Results of the Workshop on Impact Cratering: Bridging the Gap Between Modeling and Observations

February 7–9, 2003

Houston, Texas

Edited by
Robert Herrick and Elisabetta Pierazzo

Scientific Organizing Committee
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Jay Melosh, *University of Arizona*
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Atmospheric Effects and Oblique Impacts: Comparing Laboratory Experiments with Planetary Observations
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WORKSHOP OVERVIEW AND FUTURE DIRECTIONS

INTRODUCTION

On February 7–9, 2003, approximately 60 scientists gathered at the Lunar and Planetary Institute in Houston, Texas, for a workshop devoted to improving knowledge of the impact cratering process. We (co-conveners Elisabetta Pierazzo and Robert Herrick) both focus research efforts on studying the impact cratering process, but the former specializes in numerical modeling while the latter draws inferences from observations of planetary craters. Significant work has been done in several key areas of impact studies over the past several years, but in many respects there seems to be a disconnect between the groups employing different approaches, in particular modeling versus observations. The goal in convening this workshop was to bring together these disparate groups to have an open dialogue for the purposes of answering outstanding questions about the impact process and setting future research directions. We were successful in getting participation from most of the major research groups studying the impact process. Participants gathered from five continents with research specialties ranging from numerical modeling to field geology, and from small-scale experimentation and geochemical sample analysis to seismology and remote sensing.

With the assistance of the scientific advisory committee (Bevan French, Kevin Housen, Bill McKinnon, Jay Melosh, and Mike Zolensky), the workshop was divided into a series of sessions devoted to different aspects of the cratering process. Each session was opened by two invited talks, one given by a specialist in numerical or experimental modeling approaches, and the other by a specialist in geological, geophysical, or geochemical observations. Shorter invited and contributed talks filled out the sessions, which were then concluded with an open discussion time. All "modelers" were requested to address the question of what observations would better constrain their models, and all "observationalists" were requested to discuss how their observations can constrain modeling efforts.

To enhance the long-term benefit of the workshop, a number of items are included within this technical report:

> Workshop program and abstracts.

> Summaries of each session: Members of the advisory committee were asked to summarize one of the sessions.

> Transcripts of selected parts of the workshop: The co-conveners attempted to audiotape the workshop. Transcriptions of some of the talks and all the discussion sessions (at least all that we didn't foul up the taping for) are included. [The majority of talks given during the workshop utilized computer-generated slide presentations. Many speakers presented us with a digital version of their talk. We have organized these and they are available on the meeting Web site (www.lpi.usra.edu/meetings/impact2003/), but they are not formally included as part of the technical report.]

> List of workshop attendees and their affiliations as of the time of the workshop.

Also provided is a brief overview of the workshop, and the recommendations for future studies that came out of the talks and discussion sessions.

WORKSHOP OVERVIEW

The Friday morning session opened with two keynote talks scheduled. Jay Melosh gave an overview of the state of knowledge of numerical modeling efforts, outlining the limitations intrinsic to the modeling, and those imposed by current computational capabilities. Unfortunately, flight cancellations prevented the second keynote speaker, Richard Grieve, from attending the workshop. Robert Herrick filled in and presented an introductory talk divided into three components: constraints on the impact process from field studies of terrestrial craters, constraints from orbital observations of planetary craters, and primary unanswered questions in cratering mechanics. Transcripts of these talks and question session that followed them are included in this report.

After a break, the morning session continued with a session dedicated to "Rock properties that need to be known for theoretical modeling," with an invited talk by Keith Holsapple summarizing the important material properties needed for accurate modeling of impact processes. Although current models use simplified representations of material properties, we have inadequate data to properly parameterize even those simplified models. John Spray followed with an invited talk focused on the effects of the impact process on geologic materials observed in terrestrial craters. He discussed the problems with field observations, and in particular focused on pseudotachylites. His observations indicate that pseudotachylites are intimately related to deformation of the post-excavation cavity, and the deformation associated with complex craters occurs in discrete zones. A panel-led discussion followed the two invited talks addressed various aspects of impact effects on rocks, including melting/vaporization, fracturing, and the fate of the material ejected in an impact. In particular, melt homogenization and the role of melt in crater formation are not well understood.

The Friday afternoon session, titled "Effects of target properties on the cratering process" was a logical continuation of the morning session in addressing how the macroscopic characteristics of the target can affect the final crater characteristics. Kevin Housen's invited talk discussed what controlled experiments have taught us about the effect of different target properties on cratering efficiency. He surmised that the internal friction angle plays the most important role on cratering efficiency, followed by porosity . In particular, Housen emphasized the need for a set of benchmark experiments for code testing. In his invited talk, Paul Schenk used observations of plane-

tary craters to highlight a variety of effects that different target properties have on complex crater formation. Target gravity, composition, and layering all have significant effects on the morphology of complex craters and the crater sizes at which complex morphologies occur. Two talks in the session (a short-invited talk by Jens Ormo and a contributed talk by Galen Gisler) discussed the unusual morphologies and other effects that result from oceanic impacts, where a fluid top layer is involved in the impact process. David Crawford showed some initial modeling of impact in a highly heterogeneous media. The final two contributed talks of the session, by Keith Holsapple and Gordon Osinski, discussed various aspects of impact melting. Holsapple concluded that it is nearly impossible to independently separate impactor size and velocity, even if you can reliably estimate melt volume. Osinski concluded that it is a myth that less melt is produced or preserved from impacts into sedimentary versus igneous targets. The lively afternoon discussion spilled over into the evening reception and poster session.

The Saturday morning session centered around "Thermodynamics of impact cratering and determining impactor characteristics." Dugan O'Keefe opened the session discussing how to incorporate various thermodynamic effects into numerical modeling of craters. This invited talk was followed by the invited talk of Roger Gibson who presented an overview of the efforts to back out shock wave propagation and thermal histories for terrestrial impact craters. A major problem in these efforts is the extreme heterogeneity in shock effects at the outcrop and hand sample scale. Mike Dence's short invited talk followed on with a discussion of field-observed shock damage that focused on Canadian craters. In the last short invited talk, Chris Koeberl presented an overview of the geochemical methods employed to determine the composition of the impactor. Uncertainties in asteroid and comet compositions, preservation of material over time, and how impactor and target materials mix all make this an extremely difficult problem, but progress is being made.

After the coffee break, a series of contributed talks explored various aspects of the session's theme. Ahrens' talk presented some specific model results that followed on O'Keefe's earlier talk. Sugita discussed efforts to understand impact-induced vapor clouds from laboratory laser experiments. Gerasimov presented some of his work addressing the mixing of projectile and target materials, and Joeleht and

Newsom closed the session discussing post-impact hydrothermal systems for terrestrial and martian craters.

The Saturday afternoon session was devoted to "Ejecta emplacement and oblique impact effects." The opening invited talk by Natasha Artemieva focused on numerical modeling of oblique impacts, particularly the earliest stages that could produce tektites or the SNC meteorites. Pete Schultz followed with an invited talk focused on his approach of relating small-scale experiments to observations of ejecta emplacement on planetary surfaces. He maintains that a detailed understanding of the response of an atmosphere to impact is needed for an accurate prediction of observed ejecta patterns on both Venus and Mars. Herrick presented a somewhat different approach of using observations of crater morphologies on the terrestrial planets to infer various aspects of cratering mechanics of oblique impacts. The last (contributed) talk before the break was a discussion by Anderson of possible modifications of the Z-model for the purpose of more accurately matching experimental observations and numerical models. After the break, Larry Haskin presented the results of modeling basin ejecta distribution to aid interpretation of lunar sampling and remote sensing data. The final talk of the day by MacDonald discussed mapping efforts of a remote Australian impact structure that may be the result of a rare case of a very oblique impact.

Cancellations of a few talks on Sunday allowed for more extensive and wide-ranging discussions during both the morning and afternoon sessions. Morning talks and discussions revolved around "Creation of the structure of complex craters." Buck Sharpton began the morning with an invited talk discussing some of the problems of interpreting complex craters in the field. In particular, he cautioned against a "resonant feedback" down blind alleys, where models improperly guide field work and bias interpretations that are then used to confirm the model. He urged modelers to provide predictions that can be tested in the field, and geologists to focus on critical field observations that could be useful to modelers. Gareth Collins' invited talk provided a summary of the state of the art of modeling complex crater collapse. He noted that the physics that controls this stage of crater development is very different from what occurs during the early stage of crater excavation. The initial conditions, or the state of the post-excavation transient cavity, are critical for a realistic model. All models of crater collapse require that a volume of material at least equivalent to the transient crater volume must be significantly weakened from

its pre-impact strength. In the contributed talk before the break, Gordon Osinski presented some detailed fault mapping at Haughton crater, emphasizing that crater collapse at Haughton appears to have occurred primarily along discrete faults.

After the break, Bill McKinnon discussed constraints on scaling laws used to estimate transient crater diameter from orbital observations of large craters. Elisabeth Turtle presented a comparison of two different, and somewhat complementary, numerical codes that can be used to model crater collapse, as part of initial efforts to model the Silverpit structure. Jeff Plescia concluded the morning session talks with a discussion of the constraints on crater structure that can be learned from gravity data. The morning session ended with a discussion session.

The afternoon session had only three talks to allow for an extended final discussion session. The session was devoted to "Cratering on low-gravity bodies." Clark Chapman's invited talk began the session with a summary of observations from the handful of well-imaged asteroids. The most puzzling observations involve the unexpected presence of abundant regolith, its ability to organize into "ponds" and "beaches", and the lack of the smallest-scale craters in that regolith. Eric Asphaug's invited discussed cratering on asteroids from a modeling perspective, and he presented some lines of evidence that most asteroids are rubble piles. Naomi Onose concluded the session with an extended contributed talk discussing the results of some experiments designed to learn about ejecta behavior for low-g impacts.

The afternoon talks were followed by a general scientific discussion that transitioned into a discussion of future efforts. Much of the scientific discussion during the final session and throughout the workshop revolved around whether the nature of deformation observed in complex craters in the field could be reconciled with models. In the field, material movement seems to occur in zones of weakness along discrete faults, but craters are modeled with a continuum mechanics approach that has cell sizes of hundreds of meters. Many of the suggestions for future work presented below address this particular aspect of observations versus models.

distribution of older work, which is often not well known by the community.

3. Detailed field studies of mid-sized, 15–30 km in diameter, terrestrial craters. It was suggested that a field research program be developed to thoroughly characterize terrestrial impact craters in the 20–30 km diameter range. There are a handful of these craters that are well-preserved and well-exposed. They occur in a variety of targets, both sedimentary and crystalline. They are large enough that they contain many of the complex structures observable in planetary craters. Each crater should be studied with an emphasis on thoroughly characterizing the amount and nature of deformation outward from the crater. Is the deformation brittle or ductile in nature? Does deformation occur over broad areas, or in small discrete zones? The locations and volumes of melt should be noted. It is also very important to attempt to evaluate how far material has moved during the impact process.

Create a Long-Term Study Project
There was consensus that the impact cratering process would be a good candidate for a long-term organized community study project. Such a study project might be envisioned as being organized and conducted in a manner similar in nature (but not magnitude) to the Basaltic Volcanism Study Project. Different teams of scientists would work to summarize the existing state of knowledge of different aspects of the cratering process, conduct short-term research to fill minor existing gaps, and put forth plans for long-term research. Teams would work under the broad direction of an organizing committee with the goal of producing a compendium state-of-knowledge for the cratering process where the whole is greater than the sum of the parts. A suggested way to divide the cratering process into manageable topics for the teams are the session topics for the workshop, with the possible addition of topics like "Cratering flux in the solar system", and "Environmental consequences of impact."

Robert R. Herrick, Lunar and Planetary Institute
Elisabetta Pierazzo, Planetary Science Institute

A number of specific suggestions were made for future efforts from the impact community. Here, we summarize those that were met with general acceptance by workshop participants. In many regards the long-term benefit of the workshop depends on the level of successful implementation of these recommendations. They are as follows:

Create an Impact Cratering "List Serve"

To improve communication among the community studying impact craters, create a moderated email list serve that subscribers could use to notify others of data availability, discuss scientific issues, etc. Threads could be stored and posted on a web site accessible to the community and maintained by LPI.

Improve Data Archiving and Accessibility

It was acknowledged during the workshop that much of the data useful for impact studies is not readily accessible and/or was published in the gray literature. Only a few researchers know where to go to get access to explosion data that was generally collected for Defense Department purposes. Many detailed studies of impact and explosion craters were published as technical reports or in somewhat obscure regional journals. Only a small portion of laboratory experimental work has been published. Most field data, such as raw core descriptions, field notes, seismic data, etc., are not published.

Several approaches were discussed on how to make these valuable materials more accessible. It was suggested that we begin with a survey of workshop participants to determine who currently has different types of data, and in what form that data are accessible. Perhaps in conjunction with the list serve mentioned above, a web site could be developed that would be a central clearing house location for existing archival efforts. Examples of existing efforts include the database of terrestrial impacts that John Spray's group maintains and an online bibliography of impact cratering references that Buck Sharpton's group is working on. A data archiving and access enterprise that particularly appealed to many participants would be to centrally gather information regarding drill cores into craters: where the cores are located, access to samples, summary descriptions, etc. These types of efforts could be formalized and supported through the Planetary Data System as a node or subnode devoted to cratering, and it was suggested that the workshop organizers approach PDS regarding this possibility.

Develop Standardized Nomenclature

Some confusion continues to exist among researchers regarding definition of terms, and it was suggested that standardized nomenclature be developed. In the context of the topic of the workshop, there are discrepancies between how features are identified in computer models, on fresh craters in orbital imagery, and in partially eroded terrestrial craters. Examples include the boundary and nature of the central structure, how the crater rim is defined, and what is meant by crater size. No particular suggestions at how to approach this problem were made during the workshop, but perhaps a discussion of nomenclature could be developed as a subsidiary effort of the list serve and web site ideas.

Embark on a Few Focused Research Programs

A variety of suggestions for future research efforts were put forth during the workshop. A consensus began to develop for three broadly defined efforts that would be of particular benefit to the community:

1. Benchmarking and cross-comparison of hydrocodes currently used in impact cratering studies. Members of the modeling community that were present strongly advocated that a benchmark set of experimental and explosion crater data be developed and posted for the purpose of evaluating various numerical modeling codes. This benchmark data should encompass a wide range of impactor velocities and energies. A variety of targets should be used that have varying strength, porosity, layering, and so on. To be valuable as a benchmark, as many details as possible about the experiment should be recorded. Target properties of particular interest are composition, grain size, density, porosity, friction angle, and strength. Projectile energy, velocity, and angle must also be available. At a minimum, the final shape of the crater and ejecta blanket must be known. Anything that can be done to track particles during crater formation, or just recording pre- versus post-impact particle position, is of enormous benefit.

2. More systematic experimental work on material properties. This work is needed to characterize material properties in support of the increasingly sophisticated models that are being developed for impact crater collapse studies. In particular, parameters associated with material porosity and strength are not well characterized, and often values not appropriate for geological materials are used in model efforts, in lack of anything more appropriate. These experiments should be accompanied by the collection and

PROGRAM

OPENING SESSION
8:30 a.m. Lecture Hall

8:30   Welcoming Address and Introduction

8:40   Melosh H. J. * [INVITED]
Modeling Meteorite Impacts: What We Know and What We Would Like to Know

9:20   Grieve R. A. F. * [INVITED]
Observations of the Terrestrial Impact Cratering Record

10:00 – 10:15   BREAK

ROCK PROPERTIES THAT NEED TO BE KNOWN FOR THEORETICAL MODELING
10:15 a.m. Lecture Hall

10:15   Holsapple K. A. * [INVITED]
What Do We Need to Know to Model Impact Processes?

10:55   Spray J. G. * [INVITED]
Mechanisms of In Situ Rock Displacement During Hypervelocity Impact:
Field and Microscopic Observations

11:35 – 12:15   PANEL DISCUSSION
Panel: K. Holsapple, J. Spray, T. Ahrens, E. Pierazzo

12:15 – 1:30   LUNCH

EFFECTS OF TARGET PROPERTIES ON THE CRATERING PROCESS
1:30 p.m. Lecture Hall

1:30   Housen K. R. * [INVITED]
Effects of Target Properties on the Cratering Process

2:10   Schenk P. M. * [INVITED]
Importance of Target Properties on Planetary Impact Craters, Both Simple and Complex

2:50   Ormö J. * [INVITED]
Next Step in Marine Impact Studies: Combining Geological Data with Numerical Simulations for
Applications in Planetary Research

3:10 - 3:30   BREAK

3:30   Crawford D. A. * Barnouin-Jha O. S.
Application of Adaptive Mesh Refinement to the Simulation of Impacts in Complex Geometries
and Heterogeneous Materials

3:45   Gisler G. *   Weaver R. P.   Mader C. L.   Gittings M. L.
        Two- and Three-Dimensional Simulations of Asteroid Ocean Impacts

4:00   Holsapple K. A. *
        Does Melt Volume Give the Signature of the Impactor?

4:15   Osinski G. R. *   Spray J. G.   Grieve R. A. F.
        Impact Melting in Sedimentary Target Rocks?

4:30 - 5:15   GENERAL DISCUSSION

POSTER SESSION AND RECEPTION
5:30 – 7:00 p.m.   Great Room

Ai H.   Ahrens T. J.
    Dynamic Tensile Strength of Crustal Rocks and Applications to Impact Cratering

Cassidy W. A.   Wright S. P.
    Small Impact Craters in Argentine Loess: A Step up from Modeling Experiments

Hagstrum J. T.
    Antipodal Hotspots on Earth: Are Major Deep-Ocean Impacts the Cause?

Halekas J. S.   Lin R. P.
    Magnetic Fields of Lunar Impact Basins and Their Use in Constraining the Impact Process

Hargitai H.   Kereszturi A.
    Pyroclastic Flows and Surges: Possible Analogy for Crater Ejecta Deposition

Ivanov B. A.
    Educational Experience in Numerical Modeling of Impact Cratering

Ivanov B. A.
    Modification of ANEOS for Rocks in Compression

Karp T.   Artemieva N. A.   Milkereit B.
    Seismic Investigation and Numerical Modeling of the Lake Bosumtwi Impact Crater

Kearsley A. T.   Graham G. A.   McDonnell J. A. M.   Bland P. A.   Hough R. M.   Helps P. A.
    *Early Fracturing and Impact Residue Emplacement: Can Modeling Help to Predict
    Their Location in Major Craters?*

Kereszturi A.
    Crater Basin Rebound Above Plastic Layers: Model Based on Europa

Ohno S.   Sugita S.   Kadono T.   Hasegawa S.   Igarashi G.
    Sulfur Chemistry in K/T-sized Impact Vapor Clouds

Onose N.   Fujiwara A.
    Velocity Distributions of Fragments and Its Time Dependence
    Velocity Distributions of Fragments in Oblique Impact Cratering on Gypsum

Schmitt H. H.
Impact Crater Morphology as a Guide to Regolith Structure at Taurus-Littrow

Versh E.   Jõeleht A.   Kirsimäe K.   Plado J.
Cooling of the Kärdla Impact Crater: 1. the Mineral Parasequence Observations

SATURDAY, FEBRUARY 8, 2003

THERMODYNAMICS OF IMPACT CRATERING AND DETERMINING IMPACTOR CHARACTERISTICS
8:30 a.m.   Lecture Hall

8:30   O'Keefe J. D. *   Ahrens T. J.   [INVITED]
Impact Induced Target Thermo-Mechanical States and Particle Motion Histories

9:10   Gibson R. L.. *   Reimold W. U.   [INVITED]
Thermal and Dynamic Consequences of Impact — Lessons from Large Impact Structures

9:50   Dence M. R. *   [INVITED]
WIRGO in TIC's? [What (on Earth) is Really Going on in Terrestrial Impact Craters?]

10:10   Koeberl C. *   [INVITED]
Using Geochemical Observations to Constrain Projectile Types in Impact Cratering

10:30 – 10:45   BREAK

10:45   Ahrens T. J. *   O'Keefe J. D.   Stewart S. T.
Calculation of Planetary Impact Cratering to Late Times

11:00   Sugita S. *   Hamano K.   Kadono T.   Schultz P. H.   Matsui T.
Toward a Complete Measurement of the Thermodynamic State of an Impact-Induced Vapor Cloud

11:15   Gerasimov M. V. *   Dikov Yu. P.   Yakovlev O. I.
Experimental Modeling of Impact-Induced High-Temperature Processing of Silicates

11:30   Jõeleht A. *   Kirsimäe K.   Versh E.   Plado J.   Ivanov B.
Cooling of the Kärdla Impact Crater: II. Impact and Geothermal Modelling

11:45   Hagerty J. J.   Newsom H. E. *
Limits to the Presence of Impact-induced Hydrothermal Alteration in Small Impact Craters on the Earth: Implications for the Importance of Small Craters on Mars

12:00 – 12:30   GENERAL DISCUSSION

12:30 – 1:50   LUNCH

EJECTA EMPLACEMENT AND OBLIQUE IMPACT EFFECTS
1:50 p.m.   Lecture Hall

1:50   Artemieva N. A. *   Pierazzo E.   [INVITED]
Oblique Impact and Its Ejecta — Numerical Modeling

2:30   Schultz P. H. *   [INVITED]
        Atmospheric Effects and Oblique Impacts: Comparing Laboratory Experiments
        with Planetary Observations

3:10   Herrick R. R. *   Hessen K.
        Constraints on the Impact Process from Observations of Oblique Impacts on the
        Terrestrial Planets

3:25   Anderson J. L. B. *   Schultz P. H.   Heineck J. T.
        The Evolution of Oblique Impact Flow Fields Using Maxwell's Z Model

3:40 – 4:00   BREAK

4:00   Haskin L. A. *   McKinnon W. B.
        Thicknesses of and Primary Ejecta Fractions in Basin Ejecta Deposits

4:15   Macdonald F. A. *   Mitchell K.
        Amelia Creek, Northern Territory: A 20 × 12 km Oblique Impact Structure with
        No Central Uplift

4:30 – 5:15   GENERAL DISCUSSION

SUNDAY, FEBRUARY 9, 2003

CREATION OF THE STRUCTURE OF COMPLEX CRATERS
8:30 a.m.   Lecture Hall

8:30   Sharpton V. L. *   Dressler B. O.   [INVITED]
        Excavation Flow and Central Peak Rings: Is There a Connection?

9:10   Collins G. S. *   Turtle E. P.   [INVITED]
        Modeling Complex Crater Collapse

9:50   Ivanov B. A. *
        Complex Crater Formation: Verification of Numerical Models

10:05   Osinski G. R. *   Spray J. G.
        Transient Crater Formation and Collapse: Observations at the Haughton Impact Structure,
        Arctic Canada

10:20 – 10:35   BREAK

10:35   McKinnon W. B. *   Schenk P. M.   Moore J. M.
        Goldilocks and the Three Complex Crater Scaling Laws

10:50   Hildebrand A. R. *
        Linking Experimental Modelling of Impact Craters to Structural Components of the Real Thing

11:05   Collins G. S. *   Turtle E. P.   Melosh H. J.
        Numerical Simulations of Silverpit Crater Collapse: A Comparison of Tekton and SALES 2

11:20   Plescia J. B. *
        Application of Gravity Data to Understanding Impact Mechanics

11:35 – 12:15   GENERAL DISCUSSION

12:15 – 1:30   LUNCH

CRATERING ON LOW-GRAVITY BODIES
1:30 p.m.   Lecture Hall

1:30   Chapman C. R. *   [INVITED]
        Cratering on Small Bodies: Lessons from Eros

2:10   Asphaug E. *   [INVITED]
        Formation of Impact Craters on Comets and Asteroids: How Little is Known

2:50   Yano H. *   [INVITED]
        Low-G Impact Experiments in Preparation for the Muses-C Mission

3:10   Onose N. *   Fujiwara A.
        Velocity Distributions of Fragments and Its Time Dependence
        Velocity Distributions of Fragments in Oblique Impact Cratering on Gypsum

3:25 – 3:40   BREAK

3:40 – 4:30   GENERAL DISCUSSION

4:30 – 4:45   BREAK

4:45 – 6:30   CLOSING DISCUSSION

SESSION SUMMARIES

Members of the scientific advisory committee were each requested to chair one of the sessions for the workshop. They were also asked to write a brief summary of the session they chaired. These summaries are provided below.

<div align="center">

Friday, February 7, 2003, Morning Session
ROCK PROPERTIES THAT NEED TO BE KNOWN FOR THEORETICAL MODELING
Summary by Elisabetta Pierazzo

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Introduction

Material properties are a fundamental component of impact cratering studies, influencing any part of the cratering process and its results. Specific material properties govern the response of material to stress, resulting in different behaviors of different materials for nominally the same impact conditions. This has been long recognized, as is witnessed by the long list of publications devoted to understand the response of material to shock events through both laboratory and field investigations. Yet, the general feeling is that we still do not have enough information to be able to completely characterize the behavior of material during the impact cratering process. From the modeling point of view, modeling of material behavior is the still biggest shortcoming in code calculations, and the primary reason for bad results. It is thus not surprising that this is the topic of the very first session of this workshop.

Rock Properties that Need to be Known to Model Impact Processes

The session was opened by the invited talk of Keith Holsapple, who gave an overview of the data that laboratory and explosion tests have provided over the years, as well as a discussion of the main material models used in impact cratering modeling studies, and their limitations. Theoretically, material behavior during an impact can be divided into three regimes: $P \gg c^2$ (is material density, c the sound velocity, and P is pressure), which corresponds to the contact and compression stage (when material is subject to a strong shock), and is governed by the equation of state; $P \sim c^2$, which roughly corresponds to the crater formation stage (post-shock state), and is governed by the constitutive (stress-strain) equation; and $P \ll c^2$, which is characterized by fractured material whose behavior becomes similar to a fluid, and is governed by fracture and damage models.

The equation of state (EoS) is fundamental in modeling the initial response of material to very high pressures, and in particular, the amount of melting and vaporization of target and impactor material. There is a large body of data available in the literature for material response to high shocks. It includes measurements of the Hugoniot state of material and some shock unloading data (adiabats), as well as static melt and vapor points at atmospheric pressure, specific heat, thermal expansion, and critical point measurements. However, there are still many regimes (P-T) in which we really have no data, and thus we must extrapolate with models from the regimes that are better known. Various different kinds of EoS have been used for impact modeling, ranging from analytical, single phase simple models, such as Murnaghan (non-linear elastic, no thermodynamics), Tillotson (powers in density + thermal component + vapor interpolation), and Mie-Gruneisen (linear shock-particle velocity relation + thermal component + vapor interpolation), to semianalytical, multiple phase complex models such as ANEOS and PANDA. The best approach to EoSs in wave codes is that of using tabular forms, such as the SESAME tables, that can use real data; the only problem is that for some ranges of pressure and temperatures, and for many materials of geologic interest, the tables are reconstructed from models, and thus contain the same limitations. In addition, most materials occur in nature as a mixture, and with different levels of porosity (affecting pressure decay in the material), which adds yet another level of uncertainty to the modeling, and one which has not been investigated as thoroughly as needed. In summary, although the tools are there for complex EoS models, it is very hard to get the data necessary to calibrate the models.

The constitutive equation, which describes the response of a material to stresses that induce deformation, is even tougher to deal with. Modeling it

correctly would allow us to reproduce the final size and shape of an impact crater. Unfortunately, models are still limited by the lack of adequate data as well as the difficulty of scaling laboratory results (small scale, both spatial and temporal) to planetary scale events. Constitutive models used so far have difficulties in modeling adequately what happens when a material reaches the failure limit and starts bulking. Material becomes fractured and unable to resist stress. This has been parameterized in models as 'damage,' which is used to modify the cohesive strength of the material. A number of strength models have been formulated over the years, like von Mises, Mohr-Coulomb, Drucker-Prager, which do not include damage. An improved model, which also includes damage, is that of Johnson and Holmquist. Even this model, however, has limitations, most notably the lack of temperature dependence. An alternative model that has been used is the Grady-Kipp, which uses a degraded stiffness concept. The 3D version of this model has been rather popular and has attractive physics, but this model too has limitations, such as the assumption of isotropic damage, and a zero shear stiffness limit, which reduces the material to a water-like behavior. Again, there is the need of performing a lot more laboratory testing of materials and comparison of the model to real data (both laboratory and explosion, when available) to constrain the parameters used in constitutive equations.

In summary, there are still many shortcomings to correct modeling material behavior during an impact event. Unfortunately, the need for publication as a way to show productivity often causes the testing and comparison phase to be barely addressed, if not totally bypassed. This results in a general lack of objectivity when looking at modeling results, ranging from total disbelief to a complete trust of anything coming out of a model.

Rock Properties that Can be Inferred from Field Studies of Impact Structures

The advantages and limitations of field studies to infer rock properties were addressed in John Spray's invited talk. It is important to remember that field observations essentially correspond to an "autopsy" of what is left behind by an impact process. Geologists then try to reconstruct what happened from those observations. The work of a geologist is made even more difficult by the terrestrial environment, where continuous erosion and sedimentation, as well as tectonic deformation act to delete some of the features that are so clearly observed on other planetary bodies, making the work of identifying key parameters, like the crater rim, much more complicated. Furthermore, material properties change during the impact process; a geologist only sees its end result, a modeler will have to start from the initial conditions. Ideally the two approaches (forward for the modeler, backward for the geologist) should give the same answers.

In doing fieldwork a geologist looks at specific outcrops, and his observations are usually at the cm-m scale. Outside the main melt sheet, what is observed in the rocks is the presence of regions of concentration of friction melt, also called pseudotachylites. These regions occur all over the impact region, and show variations in characteristics. In the innermost zones, the friction melt is present as small veins (cm-scale) occurring at intervals of few tens of cm, and showing small displacements (few mm at most) with the rock in between being completely coherent. These are classified as S-type pseudotachylites. This friction melt may contain high-pressure polymorphs, like cohesite and stishovite, and is believed to be shock-related. In the outer region of impact structures, melt friction ranges in thickness from cm to km, exhibits large offsets (up to km), and is generally associated with faults (not as a single big pseudotachylite, but as a complex of friction melts). High-pressure polymorphs are totally absent in these E-type pseudotachylites. Because of their association with faults, they appear to be driven by the gravitational collapse of the crater, and suggest a discrete deformation of the rocks.

Another impact rock feature that has been very useful to geologist in the characterization of impact structures are shatter cones. Looking at them on end, they show an interesting pattern of fractures with offset on them of 1 mm or so, and careful TEM observations indicate that they are coated by a thin layer of melt, indicating that their formation mechanism may be somehow similar to the formation of S-type pseudotachylites.

Putting it all together, this suggests that although the shock wave initially started of in a continuous hemispheric pattern, it would change into a more "broccoli" like pattern as it rips through the rocks becoming more and more distorted and setting up shear systems. Once the transient crater develops, gravity collapse will drive the formation of faults and the displacements in the rocks, with inter-radial and inter-concentric crack being filled by friction melt. This view does not seem to fully reconcile with modeling results of a continuous fluid-like motion of the rocks.

Conclusion

One thing that seems to have emerged from the presentations and discussions that started from this session, with the panel discussion, and continued throughout the workshop is the importance of the scale of the processes investigated. At the field geology scale, (centimeters to meters), deformation appears partitioned, discrete; there is no evidence in the modification stage of fluidization in the gross scale. On the other hand, at the modeling scale, (meters to hundreds of meters) the discrete nature of displacement is lost, in favor of a more homogeneous flow-like description of the process. This is where a big disconnect appear to occur between modeling and observations. The resolution allowed by the current computer power, does not allow to model the impact process at the scale investigated in the field. Thus, processes that appear to be discrete to a field geologist are necessarily described as continuous in a model, since the discrete process occurs on a scale much smaller than the allowed resolution of the model. Field geologists appear to have a problem with a "continuous flow" of material, as this is not really observed in the field. This causes much suspicion about models that use a material flow approach to explain the opening and collapse of impact structures. On the other end, in modeling large scale impacts modelers tend to neglect the small scale characteristics, as they cannot be addressed by current model resolutions. As Melosh pointed out in his invited talk, it is just not possible to model the opening of a huge impact crater like Chicxulub and expect to model microscopic processes, in the same calculation. This limitation also applies to other problems that were touched upon during the panel discussion session, like mixing of the melt sheet, and formation of tektite strewn fields. It is very important for geolo-gists to understand the limitations of modeling work, and be aware of both advantages and disadvantages of the models. This implies that modelers must be very clear on model assumptions and their limitations. On the other hand, it seems that fieldwork has not concentrated much on the type of information that a model could indeed address. This is directly related to an important limitation field geologists have to deal with: the lack of adequate funding for systematic fieldwork at impact structures. They are thus forced to pick what they think are type locations for detail investigation. Unfortunately, has was pointed out by Sharpton in his invited talk, this also means that those type localities must be picked on the basis of a pre-conceived "model" that a geologist must have of the impact process, and what are the important features. This means that "observations" are usually biases a priori by models, and are interpreted through the prism of that preconceived interpretation.

In summary, this section has pointed out that: 1) even after decades of studies, our understanding of material properties is still limited, and much more experimental work is needed to characterize efficiently the parameters that are normally used in constructing material models; 2) both modeling and observation approaches have limitations; disregarding the results of one or the other approach can only be detrimental to our understanding of impact cratering. It has become clear from the whole workshop that only if both modelers and observationalists develop some understanding of the other approach we can hope to combine the results of both type of studies to reach a full understanding of impact cratering.

Friday, February 7, 2003, Afternoon Session
EFFECTS OF TARGET PROPERTIES ON THE CRATERING PROCESS
Summary by Kevin Housen

Discovery of how the size and shape of a crater depends on the properties of the material it forms in has been elusive, primarily because of the complicated constitutive behavior of most geological materials. For example, the strength of rocks and ice is known to be both scale and rate dependent, thus complicating the comparison of laboratory results with the much larger structures formed on planetary surfaces. Realistic numerical simulations must in-clude the effects of rate-dependent brittle fracture, pressure-dependent yield, dilatation, thermal softening, pore-space compaction, etc. Most current numerical models only consider some of these mechanisms and with varying degrees of sophistication. Therefore, we presently have only an incomplete picture of how cratering depends on the mechanical properties of the target material. Fortunately, there is a diverse set of data that can be applied to this prob-

lem, including laboratory experiments of impact and explosion cratering, field tests that have formed explosion craters over a half kilometer in diameter, results of code calculations, and observations of the great variety of craters formed on planetary and satellite surfaces. This session of the conference was aimed at gleaning what we can from these various sources of information.

Results of laboratory and field cratering tests were reviewed by Kevin Housen. He noted that most geological materials follow a Mohr-Coulomb type of behavior, in which the shear strength is the sum of the cohesion and a frictional component, $P\tan(\ )$, where P is the pressure and is the angle of internal friction. Small craters, for which the overburden pressure is negligible, are therefore determined by cohesion. This is the case for terrestrial craters up to a few tens of meters diameter in rock. Formation of small craters in rock or ice is complicated by the fact that the cohesion is both scale and rate dependent, as clearly illustrated by explosive cratering tests at various sizes, and by dynamic strength measurements. The situation for large craters is somewhat simpler, because crater size depends primarily on the target density and friction angle, which together with the gravitational acceleration form the frictional component of the shear strength. From scaling arguments, Housen noted that the dependence on target density can be determined by controlled experiments which vary only the impactor density. Numerous impact experiments have collectively varied this parameter by more than a factor of 1000 and have shown that the cratered mass is independent of target density. That is, the crater volume is inversely proportional to target density. Experiments in various materials show that crater volume varies by a factor of several when the friction angle is varied over the range typically observed for geological materials, i.e. 25° to 60°. This was shown to be consistent with the range in crater size typically observed in explosive field tests conducted in a variety of materials.

Paul Schenk discussed observations of crater populations on planetary surfaces in the context of how material properties affect crater shape and the transition from simple to complex structures. Schenk noted that the pioneering work of Pike, that showed crater depth to be approximately one-fifth the crater diameter. This relation holds for craters on the Moon, Mercury and Mars (terrestrial craters experience considerable erosion and Venusian impacts are significantly affected by the atmosphere). Preliminary analyses of icy satellites based on Voyager data indicated craters in ice are about 70% the depth of craters

in rock. Subsequent analyses of higher-resolution Galileo imagery revised this conclusion and now show similar depth/diameter ratios for rock and ice. However, the transition from simple to complex structures occurs at much smaller diameters in ice than in rock, which probably reflects the lower strength of ice. Observations of the transition diameters on icy satellites show that the transition diameter is inversely proportional to gravitational acceleration. Silicate bodies also exhibit an inverse relationship, but the exact dependence is less certain.

The significant effects of target layering were also noted. In particular the unusual, very shallow, complex structures observed on Europa may be related to the presence of an ocean beneath a thin icy shell. On the Moon, terraces and central peaks occur in smaller craters on the layered volcanic mare deposits than in the highlands. This could reflect differences in mechanical strength or possibly layering.

A particular type of layering of interest for Earth and Mars is the case of impacts in marine locations. Jens Ormö presented a summary of his research on the Lockne crater in central Sweden. The idea is to use the observed geology and morphology of a marine-target crater along with numerical simulations to infer the water depth at the time of impact. Ormö summarized some of the main features at Lockne, including a 7.5 km crater in the crystalline basement rock, surrounded by a 3 km wide brim of fractured overturned basement rock. The upper ~40m of sedimentary rock that originally overlayed the basement was stripped away before the deposition of the flap, presumably by the outward movement of the transient crater formed in the water layer. In collaboration with Valery Shuvalov, numerical simulations were performed for impacts into granite overlain by various thicknesses of water layers. A water layer that is too thin is unable to flow over the rim of the central crater and therefore cannot reproduce the evidence of resurge observed at Lockne. Additionally a thin layer does not strip away enough of the surface material prior to the deposition of the flap. Conversely, a water layer that is too thick results in a crater that is too small and reduces the amount of material in the flap. Ormö found that the paleo-water depth at Lockne must have been greater than about 500m, possibly as large as 1000 m.

Codes, such as SOVA used by Ormö and Shuvalov, used in numerical simulations have progressed significantly over the past decade, both in terms of material models and the numerical methods used. Dave Crawford described some recent modifications to the Sandia code CTH, which now includes adap-

tive mesh refinement (AMR). AMR methods can significantly reduce run times by refining the computational mesh only where high resolution is needed. He gave examples of applications including a 3D model of an impact in aluminum and a finely resolved model of an impact on Eros using the shape model determined from the NEAR Laser Rangefinder measurements. He also demonstrated the use of AMR methods to study shock propagation in heterogeneous materials. These calculations can be used to help understand impact processes in discontinuous media, with obvious applications to the study of rubble-pile asteroids.

Another adaptive-meshing code (SAGE), developed at Los Alamos and SAIC, was described by Galen Gisler. He presented the results of large-body (0.25-10 km) impacts in marine environments. In the larger impacts, collapse of the transient water cavity and jet produced tsunamis up to 1 km in height.

The effect of target properties on melting was addressed by Gordon Osinski. His talk considered the question of whether sedimentary rocks undergo melting in impact events. He noted that even though the volumes of material shocked to pressures sufficient for melting are about the same for sedimentary targets and crystalline rocks, it is often assumed that impacts into the sedimentary targets generate much less melt. For example, it has been generally accepted that impacts into carbonate-rich targets do not generate much melt because the carbonates decompose and devolatilize to yield CO_2 and CaO or MgO. Osinski presented the results of field observations at the 24-km diameter Haughton impact structure that formed in a 1.8 km thick layer of sedimentary rocks overlying Precambrian metamorphic basement material. He summarized evidence that crater-fill deposits, which originally comprised as much as 12 km^3, are impact melt rocks. This volume is comparable to that of melt sheets observed in similar-size craters in crystalline target materials. Osinski, and collaborators John Spray and Richard Grieve have concluded that impacts into sedimentary materials may generate significant volumes of melt and that the amount of CO_2 released into the atmosphere may be much less than previously thought.

One of the goals of field and remote-sensing studies of large craters has been to deduce the initial conditions of an impact event from the observed characteristics of a crater. One example is to use the observed crater size and volume of impact melt, along with scaling relationships, experimental crater data or code calculations to determine the size and velocity of the impactor. Keith Holsapple discussed this problem and noted that its solution is rendered nearly impossible by a unique property of hypervelocity impacts that has, ironically, greatly expanded our understanding of impact processes. Measurements of many of the observables of impact cratering have shown that, to a very good approximation, the impactor is a point source. As such, the impactor velocity and mass are not important separately, but only through a specific power-law combination. Consequently, the important measures of an impact crater, such as its size, melt volume, ejecta blanket, etc, are all determined by the same point source. Any combination of these measures can only determine the power-law combination of size and mass. Their values cannot be determined separately. Holsapple noted that the point source does not strictly apply close in to the impact, where much of the melt is generated. But even in this case, the solution for impactor mass and velocity is highly non-robust. Factors of two variation in the melt volume result in several decades of uncertainty in impact velocity.

Saturday, February 8, 2003, Morning Session
THERMODYNAMICS OF IMPACT CRATERING AND DETERMINING IMPACTOR CHARACTERISTICS
Summary by Bevan M. French

[*] = Senior Author of paper in this session. (I) = invited paper; [ ] = Senior Author of paper in another session.

Introduction

The two topics in this session are widely separated, but they both share the workshop's common theme: the need to build bridges between theoretical studies (theory, modeling) and geological field observations. The papers and discussions provided a good start. If the necessary bridges have not yet been built, at least we now have some idea what they should be made of and where they should go.

Thermodynamic data, and information about thermodynamic processes, are critical ingredients for models of how large and small impact craters form. The same factors also control major aspects of the

geology of real-world impact craters: the volumes of deformed rock, the production of impact melts, the distribution of post-shock temperatures, and post-impact hydrothermal activity within the crater. The problem is how to go back and forth between models and geological field observations in order to understand them both.

The other problem, i.e., how to determine the nature of the impactor from the characteristics of the resulting crater, has long been hampered by too little data. The impacting object is virtually destroyed during impact, and its traces generally remain only as chemical signatures in the crater rocks. Progress in this area requires not only connections between several different areas, but a great deal of new and specific information within the areas themselves.

Thermodynamics in Models and Real Impact Craters

A large number of theories, models, and computer codes already exist for studies of shock-wave phenomena and impact crater formation [e.g., *O'Keefe (I); *Ahrens]; detailed discussions were given in other sessions [Melosh (I); Holsapple (I); Housen (I)]. These resources, combined with high-speed computers, can generate impressive and increasingly realistic simulations of the larger-scale phenomena in terrestrial impact craters.

However, these theoretical approaches are hampered by problems with both the input data and the relative simplicity of the models compared to impact structures in the real world: (1) Equations of State are relatively simple and may not correspond well to natural complex (e.g., polymineralic) rocks; (2) the ability to model complex targets that consist of multiple rock types (e.g., the geologically common arrangement of layered sedimentary rocks over a crystalline basement) or that have a pre-existing deformation fabric (fractures, schistosity, etc.) is limited; (3) the treatment of changing rock properties (strength, internal friction) as a result of damage produced during impact is an area of special concern that is not well modeled, but it is crucial for understanding such problems as the development of central uplifts in large impact structures.

These presentations provided a major benefit to the geologists at the workshop: a better understanding of the assumptions, oversimplifications, strengths, and limitations of modeling studies. It was also important, while watching the impressive computer simulations in color-coordinated video, to appreciate the fact that models are not at the stage where they can provide specific predictions about the details of

individual impact structures. For example, depending on the model, the calculated penetration depth for the Chicxulub structure varies between 37 and 60 km [*O'Keefe (I)]. An important question that gradually took shape during the session was: What data and modeling methods can provide the best predictions for what is actually observed in impact structures, especially at the scales (cm to km) best observed by field geologists?

At the other end of the yet-unfinished bridge, where the field geologists congregated, the question was reversed: What geological observations in real impact craters can provide critical testing and improvement of models? (Virtually all this discussion dealt with terrestrial impact structures, where small-scale data are obtainable.) The rocks of impact structures are the only preserved records of conditions during and after the impact event itself: shock-pressure levels and gradients across the structure; timing of rock movements and production of impact lithologies; and the thermal conditions during and after shock, as reflected in the production of impact melts and the development of post-impact hydro-thermal activity.

Geological studies, many done back in the 1960s and 1970s, have been successful in establishing some basic principles about impact structures: (1) the qualitative understanding of crater excavation mechanisms [Herrick]; (2) the presence of higher shock pressures in the center of the structure [*Gibson (I), *Dence (I)]; (3) the decline of shock pressures away from the center by a power law which, unfortunately, appears to have different exponents in different parts of the crater [*Dence (I)]; (4) the clearly different responses between dense crystalline rocks and porous sedimentary rocks to the same shock pressures; (5) the apparently constant depth/diameter ratio (about 1/3) of the transient crater; (6) the similarly constant ratio of about 1/10 between maximum central uplift and crater diameter; (7) the measurement of post-shock temperatures and metamorphic effects involving impact melts and hydrothermal deposits.

However, in contrast to the large numbers of models and supporting data, detailed geological information is still limited and hard to obtain [*Gibson (I), *Dence (I), Spray (I), Osinski]. The major problem is not the small number of craters available for study (actually >170 are known) but the small size of the impact geology community. The number of impact geologists is probably not much larger than the number of impact craters; as a result, few craters have been studied in any detail after their original identification. (Some exciting exceptions described at the Workshop are Vredefort, South Africa [*Gibson

(I)], Sudbury, Canada [*Spray (I)], Houghton, Canada [*Spray (I), Osinski], and Kärdla, Estonia [Jöeleht, Versh].) Another basic geological problem is that it is a long, hard job to study even a small impact structure in detail at the intermediate scales between field maps (km) and thin sections (mm-cm). Another problem is funding; routine, long-term field-geology studies, regardless of their value, do not appear new and glamorous to highly-strained funding agencies, and there was a murmur of agreement at a comment that it was often easier to get several hundred thousand dollars for a new analytical instrument than to get a tenth of that amount for field work.

Despite the wide separation that still exists between modeling and field geology, some directions for closing the gap became clear during the workshop. Modeling studies should try to become more realistic and more predictive, with efforts aimed at reproducing specific craters by using detailed geological descriptions of their target rocks. [The work being done on Vredefort by Gibson, Reimold, Turtle, Pierazzo and others is an exciting example of how such a focused study can be carried out.] Secondly, to the extent possible, modeling studies can try to predict rock deformation, especially fracturing and other fine-scale deformation, at scales that can be observed and compared by field studies. New and improved (and unfortunately, probably more complex) Equations of State can be constructed to reproduce better the polymineralic nature of real rocks, and more sophisticated functions can be developed to represent the behavior of damaged target rocks during the impact process.

Geologists, in mapping impact structures, need to pay more attention to medium-scale (m-cm) deformation features and the larger patterns that they form. One example is the unsatisfactory state of information about the geological details of central uplifts. Even though many craters have well-exposed and mappable central uplifts, few of them have been mapped well enough to show small-scale details. There is still uncertainty and debate about the extent to which central uplifts are fragmented and how they have moved upward. What is their preserved degree of coherence? Are they relatively strengthless bodies of rubble (the "megabreccia" of Shoemaker's early work)? Or are they coherent rock bodies moved upwards intact like pistons? Detailed field mapping of several well-preserved structures could help settle these issues.

Another issue that appeared frequently during this session and in the Workshop itself is the nature of deformation in the subcrater rocks immediately below the crater floor. How are these rocks deformed during the impact? Can they really be driven rapidly downward, perhaps for distances of kilometers, during formation of the transient crater, then equally rapidly restored almost coherently to form the central uplift? How does this happen geologically? Geologists need to map in detail the few craters in which both the crater floor and the shallow subcrater rocks are exposed. A related problem is the fact that shock pressures and shock gradients have been measured in only a few impact structures [*Dence (I)]; more extensive studies would provide modelers with important information about the state and distribution of at least one key thermodynamic variable, shock pressure, during crater formation.

Thermodynamics in Thermal Impact Processes

Several papers in the session addressed thermodynamic processes that occur both before and after the crater itself has formed: the nature of vaporization during the impact, and the formation of hydrothermal systems and possible ore deposits after the crater itself has formed.

Vaporization is critical to modeling the earliest stages of the impact process, during which the projectile and much of the target are consumed, large amounts of vapor are ejected upwards, and near-surface target material is ejected at high velocities to form distal ejecta, spherule deposits, and possibly tektites. At the same time, realistic representation of vapor in computer models is difficult because the physical processes and the thermodynamic properties are largely unknown. Two papers described experimental attempts to provide better information: measurement of actual parameters in an impact-produced vapor plume [*Sugita], and chemical studies of the actual species produced by vapor condensation [*Gerasimov]. These two studies showed how complex the subject is and how much more there is to be done. Sugita's experiments demonstrate the possibility of actually measuring critical vapor parameters that previously had to be assumed in calculations. Gerasimov's studies showed that silicate vaporization is complex, and the most dominant species in superheated vapors may be atomic clusters rather than the more computer-friendly single atoms.

