Icarus Review Submission(02/07/02 at 20:11:43 CST)

MANUSCRIPT: I08179
AUTHORS: Hahn et al.
SHORT TITLE: Clementine observations of the Zodiacal light

Reviewer: William T. Reach

REVIEW SUMMARY

(a). Do you recommend that this manuscript be accepted for publication?
Yes, but with minor revisions

(b). The overall length of the text in this manuscript is:
About right to describe the work and its significance

(c). The amount of display material (tables and figures) is:
About right

COMMENTS FOR THE AUTHORS

REPORT BY William T. Reach ON THE REVISED MANUSCRIPT BY HAHN ET AL.

The authors appear to have carefully considered the comments in my report on the initial manuscript, and have submitted an extensive reply. This all seems to be in order, and I think that this paper is very close to being ready for publication. Below are some minor recommendations and explanations of points from my previous report.

- BREVITY: there are probably still some places where this paper can be

shortened. One easy place to save space (in terms of column-inches in the journal) is by actually making the x-axes of the panels touch each other in Figure 10 and the panels in Figure 11. These should become no more than 1/3 page (Figure 10) and single-column, 1/4 page (Figure 11) figures as their information content is not dense. Figure 16 should also become small. I will leave these extremely minor details to the editor and author to resolve.

- MASS ESTIMATE: In my initial report I called the mass estimate "ludicrous," so I should explain. The authors have tempered the paper so that the calculation does not seem to be overstated too much. Just please note that the mass estimate (equation 10) depends on the radius that is assumed for

the
cloud. Cutting it off at 2 AU "enclosing the orbits of the terrestrial planets"
is an illustrative calculation, but of course the terrestrial planets do not
produce the dust and it is arbitrary to mention them, except to choose a scale.
If asteroidal dust and short-period comets are the source of the particles,
then a nice choice would be to have the cutoff around 3.5 or 2.5 AU. But of
course the mass could be even very much higher, if we were to extend the cloud
out to larger distances. If the distribution were to extend out to the Oort
cloud (which is probably impossible for many reasons including planetary
perturbations and collisions with interstellar dust), then since the mass
scales as r to a power around 1.5, the mass estimate changes by a factor of
millions. So, while I think this mass estimate is interesting and it is not
"ludicrous" for a cutoff kept in the ~3 AU range, it is really just an

illustrative calculation. The "ludicrous" part is the extrapolation to the Oort
cloud, to which I return below.

-->I think that equation 10 should be qualified somewhat.... The values of r can
be too easily evaluated by the reader at distances where they don't really apply.
If the first term really represents asteroids, then it probably shouldn't be
evaluated outside the asteroid belt.

- PHASE SPACE VERSUS REAL SPACE: Maybe the words in my report were a little
confusing when I said that the authors "skipped a step" by going directly to
phase space and not comparing the dust distribution. The basic point was that I
needed to see the radial and vertical density distributions, in order to
compare the model to the observables. By showing only the orbital element
distributions, I was not able to make this comparison. That is the step that
was skipped. The authors have now compared their predictions with
the COBE and Helios results, which is exactly what I needed to see. And Figure
14 shows the dust distribution explicitly. So now the authors have filled the
gap in their exposition in such a way that satisfies what I had seen as a lack.
That being said, there would be significant value in making an

empirical
approximation of Figure 14 in terms of a radial power-law and a vertical fan,
for comparison with previous models. E.g. giving nu and beta from a rough fit
of sigma ~ r^(-nu) exp(-beta*z/r). Table II actually already gives the values
of nu, so only beta would be missing. Would it be possible to include
approximate values of beta in the paper?

Also I note the clear comparison to the Grun (1985) paper for the value of the
cross-section at 1 AU, which helps. The Grun paper also includes some
discussion of the stability of the meteoric complex based on the combination of
the size distribution and the radial distribution. (The beta meteoroids, which
are the small particles, are produced in the inner solar system and move
outward, so the amount at 1 AU is an integral of all the collisions closer to
the Sun. Your observations are directly tracing this inner region, so you
could say something about the stability of the meteoric complex, but this
is a rather complicated topic and it is not necessary to delve into it
in this paper.)

There is one point that is significantly incorrect in the paper and in the
response to my report. Specifically, the authors claim that particles will
maintain their original orbital inclination distribution and will not have it
modified by the effect of planetary perturbations. I am not the world's expert
on this topic, but I have seen calculations by the Florida group that suggest
significant variations of the orbital inclination due to secular resonances.
These lead to a warping of the dust cloud; or in phase space, they lead to a
dependence of the forced orbital inclination and the semi-major axis. The
extent to which this is important for the paper is probably not so great, and I
will leave it to the authors and editor decide whether to correct or modify
anything.

- OUTER SOLAR SYSTEM: Please understand that when I wrote "the connection between
the observed region and the outer solar system is weak," the subject of the clause
was "connection", the verb was "is" and the object, "weak," referred to

"connection." Thus the point was that observations in the inner solar

system do
not tightly constrain anything about the outer solar system. There can be an
infinite reservoir of material that never enters the inner solar system and which
remains unconstrained.

I take serious issue with the parts of the paper that talk about the Oort cloud.
Specifically, the last paragraph of the paper involves an extrapolation from the
inner solar system to the Oort cloud and mentions an uncertainty of a factor of
500, but I think the uncertainty is a factor of infinity. Thus I think this
section needs to be deleted. I don't mind so much the last paragraph of section 4,
which is brought up as a discussion point but not a conclusion, as long as the
uncertainty factor of 500 is changed to something even more qualifying. Just
remove it from the conclusions because it is far too much of an extrapolation.

On page 25, the comparison of the dust mass to the mass of the dust bands is not
quite relevant...the dust bands only represent the family asteroids whose dust has
retained its original orbital element distribution, while many families' dust is
scattered by secular perturbations of Jupiter and of course a large fraction of
asteroids aren't in families. So if you get a total mass that is 3.5 times the
mass of the asteroid family (band) dust, then this seems plausibly due both the
the JFCs and the non-family asteroids.