Transcript of Opening Session, Friday, February 7, 8:30 a.m.

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

INTRODUCTION (HERRICK)

JAY MELOSH: Modeling meteorite impacts: What we know and what we would like to know

The obvious questions are: What do we know and what would we like to know? One of the most important things we have to remember is that we cannot know everything.

Why should we create computer models?

First of all, we can expand (or contract) the size scale from experimentally feasible studies. We can study conditions beyond the reach of experiments.

Planetary scale impacts occur at much higher velocities that can be done in the laboratory. That is the range at which vaporization occurs, which cannot be done in the laboratory.

Another point is to verify the physics (check of what it is we know). We know exactly the physical processes we put in the models. If with those we can reproduce the observations than we have a pretty good argument that we have included all of the essential physics and processes, otherwise we can look for the important processes that we have left out. So modeling is an important tool for essentially checking what it is we know.

On the other hand, models cannot just be models, they have to be tested! One thing that has not been done enough in the field is modeling of experiments. This allows to verify the computer code. If the code agrees with the experiment over some range of observational parameter, then there is some slight reason for trusting the computer model outside the range of the experiment investigated.

Lessons from DoD code verification program – Pacific craters debacle not all bad!

DoD had a large program to check the results of computer programs. This stands for the 1962 nuclear test ban treaty; US and Russia were assured by computer modelers that they could adequately simulate everything that was needed to be known about above ground nuclear explosions so that we could just simply go underground.

In addition to the computer modeling program, there was established a program of testing of conventional explosives, where something like 500 TNT explosions were detonated, a large crater was created, and in addition a number of computer simulations were done (indeed in the US there are three National Laboratories, each of which simulated the event). The point was to check whether the computer codes were doing an adequate job by an actual testing program. This cannot be done in the planetary science community but we can look at the agreement between observations and experiments and that is absolutely essential.

The pacific craters debacle was a very instructing event in the history of modeling.

However, beware! Just because a computer image looks good, it does not mean that it represents reality. Just because a computer code looks good and looks like a simulation of an impact crater it does not mean that it is indeed a good simulation. Computer simulation can fool you. Indeed, you must ask: What is behind that? Is the physics right? Can you get out of that simulation enough information to tell you something? What do you have to know?

You have to decide what you want to know. You cannot know everything. People who are not familiar with modeling tend to get disappointed that we cannot do more. We need to know what we are modeling. Are we modeling a planet or a rock? In computer models we cannot do both. The real world contains rocks, atoms, planets, and stars; it does not seem to have computing problem of memory or speed, but we do in our simulations. In order to get any value out of numerical simulations, we have to decide what it is that we want to know. We cannot simulate the opening of a huge impact crater like

Chicxulub and than expect to answer questions about what is the size of the fragments of the breccia in the same calculation. Decisions must be made on a SCALE, *L*, before starting a modeling task.

It all comes down to resolution. All models work by discretizing a real object into a large number of smaller elements, whose properties and interactions are represented by averages over the discretization volume. The basic thing that runs computer modeling is resolution. It starts with some real complex geologic system that must be broken into individual elements to understand what is going on. How many elements are necessary depends on, again, what one wants to know. The number of elements depends not only on the desired resolution, but also upon the number of dimensions. The number of cells in one dimension is just the length scale divided by the resolution elements. Think of it as in dots per inch: how many dpi are necessary to get a reasonable representation of an image? Two or three isn't enough; is 100 or a 1000 enough? Maybe 1200 dpi would be great in an 8.5x11 inch page, but we may not be able to afford to store the file. The number of elements goes up rapidly depending upon the dimension: in 1D it is just L/r; in 2D it is $(L/r)^2$; in 3D it is $(L/r)^3$. What the resolution element translates to in terms of practicality is memory storage, i.e., the amount of memory a computer must have to run the simulation. This is why we cannot simulate a fist size rock and a Chicxulub impact crater in the same simulation; it just takes too much memory.

The next issue is the run time. We cannot let the computer run for decades. The run time depends upon how long we want to do the simulation for, i.e. the model duration, T, and the resolution. One fundamental limitation to keep the calculation stable requires that the timestep be only about 1/5 of the time for sound to traverse the smallest cell in the problem. This means that the number of timesteps is given by T/timestep, so the run time is proportional to N as well. As we make the mesh finer and finer we must do more steps to avoid the limitation mentioned above. The longest we can run any simulation and expect the results to be meaningful, is the time for sound to travel from one side of the mesh to the other. This means that in 3D the length of time to do the calculation goes like N^4.

