Icarus Review Submission(06/28/07 at 19:39:39 EDT)
MANUSCRIPT: I10116
AUTHORS: Shepard et al.
SHORT TITLE: A radar survey of X- and M-class asteroids
Reviewer I
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
This paper presents new observations of an important but poorly understood class of main belt asteroids. Even the class itself seems poorly defined with various groups using different classification schemes. The radar data presented here can give new observables to help sort things out, and this work is the largest (but still only 10!) radar study of this class to date. The paper is primarily a presentation of the data with only minimal and general attempts to interpret some of the results, but I think it will be of interest to the community and certainly appropriate for Icarus.

One downside is the range of data quality between objects which necessitate a variety of techniques: bandwidth estimation, pole solutions, shape modeling, etc., and thus this paper has to cover a lot of ground. This is probably unavoidable however, as separate papers for each object would be unwieldy. I don't see any significant way to shorter the paper, nor would suggest significant additions at this point.

The paper is organized appropriately and I see no show stoppers, major errors, or omissions. Therefore I would recommend publication. I have a few suggestions which I believe would clarify certain areas and clean up a few spots. Although my comments got a little lengthy I think in total should only require some minor editing.

Major points:
In the Background section, the paragraph after Eq 5 quickly mentions that 25% uncertainties are used based on systematic uncertainties in pointing and calibration. What is the basis for this? Why 25%? Could this be referenced? By 'pointing' do you mean errors in the telescope pointing itself or ephemeris errors? I'd be surprised if either of those are more than a few arcsecs which should be negligible in the Arecibo 2 arcmin beam. Later, repeated observations of the same objects at nearly identical longitudes/phases seem to suggest that you get very consistent results on each object, which also seems to be the point of the appendix and those repeat observations. Even if there is some systematic calibration error somewhere, it would seem to me that analysis of single objects alone, say as in Figures 4, 14, 20, and 22, that using whatever the statistical uncertainties are would be justifiable. But I'd agree that the final results as in Table 12 or Figure 23 should include all sources of uncertainty as they do now.
It has been our practice to assume that our absolute errors could be 25% (or more) given the difficulties of calibrating the radar, especially with low SNR targets. You are correct - there is unlikely to be relative errors of this magnitude within any given data set, and we have used 10% in our revised plots for targets observed during any encounter.

The citing and responsibility of the IR/VIS complementary observations is unclear. The introduction mentions that they are described in a companion paper by Ockert-Bell, Clark, Shepard, Rivkin, and Binzel (submitted). But later when optical/IR data are used they include citations in the text with initials or names not on that paper. For example, in the Zwetana section it mentions the lightcurve observed by MF, TB, AK, RB, HC, JC, and SC, and IRTF observations by F. DeMeo and R. Binzel. Are ALL these other datasets referring to the same paper? If so then some consistency is needed; if not then more explanation is needed. Secondarily, even if the details are in a companion paper(s), it would be useful to include some basic details of them: wavelength, some appropriate uncertainties, etc.

The Ockert-Bell et al. paper looks at these and other asteroids in the IR and attempts to find some correspondence between IR spectra and radar properties. The DeMeo and Binzel reference is to work which is now incorporated in the Ockert-Bell paper. The other citations refer to lightcurves taken by various amateur/professional astronomers in support of this work. The initials refer to the various co-authors who made these observations. We've added two sentences at the end of the introduction to make this clear.

I didn't look up Icarus policy, but there are several web links cited in the text and it is always a concern that these are non-permanent locations. I'd suggest carefully considering if they are all necessary especially if a published reference is available as well.
The web links are not critical to the analysis but point to additional information if desired.

The table values need to be rechecked versus the text - different values appear in several tables than what is referenced in the text, in particular for the summed results. For example, for Hertha the text gives mu_c=0.10+/-0.03 versus 0.09+/-0.02 in the table, and bandwidth B2sigma=225+/-50 Hz versus 225+60/-15 in the table. There are several cases of this sort of discrepancy.
Thanks - fixed and checked.

Also, it is not clear why some objects don't have tables of results. Why not just put all the results in one large table? It might even save some space and all objects could be included. Regardless, it would be better if all the tables had more consistent use of error bars - some have them while some don't.
This excellent comment was made by another reviewer. We have made the requested changes.

