2003 FARM WORKSHOP REPORT

Workshop on the Science, Status, and Future Needs of Experimental Rock Deformation

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SUMMARY

This was a workshop to assess the status of the field of experimental rock deformation and consider what actions might be taken to move the science and the field forward. Although it was originally conceived as a way to focus attention on possible needs for complex new experimental facilities to measure frictional resistance at seismic slip speeds, it became apparent that this could only be discussed in the context of the status and needs of the entire community. This scientific question was in fact listed as one of the most important by many of the participants, those working in the ductile field as well as the brittle field, but at least two other interesting and important questions that appear to require considerable apparatus design were also identified. The entire day and a half was taken up with discussion involving the whole group that was guided by the issues raised in the responses to an online questionnaire that all attendees were required to fill out prior to the conference. The workshop was very successful in beginning a dialog on important issues facing the field, and it is apparent that this workshop has started a discussion and process that are likely to lead to changes of many types that will be beneficial to the science, the scientists, and the infrastructure of the field as well as the many areas of earth and materials science that want and need results from this community.

Pre-workshop Publicity and Questionnaire

The announcement of the workshop and description of its purpose and logistics was made to the SCEC community, make available on the SCEC web site, and was widely circulated in the rock mechanics community, both to all those who had expressed interest in participating in the Rock Deformation Gordon Research Conference and the many people who receive email from the PPEM (Physical Properties of Earth Materials) committee affiliated with the Rock and Mineral Physics Committee of AGU.

Announcement

Third FARM Workshop:

Workshop on the Science, Status, and Future Needs of Experimental Rock Deformation

Terry Tullis, Brown University
Tom Jordan, SCEC
Robert Liebermann, COMPRES

Dates: August 13 and 14, 2004
Location: Mount Holyoke, Massachusetts, USA

The field of laboratory experimental rock deformation provides data that are critical to understanding earthquake mechanics, flow of the Earth's crust, and convection of the mantle. However, in all of these areas data are needed that are beyond the capabilities of present experimental equipment and laboratories. For example, it is not presently possible to conduct experiments that allow us to determine the mechanical behavior during coseismic slip on a crustal fault, during fluid-enhanced metamorphic reactions in the lower crust, or during high strain flow of high pressure phases in the deep mantle. In addition, the present way in which most data are collected, namely by isolated labs funded by modest grants to individual PIs, may not be capable of moving to the next generation of facilities that may be needed to answer the increasingly complex questions that the field is asking and being asked. The startup funds needed to establish a new lab are daunting, as is the cost of the technical personnel needed to keep a lab functioning. This means that few new labs are started by universities, the government, or industry, making it unclear what employment options exist for the next generation of scientists. In addition many of those responsible for running existing laboratories are approaching retirement age and it is not clear that these laboratories will continue when these people retire.

We will hold a one and half day workshop immediately following this year's Rock Deformation Gordon Conference to access 1) the scientific problems that a broad cross-section of the Earth science community feels are important and that may be addressable by laboratory experimental rock deformation studies, 2) the status of existing lab equipment and whether new equipment design and construction is needed to address the scientific questions, 3) the number and nature of existing personnel trained to conduct lab studies, the prospects for training future personnel, and the employment opportunities for existing and future personnel, and 4) whether existing organizational structures are adequate or new ones are needed to solve the scientific questions, run and create the necessary equipment, and employ the needed personnel. We have limited funds from both SCEC and COMPRES to support the incremental costs for attending this workshop by those who are already at the Gordon Conference, and to pay the travel and lodging expenses of any additional people who wish to attend. If you will not be attending the Gordon conference and decide that you need to rent a car from Logan airport, try to arrange to join with others in sharing the car to keep costs down.

The workshop itself will primarily consist of discussion, stimulated by a few carefully chosen presentations. The sessions will be organized around themes similar to those numbered 1-4 above as well as specialized breakouts on subdivisions of some of these, such as science questions focused on shallow crustal brittle deformation, deeper crustal flow, and mantle flow. In order to make the discussions at the workshop more efficient, there is a comprehensive web-based survey that participants are required to answer in order to have their workshop expenses covered. Others who are interested, but cannot attend the workshop, are strongly encouraged to respond to the survey. If possible, the survey should be filled out by July 20, and it must be filled out by workshop participants by August 1 if they wish to have their workshop expenses paid. Results of the responses will be available after July 20.

The workshop will start at 8:30 AM, Friday morning August 13, 2004 and end at noon on Saturday, August 14. Lodging will be in the rooms at Mount Holyoke used for the Gordon Conference. We hope to arrange bus transportation to Logan airport in Boston on Saturday afternoon. Registration for the workshop must be done on the workshop page at the SCEC website.

Web-based Survey

An extremely comprehensive questionnaire was prepared in consultation with some of the leaders of the Gordon Conference and of PPEM, was transferred to electronic form through the hard work of John Marquis at SCEC, and hosted on the SCEC website. The questionnaire will not be reproduced here due to its length, but is still available at the following URL: http://www.scec.org/workshops/WSSFNERD/. As it turned out it was more time consuming to convert the questionnaire to electronic form than anyone anticipated and as a result it was not possible to have it finished in time for everyone to fill it out completely and get the results summarized on the schedule anticipated in the above announcement. Nevertheless, John Marquis was able to create the tools necessary for the results in each category to be collected together and summarized, so the results of the questionnaire were extremely useful in guiding the discussions at the workshop. A summary of the survey results is attached as an Appendix to this report.

Detailed Agenda

Friday, August 13th

7:30 am - 8:30 am *Breakfast*

9:00 - 9:20 **Introduction and background**
- Distribute compilations of responses to questionnaires
- Origin of workshop idea
- Brief description of SCEC and COMPRES

9:20 - 10:00 **Discussion of what we will do during the workshop and our objectives:**
- Review suggestions from questionnaire on topics to discuss
- Brief airing of people's concerns for our field and hopes for workshop
- Do we want to add any to the distributed list?
- Adoption of tentative schedule

10:00 - 10:30 *Coffee Break*

10:30 - 11:15 **Discussion of science problems**
- Do we want to try to select a few as being "most important?"
- Look over list from questionnaire

11:15 - 12:00 **Discussion of apparatus development**
- What are the needs in order to solve scientific problems?
- What are the barriers and possible solutions to overcoming them?

