DISCUSSION ON "PRIME MOVERS." NEW YORK, FEBRUARY 19, 1909.

President Ferguson: This is a subject in which we all are vitally interested. There probably has been more done in the last five or six years in the development of prime movers than was accomplished in the previous 20 years. This serves to emphasize the importance of considering the question of depreciation in the broad way in which the author has approached it, and I wish to call attention to the happy manner in which he has expressed it. As an illustration of the truth of what he has said, I might cite something that may be of interest to you.

It is now six years since the largest steam-turbine power-house was completed in this country. There were at that time four turbines installed. Thirty days ago there was begun the dismantling of the first of these four turbines. That is a very short period of time in which to have had the turbine installation in use, but it bears out exactly what Dr. Steinmetz has said in his paper. The economy of the present turbine, compared with those which were installed six years ago, has so improved that it pays the company to scrap the turbines it put in at that time and install new ones of a more modern type; in other words, the saving to be effected, due to the increased economy of the new turbines, when capitalized, will more than pay for the cost of the new turbines. Not only has the economy of prime movers been improved, but their reliability as well, both contributing to place the producing and marketing of electricity and electrical apparatus on a more firmly established basis. This appeals, naturally, to those of us who are engaged in the operating department of engineering, carrying with it the great responsibilities not only to investors but to the general public, whose servants we really are, and we appreciate most highly the interesting and instructive manner in which the author has brought out the necessity for considering in all statements of cost the value of reliability.

Charles E. Lucke: It seems to me the first part of this paper is mainly a repetition of many elementary facts almost axiomatic, in which the only idea that strikes me as new is the proposition concerning reliability insurance. That seems worth while. As to the rest, I confess I am distinctly disappointed, especially in relation to the thermodynamic efficiency part. I have studied things of this sort a good many years, it is my own specialty, and it may be that I expect too much, but one fact I cannot overlook in this thermodynamic efficiency analysis is that thermodynamic efficiency, as it is taken up in the paper, referring only to the cycle, depends primarily on two things (a) the nature of the cycle, that is to say, the sort of phases and order of succession of the phases or processes, to use a better word, and (b) the extent to which each phase is carried.

The order in which the processes are carried out will affect

the efficiency, in fact it will determine it, with one exception, and that exception is the equation of condition that fixes the extent of the phases. If it is desired to adiabatically compress or heat at constant volume, the nature of the process is defined, and that is the first step in fixing the possibility of the cycle, the next step being, how much heat is added or how much the gas is compressed. What fixes the thermodynamic efficiency of the cycle, as such, is just these things: the nature of the processes, the order of the succession, and the extent of the heating, the expansion, cooling or compression.

The author has assumed certain cycles, which are very well known, which have been studied repeatedly, and which are in nearly all the elementary books. The study of these cycles is proper subject matter for a third year course, such as we have at Columbia, or, in a not very advanced institution, for a fourth year course. The author has assumed certain limitations to the extent of certain phases by fixing pressures and temperatures, purely arbitrarily, and the results and conclusions are worth no more than the assumptions. Any other assumptions that any one else considers reasonable will bring other results, and we might have endless controversy concerning the conclusions. Every man's conclusions would be as good as the others so far as one set of assumptions is as reasonable as the other.

The author has fallen back in two or three places on that old statement in every text-book, which is generally misunderstood: "The thermodynamic efficiency is essentially a function of the temperature difference, on which the engine operates." This is a variation on the statement of the Carnot cycle law, which is ordinarily set down as follows: the maximum efficiency obtainable for a given temperature range, is fixed by a certain expression, usually $\frac{T_2 - T_1}{T_2}$. As a matter of fact the

actual efficiency of any cycle has very little to do with this, has no more to do with it than my income has to do with the wealth of the nation, although the wealth of the nation certainly limits my income. It is quite true that Carnot's cycle yields an efficiency which is the highest possible for the temperature range, and it is likewise true that no gas cycle comes anywhere near the Carnot limit.

