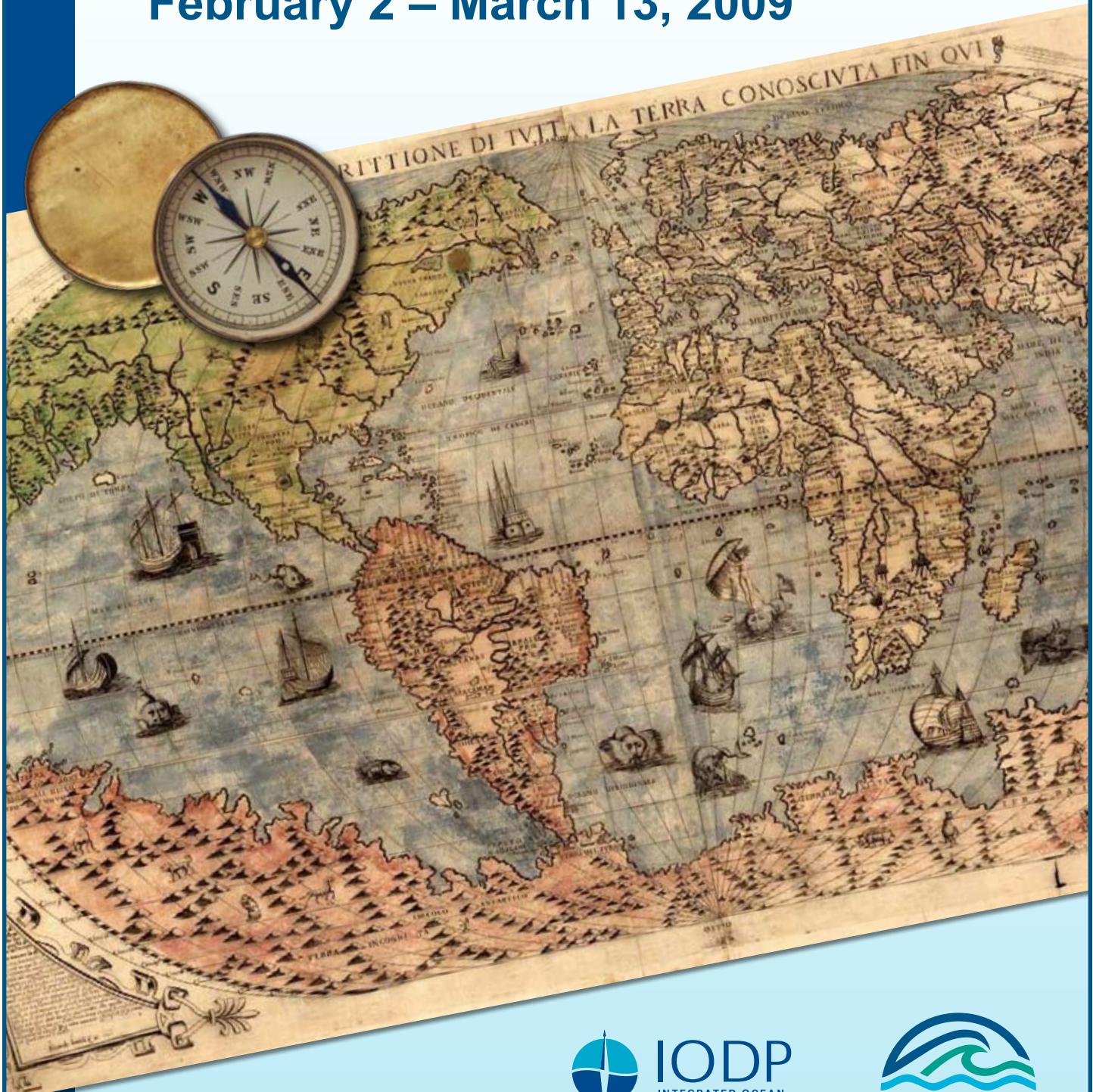


CHARTING THE FUTURE COURSE OF SCIENTIFIC OCEAN DRILLING

Workshop Report

February 2 – March 13, 2009



IODP
INTEGRATED OCEAN
DRILLING PROGRAM



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EXECUTIVE SUMMARY

Charting the Future Course of Scientific Ocean Drilling (CHART) was a six-week on-line workshop, held from February 2 – March 13, 2009, to gather input from the U.S. science community regarding future research directions of scientific ocean drilling. The Consortium for Ocean Leadership sponsored the CHART workshop and provided web support, which facilitated communication among the steering committee and the science community.

The CHART workshop had six discussion boards, each with initial questions intended to stimulate discussion on current emerging fields, unanswered research questions, implementation strategies, and potential future directions for IODP. Weekly summaries, new questions, and new discussion threads were posted throughout the duration of the meeting. For a full description of the goals and structure of CHART see **INTRODUCTION** and Appendix A0.1.

In all, there were 186 registered participants from across the U.S. and over 500 posts on the website (see **0.2 Participation**).

The main results of the CHART discussion boards are:

Emerging Fields and Crosscutting Disciplines

Eleven emerging fields and multidisciplinary topics of high interest for the future of scientific ocean drilling were identified. Potential *new directions* for IODP include future climate and environmental change, CO₂ cycling and mitigation, geohazards, ocean acidification, and atmospheric inputs to the ocean. Other topics that *integrate across multiple fields* of the Earth sciences include ice-covered seas and polar regions; continent–ocean fluxes, processes, and linkages; surface processes and geochemical cycles; and ocean lithospheric cycling. The participants also discussed the need for globally coordinated drilling strategies, or *drilling frontiers*, to advance knowledge under certain topics such as variability of heat flow and the geodynamo. In addition, the need for *global scientific partnerships* represented a recurring theme throughout the CHART workshop.

These drilling directions build new scientific avenues that could be addressed through existing drilling and technology, integrating technologies, and multi-disciplinary science approaches to address process-based science of Earth’s interior, surface, and atmosphere, and new approaches to drilling (e.g., global transects, funding, collaborations) to better understand Earth systems. Last, it was emphasized that the community must promote the idea that scientific ocean drilling delivers knowledge *beyond conventional science*, must *engage the public*, and must enhance *education and outreach*.

Solid Earth Cycles and Geodynamics

Future high priority directions in solid Earth cycles and geodynamics build on the accomplishments of IODP and further advance knowledge in topics discussed in the IODP Initial Science Plan (ISP), namely *continental breakup and sedimentary basin formation*, *structure of the oceanic crust*, *large igneous provinces* and *volcanic chains*, and *the seismogenic zone*. The participants also proposed *new interdisciplinary directions* such as exploration of the Arctic, hydrogeologic and geobiologic dimensions of ocean crust accretion and aging, linking episodes of rapid and massive volcanic eruptions and environmental crises, and developing a comprehensive geohazards program in plate subduction zones.

Technical advances in drilling, core recovery, and measurements are a prerequisite for many of the high priority goals. The importance of a strong ***observatories*** component in the new program for time series of sampling and *in situ* measurements was also recognized.

Environmental Change, Processes, and Effects

Discussion of environmental change, processes, and effects emphasized that studies of Earth system changes in the past are critical to understanding ***the sensitivity of Earth surface processes and conditions***, such as ice, wind, sea level, and carbon cycling, to ***climate forcing*** including extreme warm periods in the past. Furthermore, studies of Earth's sensitivity inform appropriate strategies to intervene and adapt to future climate change. Scientific ocean drilling is one vital exploration platform for discovery in this area and must be ***tightly integrated with other platforms*** (e.g., continental climate studies, ice dynamic expeditions, approaches that investigate both continental and oceanic processes) ***and other disciplines*** (e.g., climate and Earth-system modeling, physical oceanography, glaciology, education, outreach and policy) to achieve profound and new high-impact discoveries.

The key to solving many environmental change problems requires ***long-term planning***. For example, many successful modern oceanographic survey programs like the Geochemical Ocean Sections Study (GEOSECS), World Ocean Circulation Experiment (WOCE), the Joint Global Ocean Flux Study (JGOFS), demonstrate how fundamental discoveries require a broad spatial array of observations. To acquire comparable sets of paleoceanographic and paleoclimatic data will require a ***new implementation strategy*** beyond the traditional IODP scheduling of individual, stand-alone, two-month drilling expeditions. A “PaleoGEOSECS” program to constrain the chemistry, upwelling, and productivity regime of the past ocean in four dimensions (i.e., 3-D spatial array plus time) would be a profound scientific achievement of the new program and would require a long-term implementation strategy that capitalizes on platform travel tracks to carry out a multi-year mission.

Deep Biosphere and Sub-seafloor Ocean

Geomicrobiology should be a high priority for future ocean drilling programs, and should be ***more integrative*** with other IODP fields. This goal requires informing microbiologists outside of IODP about the opportunities available through ocean drilling. In addition, ***technologies*** should be developed to make microbiology a more standard and integrated part of drilling, observatories, and onboard science operations.

Specific topics that cut across multiple disciplines are vital to drilling and require drilling to be answered. The study of the complex interactions involved in the formation and dissolution of ***methane hydrates*** requires collaboration among specialists in biogeochemistry, hydrogeology, and geophysics. A more complete, multidisciplinary understanding of ***fluid flow*** through the ocean crust and of ***biogeochemical element cycling*** is needed. An overlooked but central component of fluid flow in the crust involves understanding the ***transition between continental and ocean crust***.

Strategies

Strategies to increase the effectiveness and impact of IODP involve expanding the development of broader impacts through enhanced ***education and outreach*** efforts. This goal requires better ***integration*** of education and outreach planning and implementation processes,

and capitalizing on *new communication technologies*. Outreach includes distinct communities: active scientists, graduate, undergraduate, and K-12 students, educators, and the public at large. IODP should be working on making scientific drilling important to all of these communities.

There was clear support for a *better balance of commitments to multi-year projects*, for which IODP is part of broader efforts to understand Earth history and processes, *and shorter-term stand-alone projects* for which IODP operations can be implemented with rapid planning. Longer-term planning may also help with securing funding commitments from non-traditional IODP sources, and establishing coordinated projects with other research programs.

Finally, IODP needs to think more broadly and have greater flexibility about expedition planning, preparation, staffing and defining scientific parties.

A vision for the new science plan

How should the new research directions (listed above and explained in the full CHART report) be presented, justified and implemented as part of the new science plan for IODP? The CHART workshop participants were supportive of designing a program that is *well integrated with other Earth science programs*. Many of the potential future research directions defined by CHART are shared across many communities studying Earth systems including scientists investigating continental systems, developing predictive models or validating theory with observations, using observatories of modern oceanographic or geologic systems, and conducting laboratory experiments. The use of ocean drilling technology distinguishes IODP from other programs, and provides a unique perspective on long-standing as well as emerging scientific questions of importance to many different programs and communities. The new science plan of IODP should both emphasize the goals of high importance that it shares with other Earth science programs, and elucidate the unique and essential information that only ocean drilling can provide to achieve these goals.

CHART participants expressed the need for ocean drilling science to be actively involved with issues of *high societal relevance*. At the same time, the *intrinsic value of new scientific knowledge needs to be emphasized*. As such, the new science plan should be justified both by emphasizing science that provides fundamental information that impacts habitability and safety, and by highlighting the innovative nature of ocean drilling and the unique insight it affords beyond what conventional techniques and technology can achieve.

CHART discussions also emphasized the need for IODP to achieve a *balance to accommodate both long-term planning of complex missions and single stand-alone expeditions* that could be planned and implemented more quickly. CHART did not produce a prioritized list of mission-like projects, but participants did support the idea that the next step in many fields is to design complex, possibly global-scale, multi-year data collection programs as part of the new science plan of IODP, thus *revolutionizing* the approach taken by scientific ocean drilling and facilitating capabilities to attack the *biggest science problems*.

CHART participants supported the idea that *new cross-disciplinary research* projects be part of the new science plan and could constitute the major initiatives that exemplify the big science approach of the new program. Examples of this approach are *polar exploration* and

CO₂ sequestration, which could be integrated programs or groups of programs designed to attack a spectrum of oceanographic, climatic, biologic, geological, and interdisciplinary questions.

INTRODUCTION

0.1 CHART purpose, goals and organization

(see Appendix A0.1)

Purpose and Goals

The current phase of the Integrated Ocean Drilling Program (IODP) ends in 2013, and science planning for the next scientific ocean drilling program is underway. Science planning is a process that relies on community input to define future scientific directions. The process of gathering community input is challenging for many reasons related to the interdisciplinary, cutting-edge nature of IODP science. It requires intense interaction between scientists with different expertise. It relies heavily on technological advances, requiring that engineering advances and scientific questions evolve synergistically. Although hypothesis-driven, it can be highly exploratory, requiring innovative drilling and sampling strategies. It spans a wide range of Earth System topics, requiring ample time and opportunity for scientists to offer their diverse perspectives, to exchange ideas, and to formulate a shared vision of the future of scientific ocean drilling. As such, to arrive at a science plan for ocean drilling beyond 2013 requires multiple meetings and venues for the science community to express and vet their ideas, and to allow those ideas to mature and evolve.

As part of the science planning process for ocean drilling beyond 2013, **Charting the Future Course of Scientific Ocean Drilling (CHART), an on-line workshop held from February 2 – March 13, 2009, was designed to begin to deliver U.S. community input to the science planning process for the next scientific ocean drilling program.** Parallel science planning activities are happening in other countries. These activities will be followed by a large international science planning meeting, IODP New Ventures in Exploring Scientific Targets (INVEST), in Bremen in September, 2009.

As part of the broader international science planning process that starts this year, the goals of CHART were to:

- gauge the level of interest of the U.S. community in scientific ocean drilling,
- initiate discussions about the U.S. community's collective vision for the future of scientific ocean drilling, and
- stimulate the creative process of defining how scientific ocean drilling will lead to transformative science in the future.

This CHART report, along with the results from other science meetings taking place in other countries, will be used as input for the INVEST meeting. Specifically, it will facilitate the planning process of the INVEST meeting, and will provide direct input to the INVEST report. The proceedings of INVEST will be used as a primary source of ideas for the science and implementation plan of the next scientific ocean drilling program.

For more information on purpose and goals of CHART see the material that was posted on the CHART website (Appendix A0.1).

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0.2 Participation

(see Appendix A0.2)

The CHART discussion boards were visited 2,242 times by 695 visitors over the course of the workshop and resulted in 535 posts. The CHART discussion boards were designed to minimize barriers towards participating in the workshop; anyone could read the posts, but participants had to register to post a comment. Visitors to the site came from 37 states, the District of Columbia, and 17 countries. (The international participants created five posts on the forum; as this report focuses on U.S. views for future scientific ocean drilling, those posts are not discussed in this report.) Interest in the CHART discussions increased over the course of the workshop and prompted the steering committee to extend the meeting by one additional week (resulting in six

weeks total) to allow time for the participants to complete reading and responding to the new activity.

In all, the CHART workshop had 186 registered users from 79 institutions across the country. California, Texas, Oregon, New York, Massachusetts, and Hawaii had the most registered users; each had 10 or more. All of the discussion boards were frequently viewed by registered and unregistered users; there were 11,446 page views by the end of the workshop. The discussion boards on *Strategies* and *Emerging Fields* received more views than the boards on the ISP themes. Of the registered users, 127 (or 68%) wrote at least one post, resulting in an average rate of 21.4 views per post.

Organization

CHART was organized by a steering committee in consultation with the U.S. Advisory Committee for Scientific Ocean Drilling (USAC) and the Consortium for Ocean Leadership. CHART was hosted and implemented by the Consortium for Ocean Leadership, under the U.S. Science Support Program associated with IODP. As travel support and available time were limited, the meeting was held online to allow researchers who would normally not have the time or resources to travel to a physical meeting to participate in this planning process. The online format also allowed Ocean Leadership to archive, in written form, input from every participant, instead of just preserving popular or consensus views.

CHART consisted of six discussion boards:

- The *Emerging Fields and Crosscutting Disciplines* discussion board was aimed at developing interdisciplinary ideas outside of the three main themes of the existing Initial Science Plan (ISP) of IODP.
- Three of the discussion boards were aligned with the ISP main themes: *Solid Earth Cycles and Geodynamics, Environmental Change, Processes and Effects, and Deep Biosphere and Sub-seafloor Ocean*. Participants were prompted to discuss and define the most exciting new directions within each theme.
- The *Strategies* discussion board stimulated discussion about potential organization structure, interactions with other programs, outreach and education, the science advisory structure and management, and a host of other issues.
- The *General Discussion* board was an open-ended discussion regarding the accomplishments, highlights and possible ways to improve IODP.

Between two and four steering committee members were assigned to serve as moderators for each of the six discussion boards. The moderators read the posts on a daily basis, interjected comments or questions to stimulate more discussion, and wrote short weekly summaries. During the CHART workshop, the steering committee and personnel from Ocean Leadership had weekly teleconference calls to discuss logistics and content, and to decide how to maintain and stimulate productive discussions. The steering committee wrote weekly summaries (see Appendix A0.1.5) that were sent out on the Ocean Leadership listserv and posted on the CHART website. Several times during the meeting, as some of the threads of discussion became long and complex, the steering committee grouped posts on similar topics together, or started new threads on new topics.

Throughout the weeks following the close of the CHART workshop, the steering committee and personnel from Ocean Leadership had teleconferences to discuss how to structure and write this CHART report. The CHART report was posted for public comment on April 23, 2009 for sixteen days, before a final draft was prepared.

1. EMERGING FIELDS AND CROSCUTTING DISCIPLINES

1.0 Going beyond conventional science and engaging the public

(see Appendix A1.0)

The goal of the Emerging Fields and Crosscutting Disciplines discussion board was to identify new directions for scientific ocean drilling and research themes that were not identified (or highlighted) in the IODP Initial Science Plan (ISP). The moderators encouraged discussions on existing fields that have not been at the forefront of IODP research, new fields that have expanded since the publishing of the ISP, and integrative ideas that crossed traditional boundaries but could advance our science and should be considered as the science plan for post-2103 drilling is developed.

The discussion board evolved into topics that focused on new science ideas, potential IODP partnerships, and public interaction. We provide a short summary for the discussion topics (below) and also include the original discussion board (see Appendix A1.0).

Two overarching ideas that spanned the discussion board were the need to emphasize that *scientific ocean drilling moves beyond conventional science* and the need to *engage the public in our science*. The essence of *beyond conventional science* is that the next scientific ocean drilling program engages in technically challenging projects and integrates science in real-time through shipboard science, models, and observations; together they drive new science. In many instances this new science is a break-through unrelated to the originally proposed science. Discovery-based science through ocean drilling should be publicized and emphasized as we move toward the next program. This tact naturally extends itself to *engaging the public*. In general, discussions highlighted the need to emphasize the societal relevance of problems investigated by the program, and many of the highly discussed topics for the future of scientific ocean drilling have direct and timely societal applications.

1.1 Future climate and environmental change

(See Appendix A1.1)

One topic with multiple discussion threads was the study of *future climate and environmental change*. This topic expands that of the ISP to look at rapid changes to Earth's climate and environmental systems with an emphasis on gaining insights into how Earth might look in decades to millennia. Research along these lines should immediately grab societal attention in light of growing concerns and predictions (e.g., Intergovernmental Panel on Climate Change report, <http://www.ipcc.ch/>). The research could be based on high-resolution studies of previous warm events on Earth (also see **3.4 Warm climate studies**) or environments most sensitive to climate change, thus providing a look at the past and present to understand our future.

1.2 CO₂ cycling and mitigation

(See Appendix A1.2)

CO₂ cycling and mitigation received multiple posts and discussions, and is linked to **1.1 Future climate and environmental change**. As our CO₂ emissions increase, there is growing interest in the carbon cycle and geologic mechanisms to sequester CO₂ (also see **4.2 New scientific questions**). Examination of the CO₂ record during thermal maxima in the geological past is clearly critical (see **3.5.1 Carbon cycle**); however, scientific ocean drilling also provides the tools

and techniques important for mitigation today. The program can seek to understand many of the potential storage reservoirs (e.g., sandstones, oceanic crust, peridotite) and the drilling and engineering capabilities to evaluate their storage capacity. This research could objectively assess the viability of different geologic reservoirs for carbon capture and sequestration and expand the technical and engineering capabilities of the IODP drilling vessels to include advanced well tests and reservoir characterization (also see **5.7 How should the new IODP science plan be organized?**).

1.3 Geohazards

(See Appendix A1.3)

Oceanic drilling to address **geohazards** has existed as components of previous DSDP/ODP/IODP drilling, but online discussions suggest it warrants focused attention in the future ocean drilling program. A wealth of testable hypotheses on the origin and implications of different geohazards (e.g., meteor impacts, landslides, earthquakes, tsunamis, volcanoes; also see **2.4 Seismogenic zone and arc evolution**) already exist. Detailed drilling proposals to test and evaluate these geohazards are starting to gain support within IODP; making geohazards a focal point beyond 2013 will encourage more proposals. With advanced geohazard drilling, we have an opportunity to evaluate future hazards, provide data and information for risk assessment, and garner the support and attention of the general public based on recent devastating oceanic geohazards (e.g., Sumatran-Andaman great earthquake and tsunami of 2004).

1.4 Ice-covered seas and polar regions

(See Appendix A1.4)

The **ice-covered seas and polar regions** topic is related to global drilling partnerships (see **1.10 Global scientific partnerships**) but can be accomplished through IODP as well. These regions are the topic of scientific and societal discussions as our climate changes and are the most sensitive to climate change (see **1.1 Future climate and environmental change** and **3.1 Ice sheet stability and sea level**). The support and interest of the public should be considered as drilling in cold regions is explored in the future of IODP. A number of partnerships may be possible given the attention the Arctic is getting from a variety of public and private organizations and agencies (also see **5.3 Where will the program find new funding and new partnerships?**).

1.5 Continent–ocean fluxes, processes, linkages

(See Appendix A1.5)

Although the shoreline is a convenient separation between land and ocean, Earth scientists understand that these **two systems are linked on many spatial and temporal scales** (also see **4.1 Sub-seafloor Ocean** and **5.4 With what other programs should IODP be coordinating?**). This linkage occurs through the transfer of sediments and ice from continents to oceans, through the delivery of solutes, and particularly nutrients and carbon to the ocean, through ocean basin formation itself, and through the complex filter of continental margins (also see **4.2 New scientific questions**). In fact, many of the major scientific ocean initiatives are to understand continental dynamics (weathering, tectonics, erosion, climate, ice) in a more temporally and spatially continuous manner than can be achieved from discontinuous and spotty continental records.

Some significant aspects of this continent-ocean linkage remain unresolved, and should be a scientific priority of the new program. These include long-term sea level and also millennial-scale sea-level impacts on continental margin storage; true source-to-sink examination of

multiple parameters and impacts on organic matter, nutrients, including the unique connection between glaciation and tectonics; and atmospheric transfer of nutrients, dust, and metals.

1.6 Surface processes and geochemical cycles

(See Appendix A1.6)

Surface Processes and Geochemical Cycles was proposed as a new topic linking two themes of the Initial Science Plan (also see **4.1 Sub-seafloor ocean**, **4.2 New scientific questions**, and **4.3 Microbiology within IODP**). This integrative theme emphasizes how IODP must cross traditional borders to address important topics. For scientific ocean drilling, such a theme would promote research emphasizing causal links and feedback mechanisms between the Earth's surface cycles and the solid rock geochemical cycles. Ocean drilling provides the sediment record to link the two cycles over the long time-scales required.

1.7 Ocean lithospheric cycling

(See Appendix A1.7)

Ocean lithospheric cycling builds on the existing themes of Solid Earth (see **2. SOLID EARTH CYCLES AND GEODYNAMICS**) and Deep Biosphere (see **4. DEEP BIOSPHERE AND SUB-SEAFLOOR OCEAN**) but emphasizes the integrative systems approach studying chemical and mass transfer from mantle to lithosphere. There are dynamic processes affecting deformation rates, geochemical alteration cycling, and exchanges between the lithosphere and the ocean. Biologic processes will impact alteration and cycling that will feedback into other geochemical and physical processes. How all of these processes evolve as the ocean crust transition from young to old is an important scientific problem (also see **4.1 Sub-seafloor ocean**, **4.2 New scientific questions**, **4.4 ISP remaining questions** and **4.7 Aging of the oceanic crust**).

1.8 Ocean acidification and atmospheric inputs to the ocean

(See Appendices A1.8.1 and A1.8.2)

Related to climatic and CO₂ studies, discussions also included studies of *ocean acidification* and *atmospheric inputs to the ocean*. By studying the paleo-record of ocean acidification we can learn the total magnitude of CCD changes, when and how the ocean acidification has changed through time, and what the biological response was to these changes. This new research could shed light on the future of our oceans. Successful drilling requires appropriate transects as well as establishment of reliable proxies for environmental changes. Recent research has shown that atmospheric deposition has significant impacts on oceanic biogeochemical cycles. The detailed study of inputs (nutrients, acids, toxins) could help us constrain the interactions and reconstruct past terrestrial inputs to the oceans (also see **4.2 New scientific questions**).

1.9 New drilling frontiers

(See Appendix A1.9)

New drilling frontiers were proposed as an avenue to explore regions where we have limited information or where multiple, individual sites could be combined to gain new and valuable scientific data. One of the issues related to both of the ideas discussed, *variability of heat flow* and *the geodynamo* (also see **2.2 Structure of oceanic crust**), is how to develop a reasonable drilling campaign that spans all of the oceans but may not require many holes, so additional discussions beyond 2013 should address how to develop and implement non-traditional drilling

strategies with important science objectives (also see **5.6 How should the new IODP Science Plan be organized?** and **5.9 IODP planning structure**).

1.9.1 Variability of heat flow

(See Appendix A1.9.1)

One proposed opportunity was a series of heat flow measurements to help understand the magnitude and *variability of heat flow* throughout our oceans. Understanding heat flow globally may yield important insights into numerous Earth processes and differentiate between end-member models.

1.9.2 Geodynamo/Paleomagnetism

(See Appendix A1.9.2)

A second opportunity was collection of *paleomagnetic data* and transects in data-poor regions. The *geodynamo* provides an excellent example of how a *new drilling frontier* may be a matter of distributed data collection to gain the necessary samples to example the hypotheses. The *geodynamo* has not been extensively studied in the IODP, but IODP drilling with current technology could evaluate how the geomagnetic field has changed in space and time. Drilling also provides high resolution and spatially variable data, which could benefit our geomagnetic studies. A technology development that could aid this research is the ability to recover oriented cores (also see **2.2 Structure of oceanic crust** and **2.3 Large igneous provinces**).

1.10 Global drilling partnerships

(See Appendix A1.10)

Another proposed mechanism to expand IODP science was to coordinate *global scientific drilling partnerships* (also see **5.4 With what other programs should IODP be coordinating?**). This idea received multiple comments looking at the potential to expand our scientific capability by combining onshore drilling (e.g., ICDP), IODP drilling, and arctic/Antarctic drilling (e.g., ANDRILL, SHALDRILL). Combined with the support for these partnerships was the idea of funding opportunities that may be facilitated by integrating drilling strategies (e.g., onshore/offshore, open-ocean/ice-covered seas and/or targeted drilling) for specific programs (e.g., MARGINS) (also see **4.5 Enabling technologies**).

2. SOLID EARTH CYCLES AND GEODYNAMICS

2.0 Overview

(see Appendix A2.0)

The posts and responses of participants in the Solid Earth Cycles and Geodynamics discussion forum fall into four science initiatives, briefly described by the organizing committee at the outset of CHART. It is not clear whether participants were not aware that they could suggest additional themes or whether these four actually cover the whole range of interests in Solid Earth Cycles and Geodynamics.

2.1 Continental breakup and the formation of new oceans

(see Appendix A2.1)

The tectonic, magmatic, hydrologic and sedimentary processes that operate during *continental extension, breakup and new ocean basin formation* can be investigated by drilling along passive continental margins. There exist a spectrum of examples, from magma-rich to magma-poor rifted margins, and it is important to understand the differences, and to study conjugate margins on either side of the same basins to explore asymmetric processes.

Key focus areas are:

- the timing of magmatism related to strain distribution across the margins;
- uplift and subsidence histories;
- the formation of hydrocarbon source rocks as these relate to atmosphere-ocean carbon flux (also see **4.1 Sub-seafloor ocean**);
- enhanced carbon burial through erosion and weathering of continental rocks.

A new area of study is *thermogenic sources of carbon release in young continental rifts through magma injection into organic-rich sediments*.

A recent workshop of specialists (Investigating Continental Break-up and Sedimentary Basin Formation; September 15-18, 2006; Pontresina, Switzerland) is particularly relevant here and echoed in the comments under this theme. The workshop grouped the salient problems associated with continental rifting and breakup into six themes:

- a) rift initiation (understanding rift dynamics, inherited lithospheric conditions, and early magmatism);
- b) tectonic and dynamic aspects of rift evolution (elucidate both strain distribution and variations in dynamics in space and time);
- c) magmatic aspects of rift evolution (key targets in magmatic aspects of rift evolution are increased knowledge of melting, magmatic flux, magma transport, and emplacement);
- d) sedimentary, paleoenvironmental, and oceanographic aspects of rift evolution (increase comprehension of paleogeography, paleoenvironments, and the development of depositional systems in space and time) (also see **4.7 Aging of the oceanic crust**);
- e) initiation of seafloor spreading (glean information on seafloor spreading ridge evolution as well as the tectonic and magmatic response on the preferably conjugate margins); and

- f) consequences and impact (target paleoenvironmental changes related to rifting and rift magmatism, and the prospects for eventual subduction or obduction).

One aspect of studying continental rifted margins and sedimentary basins is that the margins in the Arctic are virtually unexplored and could be of great interest in industrial partnerships (see **1.4 Ice covered seas and polar regions**, **1.10 Global scientific partnerships** and **5.3 Where will the program find new funding and new partnerships?**).

2.2 Structure of the oceanic crust

(see Appendix A2.2)

Complete crustal section drilling is a long-standing (and here re-confirmed) goal of scientific ocean drilling. We increasingly appreciate, however, ***the variability of tectonic, magmatic, and hydrologic processes*** responding to such drivers as spreading rate, proximity to hotspots, and sedimentary cover. A single hole alone cannot determine a “representative” crustal section given the range of accretionary processes. Significant progress in achieving a full crustal penetration has been made at the Superfast spreading site (ODP Hole 1256D), and there is clearly enthusiasm to continue drilling at this location. Even at this most successful location, however, we cannot reach Moho without a new deep-water (>3,000 meters) riser system and drilling equipment that can withstand high temperatures. This is a highly important long-term goal.

The strategy of using tectonic windows in faulted crust to obtain pieces of a ***composite crustal section*** is still worthy, does not require a riser, and benefits from programmatic experience. Locations exist for sampling slow-spreading and fast-spreading crustal types using this strategy, which is complementary to the single-hole, complete crustal section strategy. In both environments, there is a strong desire to characterize the magnetic structure of the ocean crust, through the capability of collecting ***oriented samples***. Evolution of ocean crust can be addressed by drilling age transects along ridge-perpendicular “flow-lines” (see **4.1 Sub-seafloor ocean**), to understand the important time-dependent processes related to hydrothermal flow (see **4.7 Aging of the oceanic crust**), sedimentation, subsurface biosphere (see **4. DEEP BIOSPHERE AND THE SUB-SEAFLOOR OCEAN**), and off-axis magmatism.

Drilling can provide samples, *in situ* measurements, and long-term observatories for understanding the ***interaction between tectonic, magmatic, hydrologic and biological processes*** in the ocean crust. Questions of high importance are:

- Is the lower crust accreted as sills?
- Is there significant heat input into off-axis areas?
- Is there a fundamental dependence between spreading rate and crustal thickness?
- What is the role of mechanical strain in providing fluid (and melt) pathways?
- From what pressures are the major components of the crust magmatically/tectonically exhumed?

An ultimate goal is to understand ***geological, hydrothermal and biological processes*** by drilling into “active” systems at a near-ridge location (also see **4.5 Enabling technologies** and **4.7 Aging of the oceanic crust**). This requires some technological advances to overcome problems with drilling in high-temperature regimes, rubbly zones, bare-rock drilling, and flushing of cuttings. Future drilling will take advantage of new understanding of 4D crustal architecture developed through

geophysical imaging, field and modeling programs in RIDGE, and borehole experiments. An exciting new development is the idea of CO₂-sequestration in ocean crust, either in the high-porosity volcanic section or tectonically exposed mantle (see **1.2 CO₂ cycling and mitigation**). Ocean drilling will be required to characterize the physical and chemical properties of the host rocks.

In any of these crustal drilling environments, geophysical imaging is required to be assured of optimal location, and to extend the results of the drilling to significant areas of the ocean crust (also see **5.7 How can we improve funding for technical development and site surveys?**).

2.3 Large igneous provinces and volcanic chains

(see Appendix A2.3)

Large Igneous Provinces (LIPs) and related volcanic trails of seamounts and islands are records of a style of mantle dynamics distinct from, but as significant as plate tectonics. These volcanic systems reveal an intermittently expressed, non-steady-state behavior of the mantle that probably resulted in catastrophic effects on the ocean, atmosphere and biosphere throughout the geologic record. There are purely oceanic, purely continental, and ocean-continent examples; consequently, there are obvious opportunities for coordinated land-based and ocean drilling studies to reveal the 3-D structure, petrochemical variations related to mechanisms, environmental responses, and evolution from LIPs to associated volcanic trails (see **1.10 Global scientific partnerships**).

Some geodynamic questions are:

- Do hotspots begin with LIPs? How can different eruption rates (LIPs versus volcanic trails) be explained by mantle melting models?
- What is the character and history of mantle sources for LIP and hotspot melting?
- What is the role of LIP magmatism in continental breakup? What is the lifetime of hotspots?
- Do compositions change along hotspot tracks?
- Do hotspots drift in the mantle, and if so, how fast? What can hotspot drift histories reveal about large-scale mantle circulation?

Answers to these questions require sampling by ***drilling in transects and in tectonic windows*** within LIPs, and along associated, age-progressive volcanic trails. Paleomagnetic tests of hotspot fixity require acquisition of a sufficient number of cooling units to account for secular variation.

Environmental impacts of rapid eruption and outgassing of LIP magmas is a new research direction, which was also highlighted in the recent LIPs workshop (July 21-26, 2007; Coleraine, Northern Ireland). This topic has implications for greenhouse gas loading today (and potential climate recovery) and is heavily dependent on ocean drilling (see **1.1 Future climate and environmental change**). Mass extinctions, thermal maxima, ocean anoxic events, and ocean isotopic excursions are examples of the phenomena now linked to subaerially erupted or submarine-erupted LIPs. Rapid change and the resiliency to withstand, or capacity to recover of the Earth system (atmosphere-ocean-biosphere), can be investigated in the following ways:

- Obtaining complete, high-resolution sedimentary records from critical ocean environments (e.g., the Mesozoic pelagic深深, shallow, and atoll; sediment from the

Pacific, Indian, Arctic, and Southern Oceans; and high paleo-latitudinal sites are critically required).

- Obtaining syn-sedimentary sections within or adjacent to individual LIPs to estimate the potential hydrothermal fluid release and gas release through sediment-magma interactions (also see **4.1 Sub-seafloor ocean**, **4.2 New scientific questions**, and **4.7 Aging of the oceanic crust**).
- Targeted drilling to bracket the duration, peak and volume of magmatic activity (e.g., tectonic windows and feather edges of LIP basement, and syn-sedimentary sections).
- Bracketing the chronology/duration of the environmental events through recovery of sedimentary sections containing carbonates at locations permanently above the carbonate compensation depth, or CCD (e.g., Magellan Rise).

Punctuated evolution, ocean acidification and anoxia, rapid global warming, gas hydrate release (also see **4.2 New scientific questions**), etc., can be studied and compared for several prominent LIP events. Direct drilling of LIPs is required to test geodynamic models through primary compositions, volumes and chemical fingerprints, while recovery of high resolution sedimentary sections (especially for the Cretaceous Pacific Ocean) are needed to determine environmental impacts and responses. Ocean drilling complements other international programs such as the LIPs Commission, and the ICDP, which focus on subaerially exposed provinces.

The LIPs Workshop also highlighted a number of technological advances, some of which overlap with other subjects encompassed by this CHART theme (also see **5.7 How can we improve funding for technical development and site surveys?**). These include:

- Riser drilling in >2500 m of water. This will open opportunities for drilling through the feathered edges of LIPs to the basement or sediment beneath, thereby bracketing the durations of LIP events.
- Oriented cores. These are critical for determining sediment magnetostratigraphy in low latitudes, investigating geomagnetic field behavior, studying plate motions, and establishing flow directions of lavas.
- Enhanced recovery of syn-sedimentary sections, especially those with alternating hard-soft (e.g., chert-chalk) layers.
- Sidewall coring, important for recovering soft sediment from alternating hard-soft layers, including OAEs.

2.4 Seismogenic zone and arc evolution

(see Appendices A2.4.1 and 2.4.2)

A very strong case for societal relevance can be made for drilling-centered studies of the *seismogenic zone*. Earthquakes, tsunamis, landslides, arc volcanoes and volatile outgassing are part of the recurring danger of living near plate collision zones (or even at sea level an ocean basin away) (see **1.3 Geohazards**). The new program needs a major emphasis on understanding geohazards through both drilling and long-term monitoring, to demonstrate the unique contribution of ocean drilling to populations at risk.

There is currently a large and long-term commitment in the present program to understanding the seismogenic zone of subduction complexes. Central to these studies are the physical properties

changes that lead to accumulated strain, faulting and earthquake nucleation and rupture propagation.

- Is there a “seismic cycle”?
- What conditions lead to strong earthquakes rather than creep or slow earthquakes?

Direct sampling and long-term monitoring of the rocks in the fault zone and surrounding rock volume is one key component of an integrated program to investigate earthquake mechanisms, and the conditions that govern recurrence rates, propagation length and rate.

The Nankai Trough Seismogenic Zone Experiment (NanTroSEIZE) is a complex, multi-platform, multi-year program of deep drilling holes and instrumentation aimed at understanding earthquake nucleation in the sedimented version of ocean-ocean plate collision. It will not be completed by 2013 (the end of IODP), so there will be much to finish, and other seismogenic zones where sediment thickness, convergence rate, down-going plate topography, thermal regime are quite different, such as the thinly sedimented version (e.g., Costa Rica Seismogenesis Project; CRISP) also need to be studied.

There is great interest in installing *seafloor observatories in boreholes* constructed by ocean drilling (also see **4.6 Observatories**). These would monitor seismicity, strain, pore pressures, temperature, fluid compositions, and possibly microbiological activity, as well as provide basic infrastructure for active source and perturbation studies that are essential toward defining rock properties. They would complement land-based arrays of seismometers, strain meters, and GPS stations. The history of geohazards in arcs should also be determined through drilling, including studies of paleoseismicity (large earthquakes that trigger turbidites synchronously along major fault lengths) and tephra layers (geochronology and magma compositions).

The distillation of some elements from the mantle to the continental crust, and recycling of others through the oceanic lithosphere back to the mantle, is governed primarily by melting and fluid flow at subduction zones (see **1.6 Surface processes and geochemical cycles** and **1.7 Ocean lithospheric cycling**). Many important metal resources (ores) are concentrated by volcanic activity and associated hydrothermal activity (see **4.7 Aging of the oceanic crust**). Geochemical fingerprints imparted in the subduction process are tracers for mantle circulation and can be investigated through targeted scientific ocean drilling around volcanic island arcs (e.g., in back-arc basins). Arcs are active over tens of million years, which is relevant to plate tectonic processes, and many arcs leave being a unique, temporally highly resolved (1 Ma) record of changes in the slab and mantle input by means of their tephra deposited in marine sediments. That record can only be obtained through scientific drilling (see Appendix A2.4.2).

3. ENVIRONMENTAL CHANGE, PROCESSES, AND EFFECTS

3.0 Introduction and relevance to IPCC and P2C2

(See Appendix A3.0)

The Environmental Change, Processes and Effects board discussion was initiated with open-ended questions and without any attempts to define specific themes or topics. It quickly generated discussion centered on a number of topics, many of which were aligned with the scientific themes of the Intergovernmental Panel on Climate Change (also see **1.1 Future Climate and Environmental Change**). Future climate and sea-level predictions made by the IPCC are based on climate and ice sheet models that are only as robust as our understanding of climate system processes. CHART participants suggested that ocean drilling studies of past climates and oceans be focused on refining our understanding of these dynamic processes including rates of change. In addition, attempts to hindcast past climate states can be used to validate climate models and improve their capacity to simulate warm climate states. CHART participants also noted that many of the primary research goals of NSF's program, Paleo Perspectives on Climate Change (P2C2), require drilling. P2C2 is a program that specifically focuses on using geologic archives, and coupled observation-model approaches, to advancing fundamental knowledge of climate change processes. To achieve P2C2 goals would be difficult, if not impossible, without ocean drilling of both oceanic sediments and fossil coral reefs. Many of the research directions summarized below were rationalized because of their relevance to the IPCC and P2C2 research goals. The question of whether to try to pitch all topics as "societally relevant" was debated, and there was a diversity of opinions regarding this point.

3.1 Ice sheet stability and sea level

(See Appendix A3.1)

One of the least understood aspects of future climate change is *the response of ice sheets, and therefore sea level, to climatic change*. The IPCC clearly shows that uncertainties in future sea level projections are large because of our poor understanding of ice sheet dynamics. Because of the enormous mass and the long residence time of the Greenland and Antarctic ice sheets, existing instrumental records of recent ice sheet change are inadequate for studying ice sheet dynamics and the relationship between ice sheet mass balance and climate. Geological archives provide the best opportunity to generate observational data that can be used to improve our knowledge of ice sheet dynamics and the response of ice sheet size to climate change. There was a lot of enthusiasm amongst CHART participants regarding the prospect of using ocean drilling to study the evolution of ice sheets and their response to and role in the climate change.

There are many different approaches and opportunities in Earth's history. In the Pleistocene and Holocene, the behavior of the Greenland, East Antarctic, West Antarctic, Antarctic Peninsula, Fennoscandian, and Laurentian ice sheets could be contrasted to provide the broad perspective needed to differentiate the processes that control ice sheets. Furthermore, the role of sea ice and ice shelf buttressing on the stability of these ice sheets should be explored. Specific targets that would provide valuable constraints on ice sheet behavior are Melt Water Pulse 1a, and Marine Isotope Stages 3 and 30. In the warm Pliocene, a period of time that had pCO₂ levels close to present day values, we still lack fundamental knowledge of sea level and the size of the Greenland and Antarctic ice sheets. Deeper time periods are also relevant because they can provide an analysis of the sensitivity of Antarctic volume changes to radiative forcing under

generally warm conditions. The drilling strategies for studying *ice sheet sensitivity* include detailed studies of sea level especially those that can resolve rates of change, and studies of fans and/or glaciogenic sediments that can resolve the evolution and variability of specific ice sheets. There are proposals in the system that may address some of these issues, but careful surveying of complex margin depositional settings will be required.

3.2 Plio-Pleistocene evolution

(See Appendix A3.2)

The early Pliocene warm period and the transition to cooler climate and northern hemisphere glaciations of the Pleistocene can be used as a test bed to answer some compelling first order questions regarding the *response of climate to internal and external forcing*. Recent advancements in the development of climate proxies are enhancing our ability to answer these questions. Specifically, a few long detailed sub-polar and tropical temperature records generated in recent years have elevated our understanding of the Plio-Pleistocene climate evolution and new paradigms need to be tested. Future drilling, particularly in tropical, polar and sub-polar regions, would be used to investigate the role of pCO₂ in driving major climate shifts during Plio-Pleistocene transition (e.g., northern hemisphere glaciation, mid-Pleistocene transition) and the impact of changes in tropical temperature patterns on high latitude and global climate (see **1.1 Future climate and environmental change**). CHART participants specifically emphasized the need for more records to understand the long-term evolution and the shorter-term *variability of the West Pacific Warm Pool and the polar regions of both hemispheres*.

Milankovitch cycles are of particular interest and importance to modelers because the solar forcing at the top of the atmosphere is precisely known. Records of *climatic responses to radiative forcing* can be used to study regional climate processes and feedbacks. For example, how radiative forcing, due to pCO₂ and seasonal solar variations, controls ice sheets can be studied if records that monitor both northern and southern hemisphere ice sheets, which may or may not be responding similarly, are generated. In addition, the degree that regional Milankovitch climate cycles are driven by local variations in solar forcing versus variations in far-field conditions can only be resolved with high quality sedimentary records from climatically critical regions such as the tropical oceans. If the roles of intermediate and deep ocean circulation in modifying or amplifying climate cycles were better understood, long-term climate change could be predicted with more confidence. Drilling is the only way to generate long, moderate-resolution to high-resolution continuous records needed to study climate change that occurs on Milankovitch to more rapid (century to millennial) timescales (see **1.1 Future climate and environmental change**). There was also enthusiasm by CHART participants for even high-resolution studies that could resolve seasonal and inter-annual variability (e.g. using corals). Drilling studies could address the question of whether mean climate states, and presumably the climate feedbacks associated with those states, have an impact on climate variability such as that related to the El Niño Southern Oscillation phenomenon.

3.3 Observation–model linkages

(See Appendix A3.3)

Although many of the Environmental Change themes that were developed by CHART participants included comments on how *observations could be used to inform and improve Earth system models*, there was a discussion thread that was specifically focused on this issue.

Rather than fold these discussions into the most relevant scientific themes, they are called out and highlighted on their own because of their societal relevance. Models are the tools that we use to make detailed predictions of future climate change, and paleoclimatic studies can validate models and are arguably the best way to improve the ability of models to predict long-term climate change. For example, climate changes involving deep-ocean and ice-sheet feedbacks are impossible to validate with historical records. The new science plan of the ocean drilling program could outline a more explicit *strategy to join observational and modeling studies*, and to foster multidisciplinary research between the observationalists with geological training and the modelers with expertise in climate and ocean physics and theory. The CHART participants discussed ways to create a culture of multidisciplinary research (also see **4.3 Microbiology within IODP**).

3.4 Warm climate studies

(See Appendices A3.4 and A1.1)

The study of warm climates should be an important goal of IODP because of the need to understand *climate change processes during climate states warmer than present*. Specifically, there are tremendous opportunities to resolve the relationship between atmospheric CO₂, climate, and associated biotic impacts. Although there are several locations where detailed records exist, the regional heterogeneity, global nature and basic characteristics of warm climate states have not been adequately described. For example, even reference sections in all ocean basins, such as the Arctic Ocean do not exist. Because of these gaps, much can be learned from even low resolution records of oceanic and terrestrial conditions; mapping out mean climate states to validate model simulations has proven to be one the most fruitful strategies to identify ways to improve climate models that are used to predict future climate. In addition, extracting all potential information of societal relevance also requires the ability to quantify rates of change using higher resolution and better-dated records; both are within our grasp.

One critical issue is characterizing the climate (e.g., planetary temperature gradients, circulation) of *key extreme greenhouse intervals* like the early Eocene climatic optimum (EECO), the PETM, and even the late Cretaceous (Ceno-Turo). The community has focused intensely on both observations and theories related to these particular warm intervals. The *observational and climate modeling communities have gravitated toward these intervals* to strategically evaluate climate sensitivity to the more extreme levels of greenhouse gas that we anticipate by 2100 and beyond. The data/modeling comparisons are beginning to verify aspects of greenhouse climate theory while also revealing potential weaknesses in other aspects. The data coverage is still insufficient to test some of the key findings (e.g., polar warmth) regarding the sensitivity to higher greenhouse gas levels during these key intervals.

3.5 Biogeochemical cycles and processes

3.5.1 Carbon cycle

(See Appendix A3.5.1)

Understanding the carbon cycle is of critical societal importance, and perhaps the only way that we can understand whole Earth responses to variations in carbon cycling, including rapid injection of carbon dioxide and other greenhouse gasses into our atmosphere, is by using the geologic record (also see **1.1 Future climate and environmental change**, and **1.2 CO₂ cycling and mitigation**). Although issues of ocean acidification and rapid climate change are highlighted in

other sections, several basic issues related to the carbon cycle, including ocean storage of carbon and the direct link between carbon cycling and glacial development, remain unanswered. These are fundamental questions, perhaps of such scale that they are amenable to the Paleo-GEOSECS mission approach highlighted under strategies.

3.5.2 Clathrates

(See Appendix A3.5.2)

The relatively recent discovery of the *sub-seafloor formation of methane hydrates and clathrates* and the identification of vast resources of solid-phase methane on continental margins has implications for energy security, seafloor mining, global carbon cycling, geohazards and the climatic future of our planet (also see **4.0 Introduction and overview**, **4.1 Sub-seafloor ocean** and **4.2 New scientific questions**). Additionally, the terrestrial partner of marine clathrates and methane in permafrost shows the precarious nature of storage for this very powerful greenhouse gas. As such, gas hydrate dissociation may be a powerful positive feedback on the climate system. Some headway has been made in the current program from a scientific standpoint, but this is an area with vast industry and international interest, with which scientific ocean drilling is well placed to partner constructively (also see **5.3 Where will the program find new funding and new partnerships?**).

3.5.3 Oceanic anoxic events

(See Appendix A3.5.3)

Oceanic anoxic events (OAEs) represent an extreme in Earth system dynamics for which no modern analog exists. Were these driven by ocean stagnation, high biological productivity, high nutrient inputs, changes in ocean-continental carbon storage proportions, none or all of the above (also see **4.4 ISP remaining questions**)? We still don't understand the driving causes, and given the economic importance of geological resources deposited during OAEs and the unique opportunity to capture an extreme Earth state driven by a set of positive feedbacks, there remains interest in OAE objectives for the program.

3.5.4 Conservative elements

(See Appendix A3.5.4)

A *revision of our concept of conservative elements* in the ocean is needed. Paleoceanographers sometimes assume that the limited biogeochemical reaction rates of conservative elements (resulting in near vertical oceanic profiles) mean that their mass balances are invariant. This is simply not the case, and is an important factor in considering their long-term interpretations, and also even in revisiting the assumption that we understand their modern steady state (as highlighted in a 2009 *Nature* paper by Vance et al. showing impacts of glacial-interglacial chemical weathering variations on silicate rocks). Emphasis should be placed on constraining mass balances for both non-conservative (i.e., nutrients, carbon) and conservative elements (also see **4.1 Sub-seafloor ocean**, and **4.7 Aging of the oceanic crust**).

3.6 Implementation strategies

3.6.1 Missions, or mission-type planning

(See Appendix A3.6.1)

The capacity of three platforms, with very different but complementary capabilities, to achieve groundbreaking scientific results is a great strength of the program. The current system of assessment and implementation of science would benefit by adopting a more comprehensive and integrated vision, one that adds to the current “single proposal on a specific topic” model by *attacking the bigger problems on longer timescales*. By using the platforms more as expedition vehicles that can have multiple experiments operating at the same time, we would be able to carve away at problems that require broader areal coverage or material collected from locations not typically prioritized (also see **5.6 How should the new IODP Science Plan be organized?**, **5.7 How can we improve funding for technical development and site surveys?**, and **5.9 IODP planning structure**).

For example, a Paleo-GEOSECS experiment to constrain three and four dimensional patterns in ocean circulation, ocean biogeochemistry, and continent-ocean interactions could be run as a five-year program that utilizes small modifications of ship tracks, platform positions, and transits to obtain the wide spatial coverage required. This concept was also cast as a “reference section” drilling approach, and would be critical for constraining paleoceanographic data that is critical for atmospheric and ocean heat transport modeling.

3.6.2 Multi-platform needs

(See Appendix A3.6.2)

The *integration of multiple platforms to achieve broad science objectives* was also highlighted. In particular, topics related to continental margin dynamics and integrating the various processes that impact continent-ocean transfer of sediments, nutrient, and carbon can be examined using deployment of MSP, riser, and non-riser drilling approaches. Several proposals currently reside in the system that bridge a few of these platform capabilities, but the bigger science programs that integrate across platforms have not been a priority of the current program (also see **5.6 How should the new IODP Science Plan be organized?**, and **5.9 IODP planning structure**).

3.6.3 Cross-disciplinary meetings and training

(See Appendix A3.7.3)

The concept of *community meetings focused on cross-disciplinary topics* and younger scientists has significant support. Although a few training programs like this exist (i.e., the Urbino Summer School), more effort should revolve around community interaction on science themes (for other examples see, **4.3 Microbiology with IODP**, **4.5 Enabling technologies**, and **4.6 Observatories**). USSSP has supported several of these efforts in the past, and perhaps this program should be expanded to include international partners. One suggestion is that community science workshops on specific high priority themes be one of the vehicles that drive science implementation in the program. For example, the workshops could develop community consensus on the best approach to attack a particular scientific issue (monsoon history and impacts, ITCZ history, etc.), and this consensus becomes the implementation strategy for these programs and is given high priority for drilling. This adapts the mission concept from community development through science

oversight through implementation, and ensures that the greatest participation by the most scientists guides the science plan for the program.

4. DEEP BIOSPHERE AND SUB-SEAFLOOR OCEAN

4.0 Introduction and overview

(Appendix A4.0)

The discussion in this forum was intended to provide a framework for making deep biosphere studies a priority for IODP, and using the program to address one of the fundamental scientific questions regarding life on Earth. There are also enormous ramifications of a deep biosphere, including elemental budgets and cycling, the origin and evolution of life on Earth and the possibility of life on other planetary bodies, and the potential for discovery of new organisms, biomolecules and survival strategies that could be of benefit to humans. Advances in techniques may also be applicable in biotechnology and medical fields. Scientific ocean drilling within IODP is helping support fundamental discoveries involving the deep biosphere, which can only be reached through drilling.

Fluid flow within the ocean crust is the focus of studies of the sub-seafloor ocean. Work in this field involves attempts to measure active processes and crust and fluid properties, to develop a mechanistic understanding of where, how and why vast quantities of fluids move within the seafloor, and how these flows influence a myriad of other processes and systems, including the deep biosphere. Recent breakthroughs in the study of gas hydrates have also pointed to the critical roles played by sub-seafloor hydrogeologic processes. More broadly, the study of gas hydrates is at the intersection between hydrogeology, energy supply, climate change, microbiology, and geohazards. The necessity of an interdisciplinary approach is consistent with the structure and practice of scientific ocean drilling.

4.1 Sub-seafloor ocean

(See Appendix A4.1)

IODP has resulted in a number of advances regarding sub-seafloor fluid flow in the **sub-seafloor ocean**; it was targeted in Expeditions 301 and 308 and was a major component of Expedition 311. In addition, fluid flow studies are reported for Expedition 304/305; hydrology related work is continuing with the NanTroSEIZE drilling (also see **2.2 Structure of the oceanic crust**). One of the significant results to come out of these efforts is the ability to track fluids at a crustal scale from recharge to discharge over 50 km. These efforts have also provided the tools and understanding to extend these results to other areas, determine whether they are representative and to learn how they vary with age, spreading rate, sediment thickness and basement temperatures. Many questions remain, however, and results from one geologic setting are not necessarily applicable to a much different one.

Another area of interest regarding hydrology is exploring programs that cross from land to the ocean (also see **1.5 Continent–ocean fluxes, processes and linkages**). Some of the questions that remain unanswered are:

- How does the transition from continental to ocean crust influence hydrogeology?
- How far did fresh water get into the continental margin during the last glacial episode?
- What are the fluxes moving from continents to oceans in different environments?
- What is the residence time of fluids in continental shelves?

Modeling is seen as central to hydrogeology, and it would be useful to explain how to combine models and targeted measurements to test the models in hydrogeologic studies. Improvement of the links between numerical models and field data would be an important component of any future ocean drilling program.

Among the major unanswered questions that ocean drilling can address are the role of fluid flow in geochemical cycles (see **1.6 Surface processes and geochemical cycles**, **3.5.1 Carbon cycle**, and **3.5.4 Conservative elements**) and the role of pore pressure in geohazard instigation (see **1.3 Geohazards**). IODP is on the right track in addressing these issues, but the questions remain unresolved.

4.2 New scientific questions

(See Appendix A4.2)

The main thrust of discussion of **new scientific questions** centers on the need to address **subsurface fluid flow and exchange of materials** between the lithosphere, biosphere, hydrosphere and atmosphere. The scale of these exchanges is global and would require a major, committed effort. Related to this question is whether carbon dioxide can be sequestered either in crustal rocks or sediments in the deep sea (see **1.2 CO₂ cycling and mitigation**). Sequestration also ties into mitigation issues related to climate change and may provide a high-profile topic that could draw interest to the program. Similarly, methane hydrate formation and the potential generation of this phenomenon by methanogenic microorganisms is also a topic of a great deal of interest (also see **3.5.2 Clathrates**, and **5.3 Where will the program find new funding and new partnerships?**).

4.3 Microbiology within IODP

(See Appendix A4.3)

General microbiology advances, such as cultivating environmentally relevant microorganisms and defining genomic identities of subsurface clades, are needed to make **Microbiology a more significant component within IODP**. Related to these advances, extensive sampling and laboratory studies, each one providing crucial information for the other, would go a long way in helping identify requirements for life in the deep subsurface. Other aspects that would prove to be useful for deep subsurface life studies are *in situ* determination of maintenance energies and pressure adaptations.

Such advances require improvements in technology for detecting microbial metabolic activity. One of the best uses for this technology would be in observatories, and should include downhole and seafloor sensors. Long-term deployments would be best, requiring advances in ways to power and download data from sensors and monitor and sample microbial activity.

A concerted effort to **engage the geomicrobiology community** is needed in order to entrench microbiological studies more firmly into scientific ocean drilling. International training events like workshops and tutorials could be conducted. Another suggestion is to recruit actively and involve more microbiologists in future proposals and other activities, perhaps through IODP-themed sessions at more traditional microbiology meetings such as the American Society for Microbiology or the American Society for Limnology and Oceanography meetings. A broader base and new students, post-docs and other researchers are needed. One approach that has been successful for other agencies (National Aeronautics and Space Administration; NASA) has been

to have clear, simple foci to the study of microbiology in the deep subsurface. Suggestions include "follow the water" and "follow the energy". Another avenue is to communicate excitement about IODP to microbiologists and others not involved in the program, perhaps through inclusion of slide devoted to IODP activities during seminars, colloquia, invited lectures, etc. (also see **5.2 How can IODP increase participation?**, **5.3 Where will the program find new funding and new partnerships?**, and **5.9 IODP planning structure**).

To attract technology development, the geomicrobiology community should devise a list of those technologies that would be beneficial but do not yet exist.

A set of routine microbiology measurements should be established. The IODP Scientific Technology Panel has made some progress in this regard. Routine measurements are supposed to be made, but they are often not. If/when they are not, who is/should be in charge of making sure they are? This will require, however, that the major questions to be answered are first well defined, and agreement must be reached on protocols/measurements that avoids sampling bias. One alternative is to agree upon a method for preserving samples on-board ship, hopefully avoiding the preservation bias that often occurs during storage. It has been noted that the drill ships are now well equipped for microbiological studies. Another alternative is to staff every cruise with a microbiologist, which remains somewhat difficult, but is the only way to ensure that it is done. There should be a community commitment to do everything possible to make sure microbiologists are on each expedition. The microbiologist at sea should interact with the other shipboard scientists and integrate microbiology into the cruise results.

4.4 ISP remaining questions

(See Appendix A4.4)

Regarding **ISP questions**, most of the posts suggested that of the questions regarding the deep subsurface biosphere put forth in the IODP Initial Science Plan, many if not all remain unanswered, and many are even unaddressed thus far. We do not yet know the *extent of the deep biosphere, or the nature of most of the life* in it. For example, we have not yet identified the boundary conditions for life in terms of temperature, pressure, and extremes of chemical compositions, nor the full variety of potential redox reactions. We have not yet taken samples globally and in different tectonic settings to study how lithology and porosity influence the ecosystem. We do not yet know the influence of tectonic settings on the structure, size and turnover rates of sub-seafloor communities. We do not know the carbon or other elemental budgets for the deep biosphere, nor the fluxes in and out of it to other reservoirs. These questions, and many others that remain unexplored or unresolved, can only be answered through a coordinated, concerted global effort, the scale of which is likely only possible through a program like IODP. The questions remain unanswered in part because very few expeditions dedicated to geomicrobiology have been mounted by IODP, and IODP is not particularly effective at meeting ISP objectives without dedicated expeditions. It should be noted that the deep biosphere ISP objectives are collectively very ambitious.

4.5 Enabling technologies

(See Appendix A4.5)

Enabling technologies such as molecular techniques (molecular microbiological and organic geochemical) and high through-put culturing methods that have been or are being developed for other environments, need to be adapted to deep biosphere studies, including in observatories.

Recent development of high-pressure culturing techniques could prove very useful for adaptation to in situ incubations in deep biosphere work. Avoiding contamination as best as possible is of paramount importance.

Drilling young ocean crust would benefit geomicrobiology and hydrogeology studies. The technology currently exists, but the location of the boreholes must be carefully selected. The additional challenge in young crust is core recovery.

The next scientific ocean drilling program may benefit from developing technology partnerships with NASA Astrobiology or other organizations (e.g., Monterey Bay Aquarium Research Institute; MBARI) (see **1.10 Global scientific partnerships**). In particular, NASA is concerned with many of the same questions regarding life in extreme environments or inaccessible locations, and it would be helpful to adapt lessons from their experience in developing miniaturized instruments (also see **5.1 Assessing scientific achievements of scientific ocean drilling**, **5.3 Where will the program find new funding and new partnerships?**, and **5.5 Do we need a new paradigm for staffing and executing expeditions?**).

4.6 Observatories

(See Appendix A4.6)

Observatories are inherently well suited for monitoring changes in geophysical parameters, although there are many fewer technologies for measuring changes in geochemistry or biology. Development of needed systems should be encouraged. One idea is to develop a “smart” system; the system would be automated to perform a series of measurements or collect a sample when a pre-determined compound is detected.

The question of ownership or control of long-term monitoring facilities (i.e., observatories) is cloudy. One solution could be to form a standing committee to address these issues within the programs science advisory structure of the program. **Boreholes and CORKs (circulation obviation retrofit kits) could be treated as a facility**; investigators should be encouraged to keep the concept of shared use facilities in mind as they design proposed projects, and to incorporate mechanisms that allow shared access/sampling. As observatories are important concepts, how are the sites for them chosen?

The next scientific drilling program should include long-term planning for firm commitments of selected initiatives. This planning would require community support that goes beyond the expertise of the group already involved in ocean drilling, and should be done on a large scale. Whether this represents a series of related proposals to the program, better integration of interdisciplinary work, and coordination of multiple expeditions with similar goals, needs to be worked out (also see **5.6 How should the new IODP science plan be organized?** and **5.9 IODP planning structure**).

4.7 Aging of the oceanic crust

(See Appendix A4.7)

The topic of the **impact of low-temperature hydrothermal activity and biogeochemical cycles on the aging of the oceanic crust** provides an integrative approach to study the aging of the crust. This will be accomplished by characterizing physical, chemical, and biological forces occurring in the crust, the associated positive and negative fluxes, and the potential impact of these processes on cycling at subduction zones. Ideally this research could be completed on a

range of locations because the processes depend on the spreading rate, however much could be gleaned from starting with transects of similar ages and spreading rates that have different surface expressions (sediment cover versus sediment interrupted by basement outcrops). A study along these lines will truly help the community understand the fluid, energy, and chemical fluxes through the crust and the depth of these processes. Much of the engineering and technical capability to accomplish this work already exists within the IODP.

5. STRATEGIES

5.0 Overview

(See Appendix A5.0)

The Strategies board discussion was wide ranging, with a number of topics initiated by the moderators and others initiated by participants. Participants in some topics, such as education and outreach, came to agreement quickly with regard to changes needed for the next scientific ocean drilling program. In the case of education and outreach there was broad consensus that the program should build on successes like Deep Earth Academy's School of Rock, and then go further. Other topics, such as ways to increase participation in the program, and how to develop collaborations with other science programs (also see **4.3 Microbiology within IODP**), produced a broad range of good ideas and discussion about implementation. Lastly, there were topics, such as what to do with proposals currently in the program, site survey and technological development strategy and funding, for which there was healthy debate about how to proceed. But even in these cases, there was recognition that adjustments were needed in how these challenges were being managed, and constructive and practical suggestions as to improvements that could be made going forward.

5.1 Assessing scientific achievements of scientific ocean drilling

(See Appendix A5.1)

Respondents felt that more attention needed to be paid to *synthesizing past drilling results*. They suggest that this kind of activity will be critical for building a case for the next program and would be generally useful to the scientific community and the general public. This idea is consistent with the crosscutting idea that scientific ocean drilling is a piece of a broader network of scholarship, experiments, and exploration related to Earth history and active processes – IODP results need to be placed in context, not left on their own. Synthesis efforts could be published in *Scientific Drilling*, monographs, or “case studies.” Resources should be available for synthesis activities as well as for continuing analyses of cores and other drilling data. CHART participants did offer some examples of important IODP accomplishments that they thought should be highlighted (See **6.1 Examples of key IODP scientific discoveries**).

5.2 How can IODP increase participation?

(See Appendix A5.2)

The responses to this board concentrated on *education and outreach opportunities* and increasing the availability of scientific ocean drilling to researchers, teachers, and the general public. The suggestions included:

- Education and outreach can begin at the project planning and preparation stage, so as to include the scientific process of experimental design
- Education and outreach activities could be enhanced by more real-time communication from the ship
- Building on the success of Deep Earth Academy's School of Rock success
- Using web-based tools such as Google Earth and GeoMapApp to make data, models, and analytical results available

- Making data formats and presentation more user friendly
- Providing keywords in publications for more effective searching.
- Using ship transits for education and outreach activities with teachers and others aboard (journalists, students, poets, artists, etc.)