The problem of post-shock temperatures in impact structures is becoming more important with the realization that long-lasting impact-produced heat can mobilize nearby water, form hydrothermal systems and ore deposits, and perhaps contribute to the origin and development of life. Traditional methods of mineralogy and petrology, applied to the rocks of impact craters, have been remarkably successful in estimat-

ing post-impact temperatures from mineral assemblages [*Gibson (I)] and in using mineral inclusions to determine temperatures and fluid compositions of hydrothermal systems in impact structures [*Jöeleht]. These data can then be used as tests of computer models to compare predicted and measured temperatures for the same crater. These studies have important implications for craters on other planets, especially Mars, where impact-produced hydrothermal systems may play an important role in melting permafrost and moving liquid water around [*Newsom].

Estimating Impactor Composition from Craters

Most of the effort in this field has involved geochemical analyses, trying to establish the presence of extraterrestrial projectile material in the crater rocks, and then trying to match the estimated composition of the projectile with the compositions of known meteorite types. This research area [*Koeberl (I)] has expanded greatly in capability and complexity since the simple measurements of excess Ir by the Alvarez group in distal ejecta from the Chicxulub crater established that a major meteorite impact had occurred at the end of the Cretaceous. Subsequent research has also been based on the analysis of impact crater materials, but there have been numerous new developments: (1) the use of newer, more sensitive, and more sophisticated analytical methods; (2) extensive measurements of siderophile element ratios and their comparison with known meteorites; (3) use of isotopic systems of several elements (Os, Cr, and W) to identify low amounts of extraterrestrial components and to distinguish between meteorite types.

There are now several accepted geochemical indicators for an extraterrestrial signature in impact craters, but it has been harder to relate the observed signatures to a specific type of projectile, and the appearance of better analytical methods has been balanced by the recognition of new problems. Detection of small amounts of extraterrestrial material in impact melts is complicated by the difficulty in correcting for amounts of the same siderophile elements in the indigenous target rocks. Even when larger amounts of projectile material are present, its amount and distribution in impact melts and other rocks from impact craters is not uniform; in fact, it can vary widely (from undetectable to several percent) between nearby samples. More puzzling is the fact that, at two craters (Meteor Crater, Arizona; Wabar, Arabia), small bodies of impact glass have siderophile element ratios that do not match the known composition of the preserved iron meteorites that formed the

craters. There are additional problems in using elemental ratios that actually result from secondary alteration and differential redistribution during post-impact hydrothermal activity, metamorphism, and weathering.

Even with accurate and reliable analyses, there are further problems at the other end, in trying to connect the data with a specific meteorite type [*Koeberl (I)]. Many meteorite groups are not well-characterized by modern chemical analyses, and even meteorites with good analyses may show wide ranges in the contents of key elements. Nor is there any guarantee that the compositional groups of present meteorites correspond to the populations of objects that fell to Earth tens or hundreds of millions of years ago. Finally, the critical problem of distinguishing between cometary and asteroidal impactors is blocked by the lack of data on comets and the need to use data from Interplanetary Dust Particles (IDPs) as proxies.

With the problems, however, have come ideas for studies to solve them: (1) more detailed and systematic studies (and restudies) of chemical signatures, especially in craters for which the nature of the impactor is independently known; (2) better understanding, through both analyses and modelling, of how projectile material is actually distributed into impact crater rocks; (3) more studies of how different projectile elements separate under impact and post-impact conditions; (4) better understanding of elemental behavior during vaporization, which is the process that actually transfers most (if not all) of the material from the projectile to the impact-crater rocks.

An alternative approach to determining other impactor characteristics (e.g., size, density, impact velocity) is to apply scaling laws to deduce them from measured crater parameters (e.g., diameter, impact melt volume) [Holsapple]. Although the paper concludes that the prospects are not encouraging — the various scaling laws are too close in nature to yield precise estimates — the idea should be followed up, because it provides the hope of determining projectile characteristics that cannot easily be established by chemical means. In the meantime, it seems wise to be a little skeptical about precise matches between projectiles and their craters, and even more skeptical about the more basic distinction between cometary and asteroidal impactors.

Conclusions

The papers and discussions in this session and elsewhere in the workshop provided a good start to

bridging the gaps between the "modelers" and "geologists." They provided useful contacts between the two groups and the individuals in them. Each group ended up more aware of the other's work; more important, there was a better understanding of the problems, assumptions, simplifications, weaknesses, and individuals involved in each type of research. There was an encouraging amount of spontaneous discussion of future workshops and future research projects, and everybody went away with new ideas about how to make their future studies more realistic and more valuable to the others. There were plenty of good ideas reported, generated, and talked about. Now the challenge is how to find the people and resources (both time and money) needed to get them moving.

Saturday, February 8, 2003, Afternoon Session
EJECTA EMPLACEMENT AND OBLIQUE IMPACT EFFECTS
Summary by H. J. Melosh

This session included five talks dealing with oblique impacts. The talks ranged from theoretical computations with three-dimensional numerical codes, to experimental studies, and then to field observations.

The most theoretical talk was presented by Natalia Artemieva of the Institute for the Dynamics of Geospheres in Moscow. She collaborated with Betty Pierazzo (Planetary Science Institute, Tucson) on a study of the ejection of melted materials from the Ries impact crater. Using the 3-D hydrocode SOVA, they showed that melt originating from near the surface of the target was ejected in two streams concentrated on either side of the down range projection of the impactor's flight path. This pattern matches the observed location of the moldavaite tektite strewn field very well. She also discussed scaling of melt production in oblique impacts and the origin of Martian meteorites. Discussion after the talk focused on the actual depth of origin of the tektites and how they achieved their distinctive geochemical signatures. Later, Pete Schultz stated that he has formed similar ejecta patterns experimentally.

Pete Schultz, of Brown University in Providence, then described his experimental investigations of oblique impacts in the presence of an atmosphere. He argued that the distinctive Martian rampart crater ejecta pattern was created by vortices produced by the collective interaction of ejecta and the ambient atmosphere. Discussion following the talk focused on how to scale from the laboratory to the Martian surface, and the possible importance of vaporization of ice in the target.

Robert Herrick (with co-author K. Hessen), of the Lunar and Planetary Institute, Houston, presented observations of oblique impact craters on a planetary scale. Comparing data from lunar, martian and venusian crater populations, he found that the wall slope of these craters does not depend on the angle of the impact. He also found similar numbers of highly oblique craters on all these bodies. In the discussion Schultz warned that the pre-existing topography of the target may play an important role in the final crater morphology.

Jennifer Anderson (with co-authors Pete Schultz and J. T. Heineck), of Brown University in Providence, extended the Maxwell Z-model of cratering flow to oblique impacts by fitting the model to ejecta distributions that she measured experimentally using her innovative PIV (3D Particle Image Velocimetry Technique) laser technique. The Z-model does not seem to do a very good job: Even for vertical impacts it requires a moving source to fit the observations. Most listeners agreed that the Z-model is too simplistic to be used for detailed predictions of ejecta distributions.

Larry Haskin and Bill McKinnon, of Washington University in St. Louis, presented a model for the emplacement and distribution from very large impact basins on the Moon. This talk raised the still unsettled problem of how many and how large are the secondary impacts from large impacts on the Moon, a topic that was aired extensively in the following discussion.

Finally, Francis Macdonald (with K. Mitchell), from Caltech in Pasadena, intrigued the conference participants with the report of a large (20 × 12 km) region of shatter cones discovered in a remote region of Australia. The area is tectonically complex, but no central peak, which is expected in this size impact structure, is evident. Macdonald proposed that this is a very oblique impact crater. The discussion reflected great interest, but more study is clearly necessary before this interesting structure is understood.

The session concluded with about half an hour of general discussion that ranged over the topics raised in the talks. It was clear that the subject of oblique impacts was of high general interest, but that much

more remains to be learned about its implications for impact craters and impact ejecta. Theory, experiment and observation of both terrestrial and extraterrestrial craters all have important roles to play.

Sunday, February 9, Morning Session
CREATION OF THE STRUCTURE OF COMPLEX CRATERS
Summary by William B. McKinnon

Complex craters, with the their central peaks, flattened floors, and terraced outer rims, are the arguably the most fascinating of all crater types. The largest of these, the peak and multiringed basins, are the most magnificent. Planetary images of relatively pristine complex craters on the moon, Mars, Venus, and the satellites of the outer planets never fail to entrance. But the only complex craters we can study in situ and in three dimensions are here on Earth. These are also the crater forms that are the most challenging to understand, for the mass motions that materials have undergone are complex (if not convoluted) and the material properties that seem to be implied are in many regards counterintuitive. A workshop session was devoted solely to complex craters. Nearly all the talks focused on terrestrial structures, and on both their geological exploration and modeling. Two talk cancellations by foreign guests allowed ample time for discussion, plus an additional talk at the end.

The lead invited talk by V. L. Sharpton and B. O. Dressler was given by Virgil Sharpton. In abstract, the talk was centered on field field work at the Haughton crater on Devon Island in the Canadian arctic, and whether its structure could be adequately explained by traditional excavation flow models such as the Maxwell Z. As given, the talk took a broader look (or swipe) at field exploration of impact sites and how field geologists have been (unduly?) influenced by modelers. As armchair scientific philosophy, it was entertaining, informative, and presented with a great deal of humor. As a critique of the scientific method, however, it falls into the long and basically unresolvable debate as to whether it is better to simply gather up all the facts and figure out the conclusion later or gather facts or experimental evidence in order to test a preexisting model or paradigm. My personal view can be summed up as "whatever works." As we all stumble towards a more complete truth, either approach may be the most useful or fruitful.

It should hardly be taken as a scientific failing that most modelers have taken to interpretation of planetary complex craters, and that existing modeling results should influence field interpretations of terrestrial craters. After all, terrestrial craters are often deeply eroded (or if not, they survive by being buried and thus explorable only by drill core, gravity, or seismics), so the field geologist is left with an a conundrum: to interpret afresh, or try to see the structure as a deep section of complex structure whose preexisting surface expression they can visualize. It is all a bit like the blind men and the elephant, but here we have terrestrial field studies and planetary morphological interpretations playing clearly complementary roles. Neither will come to resolution of complex impact structure without the other. If there is any failing in modeling, it is that the emphasis has been on explaining resulting external morphologies, and not what changes take place subsurface and at scales accessible to the field geologist. The clear purpose and result) of this workshop was to build a bridge between these approaches.

The second invited talk by G. S. Collins and E. P. Turtle was given by Gareth Collins. He described state-of-the-art numerical calculations of complex crater collapse and central peak and peak ring formation. This talk was related in spirit and content to earlier modeling talks but emphasized a key point. In order to collapse, the region around the crater must be weakened by some mechanism: (1) fracturing and bulking; (2) acoustic fluidization or block oscillation; (3) melt production; (4) thermal softening; (5) shear melting; and (6) combinations of some or all of the above. All these mechanisms can be incorporated in models at gross physical scales and parameters adjusted to allow collapse and peak formation. But adjusting parameters to get the right morphology may not give the right answer if the physics is not right. How can we tell? Field studies! These hold the key what actually happened to the rock during the cratering event. But modelers, who spend much of their time thinking about rock mechanics, need also to think about the field scale. John Spray's frequent cries that the cratered rocks he has examined don't look like they were acoustically fluidized begs the question of what should acoustic fluidization look like (see Friday morning discussion above).

A contributed talk by G. R. Osinski and J. G. Spray was given by "Oz" Osinski before the break. It also concerned detailed field observations at Haugh-

ton crater and the kinematics of the failure of the rim and development of the central peak, as revealed by families radial and concentric faults and the nature of the fault contacts (part of GRO's Ph.D thesis). Their studies do not indicate much of a role (if any) for melting in weakening during crater modification, but do indicate much evidence for localized, brittle deformation (fault planes). Many of the radial faults display substantial volumes of fault breccia but relatively little offset. The interesting question to this writer is whether the displacements were in fact much larger (say, at the time of the maximum transient crater) but were effectively restored during crater collapse (as seen in model calculations and lab experiments). This rich data set, based on four field seasons, should prove enormously valuable as for modelers. I predict that high fidelity simulations of the formation of Haughton crater, with its sedimentary over metamorphic gneiss target and final mapped fault and displacement pattern, will lead to a breakthrough in understanding complex crater formation.

The session continued with contributed papers. W. B. McKinnon, P. M. Schenk and J. M. Moore discussed empirical laws relating final to transient crater size for complex craters (largely lunar). Bill McKinnon pointed out that the laws published by Croft (best known), McKinnon and Schenk, and Holsapple, are sufficiently different that they cannot be all be correct. The time has come for reexamination of this issue, if only because complex crater scaling is important in using crater counts to date retention ages of terrains throughout the solar system.

Gareth Collins returned to the stage to present preliminary modeling results (with E. P. Turtle and H. J. Melosh) of simulation of the formation of the graben rings surrounding the Silverpit pit structure in the North Sea. They used an initial transient structure derived from the Z-model and incorporated weakening by acoustic fluidization near the crater and as-

sumed a weak lower chalk layer to allow sliding toward the crater center. Model results presented indicate a concentric zone of extensional stresses that can be interpreted as potentially graben-forming, but field geologists should understand that the numerical models used are incapable of predicting the form or spacing of such "localized" phenomena as graben. Much remains to be learned from both exploring and modeling Silverpit.

The official session ended with J. B. Plescia describing the utility of gravity studies for understanding subsurface deformation, shock effects (brecciation), and even the basic morphometry of terrestrial impact structures, especially if the surface expression is less than informative (think Chicxulub!). Several examples were discussed in detail.

The discussion period began with an impromptu talk by H. J. Melosh, describing some results sent to him by Boris Ivanov, who could not be present due to pointless visa delays. The results were modeling calculations of the growth and collapse of a complex crater, one in which the strain history was tracked cell by computational cell. The point of the calculations was to show, in case anyone needed reminding by this time, that strain ≠ displacement, that some computational cells at least can go through large displacements, can be highly strained, and yet the strain can be reversed so that the total resultant strain at the end of the modification stage can be modest. This may help geologists reconcile their field observations with impressions they take away from modeling efforts. The areas of the final crater that appear to achieve the highest final strains are within the central peak region and near the surface of the rest of final crater. The implication was that in an eroded section, the zone of maximum deformation (strain) should appear confined to the crater center. Much discussion of complex crater formation along these lines ensued.

Sunday, February 9, Afternoon Session
CRATERING ON LOW-GRAVITY BODIES
Summary by Michael Zolensky

This is an important and timely topic since there have recently been numerous flybys of Earth-crossing and main belt asteroids, the just completed Near-Earth Asteroid Rendezvous (NEAR) Mission, and the imminent launch of the Muses-C spacecraft which plans to return the first sample from an asteroid. Despite these new observations, our understanding of cratering processes on such small bodies is at a

very rudimentary stage. As an example, just a few years ago it was common knowledge that such small bodies, with their attendant low-gravities, would not be able to collect a regolith. The fact that all asteroids observed to date (down to ~10 km in diameter) have regoliths has been sobering.

There were three talks at this session, and the first was an invited talk given by Clark Chapman on

the general topic of impact cratering on small bodies (asteroids, really), with an emphasis on results from the recently completed NEAR mission to asteroid Eros. Clark made the point that our expectation for asteroids were based initially on lab experiments and observations of the cratering process on planets, with everything having to be scaled to small bodies with very low gravity, unknown internal structure and composition, variable spin rates, and fairly uncertain surface material characteristics. While early thinking was that small bodies could not support a regolith, it appears that all asteroids we have observed from spacecraft have substantial regoliths. The question now is whether all asteroids have regoliths, and if not what the lower size limit may be for regolith development. The best characterized asteroids appear to carry impact records that are quite distinct from that of, say, the Moon. Mathilde sports impact craters much too large for its diameter, suggesting great internal porosity. Eros is lacking the smaller impact features ubiquitous on the Moon, and discussion now centers on the reason for this difference, with most workers believing that the smallest impact features are erased as impacts shake the entire asteroid. Eros also has several surface features hinting at great internal porosity, or large open cavities. Asteroids do appear to largely be rubble piles (at least based on our initial spacecraft observations), with important consequences for impact processes.

Erik Asphaug also summarized the current situa-tion for impact processes on asteroids and comets (to a lesser degree), seconding and amplifying many of Chapman's observations. Eric is pessimistic about attempts to scale lab results for uniform, unbrecciated meteorites or terrestrial rocks to explain asteroids and comets, which he also believes to largely be rubble piles. He also made the point that regolith cohesive forces behave quite counter intuitively on bodies with very low gravity, a fact that hinders our understanding of the behavior of small grains during impacts. Finally, Eric discussed recent advances in modeling codes that show some promise of better reproducing impact processes on asteroids and comets, but cautioned that progress here will be probably be rather slow.

Naomi Onose described results of laboratory simulations of oblique impacts into gypsum, carried out by Akira Fujiwara's group at the Institute of Space and Astronautical Sciences (ISAS). The ISAS experiments were carried out as part of preparations for the Japanese Muses-C Mission. This mission, scheduled to launch in May of 2003, will attempt to rendezvous with the very small (0.5 km diameter) near-Earth asteroid 1998 SF36 (which will probably be renamed soon), and perform 1-3 touch and go maneuvers during which samples of asteroid regolith will be collected for return to earth. If successful this would be the first of many such missions planned by Japanese space scientists.

TRANSCRIPTS OF DISCUSSION SESSIONS AND SELECTED TALKS

We have collected here 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. Transcripts of the Friday and Saturday sessions were generated by Elisabetta Pierazzo. Transcripts of the Sunday morning session and the Sunday afternoon final discussion session were generated by Robbie Herrick. Mike Zolensky transcribed the Sunday afternoon session on low-gravity cratering.

Friday, February 7, 2003, Morning Session

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 followed by 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 that 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.

ROCK PROPERTIES THAT NEED TO BE KNOWN FOR THEORETICAL MODELING
(Elisabetta Pierazzo chair)

KEITH HOLSAPPLE, K.A.: What do we need to know to model impact processes?

Holsapple discusses the current state of material modeling, both in terms of equations of state and constitutive equations. He concludes pointing out that although we are far from a good understanding and complete modeling of material properties, so far we have already learned a lot. He then lists a few new directions that have been taken by different groups that already show some new and encouraging results.

QUESTIONS/COMMENTS TO HOLSAPPLE'S INVITED

Asphaug: There are a couple more complexities that I would like to throw into the mix. One is that the strength curves depend strongly on density for solids, and a lot of these codes have troubles in getting density right to 10^{-4}. Not the Eulerian and grid-Lagrangian codes, but in SPH it is not a simple trick, so when you do a lot of SPH modeling it is not easy to get these transitions into a fractured phase, for instance, where a density difference of 10^{-6} can trigger fracture, and it might be totally artificial. Rock strength depending on scale: that is another issue where, going back to Jay's talk about trying to model the small and the big, you put in a strength of a 1/10 is great to model something a thousands time as large, but that means that this little rock sitting in

the calculation somehow knows how big of a place it belongs to. It is a sort of philosophical conjecture, but that has to be addressed as well, so you need some sense of scale independence at that level. Regarding porosity, this is my question to you: To what extent is Mach number included in calculations with porosity, because sound speed will go down to tens of meters per seconds?

Yes, the P-alpha model, believe it or not, is coupled into the thermodynamics, so the sound speed of the pores is part of the output; I mean it is part of the model. Whether it is right or not, we do not have the test really very well.

Asphaug: So will it shock up again, when you encounter a more porous target material...

You squash it down and ultimately you get the wave going out away from it, just as you do a regular one. The proportional growth may be dominated or strongly affected by the porosity, which is an early time, and then it is the wings, which tend to flow out according to what we expect for normal materials. So some of the things that we think are true proportional growth may not be true, but in principle the model can handle all of that.

Schultz: Keith that was really enjoyable. But a couple of things: 1) the idea of proportional growth: even laboratory experiments for sand have long been known to be non-proportional. The idea that they go down deep first and then grow laterally has been known for a long time. In fact it has also been reproduced in NASA1 and NASA2 calculations as well as some of the explosions – well and the water – yes, but this was into dunite, the calculations have shown that there is no proportional growth for some time. 2) My real question/comment is: You emphasize the idea of the point source model, and yet we know from oblique impacts that it is definitely not a point source. In fact that persist to a very late times. So even though it maybe works in terms of your pressure decay curves, it does not seem to be expressed in terms of the ultimate flow that you see. And that extends to very late times even within gravity control growth.

But isn't that generally true only for very, very shallow impacts?

Schultz: No, we see this all the way to 45°, and some of it is velocity dependent.

Well, this is an approximation. I mean, we know that, so we should not force everything into that. (At 7 km/s the normal velocity has been reduced by...)

Schultz: Ok, but we are still using a hypervelocity into sand and we are still doing something where we can alter that if you want, but we are still dealing with this issue of point source versus non point source. My only point is that the point source approximation is incredibly valuable for testing the code especially for idealized cases, but as you go away from these idealized cases, which is typical for most materials and most impacts on a planet, it does not always work.

That is why we want to rely on the code. Codes do not build that in. If it does come out it comes out, if it does not then it doesn't.

JOHN SPRAY, J.G.: Mechanisms of in situ rock displacement during hypervelocity impact: Field and microscopic observations

Spray focuses his talk on the field observations and interpretation of different kinds of pseudotachylites observed at major terrestrial structures, in particular Vredefort and Sudbury. He then ends putting forward his own difficulty in understanding how it is possible that during the early stages of an impact the target material can be pushed down elastically into the footwall, and then come back as if nothing had happened to it (or better, without clear evidence in the field of that happening).

QUESTIONS/COMMENTS TO SPRAY'S INVITED

Asphaug: This formation of melt intersperse in these highly shocked rocks, is there any evidence that this could be something like what Keith (Holsapple) was talking about, where you shock up to the sub-solidus and then you decompress into the melt phase.

Yes, I think decompression melting would. I see it as an unloading effect. Would that work?

Melosh: No, if it were unloading the rocks around it would be highly shocked.

Oh, they are. They have maskelinite, for example.

Melosh: I think you are right, that they are friction. If it was shock decompression it would not be in planes.

Asphaug: No, not the planar one.

This is more homogeneous, localize, melting. I see that as a decompression one. It does not seem to be related to shear planes the same way.

Dressler: I have seen very similar features but in the central uplift, and I did my phD in Manicouagan. There you see about spotlight melt of about 1cm in the anorthosite, distributed something like a leopard skin. These are little melt spots, quite commonly around garnet. This has not been worked on.

Cintala: I think it is neat, because I think this is incipient melt sheet development, if you like, that has not quite gone all the way. So we are getting some history of maybe how things melt by impact.

Ahrens: You showed a diagram of the distribution of pseudotachylites, large zone periodicities, going out with the ring, which I believe was for Sudbury. Can you comment on how likely that may be for other impact craters, like Vredefort? Because obviously there are big pseudotachylites, but I did not realize they were tied to ring structures, and finally: Are there any pseudotachylites seen in the drill core from Chicxulub?

I think the relationship between pseudotachylites and rings is interesting because of ring structures on impact craters in other planets. There has been some question on what causes rings and what are they.

Ahrens: Can you see them at Vredefort?

Roger Gibson may be able to bet a comment on that.

Gibson: Yes.

I would say that a lot more work needs to be done. If you think of the size of these impact craters, to actually go and find the exposure to make up the jigsaw, the person-years involved is huge. But I would say, yes.

Gibson: I guess one of the problems in Vredefort is that you are going through a 15-km layer of target to start with, before hitting the crystalline basement. In the gold fields around Vredefort you do find layer parallel zones of pseudotachylite up to about 60 meters thick. So they are there, but I think they are being deflected into pre-existing parallel orientations.

One of the key things here is that what we need to do is more field geology of impact craters. We need detailed maps of impact craters. And there aren't many geologists working on the geology of impact craters. If you try to get money to go out and do field work, it is not that easy. It may be easier to ask for a million dollars for a piece of equipment that does some fancy analysis and get it. The amount of people working on detailed mapping of impact structures is very tiny and we need more of that. The question is how do you get funded to do it. So an answer to your question is: more work needs to be done at Vredefort. I believe some thick pseudotachylites have been found way out in (some proprietary) drill core. As for Chicxulub, I think that geophysics would indicate that there are major fault systems with large displacements. We'd love to drill one to find out if there are pseudotachylites. That would be a good test.

Melosh: Can I make a comment about pseudotachylites? I am really confused about pseudotachylites. I thought I knew about them by reading Sham paper and so on; then I visited them in South Africa, and realized that my mental picture is completely at odds with what was actually in the field. First of all, pseudotachylites are volumetrically trivial, compared to everything else. You can mostly find thin veins. The pictures that you and other people show are in quarries basically targeted at large accumulations. But if you average over a region and ask: How much of this melt is there? It is absolutely trivial and it is hard to believe that that can cause fluidization or fluid behavior of anything, because they are less that a tenth of a percent of the volume of the rock. Furthermore, there is a mechanical problem with the pseudotachylites and with this whole idea of friction melting. That is if you take a zone and you slide it, indeed you get a force time distance, it gives you the energy dissipation, but as soon as you develop melt along a plane, it lubricates it and you lose your shear stress and you cannot melt anymore. I think in looking at the melts there, and my experience is not as much as Roger's or yours, my impression is that in these zones there are very thin veins that may actually be the sliding surfaces along which friction melt occurs and that melt, which is very fluid, then flows out and fills up adjacent pockets and voids. And the thick accumulation that we see are pockets fed by sliding in the very thin veins that are adjacent to them.

Ok, firstly about fluidization, I am not suggesting that pseudotachylites are responsible for fluidization. I

agree with you in the sense that they are volumetrically small compared to the rest of the rock mass. What that indicates to me is that deformation is discrete in terms of that stage of the cratering process. We are not looking at bulk behavior we are looking at discrete behavior on fault-related type planes. I think the notion that once you get melting you do not make them any bigger, therefore they have a finite size assumes that you have a perfect system with a beautifully planar system. There are asperities and all sorts of complexities on one of these planes and you may be generating melt in one place and then because of a huge asperity, ripping that off and make huge boulders some of which would be the size of this room. Some of the fault scarps on collapsed wedges on lunar craters are several kilometers; that is a catastrophic failure, where you are going to get rubbles and angular pieces, asperities. So I do not think that is going to be just a matter of lubricating and then it just slips away merely, because it is not just a smooth plane. It is going to be curvy planar with asperities. So the idea that there is time and a pure system for feedback so that the melt can lubricate it to stop any more friction I thinks works in the lab but I do not think it works on a real slipping surface.

Gibson: *Can I just add to that? In Vredefort Uwe Reimold did some measurements a few years ago and indicated that at least 4% by volume of the rock in Vredefort, which is about 80 km in diameter, is pseudotachylite.*

Melosh: *Is it just in the collar or is it averaged over the whole thing?*

Gibson: *That is an average over the entire 80 km diameter central uplift. Now, the other thing John, I also have a bit of a problem with the S-type pseudotachylites acting as a nucleation surface for some of these large E-type zones because of this issue of lubrication. But the point is: you are indicating voluminous pseudotachylite rings outside of the shock zone. I'll be talking a little bit about this tomorrow, but the bulk of the shock effects are confined to the vicinity of the central uplift. Now I know in Sudbury it is difficult to see that, where the central uplift actually is. But in Vredefort we have got a very clear central uplift and these big 60 m wide zones are another 40-50 km beyond that. There is not evidence of shock in those rocks, so these things have be some kind of friction-related feature.*

Yeah, I agree. I would want not say that E-type

require S-type to form. In the inner zone where you have a high shock, they can evolve from the S-type. Further out from that I agree, they are just frictional gravitationally-driven fault systems.

PANEL DISCUSSION (Holsapple, Spray, Pierazzo, Ahrens)

Zellner: *I have a question for the geologists and then for the modelers. How often do you find tektites in fields that have a homogenous versus heterogeneous composition, and what does it tell you related to the stratigraphy of the crater. And for the modelers, how easy is it to model that?*

Spray: Well, maybe there is somebody in the audience who can address that better than me. Tektites: we have not done much work on those; we are starting to look at them a little bit. I think that is a great area to do some research in terms of recognizing different composition. Somebody who has worked on the Ries perhaps?

Ahrens: I have a comment on it: I think that the tektites and the impacts melts have two things in common. I think the tektites are more extreme versions of impact melts. One of the things that are very striking of the Manicouagan impact melts, as I understand it, is that it is a very good average composition of quite a range of basement rocks that the impact structure occurred in. I think what that indicates is that the turbulence in the melt during excavation and fall back of the melt was very violent. I don't think that is a part of a calculation that we do very well, because it is small-scale physics, involving viscosity that we know our calculations do not model faithfully. So I think there is a very violent mixing occurring, which we do not really calculate in any of the calculations with any precision. I think the surface materials that the tektites are supposedly representing, for example the Moldavites form the Ries crater, they are supposed to be surface sands that were at the impact point at the Ries crater. I think that there we have another very, very extreme turbulent mixing of that material, yet it is surprisingly homogeneous, and they have occasionally a small fragment of a possible projectile material. You know there are Fe-Ni spherules in them and I believe someone discover a cohesite bleb in one of them a number of years ago. So I think generally then the impact melts are a range of turbulent mixes and the turbulence part is poorly described by any of the present family of calculations.

Pierazzo: Natasha Artemieva will probably address a little more of the work she has been doing on tektite formation and evolution from the Ries, but yes, it is not an easy process to model, especially to get the ejection of the tektites. You get material ejection and in a hydrocode you model it as a continuum; but then you have to go from the continuum to the discrete case and some other physics is coming into place. You have to add a lot more to the model, and at a certain point it is not important anymore in the hydrocode. You have to shut off the hydrocode and got with this stuff that has already been ejected and it breaking up and it is trying to move through the atmosphere. It is a whole new set of problems that we are encountering that is beyond just the impact cratering process itself. Work is in progress. I think we will be doing more and get farther with that but it is not directly connected with impact cratering where you can determine melting, how much melt you get and what is the region that is melted at a certain level, but from that on there is something else that is coming into play.

Hörz: I want to follow up on the question of melt homogenization. Tom you sort of say, basically, you cannot model it due to certain pixel size because of it is a small scale turbulent phenomenon, but I think by and large the melt sheet are an average of rocks that are hundreds, if not kilometers, if not tens of kilometers apart. If you add the population of clasts you really can demonstrate that they are tens of kilometers apart originally. So it cannot be just a small-scale kind of turbulence, it must be something that is much, much larger than the cell size.

Ahrens: Yeah, I am very impressed by that too. That was my point about Manicouagan: it looks like you took samples over a hundred kilometers, put them in a big bucket and violently mix them, and that is what you got as a melt sheet.

Hörz: But that is not just Manicouagan. It is typical for most melt sheets. There are some exception, but that is the rule. Especially if you look at the clasts too, that are in the melts. You can demonstrate where they come from.

Spray: One thing with regards to the tektites that I think is important: More work needs to be done correlating the tektite composition to the layer stratigraphy of the target source. So, is indeed the initial target material contacted the most far flung, for example, tektite material? Or does that form its own discrete area of distribution? Or the lowermost layer

then is the one above that, the layer in the target beneath it laid on top with a different composition? I think more needs to be done on that. Perhaps that is what you are alluding to?

Ahrens: But I think one of the characteristics of tektites is that they are really very homogenous and there is not a special distribution of Chemistry as far as I know. For example, just recently we learned that the Chesapeake Bay impact crater probably made the North American tektites strewn field, and it is amazing to think about an impact into the Chesapeake Bay producing tektites that are the same composition all the way from Martha's Vineyard down to Texas, which is, I guess, their range.

Pierazzo: I remember actually asking Dieter Stöffler the question: Do you have any change in composition for the Moldavites? I was wondering more about the composition with distance from the crater; something that is falling earlier is compositionally that much different or does it have any difference at all from something that went much farther away. And he did not have an answer, so I do not think that they were really looked at with that intention. They were not looked at systematically to try to understand: Do you have this kind of compositional differences? They were probably looked at as a global, and looked at the range of compositions, but never, I think, they had this kind of connection (with distance from crater). So I think there is still a lot that can be done, just observationally, by looking at the tektites to try to understand where they are coming from, where they are going, and what happened to them.

Koeberl: As somebody who has worked on tektites for over twenty years, I think I have a little comment here. Tektites are not as homogeneous in composition as people want to think. In the Australasian strewn field, for example, macroscopic tektite composition ranges from about 62% SiO_2 to 85% SiO_2. We have different groups of tektites. Trace elements compositions vary by orders of magnitude between different samples. If you want to add microtektites to the picture, you have major elements compositions vary by factors from 2 to 5. So what you are looking at maybe is: If you have one sample, that is reasonably homogeneous, unless you start looking at the micrometer to some like 10 micrometer scale, and even then tektites get heterogeneous. So, I don't think tektites, for one, can be called all that homogeneous. The second thing about the stratigraphy and the distribution of where the tektites come from, I think

we are fairly sure by now that tektites have to come from the top of the target surface. And I am going to mention a few problems associated with that tomorrow in my talk. One of the pieces of evidence that we have that in fact there is a relation between the distance of the ejecta and the target stratigraphy, as John just mentioned or asked if there is such a thing, is yes, there is, and we have data to show that. That has to do with cosmogenic radionuclides, for example ^{10}Be. Muon-Nong type tektites in the Australasian strewn field have the lowest ^{10}Be content of any of the Australasian tektites, which meant they were deeper down in the target stratigraphy. If you go to Australites, those that have flown the farthest, they have the highest ^{10}Be content, and those were the closest to the surface. It is not that clear compositionally simply because we are not quite sure how thick that layer was. It was probably only a 20 meters thick layer or so that got melted up and blasted away to form tektites. So they do not come from very deep down into the crater. They come from very close to the surface, but even then we have some handle on the distribution of the target stratigraphy.

Ahrens: We don't know the crater still, right?

Koeberl: We don't know the crater yet for the Australasian strewn field. My best guess is it is hidden underneath the Mekong river delta, because of extreme sedimentation rates there.

Newsom: I want to make another comment about melt homogeneity and heterogeneity. In melt sheets, in some cases, one of the processes that has not been discussed this morning is sliding of the melt sheet back down into the crater, and homogenization occurring from that. For example, at the Ries I think we have a doubling up of melts in the drill core. So this is another process. Of course, one of the things that need to be done is to model how fast we are going to lose that transient cavity versus the response of the melt sheet flowing back down during the readjustment period. But that readjustment of the melt in a large structure means that melt is going to be moving around a lot, after the formation of the structure. I think we do see evidence for that, and I think it was one of the reasons why people argued against an impact origin: They thought they saw inter-fingering and time relationships among melt processes and they were thinking of Meteor Crater.

Dence: I want to add more to the melt story, but first I want to ask Keith a question. As it will perhaps

become clearer tomorrow, I see differences as you go up in scale from smaller craters to larger scales in terms of the effect of the shock waves and the fracturing process on the size of the transient cavity. So the question is in part is: Can you say anything about the change of strain rate with size? Is this a factor?

Holsapple: I do not think it is a factor in melt, because in modeling of course we do not have a strain effect on melt; we have equilibrium throughout the melt.

Dence: No, I am not talking about the melting, I am talking about breaking up the crater, fracturing it.

Holsapple: Well, let me answer what I thought you were going to ask, and maybe that was part of your question. There is definitely a scale effect, because we know that the melt is all enclosed and the volume of melt has to depend upon the volume of the impactor, to a first order, while for the crater that is not true. So, as you go to increasing sizes that predicts then that you should get increasing percentage of melt, because the crater is effectively smaller compared to the melt. The crater moves in because of gravity and the melt is not affected by it. Certainly, we should see a difference in the field. We should see a larger percentage, going to a few % for small craters up to, based on field estimates, 20% for the larger craters. Amazing. It is also true that the small ones act fast, so in terms of any strain rate effect on strength the big ones act effectively weaker, but generally there the strength does not matter very much, because they are out in the gravity-dominated regime.

Dence: Well, perhaps we should wait until tomorrow, where I have a chance to discuss what I see. Certainly I agree with regards to the melt proportions going up, but also it seems to me that there are more factors than that, involving the way in which the shock moves through the target material and breaks it, in the course of this Grady-Kipp fracturing process that goes on. That also changes with scale, and I wonder whether there are strain rate effects here with size.

Holsapple: There are strain rate effects, but I don't think it plays much role in the melt. The melt all happens even closer.

Dence: No, below the melt. I am wondering beyond the melt stage, with the shock wave acting at a few

hundreds of kilobars now, working on the rock and breaking it up, being less effective at larger craters, relative to the shock pressure experienced by the rock.

Holsapple: I am still not quite sure of your question, so maybe we should talk about it more tomorrow.

Osinski: I just wanted to throw in another complication to the melt story, I think. I have been looking at the Ries impact crater as part of my PhD studies. Doing an analytical SEM study I recognize at least four different compositional types of glasses and maybe even more. So it looks like there that individual target lithologies are melting and not mixing. Even individual melt particles can range over 10 weight percent silica.

Da Silva: I did a lot with melt homogenization in volcanic systems. One of the problems we find is that it is very difficult to homogenize and mix melts of different viscosities in very shot periods of time. So my question to the impact modelers is: Does the relative viscosity difference between melts generated from a heterogeneous target not matter on the time scales we are talking about, and what is the time scale of the melting and homogenization that we are talking about?

Ahrens: I can tell you for sure that there is no way that we are even close to doing two liquid compositions. What we know occurs upon melting of rocks, where you can melt and get two different liquids, we cannot begin to model that, much less the homogenization of it. There is no intrinsic viscosity that we put in as a material property in any of these calculations; the viscosity that is used is just to keep the calculations stable. We are years away, I think, from describing what undoubtedly does occur, that you have very violent flows and the interfaces between different materials break up unstably and you get mechanical mixing as a result of that, and later possibly chemical homogenization. We are years away from ever doing that even for systems where you have rock and melt; we are not even calculating in any reasonable accurate way what you might consider a pseudotachylite breccia, which we saw many pictures of today. Our calculational methods just put melt and solid in a box, we may not even be able to keep track of those average properties after several rezonings. I think your point of what is observed is very interesting, and it just shows that we have a long way to go in the modeling.

Pierazzo: I would like to make a remark. Keep in mind what Jay was saying at the beginning of his talk: You cannot model the big, hundred kilometer size impact, and at the same time model the little micro veins or the little breccia structure. We are limited in the modeling by the cell size. In the cells we can have more than one material, you can have a cell with mixed materials, but what is going on inside, that is something that you cannot model. That is the limit of what you can do: It is a modeling an average behavior of whatever material is in that cell. So what Tom is saying is right, that we are far away. Sure, if we in the future will have these monster computers that will allow us to model with a scale down to a meter or centimeter, then maybe we may be getting to the point where we can actual begin to model that, but right now we cannot.

Melosh: Let me make a comment, though, on this business of the mechanics. There are areas where brains can go and computers dare not tread yet, and that has to do with this modeling. A lot of the misunderstanding of tektites, and their many puzzles like their water content, comes from trying to compare them to things like volcanic melts. In a volcanic melt or in a glass-making operation, you take some silicate, you warm it up, and you make the melt just barely above the melting point. Remember that in these impacts the temperatures of the melts are much higher; we start out with vapor, and the melts we are talking about are all superheated, and they are much hotter than normal volcanic experiences. So they behave in a different way. As you know, the viscosity of silicate melts depend exponentially on the temperature, after you get above the liquids. As a result, if you go up a few hundreds degree centigrade your viscosity drops by a factor of a thousand or tens of thousands. These are very runny melts, compared to anything that we are experienced with volcanically, and melt mixing is a possibility in this case.

Da Silva: I suppose. Three simple questions: First of all, there seem to be some disagreement on whether tektites are homogeneous or whether they are heterogeneous. Second, what is the time scale that all this homogenization has to happen in? Third, to Jay is: Do relative viscosity differences not matter at the temperatures that we are talking about? I agree that they are really runny, but there are relative viscosity differences related to different compositions. Do those not matter at the temperatures that you are talking about?

Melosh: Relative viscosity difference can play a role in mixing. If you have to fluids with different viscosities and you deform them, you get all kinds of instabilities.

[end of tape]

Friday, February 7, 2003, Afternoon Session

EFFECTS OF TARGET PROPERTIES ON THE CRATERING PROCESS (Kevin Housen chair)

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

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

QUESTIONS/COMMENTS TO HOUSEN'S INVITED

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

No they have all been dry.

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

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

Ahrens: There is certainly that.

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

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

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

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

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

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

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

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

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

QUESTIONS/COMMENTS TO SCHENK'S INVITED

[Only partial, because of tape problems]

Stewart: To go back to the data that we may use to

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

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

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

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

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

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

Melosh: Paul, you mentioned Vesta, and I know that you cannot get a transition diameter off of Vesta because we only see one crater on it, but there is an

enormous complex crater on Vesta. It has a diameter larger than the diameter of Vesta. If you take that morphology of a complex crater and then scale it to similar morphologies on the Moon, it follows very nicely a 1/g dependence. And Vesta has such a low surface gravity that there is an enormously long lever arm, that I think is a very nice verification of the 1/g fitting on a silicate body trend.

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

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

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

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

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

QUESTIONS/COMMENTS TO ORMO'S INVITED

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

About the first question: we can see that in the sediments, these well-known marine middle Ordovician limestones, the marine sedimentation continued immediately after the formation of the impact crater. You can have an impact in marine Ordovician sediments in southern Sweden (next to a car) today! What is important is that marine sedimentation continued immediately afterward. And then, of course, we have this resurge of sediments from the collapse of this water cavity. We can see

that they are like 200 meters of fining up sequences. There must have been a lot of water for that to form. It is not like some kind of debris flow slumping in slowly. It is something like a massive movement of water inside to generate this sequence. About the second question: I think that it is an effect that the further the material has been transported, the nearer to the point of impact it was when it got ejected. At the hinge you are the farthest from the original point of the impact. It is possible that there it was just very slowly turned over; but the rest of the material has been transported much farther. We can see that that material is very finely crushed, but it is also today a very hard breccia. It is like, if you have been to Gardnos, the impact breccia there, even if it is very finely crushed.

CONTRIBUTED PRESENTATIONS

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

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

QUESTIONS/COMMENTS TO CRAWFORD'S CONTRIBUTED

[No tape recording]

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

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

QUESTIONS/COMMENTS TO GISLER'S CONTRIBUTED

Chapman: When you say "not significant" what wave height are you talking about as not significant?

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

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

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

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

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

QUESTIONS/COMMENTS TO HOLSAPPLE'S CONTRIBUTED

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

Shock features.

Chapman: Anything else?

Cintala: Projectile contamination.

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

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

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

QUESTIONS/COMMENTS TO OSINSKI'S
CONTRIBUTED

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

[end of tape]

GENERAL DISCUSSION:

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

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

Spray: Just a comment on the sedimentary rock targets and that is: The work that we have been doing and Oz is just discussing really is getting the matrix of these breccias, which people have been previously considered fragmental or comminuted material like a dust that somehow magically glues all of this together, and forms a rock. Most people have looked at the clasts for shock features and studied them in great detail, and have done great work with those. Very few people have done any detailed

electron-microscopy study of the matrices of sedimentary breccias in impact structures. If you do that you will find that the evidence is extremely strong for them having been molten. And that is the point. It is the matrix that is gluing these breccias together, and the volume of the matrix is such that if they were clasts there is no way you can generate a hydrothermal matrix secondarily, at a later time, of that volume to actually pump up the clasts so that they are actually floating in the midst of this hydrothermal matrix. We do not really understand in our group why there has been this problem with why sedimentary targets should not melt. I think there has been a myth created in the past that Robbie alluded to earlier, when there are a number of options put forward, ten, twenty years ago, and one became favorite by authors and it became cast in stone. I think this maybe one of those myths that if people do the detailed EM they would that the matrices have been molten, even if it is a carbonate.

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

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

Herrick: Let me go back to very early in the day, when we were talking about this discussion that with the proportional growth model you have this large transient cavity that is very deep and then it all

magically goes back into place. I guess I wanted to know a little bit more about that experimental work. Basically, in terms of, say, a seismic section, getting things scaled that would need to get things back into place to then have a coherent seismic section. If you scale that to an impact experiment into sand you need to be able to show that you have layers in the sand on the order of .5 to 1 mm that then go through this process and slump back down and reconstruct themselves into mm layers. Is that what you are actually observing, is that the scale of the process that takes place?

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

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

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

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

clay, and it smeared them out flat. And yet, when it flowed back it was just amazing to see that this flow was reversible.

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

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

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

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

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

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

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

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

original transient cavity.

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

[overlapping voices]

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

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

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

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

Sharpton: That is the one that everybody ignores.

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

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

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

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

margins are now around 22 km. That is consistent also with data from Sierra Madera, which is smaller, and again you have got a measure from the detailed mapping that has been done, of the amount of shortening in the material.

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

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

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

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

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

Melosh: The volume is almost conserved.

Overlapping voices.

Holsapple: Let me change the subject. I am surprised Peter is not jumping up and saying: Oblique impact. I mean, we know oblique impacts are a whole new ball game. We obviously get very shallow craters and they will still be circular. I never thought he would be quite that long, nor did I ever think I would defend his oblique cratering, but... The other comment I would make, at least for normal impact, is if in fact they do not go deep, then we are doing something

terribly wrong with the modeling if it is a homogeneous material. If you put bedrock down there (or something like iron) then you can stop it. But if you have a relatively homogeneous material, every code calculation shows that it goes deep and then it comes back. So if it doesn't do that we are doing it wrong.

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

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

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

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

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

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

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

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

Sharpton: Oh no, there is no doubt that there is uplift, but what we are talking about, again now, is whether the uplift originate from something that can be approximated by the excavation crater depth, or uplift originates by something that is considerably deeper than that, because everything has been pushed

down and it has to come up before the distance. That is the important thing right here.

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

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

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

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

Turtle: But in the excavation I am not talking about deformation in that small scale. The large blocks can stay intact, but they are moving against each other. I

do not understand how you can actually determine how much offset there has been between these large blocks. You see the large blocks that are intact in the field, and you see faults or fractures between them, but how do you know that not much deformation has occurred along them?

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

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

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

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

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

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

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

Stewart: Can you tell accurately the total displacement that the rock went through? We talked a lot today about how our strength models are not

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

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

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

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

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

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

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

Saturday, February 8, 2003, Morning Session

THERMODYNAMICS OF IMPACT CRATERING AND DETERMINING IMPACTOR CHARACTERISTICS (Bevan French chair)

O'KEEFE, J.D., Ahrens, T.J.: Impact induced target thermo-mechanical states and particle motion histories

O'Keefe presents results of a systematic modeling work where many parameters were varied to investigate the effect of impact velocity, and material properties, as well as the strength/damage model on the model results. Data should be available on the web.

QUESTIONS/COMMENTS TO O'KEEFE'S INVITED

Asphaug: Dugan, I was wondering: How do you advect damage forward in the Eulerian calculations? Does damage follow the material as it goes through?

You code it exactly the same way you code density, temperature, and so forth (you do not have to have tracer particles). It is very straightforward; in fact the code is built so that if you want to change your models you can readily put it in.

Holsapple: It is just another state variable.

It is another state variable. In fact from a physics stand point, damage is considered to be an internal state variable.

Stewart: Dugan, this is a discussion period kind of question, but can you speak to the appropriateness of using CTH to look at late time shape and crater collapse. Because in the past we have used two different codes: we have used hydrocodes to get transient craters and then finite elements to do collapse. And there has now been this movement to using these codes with the addition of more complicated constitutive relations to look at crater collapse and late time features. We have been doing it with SALES2, doing it to look at Sailor Hat crater using CTH, and then Dugan with this KT impact. Is this something that the users community agrees on? Is it a good approach? Are we now at the level that we believe these late time features or must they be taken with skepticism? We need to talk about this. I am going to be skeptical...

Can you calculate things out to late times with a code that is limited by sound speed? It depends on how you treat the boundaries so that you do not have reflection, and to go out to late times costs you from a computational standpoint. That is one of the key issues. That is the issue with: Can I model the thing without destroying it just because I did not have a big enough mesh or the zoning was correct? That is a cost problem. Then you have to say: How good are the constitutive models? The real test there is: How much of the phenomenology are you modeling and how well does it agree? Well, first of all we are seeing faulting. We do not put in any kind of weakness at a given place; it occurs naturally in the evolution of the calculation, so that is a positive thing. And also we see the general morphology in terms of melt layers; another test would be to go back and look at the ejecta distributions. I did not go through that in detail, but one of the things that we have there is distribution of the fractures and the amount of melt as a function of distance and velocity of ejection, and we relate that to the field measurements. That is really an important thing to do. There are all kinds of tests. There is no reason why the hydrocodes (which is a misnomer, I agree with Jay on that) can't really address things to very late times. It is just a matter of money.