12:00 - 1:00 *Lunch*

1:30 - 2:15 **Discussion of future of existing labs**
- Are they likely to continue when people retire?
- Should they?
- If so, what might be done to encourage their continuation?

2:15 - 3:00 **Discussion of opportunities for young scientists**
- Is it a problem any more than for other fields?
- What might be done to increase opportunities?

3:00 - 3:30 *Break*

3:30 - 4:15 **Discussion of connections to other communities**
- Do other Earth science disciplines want our contributions?
- Do we need to take a more active role in self-promotion?
- If so, how can that be done?

4:15 - 5:00 **Discussion of funding**
- Is it getting worse or better or no change?
- Is it adequate?
- If not, what can be done to increase it?

6:00 - 7:00 *Dinner*

Saturday, August 14th

Sat. 7:30 - 8:30 *Breakfast*

8:30 - 9:00 *Checkout*

9:00 - 10:00 **Organizational structures, USA and internationally**
- Is cooperation adequate?
- How might it be improved?

Are new structures needed to do the needed science, including building and running the machines?

10:00 - 10:30 *Coffee Break*

10:30 - 11:15 **Actions to consider**

What do we do next beside writing and distributing a report?

Can we use everything we gathered in the survey and how? Should more people be encouraged to fill it out? What do we do with it?

11:15- 12:00 **Revisit selected items**

Revisit topics on which we spent inadequate time

participants

Below is a list of the 36 participants and email addresses. A few others attended part of the time.

Name	Email address	Name	Email address
Boettcher, Margaret	mboettcher@whoi.edu	Liebermann, Robert	Robert.Liebermann@stonybrook.edu
Boroughs, Lydia	Lydia_Boroughs@brown.edu	Lockner, David	dlockner@usgs.gov
Cooper, Reid	Reid_Cooper@brown.edu	Milsch, Harald	hmilsch@ldeo.columbia.edu
Dresen, Georg	dre@gfz-potsdam.de	Mitchell, Tom	t.mitchell@liv.ac.uk
Durham, William	durham1@llnl.gov	Montesi, Laurent	lmontesi@whoi.edu
Faulkner, Dan	faulkner@liverpool.ac.uk	Muto, Jun	mutoh@mail.tains.tohoku.ac.jp
Fitzenz, Delphine	fitzenz@usgs.gov	Paterson, Mervyn	mervyn.paterson@anu.edu.au
Fountain, David	dfountai@nsf.gov	Renner, Joerg	renner@geophysik.ruhr-uni-bochum.de
Chester, Fred	chesterf@geo.tamu.edu	Rybecki, E.	UDDI@GFZ-POTSDAM.DE
Gratier, Jean-Pierre	Jean-Pierre.Gratier@obs.ujf-grenoble.fr	Skemer, Philip	philip.skemer@yale.edu
Green, Harry	harry.green@ucr.edu	Spiers, Chris	cspiers@geo.uu.nl
Heilbronner, Renee	renee.heilbronner@unibas.ch	Stipp, Michael	michael.stipp@geologie.uni-freiburg.de
Hirth, Greg	ghirth@whoi.edu	Tullis, Terry	Terry_Tullis@brown.edu
Holyoke, Caleb	Caleb_Holyoke_III@brown.edu	Wang, Yanbin	wang@cars.uchicago.edu
Jung, H.	hjung@citrus.ucr.edu	Weidner, David	dweidner@sunysb.edu
Karner, Stephen	karner@geo.tamu.edu	X. Xiao	xhxiao@mit.edu
Keulen, Nynke	nynke.keulen@unibas.ch	Zhou, Yongsheng	zhouysh@163bj.com
Kronenberg, Andreas	a-kronenberg@tamu.edu	Zhu, Wenlu	wzhu@whoi.edu

Summary of Results

This workshop has made and will continue to make a big difference in the experimental rock deformation community and the science it will do. It has gotten the community thinking and talking about some important issues that it faces, and there will be follow-up activities of many types. Although the workshop initially grew out of SCEC needs for more data on high velocity friction, it came at a time when many in the community were thinking that an assessment of many issues was needed. The issues include what the important science questions are, what new generations of laboratory equipment might be needed to answer them, what might be done to strengthen existing labs and create new ones so that the community has access to the facilities it needs, how to foster more cooperation among all the labs and workers so that the exiting facilities are used most effectively by many, and how to create more opportunities for employment as well as access to labs for young scientists.

An interesting development is that a number of international experimentalists attended, since the meeting occurred at the end of the Rock Deformation Gordon Conference. As a result of the discussions at the workshop, the very active European experimental community is going to have a similar workshop as part of one of their upcoming meetings in order to organize themselves for more collaborative interactions of the type we were discussing.

An interesting development is the beginning of an effort to catalog and publicize what equipment may no longer be needed by existing labs, for example when personnel and/or needs of their home institutions change, and what new locations might be found for this equipment, especially where young scientists are trying to set up new facilities.

Work is continuing among many in the community as we think about models of what might make sense for future organizational structures and laboratory facilities to promote improvements in the research climate for experimental rock deformation. The participants were universally enthusiastic about the workshop. All agreed that at least one more workshop, perhaps funded by NSF, is needed to further refine thinking on many of the topics discussed and to generate subsequent plans for how to implement some of the consensus recommendations that would emerge. The perspective and active participation of David Fountain from NSF was a very valuable contribution.

APPENDIX

SUMMARY OF SELECTED RESULTS FROM THE WEB-BASED SURVEY FILLED OUT BY ATTENDEES PRIOR TO THE WORKSHOP

The questionnaire used for the survey was divided into 7 sections:

Section One, Scientific Problems

Section Two, Personnel

Section Three, Equipment

Section Four, Support level

Section Five, Organizational Structures

Section Six, Workshop Topics:

Section Seven, Other

The responses to sections 1, 2, 5, and 6 follows, organized as much as possible so the answers are grouped with similar ones. Answers to the other sections are not included, either because it did not seem helpful to include them in this report or because the data has not yet been digested, due to the fact that the SCEC personnel were to unable to spend more time on creating tools for extracting it and without those tools too much time and effort was required to extract it. Every person’s answer has been included, but the names of the respondents have been omitted.

