MINUTES
THERMAL EMISSION MEETING #5
Fourth Meeting at ISSI, Bern, November 19-21, 2013

Björn J. R. Davidsson
Dep. of Physics and Astronomy, Uppsala University, Sweden

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Contents

1 Participants and Background

In alphabetic order and with initials to be used throughout the Minutes,

1. JB: Joshua Bandfield (University of Washington, Seattle, USA)
2. MTC: Maria Teresa Capria (IAPS/IASF, Rome, Italy)
3. BD: Björn Davidsson (Uppsala University, Sweden)
4. JE: Joshua Emery (University of Tennessee, Knoxville, USA)
5. OG: Olivier Groussin (Laboratoire d'Astrophysique de Marseille, France). *Participated remotely via Skype.*
6. PG: Pedro Gutiérrez (Instituto de Astrofísica de Andalucía, Granada, Spain). *Participated remotely via Skype.*
7. AM: Alessandro Maturilli (DLR Berlin, Germany)
8. TM: Thomas Mueller (Max–Planck–Institut für extraterrestrische Physik, Garching, Germany). *Participated remotely via Skype.*
9. HR: Hans Rickman (Uppsala University, Sweden / PAN Space Research Center, Warsaw, Poland)
10. MW: Magdalena Wilska (PAN Space Research Center, Warsaw, Poland)

The project *Deriving Physical Parameters of Atmosphereless Bodies in the Solar System by Modelling their Thermal Emission* was approved by ISSI in 2011. The previous Team meetings may be summarized as follows.

• November 9–11, 2010 at DLR in Berlin, Germany: Preparation for writing the finally approved proposal.

• October 26–28, 2011 at ISSI in Bern, Switzerland: Defining a number of modelling projects in detail.

• May 2–4, 2012 at ISSI in Bern, Switzerland: Follow–up on modeling projects, definition of laboratory projects, first discussions on publications.

• March 12–15, 2013 at ISSI in Bern, Switzerland: Primarily discussing Paper I.

The current meeting was held at ISSI in Bern, Switzerland on November 19–21, 2013, organized and chaired by Björn Davidsson and Hans Rickman. The purpose of the meeting was to discuss the status of Paper II and summarize current activities of the participants (Day I); discuss in detail the content and structure of Paper I (Day II and III).

2 Day I

HR, BD: Welcome words, summary of the Agenda. The focus of the current meeting is to discuss in detail the draft of Paper I (disk–resolved models) circulated by BD on November 14. Also, the current status of Paper II (disk–integrated modeling and observations), and prospects for a Paper III (laboratory measurements) will be discussed.

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OG: Is it possible to ask for an extension of our project?

HR: Dr. Falanga is in China, but he will be contacted in order to sort out our options.

BD: First email from Dr. Falanga reads as follows; *I confirm with you that you can have your last small editorial meeting before our financial year June 30th, 2014. Please check our calendar as soon as possible in order to confirm your last meeting dates. Of course, you are welcome to resubmit a new team proposal next year (deadline end of March 2014 or 2015).* We will have to get back to him to clarify exactly how much funds we have left.

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AM: Jörn Helbert sends his greetings.

2.1 Talk: Mueller

The presentation entitled *Interpretation of thermal emission: II. The effects of surface roughness and thermal inertia for disk–integrated measurements of atmosphereless bodies* is available on the team website, and will not be described in detail in these Minutes. But in brief, Paper II will focus on two very different bodies for which a wealth of observations are available – highly irregular Near Earth Asteroid Itokawa with relatively high thermal inertia, and quasi–spherical Main Belt Asteroid Lutetia with very low thermal inertia. The purpose is to highlight the difference between models with and without microscopic roughness, and to make an in–depth discussion about the degeneracy of thermal inertia and roughness effects, as well as practical advice on how to disentangle the two. Furthermore, the high potential of thermal lightcurves for determining body properties will be emphasized.

