Introduction: Why CIDER?

This proposal is for funding to support the first five years of a new "synthesis institute" called CIDER (Cooperative Institute for Deep Earth Studies), which has no precedent in the area of solid Earth studies. The Institute will undertake a mission of developing and educating a new generation of Earth scientists. It will also provide an environment for studies requiring a concerted effort of leading researchers from different areas of Earth sciences. The purpose is to facilitate the work of individuals, or small groups of researchers, in contrast to a "Big Science" approach. The ultimate goal is to understand the origin, evolution, and dynamics of the Earth and planets. The practical objectives are to:

- Address the most important and difficult problems that have defied solution thus far by fostering collaborations that can fully utilize existing knowledge and technology
- Provide a seed-bed for ideas that will identify the next generation of critical experiments and observations, and build support and appreciation for them
- Provide a venue for cross-disciplinary education of scientists at all career levels.

It has been 35 years since the acceptance of plate tectonics theory, but no definitive agreement has yet been reached among geoscientists on the fundamental nature of the global dynamic processes that drive plate motions. There are still vigorous debates about the proportion of heat coming from the core versus radiogenic heating in the mantle; about the degree to which the 670 km discontinuity impedes whole mantle circulation; about the origin and even the existence of mantle plumes; the chemical/thermal nature and origin of heterogeneity in the deepest mantle; the nature and importance of coupling between the mantle and the core (figure 1).

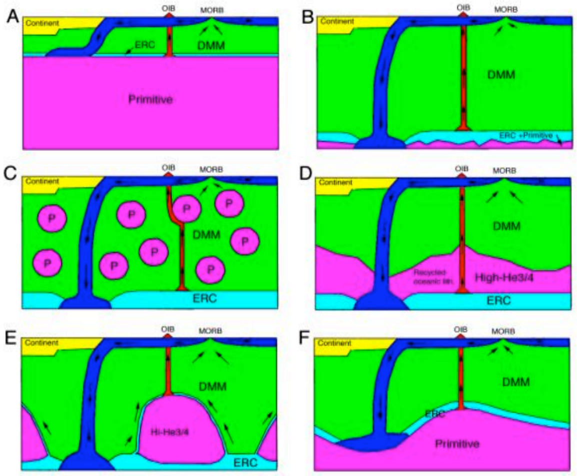


Figure 1. Cartoons representing different end member models for the location of chemical reservoirs in the mantle and their relationship to dynamics. Blue: oceanic plates/slabs. Red: hot plumes. Purple: "primitive" mantle.DMM: Depleted Mantle; ERC: enriched Recyled Crust. (A) Typical geochemical model layered at 660 km depth. (B) Homogeneous mantle except for some mixture of ERC and primitive material at the base. (C) Primitive blob model with added ERC layer; (D) Complete recycling model. (E) Primitive piles model. (F) Deep Primitive layer. *From Tackley(2000*).

The core itself represents an immense puzzle. More than 50 years after the concept of geodynamo has been accepted as the source of the main magnetic field, we still do not understand the details of its workings, some of which, such as the current decay of its dipole field, may have societal implications. Its chemical composition, in particular the content of lighter elements, are still being debated, with an impact on the overall composition of the lower mantle. Even radial variation in its physical properties such as the sometimes reported anomalous velocity (and, presumably, density) gradients at the top and bottom of the outer core present troubling questions; if their existence is confirmed, it would imply chemical stratification of the outer core. The time of the formation of the inner core is very uncertain and we do not understand how it developed its anisotropic properties.

Each of the disciplinary communities appears to have adopted a favorite set of assumptions, based largely on a limited view of the problem as afforded by their own observations, and on their understanding of the capabilities and limitations of other disciplines. In the meantime, tremendous progress has been made within these different fields in the quality and quantity of data collected, for example through the IRIS program in seismology, through state of the art analytical facilities in geochemistry, advances in computational technology in geodynamics, or through access to advanced accelerator facilities in mineral physics. The types of problems that arise from deep earth studies are challenging to solve for several reasons:

- Extreme P-T conditions, impure materials, and complex systems with many interacting factors operating over many orders of magnitude space- and time scales.
- Progress requires broad knowledge of multiple fields, more than any one expert can encompass.
- Individual fields tend to operate on their own, not always recognizing the need for communication across disciplines and often hampered by the lack of a common language.
- Problems push the limits of both knowledge and technology.

A new generation of disciplinary tools, that will provide unprecedented views of the Earth's interior, will soon be available to the geoscience community, through major infrastructure efforts that are currently under way, or in the planning stages. For example, Earthscope, and more specifically the USArray program, will provide seismologists with a high resolution "window" into the deep mantle and core with broadband seismic waveform data over the North American continent from densely spaced receivers. The COMPRES program will allow mineral physicists to perform advanced high pressure and temperature measurements on mineral properties at conditions relevant to the Earth's deep interior and compare them with results of "first principles" calculations. Other initiatives aim at providing geodynamicists with a unified, state of the art framework for convection computations, seismologists with ocean-bottom stations in order to achieve truly global coverage of the Earth, and researchers in geodesy and geomagnetism with satellite observations that are likely to revolutionize these fields. In geochemistry, the enormous volumes of high quality chemical and isotopic data gathered over the past 25 years are now assembled into systematic and broadly accessible databases (GEOROC and PETDB), and ever-improving measurement techniques are providing new perspectives on mantle processes at scales from micrometers to thousands of kilometers. It seems that our community has been building "big-science" data gathering tools but uses only "small-science" approaches to data interpretation. As a result, only partial return on these investments can be expected. In other fields of science, such as Astronomy and Atmospheric Sciences, this issue has been appreciated and addressed.

To some extent, Gordon Conferences on the Earth's interior, reinstated 7 years ago, and the biennial international SEDI Conferences, provide a forum for the exchange of information and latest ideas across the different disciplines. They are, however, short-lived, covering all possible topics in just a few days, and therefore can provide only glimpses into other fields for specialists of any given discipline. The CSEDI program, launched about ten years ago (O'Connell et al., 1993), helps individual investigators representing two or more disciplines to jointly address specific research problems. While very valuable, CSEDI in its current form does not address the need for a major leap in the level of research collaboration and education of scientists across fields.

