Transcript of Session on Creation of the Structure of Complex Craters, Sunday, February 9, 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 Robbie Herrick.

SHARPTON, V.L., Dressler, B.O.: Excavation flow and central peak rings: Is there a connection?

Buck Sharpton closes talk with suggestions for further periodic meetings of a similar nature, and a general research program focusing on fieldwork for 20 - 30 km craters.

QUESTIONS/COMMENTS TO SHARPTON'S INVITED

Turtle: I was wondering if we could get an understanding of how pervasively damaged are the rocks moved during crater excavation by looking at the central uplift, because in a central uplift we know how far that material has moved, so that could be a constraint on how damaged moved rocks must be.

I think you're right, and I think we can do that, and we've done that to some degree and that has caused some of us to be reluctant to accept some of the mechanisms proposed for central peak formation. If your model has the central uplift being a sort of homogenized, completely destroyed kind of uplift, that's not what we see in the field.

Turtle: But that's exactly the disconnect here. The models are showing that this material has to have moved, but they don't show the fine scale deformation. They're showing on the large scale that the material has moved, but the models aren't requiring that on the very finest scale that the material is damaged. So, we can use the observations of how damaged the central uplifts are in the field to constrain whether the other parts of the crater can have moved to that extent as well.

Yes, that's the approach. By models, I mean not only computer models, but there are ideas like acoustic fluidization that Jay has promoted considerably, and what we [geologists] need is some understanding of what acoustic fluidization looks like at the scales of a complex crater. If you just look at the sort of rounded material that one gets from a long run-out landslide and you compare that to a central uplift...

Melosh: My observations in the field are that run-out landslides are not rounded.

I have also looked at run-out landslides and I believe there is some rounding, edges get knocked off the rocks.

Turtle: That's exactly the type of observations we need, because there's quite a bit of disagreement over what the weakening mechanism is that allows complex crater collapse to occur.

I think that's a good approach. The thing that I worry about with respect to Chicxulub is this "resonance effect" where there has been a feedback between modeling and interpretation that has not generated an independent evaluation that you would expect between seismic interpretation and model results.

Chapman: Buck, you showed a slide of the Chicxulub seismic data earlier, and as I understand it, where the horizontal layers disappear into this jumbled zone, that's the extent of the transient crater.

I'm just saying that was the parameter used previously [on other craters] to determine the extent of the transient crater from seismic data.

Chapman: From the discussion of the first day, modeling might suggest that those horizontal layers may have moved and then come back into place. The issue here is that the mesh size of the modeling is at too coarse a mesh size to relate to the damage seen in the field, and there's no information at the sort of medium scale that's coarser than what field geologists see, but finer than the modeling. I was wondering if there wasn't the possibility for the modelers to focus in, take a small part of the problem and model just that, say the central uplift itself, or the

edge of the transient crater, and learn what deformations might be experienced by a small piece, and see if that jumbles things at the resolution of a seismic layer or not.

I think that's a good approach. The thing that I worry about with respect to Chicxulub is this "resonance effect" where there has been a feedback between modeling and interpretation that has not generated an independent evaluation that you would expect between seismic interpretation and model results.

Holsapple: There are two areas I'd like to comment briefly about. First, there's this idea that rocks are brittle and you can't put them back together. From a modeling viewpoint, and we've just recently gotten this into our codes, rocks can be fairly ductile if a little pressure is applied to them. Rocks under as little as 1 kilobar can deform up to 10-15% and it come back without breaking, so it's not necessarily brittle. You mentioned high strain rates, but the strain rates get very small when you get to these large structures. So any sort of lab experiment where we do an impact and get some sort of brittle spall, that's not what you should expect in the field.

Okay, this is a good discussion point. Let me address that before you continue. You may be right that during the compression phase you push them down under a great amount of pressure, but then you unload them, and that's the serious issue there. You might have rocks that can absorb a compressional strain, but when you unload a limestone it's very difficult to not have it fracture.

Holsapple: You're removing the compression but it's not going into tension.

How do you get the rock up to the surface; tension is implicit in these models.

Holsapple: That's something we could easily answer by looking at the codes, and we ought to do that.

O'Keefe: [Comment not audible on tape].

