

gether confirmed in practice, because it was often said, and he The Author. believed the statement was borne out by experience, that wrought iron, which was less homogeneous than steel, stood better than the latter; so that if it was really the homogeneous material that stood the better, steel ought to be better than wrought iron; but as wrought iron did stand better than steel in most cases it rather complicated the matter. However, the nature of the steel used for making boilers was a subject to which much careful study had been devoted, and he thought a large amount of attention had been given to that particular point, with a view to get as homogeneous a steel as possible. There were so many other conditions which had to be considered that he thought the steel-makers had done all they could to secure a homogeneous product. What engineers had to do was not to place much reliance on the results of efforts in that direction, but to make the strongest possible effort to keep air and sea-water out of their boilers. If that aim could be realised, boilers—whether water-tube boilers or tank boilers—would last more than the 24 years mentioned by Mr. Milton.

Correspondence.

Commodore B. F. ISHERWOOD, of the United States Navy, remarked, Commodore Isherwood. in reference to the Author's statement that corrosion in marine boilers was principally due to the chemical union of the iron of the plates and tubes with oxygen dissolved or mixed with the feed-water, the product being an oxide of iron—that if the Author meant by this that the oxygen described as dissolved in the feed-water combined alone with the iron, producing pure oxide of iron as in a laboratory experiment, he could not agree with him. No doubt oxygen was a great agent in the corrosion of marine boilers, but it was by no means the sole agent, and its action was conditioned by other substances. The Author appeared to have confined his remarks to the internal corrosion of boilers; but in the case of the boilers of steam-vessels, external corrosion was of at least equal importance, and that corrosion was certainly independent of the feed-water. If pure water, saturated with oxygen obtained from the atmosphere, were fed into a boiler, oxidation would be very slow, and of correspondingly small commercial importance. The oxygen disengaged in the boiler from the feed-water by the process of boiling was mechanically

Commodore
Isherwood.

mingled with the steam, which not only occupied the steam-room of the boiler but largely pervaded the water too, the latter being, in fact, more or less in the state of froth. The oxygen thus diluted by the steam lost proportionately its corrosive effect, and had but little chemical action upon the iron surfaces. To illustrate how the action of the oxygen was affected in a boiler by conditions independent of the oxygen, there might be cited the experimentally known fact that, other things being the same, the oxidation was less the higher the temperature of the feed-water. Passing the feed-water through a "heater" on its way to the boiler lessened greatly the corrosive action of that water, by mere increase of temperature. The interior of the shell of a boiler was rarely affected by oxidation; but if a "superheater" was used, its internal surfaces, which were of course at a temperature much higher than that of the shell of the boiler, rapidly corroded, even if the metal was of the same thickness and quality as the boiler-shell. The steam in the boiler-shell and in the superheater would be of course of the same pressure and generated from the same water; the only difference was in the temperature, but that enormously affected in this case the corrosion of the metal. Again, the corrosion of the metal in a boiler exposed to a particular set of influences was greatly affected by the mere thickness of the metal; other things being equal, the thinner the metal the greater the corrodibility. He did not mean by this that the thinner metal would corrode through sooner than the thicker metal—a sufficiently self-evident fact; but that in extreme cases the thinner metal would have become entirely corroded out while the thicker metal showed scarcely any signs of having been attacked. He believed this truth to be universal, whether applied to boilers or to other iron structures. The braces and the stay-bolts of the flat surfaces of boilers were always much more affected by corrosion than the surfaces themselves; in fact, they were frequently found to be nearly corroded through while the surfaces were practically intact; and this difference was due to the small mass of each stay or bolt relative to the mass of the surfaces it braced. The corrosion of braces and stay-bolts in the steam-room and in the water-room of boilers proceeded with equal rapidity, showing that this difference of location was not a factor in the corrosion. The carbonic-acid gas dissolved in the feed-water and the common salt held in solution in that water were potent agents of internal corrosion; more or less organic matter was also present, and its decomposition increased the corrosive action. The feed-water always contained

