Transcript of Session on Effects of Target Properties on the Cratering Process, Friday, February 7, 1:30 p.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.

HOUSEN, K.R.: Effects of target properties on the cratering process.

Housen discusses how various material properties, such as porosity, grain sizes, angle of friction appear to affect impact cratering. He then suggests ways that our understanding can be improved from the modeling point of view: 1) test codes using the large database of laboratory experiments and explosion tests; 2) measure material properties in more detail, such as triaxial or direct shear tests, crush-up curves, unconfined compression/tension; and 3) identify a standard suite of experimental data for benchmark calculations (important to understand how codes performs against each other).

QUESTIONS/COMMENTS TO HOUSEN'S INVITED

Abbott: Have all of your results on crushing been done where pore space is filled with air? Do you have any experiment that has been done where the pores are filled with water?

No they have all been dry.

O'Keefe. That is an amazing set of correlations, Kevin. Do you have any comments on the implications of damage on the material properties? You have used laboratory measurements of these, and you compared them against [inaudible]. I do not know all the answers, and you have done an amazing amount of correlations to get it.

Yeah, that is actually one question I was going to ask Tom about it. It occurred to me that those experiments that you guys did, where you are looking at damage beneath a crater, you have all of these little cubes cut out. It would be nice to do something like that, and maybe do some strength measurements on it. Then you can get a very nice correlation between strength properties and damage.

Ahrens: There is certainly that.

Holsapple: …[overlap]…damage it and go do the triax test and then go and damage it more. Then we can get a pretty good idea, at least for that material, how a failure envelope should change with damage.

O'Keefe: Frankly, I think that is the biggest area of uncertainty, that is: what are the damage properties and how do they vary?

Schultz: Very nice, Kevin. There is an interesting question, though. In the data that Gault and I have looked at, when we get down the smallest projectile, we actually saw a projectile size effect. That is if you went below ¼" to a 3/16" to 1/8" to 1/16", rather than falling on a single line, they eventually had a much higher slope as you got to smaller π_2 values. What we sort of concluded was that some of the scatter of the band is because we are seeing a superposition of different scaling laws as we go up to super high velocities. Do you have any thoughts about that?

Well, were these experiments at low speeds, high speeds?

Schultz: They were small π_2 and velocities were 6 to 7 km/s.

I see. I guess we have not seen, at least in the experiments we have done, any evidence for a size effect from the projectile, but that would be very interesting to look at. Now, this isn't the case were you are getting into projectiles that are so small that maybe you have a grain size effect?

Schultz: That was one of the conclusions we had, but we are still not clear.

SCHENK, P.M.: Importance of target properties on planetary impact craters, both simple and complex

Schenk addresses the issue of target properties from a planetary remote-sensing perspective.

QUESTIONS/COMMENTS TO SCHENK'S INVITED

[Only partial, because of tape problems]

Stewart: To go back to the data that we may use to predict the rate: We have the first Hugoniot elastic limit measurements in cold ice (previous data were all at –5 to –10°C) and we see a strong temperature dependence on the strength by a factor of a few. In addition we see a strain rate effect: As you go to higher shock pressures you can support a higher dynamic compressive strength. So there are two things going on, which are further compounding what ice is doing, and then if you have a temperature gradient on the icy satellites you can imagine that you have got a quite complicated strength model you have to put in to deal with it properly (Holsapple: and ice has 15 different phases). There are also 15 different phases, but we really only have to worry about two or maybe three.

One thing that concerns me is that these things will surely affect the growth of the crater, but once you begin to get to a modification stage do they fall into it at that point, these differences in the ice rheology? How important are they after the shock wave has passed through?

Stewart: You can imagine that the tensile strength has a similar dependence, for example, and will control faulting.

Yes, that is certainly the case. That is one reason why the craters as so different.

Asphaug: It maybe a minor effect but for a larger crater you get up to 20% melt volume. Dienes noticed in detailed image analyses of larger craters a difference of where the melt goes, because of course in ice it will sink and in basalt it will float.

Yes that is a bit of a problem. Unfortunately, Galileo was able to achieve high resolution on a handful of craters and not all of them very fresh. I have looked at the Lunar Orbiter for the Moon so I have some idea of what frozen melt may look like on the surface. I have a hard time actually distinguishing a lot of melt in the interior of these craters on Ganymede and Callisto. It is there, almost certainly but to see flat standing pools or mounds, that you see like on the floor of Copernicus it does not pop out as there being a lot of melt. Now it does not mean that it has not been splashed out or that it has not drained in fissures, but it does not stand out as there being large pools of melt.