TM and JE: Discussions on the possibility to use Spitzer IRS spectra as part of the analysis.

TM and MTC: Investigate possibility of using Rosetta/VIRTIS disk–integrated observations of Lutetia.

BD: Raised the question whether the Lutetia shape model is accurate enough on the far side (during Rosetta flyby) for our purposes; TM thinks so.

OG: Although thermal light curves have a very high potential for breaking the degeneracy of thermal inertia and roughness, it is important that we do not paint ourselves into a corner by giving the impression that "nothing can be done unless we have thermal lightcurves". There is still a lot that can be said from disk–integrated observations, even if thermal lightcurves are not available.

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TM: For Lutetia and Itokawa we have access to high–precision shape models thanks to the flybys, and we will perform the analysis using that. However, it is also valuable to perform the same analysis with the less accurate shape models obtained from lightcurve inversion. This can be used to evaluate the accuracy of measured parameters for targets for which only lightcurve inversion shapes are available.

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BD: What is the timescale for producing a first draft of Paper II?

TM: Incorporation of the Spitzer IRS data into the analysis will be complete by the end of the year, why a first draft can be expected early 2014.

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JE: What are the prospects for detecting thermal inertia variations on the targets?

TM: Some thermal lightcurve phases appear better fitted with high thermal inertia, while others are better fitted with low thermal inertia, although these tendencies are perhaps not systematic.

BD: A possibility is to generate disk–integrated spectra at different levels of thermal inertia, and consider linear combinations of these with different weights, in order to see if observed data is reproduced with smaller discrepancies, compared with single–value simulations.

2.2 Talk: Bandfield

The presentation *Strange and bizarre properties of lunar impacts revealed by LRO Diviner* is available on the team website, and will not be described in detail in these Minutes. But in brief, the talk dealt with two topics; *A Highly Unusual Series of Young Impact Melts and Rocky Exposures Antipodal to Tycho Crater*; and *Lunar Cold Spots*.

AM: Is it certain that the concentration of rocks near the Tycho Crater antipode have a lunar origin?

JB: The impact velocities of these blocks are so low that they must have had a local origin, e.g. as ejecta blocks.

JE: Are there other ejecta block concentrations on the Moon that are not associated with young craters?

JB: As far as we can tell, all ejecta blocks are associated with young craters, except this particular field, antipodal to Tycho Crater.

JB: The lunar cold spots have the same albedo, colors and mineralogy as surrounding material – they only stand out by having a 5–10 K lower temperature, indicating that the thermal inertia is exceptionally low, most likely due to an unusually high porosity caused by granular flow following impact events.

MTC: On Vesta, we also see temperature anomalies, but these cold spots are associated with unusual albedo and color as well.

2.3 Talk: Capria

The presentation *Dawn at Vesta: Derivation of thermophysical properties from temperature data* is available on the team website, and will not be described in detail in these Minutes. In brief, a thermophysical model with heat conductivity, heat capacity and density corresponding to three distinct classes of lunar dust are considered, along with a free parameter that regulates the thermal radiative loss of the surface, in order to reproduce Dawn/VIR spectroscopy of Vesta.

JB: Why does the model have such a limited range of thermal inertia, while there is virtually no limits on the strength of roughness effects?

MTC: This is primarily because it is known from previous disk–integrated studies that the average thermal inertia is 30 MKS.

TM, JB: Is it possible that the apparent correlation between thermophysial properties and latitude somehow is an observational bias due to the orbits flown?

3 Day II

3.1 Talk: Maturilli

The presentation *Emissivity measurements in vacuum with emerging angles 0, 5 and 10* is available on the team website, and will not be described in detail in these Minutes.

BD: The measurements indicate that a 50:50 mixture of quartz and magnetite looks like a modeled 25:75 mixture. But what model was used – just a linear combination, or some effective medium theory?

AM: Linear combination.

