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5 December 1988

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Dear David:

Thanks for chapters 2, 3, and 4 of your thesis, which I have quickly read. I'm dictating these comments immediately so they can get to you well before your defense of thesis, and to accelerate the process, I've asked Eleanor to send the letter to you Federal Express during a time when I'm away from Ithaca.

Overall I think it is an excellent nucleus to the thesis -- showing good physical insight, commendable scientific caution, interdisciplinary breadth and an occasional touch of humor. Some of the summaries of the arguments of others -- for example, Walker et al., 1981 -- are extremely lucid. Following are some comments on the three chapters -- with the emphasis, as you ask and in interests of time, on Chapter 4. I have mixed in here issues of content and issues of style and typography, in page order. On bibliographical issues, I am just enclosing a xerox of the pages that need correction. (Ignore the little tick-mark at the beginning of some of the references, which are to remind me to read or reread these papers.)

Chapter 2

Page 2, first line. I would add running water here explicitly: It is the principal erosion source and is not the same as weather.

Page 2, line 4. Many: only about 10.

Page 3. Bins differing by  $\sqrt{2}$ : If this didn't originate with Bill Hartmann, it certainly has been popularized by him. Should he be credited here?

Page 5, first complete paragraph, fifth line. Emission.

Page 7. "Death star." I think the boys at Berkeley more often called it "Nemesis."

Page 7, end of incomplete paragraph. You should mention that all attempts to detect Nemesis -- including serious tries by Muller himself -- have failed.

Page 8, 4 lines from the bottom. Sagdeev tells me that merely recalculating cometary masses using Halley albedos significantly increases this number, as does assuming that most comets have the mass of Halley's Comet. In any case, you indicate much higher Oort cloud masses a little later in this chapter.

Page 15, first complete paragraph, line 1. Lunar should be lowercase "l". Mare (Latin singular for "ocean") should be maria (Latin plural for "oceans").

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Page 20, middle of page. Same comment.

Page 34, last sentence, first incomplete paragraph. It does not seem either Darwinian or Lamarckian to me, because there is nothing like reproduction in this process.

Page 40. There should be a graph of a thick line (representing the likely errors) showing how  $\alpha$  is imagined to decline with time. It would be nice to see something similar for the half-life, but I recognize that this may be much more tricky.

Page 43, line 10. Remove "to".

### Chapter 3

Page 2, line 5 from bottom. Young and Young (actually, I think they're about the same age).

Page 7. Toby Owen recently gave a talk at Cornell on the D/H ratio on solar system objects, and I would strongly urge you to send him a copy of this chapter. He may not be able to respond in time for your thesis defense, but I think this is very worthwhile, nevertheless.

Pages 11-14. This seems to me a very promising argument.

On this chapter as a whole, I have sent you under separate cover some early work I was involved in on primitive oceans on Venus.

### Chapter 4

On how the K-T extinctions compare with others, see J. John Sepkoski, "Phanerozoic Overview of Mass Extinction," in Pattern and Process in the History of Life, D. M. Raup and D. Jablonski, eds., Berlin: Springer-Verlag, 1986.

Page 3, second line. "Solved." Your discussion does not reflect the degree of dissent from this conclusion, although I buy Alvarez's argument. However, shouldn't there be some reference to, for example, Hallam, Science, Vol. 238, 1987, p. 1237; or Officer et al., Nature, Vol. 326, 1987, p. 143, and the rebuttal by L. W. Alvarez in Physics Today, Vol. 40, 1987, p. 24? Also it would be good to mention the evidence for an enormous quantity of soot particles that seems to have been produced in the K-T event (Wolbach et al., Science, Vol. 230, 1985, p. 167; and Nature, Vol. 334, 1988, p. 665.) The latter works suggest that "a single global fire" was triggered by the impact, and that it began "before the ejecta had settled."

Page 3, first paragraph, second and third lines from the bottom. I don't understand how the impact-extinction hypothesis could be confirmed if there is "less consensus on the actual extinction mechanism." Geologists would argue that many species were dying before the impact and that this proves the impact did not cause the extinctions.

Page 5, first complete paragraph, line 6. Provided the dust was higher than the greenhouse gases.

Page 7, first complete paragraph. Stokes'.

Page 11, first complete paragraph, line 4. I have an analytic demonstration of isothermality at great optical depths in a planetary atmosphere in Icarus, Vol. 10, p. 290, 1969.