salts of lime—sulphates and carbonates—in solution, and the decomposition of these conduced to the same result. Without the presence and reactions of these substances, pure oxygen gas in the feed-water would cause little or no corrosion. The internal and external corrosion of marine boilers was a very complex problem, and certainly could not be accounted for by the single factor of the small weight of oxygen gas dissolved in the feed-water. Independently of the quantity of oxygen gas the feed-water might hold in solution, its quality—scarcely to be appreciated by the most refined chemical analysis—had an enormous influence on the internal corrosion of boilers, in many cases destroying the boiler in one-third or one-half the time it would have lasted with better feed-water. The corrosion of steam-boilers was a striking illustration of the impossibility of applying the results of laboratory experiments, made under one set of conditions, to cases under quite different sets of conditions. The only experiments of commercial value were those made on the scale and under the conditions of actual practice; all others misled the uninitiated and were useless to the initiated.

Where sea-water was used for feeding marine boilers, its corrosive action on iron was so great that, were not the boiler-surfaces protected by the sulphate-of-lime scale formed on them almost immediately it would be impossible to use such water. The oxygen dissolved in the sea-water played but a trifling part, if any, in this corrosion, which was almost wholly due to the alkalies held in solution. In an experiment made in the naval machine shop at Key West on a boiler fed with sea-water from which the salts of lime had been chemically removed, leaving only its other solid constituents in solution, the metal of the boiler was at once attacked in so pronounced a manner that feeding with this so-called purified sea-water had to be discontinued at once. The lime salts were so completely taken out of the water that no scale formed on the boiler-surfaces. With the Author's assertion that the sulphate-of-lime scale formed on the surfaces of a boiler was so poor a conductor of heat that the plate on which it was deposited became overheated and expanded sufficiently to crack the scale and throw it off, he could not agree. He had known heavily-worked marine boilers with scale between $\frac{1}{4}$ inch and $\frac{1}{2}$ inch thick so strongly adherent that nothing but a hammer and chisel could remove it; there were no signs of cracking, and the sea-water had never penetrated to the metal beneath, which was intact as regarded corrosion. The scale, in fact, was a good conductor of heat under the circumstances, notwithstanding its popular

Commodore character to the contrary. Scale could be cracked off the metal
Isherwood. by strongly heating the latter in the absence of water, but this drastic method could only be practised at the imminent risk of ruining the boiler. Many years ago, in the case of a very-much-scaled boiler in which the surfaces were inaccessible, he had removed the scale by cracking it off by the quick expansion of the metal beneath, obtained by lighting a fire of shavings in the furnace, no water being in the boiler; but he did not recommend the practice. Neither was he able to agree with the Author's statement that rapid boiler-corrosion had been proved to be due to air forced into the boiler by the feed-pump, which drew the air from the hot well simultaneously with the feed-water. All feed-pumps, without exception, continually did this, and if the fact asserted were true in one case it should be true in all cases, which it notoriously was not, for he had known boilers that were worked under this condition between 20 and 40 years, and which even then were far from being corroded out. If the pumping of air into boilers—and this was always being done to a greater or less extent—produced so destructive an effect, the result would be not only disastrous but remediless. Fortunately the fact was otherwise.