QUESTIONNAIRE, SECTION ONE, SCIENTIFIC PROBLEMS:

Question: What do you consider to be the most important unsolved problems that laboratory experimental rock deformation studies can contribute to solving?

The categories below into which the responses have been grouped in the answers to section 1 were created after reading all the responses. Some answers to not categorize easily and some could have been placed in more than one category.

Categories: General and Fluids, Brittle Friction, Brittle Fracture, Ductile General, Ductile Crust, Ductile Mantle, Brittle/Ductile

(* on those which author felt new organizational structures needed to solve them)

Frequently Mentioned Problems

*Interactions between fluids (including melt), chemistry, and deformation
*Fault resistance during earthquakes
*Earthquake mechanics
*Deformation of polyphase aggregates
*Determination of rock rheology
*Shear localization

General and Fluids

* Fluid flow in crust and upper mantle
Relations among permeability, fluid flow and deformation
Fluid flow properties of fault damage zones

Effects of fluids and fluid-related microstructural change on rock rheology and transport properties in crust and mantle.
*Kinetics of deformation processes that are controlled by mass transfer in presence of fluid phases under stress
Mechanical and chemical effects of fluids on brittle deformation
*Rock deformation in the presence of non-aqueous fluids
Geomechanical response of reservoirs to CO_2 disposal signature
*Chemical processes in rock mechanics
Deformation/metamorphism relations
Interplay of deformation and metamorphic reactions in rocks
*Do active deformation processes (+/- fluids) have a detectable geophysical signature
*Mechanics of volcanic edifices
*Deviatoric stresses and phase transformations
Deformation under general states of stress
*Influence of confining pressure on deformation
Non-steady state deformation
*Shear localization
Scales of heterogeneity
Scaling to between the laboratory and the field
Core formation under dynamic conditions
*Coupling of concepts from several scientific disciplines

Brittle, Friction

*Coseismic friction
Dynamic friction at co-seismic slip rates
*Dynamic friction in earthquake process
Frictional properties of rocks during coseismic slip
Dynamic strength of faults
Slip localization and deformation at seismic rates
*How do pseudotachylites form?
*Role of fluids in dynamic earthquake propagation
*Slip/healing behavior of seismically active faults
How are interseismic processes and shear strength at the onset of failure related?
*Earthquake nucleation
Earthquake triggering
Initiation of earthquakes
Nucleation and propagation of faults and earthquakes
Prediction of earthquakes
How earthquakes end
Slow or silent earthquakes
Understanding of and dynamic modeling of fault systems
*Mechanics of crustal faults
Determination of effective frictional properties for natural faults
In what way lab friction experiments are good analogs to what happens in the Earth?

Origin of time dependence in rate and state friction
Friction stability of gabbroic rocks

Brittle, Fracture

Brittle failure and fluid transport
*Chemically assisted crack growth
Rock fracture, AE, and experimental seismicity
Why 30°?
Micromechanics of brittle failure and scaling relations
Scaling in fracture
Deformation by pervasive mesofracture

Ductile General

*Flow of polyphase aggregates
Flow of polyphase rocks
Deformation behavior of multiphase aggregates
Constitutive equations for deformation of two-phase silicate rocks
Intrinsic stress heterogeneity in a polycrystalline “continuum”
Strength of ductile shear zones
Dynamic localization (strain softening) of fault/shear zones
*High strain rheology and texture development
Large strain deformations and role of different strain paths, refinement of flow laws
Flow of planetary ices
*Improved description of ice rheology
Low-strain rate deformation mechanisms
Effective pressure law in ductile rocks?
*Nucleation of new phases and the growth of grains under a differential stress: Where do new nuclei develop (near dislocations, on the same phase, on a different phase)? In which way do nuclei/grains grow (big ones eating small ones, clustering of small ones, clustering independent of size, dissolution-precipitation)? What is the influence of dislocations?
Thermodynamics of plastic flow
"Self-"assembly in plastic deformation

Ductile Crust

Diagenetic compaction of sediments
*Pressure solution creep
*Kinetics of pressure solution creep and associated metamorphic reactions
*Role of melt in continental rheology
*Rheology of crust
*Rheology of lower continental crustal rocks
Lower crust rheology
Role of phyllosilicates in crustal deformation
Water in quartz

Analog materials, lab experiments, and quantitative modeling of a "full-scale" earth

Ductile Mantle

*Rheology of lithosphere and upper mantle
Rheology of mantle minerals
Measure V* of mantle phases
*Rheology of mantle transition zone and deeper mantle
Strength of magnesium oxide at high pressures and temperatures
Reliable flow laws for major rock types at high pressure
High pressure rheology

Brittle/Ductile

*Mechanics of transitional brittle ductile transition
Transitions of modes of deformation between brittle and "ductile" behavior
Rupture mechanisms at depths
Brittle-ductile coupling during earthquakes
Mechanics of partial melts
Understanding the initiation of melt segregation during flow
Flow of partially molten granitoids and melt extraction

QUESTIONNAIRE, SECTION TWO, PERSONNEL:

Number of grad students / Number born in the same country as your institution:

USA				
5 / 5				
3 / 3				
1 / 1				
3 / 2				
1 / 0				
2 / 2				
5 / 3				
1 / 1				
2 / 1				
2 / 2	USA Mean	2.5 / 2.0	USA SD	1.5 / 1.4
Non-USA				
2 / 2				
2 / 2				
2 / 2				
4 / 4				
4 / 1				
6 / 6				
3 / 3				
2 / 2	Non-USA Mean	3.5 / 2.75	Non-USA SD	1.8 / 1.6

Total Mean	2.8 / 2.3	Total SD	1.5 / 1.5

Question 1: Comment on the adequacy of the technical support personnel in your institution to support your laboratory experimental studies. Be specific as to which job titles for technical staff that you either have enough of or need more of:

USA

MIT: More technical people are always better.

MIT: There is no support from the institution for any technical staff. Principal investigators must fund all activities within their lab or research area. On one occasion (1992), I received matching funds for equipment.