We are especially concerned with the gas cycle. The efficiency of any gas cycle is not a function of the temperature range, it is a function of, the nature of, the order of succession of, and the extent of each phase making up the cycle.

As engineers what do we care about the temperature range anyway? Do we approach the gas engine, the question which Dr. Steinmetz has finally interested himself in, by saying let us compress this gas to a certain pressure, and then have a constant volume rise to a temperature of 800 degrees cent. or

700 degrees cent., or 900 degrees cent.? We do nothing of the sort. We put into the gas mixture as much heat as we can, and do not care what the resulting temperature is. We put just as much heat into the gas as we can, to do as much work as possible in the cylinder, and the maximum temperature nobody knows the value of nor cares about. We do know how much work we can get, how much efficiency we can get, and also, that it has nothing to do with the temperature range.

It is possible, by taking proper assumptions for all variables, by setting up mathematical or other criteria by which to judge these results, for each man to arrive at results in his own particular way, and all different. If you will go through the literature on the subject you will find laws enough derived in this way to prove almost anything. Such cyclic analysis is almost as valuable as the Bible to prove things. An expression can be found to prove almost any statement, and another one to disprove it. Change the assumptions, always keeping them within the range of what you can induce some one to say is reasonable, and contradictory results can easily be obtained. I think as a consequence of this situation that this analysis of thermodynamic efficiency is not very valuable.

I have been carefully looking through the paper—for a conclusion. I have been trying to find out just what Dr. Steinmetz was trying to show, to throw, if possible, some light on the assumptions he uses, and I have not been able to find any conclusions. If the conclusion is that the gas engine cannot have as high an efficiency as the steam engine then the conclusion is absurd.

Henry E. Longwell: The opening sentences of Dr. Steinmetz's paper constitute a cheering message to those of us whose daily bread depends on the continuance of a demand for prime movers. When one who is not, and who cannot hope to be, versed in the intricacies of electrical science, hears and sees evidence of what he imagines to be mechanical work done by that phenomenon which we call lightning, he perhaps should not be too harshly criticized if he should have conceived the idea that electrical energy does exist in nature. It is not improbable that the school-books of our childhood days are largely responsible for the inculcation of this disquieting idea in the unsophisticated mind. By their persistence many of us have been led to unconsciously accept as classic, the crude experiments and deductions of that archaic philosopher and physicist, Benjamin Franklin.

During a life which is still a little inside the allotted span of three score and ten, I have seen so many impossible things happen, that for my own part, I am pretty well content to keep an open mind on all questions bearing on ultimate possibilities.

One can readily imagine a perfectly flat country, where the inhabitants could have no conception of hills and waterfalls. Such a country might be abundantly watered by lakes and

canals, and yet one could forgive the wise men of this hypothetical nation if they asserted that water-power was not found in nature; that water-power could be available only through the kind offices of a prime mover that would convert the energy of fuel or of air currents into mechanical rotation or reciprocation, thus producing an artificial head or pressure that would enable the water to do work.

Considering the marvelous developments in electrical science during the lifetime of even the youngest member of this Institute, is it safe to set a limit to the possibilities of centuries yet to come? May it not be that we are now coming ourselves only with electricity as a *medium* for the transmission of energy? We can with certainty and precision produce by mechanical agencies, a difference of potential which enables this thing we call electricity to reproduce in a large measure the energy expended upon it. But does this fact make absolutely unthinkable the proposition that there may be natural differences of potential, yet undiscovered? However, with the authoritative assurance that we can never hope to eliminate that troublesome yoke-fellow of the electric generator, the prime mover, who alone prevents the business of operating an electric power plant from being a rapturous dream of profit and pleasure, we can, with all the more satisfaction and determination, apply ourselves with renewed vigor to the task of reforming him.

As regards the determination of costs of electric power, this is not essentially different from cost accounting in any other industrial enterprise. Industrial accounting is the most flexible and accommodating science known to mankind. Set half-a-dozen men to working independently on as many different sets of industrial cost records, and you will get just that number of different costs, and the correctness of each and every one is susceptible of proof by the unimpeachable rules of mathematics.