A final, but very important, suggestion is that we must package the program better to the public. There is an ongoing shift in thinking about education and outreach within IODP. Education and outreach used to be viewed as draining resources from scientific programs but are now viewed broadly as an investment that helps to make research possible. In addition, connecting with the public-at-large *is part of doing science*, and this needs to be a conversation, not just a one way flow of information. Above all, we need to get the word out about the exciting results from IODP and future expeditions. One participant noted that, “*Every student in the U.S. has heard of NASA, but how many have heard of ODP or IODP?*”

5.3 Where will the program find new funding and new partnerships?

(See Appendix A5.3)

The responses here include discussions of ways to fund the day-rate of the *JOIDES Resolution (JR)* using stimulus funds or to distribute the amortization of the cost overruns on the *JR* over a larger number of years, and suggestions that the *ship could be funded by outside sources if it were to work more in the Arctic or Southern Oceans*, or were to work on *methane hydrate problems* (also see **4.2 New scientific question** and **4.5 Enabling technologies**). There are posts related to the ongoing efforts by IODP-MI to seek oil industry funding for a program of scientific drilling in the Atlantic. The enthusiasm for this was mixed, mostly because the academic community has not been able to do this very effectively in the past. In addition, it is not clear what the program membership rate would be for an industry consortium or foundation, or what they would get for their investment. This has been left open as current negotiations progress, for examples with consortia of petroleum companies, but the uncertainty might make it difficult to secure commitments.

The next drilling program probably needs to figure ways *to build bridges to other funding sources for the future*. There was mention of trying to *reach out to maritime nations* that have not been a part of IODP in the past; this may require different formal structures for participating in science done in their waters of interest. Finally, there was a post highlighting joint efforts to fund site surveys with international scientific partners (also see **1.10 Global scientific partnerships**). These issues are currently dealt with on an *ad hoc* basis, but there could be guidelines or structures developed that would allow non-ODP partners to become partners for the short-term or long-term, and would allow them to have confidence that their investment will provide substantive returns.

5.4 With what other programs should IODP be coordinating?

(See Appendix A5.4)

Improved collaboration with ICDP was mentioned in several posts (also see **1.10 Global scientific partnerships**). There was a sense that the coastline should not be a hard boundary for scientific drilling activities and that both programs are science driven and share many common scientific themes. This is consistent with the crosscutting idea that the next program must be viewed to exist within a broader context. Having hard geographic boundaries on operations and

experiments, based mainly on water depth, may limit opportunities for leveraging and scientific breakthroughs.

It was suggested that the *SAS have more overlap with the steering committees of other programs* like MARGINS, RIDGE, InterRidge, etc. There is an opportunity now to interface with MARGINS as it develops a science plan for its possible renewal by NSF. There was also support for building collaborations with the growing Ocean Observatories Initiative. (Also see **4.3 Microbiology within IODP**).

5.5 Do we need a new paradigm for staffing and executing expeditions?

(See Appendix A5.5)

Some participants think the current model of IODP is (mostly) too rigid, and that *there should flexibility* beyond the model of scientific drilling expeditions lasting a standard time (generally eight weeks), with full staffing in standard categories. It was noted that some studies may require fewer scientific personnel, and those spaces could be used for engineering, training, students, education and outreach activities, or other purposes. In addition, given rapid communications and the ability to move large data sets through the Internet, the definition of a scientific party could be modified in many cases to include shore-based researchers working in real time with shipboard personnel. There were additional posts supporting the scientific *benefits of continuing to sail large scientific parties*. The arguments for this included the scientific interaction, the international collaboration, and the transformative effects of the experience (also see **4.3 Microbiology within IODP**). It was also noted that the IODP member countries expect to have the opportunity to sail scientists on the ship. A new approach to implementing expeditions was also extensively discussed in **3.6 Implementation strategies**.

There was support for the *improvement of the education and outreach* activities that could come from the use of modern technology and broader bandwidth communication from the ships (also see Appendix A5.1).

5.6 How should the new IODP Science Plan be organized?

(See Appendix A5.6)

There was support for *leading the new program with the topics that are new to the program or which have clear societal relevance*. This could be followed by a thematic approach to cover a broader range of topics.

However the new science plan is constructed, the program, in its implementation, should anticipate and *welcome ideas that go beyond a small number of fixed themes*. The science plan should be used as a guide rather than as a rigid plan. There should continue to be a place for unsolicited proposals.

There was significant support, however, for building into the science plan something like *“missions” so that the program can tackle scientific questions that require extended or technically ambitious drilling* in one area or coordinated programs of global drilling, as also noted in the environmental change discussions (see **3.6.1 Missions, or mission-types planning**). It was suggested that there might be a very small number of carefully selected missions built into the science plan, much as the ideas that eventually became the NanTroSEIZE CDP grew out of discussions for studies of the seismogenic zone at the COMPLEX planning meeting. The

Seismogenic Zone Initiative was described in the ISP for the first phase of IODP, and eventually a competitive proposal worked its way through the science advisory structure.

The challenge is to balance these imperatives. Allowing new ideas to generate proposals that lead to drilling operations means keeping options open for adjustments to priorities and the schedule. However, many important scientific programs require that the program make long-term commitments; for example, to have one drilling expedition followed by another expedition two or three years later. If the program is unwilling to make commitments on a timeframe longer than a single operating year, ambitious and transformative projects could be lost. Many such programs require significant technical development, engineering, or securing external funding for coordinated activities (additional site surveys, multi-ship experiments, observatory servicing, etc.). There is nothing that prevents IODP from making a small number of long-term commitments, subject to the requirement that projects meet verifiable metrics (complete initial work, secure necessary external support, etc). This would still leave room for short-term innovation and modification of drilling plans.

As in other parts of the Strategy board, there were appeals for funding to cover twelve-month drilling programs on all three IODP platforms.

5.7 How can we improve funding for technical development and site surveys? (See Appendix A5.7)

There is strong support for *improving pre- and post-drilling site studies* as a way to reduce drilling risk and improve scientific value of ocean drilling. Fewer good sites are better than more poor sites, and fewer outstanding projects may be better than more traditional projects. This could be done by viewing IODP more broadly as a “Sub-seafloor Science” program rather than a drilling program (also see **4.3 Microbiology within IODP**, **4.5 Enabling technologies** and **4.6 Observatories**). The NanTroSEIZE project management team was put forth as a model for such a holistic way of planning site characterization and drilling in IODP. It is critical that the right data be collected as part of site surveys, and that it be collected in time to influence the planning of the science to be addressed, the locations of the sites, the drilling science plans in the holes, and the technological developments required to realize the science. All of this requires long lead-time planning and execution. There was discussion of the conundrum for site surveys in which one must have a highly ranked drilling proposal to obtain site-survey funding, but the development of a great drilling proposal often requires excellent site characterization well in advance of drilling. Similar issues apply in the case of tool development or observatory development and servicing – securing funding for these activities often requires a commitment for drilling, and if the program cannot follow through, there will be less incentive for funding and scientific partners to collaborate. Solving this dilemma points to the need for long-term commitment and project management.

Multinational efforts to acquire site characterization data can be ways to fund better results. The collaboration of Japan and the U.S. in the 3-D reflection volume acquired for the NanTroSEIZE project are a fine example of this type of collaboration.

There is strong support for expanded technology and engineering development activities as described in the IODP technology roadmap. One specific technology improvement that is desired in enhanced core recovery and quality in difficult environments.

5.8 Education of sea-going scientists for the future of ocean drilling

(See Appendix A5.8)

The main thread of this discussion was the observed lack of educating scientists in the planning and running of site-survey and other cruises. More *institutions need to teach the “hands-on” aspects* of collecting and using marine data. In other posts, this education was extended to include learning about drilling and associated activities. The causes of this lack of education include fewer cruises, fewer “practical” courses offered, and fewer institutions operating seagoing equipment. This was one of the few topics in this discussion in which students joined in the discussion, *expressing their frustration with the paucity of opportunities* to break in to seagoing science.

There was support for offering some courses, open to all, involving short cruises and diverse types of equipment. Perhaps one or more IODP partners could fund these courses. Training of a new generation of ocean drilling scientists and re-training with a focus on cross-disciplinary science was also discussed in **3.6.3 Cross-disciplinary meetings and training**.

Another way of providing seagoing experience to students is to fill otherwise empty berths on scheduled cruises; however, the point was made that this should be accompanied by real “mentoring” by someone in the science party, lest the student not gain no real insight into what is happening. Perhaps the matching of empty berths and the sailing of a mentor scientist could be coordinated and paid for by the drilling program. Of course, this presumes that there are empty berths to fill, as discussed in **5.5 Do we need a new paradigm for staffing and executing expeditions?**.

5.9 IODP planning structure

(See Appendix A5.9)

There was discussion about developing some type of *dual track system in the Science Advisory Structure (SAS)* for handling: 1) small and simple unsolicited drilling proposals, and 2) complex, large, or global, mission-type proposals and projects.

There were suggestions to *split up the Science Steering and Evaluation Panel (SSEP) into a group of themed panels*. It was argued that this would keep some themes from being lost or underrepresented and would have membership better suited to evaluate specific proposals; however, it was suggested that these groups meet at the same place and time so that they could address topics crossing the themes.

The topic of project management teams came up as a way to *better coordinate site-survey, technical development and drilling activities*.

Lack of institutional memory in the SAS was cited as a problem, because there can be a lack of follow through on commitments. This problem is particularly troubling for the large complex programs, unless they are set up with a project management team.

Finally, there was a spirited discussion on the *role of management in IODP*. While the size and role of management was debated, there seemed to be agreement that the new ocean drilling program would need to be managed more effectively in order for the program to be truly integrated.

5.10 How can IODP contribute to "broader impacts?"

(See Appendix A5.10)

The traditional view of broader impacts in scientific ocean drilling is that they are achieved through training of new scientists, putting young scientists into the international scientific community, and mixing new scientists with scientists from other specialties. ***IODP experiences can transform careers.*** In addition, many forms of basic research are societally relevant, but IODP participants may need help in connecting their work to the public at large.

In addition, we need to better utilize cores and data for education and outreach activities that will have broad public appeal. Deep Earth Academy's School of Rock has been a model of success in this area and should be built upon. Through education and involvement through programs such as School of Rock, we inform the general public about the value of ocean drilling science and hopefully generate enthusiasm for continued drilling. An educated public will support the idea of greater financial commitments by funding agencies. ***Good education and outreach programs are essential to building public support for IODP.***

There was much enthusiasm for using ***new media in reaching the public*** and telling our story from the ships. Ideas proposed include:

- Sailing a scientist/educator on each expedition to translate for the public
- More shipboard bandwidth for communication to the shore
- Expeditions prepare 2-3 slides to be used in intro Earth science classes
- Videos, blogs, podcasts, and other media

5.11 Old proposals, new proposals, and a limited schedule

(See Appendix A5.11)

In their discussion regarding how to handle the large number of proposals in the system, a number of participants argued that we should not be quick to reject proposals in the system even if they are unlikely to be drilled soon. ***A large pool of excellent proposals shows that we need more drilling.*** Keeping them in the system preserves all the hard work of assembling the data and other background material.

Other participants argued that a large pool of mature proposals that are unlikely to be scheduled soon ***should be returned to the mix with new proposals.*** Having too big a pile of proposals in the queue, more than can be drilled for many years, discourages new potentially exciting proposals from being submitted.

The issue of frequently re-ranking mature proposals produced lively discussion. Re-ranking by a changing set of panel members can make it virtually impossible for an excellent but complex proposal to stay near the top. Re-ranking can discourage proponents who seemed to have made it to the top scientifically but were not scheduled. On the other side, re-ranking gives new proposals a chance to rise to the top and therefore encourages the submission of proposals.

Some posts pointed out a problem with sending proposals lacking site-survey data to the Operations Task Force (OTF) (but in fact, this is rarely a problem because proposals forwarded to the OTF are supposed to be ready for drilling). A broader issue is drilling proposals forwarded to OTF that are part of larger programs for which external funding is needed for instrumentation,

engineering, or follow-up expeditions. In many cases, this external funding is not made available because the proposals are not formally scheduled. It would help if forwarding of a proposal to the OTF represented a commitment to drill within some time period, or if OTF identified a subset of proposals for which commitments can be made, even if drilling will not occur within the next year of operations.

One poster suggested that we deal with uncertainty in available drilling time in the future by presenting two science plans to NSF for funding. One would be based on an optimistic estimate of drilling time available; the second would be based on a more modest drilling schedule. Two science plans could solve the reciprocal dangers of promising more than we can deliver if drilling time is modest, and not being aggressive enough to argue convincingly for full-time drilling.

Arguments were offered in favor of ***1) longer-term planning and scheduling of drilling, and 2) shorter term planning and scheduling of drilling.*** Those espousing longer term planning argue that it provides more time for coordinating site surveys, technology development, tool construction, and planning science effectively. On the other side, some posters argue that the best results will be gained by planning only one or two years out and continually re-assessing and ranking the mix of remaining proposals. The first approach seems best for more complex projects, and the second seems best for simpler projects. This approach feeds back into the discussion above of dual tracks through the SAS for these types of projects.

Concerns were raised about the difficulties posed for planning in IODP of off-contract periods for the ships. These planning difficulties would be the result of being unable to operate the ships twelve months per year, would particularly cripple long-term planning, and suggests that ***the program can really only operate efficiently in a twelve-month-per-year mode.***

Several suggestions were offered to provide ***longer term planning and efficiency in the program.*** Gradual circumnavigation of the Earth provides a way to anticipate approximately where the ship will be years ahead; this facilitates site-survey planning and scheduling. Regional ranking might provide a way to build a more efficient ship track, perhaps reducing transit times.

INVEST might be a forum to identify a small number of long-term priorities for the program. With the blessing of the community, perhaps these could become the “missions” of the renewal proposal. It is hard to build consensus for long-term priorities outside of a big open meeting like INVEST.

The drilling mix should ***include new ideas and drilling concepts as well as continuing work*** to build on the continuing questions the program has addressed before.

6. IODP STRENGTHS AND WEAKNESSES

(See Appendix A6)

6.0 Overview

CHART participants were asked to provide their ideas regarding the strengths and weaknesses of IODP. Their responses indicated that the strength of IODP has been its many scientific discoveries, and they provided concrete examples that are summarized below (see **6.1 Examples of key IODP scientific discoveries** and Appendix A6.1).

Their responses also indicated that the weaknesses of IODP could be solved by having more drilling time, more resources for site survey, engineering, and technologically challenging science, and better management and outreach (see Appendix A6.2). The content of the posts on the weaknesses of IODP were summarized in the previous section of this report (see **5. STRATEGIES**).

6.1 Examples of key IODP scientific discoveries

(See Appendix A6.1)

In addition to the thematic reviews currently being conducted by IODP-MI, CHART participants were asked to provide examples of several of the key IODP accomplishments. Their posts articulated the unique accomplishments of IODP (see full text of posts in Appendix A6.1). In particular, CHART participants pointed out how technological advancements have lead to some of IODP's most impressive scientific accomplishments. Observatories and borehole experiments have revolutionized our ability to study fluid flow in the crust. Geotechnical advancements, in collaboration with industry, have transformed our ability to study hydrogeology in overpressured systems where landslides occur. Deep drilling has advanced our knowledge of the formation and evolution of the crust and upper mantle. Advanced piston corer (APC) technology in high depositional rate environments has revolutionized studies of climate change and the behavior of the geodynamo. Shallow-water drilling technology has put IODP at the forefront of sea-level change studies. IODP is also responsible for unique and transformative discoveries in Earth sciences such as the origin and formation of gas hydrates.

6.2 IODP weaknesses

(See Appendix A6.2)

As mentioned above, the posts on IODP weaknesses have been incorporated into the summary and appendices of the previous section of this report (see **5. STRATEGIES**).

APPENDIX

A0 CHART GOALS, ORGANIZATION AND PARTICIPATION

A0.1 Information posted on CHART website

A0.1.1 Overview

Charting the Future Course of Scientific Ocean Drilling (CHART) is an online workshop to maximize and broaden U.S. community input into the IODP science planning process. In addition, we hope to stimulate discussion about other aspects of IODP including priorities that ensure high-profile results, approaches that spark novel discoveries in emerging fields, and strategies for success within the current science funding culture.

The CHART meeting is an important opportunity for the U.S. community to organize its priorities and goals. Your input and support will help ensure that we retain access to ocean drilling technology for producing transformative, world-class science. Mark your calendar for this key meeting; participation is free and open to all U.S. scientists.

CHART will result in a white paper defining the U.S. scientific community's initial thoughts on future science directions, structures, and technologies for IODP. The white paper, and results from other science meetings taking place in Japan (contact J-DESC for details) and Europe (contact the ESSAC office for details), are part of the planning process leading up to the international INVEST Workshop.

A0.1.2 On-line welcome letter

Dear Colleagues:

The CHART Workshop is the first step in garnering input from U.S. scientists to plan the renewal of the Integrated Ocean Drilling Program (IODP) beyond 2013.

The science planning process that will culminate in the science plan for IODP (2013-2023) needs to begin now. The purpose of the CHART meeting, along with parallel science planning activities in other countries, is to initiate discussions about our collective vision of the future of IODP. Through the CHART Workshop, we hope to gauge the level of interest of the U.S. community in IODP and to begin the creative process of defining how ocean drilling will lead to transformative science in the future.

The results of the CHART meeting, other science meetings taking place in Japan and Europe, and recent IODP-MI sponsored workshops, will augment the large international planning meeting, INVEST - IODP New Ventures in Exploring Scientific Targets.

INVEST is an open international conference to define the new science directions of IODP and will take place September 23-25, 2009 at Bremen University. The proceedings of INVEST will be written by mid-2010 and will be used as a primary source of ideas for the science and implementation plan of the next phase of IODP. The goal is to have this plan written, vetted, and reviewed in time for approval by interested funding agencies in 2012.

The CHART meeting is an important opportunity for the U.S. community to speak up and organize its priorities and goals. Your input and support will help ensure that the U.S. community retains access to ocean drilling technology for producing transformative, world-class science.

With thanks,

CHART Steering Committee

Christina Ravelo (Chair), University of California, Santa Cruz

Brandon Dugan, Rice University

Bob Duncan, Oregon State University

Katrina Edwards, University of Southern California

Sean Gulick, University of Texas at Austin

Gabriel Filippelli, Indiana University-Purdue University, Indianapolis

Andrew Fisher, University of California, Santa Cruz

Clive Neal, University of Notre Dame

Demian Saffer, Pennsylvania State University

Dale Sawyer, Rice University

Mitch Schulte, University of Missouri-Columbia

A0.1.3 Workshop goals

The CHART Workshop will provide an online forum for discussion about future scientific directions of the Integrated Ocean Drilling Program (IODP). In addition, the workshop will stimulate discussion about many other aspects of IODP including priorities that ensure high-profile results, approaches that spark novel discoveries in emerging fields, and strategies for success within the current science funding culture.

Products

The CHART Workshop will lead to a white paper based on input from the U.S community. The white paper will discuss the U.S. community's position on future IODP scientific directions and priorities, and a compilation of opinions and ideas expressed during the CHART Workshop.

The CHART Workshop white paper, along with results from meetings in Japan and Europe and from recent IODP workshops, will be used to organize and define breakout groups at the INVEST meeting. In addition, it will potentially be used to influence the

content of the INVEST proceedings and eventually the new science plan for the next phase of IODP.

Impact on the IODP Science Planning Process

The CHART Workshop will allow ideas of the U.S. scientific community to mature and develop before the international INVEST meeting, ensuring that the INVEST meeting is as productive as possible. CHART will help to maximize and broaden U.S. community input into the IODP science planning process, beyond what will be possible at the INVEST meeting alone. After participating in the CHART Workshop and reading all of your colleagues opinions and the CHART white paper, each participant will better understand the broader U.S. community's opinions and needs. With this knowledge, those of you that attend the INVEST meeting will be in a good position to express the collective vision of your U.S. colleagues, particularly those from outside the 'traditional' scientific drilling community who may not be able to attend the INVEST meeting.

A0.1.4 *On-line instructions to register and participate*

Step #1: Please read the Welcome and Goals webpages for information about the online CHART Workshop.

Step #2: Enter the CHART Workshop Forum.

Step #3: Click any of the discussion boards to see the conversation topics. Everyone may read the discussion boards, but only registered participants may post comments.

Step #4: To contribute to any of the discussions, you will need to register. Registration is free and open to all U.S. science community members. For your username, please use your first and last name with no spaces. For example, Alfred Wegener's username would be "AlfredWegener". After completing the registration form, a password will be e-mailed to you. If you don't receive your password, please contact Ken Wantanabe.

Step #5: Click on the topic to read the conversation. If you would like to post a new topic, click on "Add Topic" towards the top of the main page for the board (e.g., "Emerging Fields and New Cross-Cutting Disciplines"). To post a reply to a question or comment, click "reply" under the post. You may revisit and contribute as often as you want. Be sure to look for new discussion questions throughout the workshop as the moderators will guide and focus the discussion based on community input.

A0.1.5 *Weekly summaries*

Week 1 Summary

Dear Colleagues:

The current phase of the Integrated Ocean Drilling Program (IODP) ends in 2013, and planning for the IODP renewal has begun. One important step in this planning is the on-line CHART meeting going on right now, and we need your input.

Why should I participate in CHART? The CHART meeting is an important venue for the U.S. community to organize their thoughts about the future scientific priorities of IODP. The actual numbers and diversity of participants will be a measure of U.S. interests. If you want to influence the new science plan and help justify renewal of IODP, now is the time.

Should I participate in CHART even if I think the initial science plan (ISP) for IODP is great? YES. To secure renewal of IODP, we need to develop a new science plan that is fresh and compelling. If there are parts of the ISP you think should continue to be emphasized, you need to speak up.

Should I participate in CHART if I plan to attend INVEST, the international planning meeting in Bremen in September 2009? YES. The CHART meeting results will directly influence the INVEST meeting. For example, the breakout group sessions at INVEST will be, in part, determined by the result of CHART. In addition, productivity at the INVEST meeting will be much higher if we have already started organizing our plan. CHART is an important part of the planning process.

Should I participate in CHART if I don't know much about ocean drilling or have not been involved in IODP? YES. In the past, the ocean drilling program has benefited tremendously from the infusion of ideas from outside those research fields traditionally served by ocean drilling. Outside perspectives are needed to keep ocean drilling science focused on far-reaching science questions. We welcome your ideas on how ocean drilling is needed to meet today's grand challenges in earth, biological and climate sciences.

Why is CHART an on-line meeting? CHART allows you to participate in science planning for IODP renewal while having time to think, consider other people's ideas, and respond. CHART has the capacity to include input from a lot of people because it is open to those that don't have the time or resources to travel to another meeting. The on-line format allows us to archive, in written form, input from every participant; most meeting reports preserve popular or consensus views as expressed in writing by a few meeting organizers. CHART does not replace the needed face-to-face discussions that will take place in at the INVEST meeting in Bremen.

Best Regards,

Christina Ravelo
Chair, CHART Organizing Committee

Now is your opportunity to influence the research priorities of IODP. The conversation has started. Everyone may read the discussion boards, but we strongly encourage you to register and post a comment or question. <http://www.oceanleadership.org/chart>

Here are some highlights of what has been discussed so far.

Discussion Board on Emerging Fields:

- Posts during the first week have focused on CO₂ storage and mitigation, extreme climate, and on geohazards. The emerging fields suggested so far encompass both promising science and societal relevance.
- Unanswered questions: What can ocean drilling provide with regard to geologic and geo-engineered carbon sequestration? Where and how can we study extreme events with sub-century resolution? What are other key themes that have been embedded in individual drilling endeavors but now have the potential to thrive as stand-alone science programs?

Discussion Board on Solid Earth Cycles and Geodynamics:

- Participants have outlined justification for drilling to the MOHO and for making the exploration of the ocean crust an important focus of the new science plan for IODP.
- What other arguments can be made in support of this idea? What other research thrusts in Solid Earth Cycles and Geodynamics should be emphasized in the new science plan?

Discussion Board on Deep Biosphere and Subsurface Ocean:

- Participants have suggested increasing funding explicitly for microbiology studies to encourage more IODP participation among the community, and supporting technology development relative to microbial observatories.
- What other advances in geobiology are needed to further the microbiology for IODP, and how do we make microbiology more of a primary focus of IODP projects?

Discussion Board on Environmental Change, Processes and Effects:

- The posts focused on capturing deep-time climate extremes on a time scale and scope that are informative for current and future climate change.
- Emerging questions from the discussion: Can ocean drilling provide more answers to Milankovitch-scale variability than we already have (i.e., is there more to be learned from ocean drilling)? What ocean regions or proxies might best be studied to further the connection between model-based hypotheses and sediment-based observations?

Discussion Board on Strategies:

- The Strategies section of the CHART Workshop discussion board was the most active during the first week. We had intended initially to delay this part of the discussion, but participants clearly are anxious to contribute to strategy-related topics.
- There was much discussion regarding the treatment of existing drilling proposals, and the possibility of a truncated annual operations schedule. Opinions differ on these points, but a consensus appears to be emerging.

Week 2 Summary

Dear Colleagues:

The online CHART workshop has reached the halfway point. The discussion so far has been fruitful with over one hundred posts, but we still need your help. The majority of comments have come from a few scientists; we need to expand the dialog to include a greater range of ideas and to demonstrate the strength of the U.S. community interest in IODP. Here is a sampling of some questions that you could help to answer:

- There is an on-going discussion about developing a sub-seafloor microbial observatory. What is needed to do this? Should this be a major effort in the next phase of IODP?
- Thus far, most of the comments in the Solid Earth Cycles and Geodynamics forum were focused on ocean crust formation and drilling to the MOHO. Should IODP make a commitment to this endeavor? What are other high priority research goals in the Solid Earth theme?
- One interesting line of discussion is about aligning IODP goals with those of the IPCC to enhance predictability of the future climate change. What are the specific scientific questions that are relevant to the IPCC that can be answered with ocean drilling?
- Participants have identified several emerging fields and potential new directions for IODP including focused efforts to explore polar environments, decipher the geodynamo, and understand the impacts of ocean acidification. How can ocean drilling advance these fields of research? Are there others?

The CHART moderators will be posting summaries and new questions periodically. Please look for these summaries. We look forward to hearing about your ideas.

Best regards,
 Christina Ravelo
 on behalf of the CHART steering committee

Week 3 Summary

Dear Colleagues,

We are now in the 4th week of the CHART workshop, and many of you have contributed to productive discussions. Some of the discussion boards have posted summaries – please read these and add your comments. Moderators are also posting some new questions for you to consider. If you haven't contributed yet to CHART, now is the time to get involved. The community needs your input.

Regards,
Christina Ravelo
On behalf of the CHART Steering Committee

CHART Week 3 Highlights:

In the Solid Earth Cycles and Geodynamics discussion forum, there is continued discussion on drilling oceanic crust in a variety of tectonic settings to truly understand its origin and evolution. And, there are new posts focused on volcanic arc-subduction and seismogenic zones. For example, there is enthusiasm for observatories in the seismogenic zone to address societal impacts (earthquakes, tsunamis, arc volcanoes). Should this be a high priority for the program beyond 2013?

In the Environmental Change, Processes and Effects discussion forum, new discussions centered around cryosphere stability, understanding the timing of tectonic events in relation to climatic events, and reinforcement of the importance of IPCC-relevant science. New questions to consider are: How might scientific ocean drilling approach Earth system responses to abrupt changes OTHER than warming (e.g., impacts, ocean ventilation events, warming and clathrate destabilization)? What processes and dynamics are important for the continental-ocean transfer of nutrient elements?

In the Deep Biosphere and Subseafloor Ocean discussion forum there is discussion about the long-term planning needed for observatories, the need to develop deep biosphere as a field including more effective outreach, and the many important unanswered questions regarding subseafloor fluid flow. Some specific questions posed: How do we decide where is best to put observatories? How does fluid flow evolve as crust ages? What is the relationship between basement permeability, sediment thickness and basement relief in evolution of fluid flow?

In the Emerging Fields and New Directions discussion forum, participants have suggested looking at global scientific drilling opportunities by linking with other drilling-related programs and addressing societally relevant themes like CO₂ management, near-future climate, geodynamo, and surface process feedbacks. What are other new research ventures? What are the key questions for these themes?

In the Strategies discussion forum, the Old Proposals/New Proposals section continues to be the most popular, with participants discussing the need to encourage exciting new ideas balanced against important topics featured in highly ranked proposals that have been in the system for a while. In the area of Technical Development and Site Surveys,

there was discussion of decoupling survey support from ranking of drilling proposals. On the topic of IODP Planning Structure, there was discussion of coordination for complicated drilling programs because a long period of review can result in contradictions in panel recommendations. A key question for the Strategies discussion is: What specific changes to IODP planning, administration, and operation should be undertaken to help achieve high-priority objectives during the next phase of IODP
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Week 4 Summary

Dear Colleagues:

The CHART Workshop ends this Friday, March 6th, and there are still many in the community who have not participated. It is not too late to become involved, and we especially encourage you to contribute in this final week.

There has been a lot of good discussion on strategies and new directions for IODP, but we also are challenged with deciding how to organize and express the scientific goals of the new science plan for drilling beyond 2013. In this final week of CHART, we would like your suggestions: Should the new science plan for post-2013 drilling be organized differently than the current IODP plan (the IODP Initial Science Plan)? Should there still be themes and initiatives? If so, what should they be? If not, what other structure might be better?

Please go to the “Strategies” board, and look for a new question posted by Andy Fisher called “How should the _new_ IODP Science Plan be organized?”. We look forward to an energetic discussion.

We thank all of you who have participated so far and hope that you continue to contribute as the workshop comes to a close.

With thanks,
Christina Ravelo
On behalf of the CHART steering committee

Week 4 Highlights

Emerging Fields and Cross-Cutting Disciplines: New topics have been posted on detailed geothermal studies, atmospheric-oceanic interactions and biogeochemical cycles, and permafrost gas hydrates. There was also further support for CO₂ mitigation, ocean acidification, polar studies, global drilling partnerships, surface-rock geochemical cycles, geohazards, future warming, and ocean-lithosphere cycling. Are there ways to develop overarching themes or initiatives based on any combination of these topics? Post your ideas and help create a coherent U.S. statement for INVEST.

Solid Earth Cycles and Geodynamics: There has been continued discussion about large igneous provinces (LIPs), including a proposed partnership with the International Continental Scientific Drilling Program to link hotspot trails and LIPs. Discussion is also focused on research about the structure of the oceanic crust, including associated technical challenges of such studies and models of seismic stratigraphy. Regarding seismogenic zones, there was emphasis on the need to follow through with what has already been started. New topics such as triple junction systems and subduction initiation were posted. Should these or other themes be developed for the new science plan?

Environmental Change, Processes and Effects: Over the past week, participants have posted numerous new topics, including intriguing new perspectives on studying how Milankovitch forcing produces different responses in time and space, the importance of ultra-high resolution records, the need to better understand fluid flow as a transfer mechanism, and the need to better understand gas hydrates as carbon storage capacitors. Along with previously emphasized polar climate studies, participants also discussed sea-level estimates during critical intervals and the need for improved efforts to integrate drilling project with paleoclimate modeling. Are these the most important topics, or are there others?

Deep Biosphere and Sub-seafloor Ocean: There is continued productive discussion on observatories, microbiology within IODP, technological needs, and the sub-seafloor ocean. New ideas include: a wish list of technology for microbiology and observatories to foster development of non-existent technologies, include IODP activities in microbiology talks and microbiology meetings, overcome the technological challenges to drill in young ocean crust for studying fluid flow and geobiology, extend the lessons from Expedition 301 to other areas to track fluids at crustal scale from recharge to discharge. Can a link be established between onshore and ocean hydrogeology?

Strategies: There remains consensus that IODP needs to remain open to new ideas and improved proposals, but there is also strong support for following through on earlier commitments. Balancing these objectives is made even more challenging with a reduced drilling schedule. Several participants have noted the importance of supporting site survey and technical developments for highly ranked projects. There was vigorous discussion about the importance of education and outreach in helping to define and/or enhance broader impacts for IODP. Although opinions vary, a majority of participants would like to see IODP do more extensive work in these areas. A new question open this week is to address what the structure of the new science plan for post-2013 drilling might look like.

General Discussion Board: This board has seen renewed activity as participants of IODP expeditions have been providing their viewpoints on the past successes of IODP and on the aspects that have not worked so well. Individual perspectives on how IODP has succeeded in the past, and how it has fallen short, are invaluable. We know that many of you have strong opinions about this. If you haven't already done so, please post your comments and suggestions.

Week 5 Summary

Dear Colleagues:

The CHART meeting has been extended and will end on March 13th. Last week we had lots of great discussion, but we also want to encourage those of you that haven't contributed to do so now. If you think ocean drilling is important, please show your support by helping to shape and focus the future of IODP.

Last week, we opened up discussion on a question posted by Andy Fisher regarding how the new science plan for post-2013 drilling should be organized. We got some response, but need more. We need to think about how themes and initiatives should be organized and whether and how we should prioritize science questions in the new science plan. If you have an opinion, post your thoughts on the "Strategies" board.

with thanks,
Christina Ravelo
on behalf of the CHART steering committee

Here are highlights of last week's postings and some new questions.

Emerging Fields and Cross-Cutting Disciplines: Discussions in the Emerging Fields and New Cross-Cutting Disciplines have resulted in nearly 90 posts. These topics propose new geographic regions to study, areas/themes with a dearth of IODP data, new-to-IODP fields, and potential expanded partnerships. How should these discussions be posed/grouped for INVEST? Are there scientific themes that combine some topics? What are the new scientific initiatives we should propose? What are the important engineering developments, technologies, and methods needed to achieve them?

Solid Earth Cycles and Geodynamics: This week we had 21 new posts, including some on carbon sequestration in peridotites, volcanic and non-volcanic rifted margins, understanding deep mantle geodynamics, and developing a SAFOD type borehole experiment to determine the strength of faults in ocean crust. There were also two new discussion topics created, one on Continental Breakup and the Formation of New Oceans and the other on Arc Evolution.

Environmental Change, Processes and Effects: This week, several new lines of discussion appeared, including the need to understand low-latitude processes using records in prime, high resolution settings and Mission Specific Platform expeditions, and the importance of investigating continental-ocean climate linkages using deep sea fan and other margin targets. Contrasting opinions about the need to focus on basic science while also posing questions of societal relevance have been expressed. This week, a new question has been posed regarding how the new science plan should highlight emerging questions. Should there still be a discrete theme on Environmental Change, Processes and Effects in the

new science plan? Or should questions in Environmental Change be better integrated with other science themes?

Deep Biosphere and Sub-seafloor Ocean: New posts include the need for in situ incubation as the key to collecting and identifying microbiological communities within the crust. It was suggested that observatories should be the focus of future IODP work for the deep biosphere, and that better support technology and instrument development is required. There is a need for understanding the relationship between gas hydrates and microbiology, the role of fluid flow in geochemical cycles, and role of fluid pressure in hazard generation potential. A remaining question is, should microbiology and deep biosphere studies be more of a priority for IODP? Drilling is essential to the study the deep biosphere, and IODP could potentially engage many new students and junior geomicrobiologists.

Strategies: There is broad consensus that outreach and education should be stronger in order to get new scientists involved in IODP. On the topic of proposal reviews, there was support for IODP identifying some complex or technically challenging projects early on and making a commitment to following through on these. There was continued discussion of linking drilling and site survey and other "auxiliary" proposals, rather than letting them remain disconnected. On the question of how the new IODP science plan be organized, there was support for highlighting genuinely "new" science, either by topic or region, and for focusing on topics that would be perceived to be especially relevant to society. The question that remains is: If and how should the program identify priorities within the new Science Plan?

General Discussion Board: This board has seen renewed activity as participants of IODP expeditions have been providing their viewpoints on the past successes of IODP and on the aspects that have not worked. For example, last week, there were posts on accomplishments in sea level studies, hydrates, hot spots and mantle plumes, formation of ocean crust. There were also posts pointing out that IODP/ODP have not done an adequate job on outreach, and that even with our past successes, we need to focus on fresh problems and new regions. Are there other ways in which you think IODP has been a success? Are there additional ways in which it hasn't worked well?

A0.2 Participant information and statistics

Table 1. Overall Statistics

Registered Users	186
Users who did not post	59 (31.7%)
Users who posted	127 (68.3%)
Users with 1 post	39 (30.7%)
Users with 2 posts	25 (19.7%)
Users with 3 posts	23 (18.1%)
Users with > 3 Posts	40 (31.5%)

Table 2. Activity in each discussion Forum

Page Title	Pageviews	Posts	Posts/View
CHART Workshop	11,446	535	21.4
Strategies	1,770	133	13.3
Solid Earth Cycles and Geodynamics	1,318	72	18.3
Environmental Change, Processes, and Effects	1,344	91	14.8
Deep Biosphere and Sub-Seafloor Ocean	1,092	93	11.7
Emerging Fields and New Cross-Cutting Disciplines	1,669	108	15.5
General Discussion Board	648	28	23.1
Frequently Asked Questions	231	10	n/a

Table 3. Participants by State

State	Visits	Registered Users	Posts
California	424	36	112
Texas	317	24	86
Oregon	237	19	68
New York	184	18	48
DC	172	5	41
Florida	137	9	33
Missouri	104	2	4
Massachusetts	91	12	34
Hawaii	79	10	26
Virginia	59	4	1
New Jersey	41	2	9
Delaware	30	1	1
Pennsylvania	30	4	2
Indiana	25	3	12
Wyoming	24	1	0
Michigan	20	3	3
Nebraska	18	1	13
Alaska	14	4	6
Illinois	14	1	0
Rhode Island	13	4	5
North Carolina	13	3	4
Colorado	12	3	6
Wisconsin	9	3	3
Maryland	7	1	0
New Hampshire	7	2	1

Louisiana	7	2	0
South Carolina	7	2	1
Idaho	5	2	1
Connecticut	4	1	5
Utah	3	1	0
West Virginia	3	1	1
Minnesota	3	0	0
Washington	2	1	4
New Mexico	2	0	0
Vermont	1	0	0
Georgia	1	0	0
Oklahoma	1	1	0
Arizona	1	0	0
Sum	2,121	186	530
Number of States	38		

Table 4. Participant Institutions

Institution	Registered Users
Oregon State University	15
University of Texas at Austin	12
Columbia University, LDEO	11
University of Hawaii	10
University of California, Santa Cruz	8
US Geological Survey	7
Woods Hole Oceanographic Institution	6
Texas A&M University	6
Scripps Institution of Oceanography	5
Consortium for Ocean Leadership	5
University of California, Davis	4
University of Miami	4
University of Florida	4
University of Southern California	4
University of Wisconsin-Madison	3
Rice University	3
University of Oregon	3
University of California, Santa Barbara	3
Rutgers, The State University of New Jersey	3
University of North Carolina at Chapel Hill	2
Brown University	2
University of Alaska Fairbanks	2
University of Rhode Island	2
University of Michigan	2

Retired	2
University of New Hampshire	2
Tulane University	2
Queens College, City University of New York	2
Idaho National Laboratory	2
Boston University	2
University of South Carolina	2
Moss Landing Marine Laboratories	1
Washington University	1
NOAA and University of Colorado	1
Duke University	1
Virginia Museum of Natural History	1
University of Delaware	1
University of Massachusetts Amherst	1
Adelphi University	1
Brigham Young University	1
Florida International University	1
Stanford University	1
Indiana University-Purdue University Indianapolis	1
Monterey Bay Aquarium Research Institute	1
Museum of Nature and Science	1
University of South Florida	1
University of California, Riverside	1
Harvard University	1
Indiana University of Pennsylvania	1
Marine Biological Laboratory	1
University of Wyoming	1
Indiana State University	1
Lafayette College	1
University of Massachusetts-Amherst	1
Legg Geophysical	1
Indiana University of Pennsylvania	1
University at Albany-SUNY	1
Texas A&M University, Corpus Christi	1
California State University, Northridge	1
Chevron and Stanford University	1
University of Tulsa	1
University of Colorado	1
University of Notre Dame	1
Wesleyan University	1
Princeton University	1
Portland State University	1
University California, Santa Cruz	1
University of Nebraska-Lincoln	1

Pennsylvania State University	1
Northwestern University	1
University of Missouri-Columbia	1
James Madison University	1
ExxonMobil Upstream Research Company	1
Yale University	1
Minerals Management Service	1
West Virginia University	1
University of Missouri	1

Table 5. CHART website visits per day

Day	Absolute Unique Visitors	All Visits
Monday, February 2, 2009	22	29
Tuesday, February 3, 2009	44	61
Wednesday, February 4, 2009	36	58
Thursday, February 5, 2009	45	70
Friday, February 6, 2009	43	74
Saturday, February 7, 2009	10	13
Sunday, February 8, 2009	12	26
Monday, February 9, 2009	43	63
Tuesday, February 10, 2009	33	52
Wednesday, February 11, 2009	43	73
Thursday, February 12, 2009	32	43
Friday, February 13, 2009	22	27
Saturday, February 14, 2009	4	4
Sunday, February 15, 2009	9	12
Monday, February 16, 2009	25	28
Tuesday, February 17, 2009	26	40
Wednesday, February 18, 2009	59	94
Thursday, February 19, 2009	22	39
Friday, February 20, 2009	30	44
Saturday, February 21, 2009	17	21
Sunday, February 22, 2009	13	15
Monday, February 23, 2009	27	44
Tuesday, February 24, 2009	58	87
Wednesday, February 25, 2009	62	84
Thursday, February 26, 2009	51	83
Friday, February 27, 2009	58	92
Saturday, February 28, 2009	81	94
Sunday, March 1, 2009	26	42
Monday, March 2, 2009	77	121

Tuesday, March 3, 2009	63	107
Wednesday, March 4, 2009	46	74
Thursday, March 5, 2009	65	102
Friday, March 6, 2009	66	96
Saturday, March 7, 2009	21	25
Sunday, March 8, 2009	23	27
Monday, March 9, 2009	50	75
Tuesday, March 10, 2009	29	41
Wednesday, March 11, 2009	28	44
Thursday, March 12, 2009	35	49
Friday, March 13, 2009	42	69
Total Visits		2,242
Absolute Unique Visitors Overall	695	

A1 EMERGING FIELDS AND NEW CROSS-CUTTING DISCIPLINES

The Initial Science Plan of the current IODP program was produced in 2001 to provide the framework and key objectives for IODP drilling from 2003-2013. Since 2001, we have expanded ocean drilling using the JOIDES Resolution, the Chikyu, and mission specific platforms. While our drilling and scientific capabilities have evolved, it is essential to assess what new directions should be addressed and what new fields can be explored through drilling.

A1.0 Going beyond conventional science and engaging the public

How to Incorporate Emerging Fields into the New Science Plan

Posted by Christina Ravelo on 2009-03-09 at 10:57

Discussions in the Emerging Fields and New Cross-Cutting Disciplines have resulted in nearly 90 posts. These topics propose new geographic regions to study, areas/themes with a dearth of IODP data, new-to-IODP fields, and potential expanded partnerships. How should we further develop these topics at INVEST? How do we incorporate these emerging fields into the new science plan? Should they stand alone as new primary science themes? Should they be initiatives within broader themes? What are the important engineering developments, technologies, and methods needed to achieve them?

Posted by Mitch Lyle on 2009-03-12 at 07:37

Reading through the posts, there are loads of exciting new drilling that could be done over a multitude of themes. As usual the problem is to expand our potential ability to drill more in IODP than in ODP or DSDP.

We need to present the material coherently to two different communities –geoscientists and an educated public-at-large. I think we have to define how the main topics in the new science plan are needed to understand how the world works--important in itself. Then we have to define how at least a part of this drilling might affect society. Astronomers have been very good at exciting people's imaginations about the science itself--after all, what is the societal relevance of a black hole? We should be able to do the same.

Much of what we need to do is to organize thoughts that have been passed around the community for years and are expressed to some extent in the ISP. We are all excited by the large-scale processes that shape our planet and make it habitable. We want to understand how the planet exchanges material between its interior and the surface on which we live, and how these processes shape our lives. In terms of climate, we want to understand how there have been such major changes in climate when the insolation driver has remained relatively constant, at least in the Phanerozoic.

Yes, the ideas need to be grouped, but we also need flexibility. Leave room for initiative by individuals and groups.

Posted by Susanne Straub on 2009-03-13 at 12:29

For one, I would favor a few highlights that 'sell' and that keep the public/political eye on the drilling program as well as they generate the excitement to draw in new people, in particular graduate students. It would be very important to almost 'guarantee' implementation (and success) of these 'highlights' during the post-2013 phase. Certainly, climate change and geohazards should be among those highlights ('what does drilling to make our world safer, cleaner and maintains it habitability for the future generations?').

To the other, we need to make sure that we can keep going everything else that is essential.

In order to combine all of these, I think a unifying header is essential. I dislike the tri-partition of the current ISP, because it (even if eminently practical) creates artificial separation of what belongs together on the Earth, and what defines the single field of understanding the ever-ongoing 'Global Environmental Change' and its consequences to humankind.

It should also be stressed that ocean drilling is unique, as it has the ability to connect all these different processes that operate on very different timescales. Only through ODP we get the perspective from which we can link processes from the outer and inner Earth that made it habitable.

Posted by Bill Ussler on 2009-03-13 at 13:21

The Engineering Development Panel (EDP) of the IODP has identified drillstring stabilization as one of the critical engineering developments needed by the current and any future drilling program. For example, a more stable drillstring will enable collection of better quality cores, permit better performance of drillstring deployed measurement systems, and the installation of borehole observatory experiments while on station. Emerging fields and science themes will benefit from any improvements that can be accomplished with drillstring stabilization.

Technology development is one of the 'Principles of Implementation' of the ISP (page 72). The EDP has developed a Technology Roadmap for the IODP, and it is posted on the IODP website (www.iodp.org/eng-dev) along with a table of high priority technological needs. The establishment of an IODP Technology Roadmap provides a mechanism for anticipating and accommodating new technologies and responding to specific needs identified in drilling proposals moving through the SAS. The EDP TR is a 'living document'. By having a Technology Roadmap and a prioritization of technical needs it becomes easier to identify critical elements and pathways for funding, developing, and implementing 'enabling technologies' that support the science. I expect that post-2013 scientific drilling continue to require advances in engineering and technology development and the current IODP Technology Roadmap will provide a template for identifying those needs.

Summary 02/19/2009

Posted by Brandon Dugan on 2009-02-24 at 09:41

Emerging Fields and New Cross-Cutting Disciplines

Summary 02/19/2009

Some great ideas have been submitted in the first two weeks of CHART. Below is a bullet-point summary of post topics and questions. Do you agree with these? Disagree? What are they key scientific questions for the new themes? What can we do better? What are we missing? Please add your contributions to the forum.

1) Global Scientific Drilling Partnerships

How do we combine efforts with DOSECC, ICDP, DOE, ANDRILL, SHALDRILL?

Where and how can we address terrestrial to oceanic problems (e.g., "Source to Sink")?

How do we manage combined projects? How link separate funding groups/cycles?

2) Emerging Fields and Key Themes

CO₂ cycling, mitigation

Ocean acidification

Future Climate and Environmental Change/Earth's Warm Intervals

Geohazards

Ice Covered Seas and Polar Regions

Geodynamo

Ocean Lithosphere Exchange/Cycling

Surface Process and Solid Rock Geochemical Cycling Feedbacks

3) Additional Comments

How do we take advantage of/ground-truth data from Law of the Sea?

How do we package and build on curiosity-driven discoveries of DSDP/ODP/IODP to ensure they continue?

How can we make drilling more appealing/relevant to society?

What is the best way to emphasize ocean drilling is Beyond Conventional Science and that it is High Impact Science at New Frontiers?

Posted by Organizing Committee on 2009-01-12 at 14:07

What are key themes that have been embedded in individual drilling endeavors but now have the potential to thrive as stand-alone science programs with societal relevance (e.g., geohazards, life in extreme environments, extreme weather events)?

Posted by Beth Christensen on 2009-02-10 at 17:09

Ocean drilling has provided enormous benefits to the community, and steered science in directions not imagined at the outset of each program renewal. The problem is, it's a well kept secret. Packaging the program is going to be important. It must be presented as a means of broadening our knowledge in a way that modeling and ocean observations cannot. Charna made up a one pager entitled "Beyond Conventional Science". That is an excellent umbrella term for the (sometimes unintended) high-impact results that the program delivers. Identifying this potential as a stand- alone theme - Beyond Conventional Science- will serve to promote past accomplishments while asserting the role of the program in certain future discoveries.

Posted by Christina Ravelo on 2009-02-11 at 15:00

I really like Beth's comment. We struggle with how to package and sell the program. We should use 'societal relevance' as a hook for obvious reasons, and it will work well for some science objectives. But, the intrinsic value of the drilling program is that it takes us beyond all other science approaches, and high-impact payback comes from exploration at the frontiers of science. Because drilling is not new, making the case that drilling is not 'conventional science' is challenging but needs to be done. I expect highlighting some of our more technologically challenging and innovative projects would be a powerful way of demonstrating the concept of 'beyond conventional science'.

Posted by Liz Screamton on 2009-02-11 at 16:44

I appreciate the importance of societal relevance, and agree this should be an emphasis area. On the other hand, I don't think we should underestimate the discovery and scientific curiosity attraction of ocean drilling.

Posted by Sean Gulick on 2009-02-17 at 09:51

Agreed. Question is what ways can we present science goals, or perhaps which goals lend themselves to emphasizing both the excitement of exploring the Earth with ocean drilling and how doing so may help us yield insights into societally relevant problems. Thoughts all on particular goals, foci, or problems where these two intersect?

Posted by Chris Goldfinger on 2009-02-24 at 20:22

I think the leading themes will be those that can innately offer both intellectual/scientific exploration, and have societal relevance as well. Not all themes can do that, and trying to force them to always winds up looking forced. But, there is a wide range of themes that do just that. Both geohazards and climate change would be my leading choices, two themes that are both relevant today, and intellectually exciting. Neither has to be sold, the needs for both are clear. I think our challenge is more difficult than finding themes that resonate though. The more difficult challenge is to develop better integration of what drilling ships can offer with other efforts in the same fields. At present, there parallel worlds with not that much cross pollination. Having some of the leaders come to workshops is what happens, but usually that's the end of it. This is a structural problem that I think we should address in the next phase.

Posted by Stephen Pekar on 2009-02-18 at 15:02

Key theme: promoting educational and media outreach

I also support the idea of better packaging of IODP (e.g., Beth and Christina comments). While ocean drilling has written major parts of the climate and tectonic story for the last 100 million years, I think that we could have done a better job at letting the scientific community and the public know what we are doing. With that said, in the last couple of years, incredible progress has been made in developing an effective educational outreach program (e.g., School of Rocks, etc.), resulting in a firm foundation to expand and create a means of outreach in ways only dreamed of before. Having an effective educational and media outreach can improve science literacy about ocean drilling, etc., which in turn can make it easier for future funding. It can also attract the next generation of scientists to ocean drilling.

A1.1 Future climate and environmental change

Posted by Stephen Pekar on 2009-02-18 at 15:02

Key theme: Going back to our future: Earth's Past Warm Intervals

I agree with the concept of targeting time intervals that can provide societal relevance to today's changing climate. While the rates of change may not be exactly applicable to changes that are expected to occur during this century (outside of the Younger Dryas and the KT boundary), I think we still have significant contributions to make in providing exciting cutting edge research to the scientific community and to society about what a warmer Earth could look like. While climate models have been successful in reproducing the recent past climate, the models still have considerable problems reproducing past greenhouse worlds (e.g., keeping the high latitudes warm during the winters). The other way to evaluate what our planet would look like with elevated CO₂ is by looking at these past warmer periods, which we have numerous examples of throughout the Cenozoic (e.g., early Pliocene, Oligocene through middle Eocene- last time CO₂ was as high as what they predict for century, EECO, PETM, etc.). Some key themes would also include the subsequent effects of higher CO₂ and warming, such as: ocean acidification, sea level rise, Antarctic ice volume, etc.

Posted by Richard Norris on 2009-02-04 at 21:17

We should use ocean drilling to explicitly target records relevant to "Future Earth" dynamics where the goal is to assess the likely response of the Earth system to future global change. This theme includes dynamics related to topics like ocean acidification, tipping point behavior in the climate system, and the dynamics of past warm climate states. While some of these themes have been addressed on long timescales in "Extreme events" drilling, for maximum relevance to dynamics that are likely to play out over the next several centuries to 100 kyr, we should target past records of extreme climate states where it is possible to resolve aspects of these systems at resolutions relevant to the particular dynamics under study. In general, targeted records are likely to be areas where it may be possible to obtain records with temporal resolution on the century-scale or less.

Posted by Tim McHargue on 2009-02-18 at 13:56

I support this idea. Some amount of climate, ocean change is inevitable so a description of "Near Future Earth" with defined uncertainties and probabilities will provide policy makers and planners with much needed information.

Posted by Anthony Fiorillo on 2009-03-05 at 16:39

Whereas I support this notion, I would suggest that we think in terms of deeper geologic time when looking at warm climate states. The Chukchi and Bering seas for example contain great potential for understanding Cretaceous polar ecosystems.

Posted by Richard Norris on 2009-03-05 at 17:17

Sure, I agree that "deep time" drilling is the way to go, since this is where our best record of analogs to the future greenhouse will come from. After all, we are probably going to have a greenhouse forcing similar to the Eocene in the next century, so it would be nice

to have a good idea how dynamics played out under those kinds of conditions. I am suggesting that what we need from deep time are records that have high enough temporal resolution to capture dynamics on a time scale relevant to understanding near future climate and ecological dynamics. It is all well and good to drill more pelagic records of the PETM or Cretaceous OAEs, (and I'd love it if we did more of this anyway). But the low sedimentation rates typical of the open ocean record hinder our ability to resolve short-term rates and dynamics and limit the relevance of such records to understanding what might be missing from climate models developed to replicate present-day or Pleistocene conditions. I think we need to target, in deep time, records from fossil sediment drifts, marginal basins, and continental margins where the sed rates are either pretty high or where there may be laminated records that span past globally warm events.

Posted by Anthony Fiorillo on 2009-03-06 at 10:49

Sure, I can appreciate that. But it seems that there is a wealth of onshore data from high-latitude Cretaceous sections that could be combined with appropriate ocean records obtained to generate finer scale climate models for this warm period.

Posted by Stephen Pekar on 2009-03-13 at 16:42

PAST WARM INTERVALS: Looking back at our future I strongly support the concept of targeting time intervals that can provide societal relevance to today's changing climate. While the rates of change may not be exactly applicable to changes that are expected to occur during this century (outside of the Younger Dryas and the KT boundary), I think we still have significant contributions to make in providing exciting cutting edge research to the scientific community and to society about what a warmer Earth could look like. While climate models have been successful in reproducing the recent past climate, the models still have considerable problems reproducing past greenhouse worlds (e.g., keeping the high latitudes warm during the winters). The other way to evaluate what our planet would look like with elevated CO₂ is by looking at these past warmer periods, which we have numerous examples of throughout the Cenozoic (e.g., possibly the early Pliocene, Oligocene through middle Eocene, EECO, PETM, etc.).

I would like to suggest a concerted effort be developed looking at the middle Eocene to Oligocene (the last time that CO₂ was definitely similar to what they predict for this century). While CO₂ was decreasing during this interval (as opposed to increasing like today), there are a number of aspects that make this interval extremely relevant to today. For example a key societal relevance to today is how did Mother Nature pull out CO₂ of the atmosphere to produce the cooling and how it affected the environment and climate.

In fact, I would like to propose submitting a request for a workshop along the lines of Middle Eocene to Oligocene (48-23 Ma): Looking Back to our Future- The last time that CO₂ was as high as what is predicted for the end of this century.

This workshop would happen after the INVEST & ACE Workshops in September so we can continue developing the ideas that come out of these workshops.

The main goal for the workshop would be to identify site locations to recover cores that

can provide us with the requisite data sets for examining critical aspects of the climate/ocean systems.

The workshop could include the following topics.

- Investigate what caused the higher CO₂ and how was CO₂ lowered. This could include: Biological oceanic pump, tectonics, terrestrial/ vegetation changes, chemical weathering, volcanic changes, etc.
- How did higher CO₂ affect the environment such as ocean acidification?
- How did higher CO₂ affect the fauna and flora and the interactions between the biota and CO₂?
- Constrain Antarctic ice volume / sea level changes. Also, identify locations for collecting ice volume / sea level records (e.g., deep sea proxy data, shallow water continental margins, proximal evidence)
- Investigate possible Transects (e.g., Antarctica, Arctic Ocean, Mediterranean Sea)
- The workshop can also take stock of the some of the climate proxies being used and being developed.
- Investigate causal mechanisms for climate change (CO₂, Gateway opening, continental configurations, etc.)
- What types of data do we need to improve our understanding of climate and CO₂ changes and the causal mechanisms?
- Target the EO boundary for investigating the climate response to changes in forcing mechanisms, tipping points, climate dynamics.

I would welcome members of the community that are interested in organizing such a workshop to contact me (Stephen.pekar@qc.cuny.edu).

Posted by Susanne Straub on 2009-03-05 at 18:01

An important aspect of assessing driving forces of past environmental changes (that are the key to the future) is getting ALL the factors together that may drive those changes. The Earth's cycles work at very different timescales which means we need a long enough record to piece this together. Longer time scales concerns among other the influence of volcanism - whether it be a major driver, or just add enough to reach a critical tipping point at a given point of time.

A1.2 CO₂ cycling and mitigation

Posted by Mitch Lyle on 2009-02-04 at 17:30

One possible emerging field is the CO₂ mitigation problem, or to study how carbon might be stored naturally or via geo-engineering. In the case of natural burial, what processes enhance the level of carbon burial and removal for geologic periods of time? In the case of a geo-engineered solution, what types of geological formation are best suited to store carbon, where are they located, and how common are they?

Posted by Dave Goldberg on 2009-02-20 at 14:46

Picking up on these questions, I believe that investigating the scientific and technical potential of deep-sea geologic strata for long-term storage of CO₂ could be a key contribution of scientific ocean drilling in the future. A field experiment – drilling, injection and monitoring – in the ocean floor is needed in order to do this. Field-scale CO₂ trapping mechanisms, particularly those active in the deep ocean, cannot be sufficiently reproduced via laboratory or computer modeling. Ocean drilling offers an ideal platform for piloting such an endeavor in the future.

Understanding climate change, ocean processes, and sub-seafloor properties have historically been part of the drilling program. Researching the potential for sub-seafloor storage to mitigate CO₂ build-up in the atmosphere seems to be a natural, if not critical, extension of ocean drilling science. Cross-disciplinary study is inherently involved, including hydrogeology, geochemistry, structural and petroleum geology, microbiology, and technology development, and of course, the societal benefits in this new direction are broad and clear.

Posted by Greg Ravizza on 2009-02-24 at 15:38

I have gotten emails from various "interests groups" encouraging participation in this web discussion - I am not NOT casting my vote for any of the traditional disciplinary communities under the IODP umbrella. Scientific drilling has made stellar contributions to making a case that climate change is real and that at 500 ppmv CO₂ all major ice sheets will melt (though we do not know how fast - only that melting seems to be accelerating). Now it is time for the drilling community to work toward solving this problem. It is the natural progression of work begun decades ago.

Therefore, I would like to echo my strong support for Dave Goldberg's argument that scientific ocean drilling can make important contributions to assessing the scientific and technical viability of deep sea sediment carbon sequestration. This should be an important future direction for the program.

Moreover, the science community needs to be aware that this issue touches upon political and legal issues. Plans for ocean carbon sequestration will almost certainly be perceived by some as "ocean carbon dumping" - an irresponsible attempt to save "business as usual" by accelerating degradation of the marine environment. Care must be taken to avoid becoming (or being perceived as) advocates of ocean CO₂ storage. Rather we must bring to bear the objective eyes of science to determine if this path makes sense, and we need to start doing this quickly.

DSDP/ODP/IODP is arguably the most successful international science collaboration in history. By making carbon sequestration a key future research topic, our community can speak as an international (& not-for-profit) authority on the prospects and likely environmental consequences of sub-seabed CO₂ storage. The program has the credibility to articulate this issue to the public in a way that national entities or industries who stand to profit from carbon cap and trade schemes cannot.

Posted by Dave Goldberg on 2009-03-02 at 11:03

The last point Greg makes is very important. The role of academic research in geological CO₂ sequestration to date has been along the fringes of large commercial and governmental projects. The most significant role for academia in the future (as he suggests) may be as independent auditors of CO₂ sequestration concepts and storage verification.

All geological options involve drilling. Ocean drilling engages a great number of scientists, many of whom are also expert in climate science, and thus one of the only groups who can offer both scientific and drilling expertise with unbiased credibility. This is extremely difficult for commercial and governmental groups to achieve alone, but remains a critical and missing piece in public acceptance of carbon sequestration. Also, in my view, perhaps the most significant step that scientific ocean drilling can take toward mitigating human impacts on climate change.

Posted by Rick Colwell on 2009-03-13 at 19:31

By investigating possible sequestration sites for CO₂ we can demonstrate the unique contributions that IODP research can make towards answering questions that society has regarding this problem. This would be a clear example of the immediate value of IODP research to policy makers and the public.

Posted by Kevin Johnson on 2009-02-24 at 18:29

I echo Dave Goldberg's comments wholeheartedly. I am very familiar with Dave and colleague's work on CO₂ sequestration in submarine basalts via mineral carbonation reactions, and, along with other potential CO₂ reservoirs above sea level (e.g., flood basalts, Hawaiian olivine basalts, Iceland, ultramafic massifs) these boreholes on the seafloor should be vigorously investigated. IODP and prior drilling programs have collected valuable downhole logging data that are required for evaluating formation physical properties; improvement of downhole logging devices should be high on the list in future priorities of IODP. As with my comments regarding subduction zone directions, I strongly believe that the societal importance of IODP programs must be strong. The scientific aspects certainly need not suffer, and in this case, as well as the active margins work, much exciting science will be accomplished at the same time as critical societal needs are addressed.

Posted by Greg Moore on 2009-02-28 at 12:38

I would also like to put in my strong support for Dave's suggestion -- this is indeed a worthwhile, NEW field that is perfect for IODP.

Posted by Andy Fisher on 2009-03-07 at 13:28

There is another important aspect to CO₂ sequestration related to IODP: the seafloor may be one of the best places to resolve fundamental hydrogeologic understanding of heterogeneous fractured and porous media. Only within the upper OC can one find an aquifer that is continuous, within a single facies, for thousands of kilometers, isolated by relatively impermeable sediments, across which very small physical and chemical perturbations can be resolved over days to years. Many experiments can be done on and within the seafloor that can not be done on land. So, in addition to testing the viability of

putting CO₂ into the OC (not sure how realistic this really is), we can do profoundly important science that will help with CO₂ sequestration efforts in general.

Posted by Marta Torres on 2009-02-28 at 16:18

CO₂ sequestration is no doubt an important issue and as Dave and Gregg have stated, IODP is perfectly placed to address this hot topic in an international arena. I would like to expand the focus from basalts to other areas/strategies so that a full comprehensive plan can be developed to fully investigate all aspects of carbon storage using natural and geo-engineering strategies. A possibility worth exploring is the use of CO₂-to-CH₄ exchange in gas hydrate bearing sequences. Such technology has been under study/development for a while and it behooves us to include this idea in the list of emerging fields that should be explored in the context of CO₂ sequestration. Several nations (e.g. India, China, Korea) are highly invested ocean drilling to map and quantify the gas hydrate accumulations in their marginal seas. We can take advantage of this interest to foster research on the potential of these deposits in carbon sequestration strategies

Posted by Dave Goldberg on 2009-03-02 at 10:53

Certainly the marine sediment column offers a number of potential CO₂ sequestration targets, including CO₂-to-CH₄ exchange in some locations, while providing a physical trap for injected CO₂ in others. Scientific ocean drilling is unique in its position to address all of these aspects and research topics related to sub-ocean CO₂ sequestration.

Posted by Peter Kelemen on 2009-03-03 at 00:02

I'd like to add my voice here to those advocating a role for IODP in investigating options for CO₂ capture and storage. Logical substrates include basalt (as has been discussed here already by Dave Goldberg and others), peridotite (see following paragraph), and high porosity sediments beneath an appropriate cap rock as another person has noted already. I strongly urge that the community not try to "pick winners"; as pointed out by Socolow & Pacala in *Scientific American* (2006), no one strategy alone (CCS, alternative energy, conservation) is going to halt growth in human CO₂ output, let alone reduce it. Also, it is difficult to foresee the political and economic forcings that might favor one technique over another in 10 or 20 years. Instead, we have to pursue lots of options in parallel, and hope that most of them work in parallel for decades to come. I believe this same philosophy should apply to the various CCS options on the table. All have their pros and cons, relative to each other and to other CO₂ mitigation strategies. Let's pursue a variety of options, and assess which are best further on down the road.

