Transcript of Session on Thermodynamics of Impact Cratering and Determining Impactor Characteristics, Saturday, February 8, 8:30 a.m.

We have collected transcripts of selected talks and all available discussion sessions. Not all discussion sessions were successfully taped, and in some cases we were unable to identify the speaker. Transcripts were edited to improve clarity and grammar. Any omissions or errors are unintentional, and the conveners apologize in advance for them. Speakers are shown in bold type. Questions for speakers are shown in italics, and responses are not italicized. The transcript for this session was generated by Elisabetta Pierazzo.

O'KEEFE, J.D., Ahrens, T.J.: Impact induced target thermo-mechanical states and particle motion histories

O'Keefe presents results of a systematic modeling work where many parameters were varied to investigate the effect of impact velocity, and material properties, as well as the strength/damage model on the model results. Data should be available on the web.

QUESTIONS/COMMENTS TO O'KEEFE'S INVITED

Asphaug: Dugan, I was wondering: How do you advect damage forward in the Eulerian calculations? Does damage follow the material as it goes through?

You code it exactly the same way you code density, temperature, and so forth (you do not have to have tracer particles). It is very straightforward; in fact the code is built so that if you want to change your models you can readily put it in.

Holsapple: It is just another state variable.

It is another state variable. In fact from a physics stand point, damage is considered to be an internal state variable.

Stewart: Dugan, this is a discussion period kind of question, but can you speak to the appropriateness of using CTH to look at late time shape and crater collapse. Because in the past we have used two different codes: we have used hydrocodes to get transient craters and then finite elements to do collapse. And there has now been this movement to using these codes with the addition of more complicated constitutive relations to look at crater collapse and late time features. We have been doing it with SALES2, doing it to look at Sailor Hat crater using CTH, and then Dugan with this KT impact. Is this something that the users community agrees on? Is it a good approach? Are we now at the level that we believe these late time features or must they be taken with skepticism? We need to talk about this. I am going to be skeptical…

Can you calculate things out to late times with a code that is limited by sound speed? It depends on how you treat the boundaries so that you do not have reflection, and to go out to late times costs you from a computational standpoint. That is one of the key issues. That is the issue with: Can I model the thing without destroying it just because I did not have a big enough mesh or the zoning was correct? That is a cost problem. Then you have to say: How good are the constitutive models? The real test there is: How much of the phenomenology are you modeling and how well does it agree? Well, first of all we are seeing faulting. We do not put in any kind of weakness at a given place; it occurs naturally in the evolution of the calculation, so that is a positive thing. And also we see the general morphology in terms of melt layers; another test would be to go back and look at the ejecta distributions. I did not go through that in detail, but one of the things that we have there is distribution of the fractures and the amount of melt as a function of distance and velocity of ejection, and we relate that to the field measurements. That is really an important thing to do. There are all kinds of tests. There is no reason why the hydrocodes (which is a misnomer, I agree with Jay on that) can't really address things to very late times. It is just a matter of money.

Stewart: Are geologic observations a sufficient test? Should we try and devise type examples for the cases of benchmarking the codes?

Ahrens: One area that I think these codes provide a good demonstration that they have a reality check is the comparison with the centrifuge calculations. The codes very closely reproduce the results of the

centrifuge calculations, and we know from our understanding of scaling that increasing the g is a way of increasing the time. So that is a real benchmark test, where you get this reconstruction of the geologic section as observed in centrifuges which we know relate to long times, and here in these calculations where we run to really long times we get exactly the same result. I think that the other point that O'Keefe has made is that there is no reason that these codes should produce a fault if it didn't exist. And we do observe these circular faults and it is a feature that many people did not ever believe you could observe in a finite difference calculation.

Let me make another point, and that is: I agree very much with the earlier statement by Kevin [Housen] and I think by Keith [Holsapple], and that is that you not only go to the field but you really need to do a whole suite and a whole series of different kinds of laboratory measurements. Not only simulating the impact, but also measuring the material parameters, and how well does the code simulate that: Does it give you bulking? Does it give you all the triaxial measurements? Do you get good correlation? Some of that is not easy.

Spray: I am not biased or anything, but I am really pleased to see you generating these large displacement fault systems in your models, it is great. For the large craters I think it is very difficult to deal with them in terms of field mapping. So, a situation where these models can actually help us target certain regions for close scrutiny I this is really helpful, potentially. I was intrigued also by the deep level faults that your model seems to be revealing: Can you comment on those in terms of how you….

That is what I am getting. Of course there are the seismologists in here, the people who make the measurements. It is very difficult to go below the Moho with any kind of reflection seismology imaging techniques. Someone else has to comment about that, but are there better ways to try to get at, did those occur?

