The Efficiency
OF
AIR COMPRESSORS.

THE subject dealt with in this paper is one which is exciting considerable interest at the present time among Engineers and Capitalists, both here and on the Continents of Europe and America, where very large schemes for power distribution through the medium of compressed air, are either at work or under consideration; while the large and increasing number of air compressors fitted on board ships for refrigerating purposes accounts for the interest evinced in this subject by those Marine Engineers who are responsible for the efficient working of the apparatus, or by those superintendents and shipowners who are called upon to decide on the respective merits of the machines constructed by different makers.

But refrigeration, though prominent on shipboard, is only, perhaps, a minor one of the very many purposes to which compressed air has been applied. In Paris and Birmingham very large installations of air compressing machinery are at work for supplying motive power for many purposes where steam or water motors can be utilised, and for others where these agents are inadmissable, such as glass cutting, tunneling, mining, sending messages, &c.

The cleanliness and freedom from the dangerous effects following the explosion of vessels or pipes containing steam, tends to predispose one in favour of an agent of such a comparatively harmless nature as compressed air, and this predisposition becomes intensified in those who have had the opportunity of observing its behaviour and general adaptability under the widely different circumstances obtaining in the various processes carried on in the grimy workshops of Birmingham, or in the palatial hotels and elegant mercantile emporiums of the French capital.

In dealing with the possible efficiency of compressed air as a power distributing medium I cannot refer for support to my views to any recognised authority who has written or spoken on the subject and whom my limited opportunities have enabled me to consult. I have therefore deemed it necessary to deal somewhat at length with elementary relations which I hope will make it apparent to you that my conclusions are based on principles generally accepted as established by men whose reputations as scientists are world wide, and includes names like Regnault, Balfour Stewart, Professor Tyndall, and many others who have made a speciality of the study of those manifestations of heat energy which are included in that branch of science called Thremo Dynamics.

It is no part of my object to convey the idea that any antagonism exists between any two authorities concerning the principles of this science, but I want to make it clear, that the present practice--sanctioned by many authorities—of regarding the heat generated in an air compressor as an evil to be minimised as much as possible by injecting water, and by other means of abstracting the heat without utilising it--is antagonistic to the realisation of the end sought to be obtained, which is, the most economical delivery of power at a distance from its point of generation.

Many of the constant numbers used in scientific formulæ are not entitled to be regarded as absolutely exact, but are—like the most familiar of them to Engineers, Joule's equivalent—deduced from experiments and calculations conducted with very great care, whose results, while they closely approach each other, are not always absolutely identical.

So many of the constant numbers used in formulæ employed in calculations relating to air, differ slightly in value owing to discrepancies observable in the results obtained by different experimenters. The number indicative of absolute zero is one of these; those numbers expressing the value of the specific heat of air at constant pressure and constant volume are others ; in this paper those values are adopted which I have reason to think are in the most general use, or are in accord with the most recent investigations.

Absolute zero is assumed to be 459° below the zero of the Fahrenheit thermometer, as I used 461° for the number expressing absolute zero on a previous occasion, I may explain that it was then used as adopted by Professor Rankine in his well-known work on the Steam Engine, but the later researches of Regnault and Magnus shew that the increase of space occupied by a portion of dry air heated from 0° to 100° Centigrade is more correctly expressed by the proportional numbers 1·3665 : 1 than by 1·365 : 1, which was the ratio of increment adopted by Rankine and based on the earlier experiments of Rudberg and Regnault. This correction makes the co-efficient of expansion 0·003665 per Centigrade degree, and absolute zero is therefore $\frac{1}{003665}$ = 272·85° Centigrade or 491·13° below the freezing point on the Fahrenheit scale; these numbers are taken as 273° Centigrade or 491° Fah.

The specific heat of water being 1·000, the number expressing the same relation to air at constant pressure is taken, as given by Rankine, as 0·238 and as 0·169 at constant volume, as these numbers are convenient for calculation, and their ratio to each other agree fairly well with the calculated ratio of the two specific heats deduced from experiments on the velocity of sound in air carried out by M. M. Bravais, Martins and others. Professor Balfour Stewart, pursuing this method of investigation, confirms the accuracy of these experiments by shewing that the calculated ratio of the specific heat of air at constant pressure is to that at constant volume as 1·408 is to 1·00. The value of this ratio is usually expressed by a symbol, the Greek letter γ (*Gamma*), and is constantly used in calculations relating to compressed air. If it can be proved that the intrinsic energy in a gas is $2\frac{1}{2}$ times its PV, then the value of γ (*Gamma*) *must* be 1·4 exactly.

Regnault found by experiments conducted in Paris, that a portion of dry air at 0° Centigrade or 32° Fahrenheit which occupied one cubic decimetre when subjected to an absolute pressure of 14·7 lbs. per square inch represented by a column of mercury 29·922 inches high, weighed 1·29318 gramme or 19·9568 grains, as a cubic foot contains 28·3153119 cubic decimetres, it will weigh 0·080726 lbs.; and one pound of air will occupy 12·38756 cubic feet under these conditions; the volume at any other temperature has been found to vary in direct proportion to its absolute temperature when the pressure remains the same.

By making the necessary correction for the increased force of gravity, due to difference of latitude, it is found that the absolute pressure represented by 29·922 inches of mercury at Paris (latitude 48° 50′) is represented by 29·914 inches of mercury at Greenwich (latitude 51° 28′). The weight of a cubic foot of air at Greenwich under a pressure of 29·922 inches of mercury and 32° Fahrenheit would, therefore, be as 29.914 : 29·922 :: ·080726 : 080728 lbs. This is the weight adopted by Professor Rankine, but the former will give the nearest approximation to the weight of a cubic foot of dry air at the given temperature under a pressure of 14·7 lbs. per square inch absolute.

It has been experimentally ascertained that the volume which a perfect gas occupies varies in inverse proportion to its pressure when the temperature is constant. This is generally known as Boyle's law, and may be formulated thus:—Let P_1 V_1 denote the pressure and volume of a certain quantity of perfect gas, like dry air, at a given temperature, and let this become P_2 V_2 for the same weight of gas at any other pressure and the same temperature, then $P_1 \times V_1$ will equal $P_2 \times V_2$.

Gay Lussac found that, for each degree of heat, a gas expanded a certain fixed proportion of its initial volume at 0° Centigrade, or 32° Fahrenheit. Let $C =$ a constant expressing this increment of volume for each degree of heat, and $V_1 =$ the volume of the gas at 0° Centigrade, then its volume at any higher temperature T, at the same pressure, may be expressed $V_2 = V_1 + CT$.

Charles's law teaches that this co-efficient of expansion is the same for all permanent gases. The value of C for air, I have before stated, was found by Regnault to be ·003665 for each Centigrade degree, or ·0020366 for each degree Fahrenheit. Combining the equations formulating these two laws, we have $P_2\,V_2 = P_1V_1\,(1+CT)$ which is a general expression for the relation existing between the pressure, temperature, and volume of a perfect gas under given conditions.

Pure air is composed of the two gases, oxygen and nitrogen, in the proportion of 21 volumes of oxygen to 79 volumes of nitrogen (very nearly) or a portion weighing one hundred will contain 23°/o of oxygen and 77°/o of nitrogen. Free atmospheric air, however, always contains traces of carbon dioxide and water vapour in addition to the above gases, and the mixture is described as mechanical because the constituent gases are not chemically united like oxygen and hydrogen, for instance, in the formation of steam.

There are several reasons for the inference that no chemical union takes place between the two gases comprising air; one of the most important distinctions, perhaps, between those compounds whose constituents are mixed mechanically, and those which are combined chemically, lies in the fact that the act of chemical combination, or dissociation, involves the creation of, or disappearance of heat, while a mere mechanical mixture, or separation at constant pressure, is unattended by an alteration of temperature. It is not certain that this absence of chemical action would continue at extreme temperatures either very high or very low, indeed the fact of air having been liquified may be said to point to a different conclusion.

It is considered very probable that all forms of matter, whether we know them as gaseous or liquid, within our ordinary range of observation would be solidified at the temperature denoted by absolute zero. Within recent years, both atmospheric air and its constituent gases, oxygen and nitrogen, with which we are so familiar in their third or gaseous state, have been reduced to the liquid state by the conjoint application of intense cold and great pressure.* Some experiments of M. Raoul Pictet point to the conclusion that the boiling point of liquid oxygen is about 234° below the freezing point of the Fahrenheit scale under a pressure of three tons per square inch; we may therefore regard air as the highly superheated vapour of a liquid somewhat similar in its nature to dry steam; and it is probable that very slight differences in the physical characteristics of either steam or air, would be apparent at observations taken at the same pressure, and at a temperature equally remote from the boiling point of their respective liquids for that pressure.

The intrinsic or absolute energy contained in a portion of air is a function dependent on its temperature only, and quite independent of its pressure. The approximate formula expressing this energy $E = \frac{PV}{\gamma - 1}$ when PV is the absolute pressure per square foot, multiplied into the volume occupied by the air in cubic feet. As the value of V changes simultaneously in inverse ratio to the pressure when the temperature is constant, the value of E remains unaffected by a change of pressure, or, to make this clearer, I may say that no more work is theoretically attainable from a given weight of air exerting a pressure of 100 lbs. per square inch than from the same weight exerting a pressure of 20 lbs., if the temperature is the same in both cases,

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* See "Tyndall's Heat a Mode of Motion."

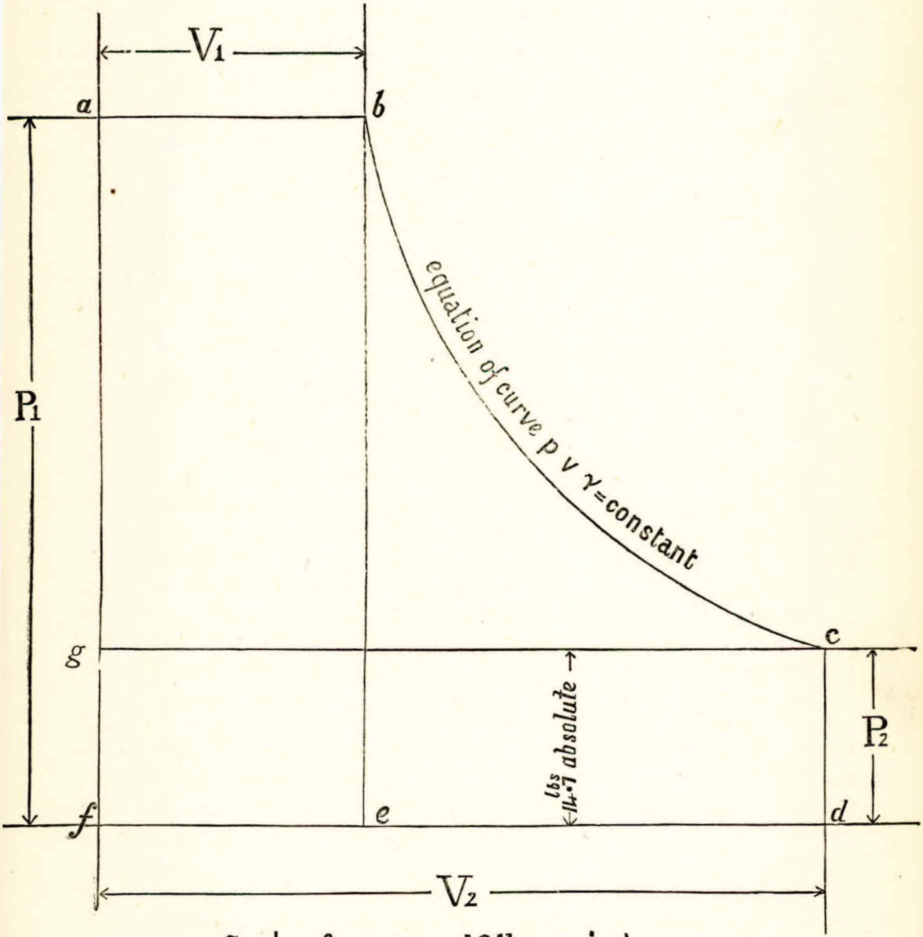
and assuming the air to remain gaseous throughout the whole range of the absolute thermometric scale. The available or useful energy, however, varies considerably with changes of either pressure or temperature, or both. When the air is not worked in conjunction with a condenser its available capacity for useful work ceases when its pressure differs only slightly from that of the surrounding atmosphere.