My particular entry point into this field is the study of mineral carbonation in peridotite, as a relatively rapid route to solid storage. There is some pretty specific IODP research one would consider in this context, involving drilling in shallow seafloor through sediment into peridotite, for example offshore of Oman, New Caledonia, and/or Papua New Guinea. Interested readers can go to:

<http://www.ldeo.columbia.edu/files/uploaded/file/2008%20Kelemen%20&%20Matter%20PNAS+.pdf>

to see what we are thinking about. With regard to seafloor science, Figure 8 in that paper is particularly relevant. What's needed is characterization of present day alteration, fluid flow, reaction rates, porosity, and permeability in 500 meter to km-scale holes, and then hole-to-hole experiments involving hydrofracture and - perhaps - pre-heating a rock volume at depth and then - perhaps - injecting pure CO₂. Each of these steps would be undertaken in order to "jump start" hydrothermal systems like Lost City, and - if necessary - accelerate them. The idea is to enhance the natural peridotite carbonation system, doing as little as possible while consuming as much CO₂ as possible. There would be a lot of synergy with traditional ODP goals, in the sense that lots of people are interested in the spatial scale of chemical variability in peridotite, the tectonic processes by which mantle peridotite is exposed on shallow seafloor, and the deformation mechanisms which these rocks record. Site survey requirements, particularly assessment of depth from seafloor to peridotite, and nature of overlying sediment, are obvious and do-able. In some cases, oil companies and others may already have obtained such data. Note that, while JR style drilling might be very desirable in some cases, alternative platforms including small islands and even jetties might be better, and cheaper, in other cases.

Posted by Ken Miller on 2009-03-03 at 14:37

Interesting that I downloaded Peter's paper today on peridotite, but it is part of a larger question on geological sequestration.

Greg Mountain and I are involved in DOE phase 1 characterization of the New Jersey onshore coastal plain, continental shelf, and continental slope. For several critical reasons articulated by Dan Schrag, slope sequestration appears to present the best location possible. Moving forward with slope sequestration will require drilling and the JR provides the best tool for this.

Posted by Peter Kelemen on 2009-03-03 at 14:46

To reiterate from my earlier post, in considering CO₂ mitigation methods, I think it is crucial not to try to pick winners at this stage, but rather to carry many options forward in parallel. Slope sequestration has its pros and cons. Dan Schrag has thought a lot about this topic, but he also has a vested interest in some options and not others. Also, none of us can see into the future, not even Dan. The way that government and international regulations are written, with regard to property, liability, verification and monitoring, and safety, will all have an impact on which CO₂ capture and storage methods are ultimately used ... if any.

Posted by Ken Miller on 2009-03-03 at 15:04

The reality check and the focus on the slope is that the JR about is not useful in shallow water settings, so slope and deeper targets are all she can get. MSP's with high attendant cost are needed for shallower water targets than the slope.

Posted by Peter Kelemen on 2009-03-03 at 15:19

Wireline diamond drilling from a small island would be hundreds of times cheaper than using the JR anywhere, but this is getting way too specific and I don't think the discussion is going anywhere.

Posted by Travis McLing on 2009-03-05 at 18:53

I have enjoyed reading the previous post, and think there are some good points made. Having participated in the US sequestration program for the last six or seven years, I observed that most of the resources (\$\$) have been directed to the geologic formations that are already the most characterized (large sedimentary basins). If large-scale geologic carbon sequestration is ever going to have an impact on atmospheric carbon dioxide levels, every type of suitable geology, including ocean sediments is going to have to be considered. After all, geologic sequestration must be available to every community, not just those that are located proximal to a large saline aquifer. If as a society we take mafic rocks, shales, and ocean sediments off the table, most of the world population will not have ready access to geologic sequestration as a mitigation option. The decision whether or not to sequester carbon in these types of rocks should be based on solid science and engineering, not politics ...my two-cents.

Posted by Rick Murray on 2009-03-05 at 19:38

Regarding this (very) broad topic, it does seem clear that IODP can play an important role for several reasons, including (a) we have available to us (at least in theory) a multitude of platform types, therefore we have flexibility in where we study (onshore, offshore, deep offshore, or whatever), (b) we have historical knowledge and datasets of direct relevance, and (c) a large community of experts from a variety of subfields (hard rock, soft rock, no rock) upon which we can draw. Clearly, for reasons articulated by others named in previous posts, marine sediments and hard rocks are viable.

It seems to me, largely, that the issue of subseafloor sequestration is now largely (but not solely) an engineering problem, not necessarily a scientific one. Different groups have focused on showing how sediments and/or hard rocks could be utilized, and both show promise...and both present different challenges. IODP engineering, in collaboration with industry, could make a significant step forward, if the community is willing to make the investment in time and (engineering) effort.

A1.3 Geohazards

Posted by SeanGulick on 2009-02-04 at 22:34

I would like to suggest in the new program it will be vital our goals are also viewed as societally relevant. To that end I concur with Dick Norris' post about extreme climate change. I would also suggest that geohazards is an umbrella under which a number of current programs could fall (seismogenic zone) as well as allow an avenue into the program for other goals such as landslides, impact events, volcanic hazards, tsunamigenesis, etc.

Posted by Cecilia McHugh on 2009-02-19 at 08:48

In view of the recent developments in Indonesia, geohazards is both societal relevant and timely. I agree with Sean Gulick that the natural hazards umbrella can encompass many problems from understanding earthquake recurrence intervals in seismogenic zones to volcanic eruptions such as Santorini, and landslides as in Papua New Guinea. Areas that have been particularly unexplored by deep sea drilling are continental transform

boundaries. Yet, these basins hold clues as to the evolution of the plate boundary, paleoclimate, and natural hazards. Given the heavily populated regions they cross and the high risk they pose, exploring these systems can provide critical information for risk assessment in addition to tectonics and climate.

Posted by Mark Legg on 2009-03-02 at 19:15

I concur with Cecilia regarding the importance of more comprehensive investigations of continental transform boundaries, and as she noted, the basins within these zones hold important clues to plate boundary evolution. Investigations of these plate boundary systems are obviously cross-cutting as we need high-resolution stratigraphy to establish rates of deformation--and this stratigraphy is equally relevant to climate issues. Major turbidite systems within these basins along continental margins may record important climatic events shown by changing character of source sediments, as well as showing changes in depositional systems due to tectonic activity. For example, along major strike-slip faults in these transform margins, major restraining and releasing bends create local basins and uplifts that provide detailed record of the tectonic evolution. We have much success with seismic reflection profiling, and advances in technology have increased our ability to obtain higher resolution and deeper image penetration around the world. Yet, stratigraphic control to get the dates and rates necessary to quantify the deformation processes are lacking except in those few chosen areas that have enjoyed ODP projects. When the timing of deformation can be precisely defined, we can start looking at some of the really big questions regarding rheology and the larger scale lithospheric structure and deformation processes. Furthermore, in some key continental transform margins or other evolving plate margins, entire subduction zones may be exhumed and drilling provides one of the best ways to get at the actual composition of the deep crust exposed in these areas. Submersible observations could also be used, and coordinating these types of programs provide very exciting research opportunities that can easily gain public attention through media such as Discovery Channel, National Geographic and so forth. Of course the geohazards associated with these plate boundary systems spans much of the gamut from earthquakes, tsunamis and volcanic activity to landslides and methane hydrate accumulations that may be susceptible to rapid environmental changes due to active faulting or other disruption of the seafloor. So, with the identification of some key locations, it may be possible to have a very productive research program that addresses many important geohazards issues in relatively close geographic proximity. Of course, we could find such locations in areas where coastal population exposure is substantial and thereby demonstrate clear societal relevance. Again, I emphasize that the processes investigated by such programs have important relevance beyond geohazards--such as lithospheric dynamics and plate boundary evolution.

Posted by Gail Christeson on 2009-03-03 at 12:01

I agree that geohazards could be an important component of a new ISP. Impact structures are one type of geohazard that can in many places be better studied in a marine environment because of better preservation. Large impact events can drastically effect short-term and longer-term climate and lead to extinction events, such as at the end of the Cretaceous. However there are still fundamental questions that must be addressed about

crater formation before progress can be made modeling the environmental effects of large impacts. I argue that IODP could play a critical role in these investigations.

Posted by David Scholl on 2009-03-03 at 13:19

For sometime the drilling community has recognized that significant advancements in understanding how the Earth's works environmentally can be extracted from the paleoceanographic and -climatic record stored in its marine stratigraphic sequences. Although, I don't believe an explicit objective, paleoceanographic and -climatic knowledge now provides a crucially needed base of understanding of past changes to assess the likely impact and consequences of an oncoming, rapidly warming Earth. In effect, these studies created a component of geo-knowledge needed to provide the public, and modelers, with a perspective about what globally might or could happen in the immediate future.

With respect to providing the knowledge needed to assess the threats of geohazards that can rapidly and catastrophically overwhelm the coastal populous, IODP is uniquely positioned to assess those posed by the global circuit of subduction zones. Subduction zones launch three types of extremely dangerous geohazards --high-magnitude earthquakes, matching tsunamis, and vigorous to violent arc volcanism and edifice collapse. Volcanic ejecta also contributes to, or modulates, global climatic change. The magnitude of the peril and threat this troika presents is not exceed by any others. IODP can inimitably recover by offshore drilling and logging the long-term record of past events and their magnitude and consequences.

Following up on the comments and recommendation already posted by others to this CHART workshop, I urge that a directly stated goal of IODP is to conduct drilling to address the science of subduction zone geohazards, including matters of cyclicity and links to coastal faulting and eruptive activity. This scientific knowledge is to be converted to risk assessments that would be widely recognized by the public as relevant to their safety, wellbeing, and preparedness. As all of us recognize, this awareness arose from the 2004 boxing day disaster of the Banda Aceh earthquake and tsunami, the potential for which was little noted by the earth science community.

What the science community gains intellectually from addressing risk assessments of the geo-troika, would be substantial advancement in understanding how subduction zones work to build ocean margins and the older geologic record. More generally, huge gains would be made in the science of comparative subductology--to use the old expression of Seya Uyeda--and why subduction zones behave differently and some so dangerously.

Posted by Susanne Straub on 2009-03-03 at 16:26

I agree with Dave Scholl's comment - if possible, future ocean drilling should be continued with clear and designated links between science and societal impact. This would enhance the purpose of science without taking off the thrill of discovery.

Posted by Peter Flemings on 2009-03-05 at 06:53

GeoHazards: Slope Stability, mass transport, etc: I have read through the general discussion under geohazards but I have not seen a clear statement advocating a process-

based exploration of submarine landslide initiation and evolution. I do so here. Scientific drilling can be used to understand the initiation, the timing, and the magnitude of submarine landslides. Obviously, their development has implications for tsunamis, and impacting sub sea structures. More broadly, submarine mass transport complexes are recognized to make up a large fraction of the sedimentary record and little work has gone into understanding their evolution. It is important to recognize that historically the geotechnical community has not been able to play a significant role in this because the depth of geotechnical penetrations is small. Broad questions include: 1) What initiates submarine landslides (hydrate dissociation, focussed flow, seismogenesis); what causes an accelerating vs. decelerating flow? What is the process in the flow and what is the composition/fabric of the material when arrested. The IODP has some, but not all, capabilities of addressing these problems. A key need is the development of better tools for measuring in-situ stress, strength, and pressure. Better coring tools must also be developed to produce less deformed core for subsequent laboratory analysis.

Posted by Brandon Dugan on 2009-03-05 at 10:56

I agree that a process-based study of slope stability is an excellent target for the future of IODP. The distribution of failures is well document in many locations, but their initiation, movement, and arresting is largely unconstrained (or only constrained qualitatively). A multi-pronged approach must involve characterizing the extent of the flow and the surrounding intact sediments (good site survey). Then we must utilize existing IODP and non-IODP technologies to study the in situ pore pressure and strength of the failed and un-failed sediments. Additionally having excellent age control is required for nailing the timing of the failure which will help modeling efforts as well as linking it to an initiation process (e.g. pressure genesis, EQ of specific size/location). The study adjacent, unfailed sediments would provide additional timing controls through stratigraphic correlation, but also would provide a snapshot of what the sediments looked like before they failed. On land, it has been shown that the bulk physical properties of the sediments prior to failure control the type of failure (e.g. Iverson et al 2000). What are the important material properties and conditions for controlling type and magnitude of submarine slope failure? In situ measurements with existing tools will help lead the charge, but shore-based geotechnical studies are also required to help us understand the dynamics.

Posted by Derek Sawyer on 2009-03-05 at 18:42

I agree with Flemings and Dugan...and I think it makes most sense to target a place to drill where: Failures are recent (not buried) and clearly mappable on the seafloor. These areas would be the best targets to address questions of initiation, phys props, and movement. A lot of good datasets through landslides exist but many are buried deposits. It is therefore difficult without high-resolution seismic reflection data to map out the exact size, transport direction, etc. It would also make it easier to establish physical properties of the surrounding intact sediments and define state of stress and pressure. This would also make geotechnical measurements easier and perhaps decrease the intensity of core deformation (i.e. we can get higher-quality cores) (just my guess).

Posted by Homa Lee on 2009-03-05 at 20:37

I think Dugan, Flemings and Sawyer have listed the basic ingredients for using IODP to advance submarine landslide studies. In addition to these ingredients I think it is

important that we look at failures that have occurred in recent human history and that produced effects such as tsunamis or coastline loss that we actually observed. Then we really know the conditions that caused the failure (e.g. a particular earthquake). Otherwise, we ultimately have to guess at the cause. There actually are a few places in the world where we can do this (Papua New Guinea, Alaskan fjords where failures occurred in 1964 [particularly Port Valdez], Grand Banks, Nice). It would be great to have borings behind the headwall, within the scar and in the depositional body and below. Where there have been repeated failures at the same site, it would be good to penetrate down through buried, older failure deposits to date them and determine their physical properties. Measuring regional pore pressure distributions is critical. A question we need to ask is why do certain environments produce slope failures that occur over and over again in geologic time whereas others never fail. What is the impact of climate, sea level and glacial stage?

Another topic is investigating the degree of mobility of sediment failure. How is strength lost during failure and how do the resulting mass physical properties determine whether a slump, debris flow or turbidity current results and how far do the mass movements move away from their source? Careful, complete case histories with deep sampling and testing of a variety of well-documented failures can take us a long way.

Posted by William Sager on 2009-03-13 at 08:50

At the risk of just saying "yeah, what they said," I agree with Flemings, Dugan, Sawyer, et al on this issue. It seems to me that this field is one that IODP is uniquely capable of making a major contribution. There seems to be a mentality around the passive margins that "tsunamis don't happen here" but clearly they do, just not very often. An obvious problem is our very poor knowledge of the processes of slope instabilities and mass sediment transport. It seems to me that this is a science area in which the IODP can make contributions that will translate very well into societal relevance and the morning newspaper.

Posted by Joel Johnson on 2009-03-10 at 22:38

In terms of understanding possible initiation mechanisms for submarine landslides/slope failures: The geologic record of slope failure is often produced via several different initiation mechanisms, which can be difficult to reconcile from the sedimentary record. Over long timescales, however, (obtainable only from IODP drilling) observable and well constrained (dated) changes in the frequency of slope failures may reveal the dominant initiation mechanisms operable along a particular margin because many initiation mechanisms operate at different frequencies (e.g. on active margins seismic activity is often more frequent than sedimentation rate changes due to sea-level change). Future drilling along margins with well constrained paleoseismic records and/or well documented changes in sedimentation rates related to sea level fluctuations could enable us to remove these signals from a long term slope failure record and to then look at the "residual" slope failure frequency as a proxy for other possible initiation mechanisms. If a drill site like this was located within the gas hydrate stability zone, we might learn something about the frequency of gas hydrate related slope failures. If outside the gas hydrate stability zone, we would be able to eliminate gas hydrates as a possible initiation mechanism. A well designed site selection process focused on obtaining long timescale

records of slope failure could really give us the best possibility for determining dominant initiation mechanisms.

Posted by Mark Legg on 2009-03-13 at 16:24

About 4 years ago, divers observed a marked change in the seafloor near the West End of Santa Catalina Island. Their description of what had formerly been a relatively smooth and gently sloping seabed to become a sharp bowl-shaped deep area sounds like a submarine landslide had occurred in the week between dives at that location. Unfortunately, when I first heard of this observation/event, I was unable to get any interest in pursuing the matter, such as obtaining side-scan sonar or high-resolution bathymetry to investigate the scale and morphology of the possible landslide. This event occurred in late Spring/early Summer of a relatively wet winter in southern California, so it is possible that the trigger was spring sapping from ground water seepage offshore. Although years have passed since the event, and it is unlikely that we could measure the physical or chemical processes responsible for this possible submarine landslide occurrence, it should be possible to at least map the feature and determine if it is a landslide. With occurrence of such events relatively rare, or at least our knowledge of such occurrence when it happens being extremely rare, it is unfortunate to miss these opportunities to gain critical knowledge about the cause of such events. Rapid response to such events is beyond the scope of IODP, but it may be very important to planning future IODP missions to examine these phenomena. It would be advantageous to set up a plan for rapid response to such events to capture the critical time-sensitive information. Certainly in areas such as southern California, where there is a significant scientific community with equipment and boats readily available, such events should not be allowed to pass without further investigation.

Posted by Liviu Giosan on 2009-03-05 at 14:17

I agree that tackling geohazards is an important topic that IODP should address. I would venture to say that geohazards on continental margins are but just a part of a larger need to understand the high-resolution architecture of these understudied regions. I cross-post a message here that I believe is relevant:

A sustained effort should be dedicated in my opinion to supporting drilling of deep sea fans and river/glacier-dominated high sedimentation rate upper continental margins and shelves which have been many times avoided in the past due to logistical complications. Besides the topics already discussed I would raise the following:

1. Linkages and direct comparison/contrast between terrestrial paleoclimate (pollen, organic and inorganic geochemical proxies) with regional paleoceanography using records in the same cores in high resolution continental margin settings.
2. Moving beyond major sea level cycles toward integrating the millennial climate cyclicity paradigm into sea level studies. Millennial cyclicity in climate is likely reflected in high-resolution sea level variability affecting the architecture, sediment and organic carbon storage of continental margins.
3. Providing understanding on the developing field of seismic or paleogeomorphology developed on continental margins and the causal links between paleogeomorphology and climate, tectonics, sea level, etc.

Posted by Floyd McCoy on 2009-03-07 at 23:18

Agree with the science mentioned by all respondents. There can be little doubt that this science is critical to civilization and its well-being. And that is the critical point - given the impending budgetary tsunami that is pending, we really must face the fact that pure science will not sell the program. Of course, for example, studies of the seismogenic zone are important to scientific advancement. We buy that. Legislators will not. So that program, and all programs, need to tie the science to the protection, if not preservation, of society from geohazards. It is good to see the subject here, and discussed (it was not here a few weeks ago). Geohazards should be the central theme to as many of the scientific programs as possible, no matter how slim the thread might seem. We have a tough sell ahead - let's relate it first to culture then to science. It can be done, and it must be done, if scientific drilling is to have a funded future.

A1.4 Ice covered seas and polar regions

Posted by Stephen Pekar on 2009-02-18 at 15:02

Key theme: "Going where no drill bit has gone before...." Another area that IODP can provide leadership is targeting areas of the world that have been under sampled by cores. This would include the coastal areas of Antarctica and the Arctic Ocean. I strongly support Frank's ideas about making these areas key themes of a post-2013 IODP program. For example, except for upper Eocene strata obtained from CIROS-1 in the western Ross Sea, no sediments have been recovered from cores from East Antarctica that were deposited during the greenhouse worlds. The idea of going "where no drill bit has gone before", is both important for moving the science forward as well as raising public awareness and promoting science literacy.

Posted by Frank Rack on 2009-02-08 at 23:22

Understanding ice-covered seas and surrounding continental margins in the polar regions of the northern and southern hemispheres should be one of the key themes of a post-2013 IODP program. The Arctic Drilling Workshop in November laid out a broad vision in this area and encouraged participants to develop pre-proposals and proposals that would address a range of science priorities across the Arctic. The same case should be made for the margins of Antarctic and the Southern Ocean to support a integrated strategy focused on the impacts of the cryosphere on Earth systems. A commitment to understanding the Arctic Ocean and surrounding seas through scientific drilling, especially when combined with a successful Wilkes Land expedition and a similar commitment around Antarctica, could set the stage for ongoing achievements beyond 2013. In anticipation of this focus, IODP should consider identifying an achievable gateway expedition in the Arctic - perhaps a return to northern Yermak Plateau, which could be achievable using the JOIDES Resolution, or an MSP prior to the end of this phase of drilling in 2013, thereby capturing some of the low-hanging fruit that would continue to raise public awareness of IODP and its impacts during the critical period leading to program renewal. These endeavors could also promote synergies with other scientific drilling programs/projects in the Arctic and Antarctic, such as ICDP drilling of Lake El'gygytgyn in northeastern Russia (2008-2009) and ANDRILL expeditions in McMurdo Sound, Antarctica (2006-

2008) and beyond. SCAR has begun to explore the concept of an integrated Antarctic Drilling Program and drilling projects have been proposed from the margins to the interior of Antarctica by various investigators. Additionally, seismic surveys supporting UN Law of the Sea claims have been initiated by numerous countries in both the Arctic and the Antarctic, but drilling to better understand and validate the seismic interpretations is sorely lacking due to logistical challenges - this is an opportunity that a revived IODP could address in the future and could lead to expanded partnerships with other drilling programs (e.g., ICDP and ANDRILL for sure, but potentially others) to achieve transects that cross these margins from the deep sea to the interior sub-glacial or continental environments. This key theme would also provide potentially expanded opportunities and funding strategies for specific projects that might promote new synergies between ocean science and polar programs in all of the IODP member countries.

Posted by Mitch Lyle on 2009-02-11 at 11:39

It is also worth noting that the Pacific side of the Arctic is now reliably ice free in late summer.

Posted by Mitch Lyle on 2009-02-27 at 15:27

Another important target is the Beaufort shelf and slope. If there are reservoirs of methane hydrates to destabilize in the modern Arctic, they lie on the warming shallow polar shelves.

Posted by Marta Torres on 2009-02-28 at 16:04

I fully agree with Frank that the Arctic Ocean is one of the unexplored frontiers where a concerted effort can generate high impact science that addresses a wide range of disciplines from tectonics to paleoceanography. An area of particular interest here would be the study of permafrost-associated gas hydrate accumulations. The Arctic shelf is currently undergoing a dramatic thermal change caused by the continuing warming effects associated with the Holocene sea level rise. During this transgression comparatively warmer Arctic Ocean waters have flooded the relatively cold Arctic permafrost areas. The resulting thermal pulse is still propagating down into the ground and should be decomposing both subsurface permafrost and gas hydrates on the Arctic shelf. Methane from decomposing gas hydrates is being released from both the top and the base of the diminishing gas-hydrate stability zone. Because the Arctic may represent the largest and most susceptible gas hydrate reservoir in the planet, a robust assessment of methane contributions from these areas is critical to constrain large-scale models of climate change. As Frank pointed out, these issues, and the challenges associated with mounting an expedition to this area (platforms, site surveys, etc.) were addressed at the Arctic Drilling Workshop, and several very feasible programs were outlined. I fully support the inclusion of Arctic drilling as a main theme in the upcoming program. If we want high impact science, this is an excellent drilling target

Posted by Bernard Coakley on 2009-03-02 at 14:43

If you peruse the last IODP science plan, the Arctic is mentioned frequently as a place to go for a variety of particular problems. Yet the program has only been there once. If we really want to go where no drill bit has gone before and answer some questions framed by drilling at lower latitudes, the Arctic Ocean is the one region that leaps out at you. We do

not understand the history of the ocean basin. We also could learn an awful lot about climate history both in the Cenozoic and Mesozoic.

Posted by Xixi Zhao on 2009-03-03 at 04:08

The largely unexplored high Arctic region has continued to challenge earth scientists. In spite of the critical role of the Arctic region in climate evolution, its geologic history and tectonic structures are still poorly known. In addition, new drilling results from the Arctic will be germane to a broad range of scientists with interests ranging from the growth of the lithosphere to deep-mantle dynamics and superchron to functioning of the geodynamo. Numerical simulations have suggested that the dynamics of the geodynamo differ inside and outside the tangent cylinder, which is a imaginary cylinder parallel to Earth's spin axis and tangent to the solid inner core. The Arctic region provides an ideal opportunity to obtain paleomagnetic directional and intensity data at sites within the tangent cylinder and thus allow investigating the core processes.

Posted by Alberto Malinverno on 2009-03-03 at 16:22

As noted by Mitch, Marta, and others, the Arctic shelf makes for a key drilling target for methane emissions and gas hydrate dissociation. This is where permafrost formed during the last glacial is degrading after the Holocene transgression and is one of (if not the most) sensitive regions to anthropogenic climate change. Methane emissions from this region could be a significant positive feedback to warming. Another target of interest are gas hydrates on the Beaufort Sea continental slope, where there is evidence of massive submarine sliding. If rapid temperature changes are expected at the seafloor of the continental slope, this area can also be relevant for climate change.

Posted by Sarah Fowell on 2009-03-04 at 21:30

I strongly concur with Frank's statement that understanding polar regions should be a key theme of the post-2013 IODP program. In addition to learning about the Mesozoic and Cenozoic paleoceanography of the Arctic, basins north and south of the Bering Strait provide an unprecedented opportunity to address long-standing questions regarding the terrestrial paleoecology and paleoclimate of the emergent Bering Land Bridge (BLB). The Bering Strait is the only area on Earth where circulation between ocean basins has been blocked and a migration corridor between continental landmasses has been opened by falling sea levels of the Pleistocene epoch. The Hope and Norton basins, located north and south of the Bering Strait, contain intercalated marine and terrestrial sediment, but these basins have never been cored for the purpose of paleoclimate analysis. Collection and analysis of terrestrial sediment from these basins will at last allow paleoecological and paleoclimatic reconstruction of the emergent BLB, test the hypothesis that a mesic refugium served as a barrier to migration of certain plant and animal species, and assess the feasibility of human habitation and overland migration. In addition, collection of 600-1000 meter cores from these basins would allow identification of previous intervals of emergence and improve the chronology of the opening and closing of the BLB throughout the Cenozoic. Identification, dating, and geochemical characterization of constituent tephra deposits would facilitate correlation of terrestrial and marine deposits throughout Beringia and Bering Sea, providing a potential link to records recovered from Arctic lakes and/or cores collected by the upcoming Bering Sea Expedition. I realize it is unusual to propose collection of largely terrestrial records to the IODP, but Hope and

Norton Basins are the best targets for records of conditions on the emergent BLB. Coring these basins will take IODP in a novel direction while encouraging involvement of and collaboration with the terrestrial paleoclimate community.

Posted by Dennis Thurston on 2009-03-06 at 13:36

There is potentially so much to gain from some projects on the US Arctic shelf.

I fully agree with Sarah that the collection of a few well placed cores on either side of the Bering Strait could provide a wealth of information on the history of the BLB including, permafrost, Quaternary paleoclimate and sea level history in a region where we really need this information. A project like this could benefit from proximity to logistical support bases and possibly could utilize alternative drilling platforms depending on the depth of the cores (even 300 m cores would be very useful).

As for 1000m cores, I think one drilled 20 miles or so NE of Barrow could reveal the age and affinity of a Devonian(?) clastic wedge (along with a thin underlying Carbonate sequence called the NE Chukchi Basin) and could possibly answer some key questions on the opening of the Canada Basin. This project would be very close to the support base at Barrow and could be drilled in ice-free conditions--and for the cost would be perhaps one of the best investments in Arctic geology and geophysics! In this location the Clastic Wedge lies just below the Lower Cretaceous Unconformity and a thin wedge of Ellesmerian rocks and is within 1000 m of the seafloor. For the location and description of the NE Chukchi Basin see Sherwood, K.W., Johnson, P.P., Craig, J.D., Zerwick, S.A., Lothamer, R.T., Thurston, D.K., and Hurlbert, S.B., 2002, Structure and stratigraphy of the Hanna Trough, U.S. Chukchi Shelf: in Miller, E.L., Grantz, A., and Klemperer, S.L. (eds.), Tectonic Evolution of the Bering Shelf-Chukchi Sea-Arctic Margin and Adjacent Landmasses: Boulder, Colorado, Geological Society of America Special Paper 360, p. 39-66.

Posted by Scott Dallimore on 2009-03-03 at 12:32

I have no doubt that the challenges of embarking on a serious scientific drilling program in the Arctic will be considerable. We will need new strategies for drilling platforms, drilling/coring methods and on site laboratories. The scarcity of data in the Arctic will also be a challenge in terms of site survey information and assessment of drilling hazards. However, as mentioned by all contributors to this session the scientific returns are certain to be substantial as there are a wide range of both fundamental and applied science questions that need answering. As a southern researcher who has worked in northern Canada his whole career, I do have a few tips should ICDP move north. First, let us not forget the northerners who live in the Arctic. Perhaps more than any other place on the planet, northerners feel a great threat from climate warming as they have seen significant changes in their environment and in their lifestyles just in the past few decades. Outreach and engagement of northerners in our science will yield great good will and offer practical experience. Following this my second suggestion would be to try to choose projects that specifically help to unravel the history of climate change and the associated geologic processes in the Arctic. When was the last time the Arctic ocean was ice free in the summer?, what are the fluxes of green house gases from thawing submarine permafrost and degassing gas hydrates? how have inputs of fresh water to the Arctic

varied in the past and how has this affected global climate change? There are of course many broad fundamental science questions that need to be addressed as well, but if we can, lets keep our eye on those research projects that have the potential to look to past and help us with questions about the future.

Posted by Timothy Collett on 2009-03-04 at 18:06

The distribution of gas hydrate in marine arctic environments is poorly documented, although conditions exists for gas hydrate to occur associated with permafrost on the submerged continental shelves of the circumarctic sedimentary basin, much like their terrestrial counterparts. As highlighted above by Alberto and Scott, scientific drilling of destabilized permafrost and dissociating gas hydrate accumulations on the Arctic shelf would be of a great benefit in characterizing the distribution of gas hydrate in the Arctic shelf sediments and in unveiling thermal and hydrogeological processes that control methane release. It would provide important clues as to the interplay between permafrost-associated gas hydrate in Arctic shelf sediments and global climatic change.

Posted by Frank Rack on 2009-03-05 at 15:23

I'd like to reemphasize that I believe we need a focus on POLAR scientific drilling, involving both the Arctic and the Antarctic regions. The various respondents have clearly pointed out the opportunities for Arctic exploration, and I strongly agree with these perspectives, but I believe we need to also learn more about circum-Antarctic continental margins and the cryosphere in order to unravel the history of this huge and largely unexplored region and understand its role in planetary processes through time. A focus on discovery through scientific drilling at both poles will lead to fundamental scientific discoveries which will have both societal relevance and will further excite the public's imagination about scientific exploration in remote areas of our planet.

Posted by Rick Murray on 2009-03-05 at 19:45

A related issue to the over-arching goals of polar/high latitude drilling has to do with age models. Particularly, but not exclusively, for younger higher resolution time scales, the field has been somewhat challenged in resolving leads/lags in the system between polar and equatorial records, let alone between north vs south high latitudes. There's been great progress towards this in recent years, but I believe we as a field need to be careful to ALSO pay attention to the fundamental underpinnings of how we'll "use" the mud we recover. Not that anyone is arguing as such, but it won't do us much good to get good records if we can't correlate them to each other and elsewhere. Thus, I would suggest that if we do make a concerted polar effort (which I would support, in either hemisphere) that we also spend resources in some of the decidedly less sexy yet vitally crucial aspects of the supporting sciences.

As y'all know, I'm not an age-model guy, so am not pushing for my own field here. But, I do need to know how old the stuff is...

Posted by Joseph Stoner on 2009-03-12 at 14:05

Following on Rick's and XiXi's comments, paleomagnetism can offer significant stratigraphic opportunities in Arctic and Antarctic setting, while high latitudes (north & south) provide a unique observational perspective on the geomagnetic field. Prior work

on central Arctic sediments has been complicated by what a diagenetically induced variability that would likely be less of an issue on the margins. Results I have seen from the Holocene suggest that the Arctic paleomagnetic record is quite interpretable with some unique features reflecting the unique observational location inside the tangent cylinder. Arctic paleointensity appears to be correlatable to records outside of the Arctic. Long, high-quality, high resolution paleomagnetic records would have both stratigraphic utility and would be extremely important for constraining the dynamics of the geomagnetic field, assessing the past evolution of the location of the magnetic poles and testing hypotheses on the influence of the lower mantle on the geodynamo.

A1.5 Continent–ocean fluxes, processes, linkages

Posted by Gabe Filippelli on 2009-02-23 at 15:09

We focus on carbon and water transfer between continental and oceanic realms, but other biogeochemical cycles, particularly those of limiting nutrients, are important to understand. Comment on the importance of understanding continent-ocean interactions and approaches that can be taken by scientific ocean drilling to examine these interactions. Potential "themes" include but are not limited to:

- 1) Sea level impacts and continent-ocean transfer as related to oceanic biogeochemical cycles
- 2) Continental margin storage of carbon and nutrients
- 3) Weathering transfer of nutrients from land to ocean

Posted by Adina Paytan on 2009-02-28 at 12:53

I agree with Gabe that this is an area of great interest. I would add another theme:

- 4) Atmospheric deposition as a mechanism for transfer of nutrients, metals, acids and pollutants to the ocean

Posted by Ken Miller on 2009-02-24 at 15:55

Our understanding of global sea-level changes in the Pliocene-Pleistocene is woefully inadequate. We know the LGM and marine isotope chron 5e reasonably well. The following are not well known:

- the maximum rate of change, particularly in MWP1a.
- the position of sea level during marine isotope chron 3 ("stage" of paleoceanographers) is remarkably poorly constrained except by models (Siddall et al.)
- marine isotope chron 30 is a peak warm interval? Where was sea level? Where do you want it to be?

And finally my pet peeve, the Pliocene. I sat through a modelers talk at GSA which defined climate sensitivity based in part on the Pliocene warm period, when it was claimed we know sea level well (+25 m). This number is essentially made up. It may be right, but it may be 10 m. Dowsett is the main dataset and that is from an uplift coastal

plain with huge error bars.

If you want to understand cryospheric evolution, we need to get firm numbers for sea-level for during chronos 3, 30, Pliocene, and others.

Posted by Liviu Giosan on 2009-03-05 at 13:50

I agree with the proponents and commentators of this topic on its importance. A sustained effort should be dedicated in my opinion to supporting drilling of deep sea fans and river/glacier-dominated high sedimentation rate upper continental margins and shelves which have been many times avoided in the past due to logistical complications. Besides the topics already discussed I would raise the following:

1. Linkages and direct comparison/contrast between terrestrial paleoclimate (pollen, organic and inorganic geochemical proxies) with regional paleoceanography using records in the same cores in high-resolution continental margin settings.
2. Moving beyond major sea level cycles toward integrating the millennial climate cyclicity paradigm into sea level studies. Millennial cyclicity in climate is likely reflected in high-resolution sea level variability affecting the architecture, sediment and organic carbon storage of continental margins.
3. Providing understanding on the developing field of seismic or paleogeomorphology developed on continental margins and the causal links between paleogeomorphology and climate, tectonics, sea level, etc.

Posted by Joseph Stoner on 2009-03-12 at 14:37

Following on Liviu's comments, such archives are also excellent for geomagnetic reconstructions and the paleomagnetic record has unique potential for marine/ terrestrial stratigraphic synchronization. This also ties into other threads emphasizing the linkage and cooperation with terrestrial drilling programs like ICDP and the need for top to bottom sub-seafloor science planning and support from surveying, to using the right technology for the job (JR drilling, long coring, alternative platform etc), to post cruise science.

Posted by John Jaeger on 2009-03-03 at 11:57

One of the original goals of the MARGINS Source-to-Sink program was to examine how climate, sea-level fluctuations, and other forcing parameters regulate the transfer and storage of solutes and sediments (including organic matter and particle-associated limiting nutrients) from their sources to their sinks. A far-ranging set of data have been collected from modern continental margins that reveal the fate of organic matter and nutrient recycling during the transit from terrestrial sources, mixing with marine sources, and sequestration into marine sinks. Yet we have little understanding of how the sources and subsequent fates of organic matter and solutes vary in the temporal dimension, as Gabe points out. This was one aspect that S2S was not able to address and is something that ocean drilling is ideally suited to accomplish. The relatively brief high-stand conditions represented by the Holocene are atypical of most of the late Pleistocene. Are there different pathways for organic matter remineralization and burial during forced regressions and low-stands, when sediments and solutes are potentially more rapidly transferred to the shelf edge, where oceanic realms (open ocean vs. coastal/fluvial)

interact? How would this rapid transfer alter the desorption/adsorption of dissolved organic matter or the oxygen-exposure time and remineralization of organic matter and nutrients? Targeted drilling of shelf-edge deltas and continental slope basins would be an ideal environments to drill to address these topics.

Posted by John Jaeger on 2009-03-03 at 12:00

Recent work in the St. Elias Mountains of coastal southern Alaska shows that there has been a rapid increase in erosion during the late Pleistocene with a corresponding rapid (<500 ka) reorganization of tectonic deformation. Numerical surface process models show that landscapes and corresponding sediment fluxes to the ocean can rapidly (within a few 100 ka) adjust to external forcing by climate (via erosion), and it's been well documented that sediment fluxes to continental margins have been enhanced during the Pleistocene relative to earlier in the Neogene. Does this increased flux simply reflect a change to a more erosive and non-steady climate or are exhumation rates and tectonic forcing also adjusting to climate-induced changes in erosion and thereby contributing to this increased flux? Although this topic needs to be addressed with terrestrial studies, ocean drilling can play a key role in that the temporal record of these changes via sediment fluxes and possibly tectonic deformation of margin strata is best recorded offshore. Drilling on tectonically active continental margins (e.g., Taiwan, Alaska, Central America, Patagonia, Papua New Guinea, etc.) could provide a sedimentary record of a climate-forced transition in tectonics.

A1.6 Surface processes and geochemical cycles

Posted by Susanne Straub on 2009-02-19 at 11:05

A future science plan should promote research causal links and feedback mechanisms between the Earth's surface cycles and the solid rock geochemical cycles. The IODP Initial Science Plan rather separates these as two major themes, with no designated attempt to link them.

The idea that global climate change and volcanisms may be causally linked followed DSDP drilling in the 70s. The PETM has also been linked to subduction processing of carbonates, and more recently, deglaciation has been considered to promote mantle melting, and thus volcanic output into the atmosphere.

Ocean drilling is virtually the only way to follow up on these issues. First, the oceanic sediment record allows for connecting these outer and inner cycles, because it combines the temporal record of either – by means of the pelagic sediment and volcanic ashes (near arcs and large hotspots). Second, these records span the time scales in which such links and feedback mechanism operate (100ka to 10 of million years).

Posted by Dan Lizarralde on 2009-03-13 at 16:14

I have just made a post to the "Rifting" topic describing C cycling driven by rifting, sedimentation and magmatism. I could see some exciting, interdisciplinary science coming out of IODP drilling targeting these processes.

A1.7 Ocean lithospheric cycling

Posted by Donna Blackman on 2009-02-18 at 12:49

Exchange within the ocean lithosphere is a theme that I think we could beneficially focus more clearly on within IODP. Ill emphasize the igneous ('basement') portion of the lithosphere since that is where signatures of both deep and surface processes are available for integrated study. The concepts are very much part of ongoing Solid Earth & Geodynamics and Deep Biosphere efforts within IODP (as well as InterRidge and InterMargins) but I frame them as a 'key theme' because a 'system' viewpoint may provide new means of identifying where drilling and in-situ monitoring, within the context of subsurface imaging and seafloor mapping, could shed new light not only on a local area but on how to address larger questions relating global geochemical exchange.

This theme encompasses chemical/mass transfer from mantle to crust and igneous evolution within the lithosphere. I envision a significant emphasis on alteration and the interplay between deformation and rates of lithosphere-ocean exchange. Biologic participation is an integral aspect. What chemical exchanges (lithosphere, hydrothermal, local ocean) are unique within the axial zone of spreading centers versus may continue long-term (and over huge spatial extents) within aging lithosphere? What types and what distribution with depth characterize alteration and microbial exchange within lithosphere at the various stages of aging? Does this impact ocean chemistry? Are rates dramatically different for different crustal architectures (e.g. formed at fast-spreading versus slow-spreading rate)?

Although focus on geochemical cycling is currently strong within ocean and atmosphere programs, an important role that academia can (should) play is to not allow the integral role of solid earth exchange to be down-played. However, quantitatively addressing biogeochemical exchange within the (huge!) igneous ocean lithosphere will require in-situ measurements over relevant time periods (typically more than the few weeks that a standard drilling expedition might allow). Either within IODP or through coordination with other programs, borehole/cork-type systems need to be designed and be able to be routinely employed. Id like to see further discussion on this topic within the Strategies forum, with those more familiar with these systems than I pitching in to help us explore what progress we can make along these lines in a next phase of IODP (and/or associated other programs).

Finally, improving characterization of the physical properties throughout the system is an integral part of the overall goal for this theme, so that regional studies have relevant groundtruth, allowing inferences about distribution of processes to be drawn with a reasonable level of confidence.

A1.8 Ocean acidification and atmospheric inputs to the ocean

A1.8.1 Ocean acidification

Posted by Richard Norris on 2009-02-06 at 21:17

As a paleontologist working on planktic foraminifera, I am concerned that my favorite microplankton may go the way of the dodo as the surface ocean becomes increasingly acidified over the next several centuries. I suggest that a suitable theme for future drilling should include investigation of past episodes of ocean acidification. We have done some of this kind of work, such as the highly successful Walvis Ridge drilling in Leg 208, but even that cruise never managed to define what the total magnitude of the CCD change was or whether the surface ocean became substantially acidified. Hence, it is important to target areas where we can assess full depth transects (or at least the shallow or deep ends of potential transients). We also need records from areas where the biological response is likely to be well preserved--namely where the microfossil record has not been entirely wiped out by the later rise of the CCD--and where sedimentation rates are high enough to record a clear signal of the biological response.

Acidification is becoming a major theme in biological oceanography and there have been a number of workshops devoted to the topic over the past several years and major efforts in Europe to employ "mesocosms" to study CO₂ effects on plankton. Recently there was also an Ocean Leadership workshop on acidification. We should exploit our ability to actually observe the past effects of large scale acidification in the geologic record--something no biologist has. Likely paleo-examples include the Paleocene-Eocene Thermal Maximum, and the numerous "hyperthermals" of the Paleogene. It is also possible that the onset of some of the Oceanic Anoxic Events of the Cretaceous were associated with acidification.

Posted by David Anderson on 2009-02-09 at 15:48

Ocean acidification is a significant new theme where the paleo record can make substantial contributions. In addition to the focus on events described below I would include slow trends in pH. For example, we need to understand the Cenozoic trend in atmospheric carbon dioxide (and ocean pH!) as well as the glacial to interglacial cycles. More generally, what causes ocean pH to remain stable, and what causes change, over timescales ranging from decades to millions of years?

Posted by Stephen Pekar on 2009-02-18 at 15:22

I agree that atmospheric CO₂ changes and ocean acidification are new themes that IODP can address.

Posted by Robert Dunbar on 2009-02-27 at 14:09

If we can make a compelling argument for the questions being asked right now with regard to oceanic CO₂ uptake, then yes, this will be a strong new justification for ocean drilling. The questions from the various global and ocean carbon cycle white papers that are now available would seem to me to be: 1) how much C can the ocean take up and how fast can it respond to an atmospheric transient event? 2) What are the impacts on carbonate producing organisms and carbonate structures and sediments? 3) What are the impacts on organismal physiology? 4) What are the impacts on ecosystem structure and

biodiversity? 5) What properties impart resiliency to CO₂ injection events? 6) What are the synergistic effects of changes in C system chemistry and temperature?

One challenge that we face is that many of the C cycle perturbations that we know about from previous ocean drilling records much more slowly than what is happening today (at least we think this is the case) and so a number of compensation mechanisms were more or less keeping pace with the perturbation thereby limiting the magnitude of the actual pH excursion and also giving organisms more time for adaptation and acclimatization. Nevertheless, careful sampling of key intervals will allow us to state the direction in which systems are moving as the C system chemistry changes as well as something about quasi-equilibrium states once the system plateaus for awhile. And the fact remains that we have a record of what did actually happen at our disposal.....but we need to be careful how we engage the broader community and the IPCC ocean acidification working group. We need to be careful what we promise but also make it compelling. Some careful thinking about this should be done prior to the Bremen INVEST meeting.

Posted by Adina Paytan on 2009-02-28 at 12:11

While ocean acidification is a worthy topic that we can not ignore it is important to note that much more work has to be devoted to the development of reliable proxies to study the changes in pH, carbonate preservation, impacts on biology and sedimentary processes and the feedbacks in the system at various spatial and temporal scales. If the community is to move ahead with making this a major topic for our research a clear strategy and approach for this research is needed and I don't think we have this at hand. What are the measurements we want to do? how good are they? what are the limitations? Indeed it is time to lay these out.

Posted by Marta Torres on 2009-02-28 at 16:34

I agree with Adina and Rob, ocean acidification is an important and current theme that can produce high impact science; however a clear strategy as to how IODP can address this question is key. Such strategies may indeed exist and I'm just not aware of them, I do encourage the people involved in this issue to clearly laid out the questions that we seek answers from in the sediment record and the means we would use to answer these. First and foremost here, as Adina points out, is the development of reliable paleoproxies. What is the state of the knowledge here, what proxies do we have, and what is missing.

Posted by Rick Murray on 2009-03-05 at 19:56

Having just gotten off the Knorr from a 6 week-long cruise, I'm a little late chiming in, for which I apologize. Regarding this string of conversation, I think Adina, Rob, and Marta are exactly on target. I'll make a comment here similar to one I made on the board discussion of drilling in high latitudes...namely, as has been stated here, we really need better work on the "tools" ("proxies") otherwise we'll end up with the same old story based on brand new (expensive) mud. Another way of looking at this is as follows: Why do we need new mud to answer these questions? Is it really new/more/better placed sites that we lack? Or is the ability to read the record accurately (and precisely)? Until we can answer those questions (and perhaps workshops and so on would do just that, answer the questions), simply saying that drilling can contribute significantly to ocean acidification is motherhood and apple pie.

A1.8.2 Atmospheric inputs to the ocean

Posted by Adina Paytan on 2009-02-28 at 12:23

It is becoming clear from recent research that atmospheric deposition has significant impacts on oceanic biogeochemical cycles. Many constituents enter the ocean via this mode including important macro (N and P) and micro (Fe) nutrients, acids and toxins. The impact of this on phytoplankton and other organisms and also on the ecosystem structure and carbon sequestration via the biological pump could be significant. A few attempts to determine terrigenous input to the ocean in general and use of geochemical indicators of weathering have been used but to my knowledge the specific impacts or reconstruction of atmospheric deposition in the ocean has not been thoroughly studied (in the present or past). It would be nice to include this in the future.

A1.9 New drilling frontiers

A1.9.1 Variability of heat flow

Posted by Dick Von Herzen on 2009-02-27 at 16:25

Geothermal measurements have been a significant component of scientific ocean drilling nearly since its inception. For the most part, these measurements have confirmed the validity of many more measurements in the world oceans obtained with relatively short (~2 – 7 m in length) probes deployed from oceanographic vessels. However, some environments with apparently unstable bottom water temperatures (BWTs) have not been sufficiently investigated, i.e., shallow continental margins (e.g., Fisher et al. 1999), and some deep ocean regions where significant BWT variability is apparently associated with variable deep currents (von Herzen et al., 1999; Geli et al., 2001).

Shallow continental margin measurements are rare because scientific drilling has not emphasized such regions for study, and drilling from industry platforms of deep hydrocarbon deposits does not normally include detailed geothermal measurements. An exception occurred during ODP Leg 150, located on the shallow New Jersey passive margin, when detailed geothermal data obtained during nearly continuous Advanced Piston Coring (APC) operations extending to sub-seafloor depths of about 150 m showed markedly different results at two nearby sites. Whereas the measurements at the deeper (802 m) site 902 showed very uniform heat flow based on 15 sub-seafloor measurements, those at site 903 (4.4 km upslope, 453 m water depth) indicated very non-uniform heatflow over about the same sub-seafloor depth interval.

The best interpretation of these puzzling results is that the measurements at site 903 were affected by a systematic BWT change over the past 100 years or so, but not at the nearby site 902. However, a search of the extant oceanographic data (admittedly sparse) over this time interval failed to resolve the source of the hypothesized change in seafloor temperature at site 903. Another possibility is that the nonlinear measurements were

caused by relatively recent changes in pore water flow in the sediments at site 903. Further geothermal measurements at other shallow margin sites, perhaps those also of interest to paleoceanographers, should be useful to clarify these puzzling initial results.

The deep ocean sites referenced above are located in active subduction (forearc) environments and on a slow-spreading mid-ocean ridge. Accurate geothermal measurements are useful to interpret the geodynamics of subduction, for which drilling may be required. Similarly, borehole measurements on slow spreading ridges at which BWT variations are significant may be needed to clarify the local variability associated with hydrothermal circulation in the ocean crust.

Although these detailed geothermal measurements have been made with the APC tools already developed for scientific ocean drilling, measurements may also be possible with thermistor strings engineered for deployment and measurement after drilling. Such deployments may complement the APC instrumentation and reduce the time needed for measurements carried out during APC operations. Although it is possible to consider such measurements distributed over multiple sites drilled on one or more drilling legs, it is also feasible (based on ODP Leg 150 experience) to implement such measurements at only a few selected sites with relatively few (2-3) trained personnel aboard.

A1.9.2 Geodynamo/paleomagnetism

Posted by Joseph Stoner on 2009-02-11 at 02:04

Not really sure if this fits here, but it seems to me that geomagnetic field and explicitly working toward deciphering the inner working of the geodynamo could be just such a theme. Ocean Drilling has been incredibly important to the vitality of paleomagnetism, which is much more than just stratigraphy. Setting up a program to deconstruct geodynamo behavior through reconstructing the space time patterns of the geomagnetic field is very doable with present technology and with the right attention huge strides could be made. Ocean Drilling is really needed to do it as many of the features of interest, if they are to be sampled at high or, better yet, ultra-high resolution become relatively deeply buried and inaccessible to other means. Societal implications can also be argued as the present field is undergoing significant change and past data that are available suggest that it could have impacts on Earth's near space environment that will affect technology and might impact Earth surface processes.

Posted by James Channell on 2009-02-18 at 14:59

Joe Stoner's comment on the role of ocean drilling for elucidating the history of the geomagnetic field is well-taken. This effort may even come under the umbrella of "societal relevance" if advocates of climate-geomagnetic connections are proven correct.

On another but related tack, improved stratigraphic correlation remains one of the great challenges in paleoceanography, and limits the understanding of "abrupt" climate change. Oxygen isotope changes in seawater are not globally synchronous on (few millennia) timescales associated with the mixing time of the oceans, and shifts in $\delta^{18}\text{O}$ are often gradual between terminations limiting correlation resolution. There would be

great advantage in coupling oxygen isotopes with an independent stratigraphic tool that is global in nature and devoid of environmental influences, eventually perhaps freeing $\delta^{18}\text{O}$ from its chronological role. The accumulation of relative paleointensity (RPI) data in the last 10 years holds the promise of global stratigraphic correlation within polarity chronos, possibly at millennial scale. Ocean drilling, and the well-proven APC/splice procedure for recovery of complete composite sedimentary sections, can play an important part in developing these stratigraphies.

Posted by Joseph Stoner on 2009-02-18 at 15:21

And following on Jim's comment. Paleointensity as a stratigraphic tool not is limited to the marine realm and therefore can be used to further marine-terrestrial correlation that might allow us to test how oceanographic processes affect and terrestrial environment. And, therefore, providing a mechanism for linking ocean and terrestrial drilling initiatives and outcomes.

Posted by Sarah Strano on 2009-03-05 at 12:32

I think that in order to use paleointensity as a stratigraphic tool, it would be very useful to have a wider distribution of climate records, namely more good southern hemisphere records. As someone new to the field, I see the lack of southern hemisphere records with good chronologies to major hindrance to really understanding and using geomagnetic parameters as a means for chronology correlation and understanding climate change. I know that it is more difficult due to a lack of survey data to drill in the southern hemisphere but I advocate that it should become a more important component of the IODP initiative. When you look at the distribution of ODP and IODP legs, it is clearly northern hemisphere biased and then when you whittle it down to the good paleomagnetic records, you find even less records in the southern hemisphere. It seems that in order to gain a true understanding of the paleomagnetic field and the geodynamo, we have to understand the southern hemisphere more completely. As part of my research, I would like to improve our knowledge of the geomagnetic field in the southern hemisphere. I realize that it is difficult to get drilling time in and it will take a long time for me to become integrated into the program and write proposals, etc. But I would also like to accomplish some of my research goals and that involves getting more records from the southern hemisphere. I would like to look at the geomagnetic field from a more global perspective and I'm still trying to figure out how to do that besides requesting lots of add-ons to each cruise that might help me out. I think this will be useful for using geomagnetic properties as a stratigraphic and chronological tool, as well as for enhancing our understanding of the geodynamo, the geomagnetic field and its relationship to climate. If we could record excursions more globally or at least constrain them more regionally, then we could enhance geomagnetism as a chronological tool, and as previously mentioned, the only way to accomplish that is to continue drilling and to drill in new places.

Once again, I'm very new to the field and may have no idea what I'm talking about but I'm trying to learn and figure this stuff out so please, feel free to give me tips. I didn't even really know where to put these thoughts in the forum but since there was talk about the geomagnetic field, I thought I would give it a shot. As a first year grad student, I decided that I just need to jump in and get involved in this workshop. I hope to

contribute in a bigger way but I also realize that this is my chance to learn about the program and get advice from people who know the system so that I can navigate it better.

Posted by Alan Mix on 2009-03-05 at 15:57

Sarah raises a great point here. Traditional drilling proposals tend to focus on reasonably constrained areas, effectively a leg-length package that maximizes efficient use of shiptime. But is there room for a proposal that isn't geographically or logically organized? Would a proposal that requests, say, ten sites, scattered broadly across the globe as part of a single scientific objective, review well? It might require an "add on" process that would package the ten sites into ten different drilling legs spread over many years. Could our planning process accommodate such a proposal? Or would our problem of shifting institutional memory on panels make that a hopeless effort. The original proponents get left out of the science, since they wouldn't be primary players on any of the legs that actually drill their sites? I hope not. This is a legitimate way of proposing innovative science, and the program should be flexible enough fit the science rather than the other way around.

Posted by William Sager on 2009-03-13 at 08:41

This is a difficult process. First, I strongly argue against "add ons" because I have seen it from the other side. You work hard to design a drilling project and virtually any such project is tight for drilling time. But then come "add ons" that threaten to cut out a portion of that carefully designed project. Furthermore, the science steering panels have not been particularly receptive to "add ons" anyway, probably because of that conflict. Unfortunately, in the present climate, I don't think a proposal that simply says we will go drill some southern hemisphere sites because we need more records is going to be successful. It has to be cleverly put together with testable hypotheses. If that can be done, then the panels have proven that they are quite receptive to paleoclimate proposals. While this discussion started with paleomagnetism, I am not sure that paleomagnetism can carry the day. However, paleomagnetic time scales integrated with other traditional paleoclimate time scales probably can, so paleomagnetists need to team with paleoceanographers for a multidisciplinary proposal.

And one of the questions was whether or not we can get sequences in different locations in the southern ocean. Because of logistics, this naturally means multiple cruises. The past record of the program on this is a bit spotty. Most notably the program recently calling for "mission" projects built around several cruises - and then going "whoops, never mind." On the other hand, there are several examples of multiple-cruise projects, such as the current PEAT-1 and PEAT-2 (Pacific Equatorial Age Transect). So a well-designed series of proposals might make it.

A1.10 Global drilling partnerships

Posted by Organizing Committee on 2009-01-12 at 13:57

Is there overlap between diverse communities that could benefit from merging their ocean drilling objectives within new paradigms?

Posted by Mitch Lyle on 2009-02-20 at 06:26

We should also remember that the present location of the shoreline does not define where the oceans have always ended. If we want to study the Mesozoic or Paleozoic, we must drill marine sections on land. Furthermore, the shallow seaways of the Cretaceous and early Cenozoic are only on land. If we want to understand why the early Cenozoic Arctic was warm, we must include studies of these connections.

Posted by Dan Lizarralde on 2009-03-13 at 16:19

I have just made a post to the "Rifting" topic describing the diverse science that could be addressed by drilling sedimented ridges. I note there how such a focus may be of great interest to the petroleum industry. I could envision an IODP/Industry/MARGINS "collaboration" that targets sedimentation/rifting feedbacks as being win-win-win.

Posted by Beth Christensen on 2009-02-17 at 16:58

The first option that comes to mind is DOSECC, but that is such a different beast that it is hard to imagine how IODP and DOSECC would individually benefit from a merger. However, there would be an obvious relationship if, for instance, we were to identify a drilling objective that spans the continental margin, from source to sink (e.g., coastal plain, shelf, slope, rise).

Posted by Frank Rack on 2009-02-18 at 13:12

I would broaden this to include ICDP (International Continental Drilling Program), which DOSECC frequently works with and is intimately related to. There are already many areas of collaboration and coordination between IODP and ICDP and the kind of drilling objectives that Beth identifies are a good example of what has been done in the past and what may be done in the future.

Posted by Beth Christensen on 2009-02-18 at 13:29

What about developing a formal mechanism that links these entities programmatically within IODP? Would that provide a benefit to the community or would it complicate things with another layer of administration? Would having a focused linked program such as source to sink objectives aid in advancing funding for the science and resulting engineering challenges? Would it provide additional benefits to the individual programs, beyond what the programs could generate separately?

Posted by Frank Rack on 2009-02-18 at 15:09

"Source to Sink" is a MARGINS initiative that has involved proposals and discussion by IODP, ICDP and other groups. So the discussion of how to integrate the needs of various programs and find ways to share resources is a good one, and we've done this in the past. The key is to find the "sweet spot" that balances scientific objectives and opportunities with available resources and potential risks within a workable contractual framework.

I think the approach of trying to plan integrated science strategies across programs to create projects that can be addressed over time by appropriate drilling platforms is one of the key requirements. For example, New Jersey margin has been the target of intermediate to deep water drilling on the shelf and slope, shallow water drilling on the

inner shelf and drilling on land and these efforts have spanned DSDP, ODP and IODP. Funding has come from IODP, ICDP and state geologic surveys, as well as other groups, and similar types of approaches could/should be taken for other margins.

I believe we have been trying to formalize mechanisms among several different programs to plan joint initiatives for many years, with some success, but the shared funding aspects are complicated by the fact that each of these programs/projects often have overlapping, but not the same national and international stakeholders, funding sources, and contractual relationships. I think that more efforts should, and are being made, to look for appropriate partnerships with well-defined roles and responsibilities and shared objectives. We leveraged DOE and European funds to help with ODP/IODP drilling projects related to hydrate characterization and the establishment of microbiology capabilities. We've (IODP and ICDP) worked together on database development and publication issues through partnerships like "Scientific Drilling". We have also shared tools and equipment between programs using clearly-defined agreements that mitigate risk related to the potential loss or damage of these items.

This has all been possible in the past and these synergies could be expanded, as appropriate, in the future. These efforts can help to build bridges across existing "smokestack" hierarchies that often exist within or among funding agencies and programs in many, if not all, of the member nations. In the U.S., scientific ocean drilling is largely funded through NSF-OCE, continental drilling is funded through NSF-EAR, and polar drilling is funded through NSF-OPP. The existing sharing of resources across these boundaries are managed by Program Managers based on proposals and requests from individual programs and scientists. There are certainly challenges, but also opportunities and benefits to be gained by these discussions. Having an open exchange of information, clear objectives, and a coherent project management process for these types of interactions is useful. Whether we want to try to develop larger integrated structures (e.g., an International Global Drilling Program) is an open question. Will bigger be better?

Posted by Stephen Pekar on 2009-02-18 at 15:19

Besides ICDP, there may be opportunities of synergistic collaborations with ANDRILL and perhaps even SHADRILL. Additionally, a modified shelf to rise transect (i.e., Beth's comment) with ICDP or ANDRILL in Antarctica could be another possible means of forging collaborative efforts.

Posted by Mitch Lyle on 2009-03-02 at 12:36

One of the real problems identified over many threads in this workshop is the difficulty to keep complex programs alive in the current IODP. Each one of the complex programs has a significant component other than drilling and a time horizon for completion of 5-10 years (or more). Most of them require partnerships. It would seem that we need two levels of commitment: first, a commitment that drilling time would be available if the program moves forward; and second, separate funding for all the experiments that do not fall under the IODP umbrella. Given that the programs are inherently international in nature, we need a continuity of management and a funding stream that is somewhat different than the traditional drilling route. The experiment does not end when the ship work ends.

Posted by Rick Murray on 2009-03-05 at 19:50

Mitch raises some very good points here. Because of the difficulties he and others have articulated in dealing with complex programs (not only within IODP but also with DOSSEC, ANDRILL, and other acronym-soup programs), a complementary approach would be to approach industry (and not just petroleum industry) for some 'one time opportunities'. The Talwani-Rice Univ led effort is a good example of that...if that does indeed come to pass it could have a long - lasting positive impact on us all scientifically. That's a situation that is certainly complicated, but in comparison to dealing with large programmatic coordinating it is incredibly streamlines. Thus, the question could be rephrased/expanded to be...what other ones out there are there (CO2 sequestering, etc.?).

Posted by Liviu Giosan on 2009-03-05 at 14:06

I agree with the points raised in this discussion and with the general idea of partnering with sister initiatives. I would like to add that although IODP comprises of more and more nations that participate to the program independently or in an associative form, there are still more maritime countries to involve in this global effort. I suggest that IODP think of a formal ways to involve smaller non-affiliated nations to drilling efforts in their region of interest, both at research and educational levels. Although such efforts have exited since DSDP time, formalizing the interaction between IODP and such nations may provide a boon of interest in the program around the world.

Posted by Liz Screamton on 2009-02-25 at 09:41

I agree and suggest a step further -- microbio and fluid flow should also be integrated rather than separated. These are important players in the global carbon cycle, and fluid flow/pressures affect heat flow and subduction processes.

Appendix to be moved to Strategies in E&O section

Key theme: promoting educational and media outreach I also support the idea of better packaging of IODP (e.g., Beth and Christina comments). While ocean drilling has written major parts of the climate and tectonic story for the last 100 million years, I think that we could have done a better job at letting the scientific community and the public know what we are doing. With that said, in the last couple of years, incredible progress has been made in developing an effective educational outreach program (e.g., School of Rocks, etc.), resulting in a firm foundation to expand and create a means of outreach in ways only dreamed of before. Having an effective educational and media outreach can improve science literacy about ocean drilling, etc., which in turn can make it easier for future funding. It can also attract the next generation of scientists to ocean drilling.

A2 SOLID EARTH CYCLES AND DYNAMICS

A2.0 Introduction

The Initial Science Plan of the current IODP program lists “Solid Earth Cycles and Dynamics” as a central theme. Centered around geologic processes driven by outer core and mantle convection, this theme was designed to examine both the creation, destruction (recycling), structure, and movement of oceanic crust and lithosphere, the rifting of continents, and large igneous provinces. Within this theme were described four initiatives: 1) Continental Breakup and Sedimentary Basin Formation; 2) Large Igneous Provinces; 3) 21st Century Mohole; 4) Seismogenic Zone.

Initial Questions

Posted by Organizing Committee on 2009-01-12 at 13:38

Should the next phase of IODP (2013-2023) focus on similar issues and initiatives?

- If so, why and what are the unsolved elements of these research foci?
- If not, what are the most compelling unresolved issues in solid earth processes that can be investigated through scientific ocean drilling?

How can investigation of the initiatives listed in the introduction contribute to mitigation of geodynamic impacts on humans? (e.g., tsunamis, volcanoes, earthquakes, environmental response to catastrophic volcanic events as analogs to greenhouse gas loading of the atmosphere)

Posted by Organizing Committee on 2009-02-24 17:20

We have moved some of the responses here to their own topics. Please follow the links for the discussion thread on:

- Structure of Oceanic Crust
- Large Igneous Provinces

Posted by Floyd McCoy on 2009-02-18 at 22:07

Where exactly are the "initiatives listed in the introduction" to be found?

Posted by Clive Neal on 2009-02-24 at 16:26

Floyd:

When you go to the first page of the Solid Earth Cycles and Geodynamics page, at the top there is written:

"The Initial Science Plan of the current IODP program lists “Solid Earth Cycles and Geodynamics” as a central theme. Centered around geologic processes driven by outer core and mantle convection, this theme was designed to examine both the creation, destruction (recycling), structure, and movement of oceanic crust and lithosphere, the rifting of continents, and large igneous provinces. Within this theme were described four

initiatives: 1) Continental Breakup and Sedimentary Basin Formation; 2) Large Igneous Provinces; 3) 21st Century Mohole; 4) Seismogenic Zone."

More information is then found when you click on "Research Topics".

A2.1 Continental breakup and the formation of new oceans

Posted by Dale Sawyer on 2009-03-02 at 10:50

I open this topic to explore community interest in ongoing study of continental breakup and the formation of new oceans.

