Cylinder Losses and the Adiabatic Expansion of Steam

I have read Mr. J. Clark's paper on this subject with great interest. The paper has not received the attention it deserves, but few papers do unless there is a commercial backing to them. We are, perhaps fortunately, so fully occupied with our own duties and our own immediate interests that we can spare but little time for the study of abstract propositions. Mr. Clark's paper must have involved a lot of work and it is worth careful study.

I think Mr. Clark should give us a great deal more information before he can ask us to believe that "there is little to choose so far as economy of fuel is concerned between condensing and non-condensing plants." Of course there are plants of both sorts that ought to be scrapped, and there are men in charge of good plants who ought to be scrapped too.

Mr. Macfarlane Gray, whose name and work are several times mentioned by Mr. Clark, was my next-door neighbour for some years. He had Thermodynamics on the brain. He came in frequently, and I don't think we talked of much else. Fortunately I was interested in the same subject. I helped him to prepare the paper he read before the Physical Society in 1882 or 1883. Mr. Macfarlane Gray at one time disbelieved in the co-relation of heat and energy. For some time also he

refused to accept the second law of thermodynamics, but he was a very deep thinker, and he eventually accepted and expounded both. His great idea was to simplify thermodynamics, and before he became a mathematician his papers in Engineering and the Nautical Magazine were models of lucid logic. Afterwards, well—— If any student of this Institution with the calculus at his fingers' ends and a searching mind will study Mr. Macfarlane Gray's paper on Ether Pressure and Steam of 1889, he will learn a lot that has not yet got into the text-books.

Mr. Clark makes full use of Entropy diagrams in his paper. When Mr. Gray's paper was being discussed, I pointed out that an Entropy Meter would be necessary to make the conception of practical use, and the letters on this, copies of which are appended, appeared in *Engineering*.

I have little doubt that a single stage engine can be made as economical as a two, three or four stage, and I believe it will soon be possible for a turbine engine taking steam at 20 lbs. absolute to be as economical as a reciprocating engine receiving steam of 200 lbs. pressure, taking into account first cost and repairs.

I have not seen Knoblauch and Jakobs' determination of the specific heat of superheated steam, but their results appear to be much the same as those given by other authorities, including Mr. Macfarlane Gray. The true specific heat of steam is not easy to ascertain accurately. At and near the temperature of saturation a good part of the heat that disappears may go in completing the conversion of contained moisture into steam vapour, a further quantity may go in converting steam vapour into steam gas, and at high temperatures a good deal of heat may disappear in partial dissociation.

So far as I know it has not yet been determined whether the dissociation of H_2O is a gradual or a sudden process. Ice melts into water at a constant temperature. Water becomes steam at any given pressure at a constant temperature. But does steam become free Hydrogen and Oxygen at a constant temperature ? I think not.

When several years ago I returned from one of your meetings with Mr. Macfarlane Gray and Professor Elliott I suggested to them that one of them should complete the physics of H_2O. I had devoted very many hours to the subject, but

business considerations compelled me to drop it. I shall be happy to give two guineas to any young member of the Institution who will give us a paper bringing the subject up to date.

In 1891—how time flies—I read a paper on Cylinder Condensation before the Institute of Marine Engineers which Mr. Clark might look over. I had prepared a fourth chapter of that paper based on some experiments carried out by Colonel English, R.E., at Woolwich Arsenal. These experiments were very instructive, but rather rough. And I had had it in my mind to repeat them with apparatus designed for the purpose, but was too busy to do so. If the Institution would care to have this paper I will look it up and send it in. And if the Institution will carry out the experiments I contemplated, I will give ten guineas towards the cost. I will also give a further ten guineas to the Institution if the results disprove my theorem.