Holsapple: I think these kinds of things are really great, in particular to me it is very interesting that we finally do have this faulting idea. In fact, it has been known for a long time in the civil engineering concrete literature, that when you have strain softening you have damage, and in fact then the damage localizes. It comes out as a fault, with all the kind of things we see in the field. That to me is very interesting, and it is nice that we do not need to introduce additional physics in order to get this late stage readjustment. A comment though, is that it would be awfully nice if we went and did a lot of physical experiments, took only that data, put it in a code and got the right answer. We are always, unfortunately all of us, in the mode of saying there is the answer, what are the knobs, what are the inputs that give us that answer, and we have so many inputs that there is no unique answer. It is the inverse problem: What do you have to put into the code to get the answer out? I think we have to be fairly skeptical of the actual numbers, but to me the idea that you see the mechanisms is very good. One final comment is, correct me if I am wrong, you talked about porosity scaling, I think you mean density scaling. I think you are using only a low density target and you do not really have any porous crush-up. Is that correct or am I wrong? Do you have a P-alpha model or some crush model?

Yes, I did have a P-alpha model. I did not get any major difference between that and just using a snowplow.

Holsapple: Ok, but it is an actual crush-up, so you get all of the thermodynamics of the extra heating.

That is right. Yes you do.

Holsapple: Ok. I thought what you were doing was simply a low density target.

Actually, I did both, and I did not show all the calculations in which we had a gold target, solid gold impacting solid granite and those. Those follow exactly the same scaling, up to the density ratios of less than 3, and then that is when you had the changes in porous targets, when you had density ratios greater than 3. In both cases you saw instabilities. What you saw in porosity cases was the reduction in the ejecta, and changes in the cratering efficiency, and I think that is consistent with experiments.

Holsapple: So you were basically using a snowplow model for most of it.

Right.

GIBSON, R.L., Reimold, W.U.: Thermal and dynamic consequences of impact – Lessons from large impact structures.

Gibson gives an overview of shock and especially

post-shock effects of impact, with special emphasis on post-impact thermal effects. He uses Vredefort as his type example, which is in agreement with work done at other large impact structures like Sudbury (current dataset of thermal effects on terrestrial structures is still small, but growing). Also, he points out how modeling has helped them to look for certain features, and constrain spatial dependence of shock effects. A problems we have to deal with, is related to the difficulties of calibrating experimental studies of shock metamorphic effects with planetary events.

QUESTIONS/COMMENTS TO GIBSON'S INVITED

Spray: I think Vredefort is really important because it is one of the best exposed deep-level central uplifts in a large crater, so this type of work is really critical. What is your feeling about the degree of uplift, and how the central uplift evolves structurally, in terms of rebound?

Since you ask, I have a slide. This is something that we can discuss if we have some more time, but basically Betty [Pierazzo] and Zibi [Turtle] work on modeling of the Vredefort has always created a bit of consternation amongst us observationalists. Maybe we ought to talk about that that sometime. Anyway, work that has been by Christiane Lana [?] on the central uplift has shown that, contrary to popular belief, what was presented about 20 years ago suggested that within the center of Vredefort we actually see the Moho exposed, in other words 30 to 35 km of exhumation has occurred, the upturning of the supracrustal rocks which occurs on the outside, and extend only within the Archean rock. You mentioned in your abstract this piston-cylinder type structure for central uplifts. What we see in this central zone is basically a uniform orientation of pre-existing fabrics, which suggests the thing has come up vertically, and there is a remarkably strong zone of rotation out to this level, and in fact the largest pseudotachylites, the example that you showed, lie at this transition. I wonder whether, because of the strain within that, we are not seeing the melt moving into that particular zone, or being generated, because that is the zone of maximum incompatibility. It is pretty exciting stuff that he has got there. As you said, it is a deep level central uplift, so this is where we need to find the answers.

Christeson: Could you make any comment about the relationship between the central uplift and peak ring formation?

Do I have another talk? The point that I made about the fact that when you go outside of the Vredefort dome most of the structures you see are actually compressional features is one that has puzzled me for years. I think that what we are seeing in the Vredefort is the root zone of a peak ring where we are actually seeing the uplift and outward collapse of the structure. We have got a PhD currently working on the structure in the central uplift itself, an almost everything there is extensional, but it is related to outward collapse as well as radial extension, or tangential extension of the central uplift. I think, because we are below that level, we have actually lost the zone of overturning where the central uplift has collapsed outward to create the peak ring. Does that help answering your question? But it is something that really needs to be looked at.

Dence: Just on that point: Do you feel that the overturning, particularly on the North-western side, is an original feature or is it possibly due, as someone has suggested, to the whole structure being somewhat tilted?

I have got another slide on that but I won't show it… Yes, again, what Christiane's work has done is to show that the only way to reconcile the structures we see is actually to have a regional North-West tilt of the major stratigraphy prior to impact. If you ever the seismic or magnetic section through Vredefort, you do not get a perfectly circular structure. It opens out towards the South-East, as a pear shape. If you actually model a vertical impact into an inclined sequence of rock, you would actually get an elliptical type of pattern on the major unconformity between the supracrustal and the basement. But it is not an oblique impact feature, as some locals have suggested.