Posted by Dale Sawyer on 2009-03-02 at 10:52

I am interested in seeing continued seismic data acquisition and complementary scientific ocean drilling to sort out the processes associated with continental breakup and the formation of new oceans. An IODP sponsored workshop in Pontresina, Switzerland in 2006 identified the key issues as follows:

(Quoted text from Sawyer et al., Scientific Drilling, No. 5, September 2007.) "Variations in the importance and, in particular, the volume of magmatism have led to the classification of margins as "volcanic" or "non-volcanic" (Mutter et al., 1988); however, this binary dichotomy fails to adequately reflect that rifted margins form a spectrum from magma-rich to magma-poor. The key distinction is whether magmatism is more or less than expected from the degree of lithospheric thinning and passive asthenospheric upwelling of normal temperature mantle. Equally important are the timing of magmatism and the strain distribution across margins, i.e., hyper-extended versus a more abrupt transition between continental and oceanic lithosphere. Because a continuum between possible end-members may exist, the focus should be on understanding the fundamental processes causing such variations. Key aspects of continental breakup can only be addressed by drilling and associated studies. More specifically, we need to determine the following at multiple, carefully selected rifted margins: 1) uplift and subsidence history; 2) ages and facies of synrift and syn-faulting sediment; 3) timing, volume, chemistry, and style of magmatism; 4) orientation of deformation fabrics, including faults; and 5) ages and facies of postrift sediment. Such information can be used to infer distribution of strain in space and time; deformation mechanics and dynamics; processes within the mantle, including depth and degree of melting, melt migration, and infiltration; and mantle composition, heterogeneity, and dynamics.

We propose drilling programs on well characterized and representative examples, conjugate where possible, of both active and mature rifted margins ranging from magmatic to amagmatic and abrupt to hyperextended."

(End of quoted text)

The workshop suggested the following regions for possible study:

- Gulf of California
- Woodlark Basin

- North Atlantic Magma Dominated Margins
- Newfoundland-Iberia Rift
- South Atlantic Margins
- NW Australian Magma Dominated Margins

Posted by Peter Kelemen on 2009-03-03 at 00:45

I was very sorry that an emphasis on active margins led MARGINS workers and related drilling away from the North Atlantic, which in my view is clearly the best place for studying volcanic rifted margins, in part because of the excellent exposures of related igneous rocks on land, in part because of the well developed conjugate sections, and in part because decades of excellent seismic projects have laid the groundwork for really comprehensive understanding of what happened there.

There was also the small matter of the JR nearly sinking ... but anyway.

I'd like to see IODP refocus efforts on the North Atlantic. If the shoreline can be crossed in the context of "alternative platforms", for example via drilling from skerries offshore of East Greenland, where the volcanics are already eroded away and one can begin in gabbroic rocks, so much the better.

Posted by Peter Kelemen on 2009-03-03 at 00:39

I've just posted something on the Emerging Fields and Cross-Cutting Discipline Topic, in the sub-topic on CO₂ mitigation, that I think has a lot of synergy with scientists interested in continental breakup. This is the idea of CO₂ capture and storage (CCS) via accelerated carbonation of mantle peridotite, as a relatively rapid route to solid storage. There is some pretty specific IODP research one would consider in this context, involving drilling in shallow seafloor through sediment into peridotite, for example offshore of Oman, New Caledonia, and/or Papua New Guinea. Note that these are tectonically very interesting areas, and PNG in particular has been the locus of a lot of research on the rifting that has resulted in tectonic exposure of mantle peridotite. Drilling peridotite in these settings would yield vital information on the nature and timing of the last igneous features, of various deformation mechanisms, and ultimately of tectonic exposure and weathering.

Interested readers can go to

<http://www.ledo.columbia.edu/files/uploaded/file/2008%20Kelemen%20&%20Matter%20PNAS+.pdf>

to see what we are thinking about. With regard to seafloor science, Figure 8 in that paper is particularly relevant. What's needed is characterization of present day alteration, fluid flow, reaction rates, porosity, and permeability in 500 meter to km-scale holes, and then hole-to-hole experiments involving hydrofracture and - perhaps - pre-heating a rock volume at depth and then - perhaps - injecting pure CO₂. Each of these steps would be undertaken in order to "jump start" hydrothermal systems like Lost City, and - if necessary - accelerate them. The idea is to enhance the natural peridotite carbonation system, doing as little as possible while consuming as much CO₂ as possible. Site survey

requirements, particularly assessment of depth from seafloor to peridotite, and nature of overlying sediment, are obvious and doable. In some cases, oil companies and others may already have obtained such data. Note that, while JR style drilling might be very desirable in some cases, alternative platforms including small islands and even jetties might be better, and cheaper, in other cases.

Posted by Brian Tucholke on 2009-03-04 at 15:20

The Pontresina workshop summary does a good job of encapsulating major issues that can be addressed only by drilling. In somewhat more detail, fundamental problems include, but are not limited to: 1) Depth-dependent extension vs. unrecognized, multiple-generation faults in brittle crust. 2) Asymmetry of rifting (there are still major debates on when and if pure shear gives way to simple shear in large detachments, and at what scale). 3) The nature and extent of transitional crust in magma-poor rifts (is it all subcontinental mantle? variably intruded by melt? truly gradational into normal ocean crust?). 4) The record of tectonic events expressed by unconformities (e.g., the 'breakup unconformity' seems to be different things in different places -- is there a consistent pattern that can be identified and used as a predictive tool?).

It is essential to drill on conjugate margins in order to address such questions. With the partial exceptions of the Newfoundland-Iberia and Hatton-Greenland margins, this has never been done. Drilling also must be not just to basement, but a substantial distance into basement, so as to examine features such as geochemical variations, nature of intrusions, deformation, etc. Furthermore, holes should not be on basement highs where the synrift sedimentary record and often much of the post-rift record can be missing.

Ocean drilling has never examined conjugate rifted margins in a systematic way. Instead of continuing the historical piecemeal approach, with consonant patchwork results, I suggest that one rift or a pair of rifts (e.g., one volcanic, one magma-poor) be selected to be the foci of rifted margin drilling. At the same time, there must be a dedicated group created within the planning structure to conduct long-range planning (site survey, engineering, drilling, assessment of results, and follow up with further drilling if needed). Admittedly, this would eliminate the option of 'pet projects' for many, but after 40 years with limited results in the old mode, maybe it's time we take a different approach. It's been done successfully with MARGINS and Ridge2000 -- and one can also invite comparisons with the U.S. space program.

Posted by Jaime Toro on 2009-03-13 at 15:26

The origin and evolution of the Amerasia basin of the Arctic remains unknown to this day. Somehow this basin formed by rifting of the surrounding continental blocks, but the geometry, timing and evolution remain obscure. If you look at a map of the distribution of IODP holes, it becomes clear why: There is not a single scientific drill hole in this basin. Even piston cores are rare. The ACEX cores from the Lomonosov ridge, a strip of continental crust on the north side of the basin, produced amazing evidence for a warm Arctic in the Eocene, but did not really contribute much to our understanding of the adjacent oceanic crust. There are several other mysterious bathymetric features that cross the Amerasia basin (Alpha and Mendeleev ridges) and have not been explained to anybody's satisfaction. Recent reflection seismic data show dramatic evidence for

extensional tectonics in the Medelev ridge which is not compatible with the current tectonic models. It is clear that there are excellent targets for drilling in the flanks of the normal-faulted blocks and the adjacent sedimentary deposits.

A major scientific priority of the ocean drilling program should be to go north to the Arctic in order to fill these gaps in our knowledge. Aside from the tectonic questions, there are equally compelling arguments that can be made about Arctic paleoclimatology and oceanography.

Posted by Dan Lizarralde on 2009-03-13 at 16:07

Sorry to come in to this forum so late. A brief pitch for sedimented rifts.

Sedimented rifts provide drilling targets that address a range of interconnected tectonic, climatic, hydrologic, and biogeochemical processes and questions that are at the center of active research in these disciplines. In addition, sedimented rifts provide modern analogs to the earliest formation of hydrocarbon reservoir rocks and the evolution of these rocks through hydrothermal processes. Thus, sedimented rifts are sites that are of interest to both a very broad scientific community and, I would imagine, the petroleum industry as well. The feedbacks between rifting and sedimentation are likely to be an important theme in any next generation of the MARGINS program. If industry does indeed collaborate in some way with the next generation of IODP, then a focus on sedimented rift systems may provide a useful point of departure for defining possible IODP/Industry/NSF-MARGINS linkages.

There are several key ideas here.

First, the formation of young rifts is thought to drive a net atmosphere-to-ocean carbon flux by enhancing erosion, which in turn enhances weathering, pulling carbon from the atmosphere and depositing it into the new basins.

Second, sediments affect shallow melt emplacement at spreading centers. We have recently found in Guaymas Basin, in the Gulf of California, that the sediments blanketing the young spreading center seem to have a profound affect on shallow magmatic emplacement. Igneous sills are observed to intrude into young sediments up to 50 km away from the plate boundary. Presumably the sediments have influenced crustal-scale hydrothermal circulation, which focuses magmatic emplacement at unsedimented spreading centers.

Third, a wide region of magmatic sill emplacement into organic rich sediments drives hydrothermal processes that release carbon as CO₂, CH₄ and higher hydrocarbons. So, the carbon sink thought to be represented by rifting may be countered, perhaps to a large extent, by the influence of sediments on magmatism and consequent thermogenic alteration. These processes also affect the source-rock potential of these initially very carbon rich sediments. DSDP data from Guaymas show that sediments go from 4 wt% C to 1 wt% C in the vicinity of sills.

Fourth, organic-rich fluids transported by sill-driven hydrothermal circulation supports

diverse microbial communities. These biogeochemical processes have been well studied at the seafloor grabens in Guaymas. With the new realization that active sill emplacement can occur over a very broad region within sedimented spreading centers, there is the likelihood that a range of conditions exist as a function of off-axis distance that may support an even more diverse range of microbial life and processes than has previously been envisioned.

Finally, there is an interesting tectono-climatic interaction within sedimented rifts. Again in Guaymas, we observe two different types of sill emplacement in the NE and SW halves of the basin, with young sills extending farther off axis toward the west than the east. These differences correlate to different sediment types, with almost exclusively biogenic sediments in the west and more terrigenous sediments in the east. Different sediment types (w/ different hydrologic and thermal conductivities) seem to control magmatic behavior differently, and this may be the cause of observed crustal-scale asymmetry, suggesting a sedimentary/magmatic/tectonic feedback. The difference in sediment type is due to the fact that the western margin is a desert whereas the eastern margin is quite wet; and this asymmetry has been set up the rifting event itself, which modified the climatic conditions of the land that is now the Baja peninsula. There is thus a type of full feedback loop from rifting, regional climate, sedimentation, magmatism, to again rifting.

A2.2 Structure of oceanic crust

Posted by Organizing Committee on 2009-02-03 at 16:30

This discussion has been moved from Research Topics.

Posted by Chris Harrison on 2009-02-03 at 16:30

21st Century Mohole. I believe that this should be a central plank in the plan for a renewal of IODP. Although it has been proposed many times, it has never been the central goal of crustal drilling. If it were to become one of the central goals I believe that it would excite the scientific community as well as those funding the program. But drilling must be done so that the magnetic structure of the crust becomes clear. For this to be done, drilling must take place at a sufficiently high absolute latitude that the polarity of the magnetization can be reliably obtained.

Posted by Henry Dick on 2009-02-04 at 10:37

The last serious attempt to drill to MOHO was DSDP Legs 45 and 46. Since then there has been some exploratory drilling in the Pacific that has reached the dike-gabbro transition, but there is no possibility that that hole could reach the MOHO without a new deep riser - which is "sometime" in the not entirely foreseeable future. The concept that a single hole will solve the problem of the nature of the MOHO harks back to the now discredited idea that the ocean crust has a simple layered structure comprised of pillow lavas, sheeted dikes, and gabbros overlying mantle tectonite and that it is typically 6-7 km thick except in anomalous places such as large offset transforms. Recent work by Smith, Macleod, Searle, Casey and Cannat south of the 15°20' FZ, by the WHOI group at

Kane Megamullion at 23°N in the Atlantic shows that the crust, where explored in detail is thin, and doesn't fit this archaic model. Work on ultraslow spreading ridges at the Gakkel by Michael and co-workers, along the eastern SW Indian Ridge by Cannat, Sauter and coworkers, and along the western SWIR by WHOI shows that the ocean crust over large reaches consists of mantle dragged directly to the seafloor with only widely spaced volcanic segments. Niu and O'hara have also recently proposed that crustal thickness may vary dramatically due to variations in the major element composition of the mantle - which it was always foolish to have assumed uniform. It is clear that our picture of the ocean crust has radically changed, and a simple uniform crust cannot be assumed even for the Pacific. A consequence of this was one of the principle results of the Mission Moho proposal and workshop - that a single hole cannot characterize the ocean crust or the nature of MOHO. That plan called for additional drilling, particularly in tectonic windows where the opportunity exists to obtain full sections of the lower crust into the mantle, and to test the nature of MOHO in diverse settings that do not require a riser system. Beyond that, however, drilling alone cannot elucidate the broader structure and variability of the ocean crust and mantle. This requires remote sensing, particularly seismics and gravity. However, as was shown by the drilling at Atlantis Bank, where we drilled 1400 m of gabbro where the seismologists said there was mantle, it is clear that we need to ground truth seismic interpretation by drilling long sections of the diverse lithologies that make up the lower crust and shallow mantle. This requires holes appropriate to seismic wavelengths (>500 m).

What seems clear to this investigator is that the nature of MOHO is very controversial; it may not mark the crust-mantle boundary as has been widely supposed; it may be different things in different places. Yet forward progress in the current IODP has been very small, and at present there seems to be little commitment from the current planning structure to seriously address the greatest unknown in the earth sciences: what is the real composition, structure and variability of some 3/5 ths of the Earth's crust.

Progress towards this goal has been so slight that if it were seriously tackled in the new program, it would in fact be one of the new initiatives. INVEST should devote a major effort towards establishing what needs to be done to understand the nature of the ocean crust, and to establish a new set of priorities for ocean crustal drilling.

Posted by Andy Fisher on 2009-02-23 at 20:22

There is a broad consensus that drilling to the base of the crust is a worthy scientific and technical challenge. Can the community come to agreement on where to do this first? If it could, I think this could be done in the next phase of IODP. Furthermore, if there were consensus, then perhaps IODP could commit to this goal, and that could help in securing necessary survey and other funding.

Posted by Doug Wilson on 2009-02-26 at 14:47

Andy Fisher asked whether the community could come to an agreement about where to site the first attempt at a Mohole. Obviously, there will never be 100% agreement. My personal opinion is that there is enough support for an eastern Pacific site with fast to superfast spreading rate, age around 15-35 Ma, at low to middle latitude to proceed with advocating a Mohole as a component of post-2013 scientific drilling. Final site selection

will depend on the actual capabilities of Chikyu's deep riser system may have to wait for sea tests. Henry Dick and others bring up good scientific reasons to hope for multiple deep holes, but realism dictates that we ask for one hole first and hope to ask for others, building on the success of the first hole.

Posted by Peter Kelemen on 2009-03-03 at 00:24

I personally agree very strongly with Doug that a Pacific Mohole - when it becomes possible - should be the highest priority for oceanic crustal drilling. We have almost no direct information about the Pacific lower crust and Moho. The relatively simple seismic structure of Pacific crust implies that results from one or two holes could be applied with some confidence to a huge region of the globe.

In considering alternatives, I am not sure I find the argument compelling that because slow spreading crust is infinitely complicated, IODP should allocate infinite resources to drilling an infinite number of holes. One could argue that ODP already drilled several places near slow spreading ridges in which holes passed from gabbro into peridotite across high temperature, non-faulted contacts, and thus that the Moho has already been drilled in these settings. While none of us finds the results so far truly satisfying, I think we do have to acknowledge that there is likely a continuum between relatively simple, fast-spread crust and very complex slow-spread crust. Because of tectonic dismemberment of slow spreading sections, we really have better direct observations of lower crust and shallow mantle in the complicated, slow-spread setting.

If technology does not permit a Pacific Mohole in the next decade or two, so be it. Let's just say so, explicitly, and go back to the very interesting and productive slow spreading targets that have fascinated all of us for a long time. However, let's not call those efforts a Mohole. Sometime, we'd like to get that hole (or those holes) through the Pacific crust.

Posted by Gail Christeson on 2009-02-11 at 16:44

Ocean crust drilling. Drilling to the Moho would give us a continuous core through the oceanic crust, but only at a single location. To really understand the formation and evolution of oceanic crust requires age transects through crust produced at differing spreading rates. Ultimately we must also conquer the technological challenges of drilling young oceanic crust.

Posted by Clive Neal on 2009-02-17 at 06:18

It seems to me that looking at the admittedly few posts there is interest in understanding the nature of the oceanic crust in terms of young and old crust as well as fast and slow spreading. If I can be permitted to focus the discussion a little, I would like to press the participants to answer the following questions:

- 1) What technological advances are required for (a) moho drilling; (b) drilling of young oceanic crust?
- 2) What type of drilling is required to answer the fundamental questions? Coring? Coring and logging? Primarily logging?

Posted by Gail Christeson on 2009-02-18 at 15:20

1) What technological advances are required for (b) drilling of young oceanic crust?

The executive summary of Engineering Leg 142 of ODP (1993) lists a number of issues with drilling and coring operations at a ridge crest, including: 1) a way to stabilize the unsupported bottom-hole assembly at the seafloor, which would enable boreholes to be initiated on hard, bare rock; 2) a means to isolate upper unstable 'rubble' zones allowing deeper drilling/coring objectives to be reached; and 3) an effective way to drill, core, and recover highly fractured volcanic rock while maintaining adequate borehole stability.

Leg 142 showed that the hard-rock guide base is operational and that holes could be started, but maximum penetration was only 15 m in what must have been a very frustrating program. My perception is that technology to drill and core highly fractured and unstable volcanic rock has not advanced since that leg. If it has I would like to know more details!

We as a community must push for the science that can be accomplished if we can overcome the challenges of drilling at the ridge crest, otherwise that technology will never be developed or implemented.

Posted by Doug Wilson on 2009-02-26 at 14:41

What technical advances are required for Moho drilling? Most important is a system for flushing cuttings from a very deep hole. JAMSTEC hopes to build a riser system for Chikyu that could be deployed in water depths to 4000 m; however, the technology is untested. The minimum depth necessary for an eastern Pacific site would be around 3300 m (~20 Ma off Nicaragua and Costa Rica), and expanding the capability to 4200 m would allow a wide range of choices of possible sites. There's perhaps an outside chance that expendable mud might be able to flush cuttings, but this option is untested as well. Highly desirable technical advances would include expanded capabilities for high-temperature logging, and a hard-rock orientation system, which would reduce the magnetic disadvantages of a low-latitude site (Chris Harrison 02-03 post).

Clive also mentioned the topic of the need for coring vs. just logging. While we would all wish to be able to core the entire hole, budget realities probably will not give us this option. Alternating 50 m of coring with 100 m of washing ahead may be the best we can hope for in homogeneous parts of the middle and lower crust. Obviously, logging becomes critical in this scenario.

Posted by Chris Harrison on 2009-02-20 at 15:38

I agree that eventually we need to drill at several locations to truly understand the nature of the oceanic crust (if that is even possible). But the first hole will probably be a great technological and lengthy challenge and we probably need to approach this task slowly. We shall learn so much from the first hole that it will help guide us towards future drilling.

Posted by Doug Toomey on 2009-02-18 at 12:08

Multidisciplinary studies undertaken at a range of sites along the global ridge system are revealing that ocean crust – and the processes that form it, alter it or depend on it for their

livelihood— is far more diverse than we ever thought. This diversity in both structure and in the underlying processes presents major challenges to our community.

First among these is redefining our working models. The variety of oceanic crustal structure can no longer be viewed through a prism of Penrose-versus-Hess. Instead, new working models recognize both the spatial and temporal complexity of the processes that build and alter oceanic crust. These new models are increasingly multidisciplinary, reflecting the coordinated efforts of geologists, physicists, chemists and biologists to understand the linkages between seafloor spreading, the deep biosphere, the ocean and the atmosphere. Secondly, there is the daunting problem of tractability. How do we characterize the oceanic crust and shallow mantle if the processes that form and interact with it are more diverse than previously conceived?

Fast Spread Crust: Geophysical studies of fast-spread crust and mantle provide compelling evidence of large-scale skew and asymmetry of mantle upwelling beneath the ridge axis as well as evidence for the off-axis delivery of mantle melt and the off-axis accretion of crust by intrusive magmatism. These recent findings have implications for future studies. For example, the processes forming fast-spread crust are by no means uniform (e.g., the Penrose Model) and this lack of uniformity presents an opportunity for the drilling community to generate and formulate new proposals. Topics to be addressed include:

- The 4-D architecture of crustal creation (multiple, shallow drilling objectives), preferably on young oceanic crust.
- The nature of the Moho transition zone (deep drilling, MOHOLE), which in some models is becoming a key interface that regulates seafloor volcanic and hydrothermal activity.

With regards to recent IODP expeditions, the successful drilling at Site 1256 places the earth science community on the threshold of a major advancement. To fulfill this promise, Hole 1256D, which currently touches the dike-gabbro transition zone, must be deepened.

Slow-spread Crust: At slower spreading rates a profound degree of variability exists in the magmatic, tectonic, hydrothermal and biologic processes that accompany crustal formation and evolution. In the broadest sense, the community is in the process of hypothesizing new modes of seafloor spreading. Some characteristics of these new models include: (i) interactions, at all scales, between tectonic, magmatic and hydrothermal processes; and (ii) the incomplete extraction of melt from the mantle. Recent findings challenge the common hypothesis that the rheology of slow-spread lithosphere is controlled by temperature, indicating instead that strain localization mechanisms favored by lithological contrasts and hydrous alteration plays a central role.

In view of the complexity of processes, the best way forward is to encourage the community at large to develop integrated, cross-disciplinary proposals that address key questions pertaining to the spatial and temporal complexity of significant processes. The drilling strategies will vary depending on the mission objectives, e.g. drilling long

sections of the lower crust in different tectonic windows versus POGO drilling in order to understand the lateral variability in crustal architecture at the scale of a ridge segment.

Tractability: Because oceanic crust and the shallow mantle are both vast and highly variable, we must use geophysical methods in order to characterize structure over broad areas. The results of drilling provide vital observations that ground truth inferences made from geophysical data. These ground truth results are necessary so that regional seismic data can be interpreted with confidence.

Posted by Gail Christeson on 2009-02-18 at 15:32

This is very well-stated. Processes of crustal accretion vary significantly at slow-spreading and fast-spreading crust, but the resulting seismic structure of older crust is indistinguishable. How do we map seismic structure and geologic stratigraphy? Is it hopeless, even though we all attempt to do so? If drilling can address this question, then we would be able to properly interpret existing seismic data sets. Ground truth data via drilling is essential.

Posted by Robert Harris on 2009-02-19 at 17:36

I agree that improving our understanding of crustal processes will require more basement drilling. Not only should we seek to understand the spectrum of processes along ridge axes, but I think understanding how the crust evolves once formed is also important. Understanding the evolution of oceanic crust might be approached through a series of multidisciplinary site surveys and basement holes along a tectonic flow line.

Posted by Anthony Koppers on 2009-03-06 at 22:45

Drilling "mature" ocean crust is as important as drilling young crust! The idea of drilling a series of basement holes along a tectonic flow line is a very interesting approach that allows us to study how oceanic crust matures and how heat flow changes over time. The geochemical evolution of the crust is similarly important, as we have very little knowledge of what exactly is being fed into the "subduction zone factory". Alteration of oceanic crust is very pervasive in mature oceanic crust and runs very deep as legacy Site 801C has shown in Jurassic-aged ocean crust in the West Pacific. The same site also has shown us that the structure of this rather mature piece of oceanic crust is very complex with alkalic off-ridge basaltic flows are covering up the "real" oceanic crust, as suggested by the ~7 Myr radiometric age difference. This may just be a regional coincidence, so to understand the structure of the oceanic crust we should not put all our eggs in one basket and drill only one MOHO hole. Instead we should drill multiple sites, whereby the systematic approach suggested by Rob seems a logical first step.

Posted by Nick Hayman on 2009-03-09 at 14:43

Lets make sure to keep things somewhat separate here. Drilling a MoHole as outlined by Doug is an important project in its own right. But drilling a >3 km deep hole is likely to be a one-time experiment that - if accompanied by good seismic etc... - will be a benchmark for seismology and ocean crust-mantle petrology. In contrast, shallower holes along a flow line could complete the picture of crustal aging that we infer from the disparate 801C and 504B+1256D. But these are much different types of experiments with

different goals than drilling young ocean crust, which has never been successfully done to any reasonable depths (i.e. >100 m).

Posted by Anthony Koppers on 2009-03-09 at 15:40

I agree that they are different experiments. Drilling the MoHole is of course integrated into this idea as it would define the starting point of this tectonic flow line in young ocean crust. For the older points along this flow line, I also wouldn't think we need to have excessively deep drill holes. Getting the time-integrated picture of the crustal aging and the changing heat-flow and fluid regimes is much more important.

Posted by Nick Hayman on 2009-02-24 at 18:03

I am in complete agreement with Doug's synopsis but wish to color it with my personal, geological perspective.

These are hard problems, and thus subject to community fatigue, as reflected in some of the comments about "21st Mohole" etc... But as nicely articulated by petrologists such as John MacLennan and Laurence Coogan, the thermal structure of the crust, and hence the shape and nature of the subsurface biosphere and seawater-rock chemical environment, are largely governed by these accretionary processes. Somehow we have to continue to strengthen and convey that connection to the broader community.

The questions I think are most crucial, that can be placed in a hypothesis-testing framework as need be, are:

- Is the lower crust accreted as sills?
- Is there significant heat-input into off-axis areas?
- Is there a fundamental dependence between spreading rate and crustal thickness?
- What's the role of mechanical strain in providing fluid (and melt) pathways?
- From what pressures are the major components of the crust magmatically/tectonically exhumed from?

Again, every one of these (or others I am probably overlooking) can and should be placed back into the context of the Earth system.

Many, if not all of these, should be developed with multiple approaches. ROV/AUV/HOV work, seismic, potential field data, etc... But drilling gives the most controlled experiment, and can provide some of the only *in situ* data (heat flow, permeability, and composition of relatively unstable minerals such as anhydrite etc..). In this vein, I'm not sure the 'existential' case for hard-rock drilling has been made recently, and I hope will be in this on-line forum and in other white papers and workshops.

Part of making that case is considering whether or not the 4 deep holes in ocean crust were successes. As Doug notes, IODP 1256D gives an opportunity to advance in this a bit because it just now has reached areas of good recovery, and lithologic diversity. The data from the sheeted dike and lava sections is forthcoming (some new observations, other data confirming previous ideas). These data will be transformative — however —

when compared to the underlying lower crustal section. Also as Doug hints, slow- and slower spreading center drilling targets obviously need to be developed in a problem-specific framework. In this sense, the controversial 1309D — billed as a mantle-peridotite targeted hole — has actually been quite successful at solving mysteries (and adding new ones) about the geophysical and geochemical structure of oceanic core complexes.

Note that each of these holes provided kilometers of core for petrologists, geochemists, rock-physicists, and structural geologists to study (split up into sometime organizationally challenged sample collections), and then integrate with large logging data sets. All of this is on incredibly (!) small post-cruise research budgets. Thus, there is quite a lag time until a drilling project on these topics begins to have an impact. I think all of these aspects are coloring how we view these 2 projects, and the two previous ones (735B and 504B).

Despite this 'passive-aggressive' cheer-leading for the deep off-axis holes, as Gail C. points out, we will always be a bit crippled until we can drill into young ocean crust. The geologic record and 'static' geophysical structure of the ocean crust is still where we can test hypotheses (i.e. 1256D), but the linking of dynamic hydrothermal, biological, and geological processes IS the 21st century frontier, if we choose to travel there. As I look at samples from deformed and hydrothermally altered ocean crust (some samples by subs, some by drilling), I always return to the numerical and analytical models of the dynamic processes, and observed processes on active ridges. At some point the two perspectives must meet by drilling closer to the active systems.

Posted by Emilie Hooft on 2009-02-25 at 18:23

Drilling young oceanic crust to gain an *in situ* understanding of the linking of dynamic hydrothermal, biological and geological processes is a strong goal for IODP. More broadly speaking, crustal drilling provides the essential observations with which we can interpret geological and geophysical data. The significant variability in magmatic, tectonic, hydrothermal and biologic processes at slow spreading rates is a fundamental characteristic of slow spreading rates. Our understanding of these processes and their temporal evolution is very limited, we need direct sampling to correctly interpret geophysical observations from these environments and answer the kinds of basic questions that Nick brings up.

Posted by James Natland on 2009-03-02 at 18:20

Part 1.

This will necessarily be in two parts. I see lots of wishful thinking in many of the letters to this forum.

The present program is much like that of the past. But the past and present cannot be the key to the future. There are too many things to do, and too many things that are easier to do than drill a deep hole (let alone several of them) in the ocean crust. This is why about 50% of all the drilling that has been done since about Leg 64 has used hydraulic piston coring and has been devoted to ocean history and paleoclimate. Drilling deep holes in the

ocean crust has been proposed for more than 40 years, but with all the things to do (easier things, mainly), and the ship (now ships) needing to get from here to there and everywhere, I suspect that everything to do with drilling in the ocean crust might, perhaps, be allowed one or two legs on one or another ship per year - IF the ships were drilling all year long. But even "everything to do" just in the ocean crust is too much competition for that little amount of time. A choice has to be made. If the past is any guide, there will be no significant progress toward Mohole in the next ten or probably twenty years. The status quo is just that. What, then, becomes the framework for making a choice, if this is what we really want to do?

To do deep crustal drilling well, in fact, to do it at all, requires persistence, continuity, and a phased program that is regularly applied, building on prior experience, and not allowing anyone to have to learn how to do it all over again. A certain frequency of attempt is advisable. We have never gotten close to the proper frequency. Some thought and investment must be given to developing equipment. Nothing seriously has been done since high-speed diamond drilling was set aside a decade ago. Some opportunity must be provided to drill at optimum targets, those easy to drill at the right time of year in the best weather windows; drilling at 23N in the North Atlantic over Christmas or in hurricane season just to give high-latitude paleoclimate its best shot is not optimal. The best approach is an industry one where milestones are targeted, with the next one not being approved before the previous one is reached and evaluated. This cannot be managed by a scientific planning structure that turns itself over every two or three years and has virtually no corporate memory, let alone the necessary expertise right at the top; nor by a proposal process that requires an entirely new vetting, from scratch, with each and every stage of the drilling; and by site survey requirements that become a higher priority (a separate unconnected approval process) in and of themselves than the drilling. That's why it took ten years to return to ODP 735B (and that, a matter for both consternation and embarrassment, was a few months before the site survey). All of this, repeat, all of this, has to change.

Posted by James Natland on 2009-03-02 at 18:22

Part 2

So if, in the welter of proposals and wish lists that will hit INVEST, this one receives enough of a voice of approval that it gets put forward into a long-range plan, then perhaps some stipulations should be made: 1) Creation of a permanent (well, for the duration) deep crustal planning group, fully equivalent to any of the old planning panels, that will vet proposals, guide the program, set milestones, and evaluate results, for just this one thing; 2) creation by this group, or sub-committees set up by this group, of a TEN YEAR PLAN, minimum, maybe longer, that will weigh in the balance what needs to be done to best advance the scientific objective of deep drilling in the ocean crust at any and all locations, determine priorities, and design the program; 3) this will depend on technology, so specify that early on, we will learn how to drill holes at least 3 km deep routinely (one needs to be done or completed using open holes right off the bat); 4) a portion of this planning group must be dedicated to understanding technological issues and make recommendations based on experience and anticipated requirements (riser, etc.); 5) guarantee doing at least one leg per year just for this one objective, and not let

LIPS, shallow hydrology with CORKS, zero-age drilling on ridges, island arcs or plume traces compete with this time.

When I really get down about this, I tend to think that an entirely separate crustal drilling program should be proposed. maybe to make up the few months of time that we now seem to be trying to get industry to take over. Why not write a proposal for a separate program, then, that could get two legs per year (or more) guaranteed, and set up a full-fledged and dedicated planning process? The old framework of scientific planning, the proposals, the workshops, the planning groups, the panels ... all of it, has proven incapable of even getting close to this. It is well past time for a sea change.

By the way, I still feel that drilling into and past Moho in any situation, single-hole or offset-sections, has to be THE major priority, THE most important project, in ALL of crustal drilling. I can quote the proceedings of countless workshops on this subject.

With all best wishes and good intentions,

-Jim

Posted by Kevin Brown on 2009-02-20 at 18:20

I hope IODP is not just a MOHO program. Very last millennia. How about something that requires some new methods and techniques to be developed. I am all for looking at active tectonic processes. I am particularly interested promoting IODP based observatory sciences along the continental margins. Looking at the nature and origins of slow earthquakes, seismic tremor, and partially coupled seismogenic faults would be one topic. We need a program to complement the Nantroseize program that cover the rest of the spectrum of subduction zone dynamic responses which we are just beginning to find are very complex and exciting. To do this we need an well developed observatory component which has almost fallen by the wayside in the current system.

Posted by Greg Moore on 2009-02-21 at 15:21

Although I do strongly support the "21st Century MOHO" program, I also agree with Kevin that we really need to get back on track with observatories, especially for seismogenic zone studies. Observatories have indeed almost fallen by the wayside because they are so expensive, but if we want to make advances in active tectonics, the best way is through instrumenting the seismogenic zone in several locations. See additional comments below under SEIZE discussion.

Posted by Andy Fisher on 2009-02-23 at 20:24

No one ever suggested that IODP is or should be "just a MOHO program." This topic has been around for a while because it remains critically important and has not been resolved (or even attempted in a serious way).

Posted by Henry Dick on 2009-02-23 at 12:06

Years ago we had a workshop at TAMU on how to drill in difficult places, and the answer was "find easy places to drill". Not a silly answer at all, because we have found easy places to drill - in the tectonic windows. Typical recoveries in intact basaltic crust

were 10-15%, once spudded into massive gabbro, recoveries are 85% and drilling rates exceptional. In other words we can do a lot. Yet this has never been well-exploited. In order to understand a diverse and heterogenous lower ocean crust, we will have to drill in a number of places at core complexes that expose this diversity. Some core complexes reflect magmatically robust crust, such as Atlantis Bank where there is a 400 km² gabbro massif at least a kilometer thick, or the Kane Megamullion in the Atlantic where large areas of the crust consisted of mantle peridotite directly overlain by dikes and gabbros, with only local magmatic centers where something like a "Penrose" stratigraphy exists. Since these are easy to drill, it should be relatively easy to assemble a suite of holes at seismically appropriate scale (>500 m) that will allow seismologists to test their ability to model their results to obtain real crustal architecture, and then to use seismology to understand the 3-D structure of the crust and the nature of Moho and how it varies.

Toomey also makes an important point. A lot of magmatic activity/accretion of the lower crust may happen off-axis, and drilling core complexes will only tell us about on-axis processes, as that is where the core complexes form. This is also true for the state of alteration of the crust, which is likely quite different for intact lower ocean crust that has cooled slowly, rather than being exposed on a rift valley wall by detachment faulting. This is why a single hole in the Pacific does not do the job either for fast or slow spread ocean crust - it is likely that off-axis processes are quite variable depending on the specific tectonic setting and whether the ridge axis actually coincides with the axis of mantle upwelling. Looking at Oman, some 20-40% of the lower crust may have accreted off-axis there in the form of wherlitic intrusions, which we do not find in dredge hauls or dive collections from core complexes, rift valley walls, or fracture zones, all of which represent solely on-axis accretion. Thus we will have to move beyond core complexes at slow and ultraslow ridges as well, and this will require full penetration of the crust beyond the Pacific.

Posted by Nick Hayman on 2009-02-24 at 20:15

One thing I've thought of before - and I'd be curious if this resonates at all - is to determine the strength (both static and dynamic) of faults in ocean crust. The current brand of numerical model that is so influential (for good reasons) adjusts crustal strength as a function of cohesion because this offers numerical (relative) simplicity. However, we are really only guessing at what the rheology of transform boundaries are, and have never really tried to understand the strength and frictional properties of faults in subaxial and abyssal hill environments. The latter, of course, tie directly into suggestions of focuses off-axis hydrothermal activity. But I think there is a larger question about the basic 'frictional' structure of the ocean crust that even affects intraslab seismicity in subduction zones, for example. A "SAFOD in ocean crust" could be a compelling project given the success of fault zone drilling in other environments.

Posted by Kevin Brown on 2009-02-25 at 15:59

Sorry all I was in the wrong session there for a moment. Well the structure (geologic, topographic including the presence or absence of seamounts, thermal etc.) of the incoming slab probably can impact the response at subduction zones and faults in oceanic crust generally do have linkages to plate driving forces. Observatories in active structures in oceanic crust might have some relevance. The emplacement of crust at slow spreading

ridges may be largely done through active faulting..... They make for tough drilling environments though and the choice of site could be very difficult.

Posted by Peter Kelemen on 2009-03-03 at 00:12

Hello everyone, I've just posted something on the Emerging Fields and Cross-Cutting Discipline Topic, in the sub-topic on CO₂ mitigation, that I think has a lot of synergy with traditional ODP goals related to the oceanic crust and shallow mantle. This is the idea of CO₂ capture and storage (CCS) via accelerated carbonation of mantle peridotite, as a relatively rapid route to solid storage. There is some pretty specific IODP research one would consider in this context, involving drilling in shallow seafloor through sediment into peridotite, for example offshore of Oman, New Caledonia, and/or Papua New Guinea.

There would be a lot of synergy with traditional ODP oceanic crust and upper mantle goals, in the sense that lots of people are interested in the spatial scale of chemical variability in peridotite, the tectonic processes by which mantle peridotite is exposed on shallow seafloor, and the deformation mechanisms which these rocks record. This method for CCS would NOT be effective in the deep ocean, since deep water is not in CO₂ exchange equilibrium with the atmosphere, and sucking CO₂ out of deep ocean water would not have any effect on the atmosphere for hundreds of years. There is still a lot to be learned by drilling tectonically exposed peridotite in the deep ocean to study ongoing carbonation processes there. Alternatively, however, I'd like to encourage the traditional ODP oceanic crust and mantle community to consider what they can do in shallow water.

Interested readers can go to

<http://www.ldeo.columbia.edu/files/uploaded/file/2008%20Kelemen%20&%20Matter%20PNAS+.pdf>

to see what we are thinking about. With regard to seafloor science, Figure 8 in that paper is particularly relevant. What's needed is characterization of present day alteration, fluid flow, reaction rates, porosity, and permeability in 500 meter to km-scale holes, and then hole-to-hole experiments involving hydrofracture and - perhaps - pre-heating a rock volume at depth and then - perhaps - injecting pure CO₂. Each of these steps would be undertaken in order to "jump start" hydrothermal systems like Lost City, and - if necessary - accelerate them. The idea is to enhance the natural peridotite carbonation system, doing as little as possible while consuming as much CO₂ as possible. Site survey requirements, particularly assessment of depth from seafloor to peridotite, and nature of overlying sediment, are obvious and do-able. In some cases, oil companies and others may already have obtained such data.

Posted by Susanne Straub on 2009-02-26 at 12:08

The structure of the oceanic crust and the LIPs are key topics, that have already been emphasized in the ISP. But I would like to stress again the importance of drilling in getting the evolution of other settings.

For example, how oceanic crust, and specifically the igneous crust, ages thru its life can

only be addressed by drilling. Are we really sure that we know what goes into the trenches all over the world?

To the other volcanic arcs, provide key information on how the oceanic crust is processed prior to its return in the mantle. Ocean drilling is vital in addressing the question of arc evolution - this has been shown in the DSDP and OPD program - by the tephra record embedded in the marine sediments. Combining the marine record with information from landbased studies is essential to get a full picture of arc evolution, and thus understanding the role of arcs in solid earth cycling. Present results from the IBM show that arcs are not 'static', but mobile and dynamic system that respond to change in input and tectonism.

Moreover, the tephra record in the sediments provides a direct link between (arc) volcanism and the Earth surface cycles. Such records at timescales relevant to the evolution of volcanic setting (tens of million years) we need to gauge the impact of volcanism on the climate evolution of the Cenozoic, that has been often proposed, but difficult to quantify.

A2.3 Large igneous provinces and volcanic chains

Posted by Organizing Committee on 2009-02-19 at 11:22

This discussion has been moved from Research Topics.

Posted by Anthony Koppers on 2009-02-09 at 16:27

Drilling is a hugely important tool if we want to better understand the geodynamic character of the Earth and its mantle. Large Igneous Provinces (LIPs) and seamount trails originate by processes linked to a (vigorously) convecting mantle, yet there are many outstanding questions about the origin, timing and duration of the emplacement of LIPs and seamounts. Within the context of the classical mantle plume model, the proposed link of LIPs (plume-head) with hotspot volcanism and the formation of age progressive seamount trails (plume-tail) is still an unresolved paradigm and heavily-debated issue in Earth sciences, yet proving or disproving this link has important implications for heat and mass transfer in the mantle, the time and length scales over which geochemically distinct mantle domains can exist, and mantle geodynamics in general.

There are many compelling issues in researching this so-called hotspot life-cycle that can only be addressed by future ocean drilling. Are all mantle plumes fixed? If not, how and where did they move? Can we establish unequivocal links between LIPs and seamount trails? What are the impacts of large-volume and rapid LIP emplacement on the ocean and Earth environment? What does the LIP/seamount volcanic record tell us about the mantle convection regime, true polar wander, different mantle domains and geochemical mantle inhomogeneities? How is the formation of near-ridge LIP and seamount trail formation influenced by oceanic spreading? Can we corroborate the results of the latest mantle convection simulations (e.g. Davies & Davies, 2009 in EPSL) that show mantle plumes of various shapes and behaving quite differently over time (e.g. merging, splitting, pulsating, dying off)?

I believe it is critical to expand the LIP theme and pair it with a similar focus on seamount drilling, as both research targets will provide us with important new data to address the above geodynamic issues.

Posted by Clive Neal on 2009-02-17 at 06:26

I would like to try and stimulate some discussion on this topic. These are all great questions, but how can scientific ocean drilling be used to address them? For example, establishing a link between LIPs and hotspot trails would be difficult, in my mind, because the plume theory posits they should be fundamentally different. Alternative models suggest there is no link and some LIPs have no hotspot trail. Establishing a link between LIP formation and environmental change is one where I see a lot of potential for ocean drilling to make a lot of headway through drilling of syn-LIP sediments at carefully selected sites. But how can scientific ocean drilling address the fundamental issue of how LIPs and hotspots form?

Posted by Anthony Koppers on 2009-02-25 at 13:36

Drilling to gain new observations linking LIPS with environmental/climate changes is an obvious goal that future drilling needs to address, that is for sure! In particular the sedimentary basins surrounding LIPS are good places to tackle this goal. A few drill holes in these basin sites will do wonders when combined with some control sites in the LIPS.

Linking LIPS and seamount trails together is an important unsolved question in Earth system sciences as well. Even if we show that these links don't exist, we will be able to draw important conclusions about the Earth convectional regime and we put strong constraints on how LIPS might have been formed. For example, the popular plume-head-plume-tail model in that case will be hard to maintain. However, if we show that some can be conclusively linked, we have validated an important but largely unproven hypothesis that has been around for many decades, showing that indeed plumes with large plume heads may exist in the Earth's mantle. This may require a concerted effort between (1) land work on continental flood basalts (e.g. Parana, Etendeka, Deccan, Caribbean) maybe involving the Continental Drilling Program, (2) an effort to drill the shallow-water continental shelf regions where LIPS are transiting into seamount trails (e.g. Etendeka) and (3) to drill the associated seamount trail (e.g. Walvis Ridge). In all this work it is critical to collect high-precision paleomagnetic data to get the paleolatitudes during formation of the LIPS and seamounts (or better yet we should aim for complete paleopoles if the next drilling program supports fully-oriented coring, in my eyes a major technical demand for future drilling). Combined with state-of-the-art geochronology and geochemistry, we would have sufficient information to prove or disprove these links.

Interestingly, this kind of data also will allow us to address questions about the motion/fixity of hotspots, true polar wander and mantle convectional regimes in general. However, it can only be done by drilling at least 300 to 500 m into the volcanic basement of seamounts and LIPS as we need to collect as many individual successions of volcanic (cooling) units as possible in order to cancel out secular variation. This is something that

cannot be done through dredging (i.e. unoriented samples) or any other kind of sampling of these features. Dredging though would be important to supplement these data sets with more geochronology and geochemistry from adjacent seamounts.

Posted by Bob Duncan on 2009-03-01 at 16:06

I'd like to expand a bit on the importance of understanding environmental effects/impacts of LIPs through ocean drilling. At the moment, the links between LIP events and rapid environmental change are tenuous but intriguing, based upon often only broadly coincidental timing and plausible but speculative mechanisms. The rapid environmental changes are important from the viewpoint of (1) mass extinctions and recovery of biosphere (and the notion of "punctuated evolution"), and (2) understanding response times, leads and lags, and recovery of the climate system (vis anthropogenically-driven impacts today). The best records of these rapid change events (ocean anoxic events, mass extinctions, thermal maxima, etc) are in marine sedimentary sections, accessible through ocean drilling; understanding the Cretaceous events requires sampling in the Pacific ocean especially. The LIP-impact models (e.g., Ontong Java Plateau-OAE1a; Caribbean Plateau-OAE2) require an array of sampling sites to document signals of effluent gradients (trace metals, Nd-, Pb-, Os-isotopic excursions) that co-vary with indicators of ocean chemistry change (e.g., C-isotopic excursions) and biology response (fossil assemblages, shell wall thickness, etc). Sampling of the LIP crustal rocks is needed to match and confirm the source of the chemical signals documented in sediments. Robust signals of submarine magmatic activity recorded in proximal sedimentary sections may also be the best way to determine the timing and duration of oceanic LIPs, which is a goal of geodynamic research, and very difficult to achieve through direct drilling and geochronological methods. Such a program requires an integrated (multi-disciplinary), global approach (sampling of a particular environmental event at "near-field" and "far-field" sites).

Posted by Anthony Koppers on 2009-03-02 at 14:19

Understanding the environmental effects of LIP emplacements (and associated peaks in hotspot volcanism) is very intriguing indeed! Drilling systematically and globally in the relevant syn-LIP and near-field sediment basins is an excellent and straightforward approach, one that definitely should be picked up by IODP and its successor program. I like to stress the point that Bob Duncan makes about placing "control drill sites" in the LIPs themselves and any other potential source of contemporaneous volcanism in the neighborhood. They are essential in deciphering the fullest possible range of geochemical variability in the LIP so that they can be readily used in comparison to the volcanoclastic sediments drilled. LIP drill sites are also needed to decipher the structural evolution of a particular LIP both through space and time. Determining across-LIP age systematics and progressions will be very important in understanding LIP construction, for putting LIP in a plate motion reference frame possibly linking them to co-genetic seamount trails, for linking LIP to tectonic setting and for reconstructing the break up of a LIP (e.g. the splitting of the greater OTJ into three volcanic provinces: OTJ proper, Manihiki and Hikirangi). These time-space relationships will directly influence how to interpret and choose drill sites in the the syn-LIP and near-field sediment basins.

Posted by Richard Ernst on 2009-02-26 at 16:01

I would like to offer a broader perspective in support of continued drilling of oceanic LIPs from my perspective as co-leader of the LIPs Commission (www.largeigneousprovinces.org) and organizer of the "LIP of the Month" feature and also from my research focus on Proterozoic and Archean LIPs.

1) First point: The young continental LIP record is far better understood than the oceanic LIP record because it is more accessible. AND YET, even in such well-studied flood basalts such as the NAIP, Karoo and Siberian Traps, there is capacity for dramatic new discoveries. For instance, in a recent series of papers (Svensen et al. 2008 EPSL and references therein; see also <http://www.largeigneousprovinces.org/08aug.html>) the Norwegian group has shown that the NAIP, Karoo and Siberian Trap LIPs each contain hundreds to thousands of hydrothermal vent complexes that are generated by dolerite sill emplacement at depths of up to 8 km in fluid-rich sedimentary rocks. Depending on the host rock type (shales, evaporites, carbonates) massive amounts of greenhouse gas, and life-threatening gases can be generated and released from host sediments into the atmosphere by this mechanism-- and, as argued by these authors, this represents the main method by which LIP events can cause global extinctions. Surely similar process must be associated with oceanic LIPs -- there must be hundreds to thousands of vent complex remaining to be discovered on the ocean floor surrounding oceanic LIPs (caused by sill injection into sedimentary sequences) and if found, these would be a significant contributor to climatic consequences due to emplacement of oceanic LIPs. Ocean drilling would have a role in finding and characterizing hydrothermal vent complexes associated with

oceanic LIPs.

2) Second Point: We have made progress in understanding continental LIPs in 3D because we not only have the young flood basalt dominated LIPs, but also Proterozoic LIPs in which the flood basalts are mostly removed thus exposing the underlying plumbing system of dyke swarms, sill provinces and layered intrusions. However despite the inferred equal rate of production of oceanic LIPs, they are much more poorly understood than continental LIPs – they survive ocean closure poorly and so they are difficult to recognize in the pre-Mesozoic record – although they are probably significant in the Archean greenstone belt record. Again I suggest that continued oceanic drilling of oceanic LIPs is essential if we are to improve our understanding of the architecture and plumbing system of this major magmatic class (oceanic LIPs), and to allow comparison with the better understood, but still capable-of-surprising continental LIP record.

Sincerely, Richard Ernst (co-leader of the LIPs Commission,
www.largeigneousprovinces.org)

Posted by John O'Connor on 2009-03-02 at 08:40

Establishing the timing and duration of LIP events can address important environmental and geodynamic questions. This is difficult to achieve by direct drilling alone so we need to drill also the proximal sedimentary sections that record the timing and duration of oceanic LIP formation. I would like to add that testing for a link between LIP and seamount trail formation is another approach to establishing whether LIPs reflect a short-lived event(s) or long-lived multiple events. For example, the 'plume-head-plume-tail' model predicts that continental flood basalts (e.g. Parana, Etendeka, Deccan) form in conjunction with continental rifting followed by the rapid onset of hotspot trail formation. Alternative models such as an 'incubating' or hybrid 'thermo-chemical' hotspot plumes

make no such prediction. So if a simple linear age-progression links CFBs and their hotspot trails then this would support to the notion of a plume-head-plume-tail'. But if a more complex age distribution emerges suggesting long-lived, widespread, episodic volcanism then this would point to 'pulsing' or 'incubation. I would like to suggest by way of this example that we emphasize that testing for a link between LIPs and environmental change will require the drilling of many and varied targets that can only be achieved by the continuation of the program beyond 2013.

Posted by Anthony Koppers on 2009-03-02 at 13:21

I concur as I view LIPS and seamounts as a single entity that gives us a long-term geodynamic perspective on how deep mantle features such as thermo-chemical mantle plumes have been developing and how they provide insights in the overall global convective regime. They are intrinsically linked to the "plume-head-plume-tail-model" that as of yet has to be ground-truthed. However, the combined LIPS-seamount system also allows us to provide constraints on the possibility of moving, pulsing, merging and tilting mantle plumes, and special features such as Superplumes, as all these have been advocated in the literature and as these are now becoming commonplace in numerical mantle convection modeling and tomography studies. However, these models and simulations ask for corroborations and drilling provides an important tool to do so. A systematic approach is needed as both LIPS and seamount trails come in different types. In a similar fashion as studies on fast vs. slow spreading centers, seamount trails form in different ways and maybe even by different processes, but with the difference that we know very little about the underlying intra-plate processes.

Posted by Cornelia Class on 2009-03-03 at 12:22

I think that the topic should be changed from "Large Igneous Provinces" to "Large Igneous Provinces and Seamounts" as has been emphasized in many of the postings. My comment in particular emphasizes the importance of drilling of seamounts. Based on the assumption that hotspots are caused by deep mantle upwellings (plumes), oceanic basalts related to mantle plumes have been considered a primary source of information on the size, composition and evolution of deep mantle regions. Noble gases in particular have a key role in mantle geodynamic models aiming to understand the long-term geodynamic evolution of the Earth's mantle. Nevertheless, many basic geodynamic questions about the mantle have not been solved yet, which is obvious from the large range of currently discussed mantle models, where the storage reservoir for primordial noble gases ranges from disseminated reservoirs in the mantle, to an isolated layer at the base of the mantle (lower mantle or deep layer or D" etc.) and even the core. Resolving this question is the prerequisite for the next step forward in understanding of the geodynamic evolution of the mantle. However, it's not easy. To this point noble gas studies have been limited to rocks from young oceanic islands and sparse data from LIPs, which is a poor sampling of the thousands of kilometers of hotspot tracks, which preserve a record of deep mantle source evolution and the interaction of mantle upwellings with the surrounding mantle. Part of the lack of noble gas data from dredged seamount samples is the lack of sufficiently fresh material to perform such analyses. The drill sites on the Hawaii-Emperor track have demonstrated that drilling can provide the sample quality that allows noble gas studies on older hotspot track samples, giving invaluable insights into the range of noble gas compositions in mantle plumes. But we need more of

that. In particular the Tristan-Gough plume system is the only LIP-hotspot track system that has $^{3}\text{He}/^{4}\text{He}$ ratios < MORB in volcanic rocks from its young islands. We need drill samples from the Tristan plume track to understand the significance of this signature in the mantle. We also need drill samples to understand the noble gas evolution of plume sources and their constraints on the storage of noble gases in the Earth. Combined with other geochemical data such studies will provide important constraints on the size of geochemical domains in plume sources and provide clues to their precursors and formation history.

Posted by William Sager on 2009-03-06 at 10:27

I am writing to concur broadly with other posters and to give my support to this avenue of research. Certainly, I think that studies of LIPs are important for a number of reasons including understanding mantle convection, volcanic processes, and the surface flux of magma, as well as environmental effects. I concur with Conny's comment that "LIP" should probably be interpreted broadly and include seamount chains. Like Conny, Anthony, and John, I am a proponent and supporter of investigating Walvis Ridge because it is a unique opportunity to examine the plume-head-to-plume-tail link. Of course, I also support the broad exploration of LIPs of various kinds because it is unclear whether there is one type (probably not) and we need to get the big picture.

I would also like to point out the evolution of LIP research, its current state, and the role of scientific drilling. To me, it seems the acme of interest in drilling LIPs (at least from outside the LIP community) seems to have occurred a decade or more ago when there was a simple model (the plume head) and obvious candidates (OJP and Kerguelen), but things didn't turn out so simply. To me, that is just the natural progression of science. Things look simple when you have few data and have not looked too closely. So complications turn up and the message seems a bit more muddled (nuanced, actually). Unfortunately, we LIP scientists have not done a good job of selling the next phases of research. But much of the blame also falls on the scientific community at large. The scientific drilling community clearly prefers problems that appear to have exciting implications (Supervolcanoes kill dinosaurs!) and have simple solutions (drill here - problem solved), but this is a bit unrealistic. Scientific drilling really needs to be a part of a larger, multidisciplinary effort to understand LIPs, including background geophysical studies and integration with studies of LIPs on land, just to mention a couple. We need to know better the structure of the interface that we are sampling because it certainly affects the answers we get. And we need to do a better job of explaining to others in the community why understanding these possibly complex features is important to the big picture of geoscience. We have no doubt about it, but we need to convey that forward.

Posted by Anthony Koppers on 2009-03-06 at 18:51

It always has surprised me that the Initial Science Plan (ISP) of IODP only focused on LIPS drilling, seemingly ignoring the study of seamount trails. LIPS are unique in that they produce vast amounts of volcanism in a relatively short time, basically providing a flash point of volcanic activity that is of great consequence, both geodynamically and environmentally. Seamounts on the other hand form over longer geological periods of time, sometimes up to ~ 100 Myr, providing information about the evolution of deep mantle source (e.g. plumes), the (absolute) motion of the tectonic plates they are formed

on and following ODP Leg 197 also the motion of hotspots/plumes. They thus provide important geodynamic constraints as detailed in the ongoing discussion in this form, yet of quite different character. I agree with Conny and like to urge the moderator to consider changing the topic from "Large Igneous Provinces" to "Large Igneous Provinces and Seamounts".

A2.4 Seismogenic zone and arc evolution

A2.4.1 Seismogenic zone

Posted by Kevin Johnson on 2009-02-11 at 14:36

The seismogenic zone is one of the initiatives in this theme, and is important from several perspectives. The impacts of earthquakes, tsunamis, and arc volcanoes make scientific understanding of the seismogenic zone a societal priority. Further, chemical cycling of materials initiated in subduction zones and passing through the seismogenic zone is one of the most fundamentally important processes shaping Earth's evolution. These cycles include, but are not limited to volatile elements, which directly affect the oceans and atmosphere, metals that become important ore deposits, and sediments, ocean and continental crust and lithosphere that become entrained in convecting mantle creating identifiable chemical reservoirs that can be geochemically tracked and are used to shape geodynamic thinking.

Oh, and I agree with everything Henry says about the MOHO.

Posted by Roland von Huene on 2009-02-19 at 22:42

Kevin Johnson has opened a topic very important to IODP that was a show piece in the IODP ISP so I am surprised at the sparse contributions to this discussion. He summarized the salient societal aspects both of hazards and mineral resources. These societal aspects are a link with public awareness of earthquake and tsunami hazards a topic the public can relate to. Understanding the basic the mechanics of earthquake nucleation has advanced from observations linking earthquake seismology with physical and petrological characteristics of subduction zones.

Should not seismogenic zone research remain in a future program? The urgency of mitigating damage and loss of life from great earthquakes and tsunamis has not abated since the beginning of IODP. It remains a significant aspect of scientific inquiry by drilling in a dynamic environment that is not accessible on shore. Since the promises of the IODP Initial Science Plan seem less realistic than a decade ago, seismogenic zone investigations with drilling can be refreshed and updated. Current financial reality makes a review of strategies to advance knowledge of earthquake mechanics with available resources appropriate. Although significant progress has been made in understanding subduction zone mechanics during the preparations for IODP seismogenic zone drilling, the targeted advances in understanding expected from drilling are still to be realized.

It can also be argued that riser drilling of a seismogenic zone requires more time than the

length of a drilling program. Drilling of the Nankai Trough will probably extend beyond the current program. The simpler CRISP drilling has yet to begin and the 3D seismic data required are not yet funded. Completion of these projects will extend beyond 2013.

Alternate strategies potentially provide benefits not realized during the planning phase of IODP. One of them is imaging with a massive 3D VSP. This survey technique has been applied in industry exploration and it may offer relatively economical data on active seismogenic plate interfaces. It provides significantly higher spatial resolution of the plate interface and it could provide optimal physical properties data (S waves) to guide coring and instrumenting a seismic zone. It will illuminate the plate interface patchwork of varying frictional behaviors over 35 to 100 km², a patchwork that has been rudimentarily observed in the Costa Rican subduction zone [ie Bilek et al, 2003, Geology, v. 31, p. 455-458].

Drilling the seismogenic zone requires more time than the duration of the present phase of IODP and new understanding, exemplified by the NanTroSeize 3D seismic images can provide the “gripping science” in an upgraded Initial Science Plan. It is a topic that non-scientists can relate to easily and it has great societal relevance.

Posted by Kevin Johnson on 2009-02-20 at 04:18

I agree with Roland Von Heune. This topic encompasses pretty much all the themes that are currently important to IODP, from basic science questions on earthquake mechanics, arc chemical fluxes, mantle hydration and carbonation, seismology, tsunami dynamics, chemical cycling in the mantle, volatile cycling into the oceans and atmosphere. I think it provides a self-contained scientific-social-political package that is hard to turn your back on in these times of increased accountability for sparse government and private funds. There are also commercial interests in these zones related to gas hydrates and very likely microbial ecosystems at work in locales such as serpentine mud volcanoes. The Planning committees should carefully consider the facets of this tectonic zone and develop priorities for drilling from now into the next phase, when societal implications will be more deeply scrutinized.

Posted by Greg Moore on 2009-02-21 at 15:30

I agree with both Kevin and Roland that SEIZE needs to stay in the next program. By 2013, we will have only finished Nankai (I hope) and will need to study other areas with different characteristics for comparison. As noted above by Kevin Brown, observatories will be essential for this endeavor. For instance, we need to establish a long-term record of pore-pressure variations, etc. during a seismic cycle in several areas in order to understand how the plate boundary system behaves before and after great earthquakes. If we can develop proper strain meters, long term records of strain will also help understand strain build-up prior to large quakes. As suggested by Kelin Wang at the recent MARGINS/SEIZE workshop, perhaps drilling and instrumenting several subduction zones in different stages of the seismic cycle would be a good approach, since we are unlikely to be able to follow a full cycle in any given location.

Posted by Roland von Huene on 2009-02-24 at 12:45

Greg has mentioned a very important point. Understanding the tectonic processes that produce great earthquakes is a long term goal that is advanced but not solved during a single scientific drilling program. New discoveries during each decade of ocean drilling investigations has helped redirect and optimize research approaches. Past interpretations of dynamic processes from seismic images and drilling have not included sufficient appreciation of the great variability in stress and strain during an earthquake cycle. An awareness of the earthquake cycle's significance to understanding seismogenesis is clarified by Kelin Wang and his colleagues. The structure in seismic images is the net permanent deformation over many earthquake cycles during each of which frictional properties change and cycle to some degree. Seldom is elastic deformation quantified nor is the strength of an asperity estimated during a single cycle. Elastic strain has been measured on shore with GPS observations but not yet at sea. Examining a part of the earthquake cycle with observatories at the Nankai margin will begin soon. The approach Wang has proposed to investigate multiple margins in various stages of the earthquake cycle is a promising approach that could be examined further at INVEST. Time series observations from down hole observatories coupled with existing and planned onshore instrument arrays is a possible direction for continuing seismogenic zone research that extends the progress made during IODP.

Posted by Nick Hayman on 2009-02-24 at 20:07

It's worth noting that the Kanto Asperity Project continues to gain momentum in Japan. In many senses, the vision for the KAP is very much an observatory, rather than coring-focused project. Focusing on areas around the Boso Peninsula (and possibly into Sagami Bay), the KAP will utilize Chikyu in a way that is very visible to the Japanese, and addressing their worst seismic hazard - the Tokyo Tosai-Genroku events. Tosai of course is too deep to address in the manner that Nankai is, but Genroku may be crustal, and associated with the structure of the accretionary prism (now non-accretionary, I suppose) off Boso.

Posted by Chris Goldfinger on 2009-02-24 at 20:42

I also support what Kelin noted. As large and expensive as explorations like Nankai are, we will still be a long way from understanding seismogenic zones when they are complete. These holes represent very important pinpricks, but pinpricks just the same. Drilling is at a major spatial disadvantage when addressing something as large and complex as a subduction zone because knowledge is high at those sites, but they are isolated and cover short temporal periods, even with observatories. Strategies that recognize the strengths and weaknesses of drilling will ultimately be more successful in advancing our knowledge. When the Sumatra earthquake occurred, a proposal was generated to address this new challenge. But it wasn't so clear at first what the strategy should be. It's still evolving, but is moving toward what Kelin suggests, a more integrated plan that addresses a different phase of the strain cycle than those areas now in progress.

In any case, I do believe that the seismogenic zone should remain a central theme in the next phase. To be successful, it has to be a long term effort in order to overcome the spatial and temporal difficulties inherent in the technology. Also better integration with

other aspects of subductology would go along way toward forgin the links with GPS, paleoseismology, seismology etc.

Posted by Kevin Brown on 2009-02-25 at 11:44

I have to agree although its probably not just differences in the seismic cycle that may be important (although I agree this is critical). There are likely to be other factors that contribute in very fundamental ways to the temporal and spatial distribution of earthquakes (where ever they are) as well as rupture propagation rates and the types of hazard they can generate.

Some types of phenomenon seem to apply to may fault zones but others may only really manifest themselves strongly in certain critical sub-groups of systems (i.e. there may be fundamental differences between Nankai/Cascadia and more complex Costa Rica/Japan trench systems). Is not well understood how heterogeneity in subduction fault characteristics (topography, mineralogy, temp/fluid pressure) relates to both initiation and subsequent propagation of damaging earthquakes (i.e. do they remain small and confined to a small critically region or propagate through non-critically stressed or relatively stable regions to grow large by incorporating numerous key instabilities). I strongly suspect it will not be simple once we get out of some of the "simple systems" (and is Sumatra even that simple in any case). Episodic creep may not just be limited to the deep portions of subduction systems. Its quite curious how some seemingly poorly geodetically/seismically coupled systems seem to be able to both release moment by seismic and non-seismic (i.e. continuous or episodic creep) processes. We could be dealing with dangerous systems that release >50% of their energy non-seismically, its just difficult to see with out key offshore evidence and sufficient time. If so then it does suggest that it is misleading to treat the seimogenic zone as a simple temp/depth bounded region. I have to wonder if we have to broaden what we mean by a "seismic cycle" (time between creep events and dangerous seismic events and presumably their interaction) and seismogenic zone in such systems. We could be dealing with very non-periodic cycles in some systems that are governed by quite non-linear processes that can dynamically jump ruptures through regions that might under many circumstances do not want to go unstable.