Page 14, line 3. The isotopic evidence for wildly fluctuating precambrian climates is indeed extremely interesting in light of our conclusions about a stochastic impact regime, and we should lay some stress on it when we write the paper up.

Page 14, end of first complete paragraph, last sentence. For a 1 bar atmosphere.

Page 15, first complete paragraph, line 9. "Subducted". But before plate tectonics begins -- and there was apparently no plate tectonics in the first billion years of Earth history -- subduction cannot return CO<sub>2</sub> to the atmosphere by devolatilizing carbonates.

Two lines later you should have a dash after "assumptions" and in the next line another dash after the final "CO<sub>2</sub>".

Page 17, references at the bottom of the page. These are largely redundant; see your p. 8.

Page 18, incomplete paragraph, middle of paragraph. Are you sure? My impression is that in nuclear explosions a yield of Y kilotons raises 0.1 to 1 kilotons of submicron dust. If this is true and relevant, your values of f are too big.

Page 19, line 4. This proportionality applies no matter what unit Y is measured in.

Page 20, line 5 (and last line p. 21 and several other places later). M<sup>2</sup> should be m<sup>2</sup>.

Page 21, second complete paragraph, penultimate line. I would change "this" to "my primary."

Page 22, 9 lines from the bottom of the text. "Organic molecules." Actually, the high imaginary index of soot is due to elemental carbon (EC, for example, graphite). A comparison of imaginary indices for a range of organic solids, showing the increase as EC increases can be found in B. N. Khare et al., Journal of Geophysical Research, Vol. 92, p. 15,067, 1987.

Page 22, penultimate line of text. "Effective optical depth" -- better to say: "effective" or extinction optical depth.

Page 22, equation 4.12. Check range of validity.

Page 23, second complete paragraph, second line. I think the Ackerman et al. paper has just been published this year -- in the American Journal of Physics?

Page 24, line 2. Here and in many other places in the text, it is important that you define what sort of albedo you're talking about -- geometric, Bond, bolometric, etc.

Page 24, line 5. The experimental demonstration of this proposition is generally credited to Bob Brown.

Page 24, paragraph beginning on this page, line 4. Impact shock: see the experimental work by Bar-Nun et al., Science, Vol. 168, p. 470, 1970.

Page 24, 3 lines later. Has the Freund et al. paper been published?

Page 25.  $\tilde{\omega}$  should be  $\tilde{\omega}_0$ , and asymmetry is misspelled at least twice.

Pages 25 and 26. I'm very glad to see that organic matter in early dust clouds can indeed make a significant counter to the early faint sun paradox.

Page 26, paragraph beginning this page, line 1. "Black"?

Page 26, equation 4.15. I think the factor 2 deserves a 1-sentence explanation; "where" should have no capitals and emissivity is misspelled.

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Page 27, first complete paragraph, last sentence. This is just the value of  $r$  chosen by TTAPS. There are several other places where references are given to later nuclear winter material although the idea or value in question was first introduced in TTAPS. Note the date of Ackerman et al. needs correction. For representative values of  $r$ , see the Khare et al. paper cited above.

Page 29, second line. Shouldn't your conversation with Tom Ackerman be referenced here? Are there any other cases where such acknowledgement should be made?

Page 29, first complete paragraph, line 4. Absorption.

Page 30, second line from the bottom. Tens.

Page 31, incomplete paragraph, penultimate sentence.  $W \text{ m}^{-2}$ .

References. Shouldn't there be some mention of our DPS abstract?

Somewhere it should be indicated that SCOPE is a committee of the International Council of Scientific Unions.

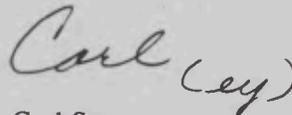
Figures: note that in Figures 4.3 to 4.5 the log of  $\alpha$  is given, not  $\alpha$ .

I think the material of Chapter 4 could now be convolved with some of the ideas I mentioned in my previous letters -- for example, about an early dark steady-state impact dust cloud on Mars, but I know we'll have a chance to discuss some of them in the paper. When you have a moment, let me know when I might expect a first draft of that. I'm also looking forward to the remaining chapters of the thesis.

This is a most interesting thesis, David, and I'm very proud of you.

With warm good wishes,

Cordially,

A handwritten signature in cursive script that reads "Carl (ey)".

Carl Sagan

CS/ey

Enclosures