to 3×10^{10} to 10^{11} crystals g^{-1}. This revised estimate has a rather dramatic effect on the number of cloud turrets which can be treated with a given quantity of dry ice. For example, with the same assumptions as those made by Holroyd *et al.*, but using our revised E value, 100 kg of small dry ice pellets could produce ice particles in concentrations averaging 10 ℓ^{-1} in about twenty-three 25 km^3 turrets, whereas, Holroyd *et al.* estimated that about 100 such turrets could be treated. If ice particle concentrations of 100 ℓ^{-1} are required in the clouds, Holroyd *et al.* concluded that ten 25 km^3 clouds could be treated with 100 kg of dry ice, whereas, with our revised E value, only about two such turrets could be seeded. It should be noted that both our estimates and those of Holroyd *et al.* are conservative in the sense that the nonunity counting efficiency of the IPC has been ignored. If the counting efficiency is allowed for, our estimates for the E value (and therefore the numbers of treatable clouds) could rise to, or even exceed, the values given by Holroyd *et al.*; but in this case the estimates would no longer be conservative.

We agree with Holroyd *et al.* that in many respects dry ice is an attractive cloud seeding agent.

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Reply

EDMOND W. HOLROYD III,[1] ARLIN B. SUPER[1] AND BERNARD A. SILVERMAN[2]

Bureau of Reclamation, U.S. Department of the Interior

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We greatly appreciate the agreement of Hobbs *et al.* (1978) with our conclusions in Holroyd *et al.* (1978) that dry ice is an attractive agent for on-top seeding of convective clouds. We also appreciate their pointing out that both of our estimates of ice particle concentration are conservative and that if the counting efficiency is allowed for, their estimates for the dry ice effectiveness (E) values and, therefore, the numbers of treatable clouds could rise to or even exceed the values estimated in our paper. This is precisely the message of our paper and for that reason the issues raised by Hobbs *et al.* are quite academic.

Hobbs *et al.* also confirm and amplify many of the problems with ice particle counters (IPC) that we mentioned only briefly. The problems they mention were known to us and we had discussed them with Dr. Radke and Ms. Politovich and with Mr. P. Lawson (who helped develop the CSI–IPC) on the day after the intercomparison flight. At that time we knew the relative difference between the two ice particle counters and attributed that difference mostly to the setting of the threshold of detection, as we mentioned (Holroyd *et al.*, p. 52). We also mentioned the scattered sunlight and large water drop problems because we recognized this in the data and excluded these effects in our analysis. The sunlight problem occurred infrequently and only at the fringes of a cloud during aircraft exits and thereafter in clear air. The large (~100 μm) water drop problem does not occur in the small continental clouds studied at about the −10°C level, where we rarely, if ever, found 100 μm water droplets (Cooper, 1978). We never observed "noise" associated with high concentrations of micron-sized water droplets in the CSI instrument. We appreciate the confirmation that the precautions we took with the data were necessary.

[1] Miles City, MT 59301.
[2] Denver, CO 80225.

Hobbs *et al.* present details of the only intercomparison we were able to make between the two IPC's. They showed that in a cloud with a natural variability ranging over several orders of magnitude the two instruments flying similar but non-identical tracks had a difference of only a factor of 4.4 over the averaging period used by Hobbs *et al.* They therefore confirm the factor of 4.3 that we calculated using the entirety of both the southward (which they show) and the northward passes in that cloud. The excellent photographs made by the side-looking camera on the University of Washington's B-23 aircraft, when combined with flight track data, show that the intercomparison cloud was a flat-topped, nearly vertically sided, naturally glaciated turret rising about 0.7 km above a solid layer of cloud which was at the 5.4 km level. Using the photos as stereo pairs some whisps of cirrus were seen above the turret, but there were no neighboring clouds rising above the general deck for a distance of many kilometers. The cloud was penetrated at the −14°C level 0.5 km below the top which had an estimated temperature of −18°C; the penetration distances were about 11 and 18 km on the successive passes, with the CSI aircraft entering and exiting the cloud first. The remarkable similarity of the crystal concentration profiles and the PMS probe data (even greater agreement on the second, unillustrated pass) underscores the excellent navigational precision of the flight crew of the B-23 in following the CSI aircraft through the same parts of the cloud.

In our paper we stress that this is a "preliminary intercomparison"; the resulting normalization was "for convenience" and the factors "should be used as general guides only." We assume *no* instrument to be measuring ice particle concentrations "correctly" and present all of the numbers we used on p. 54 so that anyone can revise our calculations as they see fit. The overall tone and structure of our paper shows that we expect revisions.