The formula expressing in foot lbs. the available energy of compressed air admitted to a cylinder and expanded adiabatically down to the pressure of the atmosphere reads thus:— $E_a = (P_1 \ V_1 - P_2 \ V_2) \frac{\gamma}{\gamma-1}$. Substituting for $\frac{\gamma}{\gamma-1}$ its value as given by Rankine the formula reads, $E_a = (P_1 \ V_1 - P_2 \ V_2) \ 3{\cdot}451$ when $_1$ and P_2 represent the absolute pressures in lbs. per square foot and V_1 and V_2 the volume occupied by the air at the different pressures. The only unknown factor in this equation is V_2 and may be found thus: Let $\frac{P_1}{P_2} - R_1$ be the ratio of the initial to the terminal pressure, then the equation becomes as under:--

$$V_2 = \frac{V_1 \ R_1}{R_1^{\frac{\gamma-1}{\gamma}}} = \frac{V_1 \ R_1}{R_1^{\ 0{\cdot}29}}$$

Let us take a numerical example and assume the pressure of the air to be that usually obtaining in refrigerators on board ships and in the air motors working in the mines of this country and elsewhere, which we may take at 45 lbs. gauge or 59·7 lbs. absolute, substituting numerals for the letters in the formulæ we have with these conditions $V_2 = 2{\cdot}7049$ and for one cubic foot:— $Ea = (59{\cdot}7 \times 144 - 14{\cdot}7 \times 144 \times 2{\cdot}7049) \ 3{\cdot}451 = 9908{\cdot}2$ foot lbs. of work theoretically obtainable from each cubic foot of dry compressed air. I have calculated the values according to this formula and tabulated them at the end of this paper for reference in dealing with pressures from 5 to 150 lbs. above the atmosphere, rising in stages of 5 lbs. per square inch. This 9908·2 foot lbs. is the theoretically available energy of one cubic foot of air *at any temperature* supplied to a cylinder at the higher pressure and then expanded adiabatically from a gauge pressure of 45 lbs. down to atmospheric pressure.

The annexed (Fig 1) will serve to show the derivation of the formula. Suppose it to represent a theoretical air diagram or plane figure with an adiabatic curve whose index is $P \ V^{\gamma} =$ a constant, that is, the pressure measured at any point on the curve varies inversely as the γ power of the number representing the space occupied by the air, then it can be proved with the aid of the integral calculus that the formulæ $\frac{P_1 \ V_1 - P_2 \ V_2}{\gamma - 1}$ represents the area b c d e, or the gross work done by the air while expanding adiabatically from P_1 to P_2; the rectangular area c d f g represents the product $P_2 V_2$ and is equal to the work done against the atmosphere during exhaust. The area a b e f is equal to the product $P_1 V_1$ and represents the work done by the air during its admission to the cylinder before cut off, or, more strictly speaking, it represents the work done during



Scale of pressure=16lbs per inch

Fig. 1.

its expulsion from the compressor which may be miles distant from the expansion cylinder under consideration.

This energy exerted before cut off, is additional to, and must not be confounded with that portion of the intrinsic energy which the air possesses in virtue of its temperature, and only parts with during its cooling or expansion. The whole area of the diagram a b c d f represents the total work done in the cylinder and may therefore be formulated, thus: $W = \frac{P_1 V_1 - P_2 V_2}{\gamma - 1} + P_1 V_1$. To get the effective work represented by the area a b c g (which I have called the available energy in the tables) we must deduct that represented by the resistance of the atmosphere, or the area c d f g so that the nett work done in the cylinder or the *available* energy is:—

$$= \frac{P_1 V_1 - P_2 V_2}{\gamma - 1} + P_1 V_1 - P_2 V_2 = \left(\frac{P_1 V_1 - P_2 V_2}{\gamma - 1}\right)\gamma = \left(P_1 V_1 - P_2 P_2\right)\frac{\gamma}{\gamma - 1}.$$

The converse of this equation is strictly true under similar conditions, and under no circumstances is it possible to compress a given volume of air from the pressure of the atmosphere, represented by P_2 in the equation, and expel it from a cylinder against a pressure represented by P_1 without the expenditure of energy theoretically equivalent to, but practically always in excess of, that which a volume of air, occupying the *same space after expanding* to the pressure of the atmosphere, can give out during its admission and adiabatic expansion from P_1.

The first law of Thermo Dynamics is thus expressed by Professor Rankine :— " Heat and mechanical energy are mutually convertible ; and heat requires for its production and produces by its disappearance, mechanical energy in the proportion of 772 foot lbs. for each British thermal unit of heat." The working of air compressors furnish striking examples of this law. During compression the visible mechanical energy exerted through the medium of the piston rod on the air is converted into its exact equivalent of invisible heat energy so that when the piston reaches that part of the stroke corresponding to the highest point of the compression curve on the diagram, the mechanical equivalent of the rise in temperature of the air will equal the foot pounds of energy expended during compression.

It was pointed out elsewhere that, when air is heated, it expands in volume or increases in pressure in a certain fixed proportion for a given rise of temperature. Under the conditions obtaining in a compressor the heat imparted manifests itself by increasing the pressure so that the pressure required to lift the discharge valves is attained much sooner with adiabatic compression, than would be the case if the heat imparted could be abstracted as fast as it was created. In this latter case the pressure would rise in accordance with Boyle's Law, PV = a constant. This is the present ideal standard of efficiency sought after in a compressor, and any rise in the temperature of the air is usually regarded as involving a direct loss of power which the advocates of pneumatic power distribution admit to be a necessary evil, while its opponents (who are sometimes interested in other systems) point it out triumphantly as a fatal defect. I think these views are unjustifiable and that this heating should be regarded in a totally different light as one ef the most important advantages possessed by air, as a power distributing medium over any of its rivals, such as. water, for instance.

My reason for this conclusion is that *it is theoretically possible to obtain a larger return of mechanical energy from compressed heated air than we expend in producing the rise in temperature.* This assertion appears at first sight to be at variance with the firmly established law of the conservation of energy, but on further consideration it will be found to involve the acceptance of nothing novel in Thermo Dynamic theory. Before attempting to prove this, it may be as well to shew that the necessity for such proof exists by quoting the opinions expressed by some of the recognised authorities on the subject.

Professor Unwin, in a paper read before the Institute of Civil Engineers (see Proc. Vol. 93), summarised its contents in these words:—"Broadly, it would seem that the resultant efficiency of pump, main, and motor, is likely to be somewhat lower for air than for water unless, the heating loss in the compressing pump can be diminished more than it has hitherto been by cooling arrangements. On the other hand air has considerable advantages in some other respects, especially that with a low initial pressure a greater quantity of power can be transmitted through a main of a given size than with water."

At the Newcastle meeting of the British Association, 1889, Professor Kennedy read a most valuable paper dealing with the system of power distribution by means of compressed air in Paris (See *Engineer*, September 28th, 1889). In this paper he is reported to have said " The air is drawn in direct from the engine house where I found it to be about 70° Fah. and after it has finally passed along the mains for some little distance it is again about the same temperature. It is, therefore, of the greatest importance to prevent its temperature rising during the compression, as all heat taken up by the air represents work done in the steam cylinders of which no part whatever can be utilised. If the air were compressed adiabatically, *i.e.* without any cooling whatever, its temperature on leaving the compressor would be about 430° Fah.—a temperature higher than that of saturated steam of 300 lbs. per sq. inch pressure. At St. Fargeau, water for cooling is allowed to run into the cylinders through the suction valve during the suction stroke in such quantity that the final temperature is only 150° Fah. So far the result is satisfactory enough; but owing, unfortunately, to the particular way in which the cooling water is utilised mechanically, the air does not get cooled until after it has been compressed, so that, practically, no benefit is obtained from the cooling in spite of the extent to which it occurs. The power expended, as we shall see presently, is practically equal to what would have been expended had the compression been adiabatic."

Mr. Isaac Shone, C.E., F.G.S., whose name may be familiar to many of you as having successfully dealt with the sewage of Henley and other places by his system of hydro-pneumatic drainage, published tables some time ago which would be of great value to the producers and users of compressed air if they were reliable. But the mean pressures of adiabatic expansion given in column N (see Shone's tables) are more applicable to dry steam than compressed air. In a footnote appended to these tables Mr. Shone says "It is obviously impossible to give a hard and fast rule for obtaining the practical mean load on a compressing piston, but for safe and approximate estimate, it may be taken as being an average of the loads given in columns N and O opposite to any desired pressure." The figures given in the columns N and O are intended to indicate respectively the mean pressure per square inch throughout the stroke in an air compressor during adiabatic and during isothermal compression.

I believe that Mr. Shone's tables have been freely used in the manner suggested by their author by many of those who have had to design air compressors and to estimate the effort to be exerted in the steam cylinders. In the tables referred to (Mr. Shone's) the mean pressure due to adiabatic compression from 14·7 lbs. into a receiver at 114·7 lbs. absolute, is given as 50·878 lbs. per square inch effective on the compressor piston. Referring to the table I have prepared, you will find that I estimate it at 41·318 lbs.; but combining the adiabatic and isothermal pressures as given by Mr. Shone, results in a mean pressure of 40·536 lbs., which is not far removed from my own estimate of the minimum resistance to be overcome by the steam.

I will only trouble you with another quotation, but as this authority, Mr. W. Donaldson, M.A., M.I.C.E., is the author of the most recent work published in this country which deals at length with compressed air, besides being the author of half-a-dozen other technical works, I may fairly conclude that his opinion with that of the other authors I have mentioned is generally representative on the subject.

He says "A very near approach to isothermal compression may be effected by injecting sufficient ice-cold water into the cylinder of the compressor. The utmost use ought to be made of this for securing the utmost possible approach to isothermal compression. The diagrams taken during partial isothermal compression, or the saving in the work done in the air cylinder effected by cooling for absolute pressures varying from $1\frac{1}{2}$ to 15 atmospheres will vary from about 5 to 16 per cent."

