A PAPER

ON

CYLINDER CONDENSATION,

STEAM JACKETS, COMPOUND ENGINES,

AND

SUPERHEATED STEAM.

BY

GEORGE BASIL DIXWELL.

Read before the Society of Arts at the Massachusetts Institute of Technology, Boston, April 29 and May 13, 1875.

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FRANKLIN PRESS: RAND, AVERY, & CO.,
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THE following paper, read before the Society of Arts at the Massachusetts Institute of Technology, Boston, on the evenings of April 29 and May 13, 1875, has been printed for private distribution among persons interested in the use and management of steam. If any errors have crept in, I shall be greatly obliged if the observer will point them out to me by letter addressed to

GEORGE BASIL DIXWELL,

Care of C. P. BOWDITCH. No. 28, STATE STREET, BOSTON, MASS.

Mr. President and gentlemen of the Society of Arts:

I propose to-night to speak upon the subject of Cylinder Condensation, and the means of suppressing it in steam and vapor engines.

I must ask your indulgence while I say a word or two in explanation, I had almost said in apology, for my speaking at all before an assembly composed so largely of scientific and practical men.

The explanation — say apology if you please — must be found in the fact that steam, air, and vapor engines have occupied my attention for the last forty years, and that the greater part of the leisure occurring in the intervals of a business life has been given to reading such public journals as were devoted to these and analagous subjects. Moreover, during the last two years, being entirely unoccupied by other matters, I have devoted myself exclusively to the study of the particular subject in question, and have given it an amount of attention which could hardly have been given in six or eight years by any one compelled to attend to the daily routine of the business of a mechanical engineer. Moreover, when I had reached some definite conclusions, I consulted a practical engineer, who is also a scientific man, and did not venture to consider them right until they had been checked by him.

And, finally, I have verified these conclusions by a large number of experiments, made here under the auspices of the Institute of Technology.

Having taken so many precautions to avoid error, I trust I shall not be considered unreasonable in wishing to lay my ideas before the Society, in the assurance that they will have at your hands a frank but liberal criticism.

And now, before going farther, I must (at the risk of being tedious to the engineers present) explain what I mean by Cylinder Condensation.

CYLINDER CONDENSATION.

Let us suppose that we are to use the steam with the piston at a cut off of one-half or fifty per cent. Now, if we imagine the cylinder to have been heated to the temperature of the steam, and then steam admitted until the piston reached the half stroke, the communication with the boiler then closed and the steam allowed to expand through the rest of the stroke, the exhaust opened and the piston returned; then upon the steam coming in on the next stroke we should expect to find the internal surfaces in the same condition as they were at first. But experiments and all experience have shown us, that in the operations which have gone on during the first stroke, the internal surfaces have become chilled to a certain extent, and that a considerable portion of the steam entering is condensed by them and converted into water. If you will follow the third line of the "Michigan" experiments, given in Table 1, and see the percentage of water which is formed, as compared with the steam which is worked, you will see that it is thirty-nine per cent, at about half cut off; and, I suppose, taking the average of engines now running (not of course looking only to the best compound engines of the newest construction), that we may assume about one-third of the fuel expended in converting water into steam to be lost in this way. Why and how this happens is a question which we shall look at farther on. It is a matter of considerable perplexity, and one for which I have found no satisfactory explanation in publications which have been put in print thus far. Now, if one-third part of the fuel used in steam engines is wasted in this way, and if, as calculated, seventy-five millions of tons of coal per annum are used in the steam engines now running, it would seem that the subject is one, at all events, to which we may with advantage give an hour's study.

My attention was first drawn to Cylinder Condensation and the means of suppressing it, by an article in a prominent Mechanical Journal of London, in which it was pointed out that a non-conducting cylinder would (if such a thing were possible) be an effectual remedy. This was in 1871.

ONE REMEDY FOR CYLINDER CONDENSATION.

Reflecting upon this, it appeared to me that such portions of the cylinder and piston as were not liable to attrition could be easily covered *upon their internal surfaces* with non-conducting materials, and that, so far, we should have the thing required; and it appeared that, in cylinders of large diameter and short stroke, this alone might be an important improvement. As regards the cylindrical surfaces which are subject to attrition, no such application seemed possible. But as to these, a consideration of what happens in the cylinder led to another device. Condensation being understood to be caused by the mist formed in the cylinder by the conversion of heat into work, and by the heat abstracted by external and internal radiation, and by the deposition of a portion of this mist upon the internal surfaces, and its re-evaporation during expansion and during the return stroke, it seemed to me that the difficulty could be in a great measure obviated, if the mist could be prevented from falling upon the surfaces in question.

Now I happened to be aware that water, in perceptible drops, was repelled by metals at a temperature decidedly below the 400°, which had been found in practice to be admissible in the working of engines, — that is to say, at about 380° F. by iron, and about 335° F., by copper, — and I proposed, therefore, to prevent the deposition of mist upon the internal surfaces, by heating the cylinder, before commencing work, to the necessary temperature, and by keeping it thereafter at the same point. This I intended to do by means of a jacket of hot air or of superheated steam, regulating the heat by the indications of a pyrometer, placed in the clearance space, or in a recess formed for it in the metal of the end of the cylinder.

CYLINDER PYROMETER.

The particular form of this pyrometer, which I adopted, was only reached after many unsuccessful experiments; but an effectual instrument was finally obtained by attaching to the internal surface of the cylinder-head a thin slip of copper, rolled up into the form of a hollow cylinder, pierced with many holes, in such a way that its surfaces should be at each stroke exposed to the action of the steam precisely as the internal metallic surfaces of the cylinder were exposed. The examination of many experiments led to the conviction, that the steam and the internal metallic surfaces of the cylinder differed but slightly in temperature, so that a pyrometer, of the form indicated, would give a very close approximation to the temperature of those surfaces.

The experiments made at the Institute have confirmed this expectation. The pyrometer has indicated the oscillations of temperature during every stroke, and has shown itself to be all that we desired.

In what I have just said, however, I have rather run before my story.

The article which drew my attention to the subject of cylinder condensation was immediately reviewed by another London Journal in a leader which ridiculed the ideas put forth by its contemporary, discouraged all efforts towards the production of a non-conducting cylinder or its equivalent, represented that cylinder condensation was after all by no means the formidable difficulty it was supposed to be, and intimated that it was practically suppressed by means of compound engines and steam jackets.

Before then proceeding to experiment myself, or consulting a practical engineer upon the subject, it behooved me to see whether records of experiments already existing were sufficient to show the precise extent of the evil, and also to show whether it had or had not been thus practically overcome. I therefore made search for such records of experiments, and was soon fortunate enough to fall in with those magnificent quartos, by Chief Engineer Isherwood, in which the matter of cylinder condensation was so ably and fully investigated. I examined them with minuteness, recalculated a considerable portion, and made other calculations, notably those of the quantity of steam present at the *Cut-off* (Chief Engineer Isherwood gave only that present at the end of the stroke), the times occupied in the strokes up to the cut-off, the thickness of the film of the internal surfaces which was alternately heated and cooled at each stroke, and various other points not touched by their author.

And here, before going farther, I must be allowed to express my admiration for the manner in which the experiments referred to were conducted and recorded, and of the loyalty with which the results were set down and acknowledged even when contradictory to convictions previously held and expressed. I look upon these two volumes as an invaluable contribution to mechanical knowledge, and as reflecting the greatest honor, both upon their author and upon the administration which directed the experiments to be made. I cannot accept all the theories advanced in these volumes ; but the experiments themselves seem to me worthy of the highest praise, as furnishing means for the solution of many questions about which the leading Mechanical Journals appear still to be quite undecided.

Well, then, I examined these experiments to see if cylinder condensation was a matter of importance or not. There were more than a hundred of them made with some fifty different engines, and I found that, in the experiments with engines varying from three feet to ninety inches, cylinder condensation showed a pretty uniform percentage for any given cut-off, and that the experiments with the steamer Michigan, at Erie, were a fair representation of the average.

EXPERIMENTS WITH STEAMER MICHIGAN.

At the end, in Table 1, is a digest of the experiments in question. Column one gives the point at which the cut-off was made in fractions of the stroke. Column two gives the point of actual cut-off, including clearance. Column three, the pressure of steam at cut-off, in pounds on the square inch. Column four, the weight of water and steam at the cut-off in pounds: this was ascertained by measuring the water which was fed into the engine, and the whole amount being divided by the number of strokes, gives the weight for each stroke. Column five shows the weight of steam alone, at cut-off, in pounds, deduced from the pressure given by the indicator. Column six shows the weight of the water alone at cut-off in pounds, that is to say, the quantity of steam, which, having entered as steam, was condensed and formed into spray, which was partly in the steam and partly deposited upon the internal metallic surfaces. In column seven, we have the percentage which the water at cut-off bore to the steam. I thought it better to compare it with the steam utilized, because it shows the actual loss more easily than comparing it with the total quantity of steam formed. At a cut-off of 92 per cent, there is 9 per cent loss; at a cut-off of 72 per cent, 16 per cent of loss; 39 per cent of loss at the cut-off of 47 per cent; 69 per cent loss at the cut-off of 34 per cent; and the same at a cut-off of 29 per cent. There is a little irregularity here, but I suppose experiments never can be expected to be precise. At the cut-off of 21 per cent, there is 95 per cent loss; nearly half of the whole steam evaporated was lost, that is to say, enough steam to have run another engine, if there had not been any cylinder condensation. Then we have 142 per cent of loss at the cut-off of 14 per cent, that is, enough steam to have run another engine and half another. In column eight we have the weight of the steam at the end of the stroke in pounds, from which we shall get information hereafter, as to the action of the steam, and see whether the condensation occurring after the cut-off is, at any given point, greater or less than the evaporation occurring after the cut-off.