The cloud used for the intercomparison was significantly colder (with respect to expected crystal habit) than those used for our calibration seeding experiment. We knew from our discussions that crystal habit affects counting efficiency and state this on p. 52. Therefore a further reason for caution in applying the normalization factor of 4.3 is that the factor may not be the same for different crystal habits.

It was not the intent of our paper to present rigorous intercomparison data between the two instruments or to imply in any way that one instrument was better than the other. If we had intended a better intercomparison, we would have requested an examination of the foil and Formvar samples made by the University of Washington aircraft in that same cloud. We are disappointed that, while stressing this intercomparison, Hobbs *et al.* (1978) make no mention of their analysis of these independent measurements of ice particle concentration that are in their possession.

The CSI-IPC is presently government property, but it is still being improved by Mr. Paul Lawson under a different contractor. Mr. Lawson acknowledges in a personal communication the inspiration he derived from a copy of an early prototype of the UW-IPC that was assembled at Colorado State University. He stresses, however, that only the polarization concept of the University of Washington prototypes was retained in the CSI-IPC flown in 1976. The CSI version uses a photodiode detector rather than a photomultiplier. The light from a polarized laser beam is not blocked anywhere by annulus rings or light traps and both it and the lesser scattered light can shine directly on the photodiode when depolarized by an ice crystal. The UW-IPC uses a light trap to protect the photomultiplier from the direct laser beam; it therefore counts its ice crystals by forward-scattered light only. The electronics beyond the two different detector systems are therefore different as well.

Particle Measuring Systems also uses the depolarized direct-beam (via a beam-splitter) and photodiode system on some of their two-dimensional instruments. They have found (personal communication) that most of the depolarization signal is in the direct beam rather than in the scattered light. Though a photodiode is less sensitive to a depolarization signal than a photomultiplier, the overall signal-to-noise ratio is much better for the direct beam system than for the scattered light system.

The CSI-IPC is therefore *not* a copy of a UW prototype but an instrument of a different design that derived only its basic inspiration from the UW version. We stated in our paper that the differences between the two are probably due to threshold settings and possibly to effective sample volume measurement problems, but the fundamental differences between the instruments may also be a significant factor. In fact, there could be many reasons why the two instruments, flown on two aircraft with similar but non-identical tracks through the cloud at slightly different times, could differ. Therefore, we are quite encouraged by the similarity in measurements and cannot take the differences seriously.

Laboratory measurements of counting efficiency are mentioned both in our paper and the comment of Hobbs *et al.* We do not consider the laboratory measurements to be of ultimate authority in the calibration of the IPC's. In the CSI study, aircraft speeds were simulated but the tubes guiding the crystals to the laser beam may have distorted the room's crystal concentration to be measured by the CSI-IPC. Furthermore, the crystals were small, unrimed, perfect, and of a different habit from those we expect to find during our seeding operations. Those of the intercomparison flight may have had a habit similar to the laboratory crystals but were probably larger, significantly rimed, and mostly irregular in shape. The Formvar samples in the possession of Hobbs *et al.* could confirm this.

Lawson (1978) quotes a 20–25% ratio between concentrations measured by depolarization and those measured by an optical array on the same PMS two-dimensional instrument. Using two-dimensional data alone and a different calculation technique, he arrives at a nucleation effectiveness of about 10^{12} ice particles g^{-1} for dry ice seeding. Multiplying the conservative effectiveness values of Holroyd *et al.* (1978) by 4 or 5 to correct for the counting efficiency of the IPC's yields about the same number. Lawson thereby confirms our previous results and suggests the size of the more absolute value of effectiveness.

We regret having to mention a minor technicality. The CSI data system recorded all of its data at 1 s intervals. Summaries of the data at 5 or 10 s intervals are readily available in hard copy, and the 1 s data are available to anyone via computer.

In summary, we are thankful for the agreement with our former conclusions and the confirmation of many items we had already stated. We applaud the closeness of the measurements when the instruments were both known to be undercounting, had different lower thresholds and operating characteristics, and flew similar but non-identical tracks through the clouds at slightly different times. We agree that the threshold of the UW-IPC was correctly set for the clouds the B-23 is likely to encounter; large drops must be excluded in maritime clouds. But the counter will then miss more of the copious amounts of small crystals produced by seeding compared to an instrument tuned to the smaller droplet sizes of continental clouds. We also see no need to stress any similarities to measurements made by foil, as foil is definitely an inferior detector of small ice crystals. We expect valid revisions of our calculations as more knowledge is obtained. One of the reviewers of our paper wrote: "As methods of detecting and counting ice crystals in clouds improve it will be possible to make the estimates of effectiveness even more quantitative. In the meantime the results presented here provide a useful guide to those wishing to use dry ice for seeding supercooled clouds."

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