SOLID EARTH SCIENCES — PLANNING AND PRIORITIES FOR THE 21ST CENTURY

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As population increases and shifts in population centers occur, knowledge of local and regional geological settings as well as the capacity to predict the impact of intense utilization of space and natural resources, especially water, will be essential. In order to address these challenges the geological community must become more knowledgeable about the details of local geology and the processes that guided the evolution of highly utilized sites. As people move into new spaces and increase utilization of resources, social, political and economic elements that come into play may require extensive and precise geologic knowledge. The Playa Vista development in Los Angeles and the Yucca Mountain nuclear waste repository are good examples of the types of projects that the geoscience community should prepare to address. Acquisition of the information that provides insights sufficient to resolve emerging issues requires good fieldwork coupled with clear understanding of the geologic evolution. Development of field skills and appreciation of complex geologic processes should be given high priority during geologic training. Initiatives such as EarthScope provide an opportunity to link surface geology to deep processes that result in crustal responses. Regional kinematics provide insight into the response and record of crust to deep, driving processes. Linking regional crustal structures to deep processes may provide insight into flow and thermal properties of the lithosphere. Transforms and associated crustal extension appear to provide a record of crustal response sensitive to changes in plate motions as well as differences in the thermo-mechanical properties of adjacent plates.

BALANCING EARTH SCIENCE RESEARCH PRIORITIES FOR URBAN GEOLOGY, ACTIVE TECTONICS, AND EARTHQUAKE GEOLOGY

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Initiatives in Solid Earth Science come and go, but our pursuit of basic knowledge and meaningful application persists. The balancing act in which we are engaged stretches between multi- and single-investigator efforts, basic and applied research, and physical science and natural history. Planning should balance large, multi-investigator initiatives (necessary for solving complex problems) with the creative, single investigator-based approach that has been a mainstay of the research enterprise. Fundamental progress in understanding Earth process requires a deductive combination of deterministic mechanically based models and observations necessary to test such models. Such a Physical Science approach requires that Earth Science education be quantitative and rigorous with foundation courses in physics, chemistry, calculus, mechanics, statistics, geometry, linear algebra, etc. However, traditional descriptions of regions provide important case histories and illustrate that certain histories and events are possible.

A few exciting decade-scale research priorities are:

- Urban geology and geomorphology (e.g., Haff, 2002, *EOS Trans. AGU*, **83**, #29).
- Here we combine basic research on urban ecology with applied research on hazards, growth, impacts, and feedbacks within and adjacent to the natural laboratories of expanding cities.
- The distribution of deformation in space and time will remain a central research topic. Do deformation rates match over different time scales (e.g., seismological, geodetic, paleoseismological, and geological)? Should they? We can learn much about the earthquake cycle and the role of faulting in continental deformation with rate studies. What is fault strength and how is it controlled and how does it develop? How is permanent deformation accumulated in a setting of rapid transient deformation around major active structures?
- Earthquake geology provides important information about the activity of faults (often through tectonic geomorphology), and the timing, extent, and magnitude of paleoearthquakes. These data are highly complementary to short time scale geodetic and seismological studies and to longer time scale geological studies. In addition, such data are directly applied in earthquake hazard studies. Long chronologies of rupture are being developed and they provide important data for testing earthquake and fault interaction models. A challenging area of research is on slow active faults. How can they be identified, are there differences in the mechanisms of accommodation of regional deformation and their sensitivity to local versus regional sources of stress, what are the best probabilistic approaches for forecasting their behavior?

Data priorities include high-resolution topography, imagery data sets, vector coverage of diverse Earth data sets (including geological maps), all rectified and projected to the same datum. Compilation of digital data and tool development are underway with GEON (www.geongrid.org/) and other projects in Geoinformatics (www.geoinformaticsnetwork.org). They will show the way, but the entire culture of Earth Science research will have to move in the direction that such tools are regularly used, and the data fusion proves fruitful. Again, we must balance our enthusiasm for these new tools and data with the research questions at hand. Facilities priorities include virtual laboratories for coordinated ^{14}C, cosmogenic radionuclide, and Optically Stimulated Luminescence geochronology; determining rates of geological processes remains a central research effort.

Balancing Teaching and Research in an Undergraduate Geology Department

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A significant challenge in an undergraduate geology department is balancing teaching and research. The Geology Department at the College of William & Mary consists of five full-time faculty and the normal teaching load involves two courses and one lab per semester. In a typical semester, I teach an introductory course with up to 200 students and an intermediate- to upper-level majors course with a lab. As an undergraduate department, we have no formal teaching assistants and faculty are responsible for all the grading. The department graduates approximately twenty majors per year and all seniors complete a year-long independent research project. I advise between three and six senior research students per year and commonly work with those students during the summer.

William & Mary students are excited about conducting independent research. However, the scope of individual projects must be 1) approachable for an undergraduate with perhaps less than two years of geology coursework and 2) discrete such that students feel a sense of ownership in the project (rather than simply being research assistant with no vested interest). Between one-third and one-half of William & Mary geology students present the results of their research at professional meetings.

I attempt to incorporate the results of my research into all of my courses. In the introductory course, first- and second-year students can see that research is done at William & Mary and also that student are conducting much of the research. The research opportunities offered in geology have proven to be a draw in attracting geology majors. During the academic year, teaching consumes large blocks of time and I commonly feel as though I am conducting research vicariously through my students. I am fortunate to be at William & Mary as I always have research students working on projects.

There are still frustrations trying to balance teaching and research. Many William & Mary students continue on to graduate school, but very few see their research through to publication. I have an ever-growing list of abstracts with student first authors, but the peer-reviewed publications resulting from that research have not kept the same pace. I am frustrated because there is an obligation to disseminate the results of new research, but during the school year delivering high-quality courses is a priority and in the summer, new research with students is key.

A priority for the Solid Earth Sciences should be to support undergraduate research. This research not only induces more independence in graduate students, but can also make significant contributions towards understanding Earth processes and Earth history. NSF-sponsored fellowships that enable faculty in undergraduate departments to collaborate with colleagues at research institutions and provide time away from teaching may help to get the results of these projects to a broader audience.

Research Opportunities in Solid Earth Geochronology, and Implications for Education at Urban Institutions

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Geochronologic research in Solid Earth Geosciences is being revolutionized through access to ion microprobes capable of elemental and isotopic analyses at ppm level, in much the same way that mineralogical analyses were revolutionized in the last century by access to electron microprobes capable of elemental analyses at ppt level. Ion microprobes provide hands-on, non-destructive geochronology using the U-Th-Pb system and U-series equilibrium, effective over a time range from a few tens of thousands to billions of years. Advances in ion microprobe techniques are fully compatible with, and indeed dependent upon, continued availability of high precision TIMS analyses, because the analytical volume utilized by ion microprobes is several thousand times less than a typical TIMS analysis, but the resulting analysis is typically about an order of magnitude less precise. Therefore, maximum advances in Solid Earth Geochronology will be made through coupled advances in ion microprobe and TIMS techniques and availability.

The benefits to research in the Solid Earth Sciences of coupling ion microprobe and TIMS geochronology are enormous. As ion microprobe techniques have become available, it is becoming possible to quantitatively address existing uncertainties in models of lithosphere formation and recycling (Barth *et al*., 2000, *Tectonics*, **19**, 616-629), as a result of the ability to unravel the complexities of chronologically important polygenetic crystals or populations of crystals (e.g., zircon, monazite, sphene). This capability has further emphasized that polygenetic rocks constitute the majority in the lithosphere (Bindeman *et al*., 2001, *Earth and Planetary Science Letters*, **189**, 197-206). It is also becoming apparent that reasonable estimates of the ages of individual rock units in the U.S. Cordillera, for example, are in error in many cases by tens to hundreds of millions of years (Jacobson *et al*., 2000, *Geology*, **28**, 219-222; Barth *et al*., 2001, *Journal of Geology*, **109**, 319-327), with obvious consequences for existing geodynamic models.

The potential educational benefits of these advancing geochronologic techniques are also substantial. At urban institutions, we are primarily training students as environmental geologists and environmental scientists, professionals who will be expected to deal with the dynamics of small- to large-scale Earth processes, generally over short to intermediate time scales. Unfortunately, most of our students have little or no intuitive feel for Earth dynamics or the rates and scales of Earth processes. Solid Earth Geochronology provides the framework for construction of the geologic time scale and for understanding the rates and scales of dynamic processes within or involving the lithosphere. Because this knowledge base, in turn, provides the proving ground for techniques applicable to short time scale Earth processes, it also provides the basis for educating students in the Earth and Environmental Sciences. Geochronologic research using ion microprobe techniques, in particular when coupled with SEM and/or electron probe imaging and microanalysis of minerals, provides a visual and highly interactive means to convey fundamental ideas about the rates and scales of Earth processes and the dynamics of lithosphere formation and recycling.

Areas in Solid Earth Sciences in Which I Can Make a Significant Impact

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As a structural geologist by training and an instructor in a teaching-oriented two-year college by vocation, I have the unique opportunity to blend cutting-edge Solid Earth Science research with recent findings in the fields of cognitive learning. I am in a position to directly influence K-14 Earth Science Education through collaborative partnerships with local high schools, developing workshops and short courses for pre-service and in-service teachers and exposing high school students to available career opportunities in Earth Science.

My present and future scholarly interests can be broadly categorized under the following topics:

Research in Earth Science Education

- Development of simple physical experiments and models to illustrate basic concepts of geology in classrooms.
- Web-based interactive educational materials in geology.
- Use of student research projects as part of geology courses.
- Undergraduate research programs to enhance career competency in geosciences.
- Programs and workshops for pre-service and in-service Earth Science teachers.

Research in Geology

- Processes involved in evolution of continental lithosphere.
- Rheology and mechanisms of shear localization within the lower crust and their tectonic implications.
- Three-dimensional geometry of ductile shear zones.
- Quantification of three-dimensional strain.
- Effects of dynamic recrystallization on deformation-induced fabrics.
- Metamorphism and fluid flow within the lower crust.
- Experimental modeling with rock analog materials to simulate tectonic processes.

The Origin and Evolution of Continental Lithosphere from study of Ancient Orogens: Old Questions and New Priorities

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Although the only record of pre-Mesozoic plate tectonics is contained within the continents, fundamental questions still surround the processes of continental lithospheric evolution and the long-term geochemical evolution of the crust/mantle system. For example: Are continents best viewed as an orogenic collage of fragments that have survived or as a reasonably robust record of all crust that has formed? Has there been secular change in the processes of formation and thickness of lithospheric keels beneath continents? Are Archean and Proterozoic lithospheres fundamentally different? Does lithospheric mantle form by stacking of underthrust oceanic lithosphere and plateaus, by advective thickening during collisional orogenesis or other processes? How is either process reconciled with strongly segmented lithosphere that is reactivated a billion or more years later? Are significant volumes of continental lithosphere or crust recycled into the mantle? Continental crust that has been metamorphosed at mantle depths and then exhumed continues to be recognized in orogenic belts but we do not know whether the exhumed crust is but a small percentage of what was subducted. What are the time scales for achievement of "cratonic geotherms" and how do small differences in the distribution of heat-producing elements affect geotherm evolution? Many large Precambrian orogenic belts are perforated by enigmatic suites of granitoids tens to hundreds of million years younger than crustal assembly. Lithospheric delamination is often called upon to explain lower-crustal heating but the long-term isostatic consequences of delamination *a*nd *t*he spatial and temporal distribution of the granitoids are generally not considered.

These problems can best be addressed by an integrated collaborative approach that includes systematic mapping of continental lower crust and mantle using seismic tools and detailed geological mapping coupled with geochemistry and high-precision thermochronology and geochronology. Ancient orogens allow investigation of the thermal and mechanical evolution of the crust over as much as 30 km of structural relief. *P-T-t* studies of deep crustal and mantle rocks recovered as xenoliths in volcanic rocks will provide an important and unique link to the history preserved in the shallow crust and allow for much more comprehensive geodynamical modeling of the thermal and mechanical history of continental lithosphere. In my opinion, many of these still fundamental questions have been neglected if not abandoned. These questions should be at the core of any review of new initiatives in Tectonics and Earth history.

BROAD INITIATIVES IN TECTONICS

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Tectonics is arguably one of the most successful areas of research funded in NSF EAR. No other discipline can claim to span nearly all funding programs within EAR. New opportunities at NSF make it essential for us to capitalize on these successes. In particular, it is important for NSF to provide broader support for integrated tectonics research, involving geologic and geophysical field studies, and related experimental, lab, and geodynamic studies. Three suggestions are offered:

1) The Tectonics program in NSF should be reorganized with an expanded budget to include a broader range of lithospheric studies, including both geology and geophysics. This reorganization should include a strong emphasis on research that integrates field and lab studies.

2) Tectonics needs to initiate an annual workshop to help bring more cohesiveness to the discipline. At present, the United States is the only first world country that I know of that does not have a national level Tectonics Study Group.

3) The tectonics community needs to identify two or three research initiatives that could be pitched for large-scale NSF support as national research centers. In the past, many in the community have objected to new initiatives because of a fear that their piece of the "funding pie" would be smaller as a result. I maintain that the tectonics research budget will shrink even faster if we do not step forward with new ideas. Special initiatives will increase the visibility of our research, attract a broader range of talent and new opportunities as well.

THE UNIQUE CONTRIBUTION OF METAMORPHIC STUDIES: QUO VADIMUS?

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Applied metamorphic petrology plays a unique role in tectonic studies because the tools of the petrologist enable increasingly precise (and generally accurate) quantification of the pressure and temperature of equilibration of crystalline rocks throughout an orogenic cycle. Knowing *P* and *T*, and the composition of any fluid attending metamorphism, contributes to understanding continental evolution and differentiation, crust-mantle interactions, crust-mantle coupling, mantle processes, lithosphere processes and lithosphere interactions (particularly subduction and collision). Combining the tools of the metamorphic petrologist with those of the geochronologist yields information about rates (*P-T-t* paths, *T-t* curves), providing critical information about prograde metamorphism (e.g., rates of burial and heating) and timing of peak metamorphism, distinguishing among processes of exhumation and identifying possible overprinting orogenic cycles, all of which place constraints on tectonic models. The critical last step in metamorphic studies has been to link the *P-T-t* evolution to deformation studies, so that a complete *P-T-t*-d history may be established for specific areas in terranes and orogens of different eras. Such information contributes to understanding lower crustal processes and crust-mantle (de-)coupling, and places constraints on models of mantle geodynamics.

Excitement in petrology is focused on the extremes. In ultrahigh-pressure metamorphism (UHPM), the key scientific questions are: How deep may upper continental lithosphere be subducted? What volume of upper continental lithosphere has been subducted to these depths? What is the fate of continental lithosphere subducted to these depths, and by what process is exhumation of the upper continental lithosphere accomplished? How far back in Earth history is subduction of upper continental lithosphere to such depths recorded in orogens? Answers to these questions will give us fundamental information about how Earth works. Another pressing line of research is quantifying the extent of extreme thermal perturbations in upper continental lithosphere, as recorded by ultrahigh-temperature metamorphism (UHTM). Locations where UHTM granulites record temperatures >1100°C are increasingly recognized and raise the intriguing possibility that large tracts of 'common' granulite might represent retrogressed UHTM rock that have back-reacted with melt retained from the prograde evolution. Between UHPM and UHTM is the realm of high-pressure granulite metamorphism (HPGM), which is characterized by Ky-Kfs in metapelites and felsic compositions, and by Grt-Cpx-Pl in mafic compositions. The very hot (UPGM) to extreme (UHTM) thermal conditions implied by estimates of the maximum temperatures of crustal metamorphism require us to rethink the source of the heat. In high-*T* crustal metamorphism, the key scientific questions are: What volume of upper continental lithosphere has been exposed to UHTM/HPGM conditions? In what tectonic setting were such thermal anomalies generated (e.g., subduction-collision orogens?), and what was the role of the asthenosphere in achieving these conditions (e.g., through lithosphere delamination and/or slab breakoff?)? How far back in Earth history are UHTM/HPGM conditions recorded in orogens?

Finally, average crust that achieves granulite facies metamorphic conditions will partially melt, which raises the question of interactions between mantle-derived and crust-derived melts in generating the petrologic diversity of igneous rock suites.

It follows from arguments presented above that understanding UHPM/HPGM/UHTM is a critical issue in Solid Earth Science, and a key priority if we are to understand geodynamics throughout Earth history.

