluctuations.

It is, of course, not beyond possibility that these were purely accidental, but it is not easy to understand how this may have been. Differences between "corresponding" eggs are ruled out as a cause because the effect is practically unaltered when the number of eggs is increased many times, and lack of symmetry in the influence on the hatching of the incubators themselves is presumably allowed for because it is present in the turning as well as in the cooling experiments, and there is no reason to suppose that it was markedly different in the two cases.

The only remaining possibility seems to be that the 57 per cent. increase in the R.M.S. is a real effect of the coolings—a view which receives some support from the fact that the two independent values of which 57 is the mean (60 per cent. in KIRKPATRICK'S work and 54 per cent. in that of the writer) are in such close agreement.

It is here that the cooling effect becomes interesting. For, though the data so far obtained are too fragmentary to warrant detailed conclusions, if further work confirms what has been done and the mechanism can be traced which connects hatching with cooling, it may well prove possible to bring the causes of the large fluctuations under control, and so to improve very considerably the hatching percentages now obtainable. There are even indications in Table XI of a possible connection between sign of percentage change and time of hatching in a given incubator which may be significant in this respect.

There seems, at any rate, a good case for further work on the effect of cooling, though it may well prove laborious.

PART II.*

HUMIDITY AND VENTILATION.

Hygrometry.—In the egg space of a hen's nest and of most incubators the temperature varies by several degrees between the top and bottom of the eggs. The volume, v, of water vapour in each cubic centimetre of nest air is thus more suitable as a measure of the humidity of this air than the relative humidity, since the latter depends on the temperature and v does not.

Further, if p be the pressure of the water vapour and B the height of the barometer

$$v = p/\mathrm{B},$$

where B may be regarded as constant for purposes such as the present; p is thus, like v, independent of the temperature, and for this reason has been used in what follows as the measure of humidity, expressed in millimetres of mercury.

The moist air under a hen is very limited in amount, and any method used for measuring its humidity must work with the moisture held in a few cubic centimetres of it.

In the only measurement known to the writer a wet-and-dry-bulb hygrometer was used ('Ont. Agric. Coll. Bull.,' 163, 1908) and at first sight seems to fulfil this condition. But the wet bulb must increase the humidity in such a confined space; and without free circulation of air there will be too little evaporation from the wet

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* A brief account of part of this section has been published in the 'Transactions of the First World's Poultry Congress at the Hague,' vol. 1, p. 37.

bulb, with the result that the value obtained from the increased humidity will itself be too high. As will be seen below, this is probably true of the number actually obtained (35 millimetres).

A suggestion of Prof. TYNDALL'S was therefore adopted, that use might be made of a method described by ORME MASSON ('Roy. Soc. Proc.,' vols. 74 and 78) and anticipated by CLAYTON BEADLE ('Nature,' vol. 49, p. 457; 'Chem. News.,' vol. 71, p. 1, and vol. 73, p. 180).

This method is based on the fact that very dry cotton-wool exposed to damp air absorbs moisture and warms as a consequence.

The temperature rises rapidly at first, reaches a maximum, and then tails slowly off until, if the temperature of the surroundings is constant, it approaches its original value. Using about 1 gramme of wool tied round the bulb of a thermometer, MASSON found that the maximum temperature was reached about half an hour after exposure to the moist air, but the approach of the temperature to its old value lasted many hours.

To avoid the effects on the thermometer of variations in the temperature of the surroundings, MASSON put his apparatus in a water-jacket kept at a constant temperature by a thermostat. This was impossible in the present work, but the same result was obtained by shortening the period of changing temperature that follows exposure. This was effected by using, in place of the thermometer, a thermo-electric junction of very thin constantan and copper wires, and tying upon it about a centigram of wool; the result being that when the wires were connected to a dead beat galvanometer, and the wool exposed to damp air after drying, the maximum deflection was reached in about 10 seconds after exposure instead of half an hour. It is this maximum deflection that has been used throughout as the measure of the humidity.

The instrument was described in some detail* at the Physical Society during the discussion on "Hygrometry," November 25, 1921. It will, therefore, be sufficient here to explain how it was used in the present work.

Bottle Method.†—The first idea was to render the apparatus independent of the position of the hen's nest on the farm by leaving a small bottle open among the eggs until the humidity of the air in it was the same as that of the nest air. Then, after sealing it, to put it quickly into a portable water-jacket in which it could be carried to the galvanometer and tested at leisure for humidity.

Much of the earlier work was satisfactorily carried out by this method in the case of incubators, the bottles being sealed without opening the incubator, by a simple mechanism which pressed a disc of waxed paper against the warm mouth of the bottle; but when applied to hen's nests it was a failure. Large enough bottles could not be put among the eggs, and there were other troubles which led ultimately to the Rotor‡ type of hygrometer being chosen for use with the sitting boxes.

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* "A Thermal Hygrometer," TYNDALL and CHATTOCK. 'Proc. Phys. Soc.,' vol. XXXIV. Pt. II.

† 'Proc. Phys. Soc.,' *loc. cit.*

‡ 'Proc. Phys. Soc.,' *loc. cit.*

In its final form (fig. 3) it worked admirably. R is a section through the rotor. C_1 C_2 are cylindrical boxes of drawn-brass telescope tubing with closed ends, about an inch in diameter and a couple of inches long. They are connected through wide tin channels, W and X, with the opposite sides of the rotor, and also with the chambers Y and Z.

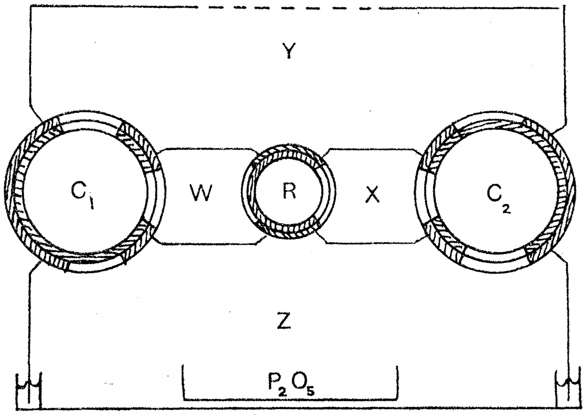


FIG. 3.

The top of Y forms part of the floor of the nest, to which it is open through a window of perforated zinc covered by thin, non-glazed paper; and, after being left undisturbed for an hour or so, the humidity of the air in Y becomes equal to that in the nest just outside the window. Z contains phosphorous pentoxide.

Inside C_1 and C_2 two other tubes turn which just fit them, and which, by the cutting of holes at the proper places, form with C_1 and C_2 what are virtually large three-way taps. Through these Y and Z may each be connected either to W or to X, and either side of the rotor made the drying side. In this way, by taking a second reading with the taps reversed, error due to temperature gradient in the hygrometer may be eliminated.

For standardisation, the whole hygrometer was arranged to slide from under the nest and fit into an incubator. Air was then drawn slowly from the latter through an ordinary chemical hygrometer from a point close to the window of the rotor box, and while this was going on direct and reversed readings were taken with the rotor, the temperature of the wool being as nearly as possible the same as when under the nest.

Diffusion (*Integrating*) *Hygrometer.*—Hygrometers measure the humidity at one particular time, whereas in incubator work the most useful quantity to know is the average humidity over many hours or days. It is, of course, possible to take the average of frequent readings, but apart from the labour involved this takes no account of the often large fluctuations in humidity that may occur during the night.

Fig. 4 shows a section through an instrument which meets this difficulty by automatically and continuously averaging the humidity of air in which it is placed, if the temperature does not vary too much.

A is a square box of about 6 cm. side, fastened airtight to its base by paraffin wax melted into the channel BB. T is a tube passing airtight into A, with its left-hand end about the centre of A. C is a glass vessel containing phosphorous pentoxide.

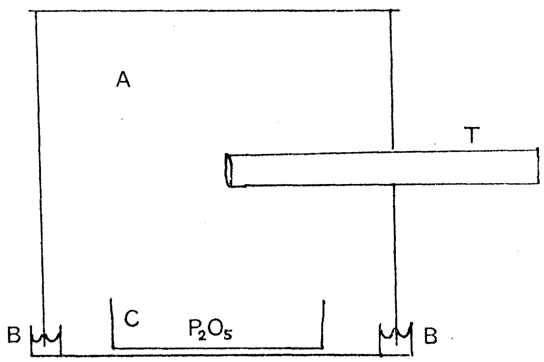


Fig. 4.

If this instrument is placed in the damp air of an incubator, water-vapour will diffuse along T; and if $\bar{p}$ be the average pressure of the water-vapour during the diffusion, D the co-efficient of diffusion of water-vapour into air, δ the density of water-vapour at pressure $\bar{p}$, l and s the length and cross-section of T, and w the number of grammes diffusing down T in t seconds,

$$w/t = \mathrm{D}\delta s/l.$$

w is obtained by weighing the instrument before and after the diffusion. D is about 0·25 at 20° or 0·27 at incubator temperature. The value of δ is thus obtainable, and from it $\bar{p}$ may be calculated.

T should be narrow and either made of or covered by some good conductor of heat, to avoid convection as the vapour diffuses along it. In the present experiments it was of brass, its length being about 6 cm. and internal diameter 6 mm. A was of tin with T soldered into its side. Strictly speaking, l is a little longer than the length of T. The amount of this correction was measured and was found to lie between 0·6 and 0·9 mm. The inner surface of T was covered with thin paraffin wax.

In using this instrument two days were usually allowed between successive weighings, and great care was taken that it was treated in the same way at each weighing, as to the time it was left on the balance, the way it was handled, and so on. The tin tended to take up water from the air or the reverse, and the care was to avoid error from this cause. Silica would have been a better substance than tin, but it was not procurable during the war.

