Transcript of Session on Cratering on Low-Gravity Bodies, Sunday, February 9, 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 Mike Zolensky, and the transcript for the final discussion was generated by Robbie Herrick..

Chapman, C.R.: Cratering on Small Bodies: Lessons from Eros

Cratering processes on very small bodies are poorly understood because modeling and calculations have had to be scaled down from very much larger scales, or is based on a very limited number of observations at a few actual asteroids. Chapman summarizes the results and, even more, questions that have resulted from the NEAR mission to Eros. Some puzzling results include the presence on the surface of large amounts of small rocks and boulders, the lack of small (cm-m in size) craters, and the presence of flat, ponded deposits of fine-grained material. Chapman concludes that the differences between Eros and other asteroidal surfaces studies so far, suggests that we should expect more surprises as more asteroidal surfaces are investigated.

QUESTIONS/COMMENTS TO CHAPMAN'S INVITED

<u>Holsapple</u>: *Two questions. I am not sure what the smallest size of crater you can image on Phobos is, but is there any similar indication of drop off for small crater sizes at Phobos?*

Phobos obviously has not been imaged at as high a resolution as here [Eros], but there does appear to be the beginning of a drop-off on Phobos, its down by a factor of several from the saturation level at craters diameters like 20m or so, but this extreme depletion at sizes of meters or tens of centimeters you can't see on Phobos.

<u>Holsapple</u>: *Just another quick one, you know we are always trying to find out if something is strength- or gravity-scaled, and in gravity regime we expect geometric similarity of ejecta, in your comment that you don't see any rims and that ejecta would be spread far and wide, that would suggest something like strength scaling, at least we're not talking about really porous kinds of materials. So is that a sort of a general statement about Eros that you don't see any kind of rims - is there any evidence whatsoever of ejecta piled up near crater rims? What can be said?*

I don't think ejecta. There are some rims around some craters, but for example the ejecta around the Shoemaker crater clearly has gone all around the body. There is a fairly extensive discussion of the distribution of that, so I'm not exactly sure what definitive observation would actually answer your question. Perhaps the scaling is different for the small craters than it is for the larger ones. It doesn't look like these are just little pockmarks that have been put there and deposited an ejecta blanket right in the vicinity.

<u>Housen</u>: *You listed on here seismic shaking, but didn't speak too much about it . It would seem to mean that with a crater you are talking about something that has been processed so probably has very little remaining cohesion, on the other hand a rock that's 10 m high and a gravity of 1 cgs has to have 10^4 cell strength so it doesn't just collapse, so I think maybe shaking could fill in craters but not be enough to affect the rocks; what do you think about that?*

Noam Izenberg, who is here, is actually working on that hypothesis. Why don't you talk about that Noam?

<u>Izenberg</u>: I'm trying to follow some of that stuff up now. I have zeroth- or first-order conclusions I could start talking about. Primarily depending on how much of a factor porosity- or density-based attenuation is, seismic shaking might be a real agent of small, 10m crater, disruption caused by the smallest saturated craters, the 100-200 m craters. I believe that is a real possibility that I am trying to explore. I order to answer the second to last question, looking at some of the very last, high-resolution,

images of Eros, where we e are actually beginning to resolve some of the smaller, couple of hundred meter craters, there aren't many craters with rims pre-se, but there are a lot of these small craters that have bunches of boulders on the edges of the craters. There is not a rim, and it is not substantially higher than the boulder population further afield from the crater but you can definitely see on a number of these craters that there are boulders sitting there on the edges of the craters. What this actually means is somewhat open.

Let me make a comment. One can imagine seismic shaking. I at least imagine it being most effective if committed by a fairly big impact. These are episodic events. It would to have to have been a very recent event to have removed these 10s of cm scale craters. So I guess I would say that it's going to be an active process that contributes to this, but I don't see it as being…..