The total cost of electric power begins with the water on the hilltop, or the fuel in the earth, and ends only with the light emitted from the lamps and the power delivered from the shaft of the motor. A single individual or corporation rarely deals with the entire chain of operations, and the cost of the electric power, and the relative importance of the several items entering into the cost, depend largely on how many links in the chain are under consideration. For example, the author in speaking of the interest on the investment as a factor of the cost, says "It is frequently very large with water power, due to the hydraulic development required," etc. It might be added that it is sometimes very low, in cases where a hydraulic development company delivers water at the plant.

If we are to accept the author's inference that the cost of electric power stops at the terminals of the generator, and that the prime mover includes everything from the generator back to "nature's stores", then we must perforce agree with his statement that "In the cost of electric power the electric machine

plays only a subordinate part; the essential element in determining the cost and the reliability of electric power is the prime mover."

There are some electric power enterprises that include expensive hydraulic development plants, or to speak more accurately, there are some large hydraulic development enterprises that include electric power plants as convenient transmitting mediums. I also know of one concern so happily situated as to be able to dispose of its output to a customer who furnished his own switchboard, transformers and transmission system. On the other hand, in how many instances does the owner or operator of an electric power plant get much nearer to "nature's stores" than the coal car on his railway siding, and in how many instances can he dispose of his output without delivering it on his customer's premises? Surely the instances are not so numerous as to warrant any such sweeping statement as that quoted.

I will not venture to intrude any discussion on the classification of electric power service into five different grades of quality. Your association may have under consideration the wisdom of setting up an official standard scale of badness. It might be useful to a manager of an electric power station as an instrument for convincing a complaining customer that he had no business to complain. To the manager whose plant is giving only mediocre service, it might be a comfort to know that he was still one or two degrees within the boundary of official recognition.

That part of the paper devoted to the thermodynamic consideration of heat motors, seems to be simply a brief resumé of several of the standard propositions that are demonstrated in most of the elementary text-books dealing with the subject. Those who have studied the mechanical theory of heat to the extent required in a modest technical course, will accept the statements put forth without argument. Those who have not a reasonable grounding in the subject must accept the statements as dogmatic.

We are introduced to a new unit of energy, which is referred to as the "kj." I am glad to be confirmed in my suspicion that this is a kilojoule, but it is a little difficult to see why it is used to the exclusion of the more conventional units that are commonly found in text-books on thermodynamics. As the value of a kilojoule differs from that of a British thermal unit by less than 6 per cent, we may accept it as a sort of "esperanto" term which conveys practically the same idea to those who think in metric measures and those who think in British measures. We also meet two old friends of the thermodynamic cycle family with new names. Isobaric and isovolumic would be convenient terms, were it not that the text-books recognize two other air engine cycles, those of the Ericsson and the Stirling engines, in which the isobaric and isovolumic features are re-

spectively combined with isothermal expansion and compression, instead of adiabatic. I wonder if I dare suggest as a solution of the difficulty, isobarabatic and isovolumbatic—words which if not more significant, are more sonorous.

Referring to the efficiency curves in Fig. 1, we naturally expect the curve for the Carnot cycle to stand at the top, as it is our standard of the highest efficiency attainable. We expect the cycles which are designated as isobaric and isovolumic to fall short of the efficiency of the Carnot cycle, because they do not receive all their heat at the highest temperature, or reject all of it at the lowest. We expect the isobaric and isovolumic cycles to be equal in efficiency, for the reason that while they deal with different total quantities of heat, the amounts received and rejected at corresponding temperatures are strictly proportional. We expect the curve for saturated steam to coincide with the curve for the Carnot cycle between the same temperature limits, for the reason that the ideal cycle for saturated steam is a Carnot cycle. The only reason the curves do not coincide in the figure is, that different minimum temperature limits have been selected. We expect a decided change in the steam-cycle curve, at the point where superheat begins, for the reason that we there depart from the Carnot cycle and begin to take in heat at varying temperatures.

