

be lacking in our duty as scientists and citizens if we were to accept, without question, assurances of the validity of which we have not been convinced.

Finally, we must admit that we would prefer to obtain "a still greater degree of safety . . ." by stopping war altogether, but we do not feel that this preference invalidates our results. Experiments are to be believed, not on account of the authority or bias of the experimenters, but because they can or cannot be repeated by anyone who chooses to do so. We would accordingly urge that the whole question of the protection of the population from aerial attack should be studied openly by representative scientists; then a rational estimate can be made of the probable efficiency of any measures which are finally adopted.

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ONE of the grievances of the authors seems to be that they issued a challenge to the Home Office to produce the data on which their gas protection experiments were based, and that this challenge has been ignored.

The Government has, no doubt, its own reasons for not publishing the data, but it would be fair to suppose that the recommendations it has made are well supported by scientific facts which have been established by its own advisers—men, as I pointed out, who have carried out all its poison gas experiments during the last twenty-odd years.

The authors quote from a book recently published in America by Colonel Prentiss, and they refer to him as one "who seems to have had much experience of these matters".

I agree; so let me in turn quote from the chapter "Protection of Civil Population": it will be seen that the Home Office recommendations are supported in full.

"Shelter from chemicals launched during air raids is to be obtained for the most part by the utilisation of existing structures with such gas-proofing as may be justified. The erection of special collective shelters designed exclusively for the protection of civilians during aerial attacks is only warranted under exceptional circumstances; at best, such buildings can accommodate no more than an insignificant proportion of the inhabitants of any community. . . .

"The individual or small-group shelter of this type will not require special air-purifying devices to counteract noxious gases, but instead will be air-sealed when occupied, its occupants depending on the air present in the room for the necessary oxygen. Arrangements should be made in advance to prevent all in-drafts of air, while the shelter is in use, and overcrowding with consequent rapid exhaustion of free oxygen must be avoided."

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An Air-borne Plant Virus

IN a recent issue of NATURE¹, Dr. Kenneth Smith described the virus of a new disease of plants as being "air-borne". The term in this connexion implies: (1) that the virus agent can escape from the tissues of the infected plant into the air under natural conditions; (2) that it is carried in the air; and (3) that the air-carried virus can infect a normal healthy plant.

In the light of the generally accepted view that the virus is intimately connected with the protoplasm of the host cell, and that it is unable to enter an uninjured protoplast, the demonstration of the easy passage of a virus from a plant into the air would be of great interest. It is not clear that Dr. Smith has succeeded in demonstrating this. All he has shown is that in three out of an unstated number of experiments, plants were found to be diseased after they had been rubbed with cotton-wool pads through which the air of a glass-house had been drawn. If the virus is present in the soil, as Dr. Smith suggests, and if it resists desiccation as he now states², it is not unreasonable to suppose that in the dust of a glass-house some traces of the virus might be present. Even if this were demonstrated, however, it would scarcely prove that the virus is air-borne; tobacco leaf dust contains the virus of the tobacco mosaics.

Dr. Smith has not shown that the virus does in fact reach the air from the tissues of a living infected plant, or that being in the air it can infect normal healthy plants. Until this has been demonstrated under controlled conditions, Dr. Smith's description of his virus as "air-borne" would appear to be premature.

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¹ NATURE, 139, 370 (Feb. 27, 1937).

² cf. Smith, NATURE, 138, 396 (Sept. 7, 1935).

DR. CALDWELL'S chief criticism is directed against a point which has already been proved¹, namely, the ability of this virus to enter the roots of a plant without the aid of any known external agencies. Dr. Caldwell says that the term "air-borne" implies "that the virus agent can escape from the tissues of the infected plant into the air under natural conditions". I fear this is wider than my conception of the term; I only meant that the virus was carried in the air, with the added implication that it was able to reach and infect virus-free plants by this means. It never occurred to me that anyone would suppose that the infected plant was discharging virus particles into the air after the manner of an influenza patient. Indeed, the following sentence from my letter makes my meaning quite clear: "It is not perhaps surprising that such a minute virus . . . and one capable of withstanding complete desiccation should be present in the air."

Dr. Caldwell further says, "tobacco leaf dust contains the virus of the tobacco mosaics". Of course it may do, but this disease cannot spread in the same way, even if a suspension of tobacco mosaic virus is poured round tobacco plants growing in pots, and herein lies an interesting difference between these two viruses. Again quoting Dr. Caldwell . . . "it has not been shown that being in the air it can infect normal