The Progress of the Internal Combustion Engine and its Fuel.*

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THE progress of the internal combustion engine has been, I think, one of the most startling developments of the last fifty years. It appeared so soon as a suitable fuel was available. At first it used gas and sat still; then petrol appeared, and at once it began to run about, and later to grow wings. Within a few years it has revolutionised all forms of transport, except the railways, for these are ever haunted by the ghost of George Stephenson, who might shake his head and rattle his chains at the mere suggestion of anything more modern or efficient that a non-condensing steam engine. To-day, well over 80 per cent. of the total power output of all forms of prime mover uses petrol as its fuel, and I am going to claim the privilege of the older generation who have watched the petrol engine almost from its birth, to be reminiscent on the subject of petrol.

I first drove a car and so came into contact with petrol in 1898. The engine of this car was an ordinary horizontal single-cylinder gas engine, with all its working parts open to the winds of heaven and the dusts of the earth below; exposed to everything, in fact, except lubrication. Its supply of gas was drawn from what used to be termed a wick carburetter—an enormous vessel kept about one-quarter full of petrol in which were suspended dozens of lengths of lamp wick. Some of the air supply was drawn past these wicks, and was saturated with petrol vapour, while the remainder passed direct to the inlet valve of the engine—thus a combustible mixture was obtained, but seldom, if ever, did the engine and I agree as to the definition of a combustible mixture, and the engine always got the better of the argument. It was an unfair debate, for the engine always had the last word, spat scornfully, and then sulked. In time, one developed a sort of sixth sense and was able to feed the engine with one hand and steer with the other.

The wick carburetter died before the end of the last century, but during its tiresome and didactic life it determined the specification for petrol, a specification which endured for nearly twenty years after its decease—such was the terror it had inspired. The wick carburetter clamoured for extreme volatility, and volatility was expressed in terms of specific gravity. The lower the gravity the better the fuel, and for many years the quality of petrol was determined solely, or almost solely, by its specific gravity. About the year 1900, the wick or surface carburetter was superseded by the spray type, which was far less pernickety about volatility, and the engine, with its major grudge removed, began to invent new grievances, while the driver had more leisure to listen to them. Fed with a more or less combustible mixture and using a higher compression, the engine then proceeded to knock, at first hesitantly, and later most insistently. Such knocking was diagnosed as premature ignition and was ascribed invariably to the overheating of some point or surface within the cylinder, a belief which became almost a creed, despite the manifest fact that better cooling proved of little avail, that no auto-ignition occurred when the spark was switched off, and that no one could explain adequately why premature ignition should cause a high-pitched ringing knock. About 1904, as an undergraduate at Cambridge, I was assisting the late Professor Hopkinson in the development of a piston-operated optical indicator for high-speed engines, and in the course of our experiments we used to test the indicator impartially on the various steam, gas, and petrol engines in the laboratory. On the petrol engines we found that whenever the knock occurred, the indicator registered a very large and sudden increase of pressure, far above the normal maximum, the mirror was often shattered, and the indicator spring permanently strained; no similar phenomenon occurred in the case of the gas engine. Hopkinson, with his utter disregard for accepted creeds, scorned the belief that the knock was due to pre-ignition, and concluded that it was to be accounted for by gaseous shock, since, as he pointed out, there was not heat energy in the working fluid sufficient to produce the high pressures indicated, nor could premature ignition show such evidence of shock. As an experiment, he fitted into a gas engine cylinder a long steel bolt projecting far into the combustion chamber; this soon reached a temperature sufficient to cause genuine premature ignition. With this arrangement, we were able to watch, with the indicator, the gradual transition from normal to premature ignition, which advanced as the bolt heated up until it became so early as gradually to pull the engine up altogether. When watching such a process, we noticed three interesting points:—

First, that genuine pre-ignition was accompanied by a dull thud, but no trace of the ringing knock was found in the petrol engine.

Secondly, that the maximum pressure never, under any circumstances, exceeded the normal maximum.

Thirdly, that the rate of rise of pressure as recorded by the indicator was no more rapid than the normal.