Melosh: Paul, you mentioned Vesta, and I know that you cannot get a transition diameter off of Vesta because we only see one crater on it, but there is an enormous complex crater on Vesta. It has a diameter larger than the diameter of Vesta. If you take that morphology of a complex crater and then scale it to similar morphologies on the Moon, it follows very nicely a 1/g dependence. And Vesta has such a low surface gravity that there is an enormously long lever arm, that I think is a very nice verification of the 1/g fitting on a silicate body trend.

Well the question is: Will we see those complex forms of smaller craters, in which case it will push the bar down, and cause the trend to warp over

Melosh: Ok, we do not see a transition, but there is some data that says that 1/g works very well even to these bodies with very low gravity.

Yes, if we did not see it that would be a problem too.

ORMO, J: Next step in marine impact studies: Combining geological data with numerical simulations for applications in planetary research

Ormo focuses on marine impacts, as an extreme case of layered targets, and how the collaboration between geologists and modelers has helped enormously in understanding what is observed and what to look for in the field, as well as constraining and improving the modeling itself. For future work he suggests that planetary connections may be of use.

QUESTIONS/COMMENTS TO ORMO'S INVITED

Ahrens: Could you review why it is that you know that there was a water cover on the target before the impact? Secondly, on the overturned flap, you did not say, but is the damage that you saw, further out on the overturned flap, damage that occurred under the projectile where it hit, or right around where it hit, and it just got projected out further upon being *overturned, or is this some other mechanism that produced the greater damage with crater radius?*

About the first question: we can see that in the sediments, these well-known marine middle Ordovician limestones, the marine sedimentation continued immediately after the formation of the impact crater. You can have an impact in marine Ordovician sediments in southern Sweden (next to a car) today! What is important is that marine sedimentation continued immediately afterward. And then, of course, we have this resurge of sediments from the collapse of this water cavity. We can see that they are like 200 meters of fining up sequences. There must have been a lot of water for that to form. It is not like some kind of debris flow slumping in slowly. It is something like a massive movement of water inside to generate this sequence. About the second question: I think that it is an effect that the further the material has been transported, the nearer to the point of impact it was when it got ejected. At the hinge you are the farthest from the original point of the impact. It is possible that there it was just very slowly turned over; but the rest of the material has been transported much farther. We can see that that material is very finely crushed, but it is also today a very hard breccia. It is like, if you have been to Gardnos, the impact breccia there, even if it is very finely crushed.

CONTRIBUTED PRESENTATIONS

CRAWFORD, D.A., Barnouin-Jha, O.S.: Application of Adaptive Mesh Refinement to the simulation of impact in complex geometries

Crawford presents preliminary results of application of Adaptive Mesh Refinement (AMR) to model mixture of low and high impedance materials, to represent impacts on asteroids like Eros. The study is aimed at the establishment of a methodology that combines AMR with Monte Carlo technique to study material heterogeneity. The first results of simple tests look encouraging, but more work is needed.

QUESTIONS/COMMENTS TO CRAWFORD'S CONTRIBUTED

[No tape recording]

GISLER, G., Weaver, R.P., Mader, C.L., Gittings, M.L.: Two- and three-dimensional simulations of asteroid ocean impacts

Gisler presents 3D model results of deep oceanic impacts with Los Alamos codes RAGE/SAGE. Code validated against laboratory and underwater landslides. Simulations are carried out at very high resolution, using AMR. Model shows first the excavation of a transient cavity in the water, which is then quickly filled in because of gravity. No strength was used for the water in the model. Results show the formation and propagation of complex wave trains that quickly decrease in amplitude. In terms of impact hazards, the results suggest that impactors less than 1-km in diameter are not expected to produce ocean-wide, fast tsunamis that can be hazardous.

QUESTIONS/COMMENTS TO GISLER'S CONTRIBUTED

<u>Chapman</u>: *When you say "not significant" what wave height are you talking about as not significant?*

As I said, I do not have any runout model in the simulation. What I was looking at is only out to a thousand km, and those wave heights were down to 1 meter.