Housen: *Also it would be more effective at Eros, which may be internally fractured but is more or less competent than at Mathilde.*

Hörz: *Seismic shaking not only will not cover up boulders, as he said, but seismic shaking is a very attractive mechanism to bring the boulders to the surface in the first place; to have a boulder-rich surface. That's a phenomenon that is well-studied in industrial technology. Coarse grained aggregates separate…*

Melosh: *I would like to agree very wholeheartedly with that. There is a well-known phenomenon and a famous paper called "Why the Brazil nut are on top". If you take a mixture of different particle sizes, even if the big particles are denser, if you shake it for a while the big particles come to the top. Because if a big particle moves its easy for a lot of small particles to fill in the hole - its very unlikely that the opposite phenomenon takes place, so the big rocks rise up in a shaken debris, so that could very easily explain why you can get rid of small craters and why the surface is covered with boulders. They are all exposed because of the shaking.*

I don't see how it gets rid of the really small craters very effectively. The shaking is episodic and separated by long periods of time relative to the rate, particularly with the steep production function by which the small craters are produced. As an explanation as to why we see more boulders than expected relative to, say, the Moon, the Brazil nut explanation sounds good to me as a start.

Ahrens: *Clark, if you just admit that you could have an effect like the seismic shaking to get rid of the small guys, if you just took the large craters and tried to assign an age to the object on the basis of the supposed flux rates in the near-Earth zone, that we get from the Moon for example and also the Earth, what numbers do you get and do you think that the resetting mechanism could also be a serious deformation of the object from a near-Earth encounter and gravitational stretching?*

I did the calculations, and I forget my exact answer, but the largest expected crater on Eros in the last 10's of millions of year in the near-Earth environment would be about 100m, maybe 200m at most, crater, and the craters are dominantly what were produced in the main asteroid belt. They are actually saturated at large sizes. It looks just like Ida. Our best guess of the age of Ida is a couple of billion years, plus or minus a couple of billion years [laughter in room], given our uncertainties in those matters; very old.

ASPHAUG, E.: Formation of impact craters on comets and asteroids: How little is known

Asphaug describes the results of modeling studies on impact disruption using the SPH code model, which includes a fracture-damage model. The model predicts that most asteroidal and cometary bodies larger than 1 km should be rubble piles. Model results are very much dependent on the initial, ad-hoc assumptions for the asteroids, such as monolithic versus rubble pile, or contact binary. Unfortunately, attempts to determine asteroid strengths from the physical properties of meteorites are doomed to failure. The properties of meteorites just do not scale up to asteroid size. The images from asteroidal surfaces collected so far have resulted in more questions than answers, and modeling efforts are far from reproducing all the features observed. However, the study of the largest craters on asteroids, spanning the transition from strength to gravity regimes and exhibit whole body effects, may yield key parameters involved in impacts which are normally masked by the much higher gravitational forces present on planets and the moon.

QUESTIONS/COMMENTS TO ASPHAUG'S INVITED

McKinnon: *I wondered about that little bump on Dactyl, but maybe you solved the problem maybe its* *just the impactor sitting there.*

That would be a very slow collision, wouldn't it? Well, you solved the problem.

ONOSE, N., Fujiwara, A.: 1) Velocity distributions of fragments and its time dependence. 2) Velocity distributions of fragments in oblique impact cratering on gypsum.

Naomi Onose gave a presentation on impact experiments performed at ISAS in preparation for the Muses-C asteroid sample return mission. They performed a series of oblique impact experiments, varying the impact angle from 0 to 70 degrees, and employing nylon impactors and gypsum targets. They measured fragment size and ejection angle of the impact ejecta, separating ejecta traveling at four different velocities regimes. They found a considerable difference in the ejection time for the 0° case, as compared to 45, 60 or 70°.

QUESTIONS/COMMENTS TO ONOSE'S CONTRIBUTED

Zolensky: *The Muses-C spacecraft is about to launch, intending to sample surface materials from a very small asteroid, a jellybean measuring only 200 by 400 m. Are there any predictions about the amount of regolith we will find on that asteroid?*

Chapman: *I'll hazard a guess -very little.*

Unidentified: *You've said that before! [laughter]*

Chapman: *Its got to have some, if you believe this transition then, if you believe its been impact battered its going to be retaining blocks. I believe it's got to be multicomponent but I don't know if that means it got regolith.*

Melosh: *I have a question for the last speaker. I have a little problem understanding the huge amount of data you have presented to us. One of the things that on the Deep Impact Mission we are interested in, and I've been asked by many people interested in craters, is what is the distribution of fragment size vs. number. Did you investigate that as part of your experiments?*

Fragment size and curated number in my experiments is also the same as previous studies.