DENCE, M.R.: WIRGO inTIC's? [What (on Earth) is Really Going on in Terrestrial Impact Craters?]

Dence discusses the re-interpretation of geologic investigation of craters from the Canadian Shield he has worked on extensively since the 60s. Shock attenuation data suggest that at the (current) surface, the shock attenuation rate is higher than directly down into the target. Brent (impact energy around $3 \cdot 10^{17}$ J) is one of the type-examples of simple craters, however it is far from being "simple". For larger complex craters, the data suggest that rebound causes central region to rise above the original

surface, and then collapses, forming a peak ring. He ends the talk with the question: What controls the depth of Grady-Kipp fracturing and its change with crater size, and how does that affect the timing of crater collapse?

QUESTIONS/COMMENTS TO DENCE'S INVITED

Ahrens: Could you elaborate a little bit on what is appears to be a new explanation of the rapid change in the PDF number of fracture directions with shock pressure. Previously, my understanding is that when Grieve looked at these he got a very rapid apparent change in shock pressure with distance and I think you are re-interpreting these data and saying that the various PDF sets of fractures are relatively insensitive to peak shock pressure, and they all occur in a relatively constant shock pressure range that is seen by the rocks in the central uplift. Do I understand that correctly now? You are re-interpreting Grieve's results on that.

Yes, I am re-interpreting the Robertson-Grieve material here. There is a strong contrast between what you see down hole at Brent and Charlevoix. At Brent the zones are highly compressed, and you can say that the material that was like that was compressed 20 or 30 times and smeared out, and was able to do that because it was ultra-brecciated, broken up. The material in the central peak, at Charlevoix in particular, does not break up that way, it moves as solid masses kilometers across perhaps, and in effect all it does is to rotate upwards. I do not think I am changing anything; I am saying that what you see at the surface, when the material rotate upwards, is diagonal slides through the shock zones, and what I was trying to implicate was a possible net trajectory for that, from depths to the surface. It does mean that you have to move things in too. It is rather sensitive to your assumptions on how far this movement is. That is why I feel that, in some ways, we get more information about what the probable shape of that transient cavity was, from this type of data at Charlevoix, than we do from trying to reconstruct it at Brent, where all of that information is destroyed in the subsequent brecciation process.

KOEBERL, C.: Using geochemical observations to constrain projectile types in impact cratering.

Koeberl summarizes how to determine impactor types from impact craters. Platinum Group Elements are the most commonly used elements to distinguish meteoritic from crustal material, as well as different meteoritic types (although those are not very clear). One question that arises is: How representative are available meteorite samples of projectiles that impacted hundreds of million years ago? Measurements are difficult, and furthermore not all impact structure melt sheets have impactor components (above the detection limit). Alternative samples are ejecta material, but tektites have a very minor, if any, meteoritic component (<0.1 weight%). In summary, from the observational point of view, it is necessary to improve mixing calculations. Theory can help in trying to understand the kind of fractionation occurring in impact events and what affects it.

QUESTIONS/COMMENTS TO KOEBERL'S INVITED

Herrick: Could you briefly address if there is any way to detect whether you have a comet or an asteroid?

There are few approaches to that question. Traditionally, I think, we should say it is somewhat between impossible to difficult. One of the reasons why I say it is almost impossible is that we really do not know what is the chemical composition of comets. They have not really been studied in great detail. The only way we can get a handle on that is to assume that the interplanetary dust particles (IDPs) are representative for the composition of some comets, and try to approach it that way. The second thing that makes it very difficult is that when you have a cometary impact, a cometary nucleus only contains a small proportion of rocky material, maybe 10% or something like that. Assuming, which some people do, that this will have a carbonaceous chondritic composition, you would say that the amount of cometary material that you get into the melt eventually is only 10 times smaller than that from a normal asteroidal impact. The two problems, I think, that we have to answer are: 1) mainly astronomical, related to spacecraft: we need a sample of a comet. But then one sample of one comet would not do as much good. It reminds me of the debate about the deuterium isotopic composition, and can comets supply the oceans? And we really only have data from two comets, and they are different, they do not even agree. So, I think we need some good measurements of cometary compositions, and really do not have those. The material that is going to come back from some of the missions will help, but I am not sure to what degree. The second thing is:

Theorists can help that by helping us understand what happens during cometary impacts, when you only have a very small proportion of rocky material in there. My personal preference for an answer is: No, we cannot tell the difference at this point, because we just do not have enough data. Let me just add to that. At the K/T boundary, for example, there has been always the suggestion that maybe this was a cometary impact. Now, Frank Kyte has found chunks of a meteorite in there (in ODP cores of the Pacific), which could have been part of a comet, but probably not. And when you look at the amount of iridium that has been found worldwide, this was used to the reconstruct the projectile size at about 10km in diameter. Now that assumes a chondritic composition. If you had a cometary object it would have to be much larger because you only have a small fraction of rocky material in there. That would mean a much larger crater than we actually observe.