This experiment sufficed to confirm Hopkinson in his belief that the knock in the petrol engine was due to something quite distinct from pre-ignition, and he described it rather as detonation. This was some thirty years ago. Unfortunately, Hopkinson seldom published his researches, and the widespread belief that the knock in the petrol engine was due to pre-ignition remained unchallenged by all but a few of his disciples. So far as I can remember, he did not pursue the matter further, for he was not at that time particularly interested in petrol engines, but without doubt, I think he was the first investigator to come to the conclusion that the knock in the petrol engine was due to the shock of a gaseous wave striking the walls of the cylinder, and this he attributed to some peculiarity of the fuel. During the same experiments with his indicator in 1904 and 1905, Hopkinson called my attention to the very great difference in the rate of burning of the same working fluid in an engine cylinder and in an explosion vessel. Bomb explosions had shown that the normal rate of burning of a gaseous mixture was so slow that it would not be half completed by the end of the stroke in even quite a slow-speed engine. So far as we were aware, this glaring inconsistency had been overlooked by previous observers, though Sir Dugald Clerk was at the time puzzled about it. Hopkinson explained it by arguing that the rapid spread of burning in the engine cylinder was due to the rapidity of movement of the gases—in fact, to turbulence—though this term was coined later. To test his theory, he fitted an electric fan inside an explosion vessel, and by speeding up the fan to a very high speed was able to obtain a rate of burning as rapid as that in an engine cylinder. Some years later, Sir Dugald Clerk carried out his now classic experiment which was the converse of this, for he allowed the working fluid to stagnate in the engine cylinder during several idle cycles, and then ignited it in the usual manner, with the result that the rate of burning was so slow as to be less than half complete by the end of the expansion stroke. To Hopkinson, then, I think, should be ascribed the discovery of the existence of detonation as a distinct phenomenon, and of the vital importance of turbulence; as his pupil and disciple, I accepted these as quite understood and matter of fact, and was surprised to find, after leaving Cambridge, that they were regarded elsewhere as outrageous heresies.

While at Cambridge, and in accordance with Hopkinson's teaching, I had built for my own amusement a two-cycle petrol engine, in which I had sought to combine intense turbulence and stratification—the former in order to obtain very rapid burning and high efficiency, and the latter, in part, for the sake of efficiency, but also because, with so large a proportion of unscavenged exhaust products, I needed a considerable measure of stratification to give that degree of flexibility I was striving after. This engine ran well on the whole, but suffered severely from excessive detonation—so much so, that I had to reduce the compression ratio to a very low figure (below 4 : 1), and thus lost by inadequate expansion much that I had won by stratification and high turbulence. After leaving Cambridge in 1906 I carried on experiments with this engine in my spare time, and determined to try and cure the persistent detonation trouble. I repeated Hopkinson's experiment with the hot bolt and found exactly the same results; the power fell away due to premature ignition, but in the case of this two-cycle engine, without even a trace of bump, the engine just silently faded out. Incidentally, in this engine, the detonation was at times so severe that it used seriously to damage my Hopkinson indicator, and, acting on Hopkinson's theory that detonation was the setting up of a gaseous wave, I sought to screen the indicator from impact by the wave, and found that this could be done in various ways. When thus screened, the indicator gave almost identical diagrams, whether the engine was detonating or not, the abnormal maximum pressure no longer appeared, nor, so far as I could see, was there any appreciable difference in the general rate of pressure rise. When severe detonation was allowed to persist, I found that the power output of the engine gradually fell, and eventually, if the throttle were resolutely held wide open, the detonation noise would die away and the engine would fade silently out, due to premature ignition. All these and other observations convinced me that Hopkinson had been perfectly right in his diagnosis and that detonation was a phenomenon wholly distinct from pre-ignition, but that if allowed to persist long enough, it would degenerate into premature ignition. I was keen to make a success of this two-cycle engine, and carried on experiments in my spare time from 1906 till about 1911, during which period I constructed several revised versions of it. With the help of Hopkinson's indicator, I learnt a great deal about the effect of turbulence and stratification, and gradually formed a theory as to the mechanism of detonation. I then came to the conclusion that the explosion wave was set up by the spontaneous ignition, not of the charge as a whole, but of the very last portion only, due to rapid and almost adiabatic compression by the advancing flame front. I came also at that time, about 1909 or 1910, to the regrettable but quite mistaken conclusion, that turbulence was incompatible with freedom from detonation, and that I must sacrifice some of the former to get rid of the latter. By one of those examples of blind stupidity to which I hope I am not alone subject, I failed, till years after, to grasp the obvious moral of my own theory, and it was not till several years later that I realised how important a part the shape of the combustion chamber and the position of the ignition plug must play. My original combustion chamber was, from the point of view of detonation, one of the worst forms imaginable, and it now seems quite incomprehensible that, having concocted and pinned my faith to a perfectly good theory as to the mechanism of detonation, I should for so long have failed to see that the form of combustion chamber I was then using outraged this theory in almost every respect. About 1910, I think, benzole came on the market, and I found to my delight that with this fuel detonation disappeared entirely, and I was able, even with my abominable design of combustion chamber, to increase my compression ratio from a miserable 3·8 : 1 to well over 5 : 1, and so obtain a really fine performance from the engine. This observation naturally caused me to focus on the nature of the fuel as the primary factor in detonation.