Melosh: *Thank you, that is a very interesting number in terms of, as we were discussing, block sizes on asteroids for the people working on Eros, for example, to compare their block size vs. number to the results of experiments of the type you have been doing.*

Stewart: *On the fragment size distribution, our 99 LPSC abstract same material gypsum/plaster, do we need catastrophic disruption experiments instead of cratering? The power law distribution for number vs. size was also the same as the basalt data from Fujiwara, so it doesn't seem to matter what you're doing, the smallest fragments show the same power size distribution.*

McKinnon: *I have a question for Clark [Chapman]. You said something very intriguing, that Gaspra may be metallic. I wonder of you could just quickly summarize what you think the evidence is and perhaps a more general question for the audience, if you could imagine a metallic body would we expect iron filings as ejecta or some sort of scaled up sort of bullets from the surface?*

Chapman: *Gaspra looks different in terms of its cratering and its shape. One piece of evidence is that its spectrum looks like it's a differentiated body. It is very olivine rich, it is outside of the ordinary chondrite field. A palasitic body, a metallic body with a little rock left over is consistent with its spectrum. That's an additional reason for thinking that maybe there a self-consistent picture. It is speculative; there is no proof. Magnetized. My understanding of the magnetometer measurements made near asteroids is that they're mysterious and can't really be interpreted in terms of magnetization. In the case of Eros, in spite the fact that meteorites have all kinds of magnetizations, Eros as a macroscopic body did not.*

GENERAL DISCUSSION:

[Some initial comments are missed]

Asphaug: Mao Tse Tung, to make his "great leap forward", sent all his violinists, and school teachers, and theorists, out into the field; and I was just going to recommend that the next time we have this kind of a meeting we do it somewhere where people like me can look at a rock, and show me what you mean by strain and things like that. In the converse, you'd have to spend some time before a workstation.

Holsapple: Of course we can get the diameter right [I think this is a reference to a previous comment about having a model match aspects of a particular crater], because we don't know the impactor size. We can get any size you want. This brings up an important point for those of us doing the calculations. We need to match as many of the large explosion craters as we can, where we know the source: some of the recent 5 kiloton high explosive tests, some of the nuclear tests. Otherwise, we can match any size you give us. That's one scalar, we have one big scalar knob.

Herrick: I guess what I was getting at [in previous comments about modeling 30-km craters] was that for terrestrial craters in the 30-km diameter range it is relatively easy to get the size of other features in the right proportion to the diameter, because we can define what the diameter is relatively easily. If you want to try and define how big the central peak is relative to the crater diameter, that's relatively easy to do in that size range. It gets increasingly difficult to do that for eroded, larger craters.

Spray: I think the geologists need to do more of, and it's not easy to do, is what we would call structural mapping. That is the detailed, internal make-up of the rocks that make up impact craters. That means creating good base maps, good topographic control, good sampling, a very thorough investigation. For one crater, that can take many years. That sort of detail is required, not only at the field scale, but also at a microscopic scale, to really get a handle on the processes. Then the deformation styles and regimes need to be passed on to the modelers. But, let me emphasize at this stage as far as we can see things, the partitioning of strain appears to be an important process rather than bulk deformation. I agree that we're looking at a scale-dependent property to an extent, but perhaps with some of the larger deformation features that focusing of deformation is really quite important in terms of how the model evolves.

Herrick: I wanted to summarize what I've heard over the last three days relating to determining impactor properties. Impactor mass cannot be decoupled from velocity, the current state of knowledge does not allow determination of whether the impactor is a comet or an asteroid, and then when you throw in oblique impact which can affect the efficiency for a given energy, it seems to me that it is an intractable problem to look at a crater and make statements about the size, velocity, and composition of the incoming impactor.

Chapman: I think that one of the most potentially useful things to do is to get the geochemistry improved. Chris gave a nice talk about what has been done and what the potential problems are but if you can figure out if the impactor was an iron, or a chondrite, or something else, then we could go a long way towards making that problem not so intractable. I think that's a fruitful area that's not necessarily intractable, it just needs some more work.

Ahrens: There are some cases where I think we know fairly well what the impactor is, like Meteor crater, and we have some convergence from the modelers on the energy, and hence the mass and velocity. Another case is the KT bolide, the original size came from putting all the iridium into a meteoritic object and we got a 10-km object. We also know if we put the iridium into a cometary object it would have been larger. There also have been other constraints since that time. We know from the work of Vickery and Melosh that you can't accrete an arbitrary amount of material, some of it gets blown back into space. We've also learned that there's a big difference in the reaction of the Earth to very large impacts vs. a smaller one. From sedimentary studies we know that the sedimentary environment in Europe was quiescent while in the Caribbean it was tsunamatic, from that we've put a constraint an upper boundary on the energy delivered to the Earth.

