Transcript of Session onEjecta Emplacement and Oblique Impact Effects, Saturday, February 8, 1:50 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.

ARTEMIEVA, N.A., Pierazzo, E.: Oblique impact and its ejecta – Numerical modeling

Artemieva reports results of the investigation of oblique impacts using full 3D hydrocodes. She concludes her talk by pointing out the need for further investigation of oblique impacts to better constrain scaling, melt production, projectile fate, and ejecta emplacement, particularly distal ejecta. Although more work is needed to improve the models, encouraging results have come from the investigation of tektite distribution and ejection of Martian meteorites. One field that needs particular attention is comparison with experimental data.

QUESTIONS/COMMENTS TO ARTEMIEVA'S INVITED

Koeberl: Natalia, when you showed the tektite ejection, from which depth did the material that melted come?

Especially for Ries we produced this 40 meters thick layer above the sandstone, and we consider explicitly the melt from this layer as tektite material. It is not geologically justified, and moreover we do not have this permanent layer everywhere at the impact site. Maybe it is more like spots or regions of sands, but it should be there. So we produce tektites only from this layer.

Koeberl: There is a follow up question to that: When you show the distribution of the material, you said you only showed the tektite distribution, but how much material other than what you call tektite shocked rocks, was material from a greater depth that is actually mixed in with this?

First of all, to show you all material deposition I need to model the crater much more accurately. Here I show you only distal ejecta, which is ejected pretty early from the crater. To show you deposition of shocked material, or simply solid material from the crater we need to model this late ejecta. It means we need to model crater with strength, and we do not have it in the code, yet. But, for example, I have deposition of projectile material, which also escapes pretty early from the crater, and it is not similar to the tektites, but still should be there. So we should also find (in the field) projectile material, and I do not know why we could not find any projectile material in the same place. Maybe we have much smaller glasses, I do not know, we should discuss that. But I have deposition at least for the projectile.

McKinnon: Very impressive. If you go to normal incidence, do you still get meteorites off of Mars?

Not me. But in the paper by Head, Melosh, and Ivanov, published in Nature, they have some material ejected with escape velocity from the vertical impact. In my modeling I do not see it; at least, it is much less material for the vertical impact than for oblique impacts.

McKinnon: Ok, but you looked at that specifically.

Sure.

McKinnon: So from your code you would imply that obliquity was a necessary condition.

Obliquity is, every time. We have no vertical impacts. I have the distribution of ejected mass with escape velocity versus the impact angle. I have maximum for a 30 degree impact (from horizontal).

McKinnon: I have a second question. Earlier in your talk you talked about the crater that would not stop growing. Did that calculation have strength in it?

No it was not strength, but it was a really large crater, so gravity should stop the crater growth. Maybe we should have some correction to the volume if we include strength, but strength is more important for smaller craters.

McKinnon: I was just suggesting, actually, the best way to try to get rid of that problem was not to have strength in it, to just use water or something like that.

Stewart: The mass of meteorite material ejected in the oblique impact, how does that compare to the Head, Melosh, and Ivanov mass.

I have no comparison with the data by Head et al.

Stewart: Can you speak to the probability of meteorite collection being representative of this process?

As to probability I also try to model small craters, because from the viewpoint of variety of Martian meteorites and from the Martian statistics we should have a rather small crater to produce this Martian meteorites. I also try to model craters, which are between 1 and 3 km in diameter. It is no problem to produce meteorites from huge impacts, but I have no comparison with mass with mine and this 2D modeling.

Osinski: Just going back to the Ries: based on the distribution of carbonate melts, Günter Graup, if I am not mistaken, suggested an angle from the North-West. I was just wondering if you can comment…

No, I discuss this problem intensively with Dieter Stöffler, and we agree that it is more probably a direction from the West to East.

Osinski: There is a very asymmetric distribution of the carbonate melt there.

We should check maybe. But are you sure it is the total geologic data you have, that maybe this asymmetry is not due to lack of geological data in some region, I am not sure. But at least now we reproduce this fan of tektites.

Holsapple: I have two questions: Earlier on you were talking about porous layers. How do you model the porosity?

I did not model porosity, really. It is simply like the snowplow model. We simply use another melting point for the porous material. So it is not modeling for porosity.

Holsapple: It is not really porous, it is low density.

It is low density and a lower melting point.

Holsapple: Second point: Obviously, all of these ejecta stuff are directly tied to the initial ejection velocity. Have you compared your code to any experimental results of ejecta velocity?

I said no. Just now no.