Ahrens: I think one route to understanding these structures is to map out the deformation and tie that to damage. I think the seismic data is very useful for understanding the state of the rock. We have very good seismic data for Meteor crater, Reis crater, and recently Lake Bosumtwi, and the severe zones of velocity deficit that most people would agree is crack damage can be identified and tied to the models.

I agree that there are fractures within the crater, but there are fractures outside the transient crater as well. You have pressures enough to fracture rocks that don't require that those rocks go through deflections of kilometers.

Holsapple: If I could just briefly make my second point. The point has to do with this issue of late-stage readjustment. We now have two mechanisms that can do that. We have acoustic fluidization, which has been promoted for some time. More recently, now that were putting in much better damage models, strain localization gives us exactly the same late stage.

With respect to strain localization, it would be very helpful to get from your group an understanding of what we would expect to see in the field from that.

O'Keefe: With respect to seismic tomography, how many distributions of faults can you come up with that are consistent with the data and what's the degree of uncertainty. You've said that their interpretation [with respect to Chicxulub] can be looked at in a number of possible ways. Can you show us the number of possible ways one can interpret that set of data? What's the degree of uncertainty and ambiguity?

I don't think there's much ambiguity about the location of the major faults at Chicxulub. I think the ambiguity revolves around what significance you place on them. Those outer faults are the ones that are transcrustal. There is no evidence that what has been listed as the basin ring is a transcrustal feature at all. It may be that it is, but we don't see the evidence because that's getting down into the zone of the crater where it's slushy and we don't preserve an acoustic signature of the fault. I'm not arguing that, what I'm arguing for is maintain an open mind.

[END OF TAPE. Appears to be at end of discussion for Sharpton's talk, taping resumed during Gareth Collins talk].

COLLINS, G.S., Turtle, E.P.: Modeling complex crater collapse

Gareth Collins ends his talk with a few suggestions for future work. He indicates that models of complex crater collapse make predictions about where zones of maximum deformation are located and where the melt has gone, and these predictions should be tested.

He would also like to see a program developed for benchmarking a variety of numerical codes both for early-stage and late-stage impact modeling.

QUESTIONS/COMMENTS TO COLLING'S INVITED

Spray: I think the geologists are looking at craters in a different way from the modelers in terms of how we define the dimensions of the structure. I think a lot of the modelers focus on the generation of the transient cavity and its demise, while the geologists look well beyond that to the maximum damage evidence, which can be twice that diameter. So, there is a confusion about what the diameter of an impact structure is. In the list of terrestrial impact craters that we manage at the University of New Brunswick we report the full maximum damage diameter of the impact crater and not the transient crater diameter or the collapsed transient crater diameter. So, there's a nomenclature issue. The other issue that this leads to is that the damage regime for different parts of the crater are different. When I see a projectile coming into a homogeneous target in the sense that, as I understand it, the block is treated as having similar properties, I have grave reservations with that. Because, what we have from field data is that there are different deformation regimes radially outward from the impact point. I'm talking about the whole thing now, not just the transient crater. If it is possible for you to put in, after the shock wave has passed through the rock, different damage intensities at different radii, then I think that would help considerably. But I would stress that bulk fluidization is a problem for many geologists. We do look at craters at a variety of scales, and we are trying hard to understand the damage behavior as well. The other point I would like to make is that central uplifts behave very differently from the bulk of the impact structure, and I say that because the damage that you see in the central uplift is very different from what you see outside it. The central uplift is highly chaotic in the field: there's a lot of faulting, a lot of damage. It is the only place where you see evidence for what might be called bulk fluidization. So I wouldn't use central uplifts as a guide to how the rest of the material behaves. So there are a number of issues in terms of material properties with different radii from the center of the structure, but I don't know how easy that is to model.

If I could comment on that, I agree with your first point. In terms of damage, we can put different damage regimes into different parts of the target, but unless you have strain localization in your code, then it's impossible to model any kind of faulting. So we need to approximate the bulk effect of what that faulting is by weakening the target out at a particular radius. I agree that where acoustic fluidization may play a role is in the central uplift. When you talk about the central uplift being very jumbled with lots of strain, that is what we see in the models. But lower down, strain is much, much less, of order of a percent or less.