JB: When the wavelength is similar to the typical grain radius, photons do not interact with one grain at a time, but with groups of grains, which means that some effective medium theory should be applied.

JB: We do not expect large changes in emissivity for emergence angles below 15°, but substantial differences at large emergence angles. Would it be possible to study angles as large as 75°?

BD: I have some difficulties understanding what "weak" and "strong" means to different people in this context. How large are the changes seen on the Moon and Mars?

JB: For high–albedo surfaces that are globally smooth (RMS mean slope $s = 1$–$2°$) we see a decrease of band depths as the emergence angle increases from zero to $e = 60°$. On Mars, it is on the order of a few percent, but on the Moon it can be up to 10%.

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AM: What kind of measurements would you be interested in?

BD, JB: Fine particles, in vacuum and at ambient pressure. Emissivity and reflectance as function of emergence angle, from zero to as high values as possible (up to at least $e = 75°$). A good wavelength coverage, particularly considering the 3–5 μm range considered by several current spacecraft spectrometers, but also continuously through the thermal and mid infrared, up to $\sim 100\,\mu$m. It is better to have a good angular and wavelength coverage for few materials, than limited coverage for many minerals. We suggest to focus on Millbillillie, and maybe on quartz as a reference material.

BD: I will provide some estimates of reflectance and emissivity versus emergence angle, as expected from Hapke theory. Our goal is not to prove or disprove Hapke theory, particularly not in some general sense, but merely to provide some sort of theoretical expectation or estimate, to be compared with the actual measurements, e.g., do they behave qualitatively similar and are the values more or less in the same ball–park? Depending on the results of this study, BD will recommend some specific measurements, to be iterated within the Team, before measurements at PEL.

BD: What is a realistic time frame for performing at least a subset of measurements?

AM: Perhaps results can be presented at the LPSC (held March 17–21, 2014; however the deadline of abstract submission was November 21, 2013; out last meeting day.)

BD: We look forward to measurements, and a manuscript led by the PEL group, with acknowledgments to ISSI, within the first half of 2014.

3.2 Paper I draft: Davidsson

Draft #1 of Paper I was sent to all Team members by BD on November 14, 2013. The draft is available on the team website, along with a previous distribution which included a large number of plots and comments. The primary goal of Paper I is to compare the SEDs of different topographies (flat, concave spherical segments, parallel sinusoidal trenches, random Gaussians, and fractal terrains), for a variety of incidence, emergence, and azimuth angles, in order to characterize similarities and differences. In order to interpret and understand the results, the terrain slope angle distributions, the surface temperature distributions, and simulations with self heating artificially switched off were considered. The secondary goal was to compare a physically realistic model of thermal emission from rough terrains, with a simplified statistical approach, in order to test the accuracy and range of applicability of the latter.

The first part of discussions primarily focused on the *Roughness Types* project. Paragraph by paragraph, BD went through the Introduction; the section on surface roughness parameters, rough surface generation, and slope angle distributions; the section of physics and outputs; the section on surface temperature distributions and SEDs for different illumination and viewing geometries. This was done in order to motivate and explain the reasons for the current outlay of the draft, but also to discuss the results displayed in figures.

MTC: The current manuscript is very long, about 60 pages. We should perhaps find a way to split it in two.

OG: It is true that it is long, but we should not exercise self–censorship, but write it in the way we consider best, and then respond to the reaction of referees and editors, whatever it may be.

BD: 60 pages in the current format will be reduced to 17 pages in the journal, according to previous examples. It is on the large side, but not excessive.

HR: I suggest that the Introduction is broken up into subsections, as there are clear points where new topics are initiated.

Final decision (all): We do not remove material at the moment, but create a new section called "Background" in which the bulk of the current Introduction is placed. Instead, the Introduction will be much shorter, to the point, and highlight the goals of the investigation.

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HR: We should explain what we mean with "atmosphereless bodies" and why we only focus on these.