Gerasimov. Natasha, I think it is to simplistic to say that when you showed the calculations from Betty Pierazzo paper that there is 50% vaporization and that is why the projectile material has to escape from the crater. The process occurs during the decrease of pressure and before it acts as a melt, and it also precipitates in the cratering process, there is a lot of time to be mixed. I think it is not so simple to say this. The question is the degree of mixing. Another comment: I see a problem with tektites if the model has a piece of surface material that is immediately ejected, because tektites show rather high devolatilization degree. Devolatilization, that is a lot of volatile elements are lost.

Yes, sure. And I tried to explain to you what maybe happened.

Gerasimov: That means that there was very high temperature before.

SCHULTZ, P.H.: Atmospheric effects and oblique impacts: Comparing laboratory experiments with planetary observations.

Schultz discusses the characteristics of crater ejecta, especially on Mars, and the experimental work done in trying to understand atmospheric effects and decouple them from target material (volatile content) effects. In terms of oblique impacts, he discusses the results of experimental work, which can now capture the entire evolution of the curtain, and track projectile ricochet, especially for low angle impacts.

QUESTIONS/COMMENTS TO SCHULTZ'S INVITED

Plescia: Pete, for the model you had for the rampart craters on Mars: Does it follow that those terminal ridges then are fine grained?

No, the terminal ridges should be coarse grained. Those are basically saltated larger fragments. Think of this as a torus of material that has a very high circulation pattern, and is capable of entraining finer

material, but it cannot really sustain the coarse material. So what you really find in the terminal should be coarse material. In fact, it is kind of interesting: When you look at craters when they get differentially eroded on Mars the terminal rampart is typically the thing that survives the best.

Plescia: Is there a size correlation with the size of the crater based on the amount of wind?

Yes, and I think eventually when it gets too big that terminal rampart no longer can sustain itself, because I think the vortices get too strong, the entrainment becomes too complex, and it collapses into a matrix supported debris flow. So it completely changes in style. That is, again, the issue that if you go up in scale you are dumping more energy into this turbulent flow, and so it moves out to greater distances and entrains much more material.

Chapman: Pete, if I properly understood your graph on the rampart craters, the atmospheric pressure, although lower than 1 bar, were really much higher than the present atmosphere on Mars. The scale height of the atmosphere on Mars, really should not give you much in the way of changes in vertical elevation, I would not think, regardless, but my real question is: How would you propose to distinguish between these atmospheric effects and the more traditional interpretation of these craters.

Let me first answer your first comment: I think the point behind this is that it is not atmospheric pressure; I mean, if we did atmospheric pressure at 6 mbar in the laboratory, the only thing we would be reproducing is a crater with a 6 mbar pressure with a quarter inch projectile. The issue is not that; the issue is that it is combination of pressure, density, and the grain size of the material that has been entrained. This when you scale this up works in terms of the ambient pressure that you see. Now, if you think about: can you distinguish between the two? I think you need to do the type of work that Sarah [Stewart] has been doing as well as Olivier [Banouin-Jha], at looking at these different stages. My bet is that the component that is going to have most likely the volatile component is going to be in the inner ejecta facies. Because if you go to higher velocities, if it is water it is going to atomize, as soon as it hits whatever residual atmosphere. If it is ice it will behave as comminuted material. I think the key may be in the inner facies. On the other hand, it turns out I have done experiments where I tried a sort of scaled atmosphere, where we used dry ice vapor to simply fill in a target. So we kept the dry ice vapor sort of filled up so it creates a sort of artificial scale height. Now the interesting thing of what this artificial scale height did it actually enhanced the rampart formation, because you actually were now including more of that vapor component. That is why I think it is going to be more interesting when you begin to start including some of these other effects, which I do not think we can realistically do in the laboratory, unless we do components of it, clustered experiments, for example.

Stewart: Just to speak to some of the issues about ejecta, you thought that Earth craters were hard. Mars ejecta blankets are a complete mystery. What I found in trying to model the water hypothesis is that it cannot answer all the questions either, by itself. There is something that you did not talk about, that the atmosphere could or could not do, the lobes, the different layers, the thickness, the differences between the inner and the outer ejecta blanket, I am leaning to the point where both processes are probably at work, and were acting on different parts of the ejecta blanket. And I still think that it is an open question on the rampart ridge itself, mostly because the atmospheric experiments that you have done so far have been limited. What I want to see is the same experiments, but, say, in a dry ice/sand mixture or something, where you may get some vapor plume as well, because in the simulations I am doing the vapor plume interacts with the ejecta curtain, and that makes everything more complicated. Now we have a third process going on.

Yes, except one interesting thing, and this is the nice thing about Mars we have, is if you look at oblique impacts, and look for ramparts or for lobes, and we know that the vapor plume will have a very strong component that goes downrange, we actually see the rampart toward the uprange side, which suggests to me that it still is related to the curtain moving out, rather than simply the interactions. On the other hand, I thing that you are right, especially at the high latitudes. And that is why you can see that these things are running out much farther than they should, and I think that has to incorporate a volatiles component.