After leaving Cambridge, my legitimate work was civil engineering, and my experiments had to be carried out in my spare time and merely as a hobby. From 1910 to 1913 I had, through pressure of other work, practically to abandon these investigations. In 1913–14 I made a fresh start, and this time I built an experimental single cylinder supercharged four-cycle engine. In this engine I again sought to work with a stratified charge, and with this in view the supercharge was admitted through supplementary ports uncovered by the piston at the end of the suction stroke. This engine behaved very well indeed; and though it had no direct means of varying the compression ratio, I could vary the compression pressure by control of the amount of supercharge added after the end of the suction stroke, and, thanks to the stratification, which proved very effective, I could vary the mean mixture strength over an exceptionally wide range. For these reasons, it proved an admirable research engine, more particularly for studying the effect of different fuels on detonation, in which I had become greatly interested, for by this time I had become convinced that it was the incidence of detonation and the incidence of detonation alone, which was setting a limit and a very early limit at that date, to the performance of petrol engines. By increasing the supercharge, or by enriching the mixture, or both, I could, at will, produce or suppress detonation on almost any fuel. From tests on this engine during 1913–14, I found that, of the available coal tar derivatives, toluene appeared to be the most resistant to detonation, while as to the various petrols then on the market, these varied widely, but that, in general, the higher the specific gravity, the less the tendency to detonate.

During the war I came a good deal into contact with Sir Robert Waley Cohen, who lent a patient ear to my theories. From him I was horrified to learn that in Borneo many thousands of tons of petrol were being burnt as a waste product, merely because its specific gravity was considerably above that permitted by existing specifications. He arranged to send me a few gallons of this fuel, which I tested in my supercharging engine, and found that I could run without detonation to a much higher supercharge than on any petrol I had used hitherto. Sir Robert was, I think, very much impressed by this observation, for he then asked me to consider and prepare proposals for a comprehensive research into the whole subject of fuels for petrol engines, and promised both the financial and technical assistance of the Shell Company. I had long dreamed of building a really efficient variable compression engine for such a purpose; to this, to my delight, he readily agreed, and with the help of Tizard, Pye, Kewley, Thornycroft, and others, a research programme was set on foot, the results and main conclusions of which were published at Sir Robert's request in 1921.

In my pre-war experiments I had found that toluene showed the least tendency to detonate of any fuel I had tried, and paraffin base petrols the greatest tendency. In order to provide standards of comparison the Shell Company prepared, by sulphonation, a large bulk supply of aromatic free petrol, and they provided also a bulk supply of toluene. The variable compression engine, known as E.35, was completed early in 1919 and proved a very satisfactory instrument; with a little practice, we were able to determine the compression ratio at which detonation occurred to within about one-twentieth of one ratio of compression, and thus to detect very small differences as between different fuels. As a standard of comparison, we decided to express the tendency of each fuel to detonate both in terms of the toluene number of an equivalent mixture of toluene in aromatic free petrol, and also in terms of the actual ratio of compression at which detonation occurred in this particular engine under one particular and standard set of conditions. So long as the original bulk supplies lasted, the toluene standard was quite satisfactory, but it was, of course, realised that an end would come eventually when neither the original aromatic free petrol nor the toluene could be matched exactly and that the need would then arise for the use of chemically pure substances. To-day, as is well known, heptane has taken the place of aromatic free petrol, and iso-octane that of toluene. To-day, the detonation tendency of a fuel for petrol engines is expressed in terms of its octane number, and the octane number of a fuel is now generally recognised as being by far its most important characteristic. Specific gravity has been forgotten and volatility relegated to a very poor second place. By scientific blending of fuels from the various sources of supply, by the addition of lead tetra-ethyl, benzole or alcohol, the octane number of fuels has risen steadily year by year, and as it has risen, so has the performance of the petrol engine marched accordingly.