O'Keefe: That was a very nice talk. Let me comment on a couple of comments that were made in the audience, and that was about damage. Damage in the codes that we've done is calculated self-consistently so that as the shock goes through the material you have a damage distribution. Also, damage there is the total integrated strain to failure so it is dependent on the local displacements of material. I wanted to make a couple of comments on material properties. Material properties are measured in the laboratory and there's quite a body of information, including the brittle-ductile transition. The biggest uncertainty is after you damage, after you start to soften, what are the material properties? There's data on fault gouge from earthquakes, which have a low coefficient of friction. There's also experiments that were done on brittle materials where they shocked them and measured the dynamic coefficient of friction, and that was 0.2. So what we did in the models was to let it vary from 0.65 and let it damage down to 0.2. Another point is that when we looked at what are the material properties that matched the transition crater diameter for the terrestrial planets we found that the damage had to go from cohesion of ~1kbar to very low values, and the coefficient of friction had to vary from 0.6 down to 0.2. That's where you matched that very broad set of data. I also wanted to mention something about peak rings. Peak rings, once you put bulking in the model, then that pushes up that peak ring, so that feature would grow.

A coefficient of friction of 0.2 might well be measured for a powdered granite on a fault, but I don't think the entire central uplift can be characterized that way.

[A discussion between Melosh, O'Keefe and Ahrens not entirely audible on tape ensued about whether or not materials appropriate for craters really get significantly reduced in their coefficient of friction under significant pressures]

Dressler: This is a comment on what John Spray

said earlier. He said that central uplifts are strongly damaged, but this is not always true. You have very damaged central uplifts like Slate Islands where we estimated ~20% of the material was matrix breccia, but in Manicouagan material is heavily shocked but is not heavily deformed. It appears to be uplifted as essentially a single block. So there is a whole spectrum.

Dence: I just wanted to reinforce that. You can walk for kilometers at Manicouagan on the anorthosite and trace 6" wide mafic bands right across, so it's clearly moved as a few large blocks, and that's true in other places as well. The other thing I'd like to talk about briefly is the distribution of melt, because here I have some difficulty. I like a lot of what you've said and I'm very intrigued by the collapse profile of the central peak because I think it's very suggestive of what we may be able to put together. A few well-placed drill holes and seismic profiles could help a lot on these intermediate-sized craters. The melt, though, somehow we have to move it faster, it can't stay in the middle in my mind. As soon as the central peak starts to rise it drains the melt off and some of the associated breccia off to the sides and I don't believe it stays anywhere near the center through most of that excursion of the center. Otherwise, I think we would see more evidence of melt in the center. In Manicouagan for example, the bulk of the melt is now off in what you would call a peak ring. Maybe because it's just much less viscous than in your model.

The melt in the model in nearly inviscid, but the reason it stays there is because it's basically in free fall.

Herrick: [referring to slide from Chicxulub section] I wanted to briefly cover the constraints imposed by seismic data in response to an earlier question. If you look at the Chicxulub section the entire interpretation of a "mushroom-shaped" central structure is based on matching packets from this deep set of reflectors to higher, undisrupted reflectors outside of the "transient crater" on a single line. There are constraints on the velocity structure provided by a couple of tomographic data sets, but these are at a very low resolution.

Spray: I agree with Mike and Burkhart that some of the central peaks appear to have come up sort of like a piston, but the point I wanted to make is that the only place we see evidence for what could be considered fluidization is in some of the other central peaks. The other point I want to make is that Chicxulub is a bad structure to model because we cannot ground truth what the modelers do. We need a structure that geologists can actually crawl around on.

OSINSKI, G.R., Spray, J.G.: Transient crater formation and collapse: Observations at the Haughton impact structure, Arctic Canada

Osinski closes by summarizing observations at Haughton crater. Gravitational collapse occurs along interconnected radial and concentric faults. Deformation is brittle, and it occurs along discrete fault zones. Even though there is a lot of faulting, especially in the crater rim, there's no evidence for internal deformation.

QUESTIONS/COMMENTS TO OSINSKI'S CONTRIBUTED

If I could begin with a few comments of my own. I agree that Chicxulub is not a particularly good crater to study to constrain modeling. There are a lot of 20-30 km craters which have not had a lot of work but for which you can get some very nice results if you go out and map them in detail, and give some better constraints for modeling.