Resolution is something that people tend not to think about anymore. The first 2D simulation of impact was done by Bjork and others in 1967, published as an internal NASA report. In it they proudly displayed their resolution (iron projectile onto an aluminum target), which was an about 20x20 calculation, and it pushed the limits computing at that time. This was significant because it was the first calculation in which the detached shock (i.e., the high pressure field separates from the impact area and expands out as a hemisphere) was discovered. It has since been seen experimentally, which gives an idea of the power of the synergy between experiments and simulations.

Modern simulations generally don't show the mesh used. One of the reasons for that is that the mesh is so fine that it would be black at normal resolution. Still to model, for example, tektites individually, one is out of luck; some other special approach must be used for this purpose. Furthermore, a modern trend is to have the mesh size be adaptive, that is the mesh spacing can be changed depending upon where one wants the highest resolution. New computer codes have adaptive mesh throughout. It looks wonderful, but may have other problems.

It is very important to test the resolution. Results should not depend on the resolution. One needs to work from coarser to finer resolution until results do not depend on resolution anymore. It is a very tedious process, but one that can give interesting information.

There are two basic kinds of hydrocode simulations: Lagrangian vs. Eulerian.

In Lagrangian simulations the cells follow the mesh. Keeps material divided (material interfaces are preserved), but cell distortion can be a problem, making the timestep exceedingly small, and eventually the calculations stop.

In Eulerian simulations the mesh is fixed and material flow through it. Mesh does not get distorted, but material interfaces do get blurred, and cells contain mixtures of materials. Furthermore, the mesh has to be large enough to contain the entire time evolution.

There are other modern tricks used. For example, there are SPH codes, which are basically Lagrangian in a mesh that does not get tangled. For a few years this type of code seemed to be the solution to everything, but we have since learned, again through resolution testing, that it is a very low resolution code. However, today there is no universal solution to all of these problems.

Hydrocode modeling rests on two main pillars:

1) Mechanics, i.e., Newtonian physics, F=ma, which is very simple but it takes a lot of book-keeping) +

2) Thermodynamics, the so called equation of state (including constitutive equation), the relationship between pressure and internal energy, which is peculiar to hydrocodes (a thermodynamicist would say pressure and temperature). The other part of that is the constitutive equation, which is the relation between stress and strain, or strain rate. There are many equations of state around: perfect gas (very ideal), stiffened gas (used early, combined features of shocked rocks and vapors), Gruneisen, Tillotson (put together to simulate impacts but does not distinguishes vapor and solid phases). In the modern era we use equations of state like ANEOS, a computer code developed at Sandia National Laboratories, that patches together different approximations to the Helmholtz free energy. SESAME tables were compiled by Los Alamos to produce tabular representations of the material response; some are good some are not (Russians have similar tables).

What is in the future: WE NEED BETTER EQUATIONS OF STATE! We have good equations of state for metals, but they are not very good representations of geologic material. In particular, in the liquid-vapor transition, which is really important in impacts, metals vaporize in gases made of individual atoms. This, however, is not a very good representation of geologic materials. ANEOS has been upgraded in part.

Constitutive equations: Elasticity, and viscosity are in most of the standard codes; there are simple models of strength around. However, rocks are rather complicated (rocks fracture; in them tension and compression are different; they may be porous; as they shear they become dilatant; etc.), and this is a frontier for computer modeling. It turns out that final craters shapes and sizes are very sensitive to the material response of rocks.

There is also the additional problem of how to treat mixed materials in Eulerian calculations. It is bad enough when there are 3 different materials, say water, granite, and iron, in a cell from the equation of state point of view, for getting the T-P calculation (thermodynamics does gives us some guidelines on how to do that right). What happens when we have fracturing rock and ductile deforming ice in the same cell, how and can we do that? This is certainly something we need to know more about.

And finally, I end up with the tale of the Pacific craters problem. It is a tale of simulations and observations thanks to DoD turf war. Some people call this a debacle.

In 1958 a number of nuclear explosions were detonated on Etawak atoll. These were the Oak (8.9 Mtons) and the Koa (1.4 Mtons) tests, performed in an atoll made of coral rocks, and the final craters produced were several 100s m in diameter, but only few meters deep. That generated tremendous consternation in the cratering simulation community because none of the simulations that people did could come close to that crater shape: they were too broad and shallow for the simulations; the volume was about right, but the shape could not be reproduced. It was decided in the DoD community that slumping was not an important process, and the defense community was told that that crater shape had to be simulated. However, no simulation was successful at doing so, and there was tremendous concern that the models were giving the wrong answers. Eventually, with new generation scientists, more exploration of the craters showed what were the real craters, inside the massively slumped craters around the original craters. So, modeling was right all along, even if the modelers were told that they had to reproduce those craters.