Humidity in the Hen's Nest.—There are two ways in which a knowledge of the humidity under a sitting hen may afford useful information as to other physical conditions in the nest:—

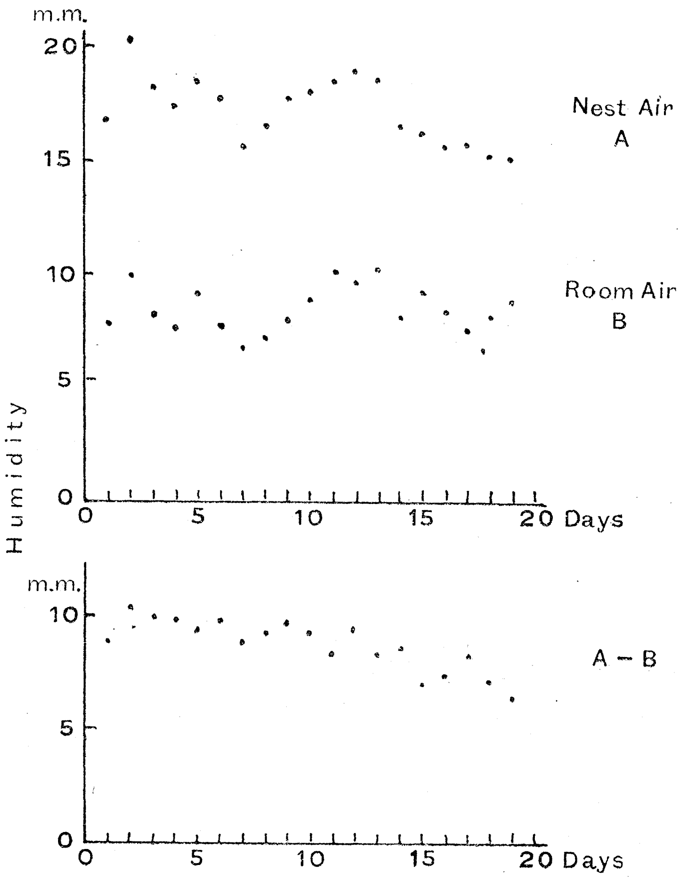
1. It may prove a direct effect of the moisture on the *hatching percentage*.
2. With moisture supplied to the nest at a given rate the humidity will depend on the rate at which such moisture escapes from the nest, *i.e.*, on the *ventilation*.

A careful determination of both the magnitude and distribution of humidity in the sitting boxes already described was therefore undertaken.

The moisture under a hen is made up of two parts: that present in the air of the room, which remains in that air and is carried with it when it passes into the nest; and the moisture added to this by evaporation from the hen, eggs, and, in some cases, nest. Any fluctuation in the humidity of the room air will thus produce corresponding fluctuations in that of the nest.

This is well illustrated by Curves I, which show the run, during the incubation period, of a typical set of humidity measurements at the centre of one of the hens' nests.

CURVE I.



Curve A is for the humidity of the nest air; Curve B for that of the room air just outside the nest. It will be seen that the ups and downs of B are well reproduced in A, while the curve A–B, which gives the moisture received from hen, eggs, etc., is much smoother, as it is independent of the fluctuations occurring in B.

In Table XIII. are given the mean humidities at the centres of the hens' nests for the seven satisfactory hatchings in Table II. They are taken to the nineteenth day of incubation. The mean value for the "earth" nests is lower than for the others because the earth used was so dry as to be powdery; but the "Tin" nest measurements were the more dependable, and a weighted mean of 20·0 mm. is probably as near the truth as may be.

TABLE XIII.—Humidity in Hens' Nests.

Date of setting.	Type of nest.	Humidity at centre of nest. millimetres.
1917.		
July 20	Earth	21·1
Aug. 6	Earth	18·8
Aug. 18	Earth	16·7
Sept. 23	Tin	19·4
1918.		
Aug. 2	Tin	20·0
Aug. 26	Tin	21·8
Aug. 26	Earth	17·2
Means	Earth	18·4
	Tin	20·4
		(say) 20·0

At the centre of the nest the humidity is at its maximum, and it was found to be about 1·8* mm. higher there than its average value taken over the whole nest. 1·8 has, therefore, to be subtracted from the 20·0 mm., giving as the

Average nest humidity, 18·2 mm.

This value is, however, to be regarded as a lower limit, for the ground on which a hen usually makes her nest is more or less damp, and this will help to raise the humidity.

It remained to apply this result to the hatching in incubators. For many years it has been customary with the users of incubators to attribute bad hatching to incorrect humidity, and it, therefore, seemed possible that if the humidity were raised to that found under hens there might be a large improvement in the hatching.

Most of the incubators on the Crowcombe farm happened to be hot-air machines in which the humidity under normal conditions ranges from 7 to 10 mm., and the hatching has usually averaged 70 per cent. to 75 per cent. The humidity of four of these was, therefore, raised by supplying water to their hot-air channels, and they were then run in the ordinary course of farm routine.

Altogether 766 eggs were set, of which 74 per cent. hatched, yet the average humidity was 16·3 mm. The effect of the moisture, if any, was evidently small, and a method

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* This correction had not been obtained when the Hague paper (*loc. cit.*) was written. It is based on the diffusion argument given in the Appendix.

of measuring it was, therefore, required in which it would not be swamped by fluctuations in the hatching due to other causes.

Method of Two Incubators.—Two sets of "corresponding" eggs, 115 in each (110 in subsequent hatches), were arranged correspondingly in two equal 160-egg Gloucester incubators, the only known difference between which was that moisture was supplied to one but not to the other.

To increase the symmetry the egg trays were interchanged every two days, the supply of moisture being transferred at the same time from one incubator to the other. In this way one of the trays was always in damp air and the other in dry, but the trays each spent as much time in one incubator as in the other.

Humidities were measured by diffusion hygrometers. P and Q are the two trays. Eggs were set on April 22nd.

In Table XIV. are given the results of this test. They show a very considerable advantage to the damp tray, P, which hatched both 23 per cent. better and 5·2 hours earlier than Q. This was an unexpectedly large effect, and seemed at first sight to be inconsistent with the results referred to above.

TABLE XIV.—Two-Incubator Method.

Test.	Date of setting.	Tray.	Mean humidity millimetres.	Percentage hatched.	Time of hatch.
1	April 22	P	21·2	74	50·7
		Q	11·2	51	55·9
	Differences . . .	P—Q		+23	— 5·2
	May 19	P	12·9	85	23·9
		Q	21·0	70	25·6
	Differences . . .	P—Q		+15	— 1·7
2	June 12	P	11·8	75	27·2
		Q	20·6	79	28·1
	Differences . . .	P—Q		— 4	— 0·9
	July 7	P	21·6	79	26·5
		Q	13·2	76	26·6
	Differences . . .	P—Q		+ 3	— 0·1

There was, however, one chance of reconciling them. Though the incubators had been interchanged with respect to the eggs, the trays had not; and if some influence adverse to hatching were present in Q it might have been responsible for some of the large difference observed between them. By an oversight the trays had been brushed before using, but not disinfected as usual.

The experiment was, therefore, repeated on May 19 with the trays interchanged and only lightly brushed before receiving their eggs. It will be seen that P still has the advantage over Q, though Q is now the damp tray; but the difference is less, P now hatching only 15 per cent. better than Q and 1·7 hour earlier.

Taken together, these two hatchings afford strong evidence that Q was exerting an adverse influence on the hatching throughout, and on the assumption that this influence was constant (which, of course, need not necessarily have been the case), the effect of raising the humidity from an average of 12 mm. to 21·1 mm. will have been to improve the hatching by half the difference between 23 and 15, or 4 per cent.

To complete the argument, it remained to remove the adverse influence and show that the difference between the hatches in damp and dry air was the same no matter which tray was damp.

Both trays were, therefore, well washed with Jeyes' Fluid and the two experiments repeated. The results are given in Table XIV., Test 2; and, it must be admitted, are in surprisingly close agreement both with each other and with Test 1. The adverse influence is gone. The times of hatching are practically equal for P and Q in both experiments—less than an hour apart—and so are the percentages hatched. But the latter show a small difference of 3 or 4 per cent. in favour of the damp tray, whether this is P or Q, and not only is 3 indistinguishable from 4 in such a measurement, but it is also the same as the 4 per cent. obtained in Test 1.

On the other hand, the standard error of the differences between the P and Q percentages is large; in Test 2 it works out at 5·6 per cent., and it would be of the same order in Test 1. No stress can, therefore, be laid on the exact values of differences such as 3 or 4 per cent., since they are just of the order of probable error. But there is the further fact that the differences are of the same sign throughout and of the same order of magnitude (when correction is made for the hypothetical adverse influence in Test 1), and if this is taken into consideration it does suggest that they are very probably real.

The mean values of the two humidities were 12·2 and 21·1 mm. respectively, and in the writer's opinion it may be safely concluded that when the humidity is raised from the lower to the upper value the hatch percentage is also raised, and by an amount which is not far from 4 per cent. 12·2 mm. is a higher humidity than the usual 7 to 10 mm. of these incubators run "dry." The reason is that their walls absorbed moisture during each two days that water was supplied to them and gave it out again in the two days that followed, with the result that the air did not return to its normal humidity at once.

KIRKPATRICK'S *Experiments*.—Besides the work on turning and cooling already referred to, Prof. KIRKPATRICK* studied the effect of humidity on hatching. With access to a much larger number of eggs than the writer, he varied the humidity in his experimental incubators from hatch to hatch and obtained hatch percentages for widely different humidities.

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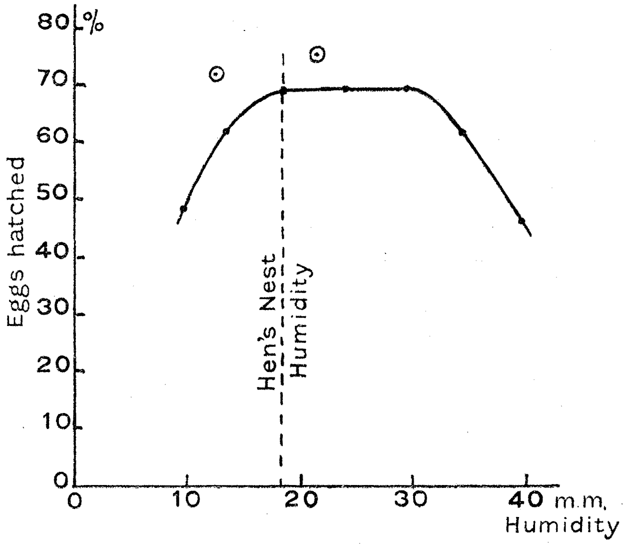
* *Loc. cit.*

His results (expressed as vapour pressures instead of relative humidities) are plotted in Curve II, and show that, while for extremes of moisture in either direction the percentage falls, there is a range of humidity between about 18 mm. and 29 mm. over which it is constant and at its maximum value.

The writer's value 18·2 mm. for humidity under hens thus falls just inside the lower end of the critical range, and it follows that, so far as moisture is concerned, the hen's nest conditions are those of maximum efficiency in hatching, and the two sets of experiments are in satisfactory agreement.

Curve II.



So, too, are the effects upon hatching of changing the humidity, but in this case the agreement is less striking. While Kirkpatrick finds that a rise of humidity from 12·2 mm. to 21·1 mm. raises the hatching percentage from 59 to 69, the same change in the present experiments only raised it from 72 to 76, or less than half as much (Curve II, circles).