Posted by Sean Gulick on 2009-02-25 at 16:39

Glad to see all the contributions on the Seismogenic Zone topic. I would like first agree with much of the preceding posts in terms of importance of well characterizing a single subduction system and of need to study a variety of subduction zone settings in order to understand the parameters that may be important for the geohazards associated with a given margin. To that end, we can examine ranges of subduction zones in terms of "when" they are in the seismic cycle; however I concur with Kevin that the seismic cycle may be limiting as a model given the need to consider slow earthquakes and potential non-linear processes. This issue then might yield the need for a more complex model or set of models to aid our description of seismic hazards. I would suggest, therefore, that its important to examine ranges of subduction zones with differences in physical parameters that might ultimately effect hazard and therefore provide data to aid in broadening our understanding of the "seismic cycle". One such parameter that seems to be important is thickness of sediment on the incoming plate, and to that end it could be quite useful to compare a margin like Nankai with its 1-2 km of sediment in the "trench" to one like

Sumatra with its 5-6 km of sediment in the "trench". Probably preaching to the choir here, but it also may be important to examine the effect of differing thermal regimes, sediment lithologies, degree of lithification, amount of cementation, and margin structure on moderating the updip and along strike rupture extents and pathways. I think NanTroSEIZE, Kumano Basin area, is an important case study with one type of margin structure (megasplay) that will yield globally relevant results. As we move to new areas, its likely important to evaluate the findings from NanTroSEIZE and past subduction zone drilling and imaging efforts and consider what other margins should have resources invested in them to continue to improve our understanding subduction zone processes and the associated geohazards.

Posted by Casey Moore on 2009-02-25 at 12:21

This comment is general and related to the overall theme: I agree with virtually all the preceding responses regarding the societal relevance and scientific importance of investigating subduction zones. And, I support Nankai, Costa Rica, and the Kanto Asperity programs that have active groups striving for deep subduction drilling and instrumentation. Although I am a geologist, I believe some of the most important and difficult-to-achieve results will come from the instrumentation of the borehole through actively deforming areas.

The critical point that I would like to emphasize is the need for follow-through. Lets bring this program to some level of completion, in view of the both its scientific goals and the substantial previous investments. It would be short-sighted to wind down without significant deep holes and good instrumentation. Not only are the scientific results important but the lessons learned will be applicable to many other deep drilling and instrumentation programs.

Posted by Kevin Brown on 2009-02-25 at 15:35

Fewer and better is correct given the expense. How do we survey in future observatory and/or deep holes? We need to know we will measure something of interest, which means active and critical. How can we show a particular place on a fault is the key region? We don't have that many data points offshore in most places. Its not possible to spot a critical regions through seismic surveys alone although there maybe some places that seem a little easier to put a finger on than others from a variety of existing data (including seismicity and in a few places on shore geodetics). For some of the future dynamic plays in seismogenic regions we may have to approach things in increments starting with more limited endeavors (surface instrumentation, marine geodetics etc.) and scaling up to drilling deep holes. I do support completing things by the way so I am not saying we should stop Nantroseize its just that there needs to be a variety of methods applied particular when choosing future region for comparison. I am concerned that we will miss the key places to study if we don't keep looking and having some exploratory programs going is one way to try to find these key regions.

Posted by Mike Underwood on 2009-02-28 at 13:05

I don't have much spare time to chat about SEIZE because I'm up to my ears in project management for NanTroSEIZE, including preparation for Expedition #322. Having said that, I am 100% certain that our reasons for continuing (including Nankai, Costa Rica,

and Kanto) are compelling, and we are nowhere near finished. Nothing has changed in terms of the rationale. On the other hand, the operations time on Chikyu has been cut by ~50%, which means that it will take more than 2X longer to get any of these project started and finished. No one anticipated that brave new reality when we began planning for SEIZE more than 10 years ago. NanTroSEIZE will not be finished before 2013, not even close, and I have doubts about any of the others even getting started before 2013. The borehole observatories are going to be very complicated and time consuming. I agree completely with Casey, and many of the other threads, about the importance of the observatory science. This merger of basic science and technology will be "transformative", as they like to say in Washington. Moreover, the results will affect innumerable people's lives, unlike some of the long-held "pets" of solid Earth ocean drilling.

I will be aboard Chikyu during the INVEST meeting, but I know that Casey, Greg, Roland, and the others will be effective in delivering the group's message.

Posted by Eli Silver on 2009-03-02 at 16:29

This is a good discussion about the need for seismogenic zone research. My feeling is that the new program should have a major emphasis on geohazards, to demonstrate the critical contribution of the drilling program to the well-being of the global population. The seismogenic zone represents the sites of greatest geohazard from a variety of sources (earthquakes, tsunamis, volcanoes, landslides, etc) and thus should represent a central focus. It makes abundant sense to build on what has been started by a huge effort of IODP and supporting programs, such as the US MARGINS program and a number of other international programs. Drilling is just getting underway and the primary problems will not be solved by 2013. It is very likely that the primary targets will not have been drilled by that time. As we break new ground we learn a great deal about the process, and some of our initial targets will need to be altered based on new knowledge. Drilling into the seismogenic zone will be a first step in understanding the process of seismogenesis, and further studies will build on these results and the observatories established by them. Selling the new program under different packaging (such as Geohazards or other focus) is probably necessary to generate broader interest, and placing it in the context of a much broader program of observation is essential. However it is packaged, continuing this program to the point where we have a much clearer understanding of the processes involved will be essential.

Posted by Jon Lewis on 2009-03-02 at 21:40

I agree with the above posts and simply want to restate the need for follow-through from a slightly different perspective. The failure to make good on commitments to science goals, for example NanTroSEIZE, would set a very damaging precedence. It would undermine the credibility of important, albeit expensive, research at the very moment when our collective capabilities are getting us closer to drilling and monitoring targets that have enormous societal relevance.

Posted by Harold Tobin on 2009-03-06 at 11:40

There isn't too much to add to what has already been said, but I want to add my voice to the support for a couple of things:

1. Sustained effort to complete what we've started. The SAS and the program in general has so far lived up to its commitment to NanTroSEIZE as a long-term investment and Complex Drilling Project. The recent re-affirmation of that support by SPC in August 2008 was really heartening to those of us involved in steering that project. The next 2 years of NanTroSEIZE are going to be a real test -- we will be mainly drilling and preparing holes for long-term observatories, and the scientific payoff will mostly be down the road. I think that's a dangerous situation, and we are going to have to convince everyone from the SAS up to high levels of government involved that this sustained effort is worth it -- all at a time of enormous pressure. As a new program structure is developed (and I am pretty sure we will see changes) for post-2013, I really hope that corporate memory is strong.

Since this is a US-only discussion, I want to raise a point of perspective: Both the US and Japanese side have seen much less time invested in drilling than envisioned, and it is not only the Chikyu schedule that has slowed down our efforts. It may not be widely appreciated in the US that the fact that canceled JR expeditions at Nankai have not been re-scheduled is seen by some in Japan as a default on our (US) commitments. Of course, the budget/ship-time situation has changed massively, and I am not suggesting that the JR should be doing Nankai now instead of what is scheduled. But I wonder if that reality has been handled as diplomatically as it might have been?

2. Observatories! Real commitment to the future of ocean drilling science truly must include sustained effort in building borehole-based observatories. Our community has to speak loudly about this, since it is obviously not going to be as important to the paleoceanography group. This is critical for all the SEIZE initiative projects, as Casey pointed out, and also in many other areas of interest. When IODP was set up, there was a missed opportunity to make observatories part and parcel of an IODP project. We all know the funding constraints that emerged, but even before they were apparent, we set up a program that continued the focus on coring + logging as the primary mode of operation. As we have implemented NanTroSEIZE, we've seen many ways in which the structure at both the national level (e.g., staffing, post-expedition funding, non-shipboard scientist support), and at the IODP level also, is designed around the implicit assumption of ODP-like expeditions. There's going to be an opportunity to make this much more flexible and project-driven for sustained multi-year projects, and it's going to fall on us in the seismogenic zone community to push for that.

Posted by Kathie Marsaglia on 2009-02-27 at 15:18

Subduction initiation, a fundamental tectonic process, is poorly understood; this process might be best documented (timing and stratigraphic signature) through ocean drilling.

Posted by Kathie Marsaglia on 2009-02-27 at 15:23

Fifteen years ago I attended a GSA/JOI/USSAC sponsored Penrose conference on "The effects of triple junction interactions at convergent plate margins." In our report to JOI (Pavlis et al., 1994) we deemed studies of triple junction systems to be in their "infancy" and "a frontier area in tectonics research." The report highlighted several characteristic manifestations of triple junctions (kinematics, forearc magmatism, thermal/fluid

overprints, polyachronous basin evolution, and ophiolite emplacement) and where hypotheses concerning these topics could be tested by ocean drilling. Given the importance of migrating triple junctions on plate margin evolution, perhaps they should be revisited.

A2.4.2 Arc evolution

Posted by Susanne Straub on 2009-03-04 at 15:24

I am posting arc evolution as separate topic, as I felt that my two prior entries to that subject were misplaced in the Large Igneous Provinces.

I am copying my prior posts here, and deleting them from the LIP discussion.

See also entries under research topics.

19 February: I would like to add here that the geodynamic character of earth can be excellently studied through arcs that react sensitively to changes in the slab and mantle input. Arcs are active of tens of millions of years which is relevant to plate tectonic processes, and many arcs leave behind a unique, temporally highly resolved (1 Ma) record by means of their tephra deposited in marine sediments. That record can only be obtained thru drilling. Some hotspots also do have great tephra records (e.g. Canaries). While I have seen many theoretical models of arc evolution and arc impact on the environment, the actual testing can only be done, if one gets the data from ocean drilling.

24 February: It's not only evolution of Large Igneous Provinces that we can study thru drilling. It's the evolution of any volcanic setting - for example, ocean drilling has been and will be instrumental to understanding the 50 millions years of evolution of the Izu-Bonin-Mariana arc/backarc/trench system. None of the three basin volcano-tectonic settings operates independently - ocean drilling, in one or the other way, gives us the needed time frame (tens of millions of years) that may allow us for investigating potential links between these systems more closely. Moreover, having time, we can try to search for causal links with the cycles on the Earth's surface.

While we might still have a lot of data needed for such approaches already sitting (either distributed thru the literature, or as samples in the repositories), new drilling will be essential to fill the still existing gaps in the rock record.

A3 ENVIRONMENTAL CHANGE, PROCESSES, AND EFFECTS

A3.0 Introduction and relevance to IPCC and P2C2

Message from Organizing Committee:

Welcome to the discussion forum on Environmental Change, Processes and Effects. In the Initial Science Plan of the current IODP program, "Environmental Change, Processes, and Effects" is a central theme. This theme focuses on internal and external forcing of climate change on all time scales. It describes two initiatives, one in Extreme Climates and the other in Rapid Climate Change. The goal of this discussion forum is to explore and start to define the research initiative and goals of the program, beyond 2013. To start, please click on 'Initial Questions' (below). You can either respond directly to the Initial Questions and start a new discussion thread, or read what other participants have contributed and respond to their comments.

Initial Questions

Posted by Organizing Committee on 2009-01-12 at 14:18

Should the next phase of IODP (2013-2023) focus on similar issues and initiatives? If so, why? If not, what would be a better strategy and approach to promote the most compelling research in Environmental Change? In other words, what is the most compelling unresolved issue in Environmental Change that can only be solved with scientific ocean drilling?

These questions have been left open-ended on purpose. To respond, you can start a new thread by hitting 'reply' to this message. If others want to comment on your idea, then can 'reply' to your post. If they don't, they can start a different thread by hitting 'reply' to this message.

Posted by Katharina Billups on 2009-02-11 at 08:55

It seems to me that the NSF P2C2 document has identified several research areas that are currently given funding priority. Many of these research areas can be addressed specifically by scientific drilling. For example, the question of the sensitivity of ice and sea level to rapid climate change, especially during intervals of relative global warmth, could be addressed by drilling. Perhaps the science plan can build on how drilling would help meet the objectives already outlined by NSF.

Posted by Ingrid Hendy on 2009-02-11 at 09:05

Back out of deep time (excuse the personal bias) and following the IPCC ice sheet questions: Is the present response of the Greenland Ice Sheet and Antarctic Ice Sheets unprecedented?

Posted by Yair Rosenthal on 2009-03-03 the 21:42

I am getting late into the discussion and would agree with many of the suggestions. To some extent this discussion reminds me some of the meeting we had in planning the new ASH now called P2C2. Many of the suggestion reflected the interests of the proposers,

which were often consistent with some of the program goals but also limited to specific areas closed to the proposer heart / interests. The drilling should be guided by the science questions. I am in favor of following the P2C2 solution i.e., an emphasis on mechanisms of climate change which are critical for understanding with relation to present and future change. Depending on the questions we should thrive to find the right site that have both the appropriate resolution, geographical extent and needed preservation conditions, which is critical for deep time studies. Given time and budget constraints we should identify which new records would have the greatest impact. Sea level reconstruction, and tropical SST record are at the very basis of our needs for any climate reconstruction and after many years we are still arguing in a big way on these records at certain critical periods. We know what are the largest uncertainties in our predictions (e.g., climate sensitivity, ice sheet melting) and we should devise ways to generate these information much in the same way stratigraphers worked out the Cenozoic record. We know enough now how to do it and we should try to finally get trustworthy records of these critical parameters.

Posted by Alan Mix on 2009-03-05 at 16:16

On the ultra-high-res, P2C2 style stuff, I agree (with many posts) that there is much more to be done in this business. I hope we don't become slaves to societal relevance, however, as that is a slippery slope. At one point there was the "Holocene" initiative, because that was societally relevant. So I submitted a Holocene proposal. And a reviewer responded that only the last 2000 years of the Holocene was "relevant". ... and so on. We all have whiney stories like this, but my point is really that we should do good fundamental science. Relevance is in the biased eye of the beholder.

Priorities and the New Science Plan

Posted by Christina Ravelo on 2009-03-09 at 01:42

The Environmental Change Processes and Effects thematic area of the current Initial Science Plan (ISP) includes sections on: (1) Internal factors, including Tectonically-induced changes, volcanically-induced changes, sea level, and transient changes, and (2) External Forcing, including climate interaction with orbital forcing, and impact events. It also includes Initiatives involving (1) Rapid climate change and (2) extreme environments.

How should the NEW science plan be organized to highlight the new emerging questions in Environmental Change, Processes and Effects? Should there still be several themes in the new science plan, of which Environmental Change is one? Should Environmental Change be integrated with the other science themes better?

In the current ISP, selecting "Initiatives" was a way of prioritizing certain more specific avenues of research. Should there be prioritized research directions in the new plan? What should they be?

Posted by Susanne Straub on 2009-03-09 at 09:35

"How should the NEW science plan be organized to highlight the new emerging questions in Environmental Change, Processes and Effects?" Because of the multitude of questions (existing or emerging), prioritization for the New ISP seems unavoidable. I think we should find a consensus on those themes/initiatives that are most pressing based

on scientific criteria AND societal relevance, and that can be reasonable implemented during the post-2013 installment of the ocean drilling.

"Should there still be several themes in the new science plan, of which Environmental Change is one?" I think that Environmental Change is THE overarching theme of the ISP - maybe bring it as title of the new ISP? Environmental Change is also now in the mind of the public and the politics, which would enhance our ability to emphasize the importance of scientific drilling.

"Should Environmental Change be integrated with the other science themes better?" Absolutely. The unique opportunity of the drilling is that it brings all these themes together- from the Deep Biosphere to Climate Change to Solid Earth. Unless other programs, we can thus seek the links between them, rather than pre-installing boundaries that are not really crossed. The current ISP contains a paragraph describing potential links between igneous processes and environmental change that had emerged from the ODP - to what extent have these been followed up in the IODP?

A3.1 Ice sheet stability and sea level change

Posted by Katharina Billups on 2009-02-11 at 08:55

It seems to me that the NSF P2C2 document has identified several research areas that are currently given funding priority. Many of these research areas can be addressed specifically by scientific drilling. For example, the question of the sensitivity of ice and sea level to rapid climate change, especially during intervals of relative global warmth, could be addressed by drilling. Perhaps the science plan can build on how drilling would help meet the objectives already outlined by NSF.

Posted by Ingrid Hendy on 2009-02-11 at 09:05

Back out of deep time (excuse the personal bias) and following the IPCC ice sheet questions: Is the present response of the Greenland Ice Sheet and Antarctic Ice Sheets unprecedented?

The ocean records can provide information not available on land because ice sheets tend to destroy information on their previous advances. We'd still have 4 glaciations without the oceans for example. We need drilling to get to the depths below sea floor to see previous glaciations.

We have large data gaps in the North Pacific. The Cordilleran Ice Sheet was a temperate, wet-based ice sheet that give a different perspective on ice sheet behavior than the Laurentian or Fennoscandian.

Is the Antarctic Peninsula melting just a continuation of Holocene retreat or not did not occur in previous interglacials? Was there faster calving when ice input (precipitation) was greater (late Pliocene-early Pleistocene)? Is a similar response in all ice sheets or do they behave differently (internal forcing versus external forcing)?

Given more time I might be able to come up with more if people like these issues....

Posted by Gabe Filippelli on 2009-02-11 at 15:15

I think that working toward mapping onto some of the P2C2 objectives is very important, and is highlighted as well by Ingrid's comments on ice dynamics and deepwater formation (at least for the North Pacific). Seems that we could hit complementary targets for deep-time AND more recent intervals for certain issues. For example, the process of acidification due to extremes is also reflected in more subtle ways in G/IG climate transitions.

The main objectives of the P2C2 are particularly telling, and I believe that we could work toward these:

1. provide comprehensive paleoclimate data sets that can serve as model test data sets analogous to instrumental observations; and
2. enable syntheses of paleoclimate data and modeling outcomes to understand the response of the longer-term and higher magnitude variability of the climate system that is observed in the geological record.

Posted by Mitch Lyle on 2009-02-12 at 13:07

While P2C2 gives a set of good problems, we should not necessarily limit ourselves to these. For example, we can study how different levels of CO₂ are maintained and cycle via studying different parts of the Cenozoic, including Pleistocene. The milankovitch metronome gives us a reasonably calibrated natural forcing function to the system.

Posted by Gabe Filippelli on 2009-02-12 at 20:22

Mitch--I agree completely, but they might be a good starting point for some issues. I do especially like your comment about using the Milankovitch metronome as the constant and earth response as variable.

Posted by Christina Ravelo on 2009-02-13 at 00:28

Yes, I like the direction you are going in, Ingrid. I agree that a focus on IPCC-related questions is potentially an effective way of framing some of the science plan for IODP, and past ice sheet behavior is the first thing that comes to my mind, too. What about an initiative specifically on Greenland ice sheet response to climate change?

Posted by Kira Lawrence on 2009-02-15 at 11:30

I agree that understanding the response of the Greenland ice sheet to past climatic changes is an important question in light of present observations and the IPCCs future projections of warming. But, I would also submit that for the same reasons, we need a much better sense of past Antarctic ice sheet behavior and a clearer picture of what climate was like at mid to high southern latitudes during past warm climate states. How stable is the W. Antarctic ice sheet? What was the response of the E. Antarctic ice sheet to climatic conditions like those projected by the IPCC for 2100? Were there changes in the Southern Ocean during past warm climate states that affected the amount of carbon

stored in the deep ocean? During past warmer climate states how similar were NH and SH climate patterns and responses to climatic change?

Posted by Mitch Lyle on 2009-02-21 at 07:56

I like the idea of a 'stability of the cryosphere' theme. We should include sea ice history and processes (both Arctic and Antarctic), existing ice sheets, and ice sheets recently defunct. We should also trace the meltwater.

Posted by Peter deMenocal on 2009-03-09 at 11:10

Hi Everyone,

I think the "stability of the cryosphere" theme is a very good one for several reasons. First - this is one of the most important and immediately relevant topics on the table for climate change research. Also, this is a problem to which IODP can uniquely contribute. We've had initiatives before on 'responses to known forcing' but a dedicated initiative on the past stability of large ice sheets would be a winner in terms of deliverables and relevance.

Cheers,
Peter

Posted by Ellen Thomas on 2009-02-24 at 20:41

Was there not a proposal in the works on the Laurentide ice sheets –melting away? One of the ways in which Ocean Drilling could look at cryosphere is looking for signs of catastrophic outflows of the past.

Posted by Ingrid Hendy on 2009-02-25 at 08:29

Last time I used catastrophic in a paper I got into trouble....but I think it was a case of someone still stuck on uniformitarianism.

I think we need to look at more sources of catastrophic meltwater inputs than just the laurentide. MWP1 does not have a clear source. Where meltwater is coming from (being input into the system) is going to play a role in the climate response. And then we need to know whether the last deglaciation was special in anyway...is the response to meltwater input from the different sources the same back through time.

IT would great to be able to pinpoint the time when ice sheets begin to respond to climatic warming...the more I think about the more I'm convinced the initial warming is not going to match up with terminal moraine ages. The oceans have the advantage of usually continuous records and datable material.

I think it would also help to know more about the relationship between sea ice and ice sheet buttressing. There is a lot of concern about southern Greenland, but if the sea ice goes around northern Greenland the outlet glaciers will definitely speed up.

Posted by Sidney Hemming on 2009-02-24 at 20:40

I like them Ingrid! One thing I think would be a really worth effort, but maybe a challenge, is to target the major trough mouth fans- the ones I have dreamed about most are those in the North, but the same thing likely applies around Antarctica. These deposits may represent exceptional times of glaciation, but they are also great archives of integrated continental material as well as being places where there are glaciogenic debris flows interlayered with hemipelagic sediments that presumably provide potential for working out the timing. And by targeting the fans and transects off the fans, it should be possible to tie the land to the ocean for some of these important and sensitive areas.

Also, around Antarctica, as far as I can tell, the trough mouth fans seem to be mostly older than Pleistocene. Maybe people think they already know the reason for this, but I haven't been able to infer it from the reading I've done. And in general I think it would be really interesting to track the record of variation around Antarctica, from questions about whether there could have been fundamental changes in catchments areas between pre-glacial and glacial times and as the ice sheets evolved.

Posted by Ellen Thomas on 2009-02-24 at 20:46

It may be a good idea (though not really that novel, but in line with 'shovel ready' science) to look back at older proposals in the system and see which ones (by ourselves and others) would do well with rewriting, refocusing, updating. One proposal that comes to my mind, for instance, is one of Ross Sea Drilling. That would have excellent potential, in my opinion (cryosphere development), but needs serious updating. and there are more such proposals.

Posted by Sidney Hemming on 2009-02-24 at 20:53

I think combining old thoughts with new vision is a great idea!

Posted by Mark Leckie on 2009-02-26 at 14:28

The Ross Sea would certainly be a place to return to with a focus on deeper time glacial-interglacial cycles, and a great opportunity to build on the successes of ANDRILL.

Posted by Amelia Shevenell on 2009-03-02 at 04:37

I believe that the Ross Sea proposal is being resurrected at the moment and updated with a stronger focus on climate questions- Phil Bart is taking the lead on this and I think they/we were hoping to submit this round. The Ross Sea SHALDRILL proposal had a strong climate component has been ranked at the top of the pile, and is in funding/ship-time limbo (that is my latest information as of AGU). Frank Rack might know more about the current status of both of these proposals.

Posted by Yair Rosenthal on 2009-03-03 the 21:42

I am getting late into the discussion and would agree with many of the suggestions. To some extent this discussion reminds me some of the meeting we had in planning the new ASH now called P2C2. Many of the suggestion reflected the interests of the proposers, which were often consistent with some of the program goals but also limited to specific areas closed to the proposer heart / interests. The drilling should be guided by the science questions. I am in favor of following the P2C2 solution i.e., an emphasis on mechanisms of climate change which are critical for understanding with relation to present and future

change. Depending on the questions we should thrive to find the right site that have both the appropriate resolution, geographical extent and needed preservation conditions, which is critical for deep time studies. Given time and budget constraints we should identify which new records would have the greatest impact. Sea level reconstruction, and tropical SST record are at the very basis of our needs for any climate reconstruction and after many years we are still arguing in a big way on these records at certain critical periods. We know what are the largest uncertainties in our predictions (e.g., climate sensitivity, ice sheet melting) and we should devise ways to generate these information much in the same way stratigraphers worked out the Cenozoic record. We know enough now how to do it and we should try to finally get trustworthy records of these critical parameters.

Posted by Alan Mix on 2009-03-05 at 16:16

On the ultra-high-res, P2C2 style stuff, I agree (with many posts) that there is much more to be done in this business. I hope we don't become slaves to societal relevance, however, as that is a slippery slope. At one point there was the "Holocene" initiative, because that was societally relevant. So I submitted a Holocene proposal. And a reviewer responded that only the last 2000 years of the Holocene was "relevant". ... and so on. We all have whiney stories like this, but my point is really that we should do good fundamental science. Relevance is in the biased eye of the beholder.

On practical matters, I think the limit to the best ultra-high-res paleoceanography is excellent survey to find the very best sites, rather than the limitations of the drill bit. Let's make sure we have an integrated program that does all the important pre-drilling stuff first. This is especially true for ocean margin studies, where the good paleo sites may be very small targets in very complicated settings. Also, it seems like this discussion is pretty JR-centric. Let's not forget the effective use of alternate platforms, which may be very appropriate for hitting smaller targets, for example in fjords or estuaries. Some time ago there was a vision of incorporating European long-coring technologies as one of the alternate platforms in the IODP structure. Now that the US has long-coring technology, should that be broadened and included as one of the options in a fully integrated sub-seafloor strategy?

Posted by Joseph Stoner on 2009-03-12 at 12:07

Following on the 'stability of cyrosphere theme' and the ultra-high resolution P2C2 stuff, echoing Alan comment, all tools need to be brought to bear. If we are to understand ice sheet dynamics in perspective of local, regional and global climate conditions, increased emphasis on site surveying will be needed to find the hidden sediment archives that dot complex marginal setting that will allow us to separate the various processes. The surveying is also needed not only to find and characterize these sites, but to link these locations to the processes that drive sedimentation. Better proxies are also required that will allow us to define glacial or sea ice processes and to accurately date these. Likely, different sites will provide high quality information on different time intervals or processes, even within the same region. Those that are best for deglaciation may not be as optimal for glacial inception or maximum conditions, yet nearby locations maybe. And all may not require the same need for drilling technology, some could be maximized through long coring, others will need alternative platforms. If we look at this holistically it is very doable, but really requires science planning that asks the question of how we

solve the problem, not what problem can we solve with ocean drilling. I can't think of a better way to do this than under the auspices of IODP using its resources, knowledge and experience to, as Alan said, develop a fully integrated sub-seafloor strategy.

A3.2 Pliocene–Pleistocene evolution

Posted by David Lea on 2009-02-24 at 13:36

I think that one of the most interesting paleoceanographic/paleoclimatic questions that has emerged in the last 5 or so years relates to new findings on Plio-Pleistocene evolution. What I think has really opened up this area is the combination of new proxies and new information on the low latitudes. There are some really great research questions here:

- What was the role of CO₂/greenhouse gas changes in driving Plio/Pleistocene cooling and glacial intensification, and how did those changes relate to carbon cycle shifts?
- How did internal changes play a role (i.e., oceanic shifts as such the development of the EEP cold tongue)?
- What role if any did tectonic changes play?
- What earth system changes happened over critical boundaries such as the time interval of intensification of NHC or the MPT?
- What was the full earth system role/response to the gradual shift in Milankovitch periodicities over the time scale of Plio-Pleistocene evolution?
- Answering these questions requires new drilled sequences focused on the low latitudes as well as the Southern Ocean region. Sites with sufficient accumulation rates to address these questions could be targeted.

Posted by Susanne Straub on 2009-02-24 at 14:50

Not to forget the influence of global volcanic pulses The idea that an increase in (arc) global volcanism and the intensification of glaciation are related is supported by results from ocean drilling. However, causal connections have been elusive. Choosing drill sites where information from all or several of these factors can be detected next to volcanic deposits, should be a great way to evaluate whether, how and may volcanism may affect the Plio-Pleistocene evolution.

Posted by David Lea on 2009-02-24 at 15:58

I don't know much about this influence, but I would expect that any consideration of long-term secular evolution of CO₂ is going to require a consideration of how the volcanic release of CO₂ is balanced by weathering and the deposition of CaCO₃ on the seafloor. If higher atm-CO₂ levels supported the warmth of the Pliocene, then those higher levels would require a shift in the balance between volcanism, weathering and the deposition of CaCO₃.

Posted by Susanne Straub on 2009-02-24 at 20:01

Increase in global volcanism has been suggest to go either way – lowering temperature by input of aerosols into the atmosphere, or assisting temperature increase by increased input in CO₂. The increase in global volcanism in the Quaternary suggests some link with

global cooling. However, which direction this works, how the volcanic release of climatically active gases relates to the other controls of CO₂, and how significant volcanism is as factor in the CO₂ equations is still unresolved.

Posted by Ken Miller on 2009-02-24 at 15:55

Our understanding of global sea-level changes in the Pliocene-Pleistocene is woefully inadequate. We know the LGM and marine isotope chron 5e reasonably well. The following are not well known:

- the maximum rate of change, particularly in MWP1a.
- the position of sea level during marine isotope chron 3 ("stage" of paleoceanographers) is remarkably poorly constrained except by models (Siddall et al.)
- marine isotope chron 30 is a peak warm interval? Where was sea level? Where do you want it to be?

And finally my pet peeve, the Pliocene. I sat through a modelers talk at GSA which defined climate sensitivity based in part on the Pliocene warm period, when it was claimed we know sea level well (+25 m). This number is essentially made up. It may be right, but it may be 10 m. Dowsett is the main dataset and that is from an uplift coastal plain with huge error bars.

If you want to understand cryospheric evolution, we need to get firm numbers for sea-level for during chron 3, 30, Pliocene, and others.

Posted by David Lea on 2009-02-24 at 16:07

I agree completely with Ken's post, and I think sea level in the Plio-Pleistocene is a natural target for future IODP research objectives. This can be readily justified from the argument that the Pliocene world is often taken as an analogy to a future global warming world with higher sea level (cf. Hansen). But it's also a great science question that goes right to the heart of climate system sensitivity: what did a warmer, higher CO₂ (?) world look like, and how did it evolve from that state to the present (pre-anthropogenic) state?

Posted by Maureen Raymo on 2009-02-25 at 14:04

It is a bit unclear to me exactly how all this information will be sifted, however I assume that adding support to prior suggestions would be a good thing in the overall effort of setting priorities.

To that end: Within the history of the Pliocene and Pleistocene lies important information DIRECTLY relevant to assessment of near and far future climate change. First, I agree completely with Ken that maximum Pliocene sea level just prior to NHG is essentially unknown with estimates ranging from 5 to 40 meters above present. The 25m typically assumed by the climate modeling groups for their GCM experiments is essentially an average of a number of poorly constrained estimates. Very few attempts have been made to assess Pliocene sea levels and Dowsett, Miller, Brigham-Grette and a few others deserve credit for addressing this important topic. Probably one reason there is so little known is that the best place to study Pliocene sea level may be on land and most of us who are interested in question work in the ocean. (To this end, USGS

(Dowsett) has funded P. Hearty, myself and a few others to undertake field work in Australia this summer). However, ocean drilling can certainly shed light on this problem as well.

A firm knowledge of Pliocene sea level would provide one important constraint on data-model comparisons. Does your Pliocene GCM-nested ice model get EAIS volume right when climate is slightly warmer and GHG are slightly higher? There are many large climate modeling groups that would like to answer this question. Which bring us to atmospheric CO₂/GHG levels in Pliocene (and all of Neogene for that matter). Again, most modelers assume Pliocene CO₂ level is around 400ppm, the average of a number of poorly constrained studies. But no serious evaluation of model sensitivity and accuracy can be done for Pliocene until we know this important boundary condition as well. I think most of us recognize that this is another area of research that hasn't gotten the attention it deserves, probably because the methods are so controversial. However, the time has come to realize how critically important this parameter is and also recognize that IODP material is probably going to be essential in the measurement of paleo-pCO₂.

Finally, another related topic is understanding how ice sheets respond to insolation and GHG forcing. We still have no idea why there is a "41kyr world". What part of seasonal heating cycle exerts the most control on ice sheets, what role do GHGs play, are there G-I ice volume changes of any significance in the southern hemisphere and are they in phase or out of phase with those in the northern hemisphere? These are all active research questions that ocean drilling can provide important answers to---what is the frequency history of run-off of the Laurentide and other ice sheets? where and when did marine ice margins replace terrestrial ice margins around the perimeter of Antarctica? etc. etc. Some beautiful long records are starting to be found around Antarctica as we continue to explore this large unknown region. And the same goes for the Arctic.

Posted by Lorraine Lisieck on 2009-02-26 at 16:46

I agree with David, Mo, and the other posts in this discussion. I also want to add another item to the list that need further study with respect to Plio-Pleistocene climate evolution: overturning circulation in the North Atlantic and Southern Ocean. The importance of this work is demonstrated by the large uncertainty in predictions of future overturning change. Because a lot of progress has been made in finding many ways to measure changes in overturning (e.g., Pa/Th, Nd, and d¹⁸O gradients in addition to the traditional d¹³C and Cd/Ca) continuing research on overturning should soon yield greatly improved reconstructions on a variety of time scales. Given the poor agreement between model simulations of overturning (in the past and future), ocean drilling data is essential to understanding circulation change (e.g., in a warm Pliocene-like world) and its interactions with the rest of the climate system.

Posted by George Philander on 2009-02-28 at 11:39

Let us assume that we are launching a project to study the seasonal cycle. Indians will focus on the monsoons, Canadians on harsh winter storms, and Peruvians on oceanic upwelling. These phenomena at first appear to be independent of each other, each with its own separate explanation. Then El Nino occurs and we discover that they are all connected, in complex ways.

As in the case of the seasonal cycle, so the Milankovitch cycles are likely to involve several separate phenomena, which to first order can each be explained separately, and which to the next order are all connected. (The Milankovitch forcing, variations in sunlight, corresponds to modest changes in the seasonal forcing.)

For a long time the focus in studies of Milankovitch cycles was on the waxing and waning of glaciers. So impressive are these phenomena that they are often assumed to drive Milankovitch cycles in parameters distinct from global ice volume. For example, the glaciers are sometimes proposed as the drivers of the signals found in Devil's Hole, and in the coastal upwelling zone off California. It is as if Indians were to insist that an explanation for the monsoons also explains winter storms in Canada and upwelling off Peru.

Exciting, and controversial, recent results concerning Milankovitch cycles in the equatorial Pacific could have explanations independent of those for the waxing and waning of high latitude glaciers. We are probably in the first phase of Milankovitch studies, the phase of identifying separate phenomena, each with its own explanation, all induced by the same variations in sunlight. At this stage we are far from the phase of finding the connections between the different phenomena.

Deep sea cores are of critical importance in identifying the various phenomena associated with the Milankovitch cycles. Records from the western and eastern equatorial Pacific have remarkable similarities and differences. What about records from the coastal zones of southwest Africa and Brazil at the same latitude? Or the coastal zones of southwest and southeast Africa? We need many more cores.

The Milankovitch cycles are of major importance to climate modelers because the forcing function is known precisely, and because the response is so varied in space and time. Explaining and simulating aspects of these cycles will be a major contribution to our understanding of climate changes in the past and future.

Posted by David Lea on 2009-03-02 at 12:00

George - thank you. I found your statement inspiring and exactly the kind of broad rationale we need for a program of drilling focused on the low latitudes!

Posted by Peter Molnar on 2009-03-02 at 15:37

I just want to weigh in support of George Philander's last paragraph.

Because we know the forcing at the top of the atmosphere, and we see responses in ocean sediment (and in a few records from continents), Mother Nature hands us a template for using paleoceanography to test hypotheses of climate dynamics. One challenge of course to pose the right questions, but the other, obviously, is to obtain records that sample well Milankovitch variability in different environments, which depend not only on latitude but also on other aspects of the climate system regardless of latitude (e.g., regions of marked upwelling).

This cannot be accomplished without deep-ocean drilling

(I say nothing new here, but merely offer support from a former seismologist.)

Posted by Delia Oppo on 2009-03-01 at 09:58

I wrote a paragraph on the need for low-latitude drilling of ultra-high resolution sediment, and this seems like the best place to put it. Much of this has been touched on by other subsequent replies to David's post, and also in other parts of the document, but I thought putting in one place might be helpful:

As noted in the successes of the program section, high accumulation rate sites have provided important insights. Many, although not all, of the very high accumulation rate sites that were drilled are from subtropical and higher latitudes. The IMAGES program and subsequent work identified regions in the Indo-Pacific Warm Pool with very high accumulation rates, some in excess of 100 cm/1000 year. Without ocean drilling, we can't even reach the previous interglacial in some of these sites. There are probably other areas of the tropics with such high accumulation rates as well. Balancing ultra high and modest accumulation rate sites might allow us to address questions ranging from the evolution of the WPWP on tectonic to Milankovitch time scales, and the role of this evolution in broader climate (e.g., ice ages, El Nino, monsoons, carbon cycle, temperature gradients, heat and salinity distribution) to detailed evolution of the WPWP (tropics more generally) over several glacial-interglacial cycles. Since many very high accumulation rate sites in the tropics are in margin environments, they are also ideal for making direct comparisons of marine and terrestrial environments in the same sediment. There are old proposals in the system to drill at least one of these areas, and so I agree with comments that suggest exciting "old" proposals should be kept alive and perhaps refurbished. At the same time, a new focus on the tropics, with an eye towards the kind of material that came from the high latitude drift sites and sites like Santa Barbara Basin, might promote us to identify other great tropical sites, as a spatial distribution of sites is needed to really understand the mechanisms of tropical climate change and its relationship to global climate change.

Posted by Mitch Lyle on 2009-03-03 at 13:24

I believe that good tropical sites exist, but they will require some exploration to find the equivalent of a Santa Barbara Basin. We already have clues from good margin cores, but we need to identify whether the high deposition extends back sufficiently in time for success.

Posted by Yair Rosenthal on 2009-03-03 at 22:00

I think there is an agreement on the need of new records to study the plio-Pleistocene as they are the closest analogs to what we perceive for the future (and I may add the late Miocene is equally good a target). Understanding of these periods won't only requires good records of ocean temperature, sea level and ocean circulation at orbital frequencies but also higher frequency records as many of the mechanisms discussed at these periods operate on these shorter time scales (e.g., ENSO and decadal oscillators). Most of our proxy records are not capable of directly addressing these question because of low

resolution. Nevertheless, we use changes in long-term climate state to imply changes in, for example ocean dynamics. The models, however, use the high frequency results to "test" the data and often fail. We need to think more creatively on new archives that can bridge these gap and provide the necessary baseline for model simulation. Corals are just one such archives but there are others.

Posted by Rick Murray on 2009-03-05 at 20:29

Not to reiterate, but rather to just support this... Yair's, Delia's and Mitch's last three postings are spot-on target. We need more higher resolution archives in the lower latitudes (a la Cariaco), and also need to create new archives by thinking out of the box (e.g., corals). Drilling can and should be important in all areas of this.

Posted by Terry Quinn on 2009-03-06 at 16:29

We should try to approach challenges such as understanding the dynamics of ENSO and other high-frequency oscillations in the climate system using a coordinated proxy approach that maximizes the strength of each proxy. The ENSO system provides a good example of how we can integrate information gleaned from continuous, high-res marine sediment cores to explore EW gradients, thermocline depths etc with subannually resolved records from fossil corals, which provide discrete windows on ENSO variability. The MSP side of IODP has proven it can obtain remarkable recovery in these challenging drilling environments. We need to do more of this style of drilling, targeted at high profile opportunities and locations that we know exist. It would be great to obtain marine sediment and coral records that overlap in time from the same locality/region so that we can integrate records derived from these different archives in search of the common climate signal encoded in them.

A3.3 Observation–model linkages

Posted by Mitch Lyle on 2009-02-10 at 07:13

One of the key themes in paleoceanography for the last decade is to link models and observations, i.e. to compare what we drill with physically consistent representations of past worlds in order to detect inconsistencies and better understand processes. Can we develop this strategy more explicitly for the renewal?

Posted by Gabe Filippelli on 2009-02-11 at 15:52

This came up with a workshop that USAC supported several years ago bringing paleoclimate people together with standard paleoceanography folks (although the groups did overlap somewhat). The important lesson I learned from that workshop is that those beautiful high resolution drift sites that us folks prefer would not necessarily be a prime target for the modeling people. Instead, they would like to see a time slice approach spanning an array of latitudes/longitudes across a couple of critical interfaces in the ocean, in order to capture temperature, wind stress, etc.

This goes a bit back to CLIMAP does, but it also takes the GEOSECS approach, but through time.

Posted by Frank Rack on 2009-02-11 at 16:25

The paleoclimate summer school in Urbino is a prime example of this approach <<http://www.uniurb.it/ussp/index.html>>, these schools bring paleoceanographers and modelers together to understand their respective and often different needs and approaches while also exposing students to project development and proposal strategies that integrate data and models. We should expand support for these programs and think about how we can better utilize these types of meetings to provide new directions for scientific drilling integrated with modeling requirements. The outcomes from these activities can also help to inform the IPCC process and highlight the role that scientific drilling plays in providing primary data for hypothesis testing by modelers.

Posted by Mitch Lyle on 2009-02-12 at 12:55

This would be a different concept for the drilling program, in essence including a significant long-term commitment to shore-based research/education/meetings. The nice thing about a regular meeting like the Urbino summer school is that there can be different themes to help coordinate recent fieldwork (i.e. drilling) with other observations and modeling.

Posted by Christina Ravelo on 2009-02-13 at 00:43

I like the idea of student training, but I also think that there should be community meetings (I don't really think of Urbino as a community meeting, is it?) to provide the new directions that you are talking about. Collaborative efforts between modelers and observationalists have already enriched some IODP projects, but we should add structural elements or built-in opportunities to foster more integration between the two communities (modelers and data-people) interested in the same problems.

Posted by Mitch Lyle on 2009-02-16 at 14:51

I agree with the idea of a community meeting--maybe an annual symposium/workshop with plenty of room for students.

Posted by Frank Rack on 2009-02-18 at 13:20

I agree that community meetings are important and support expanded effort to bring IODP scientists and modelers together. The Urbino school is supported by ECORD as well as other groups, so I don't see these as mutually exclusive approaches. I guess I was somewhat blurring the lines between support provided by USSAC and programmatic support funded through IODP directly, but I think we need both approaches to really galvanize community engagement and encourage the next generation of students and researchers to understand and integrate data and models throughout the life cycle of a proposal/project.

Posted by Ellen Thomas on 2009-02-24 at 20:59

Urbino concentrates more on older parts of the record (Paleogene, Greenhouse Earth), and the main idea is education. But in fact, if you throw groups of people with these various specialties together it can't help but becoming some sort of community get together where you can throw ideas around. I have particularly enjoyed working with not just climate models but earth system models - in my opinion that really helps figuring out where data are needed in order to do useful things with the models.

Posted by Bradford Clement on 2009-02-25 at 07:38

Gabe has a good point here. Back in the '90s, the Ocean History Panel worked on a strategy of obtaining age-depth transects in each ocean basin with the goal of obtaining a geographically distributed set of records of how water masses changed through time. It may be time to recast that strategy with the goal of providing some much needed constraints for the climate modelers.

Posted by Adina Paytan on 2009-02-28 at 12:32

I agree with the above comments paleoceanographers (observations) and modelers must work together and maybe a community meetings is the way to initiate this effort. While the Urbino school is fantastic it exposes only selected individuals in the community and many more can benefit from such a conversation.

Posted by Debbie Thomas on 2009-03-02 at 12:42

Adina is right here – while Urbino is a spectacular experience, a better strategy would be through coordinated community meetings and collaborations dedicated to more direct and intensive model-data integration.

Posted by Marta Torres on 2009-02-28 at 17:17

Great discussion on the role of modeling in paleoceanography, and I agree with the idea of meetings and student training to fully develop this approach. I want to expand this idea though, to all other fields within the drilling community, for example: geochemistry, hydrology, microbiology, and the response of these to changes in a wide range of time scales and spaces could become powerful contribution of the new program. How to best integrate models in all aspects of a project, from proposal to post-cruise results? Having a formal pathway to accomplish this on an ongoing basis, with student training opportunities will move many of the IODP research arenas to new and higher levels.

Posted by Rick Murray on 2009-03-05 at 20:25

I support this concept fully, as the spatial and temporal coverage that the modelers provide is really what we more "traditional" (whatever that means) mud-slingers need, and the modelers certainly need us for prime data input and groundtruthing. But...here's an observation...have any modelers chimed in on this subject on this message board? I think the answer to that is 'no'. How do we engage them better?

My feeling is that this overarching issue won't necessarily be solved by more or better drilling, but more in the ways bandied about here, namely, getting people together in the same room. Having said that, however, how many workshops in a year can we all handle? What is really needed is to build departments that include modelers and mud-folks together...all too often, either for analytical or computational critical-mass reasons, we tend to naturally self-hire within our own comfort zones, instead of reaching out to the other community as we build our own programs. There are notable institutional exceptions to this, of course, and many of you (perhaps non-coincidentally) are at those institutions, but until we as a community begin to interact on the daily basis I think we're doomed to only really do this at sporadic workshops and special sessions.

A3.4 Warm climate studies

Posted by Richard Norris on 2009-02-04 at 21:28

Studies of "deep time" archives of global warming have mostly been targeted for drilling in areas where sedimentation rates are low, and the opportunities to evaluate of short-term Earth system dynamics are small. We need to use strategies like those employed in late Pleistocene and Holocene drilling to recover highly-resolved records of extreme climate events, particularly events with relevance to human-caused future global change. We should shift our focus from long-timescale, low sedimentation rate records to targeted drilling of records that will permit us to evaluate short-period Earth system dynamics. Examples could include evaluation of century-scale inputs of greenhouse gases during rapid climate change, studies of the impact of changes in greenhouse gases on surface ocean acidification (ie less than the ocean mixing time), and changes in biotic response on ecological time scales to rapid climate change.

Having said all this, the one area where we should put up with low sedimentation rates is renewed drilling in the Arctic. In particular, it is high time to drill the deep basins of the Arctic and to target the mid Cenozoic and Cretaceous sections that have not yet been sampled by IODP.

Posted by Mitch Lyle on 2009-02-09 at 15:48

I agree that we need good resolution of important events, and take this posting as an argument to find sites that have high sedimentation rates over the appropriate intervals, without km of burial.

However, considering that we are still finding warm transients in the Eocene and that we don't yet have good representative records from the basic parts of the earth's climate system, I don't think that we should trash 'low' sedimentation rate objectives yet. Also, we should define what is meant by 'low' sedimentation rates--do you mean less than 1 cm/kyr, or less than 10 cm/kyr?

Posted by Ingrid Hendy on 2009-02-10 at 09:12

I agree with Mitch that there is still information to be garnered from low sedimentation sites. I'd like to make two points:

- We do not have long cores from every ocean basin, and we should not extrapolate an event as global when we don't know what that event looks like in other ocean basins (for example does the South Atlantic Southern Ocean represent the Pacific and Indian sides at all times).
- High resolution cores have high sedimentation rates due to unusual sedimentary process which may mask or exaggerate ocean responses.

Posted by Richard Norris on 2009-02-10 at 20:26

I agree that there are questions that can be best assessed with low sedimentation-rate cores (assembling orbital chronologies springs to mind). My point is that the "deep time" community has mostly not tried to address questions (or target drilling) on topics that

require high enough sedimentation rates to assess short term dynamics with time scales relevant to understanding tipping point behavior or assess the rapidity of changes in major ocean properties. Instead, we are usually stuck saying that the change was faster than 10,000 or 1,000 years when what we would like to know is whether a particular transition occurred on ecological or century-scales. I'm thinking particularly about the Paleocene-Eocene Thermal Maximum (PETM) here, where the crucial need is to assess rate of change, since these affect our ability to accurately model the sources and amounts of greenhouse gases that are responsible for the event. The PETM is also a case in point since to determine whether the surface ocean was acidified, it would certainly help to be able to resolve the microfossil record at better than the mixing time of the ocean (~1000 years). We also suspect that there are short-lived transients that precede the main PETM excursion (as suggested by terrestrial and very high deposition-rate North Sea industry records), but we do not have deep oceanic records suitable to detect short (~century-scale) events. Many of these same questions also pop up in consideration of hyperthermals, the d₁₃C anomaly at the K/Pg boundary, the onset of OAEs and glacial transitions. We need to know time to place events into a broader context. But we also need rates to understand process.

Now, of course, the PETM is not everything and there are other valuable things to study. However, I think there are lots of very interesting and socially-valuable problems that require knowledge of rates, particularly at time scales relevant to future global change.

Posted by Ellen Thomas on 2009-02-24 at 20:43

In my opinion one of the very important issues is not just sediment accumulation rates, but also records which we can date at high accuracy-precision, in principle records where we can use orbital tuning and/or lamina counting. High-sed rates by themselves are less useful unless we can indeed use them optimally, i.e., we need good dating

Posted by Sidney Hemming on 2009-02-24 at 21:01

Yeah, I was going to weigh in on dating – I agree it's essential to have good time. Some of the new developments in the U-Pb zircon world are making it very possible that dating of volcanic ashes in some of these deeper time cores could actually put some real absolute constraints on the picture at a precision comparable to orbital tuning. And some of the places that are interesting to study for other reasons would happen to intercept ashes- and if we were thinking that way, more of them might. The Bowring lab at MIT is getting as good as ~20 ky precision on 50 Ma zircons, and still working on improving!

Posted by Christina Ravelo on 2009-02-10 at 16:36

Seems that there is both a need for resolving rapid events, but also a need for characterizing mean states and open ocean conditions where lower sedimentation rates prevail. I would suggest taking a step back, first, and focusing on the science questions.

For the science plan, we need to first articulate the science questions that need to be answered, and justification for the locations and types of sections we go after will follow. It would be good for us to first work on defining/formulating the compelling questions that drive the need for drilling. We can then follow this up with the rationale (i.e., the answer to the question, 'why would anyone outside our community care about

this?') and then with the drilling, data-collection, and even data-model comparison strategies for answering the questions.

A3.5 Biogeochemical cycles and processes

A3.5.1 Carbon cycle

Posted by Kira Lawrence on 2009-02-15 at 11:30

Were there changes in the Southern Ocean during past warm climate states that affected the amount of carbon stored in the deep ocean?

Posted by David Lea on 2009-02-24 at 13:36

What was the role of CO₂/greenhouse gas changes in driving Plio/Pleistocene cooling and glacial intensification, and how did those changes relate to carbon cycle shifts?

Posted by Mitch Lyle on 2009-02-03 at 08:04

Paleoceanography can provide important information about how earth systems evolve under different climate states. We should build at least part of the plan about getting observations about cycles and systems.

- 1) Carbon cycle--we can drill sediments that document earth under high CO₂. What maintained high CO₂? What was the pH of the oceans? How did carbon cycle?
- 2) Evidence points to CO₂ drawdown as a main driver of initial Antarctic glaciation. What drew down CO₂ and why? Linkages between 'brief' 2 myr cold periods and carbonate in the Eocene suggest major carbon cycle links--what were they?
- 3) Events like the PETM and other Paleocene/Eocene warm transients suggest that the warm system had instabilities leading to rapid warming. What were they, and how did the earth recover?
- 4) Climate sensitivity for future warming depends on water vapor feedback. We know that the water cycle has not been constant through the Cenozoic, and warmer climates seem to be wetter. Can we better constrain the history of water cycle, and use this information to understand water vapor feedback?

A3.5.2 Clathrates

Posted by Marta Torres on 2009-02-28 at 17:23

The importance of gas hydrates was highlighted in the ISP, and although we have made considerable gains in our understanding of these systems through ocean drilling, there is still an intensive debate in the science community as to the role of these deposits in the overall carbon cycle, geohazards and climate change.

Posted by Alberto Malinverno on 2009-03-03 at 16:24

I agree with Marta that while there has been significant progress, there are still big holes in our understanding of the marine gas hydrate reservoir. On geological time scales, we still do not have a clear mechanism for abrupt, large releases of methane and cannot reliably estimate the size of the gas hydrate reservoir in the past (eg. at the PETM). On human time scales, gas hydrate dissociation can be a powerful positive feedback to warming. While the current understanding is that gas hydrates probably dissociate on millennial time scales (eg., Dave Archer's 2007 review in Biogeosciences), our knowledge is incomplete and the potential impact of gas hydrate dissociation is large. Improving our understanding of the large marine gas hydrate reservoir would have a major impact on our understanding of past abrupt climate changes and our forecasting ability. In the program renewal, we should make a strong case that scientific ocean drilling has collected crucial observations that led to major advances; just think where we would be without the drilling results from Blake Ridge and the Cascadia margin.

Posted by Rick Colwell on 2009-03-13 at 19:58

Recent reports indicate rapid flux of gas (much methane) from permafrost (and hydrates?) and shallow marine sediments in high latitudes. This is a difficult location to core (shallow water, gassy); however, to address issues of imminent importance to the carbon cycle and climate change it seems that this is an area that IODP should target. The arctic shelves seem to be one of the most dynamic geologic systems, and likely interactive with upper earth, water, and atmospheric processes. There is a real opportunity to address the geologic, chemical, and biological issues associated with gas flux from these systems. The value of IODP's scientific contributions to understanding climate change could be very clear through studies of these arctic systems.

Posted by Marta Torres on 2009-02-28 at 17:03

Clearly an important topic [Appendix A1.3] as it addresses fundamental issues of carbon and water cycling. I want expand the topic of "continental margin storage of carbon and nutrients" to include two sub categories:

- 2.1) Role of submarine fluid discharge- Ever since the discovery of cold seeps it has become apparent that these loci of fluid/mass/energy discharge play a key role on the recycling of elements from sediment reservoirs to the ocean
- 2.2) Gas hydrate "capacitors". This topic was highlighted in the ISP, and although we have made considerable gains in our understanding of these systems through ocean drilling, there are outstanding issues pertaining to just how these deposits modulate the input of carbon to the bottom water, and how these inputs respond to oceanographic and tectonic forcing

A3.5.3 Oceanic anoxic events (OAEs)

Posted by Mitch Lyle on 2009-02-24 at 15:20

The Kent-Muttoni hypothesis, that the amount of CO₂ recycled back at subduction zones radically changed after the India-Asia collision because subduction switched from young, carbonate-rich subduction zones to old, carbonate-poor ones, provides an interesting tie

to the seismogenic zone drilling. Costa Rica is an example of a young carbonate rich subduction zone, and Nankai is the old, carbonate-poor one.

OAE drilling might be a good example of a program with both offshore and onshore components. They are an example of an extreme in the carbon cycle that is clearly not understood.

Posted by Ellen Martin on 2009-03-03 at 08:27

I want to add support for a focus on OAEs. This is one topic that I know industry was potentially interested in as well. Clearly OAEs represent a time of major environmental change and recent Nd isotopic data suggest this isotopic system may provide the data necessary for testing some of the hypotheses for OAE conditions. In particular, Nd isotopes suggest the ocean was far from stagnant during OAE2 deposition. Some of the basic questions we need to answer before we can start to understand these events are:

- 1) How widespread are they?
- 2) How much of the water column is impacted?

We refer to them as global in nature, but in reality the record is strongly biased toward the North Atlantic. There is evidence that the photic zone may have been anoxic for some period of time and that the seafloor was anoxic, but does that mean the entire water column was anoxic? Depth transects similar to Demerara Rise, but in other ocean basins would represent a good beginning, along the lines of Ingrid's comment that we can't extrapolate global conditions for one ocean basin. Given that ocean anoxia may have been an earth system response to elevated atmospheric CO₂, there is a clear tie to modern climate change and carbon cycling.

A3.5.4 *Conservative elements*

Posted by Adina Paytan on 2009-02-28 at 12:39

Many recent publications suggest that the paradigm of "conservative" elements - major ocean constituents that do not change much over time is not consistent with observations. How fast can major constituents in seawater (sulfate, Sr, Ca, Mg etc..) change? What are the mechanisms that cause changes in these constituents? does the concept of "steady state" even exist? Changes in these major constituents imply large changes in global biogeochemical cycles and have implications to the C cycle and to the use of many of the proxies that assume these to be constant. This is a paradigm that needs to be reformed.

Posted by David Lea on 2009-03-02 at 12:14

I agree strongly with Adina and think this is an area ripe for major advances. The evolution of Mg/Ca in seawater is an example of one area where ocean chemistry and paleoceanography have a strong intersection (given the desire to use Mg/Ca paleothermometry in longer time scales). Matthew Fantle's innovative work using pore water profiles from ODP807 (Ontong Java Plateau) to constrain Mg/Ca-seawater changes in the past is an example of such work. We will need much more of this kind of research to confidently establish how conservative elements have varied back to the Cretaceous!

Posted by Debbie Thomas on 2009-03-02 at 21:41

This discussion could be extended to the notion of the "residence time" of any element, tracer, species. Our proxies rely on the assumption that the residence times in the ancient were the same as they are today (or at least our best estimate of what they are today). What happens to the residence time of a tracer in an ocean that may have circulated more "sluggishly?" When there is a massive hydrothermal perturbation (e.g., Ontong Java)? When there is a massive injection of carbon (e.g., using d13C as a deep-water tracer during the PETM)? When the redox state of the oceans shifted rapidly? [just a few examples]

A3.6 Implementation strategies

A3.6.1 *Missions or mission-types planning*

Paleo-GEOSECS

Posted by Peter deMenocal on 2009-03-09 at 11:49

One long-term research commitment that might be worth exploring would be multi-leg, staged drilling ops to recover sites representing key zonal and meridional transects across the major ocean basins. The rationale for this is that we know that climate changes are not restricted to poles or equator, nor surface, intermediate, or deep-water depths.

Having a set of 'reference section' drill sites that define these sorts of transects in the major ocean basins would open up a number of scientific studies that just aren't possible now (such as 4-D deep circulation changes, surface temperature/salinity gradient changes, productivity, past ITCZ shifts, winds, etc...).

Our current model is basically led by successful proposals for specific legs in specific regions. Were we to pursue this paleo-Geosecs approach we might be able to coordinate drilling to both address leg-specific proposals AND build the network of sites that would provide relatively continuous zonal and meridional transects.

With the current inventory of drilled sites it's quite difficult to construct meridional or zonal transects across most of the ocean basins (except for zonal tropics). A general strategy could be to core along the flanks of the mid-ocean ridges to obtain meridional transects, and to drill new areas to complete zonal transects. Along the ridge flanks sedimentation rates are slow but should be continuous and at/near the lysocline for much of the mid-late Cenozoic.

I came up on the idea after trying (unsuccessfully) to build these transects myself from available DSDP/ODP sites. Maybe someone has already proposed this, but I think this might be a useful 'overarching concept' model to guide future drilling. We could 'tag on' sites to scheduled drilling ops in basins where sites are needed, and whole legs could be proposed to obtain key transect segments.

Posted by Gabe Filippelli on 2009-03-11 at 13:26

Peter: I couldn't agree with you more! We have a string on this in the modeling discussion as well. We simply have not approached scientific ocean drilling with the longer-term vision of collecting data sets that are comparable to those of GEOSECS but in deeper time than is possible with normal piston cores.

Interesting issue, however, is that proposals to do these longer-term multi-basin projects do not typically fare well in the review system. I have seen this first-hand, and I think that if the community makes it clear that our process has to accommodate these mission-type efforts (i.e., ones that can't necessarily be knocked off in one 2 month chunk, but rather could occupy some portion of drilling time on many different expeditions), that we will revise the evaluation and operations process accordingly.

Posted by Christina Ravelo on 2009-03-11 at 15:20

I also agree that a more long-term (mission-type) comprehensive approach to solving some paleoceanographic problems could work well, but we would need program flexibility in how they schedule expeditions and decide on sites, and we would need long-term planning. The challenge is to refine this approach, of generating continuous transects, with careful site selection based on where the good sediments are and more importantly where records are absolutely needed to solve process-oriented questions. I would like to see inverse or predictive modeling approaches used to determine the hot-spots where critical records are needed to resolve things like deep ocean circulation or movements of the ITCZ, for example. It may be more practical to do it in this more focused way, given the limited drill time available.

Once we come up with a list of critical drill locations around the world's ocean, rather than try to package them in neat 2-month expeditions, each in one region, we package them as necessary drill sites to address a more global or comprehensive problem. They could be drilled either as a series of expeditions, or at various times during other unrelated expeditions when the opportunity arises. I think this is aligned with Peter's suggestion.

Posted by Peter deMenocal on 2009-03-11 at 16:00

Christina and Gabe,

If the broader community agreed with this sort of strategy then we might be able to accomplish it by adding 1-2 sites onto scheduled legs. We might be able to These sites would not be particularly deep holes nor in particularly deep water. It might add on 1-2 days of time onto a planned leg and we could start to fill in transects this way. Think of all the transits we've sailed on, passing over great targets along the way.

To be sure, we would need some dedicated legs to fill in large transect segments, particularly for the more remote sites. These legs could be packaged as discrete science units in and of themselves.

The big hurdle here is whether the community would buy into this sort of mission-style, overarching approach, or whether we're still following 2-month science quanta.

Posted by Mitch Lyle on 2009-03-12 at 06:49

The time added will be more than 1-2 days for any given drill site because that does not count transit, and the length of drilling time depends on water depth and age of objective. Triple-coring takes time. We should be thinking about legs of drilling. Adding sites to an existing leg requires that the leg is already scheduled and that we have a set of sites for which we already have clearance and safety approval.

One of the most striking pieces of missing information is North Atlantic drilling to understand how NADW initiated, and would require sites that spanned the water column and reached into the Miocene.

Posted by David Anderson on 2009-03-13 at 15:23

Regarding 'reference section' drill sites, a great concept, the importance of high sedimentation rate cores cannot be overstated. Low sed. rates cores are one reason that the current archive is not sufficient. We need future drilling to build reference sections from high sed rate cores. This will require extensive surveying, but the Bermuda Rise drift and other successes show the sites are out there, even in the gyres.

A3.6.2 Multi-platform needs

Posted by David Anderson on 2009-03-13 at 15:23

On practical matters, I think the limit to the best ultra-high-res paleoceanography is excellent survey to find the very best sites, rather than the limitations of the drill bit. Let's make sure we have an integrated program that does all the important pre-drilling stuff first. This is especially true for ocean margin studies, where the good paleo sites may be very small targets in very complicated settings. Also, it seems like this discussion is pretty JR-centric. Let's not forget the effective use of alternate platforms, which may be very appropriate for hitting smaller targets, for example in fjords or estuaries. Some time ago there was a vision of incorporating European long-coring technologies as one of the alternate platforms in the IODP structure. Now that the US has long-coring technology, should that be broadened and included as one of the options in a fully integrated sub-seafloor strategy?

Posted by Joseph Stoner on 2009-03-12 at 12:07

Following on the 'stability of cyrosphere theme' and the ultra-high resolution P2C2 stuff, echoing Alan comment, all tools need to be brought to bear. If we are to understand ice sheet dynamics in perspective of local, regional and global climate conditions, increased emphasis on site surveying will be needed to find the hidden sediment archives that dot complex marginal setting that will allow us to separate the various processes. The surveying is also needed not only to find and characterize these sites, but to link these locations to the processes that drive sedimentation. Better proxies are also required that will allow us to define glacial or sea ice processes and to accurately date these. Likely, different sites will provide high quality information on different time intervals or processes, even within the same region. Those that are best for deglaciation may not be

as optimal for glacial inception or maximum conditions, yet nearby locations maybe. And all may not require the same need for drilling technology, some could be maximized through long coring, others will need alternative platforms. If we look at this holistically it is very doable, but really requires science planning that asks the question of how we solve the problem, not what problem can we solve with ocean drilling. I can't think of a better way to do this than under the auspices of IODP using its resources, knowledge and experience to, as Alan said, develop a fully integrated sub-seafloor strategy.

Posted by Sidney Hemming on 2009-02-24 at 20:40

One thing I think would be a really worth effort, but maybe a challenge, is to target the major trough mouth fans- the ones I have dreamed about most are those in the North, but the same thing likely applies around Antarctica. These deposits may represent exceptional times of glaciation, but they are also great archives of integrated continental material as well as being places where there are glaciogenic debris flows interlayered with hemipelagic sediments that presumably provide potential for working out the timing. And by targeting the fans and transects off the fans, it should be possible to tie the land to the ocean for some of these important and sensitive areas.

Also, around Antarctica, as far as I can tell, the trough mouth fans seem to be mostly older than Pleistocene. Maybe people think they already know the reason for this, but I haven't been able to infer it from the reading I've done. And in general I think it would be really interesting to track the record of variation around Antarctica, from questions about whether there could have been fundamental changes in catchments areas between pre-glacial and glacial times and as the ice sheets evolved.

A3.6.3 *Cross disciplinary meetings and training*

Posted by Mitch Lyle on 2009-02-10 at 07:13

One of the key themes in paleoceanography for the last decade is to link models and observations, i.e. to compare what we drill with physically consistent representations of past worlds in order to detect inconsistencies and better understand processes. Can we develop this strategy more explicitly for the renewal?

Posted by Gabe Filippelli on 2009-02-11 at 15:52

This came up with a workshop that USAC supported several years ago bringing paleoclimate people together with standard paleoceanography folks (although the groups did overlap somewhat). The important lesson I learned from that workshop is that those beautiful high resolution drift sites that us folks prefer would not necessarily be a prime target for the modeling people. Instead, they would like to see a time slice approach spanning an array of latitudes/longitudes across a couple of critical interfaces in the ocean, in order to capture temperature, wind stress, etc.

