

DISCUSSION OF PAPERS AT THE FOURTH SESSION

Mr. E. WARDE stated that there were many items of equipment fabricated from Monel metal in use in the chemical and allied industries, the majority of which had given exceptional service and had been produced, in most cases, by the fusion-welding of plates, sheets, bars, tubes, etc. The most severe radiographic standard which had been demanded up to the time at which the Springfields project was conceived, was that of the then current version of the A.S.M.E. Boiler and Pressure Vessel Code. The various electrodes and the filler-wire discussed by the authors were covered by the relevant A.S.T.M./A.W.S. Specifications. The appendix to the electrode and filler specification suggested that if radiography were required, it should be in accordance with the A.S.M.E. Boiler and Pressure Vessel Code, Section VIII latest edition, Paragraph UW-51. Radiography of weld deposits was not mandatory for acceptance of electrodes and fillers. It was a remarkable tribute to the British chemical plant industry that it had achieved a radiographic standard in excess of Lloyd's Class 1, using electrodes and fillers primarily developed to meet only A.S.T.M./A.W.S. Specifications.

Regarding the welding of nickel-base alloys, Mr. Warde considered that the documentation was sufficient to enable the British chemical plant industry to fabricate equipment in those materials in accordance with the requirements of the A.S.M.E. Code. Such fabrications were regularly carried out in various shops in the U.K.

Monel metal had a long record of service with hydrofluoric acid, particularly in the petroleum and the petro-chemical industries. The production of high-octane gasolines for aviation was associated, to a very great degree, with the Phillips alkylation process using hydrofluoric acid as a catalyst. Usually the acid was regenerated by distillation, to recover both anhydrous and azeotropic (nominal 40%) acid as top and bottom products respectively: hence the use of Monel for the distillation column and ancillary equipment. One such unit, producing detergent alkylate, had been in service in the U.K. for over five trouble-free years. Literature references go back as early as 1943 detailing actual plant experiences which did not follow those of the present authors.

Most of the references emphasised the importance of avoiding oxidising conditions, for example, with dissolved air or an inorganic radical such as ferric or nitrate ion. The technical publications of the speaker's company made similar emphasis and there were case histories on file of the premature failure of Monel and other nickel-base alloys operating under acidic oxidising conditions. The N.A.C.E. Technical Committee Report publication 59-14 was pertinent in that connection.

The presence of even traces of impurities could have a most exceptional effect and it was the speaker's opinion that the influence of sulphides was similar, and even greater than that of nitrates. Furthermore both, in combination, exerted an even greater corrosive influence than either species individually. That comment was specific to the writer's own work on Monel and hydrofluoric acid. A similar case involving an 18/8 Ti vessel handling traces of sulphides in an acid fluoride environment has been reported from Springfields and in that

case both parent and weld metal suffered accelerated attack. Excessive corrosion might well be associated with impurities introduced by the material being processed at Springfields. Mr. Warde emphasised that his company was actively engaged, in close collaboration with the Culcheth Laboratories of U.K.A.E.A., on the general problem of excessive attack on both weld and parent Monel metal under conditions similar to those at Springfields.

The problem of the apparent "galvanic" variation of the weld deposits was most complex and reports from the United States of America indicated that while excessive corrosion of parent Monel metal had been experienced, problems had not been encountered with weld deposits. The plant in question was handling materials similar to those at Springfields.

Mr. Warde continued that it would be of interest to know:

If the welds were radiographed prior to test and, if so, what standards were achieved?

Were tests, carried out in a media with 1% oxygen in the atmosphere, expected to provide a valid indication of plant behaviour under the more rigorous conditions of oxygen control currently practised?

In describing the relative corrosion of welds, the terms "anodic" and "cathodic" were used. Had potential measurements actually been made on 140, 130, and 60 weld metal-deposits and parent metal under conditions which simulated those associated with plant operation? As the cause of the accelerated weld corrosion was still a matter of conjecture, would it not be prudent to suspend judgment until the matter had been investigated more exhaustively?

Mr. HILL said that the welds on the specimens shown had been radiographed and passed to the standards described. Further tests, not described in the paper, had been carried out on specimens having repeat welds, *i.e.* the weld had been cut out and remade a number of times. That had been done because the joints in the plant which had been re-welded in order to establish the necessary weld standard appeared to corrode faster than joints made correctly on the first try.