At higher temperatures we expect this superheated-steam curve to fall below the isobaric and isovolumic curves, for the reason that a larger proportion of the total heat dealt with was received at lower temperatures.

The curves of relative output per unit weight do not uncover any surprises. It is the heat and not the substance, that does the work. As steam has a greater heat-carrying capacity than air, naturally the work done by the heat that can be put into a unit weight of steam will be proportionately greater than that done by the smaller quantity of heat that can be put into a unit weight of air, temperature limits being the same.

The output of the isobaric cycle we expect to be greater than that of the isovolumic cycle, just in proportion as the specific heat of air at constant pressure is greater than the specific heat at constant volume, which corresponds with the ratio of the figures given in Table II, column 1.

The expression $1 - \sqrt{\frac{T_2}{T_1}}$ for the efficiencies of the isobaric

and isovolumic cycles is novel, but applies only to the special case in which the temperatures at the end of adiabatic expansion and compression are equal, which satisfies the condition for maximum output per unit weight of air as the working fluid.

In the Carnot cycle for air, it is quite reasonable that for a given lower limit of pressure and temperature there is for each upper pressure limit a corresponding temperature that gives the maximum output per unit weight of the working substance. If

the temperature were increased to such an extent that it would require the expansion to be adiabatic over the entire pressure range in order to reach the lower temperature limit, then there could be no isothermal expansion, no intake of heat and consequently no work.

It will be also noted that it is not alone with air as a working medium that the use of the Carnot cycle involves high pressures in order to utilize moderate temperatures. By a curious coincidence, the temperature given by Dr. Steinmetz as best for an air cycle with 24 atmospheres maximum pressure, corresponds almost exactly with the temperature of saturated steam of the same pressure. And this is why we use superheated steam. We cease to attempt to approach the Carnot cycle at the higher temperatures, and turn to a less efficient one in order to avoid inconveniently high pressures.

Superheat is not theoretically the most efficient means for utilizing great temperature differences, with steam as the working fluid, but for the present it is the most practical means available, and is much better than nothing at all.

Thermodynamics is a very interesting and almost limitless subject, but thermodynamics and practical commercial engineering overlap only to a moderate extent. The cycle of the heat motor is but a small link in the chain of processes involved in converting the energy of fuel into mechanical energy; and if one concentrates too much on this one link, he is apt to contract his field of vision.

The proper selection of a prime mover depends more than anything else on that valuable human faculty that we call "horse sense",—which, like greatness, one may be born with, or may acquire, but which, unlike greatness, cannot be thrust upon him. It may be taken for granted that no manufacturer who has been prominently in business for ten or twenty years is putting out a distinctly bad heat motor.

The steam turbine may, for obvious reasons, without attaining prohibitive dimensions, be made large enough efficiently to utilize exceedingly low pressures and temperatures, when the low temperatures are available, but it does not follow that it must necessarily be so made. Because of the especial adaptability of the turbine for using large volumes of steam, and the excellent way in which the reciprocating engine handles steam at the small volumes consequent on high pressures, a high-pressure reciprocating steam engine compounded with a low-pressure steam turbine often presents a very attractive combination.

The mechanical difficulties involved in the attempt to utilize the full range of expansion of high-pressure steam in a single cylinder of a condensing engine, have long been recognized as being at least fully as important as the thermodynamic difficulties. We are not confronted alone by high mechanical stresses but by prohibitive clearances, unendurable leakage losses, and

the impracticability of a valve gear for the very short cut-offs that would be necessary.

The pressures in gas engines,—say 25 to 30 atmospheres—have not been found unduly difficult to cope with. The heavy reciprocating masses smooth down the peaks of pressure so that in general the mechanical stresses are well within the limits of conservative engineering practice. Gas producers are now obtainable which may be operated with the same degree of efficiency, reliability and continuity as the best boiler plants.