We perceive a need for a long-range intellectual framework that will promote more effective cross-fertilization of the disciplines. Not just funding, but a properly designed and equipped venue, whereby senior and junior scientists alike can thoroughly educate each other about the approaches, the fundamental achievements, the future potential and limitations of each discipline, develop a common language and then design, plan, and even carry out the collaborative research that is required to achieve scientific breakthroughs. Given the enormous amount and diversity of observations becoming available, a quantum leap in the understanding of the constitution and evolution of our planet can be expected, if we can identify and focus on the key issues, and how best to address them by fully utilizing complementary disciplinary data and modeling tools.

Although we can expect new observational constraints from existing and planned programs for data collection and archiving as noted above, there is a need for continuing dialogue to identify key data that are needed from the next generation of field and laboratory experiments. At present there are few opportunities for developing new types of data sets, because of the expense involved for major projects and the difficulty in developing a community consensus about what data are really needed and how best to acquire them. The difficulty in targeting directions worthy of major new projects and in developing broad support for them, is a continuing, self-imposed limitation of the Geoscience community.

With the increasing scope of our investigations, it is necessary to attract more talented undergraduates and graduates to consider careers in geophysics, geochemistry and mineral physics. In addition, it is becoming increasingly difficult to properly educate them in, or even expose them to, the breadth and depth of subject matter in deep Earth studies. Most educational institutions do not have adequate faculty numbers to cover the breadth of the discipline.

The framework that we propose consists of an institute (CIDER), which at least in its early manifestation, will be modeled after analogous institutes in other fields, for example the Kavli Institute of Theoretical Physics (KITP) in Santa Barbara, and the Institute of Mathematics and its Applications in Minneapolis. These comparison institutes, however, serve large communities of theorists and consequently are not exact templates for CIDER. An institute for deep Earth research needs to be adapted to the geoscience community, which has fewer scientists (depending on how broadly one defines the boundaries of the subject matter) and significant experimental and observational components that lead to more challenging communication issues.

There have been significant recent attempts at an interdisciplinary approach to key global research problems. For example, geochemical tracers have been added to mantle convection simulations, and seismic tomographic models and patterns of seismic anisotropy have been used to constrain geodynamic flow models. These efforts signify that the community is ready to move to the next level of integration, and also illustrate the need for the proposed institute. Too often, these imaginative attempts at integration are heralded for their failures rather than as a window to what might eventually be possible.

The CSEDI program exists already to encourage cross-disciplinary research by stipulating that it be a major part of any proposal submitted. The proposed CIDER institute is not viewed as interfering with the objectives of CSEDI, but instead as providing an additional, and critical, new mechanism to accomplish the goals of CSEDI. CIDER would complement and substantially broaden CSEDI impact through a range of specific activities. An overview of the goals and activities is provided in Table 1. Additional detail is provided in the management plan.

The proposal is submitted by the PI (Barbara Romanowicz). It has been developed with active participation of other members of the CIDER Steering Committee, who represent each of the main relevant disciplines. Insight in Geomagnetism was provided by Cathy Constable.

Table 1: Outline of CIDER goals, activities, products and location

Overall Goals ☐ ☐ ☐ ☐ ☐
☐ Provide common facilities and environment for integrative, ☐
☐ cross-disciplinary "thinking" and research work planning ☐
☐ Provide an intellectual home for the science resulting from disciplinary
☐ infrastructure activities ☐
☐ Develop the next generation of geoscientists at graduate and ☐
☐ post-doctoral level. ☐
*Research activities** ☐
☐ Long Programs ☐
☐ Three-to-six month programs; resident visitors for the duration
☐ Working group meetings ☐
☐ 1-week workshop followed by 3-4 day working meetings at 6-mo. intervals
☐ *would include ☐
☐ post-docs (6 months - 2 years) ☐
☐ visitors (3-12 months) ☐
☐ . access to local and remote facilities ☐
Development of tools ☐
☐ Education and training in methods outside own disciplines ☐
☐ Develop interfaces between codes/methods ☐
☐ Framework for integrative community efforts (such as REM, GERM...)
Educational activities ☐
☐ Workshops; short-courses (with product: book, web site). ☐
☐ Summer schools (2005-). ☐
☐ Science journalism program. ☐
☐ program for K-12 students; ☐
☐ connection to undergraduate programs. ☐
Location requirements ☐
☐ Easily accessible by air ☐
☐ Critical mass of intellectual resources ☐
☐ Adequate facilities: labs/computers etc.. ☐

Table 2: CIDER Steering Committee Members (see letters of collaboration in Appendix)

Name	Discipline	Affiliation
D. DePaolo	Geochemistry	U. C. Berkeley
A. Dziewonski (Chair)	Seismology	Harvard
S. Hart	Geochemistry	WHOI/MIT
R. Jeanloz	Mineral Physics	U. C. Berkeley
D. Kent	Paleomagnetism	Rutgers U. & Lamont
L. Kellogg	Geodynamics	U. C. Davis
M. Manga	Geodynamics	U. C. Berkeley
G. Masters	Seismology	U. C. San Diego
P. Olson	Geodynamics	Johns Hopkins
B. Romanowicz	Seismology	U. C. Berkeley
L. Stixrude	Mineral Physics	University of Michigan
E. Stolper	Geochemistry	Caltech
D. Weidner	Mineral Physics	SUNY Stony Brook

Results from prior NSF support

"Towards an Institute for Cooperative Earth Studies: Exploratory Workshops", NSF Grant #EAR-0215587, Amount: $150,000, Dates: 08/15/02-07/31/04 *CIDER Workshops - 1: Marconi Center 05/24-05/29/03; 2: UC Davis 08/08/03-08/10/03*:

Background

The May 24-29, 2003, Marconi Center CIDER Workshop brought together 77 researchers and educators representing the five disciplines involved in the study of deep earth structure and dynamics: geochemistry, geodynamics, geo and paleo-magnetism, mineral physics and seismology (http://www.seismo.berkeley.edu/cider/list03.html). The goal of the workshop was to define the function and form of CIDER. Participants were asked to think about the long term format and scope for the Institute. Is the perceived need for better cross-education among the disciplines in deep earth research valid? What key cross-disciplinary research topics can be identified for CIDER programs? Should CIDER activities be limited to a few weeks summer program centered on tutorials? In what other activities besides short courses, tutorials, multidisciplinary workshops should CIDER engage (i.e. databases, relationship to other initiatives such as CCIG, COMPRES...)?