These authorities appear unanimous in the opinion that the heat developed in the air during compression involves the greatest loss, and is the primary disadvantage which can be urged to discount the many unquestioned advantages attending the use of compressed air as a power distributing medium. This is a conclusion I cannot agree with, and if a fractional part of the time and money which has been expended in consequence of this idea had been employed in utilising the heat imparted to the air during its compression, instead of dissipating it uselessly in the atmosphere, or throwing it away into the sewers, the use of such a very convenient agent as compressed air would be much more general and would not furnish so much ground for speculative debate as to its relative efficiency as a power distributing medium as it does at present.

Although it is easy to imagine better examples, let us take as an existing one, that furnished by the Parisian compressors alluded to by Professor Kennedy. It is scarcely necessary to combat the assertion that no portion of the heat contained in a volume of air with a temperature range of 360° Fah. above that of the atmosphere can be utilised; many ways of utilising some portion of this heat may be suggested, such as heating feed water at the central station, or heating the air required for the combustion of the fuel in the furnaces, it could also be used to impart heat to a low pressure boiler, or to a high pressure one containing a more volatile fluid than water similar to the petroleum spirit employed by Mr. Yarrow in "launch" boilers. Suppose we employ the most suitable of these methods for the purpose of reducing the range of temperature to 100° above that of the atmosphere or to 170° Fah.

while the pressure is kept constant at 6 atmospheres, we may then admit it to a cylinder and further reduce its temperature to about 15° below that of the atmosphere, by making it perform work on a piston while expanding down to half its pressure or 44.1 lbs. absolute before it leaves the central station, and we may imagine this engine to exhaust into the power distributing mains so that on leaving the station the whole of the heat generated during its compression has been utilised while the 5979 foot lbs. available energy contained in each cubic foot of air in the supply pipes includes about 1750 foot pounds of intrinsic energy previously existing in the atmosphere which has been imported *without any theoretical expenditure* of steam power, and this surplus energy may be drawn upon when estimating the practical efficiency of the system to supply the losses inseparable from the working of all machinery.

A much greater saving could be effected with simpler appliances if a higher initial ratio of compression is resorted to, and I should anticipate no formidable difficulty to be overcome in designing or working a compressor to raise the temperature of the air to 500° Fah. corresponding with a pressure of 10 atmospheres, and it is theoretically conceivable that by compressing the air in successive stages, and utilising the heat between each stage, that the whole of the work done at the central station may be used to overcome friction and other losses, while *the whole of the power transmitted to consumers is drawn from the inexhaustible storehouse of intrinsic energy which is ever present in the surrounding atmosphere.*

I have before stated that a certain proportion of water vapour, or steam, is always present in free air, and its effect in the compressor tends to reduce the final temperature due to adiabatic compression owing to the higher specific heat of the vapour. Its influence, however, in this direction is too slight to materially affect the air diagram, so that its presence may be ignored in estimating the temperature due to compression when the initial temperature does not exceed that of the atmosphere. When the air is cooled almost to its initial temperature, after it is compressed, before being used expansively as in refrigerators, the moisture contained in the air has a much more important effect on the final temperature in the expansion cylinder than it had in the compressor, as it not only parts with its sensible heat, but gives up the whole of the latent heat of the vapour as well as that of the water before it turns into ice, and although this tends to increase the amount of power returned, it tends at the same time to defeat the primary object of a refrigerator—the extraction of heat from the air—besides giving rise to the annoyance arising from the accumulation of snow in the discharge passages. The proportion of moisture which the air can contain increases rapidly with its temperature, thus at a temperature of 53° Fah. the maximum weight of moisture in a cubic foot of air is 5 grains, while it can contain over 10 grains at a temperature of 76° Fah., showing in this case 100 per cent. increased capacity for moisture with a rise of less than 4½ per cent. in its absolute temperature. This capacity is solely determined by the temperature, and is unaffected by the pressure of the air, so that 10 cubic feet, say, of free air compressed to one-tenth of its bulk can only sustain one-tenth of the moisture it was capable of holding in suspension when free at the same temperature (see Rankine, page 240). Hence the great advantage of cooling and thereby drying the air to a considerable extent before expanding it, when refrigeration is the object in view.

Suppose we take 76° Fah. as representative of the temperature of the compressed air entering the expansion cylinder after being cooled as far as is practicable in ordinary work (in the Tropics it will, of course, be more than this), then the 10·1 grains of moisture contained in each cubic foot of saturated air will part with 1·8 unit of heat before solidifying in the form of snow or ice. The amount of heat thus liberated is sufficient to raise the temperature of nearly 102 cubic feet of free air at 76° one degree Fah. If the compressed air is expanded from a pressure of 59·7 lbs. to 14·7 lbs. absolute, the possible limit of temperature attainable will be raised from 134° below freezing with theoretically dry air, to about 64° below the freezing point on the Fah. scale when the air is saturated; therefore the disadvantages accompanying the presence of moisture in the air, when used for refrigerating, is not confined exclusively to the mechanical one of dealing with the snow formed, but it exercises a very considerable influence in reducing the range of temperature practically attainable, an effect which is more often attributed to the conductivity of the metal envelope enclosing the air, than to the true cause.

I have tried to make the table appended to this paper as self-explanatory as possible with the aid of the notes at the top of each column of figures. I must not omit to mention that the tables possess all the probability of error attending the results obtained by a single calculator, but I have done my best to ensure accuracy by making two separate calculations for each result and further ones where I observed any discrepancy. I have also had the advantage of comparing the figures in columns 3, 4, 5, and 6, with the corresponding results tabulated by Mr. Shone up to a gauge pressure of 100 lbs., but I have not been able to compare the figures in the other columns up to this pressure, nor any of the figures dealing with pressures beyond this with any existing tables within my knowledge, and the absence of such tables, together with the existing necessity for them is one of the principal reasons which induced me to prepare the present one.

1 Gauge Pressure	2 Absolute Pressure.	3 Rt or Ri Ratio of Adiabatic expansion or compression to or from 14·7 lbs Absolute.	4 Ratio of Isothermal expansion or compression to or from 14·7 Absolute.	5 Relative volume after Adiabatic compression, or cut off to obtain theoretical maximum effect.	6 Relative volume after Isothermal compression, or after cooling to initial temperature.
0	14·7	0·0000	0·000	1·00000	1·00000
5	19·7	1·2310	1·3401	·81231	·74619
10	24·7	1·4455	1·6803	·69180	·59514
15	29·7	1.6476	2·0204	·60693	·49495
20	34·7	1·8401	2·3605	·54345	·42363
25	39·7	2·0246	2·7007	·49392	·37027
30	44·7	2·2025	3·0408	·45402	·32886
35	49·7	2·3747	3·3810	·42110	·29578
40	54·7	2·5420	3·7211	·39339	·26874
45	59·7	2·7049	4·0612	·36970	·24623
50	64·7	2·8638	4·4014	·34918	·22720
55	69·7	3·0193	4·7415	·33121	·21090
60	74·7	3·1715	5·0817	·31531	·19679
65	79·7	3·3208	5·4218	·30113	·18444
70	84·7	3·4674	5·7619	·28840	·17355
75	89·7	3·6115	6·1020	·27689	·16388
80	94·7	3·7533	6·4422	·26643	·15523
85	99·7	3·8929	6·7823	·25687	·14744
90	104·7	4·0306	7·1224	·24810	·14040
95	109·7	4·1663	7·4626	·24002	·13400
100	114·7	4·3003	7·8027	·23254	·12816
105	119·7	4·4326	8·1428	·22561	·12280
110	124·7	4·5632	8·4830	·21914	·11788
115	129·7	4·6924	8·8231	·21311	·11333
120	134·7	4·8201	9·1633	·20746	·10913
125	139·7	4·9465	9·5034	·20216	·10522
130	144·7	5·0715	9·8435	·19718	·10158
135	149·7	5·1953	10·1837	·19248	·09819
140	154·7	5·3179	10·5238	·18804	·095022
145	159·7	5·4394	10·864	·18384	·092048
150	164·7	5·5598	11·204	·17986	·089253

7 Mean effective pressure in lbs. per square inch during Adiabatic compression or expansion.	8 Available energy in foot lbs. per cubic foot of dry compressed air.	9 Theoretical maximum number of cubic ft. delivered per 1HP. per hour at temperature due to adiabatic compression.	10 Theoretical maximum number of cubic ft. delivered per 1HP. per hour after cooling from adiabatic to initial temperature.	1 Constant number to find difference in absolute temperature due to adiabatic compression or expansion of dry air. *
0·0000	0·0000	0·0000	0·0000	1·0000
4·4950	796·83	2484·8	2282·5	1·0886
8·2382	1714·8	1154·7	993·32	1·1624
11·477	2723·2	727·09	592·92	1.2262
14·315	3793·2	521·98	406·89	1·2828
16·910	4938·7	400·92	300·55	1·3340
19·263	6109·7	324·07	234·73	1·3806
21·495	7350·6	269·36	189·19	1·4237
23·530	8613·4	229·88	157·03	1·4638
25·438	9908·2	199·83	133·09	1·5014
27·236	11232	176·28	114·70	1·5369
28·938	12581	157·37	100·212	1·5704
30·554	13954	141·89	88·559	1·6023
32·095	15348	129·01	79·017	1·6327
33·768	16861	117·44	70·669	1·6617
34·984	18194	108·83	64·410	1·6896
36·343	19642	100·80	58·728	1·7164
37·652	21107	93·808	53·844	1·7422
38·915	22586	87·664	49·609	1·7671
40·136	24080	82·228	45·907	1·7912
41·318	25585	77·387	42·649	1·8145
42·464	27104	73·034	39·756	1·8370
43·577	28635	69·147	37·195	1·8590
44·658	30175	65·616	34·897	1·8803
45·711	31728	62·406	32·827	1·9010
46·738	33291	59·475	30·956	1·9212
47·734	34860	56·798	29·263	1·9409
48·709	36440	54·335	27·719	1·9601
49·661	38029	52.065	26·319	1·9789
50·591	39627	49·965	25·017	1·9920
51·501	41232	48·020	23·829	2·0152

* Note.—Figures in this column are *multipliers* for compression and *divisors* for expansion ; in the latter case the results require correction as indicated in the paper.

MR. W. H. NORTHCOTT.

(MEMBER)

Mr. Williams in his paper on the Efficiency of Air Compressors, rightly assumes that the subject is one of considerable interest to Engineers and Capitalists, because Compressed Air is rapidly coming into use for so many purposes, especially on the Continent I fear, however, that Mr. Williams' theories will need some little modification in the direction of those taught by Regnault, Stewart and Rankine, to make them acceptable to the members of this Institution. The points raised by Mr. Williams, and, in regard to which he expresses himself as being opposed to our leading scientists, are points so easily, and perhaps so often misunderstood, that Mr. Williams has done better service by calling attention to them than he would have done had his paper been more orthodox. The discovery of an error will often teach one a great deal, and Mr. Williams has no doubt found by this time that Regnault, Stewart and Rankine are right, and he himself wrong.

Mr. Williams calls attention to the fact, that the total work obtainable theoretically from a given weight of air is a function of the temperature only, and quite independent of the pressure. This is a very important point, but many Engineers are not to be convinced that a pound of air at one pound pressure and, say, 60° F. contains as much energy as a pound of air at, say, 100 pounds pressure and 60° F. Theory is no doubt right, but the Engineer, who expected to get equality of power in practice, would of course be disappointed.

