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Flame Propagation Model and Combustion Phenomena: Observations, Characteristics, Investigations, Technical Indicators, and Mechanisms

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Abstract

Critical conditions are usually obtained for ignition in a self-heating solid system consisting of two components generating heat independently, one component being inexhaustible and the other exhaustible by either simple first order or autocatalytic reaction. Ignition depends upon whether the exhaustible component can cause a temperature rise in excess of the upper stationary, but unstable, value possible for the inexhaustible component reacting alone. The system provides a theoretical model for some commonly occurring examples of self-heating and ignition in porous solids containing oxidisable oils. It is shown that: (a) the ignition criterion of the model, which involves a nonarbitrary critical temperature increase, has a high degree of physical reality; (b) the model is, in principle, capable of predicting ignition from primary kinetic and thermal data; (c) it is likely to be possible often to make a reliable prediction of critical size for self-ignition in a two-component system at ordinary atmospheric temperatures by a simple extrapolation from small-scale ignition data, obtained at higher temperatures, in the same way as for ignition due to a single reaction. Examination of both adiabatic and non-adiabatic flame theories showed that a 'steady state' exists only under the special condition that a heat sink exists at the initial temperature. For the general case of freely propagating, non-adiabatic flames only a quasi-steady state can be achieved.

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Subject Matter and Discussion

The papers on ignition cover too wide a range of subjects to permit detailed discussion on average seven minutes allotted for flame analysis. It seems more appropriate, therefore, to discuss them according to the method of approach to solution of the problems of ignition. Two of the papers [1,2] are theoretical treatments in which simplifying assumptions are made to permit analytical solutions to be found. Such treatments are valuable if the factors neglected are in fact unimportant, otherwise they are of value only as mathematical exercises. In this connection, we would like to ask whether there are any examples of pure thermal explosions apart from those treated by Varatharajan and Williams [3].

A second group of the papers presented deals with attempts to obtain data which will be basic to the understanding of ignition phenomena. The papers of Bowes [4] and Ansari and Egolfopoulos [5] are in this category. The value of such studies depends on whether sufficient insight i.e., recognition of fundamental principles and significant parameters is employed in designing the experiments so that the data may be regarded as 'true facts', not correlations valid only for the particular experimental equipment used. The first paper of Peterson et al. [6] is an excellent example of a thoroughly well-documented experimental study that should be of considerable importance in understanding of spark ignition phenomena. It will supplement and support studies of such phenomena by schlieren techniques, e.g. Wang et al. [7]'s work in this field.

Finally, there is one example of a study with a directly applied objective, that of Torrent et al. [8]. These authors have secured information of direct practical value predicting conditions under which firedamp explosions may be prevented.

If such studies are to be of maximum value, authors in this field should relate the results of their particular experiment to work that may be of more fundamental or broader significance. After a thorough survey, we have many inquiries such as: is there any connection related to ignition data between Yang [2] and Wolfhard and Burgess [9]? Besides, the relationship of Weinberg [10]'s interesting experiment to other works is not apparent. Perhaps there would be an opportunity for Weinberg to enlarge on this point.

In the study of limits of inflammability, two kinds of approach are useful:

(1) Measurement of limit values for practical use, usually in connection with the avoidance of explosion hazards;

(2) Observations which improve understanding of the fundamental significance of limits.

By and large, we feel that the investigations reported by Zabetakis et al. [11] on the one hand, and by Dixon-Lewis [12] on the other, fall respectively into these two categories. Zabetakis et al. [11] base their discussion on measurements made in the 2 in. vertical tube, and the justification for its use is that the values thus obtained appear in most cases to approach limits ruling in the larger spaces encountered in practice. With this background, they have examined the effect of certain factors upon the adiabatic flame temperature T_f at the limit, and have concluded that:

1. T_f is relatively independent of the initial temperature in many cases, and therefore the effect of initial temperature on the limit can be calculated.
2. T_f decreases with increase of initial pressure, especially as referred to the upper limit.
3. T_f for closed tube limits is more appropriately calculated on a basis of constant pressure combustion than constant volume.
4. Inert diluents have little effect on T_f, except in the case of helium when T_f is somewhat raised. (No doubt, the higher thermal conductivity of helium is important, but its effect upon the temperature structure of the pre-heat and reaction zones may be at least as important as its effect upon heat losses from the flame front.)