Kyte: We can distinguish between some types of projectiles using chromium isotopes. This has been done for impact deposits in a couple of craters. Carbonaceous chondrites have a different chromium isotopic composition than ordinary chondrites, HED meteorites, and iron meteorites. For example, the KT boundary has this chromium-54 anomaly that is only found in carbonaceous chondrites. Presumably comets are somewhat like that. My understanding of the asteroid belt is that the outer belt is probably mostly carbonaceous chondrites, and ordinary chondrites are in the inner belt. Long-period comets probably came from the region outside of Jupiter somewhere. We often think of comets coming from the Oort cloud, but they probably originate a lot closer in and they may be a lot like carbonaceous chondrites. So we can begin to approach this problem. The main problem is finding rocks with a significant meteoritic component in them so you can run this kind of analysis. Beyond that the other big problem is that we don't know physically much about these objects.

Melosh: The problem with learning about an impactor at any given crater is we've had a tendency to concentrate on the crater, which is really the wrong place to look. The crater is vastly larger than the projectile, so whatever the projectile was tends to get lost. Where we need to look is in the most distal ejecta, the very highest velocity stuff is where the projectile came from. Not many people have mentioned microspherules. We see them scattered far and wide, Popigai spherules are probably in the South Atlantic. They do have significant component of contamination. So the places to look are not in the crater, but far away from the crater.

Herrick: Allow me to retort. Just taking the simplest crater, Meteor crater, you can take the total energy, and we know exactly the composition of the projectile, but there is still a problem in terms of mass-velocity trade-offs and the angle of impact.

Gerasimov: I just want to direct the attention of modelers to the problem of mixing of projectile melt and target melt. I agree with Jay that it is necessary to search for projectile material in the distal ejecta. I also want to say that in the impact melt of the crater it is also possible to find the projectile. As I wanted to show in my presentation, the dispersion of iridium all over the globe does not necessarily meant dispersion of the projectile, just the dispersion of the iridium from the projectile. I think that by geochemical methods it will be very difficult to identify the projectile in the melt. I invite the modelers and geochemists to work on the mixing of projectile and target melt.

Koeberl: I want to reply to Jay's suggestion to use the distal ejecta. With that you run into a number of problems that are more severe than looking at materials near the crater. When you look at the crater you have your object right there, you can look for melt rocks, you can look for ejecta, you can look for suevites. The problem we have in finding distal ejecta is that we often don't know the age of the crater very well. The age is constrained by radiometric dating in most cases. When we go out and look at distal ejecta those ages are constrained by biostratigraphy. To correlate the two is not so easy, and that is why we don't have very many distal ejecta layers that have been found. Furthermore, for distal ejecta layers you need a certain minimum crater diameter. So, to look for distal ejecta is often like looking for a needle in a haystack. You can count on the fingers of one hand how many distal ejecta layers have been found. I had a student who spent about 3 years doing his PhD thesis trying to find in northern Italy trying to find distal ejecta from the Ries. The problems we ran into is that he looked at about a 100 m section of rock that had approximately the correct age, but to pin it down you have to find about a 1 mm thick layer. We did a similar exercise for the late Eocene ejecta layer at Massignano, where we looked to see if we could separate the Popigai and Chesapeake Bay ejecta. In both cases what you find is 0.1 - 0.3 ppb concentrations. There are two spikes in the iridium, but you run into the same problem you have with the craters themselves in the sense that the ejecta are diluted. So it's not any concentration enhancement. There's another problem in that distal ejecta can undergo hydrothermal alteration as we saw with Acraman. It is a myth that the platinum group elements are completely stable over time. Under the right conditions inter-element ratios can be changed. So, in summary, you replace one problem with a whole suite of problems.

McKinnon: To generally support the line that Jay initiated, if I were Larry Haskin I would say that most people are interested in the holes, and I'm interested in the stuff that comes out of the hole. It's a general plea to pay more attention to the ejecta; its provenance, its mechanical and physical state, its chemistry. This is of enormous interest to geologists and geochemists, and where modeling can be very helpful as well.

O'Keefe: Not that I know the answer, but it seems to me that in terms of presenting things, it should be clear what the observations are that have to matched. Then, an observationalist says "this is my model or interpretation that matches that."