The reflection of the use of high octane or non-detonating fuels upon engine performance can best be seen in the field of aircraft engines. In 1920, the best petrol for aircraft engines allowed only of a compression ratio of less than 5·0 : 1 with no supercharge. To-day, with fuels of an octane number of 87 or thereabouts, we can employ a compression ratio of well over 6 : 1 and can superimpose on this a very considerable supercharge, with the net result that we can now operate throughout a prolonged test with brake mean effective pressures as high as 185 lb. per square inch, and attain, under cruising conditions, a fuel consumption as low as 0·44 lb. per b.h.p. hour, and this with a specific weight of only about 1·3 lb. per horse-power reckoned on the ground level or take-off power. If, as seems likely, the octane number of aviation fuels is to be increased in the near future, the performance of petrol aviation engines will receive a further very large impetus. If we review the development of the aero engine in all countries since the war, we find that the specific output of the average of the leading makers increased only from about 14 h.p. per litre in 1920 to 21 h.p. per litre in 1930, an increase of 50 per cent. in ten years. The last four years has seen an increase to 30 h.p. per litre, while, during the last few months, using 87 octane fuel, type tests have successfully been completed at a specific output of 40 h.p. per litre. Once we depart from standard aviation petrol and allow the chemist a free hand to use alcohol or acetone, as we did in the case of the Schneider engines, we can attain 55 to 60 h.p. per litre even from an engine of about 3 litres capacity per cylinder. It would seem, therefore, that the performance of the petrol engine is mainly one of dietary and that it is to the chef rather than to the engineer to whom we must turn our attention. I have said that to-day we must turn our attention to the chef, but while the chef's menu may be invigorating, it is also apt to be indigestible, or even poisonous—we have known only too well the pains of lead poisoning—and the engineer has his work

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cut out for him to improve the digestive organs. We have found that the sleeve valve helps greatly in this respect, for in the first place, the abolition of the hot exhaust valve allows us to use nearly a whole ratio higher compression or a corresponding increase in supercharge on the same fuel; in the second, the sleeve is impervious to lead poisoning. With experimental sleeve valve units of present-day aircraft dimensions, we have been able to reach and even to run for long periods at a specific output of 100 b.h.p. per litre on specially prepared fuels, and well over 60 h.p. per litre on an 87 octane fuel. The former figure corresponds with a brake mean effective pressure of 540 lb. per square inch, and a supercharge pressure of 4 atmospheres absolute. Apart from the question of fuels, the exhaust valve and the piston control the performance we can obtain to-day, but the exhaust valve and the fuel are intimately connected, in that the higher the compression the fuel will allow us to use the greater the expansion and the cooler will the exhaust valve keep, so that an improvement in fuel eases the conditions of the exhaust valve, and thereby allows of a yet further improvement in performance. In the sleeve valve, this limit is removed and we are free to ring the changes between compression and supercharging as we will. When a very high performance, regardless of fuel consumption, is required, we can, if we so desire, employ a low ratio of compression and a very heavy supercharge, and thus obtain an enormous specific power output, a thing not possible in the case of a poppet valve engine.