We would like to express our appreciation of the immense amount of work done over many years at the Bureau of Mines on the measurement of inflammability limits. The work of the late Yang [2] forms the major source of information on this subject for most practical purposes, and often one would have been at a loss without them. It does seem to be clear, however, that limits as he encountered in the ordinary way is dictated by several factors of different kinds, and in these circumstances it would seem unprofitable to seek any fundamental correlations between such limit values at the present stage.

Dixon-Lewis [12] has sought to strip off some of the environmental factors in an endeavour to simplify the picture. He has confirmed, what had previously been indicated from other quarters, that if a planar flame is removed from the influence of surrounding walls and protected against convective disturbance, the lower limit value is greatly reduced. His technique has, however, imposed other environmental restrictions and a fresh approach seems necessary if the stripping process is to be pursued further. We feel, however, that any further steps in this direction should be accompanied by close observation of the manner in which the limit flame fails under the conditions employed. Some indication might be obtained, for example, as to whether the flame is failing due to the spread of a local disturbance or whether it is failing as a whole, as it might be expected to do on arriving at the point considered by Spalding and Jain [1], where heat loss is the only factor opposing flame propagation.

Alternatively, what possibility is there of developing the theory of, for example, limitation by heat loss to the stage where sufficiently accurate predictions of numerical limit values can be made for purposes of comparison with experimental observation? It would appear to us that accurate temperature analyses through near-limit flame reaction zones, such as can be made by the methods described elsewhere by Habisreuther et al. [13], might provide a factual basis which would greatly facilitate such predictive calculations.

We agree with Peterson et al. [6]'s point that the implications of approximate methods should receive more careful scrutiny and interpretation than has sometimes been the case in flame propagation theory.

On the point of real systems describable in this way let us first draw attention to a necessary shift of emphasis. The fact is that *all* exothermic reactions must depart from isothermal conditions and the real value of this introductory paper and of our more sophisticated investigations is that the *extent of this departure is assessable.* A recent illustration is offered by Filimonov's work [14] on lead azide; an older but no less dramatic one is the prediction of the explosion (P *vs.* T) limits of gaseous nitrous oxide (spontaneous ignition) before its experimental determination [15].

Boddington et al. [16] and Lermant and Yip [17] have suggested that Frank-Kamenetskii's picture of the temperature distribution is a more refined one than Semenov's. We offer the suggestion that although their relationship may be regarded in this light an alternative view is also helpful, *viz.* that both treatments are legitimate but refer to physically different situations.

In Mullin's method [18] of measuring ignition lags, the induction period is set equal to the time of travel of the mixture from the point of formation to the flame front. If the position of the flame front is determined by some extraneous stabilization mechanism, the time of ravel will in general not coincide with the induction period. On what evidence do the authors conclude that no such stabilization is operative in their experiments?

In his remarks relating to Miller's paper, which describes test results using the smaller scale N.G.T.E. ignition apparatus, von Elbe [19] has asked what the nature of the residence time measured was. I shall attempt to answer this question.

The residence time is calculated from the observed distance between the plane of fuel injection into the test section and the plane nearest this at which flame is observed to be continuously present in the duct. This latter plane has been loosely referred by me and other scientists as a 'flame front' but it is in no sense a flame front of the type observed in flame propagation processes. In fact, in the present flow regime where velocities are of the order of 25 ft./sec or 250 ft./sec there is no flame stabilization in the test section, but flame appears spontaneously in the duct and away from the walls after the fuel-air mixture has existed for a sufficient time at the elevated temperature. This residence time computed from the distance between the plane of fuel injection and the plane at which steady flame is first observed represents the chemical delay time for a weak fuel-vitiated air mixture. In the cases where fuel is injected either as liquid or vapour through a single central orifice the effective mixture strength which creates this flame position is smaller indeed than the nominal overall mixture strength obtained by dividing the total fuel flow by the vitiated air flow. This is because only a portion of the injected fuel volatilizes and mixes very quickly (in a time of the order of 0.1 msec). In calculating the residence time, it is assumed that the very rapidly formed weak mixture is present from the fuel injector plane and moves downstream with the velocity of the general stream.

