INSTITUTE OF MARINE ENGINEERS
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SESSION 1899-1900.

President: SIR JAMES LYLE MACKAY, K.C.I.E.

Volume XI.

EIGHTY-FOURTH PAPER

(OF TRANSACTIONS)

Experiments on the Heat-Absorption Power of Water.

BY

Mr. GEO. HALLIDAY

(MEMBER).

READ AT

THE INSTITUTE PREMISES, 58, ROMFORD ROAD, STRATFORD.

ON MONDAY, MAY 8th, 1899.

PREFACE.

58, Romford Road,
Stratford,
12th June, 1899.

A Meeting of the Institute of Marine Engineers was held here on Monday, 29th May, presided over by Mr. A. Boyle (Chairman of Council), when a Paper, read by Mr. G. Halliday (Member), on "Experiments on the Heat-Absorption Power of Water," at a former meeting, held on Monday, 8th May, was discussed.

The experiments referred to were conducted by Mr. Halliday, Convener of the Experimental Committee, by means of the apparatus recently presented to the Institute by the Editor of *The Engineer* and Mr. Joseph Hallett (Member).

Further discussion on the subject was postponed, and the Paper is now printed and issued, to give an opportunity to Members of contributing to the discussion by correspondence, before the session re-opens on the second Monday of September; the illustrations are from electros, kindly lent by the Editor of *The Engineer*.

JAS. ADAMSON,
Hon. Secretary.

INSTITUTE OF MARINE ENGINEERS

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President: SIR JAMES LYLE MACKAY, K.C.I.E.

EXPERIMENTS ON THE HEAT-ABSORPTION POWER OF WATER.

BY

MR. GEO. HALLIDAY (Member).

READ AT

THE INSTITUTE PREMISES, 58, ROMFORD ROAD, STRATFORD,

ON MONDAY, MAY 8th, 1899.

CHAIRMAN:

MR. A. W. LAWRIE (Member).

The apparatus—Fig. 1—consists of a glass vessel H with a spiral tube sealed into it. Water flows from F through the spiral tube and out by L into the cubic centimeter measure G. The quantity of water flowing through the tube can thus be determined. Steam is generated in the flask D and flows through the copper spiral C, where it becomes superheated, and flowing into H heats the water as it flows through the coil. The temperature of the water as it flows into the glass vessel

H is measured by a thermometer at B, and its temperature as it emerges from the coil is measured by a thermometer at A. The water then flows through L into G, and the number of thermal units given to the water as it flows through the coil J can be determined.

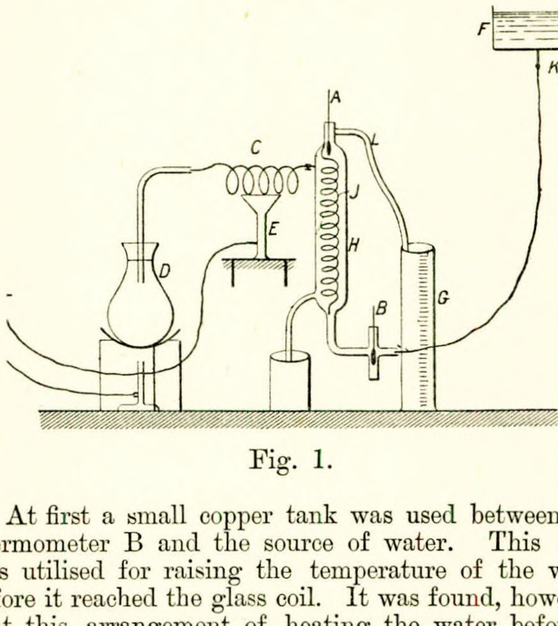

Fig. 1.

At first a small copper tank was used between the thermometer B and the source of water. This tank was utilised for raising the temperature of the water before it reached the glass coil. It was found, however, that this arrangement of heating the water before it reached the glass coil did not give constant temperature, as the following results show.

The experiments were made to determine what influence the rate of motion through the tubes had on the water. It will be observed that the quantity of water flowing through the glass spiral J was 5 cubic centimeters per minute. The difference of temperature in the first case was 45·16 deg. Fah., and in the second case 43·06 deg. Fah. The times and quantities of water flowing through the glass coil J were taken at the end of each minute.

A.

Experiment when Water flowed through the Coil at an Average Rate of 5 *Cubic Centimeters per minute.*

Time.	Quantity of water in c. c. measure.	Temperature of water when it emerged.	Temperature of water on entrance.	Difference of temperature.
4.39	705	154	109	45
4.40	710	158	106	52
4.41	720	163	107	56
4.43	725	155	107	48
4.44	730	161	106	55
4.45	735	147	106	41
4.46	738	136	106	30
4.47	740	139	105	34
4.48	749	148	104	44
4.49	752	151	104	47
4.50	760	151	102	48

Mean quantity of water flowing per minute, 5 cubic centimeters; average difference of temperature, 45·45.

B.

Time.	Quantity water in c.c. measure.	Temperature water at outlet.	Temperature of water at entrance.	Difference.
5.7	40	180	135	56
5.8	68	182	134	48
5.9	91	182	134	48
5.10	118	182	134	48
5.11	140	182	134·5	48
5.12	172	183	135	48
5.13	192	184	137	47
5.14	218	178	139	39
5.15	239	173	140	33
5.16	256	172	140	32
5.17	270	174	140	34
5.18	282	175	136	39
5.19	300	175	134	41
5.20	312	175	132	43
5.21	330	175	131	44

Mean quantity of water flowing through in cubic centimetres, 20·7; average difference of temperature, 43·2.

	Quantity of water per minute.	Thermal units per minute.
In Experiment A. ..	5 ..	227·25
In Experiment B. ..	20·7 ..	894·2

The source of heat for the generation of steam and heating the coils remained the same throughout. The mean difference of temperature was about the same in each case. The increase of the number of thermal units which were taken up by the water when moving at the higher velocity is very marked.

In the next set of experiments the water was led directly from the tank F to the thermometer B. When it was led through the small heating boiler the flow was somewhat intermittent, due to air getting into the boiler. There was also some trouble with the arrangement of the thermometer for taking the temperature of the water as it issued from the spiral. In the new arrangement the thermometer was directly inserted in the end of the glass spiral, and the fluctuation of the thermometer was then the fluctuation of the temperature of the water as it came out of the spiral. One set of readings will show the steady nature of the temperature which came from the tank under the new conditions.

C.

Time.	Quantity of water in c.c. flowing per min.	Temperature of water at A.	Temperature of water at B.	Difference.
3.0	.. 610	.. 140	.. 52	.. 88
3.1	.. 620	.. 131	.. 52	.. 79
3.2	.. 640	.. 136	.. 52	.. 84
3.3	.. 665	.. 137	.. 52	.. 85
3.4	.. 680	.. 141	.. 52	.. 89
3.5	.. 700	.. 143	.. 52	.. 91
3.6	.. 720	.. 143	.. 52	.. 91
3.7	.. 740	.. 143	.. 52	.. 91
3.8	.. 760	.. 141	.. 52	.. 89
3.9	.. 780	.. 135	.. 52	.. 83
3.10	.. 800	.. 142	.. 52	.. 90

Average c.c.'s flowing through per minute, 19 ; average difference of temperature, 87·2 deg. Fahr.

Thermal units per minute given to the water = 1656·8.

The result of this set of experiments, arranged in order of the velocity of water flowing through the tube, is as follows :—

D

Average quantity of water per minute, c.c.	Average differcnce of temperature.	Thermal units per minute.
9·2 ..	.. 150·6 ..	.. 1388
19 ..	.. 86·2 ..	.. 1687
32·2 ..	.. 147·9 ..	.. 4759
47·7 ..	.. 39·5 ..	.. 1884
49·4 ..	.. 86 ..	.. 4278
84·5 ..	.. 52·6 ..	.. 4445

In this set of experiments there was considerable variation of the heat applied to the steam superheater and generator, and the result shows considerable fluctuation.

In the next set of experiments the heat of the gas generating steam and superheating was kept constant. The result of the sets of readings arranged in order of the speed of flow through the spiral are as follows :—

E

Average quantity of water per min. in c.c.	Difference of temperature.	Thermal units per minute.
7·2 ..	.. 152·1 ..	.. 1095
10·55 ..	. 151·5 ..	.. 1666·5
15·5 ..	.. 118 ..	.. 1829
20 ..	.. 88 ..	.. 1760
30 ..	.. 61·7 ..	.. 1851
47·4 ..	.. 37·7 ..	.. 1786.9
61 ..	.. 32 ..	.. 1952
61·3 ..	.. 28·5 ..	.. 1746
75.5 ..	.. 30·8 ..	.. 2325
89·1 ..	.. 26·6 ..	.. 2370
91·6 ..	.. 24·9 ..	.. 2280
150 ..	.. 16·4 ..	.. 2460

Two trials were made with the gas at the generator and superheater the same throughout the experiments.