The workshop featured review talks in each discipline as well as short presentations on selected topics (http://www.seismo.berkeley.edu/cider/presentations.html). The speakers were instructed to focus on "what we know and what we don't know", rather than their own latest research, and to highlight important research issues that require a multidisciplinary approach for further progress. There was ample time for discussion in each session. Each day was organized around one of the disciplines by members of the CIDER Steering Committee, specialists in that discipline. Afternoon break-out sessions gave participants the opportunity to discuss these issues. The program of the last 1.5 day of the workshop was modified to allow for a plenary discussion of the scope and format of the Institute and further discussion of the focus and organization of the July'04 Workshop at KITP.

Recommendations

There was widespread support for developing the Institute - not *only* focused on tutorials for graduate students. The Institute should *also* provide the framework for research-related activities. The idea that CIDER programs would foster new collaborations that might then lead to CSEDI type proposals seemed to be viewed particularly favorably. CIDER should not be about building databases or software development, but should work closely with groups that do that (such as CIG or GERM), providing them input on needs from a multidisciplinary point of view, potentially hosting some of their meetings.

The green light was given to those who initiated the CIDER concept to develop their vision and find the best way to implement it. The Steering Committee was enlarged by 2 members (Lars Stixrude, U. Michigan and Dennis Kent, Rutgers University) to achieve balance of disciplines and bring in representation of geomagnetic community. The CIDER Steering Committee has been entrusted with putting together the NSF proposal, and this, as well as the organization of the KITP Summer'04 program were further pursued at a smaller workshop held in Davis, CA, Sept 8-10, 2003. This second workshop was organized by L. Kellogg and B. Romanowicz and brought together ten members of the CIDER Steering Committee (Kellogg (UC Davis), Romanowicz (UC Berkeley), DePaolo (UC Berkeley), Dziewonski (Harvard), Hart (WHOI), Jeanloz (UC Berkeley), Manga (UC Berkeley), Masters (UC San Diego), Stixrude (U. Michigan), C. Constable (UC San Diego - representing D. Kent).

July'04 KITP Summer Program

The July'04 KITP Summer Program can be viewed as a "dry-run" for a "short" CIDER program, although the submission of the CIDER proposal to NSF should not be linked to the success of this particular initiative. This program was discussed throughout the Marconi, May'03 meeting, and a consensus was reached around the following theme: "Relating Geochemical and Seismological Heterogeneity in the earth's mantle". This theme broadens the original scope of the proposed KITP Program, maintaining much of its ingredients: going from seismological 3D models to geochemical observations necessitates input from mineral physics to provide the intermediate step of converting seismic heterogeneity to temperature (and/or composition). There is a clear role now for geodynamicists who can interpret the results in terms of dynamics. Variability among seismological models, mineral physics "conversion factors", geochemical observations could be explored in teams focused on particular cross-sections of the mantle, or regions, comprising specialists of all the disciplines. Further discussions and construction of the KITP'04 Summer Program were held at U.C. Davis, Sept 08-10, 2003.

The KITP Summer Program will be held at the KITP facilities in Santa Barbara, CA, from July 12, 2004 to August 6, 2004 (4 weeks). It is supported jointly by KITP and a complement from NSF/EAR to the CIDER Workshop grant. The Program consists of two parts: a tutorial part (first two weeks) and a workshop part (last two weeks). The purpose of the tutorial part is to familiarize participants (20 post-qualifying exam graduate students and 10 post-doc's) with the tools of geochemistry, geodynamics, mineral physics and seismology that can be used to unravel the properties of the Earth's interior, with a focus on multidisciplinary approaches and on the general theme of the Program. The workshop part will feature a limited number of talks: some introductory ones on the 1st day, progress reports from subgroups subsequently, leaving lots of time for discussion/work. The goal is to quickly develop specifictasks/activities. The participants will form several interdisciplinary groups of 4-6 participants, and brainstorm on novel ways to advance the science and initiate research in this direction. A series of original papers on novel interdisciplinary approaches may be a possible outcome of these activities, as well as research proposals to the CSEDI program. Application to the Workshop Part is open to post-PhD junior and senior researchers. There will be space for about 10 graduate students that will have participated in the tutorial part. The following list of topics has been selected to guide the activities of the Workshop:

- Mixing in the mantle: Cycling and recycling
- Spatial spectrum of heterogeneities: From observational constraints to theoretical interpretation
- The critical role of melting in Earth evolution
- Plumes, superplumes, and superswells: are they there?
- The role of the transition zone in Earth evolution
- Why is Earth the only planet that has plate tectonics?
- The role of water in the mantle dynamics
- What is the nature of thermal and chemical interactions across the CMB?
- Upper mantle processes linking geodynamics to geological, geophysical, and geochemical observables.

Applicants will be given the opportunity to propose other topics, and the final selection will be determined once the list of participants is established.

CIDER scientific goals: examples of key interdisciplinary problems that could be topics for CIDER programs

In what follows, we give examples of scientific themes which require a multi-disciplinary approach, some of them in part overlapping in scope. The essays have been written by the members of the Steering Committee, and we decided to preserve the original style of contributions, because we think that this contributes to illustration of the variety of possible approaches to problems. The nature of the essays is so general that we avoid citing specific references deliberately.

Example 1: The Scale Length of Thermal and Chemical Heterogeneities

Earth's mantle is chemically heterogeneous; the isotopic evidence for this is unequivocal (Figure 2). The likely scale length of this chemical heterogeneity ranges from the largest scale (1000's of km: upper mantle versus the rest of the mantle; the DUPAL anomaly, etc.), to scales at least as small as that of melting regimes (~10 km). Part of this heterogeneity may be lithological (peridotite versus pyroxenite/eclogite), and the scale lengths of these lithological heterogeneities could be very small (10's of meters; e.g. the veined or plum pudding model). Thermal heterogeneities may or may not be correlative with these chemical and lithological regimes, but clearly exist also on a range of scale lengths from 1000's of kilometers to mesoscale (100's of kilometers). The small-scale limit is constrained by thermal diffusion times. In part, the scale length of heterogeneities is related to the mode of their formation and the relative rates of destruction versus construction. Some heterogeneity may be inherited from the early days of earth's formation, while some are certainly formed as a result of ocean crust formation and subduction zone processing and re-injection. There may be a role for de-lamination of continental or oceanic lithosphere and there may be internal modes of chemical fractionation as well, though models for these do not exist at present. The isotopic evidence definitively shows that at least some classes of these heterogeneities have survived for billions of years. Consequently, one of the central issues of the scale length problem, and one that has defied many years of study, is the rate at which mantle heterogeneities are stirred or mixed out over time by mantle convection. Existence of lateral heterogeneity in the mantle is detected also through geophysical methods, including gravity, electrical conductivity but, most importantly, by seismology.