McKinnon: Peter, in any of your low angle oblique experiments do you ever see the ejecta spray come out as flying V as in the calculation we saw yesterday?

Yes. In fact one thing I did not really show: What we

do in looking at the fate of the projectile is there are two ways. One, we actually have high speed imaging, looking at the witness plates, so we find out at what time it arrives. When you do that, you find that there are different components that arrive; there is a V component, there is also a vertical component, and there is a late arriving component that is higher up. We can use those to deconvolve and figure out where that stuff is coming from. The other question is: do you get a V, which is separate. We do get Vs, and this V-shape changes as function of impact angle. What I think those Vs are, based on what we see when we isolate the ricochet component, it is the sides of the projectile that are coming off and going downrange. The reason I say that, it is that when you look at these scour marks you can find that this is again dominated by the projectile component. So it is not just simply the target that is creating that V. So we see the V, but a lot of it is controlled by the projectile, and when you start doing the game with the sand, then it becomes different. We have actually done this PIV to be able to see it. It is more complicated, because then we do see this problem with the interaction with volatiles. I think the way to do the volatile issue really is to try to use the PIV system. That is much more effective than doing it this way.

HERRICK, R.R., Hessen, K.: Constraints on the impact process from observations of oblique impacts on the terrestrial planets

Herrick presents results of a survey of impact craters on the Moon, Mars, and Venus, to characterize the obliquity of the impact from the ejecta blanket distribution, and differences in ejecta pattern for the various planetary bodies. Results suggest no change in depth/diameter ratio or wall slope with impact angle, with similar percentage of highly oblique craters on all three planetary bodies.

QUESTIONS/COMMENTS TO HERRICK'S CONTRIBUTED

Schultz: A couple of things, Robbie. First, the business of the role of atmospheres for oblique impacts. The real point behind all of the experiments was the decoupling of the high velocity phases. This is seen in the experiments, it is seen in the computational experiments, and that decoupling can occur on the Moon, and on Mars (because of the tenuous atmosphere, but on Venus it gets stopped. Segi [Sugita] did a very nice numerical calculation showing the same thing. So, it is really sort of a gimmick to on that. The other point I want to make though is you have to be careful about failed experiments. You call this anecdotal, but you have failed experiments on the planets as well as the laboratory. By failed experiments I mean that if you have any topography, it can screw it up. As you go to larger sizes the crater will circularize, so the standard rules that you use, either for the uprange offset goes away. Turns out that the best places to see the effects of oblique impacts is for the very small and for the very large. The reason for the very large is because crater efficiency reduces tremendously, and because reducing the impact angle, the peak pressures that controls the size of the crater is reducing, whereas the size that defines the limit of where the projectile is, is the same. In other words, you are seeing a bigger consumption of the crater by the projectile. The point is that this is not all the same. There are failed experiments, especially for Venus, and especially for Mars whenever there is topography. I give an example: if we do a ridged target, and you fire into a ridged target where the height of the ridges are comparable to the height of the projectile, you get the equivalent of a 90 degree impact even though you come in at 15 degrees. The reason is that you fully couple that energy to the target. So there are many ways to get failed impacts, and you have to be careful. Venus is especially important because on Venus as long as you are below a crater about 30 km across you have the problem of having breakup of the projectile as it is coming in.

Osinski: I am actually not too familiar with Venus, but haven't those same lithologies been interpreted as impact melt outflows as well and not ejecta?

One interpretation of those stippled flows that where drawn in on Pete's diagram is that they are melt that was in the ejecta and then flowed outward, afterward. That is one interpretation. It depends on the crater. I think that in some cases there are actually volcanic flows that mixed with the ejecta, that came long after the crater formed, but… [overlap]

Zellner: If you look at the thermal neutron maps from Lunar Prospector and the Fe-Al-Ti maps from Clementine that Paul Spudis made, you can almost imagine that Imbrium on the Moon was formed from an oblique impact coming in from the North-East and sputtering gunk all over the front side of the Moon. What are your thoughts on that?

Well, when we did the survey the largest crater we ended up considering was about 100 km in diameter.

Imbrium has the problem that there are a lot of things that happened after it. It is really hard though to say whether the distribution of ejecta is pristine enough that you can evaluate it.

Schultz: I think you can, and I think you can actually trace it back.

ANDERSON, J.L.B., Schultz, P.H., Heineck, J.T.: The evolution of oblique impact flow fields using Maxwell's Z model

Anderson discusses further limitations of the applicability of the generalized Maxwell Z-model (from Croft) encountered using the PIV (3D Particle Image Velocimetry Technique) method for oblique impact experiments on sand. Different flow fields (and depth of origination) are observed for different curtain (uprange, downrange, lateral) segments. Even for vertical impacts, it appears that the subsurface flow field is evolving, which implies a moving source region.