Brown: Technical support is inadequate. We have some access to a thin section technician, but not enough. We have adequate access to a satisfactory machinist, although sometimes we have to wait too long for his services. We do not have access to an electronics technician and need that. We have some support for general computer problems, but need more that is specifically aimed toward our experimental data collection and control system.

Brown: We have a machinist and a thin section preparer for our samples, so I am satisfied with the level of support.

Brown: Department has a 0.6-time machinist who supports the efforts of the rock-mechanics and experimental petrology faculty. We have recently requested additional help in the area of digital and analog electronics; request is pending.

WHOI: I do not have any technical support at WHOI.

Chicago: Electronic technician
Mechanical engineer
Mechanical technician
University machine shop, APS machine shop available with fees
Adequate support for technical development and engineering design work

UVM: No technical staff, but undergraduate support for part manufacturing.
I use the Instrumentation and Modeling Facility of the University of Vermont for machining and repairs.

Louisville: Machine shop technician shared with physics department

Texas A&M: Clayton Powell serves as our "Assistant Research Specialist". He has replaced our long-standing technician (Jack Magouirk) and has a technical degree in electronics. In the first two years, he has quickly picked up the mechanics and specialized technical skills required and voluntarily sat in on the graduate rock deformation course. Initially, he was hired by Texas A&M on hard money, but recently cut-backs at the University have threatened his long-term support. Clayton is excellent and between Fred Chester and myself, we plan to keep him fully supported.

Texas A&M: The support we presently have for our technician, while good, is barely adequate. It would be ideal to have a full 12 months support each for at least two techs in our laboratory. At present, we have (I believe) less than 12 months and only for one tech.

Riverside: My univ provides me a 1/2-time advanced-level machinist, Frank Forgit (negotiated upon my hire). I support the other 1/2 on grants. My univ provides nothing more in technical support of any kind to my department.

Laurence
Livermore: General technical support is excellent (during those rare periods when I can afford it). Specifically trained technical support is nearly non-existent, however, since I could never afford the cost of full-time support.

Non-USA

China EQ
Admin: There are 3 old technicians but they are too old (older than 62),and there are 2 young technicians but they graduated from university last year. So we need more technical staff.

Basel: Mechanical technician: excellent
Electricity technician: moderate

Grenoble: We are very very short for technical support.

Potsdam: Deformation and Rheology at GFZ has 12 permanent staff (6 scientist positions and 6 technical staff) plus one post-doc and 4 students.
GFZ has excellent and well staffed workshop and sample preparation lab.

Utrecht: We have excellent technical support though this is increasingly under pressure:
1 workshop technician, 1 electronics, 1 general/analytical

Manchester: We have a dedicated experimental officer (Robert Holloway) in the rock deformation lab. He is indispensable and has been with us continuously since 1968. He has a vast range of essential skills and experience. He retires in 3 years and will be very difficult to replace.
We also share the general support of staff of rock cutting and sectioning, mechanical workshop and electron optical facilities within the dept. The dept is relatively well off in the UK for a wide range of hi-tech facilities.

Liverpool: Need more technical support. There is limited support for electronics, but a dedicated mechanical workshop technician is needed.

Question 2: What thoughts or concerns do you have about the number, training, quality and outlook for the future of personnel trained in the area of experimental rock deformation?

USA

I am quite concerned. I am NOT an experimentalist: I have classwork in it, and have published results. I am VERY concerned about the lack of INTEGRATED rock mechanics-experimental-field programs of the type I went through: A&M, Brown, UCLA, MIT; I think that objectively all programs have evolved (all do, which is natural) to a more broken up set of programs.

I don't really have a firm grip on what the outlook, training, and quality for the future of people trained in this field is.

I am concerned that too few people are going into this area, that some of them are not strong enough, while at the same time that insufficient opportunities exist for them. This might suggest that the balance is about right, but the number of important problems is large and the total research effort is not what it ought to be, given the amount of work to be done.

Talking to a few friends in that field, it is clear that there are too few satisfactory positions in academia. I don't know of many universities that would seek to hire in experimental rock deformation, which leaves only the possibility of being added to existing labs, which is not better than going to industry, in my opinion.

It is a bit grim because after a PhD is obtained (from one of the few institutions offering a PhD in rock mechanics), there are few opportunities for post-doctorate studies. I am particularly worried about the lack of post-doctorate funding from NSF. Although with EarthScope funding, this might change.

I think that we have enough people in the US working on various rock deformation problems. The problem is having students that aren't just sitting in a lab deforming rocks and making models of deforming rocks without ever doing field based research.

I am worried that the experimental rock mechanics community is shrinking in this country.

Near panic. While I have worked with many students during their thesis research, few have gone on to apply their experimental rock mechanics training in their chosen careers. I perceive this as not the result of free choice on the part of the students, but of the paucity of jobs in the field in the U.S.

In the US, any shrinkage of the experimental rock deformation field might be primarily reflecting reductions in the number of basic geology programs. If so, a tight job market for experimentalists should be expected.

Some of my best students and postdocs have gone into the oil and gas industry, and have thereby not been able to continue research in rock mechanics. None of my own students has continued in an academic career. Given our current numbers of PhD students, we will not contribute many new academic researchers in rock deformation.

Quality and training are good. Outlook for employment is less than good.

In certain institutions, the training of personnel in experimental rock deformation is exceptional. However, outside of these places that currently have labs, I sense that the training is totally inadequate. As such, from a national standpoint the number of persons who gain experience in the field of rock deformation is too low.

I have watched the number of students interested in experimental rock deformation decrease progressively for at least 20 years.

One major concern: finding geoscience faculty positions for the best and brightest. The (academic) field could easily expand by 25% in the USA without reaching "saturation" from a scientific-needs perspective.

Non-USA

We do not plan to have a lot of students. We do a lot of our experimental research alone or with the help of the technical staff.

In Germany there are only very few academic positions available in this field. In geomechanics and industry the situation is more promising recently

MSc student projects rarely provide a framework that leads to serious training or publications in our field.
PhD students are under pressure to publish results so that training in methods, apparatus design etc is slipping away!

Whilst it is hard to find a real 'gem' who will become a future star experimentalist, who has an exceptional degree of familiarity across the board in basic physics, chemistry and engineering, there is a fairly steady supply of reasonably competent people who will be able to find good employment after graduating. this is not to bad provided one finds a real star every few years or so.