SUPPORT FOR A WORKSHOP: PRIORITIES IN SOLID EARTH SCIENCES

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The Solid Earth Sciences concern the characterization, origin, and evolution of planetary lithospheres. Various processes have modified the lithosphere throughout geologic time. Investigating them requires studies of not only active environments but also the geologic record of ancient events. Research in the Solid Earth Sciences is inherently multidisciplinary and increasingly interdisciplinary. This approach characterizes not only larger projects supporting multiple scientists (e.g., Continental Dynamics and Instrumentation & Facilities Programs at NSF) but also most of the smaller projects within the Solid Earth Sciences (e.g., Petrology & Geochemistry and Tectonics Programs at NSF). The Solid Earth Sciences represent an essential element at the core of Earth Sciences; educational programs should reflect the inherently multidisciplinary and increasing interdisciplinary nature of the Solid Earth Sciences, and the foundation the Solid Earth Sciences provide for the other elements of Earth Science. We contend that any new long-range plans for Earth Sciences at NSF must advocate continued and readily identifiable support for research, education and outreach in the Solid Earth Sciences. The record shows that advances in Solid Earth Sciences are achieved when research support is provided through small grants to individual PIs (e.g., Geophysics, Petrology & Geochemistry and Tectonics Programs), through support of infrastructure (e.g., Instrumentation and Facilities Program), through large grants to several PIs (e.g., Continental Dynamics Program) and through integration among the sub-disciplines (e.g., the Plate Tectonics revolution). Our agenda for the Solid Earth Sciences will complement the EarthScope initiative, but it is not dependent upon EarthScope being funded. This request is for support to enable attendance by 50-100 participants (minimum 50, maximum 100) representative of the spectrum of Solid Earth Sciences and sympathetic to the symbiotic relationship between research, education and outreach. The participants will be selected from a pool composed of invited and self-motivated applicants to ensure balance, both across the Solid Earth Sciences and among research, education and outreach. The primary goal of the Workshop is to promote an integrated and coherent approach to planning for the future of the Solid Earth Sciences. The integration will be both across the sub-disciplines in the Solid Earth Sciences and among the synergistic activities of research, education and outreach. The outcomes of the Workshop will be: a report to be submitted to the Division of Earth Sciences by December 1, 2002, and customized versions to be submitted to *Eos*, *Geotimes* and *GSA Today* during the month of December, 2002; and, formation of an ***Integrated Tectonics Forum*** to continue the process.

INFRASTRUCTURE NEEDS IN GEOCHEMISTRY

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The NSF Instrumentation and Facilities Program has dramatically helped upgrade the instrumentation used for geochemical, geochronological and isotopic analysis in the United States over the last 20 years. Although some regional analytical centers were created for particularly expensive techniques (e.g., ion-probe, AMS), most of the instrumentation has been distributed widely to individual investigators. As a result, the analytical geochemistry capability in the U.S. is modern and well developed, but is distributed without much, if any, central organization. This approach to geochemical instrumentation is beneficial for student training in that it allows students to become actively involved in serving their own analytical needs. Furthermore, the availability of state-of-the-art instrumentation to a wide-range of investigators has proven extremely beneficial in the development of new techniques that otherwise may not have been recognized for their important potential contributions to the science objectives of the field. Given the success of this instrumentation funding approach, there appears to be no clear consensus in the geochemical community that moving towards more centralized analytical facilities would offer dramatic improvements either in productivity or creativity in geochemistry or in the efficiency of use of this sometimes expensive instrumentation. Some form of identified organization, accompanied by additional technical support, could make this “distributed” analytical facility more obviously available to serve the needs of the broader Earth Science community, but the capability of existing geochemical equipment, for the most part, is sufficient to meet foreseeable demand.

Where coordination of activity in geochemistry is needed is in the areas of electronic data archiving and the development of sample repositories to archive, and make available for additional study, the samples collected by federally funded research projects. The former issue has been admirably addressed by the Ocean Sciences community in creating the PetDB database for geochemical analyses of MORB. The Geoinformatics initiative and other ITR-funded efforts, such as the NAVDAT database for western U.S. Cenozoic volcanism, offer a possible path towards similar databases for on-land geochemistry. In my opinion, however, there remains no clearly articulated input from the geochemical community regarding development and maintenance of a long-term "facility" that will efficiently serve this community’s needs for data and sample archiving. Developing the necessary information technology approaches for geochemical databases is one issue, but another critical issue that has not been well addressed is that data and sample archives require continual inspection and updating in order to stay useful. The piece-meal nature of most current geochemical databases (with PetDB and GEOROC as notable exceptions) is clear testimony to the fact that such efforts are unlikely to be done well, and comprehensively, by individual investigators whose primary responsibility is producing new data, not archiving old data. Efficient and effective geochemical databases require a combination of long-term support and oversight by individuals, a group, or organization whose primary attention is devoted to ensuring that such databases continually evolve to best serve the needs of the geochemical/geological community. Expecting this service to be provided by professional organizations such as GSA, AGU or the Geochemical Society is unrealistic. In my opinion, this need is best served by an organization representing active research geochemists whose size, structure, governance, and duties, need to be defined by the geochemical/geological community in sufficient detail as to produce a competitive support proposal to potential federal funding agencies.

KINETICS OF REACTION AND DEFORMATION: RATES AND SCALES OF CHEMICAL AND TEXTURAL EQUILIBRATION IN ANCIENT OROGENS

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Much of the tectonometamorphic history of ancient orogens is written in the form of mineral assemblages, compositions, and fabrics, and the most valuable information comes from rocks that contain a sequential record of successive stages of partial re-equilibration to changing physical conditions: temperature, pressure, fluids, and stress. Quantitative understanding of the mechanisms and kinetics of subsolidus crystallization and recrystallization — the fundamental processes by which rocks are transformed during their passage through the crustal cycles — is therefore vital to correct and meaningful interpretation of the geologic evolution of orogens.

A decade of recent work has begun to revolutionize the vision of Earth's interior held by metamorphic and structural petrologists: instead of imagining that deep rocks progress through a set of fixed, well-equilibrated thermodynamic states, we now must conceive of them as highly dynamic systems, continually struggling to approach equilibrium, but rarely achieving it fully. This novel viewpoint holds profound implications for the ways in which geologists regard deep-earth processes, and even for our methods for studying those processes.

At (sub-)crystalline length scales, different physical and chemical subsystems respond to changes in their environments over markedly different timescales, thus preserving, by virtue of distinctly different rates and scales of chemical and textural equilibration, a wealth of historical information on the physical environments through which a rock may have passed. For instance, intracrystalline diffusion of divalent cations in garnet at temperatures near 700°C over geological timescales commonly homogenizes crystals with respect to divalent-cation concentrations (recording the persistence of such temperatures for significant time), but leaves largely unmodified the distribution of higher-valence components, which can preserve their distributions during crystal growth, or even — when intergranular diffusion of those species is ineffective — their original dispositions prior to crystal growth. Another example would be the observation that the dimensional preferred orientation of plagioclase crystals may re-adjust comparatively rapidly to applied stresses, aligning with predominant foliation surfaces, whereas those same crystals may contain within their interiors a chemical differentiation that preserves the directionality of an earlier foliation surface detectable nowhere else in the rock. In both cases — and in a host of others — the orogenic history is revealed by the difference between the kinetics of distinct processes fundamental to the rock's chemical and textural re-equilibration.

An initiative aimed at precise and accurate quantification of the rates of such fundamental reaction mechanisms under a variety of crustal conditions has the potential to markedly expand the types, kinds and amounts of tectonometamorphic information that can be gleaned from rocks. Experimental, observational and analytical approaches to these problems should be combined. Immediate targets should include: (a) the kinetics of intracrystalline diffusion of major and especially trace elements in a variety of common phases; (b) the kinetics of intergranular diffusion of major and trace elements, especially as functions of variable fluid chemistry; (c) the kinetics of deformation, especially at low temperatures and at low strain rates; (d) the kinetics of dissolution and reprecipitation reactions, especially in the presence of supercritical fluids of variable chemistry; and, (e) the kinetics of nucleation, especially in polyphase assemblages. The interplay of these mechanisms, and interactions among them, should constitute a major second phase of such an initiative, for in that realm lies the greatest potential for geologic revelations.

ACTIVE MARGINS – AN ARC PERSPECTIVE

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Processes dominant in arcs can be broadly classified as tectonic and magmatic – determining the spatial and temporal scales over which these processes interact and influence each other underlies most Solid Earth pursuits at active margins. Advances in geophysical observational techniques are providing new perspectives on the spatial scales over which tectonism and magmatism interact. Major challenges for the Solid Earth Science community are (1) incorporation of these new data into the design of new research goals and (2) providing the geologic perspectives of time and Earth materials to the interpretation of geophysical data.

Some basic research questions related to processes at active margins remain unchanged despite years of research. How do arcs initiate, mature, and die? How do variations in plate convergence rates and subduction geometry affect plate coupling, stress distributions, and magmatism? How do we resolve geochemical and geophysical views of the thermal structure of convergent margins? What is the origin of arc magmas? What are the relative roles of steady vs. transient deformation in the generation and transport of magmas? What is the relationship between input into crustal magma storage regions and output as volcanism? What are the temporal and spatial relationships between magmatism, volcanism and ore deposit formation? How do surface processes (glaciation, sea level change, large-scale landslides) affect volcanism and tectonism? Answers to these questions require integration of all Solid Earth disciplines and incorporation of Earth Science objectives into EarthScope initiatives, while research progress on these questions will, in turn, feed back into the design of geophysical experiments.

Research questions involving tectonic and magmatic processes at relatively shallow depths and short time scales have already evolved as the result of new technologies. Identification of slow and fast earthquakes, stress-triggering of one earthquake by another, simultaneous along-arc seismic (and volcanic?) activity, and earthquake-triggered melt injections to magma reservoirs raise new questions about the temporal and spatial scales over which transient deformations may act and their potential to affect magma transport. New isotopic analyses are constraining time scales of melt formation and transport; new analytical techniques permit mineral-scale evaluation of volatile, trace element and isotope distribution; improved monitoring techniques permit early detection of melt migration by identification of deep long-period earthquakes and InSAR imaging; broadband seismic instruments provide tomographic images of magmatic systems; and, satellite observations yield real time measurements of thermal and volatile emissions. Thus, two decades of monitoring data have transformed our views of arc volcanism and associated hazards, while raising new questions related to the interpretation of these data, particularly from the perspective of hazard monitoring. The challenge for Solid Earth Sciences is to integrate geophysical, remote sensing and geologic data into a comprehensive picture of active margins that both extends existing models in time and space and applies that knowledge to human pursuits.

IMPORTANCE OF FIELD-ORIENTED COMPARATIVE RESEARCH IN EXTENSIONAL TECTONICS

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The southern Basins and Ranges and western Turkey are two of the best examples of large continental extensions. It is generally accepted that the Basins and Ranges extension was initiated by the formation of the San Andreas fault zone about 30 Ma ago. There are three proposed models of extension in western Turkey: 1) tectonic escape/lateral extrusion; 2) back-arc spreading/subduction roll back; and, 3) orogenic collapse. The two regions exhibit remarkable similarities and some unique differences. They both contain structural features unique to extension, such as metamorphic core complexes, extensional folds, turtleback faults, shear zones, detachment surfaces, and extensional basins with syn-extensional sedimentary rocks. However, the origin of extension is different.

Since 1999, I have been working on comparing extensional tectonics in the two regions with my colleagues in order to better understand the processes and products of extensional tectonics. One outcome of this work is the discovery of the presence of turtleback faults in western Turkey, which were considered unique to the Death Valley region of the southern Basins and Ranges. We have also discovered that what was previously interpreted as late Miocene contractional folds in western Turkey were actually extension perpendicular folds, such as rollover anticlines and associated structures.

Interesting differences can be found between the two regions. For example, western Turkey has large extension parallel folds that are tens of kms in length and more than one km in amplitude but the southern Basin and Ranges do not. On the other hand, western Turkey does not have a well-pronounced main breakaway zone that is well observed along the western edge of the Colorado plateau in the Basins and Ranges.

This research suggests that it is important to conduct field-oriented geological studies in different geographic regions that were affected by extensional tectonics in order to better understand the response of the upper crust to processes of extensional tectonics. This type of research may actually lead us to better classify the extended terranes based on the products that may be controlled by the amount and processes of extension. The outcome may shed new light on the long-standing question of extensional tectonics; under which conditions crustal extension is related to rolling hinge and which to deeply rooted normal faults.

GEOANTIQUITIES – PARTNERSHIPS FOR COMMUNITY AND GEOCONSERVATION

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A vital key to ensure the future of the Solid Earth Sciences is an interested and informed citizenry. Efforts to provide innovation and change should address the following:

1. More **relevance** of our science to society through building of **partnerships**, providing opportunities for non-Earth Scientist individuals and groups to be involved.
2. More **involvement** of Earth Scientists in politics, with infrastructures and support from funding agencies and professional societies.
3. More **outreach** with continued efforts on education K-12 and college, and emphasis on "recruiting" students to major in the Earth Sciences.
4. More **integration** of Earth Science with other disciplines Examination of more degree options and/or research collaborations utilizing Earth Science (combining with law, journalism, natural or science philosophy, urban planning, GIS, etc.)

Earth Science is relevant in rapidly growing urban communities. **Partnerships promote an informed citizenry and can help a community realize what Earth Science is about and why it is important.** The concept of "geoantiquities" raises the conscientiousness of geoconservation, and encourages the involvement of the community. A geoantiquity is a natural record of Earth history that documents environmental change at local, regional, and global scales. These records commonly are natural landscapes that preserve material evidence of geologically recent surface processes and environments. However, especially in regions undergoing rapid rural-to-urban land conversion, many geoantiquities are in danger of being permanently lost. This concept of geoantiquities is consistent with international trends in geoconservation, and parallels the well-established cultural antiquities management for preservation of prehistoric human artifacts. Although geoconservation is a strong movement in Europe, there is little heard of it in the U.S. outside of our park systems. In geoantiquity research, we met and partnered with national, state, county, and special interest groups. We provided a mechanism for individual citizens to be participants that then became educated and informed. In a rural town we made a seemingly small yet very significant contribution to save an important geoantiquity through meetings with the public and testimonies to the planning commissions.

Although some of these efforts are long-term and may not bear immediate fruit, it is partnerships like these that people will remember. Partnerships help build community and link the public to Earth Science and also serve as a part of outreach. The more this is done to truly integrate Earth Science into society, the more we can help ensure the relevance and future of our science.

SETTING PRIORITIES IN SOLID EARTH SCIENCES

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Community colleges hold a strategic position to build foundation math and science skills and attract students into the Earth Sciences. This essay addresses the importance of community college involvement in science and technology teaching and identifies some of the obstacles that need to be addressed. There are opportunities for community colleges involving education, community-based projects and research. This includes the strong relationships between community colleges and local school districts, community organizations (which also include state and federal agencies) and articulation agreements with institutes of higher education.

The challenges faced at this level are many. There is a general lack of coordinated emphasis between science teaching at the high school and community colleges. The Earth Sciences are not generally regarded as college-track, honors-level subjects in the same manor as chemistry and physics. Despite an overall need for more development of science and math skills at the introductory level, there is also a need for stronger, more cohesive articulation between 2- and 4-year schools. Community colleges are currently involved in "bridging the gap" activities geared towards providing education and community programs that target underserved communities and strengthen the foundational skills needed to pursue higher degrees in the Earth Sciences. The integration of technology and community-based research projects are also challenging given the level of preparation for the sciences by most community college students and the short time that students attend community colleges.

The educational program that I am developing at Temple College addresses a number of these issues. I teach courses in geographic information systems and physical and human geography. Theses courses are integrated and multidisciplinary, combining the use of spatial technology in teaching geology, physical geography, environmental science and archeology/ anthropology. Projects related to these courses join local, state and federal agencies (USDA Natural Resources Conservation Service, Texas State Soil and Water Conservation Board and local governments) and field data collection and analysis. Current programs are being developed to integrate USDA GIS applications and field methods as part of a suite of crosscutting Earth Science labs and lectures. Curriculum enhancements also include the use of GIS to explore and analyze Earth Science topics and tools and techniques adapted from NSF-sponsored geoscience education workshops.

Challenges that may affect the success of this program span the range of local/higher education. Stronger foundational skills need to be developed at the high schools level with active involvement and mentorship from colleges and universities. Students of Earth Sciences need to be able to articulate their work and success to fulfill higher education degrees. Finally, opportunities to conduct research at the community level need to be integrated into programs that can support learning throughout the K-18 educational curriculum. Participation in this workshop will enable me to be better prepared and have a strong network of colleagues to address these challenges.

