

## DISCUSSION—FIRST SESSION

Mr. G. W. V. STARK referred to the paper by Grove *et al.* The authors had found ethyl bromide not to ignite in their experiments and they had shown certain marginal differences in the behaviour of those materials in two inch and four inch tubes.

The point he had in mind arose out of the work performed at the Fire Research Station where they had used a tube of just over two inches (about 6½ centimetres) internal diameter. He had found that the marginally combustible materials were extremely sensitive to temperatures and dirt deposited inside the tube. He asked the authors whether the tube was cleaned thoroughly after each ignition experiment and whether attempts had been made to control the temperature to a given limit for each test.

Mr. WEBSTER replied that the tube was not cleaned after each experiment but was evacuated. The ambient temperature was not controlled. Ethyl bromide was observed to ignite but flame would not propagate if the tube was open; propagation was possible if the tube was left closed.

Dr. D. H. NAPIER asked whether Mr. Stark was commenting on conditioning of the wall of the tube or if he was referring to dirt in the tube.

Mr. STARK replied that the wall had not been specially conditioned; he was referring to dirt.

Dr. J. H. BURGOYNE asked Dr. Artingstall to comment on the situation where there was some complexity of equipment between the vessel and the point of escape which provided resistance to outflow. It was a situation that one often met in practice.

Dr. ARTINGSTALL said that the flow-rate depended upon the critical area in the system provided that the gas was escaping and that the rest of the system was not being pressurised. He was not quite certain how the resistance would effect the discharge times.

Dr. M. A. NETTLETON referred to figure seven of Mr. Phillips' paper which gave the relationship between the experimental and theoretical gaps for a variety of gas mixtures with different activation energies. He wondered how a particular flame, for instance, coal or aluminium dust, would fit with this theoretical treatment. Had Mr. Phillips any suggestions to make on this?

Mr. PHILLIPS replied that he had no suggestions. In order to get the value of safe gap for a gaseous fuel one would need the activation energy and this was related to the lower limit of downward flame propagation. This data was not available for dust clouds.

Mr. ARTINGSTALL added that presumably one was liable to get ignition by particles getting through the gap. Whether that would be sufficient to ignite, he did not know; nor did he know whether any work had been done on this.

Mr. PHILLIPS noted that there had been work done in Canada on aluminium flakes trapped between flanges. If a small piece of aluminium swarf is trapped between the flanges, the internal gas explosion would blow it out and the burning aluminium could ignite the external atmosphere.

Mr. S. J. G. TAYLOR, referring to the paper by Grove *et al.*, said that he was interested in the length of time that was allowed for mixing the fuel and air. He was surprised that it was necessary to allow 10 minutes for smaller vessels and 30 minutes for larger ones. Had that time, he asked, been determined by actual experiment?

In reply, Mr. WEBSTER said the mixing times were chosen to ensure smooth propagation and reproducible results. A test of mixing was also made in the nine inch tube using CO<sub>2</sub>/N<sub>2</sub> as indicated in the paper; mixing was incomplete if the mixing time was less than 15 to 20 minutes.

Mr. A. P. COX asked Dr. Marshall whether he could comment on the purge time for furnaces and boilers. One should allow time for air to purge a furnace or boiler before lighting. Could anybody comment on the methods to be used for determining this purge time?

Off hand Mr. PHILLIPS could not give figures, but for purging experimental boxes for explosion tests it was usual to allow about five or six volume changes in the chamber. It could be assumed that by then any combustible materials would be reduced to such a level that they would not contribute to the explosion. It was purely a dilution process really.

Mr. CUBBAGE (Midlands Research Station, Gas Council) in reply said: Adequate purging of industrial plant is under constant review at the Midlands Research Station. The current recommendations for gas fired equipment given in the "Interim Codes of Practice for Large Gas and Dual Fuel Burners", Gas Council Report No. 764/70 call for a purge of at least five volume changes.

The requirements for oil fired equipment are given in B.S.799, Part I, 1962, "Atomising Burners and Associated Equipment", and are based on similar purge rates.