QUESTIONS/COMMENTS TO ANDERSON'S CONTRIBUTED

O'Keefe: It is very interesting. I did a series of calculations in which we looked at the ejection angles for three different strength models: one is the von Mises strength model, and we got this nice uniform angle as a function of distance for the ejecta angles across the surface. Then we took the Mohr-Coulomb model, and that got a variation in the ejection angle as you moved across the surface. Then we looked a damage model; there we got a variation from near vertical, where it is highly damaged and near strengthless, and it varied to shallower angles near the edge. Also, Sarah [Stewart] and I looked at the effect of mixtures. There I believe that near the center the ejection angle was near vertical again, varying out. The point is that you should look at those and compare them. I think more importantly in all of this is: what do the stream tubes look like underneath? All of this does is to tell you how the ejection angles vary across the surface. What you really want is to use the stream tubes to tell you for that velocity what is the amount of mass that is going to flow out the surface. So you really need to get some handle, and probably look at the oblique calculations being done here, to see what the stream tubes look like, and see what kind of match you can get with the total amount of mass that is being ejected.

Right, that is definitely something that we are going to work on with these data. Our data does just extrapolate back to the surface at this point, but I think we can go beneath that. Once we get a handle on what the Z values are telling us, we can start asking those kinds of questions about the subsurface flow.

Sharpton: That was a nice piece of work. I think you kind of capped out a little bit at the end, though. Because I think that your results are a lot stronger than your "Nevertheless the Z-model is a good approximation." It seems to me that what you have shown is that it is not a particularly reliable approximation, at least, if you do not allow the Z to vary. A time constant Z model is probably not very good, and it would certainly tend to overpredict the amount of ejecta that you would have. I am going to show an example tomorrow morning of Haughton crater. It is extremely difficult to reconcile Haughton's excavation cavity shape with the Z model. I think that it is impossible.

What we are trying to do with the Z model is really stretch it to its limit to see if we can model oblique impacts using this kind of point source model. I am going to be working on time-varying Z and time-varying depth for LPSC too.

Holsapple: You are right that the Z model is a point source, but in fact it is a lot more, it is a very special point source. In order to have constant Z you have to be in regimes where there is not dependence on things like compressibility, so it is very clearly early time only. Looking at the Z model for late stages, you should not even try. It simply does not apply. It is probably a first order approximation, but I also tend to agree: If you have to start looking at varying the depth, varying the Z, then I am starting to wonder about how useful it is.

HASKIN, L.A., MCKINNON, W.B.: Thickness of and primary ejecta fractions in basin ejecta deposits

Haskin discusses a model of ejecta deposits from large impacts. He concludes that the model gives reasonable first order approximations, but it over predicts the density and size range of secondary craters.

QUESTIONS/COMMENTS TO HASKIN'S CONTRIBUTED

Zellner: Is this 3 km deposit one layer at the Apollo 16 site, or has it been churned over billions of years of impacts and that was incorporated into the underlined regolith?

Further impacts, actually, come from small enough craters that it only affects the very surface, so basically everything that you see at the Apollo 16 site is the Imbrium deposit. The Orientale deposit came in on top but it is very shallow and only has about 2 or 3% Orientale material in it. It is a single layer, based on this kind of modeling, and of course it goes down to less than a half a kilometer in some areas.

Zellner: Ok, then I invite you to come to my LPSC talk where we show that the Apollo 16 site has a composition that is mostly lunar far side, which contains very little KREEP, and also when you compare the regional composition of Apollo 14 with the regional composition of Apollo 16 their KREEP compositions are very different. I don't know that that much Imbrium ejecta is at 16.

I have to respond that we calculate only 20% of Imbrium material in this deposit. The rest of it is stuff that was already present that got stirred up when those primary fragments came in. So we may not be as far apart as you think.

Chapman: Larry, Don Wilhelms of course is probably the most astute observer of the Moon of anyone around, but I am not aware of hardly anyone who thinks that his large basin secondary craters really are secondary craters. I would not take very seriously the problem you have with that.

I'd be happy not to have to take those seriously, but I do not have an independent opinion on that, I guess.

Schultz: Larry, when you applied the scaling model for the secondary craters, were you using the strength-controlled or the gravity-controlled?

Well, both, actually, assuming a transition. But mostly the side is affected by gravity-controlled because the pieces coming in and the velocities are high enough that we would expect that. If we actually go to a strength-controlled it changes things but not by all that much.