AHRENS, T.J., O'Keefe, J.D., Stewart, S.T.: Calculation of planetary impact cratering to late times

Ahrens discusses impact simulations studies carried out by his group, mainly simulations of the Chicxulub impact event. Simulations are carried out using CTH, modeling both early and late stages of the impact, thanks to the improvement of the strength model (as discussed earlier by O'Keefe).

QUESTIONS/COMMENTS TO AHREN'S CONTRIBUTED

Spray: Tom can you say something about this oscillatory behavior and does your model show this oscillation as it progresses?

Yes. Depending on what strength you put in, if you have a completely fluid situation, it'll have several oscillations, whereas if you put in a strong material it will just make one crater and it won't even relax, it will stay a crater and be strength controlled, and you'll get anything in between. O'Keefe wants to make a comment.

O'Keefe: It is the deep-seated faults that can have reversal in direction, due to what Tom is talking about. The near-surface faults, which are due to the overall rebounding of that crater and the collapse of the ejecta curtain, I do not think oscillate that much. It is just a series of faults that are just transferring the energy finally to the one major fault, which is the crater rim. So there is both.

Well, I guess we do not agree on this.

Herrick: I guess on the Chicxulub modeling, this is the case where there are some very tentative conclusions about the structure in particular, the whole mushroom head concept. There is a lot of debate as to whether the seismic evidence is there for that. I would say it is not, I would say much more it looks like simply the structure went up and you have that there is no evidence for a mushroom shape to the central structure that you get from the seismic data. So, I hate to see a whole suite of model runs based on a conclusion that is tentative.

Some seismic today show normal faults, and some show reverse faults around that central ring, so it is not clear to me what the truth is.

O'Keefe: What we found is: when we did not put in any damage then you got more of this mushroom-like central area. In other cases the mushroom shape was not pronounced. …[end of tape]… the central columns is the melt. The melt distribution is: you have got a central melt zone and you have got this very thin layer across the top.

Herrick: Ok.

Osinski: I am going to do a talk tomorrow, looking at faulting around the Haughton structure. Can you do this modeling in 3D? If not, has anyone just done modeling in 2D but in the horizontal plane? Because we see radially oriented faults play a big role in the modification of the structure, and would you be able to pick it up?

That [Chicxulub] is a 3D calculation.

Osinski: Could you do it in 2D in a horizontal plane? Is it at all possible?

O'Keefe: These are all 2D cases, but you can just make a 3D…. [too low to be picked up]

But the calculations have been done. I do not know that anybody has calculated radial fractures in a 3D calculation, but perhaps somebody in the room can comment about that. I think that is beyond what people have done.

O'Keefe: This [full 3D] would be an expensive calculation to go out to late times.

Yes, the time on this slide is 568 seconds. So we are talking quite a few minutes here. Time is money as they say.

Sharpton: ... [away from microphone; something about space issue] ... you are pushing things in toward the center so you need to accommodate that somehow.

Are you talking about mesh size?

O'Keefe: ... it is conserved mass…

You have lower resolution at later times, unless you have infinite resources.

Herrick: This is an axially symmetric calculation.

Yes it is. This is a 2D calculation.

O'Keefe: It is a 3D calculation with axial symmetry.

SUGITA, S., Hamano, K., Kadono, T., Schultz, P.H., Matsui, T.: Towards a complete measurement of the thermodynamic state of an impact-induced vapor cloud

Sugita discusses the problem of understanding the thermodynamic state of a vapor cloud from an impact. One needs to know at least two thermodynamic parameters, which is very hard to do in the laboratory. He presents a new method, based on high-speed spectroscopy, and shows some initial tests, based on experiments that use laser-simulated vapor clouds.

QUESTIONS/COMMENTS TO SUGITA'S CONTRIBUTED

Gerasimov: Segi, again the question is about the optical depth of the cloud. If you have small-scale impacts, maybe the range of the cloud is so small that the optical depth is larger than the cloud's depth. But if you have large impacts the optical depth will be very small compared to the dimensions of the cloud and you will measure only the outer range of the cloud, Then there is something close to the quenching point, then about 50% of the material will condense and you will not have the right chemical composition of the cloud (because you believe that you know this chemical composition), and I think there will be problems in discussing the whole cloud.

It really depends on how to use this technique. This technique was developed mainly to obtain data about fundamental material properties like, how let's say gypsum evolves at high temperature, or dunite vapor would evolve at high-T. In this case the small-scale impact experiment is just fine. To answer your question about large scale: If we are lucky enough to be able to watch some big event like a SL9 type, a 1 km size impact event on a surface of planets, there is actually a way to use this type of technique. When you look at the really strongest line, you are right, the optical depth would be too large, so you do not get to see the deep inside of the vapor cloud. But if you look at a weaker line, it takes so much length to get the optical depth, so we can still use the thin approximation like in this case.