In comparing these two results there is first the fact that the number of eggs incubated by Kirkpatrick per point on his curve is about three times the number per point of the present experiments. This, of course, gives considerable weight to his result. On the other hand, in his method of supplying moisture it was not only the humidity that altered. The air flow varied and with it probably the temperature gradient, so that the influence of other conditions was not so completely eliminated as in the method of two incubators here described. There was also the fact that values of the humidity obtained by the use of the diffusion hygrometer were the true mean values during the 24 hours. When these considerations are allowed for, the apparent advantage of the larger number of eggs comes near to vanishing, if it does not actually reverse.

Be this as it may, the writer ventures to think that the agreement found between the results of experiments on such different lines is sufficiently close to put these results beyond any reasonable doubt, and to settle the problem of moisture so far as the practical incubation of hens' eggs is concerned. It seems clear that too much weight has been attached to the influence of moisture. For hot-air machines run "dry," which therefore depend for their moisture on the evaporation from the eggs and on what is present in the air of the room, the hatch percentages are perhaps 5 or 6 per cent. lower than they would be with water supplied; and this number will be larger in frosty weather, or for an incubator containing too few eggs, or one in which the air flow is very rapid. But for a machine like the Hearson, to which water is already supplied, the humidity is so near that of the hen that the effect of further increase is not worth troubling about.

Connection between Humidity and Hatching.—Physically speaking, there are two ways in which a decrease of humidity might affect the hatching. Its direct effect is, of course, to increase evaporation from the eggs, and this will result in:—

(*a*) A lowering of the temperature of the eggs;

(*b*) More rapid removal of water from the eggs, implying a larger air space and less water in the egg contents.

Both these effects were measured: (*a*) in the four hatches of Test 2, Table XIV.; (*b*) in the first hatch of Test 1.

Somewhat elaborate precautions were required in obtaining (*a*), as a change of temperature in the eggs themselves is difficult to measure with certainty, even when larger than this one proved to be. Daily readings were taken of four clinical thermometers lying on the eggs in each incubator. The hanging thermometer was also read at the same time.

The eggs (110 in a tray designed for 160) were arranged in a square, and the bulbs of the four clinicals were placed about half way between each corner and the centre, the tops of the bulbs being roughly on a level with the tops of the eggs. The mean temperatures of the hanging thermometers were respectively 103°·2 and 103°·1 F., with a mean divergence from these of about a quarter of a degree in each case.

In Table XV. are given the differences between the hanging thermometers and the means of the four clinicals on each day—a quantity which becomes negative towards the end of the incubation period when the eggs become warmer than the hanging thermometers.

Each set of four clinical thermometers remained in the incubator it started in, and thus measured alternately the temperatures of eggs in damp and dry air. Hence as the values of Table XV. are tabulated according to whether they were measured in damp air or dry, about half the numbers in each column were measured by one set of clinicals and half by the other, and there is consequently no error due to the zeros of the thermometers being incorrect.

Error in the zero of the hanging thermometers will alter the numbers in each pair of columns by the same amount, but will not affect their difference. Lastly, error due to the level of the floors of the two trays not being quite the same is eliminated when the means for the two pairs of hatches are taken, since the trays were interchanged in the second pair as regards their humidities.

TABLE XV.

Day of Incubation.	Difference between hanging thermometer and clinicals.				Means of all four columns.
	Set June 12.		Set July 7.		
	Damp.	Dry.	Damp.	Dry.	
1	2·7	3·2	—	—	—
2	3·6	3·1	2·1	2·5	2·82
3	3·4	3·1	2·4	1·8	2·68
4	3·6	2·9	2·0	1·7	2·55
5	2·4	2·7	1·5	2·4	2·25
6	1·5	2·0	1·9	2·3	1·93
7	2·2	1·7	1·8	1·6	1·82
8	1·6	1·6	1·7	1·5	1·60
9	1·4	1·9	1·4	2·3	1·75
10	1·7	1·7	1·3	1·6	1·57
11	1·5	1·2	1·2	1·3	1·30
12	1·4	0·9	1·0	1·0	1·07
13	0·5	1·1	0·7	1·4	0·92
14	0·1	0·5	0·5	0·5	0·40
15	–0·1	0·1	–0·3	–0·6	–0·22
16	–0·4	–0·3	–0·1	–0·4	–0·30
17	–0·5	0·2	–1·4	–0·1	–0·45
18	–1·0	–0·1	–0·8	–0·6	–0·62
19	–0·4	–0·5	–0·7	–0·9	–0·62
Means	1·33	1·42	0·90	1·07	
Differences	0·09		0·17		

Table XV. shows that the temperatures of eggs in damp air were definitely higher than of those in dry by 1°·42–1°·33 in the first hatches and 1°·07–0°·9 in the second, the mean of these two differences being 0°·13 F. It is almost impossible to believe that so small a change of temperature is sufficient to account for even the small gain of 4 per cent. in the hatching.

(*b*) may, of course, be measured by weighing the eggs at intervals, but in order that the change of weight might be due to nothing but loss of water some new-laid shells were "blown," nearly filled with water, and sealed with a scrap of rubber and elastic glue applied hot. Five of these water eggs were then put in each of the incubators set on April 22 (Table XIV.) and weighed every two days when the trays were interchanged. The water eggs were not transferred with the trays, and were consequently in damp and dry air on each alternate two days.

The results are given in Table XVI. and show that the presence of 21·2 mm. of water vapour causes a smaller rate of evaporation than 11·2 mm.; the difference being 0·78 gramme per 10 eggs per 24 hours or 1·6 gramme per egg in the whole incubation

TABLE XVI.

Day of Incubation.	Change of weight of 10 "water" eggs in grammes per 24 hours. D, dry; W, wet air.	
	Incubator 1.	Incubator 2.
2	3·00 D.	2·54 W.
4	2·78 W.	4·36 D.
6	3·76 D.	3·66 W.
8	2·86 W.	4·46 D.
10	3·64 D.	3·63 W.
12	2·93 W.	4·61 D.
14	3·84 D.	3·81 W.
16	3·30 W.	4·73 D.
18	3·81 D.	3·97 W.
Mean D.* . . .	3·76	4·54
Mean W.* . . .	2·97	3·77
Difference . . .	0·79	0·77

period. This is a considerable quantity, and may well account for the corresponding improvement of 4 per cent. in the hatching.

Ventilation in the Hen's Nest.—During incubation the air of the nest is continuously receiving water vapour and carbon dioxide from the eggs, hen, and materials of the nest; and it is the function of ventilation, by facilitating their escape, to prevent the undue accumulation of these substances in the nest air. The escape may occur in two ways—by convection, when the two gases are carried bodily out of the nest by air currents; and by diffusion, when they travel out through air which may itself be stationary. These two processes are independent of one another; but there is a close connection between the quantities of H_2O and CO_2 escaping by either process separately, because the two substances travel along the same paths, and, generally, under the same geometrical conditions. It is thus possible to obtain accurate information concerning the movements of CO_2 in the nest from the much more easily studied movements of H_2O.

As the CO_2 comes mainly from the growing embryos it is necessary, for a dependable comparison, that the H_2O shall also have come mainly from the eggs, and the following plan, which virtually amounts to earmarking the water from the eggs, was therefore adopted.

Two hens were set simultaneously on "tin" nests (August 1) one on 13 normal (fertile) eggs, and the other on 13 similar eggs which had been dipped in hot melting wax. The two sets of eggs were weighed and interchanged every three days, and the humidity

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* Omitting the first value in each column which is too low. The water eggs take time to settle down to a steady state.

measured every day at the centre of the nest and in the air of the room by cotton wool hygrometers. By using "tin" nests fluctuations of moisture due to variable evaporation from the earth floor were avoided, and as the evaporation from the hen could be taken as constant after a small correction had been applied (see p. 429) the changes in humidity which resulted from the interchanges of the eggs could be attributed to evaporation from the normal eggs alone, less the very small amount from the waxed eggs.

A similar pair of hens was set on August 25, the record of one of which is given in Table XVII. as typical of the rest.

TABLE XVII.

Set August 25.	Humidity* in air of :			Difference between humidities of nest and room.		Loss of weight of 10 eggs, in grammes per 24 hours.	
Day of incubation.	Normal eggs.	Waxed eggs.	Room.	Normal eggs.	Waxed eggs.	Normal eggs.	Waxed eggs.
2		(14·9)	7·9		(7·0)		
3		(24·6)	9·9		(14·7)		(0·56)
4		(17·3)	8·2		(9·1)		(0·34)
5		(16·0)	7·6		(8·4)		(0·38)
6	(23·3)		9·2	(14·1)		(3·92)	
7	20·8		7·8	13·0		4·15	
8	22·0		6·8	15·2		4·34	
9		(17·0)	7·1		(9·9)		(0·34)
10		15·6	8·0		7·6		0·31
11		16·8	8·9		7·9		0·39
12	(23·3)		10·1	(13·2)		(4·12)	
13	23·3		9·6	13·7		4·07	
14	24·3		10·2	14·1		4·13	
15		(14·5)	8·0		(6·5)		(0·38)
16		14·6	9·2		5·4		0·40
17		13·6	8·2		5·4		0·38
18	(19·5)		7·5	(12·0)		4·70	
19	19·0		8·2	10·8			
20	20·6		8·8	11·8			
Means :	21·7	15·1		13·1	6·6	4·28	0·37

On the third day the hen broke one of the waxed eggs, which accounts for the high humidity on that day and vitiates the humidities on the two succeeding days. These three values are enclosed in brackets to show that they are not used in calculating the means, and the same is true of the first humidities measured after each interchange of the eggs as the nest was slow to settle down to a permanent state after the humidity had been altered.

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* Each humidity is the mean of three values, taken about 8 a.m., 2 p.m., and 10 p.m. respectively.

The mean values of Table XVII. and those of the two other similar experiments of August 1 are collected in Table XVIII.

TABLE XVIII.

Date of setting.	Nest.	Nest humidity with normal eggs.	Difference between humidities of nest and room. Normal eggs. H_1.	Waxed eggs. H_0.	$H_1 - H_0 = H$.	Effective‡ loss of weight of 13 eggs in grammes per 24 hours.
August 1	1	21·5	12·6	5·8	6·8	4·47
August 1	2	18·6	10·1	3·5	6·6	4·95
August* 25 . . .	1	21·7	13·1	6·6	6·5	5·08
	Means :	20·6	11·9	5·3 (4·2)†	6·6 (7·7)†	4·83

Put into words, the bottom line of this Table means that the average evaporation from three hens and the waxed eggs is at such a rate as to produce a humidity (H_0) of 5·3 mm.; while, if this rate is constant, that part of the humidity (H) which is due to evaporation from the normal eggs is 6·6 mm., when they lose weight at the rate of 4·83 grms. per 24 hours.