Dence: Let me say a word of encouragement regarding medium-sized craters in different materials. This offers the best chance of sorting out the effects of target materials. There are a dozen or so craters around the world that are in many cases well exposed and easy to work with. That includes the Ries of course. I think also we might build the case to make a few more strategic drill holes outside what we are doing with Chicxulub, because even some small craters could well benefit from drilling. One thing I'm thinking of, even though it is logistically difficult, is going to Wolf Creek crater, where you have a bit of ejecta, you know what the projectile was, and you have clear evidence in the rim in my mind for an oblique impact. It's a crater about 2/3 the size of Meteor Crater. So, that may be an interesting one to get a few drill holes. It has to be more than one, you need several drill holes to do a decent job. And finally, it took me 10 years to get John Spray up to Charlevoix, so I'm going to start now to invite the rest of you there, where you have the country club of craters in North America. We have excellent facilities, wonderful local culture, great food, a casino if you're bored with everything else. I'd be happy to indulge in that exercise at your convenience. The fall is the nicest time of year. In the process some of the problems that the field geologists will be laid bare for the benefit of the rest of you, and there is some good work going on there that we hope to present within a year or so.

Herrick: That's a good lead in for what I wanted to bring up. Before everyone left, I wanted to get some input regarding future efforts. I wanted to get input regarding a few different issues that have come up. One is a long-term BVSP-style effort where future workshops might pick out a particular topic from this workshop and focus on it, and there would be task forces for different problems, and so on. I wanted to see if there was any interest in that. A second topic regards data gathering and organizing efforts. We are in an interesting field in that a lot of material is in what one could call the "gray literature". There's a variety of data that only a few people in the audience know certain things about. For example, only a few of us know how to get the explosion data that's been mentioned, and similarly only a few people have access to extensive field notes. A lot of the literature is published in something like the "Proceedings of the Iowa Geological Survey". So, let's get some input on that.

French: Regarding gaining access to the data, I hate to generate more paperwork but I did work for NASA for 30 years. I think that maybe an initial step may be to develop a questionnaire that you circulate to the community asking them to identify the types of data they are familiar with, what they have, what they might like to contribute. I think that will give you a feel for the entire range of material that is available. I don't think any single worker here has much of a feeling for the entire range of material that is available. Then, I think you'll have a feeling for what the magnitude of the job would be. A small byproduct would be that it would give a few people like me who are in retirement a chance to look forward to clearing out our basements.

Koeberl: I wanted to comment on what Mike Dence said about drilling impact structures. You might be aware that Chicxulub was drilled by an international consortium called ICDP. The same outfit is providing finances to drill at least one other impact structure, and that's Bosumtwi in Ghana. We are coordinating geophysical studies, seismic studies, gravity studies, geochemical studies, petrological studies, paleoclimatic studies, etc., modelers are involved and so on, to have a large international program. Three other proposals that are being considered are to drill El'gygytgyn, Sudbury, and Chesapeake. There is a mechanism to do these. Unfortunately these things are not cheap.

Ahrens: I think the concept of the LPI being a focus of data is very good. It can be well done in the framework that this is all information that's either on paper, and can be scanned, or is in computer files. I think the area that LPI can do something unique is in the cores area. There isn't any central source of information on cores. There are water wells and other types of efforts all over the world that have been drilled in craters. All kinds of governments have been involved in this, all kinds of geological surveys. The only comparable set of data are the oceanic cores, and things there are under very good control. It would seem to me that it would be a unique enterprise to either get the cores or have a central point of access on how to get the cores. All these other things are great and doable, but I think access to the cores would be unique, and I find that very attractive.

Chapman: I'd like to follow up on your BVSP-style project. I was involved in that monster enterprise, I don't know how many scientists were involved, it might have been 80 or so. That focused on planetary volcanism, a concept that was only a decade old at the time. Much of the focus of that was to bring people from other fields who had never even thought about planets to work on a planetary problem. Here, even though there are disparate specialties, these are not people who don't know who each other are. Cratering as a field has been around for decades. Another problem is the funding. Back then there were adequate funds to bring people together and there were funds to support that type of effort. Now, the way life is fragmented now and the funding is scattered I don't see that happening. I think this particular workshop has been very beneficial in getting people to talk about problems in ways that haven't been done before. A focused study is one thing, but BVSP was big science in a way that doesn't exist any more.