THE DANGERS OF PROACTIVE RESEARCH PROGRAMS IN BASIC SCIENCE

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The progress and advance of basic science has many facets and components. Big advances result from instrumental breakthroughs that allow new measurements (e.g., mass spectrometers), calculations and the analysis of massive data sets (high speed computers). Communication via the Web and email has enhanced the rapid dissemination of information and ideas. Large international programs of data collection (e.g., DSDP) have generated a wealth of data available for people to think about and analyze. Large paradigm shifts (plate tectonics) seem to be developed in the collective consciousness at a time when a lot of data 'falls into place' following a short period of awakening, but it needs a few clever and original lateral thinkers (e.g., Tuzo Wilson, Dan McKenzie) for a theory to be triggered and formulated. Science advances principally by people with clever ideas and enthusiasm being allowed to do the research that they find exciting and wish to do. This research may lead to incremental or major advances but the immediate or long-term value to either science or Society cannot be predicted. In basic science, predictive questions about value, wealth creation, and societal needs are irrelevant and impossible to answer. It is inherently no more important to study Franciscan blueschists than the Cenozoic evolution of crocodiles. Granite fabrics, mantle convection, rivers, the core, and the alkali feldspars are all legitimate and important topics in basic geology, as indeed is anything else of which one can think. Proactive attempts to design the future of geology will distort the intellectual efforts of our best young people. Apart from the big international programs, science should be funded by giving support to clever people and groups who submit excellent research proposals in competition with, and judged by, their peers. Priorities benefit nobody but senior administrators and the committees who determine them. Nobody, however senior, distinguished or clever, has the right or fatidical power to determine the future of geology. The only criteria for funding must be originality and excellence in the pursuit of Earth process and history, not stamp collecting. If we tailor basic geology to fashion, perceived importance, or 'initiatives' that we believe might appeal to our funding masters, we will kill it. If 'society' wishes to undertake applied research such as radwaste, urban geohazard, etc, it should fund it separately and directly, not force us to channel and distort the random walk of basic geology that underpins applied research. We should constantly promote geology, as advocated by Sharon Mosher, in the minds of the public, administrators, and lawmakers as a subject of enormous societal benefit; we have been reactive and dormant for too long.

A second serious concern is that the reality of the outcrop, the hand specimen, and thin section are being progressively strangled and diminished in favor of numerical and analog modeling, and remote sensing methods. Modeling is important for giving us ideas but has no inherent reality and cannot supercede nature. Nature may be difficult but the truth resides in the rocks not in an alterable model on a computer screen. Geological maps cannot be made from satellite data. There is now at least one Professor of Geology who has never made a geological map in the field, looked at a thin section, or worked on rocks, minerals, or fossils in the field and laboratory. If this represents the future of geology, we are in bad trouble. Although diminishing pockets exist worldwide, North America, Australia, and Southern Africa are the last bastions of serious basic geology. Let us preserve this.

Systematics of Process Feedback: An Integrated Computational, Field and Theoretical Approach to Time-Serial Behavior of Earth Processes

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Field observations of mineral and fossilized networks of pore patterns in rocks have elucidated many geologic problems. However, because of the increased need to predict the general and specific behavior of geologic systems rather than to simply understand their past behavior, computational methods are increasingly relied upon. Advances in algorithm development, computational technology, and visualization techniques required to evaluate large, complex problems have placed computational methods at the forefront of techniques to interpret and predict phenomena related to Solid Earth Sciences. Whereas these methods have been widely used in geophysics, they have been under utilized to explore important problems in mineralogy, petrology and tectonics. Computational methods intimately interwoven with field, theoretical and chemical data have potential to provide insight into Earth System Processes.

Commensurate with the rise in computational methodology is the realization that many geologic (and Earth) processes are coupled and that feedback effects exert a significant control on the outcome of these processes. For example, this is particularly well known for thermally driven fluid-rock systems. Fluids are important in nearly every crustal process; their interaction with minerals and rocks drives chemical alteration, ore deposition, global geochemical cycles, etc. Consequences of fluid-rock interactions are well understood, but the controlling mechanisms are not. Associated with understanding the feedback amongst processes are the patterns that these processes leave in the rock record. Patterns of fluid-rock interactions and mineral alteration repeat over a range of scales but their abundance varies in accord with the quality of feedback. The geometric properties of these patterns measure the interactions of the processes, yielding the time-serial behavior of the system. Typically these patterns can be replicated by simple computer codes on the basis of deterministic chaos and iterated function systems. The actual patterns can then be correlated to computational analogs of the processes responsible for their creation and the elapsed time between events determined. With these techniques, the time-serial evolution can be reconstructed with precise resolution. Spatial variations in the extent of metasomatism, degree of reorganization of pore space into fracture controlled percolation networks, and self-organization of rock textures produced by system oscillations about an equilibrium state can be elucidated. Examining and understanding the features, behavior and controlling mechanisms in coupled processes with feedback from both the computational and field perspective is an important research avenue for the future of petrology, geochemistry and tectonics. It has the potential to uncover fundamental behavior in these systems.

Earth science students must be broadly educated requiring sufficient computational and analytical facilities for students to analyze geologic problems and processes from both the field and computational perspectives. A solid foundation in mineralogy (taught from an earth systems approach), which forms planetary materials, and in the fluids that alter these materials is paramount. Additionally, our students must have the skills to effectively communicate research results to the community at large. These communication skills involve not only oral and written methods, but also techniques for visualizing and analyzing large datasets. Studies must utilize the information in rocks closely integrating it with the application of numerical methods. For it is the combined use of classical observation within the context of system behavior derived from simple algorithms that will lead to new revolutions in the Solid Earth Science.

FUTURE PRIORITIES IN HIGH TEMPERATURE GEOCHEMISTRY

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Research in high-temperature geochemistry, whether in igneous and metamorphic petrology or economic geology, has played a vital role in the understanding of the origin and evolution of the Earth. However, in many U.S. research universities, faculty in hydrogeosciences, environmental geochemistry, paleoclimate and even astrobiology are slowly replacing high-temperature geochemists. Although this turnover can be seen in some cases as a natural response to changing national research priorities and to employment opportunities, there is some risk that the trend will effectively drive high-*T* geochemistry research into extinction, or into only a few (enlightened) research universities and/or private research institutions. Given the obvious relevance of rock and mineral structure and chemical and isotopic compositions to water quality issues, paleoclimate studies, microbiology, and paleobiology, it is clear that high-temperature geochemistry must remain a viable sub-discipline in Earth Science Research. This is true even if low-temperature processes represent the main priority for U.S. geochemical research for the foreseeable future. In formulating future priorities for high-temperature geochemistry, then, the task should include not only a typical shopping list of future disciplinary research directions, but should also outline how high-*T* geochemistry can be more pointedly integrated into, for example, biogeochemical and paleoclimate studies.

High-temperature geochemists have also been remiss in integrating existing data sets into current research, due primarily to the fact that easily accessible databases have not been generally available. Efforts to compile geochemical data for Web-based querying and dissemination (GERM, PetDB, GeoRoc, NAVDAT) represent nascent efforts to remedy this situation. The development and maintenance of such databases represent an important priority for high-temperature geochemistry, primarily because the efficient manipulation of large data sets afforded by these databases can provide insights into geochemical processes that might not otherwise be obvious. Compilations of igneous rock data, for example, can provide the means to define regional, space-time patterns in igneous activity and magma sources, and provide insights into what areas and rocks should be studied in the future in order to better define those patterns. The ability to clearly identify what is known, or needs to be known, regarding the geochemistry of rocks in a given geographic area will be of particular use in integrating geochemical studies into national Earth Science research initiatives, such as EarthScope.

Development of instrumentation for geochemical research represents one other important issue for the future of high-*T* geochemistry. The problem is that many geochemists, specifically isotope geochemists, are at the mercy of commercial manufacturers for the development of new instrumentation, not having the expertise and/or the infrastructure necessary to build custom machines themselves. There has been the suggestion in the past of developing a “national facility” for instrument development, in which new instrumentation could be built and tested, regardless of how commercial the final product might be. Perhaps the time has come to explore this idea more seriously.

INTEGRATIVE RESEARCH IN LITHOSPHERIC GEODYNAMICS

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As the modern embodiment of plate tectonics, lithospheric geodynamics links a wide variety of geological disciplines. Many of the questions that arose more than three decades ago in the initial days of the plate tectonic revolution remain to be answered. Renewed effort to address these fundamental questions is being driven by advances in the geologic subdisciplines. Mapping the three-dimensional structure of and defining patterns and mechanisms of deformation along plate boundaries remains a research priority. Resolving these issues requires an evolution in our style of research.

Understanding the processes that drive lithospheric deformation requires linking observations from the wide range of disciplines represented by NSF EAR with increasingly more realistic modeling of tectonic processes. Although few would likely disagree with this statement, implementing and encouraging such research is non-trivial. There are obvious logistical hurdles to be overcome – finding the appropriate program at NSF, obtaining reviewers who recognize the character of such interdisciplinary research, and keeping collaborative projects within viable budgets – but the more critical problem is developing graduate programs that support and nurture the development of scientists for which such integrative research is the norm. Success in integrative research requires both state-of-the-art expertise in several subdisciplines and the capability to work and communicate collaboratively. Very few academic programs have the breadth to independently train such scientists, and thus we need to develop straightforward strategies to encourage inter-institution graduate education.

THE ROLE OF FLUID FLOW AND ASSOCIATED HEAT/SOLUTE TRANSPORT IN GEOLOGIC PROCESSES

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As a hydrogeologist, my research focus is on the interactions among subsurface fluid flow, tectonic stress, rock deformation, and heat/mass transport in Earth's crust. I started my research in this realm with working on paleo-fluid flow systems in sedimentary basins, seeking to understand how pore fluid pressures evolved both temporally and spatially through basin evolution and during periods of mineral deposit accumulation. In recent years, I am gradually attracted to more contemporary geologic processes such as fluid flow responses to earthquakes and fluid flow in active subduction zones. My research focus, however, stays at the interface of fluid flow and tectonic stress/rock deformation to further explore the coupling between fluid pressure dynamics and geologic processes.

My observations seem to suggest that the importance of fluid flow and rock deformation is generally well recognized but often overlooked in research practice. I would like to suggest one of the priorities of the Solid Earth Sciences to be further understanding the role of fluid flow and associated heat/solute transport in geologic processes. Here are a few example issues that studies in this priority area can potentially address: fluid pore pressure responses to seismically induced stresses (pre-, co-, and post-seismic); roles of fluid in initiating and facilitating faulting; evolution of fluid pressure and temperature regimes in metamorphic processes; and, influence of rock deformation on hydrologic properties such as permeability of fault zones.

Considering this area of research as a priority would not only help maintain the recognition this interdisciplinary area has received, but also more importantly encourage rigorous research efforts be made in the future. Integrating with other efforts, this priority would contribute to making the disciplinary boundaries within the Solid Earth more permeable.

GEOCHEMICAL AND MECHANICAL BALANCES AT CONVERGENT MARGINS

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Although the gross outlines of ongoing Earth differentiation processes are known, subduction zones remain mysterious black boxes. Somehow low-density continental crust is produced and recycled there, but even the main features of this process are not well understood. Significant first-order problems that remain unsolved include:

- What are the source materials for granitoid rocks in convergent-margin batholiths?
- How are granite and its kin distilled from these source materials?
- Where does the massive residue from granite formation reside?
- What are space-making processes for magmatic rocks in arcs? What are the roles of lateral (e.g., lithospheric extension) vs. vertical (e.g., isostatic settling) processes?
- What is the four-dimensional distribution of strain in arc lithosphere? Can extension and contraction coexist, or do they alternate?
- How does strain in arc lithosphere depend on the nature of subduction? For example, are thrust belts driven primarily during subduction of oceanic plateaus?
- What happens to the deeper parts of the continental lithosphere during back-arc thrusting? Structural studies indicate that it must feed back into the subduction zone, but how this happens and how it might contribute to magma production are poorly known.
- What magmatic and structural processes serve to density stratify and geochemically stratify the continental crust?
- Is continental crust in steady state, increasing in mass, or eroding?

Some of these problems are being studied by the MARGINS program, but MARGINS focuses primarily on active subduction systems where only the shallowest portions can be sampled. Exhumed ancient arcs provide a readily accessible window into deeper parts of the arc system. However, such areas have not received focused, interdisciplinary study aimed at answering the questions above. Indeed, far more research money has been spent in refining the details of how mid-ocean ridge basalt is produced than has been spent to gain even a basic understanding of how the continents form. Our understanding of these processes would be greatly increased by:

- Interdisciplinary (e.g., geochemical, geophysical, structural) studies of arcs where basic data (isotopic dating, quadrangle mapping) are abundant.
- Increased communication between petrologists, structural geologists, and geodynamicists.
- Increased communication between those working on active arcs and those involved in studies of ancient ones.
- Use of emerging tools in geoinformatics to analyze large geochemical, geochronological, geophysical, and structural datasets.

Setting Priorities in the Solid Earth Sciences: Suggestions From the Top and Bottom of the Crust, and Questions About Resource Allocation

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The top priorities in the Solid Earth Sciences cannot be summarized in a single page. I have therefore chosen to focus on just two key priorities, emphasizing areas to which I can best contribute: 1) fluid/fault interactions in the shallow crust and 2) the mechanics of deformation under granulite facies conditions and implications for degree of coupling between the crust and mantle. My choices (deliberately) raise questions about research funding, which follow the introduction of these priority areas below.

Studies of fluid/fault interactions to date generally fall into two categories: a) fault impacts on fluid flow and b) fluid impacts on fault mechanics. The latter research typically does not address how fluids reach or depart a fault zone, and the former tends to address only those aspects of fault-zone mechanics that impact the type of fault-zone structures that form, which directly affects fault-zone permeability. A key future direction involves bringing these research areas together to address a myriad of issues in a crossover zone between basic and applied research. These issues include the distribution and accessibility of natural resources, such as water and hydrocarbons, as well as the rheology of different earth materials and earthquake mechanics. Particular attention should be devoted to the near-surface environment, which is not only the area in which we happen to live, but also the area from which we can collect the most data. These studies should be linked, but not restricted to, the ongoing efforts of SAFOD and NanTroSEIZE. To facilitate these studies, we must address the issue of how to better integrate the hydrologic and Solid Earth communities, which have been kept apart not only by the boundaries of tradition but also by the programmatic divisions of funding agencies.

The degree of coupling between the crust and mantle is arguably one of the most important outstanding questions in Tectonics. It may also be one of the hardest questions to answer. Key information is preserved in lower-crustal rocks such as granulites. Effectively reading this record will require a concerted effort to better understand how both deformation mechanisms and microstructures in granulites are represented by macroscopic fabrics, and the geophysical signature of such fabrics. In other words, we need to be able to de-convolve the 'geophysical signal' of structures that we can assign kinematic meaning. This type of research, which integrates geologic and geophysical studies, would theoretically fit nicely under the auspices of EarthScope; however, key exposures of granulites lie outside the boundaries of the United States. This priority area therefore raises the question of funding in the post-EarthScope era, and whether research will be increasingly restricted by geographic boundaries, which will also restrict the research questions we can address.

"What's Underneath Our Classroom?" Helping Undergraduates Visualize Solid Earth Processes of the Continental Interior

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The structure, evolution, dynamics and processes of continents and the subcontinental mantle are key research priorities for Solid Earth Sciences. They have been repeatedly endorsed in national workshops and assessments, such as *A National Program for Research in Continental Dynamics*, DOSECC/IRIS 1989; *Solid Earth Sciences and Society*, National Academy of Sciences 1993; *Basic Research Opportunities in Earth Sciences*, National Academy of Sciences 2000; and most recently the Project Plan for *EarthScope: A New View into Earth.* Meanwhile, the Geoscience Education Working Group in 1996 has cited "modeling, simulation, visualization, quantification and other graphical representations" among the improvements needed in undergraduate education in geology for majors and non-majors, including future pre-college teachers (*Geoscience Education: A Recommended Strategy*, 1997). We concur with these recommendations. We urge NSF during the coming decade to expedite and encourage wider applications of advanced scientific visualization to make Solid Earth phenomena of the continental interiors more appealing and accessible to the general undergraduate population.

Scientific visualization of large complex data sets is becoming more affordable in traditional academic settings. Applications of visualization are currently more extensive in biology, medicine, engineering, astronomy, the petroleum industry, oceanography and climate sciences than in the Solid Earth Sciences. With the large new data sets anticipated in EarthScope, there will be excellent opportunities for new interactive 3-D and 4-D visualization to enhance scientific understanding and interpretation in Solid Earth Research. A complementary effort should be made to extend visualization to undergraduate students. These students should experience and experiment with processes of the Solid Earth in virtual reality, in addition to imagining them.

Many students have not had the personal opportunity to see large geological exposures in mountains or canyons. They have great difficulty visualizing the continent and mantle underneath their classrooms or hometowns. It is also very difficult for students to relate the dynamics of continental interiors to rock samples that they study in hand specimen or under the microscope. The introductory course in physical geology is taken as a science requirement by large numbers of undergraduates and by students preparing for careers in pre-college teaching. It is essential that all these students be able to visualize the geology and processes within continental interiors. Expansion of visualization capability will enable students to manipulate and navigate data sets and experiment with data both individually and in collaborative teams.

We encourage NSF to promote increased interaction among researchers in the Solid Earth Sciences, the developers of scientific visualizations of large data sets, and potential educational users to stimulate new applications of scientific visualization for the Solid Earth Sciences in the undergraduate classroom. In our experience, workshops that are collaboratively developed by teachers and scientists are the best format to introduce innovative technology and teaching strategies into educational settings at all levels.

SETTING PRIORITIES IN SOLID EARTH SCIENCES

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Scientific issues that the geologic makeup of the US provides as provocative and excellent targets include:

- Processes and rates of continental extension.
- Processes involved in large-scale strike-slip faulting.
- Cordilleran-type ophiolite generation and emplacement.
- Growth of continental crust through the accretion of oceanic materials.
- Subduction factories with young slabs.
- Evolution of continental margin arcs.