This goes a bit back to CLIMAP does, but it also takes the GEOSECS approach, but through time.

Posted by Frank Rack on 2009-02-11 at 16:25

The paleoclimate summer school in Urbino is a prime example of this approach <<http://www.uniurb.it/usssp/index.html>>, these schools bring paleoceanographers and modelers together to understand their respective and often different needs and approaches while also exposing students to project development and proposal strategies that integrate data and models. We should expand support for these programs and think about how we can better utilize these types of meetings to provide new directions for scientific drilling integrated with modeling requirements. The outcomes from these activities can also help to inform the IPCC process and highlight the role that scientific drilling plays in providing primary data for hypothesis testing by modelers.

Posted by Mitch Lyle on 2009-02-12 at 12:55

This would be a different concept for the drilling program, in essence including a significant long-term commitment to shore-based research/education/meetings. The nice thing about a regular meeting like the Urbino summer school is that there can be different themes to help coordinate recent fieldwork (i.e. drilling) with other observations and modeling.

Posted by Christina Ravelo on 2009-02-13 at 00:43

I like the idea of student training, but I also think that there should be community meetings (I don't really think of Urbino as a community meeting, is it?) to provide the new directions that you are talking about. Collaborative efforts between modelers and observationalists have already enriched some IODP projects, but we should add structural elements or built-in opportunities to foster more integration between the two communities (modelers and data-people) interested in the same problems.

Posted by Mitch Lyle on 2009-02-16 at 14:51

I agree with the idea of a community meeting--maybe an annual symposium/workshop with plenty of room for students.

Posted by Frank Rack on 2009-02-18 at 13:20

I agree that community meetings are important and support expanded effort to bring IODP scientists and modelers together. The Urbino school is supported by ECORD as well as other groups, so I don't see these as mutually exclusive approaches. I guess I was somewhat blurring the lines between support provided by USSAC and programmatic support funded through IODP directly, but I think we need both approaches to really galvanize community engagement and encourage the next generation of students and researchers to understand and integrate data and models throughout the life cycle of a proposal/project.

Posted by Ellen Thomas on 2009-02-24 at 20:59

Urbino concentrates more on older parts of the record (Paleogene, Greenhouse Earth), and the main idea is education. But in fact, if you throw groups of people with these various specialties together it can't help but becoming some sort of community get together where you can throw ideas around. I have particularly enjoyed working with not just climate models but earth system models - in my opinion that really helps figuring out where data are needed in order to do useful things with the models.

Posted by Bradford Clement on 2009-02-25 at 07:38

Gabe has a good point here. Back in the '90s, the Ocean History Panel worked on a strategy of obtaining age-depth transects in each ocean basin with the goal of obtaining a geographically distributed set of records of how water masses changed through time. It may be time to recast that strategy with the goal of providing some much needed constraints for the climate modelers.

Posted by Adina Paytan on 2009-02-28 at 12:32

I agree with the above comments paleoceanographers (observations) and modelers must work together and may be a community meetings is the way to initiate this effort. While the Urbino school is fantastic it exposes only selected individuals in the community and many more can benefit from such a conversation.

Posted by Debbie Thomas on 2009-03-02 at 12:42

Adina is right here – while Urbino is a spectacular experience, a better strategy would be through coordinated community meetings and collaborations dedicated to more direct and intensive model-data integration.

Posted by Marta Torres on 2009-02-28 at 17:17

Great discussion on the role of modeling in paleoceanography, and I agree with the idea of meetings and student training to fully develop this approach. I want to expand this idea though, to all other fields within the drilling community, for example: geochemistry, hydrology, microbiology, and the response of these to changes in a wide range of time scales and spaces could become powerful contribution of the new program. How to best integrate models in all aspects of a project, from proposal to post-cruise results? Having a formal pathway to accomplish this on an ongoing basis, with student training opportunities will move many of the IODP research arenas to new and higher levels.

Posted by Rick Murray on 2009-03-05 at 20:25

I support this concept fully, as the spatial and temporal coverage that the modelers provide is really what we more "traditional" (whatever that means) mud-slingers need, and the modelers certainly need us for prime data input and groundtruthing. But...here's an observation...have any modelers chimed in on this subject on this message board? I think the answer to that is 'no'. How do we engage them better?

My feeling is that this overarching issue won't necessarily be solved by more or better drilling, but more in the ways bandied about here, namely, getting people together in the same room. Having said that, however, how many workshops in a year can we all handle? What is really needed is to build departments that include modelers and mud-folks together...all too often, either for analytical or computational critical-mass reasons, we tend to naturally self-hire within our own comfort zones, instead of reaching out to the other community as we build our own programs. There are notable institutional exceptions to this, of course, and many of you (perhaps non-coincidentally) are at those institutions, but until we as a community begin to interact on the daily basis I think we're doomed to only really do this at sporadic workshops and special sessions.

A4 DEEP BIOSPHERE AND SUB-SEAFLOOR OCEAN

A4.0 Introduction and overview

The Initial Science Plan for IODP identified three primary scientific themes for exploration, the first of which was "The Deep Biosphere and the Subseafloor Ocean." This theme was envisioned to comprise studies of (1) microbial populations, energetics, and dynamics within deep sea sediments and volcanic crust; (2) processes and properties responsible for massive fluxes of fluids to, within, and from the lithosphere, and the influences of these flows on a broad range of tectonic, volcanic, biological, and biogeochemical phenomena; and (3) the formation and impacts of gas hydrates as geohazards and agents of global change.

Introduction

Posted by Organizing Committee on 2009-01-12 at 14:26

In a 1992 essay, Thomas Gold postulated the existence of a “deep, hot biosphere”, supported by geological energy sources. A later study by Whitman and colleagues expanded this provocative assertion by collating all available data on aquatic, soil, and subsurface prokaryotes, and concluded that the majority of biomass on Earth may be harbored in the subsurface. There are enormous ramifications of a hidden biosphere, deep below the ocean floor, including implications for global carbon (and other elemental) budgets and cycling, the origin and evolution of life on Earth and the possibility of life on other planetary bodies, and potential to discover new organisms, biomolecules and survival strategies that could be of benefit to humans. Much as the DSDP (Deep Sea Drilling Program) helped to prove the existence and explain the mechanics of plate tectonics, scientific ocean drilling within the IODP is helping to support fundamental discoveries involving the deep biosphere.

The importance of the subseafloor ocean has been highlighted at numerous scientific meetings and in documents generated from these discussions. Ongoing research within IODP includes exploration of marine hydrogeology at convergent margins, near and on the flanks of mid-ocean ridges, on continental margins, and at mid-plate locations far from the influence of active tectonic or magmatic processes. Much of this work has moved beyond traditional documentation of the geological record of hydrogeologic processes, and now involves attempts to measure active processes, and associated properties and driving forces, to develop a mechanistic understanding of where, how and why vast quantities of fluids move within the seafloor, and how these flows influence a myriad of other processes and systems.

Recent breakthroughs in the study of gas hydrates have also pointed to the critical roles played by subseafloor hydrogeologic processes. Fluids advect heat, and thus have a strong influence on the conditions under which gas, fluid, and solid hydrocarbon phases are present. In many systems, the maintenance of hydrate deposits over the long term requires a continuous input of new gas, often associated with microbial activity at depth. More broadly, the study of gas hydrates is at the intersection between hydrogeology, energy supply, climate change, microbiology, and geohazards. The necessity of an

interdisciplinary approach is consistent with the structure and practice of scientific ocean drilling.

New Approaches

Posted by Organizing Committee on 2009-01-12 at 14:20

Are there new ways of asking questions, and of doing science, that will allow IODP to make rapid and significant progress in these fields during the next phase? How can IODP and related programs become more integrative and cross-disciplinary?

A4.1 Subseafloor ocean

Posted by Liz Screamton on 2009-02-13 at 07:12

Looking back over IODP results so far, I was impressed by the variety of activity and results concerning subseafloor fluid flow. The subseafloor ocean was a primary target of 308 (GOM Hydrogeology) and 301 (Juan de Fuca Hydrogeology), a vital component of the Cascadia Gas Hydrates leg (311). I have also seen fluid flow results from 304/305 (Ocean core complex), and there is a lot of ongoing hydro-related work from last year's NanTroSEIZE drilling. In addition, results are still coming out of ODP, such as from Hydrate Ridge (204), Costa Rica (205), and the Peru Margin (201). I'm sure I've missed others. My overall view is that we are building up good understanding concerning fluid flow, especially in those questions that can be answered by drilling and cores (with slower progress in questions requiring observatories). I'm not aware of any large paradigm shifts. Other opinions?

Posted by Robert Harris on 2009-02-19 at 18:41

I agree that much progress in understanding subseafloor fluid flow has been made through ODP and IODP. However I think there are many important questions that remain. An interesting question to me is how subseafloor fluid flow evolves as the crust ages. For example a popular model for the cessation of advectively significant fluid flow calls on a decrease in basement permeability, yet compilations of upper crustal seismic velocity, permeability measurements, estimates of bulk density, etc., suggests the bulk of crustal evolution occurs within the first 10 Ma. What are the relationships between basement permeability, sediment thickness, and basement relief in influencing the evolution of advectively significant fluid flow? Admittedly this is not a question addressed purely by drilling, but basement drilling (and perhaps observatories) would be an important component.

Posted by Liz Screamton on 2009-02-21 at 19:18

Good point. The extent and importance of crustal flow is turning out to be larger than expected --- going further into subduction zones (such as the recent Spinelli and Wang modeling concerning thermal effects on seismogenesis) and dissolving overlying carbonate sediments (such as in the 2007 Bekins et al. Geology paper).

Posted by Andy Fisher on 2009-02-23 at 20:38

I've got a bias here, but I'll make the pitch anyhow. A site survey for Exp 301, and follow up modeling, showed for the first time that we can track fluids at a crustal scale, from

recharge to discharge across 50 km, determine how these flows influence and are influenced by related processes. This work helped to show the importance of the 100k seamounts and other basement outcrops dotting the seafloor in channeling massive fluid, heat, and solute fluxes. The drilling, observatories, and follow up experiments from Exp 301 showed that we can run cross-hole experiments across distances of km in the crust, and suggested that basement permeability may be anisotropic. These are all "firsts" related to IODP, none of which could have been demonstrated from earlier work. The other thing that is interesting here is that the technology that made all this possible is, essentially, vintage late-ODP. In other words, we have the tools and understanding to extend these results to other areas, find out if they are representative, learn how they vary with age, spreading rate, sediment thickness, basement temp, etc. We could not understand subduction from drilling only Barbados or Nankai or Costa Rica. Same holds for ridge flanks. IODP has made solid progress, but we have also suffered from the partial drilling schedule and budget limitations.

Posted by Brandon Dugan on 2009-02-25 at 10:06

I agree, the contributions of DSDP/ODP/IODP to sub-seafloor hydrogeology have been enormous, but we still have lots more to learn - both at the basic science level, and at the technological level. While hydrogeology has been a major component of many recent ODP/IODP expeditions, much of what we do to understand hydro (pressures, temperatures, permeability, storativity) are not routine for the sci/tech staff especially when it comes to long-term observatories. So I think a technical education of the proponents is key to making sure the planning and implementation of hydro tests/measurements gets the best data it can. From the scientific level, I would like to see a linkage with onshore hydrogeology. In most studies (and funding agencies) the continent/ocean boundary is effectively a permeability barrier and studies do not cross them. Sub-surface fluxes from the continent to the sub-seafloor may be small today but are poorly constrained and they definitely change as driving forces change (e.g. topography, sea-level, permafrost extent). These fluxes may create local and important changes in ocean chemistry as well.

Posted by Andy Fisher on 2009-02-26 at 00:44

Excellent points. It would be great to see hydrogeology programs cross from land to the ocean. How does the crustal transition influence hydrogeology? How far out did the fresh water get during the last glacial? What are those fluxes moving from continents to the oceans in different environments? Lots of questions to be resolved.

Posted by Alicia Wilson on 2009-02-26 at 15:56

We really do need to get the linkages to onshore hydrogeology. We know where the freshwater/saltwater interface is in approximately 4 places *in the world* -- and in those places it is pretty far offshore. Also, the oceanographic submarine groundwater discharge (SGD) community has shown that there is huge fresh and saline fluid exchange in nearshore areas (i.e. easily accessible areas), but there is increasing evidence that there may be significant SGD farther offshore, too (again, fresh and saline). We get some volume estimates from the chemical oceanographers (Radium quartet) -- but we simply don't know enough about the composition of the porefluids or the flow systems involved to say very much about chemical fluxes to the ocean.

As to communicating this to people other than the choir: there is always somebody who says that the flow in continental shelves is 'slow,' so why care. (I once heard a very astute NSF program manager describe Pleistocene freshwater preserved in continental shelves as 'water in jail.') But really the issue is that we just don't know the residence time of fluids (fresh or saline) in continental shelves.

Again related to communicating the importance here: you can't go too far down the petroleum route without becoming unfundable by NSF (sedimentary diagenesis is so 80s), but carbon budgets are very fundable and pretty much unknown in these systems. Basically any chemical budget you choose is unknown for these systems. Can we sequester CO₂ down there? How fast do gas hydrates form? Does the presumably high concentration of Sr in continental shelf fluids (compared to river water) affect the ocean Sr isotope record? (The lore for all that kind of thing was developed before we thought there was any flow in continental shelves...)

And if all else fails...80% of the sediments in the world are currently stored in continental shelves, slopes, and rises. Probably worth some investigation.

Posted by Barbara Bekins on 2009-03-05 at 16:40

All good points. I'd like to follow up on the issue of the types of measurement and approaches that are specific to hydrogeology. For the first time we have a trained hydrologist at TAMU, Liz's student, Kusali Gamage. She brings expertise in lab permeability measurements, modeling, and subduction zone fluid flow to the organization. This expertise is critical to have at TAMU going forward to the next phase. Shipboard temperature measurements have been added as a routine requirement but that will need to be monitored in the future because of concern by co-chiefs about the time required away from their primary goals or other logistical concerns. I think the biggest techniques we are missing now are routine permeability measurements and a device like the proposed SCIMPI to cheaply monitor fluid pressures in sediments without having to case a hole. Another area where we need improvement is comparing field data to past model results and using the results to guide the next iteration of field studies and models. Because modeling is central to hydrogeology, we should be insisting that proposed work adequately cover the existing model results and the questions they raise before moving forward. The panels understood this in the past when, for example, the Leg 190 proposal stalled until modeling was added. The Hydrogeology PPG Report provides text explaining why modeling is central to hydrogeology. Something like this text is needed in the new plan to explain how the combination of models and targeted measurements that test the models are used in combination in hydrogeology. Such text might make the point in a manner clear to the nonspecialist that it is almost never premature to implement a numerical version of a simple conceptual model.

Posted by Julia Schneider on 2009-03-06 at 11:05

I agree with Barbara Bekins on the issue of types of measurements and approaches specific to hydrogeology. As also pointed out in section Enabling Technologies, improvement of in-situ measurements like in-situ stress, temperature, and fluid pressure as well as routine permeability measurements are essential to advance hydrogeological

studies. Unfortunately, these techniques are not routinely performed yet or still show a low success rate (e.g. pore pressure penetrometer measurements). So far permeability is often either derived or determined in expensive and time-consuming laboratory experiments. This becomes even more difficult as the permeability gets lower. These methods yield low sampling frequencies and it is not trivial to establish a good in-situ permeability versus porosity and in-situ permeability versus depth relationship. Problems like scaling issues between field and laboratory are present as well. However, permeability which can easily vary over several orders of magnitude is a key input to basin models. I also think that an improvement of the linkage between these existing numerical models and field data is important for the future of IODP.

Posted by Barbara Bekins on 2009-03-05 at 17:16

There has been very little discussion of subduction zone pore pressure and fluid flow issues. The major questions about the role of fluid flow in geochemical cycles and the role of pore pressure in fault strength, tsunami risk, and size of characteristic earthquakes are still unanswered. The fact that there have been no major paradigm shifts means that the hypothesis that pore pressure plays a central role in subduction zone seismic hazards is still on the table, but also not yet proven. Evidence of the central role of fluids in these systems continues to accumulate. There is growing list of places where harmonic tremor has been measured at the downdip limit of the seismogenic zone and now velocity data show that overpressures are present in the ocean crust at the hypocenter of the harmonic tremor. Flow is recognized as central to gas hydrate accumulation and other aspects of the fate of carbon in subduction zones (CR mud volcanoes). The data on subduction inputs, residence time, reaction kinetics, permeabilities, and temperatures are slowly being assembled and added to models. Progress has been made, but the task is large and the hypothesis remains just that. We need to formulate the *specific* goals needed for progress in understanding the role of fluids in subduction zone processes in the next phase. The drilling program can contribute specifically to measuring input materials, permeabilities, and boundary conditions of temperature and pressure. In addition, measuring composition of fluids flowing out of the system is a crucial test of the flow and reactive transport models that describe reactions beyond where we can reach with drilling. This needs to be clearly conveyed to the non specialist. Paleoceanography has clearly established that we can make inferences about deep time based on present day sediment records. I think we have not been so successful in establishing that we can make inferences about unreachable depths based on near-surface fluid measurements.

Posted by Liz Screamton on 2009-03-06 at 06:58

Agreed. My original statement was not meant to downplay the need for future research, but to say that we were on the right track in the ISP.

Following up on your post, I suggest that we're at the point when we can start to outline a community modeling effort -- establishing the crucial processes/reactions to be included, and highlighting data needs. I don't think it necessarily means all use the same code/software, but benchmarking would be useful as well and perhaps code sharing so efforts are not repeated.

A4.2 New scientific questions

Posted by Organizing Committee on 2009-01-12 at 14:20

What new questions have become apparent based on initial IODP and related studies, and/or based on progress in general in subseafloor hydrogeology, microbiology, and hydrates?

Posted by Radu Popa on 2009-03-02 at 01:15

Subsurface flow and global buffering. To the scientist interested in global geochemistry, but equally important to a non-scientific audience (public or decision maker alike), the ocean's subsurface can be presented as a sizable bio-geochemically re-active interface. If one considers the global subsurface fluid flow and the volume of exchange with the hydrosphere, studying, monitoring and modeling the capacity of the subsurface to buffer chemical changes produced by the surface biogeosphere should become a more important component of the future IODP activity. To cite only a few chemistry altering processes caused by the disequilibrium between the subsurface biogeosphere and the surface fluids (atmosphere/hydrosphere): changes in pH due to interaction with basalt glass, CO₂ sequestration by subsurface microbial activity, Mg/CaCO₃ precipitation on alkaline surfaces, sequestration and changes of organic carbon and organic nitrogen, O₂-uptake, release of reductants and micronutrients.

One sees here an interesting package of topics, research avenues and monitoring frameworks addressing a set of important topics. To what extent (and in what time framework) can the global subseafloor circulation act as a buffer for changes of the surface water and air? What are the major chemical changes, turnover time, buffering capacity, and predicted historical trends? What changes produced by subseafloor circulation are of global relevance? Is the infrastructure and present activity of IODP up to the task of addressing such global questions to a significant level? Can this infrastructure be improved toward achieving a satisfactory network of monitoring programs and observation stations worldwide?

Posted by Mitch Schulte on 2009-03-03 at 13:31

These are good questions. One thing that tends to get overlooked from a geomicrobiology point of view is the interaction of life with the geology; there clearly are direct and indirect influences of microbiology on geochemistry, and understanding the flow of elements between the biosphere and geosphere is crucial to understanding what's going on in the deep crust.

Posted by Peter Kelemen on 2009-03-03 at 00:33

I've just posted something on the Emerging Fields and Cross-Cutting Discipline Topic, in the sub-topic on CO₂ mitigation, that I think has a lot of synergy with scientists interested in the deep biosphere and early life. This is the idea of CO₂ capture and storage (CCS) via accelerated carbonation of mantle peridotite, as a relatively rapid route to solid storage. There is some pretty specific IODP research one would consider in this context, involving drilling in shallow seafloor through sediment into peridotite, for example offshore of Oman, New Caledonia, and/or Papua New Guinea. There would be a lot of synergy with studies focused on the deep biosphere and early life, in the sense that lots of

people have concluded that generation of H₂ during serpentinization of peridotite may have provided an energy source for early life on Earth and elsewhere, and that serpentinization may still provide a niche for a unique deep biosphere preserving genetic clues regarding the nature of early life.

Interested readers can go to

<http://www.ldeo.columbia.edu/files/uploaded/file/2008%20Kelemen%20&%20Matte%20PNAS+.pdf>

to see what we are thinking about. With regard to seafloor science, Figure 8 in that paper is particularly relevant. What's needed is characterization of present day alteration, fluid flow, reaction rates, porosity, and permeability in 500 meter to km-scale holes, and then hole-to-hole experiments involving hydrofracture and - perhaps - pre-heating a rock volume at depth and then - perhaps - injecting pure CO₂. Each of these steps would be undertaken in order to "jump start" hydrothermal systems like Lost City, and - if necessary - accelerate them. The idea is to enhance the natural peridotite carbonation system, doing as little as possible while consuming as much CO₂ as possible. Site survey requirements, particularly assessment of depth from seafloor to peridotite, and nature of overlying sediment, are obvious and do-able. In some cases, oil companies and others may already have obtained such data. Note that, while JR style drilling might be desirable in some cases, drilling from alternative platforms including small islands and even jetties might be more effective, and cheaper, in other cases.

Posted by Radu Popa on 2009-03-03 at 21:53

Totally agree here. As far as the public/politicians/decision makers curiosity goes ... CO₂ sequestration, methanogenesis and methane clathrates are probably some of the most relevant topics. Since the subsurface circulation of water is a major contributor in this equation the next obvious questions to dwell in are: how large is this water flow, what restricts it, what would happen if the overall flow was larger, can it be assisted, can the subsurface microbial community handle a larger water flux (i.e. are they underfed), are there any systems with punctual inlets near outlets that can be used in experiments and models?

Posted by Rick Colwell on 2009-03-13 at 19:38

Yes, there is great overlap in the geologic and biologic areas here. How do microbial communities change - diversity, biomass, activities - as a result of the input of CO₂? Are there sentinel species that become apparent as a result of CO₂ placement in the system? The microbes may not be active within a CO₂ puddle but they will occur on the fringes and could look like communities observed by Inagaki et al 2008 (PNAS 103 (38): 14164-14169).

Posted by Alberto Malinverno on 2009-03-03 at 16:23

Microbial methane generation and gas hydrate formation. A major source of methane for gas hydrates is microbial methane produced in situ within the sediment column. We should be able to learn something new by combining microbiological studies of methanogens, geochemical data, observations of gas hydrates in sediments, and diagenetic modeling that accounts for microbial methane generation. For example,

modeling results suggest that the reaction time constant for microbial methane production is very long (on the order of hundreds of kyr); can these slow rates be confirmed by experiment? Also, microbiological studies may shed some light on the relationship between the quality (rather than the quantity) of organic matter and microbial methane production. There are significant challenges, but this seems an area where a collaboration between microbiology, geochemistry, modeling, etc. has great potential.

Posted by Timothy Collett on 2009-03-04 at 18:34

I agree with Alberto that in recent years we have seen the evolution of an integrated science approach to our understand of gas hydrates in nature. The concept of a gas hydrate geologic (petroleum) system, is gaining acceptance; in which the individual factors that contribute to the formation of gas hydrate can be identified and quantified; these include gas hydrate pressure-temperature stability conditions, gas source, availability and nature of the formation waters, gas migration mechanisms, the growth of the gas hydrate in suitable host sediment , and others. Dedicated science drilling has contributed significantly to our understanding of the geologic controls on the stability and formation of gas hydrate in nature.

A4.3 Microbiology within IODP

Posted by Organizing Committee on 2009-01-12 at 14:22

What advances in geobiology are needed to further the 'microbiology' agenda for IODP, and how do we make microbiology more of a primary focus of IODP projects? What can be done to expand the IODP microbiological community of researchers, educators, and technology developers?

Posted by Jennifer Biddle on 2009-02-05 at 13:11

One general advance that needs to be made to further microbiology for IODP is in the area of microbial detection. Already we are finding that the most examined sites still have more to explore, part of this is due to the slowing evolving fields of environmental microbiology in terms of sensitivity and detection abilities. Of course, general microbiology advances are greatly needed, such as cultivating environmentally relevant microbes and defining genomic identities of subsurface clades. In order to make microbiology more of a focus and also expand the community - I hate to admit this - but I think a generous funding structure is the way to do it. Microbiologists have many areas to draw on for funding sources, as such they are a difficult group to herd. One commonality though, is they tend to gather around funding.

Posted by Peter Girguis on 2009-02-06 at 18:04

This is a good point. I think that we should start by A) extensive (cleaner) sampling, followed by B) additional lab studies. What we do in the lab will form the basis for whatever we want to do in situ.

Posted by Katrina Edwards on 2009-02-09 at 01:02

What about vice versa? i.e., letting what we want to do in-situ drive what we do in the lab in terms of testing?

Posted by Jennifer Biddle on 2009-02-10 at 18:26

I think that type of approach would be good in some ways, especially in determining pressure adaptations. Doing something in-situ that determines maintenance energies would be good also.

Posted by Jan Amend on 2009-02-18 at 15:43

There are very provocative hints at a vast subsurface biosphere. Certainly one of the big unknowns concerns how active this community might be. Figuring out maintenance energy requirements for microbial life is critical. This also brings in theoreticians and experimentalists, biochemists and geochemists to this fundamental microbiology topic.

Posted by Beth Orcutt on 2009-02-08 at 13:43

In addition to improving methods for DNA-based methods for microbial detection as discussed above, the geobiology community would benefit from targeted funding, perhaps outside the IODP framework, for improving technology for microbial metabolic activity detection in the deep biosphere in conjunction with observatories. This would include development of downhole and seafloor sensors, and also for development of innovative ways to power and download data from such sensors during long-term deployments.

Posted by Katrina Edwards on 2009-02-09 at 01:07

Funding is really tough for IODP related work and particularly for the biological sciences, which often come in with higher price tags than is compatible with traditional geo-science work. i.e., jamming the new bio-aspects of IODP into ODP-designed funding structures (geo-based) within the NSF is a bit of an enlarged square peg-round hole scenario. How to fix?

Posted by Beth Orcutt on 2009-02-08 at 13:43

In regards to expanding the community of microbiology researchers, I believe that training events such as the recent ECORD student workshop on the deep biosphere held at the University of Bremen are important first steps to educate researchers about the microbiology agenda and associated issues. I could imagine that a similar event that was open to all researchers (not just students) and promoted across discipline would help promote microbiology science within the IODP framework. Another avenue to promote microbiology science in IODP involves stronger public outreach (public including non-IODP scientists as well as the 'general' public) about deep biosphere science (although this currently may be difficult since the last microbiology dedicated leg was in 2002, it should be a strong component of future microbiology-related drilling campaigns).

Posted by Katrina Edwards on 2009-02-09 at 00:37

I rather agree that workshops like the recent ECORD deep biosphere workshop are important first steps - one thing I recognized as a lecturer with many of the *deep biosphere* active researchers in the field was that the dialogue among faculty was amazing - as part of the lectures. The student participation in this dialogue was hit or miss - to be expected - but they got to witness the people in the trenches discussing and debating many critical issues and important frontiers. the thing i was bummed about, and

continue to be, is how hard it is to raise money for international meetings - ECORD was a European workshop and it was very tough for US participants to come as students. I know there is interest in the US of doing a similar US meeting - but why not work towards reducing the barriers to making these workshops and meetings as truly international as the IODP is?

Posted by Andreas Teske on 2009-02-09 at 17:03

A crucial issue is to train and to involve more microbiologists in IODP activities. A first step could be IODP-themed sessions at meetings such as ASM or ASLO (hint: ASLO 2010 in Santa Fe). Focused workshops such as ECORD are great and necessary, but we also need to broaden the base and attract new students, postdocs, and researchers at all levels.

Posted by Julie Huber on 2009-02-10 at 08:20

I think it all goes back to funding. We can attract the best students, the best postdocs, etc. but if NSF doesn't have the funds to support that community, then we're out of luck. Finding a home for deep biosphere research at NSF is tough; NASA used to be a good spot, but their support of microbiology is less than stellar of late. We need to convince our funding agencies that deep biosphere research is important. Perhaps broadening the base is the way to go- more demand, more return?

Posted by Katrina Edwards on 2009-02-12 at 23:25

Supply and demand. If we remain a niche market we only tap niche funding and we remain obscure. Broadening the base I think is critical!

Posted by Rick Colwell on 2009-02-18 at 02:09

NSF should be alerted to deep biosphere interests. Certainly the DEBI RCN demonstrates a commitment of the community and the efforts that are rallying around the Deep Underground Science and Engineering Lab (DUSEL) may mirror the IODP. DUSEL is a geographic stretch from IODP efforts (so distant in South Dakota, so landlocked) but there may be intriguing and parallel microbiology questions that could be asked in the respective environments.

Posted by Jim Cowen on 2009-02-18 at 15:11

Right. There is strong interest in the deep subseafloor biosphere in IODP, other parts of NSF, still at NASA and within some of the private funding foundations. However, some of this interest is still 'cautious' or soft. IODP, for example, demonstrates this interest in its science plan and at the various committees (USAC, expedition staffing, SEP, etc.), but economic and competing science has so far allowed only a single 'microbiology'-oriented expedition, but has resulted in dedicated microbiology facilities on the JR and a (soft) commitment to try to staff many expeditions with a microbiologist. We need to continue to motivate IODP, the rest of NSF and other agencies to elevate the practical status of the Deep Biosphere; we need to convince the larger subseafloor geoscience community that the deep biosphere's time has come. 'RIDGE' science traveled a similar path: early years, it was totally dominated by geology/geophysics; this domination continued with 'RIDGE' but the biosphere was given significant air-time; the hydrothermal biosphere really came

into its own and dominated RIDGE2000. Elapsed time >15 years. We are poised for action here.

Posted by Jan Amend on 2009-02-18 at 15:56

One approach that has worked well for NASA science, especially as related to astrobiology, is a clear, simple, exciting focus -- everyone bought into 'follow the water'. Perhaps one or two common foci could benefit and unify the subsurface biosphere community of scientists as well. One example might be 'follow the energy'. All life, and certainly subsurface life, requires energy; and in this case we're not thinking about solar radiation, but redox disequilibria.

Posted by Andy Fisher on 2009-02-23 at 20:11

Great suggestion. I think that "follow the water" could be a great approach for an IODP focus on marine hydrogeology.

Posted by Marta Torres on 2009-02-28 at 17:37

Way to go Andy and Jan! I fully agree with you, follow the water can be a key theme to link several disciplines that beg to be linked- hydrology-chemistry-microbiology that need to be thought of in all cases with a combined approach in mind

Posted by Beth Orcutt on 2009-02-23 at 19:19

An idea for getting more technology developers involved in IODP microbiology - posting somewhere a 'wish list' of gadgets that the microbiology community could benefit from, but that currently don't exist. With such a list available, we could approach engineering programs that are looking for activities for students to work on, or maybe existing companies/engineering groups would answer the call and develop things on their own, assuming future use. The engineering involvement in designing high pressure core recovery systems for gas hydrate research has been successful, and I hope a similar connection could be made for other IODP deep biosphere needs.

Posted by Rick Colwell on 2009-02-25 at 12:55

I like Jan's idea about specific themes within the deep seafloor biosphere area that could focus (and inspire?) a group of scientists. And also Beth's consideration of how to get technology developers involved with a wish list. All of this gets me back to a consideration of how we can communicate our excitement about this to microbiologists - and others - outside of IODP. When I give a seminar, I rarely go into much detail about IODP and the process of getting the samples or becoming a part of a cruise or the program needs, etc. On reflection, this is really a missed opportunity. I know that a recent seminar by Katrina accomplished this really well. Of course, the talk mostly focused on her research and directions. However, once we were all into it she spent a slide or two talking about the Dark Energy Biosphere Initiative (DEBI). Maybe this is a model for us. Whenever we talk about our science in a seminar, be sure to chuck a couple of those extra data slides (no one will miss them, really) and put in a few that speak to our interest in the IODP program and where it is going. Sure, we need to be realistic about current funding, etc. Some ideas:

- the science that has been/is being accomplished,

- IODP events on the horizon,
- IODP microbiology needs,
- specific themes within the deep biosphere area,
- technology wish list with examples of past success

We might circulate, refine and share these slides so that we've all weighed in and we're all in harmony.

Posted by Jan Amend on 2009-02-27 at 10:51

Again, I'll refer to a NASA strategy: excellent graphics showing mission planning for the next 10-15 years. These visuals are used by many planetary scientists at conferences but also invited seminars, and they really get the word out -- and often a unified word. You hear something often enough (backed by visuals; 1 picture is worth ...), it becomes part of the field and will be discussed. As Rick alluded to, DEBI can help. DEBI can be the vehicle through which slides for powerpoints can be created, shared, modified, etc. Many NASA mission talks use a slide that shows current missions, planned missions, and future hopeful missions. Something like that could also work well for us.

Posted by Jason Sylvan on 2009-02-27 at 13:31

I agree with Jan's idea of developing a few ideas that we can push as a group. I don't think it should be too hard to interest the public in this type of work, partly due to all the unusual sampling strategies needed. As a newcomer to the deep biosphere, I have found people more receptive to the work I do now since it involves mysterious organisms that one needs submarines, robots, and drilling ships to get to- very exciting! The public outreach is crucial- public interest can only lead to benefits, including more people going into the field and greater funding. Public interest will also serve Beth's comment, which is absolutely true. Just like over the last decade microbiologists have enlisted the use of computer scientists to help them make sense of the huge amount of molecular data collected, we need the help of engineers to meet our sampling needs. How is one to look at molecular activity (gene expression), which can change on the course of minutes to hours, if we don't have ways to sample in situ? These samples take too long to reach the surface if we want to do those kinds of studies and we need engineers to help us devise efficient observatories for the deep biosphere.

Posted by Marta Torres on 2009-02-28 at 17:46

Not being a microbiologist I may truly be stepping on quick sand here, however I want to throw in this comment as a chemist looking from outside your field. What I have noticed in the recent times I have sailed is a lack of general agreement within the microbiological community as to what are the key fundamental things that need/should be measured in a routine basis on the ship, similar to the list of measurements/protocols we chemists have to follow out there. Even though the list is not complete, and each cruise/project requires a specific set of analyses/sample treatment etc, having a "must do" minimal list of tasks have resulted in the acquisition of a data-base that is common across the expeditions and have proven tremendously valuable (here kudos to Joris Gieskes and other pioneering geochemists who got this going). This has the advantage that there could be a technician onboard that is trained on this basic analytical needs (sampling, standard measurements),

ensures supplies are kept on stock and that even in legs that may look "microbiologically boring", some basic data would be obtained to generate a valuable database. I understand that the new ship has a brand new microbiology facility, a standard protocol as to how best make use of this on a routine basis is key and urgent, but should also be part of the plan for post 2013 science.

Posted by Martin Fisk on 2009-02-28 at 18:07

Also being in the "not a microbiologist" club, I think this is a grand idea. Routine measurements will require us to first define the major questions. Are these, "What is the microbial biomass?", "What is the microbial diversity?", "What are the energy sources?" "Can we culture and isolate them?". (Sorry, if I am repeating questions that have been asked elsewhere in the dialog.) My guess is that these questions require too much work to be addressed by routine shipboard analyses, but with technology advancing at a breakneck pace, maybe not. An alternative is to preserve samples to address these questions at home institutions. Another side of this issue is do we worry about questions we have not thought to ask or do not know enough to ask. Perhaps preserving samples for future microbial studies is all we can do at this time.

Posted by Beth Orcutt on 2009-03-01 at 14:32

Marta, you bring up an excellent point, and one that I feel deep biosphere microbiologists agree with - I, for one, would love to see routine microbiology measurements beyond cell counts. I believe that one reason such a list of 'must collect' microbiology samples hasn't been developed yet revolves around the fact that deep biosphere microbiologists are still trying to determine the best way to collect samples and analyze them without bias. Experience with analyzing sedimentary samples from the ODP Leg 201 and other legs by multiple researchers has revealed disparities in how different microbiological methods or protocols leads to different interpretations of the deep biosphere. Groups are currently working together to resolve the source of these disparities, with an end goal of developing a sort of 'best practices' guideline for collecting and analyzing drilled samples for microbiology. Additionally, the microbiology community is also trying to determine the best way to collect drilled microbiology samples for future shore-based analysis. We are learning that simply bagging and storing samples is not robust, as communities change drastically during storage. Again, people within the microbiology community are actively working to address this issue and come up with suggestions for how samples should be handled on board for future microbiology studies. I think it is reasonable to assume that by the time of the 2013 renewal, a list of 'must collect', as well as a 'how to collect' guidelines, could be developed.

Posted by Jan Amend on 2009-03-05 at 16:26

Marta, you bring up a very interesting idea, but one that has both pros and cons. I think that early on, and that is where we are now, a "must do" or "must have" minimal list would be great. It could get us up and running quickly and help us achieve some basic goals. However, we need to be cognizant that this list does not get interpreted later as the only thing we should do. It should clearly be only a baseline, a starting point.

Posted by Mitch Schulte on 2009-03-01 at 14:23

One thing we should remember as we try to insert microbiology and biogeochemistry more into IODP is that there really is only one way to understand the deep biosphere, and that is by drilling. Studies of hydrothermal vent systems help, but we're never able to observe biology directly in the subsurface. This can also be a big selling point for IODP and making microbiology a part of it. We have the opportunity to make discoveries on the order of those from the sea floor and perhaps revolutionize the field in a similar way. We have to be able to drill to do that, and be clever enough to devise ways to understand what's happening once we do.

Posted by Chewie Jin on 2009-03-04 at 13:00

Hi Everyone! I am studying kinetics of biogeochemical processes and very interested in the deep biosphere. After reading your posts, I would like to share with you my quick thoughts:

As Jan Amend stated, a theme with an inviting name would help sell the research of deep biosphere. In addition to "follow the energy" and "follow the water", I would add another one for you to consider – "follow the cycle". This is a topic NSF is interested in; the recent "Dear Colleague Letter" sent last month is one of the many examples. It includes cycling of carbon, water, energy, etc. Scientifically, it is an exciting topic: globally and socially significant, multidisciplinary, BUT relatively under-studied!

As Rick Colwell, Marta Torres, and Martin Fisk proposed, there is a great need of close communication between outsider and insider of microbiology. I can give you my own testimony. Sulfate reduction coupled to acetate oxidation is a significant process in cycling natural organic matter in anoxic environment. To evaluate thermodynamically the occurrence of this process and/or the magnitude (rates) of the process, we would need measurements of (at least) temperature, pH, and concentrations of acetate, bicarbonate, sulfate, and sulfide. Despite the extensive study in almost a century of duration, only a few studies measured these parameters simultaneously. >99% studies only reported the parameters of their own interest – sulfate concentrations.

At the end, in response to "to expand the IODP microbiological community", I would like to make a suggestion - foster young scientists who just start their career. (I am old now so this does not apply to me.) Young scientists will bring energy and new ideas, which help the community of deep biosphere grow strong.

Thanks for reading!

Posted by Rick Murray on 2009-03-05 at 20:48

To follow up on some aspects of this string, particularly that regarding "minimum measurements" and "routine measurements" and so on. During the end of ODP and the transition to IODP, I was the Chair of the Scientific Measurements Panel charged with all lab hardware, software, data bases, sampling/archiving protocols, etc., and we tried to do exactly as you suggest. I was heavily involved in this at the time as well because I was part of the Teske et al LExEn team that got the original JR microbio lab funded and in place. Some of the difficulties we faced, frankly, with getting routine measurements to be a truly active part of the protocol was the resistance from the rest of the drilling

community. This was not their "fault" but ours, because we did make clear ourselves why it was important to THEIR science too...we pretty much took the road of "well, heck, this is cool stuff, of COURSE it is needed". Thus, the paleoceanographers were concerned about losing still more whole-round material, the hard-rockers were concerned about even more legs (er, expeditions) being devoted to non-hard rocks, and so on. In other words, we as a large field (of scientific drilling) sort of outcompeted ourselves.

Things have gotten better. More microbiologists are sailing, although I don't see a consistent staffing strategy throughout the program yet, but it is trending right. We are also doing a better job explaining ourselves. The younger generation of scientists is intrinsically trained in interdisciplinary studies so they don't need to be as convinced as others did.

To help guide us strategically in our endeavor to make microbiology truly routine and to come up with a suite of minimum measurements (which we did finally do...is it in fact being followed?, if not, who is riding herd?), I spoke at length to Joris Gieskes, Frank Manheim, Miriam Kastner and others who either led the initial way or did herculean work in the porewater program through the years when DSDP initiated and codified their porewater programs. The single common denominator in their comments was "Get people to sea to do the work". Microbiology had this in their early days with Barry Cragg and others (who still do), but if this field is truly going to have legs in the IODP I strongly suggest (and this is a huge opportunity for folks as well) that the ONLY way to be sure your samples are going to be taken, or that a consistent protocol be in place, is to go to sea and do it. Expedition after expedition, leg after leg, ocean after ocean.

Posted by Steven D'Hondt on 2009-03-09 at 10:04

Progress with respect to minimum measurements and routine measurements is slow, but there's hope. Scientific Measurements Panel and the Science Planning Committee have identified cell counts and some key chemical concentration data (e.g., DIC concentration) as routine measurements. IODP-MI appears committed to the issue, but the data are not yet being routinely measured.

Posted by Liz Screamton on 2009-03-12 at 05:15

As a non-microbiologist and recent co-chief, I echo Rick's comments. A microbiologist needs to be at sea to not only collect the samples but to interact with the rest of the scientists and integrate the microbiology into the cruise results.

Also, as Rick says, one of the biggest concerns is the volume of core required. Any efforts the mbio community can make to minimize the whole round size and to maximize return on each sample through coordinated efforts, would help a lot. It is difficult to see whole rounds taken for any reason. One thing that helped on last year's Chikyu cruises was fast-tracking the intended whole rounds through the CT scan. This helped mbio and geochem know what they were getting and helped avoid loss of key structures.

A4.4 ISP remaining questions

Posted by Organizing Committee on 2009-01-12 at 14:20

The initial science plan for IODP described numerous hydrogeologically-active settings and listed a number of questions related to the Deep Biosphere and the Subseafloor Ocean. [Please find the Initial Science Plan for IODP here:

http://www.oceanleadership.org/files/init_sci_plan.pdf] How much of what was discussed in this part of the ISP remains to be addressed by IODP?

Posted by Beth Orcutt on 2009-02-13 at 12:08

A few things to still be addressed:

1) Better constraint on fluxes of heat and mass between the hard rock crust, sediments, and the upper ocean. Recent work by Hutnak et al. suggests that our previous understanding of how fluids move off-axis was inaccurate (i.e. their evidence suggests ginormous fluid flux in concentrated locals and far less conductive exchange). If focused flow is the case, this would likely have an influence on the relative size and activity of microbes in the deep biosphere. One part of the ISP indicated a need for measurements of the pathways and residence time of fluid movement, which requires long-term observation, but I don't think this has happened yet.

2) State of the microbial biosphere in deeper hydrothermal environments. Most sampling to date has relied on sampling of near-surface or expelled hydrothermal fluids. What is happening deeper down in the hydrothermal systems? The currently accepted temperature limit for microbes is 125C, but is this true in the environment?

3) Need for more sophisticated technology for drilling into unsedimented hard rock sites, like the Hard Rock Reentry System that has had limited tests so far.

4) What happens in old crust?

5) Is there an 'end' to the deep biosphere, or will microbes be found in every sediment and rock sample that is collected by drilling? if it does 'end', is it related to high temperature, pressure, something else?

Posted by Andy Fisher on 2009-02-23 at 20:17

I agree with Beth. A number of fundamental issues related to formation properties, driving forces, fluxes, and impacts were identified in the ISP and remain to be resolved.

Posted by Steven D'Hondt on 2009-03-05 at 15:15

Almost everything in the Deep Biosphere part of the ISP remains to be addressed by scientific drilling.

We don't yet know the extent of Earth's deep biosphere or the character of the life forms that populate it (ISP, p 25).

We don't yet know the extent to which weathering reactions traditionally viewed as abiotic are catalyzed by microbes (ISP, p 26). We don't yet know the extent to which thermogenic hydrocarbons fuel subseafloor life (ISP, p 26).

We have not yet defined the bounding temperatures, pH and redox potential of the deep subseafloor ecosystem (ISP, p 27).

We have not yet taken samples globally and in different tectonic settings to study how lithology and porosity, organic carbon content, and rates of sediment accumulation and depths of burial influence this ecosystem (ISP, p 27). We do not yet know the influences of tectonic settings on the structure, size and turnover rates of subseafloor communities (ISP, p 27).

We don't yet know the carbon and redox budgets for the deep subseafloor biosphere (ISP, p 27).

We have not yet provided robust constraints on the hypothesis that the deep biosphere is nourished by abiotic food sources (e.g., hydrogen or organic molecules produced abiologically at depth) (ISP, p 27).

We don't yet understand the trophic strategies, means of survival, & molecular, cellular and ecological mechanisms used by subseafloor communities. We don't know the processes by which these cells maintain viability. We don't know their community structures (ISP, p 27).

We don't yet know the role of the deep biosphere in biogeochemical cycles (ISP, p 28). We don't yet know the microbial role in the subsurface degradation of fossil fuels. We don't know the full extent and global scale of the interlinked cycling of carbon, sulfur, iron and other metals (ISP, p28).

Etc.

Posted by Rick Murray on 2009-03-05 at 20:55

To follow up on part of D'Hondt's concise and accurate statements, I would like to further point out that many of these questions can ONLY be answered by a truly coordinated, concerted global effort. I don't see quite yet a true embracing of microbiology within the IODP, although many IO's, institutions, and individuals are working very hard at it...too much of it is piecemeal. There is so much to learn, and the best way to do this is to get away from the expedition-by-expedition approach and have a concerted drilling effort, whereby an entire PROGRAM is approved and then scheduled out over x years (x could be 2, 3, 4, 5, 6, or ?). I'm not arguing to monopolize the ships at all, but rather to make the organizational commitment of 'y' expeditions over 'z' years. It is a different model of doing things, but should be seriously considered.

Hand in hand with this goes the issues of "routine measurements" and so on. I have written about this elsewhere in CHART.

Posted by Mitch Schulte on 2009-03-08 at 17:56

Is this because deep biosphere hasn't been a priority for IODP?

Posted by Steven D'Hondt on 2009-03-09 at 10:17

It's probably mainly because (1) very few expeditions have been mounted by IODP and (2) IODP is not particularly effective at meeting ISP objectives without dedicated expeditions. I think this area of study has been a priority for IODP as a whole, in that a few deep-biosphere expedition proposals have been ranked very highly. It could be a higher priority for IODP; no deep-biosphere expeditions have been scheduled yet. I hope that they will be scheduled, now that the Resolution is back in service.

Posted by Steven D'Hondt on 2009-03-09 at 10:21

It should also be recognized that the ISP deep-biosphere objectives are collectively very ambitious. Even if multiple deep-biosphere expeditions had occurred by now, they probably would not yet have fully met the ISP objectives (although they could have gone a long way toward meeting the ISP objectives).

A4.5 Enabling technologies

Posted by Organizing Committee on 2009-01-12 at 14:21

What enabling technologies and capabilities are necessary to facilitate major advances in these areas? Please note that there is a separate discussion on programmatic Strategies – for the present discussion we wish to focus on specific tools and methods that need to be developed, adapted, or sustained to answer critical questions.

Posted by Katrina Edwards on 2009-02-12 at 23:32

I think some of the "technologies" we need to think about are fundamentally tied to the "observatories" topic in the deep biosphere that is the part of another thread. but not all!! In particular molecular technologies - both molecular microbiological and organic geochemical - technologies that are crucial for the deep biosphere are developing or need developed for shore-based analysis. what are our obstacles?

Posted by Beth Orcutt on 2009-02-13 at 11:33

By analogy to the upper ocean, high-throughput culturing techniques have lead to major advances in understanding key microbial players in the water column (i.e. culturing of SAR11 and other microbes). Of course, these methods were developed for manipulating water samples, and they are not readily applicable to deep biosphere sediments, rocks and fluids. I think one 'enabling technology' would be development of a similar-in-concept method for high-throughput culturing of deep biosphere samples. For instance, if we really want to get a handle on the uncultivated alphabet soup of Archaea in the deep biosphere (the MBGB, MCG, DHVE clades, etc.), getting representatives into culture would be a big help. The recent work by Biddle et al. on metagenomic approaches to analyze deep biosphere microbial communities was very revealing, but has a high degree of sequence without known function or close attachment to known organisms, which I think can be directly related to not having the relevant microbes in culture. If we want to understand how the deep biosphere survives in this 'extreme' environment, having representatives in culture to examine protein expression, etc., would be a big help. Recent high-pressure vessel culturing developments (i.e. Takai et al.) I think are a crucial first step to overcoming traditional limitations in culturing microbes from the deep

biosphere. Of course, I cannot imagine IODP paying for this kind of method development, so again, we need to look to other programs for the support of these endeavors :)

Posted by Nick Hayman on 2009-02-24 at 20:41

One thing of great interest to the crustal community is the drilling of young ocean crust. I should think that the geobiology and hydrothermal communities will also rely on this if they wish to continue to encroach on the most extreme hydrothermal environments. Obviously RCB coring with recovery is (probably) out of the question for the upper sections of the young basaltic crust, but can we generate a stable hole for logging and study, and begin to recover core for geologic, chemical, and biologic analysis. (Obviously mixed results on the legs devoted to this in the past.)

Posted by Andy Fisher on 2009-02-26 at 00:41

Absolutely right. And this is possible now, with current technology. Holes can be created in carefully-selected locations to establish a presence below the seafloor. Coring might commence at depth. The ideal locations to "creep up" on the active ridge are places where flank sediment impinge on the spreading center, but where crustal creation processes are nominally normal. Examples include Alarcon Basin, northern JdF, probably some other spots if we looked carefully. 50-100 m of sediment allows installation of a stable casing, next step is to get down 200+ m, put in more casing. Core recovery is going to be a challenge in the young crustal environment, but this is no reason to avoid establishing monitoring points, and learn along the way how to do more.

Posted by Martin Fisk on 2009-03-01 at 21:14

Perhaps in situ incubation is covered elsewhere in this dialog. This seems to be a key to collecting and identifying the microbes that occupy the ocean crust. The devices IODP paid for and which we deployed in Holes 1301A and 1301B and recovered from 1301A (thanks in great part to Andy Fisher) might be a small step toward developing in situ incubators. These incubators were coupled to Wheat-Jannasch osmopumps so over four years there was a small flow of borehole water through the minerals in the chambers. These are passive samplers that must be retrieved to be analyzed, but there is also potential to monitor growth within the mineral chambers if power is available, so they could be included in an observatory package.

Posted by Brian Glazer on 2009-03-03 at 21:28

Great points. And I'm certainly a big fan of improving technologies for advancing subseafloor science--especially sampling & measurement instruments. We have all worked hard to overcome frustration with sources of contamination and limitations in recovery of formation fluids, and while we are recovering better samples than ever before, we still likely have a long way to go to get truly pristine subseafloor samples. Does anyone else see the need for IODP to support instrumentation & technology development and support for testing for subseafloor biosphere studies? Maybe some sort of leveraged partnership with NASA Astrobiology? Many of the challenges we are facing are fairly unique within ocean sciences, but our need for technique and instrumentation improvements have direct overlap with long-term goals in NASA programs--clean sampling, miniaturization of electronics, decreased power consumption,

mechanized rosettes, chambers, winches, etc. How about an easily accessible test bed or partnership with the MBARI MARS observatory?

Posted by Peter Flemings on 2009-03-05 at 07:12

In Situ Measurements: When discussing the subseafloor ocean, a topic I always come back to is that the IODP has a long way to go to advance our ability to make some of the most basic in-situ measurements. The foremost example of this is that we are not very good at measuring pressure, which is at the core of any study of fluid flow. Currently we can either install a cork and wait a long time or use a pore pressure penetrometer in soft low permeability rocks (the DVTP), which requires long equilibration times, and is very often unsuccessful. The result is that in the DSDP/ODP/IODP there have actually been extraordinarily few measurements of pressure. Recently, the IODP made a decision to outfit the ship for wide diameter pipe, which would allow the use of the large diameter MDHT (Schlumberger's pressure tool or equivalent). However, this has never been used. Other examples include measuring in-situ stress, permeability, etc. Development of better downhole tools could advance all of these areas in the 5 year time frame and could fundamentally advance progress in the ISP in areas ranging from hydrates, to accretionary prisms. My feeling is that over the last 10 years there has been insufficient focus on developing these capabilities. Many, if not all of them, can be done without installation of long term monitoring.

A4.6 Observatories

Posted by Organizing Committee on 2009-01-12 at 14:22

How important are observatories for microbiology and hydrogeology studies, and what role should IODP play in implementing and monitoring them? How can observatories be used in a more integrative capacity for hydrogeology and microbiology studies?

Posted by Peter Girguis on 2009-02-05 at 19:46

Observatories are inherently well-suited for "monitoring" changes in geophysical processes or phenomena, e.g. temperature, pressure, flow, etc. There's a shortage of technologies, however, that enable observatories to measure changes in geochemistry, let alone biological processes. There are pH sensors, dissolved O₂ probes and a few other chemical sensors, but that's it. As for microbiology, there are a few sampling systems (the McLane sampler, bio-osmo-samplers) and only one in situ microbio analyzer (the env. sampling processor). I think we need to really decide how we want to use observatories to study chemistry and biology in situ. For example, do we want to develop adaptive sampling systems, that collect and preserve microbio samples in response to events (or at the request of the user). Alternatively, do we want to develop in situ analytical technologies. These are -in essence- quite different approaches and warrant further discussion.

Posted by Julie Huber on 2009-02-10 at 08:17

I think we also need to acknowledge that it is tough to observe a system we know little about, and that is one of the challenges with deep biosphere work. We have to make a priori assumptions about the numbers, types, etc. of microbes that we are going to find

and develop our technology around that. Often we're wrong and that presents unique challenges.

Posted by Jim Cowen on 2009-02-18 at 13:52

Like all marine environmental monitoring programs our in situ analytical needs/desires far out-distance our current capabilities. Nevertheless, I urge all of us to keep pushing for instrumentation advances. At the same time we should try to move quickly toward universal (compatibility and versatility) conventions with respect to such things as connectors and overall access systems (e.g., the CORKs; primary instrument sleds, etc.). I think that folks should be encouraged to continue to develop the systems that they feel they need, that will advance their and others science; there is room and need for both collection/preservation and in situ analytical systems. Discussion is definitely needed for both types of systems, especially to help minimize conflicts in their ultimate interfacing with CORKs and other sampling/analytical systems.

Posted by Brian Glazer on 2009-03-03 at 22:06

Yes, these are all really good points. Pete, my answer would be yes & yes. I don't think that in situ analytical instrument development and adaptive sampling are mutually exclusive, but rather complementary and necessary--especially at such a remote and difficult-to-sample system. Because there is no single end-all technique or instrument that measures all we need to know, a coordinated coupling of in situ analyzers with time-point samplers is crucial. I envision a package that is smart enough to detect something easy at first--a threshold temperature value or a key analyte concentration--and then trigger a series of filters or whole water to be collected/preserved/etc. Once reliable instruments are developed, with miniaturization of electronics, they could be applied for downhole applications, too. It definitely takes a special mix and collaboration of subseafloor enthusiasts, mechanical engineers, and software folks to develop these kinds of systems (and a steady supply of \$\$), but I don't think we're ~that~ far away from such capabilities becoming more widespread.

Posted by Beth Orcutt on 2009-02-08 at 13:44

In regards to monitoring, the “ownership” of long-term observatories, both for microbiology and hydrogeology, is something that is unclear. Do the original proponents of a research project that installs CORKs or other observatories have ‘ownership’ or first-rights to the observatories indefinitely, or does the broader research community have public access after some period of time? If observatories become public access, who is in charge of making final decisions on future changes – a committee within IODP that may or may not include the original project proponents?? I think this is particularly important in regards to raising private funds for the hardware required for observatories – it needs to be clear to private/industry funders whether they somehow “own” observatory hardware or not. NASA scientists undoubtedly deal with similar issues regarding satellites and space probes – we could perhaps learn something from them.

Posted by Liz Screaton on 2009-02-10 at 14:53

I like the NASA analogy. It is clear that borehole observatories are going to be rare due to the installation cost and difficulty, and maintenance costs. In the past, sealed boreholes have been installed for a particular science objective (hydro, microbio, strain). Some get

re-used later for different purposes, but this has not been planned out. Perhaps sealed boreholes could be treated more as a facility than only part of one science objective, so that different experiments rotate through?

Posted by Jim Cowen on 2009-02-18 at 14:03

I agree that this is and will continue to be a sensitive issue, especially for studies requiring long-term deployments. This will require significant discussion, on an on-going basis. Hopefully we can establish some reasonable guidelines. I think we need to start by encouraging investigators to keep the concept of shared use facility firmly in mind as they design their proposed projects, and to actively incorporate mechanisms that allow shared access and/sampling. New experiments will need to respect the expectation of established experimental systems to continue; while the latter must seek ways of accommodating new experimental systems. Forward and backward collaboration could facilitate this.

Posted by Brian Glazer on 2009-03-03 at 22:46

Great points. We're talking about accessing a global resource of subseafloor hydrothermal samples--enabled through IODP drilling and installation operations, and in most cases, a federally-supported submersible operation, regardless of whether federal or private funds supported the initial hardware purchase. Presumably, if you write a proposal that is good enough to be recommended for funding by your peers, you should be able to gain access to your desired sample if it doesn't negatively impact an existing project. But we can't really expect review panels to make those decisions, right? I like Beth's committee idea. Does anyone know what kinds of guidelines have to be followed to deploy an instrument next to someone else's instrument in Volcanoes National Park or Yellowstone? Would that be a relevant model to use as a template to establish some sort of IODP-sponsored subseafloor borehole observatory access guidelines? Maybe a required letter of intent or pre-proposal to allow for logistical screening, scheduling, etc?

Posted by Beth Orcutt on 2009-02-08 at 13:44

In regards to integrating observatories, at the moment, I believe that the hardware-manufacturing lead-time, expense involved in installing observatories, and issues regarding fund-raising for return visits to service observatory science all currently prohibit them from being an 'integrated' part of most drilling expeditions. Just consider the name – the Integrated Ocean DRILLING Program, not the Integrated Ocean DRILLING AND LONG-TERM-OBSERVATORY Program. Developing standard designs for the community to use, improving and simplifying observatory technology, and establishing protocols for long-term observatory science (and funding) will hopefully allow the community to overcome this obstacle.

Posted by Katrina Edwards on 2009-02-10 at 18:39

This is absolutely true – IODP is a drilling program! National funding agencies foot the bill for these international observatories and who should pay what and when is unclear. From my perspective, some of the most exciting science in the deep biosphere and subseafloor ocean hydrogeology studies for the future lies in using observatories and planning out long-term experiments. If we want to capture this ripe potential we need to prioritize and invest in it.

Posted by Jim Cowen on 2009-02-18 at 14:27

IODP is just a catchy acronym; I think the scientific side of IODP sees it as a lot more than just drilling; which is why, for example, we have the core repositories. But I agree that we need to provide CORK observatory science with a higher visibility. CORKs are expensive, but they also lend themselves to long-term high yield scientific studies; they are a long-term investment.

Posted by Andreas Teske on 2009-02-09 at 17:16

Random thought: I understand observatories in the sense of a specific site with interesting geochemical and microbiological characteristics that have to be explored in repeat visits over time, similar to NSF Microbial Observatory sites or RIDGE 2K Bulls Eye sites which tend to have a monitoring component and which are visited many times with an ever-evolving scientific agenda. Many IODP targets are at the stage where the science is mature enough to justify repeat visits, CORKS, borehole monitoring and repeated sampling far beyond what the first generation of researchers imagined: Juan de Fuca, Peru Margin, and soon North Pond. IODP should nurture this development and but without being too dogmatic about choosing only site A but not site B. However, in the future a special call for IODP target areas or focus areas could make sense...

Posted by Katrina Edwards on 2009-02-12 at 23:29

I hate to even bring it up but this does come back to the concept of "missions" in the IODP. In many ways I agree with you Andreas, but how do we bring out strategic long term planning for deep biosphere research? I think the core of the mission concept critical - but it needs to be flexible and adaptive enough to keep up with where the new questions develop, and what the new technologies enable us to accomplish.

Posted by Jim Cowen on 2009-02-18 at 14:47

Yes, I agree; deep biosphere research is still very new and the facilities, sampling, and experimental strategies are at the early stages of their evolution. Our thinking, planning and implementation need to stay very flexible for the foreseeable future. At same time, we need to move forward.

Another random point: Since this deep biosphere research field is at such an early stage, it is difficult to anticipate every important aspect of future facility needs; however, there are certain things that I think we need to error on the side of over-planning or over-build. For example, materials used for key components of a CORK installation (e.g., casings near depth of experiments and intake ends of fluid delivery lines; the fluid deliver lines) need to be as high integrity, low bio- or chemically-reactive as possible; it is worth the higher price. As others have noted in this chat room, we will not have lots of different opportunities to 'try again' to redeploy expensive CORKs, so splurge on key components.

Posted by Keir Becker on 2009-02-20 at 13:28

Whether we call them missions or not, I think we have to develop some long-term planning mechanisms that include firm commitments for selected initiatives. I made a post about this Feb 11 in the strategies forum, "old proposals..." section that looked back at how the commitment was made for what might be our only de facto IODP mission to

date – NanTroSEIZE. Without repeating that post, I think a key was the cross-expertise community-wide endorsement that project got at the CONCORD meeting. If we are to make some long-term commitments, e.g. to deep biosphere observatories, I see the INVEST meeting as critical in demonstrating community support that goes beyond the immediate expertise group - but INVEST will have to be structured carefully to achieve some sort of prioritization and lasting commitments for the post-renewal phase.

Posted by Mitch Lyle on 2009-02-21 at 08:46

I agree at one level. A significant part of the science spectrum requires a longer term commitment than one or two drilling legs, and needs an additional commitment for post-drilling science support. We should figure out a way to do this, like Roland von Huene's comment also expressed.

There is also a large amount of science that can be done in the traditional package, however. As you are aware, much of the one-leg drilling eventually assembles itself into larger programs. We need to develop the capacity to do the larger scale science, as part of the overall scientific drilling package. We also need to be cognizant that much of the science can be broken up into one-leg puzzle pieces that can be assembled with patience.

Do we know where we want to put deep biosphere observatories?

Posted by Andy Fisher on 2009-02-23 at 20:14

Absolutely, and there are several active projects in the system now to emplace observatories to do MBIO work (and other work). Site survey data is in place, and coordinated surface and dive programs have been funded.

Posted by Nick Hayman on 2009-02-24 at 20:31

A question I have about this topic of both observations of heat/fluid flow in older crust (i.e. JdF, ca 1 Ma) is: what are workers envisioning in terms of the subsurface hydrologic and thermal structure (and hence limits on ecosystems)? Suggestions for Baby Bare etc.. of elevated fluid flux and permeability on the order of 10^{10} m² requires a fairly well developed interconnected and open fracture system, for example. Yet, I haven't heard of how to integrate observations of ocean crustal fracture and fault systems identified on submarine escarpments, drill cores, or ophiolites, with this surface-and shallow-hole based dynamic data set. I think this is a tie-in between the biosphere, hydrogeologic, and ocean crustal studies that hasn't been properly expressed (or pursued much, recently anyway).

Posted by Andy Fisher on 2009-02-25 at 13:32

Actually, the area around BB on 3.5 Ma seafloor contains fluid that is anomalously warm, old, slow-moving, and altered. More typical examples of ridge flank fluxes, hydrologic conditions, and MBIO environments are found around North Pond (Edwards et al. proposal) and the Dorado outcrops area on 23 Ma seafloor of the Cocos Plate (see Hutnak et al. 2008, Nature Geosci.). In these areas, temperatures are more like 10-30 degC and fluxes are massive. Baby Bare is a wisp of its former self, in the process of shutting down due to nearly complete burial and a lack of sufficient nearby outcrops. It has no regional thermal impact, lithospheric heat is accounted for, except for the

remaining stages of hydrothermal rebound (see Hutnak and Fisher, 2007, JGR). Yes, there needs to be better integration of surface and tectonic window and ophiolite data, absolutely. The problem is that permeability is an empirical construct, not based on independent observations. So we need to work where we can do the active flow experiment, and also sample other parameters, look at MBIO, etc. Absolutely, there needs to be better integration of interdisciplinary work.

Posted by Jason Sylvan on 2009-02-27 at 14:02

It seems that the designation of a site (or sites) should lead to later interdisciplinary work if a CORK (for example) or some other type of observatory could be maintained. Is there any reason not to look to observatories such as the Neptune Observatory (U. Washington) or LEO-15 (Rutgers U., NJ) for guidance? The big difference I see between these and anything IODP can do is that they are cabled back to land with power to run some of the instruments, but a similar idea in the middle of the ocean (as opposed to coastal) may be more difficult. However, with return trips, we could use batteries and replace them in instruments (perhaps?). And the more visits to a site, the more background data there is in one area, be it microbiology or hydrogeology, etc, that can be incorporated into other studies, making the interdisciplinary nature of the work more attainable. Plus, I know LEO-15 and Neptune Observatory get a lot of press, so they're great for outreach. I think observatories should be a focal point of future IODP work, at least for the deep biosphere work, if not other. Think of all the valuable science that has been done in water column observatories (the two mentioned, plus BATS and HOTS), it is large part of where marine science is headed. A big difficulty here is maintenance, though, but that shouldn't stop us from trying.