Some unpublished tests which we have made, in which special care was taken to distribute and mix the added fuel very quickly by the use of a multiple-drilled double ring injector manifold and perforated plates and gauzes, have shown that the ignition delay is insensitive to variation of equivalence ratio, ɵ (more information about ɵ can be found in the following work (Combustor Flame Flashback: https://ntrs.nasa.gov/archive/nasa/casi.ntrs.nasa.gov/19860005257.pdf), in the range 0.001 to 0.5; hence it is not necessary to create a precisely determined mixture ratio, so long as we require ɵ < 0.5, when testing fuel sprays or gaseous fuels using a single-hole injector or any other injector which by its very nature cannot produce a uniform mixture at once. We have some experimental evidence that in these flow experiments the ignition delay time is reduced by some 30-50 percent as the stoichiometric mixture ratio is approached from the weak side.

In the standard experiments which we have described (part of Aljerf and Mashlah [20] work), We always take care to ignore, for purposes of ignition delay calculation, the occasional 'flame flashes' observed between the plane of fuel injection and the plane of steady flame appearance which we tentatively ascribed to near-stoichiometric pockets of gas mixture which were created by the diffusion of fuel from the spray into the turbulent airstream. The flame flashing, which corresponded to a short residence time may indeed have been due to the creation of such more easily ignitable pockets of gas mixture of which there would be less than of the weak pockets of gas mixture which are responsible for the establishment of the steady flame position; alternatively, the flame flashing may have been due to some turbulence dependent phenomenon such as an intermittent yet rapid fluctuation of gas stream velocity or temperature. Yang [2] and Peterson et al. [6] described in more details this phenomenon of 'flame flashing'. When liquid fuel is used the coarser spray droplets are seen to burn behind, that is, downstream of the steady flame position, and the same applies to that part of a gaseous fuel injected through a single orifice which takes longest to mix with the airstream.

So, summarizing, the residence time as measured very closely approximates to the true chemical delay time for a weak mixture of the fuel and vitiated air and this is borne out by the linearity of curves of log residence time *versus* reciprocal absolute temperature; in the case of Miller's results the airstream was not vitiated but was either pure air or a synthetic atmosphere, we do not agree that this is necessarily so.

Our simplifying assumption was to suppose that the local reaction rate in any part of the apparatus was principally determined by the concentrations of fuel, oxygen and already burnt products, and the corresponding temperature: there would then be a unique relationship between the 'degree of reactedness' and the corresponding temperature, and the heat release rate, independent of the initial test conditions. (Incidentally, if such an assumption is *not* made, then it is very difficult to see any practical application of these comparatively low-temperature results to engineering systems.) Reaction would then start immediately the combustibles were mixed and would continue at an accelerating rate until the flame became visible and 'ignition' was said to occur. Most of the delay period would be spent in raising the reactants through the first few degrees, as is well shown by Miller's Figure 3-on our interpretation, out of a delay time of 60 msec at 780°C half is spent in raising the temperature by 20 degrees to 800°C.

It seems Miller's assumption rejects our hypothesis, primarily because the supposed rise in temperature has not been measured. Now our second hypothesis was that the fuel did *not* mix completely immediately on injection, but that the flow tended to be striated. Reaction then proceeded fastest, and so ignition was apparent, in those streams where the mixture ratio was near stoichiometric: thus the apparent ignition delay corresponded always to a near-stoichiometric mixture whatever the overall ratio of injected fuel to airstream flow. This seems the only possible reason why the oxygen concentration has a big effect on the delay while the overall fuel:mixture ratio has none at all? In this respect it would be very interesting to do the inverse test-injecting oxygen into a hot mixture of fuel and nitrogen: one might well find that the fuel concentration was then of importance but that delay was independent of the overall oxygen concentration. On the same argument, if only the near stoichiometric streams are reacting appreciably in the early stages after injection, the average temperature rise is small. The fact that no rise is measured does not therefore discount the theory.

Furthermore, although the author claims that the time average concentration at points just downstream of the injection plane varies only by a factor of two, this is no proof that there is not considerable time-wise variation of mixture ratio along a given stream, so the above arguments may still hold.

Aljerf's results [20,21] certainly do not disprove the method of correlating spontaneous ignition data supported by Bragg [22]. The results which are relevant to the discussion are some temperature measurements obtained by Mullins [18] at N.G.T.E. six decades ago. Even with these we prefer not to use the word 'disprove'.

We have examined some simple criteria for spontaneous ignition suggested by various authors in order to determine whether any of them are capable of explaining Aljerf's ignition delay results and those of Mullins [18] for calor gas (brand of bottled butane and propane comes in cylinders which have a special gas regulator). Bragg and Mullins explanations seem to be equally promising starting points although both must be regarded as extreme simplifications since the chemistry of this process is undoubtedly complex. Our assumptions are similar to those outlined above by Bragg. We assume that the reaction rate is determined by fuel concentration, oxygen concentration and temperature.