It can scarcely be claimed that one type of prime mover demands a higher grade of attendance than another, or that there is any serious difference in reliability as between the several types. The skill required to obtain the best results from the turbine or the gas engine, as compared with that required for getting equally good results from reciprocating steam engines is different in kind, but not necessarily in degree. Skilled attendants for steam turbines and gas engines are more uncommon than for reciprocating steam engines, but that is because we have been training attendants for reciprocating engines for 150 years, and the school has been a very large one. We have been training gas-engine and steam-turbine attendants for perhaps less than 30 years, and our educational facilities have until recently, been exceedingly limited in extent. There are dozens of men who can detect symptoms of trouble in reciprocating engines, and who can correct troubles before they become serious, to every one man who can do the same for a gas engine or a steam turbine. Yet the skilled gas-engine or steam-turbine attendant might not be able to display much resource in an installation of reciprocating steam engines. When we see a particularly fine piece of work, do we compliment the workman, or his tools? When we hear of an especially good record of electric power station service, both as regards reliability and overall economy, shall we compliment the prime mover, or the manager? The final overall efficiency and reliability of the plant depends on the human factor as well as the mechanical factor. The variations possible in the latter—considering, of course, only reasonably modern and accepted types of equipment—are so small as to be over-shadowed in the variations of the former.

Always the two-thousand-year-old parable of the talents is recalled to us. The talents differed not in goodness, and yet they produced widely different results, according to the respective efficiencies of the men to whom they were entrusted. And the parable would have been more realistic, and more in keeping with what we often see in our daily contact with the great and growing electric power industry, if he who was entrusted with only a single talent had, by his energy and foresight, equalled or surpassed him that was entrusted with ten.

David B. Rushmore: Almost every engineer has to do with a prime mover in one form or another, and questions arise in

that connection, whether the engineer is an operating engineer, or connected with a manufacturing company, or whoever he may be. One of the questions to be considered is, what shall be called the output or the rated capacity of these machines? This question nearly always arises in a comparison between machines manufactured by different concerns, where the basis for the rating has not been definitely fixed.

In the steam engine, for example, which is one of the oldest forms, we have a fairly definite idea when we speak of so many horse power output for the engine. The turbine, which is not as old, has a variable overload capacity, or an overload capacity not always expressed in the normal rating of the machine, and so there is considerable lack of definiteness which is sometimes confusing.

The gas engine is peculiar in a certain way, in that the rated output of the gas engine is one with a very small overload capacity. In comparing estimates on certain installations, which I have recently looked over, the engineers did not bear this fact in mind, and in certain industrial establishments which are now in process of construction, an active discussion is taking place as to which is the more desirable form of prime mover to use, in order to generate electrical energy from the gases of blast furnaces. This is the particular point I desire to speak on, not to reach any conclusion, because there is no conclusion which can be had at present. The factors which are going to form this conclusion are those not to be reached in a way which is positively acceptable.

A steam turbine may have a thermodynamic efficiency under the conditions of operation which will approximate 13 or 14 per cent. A gas engine under the conditions of operation may reach 24 or 25 per cent. The output in the case of the gas engine is approximately 100 per cent more than we will get in the other case, and naturally, as has been the fact, people have wished this output, but the lowest cost of electrical energy is not the real thing that the owners of this establishment are after. It is the lowest cost of their output, and the factors which should be introduced into the problem are depreciation, repairs, and interruptions to service. In making a final comparison we must have more or less of that information at hand. Reliability insurance, which Dr. Steinmetz speaks of, appears not exactly in the way in which he has presented it, but in the choice of machinery to be used. If we have engines which must be out of service frequently, if we have engines with very high charges for repairs, if we have engines on which the depreciation is very high, if we have an installation the cost of which is from 50 to 100 per cent greater in one case than in the other, a thermodynamic efficiency which is twice in one case what it is in the other, will not make up for the influence of the other factors.