Considerable geophysical data will be generated for each of these targets as a result of EarthScope, such that the marriage between geological and geophysical data should be fruitful.

Tools that the geologic community needs to break out of the current way of doing business include:

- Databases that can be easily and profitably used (e.g., comprehensive databases of western U.S. xenoliths or western U.S. volcanic rocks that can be exploited by everyone);
- Virtual laboratories so that research groups can collaborate on-line more effectively or so that individuals can use facilities remotely (e.g., remote access to an electron probe);
- More geochronology facilities (e.g., secondary ion mass spectrometry, 40/39 spectrometry, thermal ionization mass spectrometry).

The geophysical community is going to get a massive shot in the arm from EarthScope, but in order for the geophysical data to be used to its full potential, geologists must make an equivalent leap forward in instrumentation.

MINERALOGY, PETROLOGY AND MATERIAL SCIENCES

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The knowledge base and conceptual approaches used by mineralogists and petrologists have a great potential to impact many of the new directions in materials science. Mineralogists and petrologists are accustomed to dealing with complex multidimensional chemical systems that few material scientists would dare consider. Furthermore, most material scientists do not have the perspective of geologic time and its relation to material stability. Interdisciplinary alliances among these scientific communities could have a very far-reaching synergistic effect.

Twenty-five years ago mineralogical curiosities such as metamict minerals and perovskite-type minerals were being considered in relative obscurity by a few mineralogists and petrologists. Yet, today metamictization of materials is a central issue to safe disposal of high-level radioactive wastes, and perovskite-type structures are key structures for the development of high-temperature superconductors. In many respects, this is analogous to the influence that biologists who work on tropical ecosystems have had on pharmaceutical development.

Mineralogists and petrologists employ a large bag of tricks to characterize and model complex geological materials that could benefit the material scientist. Among these tricks are adaptive thermodynamic approaches with phenomenological activity models, unique views of multidimensional composition space, areal mapping of anions and cations of geological materials with a variety of microbeam instruments, 3-dimensional mapping of phases with tomographic techniques, and many others.

While the directions of material science are continuously evolving, there are several current themes that clearly can be impacted by the mineralogical and petrologic communities. In addition to the development of materials to store high-level radioactive wastes and the high-temperature superconductors, nanomaterials are commonly encountered in geologic settings. Buckyballs, and their many derivatives, are becoming increasingly noted in ancient natural environments. Piezoelectric and pyroelectric minerals are relatively common, but have yet to be fully explored for their material science potential. Many mineral surfaces are potential catalytic interfaces for organic and inorganic reactions. There are certainly many other geologically-related materials that are relevant to material science.

PROCESS-INTERACTION RESEARCH IN ANCIENT OROGENIC SYSTEMS

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Mountain ranges are complex dynamical systems with behaviors dictated by a vast array of physical, chemical, and biological processes. Their evolution depends on feedback relationships among these processes, how these relationships develop, how temporally persistent they are, and, eventually, how transitions to new process relationships take place. As a consequence, a deeper understanding of orogenic systems will require the focus of continental tectonics research on process interactions, not just the processes themselves, over the dimensions of time and space.

The notion of time is crucial to the design of an effective strategy for the study of orogenic systems. For obvious reasons, initiatives such as EarthScope focus on processes in tectonically active regions of North America or on “remote-sensing” the modern deep crust and mantle. As significant as the data that flow from the proposed studies will be, studies of current Earth structure and active tectonics cannot provide all the information critical to the development of a new generation of models for orogenesis. Studies of orogenic systems worldwide suggest that important transitions in process relationships occur over timescales of 105-107 years, making them impossible to study in any effective way on human timescales. Remote sensing of the lithosphere provides only a snapshot of a temporal continuum; the imaged structure is a cumulative product of billions of years of evolution and it provides no unique constraint on how that structure might have changed through time.

Fortunately, we have the means to augment a developing database on modern processes and orogenic structure through coordinated, multidisciplinary research on ancient orogenic systems. The most valuable insights will be related to how the lower continental crust behaves during orogenesis, and how heat and mass are transferred from the lower crust to the upper crust as a consequence of processes such as the emplacement of igneous complexes, as well as both tectonic and erosional denudation. Such research might proceed by: 1) the development of process network maps for various orogenic settings; 2) simple analytical models designed to elucidate the physics of process-interactions and make broad predictions regarding system behavior; 3) a broad effort to make field and laboratory observations aimed at challenging these predictions; 4) a series of directed studies aimed at uncovering the timescales of process-interactions for which evidence is found; and, 5) feedback of these results to develop more sophisticated models of orogenesis.

A fundamental question before us is whether such research should be done through large, multi-investigator campaigns or through the coordinated efforts of individual investigators or small collaborative groups. In my opinion, there should be room for both styles of doing science. But there is no doubt that coordination of effort is key; there are no independent processes responsible for the evolution of orogenic systems, and there is little value in disciplinary myopia.

BUILDING THE BEST INFRASTRUCTURE FOR GEOCHRONOLOGY AND ISOTOPE GEOCHEMISTRY

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Geochronology plays a fundamental role in determining rates of geological processes, from the study of both ancient and modern orogenic systems to major events in the history of life such as extinctions and radiations. Likewise, isotope geochemistry can provide valuable tests of tectonic and crustal evolution models that arise from other types of geological studies. However, the costs of operating and maintaining state-of-the-art laboratories to support such studies have increased dramatically, a fact not always appreciated by non-practitioners. While NSF has funded a large number of instrument facilities for universities, there has not been a parallel increase in the support needed either to maintain facilities or to encourage innovation. As the supervisors of laboratories that welcome collaborations with Earth scientists from outside MIT, we find that the demand for high-quality geochronologic and isotopic data far exceeds the capacity of U.S. laboratories at present. In addition, many of the best laboratories cannot encourage collaborations or are forced to limit the scope of collaborations because potential collaborators have limited research funds for obtaining data and it is left to the laboratories to subsidize the unrecovered cost. The problem is, of course, that the vast majority of these laboratories do not have a general "operating grant" that can make up the shortfall, and the result is that many research efforts in Earth Sciences that could benefit greatly from geochronologic or other kinds of isotopic data are left unsupported.

As plans are laid for future research directions in the Earth Sciences, we believe that the idea of creating a number of national centers for geochronology and isotope geochemistry should be considered seriously. There are several laboratories that currently function as "national facilities", such as the National Ion Microprobe Facility at UCLA and the National Ocean Sciences Accelerator Mass Spectrometry Facility at the Woods Hole Oceanographic Institution. These are fundamentally important resources for Earth Science research, but they focus on a specific type of geochronology or a specific instrument. We envision more comprehensive facilities, with state-of-the-art analytical laboratories that would provide access to the complete range of tools necessary for research over all time scales, from those accessible by U-Pb geochronology to those accessible by ^{14}C dating. They would be supported by high-throughput sample preparation laboratories, and by sample characterization laboratories with the instruments necessary to determine the structure and major and trace element chemistry of materials destined for geochronologic analysis. These facilities would be hotbeds for the development of new and improved analytical protocols, providing a resource of ideas for geochronological facilities run by individual isotope geochemists.

These laboratories would be staffed by research scientists and technicians who would be assigned to work in collaboration with visitors from other institutions as well as ensure smooth operation of the facilities. In addition to permanent staff, a facility would engage temporary staff who spend one or two years in intensive, hands-on training that would prepare them to take up permanent staff positions in research laboratories at other institutions.

We feel strongly that scientists who use geochronological data should understand fully how those data are obtained and how they are best interpreted. We foresee these national facilities as having an educational function as well as a research function. Graduate students would be eligible for travel stipends to support "pre-doctoral" appointments of varying duration, during which they could attend short courses on analytical methods and data interpretation in addition to collecting data for their research. Similarly, faculty would be welcome to spend sabbaticals at the facility to broaden their perspectives.

The financial burden of establishing and maintaining such facilities would be great, but we believe the investment would be worth the expense. The current structure of funding geochronological research through individual-investigator grants has a negative impact on the quality and quantity of the data produced. With no steady source of income, existing laboratories can ill afford to maintain the research staff and equipment to provide the kind of service that NSF-sponsored researchers deserve. By funding a number of service laboratories directly, the Federal Government can take advantage of the economy of scale and ensure high-quality science.

EVERYTHING'S MAGNETIC: AN INCLUSIVE AND INTERDISCIPLINARY APPROACH TO EARTH SCIENCE RESEARCH AND TEACHING

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One trend in Earth Sciences is the increase in the number of interdisciplinary, multi-institution collaborative research projects. Although the root causes for this trend are likely complex, one contributing factor is the recognition of the creative synergy produced within the context of an interdisciplinary project involving a small group of PIs with diverse expertise but common research interests. These collaborative projects are very effective vehicles to serve not only the core research problem addressed by the project's research, but also NSF goals of increasing student participation in research and dissemination of research activities. This is accomplished by exposing students involved in such projects to the research, expertise, and (in some cases) facilities of the whole team of PIs, rather than just the PI from their home institution. Research activities of such PIs, especially those at smaller colleges or universities whose faculty have greater teaching loads, are also transmitted to a larger number of students as course content.

There are many examples of research methodology (i.e., geochronology, sedimentology, geochemistry) that lend themselves to interdisciplinary projects. One of these is paleomagnetism and rock magnetism, whose primary research applications range in scale from biomagnetism of bacteria to global plate tectonics and planetary magnetic fields. At WWU, through NSF and limited internal funds, a modern paleomagnetism lab was established in 1970 and significantly upgraded in 1998. In addition to the direct research needs of the WWU PIs, justification for the lab was based on its proposed use to serve the Pacific NW region, as well as its use by WWU students as a research component of their coursework. The impact on WWU and the region has been positive. During the past 5 years alone, a total of 60 undergraduate students have conducted research in this lab as part of two courses ("Practical Paleomagnetism" and "Applications of Magnetic Anisotropy"). Eight of these students elected to follow up their class projects to complete senior theses or senior independent study projects. During this time five MS students have completed theses in the lab (8 more are in progress), and collaboration with faculty at twelve other universities has taken place.

"Bridging disciplines" such as paleomagnetism can form vital parts of collaborative projects on many scales. One method to support such collaborative research is to encourage the development of regional laboratories or centers of excellence for such disciplines. Justification for most research equipment is primarily based on the accomplishments and needs of an individual PI's research program. By explicitly considering broader impacts to a field by any research (including equipment requests) proposed to NSF in its review criteria, use of research facilities in a broad and collaborative manner is being implicitly encouraged. This would seem to blur the lines between current equipment programs in the Earth Science, such as EAR-Instruments and Facilities, and others such as DUE-CCLI Adaptation and Implementation. The Earth Sciences should take advantage of this opportunity to promote the success and increased accessibility state-of-the-art regional facilities provided to both students and the Earth Science community by developing criteria or programs designed to increase the number and research scope of such facilities.

COMPUTING, MODERN TECHNOLOGY, AND FUTURE OF THE SOLID EARTH GEOSCIENCE

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Since the plate tectonic revolution, dynamic features of the solid Earth have received increasing attentions. Current studies in both research and education recognize the importance of geodynamics in explaining various geologic phenomena. Geodynamics requires a stronger quantitative background and training to fully understand their implications to Solid Earth Science. In addition, data obtained from modern space borne measurements also call for a significantly different quantitative background and the use of computers to interpret correctly. Because of the modern trend of geologic measurements (e.g., GPS and remote sensing using radar and/or laser techniques), some knowledge of computing and modern technology needs to become a part of future geology training. In other words, to prepare the next generation of geologists, our education curriculum needs to include a stronger mathematical training. Defining an appropriate level of quantitative training for geology students and how to provide these trainings should be an important objective of this workshop.

Furthermore, Earth Science is driven by observations. Students should be taught how geologic observations are conducted and how scientific data are obtained. However, geologic phenomena are usually slow and geologic processes often take a long time to complete. Thus, it is of considerable interest to accelerate many geologic processes so that students can carry out the necessary observations and learn the geologic processes within a reasonable time frame (e.g., within a class period). One way to accelerate geologic processes is to employ virtual laboratories based on computer simulations. Current technology has the ability to accurately emulate various geologic phenomena such that students are able to work with these computer-based modules interactively and obtain realistic data. For example, an orbital dynamics module is available at the following website "http://www.geology.uiuc.edu/~hsui/flashdemo/flash/orbital_v2a.swf". In this module, students are able to explore the relationships among three controlling parameters of orbital dynamics, which are the mass of the central body, the orbital radius, and the orbital period. In addition, students are able to record the observed values of these parameters and conduct scientific analyses designed by instructors. For example, students can be guided to use the lunar orbit to determine the mass of the Earth. Thus, research and development of virtual laboratory modules for geological education should be of interest to the effective teaching and learning of geology in the near future.

Because of my research in geodynamics and geologic modeling and my current interest in using technology to enhance teaching and learning in geology, I can make significant contributions to this workshop that plans for the future of the Solid Earth Science, especially in the areas discussed above.

QUESTIONS TO BE ADDRESSED DURING THE NEXT DECADE AND RECOMMENDATIONS FOR PRIORITIES FOR RESEARCH AND EDUCATION IN THE SOLID EARTH SCIENCES

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I suggest the following two questions be addressed during the next decade and recommend that they should be given high priorities.

One question, which has far-reaching implications, is the solubility of sulfur in silicate melts at high pressures. Experimental study sulfur is important not only its effect on the oxygen fugacity of magmas, which affects many properties of magmas but also, its effect on the global climate change. Because sulfur has many species, measurements of sulfur or a specific species of sulfur in a silicate melt is not a trivial problem. However, if this problem is given a high priority, the experimental problems can be overcome.

A second question that needs to be addressed is placing more emphasis on developing computational models that combine both physical and chemical properties of magmas. Although algorithms such as MELTS have contributed significantly in our ability to test various hypotheses, it focuses only on the chemical properties of magmas. In recent years, there has been a strong drive to understand the physics of magmas, but a comprehensive model dealing with the physics of magmas complementary to MELTS is not yet available. A more comprehensive program that can handle both physical and chemical changes in magmas will not only help scientists working on terrestrial problems but also those working on planetary problems.

Integration of Field, Lab, and Numerical Approaches to Research in the Solid Earth Sciences

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Two of the strengths of the Solid Earth Sciences (SES) community are its diversity and a long tradition of directly observing and studying geologic phenomena in the field. As we consider new ways of organizing ourselves, we recognize the need to maintain and improve programs that foster collaborative, interdisciplinary science. We also emphasize the critical role of field-based research and the rigorous testing of numerical models. Here we review some of the benefits and goals of an interdisciplinary approach to SES research and examine some reasons for its success.

Interdisciplinary research fosters new ideas and leads to scientific breakthroughs that would not otherwise occur, and it commonly breaks new ground in areas that are of interest to a broad range of scientists, teachers, and the public. Much of this effort is conducted by small groups of investigators who work together using well-established networks and support systems. Field-based research forms the basis of many of these projects, and commonly involves collaboration with geochemists, geophysicists, experimentalists, geochronologists, and modelers. Pooling resources from multiple institutions enhances this effort by reducing costs and increasing opportunities for research, education and student training. The value of synergistic activities that result from integrative research programs cannot be overstated. Thus, enhancing the quality and frequency of interactive research should be a priority in any new SES initiative.

Periodically the value of field studies in quantitative SES research is questioned. Common complaints are that fieldwork is imprecise and poorly suited to quantifying geologic parameters used in many numerical models. We wish to counter this opinion in the strongest possible terms. Numerical models of Solid Earth processes would be pointless without the “Earth” component, but this is the path we risk taking if we trivialize the field aspect of geologic data collection. As in all quantitative sciences, the validity and success of geoscience models depend on their ability to survive rigorous tests. It is well known that geologic data form the basis for testing Earth-system models, but far too often it is assumed that the data already exist or are easy to collect or can be treated casually. This view is flawed and should be discouraged. By necessity, the natural world is the testing ground for our understanding of Solid Earth processes. Moreover, the quality of geologic data depends on the accuracy of field relationships where data and/or samples are collected, which in turn requires skilled geologic mapping and manipulation of geospatial data. Collection of high-quality field data and in-context sampling is not a trivial undertaking because as scientific questions evolve so must the targets of mapping and data collection. All forms of geologic data and samples must be firmly grounded in accurate field relationships if they are to provide adequate tests of Earth-system models. We believe this should be a fundamental guideline and underlying requirement of all SES research.

In this context, we argue that funding or organizational schemes that promote the growth of large, centralized research groups that control collaborative research actually limit productivity and innovation. While lab facilities and technical support are expensive and must be somewhat consolidated, interdisciplinary research among small and intermediate-sized groups enhances innovation by bringing new methods and technologies to a large number of programs. To teach students integrative field- and lab-based science, most field programs require support for and access to modern surveying equipment and laboratory facilities such as SEM, LA-ICPMS, XRF, microprobe, chemical labs, etc. Field-based structural programs now require access to new 3-D and 4-D visualization techniques. At the workshop we will explore existing facilities, networks and mechanisms that currently allow groups to pursue integrative field and laboratory research programs, and discuss potential ways of improving, replacing, or adding to them.