Stewart: Are geologic observations a sufficient test? Should we try and devise type examples for the cases of benchmarking the codes?

Ahrens: One area that I think these codes provide a good demonstration that they have a reality check is the comparison with the centrifuge calculations. The codes very closely reproduce the results of the centrifuge calculations, and we know from our understanding of scaling that increasing the g is a way of increasing the time. So that is a real benchmark test, where you get this reconstruction of the geologic section as observed in centrifuges which we know relate to long times, and here in these calculations where we run to really long times we get exactly the same result. I think that the other point that O'Keefe has made is that there is no reason that these codes should produce a fault if it didn't exist. And we do observe these circular faults and it is a feature that many people did not ever believe you could observe in a finite difference calculation.

Let me make another point, and that is: I agree very much with the earlier statement by Kevin [Housen] and I think by Keith [Holsapple], and that is that you not only go to the field but you really need to do a whole suite and a whole series of different kinds of laboratory measurements. Not only simulating the impact, but also measuring the material parameters, and how well does the code simulate that: Does it give you bulking? Does it give you all the triaxial measurements? Do you get good correlation? Some of that is not easy.

Spray: I am not biased or anything, but I am really pleased to see you generating these large displacement fault systems in your models, it is great. For the large craters I think it is very difficult to deal with them in terms of field mapping. So, a situation where these models can actually help us target certain regions for close scrutiny I this is really helpful, potentially. I was intrigued also by the deep level faults that your model seems to be revealing: Can you comment on those in terms of how you....

That is what I am getting. Of course there are the seismologists in here, the people who make the measurements. It is very difficult to go below the Moho with any kind of reflection seismology imaging techniques. Someone else has to comment about that, but are there better ways to try to get at, did those occur?

Holsapple: I think these kinds of things are really great, in particular to me it is very interesting that we finally do have this faulting idea. In fact, it has been known for a long time in the civil engineering

concrete literature, that when you have strain softening you have damage, and in fact then the damage localizes. It comes out as a fault, with all the kind of things we see in the field. That to me is very interesting, and it is nice that we do not need to introduce additional physics in order to get this late stage readjustment. A comment though, is that it would be awfully nice if we went and did a lot of physical experiments, took only that data, put it in a code and got the right answer. We are always, unfortunately all of us, in the mode of saying there is the answer, what are the knobs, what are the inputs that give us that answer, and we have so many inputs that there is no unique answer. It is the inverse problem: What do you have to put into the code to get the answer out? I think we have to be fairly skeptical of the actual numbers, but to me the idea that you see the mechanisms is very good. One final comment is, correct me if I am wrong, you talked about porosity scaling, I think you mean density scaling. I think you are using only a low density target and you do not really have any porous crush-up. Is that correct or am I wrong? Do you have a P-alpha model or some crush model?*

Yes, I did have a P-alpha model. I did not get any major difference between that and just using a snowplow.

Holsapple: Ok, but it is an actual crush-up, so you get all of the thermodynamics of the extra heating.

That is right. Yes you do.

Holsapple: Ok. I thought what you were doing was simply a low density target.

Actually, I did both, and I did not show all the calculations in which we had a gold target, solid gold impacting solid granite and those. Those follow exactly the same scaling, up to the density ratios of less than 3, and then that is when you had the changes in porous targets, when you had density ratios greater than 3. In both cases you saw instabilities. What you saw in porosity cases was the reduction in the ejecta, and changes in the cratering efficiency, and I think that is consistent with experiments.

Holsapple: So you were basically using a snowplow model for most of it.

Right.

GIBSON, R.L., Reimold, W.U.: Thermal and dynamic consequences of impact – Lessons from large impact structures.

Gibson gives an overview of shock and especially post-shock effects of impact, with special emphasis on post-impact thermal effects. He uses Vredefort as his type example, which is in agreement with work done at other large impact structures like Sudbury (current dataset of thermal effects on terrestrial structures is still small, but growing). Also, he points out how modeling has helped them to look for certain features, and constrain spatial dependence of shock effects. A problems we have to deal with, is related to the difficulties of calibrating experimental studies of shock metamorphic effects with planetary events.

QUESTIONS/COMMENTS TO GIBSON'S INVITED

Spray: I think Vredefort is really important because it is one of the best exposed deep-level central uplifts in a large crater, so this type of work is really critical. What is your feeling about the degree of uplift, and how the central uplift evolves structurally, in terms of rebound?

Since you ask, I have a slide. This is something that we can discuss if we have some more time, but basically Betty [Pierazzo] and Zibi [Turtle] work on modeling of the Vredefort has always created a bit of consternation amongst us observationalists. Maybe we ought to talk about that that sometime. Anyway, work that has been by Christiane Lana [?] on the central uplift has shown that, contrary to popular belief, what was presented about 20 years ago suggested that within the center of Vredefort we actually see the Moho exposed, in other words 30 to 35 km of exhumation has occurred, the upturning of the supracrustal rocks which occurs on the outside, and extend only within the Archean rock. You mentioned in your abstract this piston-cylinder type structure for central uplifts. What we see in this central zone is basically a uniform orientation of pre-existing fabrics, which suggests the thing has come up vertically, and there is a remarkably strong zone of rotation out to this level, and in fact the largest pseudotachylites, the example that you showed, lie at this transition. I wonder whether, because of the strain within that, we are not seeing the melt moving into that particular zone, or being generated, because that is the zone of maximum incompatibility. It is pretty exciting stuff that he has got there. As you said, it is a deep level central uplift, so this is where

we need to find the answers.

Christeson: Could you make any comment about the relationship between the central uplift and peak ring formation?

Do I have another talk? The point that I made about the fact that when you go outside of the Vredefort dome most of the structures you see are actually compressional features is one that has puzzled me for years. I think that what we are seeing in the Vredefort is the root zone of a peak ring where we are actually seeing the uplift and outward collapse of the structure. We have got a PhD currently working on the structure in the central uplift itself, an almost everything there is extensional, but it is related to outward collapse as well as radial extension, or tangential extension of the central uplift. I think, because we are below that level, we have actually lost the zone of overturning where the central uplift has collapsed outward to create the peak ring. Does that help answering your question? But it is something that really needs to be looked at.

Dence: Just on that point: Do you feel that the overturning, particularly on the North-western side, is an original feature or is it possibly due, as someone has suggested, to the whole structure being somewhat tilted?

I have got another slide on that but I won't show it... Yes, again, what Christiane's work has done is to show that the only way to reconcile the structures we see is actually to have a regional North-West tilt of the major stratigraphy prior to impact. If you ever the seismic or magnetic section through Vredefort, you do not get a perfectly circular structure. It opens out towards the South-East, as a pear shape. If you actually model a vertical impact into an inclined sequence of rock, you would actually get an elliptical type of pattern on the major unconformity between the supracrustal and the basement. But it is not an oblique impact feature, as some locals have suggested.

DENCE, M.R.: WIRGO inTIC's? [What (on Earth) is Really Going on in Terrestrial Impact Craters?]

Dence discusses the re-interpretation of geologic investigation of craters from the Canadian Shield he has worked on extensively since the 60s. Shock attenuation data suggest that at the (current) surface, the shock attenuation rate is higher than directly

down into the target. Brent (impact energy around $3 \cdot 10^{17}$ J) is one of the type-examples of simple craters, however it is far from being "simple". For larger complex craters, the data suggest that rebound causes central region to rise above the original surface, and then collapses, forming a peak ring. He ends the talk with the question: What controls the depth of Grady-Kipp fracturing and its change with crater size, and how does that affect the timing of crater collapse?

QUESTIONS/COMMENTS TO DENCE'S INVITED

Ahrens: Could you elaborate a little bit on what is appears to be a new explanation of the rapid change in the PDF number of fracture directions with shock pressure. Previously, my understanding is that when Grieve looked at these he got a very rapid apparent change in shock pressure with distance and I think you are re-interpreting these data and saying that the various PDF sets of fractures are relatively insensitive to peak shock pressure, and they all occur in a relatively constant shock pressure range that is seen by the rocks in the central uplift. Do I understand that correctly now? You are re-interpreting Grieve's results on that.

Yes, I am re-interpreting the Robertson-Grieve material here. There is a strong contrast between what you see down hole at Brent and Charlevoix. At Brent the zones are highly compressed, and you can say that the material that was like that was compressed 20 or 30 times and smeared out, and was able to do that because it was ultra-brecciated, broken up. The material in the central peak, at Charlevoix in particular, does not break up that way, it moves as solid masses kilometers across perhaps, and in effect all it does is to rotate upwards. I do not think I am changing anything; I am saying that what you see at the surface, when the material rotate upwards, is diagonal slides through the shock zones, and what I was trying to implicate was a possible net trajectory for that, from depths to the surface. It does mean that you have to move things in too. It is rather sensitive to your assumptions on how far this movement is. That is why I feel that, in some ways, we get more information about what the probable shape of that transient cavity was, from this type of data at Charlevoix, than we do from trying to reconstruct it at Brent, where all of that information is destroyed in the subsequent brecciation process.

KOEBERL, C.: Using geochemical observations

to constrain projectile types in impact cratering.

Koeberl summarizes how to determine impactor types from impact craters. Platinum Group Elements are the most commonly used elements to distinguish meteoritic from crustal material, as well as different meteoritic types (although those are not very clear). One question that arises is: How representative are available meteorite samples of projectiles that impacted hundreds of million years ago? Measurements are difficult, and furthermore not all impact structure melt sheets have impactor components (above the detection limit). Alternative samples are ejecta material, but tektites have a very minor, if any, meteoritic component (<0.1 weight%). In summary, from the observational point of view, it is necessary to improve mixing calculations. Theory can help in trying to understand the kind of fractionation occurring in impact events and what affects it.

QUESTIONS/COMMENTS TO KOEBERL'S INVITED

Herrick: Could you briefly address if there is any way to detect whether you have a comet or an asteroid?

There are few approaches to that question. Traditionally, I think, we should say it is somewhat between impossible to difficult. One of the reasons why I say it is almost impossible is that we really do not know what is the chemical composition of comets. They have not really been studied in great detail. The only way we can get a handle on that is to assume that the interplanetary dust particles (IDPs) are representative for the composition of some comets, and try to approach it that way. The second thing that makes it very difficult is that when you have a cometary impact, a cometary nucleus only contains a small proportion of rocky material, maybe 10% or something like that. Assuming, which some people do, that this will have a carbonaceous chondritic composition, you would say that the amount of cometary material that you get into the melt eventually is only 10 times smaller than that from a normal asteroidal impact. The two problems, I think, that we have to answer are: 1) mainly astronomical, related to spacecraft: we need a sample of a comet. But then one sample of one comet would not do as much good. It reminds me of the debate about the deuterium isotopic composition, and can comets supply the oceans? And we really only have data from two comets, and they are different, they do

not even agree. So, I think we need some good measurements of cometary compositions, and really do not have those. The material that is going to come back from some of the missions will help, but I am not sure to what degree. The second thing is: Theorists can help that by helping us understand what happens during cometary impacts, when you only have a very small proportion of rocky material in there. My personal preference for an answer is: No, we cannot tell the difference at this point, because we just do not have enough data. Let me just add to that. At the K/T boundary, for example, there has been always the suggestion that maybe this was a cometary impact. Now, Frank Kyte has found chunks of a meteorite in there (in ODP cores of the Pacific), which could have been part of a comet, but probably not. And when you look at the amount of iridium that has been found worldwide, this was used to the reconstruct the projectile size at about 10km in diameter. Now that assumes a chondritic composition. If you had a cometary object it would have to be much larger because you only have a small fraction of rocky material in there. That would mean a much larger crater than we actually observe.

AHRENS, T.J., O'Keefe, J.D., Stewart, S.T.: Calculation of planetary impact cratering to late times

Ahrens discusses impact simulations studies carried out by his group, mainly simulations of the Chicxulub impact event. Simulations are carried out using CTH, modeling both early and late stages of the impact, thanks to the improvement of the strength model (as discussed earlier by O'Keefe).

QUESTIONS/COMMENTS TO AHREN'S CONTRIBUTED

Spray: Tom can you say something about this oscillatory behavior and does your model show this oscillation as it progresses?

Yes. Depending on what strength you put in, if you have a completely fluid situation, it'll have several oscillations, whereas if you put in a strong material it will just make one crater and it won't even relax, it will stay a crater and be strength controlled, and you'll get anything in between. O'Keefe wants to make a comment.

O'Keefe: It is the deep-seated faults that can have reversal in direction, due to what Tom is talking about. The near-surface faults, which are due to the

overall rebounding of that crater and the collapse of the ejecta curtain, I do not think oscillate that much. It is just a series of faults that are just transferring the energy finally to the one major fault, which is the crater rim. So there is both.

Well, I guess we do not agree on this.

Herrick: I guess on the Chicxulub modeling, this is the case where there are some very tentative conclusions about the structure in particular, the whole mushroom head concept. There is a lot of debate as to whether the seismic evidence is there for that. I would say it is not, I would say much more it looks like simply the structure went up and you have that there is no evidence for a mushroom shape to the central structure that you get from the seismic data. So, I hate to see a whole suite of model runs based on a conclusion that is tentative.

Some seismic today show normal faults, and some show reverse faults around that central ring, so it is not clear to me what the truth is.

O'Keefe: What we found is: when we did not put in any damage then you got more of this mushroom-like central area. In other cases the mushroom shape was not pronounced. ...[end of tape]... the central columns is the melt. The melt distribution is: you have got a central melt zone and you have got this very thin layer across the top.

Herrick: Ok.

Osinski: I am going to do a talk tomorrow, looking at faulting around the Haughton structure. Can you do this modeling in 3D? If not, has anyone just done modeling in 2D but in the horizontal plane? Because we see radially oriented faults play a big role in the modification of the structure, and would you be able to pick it up?

That [Chicxulub] is a 3D calculation.

Osinski: Could you do it in 2D in a horizontal plane? Is it at all possible?

O'Keefe: These are all 2D cases, but you can just make a 3D.... [too low to be picked up]

But the calculations have been done. I do not know that anybody has calculated radial fractures in a 3D calculation, but perhaps somebody in the room can comment about that. I think that is beyond what

people have done.

O'Keefe: This [full 3D] would be an expensive calculation to go out to late times.

Yes, the time on this slide is 568 seconds. So we are talking quite a few minutes here. Time is money as they say.

Sharpton: ... [away from microphone; something about space issue] ... you are pushing things in toward the center so you need to accommodate that somehow.

Are you talking about mesh size?

O'Keefe: ... it is conserved mass...

You have lower resolution at later times, unless you have infinite resources.

Herrick: This is an axially symmetric calculation.

Yes it is. This is a 2D calculation.

O'Keefe: It is a 3D calculation with axial symmetry.

SUGITA, S., Hamano, K., Kadono, T., Schultz, P.H., Matsui, T.: Towards a complete measurement of the thermodynamic state of an impact-induced vapor cloud

Sugita discusses the problem of understanding the thermodynamic state of a vapor cloud from an impact. One needs to know at least two thermodynamic parameters, which is very hard to do in the laboratory. He presents a new method, based on high-speed spectroscopy, and shows some initial tests, based on experiments that use laser-simulated vapor clouds.

QUESTIONS/COMMENTS TO SUGITA'S CONTRIBUTED

Gerasimov: Segi, again the question is about the optical depth of the cloud. If you have small-scale impacts, maybe the range of the cloud is so small that the optical depth is larger than the cloud's depth. But if you have large impacts the optical depth will be very small compared to the dimensions of the cloud and you will measure only the outer range of the cloud. Then there is something close to the quenching point, then about 50% of the material will condense and you will not have the right chemical composition

of the cloud (because you believe that you know this chemical composition), and I think there will be problems in discussing the whole cloud.

It really depends on how to use this technique. This technique was developed mainly to obtain data about fundamental material properties like, how let's say gypsum evolves at high temperature, or dunite vapor would evolve at high-T. In this case the small-scale impact experiment is just fine. To answer your question about large scale: If we are lucky enough to be able to watch some big event like a SL9 type, a 1 km size impact event on a surface of planets, there is actually a way to use this type of technique. When you look at the really strongest line, you are right, the optical depth would be too large, so you do not get to see the deep inside of the vapor cloud. But if you look at a weaker line, it takes so much length to get the optical depth, so we can still use the thin approximation like in this case.

O'Keefe: But at some point during this expansion it [the cloud] becomes transparent (the Rosseland mean free path becomes large), so you can penetrate through the cloud. The issue is the temporal resolution to measure it. You'll always get an average over the whole resolution.

GERASIMOV, M.V., Dikov, Yu.P., Yakovlev, O.I.: Experimental modeling of impact-induced high-temperature processing of silicates

Gerasimov discusses the results of experimental work aimed at investigating impact-induced high-temperature processing of silicate materials. The experiments indicate that volatilization during an impact event is not a linear process: clusters tend to form during melting/vaporization; also, strong thermal reduction of Fe with subsequent agglomeration of Fe-droplets and their dispersion (mechanical volatilization) from the silicate melts occurs.

QUESTIONS/COMMENTS TO GERASIMOV'S CONTRIBUTED

O'Keefe: I have a couple of questions. One is a general one: You use two different techniques, the impact and the laser. How good is the laser in simulating impacts?

It is rather good. We have very high coincidence with both, the light gas gun and the laser.

O'Keefe. The other questions is: Have you looked at the condensation kinetics, and do you have anything that you may want to say on that, in trying to model the growth of the particles/droplets? I mean, have you modeled the growth to be able to scale it up to larger impacts, what would be the growth rater of the droplets?

I think that it is about the same kinetics, because there is oversaturation in the expanding vapor, but also the scale does not provide larger particles, because since they become large enough, they become melt droplets. So the condensed droplets have to follow the temperature range of the cloud and they must be very small to accomodate with the surrounding material. If they get too large the heat will be accumulated inside and by expansion they would be overheated and become evaporated. So, I think it is some kind of outer mechanism.

JOELEHT, A., Kirsimäe, K. Versh, E., Plado, J., Ivanov, B.: Cooling of the Kärdla impact crater: II. Impact and geothermal modeling

This is part two (part one was a poster) reporting on the thermal history of the Kärdla impact crater, a marine structure in Estonia. Data are obtained from three boreholes in the center of the crater. Impact modeling suggest a quick cooling (~ 100 years) right after the impact, with the hottest region (temperatures high enough to get water vapor) very close to the central uplift, contrary to the data which suggests a hot region near the rim. Modeling of the cooling requires good measurements of rock permeability, although there is no guarantee that present-day rock permeability corresponds to the true post-impact permeability of broken up rocks. The model shows that convective cooling is comparable to the conductive case.

[end of tape, no comments recorded]

Hagerty, J.J., NEWSOM, H.E.: Limits to the presence of impact-induced hydrothermal alteration in small impact craters on the Earth: Implications for the importance of small craters on Mars

Newsom reports results of a study of the Lonar crater (India) investigating hydrothermal alteration. They apply those results to Mars, finding a layer of altered material about 2 m thick over the course of Martian history, corresponding to about 0.7 m of water. Not a huge amount overall, but it can be important. Results

are also consistent with explaining the origin of Martian soil.

QUESTIONS/COMMENTS TO NEWSOM'S CONTRIBUTED

Koeberl: Horton, in your study of Lonar, did you look at fluid inclusions in some of those samples, because that could give you an interesting composition of the hydrothermal fluids there.

No, we have not looked at fluid inclusions. That would be a good thing to do. This is such a low temperature system that we have not really seen that.

Melosh: Horton, with all due respect about your 800 km diameter crater on Mars, I see plenty of terrestrial geologists finding circular structures, or partially circular structures on Earth and claiming that they are actually impacts. Do you have any other evidence than you can fit a circle through a couple of topographic features, to indicate that this is really an impact basin.

Unfortunately, I have looked at the geophysical signatures, and geophysical signatures for large craters on Mars are highly varied. Most of them have no geophysical signals in magnetic or gravity. The neutron data show some kind of similarity between the Cassini structure and this structure, so there are some similarities there. It really is rather remarkable the similarity with the Cassini structure in terms of having a central ring and an annular trough surrounding it. So at the moment that is our best evidence. We are going to be out on the ground in a year, and be able to find more about it. There is some chance that we will be able to find more about it from that point of view. The other thing is, of course, we have a vast amount of high resolution data pouring in. That is going to allow a better examination of the geologic situation. But if it is a crater, it is an old one and more degraded. So, it is going to be hard to confirm that.

GENERAL DISCUSSION:

French: I would like to start off by throwing out a question for people to talk about. Presentations by both modelers and geologists, or as they seem to be called now, observationalists, have provided a good deal of information about the type of things that can be done, and the type of data and models that can be produced. The questions I would like to throw out is to each one of these two communities. I would like

perhaps the modelers to comment on what geological observations of large and small impact craters might be most relevant to testing the models, and I would like to throw the reverse question to the geologists about what modeling features may be the best one for trying to check in the field or even to guide fieldwork.

Ahrens: I can only speak about some things that you would like to have more data about to compare the calculations. I think the mapping of deformation features is really very important because that is something that you calculate. The models that you calculate depend very much on material properties, and at least you can measure those to some degree. For example, we have been looking at Meteor Crater (Ai has a poster in which she has looked at the depth of cracking underneath Meteor Crater), but there is very little data on other craters. There is no data on Lonar crater, and it would be very nice to understand how deeply it has been shattered. It would also be useful to have a map of faults around such craters. Drilling provides really hard-core information about where the zones of localized shearing are. I think that is really important. There has been a lot of work done on PDFs in the Canadian craters, but relatively little has been done on other minerals. I think the observations of impact melts, particularly in carbonates, are very poorly understood right now, and there is very little data. So there is a lot of information about shock metamorphism, that if you can understand where it occurs, it ties very closely to calculations, hence our ability to put models together.

Chapman: A lot of the discussion so far has been about terrestrial craters and observing terrestrial craters. Horton [Newsom] has reminded us that there are craters elsewhere and Jay's question about how do you interpret morphology illustrates the fact that we know and can learn far less about these craters from observations. Seems to be that what we observationalists would like from some modelers are some things pertinent to some other kinds of features of impacts than those that have been chiefly addressed here. On planetary surfaces there are several kinds of issues related to ejecta, including very far field ejecta, which is generated perhaps from things in the very early times of the cratering. On Europa we have very extensive, widely distributed secondary craters. I would like to know what processes produce those and what attributes one would expect of secondary craters in the far field. Another major problem is understanding the sampling, say, on the Moon. The kind of geology that

was done in the Apollo program is far less extensive that what we can do on the terrestrial craters, and there are all kinds of issues about the lunar samples and how they relate to craters and impact basins that caused them. There, the ejecta processes, blanketing, etc., need more study and modeling, so we can understand those sampling issues.

Herrick: I wanted to make a comment. In terms of the modeling, I have seen models of the basic processes in homogeneous media, and I have seen a jump to models of very specific craters. There is a range in there of some generic suites of models. In particular, what I would like to see are some generic models covering impact into a layered media. It would help a lot in understanding a large range of craters to have a general feel of how excavation, for instance, proceeds if you have a two-layer model, with a relatively weak layer and a relatively strong layer, and what happens as you vary the relative strength of the layers and the relative thickness, and even start including a fluid layer on the top, such as you might get in an oceanic impact. Just a generic suite of models rather than, like we have seen in the past couple of days, some very specific models where we put in a water layer that we thought matched what was happening in this particular crater at this particular time.

Pierazzo: It seems to me that a lot of geologists do not believe anything the models come up with for specific cases. So if you come up with a very general case they will believe it even less. I don't know, it is kind of a tough call.

Kyte: There are some of us who work on things that blow out of the craters. Twenty years ago when we start working on the K/T boundary and found all this iridium in it. Models back then could not make ejecta that had 10% meteoritic component in it. It was kind of a challenge, and I guess this is an acceptable thing now. It would be interesting to see these models try to take the vapor plume and figure out what is going on in it. How do you get large concentrations of meteoritic material in it? How do meteorites actually survive impacts. We know now that they do survive these big impacts events. And just as an example to toss up at you, we have got a paper coming out in Geology in March on one of these Barberton greenstone belt spherule beds, in which chromium isotopes, iridium suggests there is a meters thick spherule bed that is 50% meteoritic material. How are we going to make that? It is definitely meteoritic material in there, there is no question on that, but can

you make that with one of your models. That would be a really interesting contribution.

Pierazzo: There is some work that is being done. We are very interested in modeling the vapor plume and try to understand what is going on in there. We are still limited by of course, resolution and equation of state at this point, but there is also some experimental work that they [experimental: Sugita, Gerasimov; modeling: Abel, Rocchia, etc.] are trying to do, and coupling also modeling work of chemical fractionation. I think we just need to keep working on that and try to get as far as we can, although it is going to take a while before we can actually get anywhere with that.

Newsom: I have two comments. One about what Jay was saying, and that is: we are actually working at trying to measure the properties of these large structures that people propose. There are structures that people propose that clearly do not exist as real structures, and they are still on the list. And we may even be able to come up with some kind of confidence criteria, so that our lists are actually a little more realistic for large basin structures on Mars. But there are very distinctive features of these large structures, that if we can get some feedback from the modeling, now that we can start to address these with new versions of the codes, it could help establish whether these really are structures or not. The second part is on the thermal structure, and that is: We are beginning to get to where we can really get the amount of heat particularly in the smaller craters. This could conceivably be used to help calibrate the energy deposition and energy distribution in the models. Certainly, we should have some agreement between those.

Spray: I think one thing that would help the geologists is if the modelers could tell us the degree to which rock masses have moved by the end of the cratering process, so that we may delineate, it is probably something that can be done now actually, zones of different total displacement. I think that can help us, can guide us in the field to look for discontinuities and zones of different deformation regimes.

Ahrens: I was going to ask a question of Gerasimov: He showed some very interesting plots of volatilization, and you had a table that showed the chemical composition, and then you plotted it versus increasing volatilization. But I was not clear on what exactly you were doing experimentally. You had

these ultramafic compositions, losing all its silicate and magnesium, and ending up with a calcium and aluminum rich residuum. Perhaps you could explain what is going on here, and what relationship those experiments have with impact vaporization.

Gerasimov: It is very clear what I wanted to show. The compositions of melted droplets, which were dispersed from the melt at temperatures around 5000K, from a single impact. These droplets still fly inside this hot vapor. So volatilization proceeded to a very high level. Different droplets have different exposure, so the degree of volatilization is different, and there is a sequence of such volatilization. What I wanted to show is what direction we have and what is the effect.

Ahrens: But does the most devolatilized have a higher velocity or a lower velocity, or is it independent?

Gerasimov: It is not independent. The higher volatility is for droplets which have the higher exposure, and higher temperatures.

Abbott: I would like to see some more models of abyssal impacts. In particular, we know we get resurge gullies in the shallow water craters. How much of the crater rim gets eroded in particular in deep water impacts. Are the resurge gullies evenly spaced? Is it a function of crater size and water depth? Now with these 3D models I think it is may be possible to actually get a picture of this.

Stewart: I have question. What do we need to be able to model basin-forming impacts on planets? Self-gravity is a big problem. Can the SPH codes do basin-forming impacts? It is not even on the schedule, I think.

Asphaug: I am going to comment a little bit about SPHs capabilities tomorrow, but the issue is always how do you evolve a model so that it responds to the succession of cratering, which is some event at very low strain rate, when the model is designed to model the impact event as well. I think you almost need to have it in two separate models. I really think basin formation needs to relax from initial conditions from an impact code, just because you are trying to bracket almost tectonic strain rates to impact strain rates within the same code. As Jay said in his first talk, there is only so much you can do within a code. I wanted to enhance upon what Clark [Chapman] said, that there are other bodies that we can study, and kind

of bridge from Paul Schenk's talk, where the last little data point that could have fit his plot could have been this crater on Vesta. I wanted to emphasize the point that there are these low-gravity bodies, with a thirtieth the gravity of Earth, in the case of Vesta, or 1/10,000 the gravity of Earth, where you also have large basins forming. It is not so much that these might mimic the processes of slumping and relaxation on Earth, but you have this end-member in the other direction that you cannot achieve in the centrifuge, and you cannot really achieve things at this scale in low-gravity experiments. So you have a direct analogue but of very low gravity, and you have direct samples of these events on the Earth in the form of meteorites. I think that is a very significant crater probe as well, connecting meteorites, especially in the case of Vesta, to a large basin forming impact.

Melosh: The trouble with trying to look at the simple-complex transition on anything much smaller than Vesta is that for anything much smaller you blow it up before you get to the transition. Once you get down below about 300 km diameter you cannot get a complex crater because you destroy the asteroid first. So there is a limit to how low you can go.

Asphaug: Yes, I mean Vesta is the perfect case.

Melosh: Vesta is pretty close to having been blown up by that crater that formed on it.

Macdonald: I would really like to see the relationship between the diameter of central uplifts and the outer diameter of impact structures to be addressed in a more sophisticated manner, largely because on Earth we are dealing with so many eroded structures that we can get a good feel for the diameter of central uplift, but we really do not know the outer diameter on a lot of structures, and to be able to really address the size in a good range. It is just the estimates right now are so wild for so many impact structures on where that outer diameter is.

O'Keefe: I would like to see a definition of what a crater diameter is.

McKinnon: I did want to say that since there is this very large central peak crater on Vesta, and there is a mission going there, there is this obvious chance to look at a kind of Copernicus scale structure and even larger, where the details may be more explicit and with less erosion. Also, I do not know if Paul [Schenk] mentioned it yesterday, but there is the

large crater Herschel on Mimas, which is a small icy body, but is also a central peak structure, and it would be interesting to look at it. The mission will get there in a year and a half. You mentioned earlier about the fidelity of modeling, whether anybody would believe anything, and Tom made a very interesting comment this morning that Dugan [O'Keefe] or the two of them, the AOK team, have made calculations of just laboratory craters in sand or elevated gravities. Maybe this was discussed yesterday when unfortunately, I could not be here, but are you able to reproduce exactly laboratory sand craters with your code calculations Dugan, and my specific question is: What is the appropriate angle of internal friction in those calculations that that you measure in static experiments on sands?

O'Keefe: I leave this to comment to Kevin [Housen)] and Keith [Holsapple] who are really trying to analyze their experiments in sand and closely looked at the material properties and they are the most appropriate.

Holsapple: Let me make a couple of comments. First of all I want to go back to the previous one: People would really like to see the codes come out and show you where the rings are to be, etc. I think that for a long time it is going to be the other way: You tell us where the rings are and we'll get there. Honestly, the problem is that they are dominated by very subtle, very late stage strength things, and it is only recently that we are starting to even put these things in codes. Dugan [O'Keefe] was one of the first, and this idea of strain softening, strain localization, and all of that, is in rock mechanics but we have not had it in the codes. And those are very subtle differences: you find that if you change the angle of friction a little bit that it changes everything, because you have got this oscillation. The question is: when does it stop? So I do not think we are going to be in a good way of predicting these things. We can postdict them, but we cannot predict them. I think that is just the name of the game. Now, with regard to sand craters, everything that we have done, shows that you get very close. We use CTH the same as Dugan [O'Keefe], and you get as far as volume, you can put it right on the π_v-π_2 curve. You can do that for water, you can do it for dry sand. But there are subtle differences. When you start looking at the details, like ejection velocity, it is very hard to get them correct. I think that what is missing in our calculations in the past, is we did not let the strength depend upon the crush state, the amount of damage. That is, there was basically no damage in it. You end

up getting craters that stop, while in the real laboratories you see they go up there and then they come back. We have got quarter-space tests and actually see that happen. So, it is this subtle interplay of how do these strength models then depend upon the state it has gone through, and that is something we are just learning and we have a long ways to go yet. But we can have gross features very easily.

McKinnon: You are talking about dry sand...

Holsapple: There is not a lot that comes back in sand, but we get things like, wrong velocities, when you actually map particles. Do the laboratory test and put in tracer particles with, you can actually look at the velocities. That is a much more difficult test than simply getting the right crater shapes. There are a lot of knobs that you can turn to get the right crater shape. You can play with angle of friction, cohesion.

McKinnon: Ok, but when you measure the friction in a wooden box, I mean, does that work or do you have to use a different angle to get it.

Holsapple: I think to a first order it works, but then you need to degrade it particularly for large craters. You need to have it go way down with damage.

Unnamed: I just have a question of whether there is some kind of fluidization at all that we see in the lab.

Housen: No, none at all (you do not need it). Keith just basically said everything, but just two other comments. One is: You put in a reasonable friction angle for sand, like 30-35 degrees or somewhat in there, and it works just fine. In fact, there have been a number of dynamic shear tests that suggest that friction angles are not terribly rate dependent. And also this problem, like Keith said you can have the crater shape right and you can get the crater growth right, but you may be a factor of 2 off on ejection velocities. That is, at least, partly due to the fact that, Keith was talking about this yesterday, the P-alpha model does not really model crush-up very well, so...

Gibson: I put the summary of the diameters for Vredefort up there, and the thing I want to know is why a modeling attempt by Zibi [Turtle] and Betty [Pierazzo] have a factor of two of half of what the actual estimate from the field based study is. We are talking about a 250-300 km crater, yet the modeling, which is attempting to do exactly what everyone wants it to do, look at the distribution of shock features, the distribution of thermal features is

actually giving us such a small estimate. Who needs to do what?

Turtle: Well, I'll respond to a couple of those points. One of the things we have done in our models is to explicitly figure out the locations of various shock features and where materials have been raised to various temperatures, and compare that to what has been observed. We actually ran a series of simulations with different impact projectiles, different size craters, and the ones that matched the observed geologic data are the smaller craters. Those are the shock features. One of the reasons this is different, for example you put up Therriault et al. results, which show a crater which is twice the size as our, and that is looking at exactly the same shock features. The point we made is that in their analyses they assume that the shock contours parallels the size of the transient cavity, and that those are always at the same location at every depth, proportional to other features. One of the things that our shock model shows, for example, is that is exactly not the case. The shock contours are actually curved back in toward the surface. So you cannot scale from the shock contours directly proportionally. That is part of the things that are coming out the modeling, that kind of detail. There clearly is still some disconnect in some of the other features, and we need to look at other features in the models and see if we can match everything. I think the amount of uplift we are getting is fairly comparable to what is observed at Vredefort as well.

Dence: Basically I have tried to be very conservative in my estimates of crater sizes. I have a rule of crater economics, which goes: The rate of inflation of the sizes is a function of the interest, and the desire to have the biggest in your own backyard. There is a sociological aspect to this as well. I would like to see us really get down to careful analysis and comparison of all of these things. I think there is a fair bit of consistency in the data, and a certain amount of distortion has been entered into, not just the ones that we have discussed, but also the other ones. This aspect of layered structures versus non-layered is very important here. Certainly, layering can give you a much larger crater, proportional to the energy input, of the non-layered material. I would also like, particularly for very large craters, to see a bit more attention given to what Roger touched on, and that is the thermal gradient in the crust. I think this is particularly important for cases like Sudbury, where the impact occurred more or less in the middle of an orogenic episode. The thermal gradient was probably

quite high. We have got a body similar to Vredefort, but I think a stage beyond Vredefort in terms of degree of melting in the target material belong the zone of direct shock melting, which I think is something that we need to ponder and try to get the model. Some of the things that have come out today are very interesting from that point of view, the way the central uplift may collapse, etc.

Saturday, February 8, 2003, Afternoon Session

EJECTA EMPLACEMENT AND OBLIQUE IMPACT EFFECTS (Jay Melosh chair)

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.

Sunday, Febraury 9, 2003, Morning Session

CREATION OF THE STRUCTURE OF COMPLEX CRATERS (Bill McKinnon Chair)

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

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

QUESTIONS/COMMENTS TO SHARPTON'S INVITED

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

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

Turtle: But that's exactly the disconnect here. The models are showing that this material has to have moved, but they don't show the fine scale deformation. They're showing on the large scale that

the material has moved, but the models aren't requiring that on the very finest scale that the material is damaged. So, we can use the observations of how damaged the central uplifts are in the field to constrain whether the other parts of the crater can have moved to that extent as well.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

damage can be identified and tied to the models.

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

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

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

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

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

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

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

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

melt has gone, and these predictions should be tested. He would also like to see a program developed for benchmarking a variety of numerical codes both for early-stage and late-stage impact modeling.

QUESTIONS/COMMENTS TO COLLING'S INVITED

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

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

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

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

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

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

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

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

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

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

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

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

QUESTIONS/COMMENTS TO OSINSKI'S CONTRIBUTED

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

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

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

I'm not sure I understand.

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

were a different age.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

QUESTIONS/COMMENTS TO MCKINNON'S CONTRIBUTED

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

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

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

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

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

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

QUESTIONS/COMMENTS TO TURTLE'S CONTRIBUTED

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

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

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

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

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

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

We're predicting concentric faulting there.

Osinski: *You did mention an area of strike slip.*

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

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

In Tekton you can put faults in.

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

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

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

I'll defer to my coauthor, Gareth Collins.

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

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

Melosh: *On this strain-softening business, Tekton*

does have a strain-softening option, Zibi didn't use it, Andy Freed did, and we found it was incredibly destabilizing. And that's something I have a question for Dugan about, I suspect those deep-seated faults are numerical instabilities. Let me emphasize an important point: strain softening amplifies instabilities, and if you use it you have to very careful to tell the difference between real faults and those that are artifacts that are introduced by this constitutive relationship.

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

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

QUESTIONS/COMMENTS TO PLESCIA'S CONTRIBUTED

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

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

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

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

Dence: The aspect that I would put some emphasis on now is the ability of the anomaly to give you some feel for the fractured porosity of the material underneath. The intriguing thing is that a large amount of fracture porosity in the craters up to ~20 km across and a simple distribution in a bowl-type configuration. Above that, things flatten out and you get more complexity in the center. Apparently, the

centers seem to generally be relatively nonporous. The fractures tend to seal up. Manicouagan is a good example of that. That structure has essentially no gravity anomaly over the center.

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

Impromptu presentation about strain under craters by Melosh

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

GENERAL DISCUSSION:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Asphaug: I think it's kind of interesting about how we've progressed from the very first talk of the workshop cautioning about how we play with

numbers and look at pretty pictures to one code that does everything. You can easily get fooled from some assumptions that go into the code very early on, such as porosity. For example, you can have an assumption that crack growth is half the sound speed, which is okay early on but not valid late in the calculation. So, you've got to be very cautious and conservative.

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

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

[Inaudible comments]

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

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

Dence: We have cases, Gosses Bluff, for example, where you have a pretty clear idea of the

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

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

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

Ahrens: You don't have healing?

Hörz: The temperatures are generally higher there but I don't think it's annealing. You yield in a different way and there could be melt in what would be fractures. My point is, though, that fracture density alone is not necessarily a good indicator of

disruption. Coming back to central uplifts, I think what we can say is that many, many central uplifts are undeformed in the sense that they are stratigraphically coherent layers. Clearly the center of the central uplift is deep-seated material and as you go away from the center radially you get shallower and shallower units. But, each individual element can be fractured quite a bit. For instance, shatter-coned units are intensely fractured but they are in a coherent block. So, by and large central uplifts are stratigraphically coherent but they can be intensely fractured. There is a big difference between fracturing and being chaotic and being fractured and coherent, and we see the latter.

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

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

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

Sunday, February 9, 2003, Afternoon Session

CRATERING ON LOW-GRAVITY BODIES (Mike Zolensky Chair)

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

Holsapple: 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.

Holsapple: 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.

Housen: 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?

Izenberg: 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

Abstracts

CALCULATION OF PLANETARY IMPACT CRATERING TO LATE TIMES. Thomas J. Ahrens[1], John D. O'Keefe,[1] and Sarah T. Stewart[2], [1]Lindhurst Laboratory of Experimental Geophysics, California Institute of Technology, Pasadena, CA 91125, tja@caltech.edu, [2]Geophysical Laboratory, Carnegie Institution of Washington, Washington DC.

Simulation of impact cratering on planetary materials is crucially dependent on adequate description of shock processing of surface materials. Two recent examples of the importance of these processes is demonstrated by the simulation of impact induced flow from the impact of a ca. 10 km bolide at 20 km/sec onto the Earth. This has been inferred to have occurred along the Yucatan (Mexican) coast, 65 million years ago. This impact is inferred to have triggered global climatic change, induced by the impact devolatilization of the marine anhydrite ($CaSiO_4$) and gypsum ($CaSO_42H_2O$) deposits of the target rocks. These calculations conducted with Sandia's CTH code depend crucially upon utilizing a rock damage model which reduced crustal rock strength from 100 MPa to 1 MPa over a volume some 10^2 times that of the bolide in about 1 minute and gives rise to a 100 km diameter central peak, flat-floored crater with overturned target flap some 8 minutes after impact. Comparison of calculated post-impact deformation compares favorably with seismic profiling and drill-core data.

A second example is the formation of ejecta blankets giving rise to rampart Martian craters by fluidization with liquid water by a new impact cratering simulation and recent shock wave data on H_2O ice. We demonstrate that ground ice is melted by the impact shock within a hemisphere of radius equal to the final crater radius, resulting in excavation of a mixture of liquid water and brecciated rock into the continuous ejecta blanket. Our shock wave experiments demonstrate that ice at Mars temperature, 150 to 275 K, will begin to melt when shocked above 2.2 to 0.6 GPa, respectively, lower than previously expected. Hence, the presence of liquid water near the pre-impacted surface is not required to form fluidized ejecta. The amount of ice melted and incorporated into the ejecta blanket debris flow is within a factor of two of the subsurface ice content; therefore, debris flow modeling of fluidized ejecta morphologies may be used to quantify the amount of near-surface ground ice on Mars.

DYNAMIC TENSILE STRENGTH OF CRUSTAL ROCKS AND APPLICATION TO IMPACT CRATERING.

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Dynamic tensile strengths of two crustal rocks, San Marcos gabbro and Coconino sandstone (Meteor Crater, Arizona), were determined by carrying out flat plate impact experiments. Porosity of San Marcos gabbro is very low,[1] and the reported porosity for Coconino sandstone is ~25%.[2] Aluminum flyer plates were used for gabbro with impact velocities of 13 to 50 m/s, which produce tensile stresses in the range of 120 to 450 MPa. PMMA flyer plates were used for sandstone with impact velocities of 5 to 25 m/s, resulting tensile stresses in the range of ~13 to 55 MPa. Impact was normal to the bedding of sandstone. Tensile duration times for two cases were ~1 and ~2.3 μs, respectively. Pre-shot and post-shot ultrasonic P and S wave velocities were measured for the targets.

Velocity reduction for gabbro occurred at ~150 MPa (Fig. 1a), very close to the earlier result determined by microscopic examination.[1] The reduction of S wave is slightly higher than that of P wave. This indicates that the impact-induced cracks were either aligned,[3] or there were residual fluids within cracks,[4] or both. Data for sandstone velocity reduction was few and scattered caused by its high porosity (Fig. 1b). The range of dynamic tensile strength of Coconino sandstone is within 25 and 30 MPa (Fig. 1b). Obvious radial cracks at certain stresses indicate that deformation was not restricted to one dimensional strain as being assumed. Spall fragmentation occurred above 40 MPa (Fig. 1b).

The combination of impact velocities, U (km/s), and impactor radii, a_0 (m), are constrained by Meteor Crater fracture depth, ~850 m,[5] and the dynamic tensile fracture strength from our experiments, 40 MPa (Fig. 2). Volume of the crater for each impact was calculated using $V = 0.009 m U^{1.65}$,[6] where V is crater volume (m³), m is the mass of the impactor (kg). Volume of impact with U = 28 km/s, a_0 = 10 m is close to the real Meteor Crater volume, 7.6e7 m³.[7] Impact energy for this case is 3.08 Mt., which agrees well with theoretical calculation (3.3 to 7.4 Mt.).[10] (1 Mt.=4.18e15 J)
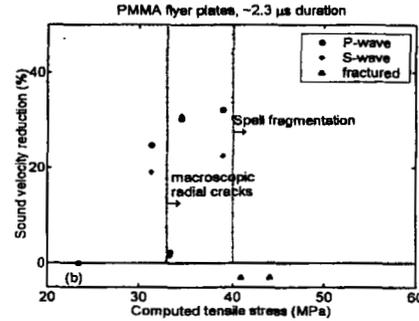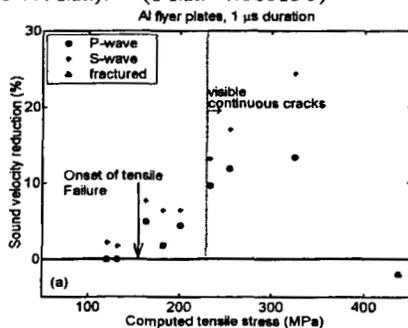
Figure 1: Velocity measurements for (a) gabbro and (b) sandstone experiments. Dashed line in (a) indicates pressure above which visible continuous cracks occurred. Dashed lines in (b) indicate pressures above which macroscopic radial cracks and spall fragmentation occurred.
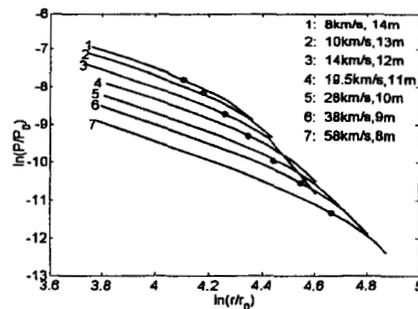
Figure 2: Normalized tensile pressure P at different depths, r, for sets of impact velocity U (km/s), and impactor radius r_0 (m) constrained by fracture depth of Meteor Crater and dynamic tensile fracture strength of Coconino Sandstone. P_0 is initial impact pressure, $P = P_r - P_z$, where $P_r = P_0(r/r_0)^{-n}$,[8] n is function of U[9] and P_z is the lithostatic pressure. Circles represent r = 850 m and P = 40 MPa.

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THE EVOLUTION OF OBLIQUE IMPACT FLOW FIELDS USING MAXWELL'S Z MODEL. J. L. B.
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Introduction: Oblique impacts are the norm rather than the exception for impact craters on planetary surfaces. This work focuses on the excavation of experimental oblique impact craters using the NASA Ames Vertical Gun Range (AVGR). Three-dimensional particle image velocimetry (3D PIV) [1, 2] is used to obtain quantitative data on ejection positions, three-dimensional velocities and angles. These data are then used to test the applicability and limitations of Maxwell's Z Model in representing the subsurface evolution of the excavation-stage flow-field center during vertical and oblique impacts.

Three-Dimensional Particle Image Velocimetry: A laser sheet is projected horizontally above the target surface during impacts at the AVGR. A ring of particles within the ejecta curtain are illuminated and imaged twice in rapid succession by two cameras above the target surface. Processing software tracks the movement of ejecta particles between time frames and combines the data from the two cameras to obtain three-dimensional velocities of ejecta particles within the laser plane. Entire ballistic trajectories are reconstructed for ejecta in all directions around the impact point, leading to ejection positions, vector velocities and angles. These quantitative data can be compared directly to numerical models and predictions from empirical models such as Maxwell's Z Model.

Maxwell's Z Model: Maxwell's Z Model [3, 4] is based on three main assumptions: (1) subsurface material flow is incompressible, (2) material moves along independent, ballistic trajectories after spallation at the surface plane and (3) the subsurface radial component of velocity is given by $u_R = \alpha(t)/R^Z$.

The Z Model, an empirical model based on explosion cratering data, places the flow-field center at the target surface. However, the flow-field centers of vertical impacts best match a moving source located at some depth below the target surface [5, 6, 7]. Croft [8] generalized the Z Model to include a term for the depth to the flow-field center.

Maxwell's Z Model predicts constant ejection angles at all ranges from the flow-field center. Croft's modified model predicts higher ejection angles than the Z Model at all ranges, but allows those ejection angles to vary with the radial distance to the flow-field center. 3D PIV measures the ejection position and ejection angle directly. With inverse modeling, it is possible to determine best-fit values for the Maxwell Z parameter and the depth to the flow-field center using Croft's modified Z Model as the forward model.

For oblique impacts, the ejection angle varies with azimuth (Figure 1) and so the value of Z will be allowed to vary as a linear function of the cosine of the

azimuth in the forward models. In Figure 1, the uprange (0°/360° azimuth) ejection angles for this 30° impact are very high, while the downrange (180° azimuth) ejection angles are low. The Z Model suggests that high ejection angles, such as those observed in the uprange curtain, imply a deeper flow-field center, while low ejection angles (such as those downrange) imply a shallower flow-field center. As time progresses, the ejection angles increase downrange and decrease uprange. This indicates that the depth to the flow-field center is changing. Conversely, if the flow-field center depth were held constant, the Z value would depend on azimuth and change with time. These data imply that a superposition of point sources may better represent the subsurface flow during oblique impacts.
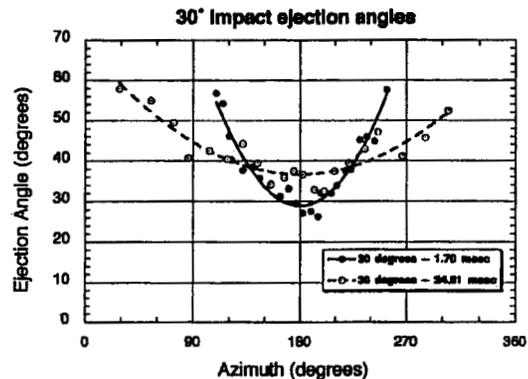
Figure 1. Ejection angles as a function of azimuth around the impact point for 30° impacts. Times represent the time after impact that the data was obtained.