What tools do we have for visualizing these heterogeneities and determining their scale? Geochemists use melts derived from the mantle to assay the chemistry of the mantle that is supplying these melts. Since only mantle shallower than a few hundred kilometers melts, the geochemical evidence only poorly delimits the depths of origin of the mantle regimes prior to their ascent into the melting zone. Thus, geochemical mapping is largely plan-form in nature. In contrast, seismic techniques, in particular tomography, map heterogeneities in velocity in 3-dimensions, and have detected heterogeneity in the mantle at scale lengths from 10's of km (using scattering techniques) to hundreds or thousands of km (using tomographic techniques). On the background of radial variations in elastic and anelastic properties, which may be as large as 30% at the Moho, or even more extreme at the CMB and which are due to chemical differences or phase transformations, there are more subtle lateral variations on the order of a few percent that can be resolved at various scale lengths. It is however difficult to deconvolve these anomalies into compositional and thermal components. Seismic data, except for the amplitudes of the waves reflected and converted at discontinuities, are rather insensitive to density variations, which unfortunately eliminates one vital source of information on chemical vs. thermal heterogeneity. Comparison of lateral variations in P and S velocity may help distinguish the relative contributions of these two types of heterogeneity. At the present time, the resolution is however not uniform, making a rigorous comparison difficult: seismic images of the whole mantle are most robust for S velocity, albeit at relatively low resolution. For example, large-scale variations in S-velocity at depths up to 200-300 km are largely consistent with tectonics. Compressional velocity variations are best constrained in the lower mantle and rather incomplete at shallow depths, since the surface wave dispersion is relatively insensitive to P-velocities, except at crustal depths. Bulk sound and shear velocity anomalies in the lower mantle are anti-correlated providing the strongest evidence for chemical heterogeneity in the lowermost mantle. Mapping attenuation provides another type of constraint, as variations in Q should be primarily sensitive to changes in temperature and water content, as well as degree of partial melting (in the uppermost mantle). Mapping of 3-D variations in attenuation is technically difficult, but substantial progress has been achieved. Finally, seismic anisotropy has been documented in the upper mantle and in D", and mapping it (both radial and azimuthal anisotropy) may be helpful in detecting past and present direction of flow in the mantle - but data coverage limits the possible resolution.

Thus one elusive but critical goal is to link the 2-D geochemical "mapping" of composition with the 3-D seismic mapping of temperature and composition. Dynamical modeling has been successful in illuminating the large-scale features of mantle flow, and the interplay of thermal and compositional buoyancy. There has been some progress in using constraints from seismic elastic and anisotropic tomography. These models currently provide reasonable fits to "observables" such as plate-scale features and surface heat flow, and the amplitudes of the topographic and gravity fields. Numerical experiments with passive tracers provide at least qualitative insights into convective stirring and mixing phenomena. Calculations attempting to reproduce the time evolution and current heterogeneity of geochemical tracers have largely been unsuccessful. Furthermore, testing of the survivability of small-scale geochemical domains ($\approx$10 km, for example recycled ocean crust) against convective stirring and mixing is at the current limits of computational resolution. Reconciliation of dynamical and geochemical models is a major interdisciplinary hurdle facing our community. Mineral physics plays a key role in illuminating these puzzles, insofar as the density and rheological properties of created heterogeneities will control the survivability of these heterogeneities. For example, the generation of ocean crust creates complementary enriched crust and depleted lithosphere. At depth, these components are heavier (and stiffer?) and lighter, respectively, than ambient mantle. Is the package subductable, or will they become eventually separated by buoyancy so that only the crust subducts? Will the crust be more resistant to mixing and stirring than the lithospheric (harzburgitic) residue? How will the greater internal heating of the enriched crustal component affect its future storage and recycling?

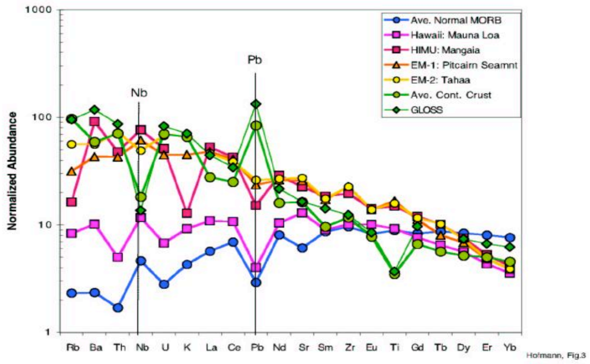


Figure 2. Concentrations of selected trace and major elements, arranged in the order of ascending compatibility and normalized to primitive mantle concentrations. From Hofmann (1997).

Example 2: Thermal evolution of the Earth

Understanding the origin and evolution of the earth remains perhaps the most important problem in all of geosciences (Figure 3). By its nature, the problem encompasses all aspects of CIDER. There have been advances in many of the individual disciplines represented by CIDER including mineral physics, geochemistry, seismology, geodynamics, and geomagnetism pertaining to this problem and the time is ripe for a fresh interdisciplinary attack.

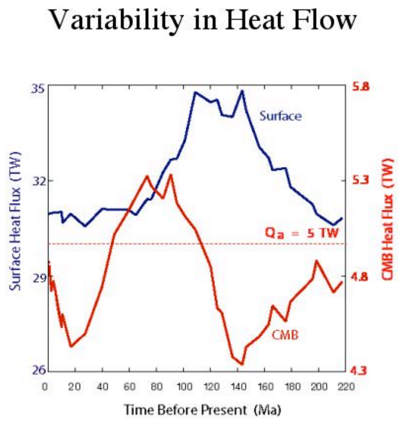
The standard model assumes the Earth is gradually cooling with slow growth of the inner core. This growth of the inner core is commonly thought to be important for driving the geodynamo. In particular, the fact that there is a compositional difference between inner and outer cores leads to a gravitational energy source for driving the dynamo, which is particularly efficient. It has been speculated that the reason that Venus has no magnetic field is that there is no inner core growing. The surface heat flow is composed of contributions from radioactive heating and cooling of the planet with radioactive heating typically confined to the mantle.