Some Priorities in Geodynamic Modeling

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This presentation will focus on one objective; that of understanding the three-dimensional behavior of the outer Earth, subject to the variable influences of internal and atmospheric dynamics at time scales relevant to society. This objective may be set in a traditional mechanical framework, that is: To model the kinematics and dynamics of the Earth, given boundary and initial conditions, constitutive relations and surface processes. Such an objective requires information on driving forces, thermal state, kinematics at depth and along the surface, behavior of the deforming and reacting material, and rate and extent of surficial processes. Viewing each of these in turn provides one perspective of significant gaps in our discipline.

Boundaries, Lower: Bounding geometries of deforming regions are partly identified by standard and evolving geophysical techniques, while bounding velocities are often poorly defined. Geometrical definition of mechanical boundaries is in part dependent upon a poorly known rheological state which links back to the problem of identifying bounding velocities. Seismic anisotropy in the mantle provides perhaps one of the more tantalizing pieces of information in geodynamics, holding as it does the suggestion of identification of mantle strain, past and active, and therefore information on bounding velocities. The physical remoteness of mantle anisotropy offers an excellent opportunity for broad interpretation that is seldom resisted. Because of its importance, we need to address first the observational questions on seismic anisotropy and then its interpretation. The latter will require extensive experimentation over a broad range of temperatures and strain rates.

Boundaries, Upper: Characterization of the geometry of the upper boundary of the model, the Earth's surface, is no longer a problem, but identification of surface and material vertical velocities remains elusive. In the absence of high-resolution temporal information on surface age and evolution, we are limited to the "Chicken Entrails" school of modeling. Fortunately, the identification of the Earth's horizontal velocity field at wavelengths approaching the lithospheric elastic thickness is becoming possible and provides one necessary check on model verification.

Constitutive Relations: Recent interpretations of lithospheric rheology based upon improved data acquisition have brought into question the completeness of our existing constitutive models. Currently lacking is a theoretical framework that links observations from natural assemblages to rheologies appropriate for geodynamic scales, incorporating time- and rate-dependent effects as well as pore-pressure fluctuations. These model rheologies must be sustainable in light of natural data including petrologic, geomorphic, and seismological observations and must embrace equilibrium and non-equilibrium transitions. As with surface characterization, construction of a new generation of rheological models will require high-resolution temporal data. In order to satisfy a requirement for societal relevance, we must meld continuum models, constructed from observations at very long time scales, with the shorter time and space scales dominated by discontinua.

On the Future of NSF Solid Earth Research: Perspectives of a Young Investigator, 2002

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Introduction

As a relative newcomer to Solid Earth Research, this workshop is of great interest to me. My Ph.D. research was funded by an NSF grant from the Tectonics Program that, as a student, I helped to propose (under the tutelage of Tim Byrne). This work was also selected for support by the Structure and Tectonics Division of GSA and by Sigma Xi. My recent postdoctoral research was funded by an NSF Earth Sciences Postdoctoral Fellowship and my current work is funded by a grant from the U.S. Navy Geothermal Program Office. I believe that my funding experiences, and interests for future research will be valuable to the discourse envisaged for this workshop.

Current Funding Patterns

There seems to be increasing emphasis on large, multidisciplinary research in the Earth Sciences (e.g., EarthScope, Biocomplexity in the Environment, Margins). This model for funding poses obvious challenges to investigators working individually, or to what might be described as small projects. A recent change that highlights this pattern is the termination of the NSF Earth Sciences Postdoctoral Fellowship Program. I find this disappointing as I see many successful investigators whose careers were launched via this program. Moreover, the argument articulated to me by an NSF representative that recent Ph.D. recipients should be able to vie for core funds as PIs ignores the realities of institutional constraints that forbid or make it difficult for non-faculty to PI grants. These aspects of funding are certainly worthy of discussion in the context of future Solid Earth Research directions. It is critical as a community that we recognize the political realities of the NSF and plan and/or lobby accordingly. To this end, the scheduled workshop represents a key opportunity for the Solid Earth Research community to prepare for participation in EarthScope.

Cross Disciplinary Opportunities From My Point of View

My research focuses on shallow crustal deformation at timescales from instantaneous to neotectonic to geologic. A primary interest is developing a better understanding of ongoing plate-boundary processes so that we can more critically evaluate the time-integrated record of deformation provided by ancient plate boundaries. Other primary goals include understanding the role of fluids in shallow crustal deformation processes, quantifying the seismogenic contribution to non-recoverable shallow crustal strain contained in geodetic signals, and using these quantitative observations to understand large-scale deformation patterns at plate boundaries that pose substantial hazards to populations. These interests obviously require close collaboration with the seismology, geodesy, hydrogeology and geochemistry communities, and thus offer opportunities for developing large-scale projects. At the same time, it is noteworthy that to date I have made considerable progress on these areas of inquiry with modest funding. The nature of future NSF programs will to some extent shape my proposal efforts. In the context of the proposed workshop, I would advocate a strategy that includes both small and large project opportunities. Naturally, at this early stage in my career I'm open to discussing this point and I'm keenly interested in ideas for Solid Earth community participation in EarthScope or comparable (i.e., to be developed?) programs.

AN INTEGRATED STUDY OF INTRAPLATE TECTONICS IN NORTHERN CHINA

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Most earthquakes and volcanoes occur along plate boundaries and are well explained by plate tectonics. Some intraplate volcanoes can be explained by hotspot tracks (such as Hawaii) or rifting (such as East Africa Rifts). However, many intraplate earthquakes and volcanoes are still a mystery. One of the priorities in Solid Earth Sciences in the next decade should be on integrated studies of intraplate tectonics (including earthquakes and volcanos). An excellent study area is Northern China.

The Northern China platform is a "stable" Precambrian shield, yet there is widespread volcanism in this region since late Mesozoic. The 1199/1200 A.D. eruption of Baitoushan is one of the world's largest Holocene eruptions. Strong active tectonics in this heavily populated part of China is also evidenced by abundant large earthquakes, including the most devastating earthquake in modern history – the 1976 Tangshan earthquake (M=7.6) that killed >500 million people. Several factors make this region an ideal natural laboratory for an integrated cross-disciplinary study of the Solid Earth Processes: 1) With the abundant seismicity and modern instrumental records, Northern China is one of the best regions to study intraplate earthquakes. It is well recognized that intraplate earthquakes are fundamentally different from earthquakes in the plate boundary zones, and the pressing need for a better understanding of intraplate earthquakes is signified by the 01/26/2001 Bhuj earthquake in western India (M=7.6) and the most recent (06/18/2002) Indiana earthquake (M=5.0) near the New Madrid seismic zone. 2) There is widespread Cenozoic intraplate volcanism in Northern China and Mongolia. This volcanic field is unique in its large diffuse area and absence of linear trends, contrary to intraplate volcanisms in Africa, East Australia, and Western United States. Their evolution may have a causative relationship with the Indo-Asian collision and the subduction of the Pacific and Philippine plates. 3) The Northern China lithosphere was thermally thinned starting from the Mesozoic, perhaps reached its peak during the early Cenozoic. Since the Miocene, the lithosphere has been thickening again, accompanied by significant subsidence in the Bohai and other sedimentary basins. Investigating the mantle processes causing the thinning and thickening of stable continental lithosphere is essential for understanding the evolution of continents.

We suggest intraplate tectonics as one of the priorities in Solid Earth Sciences, and North China as one of the best places for investigation. We plan to initiate a project using an integrated, crossing-disciplinary approach (including geodynamics, seismology, tomography, geochemistry, petrology, and thermobarometry to infer geotherm as a function of time, etc.) focused on earthquakes and volcanism in Northern China. The societal benefit of such a project is clear since the area is densely populated and earthquakes and volcanic eruptions are two main geologic hazards. The project attempts to take a major step towards involving broader sections of the society in a more holistic understanding the Solid Earth Processes.

TECTONIC BEHAVIOR OF CRATONIC CONTINENTAL INTERIORS

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Structural geologists have tended to focus their efforts on studying Phanerozoic orogenic belts, because tectonic features are well exposed in these belts. Yet most continental crust on the planet lies within cratonic regions, regions that have not endured penetrative deformation and metamorphism for at least the last 1 billion years. Relatively little is known about the Phanerozoic tectonic behavior of these regions, and thus to some extent, they can be viewed as frontier areas for interdisciplinary geologic research. A number of interesting tectonic questions pertaining to cratonic interiors have never been fully answered. For example: *Why do so-called epeirogenic structures (regional-scale basins and domes) exist, and what drives their movement? *How old is the topography of interiors, and how does it evolve? *What causes stress and seismicity in interiors? *What features localize seismic activity and why? *What are the fundamental differences between shields and platforms, and why did they develop? *Is there a linkage between mantle dynamics and tectonism in the continental interior? *How was the crystalline crust beneath the sedimentary cover assembled? *How is the character and depth of the Moho related to ancient and contemporary continental-interior tectonism? *How does a craton respond to stress? *What are the effects of marginal orogeny on cratonic continental interiors? *How are cratons formed? *What provides the strength to cratonic lithosphere? * How do interior landscapes evolve?

Answering the above questions relies in part on surface geological analysis – mapping and structural analysis can provide constraints on regional strain fields, paleostress trajectories, and fault kinematics. But the solution to many problems will also require collaborations that involve collection of data from seismic-reflection, magnetic, gravity, heat flow, geochronologic (including fission track), and geochemical studies. EarthScope, in particular, provides a spectacular opportunity to collect data with sufficient resolution to map deep crustal and underlying mantle features, so as to determine their relation to intraplate seismicity and to upper-crustal structures. Long-term high-precision GPS campaigns have the potential to characterize the very slow, but non-zero strain rates in the continental interiors, providing opportunities to relate tectonic processes in the interior to landscape evolution. Finally, in addition to the collection of new data, it is essential for researchers to mine existing archives of data, already on file in state geological surveys or in departmental libraries of Midcontinent universities, to obtain results of irreplaceable detailed field studies carried out by geologists decades ago. In order to be utilized in the 21st century, these archives, along with new data, must be compiled and synthesized into a GIS-based accessible format.

SETTING PRIORITIES IN SOLID EARTH SCIENCES

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Research Priorities in Solid Earth Sciences

Priority areas in Solid Earth Sciences research in America should include:

1) energy, water, and mineral resources: extraction, land restoration, and waste disposal;
2) non-applied research, with specific directions set by the scientific community (as concerned, educated citizens, Earth Scientists will have the best grasp on priority issues);
3) emphasis on interdisciplinary research initiatives (research between major scientific disciplines as well as between geologic subdisciplines); and,
4) increased access to facilities (standard equipment to state-of-the-art instrumentation) by talented faculty and students from any university (the nation's research effort is meant to extract as many new ideas from the scientific community as possible; maximum results will be achieved by maximizing the number of qualified people involved).

Education Priorities in Solid Earth Sciences

In America, we do a great job preparing new Ph.D. scientists. We need to bring other areas of education up to the same level of excellence. These are:

1) educating the general public about Earth Processes, so that voters and legislators can use both scientific and societal information in their decision making;
2) starting this education process early by educating K-12 teachers; and,
3) improving BS and MS education by concentrating on research opportunities; these people are the bulk of the Earth Sciences industry workforce and need to be aware of instrumentation and techniques that could be applied to the variety of Solid Earth problems in industry.

My Unique Contribution to the Workshop

My main contribution to the workshop will be in the arena where research and education intersect. As a professor in a research geology department that offers the BS and MS degrees but not the Ph.D., I compete at NSF and publish in high-quality journals, but also carry a heavy teaching load. For 13 years, I have run the department's analytical facilities (ICP-AES, XRF, XRD), being responsible for all aspects of lab function (financial health, maintenance, training, supervision, standard development, quality control, etc.). Thus, my career has been a blend of teaching, research, and facilities management. I have worked in this arena at the university and national levels. Here at NMSU, I developed and coordinate a unique undergraduate research program. I have served on two NSF panels: Course, Curriculum, and Laboratory Improvement in the Division of Undergraduate Education, and more recently on the Panel for Graduate Research Fellowships. In addition, I have worked extensively on the problem of K-12 education in the Solid Earth Sciences. I am the Chair of Mineralogical Society of America K-12 Outreach Program, in which we developed a web site for teachers and students, and have authored NSF proposals that involve K-12 initiatives.

3D Geologic Mapping as the Foundation For Numerous Societal Applications – The Central Great Lakes Geologic Mapping Coalition

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The State Geological Surveys of IL, IN, MI and OH joined together in 1998 to form the Central Great Lakes Geologic Mapping Coalition. The purpose of the Coalition is to cooperate in joint investigations and scientific exchanges concerning the Earth Sciences (including geology, hydrogeology, geochemistry, geochronology, geophysics, geotechnical and geological engineering, and related investigations) on topics of mutual interest. The Parties have agreed that reaching a better understanding of the 3D framework of the region's unconsolidated geologic materials is needed to provide local, regional, and national land-use planners and managers with the Earth-Science information required to make informed decisions regarding urban and agricultural land use, the identification and protection of aquifers, identification and assessment of geologic hazards, identification of aggregate resources, and the environment of this unique region.

Starting in FY2000, Congress has provided a modest level of funding to the Coalition, through the USGS, to conduct a beginning pilot program. Additional funding has been made available from the USGS as well. A general overview of the effort (USGS Circular 1190) can be seen at http://pubs.usgs.gov/circular/c1190/. A one-page Fact Sheet (USGS FS-153-99) can be seen at http://pubs.usgs.gov/factsheet/fs153-99/. In addition, a detailed 17-year plan was developed by the Coalition (USGS OFR: 99-349). It is not available on the Internet. Within each state, high priority area were identified for detailed geologic mapping by State advisory committees (see Figure 2 of FS-153-99).

Since its inception, I have served as the USGS project chief and USGS coordinator for the Coalition effort and funds. Initial USGS work under the auspices of the Coalition is multidisciplinary and broadly conceived. Fieldwork was begun in Berrien County, Michigan and the Bering Glacier, Alaska. The Bering Glacier advanced rapidly in 1994-1995 on approximately a 40 mile front, and has retreated since leaving new deposits, many on deposits from previous advances. These are the same processes that left the glacial deposits of Berrien County, thus providing an outstanding opportunity to apply modern processes to an area that is approximately 7000 to 14,000 years old. In Berrien County, the USGS 3D geologic mapping incorporates fieldwork, drilling core and bore holes, air-borne, ground-based, and subsurface geophysics, and coastal seismic surveys (the county's western border is Lake Michigan). Numerous interactions and briefings have occurred with various County officials and citizens and more will occur as the project concludes in the near future with the completion of a 3D geologic map of the County at 1/50,000-scale and as the USGS works with the County and other officials as they seek to use the map (actually a database and model) for their planning and land management purposes. A two-dimensional surficial geologic map at 1/100,000-scale was published (Stone, 2001). It can be viewed at http://greenwood.cr.usgs.gov/pub/open-file-reports/ofr-01-015.

Based on my current experience in the Great Lakes Coalition, interest in communicating science to the public, current and past experience in developing and coordinating other multidisciplinary geologic research projects in the USGS, and having had the opportunity in past positions to develop a strong background in understanding and representing the breadth and depth of USGS geology and water resource research programs, I will make a positive contribution to the workshop in all three areas (research, education, or outreach).

FUTURE PRIORITIES IN ACTIVE SOURCE SEISMOLOGY: A TOOL FOR INTEGRATION

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Active source seismology is perhaps the key tool in tying geologic observations at or near the surface to the much larger scale features and processes that will be discovered by the natural source component of USArray. Seismic reflection data gives geoscientists a crisp, detailed image of the subsurface that can be used as a tool for tracing geologic structures seen at the surface to the base of the crust and even occasionally into the upper mantle. Analysis of coincident seismic refraction data provides estimates of seismic velocity that can be related to lithology. Thus, these data give us the crucial third dimension that surface data lack. It is important, however, that these data not be acquired in a vacuum. Geologists and geophysicists must decide together on targets and on the best interpretation of the geophysical results. In addition, data sets need to be integrated and available to subsequent workers, so that even more integration can take place in the future. To accomplish this, the program structure at NSF has to better accommodate cross-disciplinary activities, perhaps by being more process-oriented than discipline-oriented.