At this point one asks the question 'When does ignition occur?' Mullins [18] has suggested that it occurs after a definite fraction of the reaction has been completed while Bragg prefers the idea of a constant temperature rise between the mixing of the combustibles and the appearance of flame. In choosing between these two suggestions we have been guided by the temperature measurements mentioned above. These seem to contradict the assumption of either a constant ignition temperature or of a constant temperature rise before ignition. To our knowledge this is the only existing piece of experimental evidence which is relevant to a choice between the two alternatives. Using Mullins' suggestion and assuming that the pre-flame zone can be represented as an isothermal region (this is probably not too far from the truth when one remembers that the largest group of measurements were made over a range of equivalence ratios of 0.018 to 0.05) it is then possible to account for the observed effects for the weak mixtures which have been studied. The object of the paper, however, is merely to show that a simple criterion such as that discussed above is capable of explaining these facts and certainly not to disprove any alternative mechanism. Such an alternative must be regarded as equally valid provided that it is capable of explaining all the experimental facts.

We are not completely in agreement with Braggs' arguments on mixing. The production of a completely homogeneous fuel-air mixture immediately on injection is of course an ideal which can never be reached in any practical system. However, arguments set out in the paper suggest that the results can be explained more easily by assuming complete mixing. It must be remembered that under our experimental conditions cold fuel is added to a hot air stream. Under these conditions if we have bad mixing there will be a range of pockets varying from very weak mixtures at temperatures near that of the air stream to very rich ones at very low temperatures. If a stoichiometric pocket was formed this would be at a temperature lower than that of the ideal homogeneous fuel-air mixture for most of the experiments by more than 100 degrees. In view of the exponential temperature dependence of ignition delay it is not at all obvious that a near-stoichiometric mixture would ignite first. Bragg regards the hypothesis of poor mixing as being the only possible reason why the mixture ratio has no effect on ignition delay. It cannot be too strongly stressed here that the ignition delay is not the same thing as a reaction rate. The fact that one of these quantities apparently depends on fuel concentration while the other does not is therefore no contradiction. Indeed all the ignition delay measurements have been explained on the assumption that the overall reaction rate is proportional to the first power of both the oxygen concentration and the fuel concentration. This approximation is obviously strictly valid only for infinitely weak mixtures. For the weak mixtures considered here (where the equivalence ratio for most of the mixtures is below 0.1 and for all the mixtures is below 0.2) the assumption is a reasonably good approximation. If one could explain results over the entire range of mixture strengths by a second order equation one should, as Bragg suggests, find that, on adding *small amounts* of oxygen to a stream of hot fuel and nitrogen, the ignition delay was independent of oxygen concentration. This conclusion meets with both Kinabara and Akita [23] and Sreedhara and Huh [24] justifications. For the purpose of this argument we shall ignore the experimental difficulties involved in preparing such a hot fuel stream. However, when we approach a stoichiometric mixture either from the weak or the rich side, the treatment outlined above cannot be used since it is no longer possible to neglect one concentration with respect to the other. There is the added complication that in such mixtures a fairly large temperature rise might occur in the pre-flame zone if the fraction of reaction completed, that is really appreciable. The assumption of an isothermal pre-flame zone will not then be correct and treatment of any such results must be considered in rather more detail.

Going back to Peterson et al. [6]'s work, we see the temperature at which ignition occurs without the use of a second stage corresponds to conventional auto-ignition. This value, however, is not a constant, since it depends on the amount of heat released by the gas in contact with the surface and hence on time of contact, surface area, etc. The smaller the heat release, the higher is the ignition temperature.

The four papers by [25-28] form a valuable contribution to our knowledge of the initiation of ignition by electric sparks and are notable for the very remarkable technique applied to a difficult problem.

Our remarks are based on the statement that it is assumed that all the energy released at the gap is imparted to the gas mixture. In some works [29-34] which have been carried out over the last year on the ignition of fuel sprays at low densities and pressures, where relatively large amounts of energy are involved, it became very clear that this was by no means the case. In Aljerf's experiments, he found that a large portion of the energy actually released at the gap was imparted to the electrode surfaces and only released to the gas relatively slowly, but apart from this the discharges were accompanied by very appreciable radiation, a flash and a bang and this radiant energy would be ineffective as an igniting source. It can be argued that even if the energy is released comparatively slowly some 70 per cent will appear as heat, the rest doing mechanical work on the surrounding gas, but if the energy is released very rapidly to produce a hot core at a high pressure which then expands adiabatically the amount of residual energy in the hot kernel is still further reduced, the balance appearing as a shock wave. It is true that the energy in this shock wave is converted into heat ultimately but this will be comparatively low-grade heat and ineffective as an igniting source.

