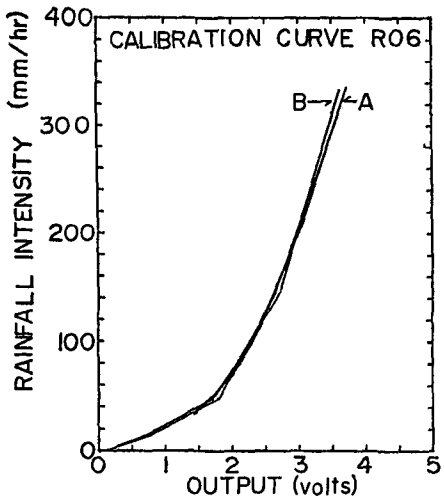

FIG. 4. Sample calibration curves for the rain gauge. A and B represent, respectively, the calibrations for distilled and tap water.

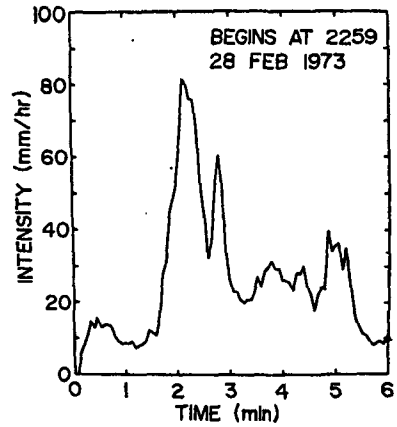

FIG. 5. Typical record of a trade-wind shower. Note the rapid fluctuations in rainfall intensity.

electrodes C, D and E is broken, the multivibrator turns off, and the output drops to zero. If a conductive film is present, the multivibrator will continue running at a slow rate, R_1 will be finite, and the gauge will "hallucinate."

It is not clear whether the conductive deposit is from the rainwater, from ordinary dust and dirt, or is a result of chemical action on various metallic parts of the gauge. The effect becomes noticeable after about one month of operation. However, periodic cleaning with ordinary household ammonia appears to restore the gauge to its original condition.

Fig. 5 shows the trace from a typical high intensity trade-wind shower. The output of the gauge was fed into an analog-to-digital converter connected directly to the CPO computer. The signal was sampled every 4 sec, and voltage was converted to rainfall intensity by means of a four-term polynomial fitted to the calibration curve. Note the extremely rapid variations in intensity.

Fig. 5 amply justifies the search for a rapidly responding rainfall intensity gauge. The observed microstructure in the rainfall must correspond to a similar microstructure in the convective clouds. This structure may now be studied on a routine basis.

Acknowledgments. We would like to acknowledge the excellent work of Arne Austring, who constructed both the mechanical and electrical parts of our rain gauges. This work was supported by Office of Water Resources under Grant G-026-HI and the National Science Foundation under Grant GA-28624.

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Comments on "A General Description of the Hail Problem in the Po Valley of Northern Italy"

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9 August 1973 and 9 September 1973

Inasmuch as we have read with interest this extensive historical review (Morgan, 1973), we feel that a few comments on some of the points which were raised and on more recent hail supression activities in Italy are in order for the benefit of the reader. A large proportion of our work, which has been carried out during the last five years or so, had to do, under some form or other, with the hailstorms of Asti

Province, whereby some local experience was gained. Our remarks will hence be restricted to Morgan's statements to that specific effect, especially since some of those statements appear to call for both revision and addition.

Thus, there hardly seems to be much point in quoting an article published in a local Turin daily newspaper (*La Stampa*) when some of the specific information contained therein is extremely biased. It would have been just as fallacious to rely, for example, on other similar daily local newspaper estimates, since differences in evaluations of this kind may reach an order of magnitude, from day to day. It would have been much safer to rely on the official reports of the Provincial Inspectorates of Agricultural Statistics. Asti, in fact, appears to be one of the few Italian Provinces where meticulous estimates of hail damage to crops have been carried out, by this Government-operated agency, for more than a decade now. Incidentally, their figures which deal with hail damage to crops have already been discussed, in detail, in a number of recent publications by Gori *et al.* (1971a–e) and there is hence no need to repeat those data in this comment.