Actually, however, the evaporation from the hen is less when the humidity of the nest air is high (normal eggs) than when it is low (waxed eggs), and this must be allowed for. Evaporation from a hen is proportional to the difference between the water vapour pressures inside and outside her skin, that outside being roughly equal to the humidity of the nest air (20·6 mm. for normal eggs and 20·6–6·6 or 14·0 mm. for waxed), while that inside is roughly the saturation pressure of water vapour at her average temperature (say 55 mm.).

It follows that evaporation from the hen with normal eggs is to that with waxed eggs as (55–20·6) is to (55–14·0) or 0·8/1; and that part of the nest humidity due to the hen is 5·3 × 0·8 or 4·2 mm. with normal eggs, H being thus raised from 6·6 to 11·9 — 4·2 or 7·7 mm.

With this correction it is possible to estimate within fairly narrow limits the ventilation occurring under the hen. If p is the pressure of the water vapour at the centre of the nest in mm. Hg., n the number of parts of CO_2 in 10000 by volume at the same

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* The results for the other hen set on August 25 are not available, as the electrical connections with the hygrometer were found to have been faulty.

† Corrected for change in evaporation from hen.

‡ Calculated from the difference between the loss of weight of the normal eggs and that of the waxed eggs in Table XVII.

point, 0·27 and 0·19 the respective coefficients of diffusion of H_2O and CO_2 into air at incubation temperature, 0·273 the weight of carbon in 1 gramme of CO_2, and ρ the ratio of the weights of water and carbon* which are lost by the eggs in the same time, we shall have for the density of H_2O in the nest air $p\ 1{\cdot}06\times10^{-6}$ and for that of CO_2 $n\ 1{\cdot}98\times10^{-7}$ grm. c.c.

Hence

$$\rho_d = p\,1{\cdot}06\,.\,10^{-6}\,.\,0{\cdot}27/n \qquad 1{\cdot}98\,.\,10^{-7}\,.\,0{\cdot}19\,.\,0{\cdot}273 = 212/n$$

$$\rho_c = p\,1{\cdot}06\,.\,10^{-6}/n \qquad 1{\cdot}98\,.\,10^{-7}\,.\,0{\cdot}273 = 150/n,$$

where ρ_d and ρ_c are the ratios for what escapes from the nest by diffusion and by convection respectively; and p is the 7·7 mm. of Table XVIII., corresponding to the observed loss of weight of 13 eggs in 24 hours, viz., 4·83 grms.

The most convenient way of expressing the nest ventilation is to regard the process as one of convection only, and to calculate the number, N, of litres of air that must pass through the nest per hour to remove the CO_2 as fast as it is generated.

Actually, of course, the CO_2 escapes partly by convection and partly by diffusion, and as unfortunately the coefficients of diffusion of CO_2 and H_2O are not the same, the proportion between these two processes must be known if both are operative. But for the two extreme cases of escape by convection alone or diffusion alone this complication is not required, and it will be useful to consider them first.

Let C stand for the weight of carbon lost per egg per hour. For convection alone

$$C = \frac{4{\cdot}83}{13\times24}\cdot\frac{1}{1+\rho_c} = \frac{4{\cdot}83}{13\times24}\cdot\frac{n}{n+150}$$

$$\simeq \frac{4{\cdot}83}{13\times24}\cdot\frac{n}{150} = n\ 1{\cdot}03\,.\,10^{-4}\text{ approximately,}$$

and, similarly, for diffusion alone,

$$C \simeq \frac{4{\cdot}83}{13\times24}\cdot\frac{n}{212} = n\ 0{\cdot}73\,.\,10^{-4}.$$

If C/0·273 = grammes of CO_2 removed per egg per hour from the nest,

g = grammes of CO_2 per litre of nest air

= $n\ 0{\cdot}8\,.\,1{\cdot}98\,.\,10^{-4}$, where 0·8 gives to g its average value over the whole nest, according to the diffusion argument in the Appendix, and

N = litres of nest air removed per egg per hour.

Then

$$Ng = C/0{\cdot}273$$

or

$$N = 2{\cdot}4 \text{ for convection alone} = 1{\cdot}7 \text{ for diffusion alone.}$$

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[*] It is assumed in what follows that all the O_2 received by the eggs is given out again as CO_2. Some no doubt remains in the eggs; but its amount is unknown, and in any case its effect on the value obtained for N is almost certainly unimportant.

It will be seen that the expression for N does not contain n. This is because n is tacitly assumed to be small compared with 150 or 212 in the equations for C. LAMSON and EDMOND (*loc. cit.*) have, however, found that n in a hen's nest rises to values which are considerable compared with these two numbers, so that when N is obtained under such conditions a correction has to be applied. That it is not a large one is shown by Table XIX., which contains values of N calculated on the assumption that the corresponding amounts of CO_2 entered in the first column were present in the "tin" nests of Table XVIII. (The two extreme values of N just obtained appear at opposite ends of the line for "small" values of n.)

TABLE XIX.

CO_2 in 10000.	Ventilation constant, N, for hen's nest.				
n.	0·00	0·35	0·52	0·70	<1·00
Small	2·4	2·1	2·02	1·9	1·7
15	2·2	1·94	$\boxed{1{\cdot}83}$	1·72	1·6
30	2·0	1·7	1·67	1·5	1·5
45	1·8	1·6	1·5	1·4	1·4

LAMSON and EDMOND have measured n for hens' nests containing both normal and boiled eggs, the mean value for normal eggs being 39, and for boiled eggs 24. The difference, 15, between them is due to that part of the CO_2 in the nest which comes from the eggs, and is, therefore, comparable with the 7·7 mm. of H_2O in Table XVIII.; in the sense that the 15 volumes of CO_2 in 10000 bear the same relation to the loss of carbon from the eggs that the 7·7 mm. of water vapour bear to the loss of water from them.

The correct value of N should consequently be found somewhere on the line for $n = 15$, *i.e.*, between 2·2 and 1·6, these numbers taking the place of 2·4 and 1·7 obtained above for too small a value of n.

It is true that n varies much from nest to nest, and that the 15 just quoted was not measured on the actual nests used in the present work. But, as Table XIX. shows, a large alteration in n implies a very small one in N, and it is thus not more than the order of magnitude of n that is required.

2·2 and 1·6 are thus limiting values for N, and while they are not too far apart to be useful in determining nest ventilation, it is possible to bring them much nearer together by estimating the relative proportions in which convection and diffusion are present.

As the argument used for this is somewhat intricate, it is given in the Appendix (p. 446). The deductions obtained from it are briefly the following :—

The numbers heading the five columns for N in Table XIX. are calculated for different (hypothetical) proportions between the weight of H_2O escaping by diffusion alone and the total loss of weight of the eggs.

The heading 0·00 thus implies that there is no diffusion ; the vapour all escapes by convection.

0·35, or 1·7/4·83, means that a basis of 1·7 grm. of H_2O escaping by diffusion out of the total 4·83 grms. lost by the eggs is used in calculating the values of N given below it.

In 0·52 and 0·70 the 1·7 grm. has been increased by 50 per cent. and 100 per cent. respectively, and in the last column there is no convection at all. (Part of the loss of weight of the eggs is due to carbon, otherwise the heading for this column would have been 1·00).

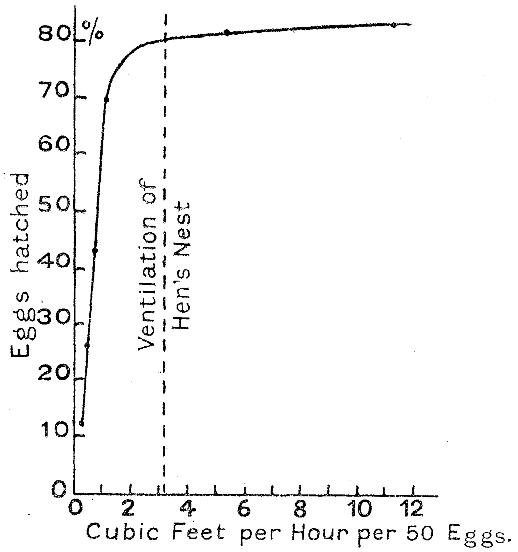
Now the argument in the Appendix shows that the values of N in columns 0·00 and 0·35 are certainly too high. It is also very unlikely that there is no convection in the air of the nest. This rules out the last column, and narrows the issue to that between columns 0·52 and 0·70, *i.e.*, between 1·83 and 1·72 for the value of N. Finally, by what is little more than a guess the value,

$$N = 1{\cdot}8 \text{ litre per egg per hour,}$$

is taken as the most probable.

It is interesting to compare this result with those of LAMSON and EDMOND. They give data on pp. 236, 238 of their paper (Tables A and B) for the connection between air flow through their experimental incubator and hatching percentage, and the means of their results have been plotted in Curve III. This shows the percentage rising rapidly until a flow of between 2 and 3 cubic feet per hour is reached ; beyond that, increase of flow does not much affect the hatching, which is then close to its maximum.

CURVE III.



Reducing the ventilation number for the hen's nest (1·8) to cubic feet and multiplying it by the number of eggs (50) used by LAMSON and EDMOND in their incubator, we obtain 3·2 cubic feet per hour per 50 eggs. This is plotted at the dotted line and falls well within the region of best hatching, the hen's nest conditions thus again turning out to be those of highest efficiency.

LAMSON and EDMOND found, moreover, than when air was pumped through their experimental incubator at the rate of 5 cubic feet per hour per 50 eggs, the density of the CO_2 present was the same as that in their hen nests. There should, therefore, have been agreement between this 5 cubic feet and the 3·2 cubic feet just obtained.

The accuracy with which each of these figures represents the ventilation for which it was calculated is higher than the difference between them suggests; but, apart from the fact that the determinations were upon such different lines, the nests were not the same in the two cases, and this probably affords sufficient explanation of the discrepancy. The important point is that both numbers fall satisfactorily within the region of highest hatching efficiency.

The "tin" nest data also admit of a comparison between that part of the loss of weight of incubating eggs due to loss of carbon with that due to loss of water.

The number of grammes of carbon lost per egg in 20 days $= 1{\cdot}8 \times 23^* \times 0{\cdot}8 \times 1{\cdot}98 \times 10^{-7} \times 1{,}000 \times 0{\cdot}273 \times 24 \times 20 = 0{\cdot}87$.