Mr. Maginnis. Mr. A. J. MAGINNIS did not altogether agree with the Author's statement that corrosion of marine boilers was principally due to chemical causes; as after a good many years of careful watching he had come to the conclusion that the main cause of corrosion was the difference of temperature on either side of the plates. For instance, the plates of the furnace referred to by the Author were admittedly parts of marine boilers which corroded quickly along the line of fire-bars. This corrosion, in Mr. Maginnis' opinion, was due entirely to the wide difference of temperature which was experienced by the plate, as the portion of it just above the line of fire-bars must expand in proportion to the temperature of the fire, whereas on the other side of the plate the temperature was only that of the cooler water coming up from the bottom of the boiler. The result was that the surface of the plate on the water side must be stretched or torn to allow for the expansion on the fire side. Another part of the boiler which gave considerable trouble through rapid wasting was the bottom of the furnace a little farther in than the bridge. Here the intense heat struck downward from the bridge and raised the temperature of the upper side of the plate above that of the water side. Considerable trouble was also caused by the wasting of the stays. This again was also caused by the intense

heat of the flame in combustion-chambers and elsewhere being transmitted into the centre of the stay, whereas at the surface of the stay there was only the temperature of the water, with the result that the surface of the stay had to be stretched to allow of the expansion of the centre portion. He had been strongly confirmed in this opinion by finding that the longitudinal stays which were at the back of the uptakes wasted considerably, whereas those not in the wake of the uptakes were not affected. Acting on the principle that great heat was conducted into the heart of a stay, he had lapped stays with silicate cotton in strips, and by so doing allowed them to attain the same temperature throughout, with the result that the corrosion was altogether stopped. Of course there was no doubt that chemical corrosion did take place in parts, and that air formed an important factor in corrosion; but he did not think the suggestion put forward by the Author, of forming a hot-well between the air-pump and the condenser, was a practical one.

Mr. Maginnis.

Mr. F. J. ROWAN remarked that, considering the great amount of study which many investigators, over a long period of years, had devoted to the subject of the Paper, it was not surprising that the Author had not been able either to bring forward any new facts or to add materially to existing information concerning it. The classic researches of Robert Mallet, which were embodied in reports made to the British Association,¹ had gone to the root of the general subject of the corrosion of various qualities of iron in both fresh and salt water, and, although the main object which Mallet had had in view was the preservation of iron ships, yet the nature of much of the action which took place in boilers was indicated by his investigations. It must be admitted, however, that engineers remained practically unacquainted with his work and ignorant of its bearing on the difficulties which they encountered as soon as the days of low-pressure steam passed away, until some years after even the publication of the summary of his experiments.² Professor Bonsdorf of Helsingfors, long ago, and Professor Crace Calvert of Manchester, about the year 1866, had demonstrated the action and effect of oxygen and carbon dioxide on iron in presence of moisture, and Faraday had drawn attention (in a Report of the Committee of the House of Commons concerning the Holyhead Roads) to the powerful action of the magnesium chloride

Mr. Rowan.

¹ Report of the British Association for the Advancement of Science, 1838, p. 253; 1840, p. 221; and 1843, p. 1.

² Transactions of the Institution of Naval Architects, vol. xiii., p. 90.

Mr. Rowan. in sea-water on iron plates; but these were for some years considered as observations of merely abstract scientific interest. Having been practically engaged during the years 1860-1873 in the construction of the high-pressure marine boilers and engines introduced by his father, Mr. Rowan's attention had been directed to this subject, and (Mallet's work being at that time unknown to him) he had found in the demonstrations of Professor Calvert, previously alluded to, the first clear indication of a clue to the difficulties of corrosion in marine boilers. Corroborative evidence and additional information had soon appeared, principally from researches carried out in Germany, such as those of Wagner, Fischer, Stingl, etc., and by J. Y. Buchanan and others at home; whilst contemporaneous Papers published by various engineering societies had shown by contrast the want of light in the ideas of engineers. The results of his study of the subject had been published in a Paper read before Section G at the meeting of the British Association in 1876,¹ and this Paper covered the entire field which was embraced by the Author's remarks. A year later the third Report of the "Committee appointed by the House of Commons to enquire into the causes of the deterioration of boilers in the Royal Navy" had been issued. It emphasised the facts then already published, but added no fresh suggestions as to the causes of corrosion. For some years after the issue of that report the main interest in the subject had centred in the question of the comparative rates of corrosion in wrought iron and in steel, many Papers dealing with this question appearing in the Proceedings of this Institution and of other societies. The effects of the presence of mill scale, of different kinds of oil, and of different alloys in steel and iron, had also been studied, as well as the range of galvanic action and its causes, and the influence which was exerted by stress of various kinds upon the corrosion of iron and steel. Even the revelations of the microscope as to the physical structure of iron and steel had been laid under contribution in connection with this subject, so that mechanics, chemistry and physics had all been appealed to in the matter, and facts had been accumulated which tend to invalidate some of the Author's theories of the actions which took place in boilers. With regard to the action of the air held in suspension in fresh or distilled water and in sea-water, the Author was right to some extent, because the constituents of the air or gases were the same in both cases, and