Herrick: If I could briefly comment. Yes, you're right, that was sort of a starting example. However, there are some big advantages that exist now that didn't exist then. During BVSP there was no internet, no email, and I'm not sure if teleconferencing evening existed back then. So, the only way to bring people together to have a dialog was to physically fly them to the same place. So we do have some huge advantages now over what existed then. The sort of general model of getting groups of scientists together as a task force, with the groups having a chair, to produce a compendium on a particular topic, and then building the topics into an overall picture, I think there are some ways to do that in a meaningful way even within today's budgetary constraints.

Spray: Just commenting on data, in the 1950's and 60's C.S. Bealls, whose name some people may be familiar with, had the foresight to initiate a drilling program in a number of Canadian craters. Through the leadership of Mike Dence and subsequently Richard Grieve, the Dominion Observatory and later the GSC acquired 11 km of core through those impact craters. Because of the demise of that impact group at the GSC, much of that collection has moved or will soon be moving to the University of New Brunswick where I am. That will be a set of cores that will be available for loan. In terms of terrestrial impact craters, that list, which was maintained by the GSC, has now been handed to the University of New Brunswick. If you go on our web site you can access the impact crater database which we monitor and update. So, along with that and the cores.

Herrick: That brings up a good point. There's no reason why everything has to be housed at a single place. If we coordinate efforts, perhaps some institutes would be responsible for maintaining different chunks of data. So there are ways to distribute the load. I still wanted to put Buck on the spot. As I recall you were in the process of redoing the 1981 bibliography of terrestrial impact references, what's the status of that.

Sharpton: We've completely digitized that now and put it into a relational database, and it will be up on our web site within a few months. It's only up-to-date through 1981, we're trying to increase the bibliography to make it current. You can imagine how the literature has mushroomed in the intervening 20 years, so if you'd like to have your citations included in our bibliography then send me an email message or there is an online form you can fill out at our web site. Essentially you'll be able to search the bibliography by a variety of key words or crater name.

French: I don't thinks it's so important that all the data or all the cores be in one place. I think it's very important that one know where all the types of information are, and accumulating a list of who is doing these things and what they plan to do is going to be important. I do wish to take a small issue with what Clark Chapman said. I think the idea of a BVSP may not be relevant partly because you don't have a large space program and there's not a large community. I think there's a real parallel in the sense that one of the things I see is that there is a real need to bring in terrestrial geologists. Even for BV the problem was a little less in that there was a terrestrial volcanic community, you were just trying to get them to focus their experience onto different planets. In the case of impact, the active terrestrial community is extremely small and anything we can do in terms of outreach and communication and bringing people into the field would be a big help.

Pierazzo: Once in a while I go on the web and look up impact, and not all the sites are connected, so you're not led from one site to another. What we should try to do is have a master list web site, so if you email your web site to me or Robbie, then we can make a compilation, and we can send it out to everyone and you can upgrade your web site. That's one way to have a more connected series of links.

Koeberl: What you are saying is a problem that I always point out to my students that the world doesn't begin and end with the web. There's plenty of interesting material that has never been digitized. There are materials and papers that are rare and difficult to find that have lots of information, but are not in a digital form. So the web is not a cure-all.

Pierazzo: Ideally it would be great to get those documents digitized, but you have to have somebody doing it.

Dressler: I went through an exercise with the Ontario Geological Survey where we wanted to digitize what we had in house, and the cost was $20 million, and the cost to maintain that was 10% of the original cost. It is extremely expensive and time-consuming.

Osinski: I was at a conference in Sweden, and especially among the students, an email discussion list has been very helpful. Would that be something people would be interested in? You would need someone to coordinate that.

Sharpton: One of the things we have on our web site is an "experts" page. When you click on a crater or an approach it will provide someone who is doing research in that area. There is a point I want to make. While you can't digitize everything, I'm getting concerned that we are losing some of our valuable legacy data, that people are passing on or going into new jobs, and they are leaving a lot of things unpublished or undistributed. I think we need to, as a community, think about ways of preserving some of these data sets that can be thought of augmentations to publications. Perhaps these can be digitized, or by having some clearinghouse for these data.

French: I'd like to echo the cautions that Burkhart made about the web. This may be a prejudice of someone of my generation. Most of the uses of the web are set up by people who want to reach large numbers of people. We need to decide what we want to use the web for, because we don't need to reach large numbers of people. There might be other mechanisms that might be easier or more appropriate.

Pierazzo: I think for people who are just getting into this field, the web's probably the first place they are going to look to learn more about us.