The test specimens agreed qualitatively with the plant findings, but it was still too early to state rates.

With regard to oxygen contents of the atmospheres, he agreed that 1% was very high. All it probably indicated was that the solution was saturated with oxygen. That could be achieved at figures fairly close to 0.1%. It had been found that tests of such nature, where oxygen was passed through the atmosphere at a properly controlled rate, could be related to plant performance.

The use of the term "cathodic" was made on the assumption that electrolytic corrosion was involved. He meant to imply that the welded metal was not corroding as fast as the parent metal. He agreed that it was assuming a lot to say the weld was cathodic.

It was intended to make measurements of the potential across the welds, but they were very difficult to do in a way which would yield results of value.

Mr. Warde had commented on the fact that the weld specifications demanded for the Springfields plant were higher than the then currently accepted codes. A similar situation had previously arisen at Windscale with regard to stainless steel welding. The necessity for such strict welding specifications should be understood.

In many processes the change from mild steel to stainless steel or some other alloy frequently resulted in a change from corrosive to virtually non-corrosive conditions, and all that was required of welding, apart from considerations of strength, was that it should plug the gap. But when dealing with a medium that corroded the corrosion-resistant alloy and where the corrosion allowance was significant in terms of plant life, then porosity in welds and similar faults assumed added importance apart from structural soundness. If there were an air bubble in the middle of the weld it reduced the thickness which the acid had to penetrate.

He supported Mr. Warde completely on the matter of the presence of impurities. If a test were made in pure hydrogen fluoride, one did not get the correct corrosion performance. It was not yet known what the impurities were at the Springfields plant. It had a general relevance insofar as he did not know any chemical processes in existence which did not have impurities associated with them. The contributions of himself and his co-author were given in the hope that people wishing to use hydrogen fluoride would have some idea of the problems.

Mr. J. I. KRZYMUSKI said that MacDonalD had described a number of steps taken during the design stage to eliminate the emission of toxic vapours. What warning was given to the operator that the plant was dangerous? The compound could not be smelt so easily as hydrogen fluoride.

Mr. MACDONALD said that the only direct indication of a safe plant would be the absence of cholinesterase depression on the part of the operators. Any leakage of compound 'B' would be shown by an oily appearance and vapours would be detected by the analyst if they persisted. Beyond that, there was little that could be done to detect toxicant 'B'. All joints had to be scrutinised regularly to make certain there was no leakage, alarms were fitted to the condensers on the water supply of the distillation column and the refrigeration vent-condenser. There were only two or three cases of any serious cholinesterase depression and they occurred before the original plant was altered but they had had a month's operation of the unaltered plant under very careful conditions. He believed that the cholinesterase of the person in charge of the plant dropped from 100 to 70 over a period of four weeks, one operator experienced a depression to 30, another to about 40-45; those results were considered to indicate an unsafe plant. The arrangements made were precautionary and they had no means of rapidly detecting the presence of compound 'B'; chemical analyses performed on manual samples taken at frequent intervals took about 3 hours per analysis.

Mr. HILL said that detection gear had been installed at Springfields to detect hydrogen fluoride but the nose was far better. Many cases had occurred where a leak could be seen and smelt, but because the wind did not happen to have blown the vapours near the instrument the alarm had not gone off. The whole room or the whole area had to be scanned for effective detection by instruments.

Mr. D. B. PURCHAS said that while MacDonalD was making the obnoxious compound he had been making the same compound elsewhere. It was most interesting to compare in retrospect their respective methods.

There was a marked divergence in basic philosophy. Thus Macdonald had put the entire plant in the open air with the idea that anything escaping by leakage would be able to get right away. In his own company they had first split the process into two independent stages. The first stage produced a non-toxic intermediate, and was therefore treated as a normal industrial process in one of the ordinary factory buildings. The second stage, wherein the highly toxic product was formed, was placed right at the back of the factory site, well away from other operations. It was then subdivided into two connected parts, the first part handling safe materials and the second toxic materials. The toxic section was housed in a locked building, with the safe section for convenience located adjacent to it in the open air. All operation of the toxic section was by remote control, using simple extended spindles, etc., protruding into a control cabin. Fans kept the locked building under a slight vacuum and discharged exit gases high into the atmosphere.