Posted by Marta Torres on 2009-03-01 at 13:01

I agree that a linkage with OOI is very important, and indeed efforts have been made to coordinate these programs. There are 2 workshops that deal specifically with this issue, one is a general IODP-OOI effort and the other a specific gas hydrate observatory one. Within the last one there are very specific key linkages with both Neptune Canada and the US OOI node on Hydrate Ridge.

Posted by Beth Orcutt on 2009-03-01 at 14:36

The existing CORKs on the Juan de Fuca ridge flank are also in development to connect with the Neptune network, but Andy Fisher or Keir Becker would probably be better than me at commenting on that.

Posted by Keir Becker on 2009-03-01 at 21:12

Beth is right - one of the Neptune Canada nodes is at the Expedition 301 area, and there are plans to hook up at least one or two of the CORKs this summer. The temperature and pressure data from those CORKs should then be available online sometime this fall. Also at this node will be a seismometer and a network of 3 bottom-pressure recorders that will be part of a regional tsunami-monitoring network. Neptune Canada also has a hydrates node near the Exp 311 transect and the old ODP CORK at Hole 889C that could be refurbished and hooked up.

A4.7 Aging of the oceanic crust

Posted by Miriam Kastner on 2009-03-04 at 19:58

The topic is: "A Multidisciplinary Initiative on the Impact of Low-Temperature Hydrothermal Activity and Biogeochemical Cycles on the Aging of the Oceanic Crust" The initiative will concentrate on two drilling transects from ~4-5 to ~60 my oceanic crust; one with sediment cover and the 2nd where sediment cover is interrupted by basement outcrops and low-temperature seeps (both at similar spreading rate crust). The main objective will be to characterize the physical, chemical, and biological forces that control the aging of the crust, the associated positive and negative fluxes, and the potential impact of these processes on cycling at subduction zones.

Posted by Rick Murray on 2009-03-05 at 20:08

This is very interesting stuff and, quite coincidentally, I lectured on aspects of this today in my Marine Geology class (!). What is particularly exciting about the way Miriam has framed this is that it is truly interdisciplinary. We have now learned of the strong role microbial activity plays in the sediment column (and still have a long way to go learning-wise), and have confirmed in a more quantitative way how the physics-chem-microbio interact in the mud. Bringing this all to INTEGRATED studies of the crust is really exciting...keep in mind we're talking about mass and energy fluxes of the highest order and of planetary scale. This could truly be "transformative" in how we think of such topics. It is also very do-able in terms of available engineering and technological needs.

Posted by Robert Harris on 2009-03-08 at 19:28

Miriam provides a nice concise statement that ties together the evolution of oceanic crust and fluxes within the multidisciplinary framework needed to understand these processes. I too find these questions exciting. Since the architecture of oceanic crust is sensitive to the spreading rate I would not want to rule out transects on crust of different spreading rates. In any event these holes would be the first basement holes in intermediate aged crust.

Posted by Jim McClain on 2009-03-08 at 22:17

As a late-comer in this process, I have to say that Miriam's concepts is scientifically exciting. I would like to include some, perhaps impractical, goals to this concept.. It is clear that the upper ocean crust and sediments evolve with age. This has been documented over the years, and indicates substantial amount of geochemical activity and exchange at those depths. However, the deeper age-related changes are not well documented or even accepted. Early studies indicated that the ocean crust thickened with age. Subsequent work indicated that was not the case, but the evidence for changes in the seismic velocity structure with age (but not crustal thickening) remain intriguing. This leads one to question if geochemical exchange extends into the lower crust or even the mantle. While drilling to the Moho in a trans-oceanic transect is a pipe dream, perhaps a few sites could be extended into the lower crust. Then the lithological and fluid sampling, when combined with surface geophysical observations may provide support (or not) for the penetration of fluids over greater depth intervals.

Posted by Miriam Kastner on 2009-03-10 at 01:10

Indeed, I agree with Bob that eventually, in phase 2, drilling on crust of different spreading rates would be essential. In phase 1, however, I thought that it would be important to control the number of variables; by keeping some of them approximately constant, data interpretations would be easier.

Posted by Mitch Lyle on 2009-03-12 at 06:34

This is one of the classic problems--what are the processes and what is the effect of fluid flow through the ocean crust?

A5 STRATEGIES

A5.0 Overview

The key question for this board is: Given the specification of high-priority scientific goals (in the other Discussion Boards), how can the IODP community position a renewed program so as to maximize opportunities to attain these goals, maintain and expand community enthusiasm for the program, and secure necessary financial and other support in an increasingly challenging funding environment?

A5.1 Assessing scientific achievements of scientific ocean drilling

Posted by Organizing Committee on 2009-01-12 at 14:30

What kinds of community oversight are needed to make sure that program managers and contractors fulfill their scientific obligations and remain focused on activities of greatest importance to the community? What means can be used to assess scientific success?

Rationale: There has often been a focus in the past by the implementing organizations on _logistical_ achievement, rather than scientific achievement (e.g., kilometers of core recovered, days at sea, etc. rather than whether or not key questions were answered). Perhaps there should be a more direct connection between scientific goals and IO activities, so that the focus is placed more strongly on achievement of critical scientific objectives.

Posted by Bob Duncan on 2009-03-01 at 16:39

There is considerable overlap between this topic and the "Broader Impacts" topic and discussion. But Assessment has been largely ignored in ODP and up until now in IODP. It certainly gets an airing, however, when renewal comes around! There have been rather few attempts to synthesize results from many legs of drilling over significant periods, in broadly defined topics (arcs, LIPs, etc) or regions (Indian Ocean, western Pacific). There was never any dedicated resources for this -- each was argued, case by case. However, the general scientific community and certainly the larger public would use a well presented synthesis rather than go through the leg summaries. A desirable aspect of such a "retrospective" view of results is making the connection to the Initial Science Plan (ISP), with assessment of how completely goals set out there have been achieved, what new questions emerge, paradigm shifts, etc. This effort could be advanced through regular articles in the journal Scientific Drilling, monographs (AGU, GSA?), and "case examples" for introductory texts.

Posted by Xixi Zhao on 2009-03-02 at 04:51

Bob's point on producing regional synthesis with assessment is well taken. Indeed, only rarely regional synthesis of existing data has been made on ODP and DSDP drill sites. Resources should be made available to allow researchers to exploit new advances in science disciplines (such as geochronology, regional stratigraphy, paleomagnetism and

rock magnetism), to make full use of existing ODP and DSDP core materials, to synthesize related research projects conducted by a single individual or group of investigators previously to a composite record, and thus to enlarge science database and provide useful information for new IODP drilling in the region.

A5.2 How can IODP increase participation?

Posted by Organizing Committee on 2009-01-12 at 14:29

What can IODP do to increase interest and participation within scientific disciplines that have not previously been active in scientific ocean drilling? What can IODP do to increase participation by K-12 students and/or undergraduate researchers? What can IODP do to involve non-scientists in drilling activities? What can IODP do to enhance diversity in IODP participation?

Rationale: The vitality and fresh viewpoints that arise from "outsiders" participating in the program have driven some of the successes that we have had in the past, and it is important to maintain or even enhance these efforts. In many ways this goal has been approached by "adding on" opportunities to involve junior scientists and non-scientists. Perhaps there is a better way of integrating E&O from the beginning of individual programs.

Posted by Frank Rack on 2009-02-03 at 17:58

See the comments that I made in the "Broader Impacts" discussion, which also apply herein. In addition, I think we should be actively encouraging and pursuing individuals and groups to utilize the data and resources that are available from scientific drilling programs over 40 years. In the past we've designed coffee mugs with interesting core images from past expeditions, as an unconventional use of scientific data. By making these images freely available - not just as 300 dpi PDF images, but at various uncompressed and lossless resolutions that people can easily browse and select - and advertising their availability, we may find that unexpected or unanticipated use of these products may result. We may want to sponsor contests that foster creative use of visual data from these natural archives or that support the development of a better understanding of what they represent. We may want to focus on creating case studies that use cores and data to illustrate specific topics or lessons and incorporate these into the National Science Data Library - some of this has already happened, but not nearly enough. Ocean, Climate, Geoscience, and Environmental Literacy have all been the subject of much discussion in recent years and scientific ocean drilling holds some of the best datasets that illustrate some of these concepts, if we would support efforts to package them appropriately for various audiences (e.g., K-3, 4-8, 9-12, undergraduate, graduate, general public). I think it would also be advantageous for each expedition to develop an E&O plan - perhaps one that is incorporated into the scientific prospectus and annual program plan - to increase the profile of these activities and to formalize a commitment to achieving their successful outcomes, especially in the face of funding pressures and possibly conflicting priorities.

I think this discussion should also include seismic and environmental survey activities or long-term observations in addition to drilling. Now that there is an ocean layer in Google

Earth 5.0 and expanded resources are becoming available through GeoMapApp and Virtual Ocean, among others, there can be efforts to involve non-scientists in planning their own virtual expeditions involving the use of real data and map products, either 2D or 3D, which they can fly through and explore. There are many opportunities that are possible and that we can promote, if we commit to brainstorming and to supporting interesting ideas with dedicated funding. The expected expansion of STEM funding provides a great opportunity to capitalize on this.

Posted by Kristen St. John on 2009-02-26 at 23:45

Regarding reaching out to a broader community/broader impacts and integrating E&O earlier and more effectively:

As Frank pointed out, the 2005 shipboard School of Rock (SOR) program was very successful. It was the first large-scale programmatic effort toward E&O after Leslie Peart was hired as Education Director at JOI – which demonstrates what can be done when personnel and funding resources are directed towards E&O. (Imagine what could be done if we not on a shoestring budget!) Assessment of the SOR program also gives us data and experience to reflect on as we construct E&O future plans for IODP. The SOR leadership team outlined the lessons learned from a geoscience education standpoint and articulated a design model (St. John et al., accepted in press, GSA Special Volume) for working with educators that I think has relevance to this CHART discussion on E&O and reaching out to a broader community. Based on this experience here are my thoughts:

1. For E&O to be successful K-12 teachers and informal educators need a program leadership team that includes both research scientists and professional educators to help fulfill their scientific content, skills and pedagogical needs.
2. Transits of the JR (and other platforms) offer unique, authentic, and technology-rich field settings for educators to experience ocean drilling science. Transits are ideal venues for high profile, high impact E&O experiences.
3. Communicating in real time with classrooms and museums and media outlets via Skype and video conferencing (in addition to webpages and email) are going to be important as we now transition and regularly sail educators and/or outreach specialists. Note - these educators are broadly defined in my mind and should include scientist-educators (PhD geoscience educators and/or geoscientists who are actively engaged in E&O), graduate students in geoscience education, K-12 educators, and informal educators. I know this will mean more bandwidth and that means more money to pay for it, but E&O is a way to increase buy-in to the program and we need to reach out in ways that are meaningful and useful to those outside groups that we are trying to reach out to.
4. Making the online scientific data accessible (user friendly) to the broader audience is going to be one of the key elements for larger-scale, longer-term success in E&O. Once programs like SOR turn educators on to IODP we (Mark Leckie and I) find that the educators usually want to do more on their own (which is great because that means we have done our job) - they want to access the data when they are home or at school to make their own data-rich exercises for their classrooms. They know there is a treasure

trove of data on Earth history and Earth processes in the online IODP publications and database. However, one of the primary barriers for outsiders is actually finding exactly what they need (accessing the data) AFTER the participants go home from a SOR experience. Educators can leave SOR with tremendous enthusiasm and new fundamental knowledge and skills, but then lack the experience and insider knowledge to navigate the database and pubs to retrieve what they want (and no longer have the leadership team at their immediate disposal). The database is not user friendly if the user is an outsider. Try looking for turbidites in the database and publications. A key word search would be most helpful – we have to put key words in AGU abstract submissions, why not in the database and in pubs online? Or consider how an outsider would know what the science objectives or geographic location of a past expedition was just by looking at the Expedition IR number (which is how you have to search for most anything from the publications). Searching by key words, scientific themes, geographic regions would be much more helpful to outsiders (and maybe even insiders of the program).

Posted by Debbie Thomas on 2009-03-02 on 21:50

Kristen makes several really key points here, and I echo her sentiments that the information (data, core photos, etc) must be more obviously accessible. The School of Rock team has done absolutely amazing work involving teachers in the science, and these educators pass on the legacy to their classes. But I think even more fundamental to the involvement issue is marketing. I think it was Beth Christensen that posted the importance of how we "package" the program and how we must reach the public. Every student in the US has heard of NASA, but how many have heard of ODP or IODP? I don't really have any answers other than for us, as individual scientists, to continue with grassroots type efforts such as the School of Rock, but programatically something must be done to enhance the visibility of scientific ocean drilling. These achievements must be made as commonly known as manned spaceflight (perhaps I'm dreaming, but you get my point!)

Posted by Anthony Koppers on 2009-03-06 at 19:20

What I think hasn't been working is getting ocean drilling results to the front burner in our society. Not many people on the street and students in our high and middle schools know about ocean drilling and the scientific fruits of our labor, yet everybody knows about missions to the Moon and upcoming missions to Mars! We are exploring the unknowns (and there are many as is evident from the CHART workshop) in the deep of our oceans using an amazing but expensive tool. It is clear that in the remainder of the IODP program and in the next phase of ocean drilling we have to emphasize to society why ocean drilling is important, even if we are using it as a tool to carry out basic science with little direct societal relevance. Going to the Moon has little societal relevance, yet we can get an entire nation excited about the prospect of picking up rocks on the Moon or Mars!

Posted by Andy Fisher on 2009-03-07 at 13:19

That's exactly right. A few years back I was walking through DFW, saw USA Today on display in every shop. Above the fold was this headline: "Hubble Telescope: 10 years, 10 amazing discoveries." The 10 photos that accompanied this headline were banal - grainy images of galaxies, stars, etc. I'm not knocking astronomy or the Hubble Telescope, these

discoveries were very important. But somehow NASA managed to "sell" the use of a highly esoteric instrument to study processes that have absolutely zero impact on the lives of 99.99% of the American public. Surely we can do better than we have done with scientific ocean drilling.

Posted by Christina Ravelo on 2009-03-08 at 18:06

I am all for IODP putting more effort and resources into outreach and education. It can only help raise the visibility of the drilling related science, it is our obligation to let the public know how their money is being spent, and it is part of a cycle that mutually grows the program, advances science, and benefits society. But, remember that NASA spends more on education and outreach than we do on our entire drilling program, and that defense aerospace industry (e.g., Lockheed, NASA's biggest contractor) spends 10's of millions of dollars each year lobbying congress. Last year they spent over \$50M lobbying congress. So, while I think we can learn lessons from NASA in terms of 'selling' science that does not impact the lives of the American public, I think we also have to listen to the funding agencies and the lobbyists that advocate for Ocean Science research on the hill on our behalf - and they seem to be telling us quite clearly that societal relevance is more important now than it was in the past. This is not to say that IODP should not be mainly about supporting basic research, it is simply saying that we may have to elevate societal relevance when we package and pitch and 'sell' the program to funding decision-makers and the public in a way that we have not done in the past.

Posted by Susanne Straub on 2009-03-08 at 18:25

I agree on all points - especially given the change on public awareness towards global climate change and the recent political change, it is an excellent moment to emphasize more than ever the links between (basic) science and societal relevance

Posted by Anthony Koppers on 2009-03-09 at 15:56

When I wrote the initial comment of course I realized that NASA is the big player here and has lots of resources (to squander), yet we (that is the program and the scientists) have to start thinking about outreach in different and innovative ways. For example, the IODP research vessels could have a tele-presence with HD-TV and internet feeds to the shore and through that also to classrooms or any 8 PM news broadcast. IODP could start producing its own 5 min documentaries to be disseminated via YouTube or a special iTunes PodCast Classroom Channel on Ocean Drilling. There are many more ways to reach the larger public these days, most of them using modern internet means, yet they do not require tens of millions of dollars, just a more focused and determined effort ...

A5.3 Where will the program find new funding and new partnerships?

Posted by Organizing Committee on 2009-01-12 at 14:31

How can a renewed IODP both secure enough of currently-available scientific funding, and help to generate new funding that would otherwise not be available to the community? Are there additional funding partners who could be convinced to participate, perhaps on the basis of supporting specific projects of interest?

Rationale: The current resource base allows for only about 60% IODP usage on each of the ship-based platforms. There is work underway to develop an energy industry alliance.

Posted by Frank Rack on 2009-02-03 at 14:52

For the U.S. SODV (JOIDES Resolution), an unconventional approach would be to explore if the cost overruns on the MREFC conversion project - which have been subsidized by Transocean and amortized over the current vessel contract, thereby increasing the ship's day rate - could be repurchased using new funds requested for ocean science infrastructure as part of the NSF budget and stimulus package discussion with Congress. This strategy could potentially adjust the current USIO baseline operating budget and perhaps make it possible to operate the ship for a larger percentage of the year with the same amount of operating funds. It may be too late to adopt this strategy, but any efforts to reduce the vessel's day rate will help the overall program. Even if these additional costs could be amortized over an extended period of time (to 2023), rather than being concentrated over the existing term of the contract (to 2013), it would potentially make a significant difference to the fixed costs. These are administrative solutions that require actions at the federal level as well as contract negotiations between the USIO and the vessel operator that alter the fundamental conditions that were imposed on the program as a result of the conversion project. I only raise this option since we are engaged in other infrastructure discussions nationally.

Industry, federal agency, and international interest in methane hydrate characterization activities are well documented and ongoing. This could and should be part of an integrated strategy for IODP-like expeditions funded by external parties (industry and national entities) that include the participation of international scientists, capacity building activities, and open exchange of data after a moratorium period. This has been successfully done in the past and can provide some opportunities for research-driven, shared planning between industry and IODP in the future.

Drilling, survey, and engineering development activities focused on high-latitude regions, especially the Arctic, but also applying to the Antarctic and Southern Oceans, could also provide new opportunities for international cooperation and partnerships with industry, federal agencies, and national programs.

One of the challenges of either of the two strategies mentioned above is the limited time for response and the need to constantly chase new opportunities. For example, today there was a Call for Proposal announcement from industry for "Arctic Challenges" <<http://www.oil-itf.com>>, but the deadline for submission of proposals is March 16, 2009. To make this process viable would require having proposals on the shelf and ready to go when opportunities like this present themselves and also having the appropriate personnel on hand to be the "rainmakers" who identify and respond to these solicitations on an ongoing basis.

Posted by Dale Sawyer on 2009-03-02 at 09:58

The study of rifted continental margins and sedimentary basins is of interest to the oil and natural gas industry. The companies regularly make use of the results of rifted margin

drilling by DSDP, ODP, and IODP. They rarely make use of the results of much other drilling by IODP or its predecessors.

Within IODP, I regularly hear expressed the desire to involve the oil industry in supporting IODP or other scientific drilling activities. I suggest that the best way, and probably the only way, to get them involved is to do scientific drilling that they are interested in.

A group of about 45 academic scientists met with industry scientists last June at Rice University to discuss possible scientific drilling projects of mutual interest to both communities. We found a number of areas of common interest. The scientific drilling program we generated comprises drilling 1) to study rifted margin architecture at the Flemish Cap Margin, the NE Greenland Margin, the More Margin, the Jan Mayen Micro-continent, and the Pernambuco Margin of Brazil, 2) to study, with drilling and coincident industry high resolution 3D seismic data, the active sediment transport processes from continental shelf to deep basin reservoirs in two parts of the Gulf of Mexico, and 3) to study the distribution and characteristics of Cenomanian/Turonian hydrocarbon source rocks deposited in the South Atlantic Ocean. All of these programs are in the Atlantic Ocean. The steering committee for the workshop subsequently wrote and submitted a proposal to many companies seeking funding to carry out the program. We expect to know the results of our efforts in June 2009.

If IODP wants to attract the interest and potentially the financial support of the oil industry, these are the kind of projects that it should include in its long range planning.

Posted by Ken Miller on 2009-03-02 at 15:47

I participated in the June conference at Rice and applaud efforts to use the JR to do work of interest to energy companies outside of IODP. The data collected would be of great use and the cores and data would ultimately come to IODP. This is a no brainer considering that the alternative is to tie the ship to the dock or lease it for off contract work that had no scientific return to IODP.

But the real challenge is not to lease the JR for off contract work like this. The goal should be to do 12 months of academic drilling with the JR. Perhaps certain legs could be sponsored by industry: but this chimera has never been reached and I think a reality check suggests that industry is too busy pursuing the quarterly profit margins to engage like this.

Past performance may not be a perfect predictor for future achievements, but in the 40 years of ocean drilling we have been fabulously unsuccessful in garnering industry support.

Posted by Greg Mountain on 2009-03-02 at 22:17

[I prepared this response to the Site Survey Discussion, but now I see it fits better here.] Ken, I agree 40 yrs of non-success is daunting, but the times they are a-changin'. Greg Moore + Sean Gulick in the site survey thread describe success they've had leveraging NSF funds with non-US partners. I believe they're right on, and IMHO we should go

farther: the new IODP has to be far more effective in getting industrial support for pre-drilling surveys and instrumentation than it has in the past. Along with many who are posting here + elsewhere, I was part of the ODC workshop last June at Rice. If the proposal that came from it that Dale mentioned doesn't fly, it falls to INVEST to develop strategies for building truly effective collaborations with industry. Many hurdles confront us, for sure. And only a subset of the Earth science topics that drilling can answer can be done in this way. Of course, it can be derided as pandering to the money lenders. But at Rice we saw that advancing the knowledge of rift mechanics, basin evolution, global anoxia, and slope delta processes engages researchers and oilmen/women alike. Who won't agree that breakthrough, transformative science will very likely come from scientific drilling where we've never been before, using tools previously out of our reach, and that oil companies have the capacity to make this happen? A challenge before us is to build a path towards that common ground. I'm convinced we're there scientifically; what we need is a management and science delivery structure that meets industry's needs without surrendering our intellectual standards. The stakes are just too large to not try once again to make it happen.

Posted by Liviu Giosan on 2009-03-05 at 14:10

I agree with the points raised in this discussion and with the general idea of partnering with sister initiatives. I would like to add that although IODP comprises of more and more nations that participate to the program independently or in an associative form, there are still more maritime countries to involve in this global effort. I suggest that IODP think of a formal ways to involve smaller non-affiliated nations to drilling efforts in their region of interest, both at research and educational levels. Although such efforts have existed since DSDP time, formalizing the interaction between IODP and such nations may provide a boon of interest in the program around the world.

A5.4 With what other programs should IODP be coordinating?

Posted by Organizing Committee on 2009-01-12 at 14:28

With what scientific, technical, and governmental programs should IODP be coordinating? How can this coordination help to identify related technical and scientific goals and other areas of mutual interest? Is there a way for IODP and related programs to leverage scarce resources through cooperative projects? What might be the risks in creating these alliances?

Rationale: Many interests of IODP participants dovetail with those of others and other programs, so there can be opportunities in finding opportunities to collaborate across programs. This is a chance for the community to discuss possible benefits and pitfalls, and perhaps to suggest new ways to collaborate across programs.

Posted by Henry Dick on 2009-02-04 at 11:05

I would like to see some of the membership of the panels drawn from programs like InterRidge or Margins. Specifically, IODP should decide which major programs have a major overlap with IODP objectives, and then allow the steering committee of those programs to be able to nominate one of their members for membership on one IODP

thematic panel. This would provide better representation of different thematic communities, and provide for access to panel membership from outside the "inside" of IODP.

Posted by Kathie Marsaglia on 2009-02-27 at 15:31

I agree. There have been discussions of the next stage of MARGINS efforts (S2S Future directions at the nsf-margins.org/forum) and I hope that it is "in phase" with IODP and its renewal theme.

Posted by David Anderson on 2009-02-09 at 15:59

I think the paleoceanography community could produce a better, more extensive collection of sediment cores if we coordinated all types of coring, from multi-coring to piston coring to drilling. This might let different countries contribute different expertise and the technologies they have, leading to stronger international partnerships. Acoustic and seismic surveying to identify new coring targets could be part of this.

Posted by Mitch Lyle on 2009-02-10 at 07:35

We should also add drilling on land, since some of the best Cenozoic and Mesozoic marine sections are not in the present oceans.

Posted by Joseph Stoner on 2009-02-11 at 01:05

Dave has a good point, as does Mitch below about coordinating land and sea. It really gets to the idea of sub-seafloor (or maybe sub-surface) science that was discussed in another tread. We really need a coordinated approach toward answering specific questions and finding a way to reduce all the barriers to entry.

Posted by Xixi Zhao on 2009-02-27 at 02:27

I also support the ideas mentioned by Dave, Mitch, and Joe. Specifically, I think IODP should coordinate more closely with International Continental Drilling Program (ICDP), not only to provide clues from both ocean and continental drilling programs to answer specific questions (such as the mid-Cretaceous geologic puzzle), but also to jointly develop new technology (such as the azimuthal orientation of the cores). There are still no hard rock orientation system in place. Magnetostratigraphy and many related studies could be greatly enhanced if the core orientations (magnetic declination of the core) can be known.

Posted by Liz Screamton on 2009-03-04 at 06:35

There should be continued collaboration between IODP and observatory initiatives such as OOI. Although the seafloor interaction between IODP and OOI seems limited to the NE Pacific, this location has a lot of potential in several of the current IODP initiatives (seismogenic zone, crust hydrogeology and microbiology, gas hydrates). Perhaps also the OOI water column work would integrate with IODPs paleoclimate/paleocirculation work.

Posted by Rob Zierenberg on 2009-03-13 at 12:34

IODP states that it is a science-driven research program. The science goals in the IODP Initial Science Plan are well known to those reading following the CHART discussions, which aim to refine those goals in anticipation of renewal of the program.

The International Continental Drilling Program (ICDP) also states that it is a science-driven research program. The ICDP goals are stated as follows (http://www.icdp-online.org/contenido/icdp/front_content.php?idcat=310)

"Through the unique capacities of scientific drilling to provide exact, fundamental and globally significant knowledge of the composition, structure and processes of the Earth's crust", with particular focus on research themes such as:

- The physical and chemical processes responsible for earthquakes and volcanic eruptions, and optimal methods for mitigating their effects.
- The manner in which Earth's climate has changed in the recent past and the reasons for such changes.
- The effects of major impacts on climate and mass extinctions.
- The nature of the deep biosphere and its relation to geologic processes such as hydrocarbon maturation, ore deposition and evolution of life on Earth.
- How to safely dispose radioactive and other toxic waste materials.
- How sedimentary basins and hydrocarbon resources originate and evolve.
- How ore deposits are formed in diverse geologic settings.
- The fundamental physics of plate tectonics and heat, mass and fluid transfer through Earth's crust.
- How to better interpret geophysical data used to determine the structure and properties of Earth's crust.

In spite of the extensive overlap of scientific objectives between the two programs the stated policy on collaborative research drilling projects is that IODP and ICDP will only co-support drilling projects that drill on both sides of the strand line. In other cases, the elevation of the spud-in point divides the planet into ICDP turf and IDOP turf based on mean sea level.

The good news is that climate change (a primary research objective for both programs) is increasing IODP's territory at an accelerating rate!

The bad news is that the synergy of focusing a wider scientific community on important research problems is lost, we are excluding rather than expanding our support base while we argue for renewal of the ocean drilling program, and we loose the potential benefit of shared resources and technological development that could benefit both programs.

Is an arbitrary datum on the planet the best way to divide these programs, or is it time to really support science-driven research where we define our scientific objectives and then use the best means available to address these problems without regard to platform? Is the community ready to support the idea of an International Scientific Drilling Program with worrying where to put to the C and the O?

There may be legal, financial, contractual and administrative barriers that make it difficult or impossible for ICDP and IODP to effectively co-support drilling projects at present, but they do not have to be built into a new drilling program if we as a community decide that they hinder our ability to move science forward. There are plenty of old folks (guilty as charged) in the program who can provide lots of arguments for why the system is like it is, and why it can't change. There is plenty of hard earned wisdom among some of the old guard and I think their opinions should be considered, but I also suggest that the next generation has an obligation to challenge these arguments if they don't see that the continued separation of the ICDP and IODP communities is in the best interest of the advancement of science.

A5.5 Do we need a new paradigm for staffing and executing expeditions?

Posted by Roy Wilkens on 2009-02-28 at 19:36

I'm getting into this rather late as I've been out on the JR with some of the other correspondents and didn't realize that the discussion was closing out the day after we arrived in Honolulu.

I've been looking over a number of the other posts and I haven't really seen anyone discussing a basic change in the way we do business. Do we really need 30 scientists out here on the ship? Can't we break away from the model that was established in 1967 on the Challenger? Give us more than the wretchedly slow shore net connection presently installed on the JR and we could do much more in terms of outreach. A dozen scientists and an expanded staff of technicians at sea with a shore-based science party could pretty much do the same stuff we do now - probably better. The science crew spends an inordinate amount of time prepping samples and feeding scanners as it now stands. Why not have people skilled in the use of those tools do that work?

How about regular skype or ichat with local school classes? There is so much that we could be doing that we aren't because we're still operating on a 20th century model. I'd really like to see what we would come up with if we were setting up a new drilling project and taking advantage of what is truly the latest technology.

Posted by Andy Fisher on 2009-03-03 at 14:28

Excellent points, Roy. I agree that we have fallen into a rut with regard to staffing, but one challenge is political. Participating nationalities have "purchased" berths, and they are often reluctant to give up on the chance to fill them. That said, there should be more flexibility in staffing of techs, engineers, scientists, teachers, students, etc. so that programs can become more inclusive. There is nothing like being there to make an impression. I also agree about connecting more directly with classrooms, museums, and other E/O institutions using technology. This is a good way to entrain a larger community.

Posted by Joseph Stoner on 2009-03-12 at 14:22

From my own perspective I don't think there is anything like being out there. In many ways we are way too remote as it is. That loss of contact, in my case with the sediment, would be a disaster as its one of the few times that I really get to see what I'm working on. The connection with materials, with the other scientist and our ability to stay in the moment at sea is what really leads to ideas and innovation. Such a move would result in a slow erosion of interest and eventually kill the program. Now, I'm not saying that staffing policy shouldn't be reconsidered and there are likely better ways of developing a shipboard/shorebased relationship to maximize scientific output. However, a greatly reduced science presence at sea is, in my view, not the way to go.

Posted by Christina Ravelo on 2009-03-12 at 15:56

I agree. I think limiting scientist participation could be a downward spiral in terms of the resultant science you get out of each leg. The science party has a strong sense of camaraderie and commitment to the project that is carried into the post-cruise period when a lot of the best work gets done. The knowledge that you gain after spending 2 months of working with the material and data within an interdisciplinary collaboration cannot be matched, and creates the foundation for the science you do after you leave the ship. There is also the benefit of meeting and being part of an international team of scientist - there is some really important social engineering that goes on during expeditions that is very important to our community and the continuing success of the drilling program. Also, I think if you cost it out, it may not be a cost savings to hire more permanent technicians than it is to hire scientists temporarily for each expedition.

Having said that, I do agree that there should be more flexibility, and perhaps more consultation with co-chiefs, about exactly what expertise is really needed and not needed.

I like Roy's statement about using cutting edge technology and really increasing communication and outreach. There is a plan to do webcasts on the Bering Sea Expedition, to sail someone to handle equipment and communications, and for shipboard scientists to talk in real time with visitors at the SF exploratorium. Ocean Leadership has supported ship-to-classroom communications between scientists and students during other cruises. I believe that USIO is sending a videographer on the Wilkes Land Expedition. So, USIO is definitely thinking about really enhancing education and outreach during expeditions. Some in the U.S. community have not supported this, thinking that we should be using every penny on science. But, I agree with Roy and other posts on CHART....we need to do more about public outreach.

Posted by Joseph Stoner on 2009-03-12 at 18:45

Christina makes some very good points about flexibility in staffing and I agree with Roy that cutting edge technology could really improve outreach. In the short run it may cost us some science money, but we really need a 12 month program and the only way to do that is to get the public behind the science.

Posted by Alberto Malinverno on 2009-03-13 at 09:33

I also agree that there is no substitute to being out there. Seeing firsthand how drilling and logging is done and how cores are collected, sampled, and measured gives an immediate impression of the limitations of the data.

Roy makes an excellent point on the need to improve the ship-to-shore communication for outreach, but why not have both a relatively large number of scientists on board AND a fast connection for outreach activities? One does not preclude the other; if anything, fewer scientists would mean less time for them to participate in outreach. On the other hand, we may need people on board who are mostly dedicated to outreach, and flexibility will be necessary.

Posted by Roy Wilkens on 2009-03-13 at 16:33

I too believe that being there is a good experience. Otherwise I would not have participated in 13 Legs/Expeditions over the past 30 years. However, the degree of sophistication involved in many of the latest systems is heavily taxing current technical support. Seems to me that if we replaced 6 scientific berths with 6 technicians/systems experts we would wind up with consistently better data quality (and probably quantity as well).

Agreed also that better comms and the size of the scientific party are separate issues, other than the fact that improved links to shore could allow for shore-based participation of more scientists as well as educators, students, and the general public. It sounds like, from some of the other posts, that IODP is actually trying to move ahead in that area and this is very encouraging.

Still, if the bold new vision that comes out of INVEST looks to the public like more of the same targets with a new wrapping I doubt we'll generate much enthusiasm outside our own little world.

A5.6 How should the new IODP Science Plan be organized?

Posted by Andy Fisher on 2009-02-27 at 14:42

Thus far within CHART there has been considerable discussion of scientific topics organized around the themes and initiatives listed in the Initial Science Plan for IODP. In addition, there has been compelling discussion under the topic of emerging fields and cross-cutting disciplines.

The key strategic questions for CHART participants are: (1) How should new topics be incorporated into the new Science Plan for IODP renewal? (2) Should the new Science Plan for IODP have themes and initiatives and, if so, what should they be and how should they be organized?

Posted by Bernard Coakley on 2009-03-02 at 14:59

Everything that the program proposes to do that is really NEW might best be highlighted in a stand alone section. In the previous science plan the Arctic was mentioned repeatedly. The individual remarks, isolated in larger blocks of text, do not have a

cumulative impact equal to their collective significance. In the next science plan, I think there should be a separate Arctic Ocean section. This would help to highlight the REALLY NEW things that the renewed program might be able to do with sufficient resources.

Posted by Susanne Straub on 2009-03-03 at 16:40

In order to enhance the public understanding of the importance of ocean drilling, I think that a new Science Plan should particular stress those scientific topics that have a clear societal relevance - be it driving mechanisms of global warming, ocean acidification, sealevel rise, subduction hazards (earthquake, tsumanis, explosive volcanism) etc.

Posted by Pat Castillo on 2009-03-04 at 12:42

An effective way to enhance public understanding is to get the public's attention first with something new and exciting. Thus I also think that there should be a separate Arctic Ocean section that will stress on topics that have clear societal relevance such as global warming (of course!), sealevel rise, etc.

Posted by Keir Becker on 2009-03-05 at 09:04

I think it is reasonable to organize the new science plan on themes - in fact I can't imagine how else to organize the science plan for a program that can address so many objectives. I also think it will help to focus the plan by going one step further and identifying initiatives or missions (or some other name). However, I think we have to do more than the ISP did with its 8 initiatives - we need to get into a reasonable implementation plan. If we map the IODP work done already and likely to be done by 2013, I think we will have made good progress on some ISP initiatives, but not some others. There are many reasons why, but one may be that we didn't have a clear implementation plan for achieving these initiatives. Instead, we initially relied on the unsolicited proposal process, then we've had the unproductive experiences of (1) a competitive mission proposal process and (2) the top-down SASEC identification of certain initiatives for emphasis before renewal. Neither of the latter worked well, and in the end both were rightfully viewed as counter-productive by many elements of the community. So what is the alternative? In another post, I argued for somehow organizing INVEST so that it has some opportunity to arrive at a community-wide consensus on a phased implementation plan to address whatever initiatives are identified there as important. I am not sure whether such a consensus would be possible, but I think we need to try. (Please realize I am not imagining a rigid implementation plan that says do only one particular initiative one year, another initiative the next, etc. But I do think the community should try to identify periods when we'd emphasize working on certain initiatives, so that we can begin to plan more effectively to actually achieve those goals.)

Posted by Keir Becker on 2009-03-05 at 09:20

Please note that in my post above I did not mean to be overly critical of the ISP. It did indeed include an implementation plan that identified periods of emphasis for various initiatives, particularly for riser drilling. However, it was based on the assumption of full-time platform availability from the beginning of IODP, and of course that didn't happen. Given the limitations on platform availability and funding that may carry

through to the post-2013 phase, I think we need to carefully set out an implementation plan in more detail than was included in the ISP.

Posted by Gail Christeson on 2009-03-05 at 12:00

While I agree that a detailed implementation plan could be the best way to carry out a new ISP, I worry that it will inhibit inclusion of new scientists and new programs in post-2013 drilling. Currently SSEP reserves their 5-star rating for exciting proposals that actually go beyond the ISP - if there is a detailed implementation plan these types of proposal may never get written. Careful consideration must go into how to write a ISP that does not decrease the pool of scientists involved in the drilling program.

Posted by Keir Becker on 2009-03-05 at 13:53

Gail makes a really good point, and I agree there always has to be flexibility to accommodate really good new ideas. Probably it wasn't clear in my first post, but I was not imagining a rigid implementation plan that doesn't have that flexibility. In other posts in other forums, I've suggested a need for a post-renewal planning process with complementary tracks that allow for both long-term commitments to high-priority initiatives (or 'missions') plus a regular proposal process that allows for good new ideas to rise quickly. In that context, I think the community's best avenue for making the long-term commitments to selected initiatives (or a sequential approach to several) may be at the INVEST meeting, when all the expertise groups will be represented. Otherwise, we might be left with the same sort of approaches mentioned above that haven't been satisfactory so far - a competitive mission proposal process between expertise groups or some sort of top-down directive (from SAS executive committee or management or agencies?).

Posted by Andy Fisher on 2009-03-05 at 13:58

Yes, I agree with both Keir and Gail (who echo comments by many others throughout the CHART discussion): IODP needs to practice both long-term planning and commitment and the ability to respond quickly to new and exciting opportunities. Because of the way proposal review and scheduling is structured (takes a long time, but panel membership changes so there is a loss of memory) and the way the program has been funded (sudden spurts of funding, insufficient for full years of drilling and little opportunity to plan for the longer term), IODP has thus far been challenged in balancing these two ways of doing business. I think that a renewed IODP need to find a better balance between long commitments and rapid shifting of priorities.

Posted by GailChristeson on 2009-03-05 at 16:44

I think that we're all on the same page here - I'd definitely like to see this approach balancing long-term initiatives and new exciting science be a part of the next ISP.

Posted by Anthony Koppers on 2009-03-09 at 16:34

I have always found the ISP an interesting "guiding" document. It is curious to see how little of the ISP goals have been achieved to this day, but then with the JR being in dry dock for so long and the current fiscal realities this is a mood/sad point. The point I like to make in the following is that I think that the ISP is a good concept as long as it is treated as a "guide" to future drilling. We cannot stifle opportunistic drill proposals that

achieve top rankings during the review process. Therefore the ISP should be written with sufficient flexibility to allow for new and bright ideas. However, I like to insert another approach that was not initially included in the ISP and that is the "mission" concept.

IODP missions in my eyes only showed a glimpse of their promise, again because it was initiated just before the JR got docked and the budget went haywire! So, mission concept disappeared into the background (rather quietly) and I haven't seen much discussion on CHART about them. Yet, this concept appeals to me as it allows for (1) a broad ISP statement with maybe just four different focus areas of research, (2) missions that are designed to address specific questions related to the focus areas as they require a strategy of long-term drilling or drilling in different parts of the world as issue has a "global" aspect to it, (3) your typical IODP expedition related to the ISP, and (4) opportunistic drill proposals outside the ISP.

Mission thus only are used to address ISP goals that have a truly global character, that require monitoring over more than one year or that require a sustained drilling effort that cannot be achieved in one drill leg. There could be only one mission for one ISP focus area, but if needed, there could be more per area. The mission management teams could help to combine or fine-tune independent proposals that are already in the system, they can make certain that the overall ISP goals (often of a larger scope that can be handled by one expedition) are achieved, they can actively seek out proponents to write proposals to fill in gaps, and they can within IODP strive for technical improvements deemed necessary for the success of the overall mission.

Examples of potential future missions are ample: (1) the global environmental effects of LIPs, (2) understanding the structure of oceanic crust along a tectonic flow line from young crust to the most mature ocean crust, (3) determining the chemical input parameters into the subduction zone factory by drilling sediments and oceanic crusts outboard of the trenches, (4) studying the Earth's geodynamo at high latitudes in the Southern oceans, (5) understanding mantle convection and true polar wander by studying seamount trails in the Indian, Atlantic and Pacific oceans, and so on.

Posted by Mitch Lyle on 2009-03-12 at 06:17

Anthony refers only to the JR but IODP-MI is based on 3 platforms, so one must base the success of the program on all 3. The long period for the JR in refit has been only one constraint on the success of IODP--remember that there has not been an MSP program since 2005, and the Chikyu is doing less than half the drilling that was expected at the beginning of the program.

The fundamental problems with the mission concept is that there less drilling time to divide than in ODP and no explicit resources beyond drilling the holes. While I think that the example missions are interesting, focussing on any one mission will displace equally important science for years. The science plan should reflect the potential number of resources that can be directed at each problem.

Posted by Anthony Koppers on 2009-03-12 at 11:49

Mitch is correct that the success of IODP should be read from all 3 platforms and that (as we all are aware off) none of these platforms have seen much sailing time lately. My post is based on a science plan after the current IODP program which "has to be" based on 12 mo/yr drilling for all three platforms. In that context I think there is a good place for missions, provided we balance it out nicely with the other drilling proposals. I am definitely not saying we should only implement missions, but that some science questions are better tackled if we approach them through a mission, as addressing the problem requires drilling at different sites globally or a sustained effort over more than one year. In today's scenario the balancing clearly is out of the system and the concept of a mission disappeared to the wayside, which I personally find very unfortunate!

Posted by Mitch Lyle on 2009-03-12 at 11:58

I agree with Anthony that missions are a useful concept, especially to build in an IODP corporate memory for a program that may have to be drilled over many years. This is a problem shared by complex proposals.

Portions of comments Posted by Ken Miller on 2009-02-27 at 15:11

What will the role of missions be in the future? Though they took a hit in the first round, I suspect that missions must be part of the post 2013 program. But we cannot loose the random great idea/proposal.

Finally, I think the theme of the program should be a palineque, DRILL, BABY DRILL. Putting core on deck should be the first priority. We tend to platinum plate our science around these cores. The real legacy of the program are the cores. Getting the JR back to 12 months per year should be the highest priority. If it took closing IODP-MI, releasing the SAS, cutting everything else to the bone, I would advocate that: give me core or give me death.

Posted by Craig Fulthorpe on 2009-02-27 at 17:41

I couldn't agree more with the need to get the JR drilling 12 months per year. Ken isn't the only person to have said it within the forums and it seems vital to the long-term success of IODP. However, I don't really see an intensive focus on this effort, unless things are happening behind the scenes. Is the current part-time model even sustainable in the long run with or without industry support? What will it take to get back to 12-month scientific operations and how can we get there? How can we in the scientific community help?

Posted by Andy Fisher on 2009-03-07 at 13:23

I'll take exception to one aspect of Ken's cogent post: excellent science involves drilling and casing and coring and experiments. It is *not* just about coring. This is a trap the program has fallen into repeatedly, linking success to miles of core recovered. To study a process you have to test the process. Exploring the record can help, but understanding active processes as they occur today requires a different mindset. The ISP for IODP claimed to recognize this, but the record has indicated reluctance to embrace non-coring science. Make no mistake, there is a LOT to be learned from coring, but coring recovery

can not be the primary metric for success. Getting excellent science done should be the first priority. Sometimes this will mean collecting no core.

Posted by Ken Miller on 2009-03-07 at 13:28

Andy is right that science involves more than cores. To a certain extent, though, cores remain as a primary legacy to the generation coming behind. But I think we can agree on the mantra, drill baby drill...whether for cores, logs, or corks.

Posted by James Channell on 2009-02-18 at 15:39

Asking the question: “what’s should be the prime organizational priority at IODP?” at two IODP workshops in Germany last fall (amongst groups of IODP advocates mainly from Europe), I got the same reply: “Get the JR on a full-time drilling program”.

There is a wide appreciation that Chikyu and Mission-specific platforms are not the “bread and butter” of the IODP community in the US or elsewhere. The program has been severely compromised and the expense of a proven drilling facility (JR) at a time when the science provided by the JR has such societal and scientific relevance.

This view is echoed in the report of the Ad-Hoc Committee of IODP-MI (Dec. 2008) that lists “adequate funding for continuous 12 month operation of the JR” as the number one priority.

Posted by Chris Harrison on 2009-02-20 at 16:24

Assuming that the prime concern is to have the JR run continuously, it seems logical that if an integrated program is to be considered for the next period of time, then there has to be adequate funding for all of the platforms. This probably means that other countries or groups of countries are going to have to contribute a greater percentage of the costs than is currently happening.

A5.7 How can we improve funding for technical development and site surveys?

Posted by Organizing Committee on 2009-01-12 at 14:32

How can the program manage and find funding for important technological development activities that support drilling and scientific observation? How can the program ensure that IODP drilling science is adequately supported at appropriate levels for necessary pre-drilling site characterization and complementary post-drilling scientific activities?

Rationale: The issue of technical development and site survey support has dogged the scientific drilling community from the days of DSDP. In earlier times when budgets were stronger (at least in our memories they were stronger), funding was found for critical technical developments, site surveys, and follow-up work. It is not clear that base budgets can sustain these efforts at necessary levels going forward.

Posted by Greg Moore on 2009-02-04 at 15:17

Well, if the base budgets can't sustain these efforts (site-survey and post-cruise analyses), then the program is dead. How can we NOT fund the surveys that characterize the sites to be drilled? How can we spend millions of dollars to drill a site if we can't properly identify the local/regional characteristics of the site? How can we spend millions of dollars and many man-years of scientists' time on the drill ship if we can't afford to properly analyze the resulting data?

So, We MUST insure that adequate pre- and post-drilling resources are allocated to do the job properly. If that means drilling fewer sites, then we should drill fewer sites. A large number of poorly constrained holes with inadequately analyzed data gets us nowhere.

How to manage and find funding? If NSF can't increase the base budget available, then we have to reallocate the funds internally.

Posted by Dale Sawyer on 2009-02-04 at 17:07

I echo gmoore's sentiments on these issues.

The amounts and kinds of pre-drilling site characterization required for addressing different scientific questions varies considerably, but the bottom line is that IODP and the previous ocean drilling programs have a track record of doing too little. Sometimes this leads to putting a hole in the wrong place. Sometimes it leads to our not having adequate complementary data to effectively interpret the drilling surprises that occur more often than not. In either case, it reduces the value of the drilling we do.

I like to think of a future IODP as a sub-seafloor science program rather than as a drilling program. Good sub-seafloor science requires the application of a suite of methods, often including but not limited to drilling.

Posted by Mitch Lyle on 2009-02-04 at 17:24

I agree that IODP should be sub-seafloor science, where both pre-drilling and post-drilling science are integrated into a coherent program. Now, the 3 parts of a successful drilling program are uncoordinated and the lack of coordination inhibits the total scientific output.

Posted by GregMoore on 2009-02-04 at 17:58

Mitch is onto something here. For NanTroSEIZE, we now have a "Project Management Team", which has been coordinating the long and complicated drilling program. The PMT helped guide the 3D survey prior to drilling. We also have "Specialty Coordinators", who are helping to guide both shipboard and shore-based science. Perhaps all expeditions need to have some sort of management team in place to assure proper pre- and post-drilling coordination?

Posted by JosephStoner on 2009-02-18 at 15:59

The idea of sub-seafloor sciences becomes incredibly important if you are working on them, say for example, reconstructing the dynamics of the geomagnetic field. What is required is a particular type of site that can provide the information of choice, but from a

global array of locations. So how do you do this, when you view every project from a location prospective? To answer the question in this example, you need global information about sites with specific characteristics that could then be piggybacked on to a drilling leg when it got to a specific location. Then you have to make sure that the right people are working on it when it gets drilled. For this type of global question, as Greg Moore stated, you really need to have a scientific management team.

Posted by Alan Mix on 2009-03-05 at 15:47

I think most agree that a problem with the program has been the limited integration of pre-drilling survey, drilling, and post-drilling science – the issue Mitch Lyle raised here. Have we been caught in a trap of our own making, trying to make the overall cost look cheaper by compartmentalizing the funding of the different components? It doesn't make the costs go away, but it does increase the disconnects, and we end up with missing pieces in a complex jigsaw puzzle, and then with handwringing that we haven't accomplished as much as we could. Should we work to package it back together?

Posted by Eli Silver on 2009-03-02 at 16:50

I agree with the comments about the need for effecting site survey information. One simple thing the drilling program can do is to closely tie the recommendations of the site survey panel to decisions about drilling. There has been a serious disconnect between the SSP and decisions on ranking proposals. If proposals were required to show that background information was sufficient, there would likely be more pressure on NSF-ODP to fund the otherwise highly ranked proposals.

Posted by Greg Moore on 2009-02-06 at 12:21

One strategy that has worked well for Nankai drilling is partnering with the Japanese groups. We were able to get joint funding for our 1999 3D cruise through our colleagues at Ocean Research Institute. For the 2006 3D seismic cruise, JAMSTEC put in > \$7M for chartering the vessel, with NSF supplementing this with ~\$3M. JAMSTEC also provided a year of funding for 3D Pre-stack depth migration processing. We have MANY Japanese colleagues who are interested in all aspects of IODP drilling and JAMSTEC now has a policy of providing site survey ship time for Proponents outside JAMSTEC. Thus finding a Japanese co-proponent should open the door for a joint cruise. AND this will surely strengthen the proposal as it winds its way through the SAS. And, don't forget that our European colleagues (in Germany, France, Spain and Italy, for example) also have access to national research vessels -- we should also be forging better alliances with them. Demonstrating true international collaboration will help the folks at NSF justify additional funding for regional site characterization cruises.

Posted by Sean Gulick on 2009-02-11 at 13:13

I would like to echo this strategy. By example we recently acquired a seismic survey off Sumatra in collaboration with UK and Indonesian colleagues. The UK already had 100 funded ship days; getting an additional 10 funded by NSF so that the US had a "seismic" seat in the room on this exciting margin was a clear winner with NSF. We should seek to foster international collaborations on the site surveys both as these are viewed well in the competition for resources, but also as a cost-savings measure. Additionally, the

international teams that grow out of these collaborations can then serve as solid working groups for improving the chance for success of associated IODP drilling proposals.

Posted by Henry Dick on 2009-02-23 at 14:37

In the US funding for site surveys was decoupled from proposals to drill some years back. This led to quite a few proposals, including some that were funded, by P.I.'s who had no interest in actually drilling anything, but saw NSF/ODP as another bucket of money for their non-drilling related science. I don't know if this is still true, but if it is, it isn't good for scientific drilling. Site survey funding should be tied to a reasonably ranked pre-proposal or proposal under active consideration by the SEP's. No serious proposal in the system - no site survey funding. Moreover, proposals that fit the ISP well should have a leg up on getting site survey funding. Site surveys along with the drilling are a single scientific product, and if the proposed drilling product is not considered as part of the site survey science proposal, a lot of the routine work needed to support drilling becomes very difficult to get funded.

Posted by Greg Moore on 2009-02-24 at 00:05

Yes, Henry is correct. I agree that Site surveys are an integral part of the drilling program because they drive the location of the drill sites AND they allow regional extrapolation of the drilling results. Indeed, site survey funding should be tied to a proposal that has been "blessed" by the SSEPs. The drilling and survey proposals need to be strongly linked.

Posted by Kevin Brown on 2009-02-25 at 12:12

We also a more coordinated program for technical developments (instrumentation) at least in the US and/or in collaboration with our colleagues abroad. There seems, at the moment at least, little momentum out side of riser drilling to advance technical improvements or innovation given the lack of funding. Given the expense, combining forces might be a way to go. Also do we go for a very few big expensive things or spread our risk and have a larger number of smaller cheaper advances (i.e. in observatories, down hole experiments etc.). Technical advances are often being driven from the oil industry and we should certainly apply what we can from this source but do we also need to think smaller as some oil patch solutions are quite costly? Could cheap shallow bore hole observatories cover a lot of what we need or do we really have to drill deep to get at active tectonic processes and the subsea biosphere for example?

Posted by Casey Moore on 2009-02-25 at 12:49

Drilling is expensive. Drilling in the absence of good site surveys and appropriate post-drilling instrumentation fails to leverage the borehole and moreover could squander the drilling expense (if the borehole is in the wrong location). Industry figured this out decades ago and invests a lot before they drill their deep offshore holes.

The discussion of partnerships above is very appropriate. DSPP, ODP, and IODP have been/are first and foremost international programs. I'm particularly familiar with the cooperative efforts with Japan in site survey and instrumentation that have been of huge mutual benefit. Although, differences in national scientific cultures are sometimes hard to bridge (and I've complained a lot), the future is in spanning these gaps.

Ultimately site survey/instrumentation becomes and expense issue. If necessary I would support fewer but better surveyed and instrumented holes.

Posted by Nathan Bangs on 2009-03-04 at 16:53

Better site survey data will both help to assure drilling is in the best location to maximize results, and it will maximize the interpretation and analysis of the drilling data. What is critical is to acquire the data sufficiently in advance to do proper data processing and interpretation. This can, and probably should, take years for a 3D seismic volume. There is kind of a "catch 22" with waiting until we know drilling is highly likely to have site surveys funded, which seems to be the current situation. I agree with the comments above that SSEPs evaluations can help prioritize projects to determine how to effectively use site survey funds, SSP should have a strong role in this also, but any sort of "blessing" needs to happen soon enough for planning the site survey, acquiring funds, and actually doing the large effort that needs to go into working up the data before serious planning for drilling. We probably can't always get it right in deciding which sites have the greatest surveying needs, but thorough analysis of the data early in the planning will greatly improve our success. Industry does not think about drilling until extensive survey work is done to make good use of the most expensive part, the drilling. Even if it means fewer site surveys and less drilling, we are better off with having proper site surveys that have been worked up sufficiently in advance to be effectively used in a drilling project.

Posted by Peter Flemings on 2009-03-05 at 07:25

I completely agree with Kevin's comment that 'there is little momentum to advance technical improvements or innovation' and I strongly feel that this needs to be focused on in the future program. The Engineering Development Panel (EDP), which I participated in several years ago, attempted to lay out both short (2-5 yr) and long term goals for engineering development that would enable achievement of the goals of the ISP. It is my opinion that we need to continue to advocate for advances in engineering development. It has always been a great challenge to do this and the question of how to do it effectively is a hard one. None the less, without new technologies, we get locked into measuring the same things over and over again and science advance is limited. Many, if not most, of the advances are not blue sky research. Rather they involve transferring techniques developed in mining, the oil industry, or the space industry.

Posted by Rob Zierenberg on 2009-03-13 at 15:56

One of the most fundamental things the drill ships do is provide core. We know that the recovered core is biased in some environments and that many environments, and many important scientific problems, cannot be addressed by the program due to difficult drilling conditions. One of the most important technological developments we must pursue is better core recovery. I am extremely pleased to see that drilling data will now be included as part of the scientific data set recorded and archived on each leg. Many may not appreciate the potential scientific importance of making this engineering and operations data part of the leg results, but these data have significant value for interpreting subseafloor conditions where core recovery is spare or highly biased.

I feel we must continue to support engineering development efforts aimed at improved core recovery if we are to advance our science, and this will require community agreement that this should be prioritized and supported. I would encourage those who have long experience in the program to separate the failure of the Diamond Core System (DCS) from the potential for adapting and developing new coring technology. We must be willing to commit time and money to this effort if we agree as a community that it is a priority for opening new areas of scientific inquiry. Andy Fisher made an excellent suggestion when I was discussing these issues with him. One mechanism for advancement of our drilling technology is to have calls for proposals, which would include ship time, as a means to tap the expertise in the academic, as well as the industrial, engineering communities. We can potentially engage an entirely new community in participation in Ocean Drilling, thereby broadening the support base for renewal of the program if we do this right.

Posted by Bill Ussler on 2009-03-13 at 16:29

I agree with Bob and Peter Flemings comments. The Engineering Development Panel (EDP) has formulated an IODP Technology Roadmap and identified high priority engineering development needs of the program (www.iodp.org/eng-dev). Improved core quality and quantity (CQQ) is a priority and IODP-MI is presently conducting a core quality/quantity scoping study at the panel's request. Missing from this analysis is any drilling data; as Bob pointed out the drilling data now being acquired on the JR will make it possible to better isolate factors influencing CQQ.

Regarding Bob's suggestion concerning calls for engineering development proposals, unsolicited engineering development proposals can be submitted to IODP-MI on an annual basis NOW (see www.iodp.org/eng-dev for more information). The deadline is April 15th. A mechanism for requesting at-sea engineering testing is presently being formulated by IODP-MI. I encourage broader participation in the engineering development process from the academic community as well as industry. Engineering development is going to become more critical as more challenging scientific targets are identified in future scientific drilling proposals.

A5.8 Education of sea-going scientists for the future of ocean drilling

Posted by Alan Mix on 2009-03-05 at 15:33

Here I'd like to address the issue of maintaining the tools and expertise needed to support drilling.

My perspective comes from having run several site-survey efforts for paleo-focused drilling legs, and having assisted others in developing survey or site selection efforts. As our community has broadened beyond the "traditional" big oceanographic institutions (which is a good thing) we have lost the concentrated expertise in how to actually run the survey programs that lead to the very best use of the drill bit. Relatively few of our institutions are teaching the practical hands-on survey skills on interpretation of geophysical survey information, even the relatively simple stuff like single-channel (or

limited multichannel) reflection profiling. Some of the smaller institutions may not have the expertise in these areas. The larger ones may teach these topics, but tend to focus on the few grad students who will major in the field, and the latest whiz-bang high-end gizmos, and not on the broader array of students who need the basics. Mea culpa: Even here at OSU, some of these topics disappeared for a while from our intro grad courses, and we're big enough to be on top of it – there is just too much information to cover as the field has grown, so something had to drop. I think we've recognized it and are working on getting that stuff back in our courses. But still, with less survey cruises going out, grad students may now finish their PhD's without ever going to sea. There's a recipe for future collapse!

The result? I've been to sea with well-known paleoceanographers who don't know the difference between 3.5kHz profiles and GI-gun records, who wouldn't spot a hiatus if it bit them, and think just because there is a decent 3-meter gravity core on a site that it is a perfect candidate for a 50-m long core or a 200-300m drill site.

The issue extends to the ship operators – budget squeezes have forced them to trim the technical expertise that supports some of the tools, so there is a thinning of local institutional expertise in running the tools. My last cruise actually outsourced the swath bathy to an industry contractor (which was great in terms of the quality of the tools, but the contractor came without any particular interest in the scientific context). We have to be willing to maintain tools and expertise, recognizing that even if a tool is used three months per year, it needs to maintained for all 12 months, and that maintaining the expert human resource is included in the true cost of doing business. We are pretty good at sharing ships through the UNOLS mechanism. We try to be efficient at sharing tools on those ships but that seems to be less perfect and may need stronger attention. And paying the technical experts what they are worth is part of it. Afterall, surgeons have no problem charging \$1000 for a 15-minute procedure -- effectively \$4000/hour. That's \$100/hour for the work, and \$3900/hour for knowing how to do it! (Don't worry, I'm not suggesting we charge \$4000/hour).

So what about the issue of maintaining expertise to use ships and tools wisely? Might we fix this by funding some workshops, perhaps including some shiptime, to educate current graduate students about tool use in survey situations? Would NSF support regularly scheduled short training cruises to open up hands-on education to students from any institution? In the long run, the benefits could be huge. Could we improve mechanism to share information about open bunks on survey cruises, so that students from any institution can join in, perhaps as "free" labor, while learning the basics? Might there be an incentive to do this, perhaps supplemental funding like an REU– but FEG, Field Experience for Grad students)?

Posted by Christina Ravelo on 2009-03-09 at 11:26

Alan - Your post is full of good points and ideas. I agree that there is a foundation of knowledge that people interested in drilling should have, but not enough opportunities to acquire it. This makes it hard for new people from all backgrounds to enter the world of ocean drilling, even if they have lots of great ideas about how to use the drilled holes and

the recovered cores. We need to break down these barriers, and increase the pipeline into the drilling program.

You have listed some great ideas for doing this and it could be very productive to get some people with your level of experience together to brainstorm other ideas. I like your idea of a coordinated way to get the word out about extra berths on site survey cruises, and to use those as opportunities to provide basic training. USAC has talked about developing support services that provide expertise to those that need it - a sort of service that helps bring the right combination of expertise together. This happens naturally amongst groups of colleagues particularly from large institutions, but there are lots of people who need more opportunities to be included into big projects. I think it could be USAC and USSSP's role to help coordinate these types of activities. We have so much talent in the science community, there should be more ways for us to learn from each other. In fact, one could say that the health of the program depends on it.

Posted by Alberto Malinverno on 2009-03-09 at 14:52

A possible way to improve training and involve more graduate students and early career researchers may be some form of short course / summer school on ocean drilling. The course/school would cover the major steps involved in preparing a drilling proposal, including the basic expertise in marine geophysics for site surveying that Alan mentions above. Other topics should be introductions to drilling, well logging, shallow and deep coring, and core analysis. Ideally, the course/school would be coupled with a JR port call so the participants would be able to see the drilling hardware and the shipboard labs firsthand. This may be an effective way to involve new participants in the program and fill some of the shortcomings in basic knowledge that Alan describes. As Christina suggests, maybe USSSP can help in coordinating this initiative.

Posted by Sarah Strano on 2009-03-09 at 16:45

I actually mentioned the topic of educating us new people in the Broader Impacts part of this discussion board but I think that it fits better here. I strongly advocate the inclusion of a short course type thing that has been mentioned in these previous posts. I'm a new grad student at OSU who doesn't understand the first thing about an oceanographic cruise. Through courses, I've read Leg Summaries, Initial Reports, etc. and seen pictures but I really don't know the first thing about drilling, making it very difficult to write drilling proposals. I want to learn because I want to be able to participate in cruises as a research scientist in the future but I think that a short course or workshop would be very useful in setting me on the right direction. I realize that as a grad student, I have to do a lot of the leg work myself in order to learn the ins and outs of both the IODP program and ocean drilling but it would definitely help give grad students a leg up if we had an introduction to the very overwhelming and complicated IODP world. I don't know if it is best to organize this through NSF, CHART or if each school should be considered responsible for making their own course, however, it seems unfair to people in small oceanography programs to make each school responsible for conducting a short course. I think that you will find many graduate students very eager to participate in these courses, especially since ship time is so limited, and as a student, I would be happy to help in the organization in anyway I can.

Posted by Frank Rack on 2009-03-11 at 14:08

I like the idea about having short courses or summer schools covering these various topics - we've seen the logging school in the past and this was fairly successful in introducing the community to the techniques and importance of logging combined with drilling/coring. We can integrate video from the ship and hands-on exposure to coring tools and cores if this were held at an IODP Repository or Implementing Organization site. The idea of combining this with a visit to the ship is great, but potentially expensive if the ship is at a distant port. I'd go with involving more participants rather than spending more on travel costs, but capitalize on port calls when appropriate.

Posted by Maureen Davies on 2009-03-09 at 16:41

I'm in agreement that Alan raises excellent points here, and I really like the idea of organizing some kind of short course to introduce students to the basic knowledge they'll need to organize a successful drilling cruise. However, as a grad student I have to say that some of the most practical education I've received has taken place preparing for and going to sea. I think putting together a program to advertise extra berths on the site survey cruises is a good first step; that said, I have some concerns.

As 'free labor' on these cruises, grad students whose advisors aren't directly involved in the research can be easily relegated to some small menial task that has little to do with their own interests. I have seen this happen more than once, and particularly in the case of young grad students, it can be intimidating to ask the PI to involve and educate you in the process of site selection. Forget intimidating; a young grad student well might not even know to ask. And, as Alan pointed out, many grad students now graduate in Oceanography and Paleoclimatology without ever going to sea. As young grad students aren't as far in their research, and their flexible academic schedules are more conducive to travel, it seems that those with the least idea of what they need to learn for their future careers are most likely to have the opportunity to go to sea. Perhaps it would be worth it to have some kind of protocol, or even just an informal understanding, that the PI's should make an effort to involve grad students in site selection, at least for a day or two. On a 45 day cruise, I feel it would be reasonable to pull students from making smear slides or core logging for a few hours for that purpose. Particularly if the students have funding unrelated to the cruise at hand, and are there essentially as volunteers.

On that note, I know that IODP has a structure to fund student participation, but does NSF have any vehicle for funding students to fill extra berths on site-survey cruises? I recognize that PI's are unlikely to want to shell out for 'unnecessary' labor, but likewise I feel that many advisors could be (justifiably) reluctant to sacrifice their students' time on unrelated funding for survey cruises lasting over a month. Perhaps a funding pool of that nature does exist, in which case please forgive my ignorance. I'm an IODP grom.