The moral that should be drawn from this is that observations, experiments, and modeling cannot stand successfully by themselves. We need to communicate among all the disciplines; each has its own separate strengths, each has its own separate disadvantages and the only way we can possibly progress and learn more about the truth is to work together.

QUESTIONS/COMMENTS TO MELOSH'S INVITED

Ahrens: Can you comment on the lack of thermodynamic equilibrium, and how to deal with it?

I think of thermodynamic equilibrium in the broader sense of involving equilibrium and non-equilibrium. Equilibrium thermodynamics is pretty straightforward and gives us straightforward answers even thought there are maybe a lot of parameters, but in an impact things happen rapidly and one cannot always expect thermodynamic equilibrium to obtain. How to deal with that is a difficult issue, but we

cannot use classical equilibrium thermodynamics at every point in impact cratering.

Plescia: How does the fact that the rock is actually fractured at different scales affects the calculations?

It depends on what you want to know. If you are talking about impact melting and vaporization it is not important; if you are talking about the collapse of the crater after it is opened and you are dealing with rock that has not been shocked too much, then it is important. It leads into fracture mechanics and how strength, your overall behavior response of a rock mass to applied stresses, depends upon internal variables like fractures and fracture size distribution. That is a very contentious field that we do not really understand yet, although there has been a lot of work on that.

Dence: Observations also depend very strongly on resolution. If you look at a thin section of a rock you see very clearly that the number you may get out for the shock pressure that rock has seen is an average of a host of observations, grain by grain, and even portion of grain by portion of grain.

That is absolutely right. And that is something one would like to do, and we have been doing some impact simulations of that, were the mesh is filled with rocks and we treat individual grains as parts of it. You can do that in the computer simulations, but you cannot model what happens in the general impact at the same time, at the same simulations. We have to recognize that those scale dependencies are present.

O'Keefe: When you talked about dimensionality, there is another dimension that is very important too, and is proliferating at this stage. Because of nonequilibrium effects and also because of damage in the constitutive relations require that we do not only look at the classical thermodynamic variables, the damage and others are new internal variables and have to be incorporated into the "thermodynamics" of the situation.

You are right, and there is a new field called damage mechanics that does take that into account.

Spray: You mentioned sound speed that you use within the cells in your modeling. Do you change that speed (we are talking hypervelocity times 5)? What value do you select for that?

You are absolutely right. It is not a simple number that you put in (I glossed over that, in fact). There are different sound speeds and there are also different criteria. If the material starts moving at a velocity comparable to the sound speed, there is another part of the stability criteria that does not allow material to move across the cell at that time. There is a whole cluster of different things, not just sound speed that I kind of lumped in that one category that controls the stability of the calculations. You have to set up the algorithm on how to decide; the sound speed comes out of the equation of state, but for example if fracture is occurring that sets its own criteria for what your timestep can be. So the time-stepping is something one must worry about and must know the physics about.

Holsapple: Even further than what you said about the inability of reproducing the Pacific craters with modeling, at that time the DoD was totally convinced that gravity did not play any role in cratering whatsoever, because they had these great big craters that had the same cratering efficiency as the laboratory craters. So they were convinced that there were no gravity effects. It was only until Robert Schmidt started doing centrifuge experiments that we discovered that, sure enough, there is a gravity decay, and their estimates were three orders of magnitude too high. Interestingly enough, at that same time the planetary community already understood that very well, so in that sense they were well ahead of the DoD community.

ROBBIE HERRICK (filling in for Grieve)

I will give some general constraints that come from observations of terrestrial impacts (mostly extracted from Grieve's abstract) and then a bit about the types of constraints that you can draw by looking at the impact craters on other planets, which of course you cannot do any field work on, but there are lots of them that you can look at from orbit.

Observations of impact craters on Earth include both geophysical observations, like gravity, reflection seismology, and also just field work, by going out and get the structure by doing field mapping and the sort of detailed work that you can do by doing sample analysis.

What can we learn from field observations of impact craters on the Earth?