There are several difficulties with this model. Recent calculations of the properties of iron alloys suggest that it is very difficult to maintain an inner core for a significant fraction of earth history thereby potentially removing a significant power source for the dynamo (this issue is also discussed in Example 5 – “The Core”). Yet paleomagnetic evidence indicates that the magnetic field existed as early as 3 Ga. The inner core can be made longer-lived by adding radioactive elements to the outer core so allowing a slower cooling rate and a consequent slower growth rate for the inner core. Geochemistry appears to be currently ambivalent about the presence of significant radioactive elements in the outer core (which depends critically on what conditions core material equilibrated with mantle material during the accretion process). Adding a significant amount of potassium to the outer core (equivalent to about 25 percent of the surface heat flux) can increase the age of the inner core to about 1 billion years. It is only possible to extend the age of the inner core significantly longer than this if the dynamo process is unusually efficient.

Adding radioactivity to the outer core can alleviate another well-known problem, namely the mismatch between the observed surface heat flow and the amount of radioactive heat production in the MORB source (appropriately corrected for concentration of radioactive elements into continental crust). An alternative is to sequester radioactive elements in a hot abyssal layer that is chemically distinct in order to be dynamically stable. Unfortunately, seismological evidence for such a layer is lacking. An added complication is that the degree of inhibition of mass transport at shallower depths in the mantle is also not certain though it is unlikely to be extremely strong. Even if the mantle convects as a single layer there is still considerable uncertainty in the relative time scales of heat production and heat loss through time as encapsulated in the Urey number.

This theme covers all aspects of CIDER. There are still some physical properties of deep Earth materials, which are relatively poorly known and are critical for this problem. In particular, the transport properties (thermal and electrical conductivity) of the deep mantle and core have seen little attention and would make a good subject for a workshop. The possibility of partitioning radioactive elements into the core is part of the broader problem of understanding the conditions and processes of core formation - this could be the subject of another workshop. The problem of inner core evolution could also encompass the study of the formation of inner core anisotropy. A workshop including the seismological, mineral physics, paleomagnetic, and geodynamical constraints on inner core evolution is therefore an attractive possibility.

Turning now to the mantle, the core-mantle boundary and the D" region remain one of the most perplexing parts of the Earth with evidence for chemical and thermal anomalies at all scales. This structure can impact the behavior of the geomagnetic field and could also impact core-mantle coupling. There is some evidence for mass transport at the CMB (ultra low-velocity zones) though, again, the geochemical evidence is enigmatic. Again, these are natural topics for a workshop. Finally, the transition zone and uppermost lower mantle show some evidence for inhibition of vertical flow (though not full-fledged layering) and there is some question about the nature and continuity of slab-like velocity anomalies in the lower mantle - both of these areas have strong implications for the thermal history.



Bunge, 2003

Figure 3: Evolution of heat flow at the surface Q(t) and core-mantle boundary Qc(t) from numerical simulations of mantle convection with imposed plate motions (Bunge et al., 2002). Geological estimates of plate motions over the past 120 Ma are used in the calculations.From Buffett and Bunge (2003).

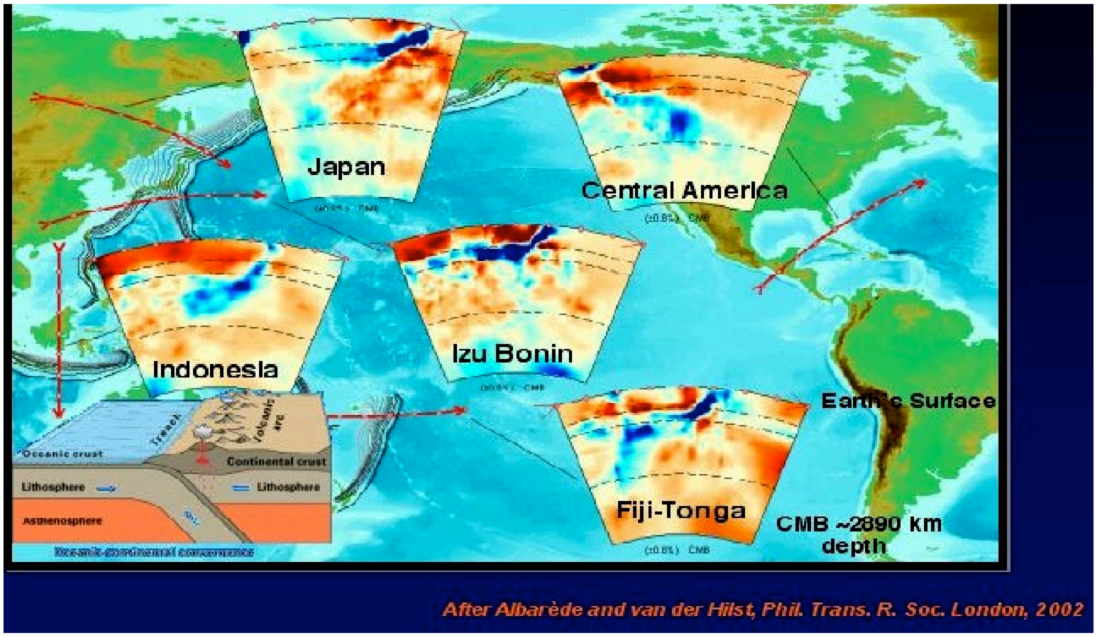
Example 3: What are the fluxes into, out of, and across the mantle?

Geological activity is characterized by the movement of materials and heat on scales ranging from global to regional. Indeed, convection of the mantle and core, the action of plumes, movement of tectonic plates at the surface, and infiltration of magma and other fluids at depth offer dramatic examples of Earth's ongoing geological evolution. The geological record provides an integrated history of our planet's mass and heat fluxes. Through seismological, geodetic, heat-flow and magnetic observations, geophysics yields information about the current dynamics of the interior; and the geochemical signatures of magma source regions and of direct (xenolith) samples from the interior provide a means of tracking the time scales of these internal processes. Combining information about the properties of Earth materials, as derived from petrology and mineral physics, the full range of geological, geophysical and geochemical observations are interpreted through geodynamical models.

Tracking of mass and energy (heat) fluxes lies at the heart of understanding how our planet has evolved over geological time. What is the differential motion of fluids at depth, leading to volcanism and metamorphism due to upward migration toward the surface? What is the potential for downward sequestration of hydrous (and other volatile-bearing) fluids, or even of dense oxide or metallic melts in the deep interior? How effectively is heat transmitted across the core and then the thickness of the mantle, providing the energy sustaining the geomagnetic field as well as the plate-tectonic processes observed at the Earth's surface?