When I was a student of Engineering, Technical schools were few and far between. Now they are everywhere, and the young man of the present day should know everything. Some think they do. Well, I suggest to those who do, and also to those who don't, a simple subject for their consideration in spare moments. We want a TELETHERM. In a sense we have more than one; but not in the sense here indicated. With a Teletherm such as I refer to the lower limit of temperature of a heat engine might conceivably be the absolute ZERO of the interstellar regions. Meantime it is possible that we could warm ourselves by heat drawn from the equatorial regions much to the comfort of those who live there, and of our own. I throw out the suggestion and shall be glad of Mr. Clark's remarks on it. Perhaps one of the students will give us a paper on the possibilities of such an apparatus.

<div align="right">W. H. NORTHCOTT.</div>

6, EARL'S COURT SQUARE, KENSINGTON, S.W.
April 30, 1910.

COPIES OF LETTERS WHICH APPEARED IN " ENGINEERING," REFERRED TO IN ABOVE CONTRIBUTION FROM MR. W. H. NORTHCOTT (Member).

<div align="center">

To the Editor of " Engineering."

</div>

SIR,—A communication from Lord Rayleigh under the above heading, at page 375 of last week's *Nature*, draws atten-

tion to a misunderstanding which has been pointed out by me on every occasion in the last twelve years when I have been explaining the theta-phi diagram in public, saying that " only the heat which superheated had its efficiency increased, according to the temperatures at which its respective portions were imparted to the working substances." Mr. Willans has also been disseminating correct views regarding this point
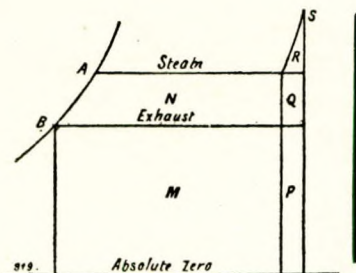
amongst those who visit his engine-testing laboratory. The diagram given by me in my paper on the theta-phi in 1880 makes this very plain. The vertical ordinates here are absolute temperatures, and the area is heat or energy. Without superheating, Carnot's law gives between temperatures A and B.

$$E = \frac{W}{H} = \frac{\text{work}}{\text{heat}} = \frac{N}{M + N}.$$

Superheating to temperature S the same law gives

$$E_s = \frac{W_s}{H_s} = \frac{\text{work}}{\text{heat}} = \frac{N + Q + R}{M + N + P + Q + R}.$$

An arithmetical expression for these quantities, practically accurate, is obtained by extending the formula given in Mr. Willan's paper on " Engine Trials " at the Institution of Civil Engineers.

A = steam temperature, not superheated.
B = exhaust temperature.
S = superheated temperature.

The temperatures are all absolute, and, to suit engineers,

in Fahrenheit measure, and the steam data of Regnault is adopted. The mean specific heat for the range of super-heating is taken $= 0.5$. This will be nearly correct at high temperatures, and this is strictly in accordance with my statement that the specific heat of steam at low temperatures is 0.39 at constant pressure. The above expressions become, without superheating,

$$E = \frac{W}{H} = \frac{\left(\dfrac{A - B}{A + B} + \dfrac{1437}{A} - 7\right)(A - B)}{1437 + 3A - B},$$

and with superheating :

$$E_s = \frac{W_s}{H_s}$$

$$= \frac{\left(\dfrac{A - B}{A + B} + \dfrac{1437}{A} - 7 + \dfrac{S - A}{S + A}\right)(A - B) + \dfrac{S - A}{2} \cdot \dfrac{S - A}{S + A}}{1437 + \dfrac{S}{2} - \dfrac{A}{2} - B}$$

Numerical example :
Say that $A = 800$ deg. $B = 600$ deg. $S = 1000$ deg.

$Es = 2301$ without superheating.
$E = 2389$ with superheating.

That is, less than 4 per cent. is gained by superheating 200 deg.