First is that because we can go out and look at craters at different erosion states we can (if done correctly)

start to build up a picture of the cross-section of an impact crater at different size scales, if we look at enough craters. This is obviously going to constrain the cratering process. We have learned that you do not have to go very far outside the impact crater before the rock starts looking rather normal. Also central peaks seem to be made up of material that has come from a deeper depth and thrust up in the center. Also something that can be done, and is difficult to do when looking at planetary craters from orbits, is to look at the composition of the rocks that make up the ejecta, various portion of the central peak and so on, which gives some information of where things have gone in the impact cratering process. Then looking at composition of impact products at small resolution gives us some information about things like where the impactor went during the cratering process. Also it is possible to look at the melt sheet, and the shock products of different rocks, giving us a few type examples of the way that high shock waves proceeds.

There are a couple of problems with the approach of studying terrestrial impact craters: one is that although more than 150 craters have been identified on the Earth, the number of those that have been studied beyond simply identifying the crater, is probably only a half a dozen, like Ries, Meteor Crater, Vredefort, Sudbury, and a few others. The interesting thing is that all of these craters look dramatically different from each other. So the question is: Are those craters type examples for impact craters or are they rare exceptions? If we had 100 craters studied in great detail, would those be in the norm or not? We have some insight into answering these questions, but not much.

Also, the field geologists and those that do reflection seismology, tend to say that they are just presenting observations, that "this is what the rocks say". In reality, as anybody that has ever been out at any impact crater knows, craters are terribly complicated on a variety of scales and it is really pretty difficult to make any sense of an impact crater in the field without having at least some preconceived idea, or better, some working idea of what you are looking at. Since one cannot collect every single rock in a crater, it is necessary to concentrate at particular points of interest to sort of confirm or deny your initial theory. So the whole process of taking field observations involves having some theories in mind to start out with.

What has happened is that after some really good initial papers written 20 or 30 years ago, where the field geologist was very good in laying out several possible explanations for the observations they made, and then suggested a favorite interpretation, over the years the "favorite interpretation" has become an accepted fact. Then subsequent observations and interpretations are shoved into that "well-known" accepted fact.

Impact craters on the planets:

The nice thing about looking at impact craters on the other planets is that there is a lot of them, so we can make a lot of observations. Planets all have different compositional makeups; some of them have an atmosphere and some do not; some of them have a lot of water and ice in the crust and some of them do not; and they all have different surface gravity. So we get a nice natural laboratory that we can use to learn about the effects of gravity or rock strength on the cratering process.

One thing in particular that we have learn by looking at craters on different planets is that definitively gravity and the strength of the crust (target properties) have major effects on how complex features form and at what diameter they form.

Here is a list of QUESTIONS I do not have an answer to, and would like to address in this workshop:

To what degree are the well-studied terrestrial examples, actually "type examples", i.e., common to all craters that form, and on a variety of scales?

Are there some observations that we ought to be making of terrestrial impact craters that we just aren't, or making too few of?

There are some observations that suggest that basically excavation of a crater always pretty much has the same flow if you have a reasonably homogeneous crater. But there are some other observations of terrestrial impacts that would suggest that the excavation flow in no way resembles what one would get out of a Z model.

As a follow-up to the previous question, is it a valid idea to think of complex crater formation as starting from a transient cavity and follow a given series of stages after that? That is a nice conceptual idea, but I question if it is a good one. Are we muddying the waters by envisioning an excavation stage <u>followed by</u> a modification stage?

I really do not know that there has been a lot of progress over the years on whether central peaks, peak rings, and basin rings are or are not related features.

In trying to go from looking at asteroids into the flux of impactors on the planets, than we need to know pretty well what size asteroid produces what size crater on a planet. Along with that it would be nice if we can take the examples we have on the Earth and using our analysis of the rock trying to back out as a constraint the size of impactor that formed a given impact crater on the Earth. It also would be nice to figure out whether a certain crater on the Earth or other planets was formed by an asteroid or by a comet.

QUESTIONS/COMMENTS TO HERRICK'S INVITED

Ahrens: When we first started looking at large craters, on the Moon particularly, students of lunar geology like Jim Head thought that possibly on a very large scale you did not get deep transient cavities on these giant impacts. I think that one of the surprising results of a lot of the calculational studies and experimental studies, in particular the centrifuge studies, demonstrated that even though a lot of these craters did not look like they had deep transient cavities they in fact did. The geology got reconstructed in a way that fooled a lot of people because you could never see a transient cavity in the resulting geology. So I think we have come a long way in understanding that, due to the work of a lot of the people in this room. The other thing I think you are completely right about the transient craters is when you have great differences in projectile and target properties such as when you fire projectiles into aerogel, like Fred Horz has done. There you do get a very different kind of cratering. Also when the projectile breaks up due to instabilities, then the transient cavity does affect the final results. But, I think some of these things are starting to be understood, so I do not think we are totally ignorant about the relationship of transient cavity to final crater shape at all. I am not claiming we know all the answers by any mean, but I think we start addressing many different problems.