Here I must say a few words in explanation. Let us consider the engine to be running at a cut-off of 47 per cent. Upon the steam entering, we see 39 per cent of that utilized is condensed by the action of the metallic surfaces, the greater part of it probably falling upon the surfaces, a part of it no doubt remaining suspended in the steam. Now, upon the moving on of the piston, and a diminution in the pressure, the water immediately begins to be evaporated; but as the piston uncovers new surfaces, they, being colder than the steam, cause a certain quantity of condensation; and it thus happens that the cylinder, after the cut-off, acts both as an evaporator and as a condenser, and we gain from a comparison of the different experiments a knowledge of where these two conflicting actions counterbalance each other. We see that at a cut-off of 92 per cent, the steam at the cut-off weighed 4.450, and at the end of the stroke 4.348: that is to say, there was slightly less steam in the cylinder at the end of the stroke than at the cut-off.

In the second experiment the same thing. In the third, 2.350 and 2.374, we have a slight increase. In the fourth, 1.696 and 1.671, a slight diminution. There is probably a trifling error in these two experiments, as they do not coincide; but at 34 per cent cut-off we see that the evaporation from the ante cut-off surfaces about counterbalances the condensation which takes place as fresh surfaces are uncovered when the piston moves on.

At 29 per cent cut-off we find that the steam present in the cylinder at the end of the stroke is greater than the steam present at the cut-off; that is, the evaporation from the surfaces before the cut-off is, as the piston moves on, greater than the condensation upon the surfaces which are uncovered. And so it goes on until at 14 per cent cut-off we find a very considerable increase; .686 at the cut-off, and .910 at the end of the stroke, showing that a very large proportion of steam, which had been condensed, was evaporated upon the direct stroke.

A mere inspection of the table of the Michigan experiments will satisfy any one of the importance of cylinder condensation, and of the fact that the suppression of it is extremely desirable even in medium and large engines. In small engines the evil is very much greater, because external and internal radiation, as also the energy with which the interior surfaces act, are very much greater in proportion to the amount of steam used — this last being as the third power, while the surfaces are as the square.

THE USE OF STEAM EXPANSIVELY.

The non-professional part of my audience would be likely to inquire, Why not use the long stroke, at which the condensation is so trifling? and this needs a few words of reply. A hundred years ago, James Watt, after making his invention of a separate condenser, drew attention to the advantage to be obtained from working steam expansively, and took out a patent for the use of it in that manner. Let us suppose a cylinder to be worked at full stroke, with an effect, which, for convenience, we will call six. Now, if we stop the piston at half stroke, the effect obtained will be three, or half of six. This is the effect up to the cut-off. But if now we close the communication with the boiler, and allow the steam to work by its own force of expansion, we shall obtain an additional effect, which (were there no Cylinder Condensation or other deductions to be made) would equal seven-tenths of the effect obtained up to the cut-off, or seven tenths of three, and if we made two strokes in order to use the same amount of steam as we had used in the former case (that of working at full stroke) the effect obtained would be seven-tenths of six additional. In short, the work done with the same amount of coal would be increased seventy per cent.

Proceeding in the same way with the case of cutting off at one-third stroke, the gain is a hundred and ten per cent. At one-fourth stroke it is a hundred and forty per cent; at one-fifth it is a hundred and sixty per cent, and at one-sixth a hundred and eighty per cent. And if we expand sevenfold, the theoretical gain is two hundred per cent, and the total work done is three times as great as would have been done by the same steam worked at full stroke. Now, during these hundred years since Watt showed the advantage in question, engineers have been striving to realize the advantage, but have been greatly disappointed and baffled in their efforts. The reason of the failure was not, I believe, suspected until a comparatively recent period, when it was suggested that cylinder condensation was probably the principal cause. But I think this was not demonstrated fully, till Chief Engineer Isherwood made his experiments. Let us look at these as shown in the case of the "Michigan." If you look at the third experiment, cutting off at forty-seven and five-tenths per cent, you will observe that the loss by condensation is nearly forty per cent of the steam utilized. So you see there disappears from this cause more than one-half of the gain expected from expansion. Clearance would take off six per cent more, and back pressure and friction five pounds per square inch, so that, after making the whole of these deductions, comparatively little is gained by expansion. And the case is still stronger at the shorter cut-offs, so that, after proceeding a very moderate distance on the road of expansion, there is absolute loss instead of gain.

This will be the case even when cylinder condensation is suppressed; that is, we shall finally reach a point beyond which, from other causes, expansion will be useless or prejudicial; but we shall be able to carry the expansion a great deal farther to advantage than it can be carried at present.

Having satisfied myself that the experiments, as shown in the "Michigan," seemed to demonstrate the very great disadvantage of cylinder condensation, and the necessity of suppressing it, the only thing remaining was to endeavor to explain how this took place, and this I found exceedingly difficult; but before accepting the experiments without reservation, it became necessary to have some idea of the manner in which we were to account for such phenomena.

PRIMARY CAUSES OF CYLINDER CONDENSATION.

There have been attempts to account for them in a variety of ways. They have been attributed to external radiation; to conversion of heat into work; to internal radiation; and to condensation from expansion *per se*.

Now, as to *external radiation:* The examination of a great many experiments led me to believe that in well-constructed and well-protected cylinders this was not likely to represent more than one or at the utmost two per cent of the steam utilized in engines over three feet in diameter. Moreover, the actual amount of this external radiation would be greater at full or nearly full stroke than at a short cut-off, because the cylinder would be hotter, and the direct effect of this cause upon the surfaces which act upon the steam up to the cut-off must be still again less in the proportion of their surface to the whole surface of the cylinder.

As to the *conversion of heat into work* after the cut-off: The last column in the table gives us the percentage of the steam utilized which would be condensed to counterbalance this cause, and, in comparing it with column seven, we see that but a very small proportion of the actual condensation in the cylinder can be attributed to it. The extreme amount at a cut-off of fourteen per cent is only 12.5 per cent, while the actual condensation is about 142 per cent of the steam utilized.

With regard to the *internal radiation:* Chief Engineer Isherwood says upon this subject, "If the change in the cylinder from steam to back-pressure vapor could be made *without the deposition and re-evaporation of dew*, the temperature of the interior metallic surfaces would be but very little affected, because vapors, like permanent gases, receive heat with difficulty either by contact or by radiation, and because of the small specific gravity of the back-pressure vapor." This was published in his first volume in 1863, when the results obtained by Professor Tyndall with respect to the absorbing and radiating power of steam were not generally known. These have shown that steam is *not* like the permanent gases in this respect, but, on the contrary, an exceedingly energetic absorber and radiator of heat. Nevertheless we may form some idea of the limits to the effects which can be attributed to internal radiation by the following considerations:

It is evident that the maximum effect possible would be attained if the whole of the steam in the cylinder were heated from the temperature corresponding to the back-pressure to the temperature of the incoming steam. If we take the last of the "Michigan" experiments, the pressure of the steam at cut-off was 33 pounds, equivalent to a temperature of 256 degrees. The back-pressure was 3.7 pounds, equivalent to a temperature of 150 degrees. Upon our supposition, then, the utmost that the steam could be heated would be 106 degrees. This multiplied by .475, the specific heat of steam, would give 50.35 degrees. Dividing this again by the latent heat of the back-pressure vapor, say 1,009, we have about 5 per cent as the maximum amount which it is possible to attribute to the heating of the back-pressure vapor, and if we consider how instantaneously the greater part of the steam rushes out upon the opening of the exhaust, I think we shall conclude that all which can be attributed to this cause will be nearer two than five per cent.

There is, however, another effect which may be considered as coming under the head of internal radiation; namely, the re-evaporation of the spray formed throughout the steam by the conversion of heat into work. But it is evident that the maximum which can be attributed to this cause will be found, if we suppose the whole of this mist to be re-evaporated; and the result in percentages of the steam utilized will be found in column ten of the table; and we see that the greatest percentage, namely, that formed at a cut-off of fourteen per cent (the last experiment), amounted only to twelve and five-tenths per cent of the steam utilized. But, if we consider that the radiation from the interior metallic surfaces into a mass of steam and mist differs materially from the radiation into a mass of unmingled steam, inasmuch as each layer of suspended spray must be an obstacle to the passage of heat until it has itself been removed by evaporation, and if we consider that this evaporation (unlike radiation) occupies a very appreciable portion of time, I think we shall conclude that it is an extravagant supposition to suppose that the whole or nearly the whole of the spray produced by the conversion of heat into work can be re-evaporated. At all events, its maximum at a cut-off of fourteen per cent, or say a sevenfold expansion, has been seen to be twelve and one-half per cent, and this has already been counted under the head of conversion of heat into work.

Yet another cause of refrigeration within the cylinder has been pointed out to me by a young engineer. This is the work done in expelling the steam from the cylinder after the exhaust opens on the return stroke. I mean the expulsion of that portion of the steam which rushes out immediately between the opening of the exhaust and the reduction of the pressure of the steam in the cylinder to the back pressure shown by the indicator. But this cause must be minute as compared with the total condensation shown in column seven, more particularly as it takes place only on the return stroke, so that whatever spray is formed must (the greater part of it at all events) pass out instantaneously without being evaporated in the cylinder.

We now come to Chief Engineer Isherwood's *condensation of steam by expansion "per se,"* as described in his "Experimental Researches in Steam Engineering," vol. i., p. 129, and upon which he relies throughout both volumes as accounting for the phenomenon of cylinder condensation. It appears to me that Mr. Isherwood was led to this theory of the condensation of steam by expansion *per se* by an error in reasoning which deduced from the experiments of Regnault the very opposite of that which they show. I will point out where and how this mistake appears to have been made, knowing that if I am myself in error it will be immediately shown. Referring to the experiments of Regnault, Mr. Isherwood says, "From them it is well known that the sensible heat increases with increase of density faster than the latent heat diminishes, and, consequently, that the total heat of a given weight of steam is greater with greater densities or pressures. Conversely, with decrease of density the latent heat diminishes faster than the sensible heat increases." But the converse proposition, as derived from the experiments of Regnault, does not seem to be that with decrease of density the latent heat *diminishes faster* than the sensible heat *increases*, but that the latent heat *increases slower* than the sensible heat *diminishes*. An example will show this as in the following table:

	Sensible.	Latent.	Total Heat.
At 120 pounds pressure....................	341	873	1,214
At 40 pounds pressure.....................	267	926	1,193
Difference..............................	74	53	21

This certainly seems to show that the total quantity of heat necessary to keep steam of the lower pressure in the vaporous form is *less* than that required to keep the steam of the higher pressure in the vaporous form, and that superheating, and not condensation, must arise from the expansion of steam *per se* when no external work is done. The percentage, however, attributable to this cause, could by no means account either for a great condensation or superheating, as it will be seen by the above example that the difference in the total heats amounted to less than two per cent.