I have had a good deal of experience in the compression of air, especially at high pressures from 500 pounds to 2,000 pounds per inch, and am led to believe that water injection does a great deal more towards cooling the air than water jacketing—comparitively speaking. This is quite opposed to Mr. Williams' views. But it must be recollected that water injected into the compressing cylinder abstracts heat by direct contact, not only with the hot air itself, but by direct contact with the internal surface, and, of course, the hottest part of the compressor cylinder. On the other hand, all the heat absorbed by water circulated round the cylinder has to come *through* the metal of the cylinder. Obviously, then, for any heat to be abstracted by the jacket water, the internal surface of the barrel will be very much hotter than the external surface—just as in the case of heat transmitted through a boiler plate, the surface one side may be at 2,000 °F. and the surface in contact with the water at 300 °F. Water jacketing within practicable limits cannot possibly, therefore, produce isothermal compression, even when the rate of compression is low.

Of course, the saving produced by water cooling, friction neglected *is* the difference between the adiabatic and actual diagrams. By disputing this, Mr. Williams' practical conclusions are more than questionable, but his paper is nevertheless very instructive and useful. In Mr. William's reply he states, and italics the statement, that *in single stage compression any cooling after the discharge is useless for power-saving* when the exit passages are sufficiently large. The statement, however, is not correct.

MR. G. T. HARRAP:

(MEMBER.)

I am sorry I was not able to make any preparations to join in this discussion, as I might have been able to bring forward some indicator diagrams and particulars of various cold air machines, which would, doubtless, have been of assistance. It seems to me, in dealing with this paper, the practical, rather than the theoretical side, is worth more attention. The question of a decimal or so appears, at the outset, of small moment, and, although Prof. Selby has gone very fully into the various reasons for supporting the old 1·408 as against Mr. Williams' 1·4. I think we can fairly ignore, for the present, the small difference between them. The whole point of the paper appears to be this:—how to make the best use of heat in the compressed air. The present method is to get rid of it by means of water injection or jacketing of the compressor, so as to save as much power as possible during the compression; but Mr. Williams raises the point that the heat should be allowed to accumulate in the air as it is compressed, and then abstracted for heating the feed water or for steam or gas generation for motive purposes. It seems to me entirely a matter to be settled by actual experiment, the highest temperature at 6 atmospheric compression being only 360°, and we know it takes a large difference in temperature between heated air and water heated by it before any satisfactory result is obtained. However, Mr. Williams says there is 81°/₀ to be gained, so that there appears to be a good margin to work upon. With reference to the part played by vapour in the air in an expansion cylinder, I may say that Messrs. Hall, of Dartford, fully appreciated the effect and took satisfactory steps to prevent the evil some years ago.

PROF. J. H. COTTERILL.

The conclusion arrived at in this paper that power may be derived from heat drawn from the atmosphere by a machine requiring no external force to drive it, except such as may be necessary to overcome friction, would obviously, if true, be of the greatest importance. It is, however, contradicted by common experience, being equivalent to the construction of a refrigerating machine which shall work of itself without recourse to external agency. The impossibility of doing this is well-known, and is expressed by that great natural law known as the Second Law of Thermodynamics. Some fallacy, therefore, must exist in Mr. Williams' reasoning, and, in fact, on examination it will be found that a cubic foot of *compressed* air is employed as a measure of the quantity of air used, whereas, in reality, a given volume of air at a given pressure represents very various amounts according to the temperature. In order to obtain a cubic foot of air at 80°F., about 50 per cent. more air must be taken from the atmosphere than would be necessary if its temperature were 350° F., and, therefore, the compressing cylinder must be 50 per cent. larger. Mr. Williams appears further to have overlooked the fact, that, although the air may be discharged from the expansion cylinder at any temperature we please, yet it must necessarily be taken into the compressing cylinder at the temperature of the atmosphere. Compression is, therefore, not the exact reverse of expansion as assumed, unless the temperature as well as the pressure of the air discharged from the expansion cylinder is that of the atmosphere. For these reasons the results of calculation should be expressed in terms of the volume (V_2) of air drawn into the compressing cylinder, at the atmospheric pressure (P_2). It is then readily seen that the energy exerted in compressing air to a given pressure and expelling it from the compressing cylinder must be much less when the temperature is kept down by abstracting heat than when it is allowed to rise. If R be the pressure-ratio, as in Mr. Williams' paper, the energy in question is in fact only $P_2 V_2 \log^t R$ instead of being given by the formula quoted. The fractional saving can be shown to be, at moderate pressures, approximately $\frac{\log^t R}{7}$ being nearly 30 per cent. in the numerical example given.* The saving is due to the compression curve being a common hyperbola instead of an adiabatic curve, so that the mean effective pressure in the compressing cylinder is much diminished.

It is true that when the compression is adiabatic heat is generated, the mechanical equivalent of which is about the same as the energy exerted in the compressing cylinder, an amount generally much greater than the saving due to the employment of isothermal compression. The ratio is, at moderate pressures, approximately

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* The formula here given is only a rough approximation convenient for showing that the saving increases as the compression increases. In compression by equal stages R is the compression in a single stage, and that is the reason why compression by stages is advantageous as it undoubtedly is apart from frictional losses in the additional cylinders necessary.—J.H.C.

$\frac{7}{\log_t R}$ or about $3\frac{1}{2}$ in the numerical example. That is, the heat generated is about $3\frac{1}{2}$ times the additional mechanical energy required to generate it; and with a less compression would be still more in excess. In a country where water power was abundant, and fuel very expensive, it might, therefore, be worth while to make use of heat thus generated, instead of obtaining it by the combustion of fuel. The process is, in fact, equivalent to one of those warming machines invented by Sir William Thomson, which, by a relatively small expenditure of energy, draw a large quantity of heat from the atmosphere and apply it to useful purposes They operate on the same principle as an ordinary refrigerating machine, but, until by improvement of the gas engine and of other forms of heat engine, we are able to utilise much more completely than has hitherto been done, the high temperature of burning fuel, they are not likely to come into use.

It should further be noticed that the maximum available energy of a cubic foot of compressed air is not that given when, by expansion, the temperature falls below that of the atmosphere, for additional work will be done if the air be allowed to draw heat from the atmosphere.

The conclusion to be drawn, therefore, is that in the transmission of power by compressed air, changes of temperature are to be avoided as far as possible ; they are always practically a cause of loss unless radiation and conduction can be prevented or in some way neutralized. In the compression cylinder the temperature should be kept down, and in the expansion cylinder it should be kept up by injection of water or otherwise. If this could be done perfectly, power might be transmitted by compressed air without any loss save that due to friction.*

MR. J. MACFARLANE GRAY.

When there is no cooling during compression, the air is discharged at a temperature which, the author thinks would admit of part of it being applied to produce additional work beyond that now obtained in the user's engine. If the whole of this heat could be so employed, using a perfect engine with a perfect regenerator and

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* The loss due to changes of temperature will be smaller the less their amount, and this is the reason why compression or expansion by stages is advantageous. If the number of stages is increased indefinitely, the result is the same (apart from frictional losses) as if the changes of pressure were isothermal. When a rise of temperature and subsequent loss of heat is unavoidable it will of course be advantageous to utilize the heat, but the smaller the rise the more difficult is it to do this. There are numerous cases in practice where large quantities of heat are wasted simply because the temperature is too low to render it of practical value.

working under theoretical ideal conditions of conductivity and adiabaticity, working from the highest temperature down to the original temperature of the air, using a specially created fluid with its temperature-pressures and latent heat, just whatever the author would prefer them to be, no restrictions, the additional mechanical return would be exactly that represented by the portion of the diagram contained between the theoretical isothermal and the theoretical adiabatic on the compression diagram, or about one-fifth of the mechanical equivalent of the heat. That is also just equal to the difference in the work of compression isothermally and adiabatically, theoretically. But, the author contends, and he gives experimental proof for what he says, that the difference between the work of compression with cooling and without cooling is not even the one-sixth part of this area difference. It is, he knows, impossible to get more than the one-fifth of the mechanical equivalent of the heat back, under ideal conditions, in useful work, but if we can practically get back anything more than one-thirtieth of that equivalent we get, by that amount, more than the practical extra cost of compression without cooling.

The author's statement about obtaining a larger return of mechanical energy than we expend in producing the rise of temperature, taken as a general scientific principle, is quite wrong, but if interpreted according to the practical conditions which suggested the thought, the statement as I have explained it, is strictly right. I consider that, while this consideration may have the same theoretical interest, the statement is misleading as it stands and it has no practical value.

MR. WM. L. SAUNDERS, MEM. AM. S.C.E. (NEWYORK)

I have been much interested in reading the paper on the "Efficiency of Air Compressors."

I take pleasure in mailing you to-day, under separate cover, a copy of a little pamphlet of mine which relates to the same subject

I am preparing a publication upon the Transmission and Uses of Compressed Air, and am particularly impressed with the following paragraphs in your pamphlet.

"It is theoretically possible to obtain a larger return of the mechanical energy from compressed heated air than we expend in producing the rise in temperature."

"These authorities appear unanimous in the opinion that the heat developed in the air during compression involves the greatest loss and is the primary disadvantage which can be urged to discount the many unquestioned advantages attending the use of compressed air as a power distributing medium. This conclusion I cannot agree with, and if a fractional part of the time and money which has been expended in consequence of this idea had been employed in utilizing the heat imparted to the air during its compression, etc., the use of compressed air would be much more general and would not furnish so much ground for speculative debate as to its relative efficiency."

By the simple introduction of a miner's lamp into a compressed air conduit, which conveyed power to one of the shafts on the New York Aqueduct tunnel, I increased the temperature of the air to such a point as not only to prevent freezing, but it largely increased the efficiency of the air compressing machinery.

I am a strong believer in re-heating compressed air by applying the flame directly within the air pipe, thus utilizing the full number of heat units in the fuel.

In my experience with compressed air machinery I have thought that too much attention has been given to cooling during compression and too little to heating during use.

MR. HOWARD LANE.

I regret I have so little time at my disposal, that I am unable to comply with your invitation to discuss the subject of compressed air.

I may say that I am dealing with air and gases on a large scale, up to 5,000lbs. per square inch pressure. I am ot opinion that shortly a great advance will be made in means of obtaining motive power, by applying heat to air under great *initial* pressure.

PROFESSOR ARNOLD LUPTON.

Mr. Williams' corrections of Mr. Shone's original tables correspond, (so far as that column is concerned) with a corrected table that I have myself used for some time. He is also, I think, right in saying that the efforts of most engineers, who have made air-compressing machinery, have been to cool the air during compression as much as possible, and Mr. Williams suggests that instead of doing that, the heat produced by compression should be utilized by causing it to do work in heating various substances and in an engine. I have heard of this plan of using the air hot being adopted at a colliery, where I am told there was no attempt made to cool the compressor ; at the Birmingham Compressed Air Works, the cooling effected is only partial and the air is used warm at some consumer's works near the compressing station. I read a paper at the Leeds meeting of the British Association on the Birmingham Compressed Air Works, and in the discussion I pointed out that a very high efficiency might be obtained by using the air hot from the compressors, conveying it in pipes carefully coated with non-conducting material, and I suggested as a theoretical possibility an indicator efficiency in the consumer's engine of 80°/o.

