

DISCUSSION—THIRD SESSION

Mr. K. N. PALMER asked Mr. Craven† whether there would be any modifications to his calculations if the gas being discharged should liquefy on passing through the nozzle and whether this would be likely to change seriously the shape of the cloud.

Dr. CRAVEN enquired whether Mr. Palmer was suggesting that the gases condensed due to compression and Mr. Palmer agreed with that.

Mr. CRAVEN thought that if condensation to droplets did occur at the point of discharge, re-evaporation would occur in the turbulent processes of the flame.

On being asked by the CHAIRMAN if he would like to express his own view on the point he had just raised, Mr. PALMER said that difficulty might arise if it liquefied to fairly large drops resulting in liquid for a larger distance than straight gas jet mixing would allow.

Mr. CRAVEN thought that if very large drops were produced the flame could get bigger but he did not think that large drops could be produced in the time available.

Mr. PALMER wondered whether it might lift off more quickly, but Mr. Craven did not think that lift-off would reduce the radiation from the flame very significantly. The flame may not blow out even if it lifted off. If the flame did lift off but did not blow out, only the thin bottom section would be affected and the radiation from this region is small in comparison with the main volume of the flame.

Mr. D. G. WILDE said that some observations of the spraying of a flammable hydraulic fluid through a nozzle and the subsequent burning of the droplet mist might throw some light on the last observations.

Flame stabilised at a distance from the nozzle such that the velocity of the droplets away from the nozzle was approximately equal to the velocity of the flame in the opposite direction. A part of the flame nearest to the nozzle was blue in colour, indicating the occurrence of some premixing of vaporised fuel and air, but further away a yellow diffusion flame, several feet long was produced. He thought that the chance of a droplet penetrating the flame was fairly remote, although some droplets might fall away at the sides. An elementary method of observing the fall-out of fuel from the spray tended to confirm this opinion. He would not expect condensation of the kind that Mr. Palmer had in mind; there might be a tendency for the base of the flame to be raised a little, but it did not seem likely that the effect would be a large one.

Mr. CLANCEY said he thought the position would be likely to be that when condensation was taking place the droplets would have the same velocity as the velocity of the gases to conserve the momentum and be very small. They would then move up into the burning zone and take part in that pheno-

menon with a little extra turbulence produced due to the condensation just above the jet. But if one was dealing with a flame of the height of 90 feet he did not think this would make any significant difference at all.

Mr. CRAVEN thought that that was probably right; it was rather a big flame to allow a droplet to survive through its length.

A SPEAKER asked if Mr. Craven could touch on a point which was not in his paper and that was the possible buoyancy forces in a jet of this sort. One could visualise gases with a density length of four feet and with a jet of 90 feet. He would have thought that a great deal of the initial velocity of the gas would have been lost and therefore the buoyancy forces would start to become significant in the higher regions of the jet. The effect of this would be to produce a mushrooming at the top of the flame and presumably shortening and the flame would increase in volume at the top.

On the question of condensation he thought it would be reasonable to assume that if such condensation did occur, the velocity of the droplets formed would ensure that they were very small. Large drops would never exist but would shatter into smaller droplets. He thought they could only consider very fine mist droplets.

Mr. CRAVEN said that he had not considered buoyancy effects in his paper. He considered that the burning gases were still moving very fast even at the tip of the flame and that buoyancy effects could be neglected.

A SPEAKER asked Mr. Craven whether he had taken into account the reabsorption which took place. How did this affect the radiation which was actually observed?

On being asked for clarification by Mr. Craven he added that on the edge of the cone the combustion was not stoichiometric. It was a weaker flame which was of a lower temperature.

Mr. CRAVEN agreed that the central core of the flame in which he had assumed that stoichiometric combustion would occur could be surrounded by an envelope of flame resulting from leaner combustion. Although this would be cooler, he did not think that it would be an efficient absorber and in neglecting this, the treatment described in the paper would err on the side of safety. In the absence of any experimental data, he considered that it was safest to assume a radiating temperature equivalent to the most efficient combustion process possible.