So far I support Lord Rayleigh's view, or, rather, he says what I have been impressing upon engineers for the last twenty years. If this had been all I had to say I would not have written now, but Lord Rayleigh adds to his statement what is to me an astounding announcement, that " by the addition of saline matters, such as chloride of calcium or acetate of soda . . . the possible efficiency, according to Carnot, may be increased." I hasten to call this assertion into question, because there are so many people ready to bring engines on new principles into the field of joint-stock bubbles, and I am afraid we may be having, quite apart from Lord Rayleigh, a new field engine syndicated and floated on the strength of this communication and the signature thereto before its meaning is understood.

As I understand thermodynamics there would be no gain

from superheating by a saline solution over the usual method of superheating steam raised from pure water. The saline mixture is not the working substance. Carnot's law refers to the working substance only and not to anything left in the boiler. The first step in evaporation from the saline mixture is to separate a particle of water from the salt. In the act of separation the temperature of the water particle falls to the temperature due to the pressure, and at that temperature it is evaporated into steam particles, which immediately become of the same temperature as the saline mixture. These steps are followed by every particle of water, each independently of every other particle. Of course we cannot practically test those temperatures, as the complete series is run through for each particle in a fraction of the twinkling of an eye, and immersed in a liquid of greatly higher temperature. A theta-phi diagram for this would give at B A, and extending upwards to temperature S, a very narrow figure eight whose loops are equal and drawn, as in a figure eight, one right-hand the other left-hand. The line for reception of the latent heat would be identically the same line, the horizontal through A, as when the evaporation was from pure water. It is evident, therefore, that according to my lights, the efficiency will be precisely the same as without the salt in solution.

Some ten years ago this plan was submitted to me for my opinion by an eminent mechanical engineer, Mr. S. Geoghegan, who, I understood, had then patented it. The above is the substance of the opinion I then expressed, and nothing I have learned since induces me to change my view of it now.

The " complete elaboration of this method " hinted at in the last paragraph of Lord Rayleigh's communication is not clear to my mind, and it is just possible that a few sentences of explanation would show me that I have been hitting away at something that was not intended by the writer. If so, my excuse must be that I have read the statement as every practical engineer would, to mean that the latent heat is imparted along the isothermal of the superheat. When I understand the first sentence of the last paragraph of the communication I may be able to confirm the anticipation of higher economy.

J. MACFARLANE GRAY.

London, *February* 26, 1892.

To the Editor of " Engineering."

SIR,—Mr. Macfarlane Gray's remarks refer, I imagine, to the theoretical economy derivable from using superheated steam in our old acquaintance the non-conducting cylinder. It is as well to make this clear, as otherwise Mr. Gray's opinion may deter engineers from trying to increase the economy of the real steam engine by superheating.

What *is* the specific heat of superheated steam ?

Mr. D. R. Clark (Rules and Tables, page 353) gives it at ·622. Regnault found it experimentally to be ·48. Rankine sometimes uses ·48 and sometimes ·475. Continental writers generally, I believe, use the latter. Mr. Gray, in his paper, puts it at slightly over ·388, and rules out Regnault's higher value as being due to the evaporation of watery particles. In his letter to you, however, he gives the specific heat of superheated steam at ·39 for low temperatures, and hints that Regnault's value is correct at higher. Will he kindly tell us what quantity of heat is required to raise a pound of saturated steam from, say, 100 deg. to 101 deg. Cent., 100 deg. to 200 deg. Cent., 200 deg. to 201 deg. Cent., and 500 deg. to 501 deg. Cent. ; also to superheat from, say, 1,000 deg. to 11,000 deg. Cent.?

The temperature-entropy method of calculation labours under the disadvantage of there being no entropy meter. We have thermometers for measuring temperatures, calorimeters for measuring heat, manometers for measuring pressures, volumeters for measuring volumes, dynamometers and indicators for measuring mechanical work, and all these quantities may therefore be considered as concrete quantities. Entropy, however, is at present an abstract mathematical conception only, and judging from the mathematics of Mr. Gray's paper, is likely to remain so until we get an entropy meter. Won't some inventor oblige ?

Yours truly,

W. H. NORTHCOTT.

Hatcham Iron Works, *March* 2, 1892.