Implications: Ejection angle data derived using 3D PIV is combined with Maxwell's Z Model, to determine Z values and the depth to the flow-field center for vertical and oblique impacts. The location of the flow-field center must evolve as the crater grows. A superposition of flow fields defined by the Z Model may be able to better model the excavation flow of oblique impacts.

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OBLIQUE IMPACT AND ITS EJECTA – NUMERICAL MODELING.

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Introduction: It is well known that impact events strike planetary surfaces at an angle from the surface. Assuming an isotropic flux of projectiles, probability theory indicates that the most likely angle of impact is 45° regardless of the body's gravitational field [1-2]. While crater rims appear circular down to low impact angles, the distribution of ejecta around the crater is sensitive to the angle of impact and currently serves as the best guide to obliquity of impacts. A fair amount of numerical modeling of vertical impacts has been carried out from the early 60-s [3] to the present time [e.g., 4-5 and references herein]. In vertical impacts, the axial symmetry of the process allows the simplification of the model to two dimensions (2D). Oblique impact modeling requires 3D hydrocodes and, hence, much more powerful computers. The first documented detailed oblique impact studies were carried out at Sandia National Labs' supercomputers less than 10 years ago to describe the 1994 collision of comet SL9 with Jupiter [6-7]. Since then, substantial progress in computer science has made 3D modeling a reachable objective for the scientific community.

Hydrocodes. The hydrocodes that are mostly used by the planetary impact cratering community for modeling oblique impacts are CTH [8], and SOVA [9]. Both are two-step Eulerian codes that can model multidimensional, multimaterial, large deformation, and strong shock wave physics. Both can be coupled with sophisticated equations of state models, and both have distinctive features: CTH allows for a sophisticated treatment of strength; SOVA contains a procedure to describe particle motion in an evolving ejecta-gas plume.

Melt production is a strong function of angle of impact. However, scaling laws for oblique impacts are still not well constrained. Pierazzo & Melosh [10] found that for typical rocks the amount of impact melt produced decreases with impact angle. For impacts from 90° to 45° the decrease is less than 20%, whereas for impacts at 30° the volume of melt drops to about 50% of the vertical case, declining to less than 10% for a 15° impact. In this study, the projectile volume was kept constant. For geological studies, however, it may be more useful to focus on crater volume. Ivanov and Artemieva [12] found that for relatively high impact velocities (>20 km/s) the efficiency of the cratering excavation, based on the maximum volume of the transient cavity, for a 45° impact appears to be comparable with that of a vertical impact. Early on, the

application of standard scaling laws for crater size to oblique impacts [11] suggested that for impact angles between 30° and 90° the melt ratio is more or less constant, with deviations within 20% of the average. Published laboratory data [13, 14] show that cratering efficiency in an oblique impact varies with impact velocity and projectile-target materials.

Complex targets must be treated with care. While the overall target melting seems to follow the general behavior described above, Stoffler et al [15] found that the amount of melting of finite thickness layers scales with the projectile's cross section (D^2), not its volume (D^3), as is the case for the overall melting. Furthermore, melting of near surface layers increases with impact angle decrease.

Fate of the projectile Oblique impacts show a downrange focusing of projectile material, becoming predominant at low impact angles [16]. Furthermore, most of the projectile is ejected from the opening crater in the early stages of the impact, and a significant amount of projectile material carries a downrange/upward velocity larger than escape velocity. Shock melting and vaporization in the projectile also decreases with impact angle [16,17].

Distal ejecta – tektites and meteorites from other planets. It is now widely accepted that both SNC-meteorites and tektites are produced by impact events. Geophysical and geochemical properties of tektites are consistent with an origin from high-temperature melt from the top few hundred meters of the Earth's surface that solidified in the upper atmosphere (low oxygen content) [18]. Martian meteorites originate from the upper layers of the youngest martian terrains [19, 20]. Different in their nature, both types of ejecta have a similar place of origin (upper target layers) and require high velocities (to travel distances of hundreds of km – tektites – or to escape Mars gravity – SNC meteorites). The main difference between them is in the degree of shock compression they must have experienced: melting must occur for tektites while, on the opposite end, meteorites must experience modest shocks. Since they are formed by the same mechanism, impact cratering, from the numerical modeling point of view both SNC and tektites may be treated in similar ways.

Transformation of continuum material into discrete particles is crucial for modeling ejecta during the late stages of impact cratering, when the properties of individual particles (i.e., mass, size, shape, individual velocity) become important. Modeling of ejecta as a

Oblique Impact and Its Ejecta: N. Artemieva and E. Pierazzo

continuum is a reasonable assumption only in the early stages of impact cratering. The trajectories of discrete particles in the atmosphere should be defined by a two-phases hydrodynamics that includes the interaction of the particles with the post-impact gas flow. Various processes influence the size and shape of individual particles [e.g., 21,22,23]. The approach of representative tracer particles [9,24,25] is used to avoid limitations due to computer capacity. A simplified treatment models material disruption when the material is subject to tension. The hydrodynamic cell velocity defines the initial particle velocity, and the particle's initial position within the cell is randomly defined. An empirical size distribution for solid particles is adopted from experimental studies of high-energy chemical explosions, where particle sizes range from 1 mkm to 10 cm. The diameter of molten particles ranges from 1 to 3 cm, while particle size drops to 0.01 cm when produced by condensation from a two-phase mixture.

Tektites. Tektites (high-temperature, high-velocity melt from surface layers) are consistent with a production by relatively high-velocity (>20 km/s) impact into silica-rich, possibly porous and volatile-rich, targets with impact angles around 30° -50° [26]. In particular, very oblique impacts must be excluded, since they tend to produce target melt that is highly contaminated with projectile material. In [15] a numerical modeling study was performed to evaluate whether a single collisional event (a 30° impact) could have been responsible for the formation of the Ries and Steinheim craters and the moldavite tektite strewn field. The modeled spatial particle distribution shows promising similarities with the observed one (Fig.1), like the formation of a relatively narrow tektite distribution fan, symmetric with respect to the downrange direction, and a modeled mass of tektite-type material that is within a factor of two of that estimated for the Ries-related tektites.
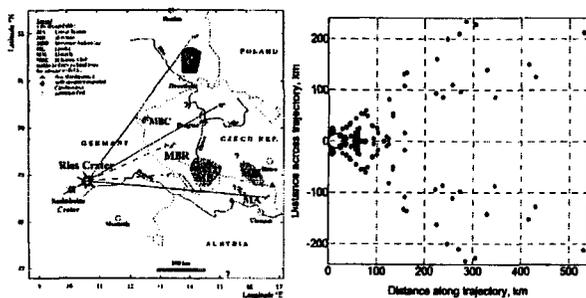
Fig.1 Observed (left) and modeled (right) distributions of moldavites.

Martian meteorites. The number of ejection events required to represent the known Martian meteorites (in the past 20 Ma) [19] combined with the known Martian

cratering rate [27] suggest the need of parent craters of 1 to 3 km in diameter. Modeling studies [28] have shown that oblique impacts (15° to 60°) are much more efficient than vertical ones [29] at producing Martian meteorites. However, the modeled crater sizes are too large (>10 km) or particles should be larger than 20 cm in diameter to keep escaping velocity in upper atmosphere [28] (the idea of large pre-entry size of martian meteorites has been confirmed independently by measurements of ^{80}Kr produced by epithermal secondary cosmic-ray neutrons of 30-300 eV energy [30]). In our study, solid, modestly shocked material (6-7% of the projectile mass) is ejected to velocities >5 km/s from a thin surface layer (~1/10 of the projectile diameter), where the peak shock pressure is distinctly limited to about 9 to 45 GPa. This pressure range is essentially confirmed by the observations [31]. Thus, recent hypotheses [32, 33] that Martian rocks can reach the Earth without being intensely shocked and heated are incorrect or at least questionable.

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FORMATION OF IMPACT CRATERS ON COMETS AND ASTEROIDS: HOW LITTLE IS KNOWN.

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Impact phenomena shaped our solar system. From the accretion of planetesimals 4.6 billion years ago to the spallation of meteorites from their parent bodies, this process has left no bit of matter untouched. The study of impact craters on small bodies therefore provides a foundation for understanding accretion and the delivery of meteorites – topics central to the origin of planets. Moreover, geologic-scale impact craters forming in low gravity reveal details of the cratering process that are hidden on high-gravity worlds like the Earth and Moon.

The detailed study of small body cratering began with efforts by Housen et al. (1979), Veverka and Thomas (1979) and others, together with efforts related to catastrophic disruption of small bodies (Chapman and Davis 1975; Fujiwara et al. 1979; Farinella et al. 1982). But the discovery of Stickney (the ~10 km diameter crater on the ~20 km diameter Martian satellite Phobos) and comparably huge divots imaged by Voyager on satellites of Jupiter and Saturn made it clear that small bodies can sustain huge wallops despite the conclusion of scaling models, notably that the impactor responsible for Stickney would have catastrophically disrupted Phobos.
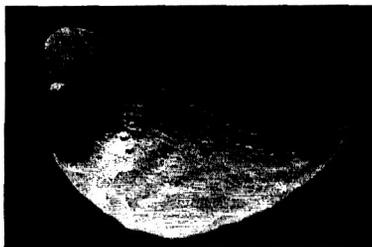
While large impact structures on bodies with significant gravity are much better understood today than they were originally, for small bodies this is not the case. We appear almost to be back-pedaling towards an earlier vision of the asteroid impact process, pioneered by Art Clokey (without much guidance from geologists) in his 1957 *Gumby* claymation adventure "The Small Planets". Although nobody today confesses to expect clear gravity signatures around ~10 m craters on ~100 m asteroids (we have yet to obtain clear images of anything much smaller than ten kilometers), few expected copious regolith on bodies the size of Eros (33×13 km) either. Surprise is the norm.
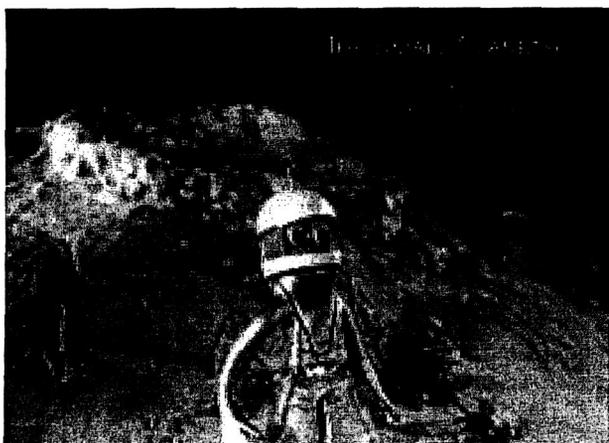
Fifteen years ago, bodies that size were widely believed to be capable of sustaining a few centimeters of regolith at best (e.g. Veverka et al. 1986). Instead, NEAR Shoemaker confirmed what had been hinted during less clearly resolved Galileo flybys of asteroids Gaspra and Ida: that Eros-sized asteroids can be awash in gravitationally bound debris (collisional or original is anybody's guess) ranging in size from ~100 m blocks (Chapman et al. 2001) to submicron grains accumulating in "ponds" (Robinson et al. 2002). Global regolith deposits on Eros range from 100's of m to undeterminable depth, and surface geophysics may even be dominated by quasi-aeolean processes such as electrostatic levitation (Lee 1996) and seismic shaking (Cheng et al. 2002; Asphaug et al. 2001).

Even on the smallest bodies yet observed, there is evidence for gravity dominance. Asteroid Ida's tiny (1.6 km) satellite Dactyl exhibits a spheroidal shape, as one would expected under self-gravitational control, and its major craters display rims and maybe central peaks[1].

But to contrast Dactyl, Phobos, Deimos and other gravity regime Lilliputians (e.g. Thomas 1998), one finds 60 km Mathilde, a body which trashes every established theory of impact cratering, and which is from impact cratering's point of view one of the most astonishing bodies. Here one sees huge craters devoid of any gravity signature, and devoid of any signature of overprinting, on a pitted spheroid lacking visible fractures or other strength-related de-

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[1] Crater rims are not unique to the gravity regime, and can form by shear bulking during plastic deformation. Bulking requires weakly cohesive granular media on the smallest bodies since plastic deformation otherwise involves impact stresses that would result in material escape. In either case an asteroid is not monolithic if one sees rimmed craters.

SMALL BODY CRATERING: Erik Asphaug

formation. Nothing is here but the huge crater bowls themselves. Ejecta has either all entirely escaped (Asphaug et al. 2002) or was never ejected at all (Housen et al. 1999), evidently in a target sufficiently porous to not communicate each blow globally, yet sufficiently cohesive for its crater rims not to collapse into softer shapes.

20 km
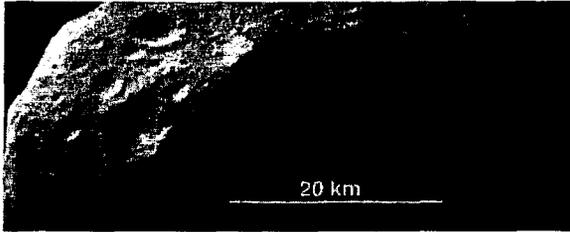
Clues to impact geophysics are everywhere. Shown below is pathological example (NEAR Image 0136819148) where four ~100 m fragments of an ejecta block appear to rest in the ~700 m diameter secondary crater they created. If this is not a chance association (the odds are small), it is the record of an impact involving geologic masses at known speed (v_{orb}^{max} ~10 m/s) and mass (~2.e10 kg). Pi-group scaling predicts a crater about half as large, perhaps because low velocity coupling is more efficient than hypervelocity coupling.

While secondary craters on asteroids may seem oddities of cratering mechanics, they have potential significance for helping us understand accretion collisions in the solar nebula which took place at similar speeds and involved similar materials, and which are a problematic theoretical bottleneck (Benz 2000).

Another kind of comparative geology can be conducted by studying the largest craters on asteroids, which span the transition from the strength to gravity regimes and exhibit whole-body effects (e.g. Stickney on Phobos; Asphaug and Melosh 1993, Thomas 1998) or the lack thereof (Mathilde, as discussed above). From these, key impact aspects can be independently derived, and exhibit a unique geologic record of the planetary impact process masked in the enormous gravity of terrestrial planets. The mechanics of cratering is preserved like nowhere on Earth.

Conclusion: Two decades of experimental, theoretical, and numerical modeling (Holsapple et al. 2002; Asphaug et al. 2002) together with spacecraft reconnaissance of asteroids has forced us to revisit pretty much everything we think we know about how asteroids collisionally evolve. Geologists have had to get used to landscapes where sunlight may be as important

a force as gravity, where cohesion less than that of dry snow can sustain cliff walls and monolithic structures, where puffballs can masquerade as rocks and viceversa. Impact theorists have had to take a big step back in their view of the process, especially for oddities like Mathilde. But Mathilde is perhaps the norm, and we await an appropriate geophysical understanding of these objects, and how craters form when gravity and strength – the fundamental forces of geology – compete for dominance.

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Small Impact Craters in Argentine Loess: a Step up from Modeling Experiments

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Introduction: Early work by Baldwin on chemical explosion craters [1] and by Moore [2] on missile impact craters allowed us to estimate energies of formation of small meteorite impact craters on the earth. Confidence in these estimates is greater if the impact craters are of the same order of size as the artificial craters. More recent work involving small-scale hypervelocity impacts into various target materials can be used for estimates of energies of formation of larger craters if gravity scaling is taken into account. This method also allows a very precise estimate of the energy of formation if certain parameters of the projectile and target material are known. The Campo del Cielo (CdC) crater field in Argentina [3] contains at least 20 small meteorite impact craters of the order of size of the artificial craters of Baldwin and Moore. Particularly valuable is the fact that the masses of the projectiles that formed these craters can be learned, because most of the craters still contain the projectiles that caused them, and they can be recovered and weighed. Estimating the energy of formation for one of these craters from its dimensions (revealed by excavation), we can use the measured mass of its projectile to estimate the velocity of impact. If we can carry out the same studies on a number of craters, we can see how well the results agree in comparing impacts from the same fall. In this case, we will have used the CdC crater field as a natural laboratory. Consistency of data from these natural replicate experiments can provide a real-world check on the validity of the small-scale model impact experiments. A sample calculation is given for Crater 10 at CdC, which is the only crater yet known in detail.

Characterization of Crater 10: See Table 1. As shown in Fig. 1, after creating a crater by shock-wave excavation, the projectile came to rest outside the crater. Presumably, shock-wave excavation ceased when the meteorite's velocity dropped below the speed of sound in the target material. Calculation of impact velocity based on the dimensions of the crater will be low by an amount equal to the speed of sound in the target material.

Impact velocity of the Crater 10 meteorite: Older estimates of impact velocity are seen in Figure 2 to be much less precise than that derived by dimensionless-ratio gravity scaling [4,5]. All of these estimates, however, are not inconsistent with an impact velocity of 3.7 km/s. While the dimensionless-ratio gravity scaling value is quite precise, it may be inaccurate for the following reasons: (1) we assumed the velocity of sound in loess to be 0.5 km/s; (2) the calculation was based on scaling factors determined for dry quartz sand, not loess; (3) the assumed density of the target material was 2100 kg/m³; (4) the assumed diameter of

the meteorite was that of an equivalent sphere; and (5) the mass of the Crater 10 meteorite was earlier believed to be 33,400kg, but later information suggests it is closer to 36,000kg. Problems (1, 3 and 5) could be mitigated by measurements in the field, and (2), the scaling factor for Argentine loess, could be determined by laboratory experiment. This would allow a much more accurate estimate of impact energy.

Discussion: While 20 small impact craters are known, there is some reason to believe there may be many more impact craters in this crater field. Campo del Cielo is a good location for linking model studies and impact craters. Further accumulation of a body of data on Campo del Cielo can lead to better interpretations of small-scale cratering on other planetary bodies. Direct analogies may be made, in general, to elongated fields of small craters on planetary surfaces, and also, specifically, to secondary crater fields around major impacts, which tend to be low-angle impacts occurring at relatively low velocities.

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Table 1. The shock-wave-excavated cavity

Depth$_{max}$	4.6 m
Length	24 m
Diameter$_{max}$	16.4 m
Mass of Projectile	33400 – 36000 kg
Impact Angle	9° with the horizontal
Azimuth of infall	N75.5°E (geog.)
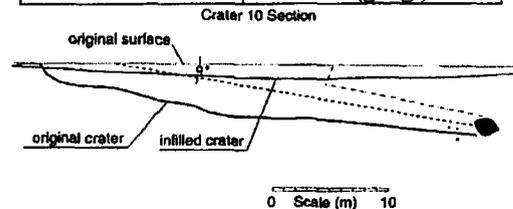
Figure 1 [above]. Cross-section of Crater 10
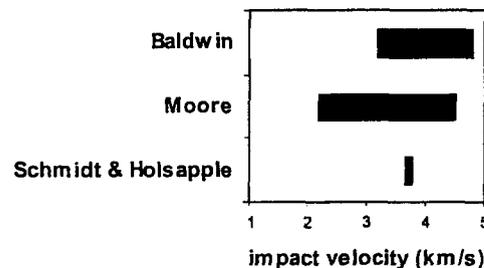
Figure 2 [above]. Impact velocity estimates for Crater 10.

CRATERING ON SMALL BODIES: LESSONS FROM EROS. C. R. Chapman[1], [1]Southwest Research Institute, Suite 400, 1050 Walnut St., Boulder CO 80302; cchapman@boulder.swri.edu.

Introduction. Cratering and regolith processes on small bodies happen continuously as interplanetary debris rains down on asteroids, comets, and planetary satellites. Butthey are very poorly observed and not well understood. On the one hand, we have laboratory experimentation at small scales and we have examination of large impact craters (e.g. Meteor Crater on Earth and imaging of abundant craters on terrestrial planets and outer planet moons). Understanding cratering on bodies of intermediate scales, tens of meters to hundreds of km in size, involves either extrapolation from our understanding of cratering phenomena at very different scales or reliance on very preliminary, incomplete examination of the observational data we now have for a few small bodies. I review the latter information here.

It has been generally understood that the role of gravity is greatly diminished for smaller bodies, so a lot of cratering phenomena studied for larger bodies is less applicable. But it would be a mistake to imagine that laboratory experiments on gravitationless rocks (usually at 1 g) are directly applicable, except perhaps to those monolithic Near Earth Asteroids (NEAs) some tens of meters in size that spin very rapidly and can be assumed to be "large bare rocks" with "negative gravity". Whereas it had once been assumed that asteroids smaller than some tens of km diameter would retain little regolith, it is increasingly apparent that regolith and megoregolith processes extend down to bodies only hundreds of meters in size, perhaps smaller. Yet these processes are very different from those that pertain to the Moon, which is our chief prototype of regolith processes. The NEAR Shoemaker spacecraft's studies of Eros provide the best evidence to date about small-body cratering processes, as well as a warning that our theoretical understanding requires anchoring by direct observations.

Eros: "Ponds", Paucity of Small Craters, and Other Mysteries. Although Eros is currently largely detached from interactions with main-belt asteroids in its Earth-approaching orbit, almost all of its cratering history must have occurred in the main belt, where it almost certainly lived for a long time and where the impact rate is orders-of-magnitude greater than in its present environment. Thus NEAR Shoemaker's year-long orbital studies of Eros should be representative of asteroidal cratering processes for medium-small (tens of km) asteroids generally – with the caveat that small bodies are made of many different materials, ranging from metal to whatever comets are made of, and we already have indications from NEAR Shoemaker's

flyby of Mathilde that responses to impacts on such bodies may be very different from what is observed on rocky Eros.

As viewed from a distance, the saturated crater fields on Eros look similar to those on Ida and, indeed, on the Moon itself. It is at smaller scales, never before studied for asteroids, where Eros' appearance diverted dramatically from expectations based on modest extrapolations from our lunar experience. Flat, level "ponds" are common on Eros and were certainly not expected. Most striking, however, is the virtual absence of small-scale (cm to meters) craters and the dominance of rocks and boulders on the surface. Apparently many of the larger boulders were distributed about Eros by the comparatively recent impact that produced the Shoemaker crater, providing insight to ejecta processes on small bodies. But, assuming that Shoemaker didn't form practically "yesterday", the dearth of small craters is extremely puzzling. Some researchers have attempted to explain the shortage by traditional geological processes; I will explain why these fail and we are being forced to turn to explanations involving shortages of small projectiles in the asteroid belt (e.g. due to the Yarkovsky Effect).

Even if projectile shortages help to explain the data, other non-lunar processes must be at work, as well. Mass-wasting processes are evident on large crater walls and the ponds reflect a still-not-understood deposition or sedimentation process. The boulder-strewn surface itself also serves to "armour" the surface against impacts. The role of seismic shaking on small bodies also must play a major role, relatively unfamiliar for larger bodies. I will summarize the observations of Eros that shed light on these various processes.

Even Smaller Bodies. An interest in sub-km scale bodies has developed in the context of imagining how a potentially dangerous NEA might be diverted. Meanwhile, observational evidence concerning their general geophysical configurations has grown rapidly. A significant proportion of these bodies (~20%) appear to have satellites or be binary in nature, and most of the remainder exhibit properties consistent with being "rubble piles" of one form or another.

Eros, with less than a millionth the mass of the Moon, turned out to be extremely non-lunar-like in its small-scale responses to impact cratering. NEAs of the size being analyzed as prototypes for deflection are a millionth the mass of Eros. We should not expect our insights from Eros, therefore, to be directly applicable to them. And as we learn more about small asteroids and comets, we must expect to be surprised.

MODELING COMPLEX CRATER COLLAPSE. G. S. Collins and E. P. Turtle, Lunar and Planetary Laboratory, University of Arizona, Tucson, AZ 85721, (Contact: gareth@lpl.arizona.edu, or turtle@lpl.arizona.edu).

Introduction: Impact crater collapse is the gravitationally driven modification of the cavity generated during the early stages of an impact event. It is the last major stage in the formation of an impact crater and has the most profound influence on the final morphology of the crater. The aim of this paper is to summarize the robust conclusions drawn from modeling crater collapse and highlight the questions that remain unanswered, particularly those that will require the collaboration of modelers and observers to answer.

Why do modeling? Abstract computer simulations provide one of the only feasible methods for studying complex crater collapse. There has been no direct observation of complex crater collapse in recorded history; large impact events are, perhaps fortunately, too infrequent. In addition, the scale of experimental studies is somewhat inappropriate for drawing conclusions about the collapse of the largest craters in the Solar System. The dominance of gravity in influencing the collapse stage of crater formation implies that the results of the small-scale laboratory collapse experiments cannot be extrapolated meaningfully to the scale of complex craters. Similarly, underground nuclear explosions, although extremely valuable in elucidating the principal features of the excavation stage, are also not of an applicable scale.

Modeling complex crater collapse: The fundamental procedure behind all numerical models of complex crater collapse is the same. First, the physical situation being simulated is simplified and divided into manageable portions, in which all properties are constant. In other words, a grid (mesh) of points and cells is defined to represent the geometry and material properties of the target. Next, the effect of external and internal forces on each of these points and cells is determined, assuming that these forces are constant during a very short interval of time, known as the time step. The mesh is then adjusted to account for the displacements induced by the net effect of the calculated forces for the duration of the time step. Repeating this process of calculating the forces acting on each cell and then adjusting the mesh allows the solution to be advanced in time until the end of the simulation.

Impact crater collapse is controlled by the competition between the gravitational forces tending to close the excavated cavity and the inherent material strength properties of the post-shock target. Thus, to simulate crater collapse, a detailed knowledge of the strength and rheologic behavior of the collapsing material is required. This is the fundamental difficulty in simulating complex crater collapse: numerous studies [for

example, 1-7] have concluded that crater collapse controlled by the well-understood standard strength models for rock materials does not involve any uplift of material from beneath the crater floor, which precludes the formation of a central peak, peak ring, or external rings; or the slumping of the transient crater walls, which precludes formation of terraces and significant widening of the crater. In other words, to reproduce the observed morphologies of complex craters, collapse requires significant, but temporary, weakening of the target material beneath the crater floor.

Several processes act during an impact event that might help explain the transient weakening associated with crater collapse. These include wholesale fracturing of the target, bulking (the decrease in density associated with the fracturing of a material and the movement of broken rock debris), acoustic fluidization (the reduction in ambient overburden pressure due to the presence of high-frequency vibrations remnant from the initial impact), melt production, thermal softening (the reduction in strength of material close to its melting temperature) and shear melting in regions of strain localization (pseudotachylite formation). Most, if not all, of these processes have been implemented and tested in numerical models of complex crater collapse; however, the relative importance of each mechanism is still poorly constrained. Thus, there is little agreement on the nature of this weakening.

Results: The impact modeling community is in strong consensus about the need for increased mobility of the target rocks surrounding large craters. Recent modeling work has constrained the required effects of the target weakening mechanism associated with complex crater collapse [6,7,8,9]. The weakening mechanism must: (1) Reduce the strength of the target material surrounding the crater by an order of magnitude or more; (2) weaken the target material surrounding the crater sufficiently for a volume of material at least equal to the crater volume to flow during collapse; and (3) in the case of peak-ring craters, mobilize this material enough such that during collapse the central uplift may overshoot the target surface, which implies an effective viscosity for the collapsing material less than $\sim 10^9$ Pa s for craters less than ~ 200 km in diameter.

There is also close agreement between the different modeling groups on the details of the collapse flow. Figure 1 illustrates the current paradigm for complex crater formation derived from recent modeling work [6,7,8,9]. Regardless of the weakening mechanism, simulation results support the observation that central peaks are the result of uplift of material originally well

MODELING COMPLEX CRATER COLLAPSE: G. S. Collins and E. Turtle

below the crater floor, and that peak-rings are the result of uplift and collapse of the central region. Figure 2 illustrates the subsurface structure of a generic peak ring crater, as inferred from various numerical simulations of complex crater collapse [7].

Figure 2 Illustration depicting the subsurface structure of a generic peak ring crater as derived from our simulation results. The dashed lines labeled A-D refer to possible stages in the erosion of an initially fresh crater. Note that the vertical scale has been exaggerated; the illustration has an aspect ratio of 1:2. Thus, the pre-impact thickness of the stratigraphic layers is on the order of D/20, where D is the final crater diameter.

Models of crater collapse have also elucidated the mechanism responsible for the formation of multiple concentric scarps around large impact structures [9]. Simulations based on the ring-tectonic theory [10] have demonstrated that inward flow of a low-viscosity layer (with effective viscosities comparable to that of the weakened material within the transient crater) is an effective way of forming rings around large craters. The mechanism responsible for this low-viscosity behavior and the degree to which it is controlled by the target structure and composition, or the impact process itself, are still not well understood.

Conclusion: Impact modeling has produced a robust paradigm for how complex craters must collapse. However, current models do not provide a complete explanation for why large impact craters collapse in this manner. Developing a complete model for the collapse of large impact craters will, therefore, require close collaboration between impact modelers, and observers. More work needs to be done to: (1) understand better each potential target weakening mechanism; and (2) establish under what conditions each mechanism may be important—does field evidence support one or more weakening mechanism? Collaboration should also concentrate on the testing and refining of numerical models of peak-ring and external-ring formation based on geological observation, geophysical data and drill cores.

Figure 1 Illustration depicting the current paradigm for how a complex crater collapses to produce its final morphology. (a) During the early stages of the impact the outward propagation of the shock wave and subsequent release wave comprehensively fractures a large region of the target (stippled) and initiates the excavation of the crater. (b) A weakened, mobile region of the target surrounding the crater (grey) enables the onset of collapse, in the form of uplift below the crater and slumping of the walls. The extent of this fluidized region decays with time, effectively freezing the crater morphology in place. In small craters the collapse is frozen before the central uplift gets too high: a central peak crater is formed. (c and d) In large impacts, however, the uplift overshoots the target surface before collapsing back down and out to generate a peak ring.

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NUMERICAL SIMULATIONS OF SILVERPIT CRATER COLLAPSE: A COMPARISON OF TEKTON AND SALES 2.

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Introduction: SALES 2 and Tekton are two numerical tools that have been used to simulate complex crater collapse [1,2]. SALES 2 is a hydrocode capable of modeling the dynamic collapse of large impact craters. It has been successfully applied to the problem of central peak and peak-ring formation [1]. Tekton is a finite-element code designed to be applied to a wide range of tectonic problems, where displacements are relatively small and the dynamics are less important. It has been used extensively to simulate the relaxation of large craters and the formation of exterior rings in multi-ring basins [2]. Here we apply both techniques to the collapse of the Silverpit crater, to compare and contrast their capabilities.

Silverpit crater: The Silverpit crater is a recently discovered, 60-65 Myr old complex crater, which lies buried beneath the North Sea, about 150 km east of Britain [3]. High-resolution images of Silverpit's subsurface structure, provided by three-dimensional seismic reflection data, reveal an inner-crater morphology similar to that expected for a 5-8 km diameter terrestrial crater. The crater walls show evidence of terrace-style slumping and there is a distinct central uplift, which may have produced a central peak in the pristine crater morphology. However, Silverpit is not a typical 5-km diameter terrestrial crater, because it exhibits multiple, concentric rings outside the main cavity. External concentric rings are normally associated with much larger impact structures, for example Chicxulub on Earth, or Orientale on the Moon. Furthermore, external rings associated with large impacts on the terrestrial planets and moons are widely-spaced, predominantly inwardly-facing, asymmetric scarps. However, the seismic data show that the external rings at Silverpit represent closely-spaced, concentric fault-bound graben, with both inwardly and outwardly facing fault-scarps [3]. This type of multi-ring structure is directly analogous to the Valhalla-type multi-ring basins found on the icy satellites. Thus, the presence and style of the multiple rings at Silverpit is surprising given both the size of the crater and its planetary setting.

The mechanics of Valhalla-type multi-ring basin formation: Theoretical and numerical modeling of multi-ring craters [2,4] suggests that external ring formation is a consequence of the basal drag exerted on a brittle, elastic surface layer by a more mobile substrate as it flows inwards to compensate for the absence of mass in the excavated crater. This model has been further constrained for Valhalla-type multi-ring basins. The formation of closely-spaced, concentric fault-bound graben, appears to require that the elastic upper layer be thin and that the mobile substrate be confined to a relatively thin layer [5,6,7]. This rheologic situation is easily explained in the context of the icy satellites; however, the presence of a thin highly mobile layer just below the surface is not a common occurrence on rocky bodies in the Solar System. In the case of the apparently unique Silverpit structure, it has been suggested that the mobile subsurface layer was caused by the presence of overpressured chalk layers at depth that acted as detachments and expedited bulk inward flow of a thin subsurface layer [3].

Numerical Simulations: We have begun to test the proposed model for the formation of the Silverpit crater using two contrasting yet complementary numerical tools: SALES 2 and Tekton. In both cases, we simulate the gravity-driven collapse of a bowl-shaped transient crater, 1-km deep and 3-km in diameter. We model the target to a radial distance of 20 km and a vertical depth of 10 km to avoid boundary effects. Our models consist of three, originally-horizontal layers, deformed using the Z-model approximation of the excavation flow. The top two layers are assigned appropriate rheologic parameters to represent the brittle upper chalk layer and the lower mobile chalk layer at Silverpit. The bottom layer occupies the remainder of the mesh. We simulate the inner-crater collapse using the acoustic fluidization model for complex crater collapse, where a fluidized region surrounding the transient crater facilitates slumping of the crater wall and uplift of the crater floor [for example 1,2]. We define the viscosity of the acoustically fluidized region to be the same as the viscosity of the mobile chalk layer.

Results: Results from our preliminary simulations suggest that the brittle upper layer must be ~1-km thick in order to reproduce the observed fault patterns and the central uplift. We will present the results of our models and the implications for both Silverpit and the two modeling methods.

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APPLICATION OF ADAPTIVE MESH REFINEMENT TO THE SIMULATION OF IMPACTS IN COMPLEX GEOMETRIES AND HETEROGEOUS MATERIALS.

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Introduction: Adaptive mesh refinement (AMR) has been used for improving computational resolution on hyperbolic problems when resources are limited [1-2]. For a mature Eulerian multi-material shock-physics code like CTH [3], adaptivity is considered a natural next step in code development [4]. Recent work has demonstrated the utility of AMR for studying shock processes in 2-D heterogeneous targets for planetary impact applications [5]. In this study, even more complex targets such as a pre-fractured 433 Eros are being simulated with 3-D AMR (Fig. 1).
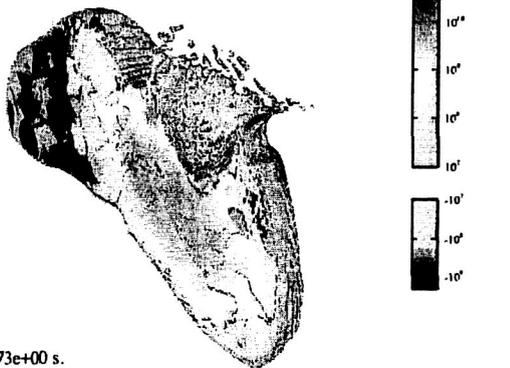
FIGURE 1. AMR CTH simulation of a 2-km dunite asteroid striking Eros at 5 km/s. Eros was constructed of thousands of random dunite spheres and tetrahedra (ρ=3.32 g/cc, C_s=6.65 km/s) with a tuff matrix and surface regolith (ρ=1.83, C_s=1.6 km/s). The density of dunite is comparable to an ordinary chondrite, the best meteoritic candidate for Eros [6]. The final density of the asteroid is 2.7g/cc similar to that measured by the NEAR spacecraft [e.g., 7]. The shape of Eros shown was obtained from data acquired by the NEAR Laser Rangefinder (shape model No. 393) [8]. In this cutaway view, color represents pressure.

Discussion: Adaptive mesh refinement allows us to maintain sufficient resolution across important features, such as the projectile or target grains, yet maintain computational efficiency. A minimum of 20-40 zones across the projectile or target grains is a requirement that has been demonstrated in many studies [e.g., 9]. Prior to AMR, such resolution has only been available for 3-D problems running on the largest parallel computers. With AMR, these calculations can be run on a small cluster of workstations. On large parallel computers, extraordinary resolution and dramatic improvements in problem scaling can be achieved (Fig. 2).

Putting together a good AMR calculation requires an artistry beyond that normally required for traditional "flat mesh" simulations. Indicators for determining regions of the mesh to target for refinement and unrefinement must be developed. CTH allows up to 20 refinement indicators constructed of operators (such as gradient magnitude) and database variables (such as pressure, density or material volume
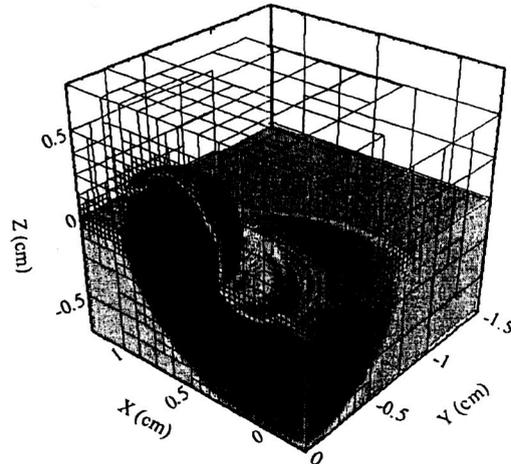
FIGURE 2. Highly oblique impact of ¼" spherical aluminum projectile onto aluminum half-space target. Level 7 adaptive mesh (equal to 160 cells across the projectile diameter) is shown. Block outlines are shown. Color represents pressure. This calculation ran 15 times faster than a comparable resolution non-AMR run.

fraction). In this presentation, we will demonstrate adaptive mesh refinement strategies for several planetary impact applications with an emphasis on understanding shock processes in heterogeneous materials.

We believe that the use of AMR should significantly improve our understanding of the cratering process because one-to-one realistic simulations of laboratory impacts are now possible even on relatively small workstations. Where once it was difficult to run a laboratory scale simulation to completion, AMR puts it within reach. AMR allows more parameter studies that can be run for much longer periods of time than was previously possible. By comparing such investigations with, for example, topographic information on the shape of craters seen on planets and asteroids, additional insights into the cratering process are within reach.

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WIRGO in TIC's?
[What (on Earth) is *Really* Going On in Terrestrial Impact Craters?]
or
Structural Evidence from Shock Metamorphism

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Canada is well endowed with impact craters formed in crystalline rocks with relatively homogeneous physical properties. Representative craters are listed below. They exhibit all the main morphological-structural variations with crater size seen in craters on other rocky planets, from small simple bowl to large peak and ring forms. They demonstrate that form is not dependent on variations in the physical properties of the target material, though such variations may be important modifiers.

Lacking stratigraphy, analysis is based largely on the imprint of shock melting and metamorphism, to give insights into the mechanics of crater formation. Attention is directed to the limit of initial brecciation due to shock wave reverberations and its position of relative to level of shock metamorphism, and the distribution of shocked target that lies in the para-autochthone outside the limit of breccia development. In addition the form and distribution of late stage shears and breccias formed by late stage movements is important.

Simple craters, exemplified by Brent (D=3.8km) allow direct comparison with models and experimental data. Results of interest include:

- The central pool of impact melt and underlying breccia at the base of the crater fill is interpreted as the remnant of the transient crater lining;
- the overlying main mass of breccias filling the final apparent crater results from late-stage slumping of large slabs bounded by a primary shear surface that conforms to a sphere segment of radius, r_s $2d_{tc}$, where d_{tc} is the transient crater depth;
- The foot of the primary shear intersects *above* the GPL at the centre of the melt pool and the rapid emplacement of slumped slabs produces further brecciation while suppressing any tendency for the centre to rise.

In the autochthonous breccias below the melt and in the underlying para-allochthone below the breccia limit, shock metamorphism weakens with depth. The apparent attenuation of the shock pulse can be compared with experimentally derived rates of attenuation in the far field of $P{\sim}R^{-2}$ (Borg, 1972) and, with due attention to low attenuation in the near field (Ahrens and O'Keefe, 1977), used to give a measure of displacements down axis. From this analysis estimates of the size of a nominal bolide of given velocity, the volume of impact melt and the energy released on impact can also be derived.

In larger complex craters (e.g. Charlevoix, D=54km, a representative, moderately eroded, peak-ring structure) apparent rates of shock attenuation measured radially form the centre in exposed para-autochthonous rocks is low, $P{\sim}R^{-0.3}$, near the centre but changes towards the margin to $P{\sim}R^{-3}$. The inflection point marks the change from uplift of deep material in the centre to subsidence of near-surface material at the margins.

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The maximum shock pressure, P, derived from the mean level of shock metamorphism in the para-autochthone of simple craters such as Brent, that is, at the down-axis limit of brecciation, is <10 GPa. This contrasts with maximum shock pressures >10GPa in the uplifted para-autochthone at the centre of larger complex craters e.g. Charlevoix. The general relationship observed indicates that P increases with increasing crater diameter, such that $P = 3.5\ D^{0.5}$, where P (in GPa) is the maximum observed shock pressure in the para-autochthone and D (km) is the final crater diameter. This can also be expressed in terms of impact energy, E (J), as $P = 2.6 \times 10^{-2}\ E^{0.14}$, with E derived from D by the relation $D = 2.75 \times 10^{-5}\ E^{0.294}$ (after Dence, 2002). The reason for this progressive weakening of brecciation due to shock wave action relative to shock metamorphism requires further study.

This result implies that, with increasing size, compression of the para-authochthone below the Breccia limit plays an increasingly larger role in contributing to the full depth of the transient crater which, in turn, appears to determine the radius of the primary shear. It follows that, where the rate of relaxation of the para-authochthone is more rapid than the propagation of the primary shear from the rim towards the centre, the shear surface projects *below* the Breccia limit and uplift in the centre is unimpeded by material slumping from the rim.

Representative Canadian Shield impact craters

Name	Type	Diameter (km)
Brent	Simple	3.8
Deep Bay	Flat-floored Central peak	9.5
Clearwater Eaxt	Central peak	20
Clearwater West	Peak ring	32
Charlevoix	Peak ring	54
Manicouagan	Peak ring	80

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EXPERIMENTAL MODELING OF IMPACT-INDUCED HIGH-TEMPERATURE PROCESSING OF SILICATES.

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Introduction: Large scale impacts of asteroids and meteorites play an important role in the evolution of planets and their satellites. Pulse input of huge energy during an impact results in noticeable changes in both mechanical and geochemical state of colliding material. The complexity of geochemical processes during an impact suggests experimental modeling as the main tool of its investigation rather than computing approach. On the other side, the modeling of mechanical issues of large scale impacts is mainly a success of computations. We need to have a good cooperation between both computer modeling of mechanical issues of an impact and experimental investigations of geochemical processes to build up a more or less realistic picture of a large-scale impact.

Experimental investigation of high-temperature modification of silicates. Experiments were done by use of hypervelocity gun facilities and laser pulse installation [1]. Some principal effects of high-temperature processing of silicates are:

Formation of clusters during vaporization. Volatilization of elements during impact-induced vaporization proceeds not only as classical volatilization of atoms and oxides but by formation of molecular clusters which can assemble a number of elements with different individual volatility. Experiments prove the formation of "enstatite", "netheline", and "wollastonite" clusters [2,3]. The formation of clusters provides less specific energy of vaporization of silicates compared to that calculated in assumption of total dissociation of materials and must be accounted for in computations.

Noticeable redox processes. The main element of silicates is oxygen which is also mobile during high-temperature processes and provide noticeable redox processes in the system. Experiments indicate simultaneous formation of mainly all possible redox states of elements [4]. Highly oxidized states of elements coexist with their reduced states. Phases of reduced carbon, iron, and other elements can be formed during impacts despite of oxidizing conditions.

Abnormal volatility of refractory elements. Experiments show a rather high mobility of elements which are usually considered as refractory and are accounted for as indicators of parts of different materials during mixing [5]. Among such elements are REE, highly siderophile elements (HSE), and other. The mechanism of abnormal volatility need more investi-

gation but it can be a result of formation of specific clusters. HSE can be mobilized into forming and dispersing metallic iron droplets [6].

Problem of mixing of colliding materials. Chemical composition of forming objects during an impact is the result of mixing of parts from naturally heterogeneous projectile and target materials and also due to selective mobility of elements. The mixing of projectile and target materials does not have sufficient coverage by computing modeling and the estimation of the volume and degree of mixing is still uncertain. Usually, the input of projectile material is considered by an account for of the increase of HSE in impactites and by isotopical considerations. None of methods is strict and can be applied only to individual samples. There is a reasonable deficit of impactites which represents a pure projectile material. Mixing seems to be a valuable factor of modification of projectile material and it should be considered using computing methods. The mechanism of mixing of projectile and target materials probably can be simulated involving Kelvin-Helmholtz and/or Reyley-Taylor instability mechanisms.

Experimental investigation of the possibility of impact-induced formation of so called "pristine" lunar glasses shows that they could be formed by an impact of a chondritic projectile into lunar basalts. The mixing of basaltic and chondritic materials together with high-temperature processing develop impact glasses with the composition similar to lunar "pristine" glasses, which is characterized by: high Mg/Mg+Fe ratio, high Al/Mg ratio, homogeneity, surface correlated volatiles, etc. [7]. The formation of metallic iron drops and their dispersion from high-temperature melts is an important mechanism for depletion of silicate melts in siderophile elements and for formation of agglutinitic glasses.

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THERMAL AND DYNAMIC CONSEQUENCES OF IMPACT – LESSONS FROM LARGE IMPACT STRUCTURES.

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Introduction: In the early years following the recognition of meteorite impact cratering as an important geological process within the Solar System, impact researchers were largely confined to inferring cratering mechanics from studies of surface crater morphologies and small-scale experiments. With the advent of sophisticated computer-based numerical simulations and high-resolution geophysics, however, researchers have begun to explore more fully the detailed 3-D structure of craters and the processes that give rise to them. This paper examines some of the issues raised by the model simulations from the perspective of the field evidence presented in impact structures, with particular reference to the Vredefort structure in South Africa.

Reality vs simulation: Impact is a short-term catastrophic process driven by the transfer of the kinetic energy of a hypervelocity projectile into a target. At a first-order approximation, the cratering process varies as a function of energy released by the impact – small impacts create simple craters whereas larger events create complex craters with central uplifts, peak rings or multiple rings. Projectiles of varying sizes, densities and velocities can effectively release similar amounts of energy and, thus, create similar structures. Additional levels of complexity can be added by varying, inter alia, the shape of the impactor, the angle of impact, and the structure and composition of the target. To a large extent, numerical simulations have allowed researchers to experiment with a wide range of input parameters and to examine the consequences of changing these variables (e.g. [1], [2]). The question remaining, however, is whether direct observation of impact structures in the field and laboratory-based experimental work can facilitate further refinement of such simulations.

The Vredefort impact structure: The 2.02 Ga Vredefort impact structure in South Africa is the world's oldest impact structure. It may lay claim to being the largest as well, however, substantial erosion (by between 7 and 10 km) has obliterated the original crater rim and impact breccias. Like the similarly large 1.85 Ga Sudbury structure, Vredefort has attracted the attention of numerical modelers (e.g. [3], [4]) in part because the high levels of erosion require indirect estimation of the size of the respective impact events and craters. In the Vredefort structure, the root zone of the central uplift – the ~90-km-wide Vredefort dome – is the best-preserved part, although impact-related structural and hydrothermal effects are evident up to radial distances of at least 100 km from the center, and possibly further afield as well. Shock effects (shatter cones, planar deformation features, high pressure quartz polymorphs and textures suggestive of diaplectic glass and mineral melt formation) are confined to the dome, and display a distribution consistent with a broad increase in maximum shock pressure radially inwards ([5], [6]). A similar broad increase in the grade of shock-induced thermal metamorphism is observed towards the center of the dome ([6]-[8]). In addition, dykes of impact melt and voluminous pseudotachylitic breccias are present in the rocks. Therriault et al. [9] estimated an original crater diameter of 270 to 300 km based on the distribution of the shock features. Henkel and Reimold [10] obtained a similar estimate from geophysical modeling. Numerical simulations by Turtle and Pierazzo [4, 11], however, have suggested a diameter as small as 120-160 km. These scaling simulations used the distribution of common shock effects such as PDFs in quartz, and the distribution of post-shock isotherms, respectively, as a basis for reconstructing the impact crater. Clearly, such a wide discrepancy requires further scrutiny. A critique of the modeling parameters and assumptions is beyond the scope of this paper. Instead, we wish to focus on the geological evidence within impact structures such as Vredefort that can assist in understanding the cratering process.