Calvert Townley: I understand that our respected President and some of his advisors have inaugurated a movement at

Washington intended to preserve the water-powers of the country for the general public. It is proposed that when water powers are developed there shall be a payment made to the government proportionate to the output of such powers. I take this opportunity, therefore, to say that I believe such a course in many cases will absolutely prevent the development of water-powers, and, instead of benefiting the general public by swelling the government income, will retard the advance of any industries that might be assisted by cheap power.

I presume that the idea of a charge is born of the thought that water-powers are enormously profitable to the owner, and that he should not be allowed to monopolize our country's natural resources for private gain. It has been my experience in New England, gained both from operating some water-powers and in examining the commercial feasibility of others with a view to possible development, that in competition with steam, the majority of power developments are so heavily handicapped by fixed charges and operating difficulties as to make their commercial development of doubtful interest.

There is a decided similarity in many of the conditions affecting water-powers in the several New England states; for example, the rain-fall, the run-off per square mile of drainage area, and the relations between maximum, minimum and average flow. We can be fairly sure, for example, that the maximum flow of any stream will be about two-and-one-half times its average flow, not considering spring freshets, and that the minimum flow will sometimes drop to the vanishing point. Let us examine then the fixed charges of a water-power which it is proposed to develop for the sale of electric energy in competition with that produced by steam. Assume that this power can be developed to 80 per cent of the maximum flow of the stream at a cost of \$100 per kilowatt capacity, including hydraulic works, electric machinery and transmission line. This is a low figure for many water-powers, but is purposely so chosen for purposes of illustration. We cannot safely contract to deliver a peak load greater than the average flow of the stream. Therefore, if it costs \$100.00 per kw. capacity to develop 80 per cent of the maximum flow, which flow is two-and-one-half times the average flow, the development cost for the average flow will be \$200.00 per kw. The load-factor must next be considered. It is a fortunate company that can show a load-factor as high as 50 per cent,—that is to say, where the average 24-hour load is as much as one-half the peak load or station capacity. This means, then, that for the average current sold, which is really the unit that has to carry the full fixed charge, the investment is not \$200.00, but \$400.00 per kw.

In time of low water, the company must still be prepared to carry its peak load; that is, a load equal to the average flow of the stream. Last year the minimum flow was not to exceed 25 per cent of the average, requiring, therefore, a relay of at

least 75 per cent from some source other than the water-power itself. If this source be a steam plant, and the cost of such plant be taken at \$100.00 per kw. rated capacity, there must be added \$75.00 per kw. to our investment on which to base fixed charges, making the total development cost for each annual kw. sold, \$475.00, or at only five per cent interest, not including depreciation, an annual cost of \$23.75 over and above all operating expenses.

By contrast, a steam plant costing to build \$100.00 per kw., and having a capacity equal to the peak load and operated with the same load-factor previously assumed (50 per cent), must provide for fixed charges on \$200.00 only, or \$10.00 per year, taking the same interest as before. This shows at once a handicap of \$13.75 per year which the hydraulic plant must overcome by lower operating expenses, before the water-power is on all fours with the steam development. Where the load-factor is lower, or the cost of the hydraulic development is greater per kw. capacity, or where depreciation is to be provided for, or a larger steam relay necessary, any or all of which conditions are frequently met, the hydraulic plant labors under a still greater disadvantage.

It will readily be seen, I think, without pursuing this comparison further, that any arbitrary governmental tax or charge will lessen the attractiveness of water-power investment, and will so seriously handicap the development of a great number of powers as to entirely destroy their commercial availability.

My excuse for speaking of these obvious facts, which are known to many of you, is that there seems to be an entire lack of knowledge on this question by a considerable portion of the general public, resulting in the danger that unfortunate laws may be enacted tending to injure seriously an industry in which many of us are interested. Therefore, if you, as engineers, will, as opportunity offers, take pains to see that the fundamentals are understood, you can have a material effect on forming proper and intelligent public opinion.