<u>Spray</u>: *You are implying that the 1km projectile did not in fact do any damage to the basalt floor?*

The projectile did not quite reach the bottom. Some cratering occurred at the bottom but it was essentially due to the expansion of the water vapor, not the projectile itself.

HOLSAPPLE, K.A.: Does melt volume give the signature of the impactor?

Holsapple discusses the possibility that melt production can tell us something about the characteristics of the impactor. Scaling laws are limited to the fact that they are based to laboratory or at most explosion data, and are limited, so we must relate to computer modeling. It only requires the early time impact cratering stage. This problem seems to have been visited and revisited about every 10 years, with apparently contradicting results. In reality, it is really a matter of interpretation. The overall conclusion is that it is practically impossible to realistically use melt volumes to determine characteristics of the impactor (e.g., velocity, size, etc.)

QUESTIONS/COMMENTS TO HOLSAPPLE'S CONTRIBUTED

<u>Chapman</u>: *Is there anything else in the cratering process that is happening in the near field that could conceivably provide any information?*

Shock features.

<u>Chapman</u>: *Anything else?*

<u>Cintala</u>: *Projectile contamination.*

It has to be some detailed measurement. Certainly melt does not do it.

OSINSKI, G.R., Spray, J.G., Grieve, R.A.F.: Impact melting in sedimentary target rocks?

Osinski discusses the possibility that impact cratering in sedimentary target does indeed produce a lot of melting, but we just do not know enough about the behavior of sedimentary rocks. Increasing body of evidence seems to indicate that, contrarily to earlier studies and conclusions, sedimentary rocks do indeed melt, with little indication instead of massive vaporization/decomposition of carbonates.

QUESTIONS/COMMENTS TO OSINSKI'S CONTRIBUTED

<u>Hörz</u>: *Oz, you said that there is no evidence for any degassing in any impact crater. That is not quite true. We analyzed the melts from Meteor Crater and see that the melts are a mixture of silicates….*

[end of tape]

GENERAL DISCUSSION:

<u>Ahrens</u>: …some rocks that had enriched ^{18}O in the melt and she (Martinez) envisioned this has being material that was a vestige of shock vaporized material but there was much less than anybody had predicted. Perhaps somebody who knows something about this might comment about it. It seems to me that this has been a continuing puzzle as to how you identify vaporized carbonates.

<u>Osinski</u>: It is hard, although we are not really talking about vaporization now. Any mineral is going to vaporize under extreme high pressures and temperatures, but with calcium carbonates, yes calcium oxide is very reactive. However, Haughton is pretty much all dolomite and if it goes to magnesium oxide that is periclase, it is a stable phase, which you would think you would be able to pick some sample of it. I would expect to see that if there was a lot of decomposition.

<u>Spray</u>: Just a comment on the sedimentary rock targets and that is: The work that we have been doing and Oz is just discussing really is getting the matrix of these breccias, which people have been previously considered fragmental or comminuted material like a dust that somehow magically glues all of this together, and forms a rock. Most people have looked at the clasts for shock features and studied them in great detail, and have done great work with those. Very few people have done any detailed electron-microscopy study of the matrices of sedimentary breccias in impact structures. If you do that you will find that the evidence is extremely strong for them having been molten. And that is the point. It is the matrix that is gluing these breccias together, and the volume of the matrix is such that if they were clasts there is no way you can generate a hydrothermal matrix secondarily, at a later time, of that volume to actually pump up the clasts so that they are actually floating in the midst of this hydrothermal matrix. We do not really understand in our group why there has been this problem with why sedimentary targets should not melt. I think there has been a myth created in the past that Robbie alluded to earlier, when there are a number of options put forward, ten, twenty years ago, and one became favorite by authors and it became cast in stone. I think this maybe one of those myths that if people do the detailed EM they would that the matrices have been molten, even if it is a carbonate.

<u>French</u>: I think if you try and look at these carbonates and try to track back to what the conditions were under which they either melt or decompose, you need to consider the local environment as well, in other words, how intimately these carbonate layers may be mixed with quartz-rich or other silicate layer, because if you have a pure carbonate section the reactions are very restricted, and generally tend to be at higher temperatures. If you have the possibility of mixing in silicates, you can get similar analogous, both decomposition and melting reactions at lower temperatures.