Owing to the nature of the work, it is only going to be possible to fund a small number of laboratories throughout the world. Some of the heads of the current labs may be due to retire soon, but there are a plethora of excellent young scientists that are capable of taking the labs forward scientifically in the future. I have no major concerns for the future, I only feel that we should be attempting to raise the profile of rock mechanics and stressing (excuse the pun) the importance of the data that we produce to the relevant funding agencies within our host countries. For a relatively small group of researchers, this is vitally important.

I think that I will not find another postdoc after this one and that is why I'll have to stop doing research. Although the job situation is better in America these jobs usually go for the Americans. In addition, numerical modeling positions are not always open for people with experience in carrying out lab experiments.

Question 3: If you have concerns that the number of institutions pursuing the field of experimental rock deformation needs to be expanded or is in danger of shrinking and becoming too few, do you have any suggestions to improve the situation?

USA

Could a MUCH smaller Earthscope, or CUHASI sort of program be set up, with several national centers of experimental teaching and research; with techs, (and teaching faculty willing to put up with) teaching grads, and faculty; summer seminars, etc ?

I think the number is not large enough and in the USA it could shrink in an unhealthy way. If only a few of those of us who are approaching retirement age are not replaced the numbers could become dangerously low, since small numbers are involved now. I suspect that two issues need to be considered. The first is a lack of understanding by many in the broad earth science area of the importance of what we can provide to many other fields, especially in a time when emphasis on the solid earth is being overtaken by emphasis on climate change, etc. Second the startup costs for someone starting from scratch in a place with no existing experimental lab may make some institutions decide not to search in this area. Some new initiative that involved funding collaboratories might help solve both problems, by drawing attention to the discipline and providing place where new workers could start while gradually building up their own labs.

In the US, we need new blood in position of control for experimental labs. As I said before, younger people can only obtain secondary positions in existing labs. I can only hope that if the economy picks up, universities will be more willing to invest in experimental rock mechanics lab (a significant investment). In the meantime, let us make sure that if anyone retires, the position remains in the hands of an experimentalist.

More funding and as mentioned above, EarthScope provides a possible hope. The other thing that we should do is to encourage more integrated studies, such as with numerical modelers who can help scale experimental rock deformation results.

I know some universities and institutes of China hope to pursue the field of experimental rock deformation, but the technical level is lower than that of USA and Europe. So I think we need to cooperate with the lab of USA and Europe.

More incorporation of undergraduate students in labwork will make students more familiar with rock deformation, especially in institutes with classical structural geology education. Design easy rock deformation practicals with observable changes in microstructures (e.g. with salts or metals and a differential stress by a dead weight on top of it). A lot of students do not choose for a PhD in rock deformation because they do not know what it is about exactly.

We need to find ways to increase our budget with NSF

Identifying the exact nature of the problem may be harder than identifying the solution. Experimental rock mechanics requires a more elaborate infrastructure than many disciplines, something that has always been true. What is new is that funding in the U.S. is becoming insufficient to support a critical mass of scientific career positions, post-docs, assistantships, and technical staff. Why is this? Compared, say, to the core rate of inflation over the past 30 years, have our costs gotten disproportionately high, or has the funding become disproportionately low? If the former, the solution lies in raising productivity (for lack of a better word) to earlier levels. If the latter, then it is our popularity (for lack of a better word) that is the problem. Then come the questions, is it the popularity of our particular discipline, small-scale experimental work in general, experimental work in general, science in general?… etc. We need data to help identify what is going on here.

Perhaps we need to, very clearly, redefine our field in terms of its applicability to natural hazards, and environmental sciences. However, this would result in preservation of a smaller corp of experimentalists.

I do have concerns that our field is shrinking.

A major barrier is the ability to establish a laboratory while doing the level of research and teaching required for tenure.

A problem unique to our field is the need to establish a laboratory, not of instruments that are manufactured by commercial outfits, but of prototypes and need to make that lab function with little technical assistance. I personally found the decision to go to an institution with an existing large, "communal"-access rock deformation lab easy.

We need to train new graduate students broadly. They should have areas of expertise that allow them to integrate with smaller educational institutions in more traditional areas of Earth science (structural geology, mineralogy, etc.) or in other areas of science (solid state physics, materials science, civil engineering, mechanics).

I do have concerns on both counts: 1. that the number of institutions (especially in the USA) that are pursuing rock deformation does need to be expanded, and 2. that over the next decade or so that the number of USA institutions will shrink.

Ultimately, the onus is on us as a group to improve the situation. This may be done by re-educating our scientific colleagues about the valuable contributions made to Earth Science through rock deformation experiments. I'm not certain how to achieve this, except through determined efforts to increase the profile of our science.

We may also need to look ahead to much more difficult experimental programs that address complex problems. While these will be very hard to do, these may show our scientific colleagues that there are still many "saucy" things we can do in rock deformation labs. For example: 1. experiments involving a wide range of "new" physico-chemical conditions that we test; 2. Incorporate into the experiments complex geometries; 3. Routinely incorporate into the experiments a variety of measurements that enable us to remotely sense the processes associated with deformation (e.g. acoustic properties, electrical measurements, NMR

techniques, in-situ fluid chemistry analyses, etc).
Finally, we need to educate the funding agencies about the costs involved in doing our science and that our work is worthy of the costs. When we are competing with colleagues who only need a $5000 computer...how can we survive?

I believe that our field is approaching a crisis. The number of laboratories is very small and appears likely to decrease significantly in the near future when senior faculty retire. The cost of starting up experimental laboratories is part of the problem, but I believe that the value of the field in the eyes of academia has also decreased.

This workshop is a start. A forceful report from this workshop, indicating the number and types of important and interesting problems that need attention combined with an articulate accounting of the educational role rock physics can play in the life of a department would prove useful, too. Getting the attention of the National Academy wouldn't hurt.

Non-USA

In France we have only a handful of teams devoted to experiments on rocks deformation. This potential is rather stable over the last ten years.

For Germany number of labs is about right, however compared to other fields in geophysics rock physics, rock mechanics and geomechanics are less well represented.