Ernst J. Berg:* In concluding the discussion I shall not try to defend Dr. Steinmetz's paper nor the premises used in his arriving at the thermodynamic efficiencies, especially since I feel that it will give him great pleasure to do so himself. I will, however, add that when reading his paper this afternoon I found it most instructive and valuable. No doubt I have seen these thermodynamic cycles described in text-books, but it is so many years ago that I have forgotten them, and I am sure that they were not given in the concrete form that Dr. Steinmetz has presented. I know that there are other members in the same situation, and to them as to me the paper has a charm of novelty and is very interesting.

Dr. Steinmetz says, if I am not mistaken, that the limits of

* In the absence of the author on account of illness, the paper was presented in abstract by Mr. Berg, who replied as follows:

pressure and temperature chosen by him in working out the thermodynamical cycles might be somewhat varied, but he also states that the conclusions will not be much affected by such variations as long as they are reasonable, and I believe him. A temperature limit of 800 degrees cent. seems as high as could well be used, since I believe this to be the temperature at which the cylinders begin to be dull red.

As to the pressure, it seems that 24 atmospheres or about 350 lb. is as high as is good for conservative engineering, therefore, I find nothing wrong about the assumption. The efficiencies are finally based upon cycles which give the maximum work for given cylinder dimensions, which seems also very fair. When finally the tables showed that the efficiencies, mechanical as well as thermodynamic, were consistent with known facts, I was satisfied there could not be any criticisms on these scores.

Professor Lucke said that there were no conclusions given in the paper. That is true. I do not think Dr. Steinmetz wanted to give any, I do not think he wanted to say that any given cycle or type of prime mover was best. He made a fair comparison between the various prime movers.

Charles P. Steinmetz (by letter): As far as I can understand, Professor Lucke's lengthy but rather unintelligible discussion is a general denial. In short, he says: the assumptions of the paper are wrong, the methods of reasoning are wrong, and the conclusions are wrong; while the second speaker, Mr. Longwell, explains in great detail, that the assumptions of the paper, the methods of reasoning, and the conclusions are old and well-known, and their correctness demonstrated in every elementary text-book, so much so that they do not need to be stated. As the statements of these two speakers thus contradict each other, I do not need to discuss them further, but merely desire to point out to Professor Lucke that the study of typical thermodynamic cycles is not useless, because no heat engine exactly follows any of these cycles. In electrical engineering also, no existing transformer exactly agrees with the typical transformer diagram, nor any existing transmission line with the equations of transmission lines. Even Ohm's law is not rigidly obeyed by any industrial circuit, as secondary phenomena, such as temperature changes, always occur. Nevertheless, we use these laws and methods in design and calculation, and the history of electrical engineering has amply justified us. Phenomena of nature are always complex, and thus can never agree completely with any typical representation; but it is the province of the engineer to find the closest theoretical representation of the actual facts, and then bridge over the gap by engineering judgment.

While Mr. Longwell desires the thermodynamic cycles omitted from my paper as obvious, for Professor Lucke I should have added an extensive discussion on the relations of the theoretical thermodynamic cycles to the actual cycles in heat engines. I

have thus chosen the middle ground, illustrated the reasoning on the typical thermodynamic cycles, and left it to the intelligence of the reader to realize that the actual engine cycles are intermediate between, and combinations of, the typical cycles, and hence are covered by the results of the typical cycles.

Professor Lucke's statement, that the temperature range has no effect on the efficiency of the thermodynamic cycle, obviously is wrong. If his idea of gas-engine design is to put as much heat into the cylinder as possible, and see what power he gets out of it, and he does not care to know what the temperature is; this obviously does not prove that the temperature is immaterial.

In general, an efficient designing engineer endeavors to know not only what is put into, and what comes out of an apparatus, but still more, what takes place in the apparatus. The rapid advance of electrical engineering is to a large extent due to the fact that the designing electrical engineer was not satisfied merely to put as much mechanical power into the machine as possible, and measure whatever electrical output he got, but has carefully studied the internal reactions of the apparatus, and thereby was enabled intelligently to choose and predetermine the actions, with the result that the waste of power in electrical apparatus has been reduced to a negligible quantity.