The current seeding program (as well as that of the previous year), aimed at preventing hail over both Asti and adjacent areas, appears to be based on the use of droppable pyrotechnics, possibly of the type discussed in detail by Summers *et al.* (1972) and others before him, as well as of fusees, presumably of a type similar to that described by St. Amand *et al.* (1970); we understand that the active nucleators delivered by those means may be lumped together under the name of silver iodide smoke. This, at least, is what some local Italian officials who now promote that activity appear to be convinced of. The test is allegedly being carried out by a private contractor and seems to be financed out of local regional and provincial funds. There is definitely no mention, however, of any "alumina" being presently used or of it being the active ingredient of those smokes, although since those pyrotechnics may sometimes incorporate a small percentage of aluminum metal, which by combustion normally yields Al_2O_3 (i.e., alumina), said smoke may, of course, contain a small proportion of such particulates. In any event what appears to be a well-endowed effort is now under way along those lines in Piedmont, and it is to be hoped that those who both promote and presently work on that project will, in due course, present their results to the scientific profession in a published form. This is a good moment, however, to state that none of the staff of our laboratory is in any way associated with this program, and that its further discussion is clearly not our responsibility.

As substantiated by Gori *et al.* (1971f), we fully concur with Dr. Morgan to the effect that Asti is a fascinating place to carry out hail suppression work. This is why, during the years from 1969 through 1971, we undertook, in that area, a project aimed at testing some of our newly-developed nucleator materials against hail. Those nucleators were first described by Montefinale *et al.* (1969a, b) after several patents were obtained thereon, said patents having been duly commented on by *Chemical Abstracts*. Some fog suppression tests were also carried out by Montefinale *et al.* (1970) with this material. The mechanism of action of those nucleators was further commented on by Montefinale *et al.* in a recent review (1971) as well as in the report of the Asti test (Gori *et al.*, 1972). On the basis of those references, it is quite clear that the bulk of the original nuclei either consists of a sodium aluminate-chloride mix which incorporates *some* alumina, or of a sodium aluminate-alumina matrix doped with Al_2S_3. For the sake of simplicity, this material might have been referred to merely as somewhat unorthodox giant condensation nuclei and/or giant ice-forming nuclei, respectively, but those compositions are obviously entirely different from simple alumina.

Also, far from being commercial, our Asti test was financed to the amount of 85% by the National Research Council of Italy, and by the local Chamber of Commerce for the rest. Its planned and actual duration was three years (1969–71), and its potential (but never executed) local extension was thereafter supplanted, possibly because of "scientific-political" reasons that are too complex to explore here, by the aforementioned commercial ventures.

Finally, to the best of our knowledge, no rocketry of any kind—with the possible exception of fireworks exploded during local fiestas—was employed in the Asti Province between 1962 and 68. Certainly none was used there over the period 1969–71. Neither was any silver iodide dispersed by any means into the atmosphere of that province during those latter three years.

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Reply

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5 November 1973

The main thrust of the comments of Papée *et al.* appears to be twofold: 1) they seem to feel that their hail prevention project was slighted or misrepresented in my article, and 2) they are critical of the new project being carried out in Asti Province and wish to make certain that they are in no way identified with it.

On the first of these I disagree. It was not my intention to give a detailed description of all the seeding projects that are under way in the Po Valley, and thus I did not dwell on any of them. I was simply developing the point that there are so many weather modification efforts under way in a rather small region that if at all effective, they are contaminating one another. I should have underlined the fact that none of them is randomized in any way.

The average yearly loss figure for 1965–69, which I derived from figures quoted in *La Stampa*, was, as best I can recall, attributed by the paper to the Provincial Inspectorate of Agricultural Statistics, Asti, and agrees favorably with the 1964–68 average of the figures reported by Gori *et al.* (1971; 3.8 vs 3.3 million dollars, using the old exchange rate of 625 lire to the dollar). Therefore, I cannot understand their objection to it.

I am sorry if I inaptly characterized the nucleating substance utilized by Papée and his group as an alumina dispersion. From their comment one can see that it is difficult to apply a simple name to it. In view of the ambitious claims made for it, it should be investigated by some recognized independent laboratory group. It may or may not be a valuable addition to the weather modification arsenal. Mr. Dennis Garvey of the Cloud Simulation and Aerosol Laboratory, Department of Atmospheric Sciences, Colorado State University, Fort Collins, has indicated a willingness to undertake tests of the substance. His laboratory has served as the national test facility for nucleation techniques for several years.

As Papée *et al.* note, their project has lost its funding and been replaced by a larger, commercial project being run by an Italian division of a large American company. I know very little of this new project except that its 1973 operations have been accompanied by disastrous hailstorms. I second Papée *et al.* in their hope of seeing a full report on this project in the scientific literature.

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CORRIGENDUM

The following correction in the article by Paul W. Mielke, Jr., entitled "Another Family of Distributions for Describing and Analyzing Precipitation Data" (*J. Appl. Meteor.*, **12**, 275–280), was kindly pointed out by Dr. Oskar M. Essenwanger. The last line of the second column on p. 275 should have been as follows:

$$\mu_2'/(\mu_1')^2=\alpha B[3/\alpha,\ (\alpha-2)/\alpha]B^2[2/\alpha,\ (\alpha-1)/\alpha]=g(\alpha).$$