The grammes of water lost in the same time $= 4{\cdot}83 \times 20/13 - 0{\cdot}87 = 6{\cdot}6$. Carbon thus constitutes 12 per cent. and water 88 per cent. of the total loss of weight of an egg during incubation, on the assumption that these are the only substances it loses. (See pp. 443, 444, and footnote on p. 430.)

Ventilation in Incubators.—It remained to compare the ventilation in incubators with that under hens, and for this purpose the Hearson and Gloucester machines were chosen as types of the "tank" and "hot air" principles respectively.

The problem was, of course, to measure the air flow without altering it in the process; and this resolved itself, in both cases, into the measurement of the very small pressures brought about by the passage of the air through the incubators—pressures which have usually lain between 10^{-2} and 10^{-3} mm. of water. A manometer suitable for this is the Fry Diaphragm Manometer, and this was used throughout the present work ('Phil. Mag.,' 1913).

Fig. 5 shows a Hearson incubator in vertical section. Owing to the tendency of the warm air in it to rise, cold air is drawn in at four holes (A), passes between the two floors (arrows) through the hole T, comes in contact with wet canvas (dotted line), passes up between the eggs and out at the holes B.

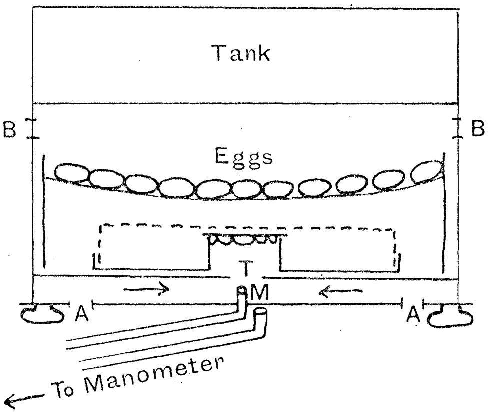


FIG. 5.

The flow between the two floors results in a difference of pressure between the two manometer tubes at M, one of which opens just above and the other just below the

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[*] 23 is LAMSON and EDMOND's value of n corrected for difference of ventilating air flow in the "tin" nest and in their own.

lower floor. This difference was taken as the measure of the flow, and the manometer was standardised afterwards by placing the whole incubator on an air-tight box, so as to form its lid and include the holes A, and passing air into the box at a known rate.

Two sets of readings were taken with a 60-egg Hearson, the first giving an average of 430 and the second one of 436 litres per hour. The extremes of flow recorded were 515 and 382 in the first and 483 and 378 in the second set.

Four hundred and forty litres per hour per 60 eggs, or 13 cubic feet per hour per 50 eggs, is thus, in all probability, a good average working value for this machine, with all its ventilating holes (B) open; and reference to Curve III shows its position on the ventilation curve to be far along the range of best hatching.

In fig. 6 is shown a Gloucester incubator in vertical section. The heating tube AB, by which the hot air enters from the heater H, is open at A and D. C, C′ are canvas screens and E the egg tray. Warm air thus flows downwards past the eggs.

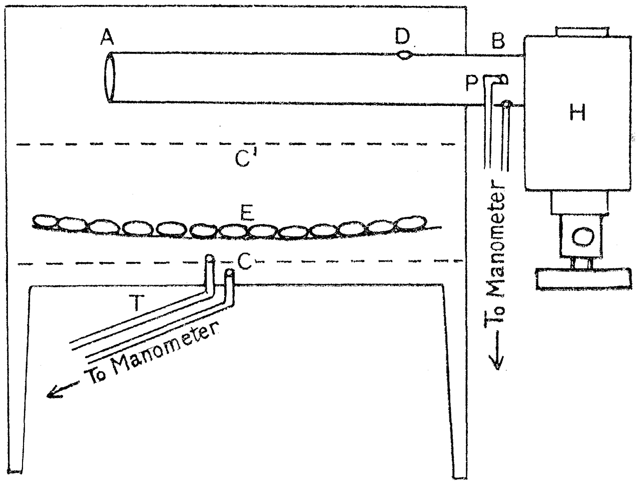

FIG. 6.

The difference of pressure on opposite sides of C was chosen as the measure of the flow. The manometer tubes (T) opened respectively above and below the canvas, and the latter (a new piece, chosen for uniformity) was well stretched on its frame. To interpret the manometer readings the bottom half of a large cardboard pill-box was pressed against the under-side of the canvas, and air drawn from it at a known rate by a tube connecting it with an aspirator. The pressure difference between opposite sides of the patch of canvas covered by the pill-box was measured by the manometer, and the process repeated at a number of evenly spaced positions on the canvas to average out irregularities.

The results obtained with a 160-egg Gloucester are given in Table XX. The felts referred to are those supplied by the makers to put between the eggs and C in the earlier stages of incubation. The fact that they produce so little effect on the flow shows

TABLE XX.

Number of eggs in Incubator.	Cubic feet of air passing through canvas per hour.		Velocity* of air at canvas in feet per hour.	
	2 felts.	No felt.	2 felts.	No felt.
162	15·3	18·1	3·9	4·6
81	16·2	21·0	4·1	5·3
0	15·1	21·7	3·8	5·5
	15·5	20·3		

that most of the resistance to the latter is in the heater and the tube AB. Taking the ventilation at its lowest,

$$15\cdot5 \times 50/160 = 4\cdot8 \text{ cubic feet per hour per 50 eggs.}$$

Here again, therefore, the position on the ventilation Curve III is well into the range of best hatching.

There is, however, more ventilation than this. As in the Hearson, the hot air in the egg chamber tends to rise, and is only prevented from doing so by the upward thrust of the still hotter air in H, which drives it downwards past the eggs. The result is that the pressure of the air above C is higher than that outside the incubator, and though the latter is well put together, there is plenty of opportunity for leakage of air through joints in the woodwork, round the frame of the door, and so on.

To test this a Pitot tube, P, was arranged to slide along a diameter of the hot-air tube at B, so that the velocity, and therefore the total flow of air past B, could be measured. The result was 24 instead of the 4·8 cubic feet found at C, thus pointing to a leak of 19·2 above C.

By a simple device it was next made possible to close the openings in the floor through which the downward moving air escapes. When this was done the flow at B, instead of being stopped, only fell from 24 to 18, the 18 affording a second determination of the leak, in good agreement with the 19·2 just obtained.† Though most of this leak air does not actually pass the eggs, it ventilates, because their H_2O and CO_2 are near enough to diffuse into it easily and so to get carried away when it escapes.

The conclusion may therefore be drawn that in both the Hearson and the Gloucester the ventilation provided is several times more thorough than it is under hens.

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* Roughly 1 inch per minute.

† These measurements were made on an early type of Gloucester machine. It is possible that in the more recent designs the leak above C is a good deal reduced owing to the use of continuous sheets of 3-ply wood in place of the earlier tongued and grooved boards. This does not, however, seriously modify the conclusion as to ventilation.

PART III.

LOSS OF WEIGHT OF EGGS.

The experiments on loss of weight are still in progress, but some of the results already obtained are sufficiently definite to be included with the foregoing.

Loss under Hens.—Every egg under the first 15 hens was weighed separately every three days during the period of incubation on a specially designed direct-reading balance of steelyard type, by which it was possible to weigh each of the 13 eggs in a setting to less than the thousandth of an ounce, while the hen was off feeding.

TABLE XXI.—Loss of Weight of Eggs under Hens.

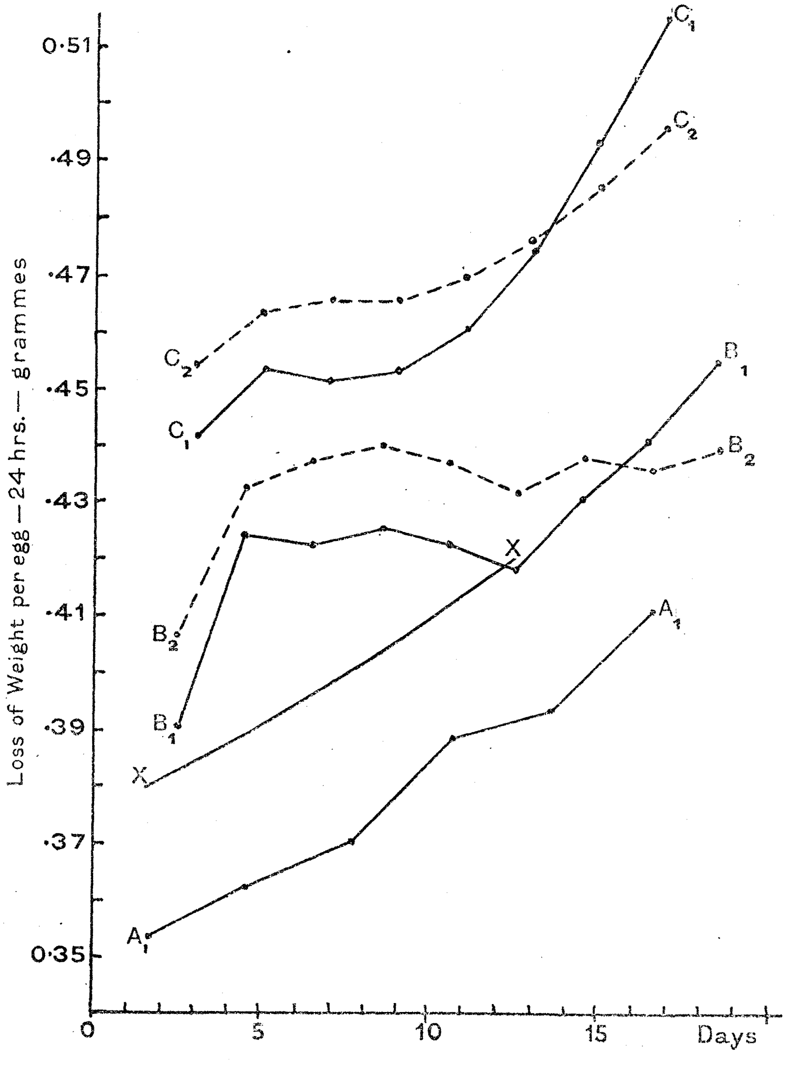
Date of setting.	No. of sitting box.	Mean weight of eggs (ounces).	Percentage change of weight in 20 days.
June 16	1	2·08	14·5
	2	2·12	13·8
	3	2·02	13·9
June 28	4	2·10	13·7
	5	2·03	13·4
	6	1·97	12·9
July 20	1	2·09	12·8
	2	2·00	12·1
	3	2·14	12·3
August 6	4	2·08	13·4
	5	2·09	11·8
	6	2·01	12·5
August 18	1	2·03	14·5
	2	2·06	13·2
	3	2·03	13·3
Means:		2·06	13·2 per cent.