<u>Newsom</u>: I think at the Ries that the work we did quite a while ago was basically along the same lines. We did not find these neat glasses, but there has been

discussion that if you look at the matrix and assume that was melted than the volume comes back up close to the predicted volumes of melt. That has been in the literatures for a long time. As far as the hydrothermal processes, those that we have studied at the Ries seem to be, again, very minor amount of alteration of the matrix material. There is a lot of work to do on the alteration, which hopefully we will be able to continue working on. In general, that ties everything together: the effects of alteration in both crystalline basement rocks and these sedimentary rocks are not that extreme except in localized areas.

Herrick: Let me go back to very early in the day, when we were talking about this discussion that with the proportional growth model you have this large transient cavity that is very deep and then it all magically goes back into place. I guess I wanted to know a little bit more about that experimental work. Basically, in terms of, say, a seismic section, getting things scaled that would need to get things back into place to then have a coherent seismic section. If you scale that to an impact experiment into sand you need to be able to show that you have layers in the sand on the order of .5 to 1 mm that then go through this process and slump back down and reconstruct themselves into mm layers. Is that what you are actually observing, is that the scale of the process that takes place?

Housen: Well, the experiment I think you are talking about was the one that possibly Schmidt was talking about earlier, the clay. I do not if you have seen the pictures of that, but it is amazing how…. Well I'll let Robert tell the story.

Schmidt: I was going to mention the other one, the saturated sand experiment. We did not really have any markers in there, though. But we did see it come back. That one we saw dynamically come back with high-speed movies. The clay experiment was done in stages. Basically, we did a 10g control shot and then we did a 500g crater. Obviously at 10g we got a very large crater. And all of these had marker columns, vertically along two crater diameters that were perpendicular to each other. Listening to your query, we obviously should have put in a horizontal layer of either clay or sand or something like that, but we fired the experiment and then probably spun it a little bit, but I do not think we kept it at rpm very long. Then we cut and compared them. Then we put them both back in and spun them. The one that was originally very large is the one that reconstructed itself. Now, we do not really have any evidence that the high-g one went through that big transient; this was 10-15 years ago. Do you recall Kevin?

Housen: As I recall the only diagnostic that was in there was that the two halves were cut apart and there was a piece of aluminum foil between them. I think you were looking for tear. That was not very conclusive, I think.

Schmidt: It did show some larger transient crater, now that I think about it, for the high-g one, but I do not think the high-g one went through fully the largest shape. But I think what we are inferring from the experiment, and I think it addresses your concern, was that the gravity flow field that pulled this crater back, brought it back so incredibly. I mean, these columns started out as half-inch diameter cylinders of clay, and it smeared them out flat. And yet, when it flowed back it was just amazing to see that this flow was reversible.

Ahrens: Well the calculations that were done really mimicked that exactly. You look at these calculations and say "it is hard to believe that that stuff ever was a deep transient cavity that got squashed right up again exactly so as the experiments say." The calculations really show the same effect.

Holsapple: And, of course it was a very smooth homogeneous material, so it is quite an idealized material, but at least….

Herrick: It is sort of my point: You have to get things back to the point were you have almost erased the fracturing of the rock at that level. The reflectors in the seismic sections are things that go away if you start moving things around and randomly scatter them on a scale of tens of meters, you get rid of the seismic reflector. That is the scale for craters where you are looking a few km deep.

Schmidt: But how thick is the reflector that you are looking for.

Herrick: It depends on the scale of your seismic experiment, but basically you are talking about frequencies that are tens of hertz, which means that the scale of the reflector you see are tens to a hundred meters across. When you start shifting things around on that scale you get rid of the reflector.

Schmidt: But if you brought everything back to within a couple of percent (it is probably size dependent for a crater that big) from where it started up originally even though things are broken up they came back and they may not be cohesive anymore, but they are close enough that the reflectors are back.

Herrick: You have to reconstruct things back enough that at the scale of tens of meters the sound velocity of your post- section is not altered from the pre-section.

Newsom: That is not entirely true. At Meteor Crater the suevite boundary is right where it is supposed to be stratigraphically, but yet there is a velocity anomaly that extend well into that so-called undeformed layer. So if you were to run a seismic profile across there, you would see a nice flat layer, but in fact there is a velocity and density anomaly well below that, presumably from the effect of that original transient cavity.

Sharpton: Or it could just be fracturing below the transient cavity. You do not know.