More thought needed.

The important thing in a given institution is that when a lab leader retires there is a 'star' in place to carry on running a lab, or the institution may close it down. It is incumbent on the lab leader to tray to ensure that his or her succession is established well in advance, if at all possible. I don't think that in many countries there is scope for much expansion of number of labs pursuing academically-oriented rock mechanics.

I think that some institutions are not opening new positions and they are merely waiting for the old ones to retire.

QUESTIONNAIRE, SECTION FIVE, ORGANIZATIONAL STRUCTURES:

Question 1: *Do you believe the scientific questions that you listed in Section One can be solved by individual investigators or small collections of colleagues working in their home institutions or visiting those of other colleagues on an ad hoc basis, or that some kind of new organizational structures may be needed?* -and- *If you believe any problems need new structures to solve them, list which problems are in this category referring to them with your "title" and priority ranking:*

Some can be solved without creating new structures, but some need new structures (16 out of 20 respondents)

All can be solved without creating new structures (4 out of 20 respondents)

Question 2: *What ideas do you have concerning types of new organizational structures that might be needed to solve the scientific questions?*

The draft list of ideas, A-D, for reference to answers below:

A. Creation of new laboratories, by NSF or other government agencies, and/or use of existing laboratories to be better supported and linked as a "collaboratory." These are envisioned as centers without walls, in which researchers can perform their research without regard to geographical location and share equipment, data and other resources, much like the new NSF-funded Network for Earthquake Engineering Simulation (NEES) (http://www.eng.nsf.gov/nees/index.htm , http://www.eng.nsf.gov/nees/nrcreport/ExecutiveSummary.pdf).

B. Become active members of an expanded COMPRES (COnsortium for Materials Properties Research in Earth Sciences) which has had study of materials at pressures characteristic of the Earth's mantle as its primary focus (http://www.compres.stonybrook.edu/). This is basically an already established collaboratory which already includes much of what our field does, but with little present focus on the Earth's crust.

C. Find one or more institutions, either universities or federal labs, that can serve as sites for expansion of relevant activities, sites for construction of new equipment, and destinations for visiting research scientists. Presumably the institutions would have to contribute space, personnel, and support funds and in return would be awarded federal resources to build up community capability. This would require not only institutional support but presumably a long-term commitment on the part of existing and/or new faculty or other professionals to ensure that the facility functions smoothly.

D. Creation of a new category of grants at NSF and/or other agencies that is designed to support technical personnel with a time frame that allows hiring for a minimum of 5 years with a rolling appointment that allows for planned termination.

Summary statement of above ideas for reference to answers that follow:
A. New and existing labs, better supported and linked as a collaboratory
B. Use COMPRES as framework for a collaboratory
C. Find institution(s) and people willing to serve as center(s) for expansion, new generations of equipment, and hosts for visitors.
D. New grant category to support technicians with 5 year rolling appointment

==

I think we need to move to a combination of A, C, and D. COMPRES is good, and focused on their role; What we "crusty" types work on is pretty different.

See how the consortium of hydrology depts. is evolving (CUHASI) within NSF; centers for data integration, IGERT proposals, instrumented watersheds., etc.

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These 4 propositions are very interesting. D is probably necessary to prevent grad students or post-docs from being sole responsible for the equipment and related software, because continuity and conservation of the knowledge and know-how in a lab are key.

At a much smaller modest scale, it would be interesting to make a sort of newsgroup on which grad students or researchers could ask questions. It would be an easy way to connect people working on the same general topic but from different viewpoints. Again, it would be a way to connect modelers and experimentalists and field geologists or geophysicists. It's a low maintenance project, and does not necessarily involve those who are already too busy with high responsibilities. If someone has a "beginners-level" question about a type of experiments, he/she can send an email to the newsgroup, and a student can answer, and in turn ask another question about a modeling technique. Word about an interesting paper could also spread faster.

==

C-D: I do believe that we need new labs with new people: the new blood. New people come with less baggage, making them more likely to find new problems and new approaches to solve them. One major hurdle is the high cost of setting up a lab. In my experience, this explains why many universities are shying away from hiring experimentalists. NSF could help if it created a special award to a young experimentalist to help him/her setup an experimental lab.

D: I do not recommend having a specific experiment rock deformation program at NSF, as it is a zero-sum game: balkanizing the pool of money is not very efficient. Some years, there may be 10 great experimental proposals, and others none, but the amount of money stays the same. Keeping larger pools of money gives greater flexibility to the program directors to seize opportunities and fund more of one type of studies if great ideas are demonstrated.

A-B: For the same reason, I do not support consortiums or "centers without walls". If people are willing to collaborate and share their equipment within such a center, they should be willing to do it without one and agree to collaborative proposals. If a center is needed, then, it should have an actual location, as to my opinion, the spontaneous exchange of ideas between people who will bounce into one another in the corridor or during a coffee break is the only thing to gain from a "center". But I am not persuaded that with modern communications, this is worth the cost. Better give the money needed to create a center to a young scientist who wants to create his/her own lab.

==

A-B are good ideas for enhancing collaboration and use of equipment

C would work with new funding and a good integrated proposal

D seems political difficult in the present climate, but a necessity that we should argue for.

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A,B, C. In addition, it need to cooperate between different country, such as there are large cooperation project about the lithosphere structure of Tibet between China and USA, China and France, China and Germany. We can propose the similar project to setup collaboratory and training students.

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Other than high pressure (up to 4 GPa) gas apparatuses to replace the Griggs solid media apparatus, I do not believe that any significant problems exist in our current range of experimental apparatuses. Everything below 150-200 km (i.e. >4 GPa) is speculation because we cannot confirm or refute our hypotheses by direct observation of outcrops or xenoliths.

==

All the possibilities listed above sound interesting, and we definitely should discuss them in details at the workshop. In general, I like the idea of sharing high-tech equipment. Through collaboratory, we can avoid "re-inventing the wheel", save money and time for better technical supports, and therefore concentrate on science. However, I also have concerns about the accessibility, adequate technical support for outside/general users, and flexibility.