Building Capacity in the Solid Earth Sciences—Intellectual, Infrastructure and Human Resources

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The research agenda for the Solid Earth Sciences can be achieved only if there is concomitant development of the knowledge base (i.e., new research discoveries), infrastructure (i.e., facilities, instrumentation, databases, and information technologies), and human resources (i.e., through education and outreach, professional development opportunities). "Basic Research Opportunities in the Earth Sciences" (NRC, 2001) has clearly identified emerging areas where there is a compelling need to focus research activities, and the Solid Earth Sciences play an essential role in each: integrative studies of rocks, soils, water, air, and organisms in the near-surface "critical zone"; geobiology; Earth and planetary materials; investigations of the continents; studies of the Earth's deep interior; and planetary geology. Research in these areas will increasingly adopt an Earth system approach, emphasizing the processes and feedback mechanisms among and between disparate parts of the Earth system. Consequently, future research in these areas will be increasingly multidisciplinary. It will be important to develop new collaborations and partnerships across the geoscience disciplines (e.g., geophysics, EarthScope), and to sister-disciplines in the sciences, math and engineering. Datasets, and the tools needed to render and represent data, must become universally accessible, and data providers should anticipate and support uses of their data beyond a specifically targeted clientele (e.g., Geochemical Earth Reference Model, Geoinformatics/GEON). New information technologies provide the means to aggregate, organize, and disseminate information to broad audiences, and to create networks in support of virtual communities (e.g., Digital Library for Earth System Education). In addition, a huge investment has already been made in research facilities and instrumentation. However, use of current instrumentation must be optimized to support basic research, to address problems and issues of importance to society, and for educational purposes. Beyond existing facilities, it may be necessary to develop national consortia to prioritize and operate next-generation facilities for the benefit of all.

Advancing the interests of Solid Earth Sciences also requires a concerted, coordinated education and outreach effort. A false dichotomy has been asserted that places research and education as polar opposites. Research and education share the common values of inquiry and discovery. Education is intrinsic to all of our professional research activities: in our scholarly publications, in presentations at national meetings, in fieldtrips, workshops and short courses (e.g., the NAGT "On the Cutting Edge" professional development workshop series). The Solid Earth research agenda is sustainable only if current researchers continue to learn about the Earth and to share that information with each other. At the same time we must adequately prepare future generations of scientists (and citizens). The health of the Solid Earth Sciences is largely dependent on the effectiveness of educational activities that a) translate new advances in science to colleagues, students, and society; b) provide training in the appropriate use of analytical instruments, databases, and interpretive tools; and c) inculcate "scientific habits of the mind" in students and citizens. The Solid Earth Sciences will further benefit from coordinated outreach efforts to other related disciplines and to the public (e.g., policy planners, journalists, teachers). Solid Earth scientists have a shared responsibility to proactively represent our science to these diverse audiences. To prosper, the Solid Earth Sciences must effectively integrate knowledge, infrastructure and human resources.
(Please visit: http://serc.carleton.edu for more related information)

THE INTEGRATION OF TEACHING, RESEARCH AND OUTREACH PROGRAMS USING THE PERKINS GEOLOGY MUSEUM DIGITAL ARCHIVE

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Two of the most pressing problems facing the geoscience community today are 1) that the general public has very little knowledge of Solid Earth Science and 2) that there is a growing gulf between the people who do cutting-edge research and the people who communicate the results of this research to the public. To help set future priorities in Solid Earth Sciences we must explore ways of strengthening the natural synergy that links many teaching, research and outreach programs.

The Digital Archive of the Perkins Geology Museum at the University of Vermont provides an in-progress example of how to achieve this integration. The Perkins Museum is archiving digitized images of its collections through a grant provided by the Institute of Museum and Library Services (IMLS). These images include fossils, rocks, minerals, thin sections, cores and maps. Data and metadata (e.g. description) for each image are updated, verified and catalogued following GeoRef hierarchical levels. The purpose of this project is to create a digital archive that provides educators and students from K-12 to the graduate level with pedagogical and curricular activities associated with Vermont geology. In addition, this initiative provides researchers interested in studying aspects of Vermont geology with information on critical field sites and raw data on subjects ranging from glacial geology, limnology, environmental studies, petrology and structural geology.

The incorporation of the Digital Archive into the classroom will enable Geology students to access data sets such as geologic maps, thin sections, cores and chemical composition files for use in geological reports, research projects and class exercises. On-line access to the archive will help reach out-of-town and out-of-state audiences interested in Vermont's Geology. This project also aims to incorporate Geology faculty research studies about Vermont and other worldwide localities to students. Communicating research activities effectively relies on incorporating new and existing material in the classroom. In one example, the NSF-funded Vermont Landscape Project focuses on showing students and educators how humans have impacted the environment over the past century. Our contribution to researchers relies on providing easy access to an estimated 40,000 images from 20,000 verified objects that the archive contains. Another problem currently facing the gesocience community is that the public has very little knowledge of Solid Earth Sciences. Yet at the same time there is great interest among the public about geology. Special on-line exhibits and activities for K-12 education will promote Earth Science inquiry at an early age.

Technical capabilities for the Perkins Museum Archive are modest and include 3 computer workstations, two digital cameras, a Wild™ macroscope system, a flatbed scanner and a Vidar™ 40" scanner. A 270 GB server storage capability allows us to archive the image files. The staff includes two geologists, a database programmer, a librarian and a digitizer.

What is needed to achieve success: Easily accessible geoscience education materials and data that enable students and teachers to connect directly to the geology around them. The Perkins Museum Archive also provides supplements and resources to K-12 teacher training courses. The project also provides an opportunity to incorporate research and teaching in order to promote scientific inquiry.

Petrology in the Core of Gathering and Disseminating Information About the Structure and Evolution of the Continental Crust

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Igneous and metamorphic petrology gives fundamental insights into the chemical, thermal, and structural evolution of the continental crust through time. The chemistry of igneous and metamorphic rocks gives information about the chemical differentiation of the crust through time, about its interaction with the underlying mantle, and about chemical transport through the crust by magmas. Mineral assemblages and chemistry of minerals give information about the pressure and temperature regimes in evolving orogens and the movement of fluids within the deep continental crust. In contrast to seismology, which provides only the current view on the structure of the crust, petrology, coupled with geochronology and structural geology, gives unique information on the temporal evolution of mountain belts within the framework of plate tectonics. Recent developments of thermodynamic data bases and advances in analytical techniques, including the improvements in x-ray imaging of minerals, dating of zoned microscopic mineral grains, and ICP-MS spectrometry, allow petrologists to understand the complexities of crustal evolution in ever-increasing detail. We can now document the ultrahigh-pressure burial of crustal segments, rapid denudation and uplift of orogens, the interactions between magmatism and deformation, ingress and egress of fluids into deforming crystalline rocks, and other crustal processes. We now understand better than ever before magma-generation processes and the interactions between magmatism and deformation. We now understand the effects of fluids on the physical properties of magmas and we can predict their liquid-lines-of-descent. Times are exciting in petrology, yet petrologists are doing a poor job in explaining its obvious relevance to other branches of geology and society in general. This is especially reflected in the replacement of many petrology positions by other geology subfields at many universities and the difficulty in attracting students into the subfield.

The role of petrology should be especially apparent for the success of proposed large-scale programs such as EarthScope. For example, petrologists will be needed to interpret imaging data from the USArray, as they are among the best qualified people to understand the composition and structure of the deep crust, from experiences gathered during studies of crystalline rocks at the surface. Ideally, the seismic component of the USArray should be complemented by selective sampling of crystalline rocks that are buried below Cenozoic sedimentary rocks. Elsewhere, petrology needs to be better integrated with geodynamic, geomorphologic, and structural studies of active orogens, because it is petrology that can place the best constraints on orogenic evolution over tens of millions of years. Therefore, it is critical for NSF to foster cross-disciplinary studies that may include components that are traditionally funded by different NSF panels. This should include studies that are on a smaller scale than those funded by the Continental Dynamics Program.

To continue exciting developments in petrology, NSF needs to expand its support of graduate and post-doctoral education, particularly as it relates to hands-on experience with instrumentation and integrated science. NSF and professional societies should develop or expand speakers programs so that the excitement and importance of petrology are disseminated among research and non-research institutions alike. A budget for a speakers program should be miniscule compared to the budgets of some grants, yet the benefit of disseminating the excitement of petrology may be far greater.

MINERAL PHYSICS, NANOGEOSCIENCE, AND A CHANGING WORLD

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The approach pioneered by mineral physics over the last two decades has broad applicability to complex problems involving solids from the Earth's surface to the core. This approach involves the application of rigorous molecular-level thinking and experimentation and the use of sophisticated instrumentation, both in individual laboratories and at national facilities. Increased understanding of the mantle and core through a combination of high-pressure experimentation and studies of thermodynamic, crystallographic, spectroscopic, and physical properties of mineral phases has been a major success of mineral physics. The emphasis appears to be shifting toward understanding the mineral reactions in the "critical zone", that part of the shallow earth (atmosphere, hydrosphere, and near-surface environment) most directly affecting and affected by human activities. The mineral physics approach is essential to understanding complex materials such as clays, zeolites and iron oxides, which control element transport in shallow environments. Such understanding is necessary for dealing with contaminant transport, including that arising from terrorist acts. Such knowledge is also essential to issues of carbon sequestration and global climate change. Solid Earth Scientists must bear in mind that, in the long run, rocks buffer element concentrations in the oceans, atmosphere, and groundwater. Thus, there is immense need to integrate Solid Earth Sciences with the study of global geochemical cycles and present-day environmental phenomena.

A workshop was held in June 2002 at Lawrence Berkeley Laboratory to assess and define the emerging field of nanogeoscience. Its findings relevant to Solid Earth Sciences will be summarized. Common needs of the nanogeoscience and mineral physics community for infrastructure and instrumentation, both in individual laboratories and national facilities, will be discussed. The need and possible mechanisms for educating graduate students in these quantitative physical approaches will be presented.

PRIORITIES IN THE SOLID EARTH SCIENCES WITH A FOCUS ON PETROLOGY AND GEOCHEMISTRY

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With the quantity of igneous and metamorphic rocks found on the earth's surface today, the major roles that igneous and metamorphic processes have played throughout geologic time, and the importance of interaction of these rocks with the hydro-and biosphere, petrology remains at the core of geology. Yet as a research discipline, it has been increasingly relegated to the back burner. Although there are many external factors that influence popularity of fields, it is time to evaluate internally how to balance research initiatives to maximize identifiable and exportable progress.

Over the past ten to twenty years, the study of petrogenesis has become increasingly detail-oriented, with contrastive rather than comparative emphases. This can be likened to looking in ever-increasing detail at apples in hope of identifying how they are related to other fruit! Similarly, the search for finer and finer-scale differences in trace element and isotopic systematics has made it increasingly difficult to identify general processes. Yet without significant progress on isolating important general processes, it has also become increasingly difficult to export information to other fields of geology.

Taking as an example the application of trace element geochemistry to petrogenesis. Since the seventies it has been recognized that trace elements show greater diversity than major elements and therefore can be used to distinguish rocks. Since that time, however, instead of being only used to distinguish rocks, they have been used to the point of exclusion of all other characteristics to interpret source region characteristics. Yet, trace element distributions in natural igneous rocks are dictated not only by source characteristics, but also by magmatic history (i.e., phase equilibria and changes thereof), magma/wallrock interactions (which may involve dehydration, partial melting, and/or assimilation), fluid-phase exsolution, and low temperature processes. Where are the experiments that constrain the effect of magmatic volatiles on phase equilibria and, in turn, on trace element distributions? Where are the experiments that constrain the types of wallrock/magma interactions that can lead to trace element mobility? Where are the experiments that constrain trace element mobility through magmatic fluid exsolution? Such experiments are vital if we are to get at the general *processes* involved in dictating observed trace element abundances. Differences between such general processes and specific processes relevant to individual natural magmatic suites bring contrastive science in again for the development of new general hypotheses for testing.

It is time now to begin a concerted effort towards a more iterative approach of comparative and contrastive research; that is, one that requires alternatively defocusing and re-focusing. This can perhaps be best done through large-scale problem-oriented (rather than technique-oriented) research projects (e.g., the origin of hotspot magmas) that involve multiple PIs working on similar questions but from different approaches, including tectonics and geophysics. This format could also readily accommodate technique development towards the common goal. Graduate and undergraduate participation in such projects would provide training in the use of comparative reasoning in problem-solving strategies – training that is generally very limited at all levels of education. In many ways, this multiple-PI research approach with strong educational component is modeled on a mini-STC and would necessitate a move away from the 2-3 year grant cycles and towards longer duration grants (e.g., 5-year cycles).

CONTRIBUTION TO SOLID EARTH SCIENCE WORKSHOP, DENVER 2002

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When new acquaintances ask, “So you are a geologist, what is it that you study?”, I never know what to answer. “The Earth” seems to be a trite response, and yet it is true. I have spent my career working in what would be considered the Solid Earth Sciences, but always at the fertile boundaries of sub-disciplines. Application forms would like to pigeonhole me as a metamorphic petrologist. Whereas I have certainly made contributions in this limited discipline, what about the ore bodies that I have analyzed, the minerals that I have dated radiometrically, the fossils of which I have studied the composition, the faults that I have mapped, or the volcanic textures that I have interpreted? To me the joy of studying the Earth is in the interconnections. I firmly believe that research in Earth Science must strive to view the planet holistically and that education in Earth Science must strive to produce an appreciation of our planet’s complexity.

As an Earth Scientist, I believe that the greatest contribution that I could make to the group, and its goal of planning for the future of Solid Earth Science, is an understanding of collaborative, multidisciplinary work. My current research project, for example, is focused on the paleo-environment of the Burgess Shale basin. Was it a lifeless, oxygen-starved burial ground as the literature would have us believe? Or was it a complex ecosystem reflecting an older deep-water lifestyle based on fluids seeping from the seafloor as our newly gathered evidence suggests? A petrologist is not going to answer this question. Neither is a paleontologist or a stratigrapher or an economic geologist. However, together as a team we can pool our knowledge and examine the complex question with an equally grand sum of intelligence and experience. Each new discovery is a shared one and each of us learns continuously from the others. Is this not a fruitful model for future Earth Science research?

I am proud to wear another hat as well, that of the educational researcher. With an advanced degree in curriculum development (in addition to my Ph.D. in Geology) I am deeply interested in how students learn geology. I am convinced that the essential component of successful teaching is relevance to the learner. Why should a student care to learn what we teach? I believe that the answer lies in two parts: 1) the information must connect to their lives as directly as possible; and, 2) the infrastructure for deep understanding must be built in their minds by continually helping to build bridges that connect the many sub-disciplines of our science. This can be achieved by stressing integrated, multidisciplinary courses and programs, as well as providing students with as many opportunities to participate in authentic science activities. If the design of these courses and experiences would also take into account the local environment and general experience of the students involved, then all the better. It would be a pleasure to help our community of researchers and educators build strategies to support such rich learning opportunities in the future.

Re-orienting Solid Earth Sciences Research to Address Issues of Sustainable Development

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A group of Canadian geologists, now convened as the Canadian Geoscience Council's Standing Committee on Sustainable Mineral Resources Development, is developing a major funding proposal for geoscientific research towards sustainability in the mining sector. This proposal has developed as an outcome of recent enquiries into the future of geoscience research in Canada, similar to those being undertaken at this NSF workshop. Two examples are the quadrennial NSERC (Natural Sciences and Engineering Research Council of Canada) Reallocations Exercise, which requires research sectors within NSERC (including the Solid Earth Sciences) to compete for reallocated funding on the basis of their proposed future research strategies; and a workshop convened in March 2001 by an earlier incarnation of the CGC Standing Committee, entitled "Developing a New National Strategy for Mineral Deposits Research in Canada".

Although the make-up and outlook of the CGC Standing Committee is oriented towards the geology of mineral resources, the underlying pretext applies, we believe, to all forms of geoscience, and in fact to a much wider collaborative research community. That pretext is the application of our expertise to researching the extent of and solutions to the problems of sustainable development.

Sustainable development has many definitions depending on the perspective of the user. For geoscientists, however, it most clearly relates to the way in which we as humans co-exist with our planet and its other inhabitants, and in particular how we use its resources and the effects of that usage. The Brundtland Commission (1987) allowed for the use of natural resources to support current populations, but not at the expense of future generations. Because natural resources, their exploitation, and the consequences of that exploitation are all systems and processes with their roots in the composition and behavior of geological materials, geoscientists are well positioned to take a leading role in ensuring the sustainable development of these resources.

But the concept of sustainable development is not the sole preserve of one science sector, nor even of the sciences alone. In developing our research initiative in Canada, we quickly learned that, although our geoscience expertise will be central in the research process, it must be guided by interaction and collaboration with a wide range of other disciplines, including obvious partners such as mining, civil, and environmental engineering, but also including the social and health sciences, economics, and law. Perhaps more important than all of these, however, is consultation and interaction with affected communities and ENGOs, because these are the people who directly or indirectly bear the potential costs of *un*sustainable development. This is an interesting challenge for geoscientists, and requires them to "think outside the box." It does not require, however, a major change in the actual research agendas of those scientists — merely a reorientation of perception and focus onto broader issues. A reorientation of this kind will bring with it a much-needed change in the focus of teaching and training. The 21st Century will require highly qualified people with multidisciplinary skills and wide sensitivities if we are to avoid making the same unsustainable mistakes of the past, but now on a much larger, global scale.

THE LONG-TERM DIRECTION OF SOLID EARTH SCIENCES EDUCATION

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I have special interests in the long-term direction of Solid Earth Sciences education, and in the interface of research and education/outreach. I can offer perspectives based particularly on my experience in informal (museum) education, in meshing research and education, and in representing the biogeological and historical aspects of Solid Earth Sciences. Priority issues toward which I can most likely make a contribution include the following.