JE: We should not only focus on previous or ongoing missions, but also mention upcoming missions like Osiris–Rex, Marco Polo et cetera. (JE agreed to provide a paragraph on this).

HR: In many places, it is written "convex" when we mean "concave" (BD had already corrected this in the embryo to Draft #2).

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OG: It is a major issue whether or not the thermal emission properties can be captured by a single parameter like the RMS mean slope s. Our results indicate that the problem is far more complex, and also depends on other properties of the topographies. This is a major result and should be highlighted. The Introduction should clearly state that we are testing the hypothesis that single parameters like s or $\bar{\theta}$ are sufficient to characterize rough terrains.

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BD: I have called JB's model "quasi–analytical" in the manuscript, but need a better name.

JB: Why not call it "a simplified statistical model".

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JE: When presenting the slope distributions, it could be a good idea to compare them to those of actual surfaces.

OG: There is a paper by Thomas et al. (2007), Icarus 187, where different slope distributions are shown.

BD: Everybody have an action item to send me references to papers that provide slope distributions for actual planetary surfaces.

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HR: It would be very valuable to know if the fractal surface is expected to have a Gaussian slope distribution from first principles. Is there any mathematical proof that shows this?

OG: I do not have the answer right now, but I will look into it.

HR: We do not mention sublimation, although we formally include comets in our list of targets.

BD: I will add a paragraph on this.

MTC: I would be good to have a table where all parameters used in the manuscript are collected and defined.

BD: Will do.

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OG: Section 3.3 should contain a statement that we compared codes for a baseline case and verified that they matched.

JB: Why not describe the simplified statistical code in Section 3 along with the other codes, instead of having a separate Section 5.1?

BD: Will do.

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JE: For the random Gaussians, a number of realizations are used that lead to a range of SED strengths at each $\{\lambda,\, i,\, e,\, \psi\}$ combination. Why not doing that for all models?

BD: There is a fundamental difference between the concave spherical segments and sinusoidal trenches on the one hand, and the random Gaussians and the fractal surfaces on the other hand. For the former, it is possible to fix one parameter (crater coverage or number of trenches) and then there is no degrees of freedom left – if we target a specific RMS mean slope s, there is only one spherical segment diameter–to–depth ratio, and one trench amplitude that can be applied. However, since the Gaussians and fractals are based on a randomized process of slope generation, there is an infinite number of realizations for each s–value.

JE: But even for the concave spherical segment, we can imagine a range of crater coverage f and diameter–to–depth ratios S, that all have the same RMS mean slopes s. We have a lot of indications that the disk–integrated SEDs are the same for different $\{f,\, S\}$ combinations when s is the same, but this paper deals with single $\{i,\, e,\, \psi\}$ cases of resolved bodies. We should investigate if solutions really only depend on s, and not on $\{f,\, S\}$ individually.

BD: I agree that we should at least make some tests, and decide what to do based on that.

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OG: If we can provide practical advice on how to disentangle roughness and thermal inertia, it will be extremely important. There should be a separate subsection on this.

BD: There are indeed several angular and/or wavelength regimes which could allow us to determine, in turn, the thermal inertia, the level of roughness, and perhaps even the degree of randomness versus order. We do have a subsection earmarked for this, although it is not written yet. I agree that this is one of the most important aspects of the paper.

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MTC: We should have a figure that shows the definition of $\{i,\, e,\, \psi\}$ graphically.

BD: Will do.

3.3 Paper I draft: Bandfield

JB discussed his simplified statistical model in a talk entitled *A quick and dirty thermal roughness model for the lunar surface*.

All: Discussing whether the paper could be divided in two along the *Roughness Type* and *Level of complexity* separation. Two back–to–back papers in the same journal was suggested. But since the original reason for merging the projects was the fear of none of the investigations might be strong enough to stand on its own legs, and since there is substantial overlap in any case, it was decided to keep the two topics in the same paper.