Schultz: The reason I ask is because if they do come in obliquely and at these velocities they are coming in, which are going to be by definition less than a kilometer per second, I am not sure if they really will have a very large zone that would really be gravity-controlled. I bet you a lot of them are going to be compression craters.

I'll let Bill answer that one.

McKinnon: We used the scaling directly out of Keith Holsapple's paper and the recommended transition for basalt as a test. It is calibrated to explosion in basalt, so your paper isn't completely crazy, Keith.

[some discussion below the noise of setting up the next talk makes it difficult to catch most comments beyond this point]

McKinnon: I was not aware there was a serious problem with Don's [Wilhelms] study of the Imbrium and Orientale secondary fields that he identified in that old Lunar's proceedings.

Melosh: Let me advertise one of my colleagues talks at LPSC: Alfred McEwen has discovered a small impact crater on Mars, about 10 km in diameter, that has observable secondaries out to 800 km. Those are very small; they actually look a lot more like your distribution than they do with Wilhelms distribution.

McKinnon: The question arising is, Jay, this kind of modeling does not really try to deal with the spall layer. So, it always seems to me that there was a way out to explain things like Don's craters.

Hörz: What are the biggest secondary craters that you make from Imbrium at a radial range like Apollo 16?

About 7 km.

Hörz: There are secondaries that are larger than that. So something needs to be addressed in your model distribution of primary fragment sizes, I think.

MACDONALD, F.A., Mitchell, K.: Amelia Creek, Northern Territory: A 20x12 km oblique impact structure with no central uplift

MacDonald presents results of a field study of the Amelia Creek structure (possibly 20x12 km in size) in Australia. This structure has clear shatter cone evidence for an impact, and the evidence suggests a low angle impact. Structurally, there is no central

uplift, all the shock features (especially the abundant shatter cones) are downrange of the structure, and the structural elements as well as the region of deformation are all oriented in the same direction. The talk concludes asking: What would an oblique impact structure look like in the field? Much more fieldwork is needed to learn more about the structure.

QUESTIONS/COMMENTS TO MACDONALD'S CONTRIBUTED

Schultz: *How deeply eroded is it?*

I don't think it is deeply eroded, because you are seeing these large breccia, and that through that I showed you, I have a hard time imagining that feature being very low in the structure. There is also some evidence of some old neo-Proterozoic land surfaces around there, and if I can really make a correlation and find these neo-Proterozoic land surfaces that other researchers have talked about, then I can really start to constraint the depth of it.

Schultz: *And the relief that you have locally?*

Lots. Big deep canyons.

Schultz: *So my comment about a failed experiment [during Schultz's invited], it could be really here.*

Yes, absolutely.

Schultz: *All you need to do is, depending on how oblique it is, you can have so much complexity that is going to be very difficult…*

Yes, it is going to be very difficult, because of the complexity. But I would like to ask you: What do you expect geologically for an oblique impact structure? Do you expect it to be able to create a large syncline? Do you expect no central uplift? Do you expect these flanking thrusts?

Schultz: *Yes. A couple of things that would happen, and I did not get a chance to get into this, but because the peak pressure goes down, the rules have changed: The strength-gravity transition is going to change, and if you have any of the top relief, that is going to transfer a lot of the energy to the shallow levels. But, I mean, at this scale, who knows? This is where you are going to have to go out, and help us see what it really does look like.*

Sharpton: *It is a nice piece of work. Your shatter cones look really good. I am wondering if you have leapt to the conclusion that this is an oblique impact prematurely, because it seems like the only evidence, at least that you presented, was that the shatter cones are pointing up. Normally, shatter cones in a crater that size would be associated with the central uplift. The central uplift goes through all kinds of deformations. So, maybe you are dealing with a feature that is pre-folding.*

I have found a couple of areas, where I find two limbs of a fold. It was a pre-existing structure, basically, of these broad folds that are in there from the meso-Proterozoic folding. And on both sides of these, what I think are pre-existing structures, you just see shatter cones up-up. You also see a lot of the faults that I think are impact-relating cutting sort of pre-existing features as well. So it really looks like over-printing to me. But I agree, I went in there thinking exactly that.

Sharpton: *The problem is that if your shatter cones are pointing up and not associated with the central structure, then they are way downrange from the center of the crater and that does not make sense.*

Schultz: *Yes it does.*

Sharpton: *Why is that it makes sense?*

Schultz: *Because if we assume it is oblique, a lot of the energy is transferred to the ricochet downrange. That was the reason I showed that one illustration of all the energy that was transferred to any type of relief that is downrange. And you can actually have a shadow [rest is not comprehensible]*

Sharpton: *Ok, I stand corrected. It makes sense to Pete!*

Dence: *There are a few cases of shatter cones at the smaller "simple" craters.*

But how about this sort of extensive area…

Dence: *Well, there is a crater in Sweden where you see there is a lake which is circular, more or less. They have got shatter cones on the margins of the lake, I was there a couple of years ago. That is about 2 kilometers across, so one would infer that the original crater was probably twice that. So, I do not think that you are totally out of that range, at this point.*

Ok, I am also just using the size of those sort of arcuate features that you saw, and this faulting that is cross-cutting all the regional trends also is sort of size constrained. So I think those features have to be related to the impact, and consequently that 12x20 is a bare minimum estimate of the size, because of the deformation.