In the report recently prepared by Mr. John Sturgeon and myself jointly for the International Niagara Commission, we recommend a partial cooling of the compressors (by external application of cold water) which would prevent inconvenient heating of the compressors and compressor chambers, but allow the air to be delivered hot into the delivery main, in that it might be used hot by the consumers in the vicinity, and thus the heat of compression utilized. Mr. Williams, however, suggests that instead of keeping the air hot, it should be cooled by useful applications of its heat, and reduced in temperature to less than atmospheric temperature ; the air thus cooled, I gather, is to absorb heat from the atmosphere on its way to other engines, and thus gain energy from the atmosphere available for further work. This accession of heat, or energy, from the atmosphere doubtless occurs in all engines using air and modifies the expansion curve, but only to a slight extent compared with what might occur in the method suggested by Mr. Williams.

Probably, however, no one is better aware than Mr. Williams that theoretical methods of improving efficiencies have to be employed only so far as they agree with the practical exigencies of situation, and compressed air has to be used, not when and how it may give the highest efficiency, but simply where a customer exists willing to pay for it.

Mr. Williams seems to have compiled his tables with care, and his paper is very suggestive, and will be useful to many.

PROFESSOR ROBERT H. SMITH.

I am obliged to you for the opportunity of reading Mr. Williams' paper, which I have done with much interest.

In so far as Mr. Williams' contention is that the heat, developed by compression of the air in the compressor pumps ought to be utilized, I entirely agree with him, and think that this is a new point to which sufficient importance has not been hitherto attached. But as for using special apparatus independent of the compressors, and built for the special purpose of extracting this heat in a useful manner, I fear that in any case the economic result would not be good—the saving of waste heat would not compensate the extra expense in prime cost and current expenditure.

But beyond this Mr. Williams proposes to omit all attempts at cooling during compression, in order that the full amount of heat may be developed and be saved by the extra utilization plant. This decidedly suggests that the proposal is founded on a belief that mechanical compression is an economical way to produce heat for the purpose, say, of warming the feed water to a boiler. The incorrectness of such an idea needs hardly to be pointed out.

It is, I think, perfectly undeniable that in order to promote economy in air-power transmission, the best attainable cooling circulation must be kept up in the compressors. But this being granted, I do not at all see any reason why the feed-water on its way to the boiler should not be used as the cooling medium. By this means all the saving of waste heat that is practicable may be accomplished.

Another suggestion of Mr. Williams is, that in obtaining the mechanical power from the compressed air the operation should be carried out in stages, the effect being that in each stage of the adiabatic expansion the temperature should be lowered below atmospheric temperature, and that in the rest between each stage, heat should be extracted by conduction from the outside atmosphere. This is certainly a very ingenious and alluring notion, but I feel certain that industrial economy could not be derived from it. To carry it out requires at least the duplication of the air-worked engine, and the increase of cost involved in this would far outweigh the small mechanical gain. To recognize that this is so it is only necessary to remember that it is not found conducive to financial economy to use any but quite a low grade of expansion in the air engine, so that really the great bulk of the work is done before the expansion begins. The mechanical gain obtainable from the proposed expansion in stages is thus only a very small fraction of a part which itself is not a large proportion of the whole work done.

REPRINTED BY PERMISSION

From "THE PRACTICAL ENGINEER," March 20th, 1891.

A FALLACY AS TO AIR COMPRESSION.

The many papers which have been read before the young but vigorous Institute of Marine Engineers have almost, without exception, been of a practically valuable and useful kind, andwe have now learned to look to the members of this institute for a particular type of engineering information which it is difficult to get elsewhere. The papers read before this society are characterised by a commendable contempt—or perhaps, more correctly, a questioning attitude—towards standard authorities, which betokens a state of mind highly favourable to progress. Sometimes, however, authority is questioned boldly, without a complete understanding on the part of the questioner on the matters in discussion. This is but natural, as all practical engineers cannot be expected to fulfil the hard conditions necessary to attain experience in workshop practice, and at the same time keep thoroughly and accurately posted up in the developments of the more abstract sciences bearing on their profession. It is therefore by no means astonishing to find occasional errors, due to an imperfect knowledge of a complex science such as that of thermodynamics.

A very clever but erroneous paper on the "Efficiency of Air Compressors," read by Mr. Joseph Williams before the Institute of Marine Engineers, a few months ago, now lies before us, and it has suggested these reflections. An abstract of this paper has already appeared in these columns, but, nevertheless,as some engineers have very probably been carried away by Mr. Williams's reasoning, and as the difficulties he has encountered may prove puzzling to others, we think our readers will take some interest in considering the matter with us.

To begin with, Mr. Williams quotes a number of writers of considerable authority, such as Mr. Lightfoot, Professor Kennedy, and Mr. Donaldson, as stating that if air be compressed in a cylinder, the power expended in compressing may be reduced if cold water be injected into the cylinder during compression, and so the temperature be prevented from rising; that is, if air be compressed approximately on an isothermal line, less work is required to compress it than when the air is allowed to heat, and so the compression line become adiabatic. Now, this statement has been for many years considered as perfectly self-evident by engineers whose practice has led them to deal with compressed air, and, moreover, it has been hitherto considered as in precise accordance with facts. Mr. Williams boldly challenges this statement, and considers that it is impossible to save work by injecting water in this manner. The reasoning by which he arrives at this conclusion is ingenious, but, unfortunately for his position, quite fallacious. He states that by the first law of thermodynamics, as defined by Rankine, "Heat and mechanical energy are mutually convertible, and heat requires for its production, and produces by its disappearance, mechanical energy in the proportion of 772 foot-pounds for each British thermal unit of heat." Mr. Williams takes his stand upon this law, and asks—

How can this law accord with the teaching and practice of assuming that the heat imparted to the cooling water costs nothing to produce? The only medium through which energy or heat is communicated to air or water in the compressor is through that furnished by the piston rod of the steam cylinder, so that, if all the heat generated in the air during its compression could be abstracted by the cooling water, it could not theoretically affect the steam diagram in the slightest degree without transgressing the elementary law I have submitted to you.

Now, here Mr. Williams has plainly and clearly gone astray, as a little consideration will show. Suppose, for example, that one cubic foot of air be compressed rapidly in a cylinder to one-fourth of its volume, and that no heat be abstracted during the process, the temperature will rise to over 200 deg. C., and the pressure will increase from atmosphere to about 100lb. total. The total heat evolved will, with air, equal the area of the total diagram, including atmospheric pressure, and the total work done by the compression will be restored on expansion of the air; so that if the compressing piston be rapidly moved in and out, the line of compression of the air, and that of expansion, will be identical.

If, however, one cubic foot of air be compressed from atmosphere to one-fourth its volume or one-quarter cubic foot in four stages, in such manner that a sufficient pause is allowed at each stage, it is evident that the air may be readily cooled at each stage. If the air be first compressed from one cubic foot to three-quarters of a cubic foot, and then cooled to the same temperature as it started with, it is evident that the work done in compressing through the second quarter will be less than that done in the second quarter of the stroke when compression was accomplished continuously; and if the air be cooled again at the second and third quarter, it is evident that the whole area of a compression diagram effected in this manner is less than when the compression is effected without cooling or pause. The total heat evolved, however, during the compression is also less by the heat equivalent of the difference of area between the diagrams. If water be injected into an air-compressing cylinder during the whole compression, or the compression be conducted so slowly that the temperature never rises much, then still less work will be performed on the air, and less heat will be evolved by its compression.

We have ourselves experimented with an air compressor, compressing air from atmospheric pressure to 100lb. above it in three stages, with cooling arrangements between each cylinder, and we have found a very large reduction of power as compared with single-cylinder compressors, apart altogether from any theoretical reasoning; and the same is true of compressors cooled by injection of water.

Mr. Williams has gone astray on a very simple point, and that is in assuming that heat is not evolved during isothermal compression. Suppose a cubic foot of air, expanding behind a piston beginning at atmospheric pressure, and suppose that the expansion is to be isothermal, then heat must be added; and although at the end of the expansion the air is no hotter than at the beginning, yet heat has been absorbed by it in amount precisely equivalent to the expansion diagram. In the same manner, if air be compressed at constant temperature, heat must be taken from it equal to the total area of the compression diagram, although at the end of the compression the air is at the same temperature as at the beginning.

Mr. Williams has plainly failed to understand this, or he would see that the idea of isothermal compression is strictly consonant with the first law of thermodynamics, and the saving of power effected by cooling air during compression is also entirely as that law requires, and in strict accord with facts, entirely apart from theory. Isothermal compression is therefore by no means hypothetical, and Mr. Williams' conclusions and the practical advice he bases on them are not to be accepted as correct by the engineer using compressed air, more especially for the work of mechanical refrigeration. Mr. Williams' paper, notwithstanding its errors, is an interesting one, and valuable as showing how our practical engineers are beginning at least to think for themselves on the subjects of heat and work.

MR. WILLIAMS' REPLY.

The first paper on this subject which was read to the Institute, contained such an obvious error in theory that I was permitted to revise it, and this was done so hurriedly, that I overlooked the fact that the second paragraph on page eight is somewhat obscure, as is shewn by Professor Cotterill taking it to mean that the formula for adiabatic expansion is also theoretically true for compression; while I should have stated more plainly that it is only *theoretically* true for *adiabatic* compression, and I will now try to explain (as I did at Cardiff) why the same formula is practically applicable under ordinary circumstances, and why the compression curve is approximately adiabatic under ordinary working conditions, as may be seen from the various diagrams shewn at the different meetings of the British Association and elsewhere.

Mr. C. W. Siemens stated seventeen years ago (see Proc. Mech. Eng., 1874) that a saving of nearly 33 per cent. could be effected with compression to four atmospheres by "injecting cold water in the form of spray into the compressing cylinder, in sufficient quantity to keep the temperature practically uniform throughout the stroke." Since that time the constant efforts of Engineers have been unavailing to secure in practical work more than a very small fraction of the possible saving thus indicated (see Proc. British Association, 1889 and 1890). I will try to account for these observed facts as follows :—In the first place there must be *some* difference in temperature between two bodies before one can increase the temperature of the other by conduction or radiation, so that the air must be hotter than the water before the latter can absorb any heat from it, and this fact will always prevent the possibility of perfect isothermal compression when the cooling water enters the cylinder or jacket at the same temperature as the air. Secondly: There must be a *considerable* difference in the temperature of two bodies placed in contact for a very short time, before any appreciable interchange of temperature becomes apparent. In air compressors, with moderate ranges, the time is usually too short, and the differences of temperature too inconsiderable to produce any marked effect on the compression curve, hence the reason that the latter is with moderately high piston speeds *always* more nearly adiabatic than isothermal.

Take the case of an air compressor working at sixty revolutions per minute, compressing to four atmospheres. Each stroke is completed in half-a-second. With adiabatic compression the required pressure is attained at about five-eigths of the stroke; suppose we assume a difference of only 50° Fah. between the air and water, before the latter produces any practical effect in cooling the air, the piston will have travelled about $\frac{1}{5}$ of the stroke by the time the air reaches this temperature, so that all the time remaining in which heat can be usefully extracted before the discharge valve opens, will be $\frac{5}{8}$ of $\frac{1}{2}-\frac{1}{5}$ of $\frac{1}{2}=\frac{17}{80}$ or less than a quarter-of-a-second. Judging from observed results, this time appears to be insufficient to allow the effect sought for to be obtained, and this may, perhaps, be accepted as one of the most important reasons to account for the very wide gap which has been hitherto observed between the theoretical possibilities and the practical realities of air compression; somewhat similar reasoning to the foregoing may account for the generally observed fact that the greater elaboration and effectiveness, of the means used to reduce the temperature of the air, during compression, is always accompanied by a *considerable* reduction in temperature during its discharge, which means a loss in the available energy possessed by it, without any corresponding saving of steam power, and *in single stage compression any cooling after the discharge valve opens is useless for power saving* when the exit passages are sufficient.

