February 18, 1997

Dr. William R. Cotton, Professor
Atmospheric Sciences Department
Colorado State University
Fort Collins, Colorado 80521

Dear Bill,

(Here's my letter, the way it started out some weeks ago..... I've been fiddling with it every now and then since.)

I just received my copy of the 1995 edition of your book, *Human Impacts on Weather and Climate*, co-authored with Roger Pielke (CP95). All in all, I think your book is a pretty fair account of things.

However, I was surprised, and I guess you'd say disappointed (read, "went into a rage and threw things"--like the Japanese do these days in specialty boutiques stocked with breakable ceramic items for this purpose), that the assessment of cloud seeding wasn't updated to reflect important events through 1992 (the apparent cutoff date of your book). I know you consider the subject of weather modification important because cloud seeding receives more attention than does global warming in your book.

Also, because Kenneth Young's 1993 book had included discussions of recent events in cloud seeding which have occurred in the 1990s, (e.g., the new analyses of the Israeli experiments by Gabriel and Rosenfeld 1990-*JAM*; Rosenfeld and Farbstein 1992-*JAM*), I was looking forward to your independent assessment of these developments in particular. I thought that these papers were going to be "big news" in your book because these researchers had presented so many interesting new results and hypotheses about clouds and cloud seeding in Israel, many of which conflicted with the traditional view of these experiments that had been expressed in so many review articles and books over the years (e.g., Wallace and Hobbs 1977, Tukey et al. 1978; Kerr 1982-*Science*; Silverman 1986, Cotton 1986, both *Met. Monogr.*).

Also, since Rosenfeld and Farbstein (1992) had attributed "divergent" results of seeding in the in Israeli II to the interfering effects of a super ice-nucleating "dust/haze" aerosol on numerous exepriment days, and because the study of ice nucleation has been an important one at CSU, I expected to see some thoughts on the "dust/haze" hypothesis in particular.

Needless to say, with the advent of these new analyses of the Israeli experiments, a lot of questions have arisen concerning them. I undertook a reassessment (with Peter Hobbs) of both

Israeli experiments (i.e., Rangno and Hobbs 1995-*JAM*). My first paper (note-sized) concerning the Israeli experiments and the clouds of Israel was submitted to *JAM* in July 1983 and rejected in January 1984 (B. Silverman, personal communication).... Gist of that paper? From plots of rawinsondes at Beirut and Bet Dagan when rain was falling at the time of the launch or within the hour, it was falling from clouds with far higher cloud top temperatures than could be explained by the reports on cloud microstructure in Israel by A. Gagin.

It is now fairly well-established (e. g., agreed upon by all parties connected with RH95, including the reviewers--Bill Woodley, Daniel Rosenfeld, and Arnett Dennis) that the many descriptions of the clouds of Israel published in numerous articles up through 1987, were inaccurate. I would say, *astonishingly* inaccurate.

The first suggestion that something was wrong appeared in the literature in 1988 (c.f., Rangno and Hobbs 1988-*Atmos. Res.*; Rangno 1988-*Quart. J. Roy. Meteor. Soc.*) These reports have been directly (Levin et al. 1994-*Atmos. Res.*; 1996--*JAM*) or indirectly (Rosenfeld and Gagin 1989-*JAM*); Rosenfeld and Farbstein 1992-*JAM*) confirmed. I thought I would see *something* about those reports which had filtered in up through 1992 in CP95. Thus, the actual clouds of Israel are markedly at odds with the descriptions of them in CP95.

In the CP95 discussion of a cloud seeding temperature window, I also wondered if you realized that Grant and Elliott (1974-*JAM*) did not measure cloud top temperatures when deducing their seeding window of -10° to -25° C? Instead, they made some assumptions about the height of cloud tops for each of the various cloud seeding locations they discussed.

As a forecaster with the Colorado River Basin Pilot Project (1970-1975), I became intimately familiar with, well, the *lack* of reliability of the methodology employed by GE74 to assess cloud top temperatures (using constant pressure levels such as 500 or 490 hPa temperatures as they did for the Wolf Creek Pass experiment). In fact, I was told about the discrepancy in 500 hPa and cloud top temperatures even before the CRBPP started (!) by Owen Rhea and Paul Willis who had learned first hand about this discrepancy during their work on the Park Range Project of the late 1960s.) You also may wish to see Paul Mielke's comment on this problem in the *J. Amer. Stat. Assoc.* in 1979. I thought his statement was one of the most courageous, straight-forward, "no excuses" assessments of cloud seeding experiments and the hypotheses on which they were based that I have ever seen in the literature.

However, both Grant and Elliott were also aware of the general lack of agreement between 500 hPa and cloud top temperatures at Wolf Creek Pass, one of the locales they discuss results for. For example, Lew had been told three times just in my presence about the lack of a sufficient correlation between the two parameters for their use as an index of cloud top temperatures before 1974 (by Owen Rhea in 1971, by Larry Hjermstad 1972 or 1973, and then Dick Medenwaldt in 1973--at a BuRec conference in Denver). Bob was the chief evaluator of the CRBPP and was also well aware of the discrepancy between cloud top temperatures and constant pressure temperatures.