Sharpton: You alluded to possibly making a modification of the final crater size. As you can see the 24-km size estimate comes from a seismic line that shows faulting out 12 km from the center which matches our field observations, but if you go in a different direction the faulting is only out 10 km. So, 24 km is still a good estimate, but it might be less if you averaged things around the structure. The faulting extends a lot further out east of the structure.

Abbot: I had a comment and a question. This reminds me of a paper by Brian Whimbley [sp.] where they were looking at the accretionary prism associated with the Japan trench, and it turned out there were a lot of invisible faults there that were only found by doing micropaleontology, and I was wondering if something like that was a possibility with this crater.

I'm not sure I understand.

Abbot: They were all thrusts and you really couldn't see them, but when someone did micropaleontology people discovered that all these different sections were a different age.

There are a number of microfaults that you can really only pick out at thin section scale. There are probably more than we have mapped.

Schultz: I'm really intrigued by the asymmetry in the fracturing; you know where I'm going. To the south, is that really a missing section of faulting. Is this an area you didn't get a chance to look at, or you just don't see much faulting right in there?

Nope, I pretty much covered the area. Definitely out here there's less faulting.

Schultz: Towards the north you see an offset concentric structure. Is there a timing offset of that structure relative to the other faulting?

Timing is very difficult to determine. The radial faults do act as transfer faults between the concentric faults.

Schultz: Oblique impacts into sedimentary targets "remember" the trajectory because of the record of peak stresses experienced. Uprange you see preexisting faulting preserved right up to the rim, where as downrange you'll see some faulting. It would be really interesting to see the relative timing and see if the projectile direction could be determined.

It's not quite that simple because the number of faults mapped does not entirely correlate with the amount of deformation and displacement [Points to map and shows examples].

Asphaug: Do you have an estimate of how much strain is taken up radially vs. azimuthally?

Radial faulting does play a big role. They probably do form early on and they take up a lot of strain.

Plescia: Some gravity data we collected a few years ago indicated the central peak might be an incipient peak ring, and I was wondering if you saw any of that in the field relations.

[Points to some items on the map] Summarizes that there might have been a structural uplift, but a topographic peak ring would not have been visible after time of crater formation.

Dence: Two things I'd like to bring attention. One is that it is comparable to the Ries and yet it is in a much greater thickness of sedimentary rock. Yet, the crystalline basement is involved. Some of the ejecta is heavily shocked, but there is not a lot of melt in the ejecta. The basement is involved and yet the central peak does not significantly involve the basement but instead is made up primarily of the material about ¾ of the way down to the basement. The other thing to point out is that this crater is about half the depth of the Ries, and that probably has something to do with the target lithologies.

MCKINNON, W.B., Schenck, P.M., Moore, J.M.: Goldilocks and the three complex scaling laws

McKinnon closes with plot showing suggested constraints on the range of possible reconstructed transient cavities, existing scaling laws, and, for lunar craters, data points of reconstructed transient craters versus final crater.

QUESTIONS/COMMENTS TO MCKINNON'S CONTRIBUTED

Unidentified female: What are the diameter differences in the Copernicus plot that you just put up?

I am not sure, it is in the abstract. 70-79 km is the range. It's an oddball, it is off this trend. However, Jeff Moore has recently finished mapping a similar-sized Copernican-aged crater and it is around here as well, but I don't really want to go too much into the new data while we're still working on it.

Unidentified male [Tom Ahrens?]: Why does it need to be a straight line?

It doesn't need to be a straight line; Keith Holsapple pointed that out in his review article. A power-law fit is a convenience. The point is, these rules that propagate through the literature have to be benchmarked against real data points.

Collins, G.S., TURTLE, E.P., Melosh, H.J.: Numerical simulations of Silverpit crater collapse: A comparison of Tekton and SALES 2

Turtle summarizes talk by saying that Tekton and SALES 2 give similar results within the limitations of each program, and those results are consistent with the basic observations for the Silverpit structure. Future modeling efforts will utilize the particular advantages of each code.

