Int. J. Heat Mass Transfer. Vol. 3, pp. 163–166. Pergamon Press 1961. Printed in Great Britain.

PIONEER PAPERS IN HEAT AND MASS TRANSFER

FOREWORD TO THE SERIES

MOST serious students of a scientific subject find that their understanding of its significance is deepened by an acquaintance with the works of the comparatively few writers who have made striking developments of a theoretical or experimental nature. Since many of the key papers are somewhat inaccessible however, and since the effort to keep up with current publications leaves few readers with sufficient energy for historical research, the editors of this journal propose to reprint selected works of pioneers in the field of heat and mass transfer. These will appear in alternate issues of the journal, as a rule. The papers will be mostly more than twenty-five years old and will be grouped according to subject. The papers will be reprinted in English, French or German, with editorial abridgement and comment where required.

The Editors

PIONEER PAPERS IN CONVECTIVE MASS TRANSFER

1. OSBORNE REYNOLDS: On the extent and action of the heating surface of steam boilers. (*Scientific Papers of Osborne Reynolds*, Vol. I, pp. 81–85. Cambridge University Press, London, 1901.) Reprinted by permission of The Manchester Literary and Philosophical Society.

Editor's Foreword

The first group of reprints will be devoted to contributions to the knowledge of convective mass transfer. It therefore seems proper to start by presenting Reynolds' often-cited but seldom-read paper relating heat transfer and friction to the "diffusion" processes in pipe flow. It is interesting to note that what has come to be known as the "Reynolds Analogy" is not spelled out in detail in this paper, which is more concerned with the influence of the flow velocity on the heat transfer coefficient: only proportionality is postulated between the constants in (1) and those in (2).

D.B.S.

ON THE EXTENT AND ACTION OF THE HEATING SURFACE OF STEAM BOILERS

OSBORNE REYNOLDS

[*From the Fourteenth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester."
Session 1874–5.]

(*Read* October 6, 1874)

THE rapidity with which heat will pass from one fluid to another, through an intervening plate of metal, is a matter of such practical importance that I need not apologize for introducing it here. Besides its practical value, it also forms a subject of very great philosophical interest, being intimately connected with, if it does not form part of, molecular philosophy.

In addition to the great amount of empirical and practical knowledge which has been acquired from steam boilers, the transmission of heat has been made the subject of direct inquiry by Newton, Dulong and Petit, Péclet, Joule, and Rankine, and considerable efforts have been made to reduce it to a system. But as yet the advance in this direction has not been very great; and the discrepancy in the results of the various experiments is such, that one cannot avoid the conclusion that the circumstances of the problem have not been all taken into account.

Newton appears to have assumed that the rate at which heat is transmitted from a surface to a gas, and vice versa, is, *ceteris paribus*, directly proportional to the difference in temperature between the surface and the gas, whereas Dulong and Petit, followed by Péclet, came to the conclusion from their experiments that it followed altogether a different law.[1]

These philosophers do not seem to have advanced any theoretical reasons for the law which they have taken, but have deduced it entirely from their experiments, "à chercher par tâtonnement la loi que suivent ces résultats[2]".

In reducing these results, however, so many things had to be taken into account, and so many assumptions have been made, that it can hardly be a matter of surprise if they have been misled. And there is one assumption which upon the face of it seems to be contrary to general experience, this is, that the quantity of heat imparted by a given extent of surface to the adjacent fluid is independent of the motion of that fluid or of the nature of the surface;[3] whereas the cooling effect of a wind compared with still air is so evident that it must cast doubt upon the truth of any hypothesis which does not take it into account.

In this paper I approach the problem in another manner from that in which it has been approached before. Starting with the laws, recently discovered, of the internal diffusion of fluids, I have endeavoured to deduce from theoretical considerations the laws for the transmission of heat, and then verify these laws by experiment. In the latter respect I can only offer a few preliminary results; which, however, seem to agree so well with general experience, as to warrant a further investigation of the subject, to promote which is my object in bringing it forward in the present incomplete form.

The heat carried off by air, or any fluid, from a surface, apart from the effect of radiation, is proportional to the internal diffusion of the fluid at and near the surface, i.e. is proportional to the rate at which particles or molecules pass backwards and forwards from the surface to any given depth within the fluid, thus, if AB be the surface and *ab* an ideal line in the fluid parallel to AB then the heat carried off from the surface in a given time will be proportional to the number of molecules which in that time pass from *ab* to AB—that is for a given difference of temperature between the fluid and the surface.

This assumption is fundamental to what I have to say, and is based on the molecular theory of fluids.

Now the rate of this diffusion has been shown from various considerations to depend on two things:

(1) The natural internal diffusion of the fluid when at rest.

(2) The eddies caused by visible motion which mixes the fluid up and continually brings fresh particles into contact with the surface.

The first of these causes is independent of the velocity of the fluid, and, if it be a gas, is independent of its density, so that it may be said to depend only on the nature of the fluid.[4]

The second cause, the effect of eddies, arises entirely from the motion of the fluid, and is proportional both to the density of the fluid, if gas, and the velocity with which it flows past the surface.

The combined effect of these two causes may be expressed in a formula as follows:

$$H = At + B\rho vt \qquad (1)$$

where t is the difference of temperature between the surface and the fluid, ρ is the density of the fluid, v its velocity, A and B constants depending on the nature of the fluid, and H the heat transmitted per unit area of the surface in a unit of time.