F

Average quantity of water per min. in c.c.'s	Average difference of temperature.	Thermal units taken up by water per min.
23·5 ..	.. 74·4 ..	.. 1748.4
120·8 ..	.. 20·3 ..	.. 2452·2
144·1 ..	.. 17 ..	.. 2449·7

The steam was afterwards generated very rapidly and superheated, and the following results were obtained :—

G

Average quantity of water per min.	Average difference of temperatnre.	Average thermal units per min.
24 ..	.. 152 ..	.. 3648
81·2 ..	.. 60·6 ..	.. 4920
118 ..	.. 43·2 ..	.. 5123·5
160 ..	.. 32 ..	.. 5120
210 ..	.. 23·8 ..	.. 4998

It will be seen from the two last deduced results that there is a maxium co-efficient of transmission of heat. This is also shown in Fig. 2, where the abscissæ represent the velocities of the water and the ordinates represent the thermal units absorbed by the water flowing through per minute. In Table F the middle one is the higher number, and the third from the top is the highest in Table G. It is also to be noted that the number of thermal units in Table G are much higher throughout than in Table F. This is in consequence of greater heat being used for generating steam and for superheating. This would appear to show that with this apparatus and with a certain heating arrangement, there is a speed at which the maximum quantity of heat is taken in by the water. At a lower speed than this, there is less heat taken in, and at a higher speed there is less heat taken in per minute per unit of heating surface.

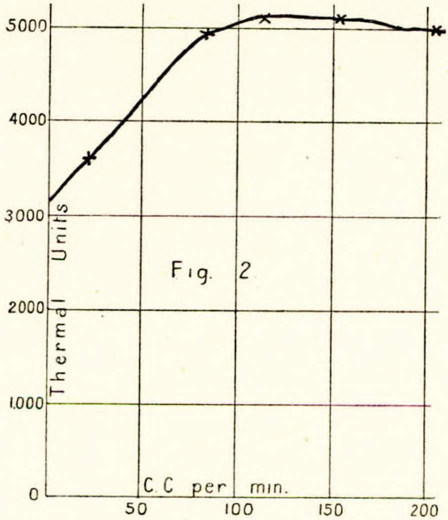

Fig. 2.

The next set of experiments was made to determine whether water more readily absorbed heat at the point of ebullition than it did at lower temperatures. In a statement made by Prof. Ser he says, " When the water has come to the boiling point the movements are more than ordinarily tumultuous, and the transmission - of heat—is notably increased." It was sought to determine by the following experiments whether this notable increase of heat given to the water was due to its more rapid motion over the heating surfaces in consequence of the more rapid circulation, or was due to a change in the nature of the water which made it more readily take in more heat.

To do this the water was made to travel through a straight tube instead of a spiral, in order that the bubbles of steam might rise quite freely. The tube was surrounded as in the last case. At first this surrounding barrel was only 12in. long, but with as strong heat as possible the water could not be got to rise to the temperature of 212 deg. A longer jacket was then fitted on and carefully covered with asbestos, and strong heat supplied both in the generation of steam and superheating. The water began to boil quite freely, and the thermometer stood at 212 deg. The record of this experiment is given below :—

H

Time.	C.c. of water per minute.	Temperature at outflow.	Temperature at inflow	Difference of temperature.
2.39	435	211	55	156
2.40	450	212	55	157
2.41	465	212	55	157
2.42	480	212	55	157
2.43	500	212	55	156
2.44	515	211	55	156
2.45	535	211	55	157
2.46	555	212	55	157
2.47	575	212	55	157

Average c.c. per min. 17·5 — Average difference of temperature 156·6

Thermal units per minute .. .. 2740·5

The water was afterwards made to flow a little faster, while everything else remained about the same. The temperature stood about 197 deg. Fah. at the outlet, so that the loss by radiation would be about the same in both cases. Steam was supplied at the same rate and superheated without alteration. The result of the experiment was as follows :—

I.

Time.		C.c. per minute.		Temperature of outflow.		Temperature at inlet.		Difference temperature.
2.51	..	35	..	195	..	53	..	142
2.52	..	65	..	197	..	53	..	144
2.53	..	95	..	200	..	53	..	147
2.54	..	125	..	195	..	53	..	142
2.55	..	155	..	196	..	53	..	143
2.56	..	190	..	197	..	53	..	144
2.57	..	220	..	197	..	53	..	144
2.58	..	245	..	197	..	53	..	144
2.59	..	275	..	197	..	53	..	144
3.00	..	305	..	197	..	53	..	144

Average c.c. per min. 30 — Average difference .. 143·8

The number of thermal units transmitted to the water per minute is 4314, as against 2740·5 when the water was at the boiling point.

The difference of the conditions in the two cases is the more rapid motion of the water through the tube, and it appears that the coefficient of transmission derives more advantage from quick motion over the heating surfaces than from the new properties in the water at the boiling point.

Keeping the heating arrangements the same, a series of experiments were made with this apparatus to determine the effect of the velocity of the water, with the following results :—

J.

C.c.'s per minute.		Average difference of temperature.		Thermal units transmitted to the water per min.
36·5	.. .. ..	129·4	.. .. ..	4684
77·5	.. .. ..	67·8	.. .. ..	5254·5
106·2	.. .. ..	51·4	.. .. ..	5458·6
135	.. .. ..	40·2	.. .. ..	5427
175	.. .. ..	26·2	.. .. ..	4585
205	.. .. ..	13·4	.. .. ..	2747

Here again there is a maximum transmission when the flow of the water is a little over 100 cubic centimetres per minute—see Fig. 3.

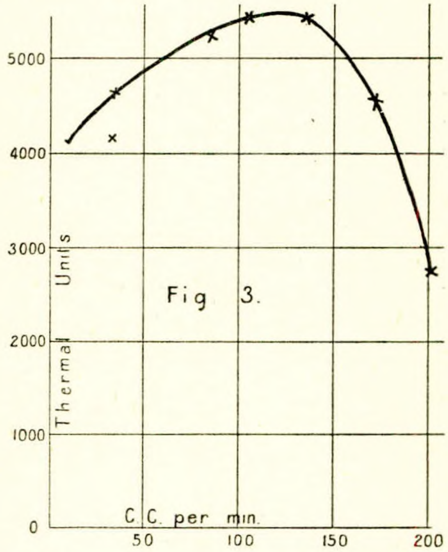

Fig. 3.

There was a fear that the bubbles of steam as they rose to the surface of the water in the tube would condense in the overflow tube, making the water in it at a higher temperature than when it was sent away under the boiling point. So the temperature of the water was measured as it flowed from the tube into the cubic centimetre measure.

It will also be noticed that the experiments made by Professor Ser, MM. Thomas and Laurens, and other experimenters, did not make provision for the separation of the circulation factor from the factor provided by the new properties of the water at the boiling point. In one case there was a pot with a steam jacket underneath it, and the water was boiled in this. In the other case the water was heated with steam passing through a spiral tube placed in the pot. When ebullition began rapid circulation took place, due to the entraining action of the steam bubbles.

In the above apparatus the circulation produced by the entraining action of the steam is done away with, and only the new properties gained by the water at the point of ebullition remain, to increase the heat-absorption power of the water.

It may also be pointed out that the condition of water when bubbles of steam are forming and mixing with the water does not appear to improve its heat-absorptive power. Here, anyway, are the records of the two sets of experiments. The steam generated and the superheating were the same throughout both.

K.

Times.	Water in c. c. per minute.	Temperature at outlet.	Temperature at inlet.
7.30	185	211	51
7.31	205	211	51
7.32	225	211	51
7.33	245	211	51
7.34	265	212	51
7.35	285	212	51
7.36	300	212	51
7.37	320	212	51
7.38	340	212	51

8)155 Average difference of

19·37 temperature 160·5

Average number of thermal units of heat absorbed by the water at boiling point, 3107·8. The temperature of the water as it left the tube to flow into the c.c. measure was 178 deg.

The water from the tank was increased in velocity until steam bubbles stopped rising. The temperature fell to 202 deg. The water at the inflow rose to 53 deg. The following was the record of this experiment :—

L.

Time.	Water in c.c. per minute.	Temperature at outlet.	Temperature at inlet.
7.42	440	202	53
7.43	465	202	53
7.44	490	202	53
7.45	320	202	53
7·46	545	202	53
7.47	570	202	53
7.48	600	202	53
7.49	625	201	53

Average c.c. per minute, 26·43 ; average difference of temperature, 149 deg. Fah.

Average number of thermal units absorbed per minute was 3933·6. The temperature of the water as it left the tube to flow into the c.c. measure was 180 deg.

The increase in the number of thermal units absorbed by the water at 10 deg. below boiling point was thus 819·9. The statement made by the French experimenters was that at the boiling point the water takes in three times as much heat as at other temperatures.

The velocity of the water was again changed in order to note the effect of the change of velocity on the heat-absorption power. The result is given in the table below :—

M.