O'Keefe: But at some point during this expansion it [the cloud] becomes transparent (the Rosseland mean free path becomes large), so you can penetrate through the cloud. The issue is the temporal resolution to measure it. You'll always get an average over the whole resolution.

GERASIMOV, M.V., Dikov, Yu.P., Yakovlev, O.I.: Experimental modeling of impact-induced high-temperature processing of silicates

Gerasimov discusses the results of experimental work aimed at investigating impact-induced high-temperature processing of silicate materials. The experiments indicate that volatilization during an impact event is not a linear process: clusters tend to form during melting/vaporization; also, strong thermal reduction of Fe with subsequent agglomeration of Fe-droplets and their dispersion (mechanical volatilization) from the silicate melts occurs.

QUESTIONS/COMMENTS TO GERASIMOV'S CONTRIBUTED

O'Keefe: I have a couple of questions. One is a general one: You use two different techniques, the impact and the laser. How good is the laser in simulating impacts?

It is rather good. We have very high coincidence with both, the light gas gun and the laser.

O'Keefe. The other questions is: Have you looked at the condensation kinetics, and do you have anything that you may want to say on that, in trying to model the growth of the particles/droplets? I mean, have you modeled the growth to be able to scale it up to

larger impacts, what would be the growth rater of the droplets?

I think that it is about the same kinetics, because there is oversaturation in the expanding vapor, but also the scale does not provide larger particles, because since they become large enough, they become melt droplets. So the condensed droplets have to follow the temperature range of the cloud and they must be very small to accomodate with the surrounding material. If they get too large the heat will be accumulated inside and by expansion they would be overheated and become evaporated. So, I think it is some kind of outer mechanism.

JOELEHT, A., Kirsimäe, K. Versh, E., Plado, J., Ivanov, B.: Cooling of the Kärdla impact crater: II. Impact and geothermal modeling

This is part two (part one was a poster) reporting on the thermal history of the Kärdla impact crater, a marine structure in Estonia. Data are obtained from three boreholes in the center of the crater. Impact modeling suggest a quick cooling (~ 100 years) right after the impact, with the hottest region (temperatures high enough to get water vapor) very close to the central uplift, contrary to the data which suggests a hot region near the rim. Modeling of the cooling requires good measurements of rock permeability, although there is no guarantee that present-day rock permeability corresponds to the true post-impact permeability of broken up rocks. The model shows that convective cooling is comparable to the conductive case.

[end of tape, no comments recorded]

Hagerty, J.J., NEWSOM, H.E.: Limits to the presence of impact-induced hydrothermal alteration in small impact craters on the Earth: Implications for the importance of small craters on Mars

Newsom reports results of a study of the Lonar crater (India) investigating hydrothermal alteration. They apply those results to Mars, finding a layer of altered material about 2 m thick over the course of Martian history, corresponding to about 0.7 m of water. Not a huge amount overall, but it can be important. Results are also consistent with explaining the origin of Martian soil.

QUESTIONS/COMMENTS TO NEWSOM'S CONTRIBUTED

Koeberl: *Horton, in your study of Lonar, did you look at fluid inclusions in some of those samples, because that could give you an interesting composition of the hydrothermal fluids there.*

No, we have not looked at fluid inclusions. That would be a good thing to do. This is such a low temperature system that we have not really seen that.

Melosh: *Horton, with all due respect about your 800 km diameter crater on Mars, I see plenty of terrestrial geologists finding circular structures, or partially circular structures on Earth and claiming that they are actually impacts. Do you have any other evidence than you can fit a circle through a couple of topographic features, to indicate that this is really an impact basin.*

Unfortunately, I have looked at the geophysical signatures, and geophysical signatures for large craters on Mars are highly varied. Most of them have no geophysical signals in magnetic or gravity. The neutron data show some kind of similarity between the Cassini structure and this structure, so there are some similarities there. It really is rather remarkable the similarity with the Cassini structure in terms of having a central ring and an annular trough surrounding it. So at the moment that is our best evidence. We are going to be out on the ground in a year, and be able to find more about it. There is some chance that we will be able to find more about it from that point of view. The other thing is, of course, we have a vast amount of high resolution data pouring in. That is going to allow a better examination of the geologic situation. But if it is a crater, it is an old one and more degraded. So, it is going to be hard to confirm that.

GENERAL DISCUSSION:

French: I would like to start off by throwing out a question for people to talk about. Presentations by both modelers and geologists, or as they seem to be called now, observationalists, have provided a good deal of information about the type of things that can be done, and the type of data and models that can be produced. The questions I would like to throw out is to each one of these two communities. I would like perhaps the modelers to comment on what geological observations of large and small impact craters might be most relevant to testing the models, and I would

like to throw the reverse question to the geologists about what modeling features may be the best one for trying to check in the field or even to guide fieldwork.