A SPEAKER said that he had consulted Mr. Craven on equation (11) in his paper which to him seemed to be a simple jet equation for the gas escaping from the pipe without chemical reaction. He wondered whether Mr. Craven had considered what the increase in this jet reaction force would be due to the acceleration of the gases by the combustion effect. He hesitated to raise this point but felt it was important because it could be as much again as the value that was given by equation (11).

† *Mr. Craven's paper may be found on pp. 7-11.*

Mr. CRAVEN replied by asking "Is this like a jet engine?". His personal view was that it was not, provided that pure fuel was being discharged which could not support combustion in the absence of air. If, however, either a flammable mixture was being discharged or a fuel capable of supporting flame in the absence of air or oxygen (*e.g.*, acetylene), combustion in the orifice could give an increase in the jet reaction above that calculated from the equation in the paper.

Mr. S. J. G. TAYLOR thought that once the gas had left the jet anything that happened beyond would not have any effect on the jet. Regarding the analogy with a jet engine, with combustion taking place inside a combustion chamber, reaction from the hot gases occurred after combustion.

He thought, therefore, that the mechanical momentum as given would be correct.

It was pointed out that the gases after combustion must be travelling faster than they were when they left the jet because there was four times as much volume to be got rid of by increasing the angle of the jet. There must have been an acceleration force to drive the gases forward at higher values.

The CHAIRMAN mentioned that in his paper (see page 74) Mr. Stark quoted flame temperatures in the region of 1000°C and in Mr. Craven's paper temperatures of 2300 K were quoted. He assumed that they could not be the same temperatures.

Mr. STARK replied that he appreciated the point. Temperatures were often calculated for the combustion of a stoichiometric mixture of fuel vapour and air, whereas the measurements referred to were made through flames in which conditions ranged from a very weak fuel-air mixture to a very rich one, from which the average temperature would be lower than the calculated value. He was concerned to measure a temperature to which objects placed in the flame would be exposed.

Mr. J. M. ALEXANDER asked Mr. Stark whether the figures he produced of the rate of burning relationship with flame emissivity could be combined with other parameters so that the practical radiation from the fire could be estimated, to which Mr. STARK replied that he was not sure that an approach could be made to that.

Mr. R. HIRST was concerned with the development of fire fighting chemicals and had a contribution to make to Mr. Craven's paper. Last year they had the opportunity to do some tests on a large methane flame in the United States. Gas was issuing at 650 lbf/in² from a six-inch line and they had a flame about 190 feet high. This was at 2250 cubic feet a second. The flame base was about 10 feet above the burner, and they could see some vapour in the unburnt region below the flame. It was very much Mr. Craven's shape but there was no mushrooming effect over the top. He took some pictures at 1/1000 of a second and the flame shape was rather a tenuous one, although the general envelope was conical. The characteristic very loud rumbling which shook the ground was probably due to the continuous ignition of turbulent volumes of gas well away from the base of the flame.

In response to a question from Mr. Craven as to whether he had had an opportunity of putting his figures into Mr. Craven's equation to see whether the value of 190 feet was right, he said that only a rough check had been made, but the result did look about right.

Mr. ALEXANDER asked whether the flame temperature on the outside of the fire was the lower limit flame temperature. Was there any relationship between what the flame temperature was outside to what it was on the inside. Did it depend on whether the flame was transparent or opaque?

Mr. STARK said the work, in which small pool fires were studied under various conditions, suggested that the outer envelope of the flame close to the pool was at the lower limit temperature. If, for example, a pool of liquid was cooled while it was burning so that it approached its flash point, the burning system would go right back through the stages from a luminous to a non-luminous flame. He thought this suggested that non-luminous flames would not continue up the flame envelope very far. How far one could not tell because of the masking by the fluctuating luminous flames.

Methanol flames were very transparent, but provided one operated in low incident light, a radiation temperature could be obtained. Once a pool was of appreciable size, flames were more luminous. There had been sufficient luminosity for measurements to be made. The work referred to was done with ethanol, not methanol, and there appeared to be a bit more luminosity from this fuel.