The key to understanding these phenomena is to recognize that each process of mass and energy flux leads to a wide range of geological consequences. That is, although one may have to work at the state of the art of a given specialty in order to make significant observations, the interpretations need to be broadly linked across the disciplines. As an example, imaging of plumes remains a major challenge for seismology, especially at great depth in the mantle. How can spatial variations in wave velocity and attenuation be resolved with adequate sensitivity to track the sources of such major surface features as Hawaii and Iceland? Seismic tomography indicates that the transition zone is dominated by large-scale positive velocity anomalies, most likely caused by accumulation of subducted material. The "Farallon" and "Tethys" high velocity anomalies, usually interpreted as evidence for penetration of subducted slabs into the lower mantle, are present in most of the models, but there are other high-velocity features that cannot be linked to recent subduction. There is some evidence for a significant change in the lateral heterogeneity at or near the 650 km discontinuity, possibly indicating impeded flow across the boundary. The ring of fast velocities around the Pacific corresponds well, with a significant in-land shift, to the current distribution of subduction zones.

Meanwhile, observations of geophysical anomalies must be interpreted in the light of geochemical and geological information about the temporal evolution of these regions, and all of the data must be woven together into reliable geodynamic models of mantle tectonics and magma "plumbing." Such models offer a means of quantitatively peeling back Earth history, revealing the processes by which our planet has evolved over geological time and perhaps glimpses into its origin and earliest history. But in order to validate the models, it is essential to formulate them in terms of testable hypotheses. Continuing with the example: Given a seismologically determined plume structure, including the uncertainties and resolution limits imposed by the observations, what are the associated geodetic and geochemical anomalies that would be expected? What new observations are required either to refine the model, or falsify its underlying hypotheses? How can laboratory experiments be brought to bear most effectively on the interpretations? Is it more important to determine elastic-wave velocities or trace element partitioning among mineral phases at conditions relevant to plume sources, for example?



After Albarède and van der Hilst, Phil. Trans. R. Soc. London, 2002

Figure 4. Cross sections of P tomographic models in different subduction zones around the Pacific Ocean, illustrating a variety of styles, with some slabs penetrating into the lower mantle, while others appear to be stagnant above the 670 km discontinuity. From Albarède and van der Hilst (2002).

Example 4: How does melt affect the larger geodynamic cycles

Volcanic activity on the Earth, as well as on other planets and moons in the Solar System, represents one of the clearest manifestations of the dynamic nature of planetary interiors. Much is known about many aspects of melting. But there are several aspects of the problem that need further development.

One manifestation of the issues is the recent move to re-examine the mantle plume hypothesis. The most consistent large-scale features in seismic tomographic images are two low velocity structures, called sometimes "mega-plumes", one under Africa and the other under the Pacific. The horizontal spread of mega-plumes near the CMB corresponds well to the surface occurrence of hot spots. On the other hand reports of "thin plume" signatures and their depth extent are "fragile", that is, their appearance strongly varies from model to model. Many geoscientists accept the need for focused upwelling of hotter mantle material to generate melts in intraplate settings such as Hawaii and Tahiti, and to generate the excess crustal thickness at Iceland. But several researchers have advanced the notion that (a) excess temperatures are not required for these phenomena, and (b) upper mantle processes associated with plate tectonics can produce enough melt to explain the "anomalous" melt production of many hotspots. This is a pivotal issue in deep earth dynamics, because the role of plumes has been taken to be rather substantial in both the circulation of mantle material and in planetary scale heat transfer. On the other hand, plumes may have become a rather automatic explanation of intraplate volcanism when in fact there are other mechanisms by which relatively large amounts of basaltic magma can be produced. Regardless of what side of this issue one is on, it is clear that there is a need to examine what we use for constraints on mantle potential temperatures and melt productivity, and to establish exactly how much melt one can expect to get in various geodynamic situations.

Coupling melt generation into geodynamic models for upper mantle processes is also necessary. When melt is generated in the mantle there is some additional contribution to buoyancy. Melt production can thereby be enhanced by acceleration of the upward advective velocity. In general, to advance our understanding of planetary magmatism, melting needs to be modeled in context of the solid-state mantle convection that generates it. For example, geodynamic models have been constructed to simulate the flow under Hawaii, but melt generation with transport has not been incorporated into these models. In order to properly assess the petrological, geochemical and volcanological data available for Hawaii, a more complete geodynamic picture needs to be developed. The mantle flow pattern, constrained by geophysical data and material properties from mineral physics studies, must be the starting point for generating magma, which then must be allowed to migrate toward the surface; interacting with the solid matrix as appropriate, and with accounting for critical major, minor and trace elements and isotopes. Only in this way can we really begin to assess the available data and test the predictions of the plume model and other competing models.

The other aspect of melting that needs to be investigated is the effect of volcanism on the properties of the residual mantle. Magma extraction changes the chemical composition and hence the inherent properties of the residual rock material. These changes, particularly the removal of water and the decrease in density, are likely to affect the subsequent fate and transport of this material within the mantle.

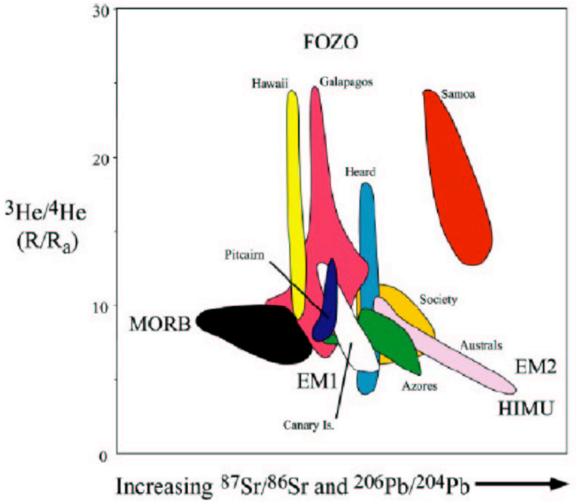


Figure 5. Two-dimensional projection of a 3D plot of Sr, Pb and He isotopes in oceanic basalts, viewed with the Sr-Pb plane in the horizontal. Data are averaged by island, volcano, or geologic formation, except for Heard, Samoa and MORB (Mid-Ocean Ridge Basalts) for which all data are plotted. FOZO: component high in 3He/4He; HIMU, EM1, EM2: three enriched components. This plot shows that hotspots have distinct chemistry and evolution from MORB. From Van Keken et al. (2002).