The mean results for each of the 15 settings are given in Table XXI., where the loss of weight of the eggs during 20 days is expressed as a percentage of their mean weight when set. The mean loss of weight of 188 Rhode Island Red eggs, which hatched, was thus found to be 13·2 per cent. of their mean weight (2·06 ounces) at setting. The mean divergence of individual measurements from 13·2 was ± 1·7, the lowest observed being 8·4 per cent. and the highest 20·6 per cent. Very wide variations in the loss of weight are thus compatible with good hatching.

LAMSON and EDMOND give 10·1 per cent. as the loss under their hens. So far as ventilation affects the loss a larger value than 13·2 was to have been expected from the ventilation measurements of Part II., but both humidity and thickness of shell are additional influences which may alter the loss either up or down, and they cannot be allowed for.

The connection between the total change of weight during incubation and the quality of the hatch is thus not a close one, but the manner in which the change is distributed over the incubation period appears to be more significant, as the following comparison of the *rates* of loss of weight, in hen and incubator hatching respectively, shows.

Curves IV.



The comparison is made in Curves IV., where the mean rate of loss of weight (not, now, the percentage loss) per egg per 24 hours is plotted with the number of days since setting. Table XXII. contains the explanation of the Curves.

Column 2 gives the numbers of eggs set, divided into those which did and those which did not hatch. Thus in the case of the normal incubators 477 is the number that hatched and 214 those that died, $477/(477 + 214)$ giving the 69 per cent. of column 4. Of the eggs in column 2 the numbers of those that were weighed appear in column 3.

TABLE XXII.

Curve.	Eggs set.	Eggs weighed.	Eggs hatched.	Method of incubation.
A_1	108	108	92 per cent.	Hens.
B_1	477	75	69 per cent.	Normal incubators. 4 Gloucesters.
B_2	214	25	Dead	1 Hearson.
C_1	238	75	45 per cent.	Incubators with motor turning.
C_2	286	105	Dead	23 settings.

For hen hatching, only the eggs that hatched are entered opposite A_1, those that failed to hatch being discussed separately below. The full-line curves are for eggs that hatched; the dotted for those that did not.

It will be understood that the position of a curve up or down the scale of loss is to some extent accidental, as it depends on the size of the eggs, thickness of shell, humidity and other conditions. If enough eggs are used the relative positions of live and dead egg curves for the same hatchings will be comparable and may be significant, but a similar comparison of curves for different hatchings (*e.g.*, of B with C) may have no particular meaning. It is the shape of the curve that matters.

When the two pairs of curves for artificial incubation in Curves IV. are compared with A_1 A_1, the curve for hens, a marked difference appears between them in the hump which the former all show in their early stages, produced by the sharp increase in the ordinates during the first few days of incubation.

C shows it less strikingly than B, but this is largely because the weighings for C began 12 hours later than for B, with the result that much of the steep part was lost, the possible significance of the curve close to its origin not having then been realised.

The existence of the "hump" was mentioned in the Hague paper already referred to, but the curves then shown have here been amended in two important particulars.

In one of the six incubators on which Curves B were originally based only one of the weighed eggs failed to hatch, and, as equal weight was given to the results for each incubator in the plotted means, this one egg exercised far too much influence upon them. The weighings for the incubator it came from have consequently now been discarded. The other particular is the inclusion in the data for Curves C of weighings for about eight times as many eggs as were available when the Hague paper was written.

These two changes, besides adding to the reliability of the B and C Curves, have brought both pairs into closer agreement with one another in all but their positions on the loss of weight scale. Both are well above the Curve A_1 A_1 for hens, and in both the curve for dead eggs is higher in the "hump" region than the curve for those that hatched by approximately the same amount.

In the later stages the live egg curves rise above those for dead eggs; but this is due partly to the growth of internal heat in the live eggs during the third week and partly to the CO_2 they produce, the amount of this gas being then for the first time important, as LAMSON and EDMOND show.

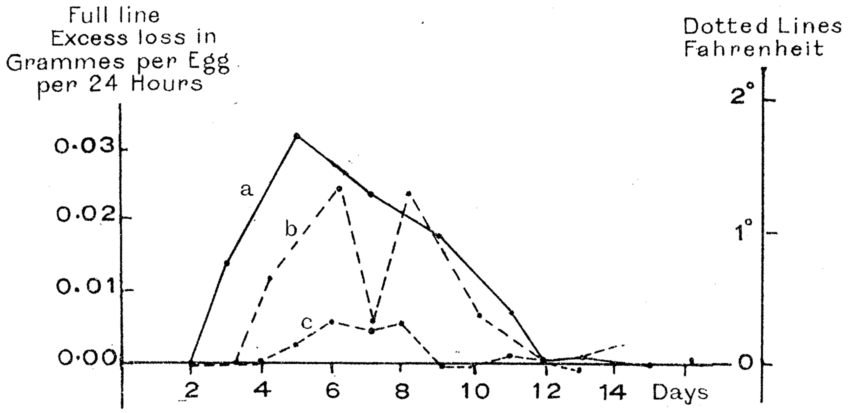
It thus appears probable that a connection exists between hatching and the height of the loss of weight curve in the early days of incubation, departure from A_1 A_1 corresponding with failure to hatch. And this may perhaps find further illustration in the fact that the percentages hatched in B and C are smaller as the distance from A_1 A_1 increases, though this comparison, as already explained, is less dependable than one between two curves belonging to the same hatch.

The effect is almost certainly one of enhanced evaporation due to rise of temperature in the eggs, as is shown by the following account of the evidence there is for this view.

External conditions equal, the evaporation from an egg is determined by the average temperature of its shell. To show that an increase in the loss of weight from an egg is due to increased evaporation, it is therefore necessary to show that the loss varies with this average temperature when the external conditions are constant.

In Curves IV. a line, *xx*, was drawn to represent what would probably be the trend of B_1 if no "hump" were present. Taking *xx* as zero, the distances above it were measured of a series of points upon B, and these distances were then plotted with time in Curve V.*a*, the result being a drawing of the "hump" itself.

CURVES V.



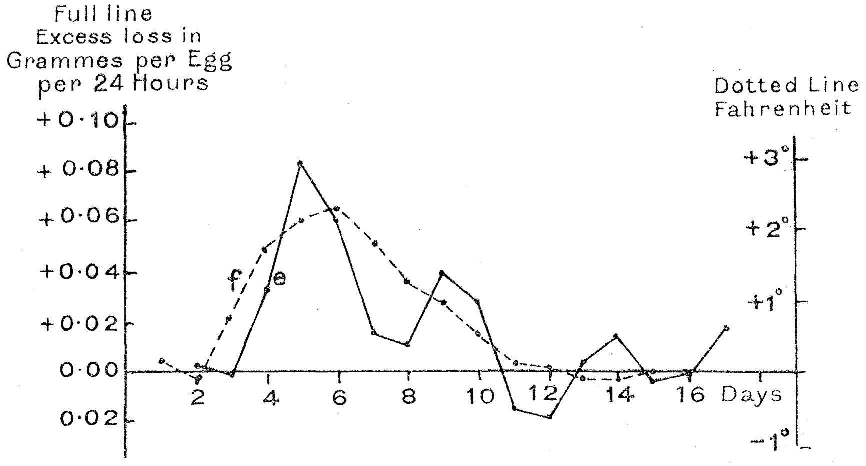
Average total loss of weight per egg per 24 hours . . . . 0·41 grm.
Mean ordinates . . . . *a* 0·018
b 0·65
c 0·20

In Curve V.*b* the same process has been applied to the means of the temperatures of the top and bottom surfaces of the eggs for the only hatch in which the necessary observations happen to have been made. It shows a marked "hump" at the proper place. So, too, does V.*c*, in which are plotted the figures in the last column of Table XV. after similar treatment.

It only remained therefore to see whether b and c were such as to account for a quantitatively on the theory of enhanced evaporation.

For this purpose the two 70-egg incubators were set with "corresponding" eggs; and, after starting with the same temperature in each, the temperature of one of the egg trays was lowered throughout the region where the "hump" had been found by an average of 1°·31 F. on the hanging thermometer; a number which has to be multiplied by 0·81 to obtain the corresponding temperature change of the eggs themselves. For symmetry the egg trays were interchanged every two days, the temperatures of the hanging thermometers being correspondingly altered so that one set of eggs was always at the lower temperature and the other at the higher.

CURVES VI.



Average total loss of weight per egg per 24 hours . . . . 0·45 grm.
Mean ordinates between 2 and 11 days . . . . e 0·030
f 1°·31 F.

In Curves VI. the results of this experiment are shown, the dotted line f giving the differences of temperature between the hanging thermometers each day, and the full line e the difference between the average losses of weight of 10 "corresponding" eggs in each tray, in grammes per egg per 24 hours.

From the data given below Curves V. and VI. the average temperature change which will account for a can now be calculated. The mean percentage change in loss of weight in a is $0{\cdot}018 \times 100/0{\cdot}41 = 4{\cdot}4$ per cent., and in e $0{\cdot}030 \times 100/0{\cdot}45 = 6{\cdot}7$ per cent. Hence, the temperature change required is

$$1{\cdot}31 \times 0{\cdot}81 \times 4{\cdot}4/6{\cdot}7 = 0^\circ{\cdot}70 \text{ F.}$$

The agreement between this and the 0°·65 for Curve b is very close—closer, in fact, than there was reason to expect, because the two values are for different incubators.

Curve c is also for different incubators to a; and there is besides a correction necessary, as c is for the top temperature only of the eggs, and it is the average temperature over the whole shell that is required.

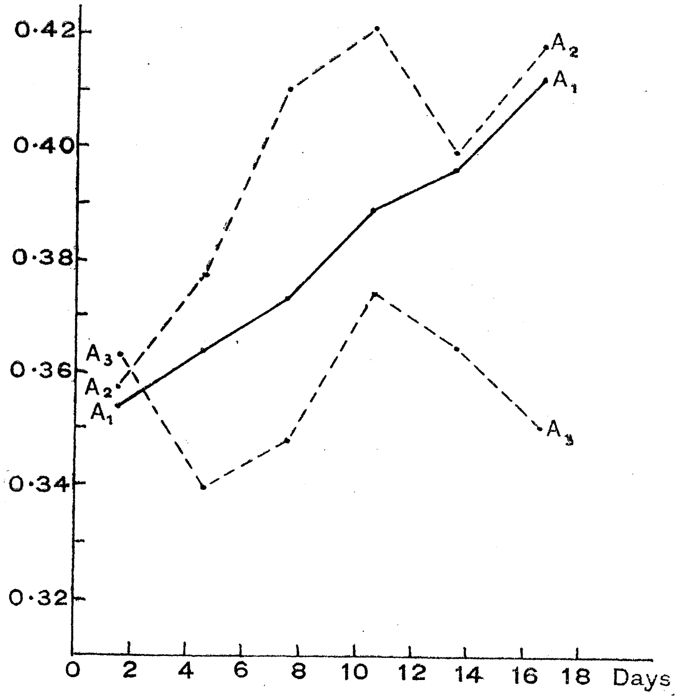
The temperature at the top of an egg varies far less than that of its lower surface when heat is generated in the egg, because the top is so much nearer the hanging thermometer where the temperature is constant. The change of temperature ratio for the two surfaces depends largely on the position of the thermometer, and may reach 2 to 1 for bottom to top, or even more. The ratio 2 to 1 would raise the mean ordinate of c to 0°·3 F.