C.c. per minute.	Difference of temperature,	Thermal units absorbed per minute.
19·4	160·5	3113·7
26·43	149	3933·6
46	111·3	5119·8
125	48	6000
158·6	40	6344
193·3	17	3286·1

The accompanying curve—Fig. 4—shows the manner in which the absorption power increases with the

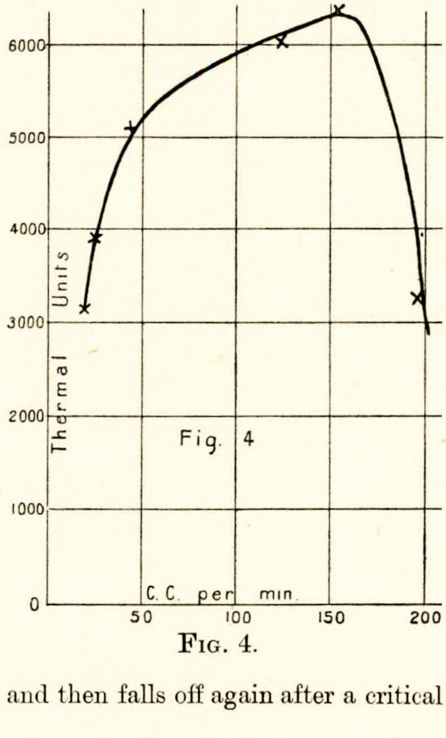

FIG. 4.

velocity, and then falls off again after a critical velocity.

Since the foregoing was written, further experiments have been made with a copper tube through which the water circulated. This tube was heated with a Fletcher burner, the flame consisting of a great number of Bunsen burner flames, which enveloped the tube. The result was that up to a certain velocity of the water through the tube the absorption of heat by it increased. Then a maximum was reached, and, after that, a falling off of absorption power. The curve representing this falling off of absorption power was in shape like an expansion curve. The details of these experiments will be seen in ***The Engineer***, July 7th.

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Volume XI.

DISCUSSION

ON THE

EIGHTY-FOURTH PAPER

(OF TRANSACTIONS)

Experiments on the Heat Absorbent Power of Water

BY

Mr. GEO. HALLIDAY

(MEMBER).

READ AT

THE INSTITUTE PREMISES, 58, ROMFORD ROAD, STRATFORD

ON MONDAY, MAY 8TH, 1899.

DISCUSSIONS

ON MONDAYS, MAY 29TH, SEPTEMBER 11TH.

PREFACE

58, Romford Road,

Stratford,

November 25th, 1899.

At a Meeting of the Institute of Marine Engineers held on Monday, May 8th, a Paper, descriptive of "Experiments on the Heat Absorbent Power of Water," by Mr. G. Halliday, was read and afterwards published to give Members an opportunity of contributing their views on the subject matter of the Paper at subsequent meetings, or by correspondence.

The resultant discussion is now published, but as further experiments have been made and will form the subject of another contribution from Mr. Halliday, the publication of the following does not close the discussion, Members are therefore invited to express in writing, their views for embodiment in the discussion which is pending.

JAS. ADAMSON,

Hon. Sec.

INSTITUTE OF MARINE ENGINEERS

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SESSION 1899-1900.

President: SIR JAMES LYLE MACKAY, K.C.I.E.

ADJOURNED DISCUSSION.

ON

THE HEAT ABSORBENT POWER OF WATER.

58, ROMFORD ROAD STRATFORD,

MONDAY, MAY 29TH, 1899.

CHAIRMAN:

MR. A. BOYLE (Chairman of Council).

The CHAIRMAN: We have before us for discussion to-night the paper which was read at our last meeting, but it was not then before us in print, a disadvantage which does not exist to-night. You will see, by looking at the paper, that these experiments were undertaken by Mr. Halliday to ascertain what influence the rate of motion through the tubes has on the heat absorbent power of the water, and presumably the advantages or disadvantages of the rapid movement of water over heating surfaces. The author gives several columns of figures as showing why he has come to the conclusion that, with this apparatus, at any rate, there is a speed or velocity at which the maximum quantity of heat is taken up by the water, his contention being that if the velocity of the water goes above or below that point less heat is absorbed. That appears to be the first point in the paper. But in addition the experiments have been conducted, and the results watched and noted, to see if they bear out the statement of Professor Ser who says: "When the water has come to the boiling point the movements are made more than ordinarily tumultuous, and the transmission of heat is notably increased." Mr. Halliday explains that it was sought to determine by these experiments: "Whether this notable increase of heat given to the water was due to its more rapid motion over the heating surfaces in consequence of the more rapid circulation, or was due to a change in the nature of the water which made it more readily take in more heat." The results of these investigations are also fully set out in the paper, and appear to controvert the statement of the French experimenters. I just offer these few preliminary remarks, quoted from the paper, to open the discussion.

Mr. HALLIDAY: The statement of the French experimenters was that water at boiling point developed new properties and absorbed heat due to those new properties much more quickly than at lower temperatures. The results of the experiments recorded in the paper go to show, firstly, that any new properties developed by the water at boiling point do not cause the water to take in more heat at that point; and, secondly, that as you increase the speed of the water through the tube you increase the amount of heat taken in by the water up to a certain point, and after that point is reached there is a rapid fall.

Mr. McFARLANE GRAY (Member), who illustrated his remarks by a frequent use of the blackboard, criticised in detail some of the results recorded in the paper, and complained that certain terms used by the author were apparently used in a sense altogether different from that in which they were generally understood. The expression, "difference of temperature," for instance, was usually understood to mean the difference between the temperature of the heating body and that which was being heated, but that was not the meaning intended by the author. He doubted, too, if Professor Ser had really expressed the view attributed to him by Mr. Halliday as to the new properties or new nature acquired by water at boiling point. The subject of the paper was one that could not be properly treated without also having regard to the important element of the difference between the temperature of the body parting with heat and that of the body receiving the heat.

The CHAIRMAN: I think that Mr. Halliday takes his applied heat as a constant.

Mr. McFARLANE GRAY continuing, said:—It is absurd to measure the "average number of thermal units of heat absorbed by the water at boiling point, 3113·7" by heating water from 51° to 211° or 212° under atmospheric pressure, an arrangement which measures just none of the heat absorbed by the water at boiling point. The author explains to his own satisfaction the fall in rate of absorption in the last two results given from 6344 to 3286, thus:—"The falling off is due to insufficient heat being supplied to meet the requirements of the more rapidly moving water" but how did the heating body come to know that the body to be heated was moving more rapidly, there being a glass wall between them.? To that glass wall 6344 heat units per minute were supplied when the cool water passed at the rate of 159 at one side of the glass, this velocity was increased to 193 when the supply of heat at the other side of the glass at once fell to 3286. A simpler explanation would be to regard this solitary exception on which the new law is based as the result of an oversight in reading the experiment. The column headings "water in c.c. per minute," "C.c. per minute," have the "per minute" for two entirely different meanings.