¹ Report of the British Association for the Advancement of Science, 1876, p. 229.

must have the same action on iron. He had omitted to notice, Mr. Rowan. however, that, in consequence of the sulphates which it held in solution, sea-water had the power to absorb a much larger volume of carbon dioxide than an equal quantity of fresh water could absorb, and this additional volume of gas was readily given off from sea-water on the decomposition of its sulphates. This of course gave to a given quantity of sea-water the power to do more damage by corrosion than could be done by an equal quantity of fresh water, and it probably explained some of the figures given in the Author's Table I. In fact, the addition of lime (or calcium carbonate), to which he had alluded, would supply the very reagent suited to decompose the magnesium sulphate of the sea-water and liberate the carbon dioxide. In these days, however, it was almost unnecessary to discuss the effects due to sea-water, because it was the aim of all good marine-engineering practice to abolish the use of sea-water in high-pressure boilers; and the great improvement made in evaporators within recent years had done much to render this possible. In water-tube boilers, especially, sea-water was not used, and this fact rendered inapplicable the remarks in which the Author attributed the pitting and wasting that occurred on the inside of heating surfaces to the action of sea-water, so far as all such boilers were concerned. The Author was unfortunately no happier in his account of the decomposition and effect of magnesium chloride. It was true that that salt was decomposed under the influence of a high temperature (even lower than that due to the pressures of steam carried in marine boilers to-day), but it was not necessary, as his theory supposed, that it should be evaporated to dryness for this result to be reached. Moreover, the statement "the plates or tubes where the fire impinges most fiercely are known to attain a temperature far in excess of the temperature of the water in the boiler, even when there is no scale," was directly contrary to all the principles and facts of heat-transmission, and to the results of every reliable measurement of the temperatures produced in boiling-operations. Moreover, if what the Author imagined were to take place, no high-pressure boiler could survive the operation for many hours without bursting, in consequence of the loss of tenacity to which iron and steel were subject at high temperatures. If it were the case that next to the heating surface there was froth, not water, in any boiler, the life of such an abortive apparatus could certainly not extend to years, nor would it be fitting that it should do so. The "theory of corrosion due to frothing" should therefore be dismissed, and not accepted. The Author's account of the effects produced by furnace-