The problem with impact structures: The fundamental problem with impact structures is that their large-scale order and symmetry disguises the chaotic nature of their constituent features at smaller scales. The heterogeneous nature of shock wave interaction with rocks at the grain scale has long been known from experimental and field studies, yet the principal aim of integrating observational data from partially eroded structures such as Vredefort and Sudbury with simulation results is to obtain a match between the large-scale morphology and the spatial distribution of peak shock isobars and post-shock isotherms, on the one side, and the model results on the other. Model predictions for complex impact structures (e.g., [3], [12]) are that the shock effects are largely confined to the central uplift and that the radial inward movements that accompany central uplift formation modify the original hemispherical pattern of shock isobars into an elongate bulbous shape with a vertical long axis. As post-shock temperatures are directly proportional to the magnitude of the shock, they will display a similar elongate bulbous pattern, enhanced by interaction between the shock heating and the heat already present in the rocks

IMPACT THERMAL AND DYNAMIC EFFECTS: R.L. Gibson and W.U. Reimold

due to the pre-impact geotherm [3]. At the large scale, results from the Vredefort dome have confirmed the simulation predictions. In fact, Melosh and Ivanov's [12] and Ivanov and Deutsch's [3] results were instrumental in directing geological investigations to the central parts of the dome where the models predicted shock pressures as high as 60 GPa and post-shock temperatures in excess of 1000 °C. Whereas a previous study based on quartz PDFs in the dome by Grieve et al. [13] had been unable to confirm shock pressures of more than 10-15 GPa in these rocks, but had speculated that pressures may have been as high as 25 GPa, these studies confirmed widespread shock metamorphism of feldspars and hydrous ferromagnesian silicates at pressures in excess of 30 GPa and possibly as high as 50 GPa ([5], [6]), and post-shock temperatures of between 1000 and 1350 °C ([6], [8]). These results confirmed Grieve et al.'s [13] original contention that post-shock annealing in the core of the dome had selectively annealed PDFs, rendering the pressure estimation technique useless.

Whilst the modeling predictions and direct observations concur on the broad scale, it is important to note that Ivanov and Deutsch's [3] models are for a 200-250 km diameter structure whereas [4, 11] maintain that they have achieved good agreement with a 120-160 km diameter structure. Apart from the heterogeneous grain-scale response to shock noted from experimental studies and many other impact structures, our group has recently established larger-scale heterogeneity in the formation of pseudotachylite veins in the dome that suggests that shock pressures varied by as much as a factor of 2-3 on scales ranging from millimeters to tens of meters. This finding, which is attributed to complex reflection and refraction of the impact shock wave through the target rocks as a result of pre-existing heterogeneities, not only makes the immediate geological context in which samples for "average" peak pressure calculations are chosen of extreme importance, but also questions whether such an "average" pressure approach is realistic. The link between peak shock pressure and post-shock temperature means that this also has implications for "average" post-shock isotherms. Gibson [8] has noted highly variable post-shock metamorphic textures in rocks in the dome and widespread evidence of disequilibrium that confirm localized thermal heterogeneity. A similar conclusion was drawn by [14] from the deep borehole through the Puchezh-Katunki central uplift.

A further issue with estimation of peak shock pressures in impact structures relates to the reliability of shock experimental data in constraining peak shock pressures in natural events. [15] have recently reviewed the problems in extrapolating data from experiments to natural rocks. They caution that, because of the short duration of experiments relative to natural events, and even the design of some of these experiments, threshold pressures for the formation of certain shock effects may be considerable overestimates. Such a breakdown in basic knowledge would have fundamental implications when attempting to use shock isobar patterns to refine numerical simulations.

In addition to the shock and thermal patterns generated by an impact cratering event, numerical simulations are attempting to explain how, on a gross scale, a well-ordered structure evolves. The Vredefort dome provides a rare opportunity to access large areas of rock from deep levels within the central uplift and to test whether models such as acoustic fluidization [12] or the block model [3] can explain central uplift formation. Preliminary data from the dome by our group have failed to identify pervasive block rotation, even where substantial pseudotachylitic melts are likely to have existed during central uplift formation. Most movements appear to reflect late-stage extensional collapse of the structure along faults at a variety of scales. Further from the central uplift, impact-related deformation involves brittle-ductile folding and extensional faulting on scales of tens of meters to kilometers that also appears to be related to the latter stages of central uplift formation.

Summary: At present, numerical modeling of large impact events provides a good first-order indication of the distribution of impact-related features. However, the low spatial resolution of the models (typically of the order of kilometers) hampers full integration of the modeling results with the observed geological features and does not allow the latter to be used to refine model parameters. More work is needed to understand the local-scale interaction between a shock wave and its target rocks to assist resolution of this problem.

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TWO- AND THREE-DIMENSIONAL SIMULATIONS OF ASTEROID OCEAN IMPACTS.

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We have performed a series of two-dimensional and three-dimensional simulations of asteroid impacts into an ocean using the SAGE code from Los Alamos National Laboratory and Science Applications International Corporation. The SAGE code is a compressible Eulerian hydrodynamics code using continuous adaptive mesh refinement for following discontinuities with a fine grid while treating the bulk of the simulation more coarsely. We have used tabular equations of state for the atmosphere, water, the oceanic crust, and the mantle. In two dimensions, we simulated asteroid impactors moving at 20 km/s vertically through an exponential atmosphere into a 5 km deep ocean. The impactors were composed of mantle material (3.32 g/cc) or iron (7.8 g/cc) with diameters from 250m to 10 km. In our three-dimensional runs we simulated asteroids of 1 km diameter composed of iron moving at 20 km/s at angles of 45 and 60 degrees from the vertical. All impacts, including the oblique ones, produce large underwater cavities with nearly vertical walls followed by a collapse starting from the bottom and subsequent vertical jetting. The initial asymmetry of the oblique-impact transient crater does not persist beyond the first two minutes. Substantial amounts of water are vaporized and lofted high into the atmosphere. In the larger impacts, significant amounts of crustal material are lofted as well. Tsunamis up to a kilometer in initial height are generated by the collapse of the vertical jet. These waves are initially complex in form, and interact strongly with shocks propagating through the water and the crust. The tsunami waves are followed out to 100 km from the point of impact. Their periods and wavelengths show them to be intermediate type waves, and not (in general) shallow-water waves. At great distances, the waves decay faster than the inverse of the distance from the impact point, ignoring sea-floor topography.

A point of crucial interest is to determine the smallest asteroid for which widespread tsunami damage might be expected. To address this, we paid special attention to the wave heights generated by the vertical impacts we simulated, and the attenuation of these heights as a function of distance away from the impact point. We placed massless tracer particles on the water surface at the initial time and tracked their positions throughout the simulations. For the smaller impactors, the tracer particles executed roughly elliptical trajectories that almost (but didn't quite) close upon themselves. For the more massive impactors, the tracer trajectories were extremely complex and difficult to resolve into simple waves. Because the tracers tended to drift away from the surface, it was insufficient to track the maximum heights reached by the

tracers. Instead, we measured the amplitudes of the maximum excursions from mean tracer-particle position as a function of distance from the point of impact. These amplitudes are plotted in Figure 1, where it is seen that for all six cases in our parameter study, the waves decay with distance r from the impact point faster than $(1/r)$. The power-law indices for the least-squares fits plotted vary from -2.25 to -1.3.
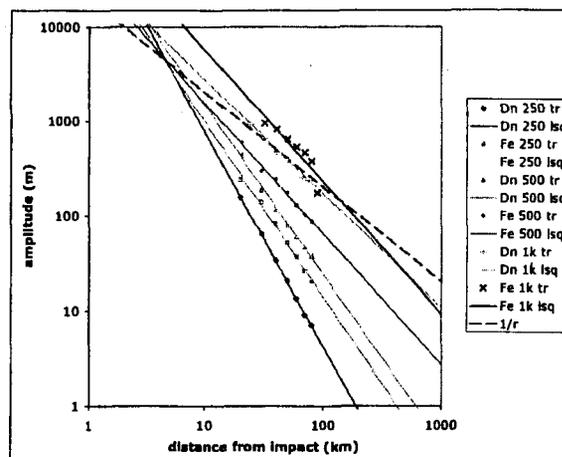
Figure 1. Tracer-particle amplitudes in asteroid-generated tsunami waves decline faster than the inverse of the distance from the impact point.

In all cases we investigated, the waves are extremely dissipative, compared to other types of tsunamis or other waves on water. The reason for this is that the perturbation giving rise to the waves, namely the impact of the asteroid and the immediate vaporization of the water along the path of entry, is hypersonic and a complex system of shocks is initiated in the water, the air, and the ocean floor basalt. The interaction of these shocks with each other and with the bounding surfaces (the air-water interface and the water-crust interface) keeps perturbing the waves that are generated so that the motion becomes, and remains, highly turbulent. This is illustrated in Figure 1, where we plot, in grayscale, density (top) and pressure (bottom) for a small portion of the computational domain near the leading wave, 34 kilometers distant from, and 5 minutes after, the impact event. Evident in the pressure plot is the highly turbulent, post-shock, atmosphere, which continues to extract energy from the propagating wave. Turbulence within the water gives rise to continued cavitation often (but not always) closely associated with the wave crests. In this frame we see the remnant of a collapsed cavitation bubble, together with the backward-propagating shock caused by its recent

Asteroid Ocean Impacts: Gisler, Weaver, Mader, & Gittings

collapse.

density grayscale, 299.5 seconds after impact

air

ocean floor basalt

pressure grayscale

air

cavitation remnant

shock from cavitation collapse

ocean floor basalt
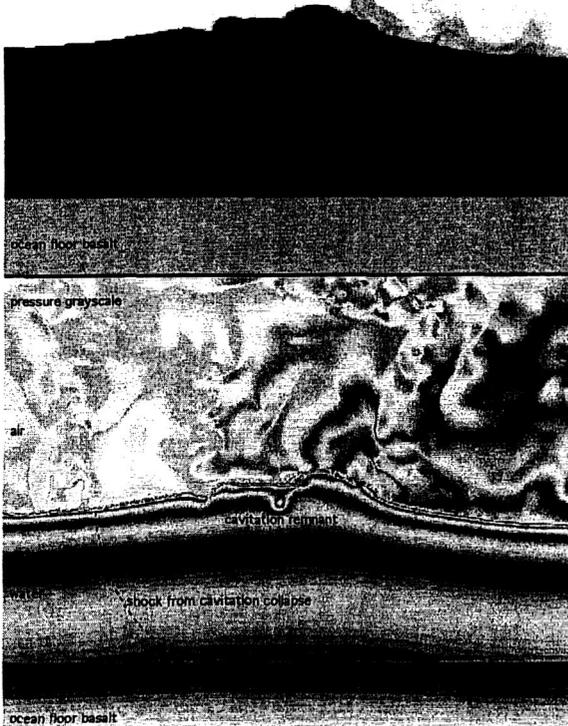
Figure 2. Plots of density (top) and pressure (bottom) in a small part of the computational volume near the crest of the leading wave in one of our simulations show evidence of continuing energy dissipation long after the impact event.

Both the extraction of wave energy by the atmosphere and the continuous generation of turbulence and cavitation within the water cause the waves to be highly dissipative; these waves are very far from energy-conserving. Unlike tsunamis generated by earthquakes or landslides, these waves decay rather more rapidly than the 1/r law expected for energy-conserving waves.

Moreover, the velocities and periods for these waves, plotted in Figure 3, are both rather less than those expected for the classical shallow-water waves generated by the usual sources of tsunamis. Both these considerations argue against significant ocean-wide damage associated with waves generated by small asteroids. To make this statement with more precision, let us establish a criterion for ocean-wide concern, in particular a one-meter wave amplitude at a distance of 1000 km from the impact event. With this criterion,

the threshold of concern indicated by our simulations is the impact of a 1000m diameter dunite asteroid at 20 km/s. Anything smaller falls below the criterion postulated above for ocean-wide damage.
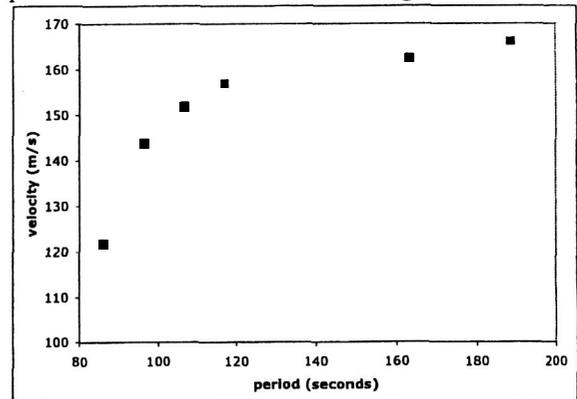
Figure 3. Velocities and periods for asteroid-induced waves are both smaller than the shallow-water values (221 km/s and ~600 seconds, in a 5-km deep ocean) expected and observed for earthquake and landslide induced tsunamis.

In fact it may even be arguable whether we are entitled to designate asteroid-impact generated waves as tsunamis, properly defined. Because of the highly dissipative and turbulent character of these waves, so different from classical tsunamis, we may need to refine our terms.

The potential for significant damage from ocean impacts of smaller asteroids must not be understated, however. The criterion adopted above (1m amplitude at 1000 km) ignores the geographical fact there are few if any parts of the earth's ocean less than 1000 km from land. Thus, while ocean-wide damage would not be expected from a 100m asteroid, for example, significant local damage will likely occur. Even for such a relatively small projectile, the input of energy to the atmosphere may be significant enough to cause disastrous (though local) firestorms.

We have also ignored ocean-floor topography in this study, and it is known that (at least for classical tsunamis) amplitudes increase dramatically as the water depth diminishes near shore. We have just begun some studies to determine if this phenomenon generalizes to impact-generated waves as well.

OBSERVATIONS OF THE TERRESTRIAL IMPACT CRATERING RECORD

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Introduction: The currently known terrestrial record of impact cratering stands at over 160 impact structures and several new examples are identified each year (1). The record, however, is a biased sample of an originally much larger population, favoring younger, larger structures in geologically stable areas of the Earth's continental crust. The largest and oldest known structures are limited to diameters of ~ 250-300 km and ages of < 2 Ga. Care must be taken, therefore, in making generalised statements regarding the record with respect to such time-integrated effects as variations in cratering rate, periodicities, etc. (e.g., 2). The terrestrial record, however, does provide cumulative observations of aspects of the cratering process and is the only available source of ground truth with respect to the structural and lithological results of large-scale natural impact events.

Some critical observations: Although attribution is often open to dispute, it is clear that detailed studies at a select number of terrestrial impact structures have provided important boundary constraints on aspects of cratering processes. Impact craters are three-dimensional structures and the ability to drill and recover core, to conduct multi-parameter geophysical surveys and to observe impact craters of similar size and morphology at different erosional levels is the ultimate strength of the terrestrial record. Concepts such as transient cavities formed by excavation and displacement and the collapse of transient cavity walls in simple craters have resulted (e.g., 3). Similarly, the confinement of significant excavation to only the central volume, with the structural preservation of near-surface lithologies exterior to this volume and the structural uplift of originally deeper-seated lithologies in the center of complex structures can be traced, in large part, to detailed and repeated observations of terrestrial impact craters (e.g., 4). Similarly, effects associated with shock metamorphism of various rock types and how its manifestation can differ (e.g., in porous targets) preceded and moved in parallel with shock-recovery experimentation. Observations have been particularly useful in understanding the effects of shock loading in the upper range of experimentally generated shock stresses, such as those leading to impact melting (e.g., 5).

Some less certain observations: Morphometric relations for terrestrial structures have been defined but are subject to considerable uncertainty, due to the effects of erosion and the statistics of small numbers (4). While it is only the more pristine terrestrial examples that can be used to define morphometries, the situation is exacerbated by the fact that many terrestrial impact craters have been studied in insufficient detail or without modern understanding of impact processes. In some cases, the literature is confined essentially to the "discovery" publication or dates from pre-Apollo to periods between Apollo missions, which were a major driver for the study of terrestrial impact structures. The impetus provided by the Apollo program has been replaced to some degree by economic and biosphere drivers. In the

U.S., government funding for studies at terrestrial impact structures appears to fall between the responsibilities of both NASA and NSF. This has tended to favor modelling studies at the expense of field work. It is clearly less costly to engage in modelling studies, but how can we, as a community, evaluate the veracity of the models without observational data from the field? (e.g., 6,7). Experimental data will not suffice to fill this gap, as there are problems with scale and understanding of the physical properties of the relevant materials, despite innovative procedures to compensate for them (e.g., 8). It is true, however, it is easier to connect observational data to later-time cratering processes because that is what they more closely reflect, representing as they do the end of the cratering process. Conversely, modeling has traditionally focussed on more early time processes in cratering events. Clearly, there are opportunities for closer partnerships of observational and modeling studies. The probem, however, is often that no one wants to be the bridesmaid!

Some closing thoughts on observations: We are very much prejudiced by the appearance of fresh lunar craters. It is the database with which we are most familiar regarding crater morphology. It is a fact, however, that some of the younger (fresher) complex craters on Earth (e.g., Ries, Haughton, Zhamanshin) do not have an emergent central peak, yet other, albeit buried, structures do (e.g., Boltysh, Moljnir). This begs a very fundamental question: Why? At first glance, it would appear to be a target effect, with the latter formed in crystalline targets and the former in mixed targets. There is also the question of the occurrence of ring or multi-ring basins on Earth (e.g.,

9). Several structures have been "proposed" as ringed basins — Manicouagan, for instance. The question is, however, are these rings erosional artefacts? Among the larger structures is Chicxulub – again proposed as a ring structure — but it is buried and inferences rely upon (sometimes conflicting) interpretations of geophysical data (e.g., 10). Drilling at Chicxulub to date has served little to address this problem. Sudbury is also often portrayed as a terrestrial example of a multi-ring basin. There are rings of pseudotachylite, or so the limited pattern of exposed outcrops suggests (e.g., 11). If these do, in fact, exist, what is their relation to the megascarps in lunar basins? Model calculations, albeit simplistic, suggest that the high-gravity environment of Earth will not necessarily produce basins in the same size range as the large multi-ring basins of the moon, due to the increased relative proportion of impact melt to cavity volume on Earth.

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ANTIPODAL HOTSPOTS ON EARTH: ARE MAJOR DEEP-OCEAN IMPACTS THE CAUSE?

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Introduction: Hotspot volcanism on Earth is restricted to relatively small areas, on the order of 100 km in diameter, and is generally believed to result from narrow upwellings of hot mantle material called 'plumes'. At first glance, hotspots appear randomly distributed. General associations with geoid highs and divergent plate margins have been noted [1], and hotspots tend to occur in provinces separated by spotless areas [2]. Matyska [3] investigated angular symmetries of hotspot distributions, and showed that the highest maxima were obtained with 180° rotations. Rampino and Caldeira [4] also conducted a statistical analysis of large and small data sets and found that more hotspots occur as nearly antipodal pairs than would be expected from random distributions.

The rise of antipodal plumes from the core-mantle boundary through a convecting mantle seems unlikely, but axial focusing of an impact's energy by the spherical Earth might underlie the antipodal pairing of hotspots [5, 6]. Such a focusing mechanism has been proposed to explain seismically disrupted terrains antipodal to major impact basins on the Moon and Mercury [7], and to explain formation of fractured crust on Mars opposite the Hellas basin—perhaps later exploited as a conduit for volcanism at Alba Patera [8]. First-order problems with this model for Earth, however, include the expected low seismic efficiency of impacts [7] and the lack of any volcanic features opposite large continental impact structures (e.g. Chicxulub).

Antipodal Hotspots: Although as many as 122 hotspots have been proposed [9], the number most commonly discussed is between 40 and 50. In a recent compilation of hotspots (plus 3) totaling 52 [10], 30 form antipodal pairs (~58%) with angular distances ranging from 168° to 179°. Deviations from 180° might be explained by an observed drift rate between hotspots of ~10 to 20 mm/yr [11].

One test of antipodal formation due to impact and focusing of seismic waves is to determine whether hotspots of a given pair began simultaneously. Tectonic recycling of oceanic crust, however, has made this impossible for most of the older pairs. For a few younger hotspot pairs, estimated initiation ages are roughly contemporaneous. Both Aitu (Cook Islands) and Tibesti (175°) are Late Miocene in age; Kerguelen and the Columbia River basalts (Yellowstone; 175°) are Early Miocene in age; the Marquesas hotspot track and Ethiopian flood basalts (Afar; 179°) are ~30 Ma in age; and the Balleny track indicates an age >40 Ma consistent with Iceland's (178°) age of ~55 Ma.

Individual hotspot pairs can generally be divided between one associated with initial flood basalts and rifting (e.g. Afar), and the other with oceanic affinities and no flood volcanism (e.g. Marquesas). It is hypothesized that the oceanic hotspots represent impact sites and those associated with voluminous volcanism the antipodal sites. Moreover, the geographic distribution of a large (122) hotspot compilation [9] shows that hotspot provinces are generally opposite oceans and that spotless areas are opposite continents [2].

Deep-Ocean Impacts: If these observations are correct, what process would cause oceanic impacts to form hotspot pairs, and continents to apparently shield their formation? A significant difference between continental and oceanic impacts is the formation of a high-pressure steam cloud above the oceanic impact site [12]. The pressure of the steam cloud might 'cap' the explosive release of energy from the seafloor impact, causing significantly more energy to be directed downwards.

A simple analog of deep-ocean impacts might be the surface blasting technique for secondary rock breaking known as 'mudcapping'. Mudcapping works due to the impulse action of explosives, which is proportional to the detonation pressure and its time of application on a rock burden [13]. A mudcap maintains the impulse pressure over a longer period of time, and the coupling effect depends partly on the amount of mudcap being used. In contrast, in a continental impact much of the energy released is likely directed upward and away from the land surface, resulting in a much lower seismic efficiency.

Conclusions: Although few impacts in the deep oceans are known, these events might have important consequences in the formation of hotspots, flood basalt provinces, and the breaking up of continental masses on Earth. Moreover, oceanic impacts, megatsunami waves, and antipodal continental flood basalts could be a major cause of global mass extinctions, and could explain rapid sea-level and abrupt ocean chemistry changes at extinction boundaries. Few models of deep-ocean impacts have been made, and it is suggested that a needed modification is the consideration of pressure effects from the steam cloud above the site upon energy release from the seafloor impact below.

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MAGNETIC FIELDS OF LUNAR IMPACT BASINS AND THEIR USE IN CONSTRAINING THE IMPACT PROCESS.

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Measurements by the Magnetometer/Electron Reflectometer instrument on the Lunar Prospector spacecraft, which completed its mapping mission in 1999, have been used to construct the first completely global maps of lunar crustal magnetic fields. Now, for the first time, we have a data set with global coverage and a sensitivity and resolution which allow us to investigate the magnetic fields of lunar impact basins and craters. As on the Earth, impact sites have a variety of magnetic signatures associated with them, ranging from nearly complete demagnetization to strong central magnetic anomalies. Observations of the magnetic fields of terrestrial basins have been used to make inferences about the impact process, and we wish to show that lunar observations can also provide valuable constraints.

It is clear that we can not achieve the same kind of magnetic field data coverage of lunar basins with measurements from orbit that we can for terrestrial basins using ground magnetometer or aeromagnetic data. Furthermore, lunar missions have only returned a limited number of samples of actual magnetized crustal rocks, while on the Earth we can study as many samples as one could wish. Therefore, one might wonder why lunar data should be used at all, when terrestrial data has these clear advantages. However, the Moon has several key advantages over the Earth for this type of study. First and foremost, the Moon currently has no global magnetic field. This means that we do not have to subtract off a huge global field when measuring local crustal fields, nor do we need to deal with induced magnetic fields. Instead, we can be sure that the signal we measure is purely due to remanent magnetization in the local crustal rocks. Furthermore, on the Earth impact basins formed in the presence of a strong ambient magnetic field. On the Moon, on the other hand, at least the younger basins and craters appear to have formed with no significant ambient magnetic field present. This means that we can more easily determine the demagnetization effects of these impacts.

Studies of terrestrial impact basins have revealed many basin-associated magnetic anomalies [1]. These range from short-wavelength anomalies with a radial extent of a fraction of the transient cavity radius (e.g. Manicougan [2]), to larger groups of anomalies which fill most of the transient cavity region (e.g. the outer ring of anomalies in the Chicxulub basin [3]). The more localized anomalies have generally been ascribed to shock remanence (SRM) or other processes in the central uplift region, while more extensive anomalies have been interpreted as thermal remanent magnetization (TRM) in impact melt rocks. Many lunar basins and craters also display central magnetic anomalies, with the older large (> 200 km in diameter) craters and basins having the most significant anomalies. These anomalies roughly fill the transient cavity region, and therefore by analogy with terrestrial basins, may be due to TRM in impact melts. If this is the case, these anomalies indicate the location of the most substantial amounts of impact melt in lunar basins. On the other hand, if they are instead due to SRM in uplifted materials, they could be used to delineate central uplift structures in multi-ring basins.

Earlier work has shown that many lunar impact craters and basins, especially the youngest ones, are demagnetized with respect to their surroundings [4]. This is also true of many smaller terrestrial craters [1,5]. However, for younger lunar impact sites, demagnetization is especially clear, probably because there were no strong ambient magnetic fields present at the time of these impacts. The demagnetization of lunar craters and basins has been found to extend well beyond the main rims of these structures, which provides strong evidence that impact-generated shock is mainly responsible for demagnetizing the crustal rocks [4].

The physical mechanism of shock demagnetization is still not particularly well understood. However, laboratory measurements of shock demagnetization of both lunar and terrestrial rocks have been performed [6,7,8]. The degree of demagnetization is, in general, dependent on the peak shock pressure and on the remanent coercivity of the crustal magnetization, and laboratory experiments have roughly quantized this relationship for terrestrial basalts [6]. The returned lunar samples show a wide variety of magnetic coercivity spectra. However, lunar breccias tend to carry the strongest remanence, and we have therefore constructed average coercivity spectra for various sets of breccias [9,10]. By combining coercivity spectra with impact demagnetization data and experimental shock demagnetization results, we have attempted to derive the radial peak shock pressure attenuation. Our preliminary results imply peak shock pressures at the transient cavity rim of 2 Gpa and power law attenuation with a power of -2 to -3. These results are consistent with modeling [11] and shock pressure reconstructions from terrestrial basins [12].

We believe that the magnetic fields of lunar impact craters and basins can provide important information about the impact process. Though performing this work with lunar rather than terrestrial data has some drawbacks, there are also clear advantages. So far, our results are encouragingly consistent with terrestrial observations and modeling.

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PYROCLAST FLOWS AND SURGES: POSSIBLE ANALOGY FOR CRATER EJECTA DEPOSITION. H.

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Introduction: We analyse a possible modell of the crater ejecta development and deposition with pyroclastic flows and surges. Because several of their characteristics and depositional structures are known and observable on the Earth it is useful to try to find resembling phases of the crater ejecta formation.

The modell: We analyzed similarities and differences of physical parameters between pyroclastic flows and crater ejecta formation. At volcanic eruptions the p, T are lower than at the moment of impact. In the origin of the pyroclastic flows we can analyse the physical circumstances at really explosive eruptions like Krakatoa-type eruptions too. The 1st seconds of the impact – contact/compression stage (CC), the kinetic energy is transfered to the rock by schock waves. In our analogy we ignore this phase because the differences are too large. The original energy is lost fast because of the expanding shock front and the conversion of the energy to heat, rock deformation etc. When the pressure drops to 1-2 GPa it behaves like normal seismic waves. [1] Heat melts the projectile and target rock layers, which is mixed to partly melted and brecciated target rocks.

At the end of the excavation stage [E] the ejecta material (the near surface ejecta curtain) falls out of the rim of the crater and its material flows away and settles down. At pyroclastic flows and surges originally high central pressure formed the fragments which later was transported by gravity at slopes. At a crater formation the impact explosion gas schock waves, reflected waves drive the upward movement of the debris. We can use the analogy at that point where the effect of the central pressure is lower and gravity driven current movement is important. Our analougue is best in the modification [M] stage when the transient crater reached its final dimensions and no more material is ejected. The ejecta blanket is now "in the air" and starts falling down. From this point the physical parametrs of this material is more or less similar to the ones in a volcanic eruption. By this time, the crater rim is higher then the surroundings so there is a slope corresponding to a volcanic dome that makes the flow movement possible.

In pyroclastic structures several distinct layers are identifiable. A crater ejecta structures can be taken as one cycle of a pyroclastic structure. The cratering process is ended after the solid materials fell down, with the finer particles gravitational settleing and the fallout of the solidified materials that were vapourized during the impact. The resulting distal ejecta can be extended to a global scale. These later stages are also analogues to the volcanic eruptions.

Characteristics	Pyroclastic flows	Pyroclastic surges	Crater ejecta emplacement
Temperature	900-1100 K and lower	900-1100 K and lower	1000-2000 K and lower
Pressure	first in the order of Mpa, later near to the atmospheric	firstly in the order of MPa, later near to the atmospheric	first [CC] in the order of GPa, later [M] nearly near to the atmospheric
Fragments	Mostly solid with gas and very few liquid phase	large gas content with fewer solid matter ration	Solid phase and gas, with more plastic components (melts) Melts from projectile/target; solid rocks from target area, vapourized gas from projectile/target
Depositional structures	Poorly sorted and bedded, graded basal zone, trains of large fragments, alternating coars to fine graded layers, oriented fragments, at pumice inverse grading	laminated, cross bedding, lenses of well-sorted-rounded pumice lapilli, better sorted, no very fine and very coarse fractions	Fallout partly influenced by flowing movements
Driving force	gravitational induced movement on slopes	gravitational induced movement on slopes	first the gas pressure of the explosion [E], later gravitational induced motion on slope
Topography	relative high slope angle	relative high slope angle	nearly no originally slope, only in the late phase of the deposition low slope angle (from the rim)
Origin	collapse of the ejecta containing (sometimes km high) gas column above the central vent	mostly explosion driven fallout of ejecta, and later the collapsed of shorter gas driven explosion column	fallout (ejecta) from the central explosion
Duration of material upload	several hours/days?		Few seconds
Source of gas content	eruption column; outgassing of melt	eruption column; outgassing of melt; water of crater lake	vapourized projectile (if comet, more) or rock, in situ ground- or surface water or ice; outgassing of melt; On Venus: atmosphere
Speed	ejected material: 200-600 m/s; flow: 100-200 km/h	flow: 100-200 km/h	ejected material: 500 to several 1000 m/s

Comparison of flows, surges, crater ejecta

It is a question whether there is an eruption column at the impact site. In the case of volcanoes, the eruption column is supported by the continuos gas thrust from the crater which is not the case at impacts where the process takes place for few seconds. Observations of nuclear explosion tests show both eruptive coloumns and gasious flows just like surges too. [3] The ejecta blanket is partly fluidized by water.

The atmosphere is important with its pressure for the gas content inside the pyroclastic flow. At the crater ejecta in the depositional phase the difference between the atmospheric and the internal pressure is relative low – just like at a pyroclastic flow. Because pyroclastic structures are known from airless body our analogy can be used at the crater ejecta deposition on airless bodies, eg. on moons. Higher gas content cab make fluidization. On Venus, the long-run ejecta flows were spread in a fluid manner, extending beyond the continuos ejecta, moving on a fluidized "bed" which are linked to impact melts, impact angle [2] and dense atmosphere.

Conclusion: In the late phase of the crater ejecta formation pyroclastic flows can be used as an analogy in the analysis of physical circumstances in the flow (flow regime, temperature, gas content, ration of liquid phases). The depositional structures can suggest to the density of the debris and fallout style/time.

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THICKNESSES OF AND PRIMARY EJECTA FRACTIONS IN BASIN EJECTA DEPOSITS. Larry A. Haskin and William B. McKinnon, Department of Earth and Planetary Sciences and McDonnell Center for the Space Sciences, Washington University, One Brookings Drive, St. Louis, MO 63130; lah@levee.wustl.edu, mckinnon@levee.wustl.edu

We have developed a model for production of basin ejecta deposits to address provenances of materials collected at the Apollo and Luna landing sites and for consideration in interpreting remote sensing data [1].

Model Steps: 1) We take the cumulative mass (m) distribution of primary ejecta fragments ("PriFrags") to vary as $m^{-0.85}$ everywhere, with a maximum PriFrag mass (which can vary with ejection velocity) [e.g., 2, p. 91]. **2)** Ejecta mass is distributed according to [3]. We map their results, for a flat surface, onto a spherical one using ejecta velocities and an assumed launch angle. **3)** Given the surface density of PriFrags of each size falling in the vicinity of the site of interest, we use Schmidt-Holsapple scaling to obtain the sizes of secondary craters. We assume *excavated* volumes of those craters have a depth/diameter ratio of 0.1. **4)** We calculate the probable range of ejecta deposit thickness and % of PriFrags in the deposits, and express them as the fraction of the area at the site of interest. We define "Coverage Level" (CL) as the fraction of that area excavated by craters of a specific size *or larger*. **5)** Beginning with the cavity produced by the largest PriFrag to excavate at a location, we consider how much additional substrate is excavated by the smaller PriFrags that land on or near that spot. We calibrate to deposit thicknesses surrounding Orientale [4] and the Ries [5]. Results suggest that the total excavation by all secondary craters at a specific position corresponds roughly to a right cylinder with the same diameter and 3 times the depth of the largest crater to affect that position.

General Model Results: Ejecta deposit thickness decreases with distance to ~3500 km followed by a modest increase on the anti-basin hemisphere due to ejecta convergence. The fraction of PriFrags in the ejecta deposits shows a similar pattern. Differences due to varying ejection angle from 35° to 55° (to the horizontal) are not substantial.

Apollo 16 Landing Site: Fig. 1a shows the range of deposit thicknesses expected in the vicinity of the Apollo 16 site ~1600 km from the center of Imbrium. Thickest deposits are produced where the largest PriFrags excavate; at higher coverage levels, relatively smaller PriFrags have excavated. Locations where different deposit thicknesses occur are not known, as the impact points of all PriFrags are random. Thus, some half of the vicinity of the site has ejecta deposits ≥1 km or so. From Fig. 1b, the fraction of PriFrags in the deposits is not sensitive to coverage level.

Estimated deposit thicknesses at the Apollo 16 site are reasonable as determined by criteria such as crater fill and fraction of Th-rich ejecta presumed delivered to the site by the Imbrium event from the Procellarum KREEP Terrane [6]. In contrast to conclusions of other studies [7,8], our modeling suggests that all materials sampled at the site, including North Ray Crater ejecta, are more likely part of the Imbrium deposit than part of a primary Nectaris deposit. The Imbrium deposit is estimated to consist of 18% Imbrium ejecta, 21% Serenitatis ejecta, 19% Nectaris ejecta, and 40% pre-Nectarian substrate, with only minor contributions from Humorum, Crisium, and later, Orientale. These materials may not be well mixed; large blocks from different provenances could presumably survive in some locations. The presence of significant Serenitatis materials at the Apollo 16 site has been discounted owing to lack of compelling photogeologic evidence [9, Fig. 10.39; 10, Fig. 10.25].

Concerns: Our model does not reproduce observed densities of secondary craters (it predicts too many) or the largest ones at Copernicus, Orientale, or Imbrium. Mutual obliteration and contributions from "spall" fragments may be responsible, respectively [cf. 11]. Nevertheless, thick deposits should have been produced at great distances from basin impact sites, and these deposits should consist of mixtures of primary ejecta and megaregolith produced by previous large impact events. How thick, however, depends on scaling parameters and factors that are still poorly known. These will be discussed. This work supported by NASA grant NAG5-10458.

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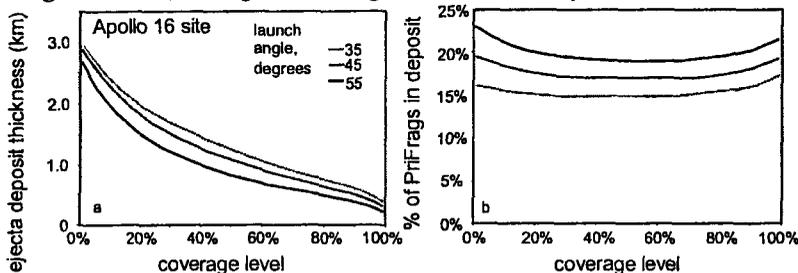
CONSTRAINTS ON THE IMPACT PROCESS FROM OBSERVATIONS OF OBLIQUE IMPACTS ON THE TERRESTRIAL PLANETS.

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Introduction: Recently there have been significant advances in both experimental and numerical modeling techniques that hold promise for providing details on how the cratering process is affected by impact at a nonvertical angle [1,2]. Anectdotal observations of craters on the terrestrial planets validated initial experimental efforts [3,4]. Recent and ongoing systematic characterizations of craters reslting from oblique impact on the Moon, Mars, and Venus provide important constraints for the detailed modeling efforts currently being conducted [5,6,7].

Observations: Pertinent observations from surveys conducted to date are:

• The general variation in ejecta pattern and crater shape with decreasing impact angle on the moon matches well with experimental work conducted in a vacuum. On the moon the following transitions occur with decreasing impact angle with respect to horizontal: < ~50 degrees, the ejecta blanket becomes asymmetric; < ~30 degrees, a forbidden zone develops in the uprange portion of the ejecta blanket, and the crater rim is depressed in that direction; < ~20 degrees, the rim topography becomes saddle-shaped, or depressed in both uprange and downrange directions; < ~15 degrees, the rim becomes elongated in the direction of impact and the ejecta forms a "butterfly" pattern in the crossrange direction [5].

• In agreement with experimental work, the presence of an atmosphere significantly increases the onset angle of oblique impact phenomena in the ejecta pattern [5]. No downrange forbidden zone occurs at low impact angles [4].

• Our preliminary work with Martian craters shows that the change in ejecta pattern with decreasing impact angle closely resembles that of the moon, with the development of uprange and then downrange forbidden zones with decreasing impact angle. While the transition angles to different ejecta patterns are generally similar on the moon and Mars, the development of a forbidden zone in the uprange direction occurs at a significantly higher impact angle on Mars than the moon.

• The transition to elliptical craters and a butterfly ejecta pattern occurs at a higher angle on the planets than in early experimental work [3,5,6].

• Adequate data on crater wall topography of oblique impacts currently only exist for the moon. Unlike in experimental work, there is no strong evidence of uprange steepening of the crater wall for oblique impacts [5]. Internal slopes for lunar craters appear largely independent of impact angle. However, interior crater wall slopes approach the angle of repose, and post-impact slumping to a uniform slope cannot be ruled out.

• There is minimal evidence that central structures are offset in any direction relative to the crater rim [7], nor could we find observations in imagery that were indicative of the point of impact.

Constraints on the Impact Process: The observations suggest the following constraints on modeling efforts of the impact process:

• That the ejecta pattern is more affected by oblique impact than the final crater shape suggests near-field versus far-field effects; material ejected from near the point of impact "sees" the impact angle the most.

• Modeling of ejecta emplacement in an atmosphere must consider the disturbance of the atmosphere by the incoming projectile.

• Whatever causes the higher onset angle for elliptical craters and butterfly ejecta on the planets relative to past experimental work, those causes are only important at the lowest impact angles.

• The lack of variation for interior shape and slope suggests that the cross-section of stream tubes for late-stage excavation does not vary with impact angle.

• Mars is clearly below the threshold for the atmospheric disturbance caused by the incoming projectile to have a significant effect on ejecta emplacement.

• While subsurface features may reflect the initial point of impact, observable surface features do not. In other words, while the shock level of the rocks can be modeled as strongly direction-dependent, final crater shape must not be (with exception of rim elevation).

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LINKING EXPERIMENTAL MODELLING OF IMPACT CRATERS TO STRUCTURAL COMPONENTS OF THE REAL THING.

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Introduction: Impact crater scaling relationships, such as for impact energy, are usually derived solely from experimental impact or explosion craters [e.g., 1]. Relating craters to a suite of possible source projectiles, and predicting what size crater a given impactor will produce in a surface of known composition, are basic requirements for reconstructing impactor populations from cratering records, comparing cratering rates derived from cratering records to those derived from observed impactor populations (known velocities), and assessing the hazard associated with a given impactor.

Impactor to Crater Size/Energy: Scaling from a given crater to impact energy is currently controversial even when the same energy scaling relationship [e.g. 2] is used. For example, energy estimates for the Chicxulub crater [3,4] vary by an order of magnitude due to interpretation differences, although agreement exists on the relevant internal crater structural element (the collapsed disruption cavity diameter; see Fig. 1). (Discussion indicates that confusion exists within the cratering community on terminology for the different crater elements illustrated in Fig. 1; agreement on a common terminology as discussed by [3] is desirable.) The difference stems from one calculation being based on the reconstructed size of D_d [4] and one being based on D_{at} [3]. The latter have been convinced by the argument (p.c., H. Melosh) that the apparent transient cavity diameter corresponds to that of the experimental craters produced by [2] on the grounds that no collapsed blanket of breccia or melt fills the craters.

Possible Link Through Ejecta Blankets: The appropriate cavity diameter to be used for energy scaling might be established by comparing the ejecta blanket thicknesses observed around Chicxulub to those around experimental craters. Figure 2 attempts this comparison (the ejecta thicknesses are plotted normalized to a D_{at} of 80 km [3]). However, sufficient observations are not yet available to make a clear distinction, and erosion by ballistic sedimentation proximal to Chicxulub has over thickened its ejecta blanket by nearly an order of magnitude (as also observed around other well preserved craters). Although the thickness of the proximal ejecta blanket has also been compromised by erosion of its top, comparison of the observed thickness to that predicted from experimental craters may be useful in predicting the proportion of the ejecta blanket that is derived from ballistic erosion. At >15 crater radii observed ejecta blanket thicknesses are greater than predicted by [1, Fig. 2], this range is beyond the thickness resolution of these experiments.

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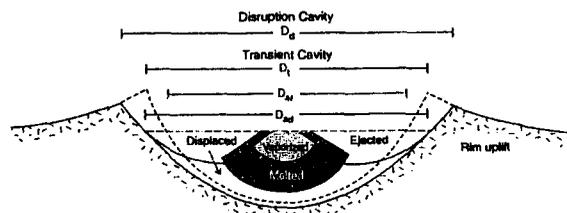
Figure 1: Schematic distinguishing a crater's transient (diameter D_t) and disruption (diameter D_d) cavities. At the pre-impact ground surface these diameters are D_{at} and D_{ad}, respectively. The horizontal dashed line indicates the position of the pre-impact surface within the crater. (from [3])
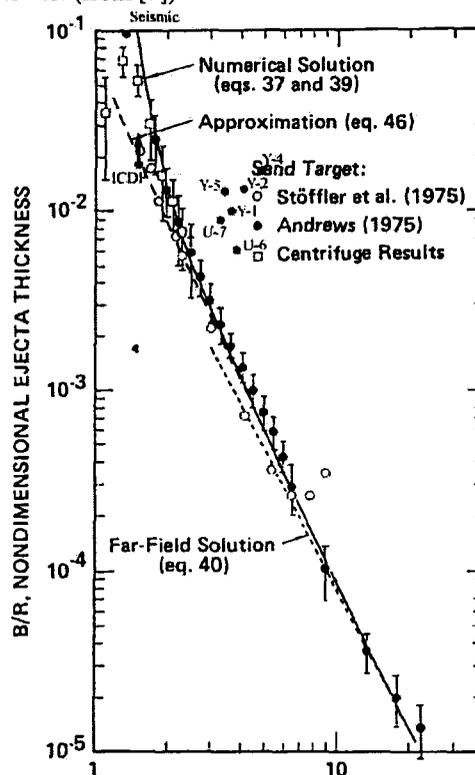
Figure 2: Ejecta-blanket profiles resulting from experimental impacts and explosions in sand compared to profiles predicted by the ejecta model of Housen et al. [1] and the ejecta blanket thicknesses observed at the Chicxulub crater. (Modified from [1])

WHAT DO WE NEED TO KNOW TO MODEL IMPACT PROCESSES?

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Introduction. The computer modeling of hyper-velocity impacts into planetary bodies is one of the most challenging computer tasks we attempt. The physical states encountered in impact events can begin with pressures measured in gigabars and temperatures measured in hundreds of electron-volts, and then proceed all the way down to the ordinary partial bars of pressure and few degrees of temperature as in our common experience in terrestrial soils and rocks. The interest in planetary science applications spans not only those common terrestrial soils and rocks, but also gases, ices at extreme low temperatures, and very loose, rubble-pile materials that could not even withstand the pressures of the Earth's gravity without crumbling.

The extreme range of physical conditions and materials makes the job of a modeler extremely difficult, especially for descriptions of the models for the material behavior. While, in principle, current computer power would seem to allow the detailed calculation of any specific impact event of interest by integrating the known physical laws, that view is specious. The cold, cruel facts are that, first, we do not yet know how to mathematically model the extreme range of conditions of importance, and second, even if we develop meaningful models, we do not have sufficient physical tests to measure the material properties needed for those models.

This state of affairs means that the community must be aware of the shortcomings, and must spend much more time and effort on the development of models of material behavior, on the laboratory and field measurements to calibrate those models, on calculations to determine the sensitivity of the results on the models, on actual physical experiments of impacts, and, finally, on calculations of those physical laboratory results and large scale field events with known impact conditions. The computer tools must prove their reliability and robustness for calculations when both the initial and final conditions are well known before they can be used with any meaning to determine unknown impact conditions.

This presentation is to review what we know and what we do not know; what needs to be known, and what remains to be discovered about material modeling for impacts.

The EOS. The evolution of the pressure and temperature states from extremely large to very small leads to a parallel separation of the required material models into two distinct but intertwined parts. First are the models for the high-pressure behavior in the early stages of the process. Those pressures are commonly much larger that the material stress scales: the compressibility modulus and various material strengths, so the stress deviators can be ignored. The state is then

measured by five state variables: the pressure p, mass density ρ, internal energy e temperature T and entropy η. Any pair can be chosen as independent, and the other three are then given in terms of those two by the "equations of state" which are material property functions. However, insofar as the solution for the motion is concerned, it is only the relation between e, p and ρ that matters.

Since impact problems encounter the same extreme conditions as nuclear events, it is not surprising that we borrow the knowledge and tools of the national weapons laboratories for those equations of state, which they have been studying for over half a century.

There are a variety of EOS models: simple algebraic models that relate pressure and density with no dependence on temperature (e.g. linear elasticity or Murnaghan); simple analytical models for single solid phases (Mie-Gruneisen and Tillotson); complex analytical models including phase changes such as melt and vapor (ANEOS); and complete tabular databases such as the SESAME and SESLAN libraries from the DOE laboratories. Those latter two are often developed from complex solid-state physics theories using the PANDA computer code [1]. The EOS equations govern the early-time response and determine a number of significant aspects of the energy coupling, including the initial pressure and velocity, and the decay of the pressure and velocity as a shock propagates through the target.
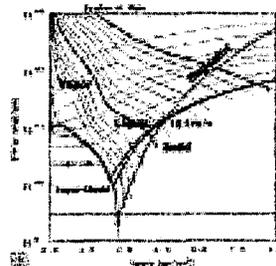
A typical EOS is as shown at the left. The important elements include the Hugoniot, which relates the conditions at the shock, and the "release adiabat" the path followed during the unloading behind the shock.

These paths determine a measure of an equivalent point source input, which in term determines most of the scaling of the final cratering or disruption results.
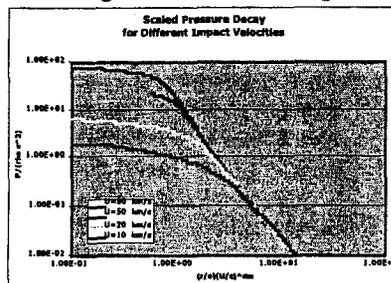
The left figure illustrates the commonality of different impact problems arising from the simplicity of the point-source measure. (See [2] and many prior references of the author

MODEL IMPACT PROCESSES: K. A. Holsapple

and his colleagues.)

These EOS descriptions are quite well developed and understood, a consequence of the fact that they are needed for calculations and development of nuclear weapons. For impact calculations, it is necessary to choose the model and its constants. However, for any particular geological material, that can often be a difficult task, so that the resulting model is usually quite uncertain.

Strength. When the shocks decay into the kilobar pressure range, material strength dominates the target response and subsequent cratering or disruption. Here we borrow from the civil engineering soil-mechanics and rock-mechanics communities.

Strength models include none (hydrodynamic), constant strength in tensile or compression, constant strength in shear (Tresca), maximum deviator invariant J_2 (VonMises), pressure-dependent shear strength (Mohr-Coloumb), pressure-dependent J_2 (Drucker-Prager), rate-dependent tensile (e.g. Grady-Kipp), and complex damage models (e.g. Johnson-Cook). This description of the fracture, flow or yielding (generically called "failure") is the most difficult part of impact calculations into geological materials.