Melosh: Maybe you just did not have enough time to explain, but it was not clear from your presentation what the relation between that syncline and the shatter cones is. What makes you think that the syncline was formed at the same time as the shatter cones?

Geometrically, it is in the center of the deformation. In mapping it out, basically, this big syncline is flanked by a series of thrust sheets, and the shatter cones are sort of at the end of the syncline, within the start of the thrust sheets.

Melosh: But there is a lot of ...[overlapping voices, incomprehensible]

Yes, but it is very consistent, and it is in one direction. It is basically a mountain range that is folding, so it is crosscutting these features.

French: Just a couple of suggestions for future sampling and examinations: First, it would be very interesting to notice whether the shatter cones themselves contain PDFs or quartz deformation, which would give you kind of a bracket on the pressures that forms shatter cones and forms PDFs. Secondly, you mentioned planar fractures in the rocks, and I think the work on a number of structures recently, including the F…l structure, in Australia, suggests that if you have multiple cleavage sets and if you can do the petrofabric studies on them, they might also be strong evidence for shock metamorphism and therefore an impact origin.

Yes, so far I have taken some thin sections of the shatter cones and I have not found any PDFs in the shatter cones but I have found planar fracture in them. One thing I was talking about this morning as well in terms of sampling there, I think it would be really nice to just do a cross section through the whole crater, sampling progressively, and see if we can measure the shock features, how they vary along the axes of the impact structure. That may give us a lot of clues to the distribution of shock, and if it is oblique. I could be wrong here, but I am going with the explanation that the simplest explanation I find right now is obliquity.

Herrick: Do the shatter cones appear in all the types of target rocks that are in the area, or just on particular compositions?

They appear primarily in quartzites, but there are also some felsic volcanics that they are appearing as well, and some sandstones. It is just localized in that area. But the quartzites take them up the best: They look really nice in the quartzites.

Herrick: Do rocks similar to the ones shatter cones appear in, are those found throughout the whole impact area?

Yes, throughout it.

GENERAL DISCUSSION:

Melosh: We heard an awful lot of oblique impact. I think, unlike the sessions yesterday, this was not dominated by the modelers, but by experiments and observations, I think for reasons having to do with the complexity of doing modeling in 3D. But we are suppose to, during these 15 minutes, take some time to ask any other question that may not have been addressed previously in the discussions, or burning questions you have left over from talks that got cut short.

Ahrens: This is a question directed to Artemieva. Could you discuss the relationship of the velocity of the ejecta to the incoming velocity? Is there any generality that comes out of the calculations, and is there any comparisons to theoretical models that we might learn from these oblique calculations?

Artemieva: What do you mean? Of course I checked different velocities of impact, and I have different melt production for different velocities. What do you mean exactly?

Ahrens: I meant launch velocities of ejecta relative to impact velocities.

Artemieva: How large is ejecta velocity compared to impact velocity? It is pretty close to the impact velocity, a little bit lower maybe. For tektites, in the initial stages it is something like 7 to 10 km/s. For Martian meteorites I have lower impact velocities, something like 10 km/s, and velocities of the high velocity ejecta is something between 5 and 7 km/s.

Herrick: I have a topic that did not get covered today. I was wondering if maybe Fred [Hörz] or a few other people could make some comments about what observations of the terrestrial ejecta blankets can offer about emplacement of ejecta in an atmosphere.

Hörz: I think, basically referring to ejecta blankets, which are practically complete around Meteor Crater and around the Ries crater, by and large, are clastic material, of course, that lack any sort of sorting. They are fairly heterogeneous grain-size wise, even out to large crater ranges in the Bunte breccia in the Ries, sort of 2 crater diameters. I don't see in the terrestrial record clear indication for atmospheric interaction, and I think out of most of the models, and that is where models can come in, the atmosphere is displaced by the ejecta curtain that is basically a solid kind of sheet. I don't think that you have a sort of winnowing of this sheet until it gets geometrically so dispersed at the very fringes. So I think that the atmosphere is basically displaced by the bow shock and displaced by the solid boards, if you wish, of ejecta curtain, making the ejecta basically very heterogeneous grain size wise, and without any aerodynamic effect.