The second proposition which Mr. Isherwood took as the *converse* of the first, appears in reality to be the *reverse*, or what would have been true if Regnault's experiments had shown the opposite of what they did show.

Now, adding together the maximum possible effects attributable to all these causes, we find they could only produce a small fraction of the condensation which actually takes place. How then are we to account for the phenomena? For a long time I could not imagine any answer to this question, but at last the following explanation suggested itself: —

CUMULATIVE ACTION OF INTERNAL METALLIC SURFACES.

Let us imagine that upon the first stroke of the piston the before-mentioned causes abstract from the internal metallic surfaces which act up to the cut-off an amount of heat to compensate which a certain amount of steam, say, for instance, 4 ounces, must be condensed upon the next stroke. Now let us suppose that upon this second stroke there is, during expansion and during exhaust, a certain proportion of this, say, for instance, 3 ounces, re-evaporated. This will chill the surfaces to an extent requiring a subsequent condensation of three ounces. Moreover, during the second stroke, the same refrigerating causes which were acting before will again abstract a quantity of heat equal to the condensation of 4 ounces of steam as first mentioned. It is evident, then, that the internal metallic surfaces will, upon the next stroke, condense 7 ounces, and re-evaporate a portion of the same, and so on increasingly until the heat taken up by the water re-evaporated by the internal metallic surfaces, plus the heat abstracted from these surfaces upon each stroke, is equal precisely to the amount of heat given up by the incoming steam condensed by them. An equilibrium will then be gained, and the engine will thereafter work on regularly. If these surfaces re-evaporate ~~one-quarter part~~ of the amount of steam condensed by them, this last will be 4 times the equivalent of the heat abstracted from them at each stroke. If the internal surfaces re-evaporate nine-tenths of the steam condensed by them, this last will then be 10 times the equivalent of the heat abstracted from them at each stroke, and so on. The action of the metal is *cumulative*, and in this idea, which I believe to be entirely new, we find a sufficient explanation of the phenomenon.*

* NOTE. — Since the above was written my attention has been called to a communication in "Engineering," March 31, 1871, signed W. Hartnett, and headed "Compound Engines." In this Mr. Hartnett points out that if heat be abstracted at each stroke from the internal surfaces, their action will go on increasing until

This cause is sufficient to account for the absolute amount of cylinder condensation, as demonstrated by experiment. The greater *percentage* of this condensation observed at the shorter cut-offs, may be accounted for, first, by the greater amount of the refrigeration caused by the conversion of heat into work, and by radiation; and second, by differences in the cumulative action of the internal metallic surfaces at the different points of cut-off.

We have already seen that the coefficient, by which the cumulative action of the internal surfaces multiplies any refrigerating cause applied to them, is greater or less in proportion as they evaporate a greater or less proportion of the amount they condense. Now, at a short cut-off, there is a gradual diminution of the pressure, and less dew will be left unevaporated upon the opening of the exhaust, when the instantaneous change causes a sudden and violent evaporation, attended probably with the formation of spray, by a process analagous to priming. It will be readily seen that this last effect is much more marked with a small measure of expansion than with a great, because the difference between the final pressure on the direct stroke, and the back pressure of the return stroke, is greater.

Having reached this point, I thought myself justified in dismissing all doubt as to the great importance of cylinder condensation; and the next question was as to what had or could be done to suppress it, by means of compounding engines and using steam jackets.

But before proceeding to this question, I would draw attention to the manner in which the experiments confute several objections which have been made to the conclusions naturally and properly drawn from them.

the condensation and re-evaporation attain a maximum at each stroke. The date of this article is previous to the one which first drew my attention to the subject of cylinder condensation, and shows that the idea of the cumulative action of the internal surfaces is not new as I supposed.

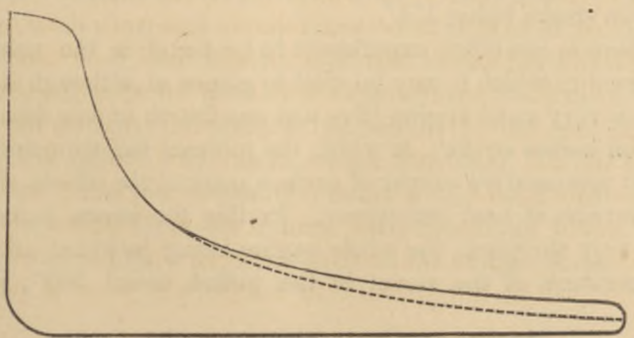
EXAMINATION OF OBJECTIONS.

In conversing with an eminent practical engineer, I was surprised to hear him question whether there really was any such thing as cylinder condensation. His theory was, first, that all boilers leaked about 3 per cent of steam; and, second, that steam, in passing from the boiler, carried into the cylinder, suspended as spray, about 30 per cent of its own weight.

Now if you will cast your eye upon the table you will see the water actually present in the cylinder at the cut-off is, at 92 per cent cut-off, equal to 9 per cent of all the steam formed, and not to 30 per cent; that at a cut-off of 72 per cent the water present is equal to 14 per cent of the steam formed; that at 29 per cent cut-off the water present is equal to 41 per cent of the steam; at 21 per cent cut-off to 50 per cent; and at 14 per cent cut-off to 60 per cent. So you see the results obtained upon the "Michigan" (and they are just like those obtained with fifty other engines) positively contradict and overturn the hypothesis in question.

Again, it has been argued, that although steam is condensed up to the point of cut-off, yet, that much of it is re-evaporated upon the direct stroke, and that it thus comes to pass that more work is done than there would have been had there been no cylinder condensation. Now, let us see what are the facts. You will observe that at the cut-offs of 92.1 per cent, 71.6 per cent, and 33.8 per cent, there was less steam in the cylinder at the end of the stroke than at the cut-off, — that is to say, there was actually less work done up to a three-fold expansion than there would have been done had there been no cylinder condensation. If we apply the information thus obtained to the last experiment (that showing a cut-off of 13.9 per cent), we see clearly that up to 41.1 per cent of the stroke, or a three-fold expansion, at all events, there would be loss and not gain, from the balance of the condensation and re-evaporation between 13.9 per cent and 41.1 per cent of the stroke. In reality, the point at which the condensation and re-evaporation, after the cut-off, balance each other, must be later than 41.1 per cent of the stroke in this case, because the refrigerating influences in action at a cut-off of 13.9 per cent are much greater than those in action at a cut-off of 33.8 per cent. But even taking it on the supposition that condensation and re-evaporation balance each other at 41.1 per cent, in the case where the cut-off was at 13.9 per cent, we see that the pressure at the end of the stroke would be increased 34.7 per cent of 5.8 pounds, or 1.52 pounds, and at 41.1 per cent of the stroke it would be neither increased nor diminished. The pressure then could only have been increased about $\frac{1.52}{2}$ pounds through 58.8 per cent, or say .448 of a pound through the whole stroke. The average pressure throughout the stroke was 8.8 pounds, so that the re-evaporation (*even had there been no loss between the point of cut-off and the point of* 41.1 *per cent of the stroke*) could only have added 5.1 per cent to the actual work done in the cylinder.

The diagram below shows graphically the case we have been considering, and, I think, disposes of the supposition that much more work is done in the cylinder when there is condensation and re-evaporation. It is evident that the loss shown at the cut-off is nearly a total loss, and that the actual loss on the whole stroke is in the same proportion. This is a very important point. The mind naturally jumps at the conclusion, that all the steam accounted for by the indicator has done full and honest work in the cylinder; but yet we see that in the example just cited, 35 per cent of the steam present at the end of the stroke had done really but 5 per cent of the work.



A similar error was made by a reviewer of one of Mr. Emery's communications regarding cylinder condensation. Mr. Emery stated, that he had tried a glass cylinder against a metallic cylinder of the same size, and saved half the steam. The reviewer replied, "Yes, and no doubt the metal cylinder did twice the work!" I presume he meant that the re-evaporated steam did full work in the cylinder, whereas we see that it does very little. It is these loose and hasty conclusions that postpone for years the acceptance of valuable experiments, and retard the progress of mechanical science.

EFFECT OF STEAM JACKETS.

Now as to the extent to which cylinder condensation can be reduced by jackets. Among Chief Engineer Isherwood's experiments we have that of the Brooklyn pumping engine, of ninety inches diameter, and ten feet stroke, covered on its ends very thoroughly with non-conducting substances, and jacketed on the sides, which constituted three-fourths of the whole surface. The engine was run forty-two hours with steam admitted to the jacket, and forty-three hours without. Unjacketed, the condensation was 24 per cent of the steam utilized; jacketed, the condensation was 18.5 per cent of the steam utilized, besides 2 per cent condensed in the jacket, making a reduction of 5.5 per cent, purchased at a cost of 2 per cent, and thus showing an actual saving of three and one-half per cent only. The steam in the jacket had a temperature of 244°, the average temperature in the cylinder during a double stroke being 174°.

There is one other experiment to be found in the volumes referred to which it may be well to glance at, although it was with a very small engine (five and one-fourth inches diameter by ten inches stroke), in which the internal radiation and the great comparative extent of surface masked the effects of the conversion of heat into power. In this the steam jacketing was very thorough, the whole engine being jacketed and the temperature of the steam in the jacket about 300°, while the mean temperature of the steam in the cylinder during a double stroke of the piston was about 180°.