[overlapping voices]

Newsom: You still have that impedance there. The question is whether the numbers are exactly the same as they are elsewhere, where there has been no impact. You still are going to see a contrast at that boundary, but the physical properties are different. But there is still a boundary where it is supposed to be.

Herrick: However, because a seismic section gets displayed in travel time, that reflector will no longer be horizontal. When you do the processing on the seismic data, even though there is an impedance contrast in the physical cross-section, the seismic cross-section, because there is now a difference in the sound wave velocity that will show up as a deflection of the reflector in the seismic section that does not exist in the physical section. That is what I am saying: You not only have to get these things back in place, you have to get them back in place with the same sound velocity.

Sharpton: That is right. That is just my point as well. Let me make one point: At Chicxulub at the base of the crust you do see some seismic indications that it has been deflected. But, as best we can tell, it has not been permanently deflected. It looks like it has been damaged, may be it has been pushed down and it has come back. At a lot of craters, Robbie is absolutely right, you can see very definitive indications of how deep you could have possibly had this thing without destroying the carbonates.

Herrick: Actually, Chicxulub is one where I would say that that is a good example where there seems to be good indication of a pretty deep zone of destruction. I think the Chesapeake Bay seismic sections are probably the most puzzling example.

Sharpton: That is the one that everybody ignores.

Herrick: The seismic section there looks pretty unaltered below pretty shallow depth. It is a pretty puzzling seismic section.

Ahrens: I want to make a comment about the seismic sections. First of all Bob Herrick is completely right, that there are two issues here: You do not expect to see an unaltered stratigraphy in reflection seismology if there is in fact shock damage, because the rocks will have a lower velocity. I think that is an effect you cannot look away from, but nevertheless there are, even in the case of the Ries crater, where there has been a big rebound of the transient cavity, a [unclear] in the article in the book that Roddy edited has shown in refraction seismic work, where they are just measuring the velocity in the rocks below the central peak, a big velocity deficit, which many people would interpret as being rock that has cracked as a result of being pushed down in the transient cavity and then come up again. It has been worked hard but it has come up a long way, because you have bedrock sticking out in the middle of the Ries crater. So that rock has come up but the point is it has been pushed down very deeply in that transient cavity and come up again and there is a very strong velocity deficit seen in the refraction, not reflection seismology.

Herrick: I agree, actually. There are some craters on Earth where it does seem that there were things going deep and then come back up. There are other craters on Earth, and Chesapeake Bay is probably the best example, but there are a few that are very puzzling.

Dence: Could I just inject another crater into the story? Particularly, the one I have in mind is Gosses Bluff, which is of a similar size to the ones we have been talking today, in the 20-25 km range. What you see at the surface now is the eroded central peak. You can go up to these rocks and they are standing on edge, dips of 90 to 70º; they are in blocks even the thin bedded ones. Carbonates which run for the length of this room and beyond without a break. So they are striking in all directions; they have been brought up absolutely vertically. The stuff a km or 2 further out, which is massive quartzite, is now in blocks hundreds of meters across, that is how we mapped them, with big faults between them. And what you can do, and others have done, is you can measure the amount of which they have been brought in as well as up. You can also do this using shatter cones, and the way in which shatter cones orientations work. That was done first by Willy Manton for Vredefort back in the 60s. So you get two different measures of the amount of inward as well as upward motion that has taken place in the central uplift. And you put those back. In the case of Gosses Bluff you have got pretty good seismic cross-sections (they have been published by the Australian press). You can reconstruct a very nice transient cavity on the order of 3 to 3.5 km deep for a crater whose margins are now around 22 km. That is consistent also with data from Sierra Madera, which is smaller, and again you have got a measure from the detailed mapping that has been done, of the amount of shortening in the material.

Sharpton: Let me interrupt you, but I think there is a semantics problem here, perhaps. What Robbie is talking about, and what I am concerned about as well, and I think what you are interpreting or using the word transient cavity to mean really what we conventionally call the excavation crater. So what you are looking at is the base of the excavation. That is what you really reinterpret when you reinterpret stratigraphy. What I think the modelers, and many of us have grown to look at as far as the transient crater, is the excavation zone plus this transient downward displacement that takes place as everything is pushed down in response to the impact event, but only pushed down transiently and it comes back up. That is very hard to reconstruct from geology.

Dence: I agree completely. You cannot reconstruct it from geology but it is in addition to the amount that you can reconstruct. And I thank you very much for introducing that, because it is what I want to talk about tomorrow, in part.