Ahrens: I can only speak about some things that you would like to have more data about to compare the calculations. I think the mapping of deformation features is really very important because that is something that you calculate. The models that you calculate depend very much on material properties, and at least you can measure those to some degree. For example, we have been looking at Meteor Crater (Ai has a poster in which she has looked at the depth of cracking underneath Meteor Crater), but there is very little data on other craters. There is no data on Lonar crater, and it would be very nice to understand how deeply it has been shattered. It would also be useful to have a map of faults around such craters. Drilling provides really hard-core information about where the zones of localized shearing are. I think that is really important. There has been a lot of work done on PDFs in the Canadian craters, but relatively little has been done on other minerals. I think the observations of impact melts, particularly in carbonates, are very poorly understood right now, and there is very little data. So there is a lot of information about shock metamorphism, that if you can understand where it occurs, it ties very closely to calculations, hence our ability to put models together.

Chapman: A lot of the discussion so far has been about terrestrial craters and observing terrestrial craters. Horton [Newsom] has reminded us that there are craters elsewhere and Jay's question about how do you interpret morphology illustrates the fact that we know and can learn far less about these craters from observations. Seems to be that what we observationalists would like from some modelers are some things pertinent to some other kinds of features of impacts than those that have been chiefly addressed here. On planetary surfaces there are several kinds of issues related to ejecta, including very far field ejecta, which is generated perhaps from things in the very early times of the cratering. On Europa we have very extensive, widely distributed secondary craters. I would like to know what processes produce those and what attributes one would expect of secondary craters in the far field. Another major problem is understanding the sampling, say, on the Moon. The kind of geology that was done in the Apollo program is far less extensive that what we can do on the terrestrial craters, and there are all kinds of issues about the lunar samples and how they relate to craters and impact basins that caused them. There, the ejecta processes, blanketing, etc., need more study and modeling, so we can understand those sampling issues.

Herrick: I wanted to make a comment. In terms of the modeling, I have seen models of the basic processes in homogeneous media, and I have seen a jump to models of very specific craters. There is a range in there of some generic suites of models. In particular, what I would like to see are some generic models covering impact into a layered media. It would help a lot in understanding a large range of craters to have a general feel of how excavation, for instance, proceeds if you have a two-layer model, with a relatively weak layer and a relatively strong layer, and what happens as you vary the relative strength of the layers and the relative thickness, and even start including a fluid layer on the top, such as you might get in an oceanic impact. Just a generic suite of models rather than, like we have seen in the past couple of days, some very specific models where we put in a water layer that we thought matched what was happening in this particular crater at this particular time.

Pierazzo: It seems to me that a lot of geologists do not believe anything the models come up with for specific cases. So if you come up with a very general case they will believe it even less. I don't know, it is kind of a tough call.

Kyte: There are some of us who work on things that blow out of the craters. Twenty years ago when we start working on the K/T boundary and found all this iridium in it. Models back then could not make ejecta that had 10% meteoritic component in it. It was kind of a challenge, and I guess this is an acceptable thing now. It would be interesting to see these models try to take the vapor plume and figure out what is going on in it. How do you get large concentrations of meteoritic material in it? How do meteorites actually survive impacts. We know now that they do survive these big impacts events. And just as an example to toss up at you, we have got a paper coming out in Geology in March on one of these Barberton greenstone belt spherule beds, in which chromium isotopes, iridium suggests there is a meters thick spherule bed that is 50% meteoritic material. How are we going to make that? It is definitely meteoritic material in there, there is no question on that, but can you make that with one of your models. That would be a really interesting contribution.

Pierazzo: There is some work that is being done. We are very interested in modeling the vapor plume and try to understand what is going on in there. We are still limited by of course, resolution and equation of state at this point, but there is also some experimental work that they [experimental: Sugita, Gerasimov; modeling: Abel, Rocchia, etc.] are trying to do, and coupling also modeling work of chemical fractionation. I think we just need to keep working on that and try to get as far as we can, although it is going to take a while before we can actually get anywhere with that.

Newsom: I have two comments. One about what Jay was saying, and that is: we are actually working at trying to measure the properties of these large structures that people propose. There are structures that people propose that clearly do not exist as real structures, and they are still on the list. And we may even be able to come up with some kind of confidence criteria, so that our lists are actually a little more realistic for large basin structures on Mars. But there are very distinctive features of these large structures, that if we can get some feedback from the modeling, now that we can start to address these with new versions of the codes, it could help establish whether these really are structures or not. The second part is on the thermal structure, and that is: We are beginning to get to where we can really get the amount of heat particularly in the smaller craters. This could conceivably be used to help calibrate the energy deposition and energy distribution in the models. Certainly, we should have some agreement between those.

Spray: I think one thing that would help the geologists is if the modelers could tell us the degree to which rock masses have moved by the end of the cratering process, so that we may delineate, it is probably something that can be done now actually, zones of different total displacement. I think that can help us, can guide us in the field to look for discontinuities and zones of different deformation regimes.