To the Editor of " Engineering."

SIR,—Mr. W. H. Northcott's letter in to-day's *Engineering* states correctly that in my letter, on page 260, I referred to adiabatic conditions to which alone Carnot's law applies.

His question, What *is* the specific heat of superheated steam, has to be answered by definition and also numerically. I answer now because I see that I have not sufficiently explained the sense in which the term "specific heat" has been applied in my paper on Regnault's experiments.

As Mr. Northcott writes, an entropy meter is sometimes awanting, and when I wrote my paper I had not a convenient one. In revising it finally I made one and stuck it in on page 440, where it now reads, "F G is proportional to $p \div \theta\frac{5}{4}$." That is the entropy meter. From that we get for steam $z = [4 \cdot 3598820]\ p\ \theta\frac{5}{4}$, where the number in brackets is the common logarithm of a constant co-efficient. The pressure is in mm. mercury, and θ is the absolute temperature Centigrade. The specific heat at constant pressure employed in my paper is $\dfrac{7x}{m}$, where m is the molecular weight, and x is the fraction which $p\,v$ is of what $p\,v$ would be at the same temperature in the condition of perfect gas. The symbol z stands for $1 - x$, so that the formula above gives x as a function of p and θ. Specific heat at constant pressure as commonly understood is really

$$= \frac{7}{m}\left(x + \theta\,\frac{d\,x}{d\,\theta}\right).$$

When writing the portion of my paper dealing with the specific heat of steam, I had not worked out the formulæ for x, and I therefore neglected the second term. This omission does not affect any of the other deductions in the paper, and when inserted in regard to the specific heat experiments, it gives the specific heat at constant pressure :—

At 0 deg. Cent.	.	·39006
,, 100 deg.	.	·40668
,, 200 deg.	.	·50284
Between 100 deg. and 124 deg..	. .	·40494
,, 170 deg. ,, 281 deg..	. .	·43922

In my letter on page 260 I assumed ·5 as the mean specific heat ; I had not calculated what it would be ; I have done so just now and give the result in the last line above, between the equivalent temperatures, = ·44 say. The advantage of superheating will therefore be even less than I stated it to be,

but the formula given may be left as it is, understanding that it is a little too high. The ·5 is arithmetically convenient in the construction of the expression.

I have given the specific heats *at* certain temperatures, the rates will be slightly less if taken through a whole degree.

At 500 deg. Cent. there is no saturated steam, and at 1,000 deg. and 11,000 deg. Cent. we have to deal with dissociation, for which I have not the necessary data, I therefore cannot give Mr. Northcott the specific heats, but I do not think that he can be wanting the last one badly.

<div align="right">J. MACFARLANE GRAY.</div>

March 4, 1892.

To the Editor of " Engineering."

SIR,—The specific heat of a substance is usually regarded as that quantity of heat required to raise the temperature of unit mass 1 deg. If I understand Mr. Gray aright he looks upon unit weight of saturated steam as being composed of x gasene $+ 1 - x$ aquene (gasene and aquene are Mr. Gray's terms for ideal steam gas and ideal water respectively, and as might be expected his gasene and aquene have very unusual properties). His specific heat at constant pressure $\dfrac{7}{m} x = \dfrac{7}{17 \cdot 9598} x = \cdot38976\, x$ is the heat absorbed in raising the temperature of the gasene of the mixture only. There must, I imagine, be a temperature when x, " the fraction which $p\, v$ is, of what $p\, v$ would be at the same temperature if perfect gas," becomes equal to unity, and then I apprehend the specific heat becomes ·38976 for all higher temperatures. Indeed I gather that the specific heats of both gasene and aquene are really constant at all temperatures, when the mass is entirely in one state or the other.

The heat absorbed in converting 1 lb. of saturated steam into gasene would, I imagine, be the sum of two quantities, one being the heat absorbed in increasing the temperature of x gasene, with x increasing, and the other, the heat absorbed in converting $1 - x$ aquene into gasene.