In this engine (which by the way was a condensing one), the steam utilized, that is to say, the aggregate steam present at the point of cut-off (when this was nineteen per cent apparent, and twenty-five and three-tenths per cent actual), was in four experiments 1052; while the amount condensed was 741; besides the condensation in the jacket of 756; making 1497 wasted by condensation to obtain the work of 1052, — the total steam formed in order to get the work of 1052 being 2549. It is true that the jacket effected a great deal in this engine, inasmuch as the steam utilized without it was only about one fourth of the whole amount formed; but the example is instructive, as showing that the jacket (even when the metallic surfaces are enormous as compared with the quantity of steam used) cannot suppress cylinder condensation. The action of this engine appears to justify the opinion that there is no advantage in adding a condenser to small machines, for reasons shown farther on.

Before quitting the subject of the efficiency or inefficiency of jackets, it may be well to look a moment at the recent experiments made at the Charlestown Navy Yard upon the "Rush," "Dexter," and "Dallas."

The "Rush" was a compound engine, thoroughly jacketed; and we can compare its performance in the low pressure cylinder, as to utilization of the steam evaporated, with that of the other two vessels which were not jacketed. In the "Rush," we are told that the indicators accounted in the high pressure cylinder for 93 per cent of the whole steam formed, and that they accounted for 73 per cent in the low pressure cylinder. Now, undoubtedly, a portion of the 7 per cent lost in the small cylinder went over as spray into the large cylinder; but it is highly probable that about 97 per cent of all the steam found its way as steam into the latter. In this case the loss in the low pressure cylinder would be 27 per cent less 3 per cent, supposed to have been absolutely lost in the first cylinder, or say 24 per cent of all the steam formed.

Now the proportion of the steam, which the indicators accounted for upon the long run of the "Dexter," was 68 per cent, showing a loss of 32 per cent; but then the "Dexter" was working at an actual cut-off of about 32 per cent, while the actual cut-off in the large cylinder of the "Rush" was nearly 52 per cent; and the condensation ought, therefore, to have been very much more in the "Dexter" than that of the "Rush."

Again, in the long run of the "Dallas" (cutting off actually at about 32 per cent), the proportion of water accounted for by the indicators was 74 per cent, showing a loss of 26 per cent, a performance very much better than that of the large cylinder of the "Rush," if we make allowance for the great difference in the ratios of expansion. From these experiments, one would almost conclude that the jacket on the low pressure cylinder was worse than useless; but I would not uphold this proposition. I think, however, that the experiments go to show that the steam jacket does but very little towards suppressing cylinder condensation in the large cylinder of compound engines or in the single cylinder of other engines. It is very difficult to form an opinion as to the effect produced by the jacket upon the smaller cylinder. The indicator shows but little condensation, but this may have arisen from superheating, about which the record of the experiments gives no information, and also from the high back pressure and consequent smallness of the difference of temperatures in the small cylinder. It would seem *a priori* to be very improbable that the jacket should have a great effect upon the high pressure cylinder, because the difference of heat between the boiler and jacket steam and the average of the steam in the cylinder is comparatively small.

Before entirely leaving the subject of the efficiency of steam jackets, I must say a word with respect to the experiments made upon the compound engine of the "Bache."

In experiment one, without jacket, the indicator of the small cylinder accounted for 65 per cent of the steam which entered, and the indicator of the large cylinder accounted for

53 per cent, giving an average of 59 per cent; while in experiment four, with jacket, and at about the same rate of expansion, the indicator of the small cylinder accounted for 58 per cent of the steam entering, and the indicator of the large cylinder for 74 per cent, giving an average of 66 per cent. This showed a gain of about 12 per cent, but there still remained a loss by condensation of 34 per cent of all the steam formed, or about one-half of the steam utilized. Had this been saved, by suppressing condensation, the indicated horsepower in experiment four would have cost 17 pounds of water instead of 25, and 1.8 pounds of coal instead of 2.7 pounds.

It must be observed that the "Bache" had a small engine, the effect of the jacket upon which must have been much greater than could be realized with engines of greater size.

This is all I have time to say upon the subject of steam jackets.

EFFECT OF COMPOUNDING.

When we turn to the effect of compounding engines, we find a great advantage. The "Rush" expanding altogether 6.2 times, lost 7 per cent in the high, and 27 per cent in the low pressure cylinder, giving an average of only 17 per cent; that is to say, 100 pounds of steam did the work of 83 pounds, whereas in the "Michigan" a hundred pounds of steam, at about that rate of expansion, would only have done the work of 50 pounds. It is not difficult to see how this happens.

The condensation in the "Michigan" experiments, at a cut-off of 47 per cent, was 28 per cent of the steam formed, or very nearly what occurs in the large cylinder of the "Rush," while with a cut-off of one-sixth, condensation in the "Michigan" experiments was fully 50 per cent. Now this fifty per cent counted against the whole work done by the "Michigan," but the 27 per cent, lost in the large cylinder of the "Rush," counted only against about one-half the work done by the "Rush," and so represented only 13.5 per cent as compared to the whole work. The steam in the small cylinder of the "Rush" lost only 7 per cent, or about 3.5 per cent compared with the whole work done by the engine.

There would be a real advantage in dividing the expansion between two cylinders, even if the condensation were equal in each, because the steam condensed in the first cylinder, being almost wholly re-evaporated, does full work in the second cylinder, that is to say, the full work that could be done by fresh steam from the boiler; but the gain may, perhaps, be much greater on account of an important reduction in the condensation of the steam of the small cylinder, growing out of the fact that it exhausts against a considerable pressure, so that a smaller proportion of the steam condensed up to the point of cut-off, is re-evaporated during the expansion and during the exhaust.

I have not been able to verify this, but I think that we may see reason to expect a small condensation in the high pressure cylinder of a compound engine as compared with that in a low pressure cylinder of the same size; for evaporation, it is well known, is in inverse proportion to the pressures, other things being equal; so that of the steam condensed up to the cut-off, less will be evaporated, if the back pressure be high than if it be low; and as in the high pressure cylinder the back pressure is several times that in a low pressure cylinder, the condensation should be much less than in a low pressure cylinder of the same size. If (to revert to our original example) four ounces of water be formed in a cylinder at each stroke, and only one-half of it be re-evaporated, then the heating and cooling will become equal, and the engine work on steadily as soon as the condensation is twice that due to each stroke. The cumulative effect of the metallic surfaces will then stop; whereas, if three-quarters of the water and spray formed at each stroke be re-evaporated, the cumulative action will cease only when the condensation is four times that due to each stroke. If seven-eighths be re-evaporated, the cumulative action will not stop until the spray formed equals eight times that due to each stroke. I think this also accounts for the empirical fact, or at all events, the belief, before unexplained, of the uselessness of condensers in very small engines. In these engines, working with condensers, the energy of the internal surfaces is enormous, entirely masking the effect of the heat converted into power. I have already quoted one of Chief Engineer Isherwood's experiments on a cylinder, five inches diameter, ten inches stroke, in which 4 pounds of steam were evaporated, to do the work of one, when the cut-off was 25 per cent, whereas, in the engine belonging to the Mechanical Laboratory of the Institute of Technology, of eight inches diameter and two feet stroke, working without a jacket and without a condenser and at 25 per cent cut-off, 1.6 pounds of steam evaporated, did the work of one; or, in other words, the percentage of cylinder condensation in the engine with a condenser was five times that obtained in the other without a condenser. No doubt the engine of the Institute being more than double the size of the other, would give better results, even if worked with a condenser, but the size was not sufficiently greater to account for the immense difference of performance observed.

The varying proportions of the condensation observed at the different cut-offs seem to be due chiefly to the varying amounts of power developed during expansion. The formidable *amount* of the condensation is due to the *cumulative* action of the metallic internal surfaces. I am speaking now of medium and large cylinders. In very small cylinders, radiation, external and internal, and the immensely greater surface, cause in themselves an enormous condensation.

I have now described to you the steps by which I reached the conclusion that cylinder condensation was a very formidable evil in steam engines; that it was but very little reduced by steam jackets, and that even in the best compound engines it still caused a loss of some 17 per cent of the whole steam evaporated, or over 20 per cent of the steam utilized. I hope to show hereafter how the performance of even these may be increased some 35 per cent by an expenditure of 10 per cent of heat.

Having reached these conclusions, I thought it time to take professional advice; and I applied to a practical engineer, who is also a scientific man, a member of this society, and a professor in one of our scientific institutions.

I laid my calculations before him, and we had many and long conferences upon the subject; and finally came to the conclusion, that the means I proposed to apply would probably be effectual, and that at all events they were worth a trial experimentally.

AMOUNT OF HEAT CONVERTED INTO WORK IN THE CYLINDER.

But during one of these conferences, we brought under discussion the question of whether it was right to calculate the refrigeration produced in the cylinder from the heat converted into work by taking the whole work done in the cylinder, as was done by Chief Engineer Isherwood, or by taking only the work done after the cut-off. We agreed that it was reasonable to take the work done after the cut-off, inasmuch as the previous work was done by the whole steam in the boilers, but that it was desirable to have this settled by experiment; and it then occurred to me that an answer to the question might, perhaps, be obtained from Mr. Isherwood's experiments with superheated steam, made on board the steamship "Georgeanna," in Chesapeake Bay;—for if Mr. Isherwood's hypothesis were correct, the temperature of the superheated steam in the cylinder, counting from a given heat in the steam chimney would be less than on the other hypothesis; or, what is the same thing, the temperature in the steam chimney, deduced from the ascertained heat in the cylinder, would be greater upon Mr. Isherwood's hypothesis than upon the other. If the correct temperature of the steam before entering the cylinder had been obtained, the problem would have been of easy solution; but, unfortunately, the correct temperatures were not obtained. The thermometers immersed in the steam chimney and in the superheater showed less than the truth: how much is not known. Chief Engineer Isherwood thought it (the temperature of the steam before entering the cylinder) was about 400°; but it was certainly much more than that, inasmuch as fully 500° was apparent in the cylinder in two of the experiments.