The practical objections to water injection has often led to its abandonment, notwithstanding its theoretical advantages, while the unsuitability of some waters for the purpose will always tend to prevent its use in many localities, and without water injection sufficient experience has been accumulated to shew that the compression curve must be practically adiabatic at all practicable speeds. That this was not long ago recognised, may be due to some of the published tables for mean air pressures in use, leading many of those concerned to think they were getting partial isothermal compression when it has been really adiabatic. I am pleased to find that the accuracy of the figures in column seven of the annexed table is supported by comparison with Professor Lupton's.

Mr. Harrap correctly summarises the case as I wish to present it, by saying that he understands the question to be a practical one, and that the whole point of the paper appears to be "how to make the best use of the heat in the compressed air." Exactly! and to practical men I submit that *no* use cannot be the *best* use. To give a better idea of the loss involved by the present practice, I will refer in greater detail to the example commencing with the last paragraph on page 11, and refer to the Figs. 2, 3, and 4, which Dr. Elliott very kindly caused to be enlarged for my use in Cardiff. Fig, 2 represents an air diagram, with an adiabatic curve from a cylinder 6 feet long and 144 square inches area, the numbers inside the boundary represents foot lbs., those at the bottom volumes in cubic feet, and the scale to the right is in atmospheres of 14·7 lbs. each. The vertical dotted line marks the boundary of a rectangle, representing one cubic foot, and is equal to the volume of the air if compressed isothermally. The full lines represents the work done during adiabatic compression and discharge from the cylinder—and I may say here that Professor Kennedy's experiments at Paris confirm my previous statements as to the practical adiabacity of the compression curve. In Fig. 2 this curve is supposed to be in harmony with the equation PV^{γ} = a constant. Fig. 3 represents the work *theoretically* obtainable from the same weight of air after cooling to the temperature of the atmosphere, and the difference between 29,862 and 17,762 = 12,100 foot lbs. represents the theoretical loss in the Parisian example, between the compressor and the motor, per cubic foot of air supplied to the consumers. A cubic foot of compressed air at normal temperature will expand into a space of 1·6813 cubic feet at the temperature due to adiabatic compression to 6 atmospheres, and if admitted to a cylinder could perform 17,795 foot lbs. of work before expanding.

Referring again to Fig. 2, we will suppose the air (which weighs 0·44958 lbs.) is discharged from the compressor at a tem-

Fig.2

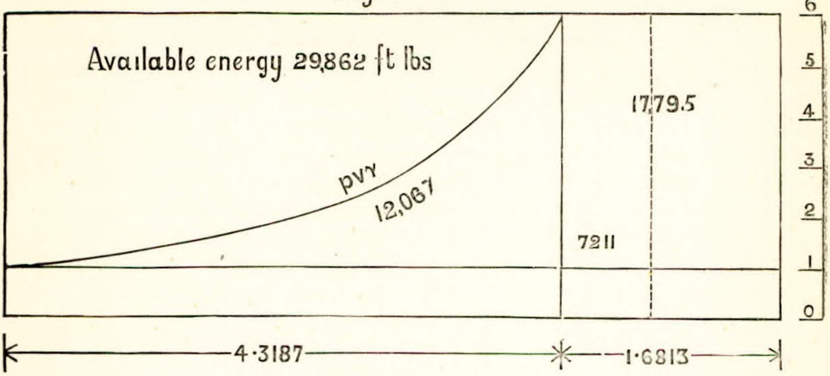


Fig.3

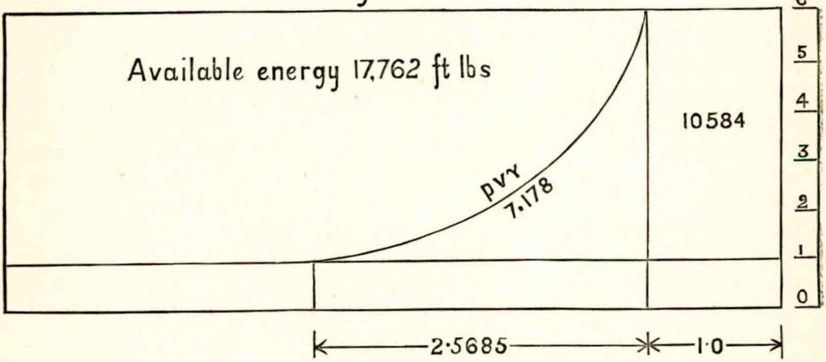
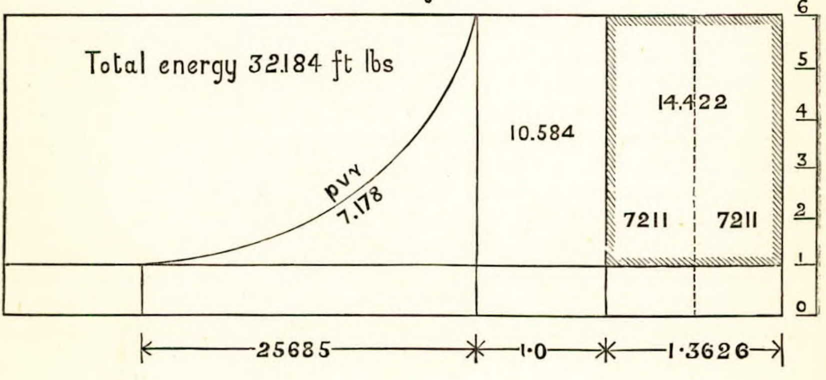


Fig 4



perature of 430 Fah. into what I shall call an accumulator for the sake of retaining the imaginary unit of area, one square foot. The piston of the accumulator will be raised through a distance of 1·6813 feet, and the work done in raising it will be 17,795 foot lbs. If we now utilize the heat of the air in some way, such as the paper suggests, and by doing so reduce its temperature to 70° Fah., we shall have one cubic foot remaining, which, admitted behind a piston, can give out 17762 foot lbs. of work (see Fig. 3). The mechanical equivalent of the heat abstracted from the air before it leaves the accumulator, will equal $0·44958 \times ·238 \times 772 \times 360 = 29,738$ foot lbs., and balancing the results at this stage, we see that with a theoretical expenditure of 29,862 foot lbs. by the compressing engine, we get a total *theoretical* return of $29,738 + 17,795 = 47,533$ foot lbs. from the air; which leaves a considerable margin for losses. If we could turn, say, 44°/o of this heat into useful work, the result would be represented by Fig. 4, which shows a balance on the plus side of Fig. 2, and is 81°/o in excess of the example of the present practice of cooling the air uselessly, represented by Fig. 3. The result is rather more than the approximate one given on page 12 of the paper, where, to illustrate the method it was not considered necessary to include the mechanical equivalent of the 15° below atmosphere, which the air left the works at; but it should be borne in mind that the mechanical equivalent of the heat acquired along the route of the supply pipes is a source of gain, additional to my previous calculation. The paper was only intended to be suggestive, and I can only touch briefly on some of those points, which each requires a paper to itself to deal with properly. I beg to point out to Mr. Harrap and Professor Smith that my contention is not that we *should allow* the heat to accumulate *during air compression*; but, that as we find it cannot be prevented, we should utilize it, but I would not distort the adiabatic curve by doing away with water jacket for very obvious practical reasons. The 81°/o *theoretical* gain between Fig. 3 and 4 applies only to this one example, and, as I have before stated, do not by any means exhaust the theoretical possibilities, indicated by a similar chain of reasoning applied to more favourable examples. The 360° is not the highest temperature, but only represents the temperature range in the given example under the conditions quoted. His reference to the large differences of temperature known to be requisite, may be used more effectively to support my contention concerning the practical impossibility of isothermal compression, where both time and space is necessarily very much more limited than in the case we are considering, and in mentioning a compressed air cooler, or boiler, I do not wish to infer that a structure of thick plates and 2½ in.

tubes is contemplated, or, that one's ideas concerning the best way to utilize the heat is necessarily circumscribed by the imperfect achievements of others in a similar direction ; on the other hand, I am not sanguine enough to hold out any hope that we can attain similar results to those indicated by Professor Kennedy, after his famous experiment with the boiler at the "Addington Pumping Station" (see "Engineer," Aug. 29, 1890), where his report leads us to infer that *no* range of temperature between a boiler and the gas leaving it is practically required.

Professor Lupton's reference to what has already been done is a sufficient reply to those who assert that *none* of the heat due to compression can be utilized, and his contribution to this question appears to narrow it down to "how much ?," instead of "can any ?"

The purport of his concluding remarks contains views more pessimistic than my own concerning the future of compressed air, and instead of the air supply having to look for a customer, a time may come when power users shall turn their faces towards a place with a compressed air supply, as in the East, true believers may be seen to turn, and gaze with longing eyes towards the spot containing the Mecca of their hopes, and, while contemplating the future, is it to much to ask you to take a far wider view, which not only includes taxing the intrinsic energy in the atmosphere, but for the reasons briefly indicated on page 13, may lead us to anticipate a time when a combined *steam and air engine may enable us to exact a more satisfactory tribute from the latent energy in steam* than we have hitherto found to be possible with the best steam engines.

In their allusions to the practical modification of the expansion curve, both Professors Cotterill and Lupton, in common with all our text books, ignore the very tangible influence of the moisture in the compressed air, and mention the comparatively negligible effect of atmospheric temperature on the cylinder of the air motor, which, for similar reasons, may modify an expansion curve, as much—and no more—as an unjacketed cylinder modifies a compression curve of the same temperature range. In this paper, the observed extension of the expansion curve beyond the adiabatic is attributed to the presence of moisture in the compressed air, and for the reasons indicated on pages 12 and 13 ; and those among you who care to follow up this subject are invited to regard a possible extension of this method of utilising latent heat as a most important factor, which may influence the future developments of the production and transmission of motive power. I gather from Professor Cotterill's remarks that I have gone astray in adopting a cubic foot of *compressed* air as the measure of the quantity of air used, but if this unit is scientifically incorrect, it is perhaps more practically useful, as the available energy in compressed air is a function of its PV *after compression*, and those who pay for its use are only interested in the quantity of work obtainable from it. Professor Cotterill appears to have followed the beaten track which has misled so many, by assuming that the air must necessarily be cooled before it is used, and that this cooling can be accomplished during its compression; but even if isothermal compression was not the chimera it is, the product of the PV of the air delivered would still be much less per unit of power expended in its production than the product of its PV after adiabatic compression.