To settle all doubts, the obvious course would have been to make further temperature measurements on the incubators from which Curve a was obtained, but they were sold long before the need was apparent. All that can now be said is that the order of magnitude of the observed temperature changes in b and c is sufficient to account for the loss of weight in a, and that this renders it practically certain that a is due to increased evaporation following a rise of egg temperature.

If this is so, the heat that produces this rise must have come from the eggs themselves. The incubators cannot have been the source, as there was no systematic change in the conditions of the five incubators concerned that would account for it.

Other points connected with the "hump" will be more profitably discussed when the dependable data of next season have been obtained.

CURVES VII.



A_1 . . . 108 eggs that hatched.
A_2 . . . 9 eggs, dead in the shell. (Raised by 0·06.)
A_3 . . . 41 eggs which were either unfertile or died within a day or so of being set.

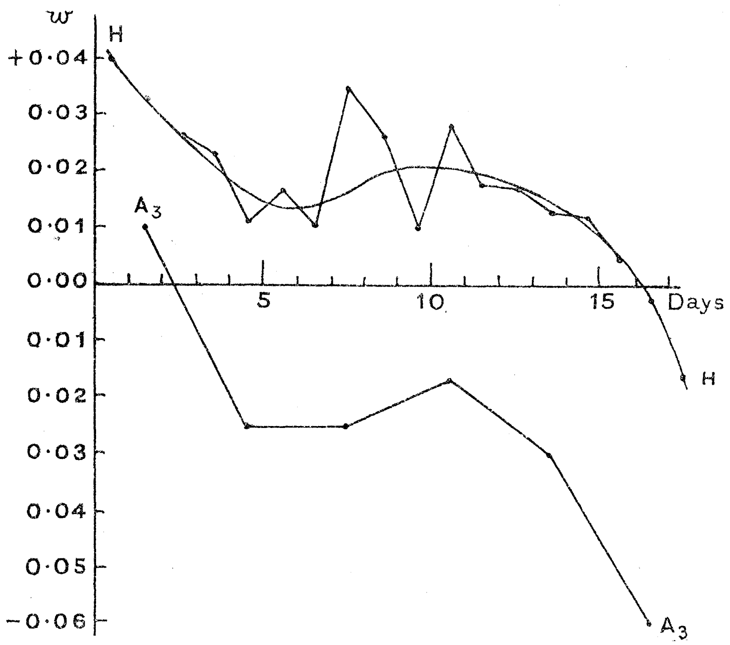
Dead Egg Curves for Eggs under Hens.—In Curves VII. are plotted all the data relating to eggs that died under hens. A_1 has the same meaning as in Curves IV.; A_2 is for

9 of the 10 eggs marked "dead in shell" in Table II.; and A_3 is for 41 eggs which were either infertile or died at the beginning of incubation under hens. A_2 has been raised in the diagram by 0·06 for easier comparison with A_1.

It will be seen that A_2 behaves much as B_2 and C_2 in Curves IV., so far as the rapid rise in the first few days is concerned. The peak shown about the tenth day, and which appears in A_3 and slightly in A_1 itself, is not found in Curves IV. That it corresponds to something happening in the eggs seems to follow from the fact that the sitting boxes from which the eggs for A_1, A_2, A_3 were taken were, in the main, different; and, in the case of A_3, that the peak was reproduced in an incubator.

This incubator, a 60-egg Hearson, was set with 50 eggs, which were weighed separately each day. Among the eggs were several infertiles, and these, with others that died at the start, furnished 15 eggs of the A_3 type. Thirty-one eggs hatched out.

Curves VIII.



w is the difference, in grammes, between the loss of weight per egg per 24 hours of the eggs that did not hatch and of those that did.

A_3 A_3 is for hen hatching (see Curves VII).

HH is for a Hearson incubator.

In both curves the eggs which did not hatch were either infertile or died within a day or two of being set

When plotted the loss of weight curves for live and dead eggs proved very irregular, probably because the capsule had been somewhat out of adjustment. At any rate, both curves were equally affected, and it was thus possible to eliminate the irregularities by subtracting the rates of loss of the live from those of the dead eggs. These differences are plotted with time in Curves VIII., H.

In Curve VIII., A_3 the live and dead egg curves (A_1 and A_3) of Curves VII. have been similarly treated, and the result leaves no doubt as to the correspondence between the shapes of the two Curves VIII.

Seven of the nine eggs on which A_2 is based were from four sittings which had been ruled out of the determination of A_1 (Table II. square brackets). It is interesting to find that the average curve for live eggs in these sittings (not given) does not correspond with A_1, but rises at about the same rapid rate as A_2 in its early stages; for this suggests that, though not more than two eggs in any sitting contained chicks which had died in the shell, the rest of the eggs from these four sittings had felt the same adverse influence, whatever it was, though they were strong enough to survive it.

Lastly, Curves VIII. afford a useful check on the estimation of the part played by CO_2 in the loss of weight of an egg during incubation.

Their ordinates—the differences between the rates of loss of weight for eggs which were respectively living and dead throughout the incubation period—represent to within a constant, the rates of both the loss of CO_2 by the living eggs and their increased loss of H_2O as their temperature rises with incubation. (See foot-note on p. 430.)

If a horizontal line be drawn through the highest point on either curve, the area it encloses with the curve between ordinates 20 days apart is the value of this loss of weight per egg during incubation; the constant having been eliminated in the process.

The curves unfortunately end at the 16th and 17th days respectively, so that the effect of the large losses of weight just before hatching is missed, and the mean ordinate is too small. The best that can be done is to replace the curve by a straight line joining its extreme points and prolong this line to 20 days span. Half the resulting drop multiplied by 20 is then a good first approximation to the area required.

Its value for the H curve is 0·7 and A_3 0·9, giving a mean of 0·8 grm. as that part of the total loss of weight of one egg during incubation which is due to its being alive. This is very near the 0·87 grm. of carbon lost per egg in 20 days in the "tin" nest experiment (p. 433).

It is true that the loss of 0·8 grm. is not all due to escape of carbon; part of it being caused by enhanced evaporation from the living eggs due to the growth of their internal heat towards the end of incubation.

A rise of 1° F. in the average temperature of the living eggs above that of the dead eggs would account for a loss of about 0·07 grm. per egg if it occurred in the last week of incubation; and a rise of about 2° F. has been found to occur under hens; but, apart from the fact that the whole calculation is approximate only, the rise here dealt with is that of live eggs above dead eggs *lying with them in the same nest*—a condition which tends to reduce temperature differences between them. When this is taken into account the agreement of Curves VIII. with the "tin" nest data seems as good as could have been hoped for.

ON FURTHER WORK.

It will have been understood from the foregoing that there is still much to be done before the discrepancy between hens and incubators can be completely explained and removed. The following is intended to define the present position of the problem:—

1. There appears to be no chance of any considerable improvement being effected by alteration of either the ventilation or the humidity of incubators.

2. Increase in the number of times the eggs are turned during the twenty-four hours from the usual twice to four or five times will raise the percentage hatched by something of the order of 6 to 10 per cent. (Table VII.)

3. It is shown that a possible line of further progress may be found in the study of the fluctuations in hatching which accompany cooling of the eggs during incubation. (Discussion of Table XI.)

4. There appears to be a definite connection between the rate of loss of weight of incubating eggs and their hatching. Loss of weight being partly a question of temperature, a detailed study of the temperature changes in the eggs themselves in connection with their changes in weight is a second line likely to lead to improvement in hatching. Experiments on this point are now in progress. (Part III.)

5. Mr. UDNY YULE suggests that if the presence of bacteria accounts for the considerable effects recorded in Table XIV., it may perhaps explain much of the discrepancy between natural and artificial hatching. For this the bacteria must be introduced by the eggs themselves, because, except in the case of its accidental omission in Test 1 of Table XIV., the incubators were disinfected before every hatch. It, therefore, follows that if the hen is to hatch better than the incubator she must either possess some property of killing detrimental bacteria which the incubator allows to live, or of keeping helpful bacteria alive which the incubator allows to die. The point is an interesting one for a bacteriologist.

APPENDIX.

DIFFUSION IN HEN'S NEST.

As a first approximation a hen's nest may be likened to a pair of circular horizontal surfaces, one above the other, at every point between which water-vapour is being generated at a constant rate, to escape radially by diffusion. For simplicity the eggs may be supposed absent and their effect on the diffusion considered later.

Let

W_D be the grammes of water-vapour which diffuse per second out of the whole nest.

w the grammes generated per second above each square centimetre of the nest floor.

D the coefficient of diffusion of water-vapour into air (about 0·27 at the temperature of the nest).

δ, δ_1 and δ_2 the vapour densities at any point in the nest, at its centre, and at its circumference, respectively.

r the radius of the nest at any point.

Then :

$$-\frac{d\delta}{dr}\mathrm{D}2\pi rh = w\pi r^2.$$

Integrating from centre to circumference this gives

$$\mathrm{W_D} = 4\pi \mathrm{D}h\,(\delta_1 - \delta_2). \qquad (1)$$

Here h is the vertical distance between the two circular surfaces just referred to, and thus stands for the depth of an empty nest which is equivalent to a (deeper) nest full of eggs in its capacity of allowing water-vapour to diffuse out radially.

This the vapour in an actual nest of eggs will do in two ways : (*a*) by passing through the air in the spaces between the eggs, and (*b*) by passing through the eggs themselves, condensing on the surfaces that face towards the centre of the nest and evaporating from the surfaces facing outwards. h may thus be thought of as the sum of two parts: h_1 corresponding to a and h_2 to b.