Mr. JOS. WILLIAMS (Member): I think the chief interest attaching to the author's experiments is contained in those results which have induced him to arrive at the conclusion that the co-efficient of heat transmission through glass to water attains a maximum value when the water passes over the heating surface at a certain velocity, and that the transmission co-efficient decreases in value with any increase in the velocity of circulation beyond a certain well defined limit. The evidence adduced in the paper for the support of such an important conclusion is not however, in my opinion, sufficient to assure its unquestioned acceptance. Referring to the tables accompanying the paper I may point out that in dealing with them I have excluded the results shown by the top lines in each one where the time is given, as the author has not given us the rate of flow during the first minute. We are told that "the source of heat for the generation of steam and heating the coils remained the same" during the experiments which are summarized in Tables A and B. Yet in Table A there is a difference of more than 86 per cent. in the thermal efficiency observed at 4·41 and at 4·46, and a difference of no less than 228 per cent. in the observations taken at 5·12 and 5·18 respectively. I would also observe that the difference in the inlet temperatures in the two sets of experiments is an undesirable element to introduce when the results have to be compared. The general inference deduced from Tables A, B, C, D, E and F calls for no special remark, as they generally support the current views which are held by engineers, and set forth in standard text books, to the effect that the efficiency of the cooling—or heating—surface in a condenser is dependent on the velocity of the circulating water. But if after directing attention to the fluctuation of nearly 169 per cent. in the results observed at 3·1 aud 3·3 in Table C, we proceed to dissociate and analyse the observations recorded in the tables, we can, I think, prove anything from them. Table E, for instance, shows that by decreasing the speed of circulation from 61·3 to 61—less than one-half per cent.—we gain nearly 12 per cent. in efficiency, while a comparison of the last lines in Tables E and F support the generally accepted view that increased speed of circulation increases the efficiency. Table G is interesting from the effect it appears to have produced on the author of the paper, who has endeavoured to graphically convey his impressions to us in Fig. 2. The very important conclusion regarding the maximum coefficient of thermal transmission appears to be based on two series of observations, which show that a difference of only $2\frac{1}{2}$ per cent. in thermal effect is produced by a variation of 78 per cent. in the rate of circulation, while Table M shows a difference of 93 per cent. in thermal efficiency by a variation of something less than 22 per cent. in the speed of circulation. In view of these suggestive inconsistencies which appear to be all associated with observations of temperatures taken below the ordinary boiling point of water, it may not be considered necessary to deal at any length with Table H, as one cannot attach much value to observations professing to show the rate of thermal transmission to water which has reached its boiling point, when the measurements have been taken, as in this case, where no provision was made for measuring the latent heat of any steam which may have formed, and which would not affect the thermometer in the boiling water, although quite a considerable amount of heat may have passed into it after it had reached the temperature of ebullition. Attention may also be directed to the author's erroneous deductions from the results of this table, as he gives a mean temperature which is less than the minimum observed. By eliminating the top line, for the reasons previously given, it may be seen that the mean difference of temperature is 156·75 degrees, and that the average thermal units are 2,743 per minute instead of 1,787 given in the paper, so that the difference between Table H and Table I is not nearly so great as the author has made it appear. The result shown by the curve illustrating Table J is negatived by Table G. On comparing the last lines in these tables it is made to appear that an increase of nearly $2\frac{1}{2}$ per cent. in the velocity of circulation increases the thermal efficiency nearly 82 per cent. The remarks I have made on the difficulty of making accurate observations at the boiling point, made with reference to Table H, apply also to Table K. Comparing the curves which illustrate Tables M and G a very marked difference will be observed in the direction of their terminals, although the velocity of circulation is nearly the same in each case. Again, by taking the results shown in the three last lines of Table G and comparing them with the results given in the corresponding lines of Table M we may deduce that an increase in the velocity of flow from 118 to 125 increases the thermal efficiency, that an additional increase of flow from 158·6 to 160 decreases it; and that a further increase from 193·3 to 210 increases it again, which, I may remark, is an exactly opposite deduction to that which is graphically depicted in the curve on Figure 4. It appears to me that the results tabulated in the paper are so inconsistent with each other that they furnish sufficient grounds for regarding the results of the crude experiments put before us as being scientifically valueless for the purpose of establishing any physical doctrine which was not well known and recognised by prominent engineers a long while ago. The broad result of the experiments appears to me as simply tending to show the correctness of the statement made years ago in Rankine's book on the steam engine that "The rapidity of the condensation depends mainly on that of the circulation of the cooling fluid at the other side of the plate." Before concluding I should like to state that I received a copy of Mr. Halliday's paper on "Feed water heaters," with the discussion upon it two evenings ago. As Mr. Halliday appears to have alluded to the experiments now under discussion in his reply to Dr. Elliott's remarks, I may be in order in joining with him in speculation regarding the doctor's opinion of them, and I can assure the author of the paper that any opinion which Dr. Elliott may care to express will be in the highest degree worthy of the careful consideration of the members of this Institute. Dr. Elliott was first in the field in publicly directing attention to the effect of feed heating on the circulation in a boiler, and he was also the first, I believe, to point out that economical feed heating lowered the funnel temperature by increasing the efficiency of the heating surfaces, and in order to enhance our facilities for studying the subject I beg leave to suggest that the Council of this Institute approach Dr. Elliott with the view of obtaining his permission to embody in our own transactions the valuable paper on Feed Heating which he contributed some time ago to the proceedings of the South Wales Institute of Engineers.

Mr. Halliday has just told us that he is unaware of any text book that teaches that the thermal efficiency of a heating surface is affected by the speed of circulation. I have already quoted such an engineer's *vade mecum* as Professor Rankine's well-known book, and in order to prove further that the advantage arising from a good circulation was well known, it would be easy to multiply instances of its public acknowledgment, but perhaps the following extract from a technical journal which I noted a few years ago may serve to illustrate my statement :—

"Free circulation of water in a boiler is often more favourable to steam making than heating surface, especially so if the heating surface is disposed in a way that will not impede the circulation of the water. This has been proved in cases where vertical rows of tubes have been removed from a horizontal fire tube boiler, thus reducing the heating surface but increasing the circulation, so that the efficiency of the boiler was increased as was also the economical production of steam. . . . Numerous experiments have proved that free circulation is a valuable thing in a steam boiler, and it seems that all of the plans employed for increasing circulation have also increased the capacity of the boiler, and the economy of steam-making, even though power applied outside of the boiler has been used for increasing the circulation inside."

I may add that, in my opinion, the manner in which Mr. Halliday carried out his experiments before us to-night has thrown light on the discrepancies to be found in his paper; and, after his demonstration, it is not surprising to find that a series of observations made and recorded in a similar way should have resulted in so many questionable figures.

Mr. R. BRUCE (Member): I have not had the opportunity of reading this paper, but I think it very desirable we should acknowledge Mr. Halliday's efforts and his work in coming here and making these experiments. It must have taken a good deal of time and thought, and I have not the slightest doubt in the world that Mr. Halliday thinks he has discovered something new. Whether he has or has not is another question. I confess I do not understand the drift of his experiments. Perhaps Mr. Halliday will clear it all up in the course of his reply.

Mr. R. D. KEAY (Member): I think there are two or three possible sources of error in connection with these experiments as carried out by the author. In the first place with regard to the steam jacket, it is assumed that there is a constant supply of heat from the burner under the boiler, and the burner under the super-heater. Therefore we may assume that the supply of heat to the jacket is approximately constant. But we see coming from the drain pipe an occasional drip of water, and probably the jacket is half full of air. You must have the temperature of the inside of the jacket constant if you are to have any useful results at all, but here the actual amount of heating surface (or cooling surface) may be varying all the time. Then again, I am not clear that in these experiments the rate of circulation actually bears the proportion assumed, to the measurements taken in the tank. The rate of circulation, in any case, is too slow to be of any use as a practical experiment. You only have a maximum head of 16 inches, and if the cock is half shut the rate of circulation is so slow that I think it no use at all as an experiment on the rate of circulation. One point Mr. Williams has referred to, and that is that the thermometer at the top does not measure the amount of heat taken up, because there is steam passing there, as well as water. Then another point: I doubt very much whether experiments made at atmospheric pressure are of any use as telling us what takes place inside a boiler working under pressure. Then, again, we are not told the diameter of the inner tube. We can assume it is very small, and it may be so small that the bubbles throttle the tube and affect the circulation in that way. I think that most of the other points I intended referring to have been touched on by Mr. Williams. The subject is a very interesting one, but I really do not think that we can draw any reliable conclusions from these experiments, as there are so many possible sources of error.

Mr. J. R. RUTHVEN (Member of Council): In the course of this discussion several references have been made to what has been written by previous writers on this subject, as embodying all the light and knowledge we can get in the matter, and it seems to be assumed by some speakers that we cannot go beyond what has been written by those authorities. But one of the gentlemen who have spoken here to-night wrote a very excellent and instructive paper on "The Errors of Experts," and there can be no doubt that there is very often more to be learned from errors and failures than from successes. Therefore, I think that Mr. Halliday is entitled to our thanks for having brought this paper before us, even although it may not be perfect. The machine is not perfect, but it is all that he had at his disposal, and by calling attention to the subject, the author has done good service, and stimulated thought and discussion on matters which are not too frequently considered by our members.

Mr. McFARLANE GRAY (responding to an appeal from the chair) said: I have nothing to add to my previous remarks, except that I think Mr. Halliday is to be commended for the paper he has written, and I have no doubt that in spite of the not very great encouragement we have given him to-night, he will go on with his experiments and may prove us all wrong.

The CHAIRMAN : Will you say if the result of the experiments conflicts with any generally accepted law ?

Mr. GRAY : What I understand that Mr. Halliday is trying to prove is the advantages of circulation ?

The CHAIRMAN : That is one point.

Mr. WILLIAMS : The great point he tries to prove is that the efficiency improves until the circulation reaches a certain well defined limit, and then drops; but I do not think that is proved.

Mr. GRAY : His experiments promised to give us some very valuable data, but directly I examined them I found mistakes which are very misleading. The author has left out the important element of the co-efficient of transmission—a very bad example for our members.

The CHAIRMAN : I am sure we are all very much indebted to Mr. Halliday for the great labour he has undertaken in carrying out these experiments, and preparing a paper on the results ; and although there has been a good deal of criticism, especially from Mr. McFarlane Gray and Mr. Williams, I do not know that the matter is completely settled one way or the other. Mr. Halliday has made certain statements and although I do not say that they have been proved, I do not know that they have been disproved.

Mr. WILLIAMS : They have been proved and they have been disproved. My view is that from this paper you can prove anything. The author can prove his point against Mr. Gray by one table, and Mr. Gray can prove his point against the author by another table.

The CHAIRMAN : I think that great advantage would be derived from continuing these experiments with the assistance of other members of the Institute. I throw out that suggestion to Mr. Halliday to-night, and perhaps other gentlemen will associate themselves with him in continuing the experiments. A vote of thanks was passed to Mr. Halliday at our last meeting, and I am sure we all feel greatly indebted to him for the trouble he has taken in the matter.

It was understood that this would be the last meeting of the session, but we have a paper on which no discussion whatever has taken place. I refer to Mr. McArthur's paper on "A Comparison between the Performances of two Steamships." That paper was read at our last meeting and the discussion upon it was adjourned until this evening, but it is quite clear at this late hour it cannot be adequately discussed. It is a paper which fairly bristles with points for discussion, and most engineers, I fancy, will find something to say about it. It has been suggested to me that I should take the sense of the meeting as to whether we should have another meeting, before closing the session, for the purposes of the discussion on this paper.