If, therefore, a fluid were forced along a fixed

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[1] *Traité de la Chaleur*, Péclet, Vol. I, p. 365.

[2] *Ibid.*, p. 363.

[3] *Ibid.*, p. 383.

[4] Maxwell's *Theory of Heat*, Chap. XIX.

length of pipe, which was maintained at a uniform temperature greater or less than the initial temperature of the gas, we should expect the following results.

(1) Starting with a velocity zero, the gas would then acquire the same temperature as the tube.

(2) As the velocity increased the temperature at which the gas would emerge would gradually diminish, rapidly at first, but in a decreasing ratio until it would become sensibly constant and independent of the velocity. The velocity after which the temperature of the emerging gas would be sensibly constant can only be found for each particular gas by experiment; but it would seem reasonable to suppose that it would be the same as that at which the resistance offered by friction to the motion of the fluid would be sensibly proportional to the square of the velocity. It having been found both theoretically and by experiment that this resistance is connected with the diffusion of the gas by a formula:

$$R = A'v + B'\rho v^2 \tag{2}$$

And various considerations lead to the supposition that A and B in (1) are proportional to A' and B' in (2).

The value of v which this gives is very small, and hence it follows that for considerable velocities the gas should emerge from the tube at a nearly constant temperature whatever may be its velocity.

This, as I am about to point out, is in accordance with what has been observed in tubular boilers, as well as in more definite experiments.

In the Locomotive the length of the boiler is limited by the length of tube necessary to cool the air from the fire down to a certain temperature, say 500°. Now there does not seem to be any general rule in practice for determining this length, the length varying from 16 ft to as little as 6, but whatever the proportions may be, each engine furnishes a means of comparing the efficiency of the tubes for high and low velocities of the air through them. It has been a matter of surprise how completely the steam-producing power of a boiler appears to rise with the strength of blast or the work required from it. And as the boilers are as economical when working with a high blast as with a low, the air going up the chimney cannot have a much higher temperature in the one case than in the other. That it should be somewhat higher is strictly in accordance with the theory as stated above.

It must, however, be noticed that the foregoing conclusion is based on the assumption that the surface of the tube is kept at the same constant temperature, a condition which it is easy to see can hardly be fulfilled in practice.

The method by which this is usually attempted is by surrounding the tube on the outside with some fluid the temperature of which is kept constant by some natural means, such as boiling or freezing, for instance the tube is surrounded with boiling water. Now although it may be possible to keep the water at a constant temperature, it does not at all follow that the tube will be kept at the same temperature; but on the other hand, since heat has to pass from the water to the tube, there must be a difference of temperature between them, and this difference will be proportional to the quantity of heat which has to pass. And again, the heat will have to pass through the material of the tube, and the rate at which it will do this will depend on the difference of the temperature at its two surfaces. Hence if air be forced through a tube surrounded with boiling water, the temperature of the inner surface of the tube will not be constant, but will diminish with the quantity of heat carried off by the air. It may be imagined that the difference will not be great: a variety of experiments lead me to suppose that it is much greater than is generally supposed. It is obvious that, if the previous conclusions be correct, this difference would be diminished by keeping the water in motion, and the more rapid the motion the less would be the difference. Taking these things into consideration the following experiments may, I think, be looked upon, if not as conclusive evidence of the truth of the above reasoning, yet as bearing directly upon it.

One end of a brass tube was connected with a reservoir of compressed air, the tube itself was immersed in boiling water, and the other end was connected with a small non-conducting chamber, formed of concentric cylinders of paper with intervals between them, in which was inserted the bulb of a thermometer. The air was then allowed to pass through the tube and paper chamber, the pressure in the reservoir being maintained by bellows, and measured by a mercury gauge; the thermometer then indicated the temperature of the emerging air. One experiment gave the following results: With the smallest possible pressure the thermometer rose to 96°F, and as the pressure increased fell until with $\frac{1}{10}$ in. it was 87°, with $\frac{1}{4}$ in. it was 70°, with 1 in. it was 64°, with 2 in. 60°, beyond this point the bellows would not raise the pressure.

It appears, therefore, (1) that the temperature of the air never rose to 212°, the temperature of the tube, even when moving slowest; but the difference was clearly accounted for by the loss of heat in the chamber from radiation, the small quantity of air passing through it not being sufficient to maintain the full temperature, an effect which must obviously vanish as the velocity of the air increased; (2) as the velocity increased the temperature diminished, at first rapidly, and then in a more steady manner. The first diminution might be expected, from the fact that the velocity was not as yet equal to that at which the resistance of friction is sensibly equal to the square of the velocity, as previously explained. The steady diminution, which continued when the velocity was greater, was due to the cooling of the tube. This was proved to be the case, for at any stage of the operation the temperature of the emerging air could be slightly raised by increasing the heat under the water, so as to make it boil faster, and produce greater agitation in the water surrounding the tube. This experiment was repeated with several tubes of different lengths and characters, some of copper and some of brass, with practically the same results. I have not however as yet been able to complete the investigation, and I hope to be able before long to bring forward another communication before the Society.

I may state that should these conclusions be established, and the constant B for different fluids be determined, we should then be able to determine, as regards length and extent, the best proportion for the tubes and flues of boilers.