Ahrens: I was going to ask a question of Gerasimov: He showed some very interesting plots of volatilization, and you had a table that showed the chemical composition, and then you plotted it versus increasing volatilization. But I was not clear on what exactly you were doing experimentally. You had these ultramafic compositions, losing all its silicate and magnesium, and ending up with a calcium and aluminum rich residuum. Perhaps you could explain what is going on here, and what relationship those experiments have with impact vaporization.

Gerasimov: It is very clear what I wanted to show. The compositions of melted droplets, which were dispersed from the melt at temperatures around 5000K, from a single impact. These droplets still fly inside this hot vapor. So volatilization proceeded to a very high level. Different droplets have different exposure, so the degree of volatilization is different, and there is a sequence of such volatilization. What I wanted to show is what direction we have and what is the effect.

Ahrens: But does the most devolatilized have a higher velocity or a lower velocity, or is it independent?

Gerasimov: It is not independent. The higher volatility is for droplets which have the higher exposure, and higher temperatures.

Abbott: I would like to see some more models of abyssal impacts. In particular, we know we get resurge gullies in the shallow water craters. How much of the crater rim gets eroded in particular in deep water impacts. Are the resurge gullies evenly spaced? Is it a function of crater size and water depth? Now with these 3D models I think it is may be possible to actually get a picture of this.

Stewart: I have question. What do we need to be able to model basin-forming impacts on planets? Self-gravity is a big problem. Can the SPH codes do basin-forming impacts? It is not even on the schedule, I think.

Asphaug: I am going to comment a little bit about SPHs capabilities tomorrow, but the issue is always how do you evolve a model so that it responds to the succession of cratering, which is some event at very low strain rate, when the model is designed to model the impact event as well. I think you almost need to have it in two separate models. I really think basin formation needs to relax from initial conditions from an impact code, just because you are trying to bracket almost tectonic strain rates to impact strain rates within the same code. As Jay said in his first talk, there is only so much you can do within a code. I wanted to enhance upon what Clark [Chapman] said, that there are other bodies that we can study, and kind of bridge from Paul Schenk's talk, where the last little data point that could have fit his plot could have been this crater on Vesta. I wanted to emphasize the

point that there are these low-gravity bodies, with a thirtieth the gravity of Earth, in the case of Vesta, or 1/10,000 the gravity of Earth, where you also have large basins forming. It is not so much that these might mimic the processes of slumping and relaxation on Earth, but you have this end-member in the other direction that you cannot achieve in the centrifuge, and you cannot really achieve things at this scale in low-gravity experiments. So you have a direct analogue but of very low gravity, and you have direct samples of these events on the Earth in the form of meteorites. I think that is a very significant crater probe as well, connecting meteorites, especially in the case of Vesta, to a large basin forming impact.

Melosh: The trouble with trying to look at the simple-complex transition on anything much smaller than Vesta is that for anything much smaller you blow it up before you get to the transition. Once you get down below about 300 km diameter you cannot get a complex crater because you destroy the asteroid first. So there is a limit to how low you can go.

Asphaug: Yes, I mean Vesta is the perfect case.

Melosh: Vesta is pretty close to having been blown up by that crater that formed on it.

Macdonald: I would really like to see the relationship between the diameter of central uplifts and the outer diameter of impact structures to be addressed in a more sophisticated manner, largely because on Earth we are dealing with so many eroded structures that we can get a good feel for the diameter of central uplift, but we really do not know the outer diameter on a lot of structures, and to be able to really address the size in a good range. It is just the estimates right now are so wild for so many impact structures on where that outer diameter is.

O'Keefe: I would like to see a definition of what a crater diameter is.

McKinnon: I did want to say that since there is this very large central peak crater on Vesta, and there is a mission going there, there is this obvious chance to look at a kind of Copernicus scale structure and even larger, where the details may be more explicit and with less erosion. Also, I do not know if Paul [Schenk] mentioned it yesterday, but there is the large crater Herschel on Mimas, which is a small icy body, but is also a central peak structure, and it would be interesting to look at it. The mission will get there in a year and a half. You mentioned earlier about the fidelity of modeling, whether anybody would believe anything, and Tom made a very interesting comment this morning that Dugan [O'Keefe] or the two of them, the AOK team, have made calculations of just laboratory craters in sand or elevated gravities. Maybe this was discussed yesterday when unfortunately, I could not be here, but are you able to reproduce exactly laboratory sand craters with your code calculations Dugan, and my specific question is: What is the appropriate angle of internal friction in those calculations that that you measure in static experiments on sands?

O'Keefe: I leave this to comment to Kevin [Housen)] and Keith [Holsapple] who are really trying to analyze their experiments in sand and closely looked at the material properties and they are the most appropriate.