A common starting point is to describe how the initial failure depends on the stress or strain tensors, which have six independent components; or, equally well, three invariants and three directions. Assuming isotropy, directions are of no consequence and the stress tensor can be measured by the three invariants. It is common to further suppose that only two are necessary, taken as the pressure or mean stress (essentially the first invariant), and what is commonly denoted by J_2, the second invariant of the deviator stress. Then the ranges of stress for which flow or fracture does not occur are described by defining an enveloping curve in pressure-J_2 space. (Changes to this envelope such as hardening or softening are described below).
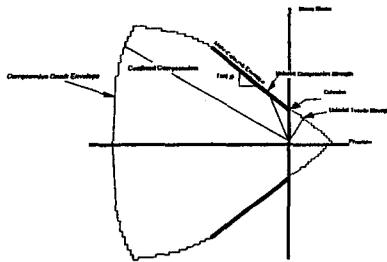
The fig-
ure at the left
indicates the
general nature
of an initial
failure enve-
lope for a
geological
material, as a
plot of the
maximum shear stress versus the confining pressure. Various different measures of "strength" exist and are indicated on this envelope. There is a curve of limit shear stress that depends on pressure, commonly modeled as a Mohr-Coloumb (shear strength versus pressure) or a Drucker-Prager envelope (J_2 versus pressure). Often those curves are assumed to be linear, but that assumption is not essential. Then since failure can also occur at sufficiently high pure compressive pressure, a "cap" is constructed to model that compressive

pressure crushing; that is the termination of the envelope at the left of this figure.

For uniaxial tension loading, the loading path as shown intercepts the failure envelope at a uniaxial stress limit known as the tensile strength. In pure uniaxial compression, the path as indicated intercepts the shear envelope at a higher stress, called the compressive strength. In pure shear, the maximum is at the intersection of the shear envelope with the vertical axis, the shear strength or "cohesion". Biaxial or triaxial loading can proceed along different paths until they intersect these limit curves, those define biaxial and triaxial strengths. A confined compression curve is shown sloped to the left and intersecting the compression cap.

The next part of the modeling concerns the question of the change of this envelope as failure proceeds. These questions involve the features of ductility (plastic flow) versus brittle (fracture or flaw growth). Commonly, brittle failure occurs at low values of confining pressure, especially tensile states; while ductile failure occurs at high values of confining pressure in compressive states. Ductile failure is modeled by describing how the material develops plastic strain (the "flow rule") and by how that flow affects the failure envelope (hardening or softening). Common metal-plasticity models include those effects. Brittle failure is commonly modeled using a "damage" parameter, which measures the internal damage of the material in a macroscopic way. It typically ranges from zero at no damage, to unity at complete damage. An equation describing its evolution as a function of the current stress or strain state is required to track its values at material points. The Grady-Kipp model is an example of a damage model for brittle tensile failure. All of these aspects can also depend on the temperature.

When failure occurs, a granular material also has a tendency to "bulk": an increase in volume and decrease in density at constant pressure. That can be suppressed by the pressure state, but then adds a component of pressure. Equally well, bulking is included if an associated flow rule is used with a pressure-dependent shear strength, since that flow rule has a component of dilation. The relative amounts of deviator and dilation can be adjusted by using a non-associated flow rule.

I will review various material property data and different models used in the community, and relate their features and failures to this overview picture.

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DOES MELT VOLUME GIVE THE SIGNATURE OF THE IMPACTOR?

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1. Introduction. Many analyses of impact events attempt to solve an inverse problem: Given the result, what was the impactor? One common example is the use of careful measurements of impact melt with the hope of deducing the impactor size and velocity.

The approach is as follows. Suppose the amount of impact melt is, for a given geological site and assuming a given impactor material, known (for example by code calculation) as a function of impactor mass m, velocity U. (I shall ignore complexities of oblique impacts here.) Then we have some known functional relationship

$$V_{melt} = F(m,U) \qquad (1)$$

Then also we have some other known quantity, say the crater size given as

$$V_{crater} = G(m,U) \qquad (2)$$

The goal is then to solve these two equations in two unknowns for the impactor mass m and the velocity U. Of course, that will fail if the two equations are not independent, and therein often lies the problem.

Equation (2) for the crater size is usually assumed to be of the form determined by the point-source approximation to impact problems, as given by the scaling relations of Holsapple, Schmidt and Housen (see, for example, the review in Holsapple 1993 [1]). The point-source approximation is expected to be valid for any measure of the cratering process that is large compared to the impactor size. Those relations have the form

$$V_{crater} = f(aU^{\mu}) \qquad (3)$$

where the exponent m is assumed to be known, it is about 0.55-0.6 for non-porous materials. One must distinguish between the strength regime or the gravity regime for the function f. Assuming as a specific example a large terrestrial crater in a hard rock geology, then a specific form is given (Holsapple, 1993 [1]) as

$$V_{crater} = 0.48 m^{0.78} U^{1.3} \qquad (4)$$

Thus, the measurement of the crater volume gives the numerical value for the product $mU^{1.67}$. (This is just the cube of the product aU^{μ} with some factors thrown in.)

We cannot perform laboratory experiments at impact velocities greater than 5-6 km/s, well below the minimum velocity for melt production. Therefore, code calculations must be used to determine the melt volume function of equation (1). Such calculations have been reported by O'Keefe and Ahrens [2], Orphal et al. [3] Bjorkman and Holsapple [4], Pierazzo et al. [5] and others.

O'Keefe and Ahrens [2] report that the melt vol-

ume for impact velocities greater than a threshold is proportional to the impactor kinetic energy:

$$V_{melt} = Ka^3 U^2. \qquad (5)$$

Later, Bjorkman and Holsapple [3] determined an importantly different result: that, for impact velocities greater than about 50 km/sec the melt volume scaled in the same way as the crater volume, namely that

$$V_{melt} = Km^{0.78} U^{1.3}. \qquad (6)$$

although energy scaling does hold for lower velocities where the majority of melt is produced close to the impactor. The problem then arises for the larger velocities: if the melt and crater volumes scale in exactly the same way, both are determined by the same combination $mU^{1.67}$. Then there is no way to determine separately the mass and velocity.

Much more recently Pierazzo et al. [5] revisited the question of melt production. Their conclusion returns to that of O'Keefe and Ahrens: that the melt volume scales linearly with the energy of the impactor. They attribute the Bjorkman and Holsapple [3] result to be a consequence of insufficient grid resolution in the calculations.

I shall reevaluate the reevaluation of Pierazzo et al. Specifically, I shall show calculations and argue that, not only does energy scaling not hold for the higher velocities, it does not hold about 30 km/s. The consequence is that melt volume cannot be used to separate the effects of size and velocity for any impact velocity greater than that value.

In fact though, the different interpretations are really somewhat moot. Numerical examples will be presented that show, that even if energy scaling for melt volume is adopted down to lower velocities, the inverse problem is highly non-robust: Factors of uncertainty of only 2 in the melt or crater volume functions result in factors of uncertainty of several decades in impact velocity.

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EFFECTS OF TARGET PROPERTIES ON THE CRATERING PROCESS.

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Impact events in the solar system occur in a variety of materials, ranging from the rocky surfaces of the terrestrial planets to the icy mantles of the satellites of the outer planets to the undoubtedly highly fractured and porous materials that make up many asteroids and comets. A major challenge to impact modelers has been to understand how the composition and mechanical properties of these varied target materials dictate the outcome of an impact event. Four sources of information have historically been used to study this problem.

Scaling theory provides guidelines as to when specific material properties may have a significant effect on the outcome of an impact event. The initial work in scaling separated cratering events into the strength and gravity regimes. In the former, crater size is determined by the mechanical strength properties of the target while, in the latter, strength is unimportant compared to the effects of the lithostatic overburden. The transition between the two regimes is determined by the condition $Y/\rho gh$ = constant, where Y is a measure of target strength, ρ is the density, g is gravity and h is crater depth. This simplistic picture has now been modified in two ways. First, Gaffney and Holsapple [1] noted that the strength of many geological materials depends on the rate at which they are loaded and that loading rates depend on the size scale of the event. As a result, mechanical strength of the target decreases with increasing event size, so the transition into the gravity-dominated regime occurs at smaller crater sizes than the simple constant-strength model would predict. Second, numerical simulations by Nolan *et al.* [2] indicate that passage of the shock ahead of the expanding crater bowl pre-fractures rocky target materials, which allows the crater to form in an essentially cohesionless (but not strengthless) material. In essence, an impact event can alter the mechanical properties of the material in which the crater forms.

Scaling considerations have also been applied to impacts in highly porous targets [3, 4], which may be representative of comets and many asteroids. In this case, craters are formed mostly by compaction of pore spaces. Crater size is therefore determined by the crushing strength of the target. Impacts in these materials may not experience a gravity regime because at large size scales (where gravity would be expected to dominate), the material crushes to a point where the lithostatic compressive stress is comparable to the crushing strength. Hence, a situation is never attained in which gravitational stresses are large compared to the important strength measure.

In addition to mechanical strength, scaling analysis has been used to identify conditions under which target viscosity is the most important property in determining crater size. Cratering in a viscosity-dominated regime has been applied to studies of Mar-tian rampart craters [5] and craters on icy satellites [6].

Scaling theory is essential to identify the conditions under which various target material properties might be important in determining crater size and morphology. However, scaling laws by themselves cannot establish the relation between crater size and material properties. Instead, experiments and code calculations must be used to determine those dependences.

Field explosion experiments are a second source of information on the effects of material properties. Field tests are especially useful in that they can be conducted at size scales much larger than laboratory experiments. The largest conventional explosion test conducted in the U.S. involved 4.36×10^9 g of explosive and produced a crater 88.4 m in diameter [7]. While still small by planetary standards, these craters are more than 100 times larger than those that can be studied in the lab. Additionally, field tests have been performed in various geologic settings and can be used to illustrate the dramatic effects of material properties. For example, Figure 1 compares the crater profiles produced in two tests involving hemispheres of high explosives with a mass of 4.5×10^8 g, one in basalt and one in unconsolidated alluvium.

Laboratory experiments have of course been the main source of information for cratering studies. An advantage of laboratory experiments is that they can be conducted under controlled conditions, whereas field tests are at the mercy of the natural settings under which they are conducted. That is, it would be difficult to determine the influence of material properties from field tests alone because a multitude of important properties may vary from one test site to the next. As an example, Figure 2 uses the results of impact experiments to addresses the dependence of crater size on target density. Cratering efficiency (target density * crater volume/impactor mass) is shown for three cohesionless granular materials whose bulk densities vary by a factor of 2.6. The results show that cratering efficiency in nearly independent of target density for this particular type of target material

A limitation of laboratory studies is that they are, by definition, conducted at small size scales. Therefore, if any important material properties are scale dependent (e.g. the strength of rock), then the experimental results will not be directly applicable to larger events and must consider the scaling issues involved with extrapolation to larger sizes.

Numerical simulations have become a popular method for studying crater formation and offer the potential benefit of being able to study the separate effects of material properties on crater size and morphology. While this benefit is alluring, a considerable drawback to code calculations is that the results are

EFFECTS OF TARGET PROPERTIES: K.R. Housen

only as good as the physical models that they incorporate. The constitutive models used in present codes such as CTH are reasonably accurate for some materials (e.g. metals), but are not well-developed for others, notably rock or highly porous soils. As a result, code results should be viewed with skepticism until validated extensively against laboratory and field tests [9]. Nevertheless, when such validations are accomplished, numerical simulations can provide tremendous insight into the effects of material properties.

Figure 3 presents an example. It was noted above that impact shock in rocky targets pre-fractures the material ahead of the expanding crater. This phenomenon has been used at times to assume that this pre-processing reduces the material strength to zero. While pre-fracturing should eliminate cohesion, the fractured rock will still have considerable strength in shear due to the effective friction angle associated with the interlocking of the rock fragments. The effect of friction angle is addressed in Figure 3, which shows the result of two CTH calculations of the Sailor Hat explosion event. Crater profiles are shown at an intermediate time during crater growth. The two simulations were identical except that the one on the left assumed a friction angle of 0° (equivalent to assuming a strengthless material), while that on the right shows a more realistic value of ~30°. These results show the significant effect that the material shear strength has on crater formation; an effect that is ignored in many calculations reported in the literature.

Additional calculations are underway. These results, along with those from scaling, field tests and laboratory experiments will be summarized to identify what is and is not known about the effects of material properties on crater formation.

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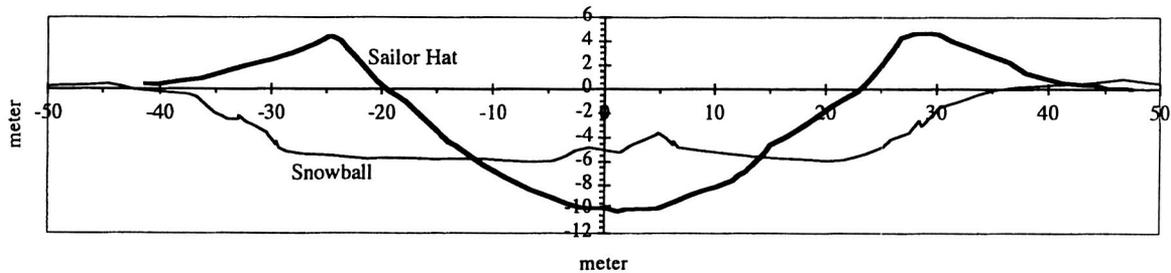
Figure 1. Comparison of crater profiles from two explosive field tests. Both tests used hemispherical charges of TNT $(4.5 \times 10^8$ g) situated at the target surface. The Sailor Hat event was conducted in basalt, whereas Snowball was conducted in unconsolidated alluvium with the water table at a depth of approximately 7 m.
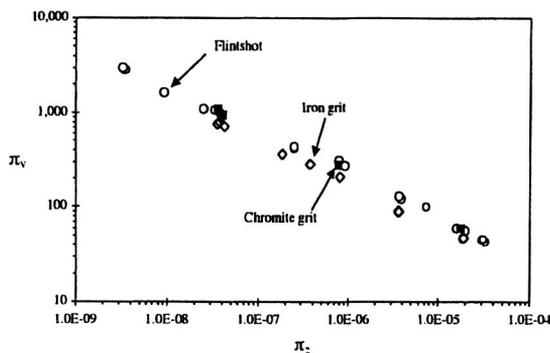
Figure 2. Cratering efficiency, π_v, vs π_2 for 1.8 km/s impacts into three granular cohesionless materials of density 1.8 (Flintshot sand), 3.1 (Chromite sand) and 4.6 gm/cm³ (Iron sand). These data show that cratering efficiency is nearly independent of target density.
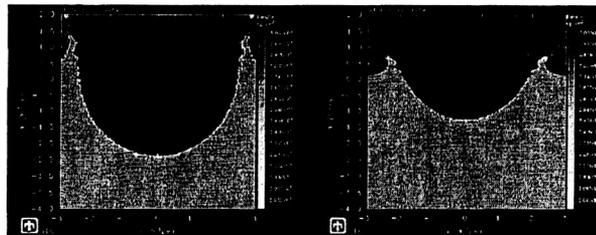
Figure 3. Comparison of two numerical simulations (CTH) of the formation of the Sailor hat explosion crater. The crater profiles are shown at an intermediate time of 0.18 s. Both models assume a Mohr-Coulomb behavior. The Profile on the left is for a case where the angle of internal friction is zero, while the case on the right is for approx. 30°. The formation time of the crater is ~0.5 s.

COMPLEX CRATER FORMATION: VERIFICATION OF NUMERICAL MODELS.

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Introduction: The growing capability of modern computers offers increased possibilities for numerical modeling of impact crater formation. However, complex crater formation include various particular models of rock massifs dynamical behavior in a wide range of thermodynamic parameters and strain rates. At the same time geological and geophysical investigations of impact craters give only the final structure of craters and geophysical fields around. The verification of numerical models should take into account comparison of computed results with maximum possible set of observational data.

Ground truth: The list of parameters one should compare includes crater morphology and morphometry, deformation of stratigraphic layers and their structural uplift; impact melt volume; shock wave decay; geometry and size of fractured zone, and individual specific features available for some terrestrial craters (presence of tektites, evidences of underwater formation etc.).

Primary experience: The list of recent publication gives an impression about strong and weak topics in the current state of model's verification.

Crater morphology and morphometry.. Models for many craters has been published, however rare papers deals with a systematic investigation of a crater shape in a wide range of crater diameters with the *same model.* A good example is done in [1] where the depth/diameter relation bend is reproduced qualitatively for the moon, Earth and Venus. However, quantitative fit of models to measurements is still an open question.

Deformation of stratigraphic layers and their structural uplift. First attempts to compare models for specific craters has been published for Chicxulub [2] and Puchezh-Katunki [3]. Again, qualitative fit of models is obtained with many quantitative misfits.

Impact melt volume is the best-studied model value [4] ready to be compared with observational data [5]. One can state the good fit of models to field data. The fit demonstrate that current scaling laws allow us to estimate impact energy for a given crater with the accuracy of factor of 2. However, the melt production in oblique impacts is still under investigation [6, 7].

Shock wave decay is easy to get in a numerical model and is very hard to compare with observations: due to a structural uplift formation the final position of shocked rocks are very far from their initial position in a target. Hence only full model of a complex crater modification allow us to verify models with a shock

wave decay [3] (Fig. 1).

Geometry and size of fractured zone are just began to be used in model/nature comparisons. Rare papers for several craters has been published (eg. [8]). At the same time namely modeling of a fracture zone allow to compare code results with available gravity and seismic survey. This direction looks like a promising way for future modeling evolution.

Individual specific features for several terrestrial craters allow to verify a complex interaction with layered targets. One can refer for recent estimates of a tektite origin [9] and underwater crater modeling [10]. The modeling of individual specific features is also fast evolving approach to verify numerical models of impact cratering.

Conclusion: Numerical models of complex impact crater formation can be and should be verified by comparison with field geological and geophysical data.

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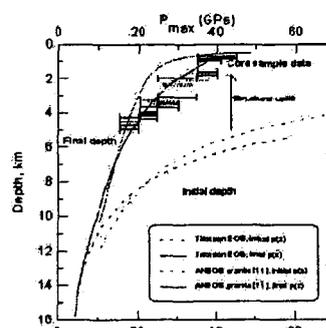
Fig. 1. The apparent shock wave decay with depth along the deep drill hole in the center of the Puchezh-Katunki crater, diameter D~40 km Maximum shock pressures recorded in minerals are shown as error bars. All EOS's used show that the central uplift top is constructed of rocks uplifted from ~6 km depth [3].

MODIFICATION OF ANEOS FOR ROCKS IN COMPRESSION.

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Introduction: The Analytical Equation of State (ANEOS) [1] is a useful computer code to generate equations of state (EOS) for rocks and minerals. An accurate EOS is one of essential points necessary for the numerical modeling of impact events. We analyze here a possibility to use a "standard" ANEOS in a "non-standard" way to make more flexible the procedure of an EOS construction.

ANEOS: The ANEOS Fortran package gives an opportunity to construct EOS for geomaterials, needed for the numerical modeling of planetary impact cratering. In comparison with the widely used Tillotson's EOS [2, 3], ANEOS has many advantages in respect to more accurate and self-consistent description of melting and vaporization. The practical convenience is that ANEOS gives the temperature of a material as an explicit output parameter. The calculation of temperature with the Tillotson EOS is possible (at least in compression) but needs an additional thorough treatment [4].

The original version of ANEOS [1] has several limitations which complicate its usage for rocks and minerals. The first one - monoatomic vaporization (good for metals and wrong for main minerals) - has been partially released by J. Melosh [5]. The second one - a simplified description of the solid-solid phase transition is the matter of the presented work.

Solid-solid phase transitions is a typical feature of shock (and static) compression for most of main rock-forming minerals (quartz, plagioclase, olivine etc.). ANEOS treats this phase transition via the modification of the "cold compression" curve. It is an elegant way to reproduce the complexity of the Hugoniot curve at a transition area. However, the simplicity of the approach has a high price: the thermal part of the EOS use the same parameters for the high pressure phase (hpp) and for low-pressure phase (lpp). For main rocks (granite, dunite) it leads to the artificially large heat expansion close to the normal pressure. Due to enlarged heat expansion an attempt to construct the Earth-like target with a typical geothermal gradient results in density decreasing with depth. for 10 to 100 km depth. Another disadvantage is that to use the solid-solid phase transition option one needs to switch out the melt curve construction.

HPP as a second material. We investigate here a possibility to "improve" ANEOS using it separately for hpp and lpp phase areas. A similar approach is used in the other "analytical" equation of state, PANDA [6, 7]. For each rock material we build the ANEOS input file as for 2 materials: hpp material and lpp material. The

hpp material has a proper "shift" for energy and entropy to use the same reference level both for lpp and hpp. A relatively simple Fortran routine is added to compute the phase equilibrium between lpp and hpp. The parameter fit is conducted, as usual, via the comparison with available thermodynamic and Higoniot data for materials under investigation. The output for the following usage in hydrocodes is assumed to be in the form of tables.

Preliminary results. Currently we have tested two materials of interest - granite and olivine. For these rocks some experimental data on shock and released temperatures are available (eg. [8, 9]). Fig. 1 illustrates the output of the updated ANEOS for olivine showing the dependence of complete (*cm*) and incipient (*im*) meting pressure for the preheated target. The preliminary estimate for the *im* shock pressure of a pre-heated peridotite is shown for a comparison. Further testing would show is it a plausible way to "improve" ANEOS for rocks and minerals.

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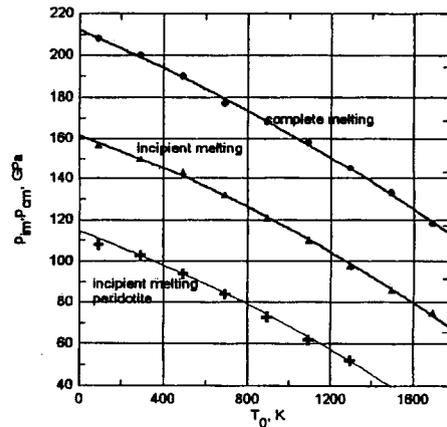
Fig. 1. Shock pressure for incipient and complete melting after a release for olivine and peridotite estimated with ANEOS.

EDUCATIONAL EXPERIENCE IN NUMERICAL MODELING OF IMPACT CRATERING. B. A. Ivanov, Institute for Dynamics of Geospheres, Russian Academy of Sciences., Leninsky prospect., 38-6, Moscow, 119334, Russia (baivanov@online.ru, ivanov@lpl.arizona.edu).

Introduction: The growing capability of the impact crater numerical modeling makes actual questions how to attract young students to the research and how to educate students specialized in general geology and geophysics. An experience in this direction has been accumulated in September 2002 during the ESF IMPACT Short Course "Numerical Modeling of Impact Crater Formation ".

Scope: The goal of the short course was to introduce basics of the numerical modeling techniques to non-professionals. "Non-professional" in this context means that the course was oriented to students and post-docs without a special background in computer science, shock wave physics and rock mechanics. However, most of students have an experience in impact crater related researches. Hence, all of them was highly motivated by their previous education and current research activity.

Attendance: 10 students from 6 European countries attended the short course (Germany - 3, France -2, Estonia - 2, Spain - 1, the Netherland -1, Finland -1). The general information about the ESF IMPACT program is available at http://www.esf.org WEB site.

Support and organization: The living and housing expenses have been covered by the ESF IMPACT program. The lecture room and the computer class have been offered by Vienna University (Prof. C. Koeberl was an excellent course manager). The computer class gives an opportunity for which student to work with a personal networked computer (PC under Windows 2000). The main lecturer (B. Ivanov) has used a beamer as for lecturing and for the demonstration of the practical work at the large screen. It was very important during the installation of the software and practice - students has seen simultaneously the output of each operation at their personal terminals and at the big ("master") screen repeated the "master" computer of the lecturer.

Short course program includes 5 main lectures and 5 practical lessons (totally 5 days with lectures before lunch and a practice in the computer class after lunch). Lecture topics include:
1. "What and how can be modeled for impact cratering. Shock waves, excavation and modification of a transient cavity".
2. "SALE hydrocode, general logic, input file, outputs"
3. "Equation of state (EOS). Ideal gas, Murnaghan, Mie-Gruneisen, ANEOS"
4. "Rock strength. Basics (elasticity, placticity, frag-

mentation/damage, dry friction). Implementation into hydrocodes. Acoustic fluidization"
5. "Examples of numerical modeling implementation in a geoscience research projects: Puchezh-Katunki deep drill core analysis, trigger volcanism, penetration of the Europa ice crust".

Practice includes software (Fortran compiler and a hydrocode) installation, the code compiling with a graphic package PGPLOT [1].

Numerical code used for the short course is based on the SALE code [2], enhanced with options to compute multimaterial problems (2 materials plus vacuum) in the Eulerian mode with a simplified description of rock's elastic-plastic behavior. The code with a working name "SALEB" is armored with 2 kinds of EOS's: Tillotson's EOS [3] with an addition for the real temperature estimates [4], and tabulated ANEOS [5] for several types of rocks.

Practice includes the solution of 3 problems: shock recovery container (calcite in the iron container), vertical crater-forming impact, oblique 2D (planar) impact. Students have been asked to compute several variants changing the input file parameters to get an impression about sensitivity of results. Naturally, only initial stages has been modeled during the class hours.

Handouts included a CD ROM with the source code and a set of publications relevant to the topic. In addition, each lecture, prepared in PowerPoint has been printed out as handouts.

Conclusion. The experience with the short course shows that it is possible to organize a "quick entry" to the topic in a relatively short time for highly motivated students. Post-course correspondence shows that at least 4 students continue to work with the code. It is early to say is the course enough to begin a real numerical research. However, one can hope that the course will help all students to understand better publications about numerical modeling.

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COOLING OF THE KÄRDLA IMPACT CRATER: II. IMPACT AND GEOTHERMAL MODELLING.

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Introduction: Hydrothermal mineralization has occurred in many impact craters including also a 4-km marine complex crater in Kärdla, Estonia. Mineralogical and fluid inclusion data [1,2] provide temperature ranges for different mineralization events and, thus, giving a starting point for modelling. Modelling includes both (1) impact modelling to get the structure and temperature distribution in crater rocks right after the impact, and (2) geothermal modelling to get information on heat transfer processes and time-scale of post-impact cooling.

Impact Modelling: The target in Kärdla was about 150 m thick sedimentary layer on top of crystalline basement [3]. The impact took place in a ~100 m deep epicontinental Ordovician sea. SALE hydrocode was used to simulate formation, modification, and impact-induced heating in Kärdla crater. Both Tillotson equation of state and ANEOS algorithm were tested.

Modelling results suggest that usage of Tillotson equation of state gives very poor estimate of impact heating effect. It gives a temperature rise of ~100 K only, which contradicts with temperature of at least 300°C proven by PDF studies, quartz fluid inclusion homogenization temperatures, and chloritization geothermometry [1]. Maximum temperature estimate of 450°C [1] relies on formation of K-feldspar prior chloritization and maximum fluid inclusion homogenization temperature estimates. Results obtained using ANEOS algorithm are in better agreement with observations and suggest maximum temperatures of 300-350°C.

The crater is filled with resurge deposits which are at least 170 m thick. Unfortunately we were not able to simulate resurge flow and formation of resurge gullies with 2-D software in axisymmetric coordinates.

Geothermal Modelling: Post-modificational temperature distribution in crater rocks was one of the input parameters for transient fluid flow and heat transfer simulations for 2-D axisymmetric case. Fluid and rock properties were temperature-dependent. Effects due to fluid phase changes and associated latent heat effects were also implemented in the software.

The phase change of water has a double effect on heat transfer. First, when water vaporizes, its density decreases by more than one order of magnitude resulting in high buoyancy and rapid upward flow. Second, vaporization requires additional (latent) heat, which is absorbed from surrounding rocks resulting in their effective cooling at the high water vaporization rates.

The preliminary results suggest that vaporization of upward flowing fluid contributes significantly to cooling, decreasing the maximum temperature below boiling point (~ 250°C in case of Kärdla) in a few tens to hundreds of years. Heat transfer by liquid fluid is not as powerful as in vapor phase. The radiative heat transfer would start to contribute noticeably at temperatures above 600 °C, but is insignificant in Kärdla-size crater because of too low temperatures even immediately after the impact.

In the early stage of cooling, convective heat transfer prevails whereas at later stage conduction dominates. The ratio of convection over conduction (Peclet number) depends largely on assumed permeability structure. Direct measurements give information only about present day permeability, therefore, detailed investigations are needed to estimate the decrease of permeability due to closure of pores by hydrothermal mineralization.

It should be noted that the same hydrothermal mineral precipitated at a different time at different location inside the structure. Because different parts of the crater cooled at different rate the lifetime of hydrothermal mineralization varied. For example, at comparable depths the rocks in central uplift are not cooling as fast as rocks near the ring depression because, in respect to groundwater convective system, they are located at discharge and recharge areas, respectively. Rocks at rim might have got additional heat by upward flowing fluid.

Cooling to ambient temperatures in the central part of the crater lasts for thousands of years. Despite of relatively rapid cooling, the thermal perturbations in the deeper part of the central uplift should be observable with geothermal tools even a few tens of thousands of years after the impact.

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SEISMIC INVESTIGATION AND NUMERICAL MODELING OF THE LAKE BOSUMTWI IMPACT CRATER.

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Introduction: The Lake Bosumtwi impact crater, Ghana, (age 1.07 Ma, diameter 10.5 km) is one of the youngest and best-preserved complex terrestrial impact structures. It was excavated from hard crystalline target rock and is the source of the Ivory Coast tektite strewn field. It is almost entirely filled by the Lake Bosumtwi.

Seismic investigations of the Bosumtwi crater identify the proposed central uplift [1] and indicate a low-velocity breccia-layer below the lake and the post-impact sediments [2]. Recent evaluation of a longer seismic refraction line extends information on velocity-depth distribution down to ~1.7 km (Fig. 1). The structure is characterized by a vertical velocity gradient. Lateral velocity variations also occur. Higher seismic velocities are observed right below the central uplift, north and south of it velocities are lower. The area of higher velocity is interpreted to consist of uplifted basement originally situated at greater depth. The area of lower velocity is interpreted to be an allochthonous breccia cover surrounding the uplift. A distinct interface between the breccia layer and brecciated crater floor cannot be resolved. Lateral velocity changes occur down to a depth of 1.6 km below the lake indicating that rocks are brecciated down to at least this depth. The structural uplift is estimated by the 3.9 km/s-isoline to be at least 800 m. The apparent depth of the crater is 550 m.
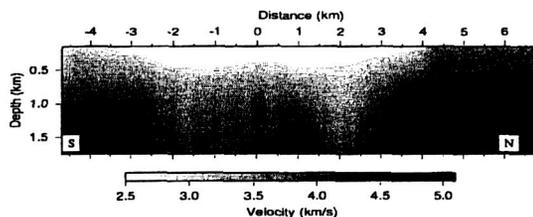
Fig. 1: Seismic velocity distribution of the Bosumtwi crater. Vertical reference is the estimated original target surface (150 m above lake surface). White area corresponds to water and post-impact sediments.

Numerical modeling of the crater is performed (1) with the SALE code to receive final crater shape after a vertical impact, and (2) with the SOVA code to model an oblique impact and tektites production. Projectile size estimates with scaling laws [3] vary in the range 400 - 1600 m, depending on impact angle and velocity. ANEOS equation of state for granite is used to describe both the target and the projectile.

Vertical impact and final crater shape. To simulate the temporal decrease of friction in rocks around a growing crater the "block model" (a version of the general acoustic fluidization model) is used [4]. Projectile velocity is 12 km/s, diameter is 750 m. The "block model" parameters for Bosumtwi have been published in [5]. The modeled rock mechanics include the gradual shear failure, an instant tension failure, the decrease of strength and internal friction close to the melt temperature, and the pressure dependence on the melt temperature. Variations of friction for damaged materials (0.2 - 0.5) and decay time for block oscillations (9 - 15 s) will produce a ~10 km in diameter crater, 200-300 m deeper than seismic data reconstruction (Fig. 2). Reasons for this discrepancy may be: (1) dilatancy of damaged rocks (not yet included); (2) deposition of fallout ejecta (suevite) inside the crater (in 2D models the ejected material is deposited outside the crater); (3) an oblique impact produces a shallower crater, but no strength model for 3D modeling is currently available.

Oblique impact and tektites. Most suitable conditions for tektite origin arise in the case of high-velocity impact (>20 km/s) with impact angle 30°-50°. 3D modeling shows that the fallout ejecta thickness inside the crater does not exceed 30-40 m. This is too thin to fill the gap between modeled and observed profiles.
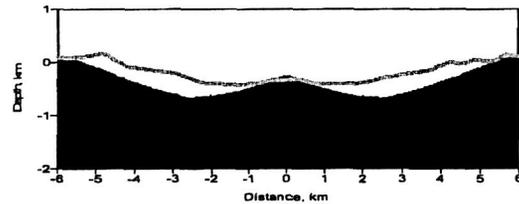
Fig.2: Density distribution after 100 s represents final crater shape obtained with the SALE modeling. Seismic-topographic profile is shown as grey line.

Discussion: Numerical modeling allows partial reconstruction (diameter, central uplift) of the Bosumtwi crater. Dilatancy and obliquity have to be included. Results from gravity and magnetic surveys and future scientific drilling (ICDP) will refine structural information of the crater and improve modeling results.

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EARLY FRACTURING AND IMPACT RESIDUE EMPLACEMENT: CAN MODELING HELP TO PREDICT THEIR LOCATION IN MAJOR CRATERS?

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Introduction: In a field investigation of a crater, where are the most effective places to look for material that could reveal the nature of object responsible for the impact? Can numerical modeling of impact processes help to predict locations in which recognisable residue of the bolide could be found?

Locations of Residue Preservation: The nature of an extraterrestrial body whose hypervelocity impact has created a terrestrial impact crater can sometimes be determined by collection of disrupted and shocked impactor fragments as loose fragments or within small bodies of impact melt from the ground surface in and around the crater: e.g. iron meteorites from the vicinity of Barringer Crater, (USA); Henbury (Australia); Sikhote Alin (Siberia), meteoritic debris and impact glasses from Lonar (India), Wabar (Saudi Arabia) and Monturaqui (Chile); and even meteoritic debris from the Eltanin impact (SE Pacific), sampled from the deep sea [1]. If a major melt component is still preserved at the crater e.g. glass bombs within the suevites at Ries (Germany) or as a substantial discrete melt body as at Popigai (Siberia), materials suitable for bulk trace-element or isotopic analysis may also be relatively easy to collect. When an impact feature has been substantially modified by subsequent erosion (e.g. Sierra Madera, Texas), it may prove more difficult, or impossible, to find chemical residue from the bolide for analysis. Characteristic large-scale impact-related structures, (e.g. central uplifts and ring-synclinoria) may remain, with diagnostic shock indicators (e.g. shatter cones, planar deformation features and high pressure mineral polymorphs), yet the ejecta-blanket and any impact melt body are lost.

Residues and Fractures: Some eroded structures do retain extraterrestrial residues and debris derived from higher structural levels, emplaced within fractures, e.g. 'Granophyre Dykes' into granulite facies basement at Vredefort (R. of South Africa) [2], and breccias in rim rocks at Roter Kamm (Namibia) [3]. For residue to penetrate along these fissures, is it not likely that fracturing must occur very early in the crater-forming process? In some craters there is also substantial outward motion of target debris along major radial fractures, such as the 'Offset Dykes' at Sudbury [4]. Outward compressive motion has been seen in the

reverse faults of the Chicxulub [5] and Silverpit [6] craters, also implying an early origin for these planes of movement. How often are major fractures created during early phases of crater growth? How widespread is residue emplacement into fractures?

Metallic residues, in melts and fractures: Distinctive, fine-scale siderophile segregations occur at a number of smaller (km scale) craters. At Lonar, Wabar, Monturaqui and Barringer, metallic residue is intimately associated with impact products, within silicate glass, vesicles, and brecciated rock fragments, or as thin coatings on target rock clasts. Residual metal may have a typical meteoritic Fe:Ni ratio, but often shows substantial modification of composition during and after impact. In a single small impactite specimen there may be metallic grains of widely differing texture and composition, with local enrichment of nickel and cobalt, and loss of iron to silicate-rich melt (Figure 1).


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X-RAY:     0 - 10 keV
Live:    100s Preset:   100s Remaining:     0s
Real:    146s      32% Dead
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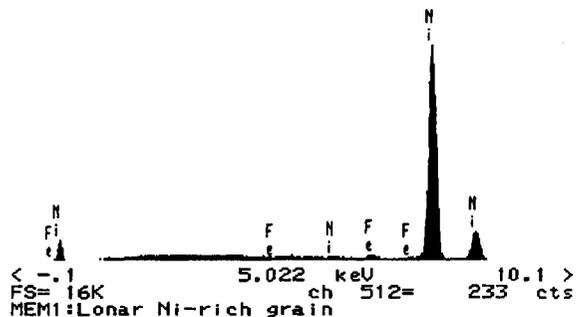
Fig. 1. Energy dispersive X-ray spectrum of Nickel grain from Lonar impactite.

Where iron-bearing oxides, such as ilmenite, occur in the target rocks, as in the basalts of the Lonar crater, metal may also be generated by mineral dissociation. The characteristic titanium content and the close proximity of melt droplets to remnant titanium oxides distinguish this metal from true impactor residue.

FRACTURING AND RESIDUE EMPLACEMENT: A. T. Kearsley et al.

At the Ries crater, brecciated and foliated granitic igneous basement rocks, cored in the Nördlingen 1973 borehole, contain dispersed tiny nickel grains, perhaps similar to [7], and whose abundance can reach levels equivalent to 660 ppm in the whole rock. Occurrence of these distinctive grains implies that modified impactor components were emplaced into deep target rock, subsequently uplifted during crater modification.

Evidence from small impact craters: As part of a separate study [8], we have made an extensive survey of millimeter-scale impact craters on brittle, laminated glass solar cells exposed to hypervelocity collision (typical velocity 25 km s^{-1}) during exposure in low Earth orbit on the Hubble Space Telescope. Craters may contain particulate impactor residue in fractures, as well as in a thin melt sheet. The fractures have previously been considered late-stage features, and due to extensional failure (spallation) close to the glass surface, following passage of a shock wave through the laminate structure. However, the presence of included micrometeoroid fragments suggests that the fractures must be formed early. Our laboratory experiments, utilising a range of mineral and metal grain projectiles accelerated to c. 5 km s^{-1} in a light gas gun (LGG), have revealed that delicate, volatile-rich residues can be emplaced into fractures around small craters on a variety of brittle substrates such as glasses and rocks.

Numerical modeling of crater formation: Modeling of small impacts [9], using AUTODYN 2D, has revealed that fractures can be generated at a surprisingly early stage in the impact process, prior to rebound of the crater floor and ejection of the bulk of remnants of the impacting body (Figure 2).
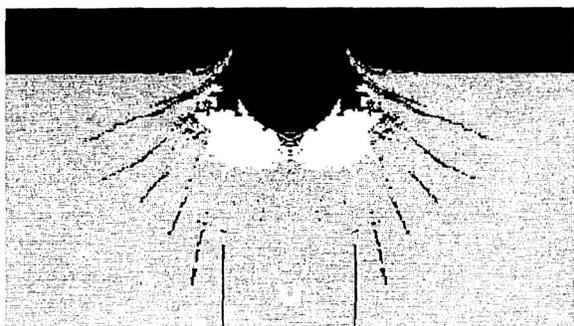
Fig. 2. AUTODYN 2D simulation of hypervelocity impact of a small metal sphere onto a glass target. Dark grey areas indicate failure, including fractures.

The model fractures correspond in position and orientation to locations in which we have observed residue in both space-exposed craters and laboratory light gas gun impacts. Numerous authors have shown that numerical modeling can be remarkably successful in simulation of larger features of crater development, and can account for many structures recognized during field examination of larger terrestrial craters. The significance of small (metre-scale) brittle structures in and around terrestrial impact craters is also becoming apparent from both modeling and field studies [10]. Important questions that have not yet been fully addressed include the timing and location of major fracture development in relation to the availability and possible pathways of bolide residue material. Tracing 'tagged' projectile material throughout the duration of the modeling process might prove rewarding. The potential role of early-formed fractures in outward transport, thickening of the pre-impact stratigraphic sequence, and localisation of structural weaknesses that permit subsequent inward crater collapse may also prove worthy of further investigation.

Conclusions: We believe that distinctive nickel-enriched residues can be used to track the presence of processed meteoritic metal. Small grains and melt droplets can be emplaced within silicate impact melts, within vesicles and within fractures in both impact clasts and deeper into basement. These observations imply that the fractures must have been present at a stage when bolide remnants were still abundant. Evidence from both space-exposed and laboratory-simulated hypervelocity impacts of small projectiles suggests that small craters can develop extensive fracturing at an early stage, when impactor residue is still available to be emplaced. Although we do not suggest that the results of simulation from a mm-size should be scaled to km-size craters, our intriguing results suggest that modeling the early brittle responses of geological materials in larger, lithified, stratified target sequences may help to explain the distribution of fracturing and residue emplacement in and around major craters. Such models may help to constrain the optimum sites for sampling around eroded craters.

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CRATER BASIN REBOUND ABOVE PLASTIC LAYERS: MODEL BASED ON EUROPA. Akos Kereszturi (Department of Physical and Historical Geology, Eötvös Loránd University of Sciences, H-1083 Budapest, Ludovika tér 2., Hungary, E-mail: krub@freemail.hu)

Introduction: Isostatic rebound and megaslumpings are important processes in the modification of large craters. Beside the exmaples for these on Mercury, Moon, Earth, Callisto (possibly Venus and Mars) we have good images from Europa. Analysis of internal rings and benches of great (usually greater than 100 km) craters and palimpsests help in the reconstruction of formation. The its young, pristine and tectonically homogene surfaced Europa can improve our knowledge in the reconstruction of crater basin formation.

The modell: Based on our up to date knowledge, the origin of the circular – and not central – ring structures are the follows (Fig. 1.) [1]: 1. Outcrops of isostatically uplifted internally layered matter [2],
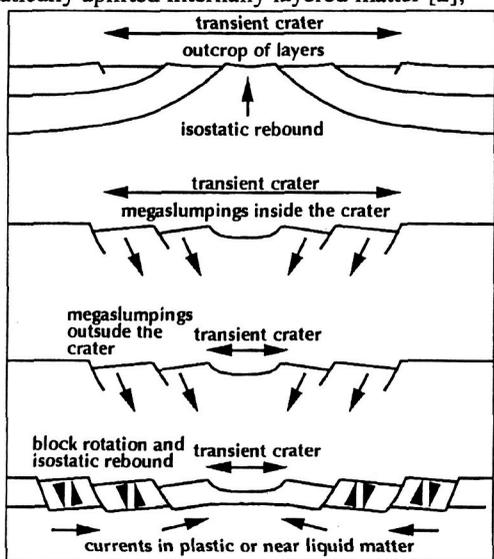
Fig. 1. Possible theories for the origin

2. Mega-slumpings inside the crater. 3. Megaslumpings outside the crater. 4. Block rotation and isostatic lifting [3]. With the analysis of the great craters of Europa we can nearly rule out the internal layering and slumping theories in the formation. Because of the thin ice crust Europa can serve as a unique modell for the crater formation on terrains with small lithospheric thickness, and it gives the possibility for the analysis of ancient craters on the Earth and current craters on Venus with relative thin litospheres.

Results: We analysed 32 relative great craters on icy moons, the best examples of them are on Europa (Fig. 2.). We make a somewhat similar analysis for the greatest basins on rocky bodies (eg. Caloris, Orientale, Argyre). We measured the diameters of the structures, the topography, the distribution of certain structures

according to the crater diameter/the possible thickness of the lithosphere/cryosphere, distance from the center. The greatest problem is the definition of the original crater rim or the transient crater and to devide the internal rings from the outer narrow tectonic structures. We suggests: 1. Structures are originated by isostatic re- bound and not by megaslumpings or outcrops of layered matter. 2. Circular faults ouside the original craters form in great number on icy bodies. In the future we will extend the analysis: 1. Relation between possible transient crater diameter and outer rings. 2. To make „evolutionary sequence" for giant craters with rebounded floors accourding to the reaction of the lithosphere and gravity [5,6], which can be useful in the analysis of ancient rheologic conditions in rocky bodies.
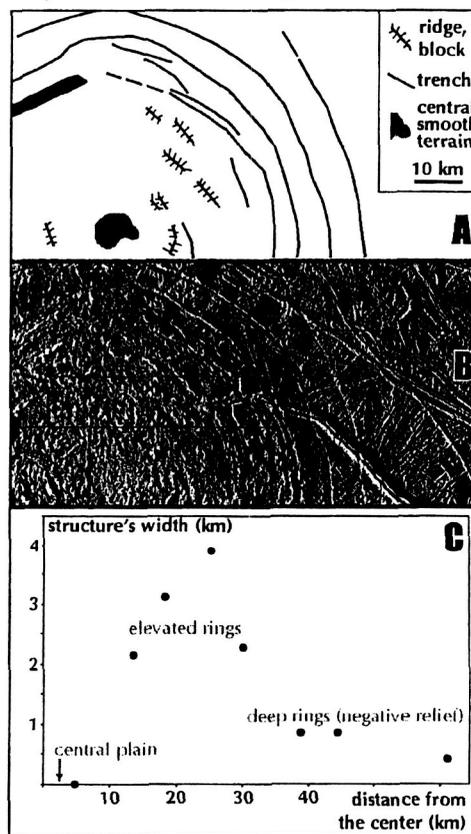
Fig. 2. Example structures

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USING GEOCHEMICAL OBSERVATIONS TO CONSTRAIN PROJECTILE TYPES IN IMPACT CRATERING.

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Introduction: Breccias and melt rocks found at possible meteorite impact structures on Earth may contain a minor extraterrestrial component. In the absence of evidence of shock metamorphic effects in such rocks, the unambiguous detection of an extraterrestrial component can be of diagnostic value regarding the impact origin of a geological structure. The verification of an extraterrestrial component in impact-derived melt rocks or breccias can be of diagnostic value to provide confirming evidence for an impact origin of a geological structure. Similar approaches are of great value in the investigation of distal ejecta layers (as we are taught by the case history of the Cretaceous-Tertiary boundary).

Qualitatively speaking, a small amount of the finely dispersed meteoritic melt or vapor is mixed during the impact event with a much larger quantity of target rock vapor and melt, and this mixture later forms impact melt rocks, melt breccias, or impact glass. In most cases, the contribution of meteoritic matter to these impactite lithologies is very small (<<1%), leading to only slight chemical changes in the resulting impactites. Geochemical methods can be used to determine the amount of such a meteoritic component (see below). However, there are plenty of open questions.

Methods: The detection of such small amounts of meteoritic matter within the normal upper crustal compositional signature of the target rocks is rather difficult. Only elements that have high abundances in meteorites, but low abundances in terrestrial crustal rocks (e.g., the siderophile elements) are useful. Another complication is the existence of a variety of meteorite groups and types, with widely varying siderophile element compositions. Distinctly higher siderophile element contents in impact melts, compared to target rock abundances, can be indicative of the presence of either a chondritic or an iron meteoritic component. Achondritic projectiles (stony meteorites that underwent magmatic differentiation) are much more difficult to discern, because they have significantly lower abundances of the key siderophile elements. Furthermore, in order to reliably constrain the target rock contribution of such elements, i.e., the so-called indigenous component, absolute certainty must be attained that all contributing terrestrial target rocks have been identified and their relative contributions to the melt mixture are reasonably well known.

Geochemical methods have been used to determine the presence of the traces of such an extrater-

restrial component (see review [1]). Meteoritic components have been identified for just over 40 impact structures [1], out of the more than 160 impact structures that have so far been identified on Earth. The identification of a meteoritic component can be achieved by determining the concentrations and inter-element ratios of siderophile elements, especially the platinum group elements (PGEs), which are several orders of magnitude more abundant in meteorites than in terrestrial upper crustal rocks. Iridium is most often determined as a proxy for all PGEs, because it can be measured with the best detection limit of all PGEs by neutron activation analysis (which was, for a long time, the only more or less routine method for Ir measurements at sub-ppb abundance levels in small samples).

The use of PGE abundances and ratios avoids some of the ambiguities that result if only moderately siderophile elements (e.g., Cr, Co, Ni) are used in an identification attempt. However, problems may arise if the target rocks have high abundances of siderophile elements or if the siderophile element concentrations in the impactites are very low. In such cases, the Os and Cr isotopic systems can be used to establish the presence of a meteoritic component in a number of impact melt rocks and breccias (e.g., [2]). In the past, PGE data were used to estimate the type or class of meteorite for the impactor, but these attempts were not always successful. It is difficult to distinguish among different chondrite types based on siderophile element (or even PGE) abundances, which has led to conflicting conclusions regarding the nature of the impactor at a number of structures (see [1]). Clearly, the identification of a meteoritic component in impactites is not a trivial problem.