Minor points (in order of appearance):
The abstract is somewhat brief but, granted, could easily get bogged down in the details of so many different objects. My two suggestions are to mention the optical lightcurves as well since that is new data that is presented, and include some conclusion about X- types; both X and M were observed according to the first sentence, so for completeness something should be said about what happened to both types even if it was just to reclassify the X one. The end of the first sentence says 'thirteen' but should be fourteen.
The ‘thirteen’ comment is fixed. The abstract mentions that we find the periods of 3 to be in error, but we don’t mention the lightcurves here because we do not present them - they either have or will be published in the Minor Planet Bulletin. We included a note in the abstract that the X-types were reclassified.

Diameter-magnitude relation (Eq. 1): Isn't this the definition of H as opposed to an empirical relationship? Either way, it has been in use for decades so surely there is a more original or standard reference than the 2007 paper cited.
This relationship has been used for decades and the traditional reference to it has been Fowler and Chillemi (1992) in an IRAS Technical Report. The relationship was not derived there and this reference was considered by some (including several coauthors) to be ‘gray’ literature. Alan Harris and Petr Pravec briefly touch on its history and show its derivation in an appendix of the 2007 paper we reference, so we use this instead.

Paragraph 2 of 'Radar Analysis' could make clear that the spectrum is due to Doppler shifts from the rotation and not the asteroid motion.
We added a parenthetical note to clarify this.

Folding the spectrum: This seems reasonable for low SNR, but it is not

obvious to me that folding the spectrum gives a useful result if the asteroid is very irregular. Can't the frequency extent be very different at either end, eg. 178deg spectrum of Psyche in Fig 1?
The short answer is yes. One can envision an asteroid with a center of mass/rotation that would wobble and inflate the bandwidth when summed. For Oceana and Heidelberga, our quoted uncertainties in bandwidth are small to moderate and are consistent with the zero-crossing bandwidths of their unfolded spectra.

Figs 17 and 18 are clearly folded but it would seem appropriate to mention this in the caption.
Added a note.

16 Psyche section, 3rd paragraph last sentence: Since the uncertainties in the IRAS diameter are only 4 km vs. the 43 km in this paragraph, the last sentence would better read "This is larger than but overlaps the IRAS diameter" or similar.
Fixed.

I think the Psyche section confuses Figs. 4 and 5.
Yes – thank you!

The crosses on Fig. 5 appear to be right on or even slightly below the 250 km contour, but the text quotes them as 260 km.
Another good catch. The figure shown actually assumed the worst case (most conservative) uncertainty in the bandwidth, and actually led to an overestimate of Psyche's major axis. We've corrected the figure and the adopted model.

Psyche section paragraph 10: By using some averaged axis ratios an ellipsoid model is obtained, which then gives an effective diameter. However isn't some effective diameter assumed first in order to convert the axis ratios into the ellipsoid model? That seems a little circular unless there is some subtly here or perhaps I've missed an obvious point?
The axis ratios we use are based on two sources: a lightcurve-based shape model by Kaasalainen et al and adaptive optics images by Drummond. Neither of these makes an assumption about the diameter to derive the axis ratios. We use the radar to find a maximum diameter, and then use their axis ratios to determine the other two axes and an effective diameter. In the case of the 'a/b' axis ratio, our radar observations are consistent with theirs and serve as a good check. We have little or no leverage on the b/c axis ratio, but these two independent studies are consistent and we use their average value.

Psyche section last paragraph: Can anything more definite be said of how the K+2003 albedo spot may be related to the 'radar anomaly'? For example, is a rough location of the optical spot known enough to compare? Or not?

Without a shape model, we cannot predict where the spot should be – there's too much time between their encounter and ours. We will eventually do this modeling using lightcurves and our radar, but not in this paper.