Holsapple: Let me make a couple of comments. First of all I want to go back to the previous one: People would really like to see the codes come out and show you where the rings are to be, etc. I think that for a long time it is going to be the other way: You tell us where the rings are and we'll get there. Honestly, the problem is that they are dominated by very subtle, very late stage strength things, and it is only recently that we are starting to even put these things in codes. Dugan [O'Keefe] was one of the first, and this idea of strain softening, strain localization, and all of that, is in rock mechanics but we have not had it in the codes. And those are very subtle differences: you find that if you change the angle of friction a little bit that it changes everything, because you have got this oscillation. The question is: when does it stop? So I do not think we are going to be in a good way of predicting these things. We can postdict them, but we cannot predict them. I think that is just the name of the game. Now, with regard to sand craters, everything that we have done, shows that you get very close. We use CTH the same as Dugan [O'Keefe], and you get as far as volume, you can put it right on the π_V-π_2 curve. You can do that for water, you can do it for dry sand. But there are subtle differences. When you start looking at the details, like ejection velocity, it is very hard to get them correct. I think that what is missing in our calculations in the past, is we did not let the strength depend upon the crush state, the amount of damage. That is, there was basically no damage in it. You end up getting craters that stop, while in the real laboratories you see they go up there and then they come back. We have got quarter-space tests and

actually see that happen. So, it is this subtle interplay of how do these strength models then depend upon the state it has gone through, and that is something we are just learning and we have a long ways to go yet. But we can have gross features very easily.

McKinnon: You are talking about dry sand…

Holsapple: There is not a lot that comes back in sand, but we get things like, wrong velocities, when you actually map particles. Do the laboratory test and put in tracer particles with, you can actually look at the velocities. That is a much more difficult test than simply getting the right crater shapes. There are a lot of knobs that you can turn to get the right crater shape. You can play with angle of friction, cohesion.

McKinnon: Ok, but when you measure the friction in a wooden box, I mean, does that work or do you have to use a different angle to get it.

Holsapple: I think to a first order it works, but then you need to degrade it particularly for large craters. You need to have it go way down with damage.

Unnamed: I just have a question of whether there is some kind of fluidization at all that we see in the lab.

Housen: No, none at all (you do not need it). Keith just basically said everything, but just two other comments. One is: You put in a reasonable friction angle for sand, like 30-35 degrees or somewhat in there, and it works just fine. In fact, there have been a number of dynamic shear tests that suggest that friction angles are not terribly rate dependent. And also this problem, like Keith said you can have the crater shape right and you can get the crater growth right, but you may be a factor of 2 off on ejection velocities. That is, at least, partly due to the fact that, Keith was talking about this yesterday, the P-alpha model does not really model crush-up very well, so…

Gibson: I put the summary of the diameters for Vredefort up there, and the thing I want to know is why a modeling attempt by Zibi [Turtle] and Betty [Pierazzo] have a factor of two of half of what the actual estimate from the field based study is. We are talking about a 250-300 km crater, yet the modeling, which is attempting to do exactly what everyone wants it to do, look at the distribution of shock features, the distribution of thermal features is actually giving us such a small estimate. Who needs to do what?

Turtle: Well, I'll respond to a couple of those points. One of the things we have done in our models is to explicitly figure out the locations of various shock features and where materials have been raised to various temperatures, and compare that to what has been observed. We actually ran a series of simulations with different impact projectiles, different size craters, and the ones that matched the observed geologic data are the smaller craters. Those are the shock features. One of the reasons this is different, for example you put up Therriault et al. results, which show a crater which is twice the size as our, and that is looking at exactly the same shock features. The point we made is that in their analyses they assume that the shock contours parallels the size of the transient cavity, and that those are always at the same location at every depth, proportional to other features. One of the things that our shock model shows, for example, is that is exactly not the case. The shock contours are actually curved back in toward the surface. So you cannot scale from the shock contours directly proportionally. That is part of the things that are coming out the modeling, that kind of detail. There clearly is still some disconnect in some of the other features, and we need to look at other features in the models and see if we can match everything. I think the amount of uplift we are getting is fairly comparable to what is observed at Vredefort as well.

Dence: Basically I have tried to be very conservative in my estimates of crater sizes. I have a rule of crater economics, which goes: The rate of inflation of the sizes is a function of the interest, and the desire to have the biggest in your own backyard. There is a sociological aspect to this as well. I would like to see us really get down to careful analysis and comparison of all of these things. I think there is a fair bit of consistency in the data, and a certain amount of distortion has been entered into, not just the ones that we have discussed, but also the other ones. This aspect of layered structures versus non-layered is very important here. Certainly, layering can give you a much larger crater, proportional to the energy input, of the non-layered material. I would also like, particularly for very large craters, to see a bit more attention given to what Roger touched on, and that is the thermal gradient in the crust. I think this is particularly important for cases like Sudbury, where the impact occurred more or less in the middle of an orogenic episode. The thermal gradient was probably quite high. We have got a body similar to Vredefort, but I think a stage beyond Vredefort in terms of degree of melting in the target material belong the

zone of direct shock melting, which I think is something that we need to ponder and try to get the model. Some of the things that have come out today are very interesting from that point of view, the way the central uplift may collapse, etc.