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When compared with other communities in Planetary and Earth Sciences, the community working on experimental rock deformation seems at first not very well organized. If we refer to Astrophysicists for example they are able to initiate the building of a new device long time before its use (sometimes 10 or more years). This is due to the need of a huge technological effort and the perspective of having only one or two facilities around the world. The best example is the ALMA facility (radio-millimeter antennas) that gathers the whole countries on the same device.

The question for the community working on experimental rock deformation is to know if we need such a unique device to overcome a specific scientific barrier. I am attending the workshop to see if there is a proposition that really need a unique (worldwide) facility. An alternative approach is a worldwide network of labs that can relies on specific facilities and allow a large exchange of competencies.

==

A combination of A and D could be envisaged. Institutions that have an established record of sharing experimental/analytical equipment may be designated as experimental centers (federal labs come to mind, but those need to clear people for security reasons). The centers would be supported based on a 4-5 yearly rating. Proposals involving experimental work could be, if the PI requires, considered with regard to use of the appropriate centers.

==

A. Possibly, but I find it difficult to see how rock deformation experiments can be performed in short visitation times, while teaching etc at home institution. Also, how is whole better than its parts when the fundamental unit of a rock deformation lab is a cottage industry approach

B Possibly, but it strikes me that one of the strong arguments for a centralized COMPRES for high pressure mineral physics is the need to centralize around a synchrotron beamline. Visits are necessary for this, and fortunately, significant numbers of measurements can be made of elastic and other properties during relatively short visits, compared with a single $10^{-7}s^{-1}$ creep experiment.

C A centralized lab/center with engineering, technical staff could serve to innovate and develop new apparatus in collaboration of rock deformation researchers - the final apparatus would go to the institution of the individual researcher (seeding of a new, improved cottage industry) but plans would be available for others to establish as well. Potentially such a center could provide machining, electronics, acquisition and control software, and other needs of individual researchers. And such a center could serve as a center where researchers could take sabbaticals at during equipment development or experimental testing, and students could take special topics courses.

D This would be welcome to researchers of our field, but I don't see how we can sell it to NSF or the panels and reviewers composed of researchers in other areas who likely could also use additional support

==

Points A-C are linked and desirable, supposed access to facilities is possible for "externals". Individual institutions should then also be awarded more funds to be able to directly host researchers on a temporary basis to perform time limited projects in terms of local "visiting scientist programs". This would be essentially attractive for young, untenured scientists. It will also help to reduce the time from proposal to award for an individual person. Technical in-house support is crucial for progress in developing new experimental techniques but also for simply maintaining an existing infrastructure. Technicians need to be hired preferably on a permanent basis. A technician lost after five years or so due to insufficient funding is a sad thing.

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Stability of funding would greatly improve planning and implementation of research facilities. COMPRES is an excellent example of one method for increasing funding, providing stability and pooling resources. But, for future initiatives, Option C provides more flexibility than establishing COMPRES as an overarching organization. Certainly, we could, as a group, think more about cooperative ventures, and perhaps, institute a change in funding options from NSF and others.

==

I applaud every one of these suggestions. Possibly the aspects that may help those seeking tenure-track faculty positions are the collaborative/visiting scientist proposals (A and C). One significant stumbling block for prospective faculty appointees is that departments are loath to think about providing start-up funds to set up rock deformation labs. If prospective faculty hires can point out that they'll be able to "visit" a "national lab", then the departments may show greater interest in hiring the person to a faculty appointment.

==

I think that there should be a closer collaboration between chemists and physicist that try to solve the problem of chemically assisted crack growth. This could give new ideas for setting up more realistic lab experiments.

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Funding possibilities needed to promote physical collaboration

QUESTIONNAIRE, SECTION SIX, WORKSHOP TOPICS:

The Question posed was: *What do you think are the 3 to 5 topics that are most important for us to discuss at the workshop?*

The summary of these suggestions groups the answers by categories that seemed appropriate based on the answers.. The question in each category was extracted from the answers in that category to initiate the group discussion on that topic.

SUMMARY OF SUGGESTIONS ON TOPICS TO DISCUSS

Science Questions

Do we want to try to list the most important problems or even prioritize them, or do we want just to be aware of the range of opinions?

Apparatus Development

In order to attack important problems, do we need to design and build new apparatus?

How can we develop and implement them while allowing individuals to maintain a competitive research and teaching schedule?

Future of Existing Labs

Does the aging and forthcoming retirement of many present workers represent a threat or an opportunity?

How can we make it an opportunity and not a threat?

Opportunities for Young Scientists

What are the job opportunities for young scientists?
How can we help young scientists get established?

Connections to Other Communities

Do we need to do Public Relations to increase our visibility, and if so how?
Do we need to do something to increase the usefulness of our results for other communities, and if so what?

Funding

Is the present level adequate?
If not, how might it be increased?

Organizational Structures, Including Non USA Issues

In order to do new and better science and use existing facilities most efficiently, do we need to increase the amount of cooperation between scientists, labs and countries, or is the status quo fine?
If we do, what is the best way to do this?
Might altering the way we work together solve any of the other problems such as increasing visibility, helping young scientists, increasing funding?

Actions to Consider

What should come out of this workshop?
What should be next step, if any?

DETAILED ANSWERS TO THE FOLLOWING QUESTION

The detailed answers to the simple question posed are grouped by the categories given above. At the end of each group in bold are the questions pulled out of these responses to initiate the group discussion.

Question: What do you think are the 3 to 5 topics that are most important for us to discuss at the workshop?

Science Questions

Which problems are of the highest priority for the next 10 years?
Scientific problems
First what are the propositions and are there any subjects either that gather a large part of the community or that seems so interesting that it rallies most of the opinions?
What new problems can be attacked, if new resources were available?
Extending the range of research projects that we tackle
What are some of the primary earthquake science questions that we might be able to answer with lab experiments?
Strategies and limits for solving the "deep earthquake problem" from the rock mechanical point of view.
How to make experiments relevant to dynamic ruptures including fluids (including CO_2), and good analogs to the architecture of fault zones
The scientific problems in laboratory experimental rock deformation studies, especially the rheology of polymineralic rocks.
Solving fault slip
Upscaling lab data
The extrapolation in time problem

As a new PhD student I feel I can only offer what would be good for me hear as I do not have the experience to make suggestions. However, I am most interested in the role of fluids in fault systems.