Open Questions: Apart from analytical challenges, there is a whole suite of problems or questions associated with the identification of projectiles, which will be listed here in no particular order.

Some meteorite types do not have chemical compositions that are well enough separated from terrestrial rocks to allow a geochemical distinction in melt rocks. The chemical composition of specimens of the same meteorite type is not uniform, but shows a range of compositions. In addition, only a few samples of each type have been analyzed with enough detail to allow use of the data for mixing calculations. It is not yet possible to distinguish between comet and asteroid sources due to the lack of trace element data on a sufficient number of comet nuclei samples.

IMPACTOR TYPE IDENTIFICATION: C. Koeberl

More peculiar, and possibly a point in which modeling calculations can be of use, is the very strange discrepancy between the interelement ratios of siderophile elements in impact glasses found at small impact craters and equivalent ratios in corresponding meteorite fragments found at the same craters (e.g., Meteor Crater, Wolfe Creek, Henbury, Wabar). No immediate physical explanation, or correlation with chemical and physical parameters, which could explain this fractionation, is available. In some other cases (e.g., Tswaing-Saltpan, Bosumtwi) there is a good fit for, e.g., Cr, Co, and Ni ratios and abundances between a particular meteorite type (e.g., chondrite), but the Ir abundances are about a factor of 2-10 too low for a chondritic projectile (which might otherwise also be confirmed by isotopic data). Why are some of the more refractory siderophile elements depleted? Is there some non-equilibrium process going on in the impact vapor plume?

Another interesting item are tektites. Tektites are natural glasses occurring on earth in four distinct strewn fields: Australasian, Ivory Coast, Central European, and North American. Ages of these strewn fields range from 0.78 to 35 million years. Geochemical arguments have shown that tektites have been derived by hypervelocity impact melting from terrestrial upper crustal rocks. Tektites are distal ejecta, which do not occur directly at a source crater, in contrast to impact glasses, which are found directly in or at the respective source crater. This has made the identification of the source crater somewhat difficult. Nevertheless, at least two of the four Cenozoic tektite strewn fields have been associated with known impact craters: the Ries crater in southern Germany and the Central European field, and the Bosumtwi crater in Ghana and the Ivory Coast field are rather firmly linked. In addition, the 85 km diameter Chesapeake Bay impact structure is a likely source crater for the North American tektites. This leaves the Australasian tektites as the only strewn field without a clear choice for a source crater.

Not much is known about the source meteorites (projectiles, meteorite types) for the four tektite fields. Attempts to determine of a meteoritic component in Australasian tektites has not yielded unambiguous results. Some Ni-Fe-rich spherules in philippinites, which were suggested to be a remnant of meteoritic matter, were later concluded to have formed by in-situ reduction from target material. Analyses of australites by radiochemical neutron activation analysis for a selection of volatile and siderophile element concentrations was not very conclusive either - only one of these samples showed a distinct enrichment in siderophile elements, while the other five do not indicate such an enrichment. On the other hand, Ir enrichments were found in several microtektite-bearing deep-sea sediment layers.

Regarding the Ivory Coast tektites, some researchers suggested an iron meteorite projectile (based on chemical data), others (more recently suggested a chondritic projectile). Os isotopic data clearly showed the presence of a meteoritic component in the tektites. Unfortunately, the Bosumtwi crater is in an area of known gold mineralization, which lead to high and irregular siderophile element contents in the target rocks.

Not much information is available regarding the Central European tektites, where an achondrite has been proposed for the Ries crater bolide. No information at all is available regarding the Chesapeake Bay crater/North American tektites. Thus, the question of projectile identification for tektites is still an open one.

In general, tektites are very poor in meteoritic matter, which led to the suggestion that they cannot form by jetting, as products formed by jetting should have high meteoritic components. On the other hand, tektites clearly formed from the rocks closest to the terrestrial surface – in some cases there is a soil component discernable. However, some recent data show that high-Mg microtektites do seem to have a significant (a few percent) meteoritic component. It seems that natural observations are still able to provide some puzzling constraints for future modeling calculations.

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AMELIA CREEK, NORTHERN TERRITORY, AUSTRALIA: A 20 × 12 KM OBLIQUE IMPACT STRUCTURE WITH NO CENTRAL UPLIFT. F. A. Macdonald[1] and K. Mitchell[2], [1]Division of Geological and Planetary Sciences, Mail Code 170-25, California Institute of Technology, Pasadena, CA 91125, USA, francis@gps.caltech.edu, [2]Gerringong, NSW, Australia.

Introduction: The Amelia Creek Structure is located in the Davenport Ranges of the Northern Territory, Australia at lat. 20°55'S, long. 134°50E. Shock metamorphic features are developed on the southern, down-range side of the structure. No central uplift is developed and the dimensions of the impact structure are at least 20 × 12 km.

Geological Observations: Geologically, the Amelia Creek structure is situated within the Proterozoic sedimentary and volcanic rocks of the southern Tennant Creek Inlier. The structure is characterized by a central syncline flanked by a series of ramping, SSW trending thrust sheets. The canoe-shaped central trough (syncline) runs NNE-SSW and is ~1 km wide and 5 km long. Shatter cones, impact breccias and hydrothermal deposits were also discovered during detailed mapping of the central region in June of 2002.

Shatter cones at Amelia Creek are prolific in many quartzite beds on the southern side of the structure (fig. 1), and are invariably oriented upward, which in itself excludes the possibility that the impact occurred before the regional folding at ~1700 Ma [1]. The surface distribution of shatter cones forms a crescent-like shape approximately 1 × 3 kilometers on the southern side of the structure, extending at least 4 km south from the central syncline. Similiar lithogies are present throughout the structure; however, shattercones are only developed on the southern, down-range side. Allogenic breccias are developed along many of the major thrust faults within the structure and show evidence of baked margins and shocked clasts.

Discussion: Most impacts occur obliquely, not vertically as typically modeled [3]. In very oblique impacts, the initial transfer of energy into the target is less efficient and the resulting craters are smaller for a given impactor mass and velocity [2]; oblique impacts should produce much shallower deformation than their more vertical counterparts, and perhaps central uplifts do not develop even for large structures.

Structurally, the level of erosion at Amelia Creek appears to be less than a kilometer, however, the exhumed land surface that makes up the flat tops of the hills across the Davenport Ranges is early Cambrian or late Neoproterozoic in age [4], indicating that the structure may have been buried for much of the Phanerozoic. Some breccias in and around the structure were originally mapped as Cambrian and Tertiary breccias [1], but they may actually be impact breccias and impact ejecta. Thus, the age of the structure remains equivocal until relationships mapped in earlier

work [1] are verified.

The rocks up-range of the structure also appear to be anomalously deformed, so there is a distinct possibility that Amelia Creek is part of a crater field or a ricochet structure. On geological maps, Aster and aeromagnetic images, the total area of anomalous deformation around Amelia Creek is strikingly similar in shape to the extremely oblique impact structures on Mars and the Moon [3].

Fig. 1 Shatter cones on southern side of structure.

Conclusion: We believe that the shock metamorphosed rocks at Amelia Creek are the relict of an extremely oblique impact event. Evidence for this includes the elongation of the deformed area, the SSW direction of movement of most of the structural elements, the presence of a central trough and syncline in place of a central uplift, and the distribution of shatter cones only on the downrange side of the structure.

The mechanics of large, very oblique impact cratering is poorly understood [2]. This is due in part to the fact that no exposed, extremely oblique terrestrial impact structures have been previously reported [5]. As such, there are very few field measurements to put constraints on theoretical models. The impact-deformed rocks in the Davenport Ranges are incredibly well exposed, and this structure promises to be the world's type locality for oblique impacting.

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GOLDILOCKS AND THE THREE COMPLEX CRATER SCALING LAWS.

GOLDILOCKS AND THE THREE COMPLEX CRATER SCALING LAWS. William B. McKinnon,[1] Paul M. Schenk[2], and Jeffrey M. Moore[3], [1]Dept. of Earth and Planetary Sciences and McDonnell Center for the Space Sciences, Washington University, Saint Louis, MO 63130, mckinnon@levee.wustl.edu; [2]Lunar and Planetary Institute, 3600 Bay Area Blvd, Houston, TX 77058, schenk@lpi.usra.edu, [3]NASA Ames Research Center, Moffett Field, CA 94035, jmoore@mail.arc.nasa.gov.

Introduction: Formed in the gravity regime, complex craters are larger than their simple crater equivalents, due to a combination of slumping and uplift. Just how much larger is a matter of great interest for, for example, age dating studies. We examine three empirical scaling laws for complex crater size [1-3], examining their strengths and weaknesses, as well as asking how well they accord with previously published and new data from lunar, terrestrial, and venusian craters.

Croft (1985): The most widely quoted complex crater scaling is due to the detailed study of S.K. Croft [1]. He gauged the upper and lower limits to the position of the transient crater rim provided, respectively, by the terrace sets and central peak complexes of lunar and terrestrial complex craters. Added to these were a range of crater enlargements based on theoretical and experimental evidence for the geometric similarity of ejecta blankets [4]. Finally, a geometric restoration model was used to get an independent estimate. Bracketed mainly by terrace sets for craters closer to the simple-to-complex transition and central peak complexes of very large lunar craters (a size range that could have included peak-ring basins), he determined that the transient diameter D_{tr} scaled as $D^{0.85 \, \bullet \, 0.04}$, where D is the final diameter. Inverting, we get

$$D = D_c^{-0.18 \pm 0.05} D_{tr}^{1.18 \pm 0.06} , \quad (1)$$

where D_c is the diameter of the simple-to-complex transition. A little remarked on aspect of this scaling law is that it nearly restores the diameter (through not the volume) of complex craters to strength scaling (i.e., D is proportional to $a^{0.92}$, where a is the impactor radius).

McKinnon and Schenk (1985): We used a transient crater restoration model for the Moon, based on Pike's lunar crater morphometric data [5]. Crater rims were restored using a range of constant slope angles for the ejecta deposit, with the restoration criterion being that the transient apparent (ground-plane) crater had a depth/diameter of $1/2\sqrt{2}$ [6]. Remarkably, the derived depth/diameter ratios for the full transient crater were close to constant, which is self-consistent support for transient crater geometric similarity. In terms of fit to a power law, we found

$$D = k D_{tr}^{1.13} , \quad (2)$$

where k is a constant. For the Moon, our k implied that simple craters near the simple-to-complex transition (~11 km from depth/diameter statistics) are ~15-20% wider than their original transient craters. This amount agrees with the amount of widening calculated for Brent and Meteor Craters due to breccia lens formation [6]. At the time it was less appreciated that all simple craters in rock are probably shallowed and widened by breccia lens formation. Breccia lens formation is something that has not been observed in laboratory impact studies to our knowledge (certainly not in dry sand), so direct application of sand crater scaling laws, even to simple craters, should be done with caution.

As for eq. (2), it can be put in the same functional form as eq. (1) if k is proportional to $D_c^{-0.13}$, and we recommend $k = 1.17 D_c^{-0.13}$. Using such, [2] were able to show that the continuous ejecta blankets on the Moon and Mercury measured by [7] could be close to geometrically similar if compared in terms of transient crater diameter.

Holsapple (1993): Holsapple presented, in his review of crater scaling, a new model for complex crater scaling, also based on volume conserving geometric restoration, but using improved functional forms for the ejecta blankets of craters derived from laboratory experiments in sand [e.g., 4]. Although details were not given, the overall functional form is familiar:

$$D = 1.02 D_c^{-0.086} D_{tr}^{1.086} . \quad (3)$$

A slightly different form was given in terms of transient excavation radius, which presumably refers to the ground plane.

Comparisons: All three scaling laws have similar forms but clearly different exponential dependences. They cannot all be correct. Each scaling law uses a different definition or value for D_c on the Moon, as well, which complicates comparisons. In terms of an "equivalent simple crater," however, eqs. (1-3) predict, e.g., 70.7, 74.1, and 79.7 km, respectively, for the 93-km-diameter Copernicus. We will discuss which of the formulations give too much or too little crater enlargement, and which if any might be considered "just right."

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MODELING METEORITE IMPACTS: WHAT WE KNOW AND WHAT WE WOULD LIKE TO KNOW.

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Meteorite impacts can be studied by computer simulation: Large meteorite impacts are among those phenomena that are either too large or too dangerous to study experimentally. Although impacts have affected the formation and surfaces of nearly every body in the solar system, we are limited to observing the results of past events. Investigation of impact processes is thus divided into observational studies of the traces of past impacts, small-scale analogue laboratory experiments and, most recently, detailed computer modeling. Computer models offer the possibility of studying craters at all scales, provided we completely understand the physics of the process and possess enough computer power to simulate the features of interest [1].

But computer models cannot do everything! One of the most common disappointments of geologists not familiar with modeling is that computer simulations cannot answer all questions we might like to ask. Numerical simulations suffer two major shortcomings: One is that they cannot treat processes that are not included in the computer code. Thus, no computer code presently treats the chemical or isotopic interactions that occur during an impact. This does not mean that such processes are untreatable, just that the appropriate codes that embody the correct physics must be created. In some cases the physics is poorly known and research must be done to improve the basic foundations. The second shortcoming stems from resolution in both space and time. All digital computer simulations depend on dissecting time and space into discrete blocks. The number of such blocks is limited by the amount of time and physical memory available for the computation. These limits can be easily exceeded by even an apparently modest computation. Thus, if an investigator wants to know about the dynamics of meter-size ejecta blocks in a 10 km diameter impact crater, he or she may discover that the required resolution far exceeds the capacity of any existing computer (a 3-D computation must include at least 10^{12} computational cells!). Models to "predict" the effects of the impacts of Shoemaker/Levy 9 fragments with Jupiter [2] were still running at the time of the impacts, more than a year after the comet was discovered! These limitations can be surmounted both by faster computers with more memory as well as by better solution algorithms, such as the recent adoption of SPH codes when both hydrodynamics and self-gravity are important in a simulation [3].

Before beginning any computer simulation it is important to ask whether the numerical computation is capable of answering the desired question. Are all of the relevant processes included in the code to be used? Can the problem be solved in reasonable time on the available hardware? Too often the answer is "no" and the potential modeler must look elsewhere for enlightenment. But there are plenty of open questions that are still ripe for computer solutions.

The three pillars of impact simulation: The physics needed to simulate large meteorite impacts lies squarely in the classical domain. The size scale is so large that quantum effects are not important (although quantum mechanics does determine the thermodynamic equation of state) and the velocities are well below the speed of light, so classical Newtonian mechanics, supplemented by classical thermodynamics, provides an adequate framework for modeling impacts. In addition, it has become clear that successful simulation of real impact craters often requires a detailed understanding of the response of real rocks to stress and heat.

Of these three supporting pillars, Newtonian mechanics is probably the least troublesome. All modern "hydrocodes" (a now obsolete term that reflects the historical development of computer codes that, at first, did not contain material strength) incorporate the standard $F = ma$ foundation of mechanics, although this is often obscured by an impressive amount of bookkeeping to keep track of all the pieces. All codes incorporate some form of gravitational acceleration, although only a few employ self-gravitation (only important in planet- scale impacts). It is notable that there do not appear to be any talks at this conference on this aspect of computer modeling.

The next supporting pillar is thermodynamics, through the equation of state [4]. The equation of state for impact modeling is a little peculiar: Instead of the conventional thermodynamic relation relating pressure P to density ρ and temperature T, $P(\rho,T)$, hydrocodes require a relation between P, ρ and internal energy E. Equations of state for metals have been vigorously pursued by squadrons of physicists since the end of WWII, mainly to support the design and testing of nuclear weapons. However, few good equations of state exist for geologic materials, such as rock or ice. More research is needed to create these important relations.

Finally, in the late stages of an impact event material strength becomes important. Very little work has been done on good strength models for rock [5]. Porosity is also now recognized to play a key role for some impacts, especially on asteroids, which recent research has shown might be as much as 50% porous. Impact crater collapse and the morphology of large craters are controlled by strength, and observations suggest that a poorly understood mechanism must operate to greatly degrade the strength of rocks surrounding an impact site shortly after an impact event

MODELING IMPACTS: H. J. Melosh

[6].

What next? Our ability to numerically simulate impact events is currently being taxed by a number of difficult problems. We are concerned about the possibility of impacts causing future extinctions, as they did at the K/T boundary. Two and three-dimensional models have already been used to estimate the mass and type of environmentally active gases released by the impact [7], but the ultimate effects of these gases on climate is still largely unknown. Chemical reactions of material in hot vapor plumes may be important for both environmental effects as well as explaining the observed oxidation state and isotopic fractions observed in the ejecta. Several new craters with unusual morphologies such as the Silverpits crater in the North Sea [8] and the Chesapeake Bay crater [9] challenge our understanding of the response of the Earth's surface to large impacts. Crater morphologies on Europa [10] may be indicating the thickness of the ice shell beneath the surface, but we must understand the cratering process better before we can cite a numerical value for the thickness. An active question is whether damaging tsunami result from relatively small impacts in the Earth's ocean. Solving this problem requires a full understanding of interactions near the surface and the physics of wave breaking, a new challenge to existing computer codes.

We currently have a list of urgent needs for making our simulations more realistic. Much work is needed in the near term on equations of state and constitutive models for geologic materials. We will hear more about these needs in subsequent talks. Nevertheless, numerical modeling of impact processes has made important contributions to our understanding of impacts in the past and will surely continue to do so in the future.

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LIMITS TO THE PRESENCE OF IMPACT-INDUCED HYDROTHERMAL ALTERATION IN SMALL IMPACT CRATERS ON THE EARTH: IMPLICATIONS FOR THE IMPORTANCE OF SMALL CRATERS ON MARS.

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Introduction: Impact craters on the earth contain evidence for hydrothermal activity. An important property of small craters is the limit to the amount of energy deposited during the impact that can lead to hydrothermal activity. Hydrothermal activity is potentially important for producing alteration minerals, trapping water, and transporting mobile elements to the martian surface. Hydrothermal systems in impact craters may also be important for astrobiological investigations in terms of providing environments for organic chemical processes to occur and as near-surface locations that could be easily investigated by surface exploration missions [1]. Another important reason for understanding the lower limit on thermal effects for small craters is in the use of small superimposed craters as probes of larger craters during surface missions. If hydrothermal material is found associated with superimposed craters it will be important to distinguish between hydrothermal events associated with the earlier versus the later crater. In the future, comparisons of our observations with numerical models for the formation of small craters can lead to a better understanding of the role of small craters on Mars.

Lonar Crater: The 50,000 year old, 1.8km diameter Lonar crater is located in Maharashtra, India (19°58'N, 76°31'E) [2]. This relatively small crater is of particular interest because of its unique morphological and mineralogical properties, which make it a valid analogue for similar craters on the surface of Mars [2, 3]. We show that even in this relatively small crater substantial hydrothermal alteration has occurred, probably due to the thermal effects of the impact event.

In addition to textural data from the SEM, microprobe and X-ray diffraction were used to determine the nature of alteration minerals in the Lonar samples. The microprobe results suggest that the majority of the clay materials in the Lonar samples are saponites and celadonites. Both saponite and celadonite are produced during the hydrothermal alteration of basalt, typically at temperatures of 130-200°C. The production of these "hydrothermal" clays at Lonar was further established through geochemical modeling of the alteration process, and by stable isotope analysis.

Limits to hydrothermal activity in terrestrial craters: The presence of hydrothermal alteration at the Lonar crater can be used to suggest that Lonar is near the lower heat limit for generating hydrothermal

processes, thus establishing a new lower size limit of 1.8 km diameter for impact-induced hydrothermal activity. A hydrothermal system has been documented in the somewhat larger 4 km diameter Kärdla impact crater [4]. In contrast, no evidence of hydrothermal activity has been found in the smaller 1.13 km diameter Pretoria Saltpan (Tswaing) crater [5], or in the 1.2 km diameter Meteor Crater in Arizona [6]. This information can be used to imply that small martian craters greater than one or two kilometers in diameter may also have the potential to form hydrothermal systems, as long as water was present in some form.

Implications for Mars: Hydrothermal alteration is important for trapping fluids, such as water in the subsurface of Mars, and for releasing material to the surface. As a preliminary example, the amount of water that could be trapped due to alteration of craters in the size range from 2 to 11 km in diameter can be calculated. Assuming an average depth of alteration of 400 m, a degree of alteration of 3% based on the average of our SEM feature scan determinations, a volume of altered material equivalent to a global layer of 2.8 m will be formed over martian history. Assuming a water content of 10 wt% (e.g. similar to the amount in Lafayette martian meteorite iddingsite alteration material) this amount of material could trap an amount of water equivalent to a global layer of water 0.7 m deep. The one-meter value compares to estimates of the amount of water on Mars ranging up to a few hundred meters. In contrast Griffith and Shock [7] estimated that 8% alteration of 10% of the Martian crust could trap 30 m global equivalent of water.

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SULFUR CHEMISTRY IN K/T-SIZED IMPACT VAPOR CLOUDS.

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Introduction: The geologic record indicates that the mass extinction at K/T boundary, 65 Myrs ago, was caused by a hypervelocity impact of an asteroid or a comet [1]. During the K/T impact event, a large amount of sulfur was degassed from the impact site [e.g., 2, 3, 4]. The degassed sulfur converts to sulfuric acid aerosol and stays in the stratosphere for a long time [3, 4]. This reduces the sunlight significantly and leads to a mass extinction. However, if the degassed sulfur is dominated by SO_3 not SO_2, then the conversion to sulfuric acid aerosol occurs very rapidly and the blockage of sunlight does not last for a long time [3, 4, 5]. The chemical reaction of sulfur-oxides in an impact vapor cloud, nevertheless, has not been studied in detail previously, and the SO_2/SO_3 ratio in a vapor cloud is yet highly uncertain. The purpose of this study is to estimate the SO_2/SO_3 ratio in the K/T impact vapor cloud. Here we discuss the results of calculation of chemical equilibrium and kinetics of sulfur-containing species in an impact vapor cloud as well as mass spectroscopic analysis of vapor plumes created by laser irradiation on anhydrite.

Chemical Equilibrium Calculation: We calculated equilibrium chemical composition in vapor clouds generated from calcium sulfate ($CaSO_4$). We assumed several different impact velocities and different types of projectiles for the K/T impact.

The result of the calculation indicates that $SO_2+1/2O_2$ is more stable at high temperatures and high pressures and that SO_3 is more stable at low temperatures and low pressures. Over the entire range of the impact conditions we assumed, the SO_2/SO_3 ratio dramatically changes in the range between 600K and 1000K. If the reaction SO_2+O to SO_3 quenches at a temperature higher than 1000K, most of impact-degassed sulfur is released to the environment as SO_2. However, if the reaction SO_2+O to SO_3 quenches at a temperature lower than 600K, SO_3 is dominant.

Kinetics of Redox Reaction of Sulfur Oxides: We estimate the SO_2/SO_3 ratio in vapor clouds at the quenching temperature using a theoretical evaluation of chemical reaction rate of the reaction SO_2+O+M to SO_3+M [6]. The result of the calculation indicates that the SO_2/SO_3 ratio is smaller for a vapor cloud with a larger mass and that the SO_2/SO_3 ratio in a K/T-size vapor cloud is approximately unity. Because the result of this kinetic model estimation is an upper limit of the SO_2/SO_3, the SO_2/SO_3 ratio in K/T-size impact vapor cloud may have been much smaller than unity.

Laser Irradiation Experiment: A YAG laser beam (1.06μm of wave length, 25-400 mJ of pulse energy, 0.5-2 mm of irradiation spot diameter) was irradiated to a sample of anhydrite in a vacuum chamber. Vapor degassed by laser irradiation was analyzed with a quadrupole mass spectrometer (QMS). The gas sample obtained in every laser irradiation experiment was dominated by SO_2, but SO_3 was also detected. The SO_2/SO_3 ratios measured in experiments were between 80 and 300, and decrease with the laser beam diameter. The dependence of the SO_2/SO_3 ratio on laser beam diameter is $SO_2/SO_3 = 120D^{-0.61}$.

The SO_2/SO_3 ratio in the experiment is about 10^{-3} time that in the kinetic model estimation for the size of vapor clouds produced in the laboratory. Our experimental results also show that the rate of decrease in the SO_2/SO_3 ratio obtained in the laser experiment as a function of vapor mass is higher than that predicted by the kinetic calculation. The power-low relation obtained in the laser experiments predicts that it will be 10^{-6} for a K/T-size impact vapor cloud. This strongly suggests the possibility that SO_3 was dominant in the degassed sulfur by the K/T impact.

Conclusion: Chemical equilibrium calculation indicates that SO_3 is more stable than $SO_2+1/2O_2$ at low temperatures and low pressures. Kinetic model calculation shows that the SO_2/SO_3 ratio in a K/T-size vapor cloud is lass than unity. The SO_2/SO_3 ratio estimated based on the laser-irradiation experiments is about 10^{-6} for a K/T-size vapor cloud. Three lines of evidence strongly suggests that the SO_2/SO_3 ratio in K/T impact vapor cloud may have been much smaller than 1. Then sulfuric acid aerosol may not have blocked the sunlight for a long time. Instead, there may have been an extremely intense global acid rain immediately after (<100 days) the K/T impact.

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IMPACT INDUCED TARGET THERMO-MECHANICAL STATES AND PARTICLE MOTION

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Objectives The first objective of this effort is to determine how the post impact measurable crater features relate to the processes that take place during impact and the second is to the determine from a given suite of measurements the uncertainty in estimating the impactor's parameters.

Approach. We have taken a numerical approach using the CTH code[1] to calculate the evolution of the near field impact process. This includes the details of the early time shock wave driven flow fields, the development and collapse of the transient cavity [2], and in a few limited cases the very late time thermal and stress histories. To quantify the impact process, we placed massless tracer particles in layers that simulate the target stratigraphy (Figs 1-4) and stored the motion and thermo-mechanical state histories (e.g. pressure, temperature, damage, peak stress/strain rate..) of these particles. We took this approach because the late time distributions are significantly different from the initial distributions. We used the ANEOS model for equation of state and a Mohr-Coulomb damage model for the strength degradation by shear strain fracture [2,3]. The key parameters for the impacts are a, the impactor radius, U, the impactor velocity, Yc, target cohesive strength, μ, internal friction, μd, damaged internal friction. We found that we could replicate the key features with values of target material parameters within the magnitudes found in laboratory measurements. We developed scaling laws for the key target metrics based upon the Mohr-Coulomb strength model. This provides a the link between the measureable features and the impactor parameters,. In addition it, bounds the effect of damage on the magnitude of the metrics.

Target Motion Histories and Thermo-mechanicl States..

Shown in Figs 1-4 are the particle motion histories and the melted and damaged (shear fractured) regions for three representative cases: 1) simple crater –strength dominated, 2) transition crater - between strength and gravity regimes, and a 3) basin forming impact represented by the Chixculub event[4].:

The geometry of the flow in the strength dominated case (Fig. 1) is very similar to that of all cases at the time of maximum penetration. The melt has two major zones. The melt layer and melt ejecta. The melt layer is underneath the impact point and is on top the damaged region, The trajectories of the melt particles are shown and labeled at the top of the computational grid.

We found that in the strength dominated region that that the depth of penetration decreases with the magnitude of the internal friction. This is due to the dynamic pressure increasing the local strength.

An example of a transition crater is shown in Fig. 2. In this case the low strength material flows over and covers part of the melt layer.

As an example of the motion histories and thermo-mechanical states in basin forming impacts, we simulated the Chicxulub event. The distribution and extent of the damaged region is critical to the crater flow and determines 1) transient cavity dimensions (e.g. depth of penetration), 2) ejecta lofting angles, 3) occurrence and number of terrace/slump faults and 4) distribution of melt. The radial extent of the damage region that replicates the Chicxulub morphology is ~ 100 km. (Fig. 4). At the time of maximum penetration, the transient cavity geometry is similar to Fig. 1. The transient cavity collapses and compresses the melt layer to a region near the center of the cavity and on top of the damaged material (e.g. Fig.3). After the transient peak collapses, the melt flows in a thin layer over the peak ring (Fig. 4), The peak ring is formed by the collision of the downward flowing transient peak with the nearly vertically launched transient cavity flow. Note that while the transient central peak is moving upward that the ejecta curtain is still impacting the surface and that slumping is occurring in front of the ejecta curtain (Fig. 3). In addition, an asymmetric fault (diameter = 150 km) is formed that bounds the terraced zone and extends downward to the Moho. This feature has been interpreted as the crater rim [4]. On the other hand, the radius of the overturned stratigraphy (Fig 4), which is a measure of the transient cavity size is probally a more accurate determinant of the energy of impact [5]. Further out, a 200 km diameter exterior ring is formed as a result of secondary impact of ejecta on the damaged region. The Mohr-Coulomb scaling accounts for basin forming impacts and shows the effect of internal friction on depth of penetration and quantifies the effect of overburden pressure.

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PARTICLE MOTION HISTORIES: J. D. O'Keefe and T.J. Ahrens
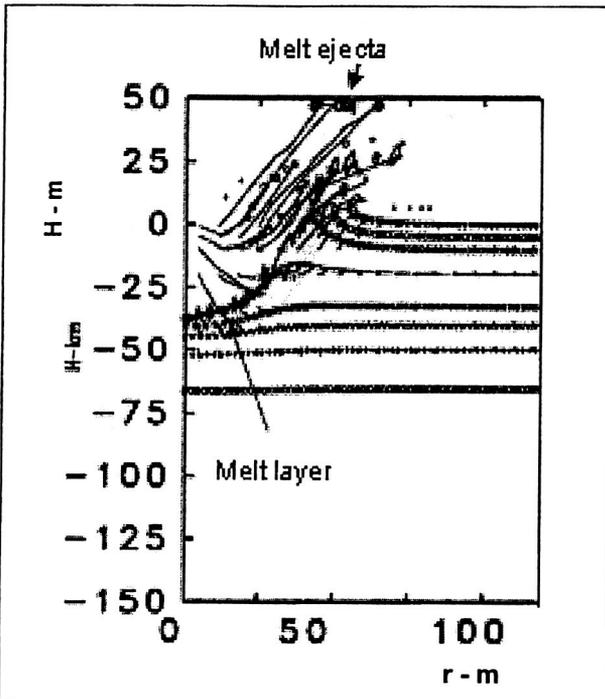
Fig. 1. **Strength dominated crater**. Particle motion histories and melted and fractured regions. Time = 0.15 s U=20 km/s, a = 5 m, Yc = 1.0e9, μ = 0.75, μd = 0.1, εf = 0.05., g = 0.0 m/s^2 · Damage colors shown in Fig.3
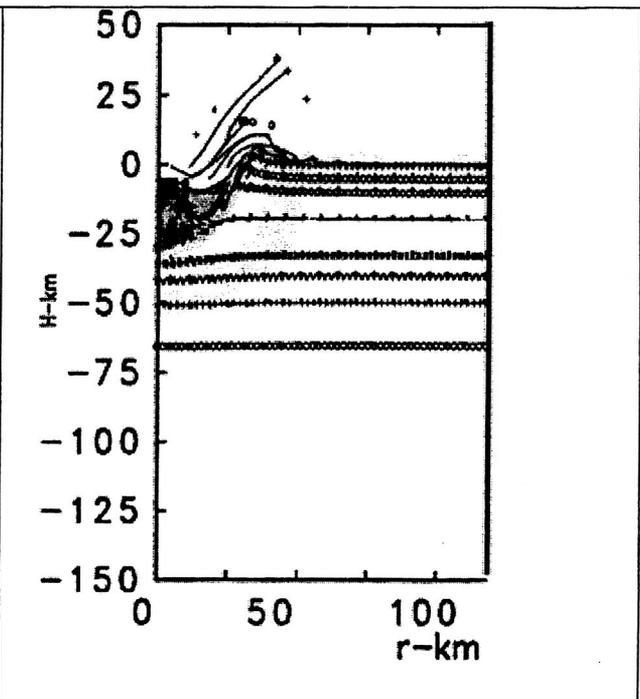
Fig. 2 **Transition crater**. Particle motion histories and - melted and fractured regions. Time = 39 s,. U=20 km/s, a = 5 km, Yc = 0.0, μ = 0.75, μd = 0.1, εf = 0.05, g = 9.8 m/s^2. Damage colors shown in Fig.3
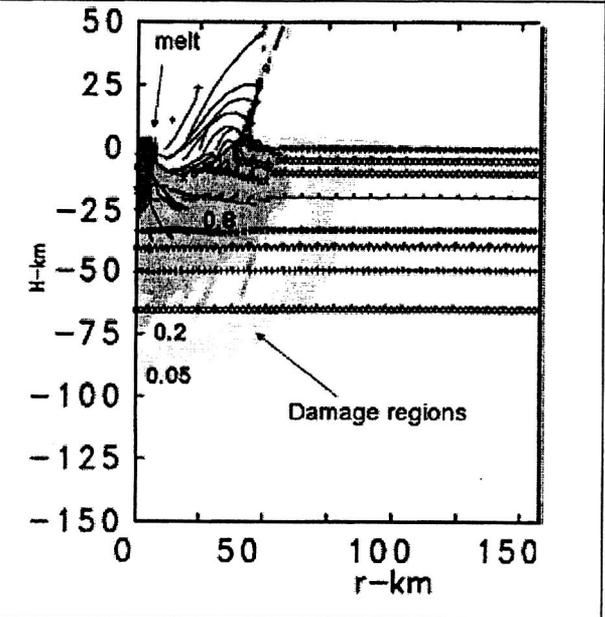
Fig. 3 Complex crater. Chicxulub. Time = 88 s.. U=20 km/s, a = 5.0 km, Yc = 2.4e9, μ = 0.75, μd = 0.1, εf = 0.05. g = 9.8 m/s^2 Note dips in damage region indicating faulting.
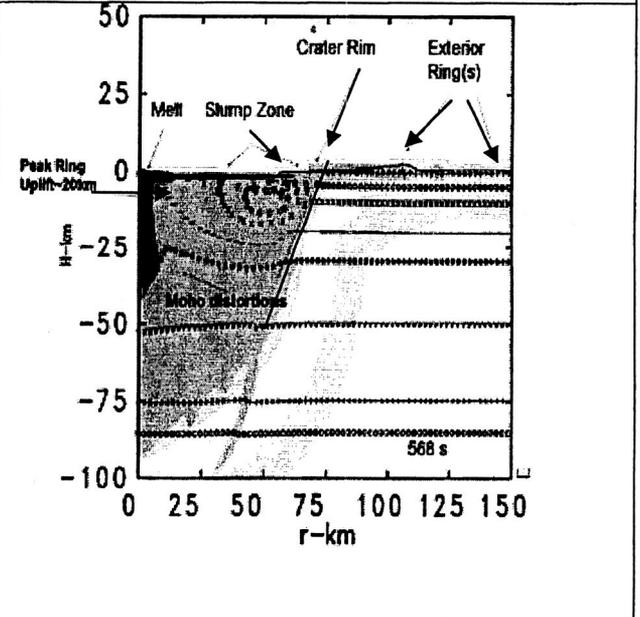
Fig. 4 3 Complex crater. Chicxulub. Time = 568 s. U=20 km/s, a =7. 5 km, Yc = 1.0e9, μ = 0.75, μd = 0.2, εf = 0.1, g = 9.8 m/s^2.Note dips in damage region indicating faulting. Damage colors shown in Fig.3.

Velocity Distributions of Fragments and its Time Dependence

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Introduction: Oblique impact cratering experiments were done, and the fragment size and velocity were measured for fragments larger than 1mm in diameter, and slower than 200m/sec. A high speed CCD video camera was used to see the fragments in flight, and secondary collisions with a window of the target chamber. The purpose of this paper is to provide a database of fragments velocity, which is essential to deeper understanding of the surface evolution of small aster-oids.

Experimental Procedure: A two-stage light-gas gun was employed, and impact velocities are around 4km/sec. A high-speed CCD video camera of 4500frames/sec and 9000frames/sec enabled us to track fragments in flight, and to measure the locations and the times of the secondary collisions. A target box with a slit of15mm width was employed to limit the ejection in the plane including the trajectory of the projectile.

Results: An example of the time dependence of the ejection pattern is shown in figure 1. In this run a target box with a slit was employed. Ejection is divided into 4 stages according to the ejection pattern. The first stage (order of μ sec) corresponds to ejection of very fine and fast fragments like jetting and the earliest conical ejecta cloud, and these particles could not be traced individually. Their typical size is less than 1mm in diameter, and velocity is over 1km/sec. The ejecta in the second stage (0-3msec) consists of 0.1 to 1mm fragments ejected conically at a few hundreds m/s, and at an ejection angle higher than about 60degree from the target surface. The 3D velocity derived from the secondary collisions also shows that the ejection at the second stage is conical. In the third stage (1-10msec), larger spall fragments, about 1cm in diameter, ejected in a cone narrower than that of the second stages. And a cluster of small and slow fragments (0.1-5mm in diameter and a few m/sec) ejected nearly perpendicular to the target surface characterizes the last stage (3msec-). 3/4 fragments are ejected normal to the target surface slower than 6m/s at this stage.

To discuss the size-velocity correla-tions, three results from the experiments of 7mm nylon sphere on gypsum target at about 4km/sec at 0degree, are shown in Fig. 2. The line in Fig. 2 shows the mass-velocity relation fit for the fragments ejected earlier than 5msec. The velocity of Fragments in this stage can be expressed as follows.

$$v_{spall} = 6 \times m_{spall}^{-0.16}$$

It should also be noted that up to 90% of perticles in number is slow (0.1 - 10m/sec), and small (less than 2mg) fragments.
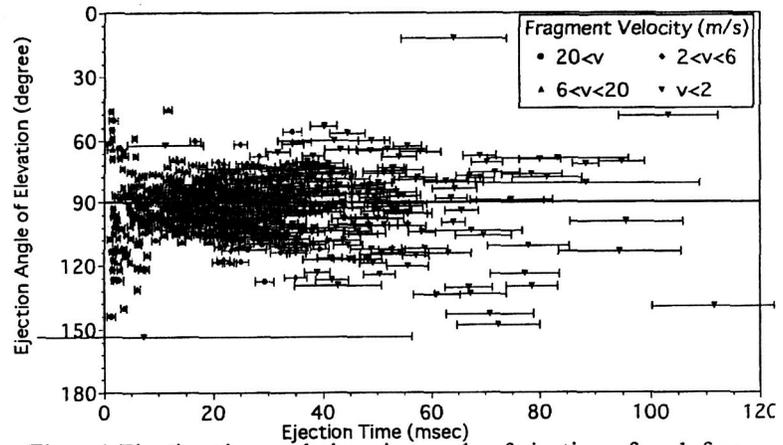
Figure1 Ejection time and elevation angle of ejection of each fragment in the impact at 0degree.
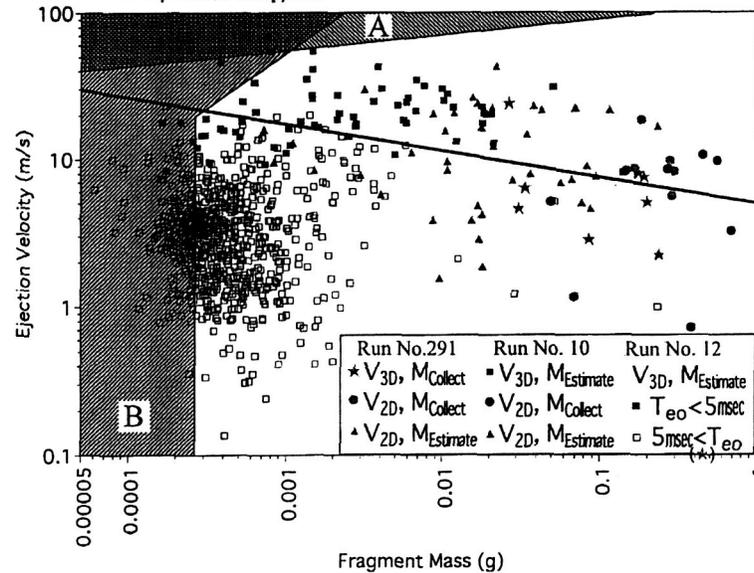
Figure 2 Fragment radius and ejection velocity of 0degree Impact: Area A and B represent the region which is difficult to measure because of too fast and too small, respectively.

Velocity Distributions of Fragments in Oblique Impact Cratering on Gypsum

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Introduction: In order to understand the behavior of the impact-induced fragments on the small asteroid, oblique impact cratering experiments were produced using gypsum targets, which were used as one of porous and low density materials. The fragment size and velocity were measured for fragments larger than 1mm in diameter, and slower than 200m/sec. A high speed CCD video camera was used to see the frag-ments in flight, and secondary collisions with a win-dow of the target chamber were also employed to measure fragment velocity. Especially, we focused to measure the behaviors of very low velocity fragments, which have special meaning for the ejecta on very small asteroids.

Experimental Procedure: We used almost the same experimental procedure as our other paper pre-sented in this meeting, Velocity Distributions of Frag-ments and its Time Dependence. Since in this series of oblique impact, we shot the target surface inclined downward, the extremely slow fragments could come out from the crater cavity.

Results: In the paper cited above, it is shown that the impact ejection is divided into 4 stages according to the ejection pattern. In the second stage (0-3msec), the elevation angle of ejection decreases slightly, and the data are more scattered compared with the case of verti-cal impact, in the impact at 45degree. In the impact at 70 degree, the secondary collision on the window only was identi-fied in the down range direction, and that was also consistent with the result of the run using witness papers.

Figure 1 indicates the ejected time and the eleva-tion angle of ejection of the each tracked fragments also mentioned in the other paper for the vertical impact one. In the impact at 0degree, and 45degree, a target box with a slit was installed to get the 3D velocity of the fragments, and there is few fragments were ejected target surface normal in the second stage. The large number of small and slow fragments ejected later, consists the last stage (3msec-). The average direction of the flow com-posed by a cluster of small and slow fragments slightly deviate from the surface normal in the oblique impact.
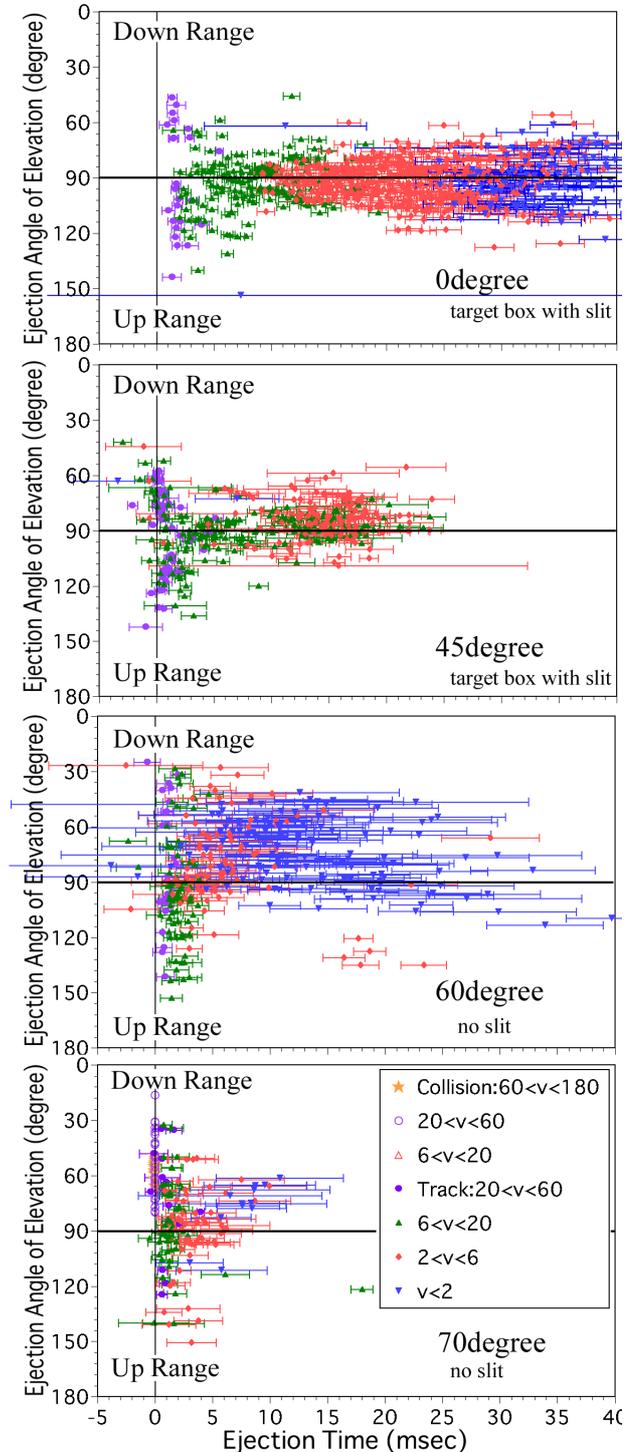
Figure 1 Ejected time and the elevation angle of ejec-tion in the oblique impacts: the later half of the film in the impact at 70degree was lost by an accident.

NEXT STEP IN MARINE IMPACT STUDIES: COMBINING GEOLOGICAL DATA WITH NUMERICAL SIMULATIONS FOR APPLICATIONS IN PLANETARY RESEARCH. J.Ormö[1] [1]Centro de Astrobiologia (CAB), Instituto Nacional de Técnica Aeroespacial, Ctra de Torrejón a Ajalvir, km 4, 28850 Torrejón de Ardoz, Madrid, Spain. (ormo@inta.es)

Introduction: Baltoscandia is favourable for geological studies of marine-target (M-T) craters. One reason is the relatively dense population of craters of different diameters, of approximately the same age, and with different target water depths. This allows comparative studies of the effects of a target water layer on the lithologies and morphologies of the resulting craters [1]. Baltoscandian craters like Kärdla [2] and Lockne [3] are well documented. Today, a considerable number of the documented craters and impact sites on Earth are known to have formed at sea. All but one, the Eltanin impact site west of Chile, have formed in epicontinental seas. This circumstance is mainly a result of higher probability of both formation and preservation in such areas [1]. Famous craters as Chicxulub, Chesapeake Bay, and Mjölnir were also formed at sea [e.g. 4, 5, 6]. Marine impact cratering is an important topic within impact research. The fact that our planet is mostly covered by water must be taken into consideration when evaluating consequences and hazards from impact events. In addition, M-T craters may have applications in the exploration of our Solar System.

Definition: An M-T crater forms from an impact into a target with an upper layer of water. In its transient stage, an M-T crater consists of a water cavity and, in some cases, a seafloor crater. Only the latter may be preserved. How much of the crater that develops in the seafloor depends on the amount of expended energy in relation to the depth of the sea. This relation has been analysed both experimentally [7] and numerically [8]. Studies by Ormö and Lindström [1] show a strong link between the water depth and the geology of the seafloor crater. At relatively shallow water depth the crater resembles a "land-target" crater, although sometimes with stronger collapse of the rim. At deeper water the crater is concentric with a deep crater in the basement surrounded by an outer crater, apparently formed by a shallow excavation flow in connection with the development of a wide water cavity [1, 8, 9]. The outer crater may in these cases be cut by gullies eroded by the resurge of debris-loaded water.

The potential of numerical simulation: Geological studies of the Lockne crater have improved our understanding of water related features to such an extent that they can be used as constrains not only for a rough simulation of the impact, but for modeling specific parameters. The codes have likewise developed so that they now better can simulate the complex process of an impact into a layered target. This development led to an attempt to make a detailed numerical modeling of the 455 Ma Lockne crater [9]. The aim was primarily to find the target water depth, which was an

unknown variable, but also to better understand the processes behind some of the special features of the crater (e.g. the development of a wide overturned flap). The model also gave the opportunity to test the code on a full-scale impact in a layered target. Main geological constraints in the Lockne modeling were (1) the occurrence of a 7.5 km wide inner crater in the crystalline basement with a slightly elevated rim, (2) a shallow outer crater with no obvious rim, (3) an about 3 km wide, overturned flap of basement rock outside the basement crater rim, (4) strong stripping of an initially 80 m thick sedimentary cover prior to the deposition of the flap, and (5) evidence for a forceful resurge. The simulations were done at various water depths of the likely depth interval (200-1000 m). Impactor size, mass, and velocity were also varied. It was concluded that for a 400 m radius asteroid striking at 20 km/s, the target water depth was slightly less than 1000 m. The study is continued with more sophisticated software (3D) to analyse the effects of impact angle and ejecta/water interactions [10].

Perspectives: Knowledge of M-T craters can be used when analysing planetary paleoenvironments and surface properties where remote sensing may provide the only information. Ormö and Muinonen [11] propose that Martian M-T craters could reveal paleo-water depths and, hence, the climatic evolution of the planet. Any low-strength material in the upper part of a layered target may respond as a water layer. Craters from impacts into hydrocarbon and nitrogen seas have indeed been suggested to exist on Titan [12]. Cassini radar data may reveal their features. Future studies of M-T craters should focus on the mechanics of the concentricity, and the influence of obliquity on the ejecta distribution, resurge flow, and how they affect tsunami formation. This is currently pursued by the new impact research group at CAB by combining experiments, fieldwork, planetary research, and numerical modeling.