How will "we" (the solid Earth geoscience community) play a role in implementing recommendations made for Earth System Science Education reform in (e.g.) the reports of the NRC (Natl Sci Ed Standards, 1996) and the NSF workshop on the "revolution" in Earth/space Sciences Education? I can contribute in part from my experience as a participant in several national meetings on Earth Science Education Reform and in designing my organization's education mission around these recommendations; I helped write the recommendations for informal science education in the NSF "Revolution" workshop report.

How can we make participation in research and repeated exposure to analysis of research data (in particular in Earth Science) a typical experience for all American students? Considering how to involve more students in research and to increase public understanding of research is a primary line of my current work (with Paul Harnik); Harnik and I will be leading a topical session on research partnerships at the Denver GSA meeting.

How can we use new data [such as from EarthScope] and visualization tools to facilitate students' deeper understanding of "real world" local and regional geology around them? Community relevance and open-ended discussion about phenomena students encounter in their own lives are priority recommendations of the NSES. I can contribute experience I have had working with teachers to create user-friendly resources for understanding regional geology.

How do we foster informal and lifelong learning in Solid Earth Sciences? I have a personal interest in this in part because my organization is building a new exhibits facility (The Museum of the Earth); I am considering in what ways our programming can go beyond more traditional approaches, using technology and active research.

What is the significance of feedbacks between history of the crust and of life? This may fall at least partially outside of Solid Earth Sciences (it falls at the crossroads of Earth Systems), but is an emerging field related to my scientific research. I would be happy to be a representative for the "soft rock" and "biological" side of Solid Earth Sciences.

What leadership, infrastructure, and alliances will facilitate achievement of long-term goals? I consider this issue at several scales, such as my organization's role in maintenance of geoscience information and specimen repositories, the national spread of student-scientist research partnerships, and the role of museums in facilitating regional alliances.

How will new scientific information and data be shared among researchers and with the general public? DLESE and NSDL are obvious starting points. I can share thoughts on the planning stages for an analogous project I am part of, to enable on-line sharing of information in paleontology (headed at UC Berkeley) that serves both research and education communities.

PRIORITIES IN SOLID EARTH SCIENCE

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The priorities of the NSF Solid Earth Science programs (as defined in the workshop announcement) must be considered independent of and within the context of EarthScope and similar-scale projects. In my view, NSF Solid Earth Science is faced with the challenge of maintaining strong support of innovative, PI-driven research with a scope and style that is both complementary to and independent of very large multidisciplinary efforts.

In this context, I feel that our efforts should be substantially directed toward finding new ways to design and nurture integrative Solid Earth Science investigations and community facilities (not necessarily emulating the EarthScope model) that can attract more funding dollars. For example, development of communal databases of software, results, and other information (e.g., a communal web-server of pre-prints such as in the Astrophysics community, or videotaped seminars such as available from the earthquake hazards team of the USGS) would be an important part of supporting exciting and rapid progress in science. This workshop should help identify other ways in which we can defragment the Solid Earth Sciences community and hence set the stage for rapid, coherent, and exciting scientific progress.

Within the field of tectonics, I think one of the most exciting emerging research areas is the interaction of multiple failure processes, for example brittle faulting and ductile flow, during crustal deformation. This interaction is poorly understood today but is important as the link between sub-surface loading (due to tectonic stresses or local buoyancy) and surface processes (such as landscape evolution). Understanding this interaction requires a combination of field and laboratory study of deformation and faulting, frictional behavior, the pressure and temperature conditions of failure (e.g., through fluid inclusions in faults) and the role of fluids in faulting. This is an inherently interdisciplinary topic, requiring interactions between petrologists, geochemists, geomorphologists, structural geologists and geodynamicists. It is also a topic that spans the entire range of tectonic settings and geologic timescales, and is therefore the sort of fundamental, process-oriented subject that needs immediate attention.

THE ROLE OF GEOCHEMISTRY IN UNDERSTANDING CONTINENTAL DYNAMICS

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Determining the chemical and isotopic composition of Earth materials (hereafter referred to as "geochemistry") is an expensive endeavor requiring laboratories with highly sensitive equipment, maintained at constant temperature, air purity and humidity. Successful operation of these laboratories also requires adequate funding of technical support. So is it worth it? What unique insights can geochemistry provide?

The first and most obvious insight from geochemistry is provided by isotope geochronology. The importance of determining ages and the timing of orogenic and extensional processes is self-evident in any study of continental dynamics. It is perhaps less obvious how other branches of geochemistry (e.g., major and trace element geochemistry and stable and tracer radiogenic isotope studies) contribute towards understanding continental dynamics. In this abstract I highlight examples of how geochemical studies of basalts and xenoliths can complement geological and geophysical data for two regions of continent extension (Basin & Range and East African Rift). As with other subdisciplines, geochemistry alone cannot uniquely illuminate dynamical processes and the greatest advances are made from integration of geochemistry with other lines of investigation (field geology, geophysical data, dynamical modeling) into a self-consistent model for how continents form and evolve.

Recent studies on the geochemistry of basalts from the Basin and Range provide evidence for the evolving lithospheric thickness during the Cenozoic. DePaolo and Daley (2000, *Chem. Geol.*) show that during the earliest stages of Miocene extension, basalts in the SW B&R have Nd, Sr and major element compositions consistent with their origin in the lithospheric mantle, but later melts derive from the asthenosphere, implying significant lithospheric thinning. By comparing the changing lithospheric thickness with the amount of upper crustal extension they conclude that the most highly extended regions in the SW B&R formed by pure shear (crust and underlying mantle extending simultaneously). Wang *et al*. (2002, *JGR*) use the major element and rare earth element compositions of late Cenozoic basalts from the B&R to place constraints on present-day lithospheric thickness and asthenospheric temperatures. Using Fe, Na and the slope of the heavy REE they are able to constrain the derivation depth of the basalts, which correlates with geologically and geophysically inferred base of the lithosphere. They show that the margins of the B&R, which experienced the most extensive lithospheric thinning, are underlain by normal-temperature asthenosphere, whereas asthenosphere underlying the relatively thick lithosphere of the central B&R, is ~200^{o}C hotter and reflects active upwelling.

Studies of mantle xenoliths carried in basalts of the East African Rift (EAR) document the effects of rifting on Archean tectosphere (Chesley *et al*., *Geochim. Cosmochim. Acta*, 1999). In northern Tanzania, Re-Os isotopic data indicate that ancient lithosphere is preserved to depths of 130 km, but between 130-150 km depth the lithosphere is either younger (Proterozoic) or is Archean lithosphere that has been modified by rift-related magmas. This lithospheric thickness inferred from the xenoliths correlates well with that inferred from tomographic data, and implies that the cratonic lithosphere has been thinned on the craton margin by about 100 km (compared to the thickness of craton center). The deepest and hottest sample has a composition similar to asthenospheric mantle yet has an elevated $^{187}Os/^{188}Os$ composition, suggesting its derivation from a mantle plume. This is the first geochemical evidence for the existence of a mantle plume beneath the East African Rift, thus supporting the active rifting model of extension.

ADVANCING OUR KNOWLEDGE OF THE CONTINENTAL LITHOSPHERE: WHAT DO WE NEED TO KNOW?

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Questions regarding the rheology and structure of the continental lithosphere continue to arise due to the acknowledgement that the theory of plate tectonics does not adequately describe their behavior. The continental lithosphere appears to be weak compared to what is defined by plate tectonic theory. We observe deformation within continental interiors and transient coupling/decoupling between crust and mantle. In addition, when we consider convergent plate margins that involve continental lithosphere, the role of overriding plate continues to be an important but relatively unknown, boundary condition. One vital link, which is currently being recognized by several disciplines, is the one between rock rheology and observations of continental lithosphere sampled by seismic data. Interpretation of the data requires understanding the range of possible behavior of rocks under different deformation conditions. One example comes from seismic anisotropy measurements and their interpretation. Data collected from mantle regions in convergent margins (e.g., Fischer) imply that the direction of mantle flow can be derived from anisotropy measurements. Seismic anisotropy, however, can arise from a variety of different mineral behavior. Currently, interpretations assume that olivine deforming under dry conditions explains anisotropy because of crystallographic orientation that aligns itself parallel to the main mantle flow direction. Recent rock deformation results on olivine (e.g., Karato) in the presence of water have found that under conditions of 'wet' rheology, the relationship between crystallographic orientation and mantle flow is changed. As a first order result, we recognize that we need to more fully understand the range of basic mineral physics and rheology of rock material before we can completely understand what the seismic data are telling us. In regard to continental interiors, we know that improvement in our understanding of continental tectonics necessitates a detailed and extensive study of the rheology of continental crust and mantle.

Future research directions require an interdisciplinary approach to major questions of continental lithosphere behavior. Significant research focus areas, which will increase our understanding of continental tectonics and anisotropy, lie in the study of: (i) mechanisms of strain localization what initiates decoupling in the continental lithosphere; (ii) the relationship between deformation and fluid flow; (iii) the relative strength distribution in the continental lithospheric column; (iv) understanding feedbacks between rock deformation, fluid transport, and elevated fluid pressures (pore pressure); and (v) determining how experimentally derived rheologies scale to deformation of mineralogically complex continental rocks. New directions in research require us to perform deformation experiments with an understanding of the complexities that arise due to mineral reactions, which are chemical in nature. Ultimately, theoretical analysis of these complex processes requires an understanding of the connections between mechanics and thermodynamics to further the use of rock rheology in considering the full nature of continental tectonics.

INTEGRATING EXPERIMENTAL MINERALOGY AND PETROLOGY RESEARCH INTO THE UNDERGRADUATE CURRICULUM

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A critical component of the Solid Earth Sciences is the fundamental study of rocks and minerals. Throughout the country, Earth Science departments are cutting back or eliminating mineralogy and petrology from the geology curriculum. Rather than being superfluous and "dead" disciplines, mineralogy and petrology are primary specialties within the Solid Earth Sciences and are critical for any endeavor to understand the earth and terrestrial planets. Mineral science and petrology courses must remain a cornerstone of any undergraduate geology program and the impact of these subjects on other disciplines, both within and outside of geology, should be emphasized. The mineral sciences, for example, are more important today than ever before with applications in technology, industry, and the health of society, in addition to traditional applications in petrology and other geology subdisciplines. While educating students and communities about the importance of Applied Earth Sciences is essential, fundamental scientific research within the Solid Earth Sciences must not be neglected. This combination of fundamental and applied research and teaching is critical to our greater understanding of the earth and we must promote these studies to future generations of Earth Scientists.

One way to foster interest in the Solid Earth Sciences within geology and the broader community is through the integration of teaching and research at the undergraduate level. As an experimental petrologist, I am keenly interested in research in fundamental aspects of the chemistry and physics of Earth materials. As a new faculty member at a primarily undergraduate institution, I am also devoted to the education of undergraduates and their training in the Solid Earth Sciences. With these interests in mind, I am working to establish an experimental petrology laboratory for research and teaching. Numerous faculty members successfully integrate experimental petrology research with teaching across the country. This is a new and ongoing endeavor for me and I hope to model others' success. Experimental petrology research is new to my institution and developing a productive research lab and becoming a successful educator are welcome challenges for me. By establishing an active experimental laboratory, new opportunities for undergraduate research and teaching possibilities are created. These activities can be integrated with field-based research and a better, more innovative undergraduate curriculum can be established. Improved teaching and learning outcomes will emphasize the importance of the Solid Earth Sciences in understanding our planet. This fosters an enhanced and long-term interest in the Solid Earth Sciences that can translate into a supportive community of graduates, some of whom may devote their professional lives to the discipline. Ensuring the development and preservation of devotees and supporters is critical for maintaining the long-term health and growth of the Solid Earth Sciences.

INTEGRATING RESEARCH, TEACHING, AND FACILITIES DEVELOPMENT FOCUSED ON THE PHYSICS AND CHEMISTRY OF SUBDUCTION

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Earth is a unique planet in four ways: 1) presence of life; 2) abundance of free atmospheric oxygen; 3) presence of liquid water; and, 4) existence of subduction zones and plate tectonics. These characteristics define (respectively) its biosphere, atmosphere, hydrosphere, and 'silicasphere' (silicasphere = solid earth above the core). Subduction plays the central role in controlling the behavior of the silicasphere. Subduction makes the plates move and collide, and is responsible for the formation of juvenile continental crust at convergent plate margins. The sinking of lithosphere at subduction zones drives mantle convection, including mantle upwelling beneath mid-ocean ridges. Subduction also is responsible for dynamic chemical equilibrium between the surface and the deep interior, as it delivers materials from the surface that mix with mantle and return to the surface over timescales of 10^6 to 10^9 years. In addition to playing the central role in the most important Solid Earth Processes and in maintaining equilibrium between the silicasphere and the other '-spheres', subduction affects society directly, via earthquakes, explosive volcanism, and formation of economic deposits. Any plan to develop an integrated approach to research, education, and facilities development in the Solid Earth Sciences should have the understanding of the physics and chemistry of subduction as a top priority. Not only is the problem of subduction uniquely important for understanding the Solid Earth, it is uniquely capable of strengthening the Solid Earth Sciences because the effort requires interactions among a diverse group of Earth Scientists, including volcanologists, structural geologists, geophysicists, geodynamicists, geochemists, experimental petrologists, and fluid flow experts, as well as scientist with computational and visualization expertise. NSF should stimulate this effort by encouraging interdisciplinary and international opportunities for this interaction. One objective could be the development of a 'Suduction Zone Community Model' along the lines of the very successful effort to develop a 'Global Climate Community Model'. Development of a community model for subduction zones, emphasizing shared development and testing of a wide range of models about processes related to subduction, would be a remarkable accomplishment in its own right as well as representing an essential module in an 'Earth System Community Model'.

Whereas research objectives should be primary, there must be a strong commitment to transmitting knowledge about subduction to future citizens and scientists. Subduction is not taught well in the schools at present, but this should not be hard to change. Good teaching about the physics and chemistry of subduction and related processes may be uniquely capable of fostering student interest in these fundamental sciences as well as Solid Earth Science. Separate programs should be designed and implemented for secondary and high school science education and for undergraduate and community college education. This could perhaps be most effectively accomplished by establishing informal 'Subduction Academies' for in-service and pre-service science teachers and instructors of large undergraduate introductory and service courses.

Applications of Solid Earth Sciences to an Integrated Freshman Engineering Curriculum

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All first-time freshman students in all engineering disciplines are required to enroll in the common two credit-hour course GE 115: Professionalism in Engineering and Science. This course was established four years ago as an attempt to integrate the freshman experience in the basics of engineering and science methodologies. Curriculum has been designed to include aspects of all engineering disciplines offered at SDSM&T but to focus on the fundamentals that are common to all disciplines. Teaming, and working as an integral member of a functional Team, is also emphasized for writing assignments and for the two laboratory experiments conducted during the course.

Laboratory work involves introduction to working in a laboratory, setting up an experiment (requiring the skill to ask the right questions), collecting data, and basic analysis. In the past years, we have focused on basic first-order systems that have been, for the most part, physics applications (determination of length of bungee cord for a given mass and fall distance for example). Planning for the fall 2003 lab project is currently underway. It is my intension to, for the first time, bring in a component of Earth Science exposing all students to our local physical environment. We are tentatively planning on having teams design and build small instrument packs that will be launched on a weather balloon. Instrumentation will include temperature, humidity, dew point, etc. A small camera will be flown as well. Photos of Earth from ~80,000 feet will provide a unique opportunity to involve Solid Earth Science into this project. This project is being modified from a NASA workshop.

Attending this conference will aid in the design of this project to ensure that the students will not only receive engineering methodologies and techniques but gain an appreciation for how engineering can be used to study Earth Science. GE 115 has an annual enrollment of ~360 students. One goal is to expose these students to Earth Science and geology; information gained from this workshop can become a vital educational component for our program.

Role of Continental, Crustal-Scale Faults in Evolution of Continents

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Large-scale basement faults play an important role in a wide range of processes within continental crust, and crustal-scale fault systems are a major factor in the attributes of continental crust. Furthermore, many recognized faults have a long, episodic history of reactivation with slip-sense in diverse directions, while others remain enigmatically quiet. Documentation of the large faults rests on data from outcrop mapping and structural fabrics, drill data, seismic reflection profiling, potential field mapping and modeling, and deep geophysical surveys. The integration of these data for structural interpretation with stratigraphic/sedimentologic studies of syn-faulting strata is essential to properly document time and sense of motion through time. In the context of EarthScope, such an integrated analysis of basement faults will blend interpretations of crustal structure, plate motions, and intraplate stress distribution with kinematic history of specific faults that are part of the larger system.