That approach had avoided some of the detailed problems encountered by Macdonald, such as, for example, the need he felt to irrigate the agitator gland on his reaction vessel. Nor did they need to worry about recovering solvent uncontaminated by the toxic product since solvent used in the toxic second stage of the process was never recycled to the safe first stage.

Although they were thus able to discharge any toxic gases well up into the atmosphere, having first diluted them liberally with large amounts of air, there was nonetheless a factor which worried them and which should surely have been of even more concern to Macdonald; that factor was the weather. It is all very well discharging gases into the upper air, but a temperature inversion such as caused fog and mist to linger would also cause stack gases to return to the ground. In Macdonald's case, in addition to that risk, was there not also the danger that the wind, free to blow through the plant in any direction, might have caused very dangerous pockets of gas to build up in quite unpredictable localities?

In connection with that type of hazard, Macdonald had mentioned the use of a refrigerated condenser to minimise the discharge of organic vapours into the air. He had also mentioned non-condensable gases. There appeared to be something a little incompatible about those two points.

The hazards associated with the hopper device for adding potassium fluoride to the reactor were rather frightening. At his company such hazards had been avoided by making that stage of the process a completely liquid reaction between aqueous potassium fluoride and a solution of the intermediate in an organic solvent. The technique had the further advantage that there was no need for a filtration step as used by Macdonald; instead, gravity separation of the aqueous and organic solutions sufficed.

Macdonald referred to the regular checks on cholinesterase level of operating and maintenance staff. The same procedure was used at Mr. Purchas' company, where in no case did the level fall to any serious extent for staff on the plant producing this particularly toxic compound. By contrast, however, an extremely serious incident occurred on a different plant manufacturing another organophosphorus insecticide which had been regarded as relatively safe to handle. Of the three people affected, two recovered fairly quickly, but the third

suffered permanent disability ; at one time seriously paralysed, she has never fully recovered. Did Macdonald have any serious incidents, or was the worst experienced merely a slight fall in cholinesterase level which resulted in the individual affected being switched temporarily to other work ?

Mr. MACDONALD said that the experience with the particular 2nd stage reaction, where they added solid potassium fluoride to the solution of 'A' in toluene, was that it was one requiring the supply of heat ; it was not an exothermic reaction. The reactions were different : why the difference should cause one to be exothermic and the other endothermic was not clear. However, the Schradan reaction was known to get out of hand if an impurity were present in the reactants. If the main reactant products were transferred from the reaction vessel to the intermediate storage tank, in pumping it down in order to do the transference, something like 000.7 lb of 'B' would be transferred. That would take place four times a day and was not a very serious load for the activated carbon adsorbers. Throughout the plant adsorption was used only for removing traces of toxicant only. On the question of the effectiveness of activated charcoal as an adsorbent for toxicant traces he was reminded of a paper by Sharp.¹

The treatment of activated carbon and the recovery of the material from it was arranged so that the recovered material flowed back again into the reactor and the toxicants, assuming they were undecomposed by the steaming-out, did not therefore reach the atmosphere but flowed instead into the reaction vessel. With regard to the treatment of material recovered from activated carbon from the water-ring pumps, the idea was to use the recovered liquids to dissolve the caustic soda used in the recovery process.

Thus in the recovery system where the caustic soda was virtually boiling the toxicants would be destroyed chemically.

There was another process that they had worked out for full-scale use but did not use because it was not economic. The idea was to combine the aqueous toxicant solutions from all sources and to use them for making caustic soda solution which would be fed continuously with aqueous dimethylamine hydrochloride to a combined reactor-distillation column. That would yield pure anhydrous dimethylamine as head product and sodium chloride solution as bottoms. The adoption of that modification would have provided a means of destroying the toxicant solutions and at the same time would have eliminated a fire hazard.

Mr. PURCHAS had over-estimated the difficulties of safely transferring solids to the reactor. In any case the slight disadvantage of handling solid potassium fluoride was more than offset by the very high yields, which closely approached the theoretical, as well as the very easily controlled and therefore safe reaction. There was no possibility of the reaction getting out of hand and rendering hazardous an area for miles around the plant. Despite the fact that the solvent medium had to be recovered the economics of the process certainly did not suffer seriously partly because raw material costs were the principle element of product cost.