Osinski: I just had a sort of more general question, I guess. It is probably just because I do not understand it, but how, in this transient cavity, do you push the rock down, and where does it go to? I mean, how do you compress granite…

Schmidt: I do not think you push it down. I think you push it out and then it comes back. It goes further out, not down.

Holsapple: And then in the end it comes back less dense.

Melosh: The volume is almost conserved.

Overlapping voices.

Holsapple: Let me change the subject. I am surprised Peter is not jumping up and saying: Oblique impact. I mean, we know oblique impacts are a whole new ball game. We obviously get very shallow craters and they will still be circular. I never thought he would be quite that long, nor did I ever think I would defend his oblique cratering, but… The other comment I would make, at least for normal impact, is if in fact they do not go deep, then we are doing something terribly wrong with the modeling if it is a homogeneous material. If you put bedrock down there (or something like iron) then you can stop it. But if you have a relatively homogeneous material, every code calculation shows that it goes deep and then it comes back. So if it doesn't do that we are doing it wrong.

Housen: Well, as Pete [Schultz] pointed out earlier today, too, in things like sand you never see proportional growth.

Holsapple: I did not say it is proportional, but I say it goes deep.

Spray: Following on from that, as a geologist working in the field, although the codes and the modeling suggests this trampoline-type effect, I see no evidence whatsoever for it in the rocks, and I am concerned about that.

It is not elastic trampoline, it is a gravity effect [Schmidt in the background: the elastic part is probably 1%].

Spray: Ok, well the trampoline analogy perhaps is a little crude, but we need phenomenal damage of huge volumes of material in a bulk mode throughout the rock to do that, and, maybe some other geologist can comment, but our group in our work cannot detect that. Now, maybe we are not looking right, or the scale is wrong.

Ahrens: You just cannot detect it. I think a whole generation of people have been fooled by that. I think you said it exactly right: you cannot detect it.

Sharpton: But Tom, you cannot have a mechanism that is so fundamental and yet argue that it leaves no expression. That is unsatisfactory. That is when I say that it calls for a miracle.

Ahrens: There is fracturing. You see it in large-scale fracture profiles on the 20 to 30 km range at the Ries crater. You see a velocity deficit. So I think it is there, and nobody doubt that the Ries crater has a big uplift.

Sharpton: Oh no, there is no doubt that there is uplift, but what we are talking about, again now, is whether the uplift originate from something that can be approximated by the excavation crater depth, or uplift originates by something that is considerably deeper than that, because everything has been pushed down and it has to come up before the distance. That is the important thing right here.

Ahrens: A lot of serious and very conscientious geologists starting very early, like Jim Head was one of the first geologists who suggested that …[interrupted]

Sharpton: That is an anachronism. Let's forget about that. Let's talk about field workers. Jim [Head] was doing his best back in the 70s and 80s, but things have come a long way from that assessment. Let's look at what uplifts and what terrestrial craters tell us right now, based on geophysical and geological analyses, and see if it is consistent with what you guys [modelers] tell us should be there if you are fluidizing rocks, and you are pushing rocks down 30 or 40% farther than the excavation zone.

Turtle: I am wondering if one of the problems here may be a problem of scale. One of the things you said in your talk this morning: you had a slide of, I don't remember, maybe a 1-m scale sample and you said at that scale it looked like plastic deformation. But when you looked at the rocks in thin section it was cataclastic, it was localized fracturing. But on a large scale what it appears to be is plastic deformation. What I do not understand is why the same thing cannot happen on a much larger scale, where you have this crater excavation and on the scale of the entire final crater basin you have got fluidized motion, but that is represented as localized fracturing on a much smaller scale, which is what you are seeing in the field. So if this can happen at the scale of the slides that you showed today, why can't it happen at the much larger scale.

Spray: Because if you look at the displacement at those discrete zones, those discrete zones themselves would look plastic and homogeneous, not the whole rock, just those zones. They are cataclastic, actually, apart from the melt. The frequency of those zones and the displacement on those zones does not allow you to move the rock around in kilometers like soup. There is not enough of them in there to do it. And the rock would be virtually reconstituted, mineralogically. You can look down at thin sections between those shears and the bulk of the rock is as it was before. It may be shocked, but in terms of its cataclastical flow behavior….[overlapping]

Turtle: But in the excavation I am not talking about deformation in that small scale. The large blocks can stay intact, but they are moving against each other. I do not understand how you can actually determine how much offset there has been between these large blocks. You see the large blocks that are intact in the field, and you see faults or fractures between them, but how do you know that not much deformation has occurred along them?