It is now being proposed to increase the octane number of aviation petrols to 100, and it is interesting to speculate as to the probable effect upon engine design which such an increase will bring about. Already with 87 octane fuel we can employ a compression ratio of about 7·0 : 1, and whatever the fuel, it will not pay us, I think, to use a higher ratio than this for, as between 7 and 8 : 1 the gain in power output and fuel economy is barely 5 per cent., while the maximum pressure is increased by 150 lb. per square inch for a 5 per cent. gain in mean pressure. If, therefore, we further increase the compression ratio, the small gain in efficiency will be offset to a large extent by the increased engine weight necessary to cope with the higher maximum pressure. It is pretty clear, therefore, that we shall seek to reap the advantage which a higher octane fuel offers by increased supercharge rather than increased compression ratio. If the octane number of the fuel be increased from 87 to 100, then we shall be able, by supercharging, to increase our specific power output by a further 45 to 50 per cent. before reaching the limit set by detonation; but with so heavy a supercharge, we shall have, I think, to reconsider somewhat our cycle of operation, and it is interesting to speculate as to how best we could make use of such a fuel. I have said earlier that our limits to-day are the intensity of heat flow to the pistons and exhaust valves. If by supercharging we are going to increase the specific output by 50 per cent., we shall increase the heat flow to these vulnerable parts to very nearly the same extent and, in the light of present knowledge, we shall pass the limit of their endurance. Our most effective method of removing heat from these two hard-pressed members will be by scavenging through the clearance space with cold air at the end of each exhaust stroke. This can be done by providing a limited and perhaps variable amount of overlap between the inlet and exhaust valves (or ports in the case of a sleeve valve). In this manner relatively cold air from the supercharger will sweep over the top of the piston and out through the exhaust valve, at some cost in wasted energy, but I think that by suitable mechanical design the cost can be kept down to a very low figure, while we shall reap an advantage in that the clearance space will be left charged with air available for combustion. If, in order to cool the internal parts, we decide to employ scavenging, then it is clear that we must scavenge with air alone, for we cannot afford to waste fuel. This, in turn, means that we must abandon the system of carburetting the whole of the air and substitute timed and measured fuel injection. With the experience available to-day with high-speed Diesel engines, this should be comparatively easy, for we need not be nearly so meticulous about timing nor have we to deal with anything like such high fuel pressures. Yet, again, with so high a supercharge, the amount of energy available in the exhaust will be very large. To-day we are carrying out the compression in two stages, and the expansion in one, and in so doing we are outraging all the thermodynamic laws. If we are going considerably to increase the first stage of compression, then it follows, I think, that we must introduce a second stage in the expansion also, preferably by the introduction of an exhaust-driven turbine which, in turn, will operate the supercharger, as in the Buchi system. With two-stage compression and only single expansion, both the temperature and volume of heat will probably be too much for the turbine, and we shall have to dilute the exhaust with cool air—in other words, we may have to resort to scavenging, not only for the sake of the piston and exhaust valves, but for that of the turbine also. The existence of the exhaust turbine and the back pressure it will create will serve also to control the amount of scavenge at varying altitudes. I am tempted to predict, therefore, that if fuels of 100 octane number become available for aircraft, we shall not increase our compression ratio beyond the present-day figure, but rather we shall employ a heavy supercharge provided by an exhaust-driven turbine combined with air scavenging and liquid fuel injection.

In the pleasure car field, I am rather doubtful whether there will be any further gain by increasing the octane appreciably above that of present-day No. 1 petrols. The small dimensions of the cylinders and the increased knowledge as to combustion chamber design already allow us to use as high a compression as is compatible with smoothness of running, and I very much doubt whether superchargers will be employed as a general practice, except in the case of sporting and racing cars, which should come under the same general category as aircraft and use the same fuel. In the case of heavy commercial vehicles, where fuel economy becomes a vital consideration, the high-speed Diesel engine using heavy oil is rapidly ousting the petrol engine, and the trend of the last few years indicates that it will probably supersede it altogether, for even though the two fuels now cost practically the same, the Diesel engine still has an advantage of about 70 per cent. greater mileage per gallon—an advantage which more than offsets its greater initial cost and somewhat greater weight.

Thus far, I have discussed only the fuel question in relation to spark ignition engines, but I would like to say a few words about the newer development of high-speed Diesel engines and their fuels. In this field the relation of the fuel to the engine is much less intimate. In the case of the spark-ignited petrol engine, not only the specific power output available, but even the whole general design of the engine, depends almost entirely on the octane number of the fuel, so much so, in fact, that an engine designed to make the best use of 100 octane petrol will develop just double the power output of one designed for 70 octane petrol. No comparable relation exists, or is ever likely to exist, between the fuel and the engine performance in the case of compression ignition engines.

In the case of the petrol engine, we are working normally with a relatively low ratio of compression and, therefore, on the steep part of the air cycle efficiency curve. Any increase in octane number allows us to work further up the efficiency curve, and it allows us also to increase the amount of supercharge. In the case of the compression ignition engine, our ratio of compression is already such that we are working on a nearly flat portion of the curve, and our efficiency is virtually unaltered over a wide range, while, as to supercharging, since we are unlimited by the incidence of detonation, we can supercharge as much as we like and to an equal extent with any fuel. Our limit in this case is determined solely by the maximum cylinder pressure we are prepared to tolerate and is independent of the fuel we use. In other words, if it will run at all, a compression ignition engine will give substantially the same power output and efficiency on any fuel which it can burn. It is in regard rather to secondary factors, such as silence, ease of starting, &c., that the nature of the fuel is likely to have any influence.