French: I think it's possible to provide an expert's list, but you don't need all the publications dating back to 1895.

Pierazzo: But that's something that's being provided, so if someone wants that then that's great. There are a variety of sights that will be of use.

French: I don't think we're that far apart. I'm nervous about these huge structured approaches without defining what it is you want to provide. I think the approach of setting up multiple things and making things easily available is a good one. I may even learn how to use the web.

Chapman: I would say that the web is the mode, and not just the medium. In my field, all the asteroid data is catalogued on the web. Journals are going to the web. I think the web is the wave of the future.

Osinski: I think people are missing the point that people down on this end were discussing. A discussion list is run by a sight owner, who generally does nothing other than occasionally moderating disputes. Let's say there's a group who wants to discuss low gravity impacts. Everyone gets access to that "chat", if they don't want to read it, they can delete it. The point is that, it's exactly this forum where you have people discussing these things, and as a passive observer one can come in whenever they want. It's just a question of someone setting that up, there are some basic rules.

Abbott [I think}: I belong to a couple of listserves, and it's so nice when someone posts an article, and its been very valuable.

Pierazzo: I would to bring back the discussion to testing. I would like to see some very specific laboratory test results and perhaps a very well studied crater, and have that data on the web available for download so we can come up with a series of model testing that we can do. Then we can compare models, and people building the codes can test their codes and see how they are doing. I would like to ask the lab people and the observationalists that if they have a test case that they think would be useful for modelers, please provide it so we can use it.

Holsapple: I think we have to start with lab tests and explosive tests, where we know the source and then go forward.

[Inaudible comments].

Housen: Everyone keeps notebooks of their lab data, and we've published it here and there, but it would be good to summarize our data and get it into a posted database.

Hörz: The important results get published in papers.

Housen: Results are published in papers, but sometimes only as just a figure or not in a form fully suitable for someone else to use. But if they were posted in an electronic form where someone else could just grab it and do what they want with it, that would be much more useful.

Hörz: It's difficult to determine what data you want to put together for distribution versus taking all the data and throwing them out, because then the databases become unwieldy and not useful. The first filter is publication, and then maybe there is a second level of filter, but not much more.

Pierazzo: I'm suggesting more like extracting two or three test cases that you think it would be good for modelers to match. I'm not saying everything, just two or three cases where you have a lot of results, a lot of data on velocity, pressure, temperature, etc. You'll never be able to put everything out there.

Housen: I wasn't suggesting that we take every shot we've taken and publish it. All I'm suggesting is that you take the data that you have published, put it into a database, perhaps with a reference so if you want to look up the details of the experiment you can go to the appropriate paper. That at least puts it into a format that's a lot easier to get to.

Hörz. We basically don't disagree. I think we should do some thinking about what are some good diagnostic tests in the lab. What is a good impact test? We should use a similar set of target materials and come up with a series of diagnostic tests and those tests should be really detailed. That's very different from the normal PI type of things that I do. I think we should start from scratch and have one test series that hangs together somehow.

Unidentified: On the subject of the data on the web, it's not the data you need it's the references. I've been starting reading Holsapple and Housen's work, and I've been working backward in time. They reference things back and forth. It would be great if I could just go to a web site and it said they did this, this, and this over time. If you know what exists, you can go find it. The problem for the younger people is knowing what form the data is in. I have a question about strength in the model, because you need some sort of softening to get the crater to collapse, but in the models your cell size is going to be on order of 100 m, but core samples are at the scales of millimeters. To my mind that's like trying to get metal plasticity from looking at tangle dislocations. It doesn't seem like anyone is approaching what is an integrated model for a huge piece of material. There has to be localization in some sense, but looking at small samples isn't telling anything because for a cell size of 100 m the physics governing it is occurring at a scale size of meters. What's being done to look at these intermediate scales.

Sharpton: It's been a great discussion, and I hope it is the last comment. On behalf of everybody here I'd like to thank Robbie Herrick and Betty Pierazzo for putting on a great workshop.

Herrick: Buck still has a mike in his hand so I get to ask one last question. Since you've made the suggestion that we should focus on a field effort regarding 20-30 km craters, can you comment on how to coordinate that effort.

Sharpton: The first thing we should do is figure out what we've done already. So over the next few months we should look at what more we need to learn.

Pierazzo: Thanks to everyone for coming by the way. It wouldn't be such a good workshop without you.

END OF WORKSHOP