However, Professor Lucke's method of gas-engine design, by putting as much heat as possible into the cylinder, is merely a repetition, in different words, of the assumption which I made in my paper for the air cycles. These cycles I consider for the conditions of maximum output, where the greatest amount of heat is put into the cylinder, which still increases the output.

The claim made by Professor Lucke and Mr. Longwell, that the assumptions made in my paper are arbitrary, and by a different set of equally justified assumptions any other conclusion can be reached, is wrong; the assumptions made in my paper are not arbitrary, but, as proper in such investigations, I have chosen the conditions most favorable for each case. That is, where the efficiency or output increases with the temperature, I have chosen the highest temperature; where the efficiency or output increases with the pressure, I have chosen the highest pressure permissible by reasonably conservative design. The only feasible variations of the assumptions thus are minor changes: some engineers may consider 24 atmospheres too high, and prefer 20 atmospheres as a more conservative limit, while another engineer may be willing to allow pressures up to 30 atmospheres. As I have stated in my paper, however, the results and the conclusions, are not appreciably changed by any reasonable change of my assumptions.

Incidentally, Mr. Longwell is mistaken in his statement that equality of the two intermediate temperatures of the air cycle fulfils the conditions of maximum output per unit weight of air. This happens to be the case for the two cycles specifically

discussed in my paper, but not in general. For instance, for the air cycle of constant pressure admission and constant volume exhaust, in the conditions of maximum output—which is the assumption of my paper—the two intermediate temperatures are not equal. His statement that the efficiency of the isobaric cycle of saturated steam is that of the Carnot air cycle between the same temperature limits, is not quite correct either, but the efficiency of the steam cycle is slightly lower, though the difference is usually small.

§ My paper discusses the economy of the production of electric power from nature's stores of energy to the switchboard. If one corporation develops the water-power, another one uses it by buying the water of the former. This obviously does not change the cost of development etc., and you do not get water-power without cost of development, as Mr. Longwell seems to think, if you let somebody else develop the water-power, and pay him for the cost of development in the price of water bought of him. The economic considerations from the station switchboard to the light radiated from the customer's lamps, are equally important, as Mr. Longwell states, but they are not the subject of my paper.

I am sorry that Mr. Longwell thinks the kilojoule a new unit: the kilojoule, or kilowatt-second, is, and has been for some decades, the international unit of energy, used throughout the world, not only by the electrical engineers, but also by other engineers, such chemists as Ostwald, and others, with the exception of that rapidly dwindling group of mechanical engineers in the two English speaking countries, who still prefer to measure distances by what in the earlier middle ages was considered the proper length of a king's foot, and to measure temperatures by a scale using as zero the lowest winter temperature of Northern Germany, and as 100 degrees an erroneous measurement of blood heat (Fahrenheit). While the two English speaking nations have not yet reached the courage of completely freeing themselves of the curse of the ancient "English system" (so-called because all the other civilized nations have long ago discarded it), with its seven or more incompatible units of length* and other curios, I would not impose such a system on the American Institute of Electrical Engineers which already years ago has gone on record as endorsing the metric system, by a ballot vote of over 90 per cent majority.

It is true, that in the English system, when expressing the energy of fuel in British thermal units, the output of the boilers in pounds of steam per hour, the indicated energy of the engine in foot-pounds, and the output at the switchboard in kilojoules, the numerical values are not so disturbing to the self-satisfaction of the engineer as when throughout the entire transformation, the energy is expressed by the same units, and the inefficiency of the performance thereby shown in all its nakedness.

* Foot, mile, chain, rod, fathom, knot, yard, etc.

But, whether such a hiding of the inefficiency is to the advantage of the art, is questionable.

In regard to Mr. Longwell's objections to my statement that electric energy is a secondary form of energy, I shall be glad to withdraw this statement, as soon as Mr. Longwell will have succeeded in harnessing the thunder cloud, and in lighting the cities from nature's stores of electricity, from lightning, or from the earth currents, or whatever other of nature's stores of electricity he has in mind for industrial use.