Mr. Gray's second equation $\dfrac{7}{m}\left(x - \theta\, \dfrac{d\, x}{d\, \theta} \right)$ giving the specific heat at constant pressure " as commonly understood "

would give results, I think, much too small at temperatures close to saturation point, especially at high pressures.

Mr. Gray is no doubt aware that others besides Regnault. have found much higher values than ·389 for the specific heat of steam gas at high temperatures.

Mr. Gray tells us that saturated steam cannot exist at 500 deg. C., and I see in his paper he takes 415 deg. C. for the critical temperature. Cagniard Latour's experiment, on which this assumption is founded, was a very crude experiment, and the water had a space to expand into of about three times its own volume. Had the space above the water been less, would not the temperature have been higher ? I fancy the critical temperature, when carefully determined, will be appreciably above 500 deg. C.

The 11,000 deg. C. in my letter should have been 1,100 deg. C.

Yours truly,

W. H. NORTHCOTT.

Hatcham Iron Works, *March* 16, 1892.

To the Editor of " Engineering."

SIR,—At page 362 Mr. Northcott's " x gasene $+ (1 - x)$ aquene " is not my view. Saturated steam is H_2O, perfectly evaporated, and at the maximum pressure or minimum volume consistent with its temperature. Before my 1889 papers it had been universally accepted that the defection in proportionality of $p\,v$ to absolute temperature was altogether due to incomplete evaporation. In my 1885 paper I had worked upon the same mistaken conception, and therefore I then failed to reconcile the ether-pressure theory with the second law. Referring to this, my 1889 Institution of Naval Architects' paper states in the introduction : " In the latter paper it was mentioned that he could not then reconcile the ether-pressure theory and the second law of thermodynamics with the then universally accepted view of what non-perfect gas is. Since that date he has discovered that that view of gas is what has to be modified ; and then the second law stands in perfect harmony with the ether-pressure theory." In the 1889 Institution of Mechanical Engineers' paper the object was to prove that " 1. In any change of molecular aggregation of matter, as from liquid to vapour or gas, the splitting heat

or energy of segregation is a constant quantity at all temperatures, if the change of aggregation is the same. 2. The heat or energy communicated to a gas, to change its $p\,v$ product by any small amount, is for constant volume $2\frac{1}{2}$ times its change of $p\,v$, and for constant pressure $3\frac{1}{2}$ times its change of $p\,v$."

According to these enunciations elaborated in the paper, evaporation is always complete, unless where mechanical causes produce priming, which is accidental and not a necessary accompaniment of evaporation. In the Mechanical Engineers' paper I had to suppress the details of the reasoning, whereby I deduced from my conception of metafilm the relation between z and p along an isothermal. The ideas were altogether too revolutionary, although my friend Mr. Willans thinks this the very best part of my work. I had to content myself with the creation of argules and ergules, adding, however, that " the author desires very emphatically to state that the ideal conditions of the molecules—as forming two classes, one at rest and the other in motion—are not supposed by him to be the actual conditions. It is not meant that certain molecules are really uniformly active, and the remainder absolutely idle ; but only that, in respect of pressure, volume, and temperature, the thermodynamic states in steam, according to these experiments, are the same as they would be if unit mass were made up of x uniformly active molecules or ergules, and z absolutely inactive molecules or argules. The root *erg* signifies activity and the root *arg* idleness." Had I been writing this now I might have coined the latter name from a Northumbrian root. Mr. Willans thinks it was an error to disguise my metafilm explanation. However, I must make it clear to Mr. Northcott that in my view saturated steam is a homogeneous elastic fluid, all its molecules averagely uniformly conditioned. That the x of saturated steam diminishes as the temperature increases is due to the increasing coincidence of metafilm as the volume is diminished. I do not intend in this communication to fully discuss this matter ; having seen Mr. Northcott's letter now, I wish to reply to it at once, and not be spending another evening over it.