Referring to Mr. Northcote's remarks, I beg to point out that the honoured names of Rankine, Regnault, Tyndall, and Stewart, are given by me in support of my views as opposed to the present practice of wasting the heat necessarily generated during the compression of air for subsequent use as a power distributing medium. The three first-named have been quoted in the paper, and the last—Professor Balfour Stewart—in the fourth edition of his "Treatise on Heat" (page 349) says, in conclusion to an example he gives of an imaginary air engine :—"The engine is now ready to start afresh, and during this cycle of operations there has been more work *done* by the air than *done upon* the air, yielding us a surplus available for external purposes." The italics in the quotation are Professor Stewart's. I have not consciously said anything in this paper which is opposed to the teaching of these eminent men, or which is not supported by facts which have come under my personal observation, and as some of our members possess opportunities for practically testing the truth or fallacy of the disputed points, perhaps they may be induced to follow the apostolic injunction to "Prove all things" disputed in the paper, and afterwards take an early opportunity of correcting my figures by giving others of their own which will be of more practical use to the users of compressed air.

Mr. Gray's illustration, as I follow it, stops at the *original* temperature of the air, but an effort has been made in this paper to make it clear that we only begin to draw on the *intrinsic energy* after the temperature of the air falls below that at which it entered the compressor. In the examples given, the range from the highest temperature down to the original temperature of the air is 360° Fah., but a *total* theoretical range of 574° is assumed, about 215° *below* its original temperature. The temperature at which the air finally leaves the motor in Fig's 3 and 4 is assumed to be 314·6° absolute.

As I understand that the criticism (on the paper withdrawn), which appeared in the "Practical Engineer" is to appear in the "Transactions," I may be allowed to refer to it. That compression in stages is now receiving so much attention, is simply due to a more general recognition of the facts to which your attention has been directed; viz., that the practical results attained with attempted isothermal compression is not worth the effort involved, considering the many disadvantages attendant on throwing large quantities of water at high pressure into the compressor. When heat is abstracted from the air during successive stages, there is less work done by the engine in delivering a given *weight*, owing to its reduced bulk and temperature, and the consequent reduction in its available energy after discharge, and this only affects the point of "adiabatic" versus "isothermal" compression, so far as the admitted superiority of the results with "stage" over "single" compression (when wasting the heat in both cases) supports the conclusions submitted to you as to the practical impossibility of extracting any satisfactory amount of heat during the actual process of compressing. Compression in stages to high pressures, and utilising the heat generated, may give the most satisfactory and economical return, by enabling us, among other advantages, to draw more extensively on the intrinsic energy of the atmosphere, which at 2½ times its PV equals 5,292 foot lbs. per cubic foot at 60° Fah. or 69,287 foot lbs. per lb. of air.

Referring to the concluding sentence of the critic, I fear that a more extended knowledge of what has been done in the past by practical engineers, will tend to discount any value this paper may possess as evidence that they are "beginning at least to think." The critic seems to be unaware that as thought dominates action, so the thoughts of practical men have been dominant in the History of Engineering, and almost to that class alone do we owe any advance or improvement worth mentioning. In corroboration of this statement I will quote from "Stuart's History of the Steam Engine."

We know not, therefore, how the remark has originated, or what "philosopher" first claimed for theoretic men any part of the honour of being instrumental, even indirectly, in the perfecting of the steam engine; . . . . The fact is, that science, or scientific men, never had anything to do in the matter. . . . . . Indeed, there is no machine or mechanism in which the little that theorists have done is more useless. . . . . It arose, was improved, and perfected, by working mechanics—and by them only.

Long, subsequent to Watt's decease, Professor Rankine traced the former's improvements in the steam engine to Dr. Black's discovery of latent heat, but Watt, while living, unequivocally disclaimed any indebtedness on his part to that discovery.

From this it appears highly probable that practical men, began "at least to think" some time ago, and many names could be given between those of James Watt, and Bryce Douglas—just passed away—to prove that this mental process has never been in abeyance during the interval.

PROFESSOR A. C. ELLIOT, D.Sc.,

(Vice-President and Chairman Bristol Channel Centre.)

*Mr. Macfarlane Gray and the writer took part in the discussion which followed the reading of Mr. Williams's interesting paper at Stratford; but the remarks made were chiefly directed towards certain passages that do not now call for separate and extended criticism. The interchange of ideas which then occurred was undoubtedly profitable to all concerned; but in the circumstances it appears desirable to pass at once to some other points which indicate valuable and suggestive matter for discussion.

Take first the question of cooling. The fearless independence of Mr. Williams's paper undoubtedly set all who heard or read it thinking, and immediately after the discussion at Cardiff the writer thereby stumbled upon the principle of intermediate cooling, only to find however, after some time, that the idea was not new, and in point of fact had been already suggested by Professor Riedler in connection with the extension of the now well-known Popp installation in Paris. This is nothing else than a revival of an old proposal to carry out compression in two or more successive stages made originally in the interests of mere mechanical convenience. But by so arranging that in addition the air shall be cooled between the stages nearly to the temperature of the atmosphere, the inconveniences of cylinder injection are swept away and a substantial gain in efficiency is also secured.

Under ordinary practical conditions the process of compression and cooling may be carried out in different ways while the available energy remains the same. The best will obviously be that process in which the energy dissipated in the compressors is a minimum.

If the compressor heat be removed just so rapidly as it is formed, the total heat dissipated (in a simple process or simple part of a compound process) will, on consideration, be seen to be less than in the case where the compression is first effected and the mass of air subsequently cooled or allowed to cool under con-

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* Received too late for insertion along with the Paper.

stant or approximately constant pressure. If the water injection system were equal to the task we should be able to carry out the compression on the first or isothermal plan ; but to practically realize isothermal compression in this way we must work our compressors excessively slowly and inject large quantities of water. It is therefore not surprising to find, as Mr. Williams does not fail to point out, that even with the jet—as distinguished from the still less perfect appliance, the water jacket—the compression curve under ordinary practical conditions falls but little below the adiabatic : showing in fact that the heat is abstracted for the most part after the temperature difference has nearly reached its maximum value and the eduction valves have opened.

With regard to the possible saving to be effected by the complete realization of isothermal compression, the writer prefers, as a matter of fairness, to state it in fraction of the whole work done by the compressor in delivering a unit mass of air under the given conditions. Take for instance Mr. Williams's example. Let P_0 be the atmospheric pressure, P the reservoir pressure absolute, T_0 the atmospheric absolute temperature in degrees Fahrenheit, and A the numeric 53·15. Then the work done in compression in foot-pounds per pound mass is, with isothermal compression

$$W_i = AT_0 \text{ hyp. log } \frac{P}{P_0} ;$$

and the work done in adiabatic compression is

$$W_a = \frac{AT_0}{n} \left\{ \left(\frac{P}{P_0}\right)^n - 1 \right\},$$

where $$n = \frac{\gamma - 1}{\gamma} .$$

Taking $$P_0 = 14{\cdot}7,$$

and $$P = 45 + 14{\cdot}7 = 59{\cdot}7,$$

we have $$\frac{W_i}{W_a} = {\cdot}8106 ;$$

or in replacing adiabatic compression with isothermal, the indicated horse-power, after all, the quantity which most concerns the engineer, would show a diminution of about 19 per cent.

Let us now suppose that we can design a compound compressor so that the contents of the receiver can be cooled, finally, nearly to the atmospheric temperature. And in passing it may be pointed out that there is practically no difficulty in effecting this cooling, since the *time* during which a given individual mass discharged from the low pressure cylinder remains in the receiver can be made just as great as we please by sufficiently enlarging the receiver; moreover, the *surface* of the receiver may be extended after the manner of a condenser.

The symbols already used standing in the same sense, let P_1 be the receiver pressure; then since AT_0 is also equal to $P_1 V_1$ after the cooling, the work done per pound mass may be written

$$W_2 = \frac{AT_0}{n}\left\{ \left(\frac{P_1}{P_0}\right)^n + \left(\frac{P}{P_1}\right)^n - 2 \right\}.$$

Determining P_1, so that this expression shall be minimum, the writer finds

$$P_1 = \sqrt{P P_0};$$

and under this condition the former expression becomes

$$W_2 = \frac{2\,AT_0}{n}\left\{ \left(\frac{P}{P_0}\right)^{\frac{n}{2}} - 1 \right\}.$$

Now, if the compression had been carried out on the old plan, which is practically of the adiabatic order, we should have had as before

$$W_a = \frac{AT_0}{n}\left\{ \left(\frac{P}{P_0}\right)^n - 1 \right\}.$$

Taking Mr. Williams's example again, the receiver pressure would be

$$P_1 = \sqrt{59{\cdot}7 \times 14{\cdot}7} = 29{\cdot}62,$$

or 14·92 lbs. per square inch on the gauge. The relative efficiency would be

$$\frac{W_2}{W_a} = {\cdot}899$$

It appears, therefore, that by replacing the old system with that under discussion, we should in this instance reduce the compressor indicated horse-power about 10 per cent.

If the reservoir pressure to be attained were higher than in the preceding case we should possibly be justified in adopting triple compression; but it will be found that the saving to be effected by resorting to the additional third stage and cylinder is relatively smaller: that is to say, one gains so much by compounding and one naturally gains still more by tripling; but the additional gain by tripling over compounding may be practically insignificant if the reservoir pressure is not sufficiently high. The question is somewhat analogous with one in steam, where similar terms are used and boiler pressure takes the place of reservoir pressure.

The day is not distant when naval engineers, at all events, will be charged with the design and maintenance of compressors working up to enormous pressures for use in connection with torpedoes and pneumatic guns. As a matter of fact compressors working in four stages to ultimate pressures of 1700 and 2000 lbs. per square inch are already making or in use.

In three stage compression with complete receiver cooling we have for the work done in foot-lbs. per pound mass the following expression, since AT_0 is again equal to $P_2 V_2$ after cooling :—

$$W_3 = \frac{AT_0}{n} \left\{ \left(\frac{P_1}{P_0}\right)^n + \left(\frac{P_2}{P_1}\right)^n + \left(\frac{P}{P_2}\right)^n - 3 \right\},$$

where P_1 is the pressure in the lower receiver and P_2 is the pressure in the higher receiver. Determining these receiver pressures as before so that W_3 is a minimum we find

$$\frac{P_1}{P_0} = \frac{P_2}{P_1} = \frac{P}{P_2} = \left(\frac{P}{P_0}\right)^{\frac{1}{3}}$$

When these conditions are fulfilled, we have the same work done in each stage per pound mass of air delivered, or the total is

$$W_3 = \frac{3\ AT_0}{n} \left\{ \left(\frac{P}{P_0}\right)^{\frac{n}{3}} - 1 \right\}.$$

Similarly for quadruple compression and perfectly arranged intermediate cooling

$$W_4 = \frac{4\ AT_0}{n} \left\{ \left(\frac{P}{P_0}\right)^{\frac{n}{4}} - 1 \right\}$$

The receiver pressures are given by

$$\frac{P_1}{P_0} = \frac{P_2}{P_1} = \frac{P_3}{P_2} = \frac{P}{P_3} = \left(\frac{P}{P_0}\right)^{\frac{1}{4}}.$$

Suppose, for example, that the reservoir pressure is to be 2,000 lbs. per square inch on the gauge, and let us try a triple as against a quadruple compressor. We find

$$\frac{W_4}{W_3} = \cdot 9384\ ;$$

or the quadruple effects, a saving of about 6·16 per cent. over the triple compressor. The adoption under these circumstances of a quadruple system would therefore be justifiable on the score of efficiency alone. The gauge pressures in the receivers would be as follow :—

$$P_3 = 574{\cdot}12$$
$$P_2 = 157{\cdot}39$$
$$P_1 = 35{\cdot}59$$

Dealing with absolute pressures the rule is simply this:—Find the ratio of the reservoir pressure to the atmospheric, and extract the root corresponding with the number of stages: thus if there are three stages extract the cube root, and if four stages the fourth root. Divide the reservoir pressure by the number just found, and the result is the first receiver pressure. Divide the first receiver pressure by the same divisor, and the result is the second receiver pressure; and so on, always dividing by the same number.