It was resolved to continue the discussion on Mr. McArthur's paper on the re-opening of the session.

Mr. Ruthven proposed and Mr. Noble seconded a vote of thanks to the Chairman for presiding, and the meeting then terminated.

THE AUTHOR'S REPLY

(Contributed after the Meeting, by Correspondence).

The experiments made on the absorption of heat by water when flowing through a tube and receiving the heat from a source outside the tube were primarily to satisfy myself, and their publication and their reception by eminent engineers, and also the animated discussion at the Institute, prove surely that they are of some interest to others besides the author. It has been said that circulation improves absorption of heat, and that this has been proved long ago and put in the text books. I never saw it in any text book that circulation had been directly measured in England, and I never saw that the effects of this circulation directly was measured in thermal units. But placing this question entirely aside, as it is our business to teach people these things, I am exceedingly glad to learn that everyone knew this long ago. Mr. Williams has kindly criticised the manner of conducting the experiments and said they could prove anything. Not content with this broad assertion, he says that the first line of the record of the results shows that they are worthless. What about the minute which came before the first? The first line must have meant what took place for a minute. But Mr. Williams confessed at the end he had been severe in his criticism, because some quotation had been referred to in the paper on feed heaters from Dr. Elliott which did not give him justice. Mr. Williams had better wait until Dr. Elliott speaks for himself. Mr. Williams also confessed that if it could be shown there was a fall of absorption of heat after a certain point the author would have discovered something new, but towards the close of the discussion he began to offer a theory why the absorption co-efficient should fall, and I am pleased to think conviction has begun to work. Mr. McFarlane Gray was content to imply doubt, but why should he try to stop the speaker when he showed that the reasons urged by Mr. Gray more forcibly proved the author's case? Surely to Mr. Gray, as to myself, Plato is his friend, the laws of science his divinity. In all the cases taken there was a fall of absorption shown after the velocity of the water reached a certain point. Through each set of experiments the source of heat was the same, but that source was different for the separate sets. Practically the temperature of the steam was the same through each set—that is, the heat source was constant. For the purpose of the experiments it was a matter of no moment what temperature the source of heat was at. But Mr. Gray pointed out that at higher velocities the temperature of the water fell, and the difference of temperature between the source of heat and the water must have been greater, and therefore, according to Newton's law, there ought to have been more heat transmitted in the latter cases than in the first. But experiment in every case said there was not, and whatever the explanation may be, whether Mr. Williams' explanation is true or not, the experiment showed there was a falling off after the velocity of the water reached a certain point. And although theoretically there ought to be a greater transmission of heat, there is, at higher velocities, really much less. The curves made by other experimenters point in the same direction. The curves all bend over.

Referring to Mr. Gray's views on the question of heating the water to the boiling point. Bubbles of steam were rising in it for a distance of four or five inches, and we conclude the water was boiling for that distance. Under these conditions a certain quantity of heat was absorbed by the water. When the water was made to come faster so as to cause the temperature for the four or five inches to fall ten degrees, there was in every case a notable increase of heat absorbed. This was the case in the experiment made before the members. Now contrast this with Professor Ser's words, "When the water has come to the boiling point or bursts into ebullition the water is more than ordinarily tumultuous and a notable increase of transmission ensues." More than ordinarily tumultuous just means Mr. McFarlane Gray's motivity, it seems to me. Quick mixing motion would be another rendering. The experiment does not bear out the view of Professor Ser and Mr. McFarlane Gray.

I must express my indebtedness to Mr. Keay for pointing out two figures wrongly added, and would also like to point out that the word boiler is not mentioned from beginning to end of the paper. Mr. Williams said "The measurements had been taken without any provision being made for measuring the latent heat of any steam which might have been found." Perhaps this quotation from the paper will satisfy

Mr. Williams :—" There was a fear that the bubbles of steam as they rose to the surface of the water in the tube would condense in the overflow tube, making the water in it at a higher temperature than when it was sent away under the boiling point. So the temperature of the water was measured as it flowed from the tube into the cubic-centimeter measure." Lower down it will be seen that in one case it was 178 deg. F., and in the other case 180 deg. F. I had no idea how the curves would turn out. Mr. Kirkland (Member) and I simply read off temperatures and centimetres when the minute struck, and marked them down as they came. The water flowed and the gases burned, careless of the experimenters, who noted what occurred, and the temperatures and the cubic centimetres were marked down without a care or knowledge of what would happen. The curves came out the same every time. Three results were taken before the members, and they were, roughly :—

Velocity	Thermal units absorbed
25	3,000
100	11,500
310	4,900

The superheating tube for the steam was red hot, and dry steam issued from the jacket all the time of the experiment. While the temperatures were being read off we had no idea what the products would be, and the result simply confirmed the other results. Surely at the least the effect of the velocity of water over hot surfaces in raising the absorbent power up to a certain point has been demonstrated in a simple manner. That it falls off after a certain point has surely also been plainly shown. That molecular change in the water at the boiling point—an opinion held by some—has not anything to do with the absorbent power, the author submits has also been proved. The author also would wish to express his thanks for the personal assistance given by several members of Council in making the experiments.

ADJOURNED DISCUSSION

ON

THE HEAT ABSORBENT POWER OF WATER.

58, ROMFORD ROAD, STRATFORD,

MONDAY, SEPTEMBER 11TH, 1899.

CHAIRMAN:

MR. A. BOYLE (Chairman of Council).

The CHAIRMAN said that in opening the forthcoming session they might congratulate themselves on the favourable auspices under which they were starting their winter meetings, which he trusted would be a great success. They had one paper ready to be discussed, and promises of several more, and he believed they would have some instructive meetings. That evening they resumed the discussion of two papers which had been read, and to a certain extent discussed. These were "Experiments on the Heat Absorbent Power of Water," by Mr. George Halliday, and "A Comparison between the Performances of Two Steamships," by Mr. J. D. McArthur.* The first-named was a series of results of experiments made by an apparatus kindly presented by the editor of the "Engineer." Mr. Halliday tried to elicit a very important principle—that the heat absorbent power of water was increased and the evaporative efficiency of a boiler was increased by the rapidity of circulation up to a certain point, and that when this was reached the evaporative efficiency decreased. These experiments had been continued, and he now asked Mr. Halliday to explain what he had ascertained.

Mr. HALLIDAY said that during the discussion a very important point was brought out by Mr. McFarlane Gray—that when the velocity of the water got up to a certain point there was a maximum of absorption of heat by water. The first experiment he made he

* Discussion on this Paper under separate cover.—J.A.

thought proved this fairly well, but Mr. Gray pointed out that he did not furnish sufficient data to prove that the rate of absorption fell off after a certain velocity had been reached; that in one or two cases he had two points, and in another one point. He, of course, maintained that in every case there was a maximum absorption point, and that in no case was there a straight line, showing that the amount of heat remained the same, whatever the velocity might be. Seeing the position Mr. McFarlane Gray took up, he made careful notes and readings of what took place when the velocity reached a certain point where the absorbent power fell. These results he had published. On the first experiments he simply got a hint that such a thing existed; on the second there was absolute certainty that such a thing did exist—that a maximum absorption of heat did take place, and that it depended on the amount of heat offered to the tube containing the water. When using the Fletcher burner he did not turn on the gas full; he allowed an easy burning row of gas to work on the tubes. In that case he had the maximum when the rate of flow was 100 cubic centimetres per minute. He turned on the heat further, and the maximum point came when the velocity of the water was measured by about 200 cubic centimetres per minute. He then put on two gas burners so that the tube was entirely enveloped by flames, and in that case it was about 500 cubic centimetres. In each case he used the same tube, and the water was measured by what flowed right through. Since this was published, the maker of a feed-water heater wrote him that this feed-water heater depended on the principle of pumping the water through a series of tubes at different speeds, and he found that under certain circumstances it would heat the water more than in others. That gentleman's argument was that their feed-water heater was the best, but the speaker would not say, that because the feed-water heater allowed the speed of the water to be regulated as it came through the pipes, therefore it was the best. Another firm had results which coincided with certain results that had been obtained.

The CHAIRMAN: Perhaps Mr. Williams will favour us with a few remarks.

Mr. WILLIAMS (Member): Our Hon. Secretary has stated in the preface that the paper now before us is the one which was discussed here on the 29th May last. As I was present at that discussion, and noted that Mr. Adamson was, unfortunately, absent from it, he will, perhaps, feel obliged to me for pointing out that there are important differences between the paper which was then and that which is now submitted to us for discussion.

The CHAIRMAN: The alterations in the paper are only numerical ones.