In the petrol engine a ready prepared combustible mixture is ignited by the passage of a single spark which leaves behind a thin thread of flame; from this single minute nucleus inflammation extends, slowly at first, but with increasing rapidity until a stage has been reached when it can be spread, by turbulence, throughout the whole of the combustion chamber. Once the initial nucleus has been established, the subsequent rate of spread of flame is a function of turbulence and of turbulence alone. For this purpose the turbulence must be in the form of a general rough and tumble, in order to distribute the flame throughout the ready mixed combustible mixture. In the case of the compression ignition engine, ignition takes place from the surface of a vast number of liquid droplets all travelling in definite directions, and the problem before us is to bring air to these droplets at at least as great a rate as it is being consumed by the burning fuel. To this end we require not indiscriminate turbulence, but an orderly movement of the air across the stream of fuel. Mere turbulence as in the petrol engine is of little help to us, since what we must aim at is that as much as possible of the air within the cylinder shall pass across the stream of burning droplets, and this can be attained best by setting the air within the combustion chamber into a unidirectional flow more or less at right angles to that of the fuel. In the petrol engine, combustion may be regarded as taking place in two stages—first, the building up of a self-propagating nucleus of flame, and, secondly, the spread of that flame throughout the combustion chamber. In the compression ignition engine, the process may be divided into three stages:—

First, a delay period during which the surface of each individual droplet is surrounding itself with an envelope of vapour, the outer surface of which must attain such a temperature as to bring about self-ignition.

Secondly, a phase of very rapid burning while the many droplets which have accumulated during the delay period and which are surrounded by fresh and uncontaminated air all burst into flame in rapid succession, and

Thirdly, a phase of controlled burning when, owing to the very high temperatures now ruling within the cylinder, the last series of droplets burst into flame almost as they enter the combustion chamber.

During this, the third phase, we have almost complete control over the rate of burning, and therefore of the pressure, since these depend almost solely on the rate at which we choose to admit the fuel. I say "almost complete control" because in spite of all the intensive study which has been devoted to the design of injection equipment we have nŏt yet gained complete mechanical control of the rate of admission.

From the point of view of pressure control and of smoothness of running generally, we want to speed up the first two phases and carry out as much of the combustion process as we can under the conditions of phase three. In many of the earlier high-speed Diesel engines, phases one and two occupied so large a proportion of the whole process that phase three was elbowed almost entirely out of existence.

The evils of prolonging the delay period are fairly obvious, for, in the first place, the more unburnt fuel that accumulates during this phase, the more violent will be the rate of pressure rise during phase two, and the greater the Diesel knock. Furthermore, if the fuel be a fairly volatile one, but of relatively high self-ignition temperature, there is a danger of small pockets of vaporised combustible mixture being formed and of their being detonated just as in a petrol engine. If the delay period therefore be unduly prolonged, not only shall we have no proper control over the rate of pressure rise, which may be such as to cause a heavy knock, but with certain fuels or under certain conditions we shall get superimposed on this heavy Diesel knock a high pitch "ping" such as we are accustomed to hear in a petrol engine when detonating.

We can speed up the delay period in several ways:—

(1) By raising the ratio of compression.

(2) By adding heat to the air during compression.

(3) By reducing the size and increasing the dispersion of the droplets of fuel.

(4) By using a fuel of high cetene value or low self-ignition temperature.

With regard to the first, we are limited as to the compression ratio we can usefully employ, for whatever the type or form of the combustion chamber we use, there must inevitably be some air left outside in the cavity provided for the valves or in the clearance space between the piston and cylinder necessitated by purely mechanical conditions. This air left outside the combustion chamber in thin and inaccessible layers cannot be reached by the fuel, nor, even if it were reached, would it be hot enough for rapid combustion. It is fairly evident that in any given size of engine, the quantity of outlying air is constant, and that as the ratio of compression is raised, so the capacity of the effective combustion chamber is reduced, and the proportion of outlying air increases; hence the maximum power output, which is purely a function of the amount of air we can burn, therefore diminishes as the compression ratio is raised. A small proportion of outlying air is always desirable, for it serves to consume completely any half-suffocated droplets which have entered the combustion chamber after most of the oxygen has already been consumed by the earlier arrivals, but this process occurs too late in the cycle; in time to clean up the exhaust, but not in time to give any useful addition to the power output. It follows, therefore, that if we raise the ratio of compression beyond a certain point and still work to a given maximum pressure, we gain nothing in thermal efficiency and lose appreciably in power output.