That is to say, the normal temperature due to the indicated pressure added to the heat necessary to produce the increase of volume observed, proved that the actual temperature was 500°; even at the end of the stroke it was 433°, and as the steam must have supplied the heat abstracted by radiation, say 50°, the actual temperature in the superheater could hardly have been less than 483°, even without supposing it to balance the heat abstracted by the work done in the cylinder. But this heat (the equivalent of the work done) must have been taken from the steam, and the only question was, whether the steam in the cylinder had to supply the equivalent of the whole work done, or only the equivalent of the work done after the cut-off. It was on precisely this point I was cross-questioning the experiments in question. Calculating the equivalent of the whole work done in experiment D, it proved to be 289°; and deducting 16° for the excess of heat contained in higher pressure steam at the cut-off over that at the end of the stroke, we still have 273°, which, upon Mr. Isherwood's hypothesis, must have been in the steam before it entered the cylinder over and above the before mentioned 483°. We arrive then at the astounding figure of 756° as the temperature of the steam in the superheater. The highest temperature shown by the thermometers placed in the steam actually used was 338°, which Chief Engineer Isherwood observed was probably 75° too low, and the highest temperature shown even in the superheater was 544°, which was thought to be 60° too low. I ought to have explained that the "Georgeanna," about which I am now speaking, had a large heating surface in the steam chimney which was sufficient to produce a considerable superheating, and that she had also a separate and very large superheater. The steam from the chimney and the superheater was led into the cylinder in such a manner that the quantity of that from one or the other source could be increased or diminished, and in this manner it was proposed to regulate the amount of superheating. Now there were seven experiments which gave a total heat necessarily existing in the steam before entering the cylinder of 538°, 609°, 684°, 756°, 592°, 678°, 761°, shown on line ten of the table on page 52, proceeding upon the hypothesis before mentioned. But calculating only the equivalent of the work done after the cut-off, I found a much lower series of temperatures; namely (those found on line ten), say 397°, 444°, 521°, 555°, 452°, 505°, 566°. But even these you will observe to be very high, as compared to the numbers on line twelve, which are those considered probable by Chief Engineer Isherwood.

In fact, the hypothesis of Chief Engineer Isherwood leads us to temperatures which could not have existed in either steam chimney, or superheater; and even upon the other supposition, namely, that the steam in the cylinder supplied only the equivalent of the work done after the cut-off we still obtained temperatures which show the thermometers to have been a great deal wider of the mark than would have been thought possible; but that they were thus erroneous, may be shown indirectly as follows: Experiment B was made using the steam chimney without the superheater, and that alone was sufficient to suppress all condensation so that the temperature of the steam before entering the cylinder must have been upon our hypothesis 444° at a cut-off of 66 per cent; and that this is not too high, is evidenced by the fact that in the "Eutaw" experiments, at a cut-off of 58 per cent, steam of 394° temperature did *not* entirely suppress cylinder condensation. We are justified, then, in taking the temperature in the steam chimney of the "Georgeanna," at say 440°.

This investigation appeared to me, as well as to my practical adviser, to settle experimentally the question we had under consideration, and to show conclusively that the equivalent of the work done after the cut-off (and not the equivalent value of the whole work) was abstracted from the steam in the cylinder. And here I cannot but point again to the importance that experiments should be fully described in every detail so that subsequent investigators may carefully examine them, and perhaps deduce from them answers to questions not thought of when the experiments were made.

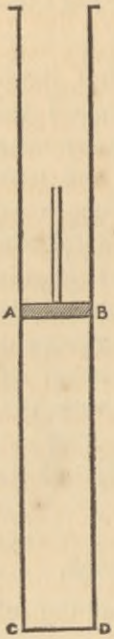
VARIOUS AMOUNTS OF HEAT ABSORBED FROM SUPERHEATED STEAM BY INTERNAL METALLIC SURFACES.

This has, I think, already been made apparent, but it will become still more so a little farther on; for while I was making the investigation, just described to you, a very unexpected, extraordinary, and important fact leaped out upon me; namely, the fact that if we heated our steam, to say 550° in the superheater, and introduced it into a cylinder working at about two-thirds cut-off, we should (*at the point of cut off*) find a temperature of about 500°; but if we introduced the same steam into the same cylinder, changing only the point of cut-off to one-half what it was before, and placing it at the point of one-third instead of two-thirds, then we shall find in the cylinder a temperature at cut-off of only 274°. How could this happen? Before going fully into this I must say a few words upon other subjects.

ADVANTAGES OF SUPERHEATED STEAM.

I had, many years ago, and many times, considered the subject of superheated steam, with a view to obtaining in practice the theoretical advantage there is in using it, which advantage I will endeavor to make manifest to the non-professional portion of my audience, inasmuch as it can be done in a very few words. The figure represents a cylinder open at the top, and having a piston at half stroke, or just in the middle of its length.

If, now, we suppose the area of this piston to be exactly one square foot, and its height from the bottom to the middle where the piston stands to be 26.3 feet, we shall have represented in the space, A B C D, included between the piston and the bottom and sides of the cylinder, the precise space which is occupied by one pound of steam at the pressure of the atmosphere. Let us now imagine the piston to be at the bottom of the cylinder, and that from a boiler we introduce steam so as to raise this piston, supposed to be without weight and to move without friction, 26.3 feet. The work we shall have got out of the steam will be equal to the pressure of the atmosphere upon one square foot of surface raised 26.3 feet, or say in round numbers 2160 pounds raised 26.3 feet; and the cost to us of doing this work will be measured by the total heat, or say 1146°, if we suppose the water to have been at a temperature of 32° F., or, say 1120°, if we suppose the water to have been at a temperature of 58° before entering the boiler. That is, a quantity of heat sufficient to heat one pound of water 1120° F., is what we have to expend in order to raise 2160 pounds 26.3 feet with steam of atmospheric pressure. But the temperature of this steam is 212° F., or counting from the absolute zero it is 673°, and by the law which has been ascertained to govern the action of gases and vapors, and which is very nearly true of both, we know that by doubling the temperature of the steam in the cylinder we can double its bulk, so that we shall have then raised our 2160 pounds, or the weight of the atmosphere, not 26.3 feet, but just double that space. That is, we have done in the last half of the cylinder exactly the same amount of work we had before done in the first half; but when we come to count the cost, we find a notable difference; for, in the first place, we expended 1120° of heat, and in the latter 673°; and this is not by any means the whole of the difference, for the 1120° of heat are degrees measured by water; that is to say, the 1120° represents an amount of heat adequate to raise the temperature of one pound of water 1120°; while the 673°, used during the last half of the stroke, represents the amount of heat necessary to raise the temperature of one pound of steam 673°; and one pound of steam can be raised 673° with much less heat than one pound of water, the proportion being as 475 to 1,000, so that, to compare our two expenditures, we must multiply 673 by 475, and divide



by 1,000. This gives us 320°, and shows that the 673°, which, added to one pound of steam, will double the work done, is equal to 320° added to one pound of water. But we found before, that our power obtained from steam, evaporated from water, cost us 1120°; so that the power obtained by superheating has cost only $\frac{320}{1120}$ths of what we had to expend to obtain the same work out of steam formed directly from water; or, in brief, an amount of fuel used in superheating will do three and a half times the work that the same fuel can do in forming ordinary steam from water. Here is an enormous advantage. This has been manifest to the scientific and mechanical world for a great number of years; but the practical difficulties, in the way of securing this advantage, lie in the high temperatures requisite. The oils used in lubrication are decomposed (often into acid substances) at about 450°; and metallic surfaces, working upon each other without oil at high temperatures, are destroyed. What this temperature is, does not appear to be accurately determined. Chief Engineer Isherwood calls it 400°, but in the experiments with the "Georgeanna," already referred to, it is evident that a temperature of about 500° existed in the cylinder in two of the experiments (those made at a cut-off of 65 per cent) during two runs, which each continued for seventy hours and upwards; and Mr. Isherwood (although prejudiced against the use of superheated steam, which he had in his first volume declared to be destructive to valves, piston, and cylinder,) bore witness to the facts, that the cylinder of the "Georgeanna" was found, upon examination, to be like a mirror, and the valves and valve seats perfect after a constant service of two years with the superheated steam.

I do not by these remarks mean to depreciate the value of the experiments of Mr. Isherwood, of which I have already expressed the highest possible opinion; but to show, on the contrary, the fidelity with which what was seen and done was reported, regardless of preconceived opinions.

We see that he reports the engine of the "Georgeanna" to have been in perfect order, notwithstanding his previous opinion that superheated steam was destructive, and he shows the "Georgeanna" developing a total indicated horse power from 16.4 pounds of water and 2.2 pounds of coal per hour at a cut-off of 28 per cent, while the engines working with saturated steam required 30.5 pounds per horse power per hour.

It is to be remarked that this performance of the "Georgeanna" with the usual type of single-cylinder American engine about equals the performance of the "Rush," built on the latest form of compound engine, which used 16.1 pounds of water, and 2.1 pounds of coal per horse power per hour. I do not say this as against the principle of compounding, for I believe this principle to be destined (when combined with judicious superheating) to give even better results; but I wish to note the fact with regard to the "Georgeanna" as indicative that the existing engines, with the additions of superheaters properly watched and regulated, can be made to do as good work as has ever been performed up to this day.

TEMPERATURE FOUND SAFE IN THE CYLINDER.

To revert to the temperature which may safely be used in a steam cylinder. This is a very important point, as upon it depends the amount of gain which we can make by superheating. We see that the "Georgeanna" worked 140 hours with a temperature of 500 degrees and a cut-off of 66 per cent, and was uninjured; but I do not know whether she was usually worked at the cut-off in question. If so, it would appear to settle the matter, and prove that 500 degrees can safely be used. But if the usual cut-off was, as is probable, 45 per cent, we have only a heat of about 370 degrees as that existing in the cylinder during the greater portion of the two years run.

Let us take 400 degrees as the limit of safety; then, as the steam at present generally used has a temperature when saturated of some 300 degrees, we have only 100 degrees left for superheating, and this only adds about one-seventh to the effect, and would hardly be worth the additional complication of a superheater. You will perceive that I am arguing upon the supposition that superheated steam carries into the cylinder its own temperature, and I think it was generally believed that such was the case, and that steam when superheated took upon it the properties of a permanent gas, those of atmospheric air for instance, so far as regarded its action towards and under the operation of heat. Now, atmospheric air parts with and takes up heat with great slowness, and if heated to 550 degrees in a boiler, and thence carried into a cylinder, would beyond question destroy the oils then present and injure the metallic surfaces. It was supposed that steam was of the same character, and you may judge then of my astonishment when (during the investigation with regard to Joule's equivalent, already detailed,) I observed that steam, which must have had a temperature of 450 degrees and upwards in the superheater, was found in the cylinder at the cut-off to have only the temperature of 274 degrees, as already stated upon page 33. In a moment it had lost something like 200 degrees of heat.