Mr. Rowan, scale, though ingenious, was not in accordance with the ascertained action of that material. The facts of that action had been widely published in the Papers of Mr. W. Parker,¹ Mr. J. Farquharson,² Sir N. Barnaby,³ Prof. V. B. Lewes,⁴ etc., and the careful investigations of Mr. Thos. Andrews⁵ had made it plain that the prime cause of the mischief due to furnace-scale in contact with iron or steel plates was to be found in galvanic action and not in boiling. Mr. Rowan did not think the substitute for these researches which was offered by the Author in the present Paper would be accepted by any one who had studied the subject. The Author seemed to have written about high-pressure and water-tube boilers whilst he had before his mind's eye only cylindrical marine boilers which were fed either wholly or partially with sea-water, and, in effect, to have mixed up some very diverse conditions of working. He was undoubtedly right in laying stress upon the exclusion of air from the feed-water, and there were few engineers who would not gladly be shown how to prevent it from being dissolved, or, more correctly, from being absorbed by the water. It appeared, however, that the Author under-estimated the difficulty of this problem. It was not known at what rate water which had been freed from air by boiling could re-absorb air on mere surface-contact with an atmosphere containing it, for exact investigations of this action had not yet been made. This was a subject well worth the attention of those who had means and leisure to carry out such investigations, and probably the rate of absorption when ascertained would be found to be astonishingly rapid. The device shown diagrammatically by the Author (*Fig. 2*) had the same aim as the apparatus constructed by Messrs. G. and J. Weir, but, however it might be managed, Mr. Rowan quite agreed that "returning the water from the condenser to the boiler without allowing it the opportunity of dissolving air" was a matter of paramount importance. A scale formed from sea-water was not the best preservative of boiler-surfaces, but there was no reason why a thin coating of lime and magnesia, formed from fresh water, as he had suggested, in his Paper "On Boiler Incrustation and Corrosion,"⁶ should

¹ Journal of the Iron and Steel Institute, 1881, p. 48.

² Minutes of Proceedings Inst. C.E., vol. lxxv. p. 105.

³ Journal of the Iron and Steel Institute, 1879, p. 53.

⁴ Transactions of the Institution of Naval Architects, vol. xxviii., 1887, p. 247.

⁵ Proceedings of the Royal Society, vols. xlii., p. 459; xlv. p. 152; xlvi., p. 176; lii., p. 114. Minutes of Proceedings Inst. C.E., vols. lxxxii., p. 281; lxxxvii., p. 340; xciv., p. 180; cv., p. 161; cxviii., p. 356.

⁶ Report of the British Association for the Advancement of Science, 1876, p. 229.

not be permanently successful. The Author considerably over-estimated the non-conducting power of lime scale unless it was of considerable thickness. The experiments carried out by Mr. Hirsch at the Conservatoire des Arts et Métiers in Paris,¹ and those of Dr. Wiebe and Mr. Schwirhus at the Reichsanstalt,² had shown clearly that the effect of non-conducting coatings of moderate thickness was simply to increase the time required for the metal to acquire its static condition for a certain rate of heat-transmission. In boiling-experiments the time required to bring the water to the boiling-point was increased, but afterwards the amount of heat transmitted was not affected, and therefore the plates suffered no undue heating. Although sympathizing fully with the object of the Author's Paper, Mr. Rowan could not conclude his remarks upon it without expressing an emphatic dissent from a part of the Author's closing sentence. There need be no frothing in a good boiler if the water and the boiler were kept clean and there was proper freedom of water-circulation; but whilst it was well that the circulation should be as perfect as possible, the Author's idea of "limiting the fire heat so as to enable the water to wet the heating surface" was, he feared, utterly contrary to all sound principles of economical working which were based upon the correct view of the steam-boiler as a heat-engine.

The AUTHOR, in reply to the Correspondence, remarked, in reference to Commodore Isherwood's statement that the oxidation was less the higher the temperature of the feed-water because it had been proved that a feed-water heater lessened the corrosion, that the effect of heating the water was to separate the air from it; and in all well-designed heaters there was a cock to carry off the air; but even if the air was carried into the boiler it was in larger bubbles and was mixed with, and not dissolved, in the water, so that it was carried off with the steam instead of corroding the boiler. The observation also made that certain thin metal corroded more rapidly than thicker metal, must have been due to a difference in the chemical composition of the plates, and had this condition been reversed the result would have been the opposite. Commodore Isherwood spoke lightly of alkalies held in solution in sea-water without even specifying them, and went on to say that the only experiments of commercial value were those made on the scale and under the conditions of actual practice. If an operation

¹ Bulletin de la Société d'Encouragement pour l'Industrie Nationale, 1890, p. 302.

² *Engineering*, 1 Jan. 1897, p. 31.