Antigone section: In describing the process of generating a shape model and Fig. 10, I find it a little unclear how the radar albedo appears in this process. Does 'homogeneous radar scattering law' refer to the magnitude as well as the angular behavior? It was clear until the last sentence in the 4th paragraph of this section confused me: "the radar albedo at longitudes of 0-90deg is less than half this and our shape model suggests that this end is not as radar bright as the rest of the asteroid." Is this just referring to the effect of higher incidence angles and/or lower projected area as seen from this end, or has some variable albedo over the surface been fit? I think the process is ok but something about the wording seems inconsistent.
When generating a shape model, we often use a homogeneous angular scattering law, as here, and let the absolute radar albedo of the surface float. When we have the shape model, we are able to compare the apparent cross-section (projected area) with the actual radar cross-section. That's what occurred here. In most cases, the radar-cross section and lightcurve closely track with the projected area, indicating a consistent radar and visual albedo. Here, we see that the visual albedo is ~constant, but the radar albedo shows significant differences. We'll try and word it better.

Antigone section, 3rd paragraph: Two consecutive sentences begin with "We adopted the Torppa et al. pole..." is awkward to read.
Fixed.

Hertha section, 5th paragraph: "much, if not all of the difference in radar albedo is likely due to differences in near-surface roughness..." If roughness is distributing the power between polarizations, wouldn't the total albedo, OC*(1+MU), then be roughly constant with most variation in the polarization ratio only? But here OC and TC both change. (Perhaps my scattering notion is too simplistic.)
The angular scattering laws of so-called specular and diffuse scattering are different, and each mechanism is responsible for some of the OC and SC polarizations in different ways. Certainly there must be some kind of energy balance between the two, but we only have information in the backscattering direction and an uncertain knowledge of exactly how each behaves with surface roughness. At this point, we can only note the apparent correspondence between radar albedo and polarization ratio and suggest a link.

Oceana section, last sentence: Do the large error bars on grain density really make that a useful estimate?
Noted. We've removed that sentence.

Mancunia section, 3rd paragraph: Specifying the error bar on sigma_oc and giving the range seems redundant.

Fixed.

Future section: Never sure how useful these sections are, however here only Klotho appears to have a better future opportunity. The rest are the same or worse than the data in this paper. I'd suggest adding some (brief) discussion of motivation to this list, i.e. why more of the same data might be useful on the same targets. For example, is a sqrt(2) improvement in SNR all that useful for these targets, or should the point be better rotation coverage, different positions, or just focus on new targets?
We included that information because people will ask if we can target any of these asteroids again – in most cases, not very soon. The future targets are listed so that, in case somebody is working on one or more or has other information, they may contact us.

Table 2 needs a superscript to link the footnote to the Eros calibration line.
Fixed.

Fig 10: Could this caption mention the shading? Is it representative of the radar or optical scattering law?
The shading is Lambertian to emphasize the shape. We've included this in the caption.
-End-

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SHORT TITLE: A radar survey of X- and M-class asteroids
Reviewer: R. P. Binzel
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:
Excessive

COMMENTS FOR THE AUTHORS
This is a fine manuscript, addressing a nagging problem with a very powerful technique. It may be published as is, but I offer some suggestions for improvement.

Major comments:

There seems to be a lack of treatment of E-types and Xe-types as possibly being related to the enstatite achondrite meteorites (aubrites). An example of this class is Norton County. The seminal paper in this area is Gaffey et al. (1992), Icarus 100, 95.

The links between Xe-types and the inner main belt is also demonstrated by Binzel et al. (2004).

Table 2: Why are the enstatite achondrites (aubrites) not included here?
E-class asteroids are covered in a separate paper. This is now noted in the first paragraph.

Many of the Tables could be combined together. Simply indicate on a separate line which object is being addressed. For example: Tables 4, 6, 8, 10, 11.
Very good comment – done.

Similarly, combine Tables 9 and 11. They are similar in style.
We'll leave these separate – the Hertha table has no radar properties.

Table 12: Include here a summary of your interpretations. This is a way to encapsulate the findings of your paper in a very nice way.
We added a column to indicate whether the asteroid is metallic or not. I think this is what this comment refers to.

Figure 23: Explicitly label each of these regions within the figure.
Done.

Minor comments:

97 Klotho. Use the word "oppositions" rather than "encounters".
Done.

758 Mancunia. "Warner (2007)" is not in reference list. Check reference list for completeness.
Done.

Appendix: This material would seem well placed in the text.
We felt that it disrupted the flow of the paper but was important to include. We settled on an appendix.