SUPERHEATED STEAM A GOOD ABSORBER AND RADIATOR.

Here was an astounding fact; for you will observe that the condition of things was totally unlike that existing where saturated steam was used, because in this latter case a portion of steam, being condensed into water, brought a fresh portion into contact with the metallic surfaces, and so on; whereas, in the case of the superheated steam, we had in presence of each other, the steam, which I then supposed to be, like air, a very bad absorber and radiator, and the metallic surfaces, which were notoriously so. How could these bad radiators and absorbers interchange their heat in so small a space of time as to be practically instantaneous? It seemed improbable to the last degree, and any other possible supposition appeared preferable. Could it not be that the steam lost its superheating in the pipes and passages before reaching the cylinder? Might it not be that the experiments were in some respect faulty?

Another equally astonishing and unexpected fact answered these questions in the negative.

In the particular experiment which brought out most distinctly the phenomena under consideration, the steam at the point of cut-off (28 per cent of the stroke) had lost all its superheating, and remained at the point of saturation, but without any condensation. There was in it no superheating to counterbalance the heat abstracted for the performance of work after the cut-off, and this equalled 188 degrees (measured by steam, not by water), and even after deducting the 42 degrees of excess of heat existing in the steam at cut-off over that of the lower steam at the end of the stroke (this also measured by steam), there were still 146 degrees to be furnished from somewhere; and, adding a moderate amount for radiation, there were certainly about 200 degrees. Condensation during the stroke would seem therefore inevitable; but so far was this from being the case that the steam at the end of the stroke was absolutely superheated some 46 degrees. Where then did these 246 degrees come from? Certainly not from the steam, for at the point of cut-off this was precisely, or almost precisely, at the temperature of saturation — it had no extra heat to part with, except the 42 degrees excess which exists in the steam of the pressure used at cut-off over that of the pressure found at the end of the stroke. At least 200 degrees then could only be accounted for on the supposition that the metallic surfaces gave it out during expansion.

I examined the experiments in every imaginable way in the expectation of finding some error, but did not succeed in finding any, and I remained for some time in a state of bewilderment regarding them, until the experiments of Professor Tyndall upon radiant heat swept away one of the difficulties by showing that the vapor of water was not like air, a bad radiator and absorber, but on the contrary one of the best to be found in nature.

METALLIC SURFACES WITH SUPERHEATED STEAM IN CONTACT GOOD ABSORBERS AND RADIATORS.

There still, however, remained the perplexing fact that metallic surfaces, especially polished ones like the walls of cylinders, were among the worst radiators and absorbers known; and it was difficult to imagine that they could be made to receive and deliver the heat, however capable the steam itself might be to deliver and receive it.

I finally, however, found the explanation in the same experiments of Tyndall, when he showed that a Leslie's cube, filled with boiling water, produced scarcely any effect upon the thermo-electric pile when a bright metallic surface was presented to the pile, whereas, when the same metallic surface was varnished, heat suddenly gushed out; and, still more, he showed that a thin film of a powerfully radiating gas, like olefiant gas, when passed over the surface of the polished metal, produced an effect analogous to the varnish, — taking up heat from the metal by contact, and passing it out upon the pile by radiation.

Now a precisely similar effect evidently takes place in the cylinder. The metallic surfaces are bad radiators, — among the worst, — but the thin film of steam immediately adjacent becomes heated by contact, and then pours out the heat with the velocity of light into the great mass of steam. All this occurs in an inappreciably short space of time, and the feeble radiator, varnished by the steam, becomes converted into a very energetic one.

THICKNESS OF METALLIC FILM HEATED AND COOLED.

The phenomena observed in the cylinder being thus found to be in accordance with others which are well ascertained, may therefore be accepted as no longer improbable. The facts in the particular experiment referred to were, that the steam at the end of the stroke had a temperature of 256°, and we may assume that a thin film of the internal metallic surfaces had also this temperature. During the exhaust a further reduction must have taken place; but this further reduction could hardly have been great, inasmuch as the greater part of the steam rushed out in an exceedingly minute space of time, leaving but very little in the cylinder to abstract heat from it. It appears to me to be a sufficient allowance, if we suppose the film in question to have been reduced to the temperature of say 244°, or 30° less than the temperature at the cut off.

Now the total weight of steam which passed through the cylinder during the experiment was 920,000 lbs., and the number of single strokes was 223,464; so that the steam present at the cut-off weighed about 4.12 lbs. As the specific heat of steam is .475 while that of iron is .114, and as 4.12 lbs. lost 200° of heat upon entering the cylinder, while the film of iron, which we will call x, gained 30°, we have the equation

$$4.12 \text{ lbs.} \times 200^\circ \times .475 = x \times 30^\circ \times .114$$

from which x = 114.4 lbs. But the metallic surfaces, present at the point of cut-off, measured 56.59 square feet. This surface, an inch thick, weighs 2124 lbs. Then 2124 : 114.4 = 1 inch : $\frac{54}{1000}$ths of an inch. We thus find that less than $\frac{1}{16}$th of an inch in thickness was sufficient to absorb and give out the heat equivalent to the power developed after the cut-off and the radiation.

SUPERHEATING PROPER FOR EACH CUT OFF.

And these phenomena led to a very important conclusion, namely, that steam might safely be superheated sufficiently to balance the radiation, and the heat converted into work without any fear of its carrying into the cylinder a temperature injurious to the working parts, inasmuch as if only superheated to that extent, the temperature apparent in the cylinder would only be the normal temperature of saturation. And it led also to the further conclusion, that the superheating might be safely increased beyond the point named, by the number of degrees existing between the point of saturation and the temperature of 400°, which we suppose to be safe, or to such other temperature as experiment should hereafter prove to be safe.

For instance, taking experiment E on board the "Georgeanna," we find the normal temperature of the steam at cut-off to have been 274°, the heat converted into work 188°, and the greater heat, contained in the steam at the cut-off over the steam at the end of the stroke, to have been 42°. Deducting 42° from 188° we have 146°, which, added to 274°, gives 420° as the point to which at this cut-off the steam might be superheated, if our object was simply to suppress cylinder condensation. To balance radiation, a further amount of superheating would be necessary, and if we take this to be 50° we should have 470° as the point to which we should go in the superheater; and this would give only the heat of saturation at cut-off in the cylinder, or say 274°. But as 400° is known to be safe, we might still increase the heat in the superheater by 400° — 270°, or say 126°, and make the heat in the superheater 470° + 126° = 596°, so that at this cut-off 596° in the superheater would bring about only a temperature of 400° in the cylinder, giving us a total suppression of cylinder condensation, and also an increase of power $\frac{126}{274+461}$ $= \frac{126}{735}$ or about $\frac{1}{6}$th of that actually developed in the experiment in question, in which the total horse power cost 16.4 pounds of water evaporated. If the superheating had been carried to 596° to obtain $\frac{1}{6}$ greater power, we should have had $1\frac{1}{6}$th horse power for 16.4 lbs. of water, or $\frac{6 \times 16.4}{7} = \frac{98.4}{7} =$ 14.1 pounds of water nearly for a horse power. Of course we should have had the expense of superheating, but this would have been only 596° — 274° = 322° measured by steam or 322° × .475, the specific heat of steam, or say 153° measured by water. The cost of evaporating the steam being 1120°, the above 153°, representing the expense of superheating, would have been about 14 per cent. That is, at an expense of 14 per cent, we should have entirely suppressed cylinder condensation, or a loss of about 69 per cent, and have gained $\frac{1}{6}$th in power besides, or about 16 per cent.

But the experiments showed another fact; namely, that what was safe at a cut-off of 28 per cent, and necessary in order to get the best results, would be unsafe at 66 per cent cut-off, and we should then have to reduce our superheating some 74°, and so on for each different cut-off; and in this fact we find, I think, why thus far so little success has been had with superheated steam. If not sufficiently superheated, cylinder condensation is not suppressed; while if the superheating be carried too far, the engine is injured, and this last might easily happen under an engineer not informed of the facts I have just been bringing before you. He might have been working safely for a year at a cut-off of one quarter, and then, upon some exigency arising for the use of a longer cut-off, he might destroy or greatly injure the cylinder and valves if he changed the cut-off without correspondingly changing the amount of superheating.

But no rule can be given beforehand for the change in superheating to be made for a given change of cut-off. This must either be ascertained by experiment upon the particular engine used, or, what is much more convenient, the heat of the steam in the cylinder should be ascertained continually by a pyrometer placed in the clearance space, or in a cavity cut for it in the metal of the cylinder cover. An instrument of this kind is described in one of the patents recently granted me at Washington, and one is to be seen here, attached to the engine of the Mechanical Laboratory of the Institute of Technology.

ACTION OF CYLINDER PYROMETER.

It may be interesting to my audience to know how the record of this instrument verifies the truth of the foregoing statements and confirms the theory that the internal metallic surfaces act in the way suggested.

The action of this pyrometer, as influenced by the heat of the steam in the cylinder, is shown on the front of the engine, where a short pointer before the face of a graduated dial is free to move backward or forward when any change in temperature occurs, thus manifesting to the eye of the observer the relative thermal condition of the steam while it is doing its work in the cylinder. To show how effective it is in recording a difference in temperature, it is sufficient to state, that when the engine is worked with ordinary saturated steam, used expansively, the pointer makes large oscillations during each revolution of the engine, jumping forward instantly, when the opening of the admission valve allows steam to pass into the cylinder, and falling slowly backward during the expansion and exhaust. The amplitude of this motion, when the steam is cut off at one-eighth stroke, reaches more than one-half a complete turn of the pointer on its pivot, and continues thus as long as the engine runs. Its sudden jump to its maximum indication, when the hot steam enters the cylinder, is made simultaneously with the first impulse given to the indicator pencil when the steam rushes in with its high initial pressure; and the two instruments show almost exactly the same relative changes going on in the cylinder during a stroke, although the one indicates the temperature, while the other indicates the pressure of the steam. This pyrometer thus tells the story that the greater heat present in the steam at the cut-off does not superheat the expanding steam of lower pressure, but simply diminishes the condensation due to the work done and to radiation, leaving still a great variation in temperature.

