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Letter to the Editor

The assessment of major hazards: The lethal toxicity of chlorine

This is a belated and brief response to the comments on our paper on the crosschecking of our model of the toxicity of chlorine to man [1, 2] from accounts of gas warfare in the First World War [3] by Griffiths and Fryer [4] and by Marshall [5], the delay being occasioned by the retirement of one of us and the extended absence of the other on public business.

At the time when we did our work there was a wide range of estimates of the lethal concentration of chlorine to man. One value which was frequently quoted as the LC_{50} for a 30-minute exposure was 30 ppm. Our own estimate was considerably greater and towards the upper end of the range of quoted values. We had already stated that the value was unlikely to be much higher than that which we had adopted and in our third paper took it as read that the question to be decided was whether the value should be lower.

Our model was cast in the form of a probit equation, which has two parameters, the intercept and the slope. The intercept determines the LC_{50} for a given exposure period. We described our work as a crosscheck on our model. Our concern was in fact to crosscheck the LC_{50}, but we did not make this clear. We agree with Marshall that our work does not give a crosscheck on the slope. For a given LC_{50}, there is a family of probits of different slopes which will give rather similar results in the type of crosscheck with which the paper was concerned.

The chlorine gas clouds behaved as a dense gas. We gave some of the eyewitness evidence for this. We used, however, a passive gas dispersion model, for the reasons given below. There was no doubt in our mind as to this distinction, but the critiques indicate that it seems to have caused some misunderstanding.

Our reasons for using a passive gas dispersion model were as follows. One was that there was no one clearly preferred dense gas dispersion model and we wished to avoid getting into the question of the merits of various models. Another was that the dense gas dispersion models did not appear to have been validated for infinite line sources. Another was that a passive gas dispersion model was expected to give a lower bound to the gas concentration so that the survivals would be for the concentrations estimated or *higher*. We half expected that others would check our results with a dense gas dispersion model and are glad that this has been done.

The results obtained by Griffiths and Fryer indicate, with the exception considered below, that, as expected, the gas concentrations given by the dense gas dispersion model are higher than those given by the passive gas dispersion model. This provides support for a higher lethal concentration.

The exception is the conditions where the top of the gas cloud is so low that it is near head height. Here Griffiths and Fryer's results for their first scenario, that with the lowest cloud height, show that the height of the gas cloud is below head height, taken as 1.7 m, as far as about 160 m from the source. They also quote results of a further run in which tuning of the inputs extends the distance at which the cloud remains below head height to 200–250 m. Further, these authors draw attention to the fact that in the Thorney Island and Maplin Sands trials there were considerable fluctuations in concentration over a vertical extent of about 2 m from the ground, quoting particularly results obtained at 100 m from the source.

We agree that this feature of a cloud near head height should be taken into account. At Hill 60 the men stood fast. The proportion exposed at distances of less than 200 m was taken as 13% and at 200 m as 25%. If the gas cloud was below head height to just beyond this latter distance, men at distances of 200 m or less would have been exposed to concentrations lower, and those at higher distances to concentrations higher, than we estimated. At Langemarck 15% of the men were taken as being at distances of less than 200 m and 50% at 200 m. Again for a gas cloud reaching head height just beyond 200 m, the effect would be to decrease the concentration at less than 200 m and to increase it beyond. In this case we believe that most of the men fled. Our results showed that an appreciable proportion of the toxic load was experienced at distances greater than 200 m.

With regard to a cloud near head height, we conclude that (a) the effect does introduce greater uncertainty into the interpretation of events, but that (b) on what we regard as the most probable scenarios the lower concentrations near the source are largely counterbalanced by the higher concentrations further from it.

In describing the gas attacks, we gave various quotations, and some paraphrases, trying to give as complete an account as we could, but did not intend thereby to endorse all the material.