The Author. was an experiment it was liable to be destructive as well as preservative: surely Commodore Isherwood would not seriously suggest that experiments should be tried on boilers when they could just as well be tried on iron dishes. The gist of Commodore Isherwood's remarks seemed to be that the corrosion of marine boilers was a very complex problem, whereas his object was to present it as an extremely simple one; so many seemed to forget the plain fact set out in the opening paragraph of the Paper, that all the iron corroded from boiler surfaces was found in the boiler combined with oxygen either as a black or red oxide. There were only two ways of forming oxide in a boiler, one was by the direct union of the iron with the oxygen pumped in as a component of the air, or carbon dioxide could not be formed in this way if there were no oxygen pumped in. When this reaction did not occur, it simply meant there was something to prevent it. His experience of marine and land boilers was that when air was forced into a boiler corrosion was inevitable unless the boiler plates were protected by scale, or there was some chemical action taking place in the boiler that used up the oxygen, such as the action of zinc in salt water and tannate of soda in fresh. The other way of forming oxide of iron had been described in the reply to Mr. Anstey's question, and was frequently referred to as the "frothing" theory. If Mr. Maginnis' theory were correct, all boilers similarly heated ought to corrode to the same extent and powdered iron should be found in the boilers. Air no doubt corroded fastest those stays and plates that were most heated, and the effect of winding silicate cotton round the stay would be to keep the air away and prevent the oxide formed from falling off and exposing a fresh surface of iron. Mr. Rowan's remarks had certainly not tended to simplify the subject. That correspondent complained that in the Paper the corrosion was not attributed to carbon dioxide instead of to air, but this theory was based on mistaken assumptions. Sea-water contained very little carbon dioxide, much less than hard land-water. It did not accord with chemistry to state that carbon dioxide would be liberated by the decomposition of magnesian sulphate and lime. Carbon dioxide acted about the same as air, and when it was eliminated as far as possible, the air corroded as fast as when there was a great deal of carbon dioxide. Mr. Rowan appeared to be firmly convinced that magnesium chloride in solution could be decomposed and would corrode the boiler. The Admiralty Committee proved this was not the case up to 60 lbs. per square inch pressure, and his experiment clearly proved that it was not the case up to 300 lbs.

pressure. It was only when evaporated nearly to dryness the magnesium chloride united with water and formed magnesia and hydrochloric acid. If a test-tube containing salt water was held in a flame without shaking it the water took up all the heat until it attained the boiling point. It would then take no more heat from the glass, and supposing the tube to be of uniform thickness and smooth, the water previously wetting it was converted into steam and blew the water out of the tube. Directly the water left the glass the latter became overheated and cracked, and it was manifest that whatever salt was previously dissolved in the water evaporated would remain on the surface of the glass and that the magnesium chloride would produce some hydrochloric acid. Metal surfaces were not uniform enough to produce a similar effect, but if a hole was partly drilled through the metal the water in the hole would attain the boiling point before the surrounding water and a quantity of steam would be given off from the hole. Being anxious to test practically the correctness of his frothing theory he had tried many experiments since the Paper was written. The most successful was one where sea-water was dropped on to a thin plate heated over a Bunsen burner, the amount of heat being regulated to cause the frothing action without coating the plate with scale. The plate was corroded through in 8 hours. These experiments showed that this form of corrosion, and also the formation of scale were both dependent upon the chemical composition of the water at the time and the amount of heat applied to the plate. However Mr. Rowan might interpret the "principles and facts of heat transmission," he must admit that water would not take heat from a plate unless the plate was hotter than the water. When the water had attained the boiling point there was a mixture of steam and water or froth next the plate. If sufficient heat was applied steam would be generated so fast that water would not readily pass it to wet the plate. The circumstances under which water wetted a surface and kept it cool required further investigation; but the margin in boilers was very narrow, as a little oil in the water greased the surface, and the water would not then wet it and the plate became overheated.

The Author.