Mr. WILLIAMS: All the evidence which the paper contains is only numerical, and an alteration of the numerals without any acknowledgment is tampering with the evidence which was submitted to us in support of the points which are in dispute, but the difference between the published paper and the one discussed at our former meeting is *not* only numerical, as a comparison of the two papers will show that the author's authoritatively expressed statement of the cause of the alleged phenomena—at the end of the paper discussed in May last—is altogether omitted without explanation. Instead of replying to the points which were raised during the last discussion, Mr. Halliday has enlarged on certain experiments which have been made subsequent to our last meeting. I do not, however, propose to discuss those experiments until some explanation is given relative to the points raised at our last meeting, as I take it that the prominence now given to the subsequent experiments and the alterations and omissions which have been made in the paper clearly indicates that my former remarks are obviously justified in the light of the substituted figures now before us in the revised paper.

As the author has made no attempt whatever in my hearing to explain the causes of the discrepancies which I showed in his tables, it may be thought unnecessary to occupy the time of this meeting with further remarks, except that I think it is necessary to repeat the statement that I made during the last discussion, to the effect that I have never disputed the *possibility* of water having a maximum co-efficient of heat transmission which may be dependent on the velocity of its circulation over a heating surface, as I regard the truth of such a proposition—or that of any proposition whatsoever—as entirely dependent on the sufficiency of the evidence adduced in its support. As Mr. Halliday's paper contained so many obvious and now-admitted inaccuracies where the evidence could be tested, the portion which could not be tested was open to the suspicion of similar results in its deductions, from which I gather that we are not warranted in accepting this paper as conclusively establishing any other fact than the author's want of care in preparing it for the notice of this Institute.

Referring to the paragraph preceding Table A it will be observed that the difference of temperature is as it was originally given, viz., 45·16°, but in the summary under the table the figures have been altered to 45·45°. It will be observed that the figures denoting the average difference of temperature in Table B have been altered, and that in the summarised results of Tables A and B, the thermal units per minute have been altered. The deductions from Table C have also been altered by correcting the errors arising from the average difference of temperature formerly given, which, of course, affected the number of thermal units deduced. The errors to which I directed attention in Table H have been eliminated, with the result that the thermal units now given are over 53 per cent. more than Mr Halliday gave us before; and the summarized difference between Tables H and I is now given as 1,573·5 thermal units, instead of 2,527 units as before. An alteration will also be observed in the results deduced from Table K.

The CHAIRMAN thought it was not necessary to go beyond the scientific value of the experiments made at the Institute, and he was sure every opportunity would be given to members to see these experiments. It was unnecessary for him to give any definite opinion.

The HON. SECRETARY: I regret that I was unable to be present when this paper was read, and in part discussed, as the corrections made during the passing of the proofs have unwittingly led to some sharp strictures. Some, at least, of the figures referred to were obviously errors, either clerical or otherwise, and being obvious, the shaft of severe criticism should hardly have been pointed so keenly, without allowing for possible errors in copying. The author's attention was called to the figures in ordinary course, before final printing. They were naturally corrected, and I was unaware of the stress laid upon them as points for critical discussion.

Mr. HALLIDAY said really there was no material difference in the figures in the paper now before them. Further experiments showed that they could not be disproved. They might question a machine which someone invented and said would do a certain thing, but the results of experiment conducted by men of experience, he had never heard questioned, and these results were entirely as they stood before. With regard to the point that the results of the further experiments were felt necessary because of the discussion, he entirely disagreed. They were different results altogether. The first results were made with steam as a heating agent, the temperature of which was never over 300 deg.; the second results were obtained by heating a copper tube by means of gas, the temperature of which was something like 2,500 deg., and in the second case they got much better results. He had spoken with two or three prominent engineers on the matter, and they did not question the results at all.

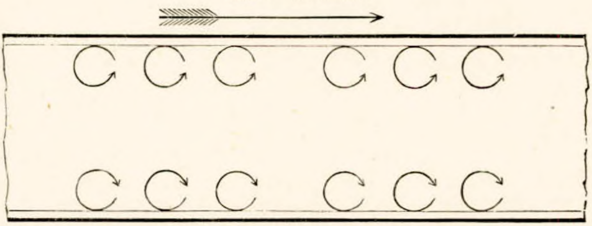
The CHAIRMAN said he had spoken to Mr. McFarlane Gray, and he had given every credit for the work done. It was very creditable to the Institute that they were able to conduct experiments and test these on the premises. The council looked upon this as a very important part of the educational work which he hoped they were doing as regarded marine engineering. They had several sets of apparatus, and he might suggest that a testing machine would be a very valuable acquisition.

The HON. SECRETARY: It is quite legitimate for an author to withdraw from a position he may have taken up in the course of writing a paper, especially on an experimental subject, when the light of discussion and reflection have revealed a weakness in his evidence. The complaint, however, appears to be that the author withdrew from the position he had taken up in the paragraph referred to, without giving acknowledgment to the members who pointed out the weakness in the evidence. The paragraph may have been deleted, whether due to fresh evidence or the discussion is not material,—it was not brought to my notice, and I am only now aware as to the importance attached to the amended figures, and the paragraph in question.

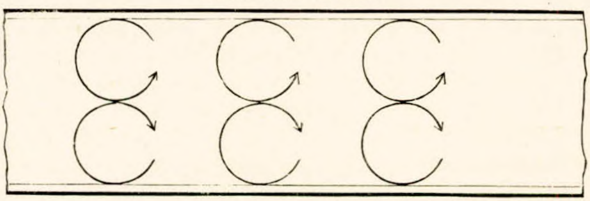
The position, as I now understand it, is that, in the opinion of some of the members, the experiments are, doubtless, valuable in themselves and educational to those who took part, to those who witnessed them, and to the members generally, yet in the paper describing the results, the evidence is not regarded as sufficient to prove any new principle or establish any new rule. It appears to me, therefore, that a continuation of the experiments in the light of the discussion and criticism is of very great importance, and the results will prove of great value, inasmuch as the present controversy may be placed in a more definite position for the general good. We are all seeking after truth, and those who assist in any way to open up new channels of thought deserve our warmest thanks and encouragement, whether we see eye to eye with them in their conclusions or not. I cannot presume to offer an opinion on the subject, not having been present when the experiments were made, or when the paper was read. Mr. Halliday has intimated that he and his committee have arranged to conduct experiments every Friday evening, so that any members, by arrangement, may have an opportunity of following the matter up, and satisfying themselves on the subject under discussion.

Mr. C. H. WINGFIELD writes on the subject as follows: *Apropos* of Mr. Halliday's paper and the results of his experiments, I think his results, if they are reliable, are, perhaps, explicable by the light of Professor Osborne Reynolds' experiments on the flow of liquids in pipes. He showed that at low speeds the liquid near the centre of the tube moved in parallel lines, and that at a critical velocity it all mixes up. I forward a sketch showing the changes that probably take place in the currents as the speed increases:—

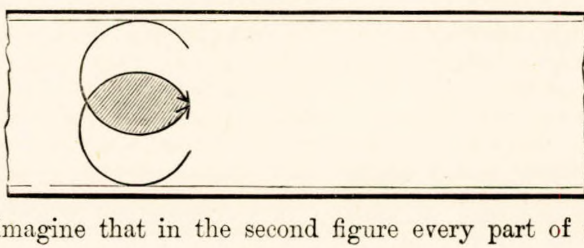
LOW SPEED.



CRITICAL SPEED.



HIGH SPEED.



I imagine that in the second figure every part of the fluid touches the tube, but that the part shewn shaded in the third figure may not be able to make up its mind which side to go to, and, like the donkey which starved when offered two equally inviting trusses of hay, may remain in the middle, and so reduce the heat absorbing power of the fluid as a whole. If the author had given the diameter of his tubes, and the speed of flow in feet per second, it would have been possible to check these suggestions.

ADJOURNED DISCUSSION

ON MONDAY, SEPTEMBER 25th, 1899.

CHAIRMAN:

MR. WM. McLAREN (Member).

The CHAIRMAN: We have this evening to continue the discussion upon Mr. Halliday's paper on "Feed Heating and the Heat Absorbent Power of Water."