In the No. 2 enunciation quoted above, I explain what the specific heat is. It is the energy required to change one $3\frac{1}{2}\,p\,v$ into another $3\frac{1}{2}\,p\,v$; it is, therefore, $3\frac{1}{2}$ times the rate of change of $p\,v$ per degree. This is for dry saturated

steam and steam gas. This includes the second term in $\frac{7}{m} (x \, d \, \theta + \theta \, d \, x)$.

The variation of x is not according to temperature only. Indeed, if the variation of temperature be produced mechanically, that is, under adiabatic conditions, x does not vary for any change of temperature. Along an isothermal x varies inversely as the pressure. The latter statement has been confirmed experimentally by Thomson and Joule, I mean Kelvin and Joule, in their porous plug experiments. Along the graph of saturation x diminishes with increasing temperature, so that at a certain moderate temperature, 216 deg. Cent., the total heat of steam has a maximum value. That is, above 216 deg. Cent., the hotter saturated steam is the less heat there is in it. This is at variance with the usual expression for total heat in steam, but a glance at Regnault's report of the total heat obtained experimentally at high temperatures shows that the usual expression does not fairly represent these results, and that the figures given in the report are likewise inconsistent. The specific heat of steam gas is ·38976, and $x =$ unity when its volume is supremely great, and also when its temperature is insignificantly low.

In saturated steam there is no $(1 - x)$ aquene. If we work on the arithmetically representative conception, argules, these must be regarded as segregated molecules of H_2O at the temperature of absolute zero. As x increases, so many of them must be endowed with the $\frac{7}{m} \theta \, d \, x$ of energy.

Why does Mr. Northcott " think " that the $\frac{7}{m} \left(x + \theta \frac{d \, x}{d \theta} \right)$ would give results much too small at high pressures near to saturation point ? I have given in my last week's letter on this subject the specific heat of steam gas at constant pressure when just starting from 200 deg. Cent. as ·50284. It is only quite near to the saturation graph that these high results are found. My investigations are therefore not inconsistent with higher specific heats than ·389.

As to the critical temperature, Mr. Northcott thinks Caignard Latour's determination 415 deg. Cent. too low, and he fancies that " when carefully determined it will be appreciably above 500 deg." I have no " fancy " for it to be either higher or lower, but I have calculated that it must be considerably

below 496 deg. Cent., and it has been " carefully determined " by MM. Cailletet and Collardeau experimenting with a mercury column reaching far up the Eiffel Tower to be at 365 deg. Cent. See *Comptes Rendus* for May 25, 1891. The pressures at the same time ascertained for the various temperatures agree with my investigations accurately throughout. I have written this only to convey information, and without any desire to combat the views of other people.

J. MACFARLANE GRAY.

March 18, 1892.

CRITICAL TEMPERATURE OF WATER

To the Editor of " Engineering."

SIR,—In your issue of March 25, 1892, Mr. J. Macfarlane Gray, in his letter on " Superheated Steam," notices that the critical temperature of water has been carefully determined by MM. Cailletet and Collardeau (*Comptes Rendus* for May 25, 1891) and found to be as low as 365 deg. Cent. I beg to say that some years ago M. Nadejdin, late physicist at Kiel, made a series of experiments on the same subject, by means of an original apparatus, invented by himself, and found the critical temperature of water to be at 358 deg. Cent., and the critical volume equal to 2·33 volumes of water at 0 deg.

Yours truly,

KOTURNITSKY,

Professor of Applied Mechanics at the Technological Institute in St. Petersburg.

HEAT ENGINES AND SALINE SOLUTIONS

To the Editor of " Engineering."

SIR,—The subjoined extract from Lord Rayleigh's letter on page 510 of *Nature* for last week will be interesting to those who have read my letters on the above subject. The definition of the phrase " superheated vapour " is very important from such an authority. The hint regarding the direction in which improvement in heat engines is likely to be attained will be useful to those of your readers whose expectation of life still includes the " distant future."