We are having this conversation today, and much of the reason I continue to work in this area today with an extemely skeptical eye, is largely because of the Grant and Elliott paper. This will sound extremely melodramatic, but THAT paper changed my life because I saw that research published in journals concerned with cloud seeding could be like Hollywood movie sets; outwardly robust and plausible, but with little substance behind them even when authored by people I admired. It was at or near this point I decided to reanalyze the Wolf Creek Pass and Climax experiments--a process begun in the fall of 1975 in Durango and which led to Rangno (1979-*JAM*, and Hobbs and Rangno (1979-*JAM*).

OK, some "bio".... Why did this paper have such an impact on me in particular? None of the other researchers who were aware of the discrepancies mentioned above were motivated to action by this paper (and that, too, was deeply troubling to me). I think it was because I was one of those strange little "weather kids", obsessed with the sky and its changes, subscribing to the Daily Weather Map in 1952, Monthly Weather Review in 1955; I'm sure I was the only kid in my high school who knew who Jerome Namias was, and, of course, I was a storm chaser (missed the eye of hurricane Carla in 1961 by 75 miles).

But I went farther than this; I tried to get Jacob Bjerknes' autograph when he was at UCLA, and had had my photo taken with Tor Bergeron in 1968 while working one summer for Bob Elliott's group in Santa Barbara. I thought of those in the upper echelons of meteorology (e.g., scientists like yourself) the way other (normal) kids thought of sports stars. As an aside, it will be a moment of great irony in this regard that, as of the March 1997 issue of *JAM*, I will become the most criticized meteorologist in the history of meteorology! (The Tech. Ed. of *JAM* has advised us that no single paper in the history of the AMS has had so many pages of criticism--this mostly due to the comments of D. Rosenfeld on RH95.)

To continue: as I learned of Bob and Lew's achievements when I came to the CRBPP in 1970, I gained immense respect for them, too. But the paper you cite by them, and I know this, too, will seem melodramatic, was painful. I never again read a paper on weather modification with the same trust as we usually impart to authors of journal articles. How this paper was even published is a question that would be interesting to answer. For example, I don't believe it would have been published had National Weather Service forecasters in those locales discussed by Bob and Lew (that is, a forecaster who plotted rawinsondes) could have passed favorably on a paper that linked temperatures at constant pressure levels to cloud top temperatures--the atmosphere just doesn't work that way. This was a fact that day-to-day forecasters knew but apparently scientists who reviewed journal articles did not. Hence, Bob and Lew's article is not reliable for detecting a cloud seeding temperature window as you conclude in CP95.

Neither can you rely on Gagin and Neumann's (1981-*JAM*) temperature stratifications as evidence for a "seeding window" either. The radar cloud top heights are not reliable. Some of the reasons why these data are unreliable are mentioned in RH95, but Dr. Rosenfeld will have something more definitive to say about the unreliability of these data later in the year (though--you figure it out--he will be seen defending these data in the March issue!)

Concerning related areas of cloud microstructure and cloud seeding windows, DeMott et al. (1982-preprint), among others, have found no relationship between cloud top temperature and ice particle concentrations (reflecting an old bugaboo which casts a shadow across any supposed cloud seeding window).

I was also surprised that you didn't mention the paper by Meyers et al. (1992-*JAM*). I believe their paper, which finds that ice nucleus concentrations are significantly higher than previously believed, also helps explain why the perceived cloud seeding window has shrunk, a phenomenon you discuss. Auer et al. (1969-*JAS*) and Vardiman (1972-CSU atmos. sci. paper, 1978-*JAM?*), Cooper and Vali (1981-*JAS*) all reported ice multiplication in the Rockies, and the Meyers et al. paper helps explain some of it.

Do the Climax experiments provide "compelling" evidence for seeding effects, as is concluded in CP95? I have to disagree. And I doubt very many people would agree with you on this.

The Climax experiments were *heroic* attempts to confirm the seeding potential outlined by Ludlam (1955-*Tellus*), no doubt about it. But they have just not stood the test of time because, I think, the progenitors of those experiments grabbed on to some questionable meteorological-physical linkages, and just would not stand back and question them when contrary evidence came in.

The erosion in confidence in these experiments, begun in the late 1970s, should have been reflected in your book. For example, Rhea (1983) addressed the problem of the lack of synchronization of the control and target gauges in the Climax experiments (target gauges were read in the morning, control gauges in the evening of that day or of the previous day!). These are among many other problems which Peter and I have discussed in *JAM* (1979, 1987, 1993, 1995) in the years before and following Rhea's comment.

It's too bad Lew can't redo those experiments with the kind of instrumentation we have these days, and with better generator locations. Maybe it would be done right this time. I do think he is a decent fellow. And Lew, unlike A. Gagin, would *never* deny a researcher access to his radars to see cloud top heights (as Gagin did to me in 1986), you can be sure of that!

In your next edition, I officially request that I write pages 10 through 17. (I'm smiling as I write this because I *am* joking, of course.) If you'd like, though, I would offer some comments for you and Prof. Pielke to at least *consider* in your next edition.

To close on a positive and optimistic note; I'm thrilled over the developments at NCAR concerning hygroscopic seeding. This is *the most* exciting development I've heard about in 27 years of involvement with this field and makes me as an enthusiastic about seeding as I was coming into the CRBPP. I sensed from your writeup on hygroscopic seeding that you, too, found this a promising avenue. Maybe we'll get it right yet... I *truly* hope so.

Well, I hope some of this doesn't seem *too* provocative; it's not meant to be.

Sincerely,

Art Rangno
(note spelling,
by the way, for your
next book...)