The HON. SECRETARY: I have had some correspondence with Mr. Northcott on the subject of heat and feed-water, and the communication I am about to read bears to a certain extent upon both of Mr. Halliday's papers. Mr. Northcott writes as follows:—

"When feed-water which would otherwise be forced into a boiler at t_a, is raised previously to a higher temperature t_b by means of heat taken from the exhausted steam, or from the products of combustion after they have left the boiler, it is clear that, other conditions being the same, a saving is effected equal in heat units to t_b-t_a. Of course, if other conditions are not the same, the saving may be less than t_b-t_a. For example, if the feed-heater increases the back pressure upon the piston, in the case of feed-heating by exhaust steam. Or if the draught is reduced in the case of feed-heating by waste gases, the saving may be partly or wholly counter-balanced and feed-heating may even lead to a loss. "When increased economy is gained by using what would otherwise be wasted heat, the cause of the increased economy is clear. But when we warm the feed-water by means of live steam, or steam taken directly from the boiler, it is not at all clear why any saving should result from feed-heating. On the contrary, as most feed-heating arrangements entail a loss of heat by radiation, one might expect a live steam feed-heater to lead to a loss rather than to a gain. If we rob Peter to pay Paul, we don't increase the sum of Peter's plus Paul's possessions. Many competent engineers, however, who have used direct steam feed-heaters are satisfied that they do lead to increased economy. We are, therefore, forced to believe that increased economy does in certain cases result. Again, other competent engineers have satisfied themselves that live steam feed-heaters do not increase economy. We are, therefore, equally forced to believe that in certain other cases increased economy does not result. The natural conclusion to be drawn from the discordant results of experimental trials is that the conditions of the trials were different. Mr. Halliday is apparently convinced that not only do live steam feed-heaters lead to increased economy, although no waste heat is utilized, but that when waste heat is used to heat feed-water, the gain is greater than the amount of heat utilized by a further saving in the boiler and due to the feed-water being hot instead of cold. He asserts that hot feed-water is 'in better form for receiving heat' than is cold water. Also that hot water 'absorbs more heat' than cold water. Both statements are somewhat nebulous The first, that 'hot water is in better form for receiving heat' would mean, I imagine, that hot water receives heat more readily than cold water, in consequence of the increased temperature. The second, that hot feed-water absorbs 'more heat' is difficult to understand. It is generally believed that the quantity of heat required to raise the temperature of, say, a pound of water to the boiling point, and to evaporate it at that temperature, is constant for any two temperatures. And that by raising the lower limit of temperature, less heat and not more is required to convert the water into steam. Does the author of the paper mean that hot water requires more heat to convert it into steam, or that steam generated from hot water is superheated? If not, what becomes of the 'more heat' absorbed? The author really means, I take it, that hot water aborbs heat faster than cold water, and that as a consequence, when a boiler is fed with hot feed-water more heat is abstracted from the furnace gases than is the case when the feed-water is colder. Further on in the paper, the author apparently lends himself to the statement that hot water circulates faster than cold water, and that it abstracts heat faster because of the faster circulation. Further on, again, he would appear to think that when a boiler is not receiving either hot or cold feed, its circulation is normal and best, but that this normal and best state is disturbed less by the admission of hot than cold feed. If this be correct it would follow that the heat going up the chimney will be least when the boiler is not being fed—that rather more heat will be wasted when hot feed is being introduced—even although the hot feed is got from live steam. Is there any evidence in support of this? Before most of our members were born, it had been ascertained, experimentally, that the rate of transmission through so-called 'heating surface' increased with the rate with which the water was caused to flow past the transmitting surface. Very early experiments showed also that when the water receiving heat was at, or about, the temperature of evaporation, the flow of heat through the heating surface was about four times as fast as when the absorbing water was at a low temperature. Profersor Ser, quoted by Mr. Halliday, has since drawn these same conclusions from experiments made by M. M. Thomas & Lawrens. I understand the Professor to state that generally when the water arrives at boiling point its circulation becomes more rapid, and transmission of heat through the plate becomes appreciably greater. Professor Ser does not say in the sentence quoted by the author of the paper, as the author himself does, that as a consequence of the more rapid water circulation the co-efficient of transmission is increased. It is possible that the increased transmission may have been due to the increased water flow, but Professor Ser does not say so in the sentence quoted. When Mr. Halliday speaks of the 'co-efficient of transmission,' the 'transmission efficiency' and the 'efficiency of heating surface' he means the rate of transmission. He does not mean the heat transmitted by the plate divided by the heat received by it—nor the heat transmitted, divided by the heat generated. I agree with Professor Elliott in thinking that 'mere velocity of matter *apart from thermal and other conditions*' would not increase the rate of absorption. Mr. Halliday thinks this statement quite disproved by his experiments, but that is because he has omitted to take account of the words I have italicized. It is not, I venture to think, the rapidity of circulation that increases the heat absorption, but the temperature and the state of the circulating fluid in contact with the plate. A good circulation clears away the particles of water that have received heat from the 'heating surface,' and the steam bubbles that may have been formed thereby, and gives other particles of colder water the chance of warming themselves by absorbing heat drawn from the plate. Further, unless all our notions of convection are wrong, increased temperature does not necessarily cause increased circulation. We may raise a body of water from say 100° F. to 200° F. without causing increased circulation. We may raise the upper layer to 200° F. and leave the lower layer at 100° F. without appreciably increasing the circulation. But if we increase the temperature of the lower stratum or reduce the temperature of the upper, then convection will increase with the difference of temperature. If in the path of a descending current we introduce cold feed-water I should expect circulation to be increased, not diminished, and that the circulation would be more rapid than when hot feed were introduced in the same place. Engineers might with advantage consider more carefully where and how feed-water can best be introduced. For hot feed a low level, and for cold feed a high level, would appear to be indicated for feed introduction, but the point of entry and direction of flow should obviously be such as to assist and not to counteract a natural convection stream. It is generally believed that the rate of transmission through 'heating surface' is a function of the difference of temperature between the gases and water. Therefore, the rate of transmission should be greater with cold feed. That it is not greater in all cases is in all probability due to the circumstance that where circulation is defective *the temperature at the face* of the plate may be much greater than that of the great body of water, so that the actual difference of temperature is much less than the apparent. There is a great deal of experimental evidence to show that hot water appears to absorb heat faster than cold water, but I doubt whether this is so in cases where temperature alone governs the rate of absorption. When, however, the particles of water receiving heat are in such a position that they can abscond at once with the latent heat, *i.e.*, when they can immediately fly off as vapour into an atmosphere of vapour, then the rate of absorption might I think show a very great increase– not, however, due to temperature. The experiments recently carried out by Mr Halliday show as other previous experiments have done, a rate of absorption increasing with the velocity with which the water flows past the heating surface, but the rates appear to have a very low limiting value. It is probable that the physical properties of the plate may limit the rate of transmission, even apart from difference of temperature. It is more than probable that there is a limit for each difference of temperature. But I suspect in this case the limit arose from want of circulation on the other side of the plate. It is as necessary to give all the particles of gas the chance of becoming cool, as it is to give all the particles of water the chance of becoming hot. The further experiments now in progress may throw some light on this point. There is a great deal yet to learn in connection with the generation of steam. If we could reverse the process of jet condensation, steam engineering would be revolutionized.

If even we could generate steam with the same rapidity and the same heat interchanging surface as obtains in steam cylinder initial condensation and re-evaporation, steam boilers would be enormously reduced in size. I don't believe it either impossible but we want someone to show us how to do it."