From its indications, however, when the engine is working with superheated steam, we may infer an entirely different state of things. In several experiments which have been made with the use of superheated instead of saturated steam with different measures of expansion, the oscillations of the recording pointer were of very much less extent. The arc passed over by the pointer in these vibrations did not exceed 7° in any experiment, which is hardly perceptible when compared with the oscillation of 200°, just referred to in a similar experiment with saturated steam.

This shows conclusively that there was very little change in the temperature of the superheated steam in the cylinder, notwithstanding the variation in the pressure which occurred in the same way as when saturated steam was used.

There seems no other explanation than that already advanced, that the hot steam on entering the cylinder instantly gave up heat to the internal metallic surfaces, which, as the piston moved on, radiated into the steam a sufficient quantity of heat to keep its temperature nearly uniform notwithstanding radiation and work done. The evidence of this instrument corrobates the theory, and shows to the senses that the internal metallic surfaces act as an absorber and radiator, and serve as an agent to retain the heat of the steam till it is needed to prevent condensation.

The results of experiments with this instrument also clearly show the necessity of regulating the temperature of the steam in the superheater to correspond with the particular cut-off in use. An example will illustrate this point. The engine was working at a cut-off of three-fourths, a boiler pressure of seventy pounds on the square inch, and a temperature of 410° in the steam pipe. Without changing any condition in the experiment, save shortening the cut-off to one-half, and varying the load to correspond, the pointer of the cylinder pyrometer, which had previously been oscillating through an arc of 6° and showing quite constant indications, immediately fell 50° and began to oscillate through an arc of 28°; that is, the vibrations which had previously taken place quite uniformly between the points, ninety-four and one hundred, immediately after the change in the cut-off, dropped 50° and began to occur between the points twenty and forty-eight. It was necessary to raise the temperature of the steam in the steam pipe 40° or to 450° in order to bring up the indications of the pyrometer to their original figure; and when this was done the previous uniformity was afterward maintained. Again, when the cut-off was changed in a similar manner to one-fourth, there was another fall of 50° in the indications, and a similarly large amplitude of the oscillations, which continued until the temperature of the superheated steam, supplying the engine, was still farther increased 100°, bringing it up to 550°.

This demonstrated fully that the temperature of the steam in the cylinder remains constant even when the temperature in the steam pipe is increased, provided that a proper shortening of the cut-off is at the same time effected. In the experiments referred to, the superheating was carried to 410° when the engine worked at three-fourths cut-off, to 450° when working at one-half cut-off, and to 550° at one-fourth cut-off.

SUPERHEATING IN COMPOUND ENGINES.

It will be seen at once, that superheated steam is particularly applicable to compound engines, as these present us with the opportunities for superheating twice, and not only entirely suppressing the cylinder condensation, which still remains to the extent of 20 per cent even in that form of engine, but *also* of adding about one-sixth to the work of the small, and one-third to the work of the large, cylinder, or say 25 per cent to the work done by both, making a gain of about 42 per cent in power produced, to accomplish which the expense of superheating would be about 18 per cent. This is on the supposition that 400° in the cylinder is our limit. If it should prove that superheating can safely be carried to 500° in the cylinder, as it appears to have been in the "Georgeanna" experiments, or even to 470°, a still farther economy of 10 per cent purchased with three per cent of fuel will be practicable.

SECOND METHOD OF SUPPRESSING CYLINDER CONDENSATION.

Although I have not thus far stated it in so many words, it will no doubt have struck you, that a totally distinct method of suppressing cylinder condensation grew out of the investigation made to determine whether the refrigeration taking place in the cylinder was the equivalent of the whole work done, or only the equivalent of that done after the cut-off; and it will be seen that this distinct method consists in the use of superheated steam regulated at each cut-off, so as not to exceed a safe temperature *in the cylinder:* and it will be seen that by this method we not only suppress cylinder condensation, but gain other very considerable advantages growing out of the increase in the bulk of the steam, by reason of the heat added to it, over and above the temperature of saturation.

REVIEW.

Let us now review briefly the ground over which we have gone.

We have seen that *cylinder condensation* causes an immense loss in the working of steam engines, varying from 20 per cent (of the steam utilized) in the best jacketed compound engines to 142 per cent (of the steam utilized) in good, tight, single cylinder engines, well protected by lagging, but using saturated steam; that is to say, whoever is now using steam may save from one-fifth to one-half of his coal bill by simply suppressing cylinder condensation, and may save a still further percentage by a judicious application of the principle of superheating. We have seen that the *primary causes* of cylinder condensation are external radiation; conversion of heat into work; internal radiation; and conversion of heat into the work done during the first rush of the steam from the cylinder after the opening of the exhaust; and that these four causes are diminished to a slight extent by the superheating growing out of the expansion of the steam *per se.*

We have seen, moreover, that the total effect of these primary causes amounts to only a small fraction of the actual condensation observed; say to about 16 per cent out of the 142 per cent (of the steam utilized), which was seen to be lost in the last experiment of the "Michigan."

Furthermore, we see that these primary causes can only act to such an extent as they are able to chill the internal metallic surfaces which are present and acting at the point of cut-off. These alone affect the quantity of condensation; the rest of the cylinder only acting to affect the working of the steam after the cut-off.

We see that the internal metallic surfaces present at the point of cut-off must, when the engine has attained a regular action, receive at each stroke from the incoming steam an amount of heat precisely equivalent to the amount of heat lost by them through re-evaporation, plus the amount of heat lost by them through the action of the above mentioned primary causes. For if this were not the case at any particular stroke, the surfaces in question would be found upon the next stroke either hotter or colder than they were before, and a smaller or greater proportion of the incoming steam would be condensed until the equilibrium was established.

We have seen, furthermore, that the internal metallic surfaces present up to the cut-off multiply by *cumulative action* the effect produced upon them by the above-named primary causes, and that the coefficient by which the effect of these last is multiplied is equal to the amount of steam condensed by the surfaces divided by the amount which fails to be re-evaporated.

I have not then presented to you simply a mass of experiments seeming to show that cylinder condensation is very important, without accounting for it; nor have I endeavored to persuade you of its importance from theoretical considerations unsupported by experiments, but I have exhibited to you the experiments which establish the facts, and have explained how theoretically these facts can be accounted for. This being done, I think the phenomena may be accepted without reservation, and that whoever uses steam may rely upon it that he may, if he chooses, save from *one-fifth* to *one-half* of the money which he expends for coal.

Two methods of doing this have been described to you. First, that of keeping the cylinder at a heat regulated at that point at which its internal surfaces will repel the spray formed by the conversion of heat into work; second, that of superheating the steam to the degree proper for each different cut-off in such a manner as to supply the heat converted into work and to furthermore superheat to such a point as shall be without danger to the internal metallic surfaces.

Singularly enough, both of these methods have been, in a hap-hazard manner, verified by experiments, the record of which have been many years before the public.

The *first method* was verified by the experiments of Sir Daniel Gooch upon locomotive engines made in 1849 and 1850, but rejected up to the present day because they could not be accounted for, or, in other words, found to be in accordance with other known facts. They are rejected entirely by "Engineering" in an article upon "Urban Railways," dated March 13, 1874, and in a leader of March 20, 1874, because it finds that the surfaces of the cylinder were very much smaller than the superheating surfaces used for the very moderate amount of superheating now practiced in marine engines.

But when we consider that the work to be done by the cylindrical surfaces was not the re-evaporation of all the spray caused by the conversion of heat into work, &c., but simply such small portion as could be re-evaporated during the short period occupied in the stroke, and this by radiation and not by contact, the objection drawn from the practice of superheating seems altogether to fail, and the experiments remain unshaken. Any engineer, who will calculate the amount of work which can be obtained from steam of a hundred pounds pressure, expanded four-fold and very slightly superheated, will find no difficulty in crediting the results described, which were nearly equal to those obtained by many compound engines.

The *second method* has been verified by the "Georgeanna" experiments, the results of which one would have thought sufficiently extraordinary to awaken an enduring interest. They certainly did attract much attention for a time, but failed to produce a permanent effect; first, because if superheating be not carried to the point necessary to supply the heat converted into work, and the amount abstracted by radiation, it fails to suppress condensation, and the result does not repay the trouble and expense which has been incurred. For instance, in the experiments made by Mr. Isherwood, with superheated steam, upon the steamer "Eutaw," subsequent to those made upon the "Georgeanna," the best result obtained was at a cut-off of 29 per cent, and with the steam superheated to the temperature of 396°, when a total horse power cost 28.4 pounds of water evaporated per hour; and a consumption of 3.4 pounds of coal, whereas with the "Georgeanna" cutting off at 28 per cent, the total horse power was obtained by the evaporation of 16.4 pounds of water per hour, and the consumption of 2.2 pounds of coal. This last was the experiment E, shown in the table of the "Georgeanna" experiments, in which the probable temperature of the steam in the steam-pipe is shown to have been 452°. Here, then, we see an enormous difference, caused by a difference of only 56° in the amount of superheating; but I think what we have said before will enable us to account for this somewhat perplexing result. The superheating used in the "Eutaw" appears to have been sufficient to counterbalance two-thirds of the refrigerating effects produced by the conversion of heat into work, and by radiation. But, on the other hand, the internal metallic surfaces having less to evaporate, would doubtless evaporate a larger *proportion* of the water condensed at each stroke, and so increase the coefficient by which their accumulative action would multiply the remaining effect of the primary refrigerating causes. We thus see why, in the "Eutaw," a superheating of 127° reduced the cost of the total horse power only from 32.75 pounds of water to 28.4 pounds of water, and the cost in coal only from 4 pounds to 3.4 pounds; whereas, a further superheating of 51° in the "Georgeanna," brought down the water evaporated per total horse power, as already stated, to 16.4 pounds, and the coal to 2.2 pounds.