Now let us turn for a moment to the motors, supposing such mechanisms are required. Shortly after the Cardiff meeting the writer met Mr. William Galloway (late H. M. Inspector of Mines, Cardiff), and in the course of conversation the question arose—What would be the meaning of a compound motor? On the instant the writer could only reply that it might possibly afford a better arrangement than obtainable in the simple motor when used in connection with auxiliary heating such as has been adopted to some extent in Paris. But in walking home it occurred to the writer that the auxiliary heat could be obtained from atmospheric or surrounding sources, thus in some part realizing Mr. Williams's speculation. In fact, the atmospheric cooling in the intermediate receivers of a multiple compressor may have its counterpart in heating in the intermediate receivers of a multiple expansion motor. Properly to appreciate this action we must understand that the available energy of unit mass of compressed air consists of two parts, as has been clearly pointed out by Mr. Williams, viz.; (*a*) what the writer calls reservoir or direct energy; and (*b*) the elastic energy. The part (*a*) may be referred to the somewhat complex regulating action of the reservoir; or more simply it may be regarded as energy directly communicated by the compressor pistons to the motor pistons. The part (*b*) is the portion of the intrinsic energy which is alone available; and since the intrinsic energy depends solely on the temperature, we can obtain the corresponding work only by destruction of heat. In other words, immediately the motor valve cuts off, work is performed at the expense of heat. Hence if we use a compound motor (with expansion in both cylinders) the high pressure cylinder will exhaust into the receiver at a temperature lower than the temperature of the atmosphere; and if we design this receiver with a large enough capacity and surface, the low pressure cylinder may take its air at a higher temperature than the temperature of the high pressure exhaust, that is to say nearly at the atmospheric temperature. The pressure in this receiver could be maintained constant by a floating loaded piston as in hydraulic accumulators.

The work done per pound mass in an ideal simple motor expanding down to atmospheric pressure may be written using the same notation as before

$$M_1 = \frac{AT_0}{n}\left\{1 - \left(\frac{P_0}{P}\right)^n\right\}.$$

If the motor is compounded and the receiver arranged for reregenerating to nearly atmospheric temperature the expression becomes

$$M_2 = \frac{AT_0}{n}\left\{2 - \left(\frac{P_0}{P_1}\right)^n - \left(\frac{P_1}{P}\right)^n\right\}$$

where P_1 is the receiver pressure. Determining P_1 by the condition that M_2 shall be maximum there results

$$P_1 = \sqrt{P\ P_0}\ ;$$

and giving P_1 this value the preceding expression becomes

$$M_2 = \frac{2\ AT_0}{n}\left\{1 - \left(\frac{P_0}{P}\right)^{\frac{n}{2}}\right\}.$$

Similar expressions hold for triple and quadruple expansion motors worked on this atmospheric regeneration principle: for example, the corresponding triple expansion formula is got on replacing the numeric 2 in the last expression by the numeric 3.

Suppose we take, for example, the reservoir pressure adopted in Paris, namely, six atmospheric absolute; and let us investigate the economy of a compound system of compressors and motors on our new principle making abstraction, for the sake of simplicity, of disturbing elements such as friction, leakage and clearance.

The ratio of the work done in the motors to the work done in the compressors will be

$$\frac{M_2}{W_2} = \frac{1 - \left(\frac{P_0}{P}\right)^{\frac{n}{2}}}{\left(\frac{P}{P_0}\right)^{\frac{n}{2}} - 1} = \left(\frac{P_0}{P}\right)^{\frac{n}{2}},$$

$$= \cdot 771\ ;$$

or the efficiency (ignoring certain losses) is about 77 per cent.

Similarly the efficiency of a triple system would be

$$\left(\frac{P_0}{P}\right)^{\frac{n}{3}} = \cdot 841,$$

or about 84 per cent.

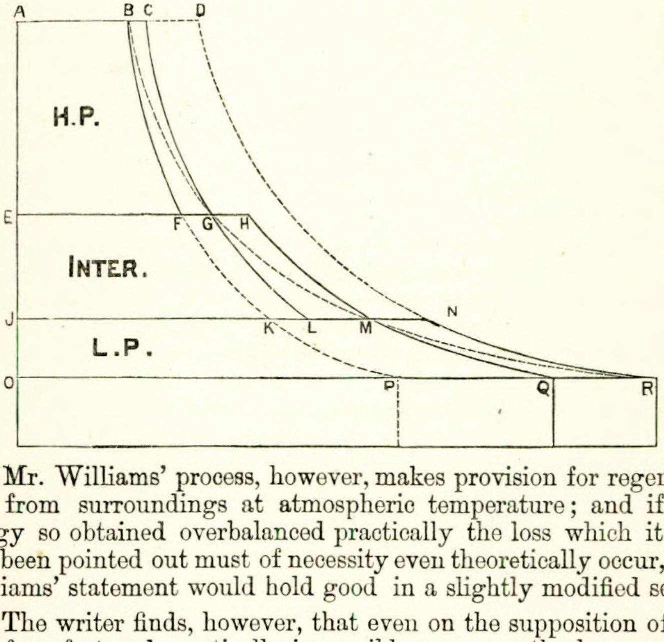
If we compare these figures with those applying to the system at present in use at Paris we find a marked difference. For in that case we have practically simple adiabatic compression and expansion; and the efficiency is

$$\left(\frac{P_0}{P}\right)^n = \cdot595 \quad :$$

that is to say the efficiency of transformation is only about 60 per cent. Or to re-state in another way: under present conditions the I.HP. of the motors falls short of the I.HP. given to the compressors to the extent at least of 40 per cent.; by the adoption of the new compound system in both compressors and motors this apparent loss would be reduced to nearly one-half, and by tripling on the same plan to nearly one-third of its former amount.

The accompanying figure shows the ideal combined indicator diagrams for a triple compressor and triple motor. OR is the atmospheric line; and JN, EH the lower and higher receiver pressure lines respectively. All the curved lines are adiabatics, with the exception of the dotted line RMGB which is the ideal isothermal compression line for the compressor, and expansion line for the motor. OR measuring the volume of one pound mass at atmospheric temperature and pressure, and RND being an adiabatic compression curve, the work done in the compressor per pound mass on the ordinary system would be represented by the area ORDA; and of this work there would be recovered in an ordinary motor (neglecting for simplicity certain losses) what corresponds with the area OPKFBA. The loss of work in the transformation is therefore represented by the area PRDB (using the corner letters). Now the compression curve on the triple system is the broken line RNMHGC, and the motor expansion line is BFGLMQ. Hence the loss of work in the transformation is represented by the sum of the three curvilinear quadrangles (taking the diagonal letters) CF, HL, NQ. The saving of work on the triple as compared with the ordinary simple system is therefore represented by the sum of the two areas CDNMHG and FGLMQPK.

The writer has hitherto refrained from criticising in any hostile spirit the statement made by Mr. Williams at p. 9 because, though the proof which is advanced contains a serious flaw, the point raised is of deep interest, and Mr. Williams is entitled to be credited with perceiving that we have here something worthy of discussion. The writer understands Mr. Williams to say this:— Inasmuch as the intrinsic energy of one pound mass of air is the same after and before compression and cooling, why should any energy on the whole be spent in mere compression? Now the natural reply to this question is a somewhat general one, namely: It is a law of nature that no continuous transformation of energy can occur without dissipation or degradation. To become particular, take the process proposed by Mr. Williams. Mechanical energy is employed to generate heat, and Mr. Williams speaks of this heat as theoretically recoverable in the form of work; but in so speaking he has apparently overlooked the second law which provides that in a continuous process, a certain portion only, depending on the lowest sink temperature available, can be so recovered.



Mr. Williams' process, however, makes provision for regeneration from surroundings at atmospheric temperature; and if the energy so obtained overbalanced practically the loss which it has just been pointed out must of necessity even theoretically occur, Mr. Williams' statement would hold good in a slightly modified sense.

The writer finds, however, that even on the supposition of the use of perfect and practically impossible processes the horse-power developed in the motors on Mr. Williams's plan would still fall short of the compressor horse-power; while on the other hand the economy calculated (86.46 $^{\circ}/_{\circ}$) might be to some extent practically approached by a triple stage system worked as above sketched.

The writer made some reference at Stratford to the part played by water in compressors and motors; but as his remarks have already extended to considerable length he must reserve the point for a future occasion. He cannot, however, conclude without venturing to congratulate Mr. Williams on the success of his paper.

PROFESSOR A. L. SELBY, M.A.

(CARDIFF.)

Professor Selby drew attention to the value of the constant γ (the ratio of the specific heats of air). He pointed out that it was necessary that such constants should be deduced, not from theory, which could give no trustworthy information as to their value, but from experiment. If theoretical results were employed in interpreting the results of experiment, they should be very carefully criticised. There were two principal methods by which γ had been found, the first resting in a comparison of the calculated and observed values of the velocity of sound in air, the second a method devised by Clement and Desormes. The first calculation of the velocity of sound in air was made by Newton, his result being only about $\frac{9}{10}$ of the true value. This was due to the assumption that the condensations and rarefactions of air take place without change of temperature, an improbable condition considering the rapidity of the changes of pressure.

Laplace supposed that each part of the air exchanged no heat with any neighbouring parts, a more probable hypothesis. On this assumption γ is found, but the view of Laplace cannot be considered to have been adequately criticised till Sir George Stokes showed that, unless the condensations and rarefactions took place either without loss of heat or without change of temperature, the loudness of a sound would diminish with extreme rapidity as the distance from the source of sound increased. The theoretical views implied in the deduction of γ by this means may then be considered as having been adequately criticised.

The second method of determining γ is as follows. Air is compressed in a closed receiver and the pressure noted, the temperature being that of the atmosphere. The receiver is opened for a moment and then closed. The temperature has then fallen, owing to the expansion of the gas; after a little time the temperature rises to its former value, and the pressure of the gas increases correspondingly. The observation of the initial and final pressures and of the atmospheric pressure suffices to determine γ.

THE LANGTHORNE ROOMS,
BROADWAY, STRATFORD,
January 24th, 1891.

PREFACE.

A Meeting of the INSTITUTE OF MARINE ENGINEERS was held this evening, presided over by Mr. F. W. WYMER (Vice-President), when the paper on "Capital and Labour," read by Mr. F. W. SHOREY (Member of Council), on Tuesday, 13th January, was further discussed.

The Paper, and Remarks made upon the subject matter with which it deals, will be found in the following pages.

The elements involved in the Discussion of such a question as "The labourer and his hire," are so many and various, that to venture on an opinion being formed from arguments brought within the compass of a single paper, would be both unwise and hasty, and it is not to be considered that the views expressed, either in the paper itself or the discussion, are endorsed by the Institute as a whole.

The paper and remarks are interesting and valuable, and are commended to the thoughtful consideration of members.

Should any member desire to contribute his views or remarks on this, or any other subject of interest, the Committee will be glad to receive such.

JAS. ADAMSON,
Honorary Secretary.