It should be mentioned too that the patent position called for a process different from the one to which Mr. PURCHAS referred.

With regard to Mr. PURCHAS's comments on the refrigerated condenser, the removal of small quantities of condensable vapours in the presence of non-condensed gases was very widely practised (*e.g.* dehumidification). The calculation might be awkward in the case considered, since there were

present three condensables, one of which was immiscible, and in fact a method had to be devised. Checked against small-scale tests it was shown to give an overestimate of the surface required for a given duty. The condenser used—a piece of equipment released from the earlier process—was shown to have a healthy safety-margin of surface and was able to handle the filling displacement even were the vessel reflux condenser to be inoperative. Moreover, the adsorber placed downstream of the refrigerated condenser also had sufficient capacity, when fresh, to handle twice the entire vapour load discharged during the charging operation.

As for design philosophy, the decision to place the plant in the open was taken when it was first designed for comparatively innocuous compound. It was true that the fact that the plant was in the open was looked upon as an advantage in the event of an unexpected leakage of toxicant, but that view did not amount to a design philosophy. In the case of the plant modified to make compound 'B', at no time was the view taken that toxic vapours could be allowed to escape to the surroundings. On the contrary it was decided to allow only the merest traces to be vented and then only at a height. Part of the paper indeed was concerned to show how little of compound 'B' could be tolerated.

Dr. J. H. BURGoyNE asked Miller and Penny about the propagation of the acetylene explosion at and around atmospheric pressure. In the study made, a 12 in. pipe was used, and initiation was over the whole area of cross-section of the pipe by means of an explosion at higher pressure bursting a diaphragm. Did the author consider that there was any prospect of the explosion propagating at atmospheric pressure in pipes of smaller diameter? Did they think that shock was essential to initiation at this pressure, or was it sufficient to initiate by flame across the whole area?

Dr. MILLER said that the incidents that occurred in the field were definitely cases of initiation across the whole length of the pipe. That was so in Germany. There were similar cases in America. In an American case, the operating pressure was 15 lb/in². The tests done by the Germans, down to slightly sub-atmospheric pressures, were definitely cases of initiation across the whole length of the pipe (which was 20m long and 12 in. diameter). In their own experiments propagation had been made to go along 600 ft of 3 in. pipe. Initiation at a point did tend to die out, certainly at atmospheric pressure and up to about 22 lb/in². At high pressures, however, point initiation did set up progressive explosion.

Dr. BURGoyNE asked if the shock was necessary.

Dr. MILLER replied that it was not and it could be done by having a flame round the whole perimeter.

Mr. D. B. PURCHAS took up Macdonald's point about the toxicity of the compounds. On the question of what level of cholinesterase the human body could suffer, one point was to realise that the figures were on a purely arbitrary scale. They did not mean anything in a direct sense. One could exceed a value of 100 ; it was not a percentage level. His company had encountered levels down below 10, which had caused considerable alarm and despondency, but the patients always recovered. How low a level could be tolerated had not been determined, so far as he was aware.

On the question of toxicity and the spraying of cocoa and other products with the compounds, the whole principle on

which they were founded was that systemic insecticides by definition entered the system of the plant. Once in the plant, they hydrolysed to form non-toxic decomposition products, and a tremendous amount of work went into it determining the rate at which that breakdown occurred. The particular insecticide had a high rate of breakdown, so the period referred to by Mr. Macdonald was quite safe.

Dr. F. SJENITZER referred to p. 93 of the paper by Miller and Penny. It was rather surprising that it was stated that bursting discs were not successful. Would the authors expound on how they had inserted the bursting discs?

Mr. PENNY said they were not very confident about bursting discs in an explosive system. In actual experimental work, they had set up systems of a long pipe with a T-piece, and at the other end equipment for measuring velocities. They had worked at 10–20 atm, not atmospheric pressure, and the velocity was 2000 m/s. They had put in bursting discs to see what the effect was on the velocity measured: it was nil. Detonation was set up in the pipe at a distance of not more than about 4 ft from the initiating point and travelled at a stable rate, blew out the bursting disc, but carried on completely unaffected.