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COMPLEX CRATER FORMATION AND COLLAPSE: OBSERVATIONS AT THE HAUGHTON IMPACT STRUCTURE, ARCTIC CANADA. G. R. Osinski, J. G. Spray, Planetary and Space Science Centre, University of New Brunswick, 2 Bailey Drive, Fredericton, NB E3B 5A3, Canada. (osinski@lycos.com).

Introduction: It is generally believed that the processes involved in the formation of an initial transient crater and its subsequent excavation, are common for all craters, regardless of their size. A critical assumption is that the depth/diameter ratio of a transient crater remains constant for any given crater size [1,2]. The morphological diversity of impact structures is, therefore, attributed to the modification or collapse of an initial simple hemispherical transient crater [e.g., 2]. The mechanisms of impact crater collapse remain one of the least understood stages in the impact cratering process. Indeed, standard strength models used in conventional hydrocode modeling techniques are not successful in describing crater collapse [2]. Numerical models have also rarely been constrained by field data from terrestrial impact structures. This is, however, a catch-22 situation because very few detailed field investigations of the tectonics of complex impact structures have been made.

Here, we present new constraints on the formation of complex impact craters based on detailed field studies of the Haughton impact structure, Arctic Canada.

Geological setting: The 23 Ma, 24 km diameter Haughton impact structure has been the focus of detailed field investigations over the course of 4 field seasons (1999-2002) as part of the PhD thesis of GRO. Haughton is superbly exposed due to the prevailing polar desert environment. The target rocks consist of 1880 m of almost flat lying sedimentary rocks overlying Precambrian metamorphic basement. Key stratigraphic horizons provide evidence for the depth of excavation and amount of structural uplift and deformation.

Reconstruction of the transient crater: Questions remain as to the exact size of the transient crater at Haughton. Seismic reflection data suggest a diameter of ~12 km [3]. The presence of basement gneisses in the crater-fill melt rocks indicates a depth of excavation (H_{exc}) between 1880 m and ~2200 m. It is generally considered that the depth of the transient crater (H_{tc}) is ~2-3 times greater than H_{exc} [4]. This would yield a H_{tc} of ~4-6 km for Haughton. However, this is incompatible with our field studies and previous seismic investigations [3] that do not indicate significant deformation and displacement of the Precambrian basement (depth to upper surface: 1880 m).

Modification of the transient crater: Our work has revealed that the tectonic modification of the early-formed Haughton crater involved the complex interaction of a series of interconnected concentric and radial faults.

Radial faults. Radial faults record predominantly oblique strike-slip movements. There is generally little (<10 m) or no displacement of marker beds across radial faults. This is despite the fact that substantial volumes of fault breccia (>8 m) are typically present. Importantly, these radially orientated faults are cut and offset by later concentric faults.

Concentric faults. It is noticeable that the intensity and style of concentric faulting changes around the periphery of the crater. They are predominantly listric extensional faults with rotation of beds in the hanging-wall up to ~75°. The outermost concentric faults generally dip in towards the centre of the crater. We suggest that these faults were initiated during the inward collapse of the crater walls. The innermost faults, however, tend to dip away from the crater centre and may represent the outward collapse of the central uplift. The outermost concentric faults typically display two episodes of deformation: (1) early major dip-slip extensional movement; (2) later minor oblique strike-slip movement resulting in the offset of radial faults. A zone of (sub-)vertical faults and bedding occurs along the edge of the central uplift (~6 km radius). This suggests complex interactions between the outward collapsing central uplift material and the inward collapsing crater walls.

Comparison with models: It appears that the transient crater at Haughton was significantly shallower than current models for the cratering process predict. This may suggest a decrease in the depth/diameter ratio of transient craters with increasing crater size. This will have important implications for estimating the size of deeply eroded large impact craters (e.g., Vredefort).

Field studies at Haughton indicate that deformation during the modification stage of complex impact crater formation was brittle and localized along discrete fault planes. We find no evidence to support the hypothesis of 'acoustic fluidization' throughout the whole crater. The presence of little offset along radial faults, despite the large thicknesses of fault breccia, may suggest limited block oscillation along discrete fault surfaces as proposed by Ivanov et al. [5]. However, the scale seen in the field at Haughton is greater than in the models [5].

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IMPACT MELTING IN SEDIMENTARY TARGET ROCKS? G. R. Osinski[1], J. G. Spray [1] and R. A. F. Grieve[2], [1]Planetary and Space Science Centre, University of New Brunswick, 2 Bailey Drive, Fredericton, NB E3B 5A3, Canada. [2]Earth Sciences Sector, Natural Resources Canada, Ottawa, ON K1A OE8, Canada (osinski@lycos.com).

Introduction: Sedimentary rocks are present in the target sequence of ~70% of the world's known impact structures [1]. One of the outstanding questions in impact cratering studies is: do sedimentary rocks undergo impact melting? This question cannot be addressed through experimentation in the laboratory, which is limited to impact velocities generally below that required for wholesale melting [2]. Numerical and computer-based modeling may offer some important information, however, as Pierazzo et al. [3] note, "there is no good model for melt production from impact craters in sedimentary targets". Studies of naturally shocked rocks, therefore, offer the only true ground-truth data on the response of sedimentary rocks to impact. We have carried out detailed field and analytical studies of naturally shocked sedimentary rocks that will hopefully provide constraints for future modeling.

Physics of impact melt generation: Theoretical considerations of the impact process reveal some important results regarding the generation of impact melt [4]: (i) the volume of target material shocked to pressures sufficient for melting *are not* significantly different in sedimentary or crystalline rocks; (ii) Hugoniot curves indicate that *more* melt should be produced upon impact into sedimentary targets as compared to crystalline targets. Impacts into sedimentary targets should, therefore, produce as much, or even greater volumes of, melt as do impacts into crystalline targets [4].

Where have all the melts gone? It is generally considered that the high volatile content of sedimentary rocks results in the "unusually wide dispersion" of impact melt [4]. However, it is becoming increasingly clear that such lithologies can undergo shock-melting and are preserved in significant quantities in some impact craters.

Haughton impact structure: The target rocks at the 24 km diameter, 23 Ma Haughton structure comprised a ~1750 m thick series of sedimentary rocks (predominantly carbonates, with minor evaporites, sandstones and shales), overlying Precambrian metamorphic basement. Osinski and Spray [5] have recently interpreted the crater-fill deposits at the Haughton impact structure as carbonatitic impact melt rocks. Importantly, the volume of these crater-fill deposits (>12 km^3) is roughly equal to the observed impact melt volumes for comparably sized craters developed in crystalline targets (e.g., >11 km^3 melt at Bolytsh (diameter 24 km) [6]).

Ries impact structure: The 24 km diameter, 15 Ma Ries impact structure comprised a target sequence of ~ 850 m sedimentary rocks (limestone in upper parts, predominantly sandstones in lower parts), overlying Hercynian granites and gneisses. Carbonate melts have been documented at the Ries impact structure by Graup [7] and Osinski [8]. In addition, Osinski [8] has also recognized the presence of SiO$_2$-rich impact glasses that were clearly derived from sandstones in the lowermost part of the sedimentary sequence.

Implications: Based on our studies of the Haughton and Ries structures, we suggest that sedimentary rocks can undergo shock-melting during impact events. Thus, it should NOT be assumed that all sedimentary rocks and minerals completely degas and disperse at pressures sufficient for melting. This will have implications for the way in which we model the cratering process.

Modeling: The Ries impact event has recently been the focus of numerical modeling studies and 3D hydro-code simulations [9]. These models suggest substantial melt generation from sandstones in the sedimentary sequence, seemingly at odds to the general held view that these lithologies were not shock-melted [e.g., 10]. Recent studies by Osinski [8] have shown that sandstone-derived melts are present. This is an instance where modeling and field studies clearly agree. This is not the case when carbonates are considered. All models to date have considered that carbonates are completely degassed above a certain pressure threshold (e.g., >55 GPa in [9]). This is despite the fact that carbonate melts are known to occur in the Ries and other structures. We suggest that the melting of carbonates should be included in any future modeling studies.

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APPLICATION OF GRAVITY DATA TO UNDERSTANDING IMPACT MECHANICS. J. B. Plescia, U. S. Geological Survey, 2255 N. Gemini Drive, Flagstaff AZ 86001, jplescia@usgs.gov..

Introduction: Gravity data provide important constraints on morphometry of impact structures and on the crustal response to the impact process [1-3]. Such data can provide insight that may not be obtainable from surface geologic mapping and may not be quickly or cheaply obtained by other geophysical means. The gravity data can be used to constrain the dimensions of a completely to partly buried structure (e.g., diameter, central uplift, etc.) and can provide information on the subsurface character of both exposed and buried structures. Gravity data can also be used to reject some structures as being of impact origin.

Morphometry: The most direct use of gravity data is establish morphometric properties of partly to completely buried structures. Gravity data have been used at several structures in Australia to establish the nature of these impacts. Mulkarra was proposed [4] to be a 9 km diameter simple crater in a sedimentary section. Gravity data [5], however, reveal positive and negative anomalies that indicate the structure is actually an 18-20 km complex structure with an 8 km central peak or peak ring. At Kelly West [6], gravity data have been used to study the central uplift area. Those data (a low surrounded by a high associated with the central uplift) suggest the central uplift is a small central peak-ring filled with breccia rather than a solid central peak. At the Manson impact [7] gravity data show that the central uplift is probably an incipient peak ring and that the zone of low density material (breccia) extends to a depth of 3 km.

Deep Crustal Effects: Gravity data can be used to provide constraints on the depth of crustal deformation. Impacts produce shock effects which reduce the effective density of rocks at depths greater than the transient cavity filled with the breccia lens. At Meteor Crater the breccia lens is 220 m thick, yet the zone of low density persists to a depth of 800 m [8]. Shock waves from the impact event had sufficient energy to significantly fracture the basement for distances of 500-600 m below the crater floor, thus providing a constraint on the energy decay rate. The breccia and the shattered basement contribute to the total 0.6 mGal anomaly [9].

Upheaval Dome is a deeply eroded complex crater in Utah [10], although apparently not everyone agrees with this interpretation [11]. Detailed geologic mapping show that the normal faults that are exposed around the margin of the structure and which cut the Navajo, Kayenta and Wingate units flatten at depth. From the attitudes of the exposed faults, the faults probably flatten into a decollement within the deeper Cutler Group. Such a geometry would imply that the deformation was restricted to levels above the Culter. Gravity data collected over the structure show that there is no gravity anomaly. The absence of an anomaly is explained in that at the current structural level deformation is entirely associated with slip along faults translating different sandstone blocks. Simple translation does not produce a density contrast. Erosion is at such a level that the breccia lens has been removed. These data indicate the shock did not have substantial influence below the level of the decollement.

The gravity data for an impact structure can also be used to model the nature of the central uplift. The Connolly structure in Australia [12] is a 9 km diameter complex crater. Gravity data reveal the presence of a high over the central uplift surrounded by an annular lower amplitude high over the crater interior. The central gravity high is due to uplift of deeper sandstones from a depth of ~1 km. These sandstone are of higher density than the surrounding rock and have shed relatively high density material into the crater interior causing the annular high.

Summary: These examples serve to illustrate that gravity can provide information on the deep structure of impacts. Such data place constraints on the cratering process by providing insight into how the crust responds to the impact: how deep the effects of the shock extend, how much structural uplift occurs, the shape of the central uplift with depth, etc.

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IMPORTANCE OF TARGET PROPERTIES ON PLANETARY IMPACT CRATERS, BOTH SIMPLE AND COMPLEX.

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Introduction: For 20 years, the issue of whether surface gravity or target properties control the shape of planetary craters has continued unabated. Periodic revisions to and questions about quality control of the planetary crater database have vexed the debate. Here I review the current status of the observations and our understanding of the results. The observational data fall into two related categories: crater depths, and morphologic transitions from one landform to another. As it turns out there is more than one way to measure these transitions. It would appear that both target gravity and properties are important.

Silicate Planets: Pike [1] made one of the first attempts to compare crater morphology on the silicate terrestrial planets, using data from the Moon, Mars and Mercury. The effort to sort out the relative importance of surface gravity and target properties (i.e., crustal strength) is complicated by the small number of such bodies for which we have data (5) and the influence of other forces. Three of these bodies (Earth, Venus, and Mars) have substantial atmospheres, which may couple to the ejecta curtain and alter landforms [2]. Earth and Mars have been subject to substantial surface erosion and modification, and crater data for Earth, which together with Venus represent the high-gravity end of the spectrum, is wholly unreliable. Magellan stereo allows depth measurements to be made [3] but the dense atmosphere prevents the formation of simple craters (by assuming lunar-like simple crater morphology, an estimate of transition diameters can be made).

Although there is clearly a general inverse trend of transition diameters with gravity from the Moon to the other higher-gravity bodies, the result of these competing forces is something akin to confusion. There appear to be major differences in morphology on Mercury and Mars, where surface gravity is otherwise similar. Pike [1] reports significant differences in the depths and transition diameters of craters on the lunar mare and on the highlands. This points to an important role for material properties, with the regolith rich highlands have a different strength than the less heavily cratered basaltic mare. Additional evidence for or against the influence of layering or rock type will be reviewed, including the latest MGS results.

Icy Satellites: The icy satellites of the outer planets are a different ball of ice. There are at least a dozen such moons for which we have data and which have complex craters. They are also of sufficiently different size that a large gravity range can be examined. Chapman and McKinnon [4] and Schenk [5] made the first satellites comparisons, suggesting that in fact there was a strong dependence of complex crater depths and transition diameters on surface gravity, but also, that

these were significantly smaller than would be expected from comparison with silicate-rich planets. These observations were based on Voyager data, but subsequent Galileo data has shown that the Ganymede data was partially compromised by resolution insufficient to resolve simple craters. Callisto and Europa have also been added. The updated transitions and depths [6] clearly show that the icy satellites all fall on a g^{-1} trend. The only exceptions are Enceladus and Mimas. Enceladus craters are very irregular even by icy satellite standards and it is likely that these craters have been modified, possibly by volcanism [7]. Mimas remains to be explained, but unusually low internal porosity conditions may or may not be involved.

The unusual complex crater landforms on the larger icy satellites, especially Europa, may point to the importance of thin lithospheres and possibly liquid layers at shallow depths [6,8]. These morphologies and their dimensions provide key constraints that can be used to model icy satellite interiors [9].

Future Shock: On silicate bodies, additional data at the low end of the gravity spectrum is needed. All asteroids observed to date are too small to allow complex crater formation. The Dawn mission to Vesta and Ceres will be important for adding rocky bodies of low to moderate gravity to the data set, and indeed I will venture a prediction as to transition diameters on these bodies. Until then, the case of the silicate planets remains uncertain. For the icy satellites, a better understanding of the internal structure of Mimas is required. We might see something unexpected on two-faced Iapetus. There is also some scatter in the small saturnian satellite data which could use clearing up. Mapping of crater morphology on Titan, similar in size to Ganymede and Callisto, will be useful for comparison, although the atmosphere there may cloud the issue. Cassini beginning in 2004 should address these needs. It is curious that we do not see substantial differences between those satellites believed to be mostly water ice, and those with more exotic (and lower strength) ices such as ammonia, carbon dioxide and nitrogen (e.g., Ariel, Miranda and Triton). Pluto and other Kuiper Belt objects may be much richer in these ices and could behave differently. We have only a decade to wait (hopefully)!

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IMPACT CRATER MORPHOLOGY AS A GUIDE TO REGOLITH STRUCTURE AT TAURUS-LITTROW.

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Introduction: Mapping of variables in primary crater morphology relative to crater size can be used as an initial guide to factors that will affect mining and processing of that material for lunar resources such as helium-3, hydrogen, oxygen and water. Although time did not permit the systematic mapping of craters during the Apollo 17 exploration of the Valley of Taurus Littrow, the writer was able to provide descriptions of the variety of crater morphologies present (1).

About 3.5 b.y. ago (2), the Valley of Taurus-Littrow and its surroundings had been blanketed with a dark, pyroclastic mantle (3,4). Orange and black varieties of this mantle were specifically sampled at Station 4, Shorty Crater (5) as well as being a significant component of most samples of the regolith (4). All of the craters investigated, observed, and described are younger than the period of pyroclastic mantling. Every later impact, however, re-mobilized the fine pyroclastic material as well as the developing regolith, partially mantling all nearby younger materials.

Crater Age: The primary process that visibly ages impact craters on the Moon is the impact of small and micro-meteors over time (6) and the associated deposition of nanophase iron on all particle surfaces (7). Micro-meteor impacts generally keep the surfaces of boulders clear of this debris.

Small-scale impact processing of the upper few centimeters of the lunar surface gradually degrades and/or buries the primary features of larger impact craters and their ejecta. Crater age Category One (C1) are ubiquitous in Taurus-Littrow [<1 m.y.?]. They consist of the youngest and statistically the smallest craters and are characterized by bright halos and irregular but coherent pools of impact glass on their floors and regolith breccia fragments scattered on their walls, rims and ejecta blankets. Category Two (C2) craters include several observed on the traverse from Challenger to Station 2 and Van Serg Crater at Station 9 [1.5-3.7 m.y. (8,9)]. Relative to C1 craters, the bright halo has faded in C2 craters. Category Three (C3) craters, such as Ballet Crater [2-5 m.y. (8,10)], the coherent masses of impact glass have disappeared but fragments of regolith breccia have been retained. Category Four (C4) craters, including Shorty Crater at Station 4 [10-19 m.y. (4,11)], are marked by the full degradation of visible regolith breccia fragments. If a C4 crater is large enough to have penetrated to bedrock, it will have visible bedrock fragments on their floors and in their walls and ejecta blankets.

Additional age categories can be defined for craters large enough to expose bedrock in their floors and/or have bedrock as part of their ejecta blankets. Category Five (C5) craters have no visible bedrock on their floors even though bedrock fragments are exposed in the walls and in their ejecta blankets. Examples of C5 craters are Camelot Crater at Station 5 [70-95 m.y. (4)], Emory Crater at Station 1 [~100 m.y. (12)]. Category Six (C6) craters, such as Horatio Crater, have bedrock fragments exposed only in their walls.

Regolith Depth: Fresh craters that penetrate the regolith have fragments of the underlying bedrock on their rims as well as exposing that bedrock on their floors. They can be used to map variations in the depth of the regolith.

Regolith Layering: Craters with continuous interior benches in their walls give an indication of a significant discontinuity in the physical properties of the regolith with depth. Generally, as apparently is the case with Van Serg Crater, a bench indicates a sharp increase in compaction or strength with depth. An extreme version of a bench crater, given the field name of "pit bottomed crater," may indicate a sharp decrease in compaction or strength with depth. Pit bottomed craters were only observed on the light mantle and may indicate better compaction near the top of the light mantle than lower down as might be expected in a fluidized avalanche deposit (5).

Buried Boulder Concentrations: Craters of insufficient size to penetrate the regolith to bedrock, but which have boulders in their ejecta blankets are indicative of a concentration of buried boulders, presumably ejecta from a larger crater. Radar scans, including look-ahead radar from a mining-processing machine, might be employed to fully map a buried boulder field.

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ATMOSPHERIC EFFECTS AND OBLIQUE IMPACTS: COMPARING LABORATORY EXPERIMENTS WITH PLANETARY OBSERVATIONS. Peter H. Schultz, Brown University, Department of Geological Sciences, P. O. Box 1846, Providence, RI 02912, peter_schultz@brown.edu

Introduction: Without direct observations of a major impact, one of the few ways to study the impact process is by assessing the effects of its environment (gravity, atmosphere) or conditions of impact (e.g., impact angle). The purpose of this contribution is to review selected consequences of both the atmosphere and impact angle as witnessed in laboratory experiments or revealed by large-scale craters preserved on different planets.

Atmospheric Effects: The lunar impact cratering record is an invaluable template for interpreting the pristine cratering record on other planets. In addition to its lower gravity, the absence of an atmosphere simplifies the cratering process. While it is often assumed that the tenuous atmosphere of Mars is overwhelmed by both the initial blast and the later advancing ejecta curtain, this assumption can be shown to be unwarranted. The atmosphere does play a significant role in modifying the late-stage ejecta emplacement but this role changes as a function of target, scale, and atmospheric pressure/density. The challenge is to identify meaningful tests to isolate this effect from other processes whether through statistical studies of the planetary cratering record or by case studies.

Laboratory impact experiments provide fundamental clues for assessing atmospheric effects since the process is complex and evolving. Such experiments are not just one-to-one comparisons between results in the laboratory and examples on the planets. Rather they should be designed to isolate variables in order to enable appropriate extrapolations. For example, performing an impact experiment at 100 bars to reproduce conditions on Venus or 6mbars to simulate conditions on Mars would only produce a crater of that particular size, in that specific target. Such laboratory observations combined with theory have yielded important predictions that can be tested by the planetary impact record. Applications to Mars and Venus illustrate this strategy which elevate the discussion beyond "look-alike" comparisons.

The distinctive ejecta facies surrounding craters on Mars have generated a range of interpretations. The fluidized appearance has commonly been used to interpret the presence of buried water (1, 2). Although popular ("follow the water" theme), this could be the planetary equivalent of a mirage. It is valid to assume explicitly that fluidized ejecta represents the presence of water and then explore the implications of this extrapolation; it is not valid, however, to simply state that fluidized ejecta deposits provide evidence for water. The problem is more ambiguous....and much more interesting.

Extensive laboratory impact experiments demonstrated that the response of the atmosphere to the crater formation is as important as the effect of the atmosphere on the ejecta. Early studies noted that the atmospheric drag acting on individual ejecta should be profound, even on Mars (3). For a given crater size (hence ejection velocity at the same stage of crater growth), atmospheric drag arrests the ballistic range over a relatively narrow size range of the ejecta (factor of 10) when scaled to the ambient atmospheric density. Con-

versely, for a given atmospheric density and ejecta size, the effect of drag increases with increasing crater size. If blindly applied, such considerations predict that ejecta would never get out of the crater for very fine-grained ejecta (25 microns in laboratory experiments and centimeter sizes for 10 km-diameter crater on Mars). But both experiments and the existence of excavated craters on Mars (not to mention Venus) demonstrate that craters do form. The paradox was resolved by recognizing that kinematic flow created by the outward moving ejecta curtain set up intense vortices that entrain sufficiently small decelerated ejecta (4, 5). Moreover, the presence of even a small fraction (10% by weight) of such a fine-grained component can change ballistically ejected material into a vortex with tornadic velocities. Then by isolating the controlling variables, later studies were able to compare models of the kinematic flow field with simplified experiments using controlled conditions in a wind tunnel (6, 7).

Such comparisons between models and observations both in the laboratory and on planetary surfaces led to specific predictions for ejecta deposits on Mars (4, 5, 8). First, onset for fluidized ejecta should depend on crater size due to the combination of increased ejection velocities and decreased ejecta sizes (comminution). Second, run-out distances scaled to crater radius should be proportional to crater size on Mars due to increasing ejecta entrainment (but decrease on Venus). Third, increased run-out distances with increasing latitude reflect an increased fraction of fine-grained sediments. Fourth, rampart-terminated ejecta facies represent coarser grained fractions that were mobilized but not fully entrained; hence, "rampart craters" should characterize the mare-like ridged plains rather than water-filled substrates. Fifth, radial facies indicate enhanced explosive expansion and hence the most (rather than the least) volatile-rich targets (or have been extensively modified). Sixth, anomalously long ejecta run-out distances can be created by autosuspension that feeds the vortex or flow with energy or gas (e.g., near-surface volatiles entrained by basal ejecta flow). Ninth, the development of late-stage ejecta-entrained vortices will not be significantly affected by the surrounding disturbed atmosphere (heated) since such blast effects rapidly equilibrate in the tenuous Martian atmosphere and do not drastically affect the results (8).

The above list of predictions and observations challenge some models of ejecta emplacement imposing only water. Nevertheless, the presence of volatiles can be recognized, whether in post-emplacement flow of water-lubricated near-rim ejecta or in enhanced run-out through autosuspension. Ironically, the critical importance of fine-grained lithologies may reflect enhanced weathering conditions (including fluvially transported sediments) during the Noachian and Hesperian and the role of climate-controlled processes (e.g., polar sinks for dust, obliquity changes, and polar wandering). Such considerations will not resolve the debate about Martian cratering. It simply challenges interpretations and assumptions to look further than the translating the term "fluid-

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ized" into "water-entrained".

Oblique Impacts: Until relatively recently, full three-dimensional models of hypervelocity impacts have not been possible. As a result, important clues about the impact process have been gleaned from laboratory experiments compared with the planetary cratering record. Advances in computing power now has not only allowed more widespread use of 3-D codes (e.g., 9) but also enabled new diagnostics in the laboratory. These parallel advances will permit unprecedented opportunities to validate the codes and to test extrapolations to large scales, whether directly from laboratory experiments or comparisons with the codes. The oblique impact process represents one of the most challenging of these tests.

Oblique impacts map time into space. During vertical impacts, rapid changes in the transfer of energy and momentum from impactor to target are generally lost or overprinted by each successive stage of formation. Oblique impacts, however, expose this transfer along the initial trajectory. Laboratory experiments have long documented the overall change in crater dimensions and ejecta distributions (10), but new studies are providing other possible strategies for identifying the initial trajectory. First, direct measurements of far-field pressures reveal that oblique impacts cannot be simply modeled using point-source assumption (11). These measurements are clearly captured in asymmetries, timing, and nature of failure in three dimensions. Such laboratory measurements are also captured in recent computational models (9). Second, three dimensional particle image velocimetry (3D-PIV) is capturing the evolving flow field expressed by ejecta leaving the crater (12, 13). The enigmatic oblong crater shape perpendicular to the trajectory for modestly oblique impacts is now recognized in the ejecta flow field in addition to failure patterns in strength-controlled craters. Third, high-speed imaging and novel experimental designs are capturing the contact and failure pattern of the projectile.

Applying such laboratory experiments to planetary-scale phenomena and processes cannot be made without analytical or computational modeling. For example, the crater/projectile dimension ratio for cratering in sand for hypervelocity experiments is 50:1. But this ratio for large-scale (100 km) craters approach 15:1. Because oblique impacts reduce the peak pressure in the target, this ratio decreases still further to 8:1. Consequently, large-scale cratering more closely resembles strength-controlled laboratory impacts in terms of the relative dimensions of the crater and impactor. This also means that the transition from the region controlled by the transfer of momentum and energy becomes a significant fraction of the crater at large scales. Hence, observational evidence of the trajectory becomes more evident as well.

Observational evidence for impact trajectory (e.g., 15, 16) includes asymmetries in shock effects expressed by erasure/survival of pre-impact structural control, crater shape in plain view (whether oblong perpendicular to or along the trajectory), uprange offset of the central peak, breached central ring downrange, and downrange ricochet effects. Not all craters will exhibit such features. In addition to changes in expression with scale, impactor density and velocity also will play a role. For example, very high-velocity oblique impacts (>40km/s) will increase the crater/projectile ratio

and partition more energy to melting and vaporization. Target topography (relative to the scale of the impactor) also can be shown to radically modify early-stage coupling processes. Consequently, statistical studies of crater morphologies may not reveal the key signatures. Such an approach is similar to including a failed experiment in laboratory impacts.

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EXCAVATION FLOW AND CENTRAL PEAK RINGS: IS THERE A CONNECTION? V. L. Sharpton[1] and B. O. Dressler[2], [1]Geophysical Institute, University of Alaska Fairbanks, 903 Koyukuk Drive, Fairbanks, AK 99775 (buck.sharpton@gi.alaska.edu); 2 Lunar and Planetary Institute, 3600 Bay Area Blvd, Houston, TX 77058.

Introduction: To approximate the conditions associated with the excavation stage of the impact process, many numerical simulations rely on some form of the Z-model [1-5], where the radial velocity of particles below the ground surface is given by:

$$u_R = \alpha(t)/R^Z$$

and R is the radial distance from the flow origin, α is a strength parameter, and Z determines the velocity change with radial distance. While inherited from studies of explosion cratering [1-3], the Z-model has been shown to provide a first order approximation of excavation flow in simple craters as long as some appropriate effective depth of Z-model flow (EDOZ) is provided. EDOZ is usually assumed to be equivalent to one projectile diameter [e.g., 1,2,4]. The most-often applied form of this model is the steady-flow version where α, Z (~3) and EDOZ are assumed to be time constants [e.g. 1,2,5,6]. This practice, however, seems to be based on convenience rather than on sound theoretical grounds as (1) the steady flow assumption allows the flow field to be explicitly evaluated at all times [2] but (2) violates conservation of energy [1]. Furthermore, studies of laboratory-scale impacts [4,5] indicate a time-dependence to the Z-model parameters. Despite these limitations, the Z-model's ability to provide qualitative insights into the dominant spatial features of the early-time impact flow field has been emphasized [1-3]. While this may be true for laboratory scale craters and even simple craters on planetary surfaces, observations from a well-studied terrestrial complex crater indicate that neither excavation flow nor the shape of the excavation cavity are well approximated by the Z-model.

Haughton Crater. The ~24 km diameter Haughton impact crater is located at 75° 22′ N; 89° 41′ W on the western portion of Devon Island in the Canadian Arctic [7,8]. The geological map shown in Fig. 1 is derived from previous studies [9,10] with modifications resulting from our 1997 field expedition. These observations, combined with the results of reflection seismic studies [11] provide useful constraints on the target and how it was affected by the impact event. Here, we use these data to evaluate models of the size and shape of the excavation cavity generated during the formation of Haughton crater and show that these characteristics cannot be reconciled with the constant-flow Z-model. Our analysis suggests that the poorly organized peak ring at this crater reflects radial inflections in the original excavation crater prior to its uplift during late-stage modification.

The target is a nearly flat-lying sequence of Paleo-
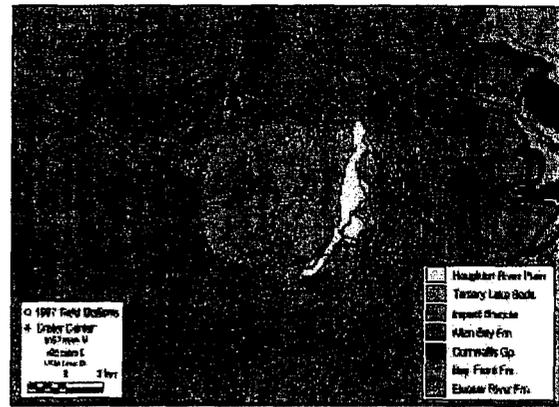
Figure 1

zoic platform rocks, ~1.8 km thick, overlying high-grade crystalline basement. The platform sequence consists of the following units [9]: 1. The Allen Bay Fm. (OSA) limestone and dolomites, ~450 m thick. This unit forms the present surface around the crater and is found to within ~4.5 km of the center. 2. The Cornwallis Group (OCTI) shales and carbonates with a combined thickness of ~110 m. OCTI crops out along the walls of steep valleys to the northeast of the crater. 3. The Bay Fiord Fm. (OCB) carbonates and gypsum, ~330 m thick. Large exposures of OCB occur within 5-7 km of the crater center, as well as in valley floors as close as 8 km east of the crater center. 4. The Eleanor River Fm. (OE) chert-bearing carbonates, ~400 m thick. Inliers of OE, representing the central uplift, occur between 0.7 and 4.8 km from crater center. The closest authochthonous OE outcrops occur ~16.5 km from the crater center. 5. Undifferentiated Lower Ordovician-Cambrian (OCU) shale, sandstone, dolomite, and conglomerates, ~420 m thick. No parautochthonous units of OCU have been discovered within the crater; however, near the center abun-
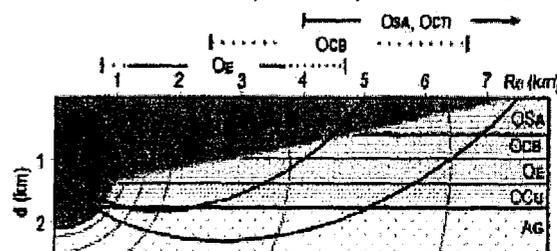
Figure 2. OCTI (not shown) is located between OSA and OCB. Shock pressures after [12].

EXCAVATION FLOW AND CENTRAL PEAK RINGS : V. L. Sharpton and B. O. Dressler

dant highly shocked blocks of sandstone probably represent the OCU Blanley Bay Fm. Authochthonous exposures have been mapped 32 km east of crater center.

Excavation Depth and Central Uplift. Filling the shallow central basin (radius of ~5 km), the allogenic impact breccia forms a nearly continuous unit that ranges from ~10 m to over 100 m in thickness. Breccia outliers also exist beyond this deposit, with the farthest mapped deposit located ~7.8 km southeast of center. The matrix and clasts of this breccia were derived primarily from the platform rocks; however, clasts of partially melted, highly shocked, and weakly shocked clasts of Archaean high-grade metamorphic rocks (AG, Fig. 2) prove that the excavation cavity penetrated into the subjacent crystalline basement. Modal analysis [9] indicates ~10-15% of the breccia clasts are derived from the crystalline basement. Extending ~1 km from the crater center are large and extensively shatter-coned outcrops of OE (with minor OCB; Figs. 1 and 2) that form a discontinuous ring of uplifted but otherwise coherent target rocks. As their structural heights exceed the basal height of the Tertiary lake beds that filled the crater shortly after it formed, these OE exposures represent a true topographic, albeit incipient, peak ring.

Reconstructing the Excavation Crater. The excavated diameter De=2Re has been estimated at 10 km based on the incoherent zone in reflection seismic data [11]. Redeker and Stöffler [10] prefer De=15 km, based on shock isobar constraints from the Kieffer and Simonds [12] model and the need to excavate crystalline rocks. Fig. 2 shows the half-space shape of the Z=2.71 model for both the 10-km *(red line)* and 15-km *(blue line)* excavation craters predicted for Haughton crater.

Discussion. When assessed against the geological constraints provided by outcrops of parautochthonous target rocks, substantial problems with these models become evident: **1.** The Re=5 model predicts excavation completely through OE to a distance of ~3.3 km; Re=7.5 removes OE to a distance of nearly 6 km. Both therefore fail to account for the central uplift (OE derived from beneath the excavation crater) that is observed within 1.2 km of the center. **2.** Similarly, the models predict that OCB would be completely removed within 4 km (Re=5) or 6.8 km (Re=7.5) yet outcrops occur within 3 km of center and are abundant within a radius of 5 km. **3.** The Re=5 model does not account for the proportion of crystalline rock clasts observed in the allogenic breccia [10].

Conclusions. The geological constraints at Haughton crater are not compatible with a constant Z excavation flow field regardless of the choice of Re. Observations presented here constrain the zone of deep excavation to be less than 1 km from center. The *yellow line*,

Fig. 2 indicates the maximum depth to the excavation crater boundary permitted by geological constraints. The resulting shape is characterized by a localized near-center zone of deep excavation – from which the crystalline rocks originate – flanked by a broad zone of shallow excavation at least 4-5 times the width of the central zone. Off-axis, deep excavation, and thus a Z-model-type of excavation flow are not incompatible with the Haughton crater observations *if and only if* Z is a strong function of time. High-Z flow (deep, near-center excavation, steep ejection angles) would occur during the earliest excavation stage and as ejection proceeded, Z, excavation depth, and ejection angle would decay.

At Haughton, the uplifted outcrops form the cusp separating two distinct sub-domains in the excavation crater: the broad outer zone of shallow excavation and the narrow, centrally located zone of deep excavation. Consequently this peak ring seems to represent a fundamental structural inflection in the base of the excavation crater that was subsequently uplifted during late-stage modification.

It is not clear whether the excavation-crater model for peak ring formation can be extended to all central peak rings, or even to those in other craters formed in layered targets. Similar excavation geometries, however, have been reported at several other complex craters with central rings [e.g. 13,14] in layered targets where such reconstructions are possible.

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MECHANISMS OF IN SITU ROCK DISPLACEMENT DURING HYPERVELOCITY IMPACT: FIELD AND MICROSCOPIC OBSERVATIONS

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Introduction: The nature of rock deformation due to hypervelocity impact is discussed, especially with regard to the larger terrestrial structures (e.g., Sudbury, Vredefort, Manicouagan). Based on field observations and thin section microscopy, evidence is presented for two end-members of rock response to extreme strain rates: (1) bulk deformation, due to pervasive fracture generation and ensuing micro-displacement with melting; (2) localized large-displacement faulting, accompanied by friction melt generation (pseudotachylytes). There is no evidence for bulk "fluidization" at the thin section scale, except where bulk melting has occurred during impact melt sheet generation, wherein truly fluid (igneous) rocks are formed.

S- and E-type fracture-fault systems: Bulk deformation in footwall rocks beneath the Sudbury Igneous Complex (melt sheet) is limited to a zone some 10-15 km beyond the contact with overlying melt. Fracture-microfault systems are typically a few mm thick and are akin to shock veins in meteorites. These have been referred to as S-type pseudotachylytes [1]. They may contain high-pressure polymorphs. Melting is probably due to a combination of shock and microslip. In this proximal footwall zone at Sudbury, there are 10-20 pervasive S-type veins per cubic meter, with the frequency decreasing progressively away from the melt sheet.

Localized, large-displacement faulting can be related to concentric and radial structures that appear to be formed during the modification stage of the cratering process. These post-date the shock wave and are primarily driven by gravitational forces and possible rebound effects. Movement on the concentric systems commonly occurs after movement on the radials. Movement on the concentric faults is typically significantly greater than that realized on the radial fracture-fault systems. Large displacement, single slip faults have been referred to as superfaults when displacement is >100 m in one event [2]. Under superfaulting conditions, thick (1-1000 m) friction melt (pseudotachylyte) bodies may result. These may be responsible for the rings seen in multiring impact basins on the moon and other planets. The thickest pseudotachylytes are formed when these faults undergo displacements of several kilometers in one slip event. Superfaults generate terraces in the larger impact structures. This class of pseudotachylyte has been referred to as E-type [1]. E-type pseudotachylytes are formed in the same way as endogenic fault-related pseudotachylytes, though displacements due to impact can be many orders of magnitude greater than those realized during regular fault-

ing (the latter typically resulting in cm-wide pseudotachylyte veins).

Central uplifts: While S- and E-type pseudotachylytes have been documented with regard to melt sheet footwall occurrences, there are very few references made to them with regard to the internal structure of central uplifts. Central uplift mechanics remains poorly understood. How is it possible for vast volumes of rock to move, supposedly downwards (during compression) many kilometres, and then back up many kilometres (on decompression), and probably within seconds or minutes? In fact, there is little hard evidence that transient cavities are pushed downwards during compression and excavation (i.e., in a gross plastic/elastic manner). In so, cannot rebound be attributed merely to pressure release at a free surface? The internal structure of central uplifts has not been studied in any real systematic detail in the field. Work on smaller impact structures, such as Decaturville [3] reveals a crude concentric piston-like form, with the deepest level rocks being exposed in the centre of the uplift and successively higher level rocks being exposed around this core. The uplift is thus not chaotic, although each concentric zone appears to comprise blocks of coherent rock in a fragmental matrix (breccia) that has been well mixed. Preliminary work thus indicates that some uplifts are similar to telescopic hydraulic rams in their cylinder-within-cylinder structure. Whether the contacts between "cylinders" are sharp (i.e., fault bounded) or gradual (fluid like), is not yet clear.

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TOWARD A COMPLETE MEASUREMENT OF THE THERMODYNAMIC STATE OF AN IMPACT-INDUCED VAPOR CLOUD.

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Introduction: Vaporization phenomena induced by hypervelocity impacts play an important role in the origin and evolution of Earth and other planets. There have been extensive research efforts made for understanding this process. However, the equation of state (EOS) and chemical reaction within high-pressure and high-temperature conditions of impact vapor are yet highly uncertain [e.g., 1, 2]. This is primarily owing to the lack of experimental data on impact vapor cloud. Here we discuss newly developed spectroscopic methods to determine the thermodynamic state of impact-induced vapor very accurately.

Thermodynamic State of Impact Vapor: Among the four fundamental thermodynamic quantities (temperature T, pressure p, entropy s, and density ρ), two of them are necessary to designate the thermodynamic state of an equilibrium system. If the system has a multiple components and is ionized, both chemical composition x and ionization ratio ϕ are also needed to describe the system. The spectroscopic methods we have developed can obtain sufficient thermodynamic quantities to designate uniquely the thermodynamic state of a system.

Spectroscopic Method: The emission spectra of rapidly evolving impact vapor clouds have to be taken with high resolution in both time and wavelength. This had been extremely difficult until an intensified charge-coupled device (ICCD) arrays were introduced. They are capable of taking a thousand of different wavelengths of light at once with an extremely short exposure time (up to ~10 ns). This permits obtaining high-quality emission spectra of impact vapor clouds.

Temperature T. When a high-resolution spectrum is obtained, the intensities of emission lines are measured to generate a Boltzmann diagram (Fig.1), which shows the logarithm of emission intensities I normalized by transition probability A, statistical weight g, and photonic energy $h\nu$ as a function of the upper energy level E of the transition divided by Boltzmann constant. The inverse of the slope in a Boltzmann diagram gives the temperature T of the measured vapor [e.g., 3,4,5].

Chemical Composition x. Once a Boltzmann diagram is made, one can also obtain the chemical composition. The vertical intercept of a fit line gives the logarithm of the number of ground state atoms, which is approximately the total number of atoms in vapor clouds generated in a laboratory [3,4,5]. Then the difference in the intercepts of two different atoms (Cu and Ca in Fig.1) in a Boltzmann diagram gives the ratio of the two atoms: atomic composition x.

Ionization ratio ϕ. Some atoms exhibit very strong ion emission lines. When these ion lines are treated as a different atom and a Boltzmann diagram is made, the number ratio of ionized to neutral atoms is obtained. This gives the ionization ratio ϕ [3,4].

Density ρ. The density of high-temperature plasma can be estimated by spectral line profile of emission lines. Some atoms such as hydrogen exhibit a large line width due to Lorentz broadening, which is proportional to 2/3rd power of electron density [3]. Laboratory experiments show that such Lorentz broadening can be observed with high enough accuracy to obtain a reliable value of electron density. The electron density can be converted to the bulk vapor density ρ using ionization ratio ϕ and chemical composition x [6].

Application to Planetary-Scale Impacts: The above methods have a wide variety of application in hypervelocity impact study. An immediate application is to study the EOS of highly compressed impact vapor, which may be highly different from an ideal gas. When the thermodynamic state of an impact vapor cloud is determined, the chemical reaction processes within the vapor cloud can be estimated much more easily. Such knowledge will help understand the problem of sulfur oxides in the K/T impact vapor cloud [7]. Furthermore, a quantitative comparison between impact- and laser-induced vapor clouds can be done with these methods. It will widen the range of the application of laser-simulated "impact vapor clouds" greatly [2,8]

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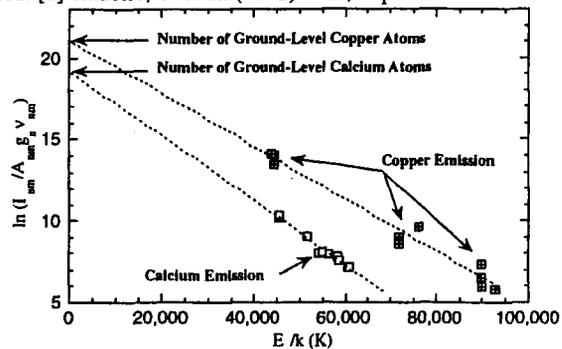
Fig. 1. Boltzmann diagram of an impact vapor cloud. The vapor cloud is induced by copper projectile impacting dolomite target. The copper and calcium emissions represent the projectile and target components, respectively.

COOLING OF THE KÄRDLA IMPACT CRATER: I. THE MINERAL PARASEQUENCE OBSERVATIONS. E. Versh[1], A. Jõeleht[2], K. Kirsimäe[3] and J. Plado[4], [1,2,3,4]Institute of Geology, University of Tartu, Vanemuise 46, Tartu 51014, Estonia ([1]desty@ut.ee, [2]ajoeleht@ut.ee, [3]arps@ut.ee, [4]jplado@ut.ee).

Introduction: Kinetic energy released to the target by a meteorite impact results in the heating-to-melting and vaporization of the projectile and target rocks, which then start to cool to the ambient conditions. In dry environments (e.g. Moon) the heat loss occurs mainly by conduction and radiation transfer. If the water is present at the crater site as on Earth and supposedly on Mars, then the cooling can include also convective heat transfer by hydrothermal circulation systems. Evidences of impact-induced hydrothermal activity have been found at many terrestrial craters [1], and it is suggested for extraterrestrial craters as well [2]. Cooling and development of such impact-induced hydrothermal systems can be recognized by the means of (1) mineralogical/fluid inclusion studies, and (2) by impact and geothermal modeling.

In this and following paper (see Jõeleht et al., in this volume) we report a complex geological observation and modeling study of post-impact cooling of a medium-to-small scale impact crater of Kärdla, Hiiumaa Island, Estonia. The Kärdla crater is 4 km in diameter and ~540 m deep with a central uplift exceeding 100 m height above crater floor. It formed in a shallow (<100 m deep) epicontinental Ordovician sea ~455 Ma ago into a target composed of thin siliciclastic and carbonate sedimentary sequence covering crystalline basement [3]. In this first part of our contribution we present the results of mineralogical, fluid inclusion and stable isotope studies.

Mineral parasequence: The crater-fill sequence at Kärdla crater hosts up to 400 m thick allochthonous and autochthonous breccias that have undergone water-rock interaction. A complex clay-feldspar-carbonate(Fe-oxyhydrate) assemblage characterizes the post-impact hydrothermal mineralization. The most intensive alteration is found in breccias and shattered basement around and above the central uplift. The results of homogenization temperature measurements of quartz fluid inclusions in allochthonous breccia encompass a wide range from 110 to 440°C, with the maximum between 150 and 300°C [4] (Fig). This temperature range is in agreement with the chloritic minerals formation temperatures of 150-325°C. However, the mineral paragenesis suggests that the main phase of chloritization was preceded by earlier cryptocrystalline K-feldspar formation, whereas the second generation of euhedral K-feldspar inside fractures and voids precipitated after the chlorite, probably at temperatures of 200-100°C. Dolomite-calcite and sulfides/Fe-oxyhydrates (hematite and goethite) reflect the final stages of cooling when temperature reached ambient conditions. Calculated fluid equilibrium temperatures for carbonates indicate that those fluid temperatures were below 100°C (in the range of 75-35°C).

Initial temperatures: Studies of hydrothermal mineral assemblages and fluid inclusions provide information about the post-impact temperatures and enables the mapping of thermal aureole. However, studies of mineral parageneses

lack in information on the life times of these hydrothermal systems and the cooling time is not assessed by this approach. Heat and fluid transfer simulations can resolve that question. However, this needs the initial post-impact temperature distribution to be known. Mineral geothermometry results suggest maximum initial temperatures at least 150-300°C in the central part of the Kärdla crater. The same is suggested by PDF studies in shocked quartz, which refer to the maximum shock pressures during the impact event in a range of 20-35 GPa [5]. The distribution of the most frequent fluid inclusion homogenization temperatures suggests also approximately the same range (Fig). However, the high temperature inclusions on homogenization temperature graph suggest trapping temperatures as high as 350-450°C.

Comparison with the preliminary results of the hydrocode modeling of impact (Jõeleht et al., in this volume) shows that the initial temperatures remaining in the rocks estimated by geothermometry are significantly higher than the model predictions using Tillotson equation of state, but are in general agreement when ANEOS is used. The details of modeling problems are discussed in part II by Jõeleht et al. (see this volume).
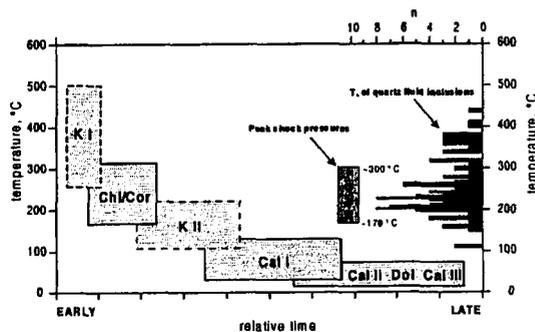
Fig. Post-impact hydrothermal mineralization parasequence at Kärdla crater. Shock pressures (20-35 GPa) from [5] and histogram of aqueous (H2O-NaCl) quartz fluid-inclusion homogenization temperatures (Th) from [4] are shown at the RH side. K - K-feldspar, Chl/Cor - chlorite/corrensite, Cal - calcite, Dol - dolomite; I, II, III - 1st, 2nd and 3rd generation. Formation temperatures for chlorite-corrensite and carbonate minerals are estimated form geothermometry and stable isotope composition, respectively. Positions of K-feldspar I and II fields are tentatively assumed from paragenetic relationships with chloritic and carbonate minerals.

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