One of the results that is sort of most troubling in terms of the whole idea of proportional growth is that if you look at the seismic profile that goes across a crater, it does look like the area that is disrupted is really shallow. It is hard to imagine that there was a deep transient cavity and then it came back and managed to come back in a way that still produces a set of fairly horizontal reflectors. It is not just the structure in the geology that you need to get back to, you also must reproduce the continuity in the geology that produces well-layered impedance contrast. It may mean that the Earth is atypical in that it is a highly layered target compared to other planetary surfaces.

Dence: A few things: Simple craters are not simple! You have to work at it to find what was the original transient crater. You find out more about transient cavity from the complex craters than in simple craters. Also, layering is very important.

Hörz: I think that what Mike Dence said is very important. You raise the question of which of the terrestrial craters are typical and which one are not; I think the real question is which target is typical and which one is not, because it is really the target that controls the ultimate crater shape and transient cavity and everything. And I think that it is in this area where we the field workers need a lot of help from the modelers. The models so far could only model more or less homogeneous media, and could not incorporate layers of dramatically different rheologies, but the models are coming around and I am really looking forward to it. I encourage models with stratified targets.

Melosh: Your sense of astonishment that you can get those flat layers that you see in reflection seismology out of something that was deformed is exactly the one we felt several years ago. Robert Schmidt saw these flat layers in his centrifuge experiments and decided there could not have been possibly a transient cavity. Then he did experiments with his quarter-space experiments where you could actually see the crater expand and grow and then collapse, and it did come back to be flat again.

Schmidt: Even more spectacular is when we had colored columns (you could easily do it with layers as well) and make a transient crater at low-g and spanned up and it reconstructed. It was like putting back toothpaste back into the tube; these columns straightened up. It was unbelievable to have a crater where the columns were smeared out, obviously not even cylindrical anymore, and they came back into cylindrical columns. So there is not doubt in my mind that layers would have done the same thing. Now the layers were fractured and previously they had

integrity; on the other hand you have a big thickness of rocks in close proximity I think it is a layering damp.

Well, ok, I am open to possibilities. It is something to think about.

Newsom: This idea of the amount of information problem that Jay alluded to: We have this with displaying the results of numerical models, but there is also the problem for the geologist (for those of us looking at craters): it is hard to publish all the observations because there is so much material and we only have few pages (in Geology) Try to get a geology paper into Science and Nature; in two pages you cannot do it, basically, because you cannot begin to show the evidence you need to show. There is a bit of a problem, for example, at the Ries; Gunter Graup's data have never been published, except in his thesis, which is probably the best collection of data on the fate of ejecta in a large crater. So, I think we have that issue to deal with, and that is something where this LPI initiative may be able to help. And also we have the old DoD data and information. There is a lot of information there; for example, Dave Roddy described a year or so ago people wading through dust at some of these explosion craters. I have never seen any reference to that, and that is a pretty interesting observation. So some of the old DoD literature and making sure that is available is something that may be useful for us in the future.

Yes, it is something to sort of keep in the back of your mind. I guess there have been various efforts that sort of stopped and started and not gotten off the ground, to trying to actually have some sort of general data repository for observations that are made on various terrestrial impacts. Such a thing would be nice, it is a possibility and one way that you could bring up in terms of who would do it, and it would get done. Since NASA does fund impact cratering studies to some degree, it could be something maybe worth seeing if there is another possibility for a PDS node or some node, or there are other ways to approach something like that.

Koeberl: Robbie, when you talk about comparing impact craters with models, and see how typical they are, we have an additional problem when we look at our geological observations: Any one of the craters we look at is of course typical, but what needs to be considered is they all have different geological ages, and have been subject to erosion and other terrestrial processes. So when we look at a modeled crater, we look at a fresh object; when we look at an observed crater on Earth, we look at something that has been subjected to differential geological forces over, in some cases, billions of years. So we add another line of uncertainty there that we do not quite know how the erosion affected the crater, what kind of changes happened afterward. And so it is not only the target that is important, but it is also the type of change that happened after the formation. I think we have not a very good idea about that, so that is I think a very constraining part of our observed record.