They had then put in a bursting disc close to the initiation point. Again the bursting disc operated, and they had explosion velocities of the order of 1800 m/s. In other words, a bursting disc simply took no notice of a detonation in the pipeline. Where bursting discs were useful was where the reaction was being carried out in a solvent and it might run away but not actually explode. An acetylene reaction was almost invariably highly exothermic. The running away could develop high pressures at considerable temperatures, fairly rapidly from the ordinary point of view, but very slowly compared with explosion and a bursting disc was then effective in relieving the vessel. Then there was always the problem as to what to do with the vented materials: so far the authors discharged them high up into the atmosphere, where a large flame would not do any harm. When really big plants came into production that provision might be inadequate.

Mr. G. V. DAY said that in view of the difficulty that had been experienced in the use of Monel, he wondered how much attention had been given to the possibility of using various types of plastic material—P.V.C. or non-porous carbons, or ebonite. With moderate temperatures they might be useful. Mention had been made of air-operated valves with Monel bellows. Had the use of diaphragm valves been considered, such as the Saunders type?

Polythene has been used successfully for cold liquid hydrogen fluoride at all strengths chiefly as piping. Rigid P.V.C. has been used successfully in contact with hydrogen fluoride vapour diluted with air, steam and nitrogen, mainly as fume ducts and fume cupboards.

Most of the problems of corrosion by hydrogen fluoride concerned the containment of hydrogen fluoride at temperatures above the safe limit for polythene and P.V.C.

Mr. KNOTT said that they had used P.T.F.E. which was rather difficult to fabricate and handle. It had been used successfully in jointing material and in bellows. It was also used successfully as a lining for pipes but that was rather a new development and they had no experience of it in a practical sense.

The problem of air-operated valves had been attacked in two ways. One was to keep the bellows warm, which had been the practical way of dealing with those already installed. P.T.F.E. bellows were being developed. There were some difficulties in application but once they were overcome the bellows would probably be successful.

Another way of dealing with the problem in one particular instance was to do away with bellows altogether, and to use a stuffing box type of gland packed with nitrogen to prevent escape of hydrogen fluoride.

Mr. J. O. S. MACDONALD asked Grover about putting one liquid on top of another to prevent the evaporation of the liquid underneath. That had occurred twice in the process reported in his own paper. The difficulty was that they could not find a liquid that would float and yet not extract the toxicant. Others which were satisfactory chemically would not float. Had Grover any experience of small plastic beads which had been used as layers on top of the surfaces for restraining evaporation?

Mr. GROVER said that it was often difficult to find a fluid which would float and which was non-volatile. Fortunately, the substances they dealt with were, on the whole, fairly heavy, so that they had quite a wide selection of liquids which would float. Beads would probably have some use.

Mr. A. H. GOODLIFFE said that appreciable portions of the beads sank after a time and caused operational difficulties. Their use was of doubtful value.

Mr. P. M. SALES, referring to the section of the paper by Miller and Penny on explosion arrestors packed with Raschig rings, asked what the diameter of the arrestor tower was. What would be the effect of varying the dimensions of the pipe on the height of packing required to quench detonation? What would be the effect of gas flow on the behaviour of an explosion arrestor? Would it be greatly different from that under the static conditions employed in his tests?

Dr. MILLER said that the Raschig rings were 1 in. diameter in a tower 9 in. diameter. They had not done any field work with a larger diameter pipe, but with a larger diameter pipe a larger diameter tower would be needed to quench an explosion: the height should be much the same as for a smaller pipe. Detonation velocity was only slightly dependent on pipe diameter; the quenching would be much the same. Tower explosion-arrestors of larger diameter were in use in various parts of the world. Gas velocity did not affect the results very much. Their experimental work was designed originally to have gas flow, but so far they had not been able to carry out tests with gas flow. The authors did not think it would make very much difference, if any.

Mr. P. M. ORTON referred to the statement of Miller and Penny that, in handling compressed acetylene where there was electrical equipment, close contact with any possible source of acetylene escape and a consequent ignition must be avoided. Did he recommend that the electrical equipment be submitted to water gauge air pressure to prevent source of ignition, or was it regarded as adequate to use flame-proof equipment conforming to B.S. 229?