After Mr. Alexander had added that it looked completely invisible, Mr. STARK continued by saying that they had only used smallish fires, and measurements were possible. The fuel used was a commercial ethanol.

Mr. P. BLEASE said that he was doubtful about the idea of a flame from cool burning being associated with flame temperature on lower limit phenomena because one of the main characteristics of that sort of flame from hydrocarbon sources was a high degree of carbon formation. This could not be associated with lower limit phenomena since this would never give carbon formation. If it was really as low as Mr. Stark's paper suggested, then one must really consider this as a temperature associated with an upper limit of flammability. That would have agreed with a production of a large amount of carbon of very high luminosity obtained from those flames.

There seemed to be some confusion about the nature of diffusion flames. The basic natures of a diffusion flame, whether laminar or not, was that on the fuel side there was no combustion and the flame represented either a laminar or turbulent boundary between the air and the fuel. Therefore, this idea of a reabsorption seemed to be something of a red herring. He suggested that Mr. Craven's assumption of stoichiometric temperature was a very reasonable one. He was a little puzzled as to why the temperature should be so low on the cool fires.

Mr. STARK agreed that the flames in the tests were constructed as was suggested. There was a cone of vaporized fuel surrounded by a thin envelope of flame, and presumably the surrounded cone of vapour was very rich. When the flame system had developed fully, the blue, low luminosity, flame that one associated with lower limit conditions existed only close to the rim of the vessel, that is, where diffusion of air into the fuel vapour had just started, and where ignition of the system could be maintained by the luminous flame above.

Because one had diffusion like this taking place through the whole of the flame cone, the major part of the fully developed flame would be a rich one forming carbonaceous matter but this need not be so on the outer surface of the flame. He was not quite sure how far a lower limit flame could exist above the rim.

The measured temperatures had come as a surprise to a certain extent. The fires were small ones and measurements were made so that one looked at two sections of the flame

separated by the unburnt vapour zone and therefore looked at flames at fuel/air ratios increasing as the vapour cone was approached.

Mr. G. H. BULMER said that when one had a fire of reasonable size, the size increased and the air velocity into the flame increased round the periphery into the container, there was a noticeable pinch, and the flame increased in height but there was a limit to that. There must be a limit on the flame temperature because the vapour going through, but not reacting, could not contribute to the flame temperature.

Mr. STARK had observed that phenomenon. Under those conditions, the formation of a waist in the flame and detachment of flame zones were clearly seen.

In reply to a question from Mr. Grantham, Mr. BULMER said that sodium did react to asbestos in the same way as lithium.

Dr. NAPIER observed that in his paper (see page 85) Mr. Bulmer had referred to the use of polyurethane. He wondered whether he could enlarge on the type of material and the way in which it was used.

He had another question on something that did not appear in the paper. They had heard from time to time of accidents where people had used alkali metals which were encrusted with oxide, peroxide and so on. Presumably,

they had obtained ignition in the hydrocarbon in which the alkaline metal had been immersed. He wondered if Mr. Bulmer could give some assessment of that hazard with old sodium. In the university they had often unearthed a bottle of such material and they did treat it with great respect; but they needed guidance.

In answer to Dr Napier's first question, Mr. BULMER said that an attempt had been made to use as many hydrocarbons as possible and of the ones that were tried, polyurethane foam was found to be the most promising but only for low temperature fires. He thought that a ceiling of 400°C was fixed because if they went above this, the plastic material itself became involved and the test became an inferno. As he recalled it, it was an isocyanurate type of polyurethane and the solutions were pumped at a few feet away from the burning sodium and applied in the form of a low velocity jet on to the surface. He believed they tried to produce foam with better fire resistance so that they could tolerate higher temperatures. He did not think one would attempt this technique on an important chemical plant. It was experimental at that time.

On the second question, super oxide formation was not the problem with sodium as it was with potassium or with potassium sodium alloy. He was not aware of anybody having detected super oxide in encrusted sodium. It was quite safe from detonation so far as he knew but it must be sodium and not a mixture of sodium and potassium.