Mr. G. HALLIDAY (Member): You will notice that a number of the periods brought forward by Mr. Northcott are very similar to the periods brought forward by Mr. McFarlane Gray at our former meeting. Mr. Gray and Mr. Northcott are, both of them, world-wide authorities on this subject of the transmission of heat and its application to modern industries, but they have not laid aside some of the old ideas that used to be prevalent. It is not necessary to accept old ideas when new experiments have proved that those ideas do not give sufficient reasons for the results. I will give one instance to show what I mean. Mr. Gray tried to prove I was wrong in my results when they showed a drop in the transmission of heat after a certain point in the velocity of the flow. The reason which Mr. Gray advanced to prove that I was wrong was, I understood to be, that Rankine held that the rate of transmission varied approximately as the square of the difference of temperatures between the fire side of the plate and the water side of the plate; and that is granted if everything be left out of account but the simple transmission due to difference of temperature, the rate of transmission does so vary. But other conditions may modify the case, for instance, the condition of the water in the tube, and of the water on the other side of the plate should be considered. If you allow the water to remain quite stationary on the side of the plate a film of air or steam comes between the water and the plate, if you rush the water over the plate the film is removed or changed; therefore, there is better contact between the water and the plate and, in consequence, better transmission. When Rankine says that the rate of transmission varies as the square of the difference of temperature, he was not considering the movement of the water over the plate. Lord Kelvin brought forward a new theory in a paper which gives new ideas with regard to transmission, and, looking at the whole question from Lord Kelvin's views, it seems to me the law put forward by Rankine is less important than that put forward by Lord Kelvin. Mr. Northcott and Mr. McFarlane Gray refer to Rankine's formula, but not to the new ideas put forward by Lord Kelvin. There is a question raised by Mr. Northcott at the beginning of his communication, where he takes up the idea formerly put forward by Mr. Gray regarding live steam feed water heaters, that we cannot expect to gain anything by robbing Peter to pay Paul. The words used by Mr. Northcott are: "If we rob Peter to pay Paul, we don't increase the sum of Peter's plus Paul's possessions." There is an error here which appears to have escaped the attention of both Mr. Gray and Mr. Northcott, and also of Mr. Weir for a time. To carry on the simile: Let Paul stand for the water and Peter for the steam. There is no attempt on the part of Paul to try and steal anything from Peter. Peter and Paul are a joint stock company, and Paul says: "Peter, if you will give a little of your heat away to heat up the feed water before it comes in to me I will have greater efficiency, I will produce more. I will gain by it more than you lose." That is an arrangement between the water and the steam. The water says to the steam: "You will lose a little in order that I may gain more." Paul, as the water, tries to get as much heat out of the heating surface as he can by travelling as quickly as possible over it, but as soon as he does this, cold water is thrown on his exertions by the feed and prevents him from taking up heat as quickly as he would under other conditions. Now, he can only keep up his speed by keeping up the temperature of the water in the boiler to boiling point, and, although there is a loss of efficiency by using live steam to heat up the feed water, the loss is not so great as the gain secured by keeping the temperature of the water in the boiler always the same. To come to another point. There is some quibbling about hot water being better for receiving heat or absorbing more heat. This appears to me to be simply scientific quibbling, and I will come to something that is of practical interest. Mr. Northcott raises a question about the reference to Professor Ser. I am sorry that I have not the paper on feed heaters here, but I did not say that it was laid down by Professor Ser that the increased absorption was due to increased circulation. There was a discussion at the Institution of Naval Architects, when Mr. Gray stated that some one had heated the feed water during the day and drew it off into the boilers during the night, gaining a certain percentage in efficiency. The reason given by Mr. Gray was that the motivity of the water was better when it was kept always at one temperature in the boiler. Professor Ser thought it due to the mixing movement of the water. In fact, the idea which both Mr. Gray and Professor Ser had, was that it was simply due to the mixing motion of the water, and this was the first thing that was considered in the experiments made here. The circulating influence was taken away and the water was simply allowed to absorb heat. The experiments showed that the absorbing power of the water is not increased at boiling point, but decreased. Mr. Northcott says: "I agree with Professor Elliott in thinking that mere velocity of matter, apart from thermal and other conditions, would not increase the rate of absorption." He also says: "Mr. Halliday thinks this statement quite disproved by his experiments, but that is because he has omitted to take account of the words I have italicized." The words italicized being "apart from thermal and other conditions." I should like to point out to Mr. Northcott and Professor Elliott that the paper on "Feed Water Heaters" dealt with the thermal and other conditions, and made no pretence to discuss the question of the velocity of matter. It was the thermal and other conditions which were discussed in this paper, and the manner in which they were affected by velocity. Mr. Northcott further says: "It is not, I venture to think, the capacity of circulation that increases the heat absorption, but the temperature and state of the circulating fluid in contact with the plate." I am going into this matter as carefully as possible. Mr. Northcott is so well known as an authority that his opinions are of great value and importance. *The state of the circulating fluid has been dealt with by Mr. Wingfield, and his idea is this: Professor Osborne Reynolds made a number of experiments with water showing through glass tubes. He had a large tank of some colouring matter, and he found that when this coloured matter was allowed to flow very slowly through the tubes the particles went in straight lines, the coloured matter went right down the middle of the tube and did not distribute either way. When he increased the speed, he found at a certain point, that the coloured matter began to get mixed, and at a high speed the coloured matter was mixed all over the sectional area of the tube. Mr. Wingfield's idea is this, that water at a slow speed goes in a straight line and, therefore, does not affect the film of gas which attaches itself to the surface of the tube. This film of gas stands between the water and the surface of the tube and prevents transmission, but when at a high speed the water begins to get mixed it brushes away the film of gas when the transmission becomes greater. At a higher speed still the water acts as a brush against the surface of the tube, and so the transmission is further increased. At that point Mr. Wingfield stops, but I want to go a little further. A member of the Institute remarked in one of our discussions here that if you take a water hose at a high pressure and squirt it through a flame of fire it goes through like a rod. My belief is that, at very high speeds, water goes through a tube like a rod, just the same as at low speeds. My experiments proved that the transmission increased with the mixing —the greater the speed and the more the mixing the higher the transmission until you reach the maximum. If, after reaching the maximum, you increase the speed still further the mixing diminishes and the transmission declines; and I believe you will find that to be the

* See Mr. Wingfield's explanatory note, pages 25 and 37.

correct theory. I lay particular stress upon these remarks of Mr. Wingfield's and upon Professor Reynolds' experiments. I am of opinion that Mr. Wingfield and Professor Reynolds did not go far enough. In another part of his communication, referring to the proposition that the normal and best circulation is disturbed less by the admission of hot than cold feed, Mr. Northcott says: "If this be correct it would follow that the heat going up the chimney will be least when the boiler is not being fed, that rather more heat will be wasted when hot feed is being introduced, and that the greatest waste of heat will obtain when cold feed is being introduced, even although the hot feed is got from live steam." That was a proposition which I put forward three years ago. I put it forward again in the paper read here, and I put it forward again now. It was proved by the result of Mr. Yarrow's experiments.

CORRESPONDENCE

(CONTRIBUTED AFTER THE MEETING.)

Professor ELLIOTT, D.Sc. (Vice-President): I have read Mr. Halliday's paper on "Experiments on the Heat Absorbent Power of Water," and the discussion thereon with interest.

In the main I agree with the remarks of Messrs. Joseph Williams, Northcott, and Gray.

Unreasoning and careless experiment is not without value, positive or negative; but applied to old, and to a large extent, investigated subjects, it must at least, in the first instance, be accounted merely bothersome.

For example, most engineers accept Newton's third law. Some do not—these are the perpetual motionists, and most, if not all, are experimenters. Many people, engineers and others, believe that so far as we now see, the most recondite Newtonion law of gravitation would be hard to upset; these mostly are educated men—the phrase being used in its true and original sense, and meaning men of humble but earnest and upright attitudes of mind, who, having drawn out knowledge and sound thinking from the wisdom of the past are prepared to seek after truth in the only way it may be found.

There are, in fact, experiments and experiments, experimenters and experimenters. False theory and true experiment are a combination just as infrequent as true theory and false experiment. Some there be that imagine theory so called has no natural connection with experiment—these are either cranks or people who are incapable of discovering the meaning of ordinary English phrases.

As an illustration of theory discovered from experiment—properly called speculation—it might be urged that after all we are not certain that absolutely and of necessity heat *left to itself* flows in the direction higher to lower temperature, inasmuch as Clerk Maxwell, in one of his moods, showed that given animate or merely intelligent agency the contrary condition might be realized by simply adopting the principle of "undue preference" (a handy railway phrase). But, even so, the heat concerned is not exactly left to matter and itself; and thus the original proposition of Thomson and his fine diction remain untouched.

It is fair to the author to say that he has not sheltered himself under the wing of Clerk Maxwell or any other great man.

By contrast the experiments are left to speak for themselves.

Some of Newton's theories were based on theory of past experiment, some on his own thinking and his own experiment. But even Newton made mistakes according to the author of the "Errors of Experts." Still the guidance of reason is, and must be, a large factor in research of any kind that is destined to be successful. Joule, for example, did not despise the guide of Newton, and in his best work called in the help of Thomson.

The curve to which the author refers should, on ascertained knowledge, become asymptotic to the horizontal. He insists that it should bend down, forgetting, apparently, how in all the world the said curve is to be continued downward without limit.

As a matter of experiment, the author has failed to make clear that the temperature of the source was constant.

In point of fact he was, in the final stage, drawing off more heat in a given time than the apparatus in question could supply when thus hardly pressed, and the available heat amongst the rush of cold water was found to show signs of dilution.

From my own knowledge of this subject, experimental and otherwise, I may say finally and at once, that the author has discovered a "mare's nest."

In some occult way the results of Mr. Halliday's experiments are referred to water-tube boilers and feed-heaters. But in the case of water-tube boilers the conditions are entirely different from those assumed; practically the temperature inside the tubes is constant, while the temperature of the furnace gases outside varies from say 2500° to 600° F.

The moral of the whole thing is never assume a temperature to be constant at this or that till it is measured and ascertained.

Mr. Wingfield writes in further explanation of his views as follows:—

"It does not appear clear that the author's experiments—in all of which the velocity of flow was in excess of that due to the difference in density of equal columns of solid water and of a mixture of water and steam—have any direct bearing on actual steam boilers, or on what takes place in water tubes, since the circulation in these is almost invariably due solely to this difference of density, the exception being such as Hureshoff boilers, in which a pump forces the water through.

"It seems probable that when the flow exceeds that due to what I may call the natural causes at work in a boiler, the phenomena will no longer resemble those met with in practice.

"The author has somewhat missed the point of my remarks. I did not refer to a film of gas, but to the fact—if fact it be—that, with such an action as the hypothetical one shown in Fig. 3, on page 25, a part of the water shown shaded would not reach the sides of the tube, and the quantity of water actively engaged in carrying heat from the metal would, therefore, be a reduced proportion of the whole.

"Fig. 3 is only an imaginary one, and is not based on the same experimental evidence as Figs. 1 and 2."

Mr. Williams writes :—

"In reading over the proof-sheets of the discussion on this paper, I find that Mr. Halliday has replied to my remarks by correspondence, and in a manner which he did not assume in my presence. What I said is now recorded, and I leave the record to speak for itself."

Mr. Halliday writes as follows :—

"I have to add only a few remarks to the contributions of Professor Elliott and Mr. Wingfield. I do not think that Professor Elliott is quite correct when he writes he agrees in the main with Mr. Gray. Mr. Gray holds that the speed of water over the heating-surface is of great moment. Professor Elliott, as far as I can understand him, says it is of no moment. As far as I understand Messrs. Northcott and Williams, they have no opinion on the matter. That is the main question.

"I do not understand what Mr. Wingfield means by the natural causes at work in a boiler.

"I have to thank the Council and Members of the Institute for their kindly interest in the experiments which have been made."