By fitting heat-insulated members in the combustion chamber, which act as regenerators, we can add heat to the air during compression, but here, again, we are limited to a certain extent in that all the heat received and stored by such insulated members is taken from that of combustion—on full load we have plenty and to spare but on light loads or when running idle we are hard put to it to find enough. By such means we can make the delay period almost as short as we like on full load, but under prolonged running on light loads it is liable to lengthen out again. None the less, I am fairly satisfied that this is the best and most practical way of curtailing the delay; all the more so since the temperature of the heat-insulated members tends to rise rapidly with increase of speed. It can thus be made automatically to keep the proportional delay constant and therefore allow of fixed injection timing being used over the whole range of speed.

By reducing the size and increasing the dispersion of the droplets, we can reduce the delay period, but in practice we are very limited in this direction, for reduction of size involves reduction of penetration, and in most types of combustion chamber we need nearly all the penetration we can get, more especially if we are using a high compression ratio. By employing a fuel of what Boerlage and Broeze term a high cetene number—that is to say, a fuel whose self-ignition temperature is relatively low—we can, of course, reduce the delay by increasing the difference between the air temperature and the self-ignition temperature of the fuel.

I have said earlier on that we want to reduce the delay period as much as possible, but this remark needs qualifying, for, like everything else, we can overdo it. Imagine for a moment what would happen if there were no delay period at all. The fuel would burn immediately as it entered from the injector and the whole process of combustion would be concentrated round the nozzle of the injector. Under such extreme circumstance, what chance would the rest of the air have of finding the fuel? We should get beautifully smooth running with perfect control, but very little power. We need, in fact, a delay period of sufficient duration to allow of fuel penetrating well into the chamber before combustion starts.

With regard to fuels in general, we can say that, in the majority of systems a moderately high, but not too high, cetene value is advantageous, in that it serves to shorten the delay period, and thus allow of a better control over the rate of burning and of pressure rise. As in the case of petrol, the cetene value of various fuels depends mainly on their source of origin, but it can be varied somewhat by varying the distillation range or by doping with such substances as amyl nitrite, ethyl nitrate, or certain peroxides. Generally speaking, any source which yields a bad—that is, a low—octane petrol, will yield a high cetene Diesel oil, and *vice versâ*. As to volatility in general, within limits, the lower the volatility the better, since too volatile a fuel is liable both to cause detonation and to give rise to gassing troubles in the fuel injection system. At the other end of the scale, if the volatility be too low, a further delay period will be introduced, due to the greater time taken to form a gas envelope on the outside surface of the droplets, as has been pointed out by Boerlage and Broeze. We are still a long way off any cut-and-dried specification for Diesel fuels, for the reason that the different types of high-speed Diesel engines have such widely differing tastes—some require strict dieting, others can enjoy the whole of the menu.

The twenty-ninth annual report on "The Results of the Chemical and Bacteriological Examination of the London Waters for the Twelve Months Ended December 31st, 1934," by Lieut.-Colonel C. H. H. Harold, the Director of Water Examination, has just been published by the Metropolitan Water Board. In the report, Colonel Harold deals with such subjects as the drought, the integrity of the river Thames as a source of supply, projected new works, filtration and reservoirs, and methods of chemical treatment. The projected schemes, he states, aim at an increase of existing reserves by 58 per cent., and within a few years the reserves will reach a figure of 30,657 million gallons. The defects which appeared in the Board's filtration systems during the recent period of heavy pumping are discussed, and the installation of chlorine and ammonia points is mentioned, together with possible improvements in continuous-flow storage systems. During the past year no less than 27,759 samples of water were examined in the Board's laboratories, and of these 15,134 were used for bacteriological tests, and 4566 for chemical tests. In addition, 6363 samples were collected for testing purposes, 500 centrifugal deposits were examined microscopically, micro-photographs were taken of 685 samples of river and reservoir waters to study their algal contents, and the filterability of 1011 samples was also tested. The fact that 88·3 per cent. of samples of water going into supply passed negative to *B. coli* in 100 c.c., compared with 85·5 per cent. in 1933, indicates the high level of control maintained. The average daily consumption of water supplied by the Board in 1934 was 264·3 million gallons.