QUESTIONS/COMMENTS TO TURTLE'S CONTRIBUTED

Schenk: I seem to recall there being some questioning by others as to whether or not Silverpit was really an impact structure or not. Do you have any comments on the geology of the structure and whether or not it is an impact structure? There aren't any samples are there?

No, it was identified with seismic data. This was presented by Phil Allen at the Impact Tectonics conference in Sweden, and the evidence was sufficient to convince everyone there it is likely to be an impact structure. The central peak and the terracing outside have the morphology of a complex crater. We don't have samples, though.

Koeberl: Two comments to that point. You can't confirm that it's an impact structure through seismic. There are a couple of drill cores outside the main structure. I've been in touch with Phil Allen on that, and they're trying to identify anything that might be of use for that. Right now they are looking at the samples for paleontological use to try and stratigraphically age date the structure. One of the problems is that the structure is in a thick sequence of carbonates and it is difficult to find any silicates to look for shock deformation, but they are working on it.

Osinski: Do you find that the numbers of radial structures that you're predicting match what you find in the North Sea?

We're not predicting at this point the number of faults. Really, at this point we're just making sure the modeling programs are doing the same things in the region of interest, and then later we'll put in faults and watch how that affects the stress field. We haven't made predictions. The region in which we see extensional surface stresses is consistent with where the faults occur which is encouraging. The stress magnitudes are fairly low, and that's one of the things we'll be investigating.

Osinski: I was just wondering if you might expect to see radial faults on that scale of reconstruction. On the screen there would you not expect to see some major radial structures?

We're predicting concentric faulting there.

Osinski: You did mention an area of strike slip.

Yes, there is an area of strike-slip in there, and we do see some of that, consistent with the models. But the models are exceedingly simple at this point.

Holsapple: I have a question about the modeling. You mentioned that one has faulting, and the other doesn't. You mean you have some model where you put in a fault.

In Tekton you can put faults in.

Holsapple: Let me make a comment that relates to the acoustic fluidization talk also. The strain-softening model, you don't have to put in faults, it predicts faults. The suggestion that he said that we don't have any strain softening in the model, if you put in the constitutive relations, gives you strain softening and localization. There may be a numerical problem with calculating it because on a continuum basis it ends going down to something small. But, in fact it's in the model already, you don't have to put it in, and I think that's what Dugan was doing. The faults he got you might have to turn off, I don't know if he did, at least the model has that kind of physics built into it. We have to learn how to calculate them efficiently, but it's not something we should have to add after the fact.

The area we're most interested in this case is outside where acoustic fluidization or strain softening takes place, but you're correct that the faults have to be put in a priori.

Gisler: With respect to the Sales calculation, how long in problem does it take for your model to reach equilibrium, and were you satisfied that equilibrium was achieved?

I'll defer to my coauthor, Gareth Collins.

Collins: It depends on the viscosity you use. I've done some test calculations using a collapsing hemisphere of water and comparing it to some analytical results with water, and the code seems to work pretty well.

Dence: You might wish to look at some subfield experimental station data of 500-ton explosions into alluvium that Gareth Jones and Dave Roddy worked on some time ago, it bears some similarity.

Melosh: On this strain-softening business, Tekton does have a strain-softening option, Zibi didn't use it,

Andy Freed did, and we found it was incredibly destabilizing. And that's something I have a question for Dugan about, I suspect those deep-seated faults are numerical instabilities. Let me emphasize an important point: strain softening amplifies instabilities, and if you use it you have to very careful to tell the difference between real faults and those that are artifacts that are introduced by this constitutive relationship.

PLESCIA, J.B.: Application of gravity data to understanding impact mechanics

Plescia ends talk by stating that gravity provides information on the surface and subsurface density structure, and he summarized some of the constraints on impact structures that gravity provides.

QUESTIONS/COMMENTS TO PLESCIA'S CONTRIBUTED

Collins: A lot of complex craters have gravity highs in the center, don't they.

Generally that's true because you're bringing up material from depth that's denser. However, that's not true if you bring up low-density material from depth. For instance, in the Connelly basin the ring actually has a gravity high, because it is filled with higher-density sandstones than are currently exposed in the central structure. So this is a simple model, in reality the data are more complicated.