4 Day III

Discussions on Paper I continued, now focusing on the Discussion section.

All: There is consensus that practical advice on how to disentangle thermal inertia, level of roughness, and potentially, type of roughness, is a key element in the Discussion.

Early in the paper, we need to explain and motivate our choice to only consider a very limited number of illumination and observing geometries.

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JB: If radar indicates that the level of roughness varies systematically with taxonomic type, while visual phase function reversal does not show such a correlation, one should remember that radar is sensitive to a cm–dm thick slab, while light scattering only takes place in a very thin surface region.

BD: On the Moon, is there any difference in thermal emission properties between the Mare and the highlands?

JB: In spite the difference in albedo, composition, and degree of cratering, there is no systematic difference in their thermal emission properties.

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MTC: Perhaps we could give some advice regarding surface roughness models – are some better suited for certain bodies than others?

JE: Other topographies that could have been considered include parabolic craters, that have no limitations on diameter–to–depth ratio.

JE: One way to remove the large zero–angle spike in the concave spherical segment slope distribution is to replace flat regions with extremely shallow craters. There is potential for more elaborate slope distributions also with this standard model.

HR: Can we claim that we have covered all ground with the topographies studied here? Have we really covered the full range?

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JE: It would be very good to compare our numerical slope distributions with actual data, e.g. for Itokawa. It is particularly important to explore if the level or type of roughness changes with resolution.

JB: Showed a 250 m–resolution image of the lunar surface, that has an RMS mean slope of only 5–10°. However, the thermal analysis indicates a slope distribution peak of 25°, which illustrates the variation of s with resolution.

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BD: Was OG surprised when it turned out that the random Gaussian and fractal surfaces are so similar to each other?

OG: Considering the fact that both models consider facets tilts to be random and independent of each other, it is not extremely surprising that their emission properties are similar.

JB: We should emphasize that while large–scale roughness is accessible through direct imaging, and composition is accessible through spectroscopy, there are very few methods of accessing small–scale roughness. Studies of thermal emission is one of the most important ones.

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JE: It is important to emphasize that our advice on observing geometry are important in the context of spacecraft orbit, pointing, and measurements planning.

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BD, JB: In order to minimize the impact of difference in albedo when comparing observations with simulations, we should compare ratios. That is to say, the SED of an observed target is divided with a model–SED of a flat surface, evaluated for the correct heliocentric distance, albedo et cetera. This ratio is compared to a similar ratio of a numerical rough–surface–SED with respect to a smooth model.

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MTC: I will search the Dawn data for intensity versus incidence angle at nadir, but also intensity versus azimuth angle for $i \approx e \approx 45°$. In both cases, intensities will be compared to a 1D smooth model evaluated for the relevant heliocentric distance and albedo.

We then moved on to discuss other issues.

BD: We will thank ISSI for their support in the acknowledgments. Do they have a standard formulation?

HR: I will ask ISSI about this.

BD: I have received the ISSI report template from Dr. Falanga. It is mostly formalities, except for a statement of highlights, as well as a comparison between initial goals and final accomplishments.

HR: In addition to our publications, we should stress that the ISSI team has had other positive effects. We have established contacts with colleagues we otherwise would not have worked with, there has been a lot of very interesting discussions, as well as exchange of knowledge between modelers, observers and experimentalists et cetera. We have accomplished many of our goals, and it is not a shame to admit that a few of them could not be met, at least not within the two–year time frame.

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BD: I will set up a doodle poll for a final editorial meeting in the April–May time frame.

Finally, we agreed on the following deadlines for completing Paper I.

BD: Implement changes discussed during meeting, and write a draft of the discussion by the end of 2013.

OG, PG, JB, MTC: Provide texts and missing data by January 20, 2014.

BD: Perform the Hapke calculations and continue the dialogue with PEL before the end of the year.

This concludes these Minutes.