Dr. MILLER replied that there was no flame-proof equipment available to a British Standard. Acetylene and hydrogen were

in Group 4. Recently, some flame-proof equipment for hydrogen had been claimed, but he did not know whether that was fully acceptable. There was no British Standard for flame-proof equipment for use with acetylene. It was possible to avoid ignition by the pressurising of the plant and it had been done on large-scale plant : it worked quite satisfactorily.

Mr. K. M. HILL said that Macdonald had described a very elegant method for purifying the toluene after it had been through the distillation column ; the necessity for that was based on the estimate that it was not possible to get down below 15 p.p.m. in the column. Was that based on vapour pressure data or carry over data?

Mr. MACDONALD replied that it was carry-over data. It was a point very well worth looking at from a more fundamental angle. Their experience with the packed distillation column was that however they manipulated the reflux ratio they could not improve upon those figures. His conclusion was that it was due to entrainment and apart from fine mesh entrainment mats which were installed early on, they could do nothing to improve the purity of the solvent.

Mr. D. F. RILEY said that in the practical design of storage tank installations for flammable liquids it was quite a common practice to put a flame trap of conventional gauze-type over the end of the vent. He had done it many times. He had been fortunate in that none of the tanks he had put in had ever suffered an accident, but he often wondered whether, if there was any fire, those flame traps would, in fact, be of value. Was there any information available which would help one to decide whether they were of any use?

Mr. PALMER said that for petroleum storage, the practice had been to use a 28 mesh gauze, which was 28 meshes to the linear inch, either a single or double layer at the end of the vent pipe. Some tests had been done using propane/air mixture which showed that, if the flame trap was put right at the end and ignition occurred that end, it would cope with the flame and hold it. But if the flow of vapour came up below the flame trap, that flame trap would get very hot very quickly and did not have much margin to spare. If the flame trap was put lower down the pipe in an attempt to protect it from external influences, the speed would be much higher and it would probably fail. The flame trap had to be put within one foot of the open end.

Mr. GROVER said that he was not aware of other research in the field.

Mr. F. FARRINGTON said that Klinkenberg and Van der Minne² had recommended limiting pipeline velocities of gasoline to 1 metre per second to avoid the build-up of static ; he asked whether Grover knew of the application of similar limitations to other liquids.

Dr. L. J. BURRAGE wrote :

The safety precautions mentioned by Hill and Knott are, in the opinion of the writer, completely misleading, particularly the statement at the top of p. 101 under the heading Conclusions " its burning effects, in liquid form, are more severe than most acids ". If ever there was an understatement, this is it. My own personal experience with this acid dates back to 1939 when in control of pilot plants producing 80% acid and, a year or so later, anhydrous hydrofluoric acid. This was

before the days of the discovery of calcium gluconate as a material to be injected into a burn by a medical officer, which stops the intense pain immediately. The writer has actually seen men nearly mad with pain from a comparatively small burn and there is plenty of evidence medically of mutilated hands, with fingers lost due to the action of this acid, and yet the authors have the temerity to class this as merely " more severe than most acids ". Personally, having handled a very wide range of extremely dangerous chemicals, the writer still puts this particular acid in the highest place.

On p. 101 it states that it has been found possible to tolerate small leaks. That is a terrible admission for a chemical engineer to make! Anyone with plant experience knows how a trace of vapour can affect the face soon after shaving, involving the necessity to rub magnesia paste over the face to avoid the reddening and stinging resulting therefrom.

Another statement on p. 101 says " operators wear face shields and gloves in addition to normal overalls ", but on a plant of this sort it should be light protective kit at least when carrying out inspections or valve operation for the risk from the acid is there.

Eyeshields are also mentioned. This should of necessity be face shields and operators who use otherwise are asking for trouble.

Finally, on p. 95, it is stated that acid of over 60% strength will cause immediate apparent damage and pain. This is definitely not a true statement. It is entirely dependent on the type of skin in contact with the acid. Anhydrous hydrofluoric acid burns can remain undetected for hours. One further extreme understatement is " contact with the eyes is particularly dangerous and can cause blindness ". The only real treatment of an anhydrous hydrofluoric acid burn is injection of calcium gluconate, but this cannot be done with an eye. It is therefore absolutely essential that perfect eye protection must be worn at all times to ensure that at all costs no hydrofluoric acid can reach the eye. The situation is a far more serious one than the article would have us believe.