Do we want to try to list the most important problems or even prioritize them, or do we want just to be aware of the range of opinions?

Apparatus Development

New equipment design and construction in next 5years.

Do we have the experimental apparatuses to solve the highest priority problems?

Developing apparati that address critical structural and geophysical questions.

What are the scientific problems that need radically new approaches in experimental equipment design?

Developing rigs that address deformation in the 400 m - 2 km depth range.

Bridging the gap between mineral physics and rock mechanics, e. g. How can experiences in rock mechanics and mineral physics be merged to build a large volume deformation apparatus that can perform controlled deformation experiments at p-T relevant to mantle conditions?

If a high speed rock friction apparatus will be developed and how it will be different from what already exists

How will we develop new apparatus and implement them to making really new experimental discoveries while maintaining a competitive research and teaching schedule.

How to carry out more realistic lab experiments

The data quality we have gotten

In order to attack important problems, do we need to design and build new apparatus?

How can we develop and implement them while allowing individuals to maintain a competitive research and teaching schedule?

Future of Existing Labs

Future of our field, including who is likely to retire in the next 5-10 years, who will continue for another 20 and who is just now getting started.

How to ensure continued support for existing labs through forthcoming retirements

Survival of the science in the U.S./aging cadre

Status and perspectives of existing labs

Is it important to keep all the existing rock deformation labs working, and how that will happen?

Does the aging and forthcoming retirement of many present workers represent a threat or an opportunity?

How can we make it an opportunity and not a threat?

Opportunities for Young Scientists

What are the job opportunities of young scientists?

Expanding the opportunities in academia for potential new faculty hires

The prospects for training future personnel, and the employment opportunities for existing and future personnel

What can be done to try to encourage universities to hire faculty with interests in experimental rock deformation? Might this be an outcome of successfully accomplishing the creation of collaboratories?
How can we help new researchers begin?
What are the job opportunities for young scientists?
How can we help young scientists get established?

Connections to Other Communities

How can we, the rock mechanics community, increase our visibility in Earth science?
How to enhance visibility of rock mechanics research
Improving the visibility of our science
How can we improve the visibility, usefulness, and applicability of our research product to other Earth and materials scientists?
Strategy for profiling rock deformation as a key source of data
Can simplified versions of our experiements be developped into school activities, thereby improve outreach and the public image of rock deformation?
Update and communicate to people what is currently happening in the field, particularly with EarthScope
We are often giving the impression that we are tackling the same old topics for the last ~40 years. What is really new and exciting in experimental rock mechanics?

Do field geologists really need us? Are they really interested in our projects?
How are we going to reconcile our experimental observations with field-based work?
How to make the results readily understandable and usable by modellers and field geophysicists
How to use the data we have gotten to deal with scientific problems.
How will we integrate our science with other geoscience areas, including mechanics theory, mechanics-oriented structural geology, tectonics, and geophysics observations?
What are the topics that need to be investigated in the near future (what is wanted by other disciplines, what data in missing in literature)?
Integration of rock mechanics/physics research with other earth science disciplines in research and studies
Will anyone understand the journal papers we write today?
Do we need to do Public Relations to increase our visibility, and if so how?
Do we need to do something to increase the usefulness of our results for other communities, and if so what?

Funding

Is there a perception that funding is more difficult to obtain now than previously?
What should be the relation between state and industry sources of funding? What are the key scientific questions that industry might be persuaded to support because there is also an economic return to be had?
How to increase the NSF funding on rock deformation?
How can we increase the level of funding to the discipline in order to support collaboratories and other more traditional means of individual investigator research at the desired level?
Is the present level adequate?

If not, how might it be increased?

Organizational Structures, Including Non USA Issues

The A-D proposals from section 5
How much fundamental research can be conducted in partnership with industry?
Establishing a strictly experimental center could cut us off from important interactions of a healthy science.
Building up lab networks
Cooperation between different lab, and different country
How can effective cooperation be built out (does it need to be build out)?
How to better utilize the existing facilities?
What cooperative ventures can be instituted for improving access and use of present laboraties? What organizations and ground rules for equipment use would need to be in place so that democratic access was established? How can we avoid organizations controlled by an "old boy" (or "all boy") network?
How can we create a collaboratory environment/atmosphere that allows people more free access to others' unique laboratory facilities while at the same time allowing the laboratory owner maximum use of their own lab and does not force them to give away competitive advantage?
Should/can we use the COMPRES framework as a means to create collaboratories?
How to develop teaching and research in experimental rock deformation in the US for the next 10-20 years?
Could a MUCH smaller Earthscope, or CUHASI sort of program be set up, with several national centers of experimental teaching and research; with techs, (and teaching faculty willing to put up with) teaching grads, and faculty; summer seminars, etc ?
If there are some propositions with a large scientific support, then what are the experimental specific requirements :
- Do we need a unique device that could be used by a lot of researchers coming from other labs and with projects that are selected by scientific committee?
- Or do the experiments are best run in various areas each one benefiting of specific neighboring facilities or capabilities?

Consequently what is the best organization? Depending on the answers to the preceding questions this may extend from a very centralized approach (eg: creation of a new lab) to a wide free network (eg: sharing results from multiple experimental approaches). Science requirements must constrain the choice.
A careful analysis of the advantages and the drawbacks of each choice is required knowing that a unique location project is often more understandable by people within one country but that wide network may also be successfully defended if it implies several countries
Collaboration between US and European scientists
How might we include other countries in whatever steps are taken in the USA?
What sort of collaboration between these other countries can be encouraged regardless of what happens in the USA?
Financial support for overseas undergraduates and postdocs to attend your workshop

In order to do new and better science and use existing facilities most efficiently, do we need to increase the amount of cooperation between scientists, labs and countries, or is the status quo fine?

If we do, what is the best way to do this?

Might altering the way we work together solve any of the other problems such as increasing visibility, helping young scientists, increasing funding?

Actions to Consider

Update colleagues on current status of active laboratories

New facility planning?

Do we need (another) white paper that demonstrates the uses of and need for laboratory studies of rock mechanics?

If we want to use the COMPRES framework, how do we proceed in a practical fashion to make it happen?

What should come out of this workshop?

What should be next?