During continental rifting and breakup, extensional crustal-scale faults frame the new continental margin; however, it is evident that extensional faults affect a wide expanse of crust, not only establishing the continental shelf edge but also spawning microcontinents and dissecting the continental crust as "failed" rifts inboard from the actual margin. Some fault systems have been traced entirely across the continent (e.g., Thomas and Baars, 1995, Oklahoma Geological Survey; Marshak and Paulsen, 1996, *Geology*), and parts of these extensive fault systems have been reactivated episodically with differing slip senses through a long span of geologic time (1.8 + b.y.). Orogenic foreland basins, as well as sub-décollement foreland thrust belts, commonly include systems of distinctive normal faults, down toward the hinterland (e.g., Bradley and Kidd, 1991, *GSA Bulletin*), possibly recording flexural extension in response to tectonic loading, reactivation of older rift-stage faults, or stretching during flexural bending. Distribution of modern earthquakes in intraplate settings has inconsistent and sometimes obscure relations to older crustal-scale faults, indicating the complexity of intracratonic fault reactivation.

My experience in continental fault systems includes the late Precambrian-Cambrian rifted Iapetan margin of Laurentia and of the Argentine Precordillera; intracratonic fault systems of the Mississippi Valley and Southern Oklahoma; reactivated basement faults of the Ancestral Rockies; basement fault systems beneath the basal décollements of the Appalachians, Ouachitas, and Montana Rockies; basement faults of the mid-continent, including the region of the New Madrid seismic zone; and Triassic extensional structures of the modern Atlantic-Gulf margin of North America. My approach has combined structural geology, geophysical imaging of faults, and stratigraphy/sedimentology to constrain times of fault movement. I am interested in pursuing further research on the kinematics and timing of crustal-scale basement faults in the context of the continental-scale setting.

FORMATION OF AN INTEGRATED TECTONICS FORUM (ITF)

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Why is a new interest group needed? An integrated approach is critical to understanding the Solid Earth. At present, the Solid Earth Science geoscience community is quite fragmentary and progress in Solid Earth Science research is limited by sub-disciplinary boundaries. Specific individuals and programs have been successful at integration, but are either restricted to distinct sub-disciplines (IRIS and EarthScope) or geographic areas (Ridge, Margins). While the results of these programs are scientifically important, they do not attempt the scale of integration proposed by the ITF.

We propose that the following sub-disciplines should be involved within the ITF: Basin Analysis, Earthquake Geology, Experimental Deformation, Geochemists, Geomechanics, Geophysics, Petrologists, Petrophysicists, Structural Geologists, and Volcanologists. We anticipate that this workshop will allow us to:

1) codify a leadership structure for the ITF;

2) develop initial priorities for the ITF to re-evaluate on a regular timetable;

3) delegate tasks from the ad-hoc group to an established forum;

4) put in place a structure to organize future meetings (the ITF); and,

5) determine how to best relate to other organizations in the Earth Sciences (GSA, AGU, AGI) and to integrate with other NSF-sponsored initiatives.

At present, we foresee the main outlets for the ITF being those below. This list will focus discussion, and should enable us to achieve our goal of integration.

1) Meetings - A one-day annual meeting to be held on the day immediately prior to the Annual Meeting of GSA. The subject of the meeting will vary from year to year, focusing on particular issues. Other meetings held at other times as necessary (e.g., on timely subjects that are not adequately addressed by the current set of scientific meetings, particularly to stimulate interdisciplinary approaches to solving problems in the Solid Earth Sciences; workshops to teach Solid Earth Scientists how to use technology and information systems).

2) Publications - The ITF would work in association with existing publication outlets.

3) Dissemination of information in the Solid Earth Sciences – A goal of this project is to create data services for the Solid Earth Sciences. We will work on this aspect of the project in cooperation with Geoinformatics and DLESE. However, our interest is to produce the broad-based support and integration of various geological needs into these initiatives. ITF will sponsor a series of workshops on creation of digital data in a common format. This is critical for the Solid Earth Sciences Community.

4) Enhanced Communication - The ITF will explore digital methods of communication among its community, such as list servers, web space, group sites, etc.

5) Education/Outreach - The ITF will: foster an integrated approach to Solid Earth Sciences education that combines the contribution of geology, geophysics, geochemistry (etc.) into a cohesive understanding of Earth processes in a Earth System framework; improve the integration of research and education in the Solid Earth Sciences by promoting successful models for educational components of research projects, transfer of research results to geology courses and the public, and fostering cohesive projects in the Solid Earth Education; increase use and contribution to the Digital Library for Earth System Education (DLESE).

Integration With Existing Infrastructure:

ITF is not a substitute for the GSA Structural Geology & Tectonics Division (SG&T). The ITF will work with SG&T, as well as with other divisions of GSA, sections at AGU, and other organizations as appropriate.

ITF is not proposed as an NSF initiative at this stage. It is advanced as an advocacy group for integration in the Solid Earth Sciences, setting of priorities, ensuring facilities and promoting synergy among research, education and outreach.

ITF may move into a role similar to CUHASI, which is a newly formed, integrated group for advocacy of hydrological science.

ITF may exist largely as a digital entity with discussion and voting done electronically.

ITF may form the basis of an International Society of Integrated Solid Earth Sciences, which does not exist at present (IASTG is the most similar organization, but is limited to structural geology and tectonics).

SEIZE: The Seismogenic Zone Experiment as an Example of Synergistic Research Across Earth Science Disciplines

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The Seismogenic Zone Experiment (SEIZE) Initiative of the MARGINS Program at NSF and the affiliated Integrated Ocean Drilling Program (IODP) SEIZE Initiative can serve as up-and-running models for the kind of crosscutting program contemplated at this workshop. SEIZE is focused on investigation of the great (M ~ 8) earthquake generating portion of the subduction zone plate interface. Topics addressed by SEIZE include the relationships among stress, strain, and fluid properties within the portion of the plate interface capable of producing great earthquakes, the nature of the updip and downdip limits to seismogenesis and co-seismic rupture, the mechanisms of tsunami generation, and the partitioning of deformation between seismic and non-seismic processes. SEIZE projects funded through MARGINS to date include studies in earthquake seismology, structural geology, laboratory rock mechanics, sedimentology, hydrogeology, and geochemistry. International programs are also addressing specific SEIZE topics, and three recent Ocean Drilling Program legs have been devoted in whole or part to SEIZE objectives.

What factors contribute to the success of SEIZE as a multidisciplinary program that transcends individual investigators? Clearly, the fact that it is organized around investigating a fundamental geologic process–fault mechanics–promotes multi-disciplinary investigation. Less obviously, but crucially, studies are limited to specific focus sites chosen at workshops early in the development of MARGINS: the Costa Rica/Nicaragua and the Nankai Trough (SW Japan) subduction zones. The difficult but absolutely necessary focus site selection was accomplished largely because the scientific cultures of both the seismological and marine geology/geophysics communities are (a) generally not tied to a specific region, and (b) used to the necessity of working together due to limited access to pooled resources (e.g., ships and seismometers). The final element is that MARGINS is organized as its own program with a clear mandate, crossing the traditional divide between Ocean and Earth Sciences at NSF.

Research and Education priorities in the Solid Earth Sciences

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Recent world events emphasize the need for Solid Earth Scientists to work together across sub-disciplines to better understand the interconnectedness of Earth processes and the effects on the inhabitants of Earth. Furthermore, the frequency of media coverage concerning items related to Earth processes highlight the obligation of Solid Earth Scientists to educate the general population about these processes.

Research Priorities

Several interconnected topics should be considered priorities in the Solid Earth Sciences:

1. A coordinated and collaborative effort should be undertaken to understand the evolution of the crust and mantle with a focus on the interconnectedness of tectonic, geochemical and biological cycles.
2. An effort to discover additional conventional resources and to explore possible alternative resources with a focus on the conservation and reuse of existing resources.
3. A concerted effort to monitor and remediate water resources with a focus on the preservation, remediation and wise use of existing resources.
4. A cross-disciplinary assessment of climate change focusing on links between various disciplines within the Solid Earth Sciences. This topic should also include recommendations for how humans can minimize and adjust to climate change.

Education Priorities

Because research and education are inextricably linked, the above priorities should carry over into education. Three educational priorities should be considered with research priorities in mind:

1. Research, teaching and learning should be integrated at all levels (pre-college, undergraduate and graduate). There should be emphasis placed on the interconnectedness of all sub-disciplines within the Solid Earth Sciences.
2. Introductory classes at the university/college level should be aimed at providing the general public with a broad understanding of Earth Processes. Most students at this level will not become majors, but it is an excellent opportunity for educators to provide a basis for informed decisions about the Earth.
3. Earth Science and Education departments should develop collaborations to provide pre-college teachers with the information and insight they need to teach at the high school level.

Areas in Which I Can Best Contribute

My research focuses on the evolution of continental crust and the igneous and metamorphic processes that modify crust. In particular, I am interested in the role that volatiles play in the processes that modify crust in arcs. My experience involves the petrology and geochemistry of continental arc batholiths. I am very interested in exploring ways to integrate my knowledge and experience into a larger global model. I also have attended a number of workshops dealing with education and modification of geology curriculum and can contribute some of my experience to the discussion of education priorities.

INFRASTRUCTURE NEEDS FOR STUDIES OF THE CRUST AND DEEP EARTH

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New research directions in petrology/geochemistry and structural geology/tectonics involve integration of field, analytical, and experimental techniques, and require new facilities – or increased access to facilities – and opportunities to acquire expertise. Some examples of promising areas of research and the tools needed are:

Microbeam analytical techniques. Improved capabilities are needed for analysis of natural and experimental samples through refinement of ultrahigh-sensitivity microbeam techniques (SIMS, LA-ICP-MS, AEM). Improvements in spatial resolution and sensitivity of analysis of trace elements and isotopes are critical for improved understanding of the geochemical evolution of the crust and mantle and for relating rock chemistry to Earth dynamics. Particularly crucial is increased analytical capability for analysis of trace volatile (C-O-H-S-N) constituents.

Advanced techniques for textural analysis provide novel information about igneous and metamorphic petrogenesis and tectonic processes. A relatively low-tech application of electron microscopy to studies of deformed rocks is electron back-scattered diffraction (EBSD), an SEM-based method for rapidly and accurately determining crystallographic orientations. Applications of EBSD methods to olivine and pyroxene-bearing rocks are well established; fewer studies of crustal rocks have been carried out. EBSD can be used as a basic tool for obtaining data on crystallographic preferred orientation, but it can also be used to investigate questions about crystal growth and deformation mechanisms. There is great potential for advances in SEM-based technology; perhaps the most interesting new technical developments will occur in collaboration with experimentalists who require very fine-scale resolution and information about mineral structures as a function of temperature, pressure, and deformation conditions. Attention should be given to spreading knowledge about the technique, including use of the software needed to utilize/interpret the data.

Experimental studies of the deep Earth. Unsolved questions about the chemistry and physics of the deep Earth require development and application of new techniques for ultrahigh-pressure research. Japanese researchers have pioneered new technology and techniques (e.g., sintered diamond anvils) for large-volume experimental studies to pressures prevailing in the lower mantle, and these can be employed and further developed. We need to support and extend our capabilities for doing *in situ* high-pressure research on synchrotron beam lines. We need reliable techniques for doing quantitative experimental deformation at high pressures, possibly by further development of the D-dia apparatus or possibly by more novel techniques.

An important issue is whether some laboratory facilities could be concentrated in centers or spread out among individual investigators. This debate should take into account the specific infrastructure requirements/applications for each type of facility.

PRIORITIES FOR THE FUTURE OF THE SOLID EARTH SCIENCES

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Micro-résumé and personal statement
In Fall 2002 I started as an Assistant Professor at the University of Missouri-Columbia, following three years at the University of Illinois where I taught classes for non-majors, geochemistry, structural geology, and graduate petrology, as well as researching Neoproterozoic tectonics in Brazil. In brief, I am a Solid Earth Scientist with a broad background in the traditional disciplines of petrology, tectonics, geochemistry, and structural geology, but I view these as complementary components of research into Solid Earth Systems. Below are my ideas regarding the future of solid Earth Sciences, which I intend to develop and put into practice as I advance my career. I hope to collaborate with geologists from different disciplinary and institutional backgrounds in promoting research in our subject, educating future generations, and explaining to the public what we do and why they should support it. If my generation does not, there may not be a next generation!

Research
New advances are most rapid at disciplinary boundaries, but this does not mean that these boundaries are a good thing! There are two types of boundary to be overcome: (i) Intellectual (intradepartmental) boundaries, e.g. 'soft-rock' vs. 'hard-rock' geology. This divide is greater in Europe than in the USA, but it should be removed since it relates to historical techniques rather than Earth System processes (e.g. sedimentology and metamorphism are intimately related via exhumation). No particular branch of Earth Science will wither if the interrelated nature of sub-disciplines is stressed; as one rises to temporary ascendance, other branches provide essential support. (ii) Interdepartmental boundaries. These are administrative in nature, and may require adjustments on the part of universities and funding agencies (both for allocating funds and reviewing proposals). I believe that research at the frontiers of geology and other subjects (often other departments) is vital to the future of Earth Science, but that our core subject area must not be forgotten in the rush to colonize new intellectual territory.

Teaching
Classes for non-majors are probably the most effective way that professors at large schools can reach a sizeable non-specialist audience. Collaboration with colleagues whose primary focus is education should produce courses that will stimulate students (and possibly attract new majors), and emphasize the importance of our subject and its supporting sub-disciplines to future taxpayers. Classes for majors need to prepare students for a career in applied geoscience, or for graduate school. Both require a thorough grounding in Solid Earth Sciences but with the flexibility to include classes from other departments. The emphasis should be on methods and applications, not on encyclopedic knowledge. Teaching students how to learn should be a basic goal of every degree program, irrespective of subject.

Outreach
To secure funding for the Solid Earth Sciences, there must be a public perception of the usefulness of such research. An important component of increasing public interest is outreach: this is the responsibility of all of us. Establishing a formal group for promoting the Solid Earth Sciences is an excellent first step. I also feel very strongly that, whereas volcanoes and earthquakes are two of our best advertising platforms for increasing public awareness of geology, we must also promote the less glamorous aspects of our subject, and the importance of maintaining fundamental as well as applied research programs must be stressed.

Volcanic Hazards Continue to be a Priority in Solid Earth Sciences

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As Earth Sciences strive to be more relevant to society, geologic hazards (including earthquakes and volcanic eruptions) should be even more prominent on the agenda. Volcanic eruptions have claimed more than 200,000 lives in the last 200 years, and more than 25,000 lives in the last 20 years. The past decade has seen major advances in volcanology and some accurate predictions of eruptions. With rapid accumulation of knowledge and data, it seems that major breakthroughs in understanding volcanic eruption are on the horizon. Understanding and predicting explosive volcanic eruptions are the ultimate goals in volcanic hazard mitigation. In addition to monitoring and investigation of individual volcanoes, priorities include:

Magma properties. Critical to the understanding of volcanic eruptions are the properties of magma, including viscosity, tensile strength, volatile (including water) solubility and diffusivity, yield strength, density, and other thermodynamic properties. Understanding each of these requires both experimental data and theory/model on how each depends on temperature, pressure, volatile content, dry melt composition, crystal and bubble contents, etc. For example, a large database is currently available on melt viscosity but no universal model is available to describe its dependence on melt composition, water content, temperature, pressure and crystal and bubble contents. Is this due to the inadequacy of models or the lack of critical data? For some other properties (such as tensile strength and surface tension of magma), experimental data are still lacking. Future challenges are to identify the critical needs and fill the needs to maximize the return. Theoreticians and experimentalists must integrate their effort in a concerted and systematic attack on magma properties. A workshop is planned to tackle the issues. A thorough understanding of magma properties will be critical also in understanding igneous processes (such as mantle and crustal partial melting) and products (such as igneous mineral deposits).

Modeling volcanic eruptions. In addition to magma flow and gas flow, there are many other component processes in a volcanic eruption, including bubble nucleation, bubble growth, and magma fragmentation. Modeling volcanic eruption requires modeling of some or all of the component processes, depending on the assumptions and sophistication of the model. The workshop on magma properties will assess the assumptions and applicability of these models and coordinate future efforts. Understanding magma properties is a prerequisite for modeling the dynamics of volcanic eruptions.

Tectonic settings of volcanic eruptions. Although the tectonic settings of most subaerial volcanic eruptions are well understood (at plate boundaries, or at plumes/hotspots or continental rifts), a small but significant fraction of intraplate volcanos are still an enigma. In Northern China, Korea, and Mongolia, numerous volcanic fields dot a vast area. One of the largest Holocene eruptions, the 1199/1200 eruption of Baitoushan at the border of China and Korea, is in this area. Unlike intraplate volcanos in East Australia and West Interior of the United States, the volcanos are not limited to linear bands. Solving the mystery of these volcanos requires an integrated approach including: thermobarometry of crustal and mantle xenoliths to infer how geotherms have varied with time and space; age of volcanic eruptions and eruptive history; tomography and seismic imaging of the crust and mantle (the role of subduction slabs and other thermal anomalies); geodynamic and tectonic modeling of the role of Indo-Asia collision to the south and subduction of the Pacific plate to the east, and age relations between volcanic eruptions and these events; and, petrological and geochemical studies.