Data for low temperature ovens is provided in "Evaporating and other Ovens", Health and Safety a Work Booklet 46, Department of Employment.

Further information is available from Section F.I.D. 2 of H.M. Factory Inspectorate, Department of Employment, London.

Dr. H. D. TAYLOR mentioned that it was of considerable importance whether ignition occurred during the process of release. What were the possible mechanisms, he asked, by which this could take place. Was sufficient static charge developed during the release to cause ignition?

Dr. ARTINGSTALL replied that in the particular problems that he discussed he had not been concerned with ignition problems. He was not aware of much work being done on this subject and had no information on it.

Mr. W. O. KEEPING commented that it was all very much a matter of conjecture when one referred to the methods of ignition of gases. He thought there were three very well recognised methods of ignition. One was by a static sparking which was not very probable if the equipment was well earthed and if there was a clean gas stream with no particles in it. Another method was from wear products of compressors, pumps or similar machinery which had been produced under reducing or inert conditions, *e.g.*, with hydrogen or nitrogen. One could get entrained in the gas stream particles of steel which had no oxide on their surface. One could get particles of carbon from pumps—bushes in pumps—which were devoid of oxidation. When these emerged out into the air, particularly if they were small particles, they could become incandescent.

The third method which was fairly well recognised, was where large size pieces of debris became entrained along the pipeline, when friction between the pipe wall and the entrained material could cause temperature rise sufficient to ignite a flammable gas mixture.

Dr. D. H. NAPIER, commenting on Dr. Taylor's question, said that they were starting to make measurements on the discharge of condensable gases from orifices to get some idea of the charge density on the droplets formed, but he doubted whether that was what Dr. Taylor was referring to.

In that case it appeared that the size of the cloud was exceedingly important, not just on a simple droplet surface area consideration, but also on a size factor consideration. He submitted that at that stage it was unknown but perhaps they might be able to comment on it in 12 to 18 months' time.

Dr. TAYLOR said the point that had been exercising him was the build-up of charge within the vapour cloud as it emerged with such velocity and in some cases static ignition followed. In others it did not and ignition only took place at a distance by a boiler or some obviously identifiable ignition source.

Dr. NAPIER added that he thought Dr. Taylor was referring to condensation and the charge on the cloud going either to earth or some system of that nature but he did not think he completely understood the mechanism.

Mr. P. GRANTHAM asked Dr. Artingstall the limitations of the figures that he had produced for rates of flow. What variation was there with the size of vessels? Could one extrapolate them to pretty high pressures and pretty small vessels?

Dr. ARTINGSTALL said that one could not. If it was very high pressure on really small vessels, then one would have had to have considered the unsteady flow. One would have to consider the wave system set-up rather than assume that quasi-steady flow conditions prevailed.

Dr. M. KNEALE pointed out that in his example on dispersal of atmosphere, Dr. Artingstall gave examples measured in metres and feet which caused a bit of ambiguity. During his talk he had suggested that this gave a ground level concentration of only one *per cent*. He wondered whether that figure was purely notional or whether it had some practical significance?

Dr. ARTINGSTALL replied that this was a particular case which was being considered, a typical first generation nuclear reactor which was above most of the buildings. There were no buildings directly in line with where a breach was likely to occur and, if it was a lower duct nearer to the ground, then

the concentrations would be higher. If there were buildings in the vicinity, such a complicated situation arose that he did not think one would ever be able to draw up any general guide lines of the concentrations expected in that vicinity.

At the moment he was debating whether the work was to continue or not. There was a real problem of concentrations near sites when things went wrong and there were no hard and fast rules or ideas as to how this problem might be tackled.

The CHAIRMAN asked whether it was not just a question of how quickly it lost its energy.

Dr. ARTINGSTALL said the equations only applied in the far field. Even if it lost all its energy, there were differences which were sufficient to affect the equations and he thought that near the source, the ordinary equations would not apply.