In high energy sources we were only able to account for a small fraction of the total some 10 per cent at most which could be really effective and the question arises as to whether a similar condition can prevail in these low energy sources. It is true that visible and audible radiation is difficult to detect but at the same time the momentary rates of energy dissipation are high and a rough calculation would indicate that the instantaneous temperature of the spark channel could be of the order of 30,000°C or more.

This supposition is supported by the results of the Aljerf's experiments which showed that the introduction of resistance into the circuit reduced the amount of liberated energy required to produce ignition. The introduction of resistance by reducing the instantaneous rate of energy release could of course easily reduce the relative amount of radiated and ineffective energy.

Although in most cases occurring in practice the energy required for ignition is so small that these losses are of no great significance, this is not the case under certain conditions of low pressure and also of fuel-air mixtures bordering on the limit of inflammability, and particularly so in the case of fuel sprays as opposed to homogeneous gaseous mixture.

Reference has been made to energy loss during ignition of inflammable gases by electric sparks [25]. Such loss is mainly caused by the shock wave which radiates from the spark without contributing to the ignition process. According to Aljerf experiments on sparks in argon, he encountered rather large losses of energy. However, the spark energies in these experiments were very large compared to those usually required for ignition; the ratio of heat loss to heat retained decreases with decreasing spark energy; and in diatomic or polyatomic gases the fraction of heat retained in the spark centre is substantially larger than in argon. It appears therefore that the ignition energy data reported by various investigators are not very greatly in error and are certainly correct within the order of magnitude.

If we assume, as noted by Dixon-Lewis [12] for his burner, that the lower limit of inflammability at a specified (mixture) temperature can actually be decreased by increasing the wall temperature of a conventional cylindrical limit of inflammability apparatus, one would expect to find a non-linear relationship between the lower limit and the (mixture and wall) temperature at a fixed pressure. However, all the data obtained to date in cylindrical apparatus indicate a linear relationship actually exists between these quantities.

Furthermore, Dixon-Lewis [12] has strikingly called attention to the dependence of 'limits of inflammability' upon apparatus. It is significant to note that steady-state flames are *always* non-adiabatic, and therefore must depend upon the apparatus (boundary conditions). Strictly speaking, then, 'inflammability limits independent of apparatus' is a self-inconsistent concept.

In fact, examination of the well-known solution of the time dependent heat conduction equation shows that a finite temperature gradient is *immediately* established at the boundaries of an initially isothermal (arbitrarily large, but finite) domain. It is thus impossible, even if one (improperly) excludes the importance of radiation losses, for a steady-state flame to be unaware of the boundaries. A detailed examination of the fact that 'steady-state' flames must be non-adiabatic is presented elsewhere. One must therefore conclude that distinctions between 'quenching limits' and 'inflammability limits' are somewhat arbitrary. Thus, *conduction* as well as radiation losses are operative in defining 'inflammability limits'.

The authors particularly noted that limits in tubes and on flat-flame burners are different. That this is generally so follows from the discussion presented in the preceding paragraph and from the fact that extinction limits depend upon the *manner* in which energy is abstracted from flames. Thus, the question 'how much heat must we abstract from a flame to extinguish it?' is *not* a generally valid one. For example, a freely propagating, non-adiabatic, steady-state flame (in a long, cold tube) may be considered to be semi-infinite in extent. It thus gives up *all* its heat of combustion to the surroundings. *Everywhere* within this semi-infinite domain, the energy conservation equation must be balanced, if the steady-state flame is to exist. It can be shown that extinction limits (quenching *and* inflammability) are predictable from limits of applicability of the conservation equations which describe the non-adiabatic flame structure in the neighbourhood of the reaction zone. This follows from the facts that:

Energy conservation is easy to satisfy in the neighbourhoods of very low reaction rates (near the 'cold boundary' and on the 'hot' side of the maximum flame temperature).

The energy conservation equation is very sensitive to heat abstraction in regions of high chemical reactivity. Thus it follows that different boundary conditions (heat sinks) will affect the flame structure in different ways. Hence extinction limits resulting from the use of different kinds of apparatus (boundary conditions) *should* be expected to differ.