Sharpton: You can follow geological boundaries, for one thing. At the Slate Islands (I will talk about it more tomorrow) there is virtually no disruption, and [there are] regional geological boundaries, major ones, across this 5-km wide central uplift. That is really hard to explain if you have done anything like fluidizing the rocks. These rocks simply have not been homogenized in any shape at all.

Turtle: That is not what I am saying, though. At the very large scale they behave as fluids, even though at the small scale they are not disrupted.

Sharpton: But somewhere you have to have a strain that on a 30-km crater integrates to a couple of km, right? So, where is it? That should be obvious.

Holsapple: Well, do geologists agree that some go deep? Is the problem only that you think some do not? You mentioned some craters, like Chicxulub. We know for example they did go down with the submergible in the big Pacific nuclear craters (I think it is a slide that Jay flipped by, probably because it is classified). After the fact they went by and did seismic and they found this nice bowl shaped crater, even though the remaining remnant was around 100:1 aspect ratio. In the middle there was clear evidence that it had gone down and broken up. Now, that is a very different geology, but my question is: Don't you see deep evidence in some craters, and it is only in other ones that you object to the idea?

Sharpton: I guess it depends all on what the definition of "deep" is. I mean, Chicxulub is a very large crater; we are still debating on its size.

Holsapple: Well, bowl shaped are generally 20 to 30% depth compared to initial radius. Then the radius may move out 20-30-40% and the depth comes up 2-to-1.

Sharpton: We are talking about something that may have gone down 30 or 35 km perhaps.

Stewart: Can you tell accurately the total displacement that the rock went through? We talked a lot today about how our strength models are not adequate in the codes? So the expression of the damage in the rock that you would see in the field, we cannot accurately characterize by looking at the models. But we do see the displacement. That comes out of all the energy conservation equations. We know things have moved, and come back, and been restored by gravity. Can you integrate the displacement in your observations in these shear zone and in the slips that you see? That is the expression that I think would be the closest link. The plastic deformation is something that is completely open right now. We do not know if everything will be compensated by fractures, or compensated by melting. It will vary widely, but the motion should be something we can predict. That is what I would look for in the field.

Spray: The point is that the motion that the rock should undergo according to the modeling, should manifest itself in a more chaotic history within the rock.

Stewart: We see [in the laboratory experiments mentioned by Schmidt] complete beautiful restoration of pillars and layers so it is not necessarily chaotic.

Schultz: I still wonder whether or not some of this problem is related to what you consider the zone that is going to be uplifting versus the zone that is going to be more lateral. All the codes show that in the vector of the motion of the trajectory you still have a trajectory that is preserved from the initial projectile. Laterally from that, that can be different. So I still wonder whether or not it is still related to the definition, and whether or not we are dealing with the rebound of the central structure of the maximum penetration, versus what is happening to the exterior of that central structure that may be more planar. I just think about Roddy's work on Flynn Creek, which is a classic example, when he looked and saw that it is really a flat line up until you get up to the central peak. That would be one way to take care of that. Some of this may be related to the sedimentary overburn.

Unidentified: The question is if the codes are wrong, what physics in the code would be wrong. The only thing I can think of is that the rock material at depth would have to be dramatically stronger not to form a transient cavity. But the level of strength that would have to be assumed not to be able to resist that, I do not think there is any evidence, and there is probably evidence that contradict it out there.

Melosh: I think there is a big confusion here between displacement and strain. There is a big difference between those two. Rocks may have gone down 5 km and come up 5 km, but the strain can still be small. You were seeing, from what you said, something like at least 2% strain in the rock. I suspect, if you look in the calculations, at what the basement is doing, not the ejected stuff that does get big strain, the strain in the basement for going down and back up, I do not think it will be much more than your 2%.

Herrick: Because the point of the workshop is actually to try to make some progress, I think a couple of things that would be nice to do, are: In terms of the field measurements, is to try to characterize in detail the nature, the scale, the extent of fracturing that is observed, and the extent of displacement along these fractures. In terms of modeling, the details of where individual horizons are moving and coming back, translating that to what to look for in the field.