Example 5: The Earth's Core

The past decade has witnessed a spectacular influx of new information on the Earth's core, arguably the greatest since the initial seismic exploration of the core nearly a century ago. Examples of this new information include: high resolution global images of the geomagnetic field and its secular change at the core-mantle boundary from the Oersted and Champ satellites; seismic discovery of inner core three-dimensional structure, anisotropy and rotation anomalies; the first successful self-sustaining laboratory fluid dynamos; the first successful fully three-dimensional self-sustaining numerical dynamos; discovery of active and ancient planetary dynamos throughout the solar system and beyond; comparison between first-principles calculations and laboratory measurements of physical properties of core materials at in situ conditions; delineation of geomagnetic field changes on millennium time scales; seismic delineation of fine structure in the core-mantle boundary region.

As a consequence of these new data and new tools, the core has emerged from its relative obscurity and now occupies a genuinely central position in deep Earth studies. Evidence of its new position is found among the interdisciplinary research themes identified in this proposal, many of which involve the core, its interaction with the mantle, and its role in the evolution of the Earth as a whole. We think that it is especially appropriate and timely to highlight problems related to the core within the CIDER initiative, because in spite of this flood of new information and new tools, most of the larger science questions about the core remain unanswered.

Consider, for example, the question of the energy and heat balances in the core and the growth of the inner core as the source of energy sustaining geodynamo; some of these issues have already been discussed in Example 2 – thermal fluxes. According to the model in which all energy is derived from the inner core growth, the inner core may be relatively young, perhaps less than 1 Ga in age. However, this model assumes there are no significant radioactive heat sources in the core. Radioactive heat sources such as K_{40}, if present in sufficient concentration, could supply the energy needed by the geodynamo without a contribution from inner core growth. With significant radioactive heating in the outer core, the inner core could be much older and may be hardly growing at the present time.

Another outstanding set of questions center on the causes and consequences of magnetic polarity reversals. Ever since their acceptance four decades ago, Earth scientists have speculated on the significance of the polarity reversals seen in the paleomagnetic record. Is the origin of reversals wholly within the core, or is there an influence of the mantle? Does polarity reversal accompany a change in core flow, core-mantle coupling or inner-outer core coupling? What controls the frequency, duration, and transition field structure of reversals? What is the significance of superchrons in the paleomagnetic record? What is the frequency of polarity reversals in the Precambrian when the inner core may have been smaller? Is the rapid decline of the dipole in historic times a precursor to the next polarity reversal?

It is only recently that we have tools for going beyond speculation on these questions. Now we have numerical dynamo models that exhibit many of the basic properties of the geomagnetic field, such as geocentric axial dipolar average magnetic fields, magnetic secular variation, and even polarity reversals. These models are, however, very far from realistic in terms of their physical properties. For example, we do not understand how dynamos work in the environment of the Earth's rapid rotation. We do not understand the effect of turbulence in the core, and perhaps most surprisingly, we do not know how dynamos work in highly conductive liquid metals such as the iron-rich outer core fluid. Equally important, these dynamo models show chaotic behavior, making it difficult to relate their behavior to the Earth. So in spite of these new tools, we do not yet understand the underlying cause of polarity reversals, how the mantle structure influences the geodynamo, and other fundamental questions.

A third area of active interdisciplinary research involving the core concerns the effect of mantle heterogeneity, particularly heterogeneity in the D" layer, and the nature of core-mantle coupling. We have already alluded to one possible coupling mechanism: mass transfer by chemical reactions near the core-mantle boundary. Several other coupling mechanisms are thought to be important, including thermal, topographic, and electromagnetic. Topographic and electromagnetic couplings affect the barotropic component of core flow, whereas thermal coupling affects the baroclinic component. These coupling mechanisms have been proposed to explain a host of puzzling observations, including decadal time scale length-of-day variations, geomagnetic secular variation including geomagnetic jerks, long-term departures of the geomagnetic field from an axial dipole, preferred reversal paths, and even the near-fixity of mantle plumes.

The common element in all these research problems involving the core is that the new tools and the new data come from the traditional sub-disciplines (seismology, mineral physics, geomagnetism and paleomagnetism, geochemistry, etc.) but the phenomena of interest cut across the sub-disciplines. It is precisely this property that makes them especially suitable for CIDER

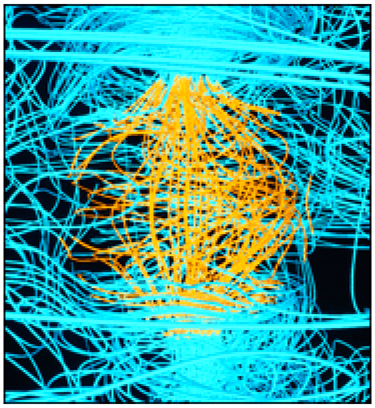


Figure 6. Snapshot of magnetic lines of force in the core of a model earth. Lines are gold(blue) where they are inside (ouside) of the inner core. The field is directed inward at the inner core north pole (top) and outward at the south pole (Bottom). From Glatzmeier and Roberts (1996).

Proposed CIDER Management Structure

We propose that CIDER be located in Berkeley, however, not directly on the UC Berkeley Campus. The Berkeley location was deemed appropriate in that: the San Francisco Bay region is an attractive, easy to access area both in winter and in summer; the proximity of the Berkeley campus provides access to key facilities such as libraries, internet, laboratories and intellectual resources, so that the CIDER residents will not work in isolation. While Berkeley faculty will benefit from the proximity of CIDER, our aim is to provide a community-wide resource. Members of other academic institutions will:

- have a voice in the management/direction of CIDER programs
- participate in CIDER activities
- send students/postdocs to programs
- have input into programs/themes
- be kept informed through a newsletter and a website

The management of CIDER will comprise a Director and a Deputy Director, and a Scientific Advisory Committee, reporting to a Board of Directors (Figure 7).

The Director (0.5 FTE, member of the U.C. Berkeley faculty) will be the PI on the Institute grant. He/she will have overall responsibility for putting into practice the scientific mission and goals of CIDER, as well as responsibility for the budget of the Institute, the supervision of personnel and the management of Institute resources. The Deputy Director (0.5 FTE, distinguished member of the scientific community whose home institution is not U.C. Berkeley), will provide advice and support to the Director, will provide leadership for current scientific programs and mentoring programs for resident post-doctoral fellows and graduate students. Together with the Director, he/she will initiate and nurture interactions of the community with the Institute. The Director and Deputy Director report to the Board of Directors.