Schultz: Can I just make a quick comment to that Fred? When we looked at Meteor Crater, this was 10 years ago, we looked at the edges of the ejecta facies, and there is a lot of ejecta that is a continuous lobate ejecta that goes up and over some of the topography, and actually has a non-ballistic barrier. Most of this looks like a matrix supported debris flow, not simply the large clasts, it is very fine grain. So there is a lot that is non known yet at Meteor Crater.

Hörz: In a sense, we are talking about two things: I am talking about grain size, and I do not see any effect of sorting of grain size. And you come in and argue that you see flow lobes at Meteor Crater, that you observe them. That could be. We have not seen those at the Ries. So I cannot address the actual flow lobes.

Schultz: I was only referring to a comment about Meteor Crater. The other issue is that the atmosphere of the Earth is still quite dense compared to Mars. So the question is whether or not you would get the type or sorting that you are sort of inferring. I think the other problem is that on the Earth if you look at the blast limit, relative to the timing of crater formation, you do get in trouble where you do not have the same trouble on Venus or on Mars. This is why you really do need to have that type of detailed calculations. By blast limit I am meaning that by the time the energy is dissipated in the atmosphere, combined with any of the vapor that is still there, you are going to have a lot of interactions, because the timing of total crater growth and ejecta advance. That is not the case for Mars because it is already cleared out, at least for part of the crater [? word not clear], and for Venus it is corked in.

Newsom: I want to say something about the Ries, about the complexities that we do see there. On top of the Bunte breccia we do see a sorted layer, with the suevite on top of that. Within the crater itself, in the drill core, there are materials, accretional lapilli and things like that, that could represent something like a collapse of a cloud over the crater. So there are some serious complexities in the depositional environment that we haven't even began to address yet.

Schultz: One of the comments is that in some of the sites we have been working in Argentina, we have about 6 impact sites in Argentina, we actually find 2 to 3 meters thick facies, and it is very difficult to see this as a secondary cratering process because there is no solid clastic material to contribute to it. So there is still maybe some other sites that we can go to, but we need these continuously exposed facies. I thought there were some in Russia…

Nyquist: This question is for Artemieva. Does your work have any implication for the minimum crater size from which you can derive Martian meteorites, or for the maximum fragment size that you could have for them.

Artemieva: The initial stage of ejection is the same for all projectile and crater sizes. The next part of the story, the history of fragments flight in the atmosphere strongly depends on the crater size and on the size of the fragments. For huge craters you have very small influence by the atmosphere, so all particles of any size should be ejected, together with this vapor plume with bow shock and so on. But from the statistics of Martian meteorites, from the Martian chronology, we know that we do not have enough large craters on Mars to produce this variety of Martian meteorites we have now. So, most probably these meteorites were produced in rather small craters, something like 1 and 3 km in diameter. For these craters, we have no strong atmospheric flow. I showed that we have strong deceleration of small fragments, and we have no deceleration for fragments that are larger than 20 cm in diameter. I forgot to say

during my presentation that we have independent confirmation that the pre-entry atmospheric mass of Martian meteorites was larger than maybe 20-40 cm in diameter. It is from measurements of ^{80}Kr in Martian meteorites.

Asphaug: I wanted to ask Galen [Gisler]: Have you simulated impactors much smaller than those you showed yesterday, and do you see evidence for turbulence in the wake, and this sort of thing?

Gisler: No, the smallest impactor I have simulated is a 250-meter impactor, and the atmospheric effects are negligible for the meteor, although they certainly do heat up the atmosphere. What I mean to say is that for a 250-meter asteroid, the atmosphere has no effect on the meteor.

Asphaug: In your K/T simulations you get a big cylinder, and I am curious if you can confirm that you sweep out the atmosphere in a big sheet or do you have these turbulent effects …

Gisler: You certainly have a big impact on the atmosphere. The effect is of two kinds: one is the bow shock on entry which is a sort of parabolic thing, and the other is that once it hits the ground, the explosion of the vapor produces another shock which is predominantly directed in the forward direction for oblique impacts. That ends up dominating, because it moves out much faster than the parabolic atmospheric shock that comes in. But I am not sure if that answers your question.

Asphaug: Well, I was just wondering if you had any resolution between this debate over here.

Gisler: No, I don't.

Gerasimov: I was interrupted during Natasha's [Artemieva] presentation, so I wanted to continue. The question was about the tektites. It seems that it is, again, a little bit simplified model that they are formed by oblique impact just from the surface, because to my feeling it is not enough to make a loading of the material and then bring it to zero pressure by ejection to the atmosphere, because it seems that tektites have very high devolatilization. They have to experience high temperatures, and if it is material from the surface it must also have contained some volatiles, for example water, and I also made estimates for the disintegration. The melt can be disintegrated by the internal movements or by the growing of the bubbles inside, and even this water must disintegrate to much smaller pieces than millimeter size. I think that there must be multiple processes to build up tektites.