Newsom: I've looked a little bit at what's available for Mars and there's a wide variety of behavior of structures in the gravity data. Do we have gravity data for the moon at sufficient detail to look at some of the large structures?

Yes, we have the Clementine data. For orbital data you're limited to a resolution at roughly the scale of the orbital elevation, so you're looking at crustal-scale phenomena that involves things like mantle rebound.

*Dence: The aspect that I would put some emphasis on now is the **a**bility of the anomaly to give you some feel for the fractured porosity of the material underneath. The intriguing thing is that a large amount of fracture porosity in the craters up to ~20 km across and a simple distribution in a bowl-type configuration. Above that, things flatten out and you get more complexity in the center. Apparently, the centers seem to generally be relatively nonporous. The fractures tend to seal up. Manicouagan is a good example of that. That structure has essentially no gravity anomaly over the center.*

It clearly depends on the geology of the site and what's happened post-impact.

Impromptu presentation about strain under craters by Melosh

Jay highlights that the models cannot distinguish about the type of strain within a cell. Shows a model that shows that in the end the total strain is a few percent. So there may be more agreement between models and observations than one might think.

GENERAL DISCUSSION:

Spray: I agree that at the scale of observation you've shown there's not a problem. What I'm hesitant about is that the rocks have gone through a lot of movement to get that end state of minimal strain. The issue is that the internal structure shows a high degree of fidelity in it for rocks that have undergone a lot of total movement to get to the final state.

Melosh: Differentiate strain from displacement. All a local chunk sees is the local movement relative to its neighbors. All we know from the outcrop is the end state.

Spray: I agree with you, we don't know the total path history from outcrop. But, I think what the outcrop is showing is that the material is more ordered than we would expect from a complex travel history.

O'Keefe: The damage plots are really integrated strain. For a typical case the map looks similar to Boris's. You have this bowl-shaped crater and then you have a long extended zone over the surface. The typical values there of integrated strain to failure 10%, with the top zone of 2-3 percent. If you want to get displacements, the maps of the particle histories are interesting.

Melosh: All I've shown is integrated strain. We have not plotted strain as a function of time for different points, and I believe after this conference we may go back and do that.

Sharpton: You would agree that those values would be larger than what you've plotted.

Newsome: A thing to keep in mind is that as geologists in the field you gravitate to the larger intact blocks, and so there is some bias in the field observations as well.

Herrick: All the talks we've heard have discussed complex crater in terms of first there being a State A, a transient crater, and then a State B where it collapses to form a complex crater [drawing of this on board]. Is this really a valid concept, given that forming state A takes a finite amount of time to form?

Melosh: We don't do that. These are fully dynamic simulations, it may be that the floor starts coming up while the rim is still expanding. That was suspected for a while and is now seen in these codes. Nobody believes that you have a static crater that then collapses.

Holsapple: Jay's absolutely right, you do the entire dynamical process.

Herrick: Well, what happens to the excavation while the collapse is starting?

Holsapple: You have ejecta still coming out, you have the radius still growing, you have the floor coming up, generally a continuous transition.

O'Keefe: At the time of maximum of maximum penetration you have about 20% of the kinetic energy still in the flow field. So, you have to put that in if you're going to start from that point. You have to put in the geometry of that flow field and do an energy balance. So no one starts from a static model much any more.

Holsapple: It's been suggested during the workshop that the physics of different stages of crater formation are different and should be modeled with different codes, but I don't really subscribe to that and I think we should go all the way through the process, and generally that's what we do now.

Turtle: It would be brilliant to have one thing that does it all, but right now we don't have that.

O'Keefe: I think we can come pretty close.

Asphaug: I think it's kind of interesting about how we've progressed from the very first talk of the workshop cautioning about how we play with numbers and look at pretty pictures to one code that does everything. You can easily get fooled from some assumptions that go into the code very early on, such as porosity. For example, you can have an assumption that crack growth is half the sound speed, which is okay early on but not valid late in the calculation. So, you've got to be very cautious and conservative.