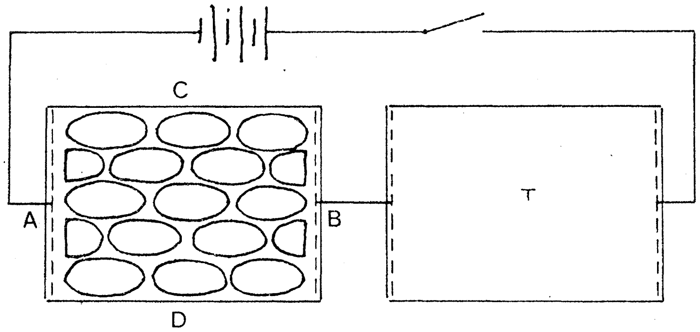


FIG. 7.

h_1 was measured by the method shown in fig. 7, where AB and T are two equal non-conducting troughs, in plan ; AB filled with eggs which had been dipped in hot paraffin wax, and half eggs which were full of wax. The eggs were loosely packed as in a nest and their trough filled with salt and water to a depth of about $4\frac{1}{4}$ cm. so as just to cover them.

T was also partly filled with the same salt solution.

The dotted lines are electrodes and the two troughs were connected in series with a battery as shown.

The electrical resistances of the two troughs were now made equal by altering the amount of solution in T, the depth of this solution being then taken as the value of h_1.

When the current passed from A to B h_1 was 1·34 cm., while for C to D it was 1·14 cm. The mean of these two numbers, 1·24 cm., was taken as probably corresponding most nearly to the irregular arrangement of eggs under the hen.

h_2 was calculated from the fact that when the difference between the vapour pressures inside and outside an egg is 1 mm. of mercury $2 \cdot 4 \times 10^{-4}$ grms. of water passes on the average through each square centimetre of shell surface per 24 hours; but the value obtained for h_2 proved to be so small (0·08 cm.) that it does not seem worth while to give the somewhat elaborate argument by which it was obtained.

It thus appears that

$$h = h_1 + h_2 = 1 \cdot 24 + 0 \cdot 08 = 1 \cdot 32 \text{ cm.}$$

in a nest for which, when filled with eggs, the distance between the two surfaces is $4\frac{1}{4}$ cm.

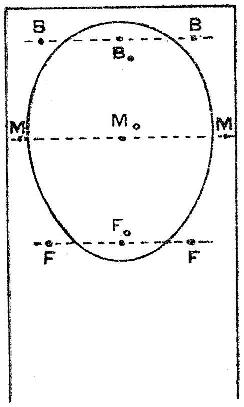


Fig. 8.

Distribution of Humidity in Nest.—To obtain the only other unknown quantity, $\delta_1 - \delta_2$, in equation (1) it was necessary to compare the mean humidity round the edge of the nest with that at the centre.

In fig. 8 the oval line is the boundary of the 13 eggs in one of the "tin" nests. BB, MM, FF are lines, to any point on which the opening, Y, of the cotton wool hygrometer (fig. 3) could be brought. For mechanical reasons they had to be straight, but by arranging that B_0 and F_0 lay inside, and BB, FF outside the oval, the humidities measured at these points gave what was equivalent to eight evenly distributed values along the oval.

B, M, F stand for back, middle and front of the nest, the hen's head being at F. At F, therefore, the moisture was far freer to escape than anywhere else, as the results show. These are given in Table XXIII., where $\overline{B}$, $\overline{M}$, $\overline{F}$ stand for the means of each pair of humidities on opposite sides of the nest, and are consequently given double weight in calculating the numbers under H_2 — Room.

H_1 stands for the mean humidity at the centre of the nest, and H_2 that along the oval.

Altogether four sets of measurements were made—two for normal and two for waxed eggs; and considering that each set took more than a day to obtain because of the

TABLE XXIII.—Distribution of Humidity* in Hen's Nest.

State of eggs.	$M_0 =$ $H_1 -$ Room.	B_0.	$\bar{B}$.	$\bar{M}$.	$\bar{F}$.	F_0.	Mean $H_2 -$ Room.	"Mean" / M_0.
Normal . . .	11·6	10·4	8·9	9·8	0·7	3·6	6·6	0·57
Normal . . .	12·5	9·5	8·0	7·4	3·0	5·9	6·5	0·52
Waxed	4·6	5·5	4·2	4·6	0·3	1·3	3·1	0·67
Waxed	4·2	2·8	1·5	2·7	1·6	2·6	2·1	0·50
Means :	8·2	7·0	5·6	6·1	1·4	3·3	4·6	0·56

time required for the chamber Y of the hygrometer to fill with moisture up to the pressure of that in the nest after a measurement, the agreement between them is fairly good. They show that when the moisture passing out of the nest is reduced in the ratio of nearly 3 to 1 (as shown by M_0), by replacing normal with waxed eggs, the ratio $(H_2 - \text{Room})/(H_1 - \text{Room})$ remains roughly constant at 0·56. The room humidity was 9·6 mm., and we may therefore write

$$H_2 - 9{\cdot}6 = (H_1 - 9{\cdot}6)\ 0{\cdot}56.$$

Applying this result to the data of Table XVIII., the 7·7 mm. represents a *rise* of humidity at the centre of the nest, and the corresponding rise at the boundary will therefore be 7·7 × 0·56 without any question of the room humidity. We may therefore write for this case

$$H_1 - H_2 = 7{\cdot}7\ (1 - 0{\cdot}56)\ \ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ \ (2)$$

Mathematical Nest.—In order to test the foregoing diffusion theory what may be called a mathematical nest was constructed. Two horizontal circular tin plates of 20 cm. diameter were supported one above the other by wires at their edges. Their distance apart was $4\frac{1}{4}$ cm., and in the space between them were arranged 14 eggs which were sufficient just to fill it. The edges of the plates were then connected by a strip of tissue paper cemented to them, which formed with the plates a drum-shaped box.

Two diffusion hygrometers were placed, one just outside the tissue paper sides of the drum and the other in connection with the centre of the egg space. The humidity was thus measured at the centre and circumference of the nest respectively. The function of the tissue paper was to prevent convection between the nest and the air outside it. So far as diffusion was concerned the paper was equivalent to a thickness of air of less than 1 mm., and the humidity was thus practically the same on both sides of it.

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* The mean room humidity (9·6 mm.) has been subtracted from the actual humidities measured before entering in the table.

The last column gives the ratios of the numbers in the last column but one to M_0.

The whole nest, with its hygrometers, was placed inside a Gloucester incubator, from which the egg tray had been removed, and left there for several days with the lamp turned low; after which a second test was made with the lamp higher. The results are given in Table XXIV. The observed loss of weight is larger than its calculated value in both tests, and by the same amount (47 per cent.). This shows that the discrepancy is not mainly due to convection currents in the air of the nest, for they would have accentuated it at the higher temperature. The escape of the moisture must thus have been mainly by diffusion, and the cause of the higher value of A is probably the fact that the eggs in the circular nest could not be packed so closely as those in the rectangular trough of fig. 7. A small increase in the width of the narrow air passages between the eggs would mean a considerable increase in the diffusion through them.

TABLE XXIV.

Evaporation from 14 eggs in "mathematical" nest, in grammes per 24 hours.

	From loss of weight of eggs. A.	By diffusion formula. B.	A/B.
Lamp low	2·04	1·38	1·48
Lamp higher	2·61	1·78	1·47

B was, of course, calculated with 1·32 cm. as the value of h. The numbers in Column A imply a value of about 1·94 cm., and we shall probably be nearest to the average arrangement of the eggs under a hen if we take 1·6 cm. for the final figure. The exact value is not of primary importance, for as already explained, the estimation of the amount of diffusion in the nest is more a question of order of magnitude than of numerical accuracy.

This completes the data required for solving equation (1), which may now be written:

$$\begin{aligned} W_D &= 4\pi\, 0{\cdot}27\, h\, (\delta_1 - \delta_2) \\ &= 4\pi\, 0{\cdot}27 \,.\, 1{\cdot}6 \,.\, 7{\cdot}7\, (1 - 0{\cdot}56)\; 1{\cdot}06 \,.\, 10^{-6}\; 60\; 60\; 24 \\ &= 1{\cdot}7 \text{ grammes } H_2O \text{ per 24 hours from 13 eggs.} \end{aligned}$$

1·7/4·83 or 0·35 is the heading of the third column in Table XIX., and is, of course, for a nest bounded top and bottom by the continuous tin surfaces. The hen's nest differs from this mainly in the fact that while its lower surface is also continuous the upper one is not, except in the central region. Wings and legs are no doubt utilised as far as possible in covering the eggs, but it is chiefly by their feathers that they do so; and the effect of this must be to increase enormously the facilities for both diffusion and convection in the outer portions of the nest.

It is quite certain, therefore, that we may rule out the column headed 0·35 (and *a fortiori* that headed 0·00) as the amount of diffusion on which it was calculated was much too small. It is also probable that an appreciable amount of convection in the nest air will always be present. This rules out the last column.

In columns 0·52 and 0·70 the hypothetical diffusion has been increased by 50 per cent. and 100 per cent. respectively. The region in which the correct value of N is to be found has thus been narrowed to that between 1·83 and 1·72. Within this the choice of N amounts to little more than guessing; but having regard to the geometry of the sitting hen the writer inclines to 1·8 as the most probable number of litres of air required per egg per hour to ventilate a hen's nest by convection alone as it is actually ventilated in practice, by convection and diffusion together.

SUMMARY.

The work here described is an attempt to find some of the causes of the considerable discrepancy at present obtaining between the results of natural and artificial incubation, with the object of improving the latter.

Taking the best hatching under hens as a standard, the plan has been to measure the physical conditions most likely to affect incubation, both in the hen's nest and in the incubators, and to compare them.

The effects on the hatching of turning and cooling the eggs, of moisture, ventilation, and loss of weight of the eggs during incubation have been studied and their causes determined as far as possible.

Daily cooling appears to be unnecessary, but the results point to an improvement in the hatching of from 6 to 10 per cent. if the usual twice-a-day turning is increased to four or five times, at equal intervals.

Very little improvement in hatching has resulted from increasing the humidity in hot air incubators to the value found in hens' nests.

The ventilation in the hen's nest has been measured and found to be equivalent to the passage through the nest of 3·2 cubic feet of air per hour per 50 eggs, a number which is several times smaller than the ventilating air flow in typical hot air and tank incubators.

Recent experiments in America on ventilation and humidity have determined a range of variation for each of these conditions within which the hatching remains at its maximum. It is now shown that in each case the hen's nest condition falls well within this range.

Lastly, the significance is discussed of the changes that occur in the weight of an egg during incubation, when separately determined for the eggs that hatched and for those that did not.

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