Board of Directors and Scientific Advisory Committee

The Board will be elected by a larger group of representatives of Member Institutions. The Board will establish a set of by-laws, establish membership institutions and "member institution representatives". Liaisons will be established with related infrastructure programs: IRIS (D. Simpson), EarthScope (W. Prescott), SCEC (T. Jordan), COMPRES (R. Liebermann), CIG (M. Gurnis), EarthRef and related database/infomatics efforts (H. Staudigel) such as REM, GERM, MAGIC, PetDB, GeoROC.

The Board will appoint the Scientific Advisory Committee (SAC) composed of members of the geoscience community outside of UC Berkeley, with the Director and Deputy Director serving ex officio. This Committee will define the long term "themes" and, within these themes, select proposals for working groups. The Board will meet at least once a year, while the Scientific Program Committee will meet as many times as needed to effectively plan the scientific activities of CIDER (i.e. at least twice a year). The executive committee will comprise the Director, Deputy Director, Chair and Vice-Chair of the SAC, and will handle urgent programmatic decisions in between meetings of the SAC.

Administrative and technical support

In the "steady state" mode of operation, to be reached progressively within 3 years, the administrative office will comprise:

- a management officer (MSO, in UC terms)
- 2 administrative assistants, who will handle travel, housing, and other needs of the short-term and long term residents.

CIDER will comprise minimum technical infrastructure, but support is needed for a computer systems administrator (0.5 FTE) and a web manager. The key ingredient for the success of CIDER will be its high connectivity to other institutions, databases, fast computer centers, etc., as well as visibility of the Institute through a well maintained website and periodic newsletter.

The University of California at Berkeley has pledged a contribution of $50,000/year towards the support of CIDER Infrastructure for the first 4 years of the Program.

Scope of CIDER activity and corresponding support

At the 09/08 - 10/03 meeting in Davis (CA), it was recognized that for an Institute like CIDER to be viable, there had to be, from the very start, a "critical mass" of residents (post-docs and senior scientists). We estimated that the presence of 10 scientists (2-3 from each discipline) would be a minimum to begin with. These could be, for example 7 post-docs residing at CIDER for a year and the equivalent of 2 full time senior scientists. Support for the latter could be split among four or more 3-6 month "sabbatical" internships at CIDER. One FTE could be split among several graduate students for 6 month internships.

Each "long" program will be overseen by one or two program PI's, partially supported by CIDER (1 FTE total to be split among 2 to 4 program PI's each year). At its steady state level, after 3 years, CIDER will support the equivalent of 10 FTE long term residents (post-docs, senior scientists, grad students) and 1 FTE "program PI", 10 meetings of working groups per year (2-3 meetings per working group, 3-4 working groups) a 2 week summer school each year with 30 students, 6 instructors. CIDER WILL compensate instructors for their time in preparing and delivering courses (to insure motivation and high level instruction).

Ramp up of CIDER activity and corresponding support

Initially, CIDER will be governed by an Interim Board (the current CIDER Steering Committee) and an Interim Director. Together they will identify the Board of Directors and establish the Scientific Program for the first two years of CIDER. Support for the Interim Director (UC Berkeley faculty) will be provided by UC Berkeley, in the form of teaching time release.

Essential support starting from Year 0 includes the Manager, 1 administrative assistant, the computer systems administrator (0.5 FTE) and the web manager (0.5FTE). These positions are necessary from the start to set up the infrastructure and give CIDER visibility in the community.
U.C. Berkeley will provide matching support in the form of 50% teaching time release for the Director, as well as $50,000/year during the ramp-up phase to be applied to operating costs of CIDER. UCB overhead rates will be reduced from 52% to the off-campus rate of 25%. The resident and workshop program and the administrative support of CIDER will be ramped up over 5 .

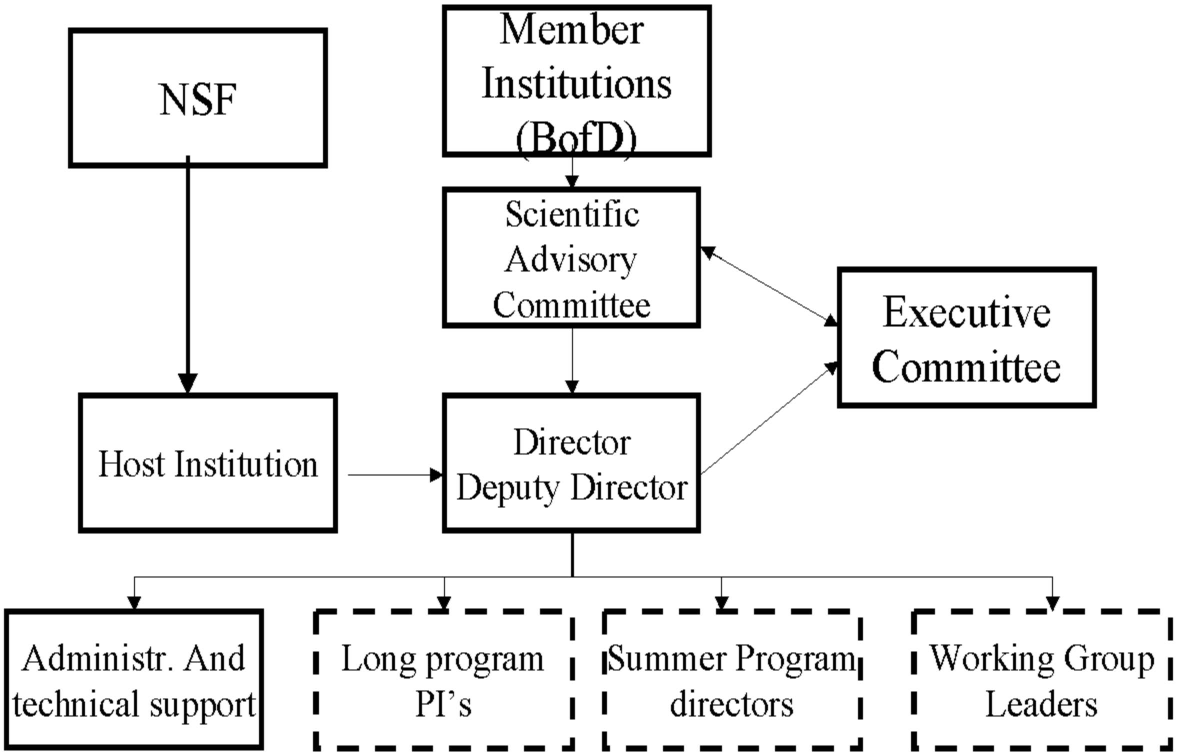


Figure 7: Proposed Management Structure of CIDER.