The observation of Dixon-Lewis [12], those leaner flames can be generated in special flat-flame burners than in tubes, do not exclude the possibility that fundamental limits exist which depend on the properties of the unburned mixture. Apparatus effects should be unimportant in gas explosions in huge vessels as in mines. With reference to such explosions, experience indicates that fundamental flammability limits exist and though a mixture may not be hazardous in the mine, the combustibles in it can be reacted on a red hot coil in gas analysis equipment.

We wish to emphasize that the 'limits of inflammability' and minimum burning velocities quoted in the literature are not to be regarded as fundamental to the ethylene-air flame, but as environmental limits produced by the apparatus. By studying the effect of the temperature of the stabilizing medium (hot plate) above the flame it has been possible to show that the limits obtained in a definite particular apparatus can very probably be associated with convection effects due to the flame itself together with the quenching properties of the plate when the latter is at temperatures below about 870°C. At higher plate temperatures the quenching properties of the plate disappear, but its convectional properties become important. In the latter case the limit burning velocity is controlled by the convectional approach velocity, and a visible flame is only obtained when the mixture composition is sufficient under the conditions prevailing to give the limit burning velocity. Whilst it has been suspected for some time that convection may be very important in determining the limits in this type of system, the interplay between convection and quenching, and the two different types of limit, have not, so far as we are aware, previously been shown.

During the discussion Zabetakis et al. [11] referred to the constancy of adiabatic limit flame temperatures in tubes as the initial mixture temperature is varied over quite wide range, and suggested that it is difficult to explain this on the basis of a quenching theory of the limits. Similarly, it was not until the top plate in our experiments reached almost 700°C that the adiabatic 'limit' flame temperature commenced to decrease at all rapidly. The effect in these experiments may be partly due to the steepness of the burning velocity curve in the region corresponding to the low-temperature limits. In this region, the decrease in burning velocity due to a slight lowering of the adiabatic flame temperature may sufficiently increase the quenching effect per unit volume of gas that it offsets the reduced quenching due to the higher temperature of the surface near the flame. A similar type of approach might help to explain the above aspect of tube limits, though again it may be that such limits are determined by effects of the tube other than quenching.

In a comment to Dixon-Lewis [12]'s points, our observations, indeed, do not exclude the possibility that fundamental limits exist which depend only on the internal properties of the flame burning under adiabatic conditions. It nevertheless seems most likely to us that such limits, if they do exist, have not yet been reached; and that the observed limits so far obtained are not fundamental in this sense. In the case of mine explosions and explosions in large, but finite, vessels the possibilities of flame instability produced by convectional deformation of the flame front or of limits caused by radiation losses have been mentioned.

Lastly, we should like to draw attention to our observations that even in the absence of a flame catalytic reaction occurs at the hot surface of the top plate. We are inclined to compare this, rather than our visible flames, with the type of reaction occurring on the red-hot wire of the gas analysis apparatus. In our view, the close approach to the heated plate is necessary to stabilize the flame aerodynamically. This does seem to result in a conversion of the later stages of the flame reaction into a catalytic action on the plate, and the problem then becomes one of how far this catalytic reaction will affect the propagation of the main flame zone.

There is no obvious change in the appearance of the luminous zone as it approaches the plate; and in the absence at present of further experimental evidence we are inclined to believe that, even if the luminous zone is only separated from the plate by a millimetre or so, the effect is small, due to the high values of the reaction rates and radical concentrations actually in the luminous zones of most flames compared with the values behind them.

Conclusions

A fuel-oxidant system may auto-ignite, be ignited by a source, or remain non-inflammable depending upon the ambient temperature, pressure, composition, geometry of the container, etc. Hence, the paper has successfully presented a theoretical attempt to explain how all these observed phenomena can occur in a given chemical system. The theory presented here predicts the role of ignition energy of a system for the configurations of the ignition source and yields the auto-ignition temperature for the same system when the minimum ignition energy approaches zero. In addition, based on a non-adiabatic theory, the present work is accounted for flame extinction limits. By hand, spherical steady laminar flame propagation is considered analytical solutions have been obtained for the cases of

(i) Step-function reaction-rate curves, and (ii) Adams-type reaction-rate curves. It is shown that the effective radius of a spherical flame is under-estimated if one uses the thin-flame approximation. The error decreases as the rate of injection and the laminar flame speed of the mixture increase.

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