Artemieva: I do not know about this micro-explosion of water drops. Maybe you should model separately, not on macro-scale but on micro-scale, this vaporization of drops. But I tried to show that in principle you have ejection of very high temperature melt. These droplets have this high temperature for a long time, and this time is enough to lose any volatile that is in the droplets. Maybe the question is about these micro-explosions within the melt droplets, this may be very important.

Melosh: Maybe I could add a little to this discussion. About 4 years ago, at the Meteoritical Society, I presented what I think is an answer to the dryness of the tektites, in conjunction with some work with Betty Pierazzo. If you look at the thermodynamics of melting in an impact, especially the upper layers that are perhaps porous, you end up in the shock state of being above the critical point. So the melt that is produced is neither liquid nor vapor, it is a supercritical fluid. As it decreases in pressure it boils, so the expanding material actually fills with bubbles. At that same time it is expanding it is accelerated to several hundred-g, which is something I get from one of Betty's simulations of Chicxulub. It is accelerating to hundreds of g, it is breaking into blobs that are filled with bubbles; these bubbles, because of the acceleration, migrate to the surface, they carry the volatiles with them. Because the viscosity of the melt is very low, it is at several thousands degrees temperature well above the liquidus, the bubbles can leave rapidly and you get the devolatilization of water, as well as decrease in Na, K, and other volatiles. I think that is what is really going on.

Gerasimov: The bubble grows, then they have pressure higher than the ambient pressure and they disrupt the unity of the melt.

Melosh: Well, the ambient pressure around is the same as in the bubbles. Remember that the whole thing is a supercritical fluid. Bubbles are appearing, there is a vapor phase, but it is at the same pressure as the bubbles themselves. The bubbles get expelled by the melt blobs as they accelerate out, again, at several hundreds g for about 10 seconds during the expansion.

Gerasimov: Yes, but during the flight out the pressure drops out and the pressure inside the bubbles

begin to overwhelm them, they expand and they disrupt.

Melosh: But as the bubbles are expelled there is less and less volatiles remaining. Maybe this is too much of an in-discussion and we should do this on the side.

Da Silva: I want to go back to Oz's [Osinski] question about the impact direction at the Ries. I am convinced I read somewhere, I think it is Günter Graup's work, about shock propagation using kink-biotites, and carbonate melts as well, that he feels that the impactor had to come in from the northwest. I was wondering if Horton Newsom or Fred Hörz could comment on that.

Hörz: I think in order to answer that question to use kink-band orientation you have to really go in the basement, and you have a hard time to do this in the displaced material. I think the alignment of the Steinheim basin, which is part of the Ries event, the Ries itself and the tektites, is just a nice alignment and I think it speaks a lot for the azimuth that was assumed in these models. Günter Graup's model was also, I think, affected by the state of preservation. There is much more ejecta in the South, which is now really recognized as state of preservation, rather than of an initial mass that was produced.

Da Silva: Does Horton Newsom have an opinion on that?

Newsom: The only thing I say is that the outcrops analyses, as I understand it, were done in the basement rocks.

Da Silva: That is what I thought as well.

Dressler: No, we really do not have any autochtonous basement rocks in the Ries. To do theses studies the rocks have to be in place. They have to be in the basement, they cannot be fragments of large blocks; that is not possible. I have done this study in Manicouagan and I got directions out of biotite kind-bands, but they do not make any sense. Even at the basement in Manicouagan they are shifted around. It is not all autochtonous. These studies do not make sense.

Da Silva: But it does agree with the carbonate melts distribution, according to Graup.

Dressler: I do not see any connection in there. I cannot see anything, sorry.

Da Silva: Ok, maybe I am reading it wrong.

Hörz: I agree with Burkhard [Dressler]. I think the carbonate distribution, unless we learn differently here, is really related to the distribution of the hot material to suevite, and that has nothing to do with obliquity coming in from the North. The present occurrence of the suevites is clearly a preservational kind of phenomenon, rather than a primary phenomenon. That is, a variety of suevites, or melt material, which has a molten matrix, which is rare in the Ries, and that occurs only in the Eastern part of the Ries, along this lineament. So this could be a coherent sheet of impact melt that oozed over, rather than being ballistic like the suevite. There are 4 or 5 relatively smaller occurrences of this type of dense suevite that was not ballistic.

Newsom: One point I make it that suevite is a late stage material. It is on top of the Bunte breccia. So, again, we are less likely to see a directional effect in late stage material. And that layer's nature is still something that needs some more work to try to understand it.