On details of the Langemarck attack, we will mention just five: (a) the numbers exposed, (b) the state of the trenches, (c) the behaviour of the gas cloud, (d) the degree of flight and (e) the level of physical activity. Our estimates of the numbers exposed were based on figures supplied by the Army Historical Branch (AHB) [6] for the numbers in the front line trenches. This source also quotes the following concerning the Canadian dispositions: "On the 13th Battalion's front, 3 of its 4 companies were in the front line. Two platoons were in support positions 700 yards behind." The rest of the fourth company were further back still. In other words, at this stage of the war it seems to have been practice to put the majority of men in the forward area actually in the front line.

The trenches appear to have been rudimentary. The AHB give the following quotation concerning the state of trenches taken over by the Canadians at this time: "The front line consisted of unconnected lengths of untraversed trench usually but 2 ft deep.... cluttered with frail shelters. Apart from these the defensive works were four groups of shelter trenches for supporting platoons between 300 and 700 yards behind the front line." An account by McWilliams and Steel [7], which is concerned primarily with the Canadians, who had just taken over on the French right, and which we were not aware of at the time of writing, quotes to the same effect. We took it as read that the gas, being dense, would fill the trenches.

The gas cloud appears to have left part of Langemarck itself unaffected. The Algerians there put up a spirited resistance. Our reference to the cloud lifting at this point was simply a paraphrase of the German accounts. We noted it, but it plays no part in our treatment. Marshall draws attention to the fact that in a gas attack there were sometimes gaps in the cloud. The aerial photograph which he gives of an unidentified but later gas attack shows gaps in the cloud originating in gaps in the line source itself. Some such effect combined with topographical features seems a sufficient explanation.

On the question of whether the men held fast and were gassed in the trenches or fled, Marshall's arguments are essentially *a priori*. We quoted various eyewitness accounts to the effect that there was large-scale flight. Further evidence is given by McWilliams and Steel [7]. In addition to the sources which we quoted, they give accounts from some six additional witnesses, five of them Canadians, each with a separate reference, to the effect that men fled in large numbers.

In any event, our interest was primarily in the survivors and the degree of exposure which they suffered. We described a scenario in which men were exposed to a toxic load by retreating through the gas cloud. Despite this exposure a large and known number survived and were taken prisoner by the Germans. Even if our estimate of the number who survived and reached the Allied 'lines' is too high, the overall picture is of a large number of survivors despite exposure to high concentrations.

Our model of chlorine toxicity includes a factor for the level of physical activity. This factor is part of our original model and is based on the experimental relation between levels of activity and inhalation rate; it is not a value assumed in order to fit the gas warfare case. In a given case, however, it is necessary to decide what level of activity is to be assumed. We assumed a level of exertion slightly greater than unhurried walking, to allow for a degree of panic, and believe this is about right. As we stated, we initially assumed a higher level of activity.

With regard to the Wulverghem attack, there is doubt as to whether the release was one of chlorine only or of chlorine/phosgene mixture. We gave an analysis assuming it was chlorine only, but in view of the doubt about the gas and the further complicating factor that by this stage troops had respirators, we stated that this attack gave less information. We should perhaps have made it clearer that our conclusions were not based on this case.

Marshall also makes a number of points concerning the problems of determining injury relations in general and the assumptions underlying probits in particular. We agree with many of these points, but will not discuss them here. Suffice it to say that the difficulties in obtaining injury relations and the possibilities of their misuse are not sufficient reasons for abandoning the attempt to derive them.

The point of our paper was to try to determine whether there is any clue from chlorine gas attacks as to whether the lethal concentration of chlorine is towards the upper or lower end of the range of estimates then current. We said no more than that we regarded our preferred reconstruction, which accorded with our model of toxicity, as more probable than alternative ones. We have discussed above some of the principal criticisms and conclude that at least the thrust of the points made is not in the direction of a lower lethal concentration. The critiques made suggest, however, that further work of this nature would be unlikely to result in a consensus view.

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