French: I've been fascinated by one thing that has come out of this conference, and that is the importance of the zone immediately under the excavation zone. It's the zone that either is or isn't driven down to form the bottom of the transient cavity, and it's also the zone from which the upper part of the central uplift develops. Two responsibilities that develop out of this: For the modelers, what will this zone look like when it is put through the codes, how much deformation do you need before the model will work. For the geologists, we need to look in detail at what's preserved in this zone and quantify things like the degree of brecciation and the amount of fracturing. I think if we do that we'll have a more common ground to argue about.

O'Keefe: I agree with Bevan [French]. For the various values of damage it would be nice to know the rock fabric. We have strain measurements, but that's not the whole story.

[Inaudible comments]

Unidentified: Buck Sharpton and John Spray have been mentioning that the rocks look like they are in "good shape", which is a somewhat qualitative measure. Whether the energy is partitioned into shock features and fractures is something that doesn't come out of that. In terms of shock features, there are huge variances over short distances. So how to measure that in the field and how to quantify what kind of energy these rocks have been through is not that easy. So, I think we should think about how to evaluate energy partitioning geologically in order to get that together with the modeling.

Spray: It's not just a matter of looking at rocks in the field, a lot of detailed analysis is required in the lab. You've got to not only get on the microscope, but also the SEM and the TEM. The levels of scrutiny required are pretty major, it's a lot of long-term work.

Dence: We have cases, Gosses Bluff, for example, where you have a pretty clear idea of the displacement that has taken place. There are

quartzites that have moved several kilometers, they are shatter-coned and shock damaged, and yet they are still massive intact blocks. We have 3-5 km into central uplifts of Canadian craters that have only been skimmed. My gross observation is that the amount of fracture damage does not correlate with the level of shock. It has more to do with late-stage movement and how damaged that rock becomes during formation of the central uplift. The extent to which it moves as blocks of different dimensions is partly a function of lithology and partly a function of the gross structure. All this has to be sorted out, and we've hardly started.

Ahrens: Gosses Bluff is an interesting case in that you have rocks that have been shocked just above the Hugoniot elastic limit, and yet there are huge displacements. I think the picture you come away with is that the rocks were processed and shatter-coned just above the Hugoniot elastic limit, and yet during rebound and making this beautiful cathedral in the center of the structure there was massive localized sliding of big blocks, so it's really true that the rock is very solid where you see it. But, where you don't see it where the weathering has taken it out the fault gouge, there was huge amounts of sliding to bring the rocks up and make it vertical. So, I think you have strains at different size scales. If you looked at the material on a small scale you might not see much deformation in a hand sample, but if you looked at units on a seismic scale you might see a velocity deficit due to a region of intense faulting.

Hörz: I had my hand up when we were talking about micro-fracturing as a function of shock pressure. If we want to muddy the waters even more, when we were doing experiments on single-crystal dunites, where you have a map of fracture density that you generate as a function of pressure. It has a high around 20 GPa and it decreases with increasing pressure. We see this in many naturally shocked rocks too, the most intense fracturing at low shock pressure and where you have maskelinite feldspars formed at pressures over 35 GPa those are generally not that fractured.

Ahrens: You don't have healing?

Hörz: The temperatures are generally higher there but I don't think it's annealing. You yield in a different way and there could be melt in what would be fractures. My point is, though, that fracture density alone is not necessarily a good indicator of disruption. Coming back to central uplifts, I think what we can say is that many, many central uplifts are undeformed in the sense that they are stratigraphically coherent layers. Clearly the center of the central uplift is deep-seated material and as you go away from the center radially you get shallower and shallower units. But, each individual element can be fractured quite a bit. For instance, shatter-coned units are intensely fractured but they are in a coherent block. So, by and large central uplifts are stratigraphically coherent but they can be intensely fractured. There is a big difference between fracturing and being chaotic and being fractured and coherent, and we see the latter.

Schenk: I hear people like Jay [Melosh] and John [Spray] rethinking how they might go back after this meeting and relook at some of their models or data, and it might good to reconvene in a year or 18 months and see if there are any surprises.

Herrick: A good thought, and this afternoon I hope to talk some more about future plans.

Stewart: Two other things to incorporate in the future. There's clearly some work on rock mechanics that needs to be done, and I wonder if this group should be expanded to include more experiments to try and validate models and that includes upgrading equations of state and not just constitutive models. That hasn't been addressed at all.