" Let me suggest that the origin of the difficulty may lie in the phrase ' superheated vapour,' which has not so definite a meaning as Mr. Gray seems to ascribe to it. Whether vapour be superheated or no, depends, not only upon the condition of the vapour itself, *but also upon the bodies with which it is in contact*. Vapour which is merely saturated in contact with a saline solution must be regarded as superheated when contact with the solution is cut off. In the first situation it would condense upon compression, and in the second situation would not.

" In conclusion, I will hazard the prediction that, if the heat engines of the distant future are at all analogous to our present steam engines, either the water (as the substance first heated) will be replaced by a fluid of less inherent volatility, or else the volatility of the water will be restrained by the addition to it of some body held in solution."

<div align="center">Yours truly,</div>

<div align="right">J. MACFARLANE GRAY.</div>

REPLY FROM MR. J. CLARK.

I have read Mr. Northcott's remarks with much pleasure. It seems to me that the scope of the observation referring to the relative economy of fuel between condensing and non-condensing plants is somewhat widened by the way Mr. Northcott puts it. Primarily it referred to Electric Power and Light Stations as stated, and the returns from these as published amply confirms it. As, however, Mr. Northcott seems to desire me to be more specific I would beg to refer him to Mr. Henry McLaren's paper read at Leeds in 1903 before the Institution of Mechanical Engineers " On Economy of Fuel in Electric Generating Stations." Among other particulars he gave the following actual results—22 Metropolitan stations' generating current for lighting amounting to 76,638,613 units (sold) in all. These stations consisted of non-condensing, partly condensing, and condensing, and their outputs were respectively :—

Non-condensing	.	.	.	24,014,141 units sold.
Partly condensing	.	.	.	11,305,581 ,, ,,
Condensing	.	.	.	41,318,891 ,, ,,

Mr. McLaren goes on to say, " these stations are fairly comparable ; they are all generating current for lighting purposes and pay London prices for coal and labour. In comparing the cost of these three types it is found that the non-condensing stations produce current at 18 per cent. and the partly condensing 21 per cent. less cost than the condensing stations. *For fuel only* the saving in favour of non-condensing and partly condensing plants is about 14 per cent. and 17 per cent. respectively."

In another part of the same paper referring to losses incidental to condensing, Mr. McLaren says, " It is not so surprising that under these conditions the economy due to condensing reaches the vanishing point or even falls 13 per cent. behind well equipped non-condensing stations, as shown by the tables."

Many other and similar facts are available confirming the statement made in the paper. The spirit of the remark was intended to emphasize the importance of the fact that all savings or economies cost something to obtain, and economies by condensing are only to be obtained by expending a considerable amount in some cases, and the whole or more than the whole of the " saving " in other cases.

Experience seems to prove that Mr. Northcott's remarks are not far from the truth regarding the relative economy of the turbine supplied with steam at 20 lbs. absolute pressure against the reciprocating engine with steam at 200 lbs. pressure, quite apart from any question as to capital and upkeep charges. Assuming an overall thermodynamic efficiency of 60 per cent. for both types, the reciprocating condensing engine working on a vacuum of 25 inches should produce a unit of electricity for 19 lbs. of water, whereas the turbine working on a 29-inch vacuum would require 25 lbs. per k.w. hour. Although the difference amounts to 30 per cent. on paper, the turbine's steam consumption is likely to remain constant after a long period of hard work, whereas there are numerous instances to the contrary in the case of the reciprocating engine, and these prove how easily the initial difference, large as it is, between these two prime movers can be virtually thrown away.

I have just had the pleasure of reading Mr. Northcott's paper on Initial Condensation ; this paper is highly interesting, and I feel sure Chapter IV on the same subject would

prove of great value and be highly appreciated by our members.

Could Mr. Northcott not also see his way to add a chapter on the abstruse subject of his Teletherm ?

J. CLARK.

Leytonstone, *May* 20, 1910.