Mr. HILL wrote in reply to Dr. Burrage :

It was not intended that the present paper should state the medical problems of handling hydrogen fluoride other than briefly ; in view however of the comments of Dr. Burrage, Dr. F. W. Meichem of U.K.A.E.A. Springfields Works has contributed the note following this reply.

The toleration of small leaks is largely dependent on how small is small. Springfields' experience with hydrogen fluoride dates back to 1946 and with the small leaks that are tolerated we have not experienced or seen any possibility of the serious casualties of Dr. Burrage's experience. It is evident that the tolerated leaks described in the paper are orders of magnitude smaller than those envisaged by Dr. Burrage.

The experience in the use of protective clothing described on p. 101 can be amplified. Face shields or eye shields, gloves, and normal overalls are worn for normal operation in the reactor cells where the hydrogen fluoride is essentially gaseous ; in those cells containing hydrogen fluoride boilers and the distillation plant, light P.V.C. coveralls are worn in addition.

The choice of eyeshields rather than face shields on maintenance work is to some extent a matter of opinion. Eyeshields are preferred at Springfields as giving positive protection to the eyes whereas face shields although giving some facial protection give only partial protection to the eyes. Cases have occurred, not with hydrofluoric acid, where splashes have entered the face shield and injury has resulted.

Dr. MEICHEN's comments are given below :—

“ . . . It is clear that Dr. Burrage has been strongly influenced in his comments by unfortunate happenings in the pre-war period, which we have been able to avoid.

“ . . . I think it is fair to say that hydrogen fluoride is a particularly nasty substance—the more so in that the type of burn produced tends to progress for a much longer period and therefore damage is more widespread than with most of the other acids in common use in industry. However, we have had no serious mishaps at Springfields, and probably the twenty-odd years of progress in safety methods and plant design have played a major part in preventing this, and in addition there have been certain developments in technique of which Dr. Burrage is obviously unaware.

“ Without in any way attempting to belittle the effects of this substance, I think that it is unwise to rush into the other extreme and credit it with destructive powers beyond those which it does possess. For example, it is not correct to give the impression that all hydrogen fluoride will produce burning, no matter how low the concentration. The example which he quotes of the skin smarting after shaving is a particular case : after shaving there are many tiny areas denuded of their protective material, and any strong acid, or even a particularly cold blast of wind, would produce a reactive effect locally. In the case of hydrogen fluoride, normal uninjured skin will tolerate 0.2% on contact for twenty-four hours without visible or irritant effect. Skin which does not do this would indicate a hypersensitivity. The results of a study of this and other acids can be found in Merewether.³

“ With regard to actual treatment, we have for some years now forsaken the calcium gluconate therapy, which can be extremely painful in certain areas which are likely to be the site of an accident (*e.g.* the finger-tips, where tissue is somewhat scanty), and nowadays we tend to use the ice therapy, followed by treatment with one of the cortisones, which, of course, were not available in 1939 ; the results of treatment of this nature have been extremely good.

“ I am in agreement with Dr. Burrage's very forcible point of view regarding eye protection : there is no doubt that the eyes are particularly vulnerable. This is the case not only with this acid but with any other, and damage to the fine tissues can easily result in scarring, with subsequent impairment of vision. In all chemical operations we do stress that it is vitally important to deal with any injury of this type at once, on the spot. What is done within the first fifteen seconds probably decides the issue, and this is why, throughout the plant at Springfields we keep a liberal supply of first-aid equipment for eyes.”

References

- ¹ Sharp, D. H. “ *Disposal of Industrial Waste Materials* ” 1957. (London : The Society of Chemical Industry.)
- ² Klinkenberg, A. and Van der Minne, J. L. “ *Electrostatics in the Petroleum Industry* ” 1958. (Amsterdam : Elsevier Publishing Co.)
- ³ Merewether, E. R. A. “ *Industrial Medicine and Hygiene* ” Vol. 3. (London : Butterworths Medical Publications.)