This shows, that if we would, at a short cut-off, obtain the advantages due to superheating, we must raise the temperature of the steam to that point which will absolutely suppress cylinder condensation; and I have shown that if we wish to obtain all the advantages derivable from the use of this agent, we must raise the temperature still farther, until about 400° F. of heat are maintained in the cylinder. But I have also shown that the superheating necessary to produce this effect at short cut-off would be too great to be used with a longer cut-off, as the engine would be damaged thereby; and this leads us to the second reason why superheated steam may have come so little into use. Many engines have been injured by it, engineers not being aware of the fact, above stated, that the degree of superheating must be properly regulated for each particular cut-off. Now, however, that this fact is demonstrated, and an indicator provided, there remains no obstacle to the use of the great economizer, and it will be seen that I stopped far short of a full statement when I said that whoever now uses steam may save, if he chooses, from one *fifth* to one *half* of the coal he is consuming.

The facts, and their explanation, as laid before you in this paper, are briefly stated in the patents which have been granted me at Washington, and have neither been contradicted nor questioned by the examiners at the patent office.

TABLE I.

EXPERIMENTS ON THE STEAMER "MICHIGAN" AT ERIE, JANUARY, 1861.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Cut-off in Fractions of the Stroke.	Actual Cut-off.	Pressure of the Steam at the Point of Cut-off in Pounds on the Sq. Inch.	Weight of Water and Steam at the Cut-off in Pounds per Stroke.	Weight of Steam alone at Cut-off in Pounds.	Weight of Water alone at Cut-off in Pounds.	Percentage of Water at Cut-off to Steam Utilized.	Weight of Steam at the End of the Stroke in Pounds.	Weight of Water at the End of the Stroke in Pounds.	Percentage of Steam condensed to furnish the heat converted into Work after Cut-off.
11-12	92.1 per ct	32.2	4.868	4.450	.418	9.4 per ct.	4.348	.520	.8 per ct.
7-10	71.6 "	31.4	3.928	3.380	.548	16.2 "	3.326	.602	2.8 "
4-9	47.5 "	33.0	3.261	2.350	.911	38.8 "	2.374	.887	5.8 "
3-10	33.8 "	33.4	2.865	1.696	1.169	68.9 "	1.671	1.194	8.4 "
1-4	29.1 "	33.3	2.453	1.453	1.000	68.8 "	1.481	.972	9. "
1-6	21.2 "	33.2	2.060	1.056	1.004	95.1 "	1.192	.868	11.5 "
4-45	13.9 "	33.0	1.659	.686	.973	141.8 "	.910	.749	15.5* "

* This strictly ought to have been the amount used on pages 16, 17, and 18, instead of 12.5 per cent, which, however, is the percentage of condensation due to the conversion of heat into work, after allowing for the greater heat contained in the steam at cut-off over that at the end of the stroke.

TABLE II.

EXPERIMENTS ON THE STEAMER "GEORGEANNA" IN CHESAPEAKE BAY WITH SUPERHEATED STEAM,—MADE IN 1862-63

	EXPERIMENT.	Calculated on the supposition that the thermal equivalent of the whole power developed is taken from the steam in the cylinder. A.	B.	C.	D.	E.	F.	G.	Calculated on the supposition that the thermal equivalent of the power developed after the cut-off is taken from steam in the cylinder. A.	B.	C.	D.	E.	F.	G.
1.	Actual Cut-off, per cent.	47	66	47	66	31	47	66	47	66	47	66	31	47	66
2.	Heat converted into work measured by steam.	266°	243°	301°	289°	328°	307°	288°	125°	78°	138°	88°	188°	134°	89°
3.	Deduct greater heat in high-pressure steam at Cut-off over that at the end of the stroke measured as steam.	25°	15°	25°	16°	42°	27°	15°	25°	15°	25°	16°	42°	27°	15°
4.	Difference between last two lines.	241°	228°	276°	273°	286°	280°	273°	100°	63°	113°	72°	146°	107°	74°
5.	Calculated temperature of the steam in the cylinder at cut-off.	263°	332°	353°	501°	274°	396°	489°	263°	332°	353°	501°	274°	396°	489°
6.	Sum of last two lines.	504°	560°	629°	774°	560°	676°	762°	363°	395°	466°	573°	420°	503°	563°
7.	Same as line 4.	241°	228°	276°	273°	286°	280°	273°	100°	63°	113°	72°	146°	107°	74°
8.	Calculated temperature of the steam in the cylinder at the end of the stroke.	247°	331°	358°	433°	256°	348°	438°	247°	331°	358°	433°	256°	348°	438°
9.	Sum of last two lines.	488°	559°	634°	706°	542°	628°	711°	347°	394°	471°	505°	402°	455°	516°
10.	Temperature of steam in steam pipe necessary to furnish the heat converted into power; 50° added to last line for radiation.	538°	609°	684°	756°	592°	678°	761°	397°	444°	521°	555°	452°	505°	566°
11.	Temperature of mixed steam shown by thermometer, but undoubtedly inaccurate.	....	....	344°	338°	336°	336°	322°	....	....	344°	338°	336°	336°	322°
12.	Line 11 plus 60°.	....	....	404°	398°	396°	396°	382°	....	....	404°	398°	396°	396°	382°
13.	Temperature of superheated steam shown by thermometer, but undoubtedly inaccurate.	....	....	530°	544°	474°	534°	498°	....	....	530°	544°	474°	534°	498°
14.	Line 13 plus 75°.	....	....	605°	619°	549°	609°	573°	....	....	605°	619°	549°	609°	573°

NOTE.—Experiments A and B were made with the steam chimney alone without the superheater. In Experiments A and E there was a slight condensation, and the heat necessary for its re-evaporation ~~should be added to line 10.~~ Came from the Steam Condensed.

TABLE III.

SUPPLEMENT TO TABLE II., EXPLAINING LINES 5 AND 8.

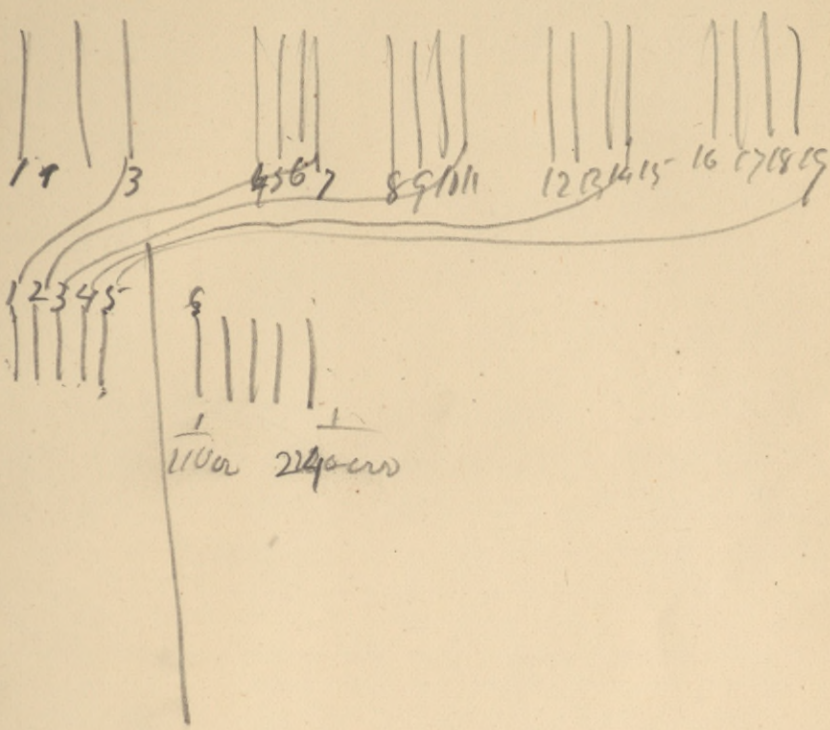
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Experiment.	Apparent Cut-off.	Actual Cut-off.	Weight of Steam passing through the Engine.	Weight of Steam at Cut-off, supposing saturation.	Temperature of saturated Steam at Cut-off pressure.	Computed Temperature of Steam in Cylinder at Cut-off.	Weight of Steam at end of stroke, supposing saturation.	Temperature of saturated Steam at the pressure at end of stroke.	Computed Temperature of Steam in the Cylinder at end of stroke.
A.	45 per ct	47 per ct	804,000	782,000	263°	263°	832,000	223°	247°
B.	65 "	66 "	823,000	914,000	253°	332°	942,000	232°	332°
C.	45 "	47 "	943,000	1,061,000	263°	353°	1,129,000	223°	358°
D.	65 "	66 "	708,000	952,000	255°	501°	918,000	229°	434°
E.	28 "	31 "	920,000	917,000	274°	274°	983,000	210°	256°
F.	45 "	47 "	623,000	738,000	262°	396°	741,000	219°	348°
G.	65 "	66 "	602,000	803,000	251°	489°	787,000	227°	438°

The above temperatures of the superheated steam in the cylinder, given in columns 7 and 10, were calculated from the following proportion:

The absolute temperature of saturated steam at the given pressure : the absolute temperature of the superheated steam, to be found = the volume of saturated steam at the given pressure : the actual volume of the superheated steam. The latter ratio = the volume of saturated steam at the given pressure × the weight of one cubic foot of that steam : the volume of the superheated steam × the same weight of one cubic foot of saturated steam; or what is the same thing, = the actual weight of the superheated steam used : the weight of that steam supposing it to be saturated.

Then, for instance,

Column 4 : column 5 = column 6 + 461° : column 7 + 461°; and in the case of experiment G., we should have 602,000 : 803,000 = 251° + 461° : absolute temperature of the superheated steam at cut-off = 712° : 950°. 950° — 461° = 489°, as given in column 7.



1 2 3
4 5 6 7
8 9 10 11
12 13 14 15
16 17 18 19
1 2 3 4 5
6