Mr. A. D. CRAVEN, returning to the paper by Grove *et al.*, commented upon closed-ended ignition in experiments of this sort. That was not meant as a criticism; for a number of years he himself had been involved in such work and he used the bottom end open. It was extremely frightening and dangerous to work with the bottom closed and the top open. The biggest proportion of the mixture was forced out of the top of the tube after ignition followed by a flash in the laboratory.

Mr. WEBSTER commented that he and Dr. Grove have had no experience of experiments with ignition at the closed ends of tubes.

Mr. CRAVEN wondered whether one was perhaps doing the wrong thing by using tubes open at the bottom since other methods may give wider limits.

Dr. J. H. BURGoyNE said that he had always felt that open ends—in other words, constant pressure measurements of flammability—referred to the situation in a large volume of mixture.

He thought that if one had a situation where ignition could occur in a pipeline—particularly near a closed end—then the closed-tube experiment would be appropriate.

One would be likely to get a slight widening at the lower limit and perhaps a more considerable widening at the upper limit. Ethylene, for example, gave considerable extension of the upper limit if you confined the mixture. The technique did need to be related to the plant conditions that one was considering. Another point was that the source of ignition became more influential in a closed tube. One could force an explosion through a confined mixture by the use of more powerful sources of ignition under conditions where one would not otherwise get propagation. If one was applying the results of these measurements to chemical plant, one should see whether any special conditions applied and make suitable provisions if that were the case.

Mr. PHILLIPS thought it depended very much on what you wanted to measure. There was a difference between the limits of upward propagation and downward propagation. He got the impression that if you wanted to measure the property of fuel alone, upward propagation was not appropriate as it introduced aerodynamic effects. Upward propagation was a useful measure of the hazard presented by a gas or vapour mixture in air.

Dr. BURGoyNE agreed with that. He thought that some work that Dr. Levy did with him when he was at the Imperial College brought out clearly that in upward flame propagation there was an element of convection flow which helped to

determine the limiting conditions for flame propagation. It was only in downward propagation that you got strictly combustion-controlled behaviour. This was important if you wanted to use limit data for fundamental comparisons with other combustion properties.

Mr. WEBSTER said attempts were made to check Levy's hypothesis according to which the rate of propagation at the limit should be proportional to  $\sqrt{D}$ . In fact experience showed them that rates of flame propagation at the limit tended to be independent of tube diameter.

Dr. BURGOYNE thought that the question of speed was confused by the gas flow. What one was observing in upward propagation was not simply a combustion phenomenon. The point he was making was that in downward propagation one could see something which was more related to burning velocity but in upward propagation the flow pattern was rather complex and what one was observing was burning velocity modified by the gas flow pattern.

A speaker said that he was interested in heat losses and had done some calculations assuming that the only heat loss was by radiation. It rather looked, on a calculation basis, that the total heat loss for a given gaseous mixture would be independent of vessel size. He thought that one could understand that in the sense that the area to which you were radiating was proportionate to  $R^2$ . The time taken was

proportional to the  $RQ$  factor. The total volume of gaseous mixture was proportionate to  $RQ$  and for a given gas mixture vessel size should not affect radiation killing anyhow.

A rough calculation for hydrocarbons indicated that something like six *per cent* pressure drop was due to radiation losses. He could see that two gaseous mixtures with the same energy release had different flame velocities and one could, therefore, get in a large vessel some small percentage difference in mixed pressure.

Moving to Mr. Phillips' paper, Dr. BURGOYNE understood it to relate to the case where an explosive mixture was contained and the tendency was for flame to pass outward through the flame arresting system. Did the author, he asked, think that it could be applied to a flame arrester which was stopping the propagation of flame in a pipe as distinct from expanding from an enclosed space into the open?

Mr. PHILLIPS thought there should be a tie-up. The difference between the flame arresting mechanisms was that for the safe gap mixturing was occurring only at the side and at the head of the jet. With a flame arrester the mixture was occurring only at the head of the hot gas stream which was flowing through the arrester. They had tried some very crude experiments which showed that with flame approaching the arrester from the closed end it was hot gas which passed through the arrester and re-ignition occurred at some distance beyond the arrester.