Posted by Frank Rack on 2009-03-11 at 14:04

Maureen raises some important points. I would suggest that graduate student involvement in site survey cruises be guided by a "mentoring plan" that outlines how they will be utilized to achieve the optimum mutual outcomes for both the cruise and their professional development. While there is often a fairly busy schedule on these cruises, there should be time carved out for these mentoring activities - perhaps utilizing senior

graduate students or post-docs in addition or in place of the PI's or Chief Scientists to guide the younger graduate students. This hands-on training would address some of the concerns that Alan raises as well.

The funding issues that are raised involve how the survey proposal is structured - we could encourage PI's to reserve some positions for students outside of their home institutions when developing the survey proposal itself - this would need to be a community initiative and may need some guidance from USSAC or NSF. The students are essentially labor, but they should be "informed" labor with mutual expectations established between the PI(s) and the student(s).

Posted by Mitch Lyle on 2009-03-12 at 08:07

Having worked hard to get graduate students from other institutions onto my site survey cruises, I think getting students to sea is a fundamental part of the health of our science.

I also agree with Maureen that one of the problems is that students in 'warm body' berths don't get as much training as they should. A major part of the problem is that the chief scientist doing the site survey is already committing 15-20 hours a day to the fundamental goal of the project, and cannot commit much additional time to a training mission. One solution for that problem is to take senior people to sea with the explicit task to mentor the students and organize the needed training. However, in the current structure it is difficult to find funding for the mentors, and to get them credit for the job that they are doing. Why not have some funding through the USIO that might accomplish this?

Incidentally, IODP does not have a structure to fund student participation. Instead, the USIO has a mandate to fill some of the US science berths for each drilling cruise with graduate students . Unfortunately, there is no training money for these students, so they have to learn on the job if they don't already have the skills. Also they will, like everybody else, be relegated to a menial task that may have little to do with their own interests. It must all be done to make the profound synthesis that comes from the drilling, however.

The international partners in IODP have their own structures for student participation in drilling.

Posted by Masako Tominaga on 2009-03-12 at 12:54

I have sailed both on site survey cruises and drilling expeditions during my graduate student period. From my experiences, I agree with many of the opinions raised here. In particular, I strongly agree with the idea that students on any cruise should be mentored more closely. I saw many students (even on the drilling expedition) "got lost" because they were not sure why they were there, and eventually gave up to engage their duty that causes lots of small mistakes and overlooking things while they are working as watchstanders. Are these "phenomena" fully their faults? I do not think so. Graduate students should try to be independent in science work and matured in work responsibility, but also must be guided when they need to be. To make our science onboard accurate as possible, to make next generation's career, and to make the health of our science, there are something we will be able to consider.

For drilling expeditions, I do agree with the intro training/short course idea. Regardless to whether a student will sail on the JR, there should be many oceanography grad students who are interested in the drilling projects, but no idea how to get involved. I think the first thing Ocean Leadership / USAC / USSSP can consider is to build a structure to help out grad students. Maybe difficult to consider extra berth on JR, but easy to start with a training/short course. For example, if Ocean Leadership can provide "school of rock" type of workshop (of which the template of structure is already there) for graduate students at IODP-TAMU to introduce program and have small lectures on drilling science & onboard duties from experts from each field (sedimentologists, igneous petrologists, paleomagnetists, logging scientists, etc.). Lecturers are on volunteer basis with travel support to IODP-TAMU, invited from experienced senior/junior scientists from past expeditions. This workshop will be able to (1) inform the students can know what's the drilling science, how to get involved, and what they can possible do, (2) encourage those students whose advisors are not involved in IODP, but the students want to do drilling science for their theses, and (3) connect scientists from one generation to next.

For cruises, both site survey cruises and drilling expeditions in general, my opinion is that co-chief scientists who invite grad students, advisors of invited students, and students themselves altogether should be on the same page to understand what the students are getting into. Co-chief should not just invite students to have a cheap workforce, students' advisor should not send student because their friends ask them to do so, and students should not sail just for resume or vacation. I think co-chief and advisors can educate each other about what is the meaning of bringing grad students onboard. They should make sure students understand the purpose of cruises, possible duties onboard (and make sure these duties are often very boring), responsibility for science (data) from multi million dollar cruise. It would be way too much for co-chief to be mentor for all grad students all the time during the cruise, so I think this part has be done prior to sailing.

Posted by William Sager on 2009-03-13 at 09:15

This is an interesting discussion and raises an important point, although I am not sure anyone has hit on it just yet. We are losing our expertise in marine geophysics because the field is starved of cruises. Furthermore, the situation on site surveys, which are critical to designing and implementing IODP cruises, is ridiculous. We are frequently expecting 21st century drilling results from mid-20th century geophysical data. And that is not reasonable.

The lack of cruises is a big problem because it is difficult to maintain expertise when the budgets aren't there to keep them up. The lack of cruises is a big problem when our students can't find opportunities to go to sea. The lack of cruises is a big problem when we don't have the best geophysical data to develop detailed geologic models of our sites or to show us the way to drill.

Although the idea of a "short course" in marine geophysical surveys is an interesting one, and probably worth doing, there is nothing like actually doing a survey. I don't think that you can replace the experience and knowledge gained at sea with that from the

classroom. In my experience, there are a lot of students going on site survey cruises, although there is no "clearing house" system to invite them (you have to know the right people) and there is no supplemental funding for these students. Both would be a good idea; although, there is the risk that if the students come along and don't have strong guidance and direction that their experience will not be a good one.

So I advocate that NSF needs to: (1) increase marine geophysics, site survey funding; (2) provide funding for data facilities (e.g., seismic, multibeam) that collect and archive data and provide training about processing; (3) provide additional funding to allow students to go on cruises, and (4) set up a clearing house (UNOLS, perhaps?) so that students can apply to go to sea on these cruises. Taking on another thread of this discussion, about training cruises, we have to be careful because NSF will not support non-competitive efforts and training cruises can be a waste if they have no purpose. So I suggest that there might be a competition in which investigators can apply for training cruise funds to get a short cruise (say 1 week or so) in which there is a good science problem and the purpose would be partly to train students. Week long cruises means that you aren't making them too expensive, too long for many students, and you aren't substituting another path to funding science-oriented cruises.

Posted by Stephen Pekar on 2009-03-13 at 16:28

After reading the thoughtful comments above, there is not more to be added. However, here are some of my thoughts. With the strong foundation that has been laid with the School of Rocks, IDOP can now move forward with developing high impact, educational and media outreach activities that can reach a large audience. While ocean drilling has written major parts of the climate and tectonic story for the last 100 million years, the ocean drilling community has not been able to capitalize on what we have provided scientific community. Making educational and media outreach a priority can help educate the public and politicians on the contributions we make to the scientific community, which could aid in leveraging funds as well as attract the next generation of scientist to ocean drilling.

Some recommendations:

Create new educational activities using the SOR as the foundation. These can be created either from scratch or perhaps better yet by forming collaborations with pre existing educational organizations that are already successfully conducting educational activities. If we go with the latter, IODP would not have to reinvent the wheel when it comes to creating educational activities and could be a means of keeping their budget reasonable. Create more berths for education and media outreach. This includes for educators, students, videographers, etc.

Some ideas include the following.

- 1) Live video conferencing to schools during expeditions, with a 6 week educational program based on curriculum developed by the SOR. There can be a small number of schools that would interact those on the ship (scientists, educator, etc.), with as many other schools that could listen in.

- 2) Live video conference or web cast teacher training during the SOR, as well as during scientific cruises. This would increase educator participation for the SOR by orders of magnitude. For example, when done last year on an Antarctic expedition I led, over 600 teachers participated reaching 15,000 students.
- 3) Work on getting more results from the drilling programs into science textbooks and science curriculum at all levels from grade school through college.
- 4) Take the greatest hits book and develop it into a “coffee table” book that explores many of the climatic and oceanographic topics that the ocean drilling community has contributed to solving.
- 5) Sponsor a US workshop on educational and media outreach.
- 6) Submit a proposal for collaborating with museums for either a permanent ocean drilling exhibit or a traveling exhibit.
- 7) Using the scientific objectives of upcoming expeditions, look into developing ideas for science documentary programs.

All of this would require a significant increase the educational and media budget.

A5.9 IODP planning structure

Posted by Organizing Committee on 2009-01-12 at 14:30

How should the IODP planning structure be configured so as to be responsive to community priorities, including follow-through from year to year to make sure that high-priority programs are completed?

Rationale: Current planning resides mainly within the Science Advisory Structure of IODP, which has among its tasks to assess the balance and importance of science proposals when constructing expedition schedules. As we approach renewal, it is a good time to consider alternative models that retain the strengths of the current system, add new strengths, and correct deficiencies.

Posted by Henry Dick on 2009-02-04 at 11:00

The current structure seems to be overly political and cumbersome compared to what we had in the past. I believe we should step back to the structure we had during ODP with more specific thematic science panels rather than just two SEP's. The current system seems to lead to misbalancing of members thematic interests to what have been defined as the principle themes of the program. If the science evaluation panels were designed more to specific themes, it would be harder for a theme to get lost, and more often than not, the membership would be better suited to evaluate specific proposals.

Posted by Katrina Edwards on 2009-02-09 at 20:50

On one hand I really appreciate the fact that unscheduled programs are re-ranked repeatedly and that this allows the very best science to rise to the top. This keeps new comers in the system interested in IODP and less daunted by long lead times. On the other hand, this makes it very difficult for complex programs to plan in advance, as NSF

and reviewer views of unscheduled programs is "maybe, maybe not" in terms of the likelihood of scheduling and thereby dedicating support towards the development of these complex programs. This can and has lead to "too little too late", sometimes for very good science objectives, for example when borehole observatories are involved. We need a system that accommodates both the quick and easy (in terms of pre-planning) and the more complicated programs, yet does not encumber the system in terms of provisions for new science.

Posted by Roland von Huene on 2009-02-18 at 16:22

As a late comer into this discussion I see no mention of Complex Drill Programs. Certainly only a couple can be drilled but without them the Chikyu will never leave Japan negating much of the "Integration" of the program. The IODP proposal evaluation process for a CDP works poorly because these proposals receive lengthy scrutiny which delays an up or down decision. Its long residency in the system results in repeated evaluated by changing panel memberships, and a depth of scientific understanding of the subject is at times limited to one panel member. Panel suggestions" on modifications are commonly reversed later and a favorable evaluation asking for minor change becomes a call for massive revision from a following SSEP review.

A suggestion to reduce this inefficiency was to have one-to-one conversations between lead proponents of a CDP and the panels. It was rejected citing cost. Once a panel finds CDP scientifically worthy, the details are most efficiently worked out in conference. At professional society conferences attended by expert panel members (watch-dogs), and lead proponents, travel costs are minimized. In an experience with the CRISP proposal, the chance meeting of proponents, a panel chair, and a panel expert resulted in revisions of the proposal that made it successful after 4 years of trying to meet a moving target. Had this meeting occurred earlier, the proposal for geophysical data required for riser drilling would have been acquired, analyzed, and reported. Note that in NanTroSeize, the analyzed 3D seismic data were not available to identify optimal drill sites. The costs for the best planning of a CDP are very small compared to the costs of mistakes at sea requiring extra days of ship time let alone the costs of information lost.

Posted by Keir Becker on 2009-03-01 at 21:34

At its January meeting, SASEC appointed a small subcommittee to consider the proposal evaluation process for the post-renewal phase. Members are Yoshi Tatsumi, Nick Arndt, and myself. I'm watching the CHART forum for any US ideas on this topic, and I'll try to represent them in the subcommittee discussions. I've seen a lot of good comments in various CHART forums, and it seems to me there are a lot of JR-specific thoughts and even biases (no criticism intended - obviously that's where most of our experience lies). One of the questions that has come up in the early subcommittee email discussions is whether more platform-specific planning mechanisms should be considered at the SPC level. E.g., it seems to me that the proposal evaluation process has worked well for planning JR operations through IODP phase I, but is that process really the optimal way to plan important ISP programs that require really long lead times and early commitments, such as major riser programs, or non-riser programs that are technically ambitious and require deep casings and/or observatories? Do we need some other complementary/parallel planning process for the latter kind of programs? Another

question that has come up is whether we should consider reorganizing the post-renewal SSEP-level proposal nurturing/evaluation along 3 or 4 thematic lines (with themes to be determined by the outcomes of INVEST). Are there any opinions on these two questions?

Posted by Liz Screamton on 2009-03-03 at 07:15

If theme groups are established for proposal evaluation, I would suggest they still meet at the same location/time so there can be interaction. It is difficult to imagine a set of themes that are mutually exclusive. One of the positive aspects of the current structure, where the entire SSEP looks at all proposals, is that it tests whether an objective is of broad interest to the drilling community. It also allows for proposals that address more than one theme.

Alternatively, a hybrid structure might include long-lasting theme committees that meet and review (possibly by phone, web, or email, allowing greater participation with a limited budget) and send representatives with their input to the "live" proposal evaluation meeting.

Posted by Sean Gulick on 2009-03-03 at 13:06

I would like to suggest perhaps instead of considering Platform specific planning mechanisms and review we should consider modifying our system of advice and review of proposals to account to variable complexity. Drilling programs range greatly in terms complexity technically from amount of shiptime to specific platform needs to types and quality of site survey data needed, to post-drilling studies integral to the science goals (observatories). However we propose science and its evaluated at the SSEP level purely on the how interesting, testable, and important are the hypotheses. While this is intellectually attractive it has some unintended effects: 1) Proposals can rise to the top of the system that are not feasible in terms of cost, safety, technical feasibility, existence of site survey data, or even sovereign rights issues, and 2) opportunities to improve proposals technically whether in engineering or imaging of the targets or integrations with efforts that are geographically/topically/technologically parallel can be mistimed or lost. I would advocate an earlier discussion of proposals technically which to whatever extent possible is integrated with the discussion of the proposals scientifically. We have the right people in the SAS to do this between STP (technological requirements), SSP (imaging the target and putting it in a regional context), and SSEP/SPC (scientific validity, testability, and merit). SPC is the level at which all of these separate thread of information are to come together so that the best and the drillable proposals get put forth for scheduling. However SPC cannot in their meetings full discuss the range of issues for the moderately to highly complex proposals. Sometimes the system works well and STP and SSP input is early enough that modifications or additional data collection can be made such that SPC can be assured they are forwarded an excellent scientific and technical drilling program for scheduling. Other times however the technical advice on a given proposal is out of sync or incomplete and key questions like: "are they drilling in the best place to answer the questions they propose" do not occur until quite late. To that end I would recommend we consider the flow of SAS advice on proposals and consider getting representatives of STP, SSP, and SSEP together to discuss the more complex proposals collectively and where necessary create a PMT for the more complex proposals

that do get sent by SPC for scheduling. As noted above, complexity and the need for this more integrated review and post-scheduling management is not directly tied to the type of platform, but could be critical maximizing our scientific results.

Posted by Mitch Lyle on 2009-03-03 at 13:35

I don't think the problem is in the review of the proposals, except that IODP has a poor institutional memory. The real problem is to develop the means to pay for all the additional science and technology that makes a proposal 'complex'. In some way we need to spin off complex drilling into their own projects, with drilling as only one aspect. This in turn means that drill vessels begin to act more like ships in the UNOLS scientific fleet.

Posted by Miriam Kastner on 2009-03-04 at 21:52

I agree with Roland that the present reviewing system of Complex Drill Programs is sometimes rather frustrating and wasteful, in particular when reversals of panel suggestions occur. Reviewing such proposals is complex, therefore the process requires special attention. Possibly, the way to avoid such problems and streamline the progress of such complex projects, is to introduce some more continuity into the reviewing process of multidisciplinary complex proposals. In addition to the existing (or similar) review panels, each such project should have an outside experts group that consists of 3 or 4 leaders in the topics proposed; they will be responsible for overseeing the review process, via interactions with the appropriate panels, and for the progress of the project, if approved, for its duration.

Regarding the question of reorganization along a few thematic lines; overarching themes for the future drilling program may be helpful as a general guideline, but the internal structure should not be structured along theme lines; the proposals should compete based on quality, innovation, and vision, within as well as outside the overarching themes, but not along thematic lines. New innovative proposals even outside the overarching themes should be seriously considered; significant new science ideas or technologies should not be bound by the state of science at the time of INVEST.

Posted by Harold Tobin on 2009-03-06 at 12:07

I think Keir is on target here. Having lived through the process for the past 8 years, it is abundantly clear to me that ANY Chikyu-based project that includes riser drilling needs a long-term careful planning process and is in fact a "mission" or CDP. The same can be said for some MSPs and some potential future JR projects, especially ones involving observatories. We need a mission approach to implement them, full stop. That implies a SAS approach that identifies and then commits to the very small number that can truly go forward. The NanTroSEIZE PMT is one model that, while imperfect, seems to be basically working. The key is early identification of the priority project, and then commitments from the program that will give the scientists involved some confidence that it is worth the years of time investment. This will also give funding agencies confidence that it is worth committing to expensive site surveys and technical development years in advance. The identification of specific key long-term projects (CDPs or missions) actually MUST start in the Science Plan document in my view -- we've already seen how quickly ten years passes. This is a huge challenge for INVEST, but I think it is critical.

Posted by Rob Zierenberg on 2009-03-13 at 13:57

Having now served on the old LITHP and SPP and the new SSEP I am sensitive to some of the criticism of the review process I read above. There is a lot of validity to the points raised Henry, Roland, Mariam and others regarding the review and evaluation of proposals. I understand the arguments for, and advantages of, moving from discipline based to more interdisciplinary panels, but I feel some rigor has been lost in the process. The old panels were forced once a year to rank proposals considering both scientific importance and feasibility. It was a difficult and painful task that forced careful evaluation and comparison of the proposals. SSEP does not rank proposals, and if they did it is my biased opinion that the ranking would be overly influenced by political factors and the "popularity" of the science proposed rather than feasibility and potential for true scientific advancement.

I also feel that comments raised about the differences between evaluating "one leg" drilling proposals suited for the JR and planning for CDPs and extended drilling campaigns using the Chikyu are right on target. The present evaluation structure is not well suited to evaluating and prioritizing the best science that is ready to drill at the moment, while simultaneously satisfying long term priorities of the program and making sure there is continuity in the program. I don't pretend to have the insight to know how to best restructure the review and planning process, but I think it will be necessary if the program is renewed.

Posted by Richard Norris on 2009-02-05 at 00:48

It is clear that we cannot sustain a system in which we spend more than we did under ODP and get half the drilling (or less!). This is particularly the case if we trade off riserless drilling for riser drilling and then drill no riser programs outside Japan (and do not even do those with some regularity). I think IODP has not been particularly good value for funds spent, not just because of the long delays in remobilizing the JR, but because of the reduced activity of riserless drilling compared to the past. The advent of the more regular use of mission-specific platforms has been the one shining success of IODP. Unfortunately, the top-down "mission" concept, the large backlog of undrilled proposals placed in limbo, and the inept duplication of effort in the repositories have been black spots. I mourn for the program.

Fortunately, we can do something about all these things. The change in leadership, the proposed reduction in management organizations, and the possibility that we may be able to fund a more regular drilling schedule are all positive developments. Drilling has been a massive success in the past and there are no end of critical questions in climate, evolution, and oceanography that can hardly be answered any other way than through drilling.

Posted by Mitch Lyle on 2009-02-12 at 12:48

We do have successful drilling, just not a lot of it. The big challenge will be to get through a sufficient number of programs by renewal time to easily justify 10 more years.

The idea that IODP must be managed through one central management structure has

proved to be a failure. If anything, the lack of success for any one operator to do drilling seems to lower the bar for the other drilling operators. There is a distressing lack of coordination among the programs, and too much platform for what the respective funding agencies are willing to spend.

Posted by Jamie Austin on 2009-02-16 at 14:35

I take Mitch's point about central management, but when the IODP Planning Subcommittee (IPSC) put the blueprint together for IODP in 1999-2001, we all felt strongly that some form of central manager was critical to the success of a complex multi-platform program. Such an entity would always see the program as a "whole", advocate for it as a fundamental (albeit expensive) scientific activity without favor towards any national facility or infrastructure, and also gently nudge the international scientific community (SAS, users,...) to be accountable for the science proposed and carried out. Without getting into the specifics of what has gone wrong with the program's current management scheme, I for one still believe that such a central entity is crucial to the success of IODP.

Posted by Mitch Lyle on 2009-02-18 at 12:22

This is an important point to discuss--I can see the importance of a central organization as a synthesizer, planner, and advocate, as well as performance evaluator and communicator. What do we need to change to get to this type of management structure?

Portions of comments Posted by Ken Miller on 2009-02-27 at 15:11

As to central management, Mitch's post points out a contradiction, "central management failed [implication is that it is not needed]" BUT "There is a distressing lack of coordination among the programs." But who will do the coordination if not IODP-MI? A fear is that the lead agencies would do this. They won't (though some would like them to). IODP-MI is an organization that has been set up to manage the unmanageable. I suggest it ain't goin' nowhere folks. The best we can hope for is that the regime change in IODP-MI together with the first operation of 3 platforms results in more coordination. Some things at IODP-MI (like OTC) work very well.

I think the discussion needs to go back to the future, doc. What is the role of the SAS? I think it is pretty darn important. As many people participate and gain from participation in the SAS as do sail. I think scientists directing the program (SAS) as opposed to scientists in admin positions is a sine qua non (admins run, scientists direct). So I think of discussions of a dramatic cutback in the SAS should be squashed.

A5.10 How can IODP contribute to "broader impacts?"

Posted by Organizing Committee on 2009-01-12 at 14:29

How can IODP have a higher profile with regard to Broader Impacts, for example as defined by the U.S. National Science Foundation? [please see: <http://www.nsf.gov/pubs/gpg/broaderimpacts.pdf>] Should IODP give priority to projects and programs that have strong societal relevance? What metrics might be used to assess intentions and success in these efforts? Should more IODP resources be spent on

education and outreach programs, curriculum development, and entrainment of nonscientists?

Rationale: Individuals within the program are well aware of the paradigm-shifting results in climate change, hydrological processes, plate tectonics, ocean biospheres, etc., that have come from scientific ocean drilling. It seems that IODP's role in these impacts has not been recognized extensively outside of the drilling community. Perhaps we need to think of a new way of linking scientific ocean drilling to society. NASA spends about 10% of its budget on E&O, whereas historically NSF-funded research projects have spent considerably less.

Posted by Frank Rack on 2009-02-03 at 17:29

Broader impacts also involves the training and professional development of scientists, students, engineers, educators and technicians. We should do a better job of capturing outcomes in these areas so that we can provide NSF and the general public with specific information about how the experience gained by participation in scientific ocean drilling translates into degrees granted, research accomplished, course material developed, collaborations established, and a wide range of additional corollary outcomes that wouldn't have occurred without exposure to these opportunities.

Cores and data collected by scientific ocean drilling are precious resources that can only be effectively collected in big science projects like IODP. We need to do a better job of utilizing these materials for research, education and outreach and make the collections more accessible online - not just the data in LIMS or JANUS, but the high-resolution core imagery as well, using visualization systems such as CoreWall and Lambdavision that support data integration and collaboration over distance among groups of scientists, students, or educators at their home institutions.

The development of the "School of Rock", both onboard the vessel and in the core repositories, has been a significant success that should be expanded as a teacher professional development opportunity and a means of involving a wider spectrum of educators and their students. The CCLI products and outcomes created by Mark Leckie, Kristen St. John, Kate Pound and others that incorporate DSDP, ODP and IODP data (as well as ANDRILL and lake coring data) in educational modules is another example of the impacts that can come from more of a focus on education.

The use of "new media" in providing opportunities for public outreach and education should definitely be expanded and capitalized upon. Providing basic resources (video cameras, editing software, network connections, etc) and a policy framework that encourages the use of these resources are prerequisites for lowering the bar for these activities to be routinely accomplished on the vessels. Blogs, podcasts, short video productions and related efforts help to personalize scientists as real people who the public can relate to, and which provide early career encouragement to students being exposed to scientific inquiry and research activities for the first time. K-12 teachers are great resources and eager participants in these outreach opportunities, if you can make the products meaningful to their mission to incorporate content that conforms with federal and state science standards. By giving teachers quality products that serve their needs, the

impact of scientific drilling could be magnified considerably. We have started to do some of this, but we need to do more. Providing stock video footage that students or educators could use to create their own products and tell stories that they are interested in would be another way to increase access to science that is done far out to sea for extended periods of time out of the public view. We can also expand the use of video and tele-conferencing during expeditions, but we need partners who have access to appropriate technologies to support this programming and we need to develop content that feeds a perceived need.

Posted by Henry Dick on 2009-02-23 at 14:55

No scientific drilling leg should be scheduled on the basis of "broader impacts". First, the raw science value of ocean drilling and the demand for it far exceeds the supply, and the final justification of this program, and any leg that sails, is the scientific product. Second, the program already has enormous "broader impacts". When I first sailed as a young post-doc, I was a parochial US regional geologist. I returned from my first DSDP cruise an international scientist. The transformative impact for young scientists, post docs, and graduate students working for two months in an international team of multi-disciplinary scientists cannot be overstated. There is always room to better document the broader impacts of this program, but they are enormous.

Metrics is an interesting question, but I bet that the publication rate of IODP scientists in international journals is unusually high. It would be interesting to follow up on this for graduate students, post docs, and early career scientists who have participated on an IODP leg; I'll bet it will be higher than would ordinarily be expected. It would also be interesting to see a compilation of the reactions of these scientists to their experience on the drill ship(s) post expedition.

Societal relevance is a tricky business. Basic research is always societally relevant, though that connection is often hard to make to a lay person. What appears to have little relevance today, can be crucial tomorrow. That is a major reason for governments to fund basic research. In recent years, it strikes me, that the community has done a poor job of defending the value of basic research to the public, and we need to spend a lot more time and effort on this.

Posted by Rick Colwell on 2009-02-25 at 14:27

I agree that basic research is always societally relevant. But the connection *must* be made, even if it is difficult to do for the public and policy makers. Each of us who is a part of IODP should have at hand a list of the research accomplishments (some anticipated and some that we never thought about beforehand) that have been achieved through DSDP, ODP, and IODP. These should be in a form - maybe graphically - that can be explained to the average person. Maybe we already have a list of such accomplishments and I am not aware of it?

Posted by Andy Fisher on 2009-02-26 at 00:33

I agree with Rick - for decades scientists doing basic research have sown the seeds of disdain for a public that is not smart enough to understand how important science is. Now we are reaping the whirlwind of mistrust and misunderstanding. I'm not blaming DSDP

and ODP in particular, but high-profile programs need to take on a disproportionate share of the burden. E+O is not an add on - it needs to be integrated as part of the program. This is the new reality, one that many involved in IODP (and in the realm of basic research in general) may not grasp at the moment. Big programs are big targets, and require especially big justification. I don't think that "trust us" is going to work anymore.

Posted by Rick Colwell on 2009-02-26 at 02:14

Well put, Andy. I think this is very important. The ISP section called "Major Achievements of Scientific Ocean Drilling" has examples of why these seafloor drilling programs were essential for obtaining our current understanding of the earth. The section reports remarkable discoveries. But a non-earth scientist might not understand the value of these discoveries. We can put our heads together and explain why elements of these drilling programs and the discoveries that they have generated have been essential to our practical knowledge of the planet.

Posted by Frank Rack on 2009-02-26 at 04:27

One potential approach that I've thought about recently is to annotate each of the DSDP/ODP/IODP site locations in Google Earth 5.0 with a brief statement about their scientific outcomes and significance as well as links to other sites with data, images, video and audio commentary. Organizing this and making it systematic in our standard operating procedures is something that could be the topic of a community workshop, or a task assigned to the implementing orgs. for IODP, or something done by IODP-MI. Being creative in the ways that we try to reach a broader public audience means thinking outside of the box. I would recommend that the scientific party of each expedition be asked to create one or more short podcasts or a video summary of the significance and outcomes of their expedition that could be posted online and linked to YouTube or some similar outlet to keep the program in the public eye. This effort could trickle down to provide resources for teachers and students in the long run. There is also a new online service provided by UC-San Diego that allows scientists to present their research papers in a video format, with text and figures accompanying them in a adjacent window. While this may seem unusual, it may be a good way to personalize the research being done in IODP, and also from past programs, so that the public and colleagues in other disciplines can follow what is being learned from the research. These are just some initial ideas to start the discussion, but I'm sure that there are many more that are low cost and could provide high impact over time.

Posted by Liz Screamton on 2009-02-26 at 10:10

Creating a series of 2 to 3-slide powerpoints suitable for Intro Geology or Oceanography would really help get IODP out to undergrads. I've searched for examples to supplement lectures, and sometimes they're tough to find.

Posted by Mark Leckie on 2009-02-26 at 15:17

It is indeed refreshing to read this discussion. I completely agree that we must do a better job communicating the excitement and relevance of basic science, as well as the discoveries made with each expedition. I agree with Andy that the time has come for scientific ocean drilling to be much more engaged in education and outreach. I like Frank's idea about annotating each of the site locations in Google Earth. Kristen St. John

and I are currently sailing on the JR as members of the Readiness Assessment Team. We have talked a lot with Leslie Peart at Ocean Leadership about ways to enhance E&O within the USIO. One thing that we've suggested is sponsoring REU-like summer research experiences for undergraduates focused on scientific ocean drilling materials and presenting their findings at AGU. This of course would require a substantial increase in the education budget to Deep Earth Academy. Other ideas we have floated: 1) collecting education cores (E-cores), e.g., a single wireline trip to retrieve a mudline core that would be dedicated for educational purposes (we're talking about a hour of ship time that could have far-reaching benefits for K-16 educators). 2) Sailing K-12 educators, and/or scientist-educator on each expedition; many of us involved in basic ocean drilling science also teach at colleges and universities; an interested scientist-educator would be a person who could interface with scientists on an IODP expedition to translate our science for a general audience, just like we have to do in our large lecture classes. This expedition-related activity is also likely to build new student-active learning activities based on authentic data and real-world science questions, to be posted on the Deep Earth Academy website and build additional teaching materials (e.g., School of Rock) that all of us can use across the spectrum of ocean drilling science. I think there is a lot of bang-for-the-buck potential here. As a father of an 18-year-old, and a long-time veteran of teaching large introductory oceanography courses, I'm very concerned about the disconnect between our young people and issues related to global change as well as a basic understanding of how our home planet works. Scientific ocean drilling has much to offer and a genuine opportunity to increase public awareness and understanding about earth system science; we need to seize this opportunity.

Posted by Matt Knuth on 2009-02-26 in 17:27

Being involved in NanTroSEIZE definitely had a big impact on my approach to the field in general. Going to big meetings as a grad student obviously provides a great way to see what other people are doing, and to discuss results with the original authors. But being involved in an IODP cruise gave me a chance to see how other scientists with a wide range of backgrounds actually work, and how different disciplines approach research problems. It definitely gave me a better sense for the level of interaction between groups in different countries and in different academic settings. And that in turn made the large meeting setting a lot more productive for me. I do think there is societal value in how IODP studies problems that extends beyond the focus of individual cruises. It seems like that should show up in how the program is marketed in terms of its broader impacts.

Posted by Sarah Strano on 2009-02-27 at 16:06

This is on a slightly different note, but as a new graduate student in the Ocean Sciences, I'm finding it difficult to find a way to enter into the "IODP world" in order to contribute my ideas. As another Broader Impact, it might be useful to include "the education of future scientists" about the program and ocean drilling in general. From reading through the comments on the forum, it seems that there is a clear need to get new ideas into the program but I don't know what all of the old ideas are. There is so much that has already occurred and it is difficult to understand the system, the terminology and how putting together and conducting a cruise actually happens. The problem is that my fellow students and I will need to be writing proposals and thinking about these things now so that 5-10 years down the line we can be the scientists working on the cruises and build

our careers. We have goals and ideas that we would like to contribute but since most people in these forums have been on cruises, worked with IODP and understand the process, it is difficult for us to know whether our ideas are already in the works, whether they sound stupid, etc. I think it is undeniable that ocean drilling is necessary to understand fundamental questions we have about Earth processes and in order for drilling to continue in the future, us new students need to find ways to enter the program and understand the process. Through classes and personal research, I am learning about aspects of ocean drilling but nothing compares to hands-on experience and with little drilling occurring, we are not getting experience. It is difficult to make contributions to workshops such as this one when I know very little about the inner-workings of IODP. I would suggest that maybe CHART could hold a workshop or short course for new students to facilitate more graduate student involvement in the planning of future ocean drilling programs, in order to effectively catch us up and make involvement a little less intimidating. I recognize that much learning will occur through listening and attending meetings but I do think it would be valuable to consider coming up with ways to usher in our new generation of scientists.

On another note, I really like the idea of making ocean drilling programs accessible to younger students, through whatever means necessary. Videos and live podcasts seem like a great idea. Another one is to send people who have participated into classrooms and let them show pictures and explain to the public why what we are doing is important and to encourage students to want to work in this field in the future. This could work from talking to 3rd graders about oceanography, which is something I have done and found to be very rewarding, to giving public seminars in our local communities. I hope that this comment helps and I really do hope that the new ocean drilling project is something that I and my fellow students can feel that we are involved in.

Posted by Robert Harris on 2009-02-27 at 22:26

I think both Matt and Sarah make important points. I've heard many people talk about how transformative the drilling program has been on their science and science career. This might be said about many facilities, but being on the drill ship is relatively unique in living with other scientists, its cultural environment (both scientifically and internationally), and in learning about disciplines far from ones immediate training. These points should not be lost. I would echo Sarah's comment that the drilling facility is a complex tool within a complex program. The health of the program and the science we do with this facility will depend on getting students to sail and keep them coming back for more. It takes a big investment of time and energy to get up to speed on ocean drilling and is daunting from the outside looking in.

Would a student section of this workshop be helpful? Perhaps it could have direct links to workshop reports, history of the drilling program, etc, and a place specifically designed for students to ask questions about the program.

Posted by Liz Screamton on 2009-03-01 at 09:31

It is helpful to get new perspectives -- so students or outsiders should not be too worried about not being familiar with the IODP structure. It is a good test of a science objective whether it resonates with those new to, or on the edges of, the community.

The Initial Science Plan summarizes the current science objectives: <http://www.iodp.org/isp/>. Of course, our science has evolved since the COMPLEX meeting where the science plan was developed, and new thoughts/goals should be making their way to CHART. So reading through the Science Plan and CHART should help you get up to speed.

Posted by Sarah Strano on 2009-03-01 at 17:57

Thanks for the great responses. I no longer feel quite so nervous about posting on this workshop. I actually did look at the current scientific objectives website before I posted, in order to seem less naive, however I did not have time to read the whole thing and read little bits. I will try to read more of it and get a sense of the program. And I do think that something like Rob suggested would be very useful for me and other younger scientists. I would also like to emphasize that from the little I can tell, it seems that professors might need to very strongly encourage their students, as Rob Harris has done, to post on these sites and write our thoughts and opinions on the site. I will not write my specific scientific goals in this forum but I do have goals and I hope that they can become a reality some day. So I would like to thank both of you for responses and understanding of my concerns.

Posted by Andy Fisher on 2009-03-03 at 14:24

Please also apply to sail! Being on the ship as a student was a profound influence for many active scientists. You do not need to have a formal connection to a proposal or program to be staffed on an expedition, and this is the best way to understand how the program works. You can also attend special sections at AGU and GSA and otherwise hang around when there are meetings nearby, etc. Many of the SAS and USAC and other meetings are "open" if you ask ahead, and this is how you can see the program being planned and discussed.

Posted by Alan Mix on 2009-03-05 at 15:41

I agree with Andy that students should apply to sail. However, past experience is that one of my student's applications to sail never even got a reply (I can't actually verify this, but this is what she told me). An answer of "no" would be better than a non-reply. We should recognize that even submitting an application may be intimidating to students, and their applications should get some special treatment.

Posted by Julia Schneider on 2009-03-06 at 11:21

I totally agree with you all. Being on an IODP expedition as a grad student was an enormous experience for me and opened new doors for my career. It was the best thing I could have ever done during my studies. My feeling is, though, that students either don't really know about IODP and these opportunities or are nervous/intimidated and hesitant to apply. At the university I formerly studied, where there is actually an IODP core repository, I didn't even know many students who sailed on a DSDP/ODP/IODP expedition. We need to get the students better informed and involved. So I can only encourage everyone too to apply to sail.

Posted by Anthony Koppers on 2009-03-06 at 19:55

I agree that scientific drilling legs should be decided on science quality only, that is the only way! However, as soon as an expedition sails we have to make certain that we do everything possible to make the science being carried out relevant to lay audiences. The drilling program is a very large investment and society should be aware of what is being targeted, why it is important to science and/or society, and what the results are! Classrooms should be able to follow expeditions (and the scientists aboard) while they sail via the internet and other media, exciting results should not only appear in Nature or Science, but also on the National Geographic Channel and in NOVA documentaries, for example. That requires a concerted effort by both the IODP program and the scientists, something that hasn't happened too much previously. Luckily, technology is on our site and I expect that HD data and video feeds and high-speed internet connectivity are going to provide us with ample opportunities to make something of the huge potential ocean drilling has for Broader Impacts!

A5.11 Old proposals, new proposals, and a limited schedule

Posted by Organizing Committee on 2009-01-12 at 14:30

How should the program treat the standing crop of proposals and future proposals that may be submitted, if it is unlikely that some of these will ever be scheduled? Should more proposals be rejected outright? Should all proposals be considered "rejected" if they are not drilled within some time after initial submission? This could require periodic updating and/or rejustification of proposals that remain in the system after this time. Should the program stop soliciting new proposals after a sufficient number of excellent proposals are received so as to fill out the anticipated drilling schedule?

Rationale: Given limited resources, time, and access to vessels, even if a renewed IODP were fully funded, there are many more proposals in the system than can possibly be drilled. For example, current drilling capacity is limited to about 7-8 expeditions per year, whereas the SAS may receive 10 or 20 expedition proposals per year. On the other hand, it seems like the program should provide opportunities for new and exciting ideas to result in drilling activity relatively quickly, if there is sufficient support for these efforts. Similarly, if the program endorses/ranks a proposal highly but work is delayed because of budgetary or logistical reasons, should that highly-ranked program have priority over new programs that are also strongly received? In other words, does the program need to follow through on priority-based commitments?

Posted by Mitch Lyle on 2009-02-03 at 08:15

Drilling proposals are not NSF proposals. Throughout the history of the program, proposals have been recycled. In fact, there was a point of pride in ODP that proposals were not rejected, but were allowed to be recombined and reworked. Since most of the information about potential drillsites (except those already drilled) is not very accessible, keeping the proposal in the system preserves important information. Why should we now get suddenly concerned?

Furthermore, drilling proposals take a long time to mature. Look at the current crop to be

drilled--Wilkes Land, Canterbury Basin, Shatsky Rise, and Bering Sea all started in the early-mid 90's. Let's not shoot ourselves in the foot.

As far as priority-based initiatives--priorities change, and given the long lead time for drill proposals (5-15 years), this would only lead to chaos. Who sets the priorities?

Posted by Ellen Thomas on 2009-02-03 at 16:23

I overall agree with Mitch. This does not mean that no proposal should ever be rejected, but good proposals should stay somewhere in the pipeline even if there is little chance for drilling in the immediate future. As Mitch says - it is not very easy to collect all background material of a drilling proposal, and it would be rather stupid to remove this material from the 'common consciousness' by rejecting a proposal only on grounds of 'not probable to be drilled in the near future'. I would like to add the Arctic Drilling leg, which was accepted as high priority before we actually had a set of drilling vessels lined up: having a proposal in the pipeline helps getting the infrastructure together. If we do not have a large number of scientifically exciting proposals around, how then can we argue that the program should be renewed-continued?

I do think that there should be something in the panel structure that would get proponents of old proposals warned if there appears to be a possibility for drilling (e.g. the ship gets to the region), so that they have an opportunity to update a proposal, and that old proposals are not deleted just because they are outdated.

To some extent, debate in the panels will address issues of priority.

Posted by Greg Moore on 2009-02-04 at 15:07

I also agree with Mitch. I don't see how we can encourage people to submit proposals if they know that, sometime down the road, we might say, 'sorry, but we haven't had the resources to drill your excellent proposal, so now we will dump it. But, why not submit a new proposal for us to chew on for 10 years?' If we continuously re-rank all the proposals, we will surely discourage people who have spent years getting their proposals to the mature, drillable stage.

Posted by Marta Torres on 2009-03-01 at 12:16

I could not agree more with Mitch's comments on "prioritization". This was actually recently tried as part of an "implementation plan" for the remaining of the program and received very negative feedback from the community at large. The focusing of science within a few priority areas seems very narrow and not conducive to submission of potential excellent proposals that may be perceived to fall outside these areas. I think a strong new science plan should govern the new program in a same way as ISP has served to guide the current one.

Posted by Andy Fisher on 2009-02-03 at 22:33

I agree with points raised above. But there is a risk in having proposals continue to accumulate in the system, in that potential new participants may be reluctant to take the time to submit a new proposal if it appears that the rest of the program schedule is full. Perhaps this is a perception issue rather than a matter of substance, and IODP just needs

to remind the community that there is always an opportunity for an excellent proposal to be scheduled.

On a related note, is there any sense that the SAS may be reluctant to "reject" a weak proposal for political reasons, but instead just lets it keep going indefinitely with mediocre reviews? Maybe this is not a problem.

Posted by Henry Dick On 2009-02-04 at 10:53

From where I sit, it appears that SPC has forwarded more proposals to be scheduled than drilling time exists in the remaining program. It also appears that drilling time is far more limited than was originally envisaged. This would appear to effectively shut out new proposals, new ideas, and major initiatives that have taken time to mature or to grab the imagination of the current or past SPC members. At this juncture, there should be an overhaul, and we should do things differently. All existing proposals that have been sent up for drilling, that cannot be accommodated in the next two years should be put back into the planning structure designated as mature drillable proposals. They should then compete for scheduling with new proposals coming up through the system with SPC defining the direction of the drill ship for future years either on the basis of where on the planet the ship is headed, or what the thematic priority will be for the coming year. In no case should there be a backlog of proposals to schedule that goes beyond about a year beyond what is presently scheduled - and the ship should be scheduled no more than about 12 months out at the time of any new scheduling meeting. This would return the program back to the way it was run during ODP and DSDP, and would be a better way of ensuring the best possible program serving the broadest community.

Posted by Mitch Lyle on 2009-02-04 at 17:14

Henry illustrates a problem--it is unclear how proposals rise through the system. I thought proposals sent to SPC were the 'mature' pile, and that they did compete with new proposals coming forward in a yearly ranking. Perhaps someone from SPC can explain the current system?

Posted by Gabe Filippelli on 2009-02-05 at 08:52

To answer Mitch's question, the proposals received by SPC (Science Planning Committee) are indeed "mature" in the sense that they have been vetted by SSEP (Science Steering and Evaluation Panel) and other SAS committees. That doesn't mean that they are necessarily ready to drill, as they may not have adequate site survey or safety evaluation. This fact has always troubled me, as SPC is often ranking proposals for which we do not always have a reality check, meaning that a subset of top ranked proposals end up at the Operations Task Force (OTF) for potential implementation, even though they are not always implementable under realistic technology or financial realms.

But regardless of this issue, the SPC does rank both newer mature proposals alongside "older" proposals that either have not been forwarded to the OTF or have resided at OTF for a period of a few years. This ensures that great new ideas can be implemented without waiting in the long queue that is the result of limited drilling and unlimited proposal submission.

Posted by Richard Norris on 2009-02-05 at 00:31

I think the key problem is to continue to have new proposals compete in the system. A large backlog of mature proposals discourages new submissions and having good proposals languish in the system for long periods of time because the ship is not in the right ocean, also discourages new submissions. Yet the program dies if we do not have new people coming into the proposal system, and the whole review system becomes ossified when there is much less drilling than is suggested by proposal pressure. The solution is to drill more, to re-rank current mature proposals, discourage people whose proposals have become seriously dated, and do a better job matching proposal rankings to the available slots for drilling. Frankly, I do not think that the system will work with half-time drilling by the JR. We are going to lose new people and discourage our stalwarts.

Posted by Greg Moore on 2009-02-05 at 20:31

Of course Dick is correct that the key problem is being able to drill only half time -- on ALL three platforms. We made promises in the past as to what we would accomplish and we haven't been able to do what we promised. Will we again make promises based on our hopes for a full drilling program and then be faced with half the drilling we expected? I agree that we are not going to attract very many new people and that we are going to even lose some of the stalwarts. If this happens, the program will die. We need to figure out how to "get back" the other 50% of our drilling time.

Posted by Christina Ravelo on 2009-02-05 at 23:02

I agree with a lot that has been said, particularly regarding the difficulties in trying to run a program without sufficient funds. We have struggled with this issue over the years, and have been further discouraged by delays in the SODV.

One thing that is absolutely clear is that the key to the health of the drilling program is putting together an innovative and compelling science plan to justify funding beyond 2013.

We have some strong advocates at NSF and within the other national funding agencies - and they need a great science plan from the science community. We need to arm them with indisputable scientific justification for an expanded program.

I agree with your comments about the tough spot we are in regarding whether we might have to make promises we can't keep again. I think the smart way forward is to first define the best new science questions that we want to answer through drilling, and then come up with two plans - one that assumes the most optimistic estimates of available drilling time, and one that plans for possibly less. But the truth is, we can't take anything for granted. NSF has told us over and over again that whether we are successful garnering funds for the program will not depend on even our finest accomplishments, but rather it will depend on having a compelling, even gripping, science plan. We really need to put our heads together to come up with a fresh plan of the highest caliber.

In terms of the proposal review system - I think there is something about the structure of the management and advisory structure that makes it difficult to nimbly respond to unanticipated changes in the program. Our proposal review system has served the

program well, but it also has a lot of inertia - if the boundary conditions of the program change, we need to adapt more quickly.

Posted by AndyFisher Date 2009-02-06 12:59

And there is another potential problem. Programs can be forwarded to OTF with very high ranking from SPC but not get drilled because of the ship schedule/track. Then time goes by and new panels forget about the earlier enthusiasm. Should a proposal ranked highly have to run the gauntlet again and again, or should the program make some commitments and then stick to them (pending some reasonable time frame and/or conditions)? This is important if people are going to get site survey funding, instrument funding, etc. One reason for NSF proposals not getting funded is that NSF (and reviewers) sometimes say, "we don't know if it will be drilled, so we can't fund this proposal for related work." My personal view is that IODP needs more forward planning for many kinds of projects, and this requires making a commitment, such as "IODP will drill this proposal sometime in the next X years, provided Y and Z are accomplished." This will open the door to secure additional support, build instruments, etc. It is stunning that IODP seems to be operating on a shorter planning schedule than did ODP! If the program is serious about doing new kinds of science, it has to commit to at least a small number of projects more than one year in advance. In practice, sometimes planning is done with 6-8 months, not nearly enough for many projects.

Posted by Frank Rack on 2009-02-08 at 03:39

Some of the many challenges of scheduling highly ranked proposals coming from SPC and the SSEPs is the global geographic distribution of these targets and the highly variable difficulty (both technical difficulty and cost) of implementing these projects - ranging from APC-XCP paleoceanographic expeditions (relatively easy) to complex borehole completions that require long-lead time equipment purchases and a much higher level of project planning (fairly difficult). It might be wise to group the highly ranked proposals coming from SPC to OTF into geographical portfolios, with optimum weather windows defined, so that we can clearly see the choices that available to create the "string of pearls" that optimize science time and minimize transits. We may have to plan several years in advance to say which ocean basin the vessel(s) will operate in and only consider the proposals in that basin or operating region for a block of projects to schedule. In the past we made long term commitments based on thematic objectives - for example, we will drill 2 microbiology-focused expeditions over the next 5 years; we will drill 3 hydrate expeditions over the next 4 years; etc., - this provided some flexibility in executing the schedule within the scope of the long term plan and allowed for some prediction of operating requirements and acceptable costs. I agree with Andy on his point that we must set priorities and stick to them as well as we can given the short-term funding horizon - we can only see one year ahead in terms of available incremental funding, which makes long-term planning increasingly difficult to sustain.

This process will become even more difficult when we are faced with periods of off-contract/industry operations as we try to solve the fiscal dilemma. We need to have a better system of prioritization that is integrated with the long-term science plan, a focus on meaningful outcomes, and an understanding of available funding. If we can only drill 3-4 expeditions per year we will have to plan differently to be sure to utilize resources

efficiently and understand the trade-offs, both in terms of science and logistics that we are basing planning decisions on.

Posted by Dale Sawyer on 2009-02-08 at 17:17

In the past, the drilling program scheduled on the basis of gradual circumnavigation. I suggest that it is time to think about this concept again.

While I am in favor of the concept of drilling only the highest quality science at any given moment no matter where it is, this sometimes leads the program to accept very long transits and inhibits long-term planning. The notion of circumnavigation allowed investigators and NSF to anticipate that the drillship would come to a particular ocean in N +/- 2 years down the line. It provided a focus for proposing a site survey at the right time to build the strongest possible drilling proposal. It provided the lead time to approach a Foundation or Company for supplementary funding for drilling, while knowing approximately when it could actually happen. It allowed for rational prioritization of specific technological developments. A concept of gradual circumnavigation might also help with attracting additional countries to support the program.

While I am throwing ideas out: Why does it make sense for SPC to decide that a proposal is of the highest scientific quality and send it to OTF when the proper site characterization has not yet been done? I hear people saying that IODP has to decide to do this in order for a proponent to get the funding to do the site characterization. The cart is really in front of the horse here. For many types of targets, particularly tectonic, lithosphere, and complex sedimentary targets, we cannot even know if we stand a chance of achieving the proposed scientific objectives without thoroughly interpreting high quality site characterization data. The oil industry, which drills expensive holes all the time, does extensive exploration work prior to siting, planning, and drilling a well. They do this because it increases their chance of success. I know that they have more money than we do, but that is not the point. Even when they have less money, they know that it is prudent to choose the correct balance of site characterization and drilling.

Posted by Greg Moore on 2009-02-09 at 21:43

I agree with Dale. In the past, getting a favorable review by the SSEP's was taken by the proponents to obtain site survey funding. We should NOT be sending drilling programs to OTF until they are READY to drill. It does little good to have a proposal sitting at OTF for several years while the proponents write proposals for site surveys, carry out the surveys, analyze the data, etc.

Posted by Marta Torres on 2009-03-01 at 12:09

I don't think this is currently the case, proposals are not sent to OTF until all the panels (SSEP, SSP and ESP) have given their blessing. Within the current SSEP all efforts are made to make sure the proponents get rapid feedback on the deficiencies of the proposal. What is more problematic is the proposals that have a technology component that goes beyond traditional IODP tool. SSEP is mandated to evaluate the science, but in some innovative proposals this is tied to a technological development that may not be fully

developed. A new positive development here has been the coordination with the Engineering Development Panel to ascertain that the technology is ready.

Posted by MartaTorres Date on 2009-03-01 at 12:22

My understanding is that the OTF was the mandate to schedule the highest ranked proposals on the basin on the most logical schedule/ship track. But, as Frank point out, this gets increasingly harder when there are only a few expeditions funded per year. Maybe there is some value in reinstating some sort of “regional” ranking at some level to determine, which are the highest ranked proposals in each region and how to best combine them to achieve maximum science. Not real sure how this would work out, just a thought.

Posted by Keir Becker on 2009-02-11 at 09:15

I think Andy's last sentence contains a really important point - that the current financial constraints have pushed us into a very short-term planning process for JR, whereas the goal at the beginning of IODP was to identify programs for scheduling and preparation (potentially including site surveys) much earlier in the process. Short-term scheduling may work out for some kinds of proposals, but the situation is not allowing for effective long-term scheduling of technologically complex programs, e.g., anything involving observatories, major casing strings, deep drilling, or perhaps any riser programs besides NanTroSEIZE.

So, like others I have to wonder if we need both our traditional proposals plus some sort of a complementary planning process for complex, longer-term objectives. Why did NanTroSEIZE succeed in this regard, and is there something in that experience that could guide us toward some sort of dual-track planning process that allows for a regular proposal process plus some way to make early commitments to initiatives that may be especially important to IODP and may involve long technological preparation. I think an answer may be in the early, cross-disciplinary international endorsement that NanTroSEIZE earned at the 1997 CONCORD workshop. That amounted to a virtual invitation for the NTS proposals, and the proponents responded in strong fashion, as did SAS and OTF (with a couple of minor hiccups). In contrast, the frustrating mission proposal process in 2007 did not provide for cross-community evaluation and prioritization except by a very small review committee, but to some in SAS it seemed to pit into direct competition the expertise communities who made the effort to represent three of the ISP initiatives in mission proposals.

Taking the NTS/CONCORD model, I see INVEST as a great opportunity for the full community to go beyond enunciating important science goals but to actually make some implementation commitments and priorities, or at least endorse an implementation sequence among our scientific objectives for the post-renewal phase. I really hope INVEST is structured to try to arrive at some sort of community-wide implementation priorities that go beyond simply allowing each discipline group to summarize its prime objectives, as compelling as they may be on their own.

Posted by Bradford Clement on 2009-02-09 at 13:45

As Christina said, we need to be able to put together a compelling science plan to justify renewal of the program. The flip side of that is that we will need to be able to demonstrate that we have been successfully using the Initial Science Plan to prioritize the programs we have drilled to date. I've heard several complaints that the SAS is not considering the ISP strongly enough in ranking proposals. Evidence to the contrary? Even if we put together an innovative science plan for renewal, we will have to argue that we can implement it successfully.

Posted by Mitch Lyle on 2009-02-10 at 07:32

The fundamental problem with IODP right now is the small number of completed programs. I think it is clear that the drilling we have done is well-represented in the ISP. But, in the first 5 1/2 years of drilling we have only done about 15 expeditions with the combined platforms, on about 10 programs. For the renewal we need to run between twin dangers--an over-promise to the community about the science they will be able to do in the next decade versus not having something that NSF administrators (or us) can see as exciting.

Posted by Roland von Huene on 2009-02-18 at 16:16 New projects can inject exciting new ideas. However will that bring along the experienced scientific community that may have contributed much effort without a pay off. Will science communities of other nations be encouraged to contribute if proposals involving many of them are dumped? Those of us that have sailed know the great value of experienced and mature shipboard scientists in a team. So we need to define the best new AND CONTINUING science questions that we want to answer through drilling.

I think a good goal is solidly formulated science involving mature proposals with complete site surveys blended with exciting new leading edge but untried scientific ideas. Drawing the line between them is going to be an art. The procedure of encouraging disappointed proponents to refresh proposals that are not drilled or not finished by 2013 will require that special evaluating guide lines be formulated. I agree with Bradford Clemment that "Even if we put together an innovative science plan for renewal, we will have to argue that we can implement it successfully." This means sufficient human resources.

Posted by Casey Moore on 2009-02-25 at 13:20

It is critical to encourage injection new science into the program. It would be insane to stop accepting new proposals because the program has enough excellent mature proposals to fill all available slots. Existing proposals need to re-compete. Some bar has to be established for proposal activity, below which proposals are removed from the system. Certainly good problems (the basis of existing proposals) are constantly being re-molded and reinvigorated by new related investigations. I don't think good mature proposals will fall off the wagon.

Elsewhere in this document I've argued for follow through in the SEIZE program. Well, this endeavor has to show that its development justifies continuing investment.

Posted by Marta Torres on 2009-03-01 at 12:11

Brad, I can attest that, at least in SSEP, we closely follow the ISP mandate and guidelines when evaluating ALL proposals.

Posted by Susanne Straub on 2009-02-19 at 11:33

Active drilling is sure the core of the IODP and its future. But with cutting down ship time and having a proposal overload - could it not be an option to encourage more work on the existing samples and data? Working the results of one leg takes many years beyond the end of the moratorium, and comparative synthesis work needs to wait for the results of several legs. Possibly, much work can be done in the area which might help optimize the future and more limited ship time.

Posted by Henry Dick on 2009-02-23 at 14:24

How big is the current crop of proposals sitting at the Operations Committee - does it exceed the number of available legs in the remaining program? Can someone inform us?

Posted by Gabe Filippelli on 2009-02-23 at 15:25

Yes, I can reply. There are currently more proposals sitting with the Operations Task Force than can be drilled by the completion of the current phase of IODP in 2013. This in part is what motivated the Science Planning Committee to begin re-evaluating proposals currently at the Operations Task Force in parallel to the new proposals that come up, in order to ensure that the best science, not just the first science in the queue, gets pursued.

Posted by Henry Dick on 2009-02-24 at 14:47

Thanks, it is excellent that SPC is reviewing the proposals at OPCOM with the new proposals. That effectively turns the program back to considering each years schedule on the basis of all the available good science, which is a better way of doing things. They should be scheduled out by about 1-2 years, no more, and then only schedule one new year each year.

Posted by Craig Fulthorpe on 2009-03-02 at 11:09

I think that IODP faces two challenges. The first is renewal and the second a return to 12-month scientific drilling operations on the JR. These challenges may be linked to some degree, but I doubt that our present strategy can achieve both. A new science plan and some successful drilling operations over the next couple of years may be enough to achieve renewal. We all hope so. However, the strategy for achieving continuous operations must involve some combination of increased funds and reprogramming of existing funds. I'm not aware of any plan in the works to accomplish this. Does anyone know of such a plan? I suggest that CHART, at the very least, includes a strong statement that 12-month scientific drilling by JR is essential to achieving existing and future scientific objectives.

Posted by Steven D'Hondt on 2009-03-06 at 14:28

I second Craig's point.

A6 GENERAL DISCUSSION BOARD

A6.0 Overview

[Note that because of the open-ended nature of the questions and discussion in this board, the full transcript of the posts has been included in this appendix. However, there were a number of posts that were relevant to discussions in **5 STRATEGIES**, and copies of those posts were included in Appendix A5].

What are IODP's successes?

Posted by Organizing Committee on 2009-01-26 at 23:28

Since the start of the program in 2003, IODP has been addressing the Initial Science Plan. What have been IODP's biggest successes? What aspects of IODP have not been working?

A6.1 Examples of key IODP scientific discoveries

Posted by Andy Fisher on 2009-02-23 at 20:48

This is a modification of a post I put up in the Subseafloor Ocean section. A site survey for Exp 301, and follow up modeling, showed for the first time that we can track fluids at a crustal scale, from recharge to discharge across 50 km, and determine how these flows influence and are influenced by related processes. This work helped to show the importance of the 100k seamounts and other basement outcrops dotting the seafloor in channeling massive fluid, heat, and solute fluxes, as much water as flows into the ocean from rivers. The drilling, observatories, and follow up experiments from Exp 301 showed that we can run cross-hole experiments across distances of km in the crust (pump in one hole, monitor in another), and suggested that basement permeability may be anisotropic. These are all "firsts" related to IODP, none of which could have been demonstrated from earlier work. The other thing that is interesting here is that the technology that made all this possible is, essentially, vintage late-ODP. In other words, we have the tools and understanding to extend these results to other areas, find out if they are representative, learn how they vary with age, spreading rate, sediment thickness, basement temp, etc. Unfortunately, these experiments have been hindered by the delay in drilling activity and budget difficulties. NSF has done its part, funded instrument development and support operations, etc. but IODP has not been able to complete the experiments. Before 301, there was a reasonable question about whether the cross-hole experiments could work - this is no longer true. Ironically, now that we know that the complete experiment will work and all design work and prep work is done, there is insufficient IODP funding to pay for the necessary hardware.

Posted by Brandon Dugan on 2009-02-25 at 09:51

IODP Expedition 308 (Gulf of Mexico) Hydrogeology was a scientific success that also included technological/drilling advancements and a strong collaboration with industry. Through direct in situ measurements with downhole tools, Exp. 308 documented overpressures that begin near the sea-floor and provided a solid test of the flow-focusing

hydrogeology model. The properties, measurements, and geotechnical study made during and after the Exp. are providing insights into the coupling of fluid flow and slope instability. This drilling would not have been possible without two fundamental steps: drilling with a pump-and-dump riserless strategy and collaboration with industry. Pump-and-dump drilling enabled the safe drilling of known overpressures and facilitated getting data without casing or a riser. Industry contributed by providing drilling guidance for these shallow overpressures and also by supplying multiple vintages of high resolution seismic data for hazard analysis and for understanding the (hydro)stratigraphic system away from the borehole. These seismic data, the in situ pressure & temperature data, and the routine IODP core/log analysis have helped advance our understanding of basin-scale hydrology and submarine landslides. I hope that we can continue to entice other creative collaborations and use outside-the-IODP-box (but relatively standard) drilling technology in the future to help tackle our scientific questions.

Portion of post Posted by Ken Miller on 2009-02-27 at 15:11

The IODP has completed and posted a summary of its climate successes. <http://www.iodp.org/trc/> I suggest you look this over (I was on the committee, but Hans Christen did a very good job in writing this up).

Posted by Nick Hayman on 2009-02-27 at 18:44

I was involved in both IODP hard-rock drilling efforts, Hole 1309D along the Mid-Atlantic Ridge, and Hole 1256D on 10 m.y.-old East-Pacific-Rise spread crust in the Cocos Plate. Both were operational successes, penetrating >1 km of ocean crust making them 2 of only 4 deep crustal drill holes. Both were successful in testing (both negatively and positively) outstanding hypotheses about crustal accretion and hydrothermal alteration. Both will continue to have an impact as further work continues both on drilling data (cores and logging data), and other related projects in the ocean crust and ophiolites. Perhaps most importantly, both were successful at providing a down-hole data set that bears on very expensive and elaborate geophysical projects; many of us feel that such projects absolutely require corresponding geologic investigation by drilling and submersible investigation.

Posted by James Channell on 2009-02-28 at 14:24

One of the successes of ODP/IODP since the early 1990s has been the capability aboard the JR for recovery of demonstrably complete sedimentary sequences, to several hundred meters below the seafloor, by real-time control of core offsets from advanced piston coring (APC). This capability combined with exploitation, since the mid-1990s, of pelagic sediment drifts as (relatively) high-resolution archives of environmental change, have made drift-legs (such as ODP Legs 162, 172 and 177; and IODP Expedition 303/306) notable successes in the challenge of obtaining well-calibrated paleoceanographic records for the last few million years. The exploitation of sediment drifts in the world's oceans has just begun, and the theme could be one of the vanguards for renewal.

Posted by Joseph Stoner on 2009-03-13 at 16:25

Following on Jim's comment, this has lead to paleomagnetic records extracted from ODP/IODP drilling of sediment drifts and other high to ultra-high resolution continuous

archives that have completed changed the way we think about the geomagnetic field. We now recognized that the geomagnetic field shows a large range of variability during times of constant polarity, that these changes in intensity are globally coherent and that this coherency may be as short as a few millennia. Excursions are now known to exist as extremely short, though relatively common features and the details of polarity transitions are beginning to be resolved. Because of this, we can now begin to ask question of the geomagnetic field that with continued drilling (optimally with 12 month JR schedule) at some point could be answered. These include, among others, clearly separating regional from global scale dynamics field, assessing timescales of variability, determining forcing functions and assessing whether the geomagnetic field, through its influence on the terrestrial influx of cosmic rays, plays a role in the evolution of Earth's surface environment.

Posted by Craig Fulthorpe on 2009-03-02 at 11:39

A great deal has been achieved in the study of global sea level timing, amplitudes and stratigraphic response, especially from New Jersey drilling, but also from Bahamas, Marion Plateau and Tahiti (MSP) operations. Future IODP drilling involving sea-level objectives includes New Jersey inner shelf MSP drilling (May-August 2009), Canterbury Basin, New Zealand (November 2009-January 2010) and Great Barrier Reef MSP drilling (September-December 2009). These expeditions, and existing IODP proposals (e.g., Maldives, Northwest Australian Shelf, Gulf of Mexico - Southern Bank, Belize margin, Gulf of Papua) all address icehouse objectives. Such icehouse drilling is vital, in particular to constrain icehouse eustatic amplitudes, calibrate stratigraphic response ("stratigraphic signature of the Neogene") and assess related sedimentary processes. However, the next phase of sea-level studies must also include the greenhouse world, where eustatic mechanisms remain uncertain. A workshop was held in 2007 to discuss sea-level results and scientific drilling strategies (report at:

http://www.oceanleadership.org/files/Sea_Level_Workshop_Report.pdf). Sea level has been a longstanding ODP/IODP objective, but there remains a great deal to do.

Posted by Bernard Coakley on 2009-03-02 at 15:08

Indisputably ocean drilling has been a great series of scientific successes from DSDP to ODP to IODP. In any science planning those successes should be celebrated and, where appropriate, built upon to develop the next phase(s) of ocean drilling. Past success should not blind us to the potential of other pursuits. If we use the past as a road map for the future, the science plan will only engage the enthusiasts and not find the support it deserves and, honestly, needs to go forward. The next phase will have to bring new regions and problems to the fore to engage the politicians who might support it and the public that will pay for it.

Posted by Nick Hayman on 2009-03-02 at 17:56

I think that's a really good point Bernard. Its one reason I think the ocean crust researchers should consider drilling into a broader range of localities - including younger ocean crust near (or within) active environments.

Posted by Peter Kelemen on 2009-03-03 at 01:20

ODP was fantastically successful at putting a very expensive and highly sophisticated tool into the hands of a very broad spectrum of people in the earth science community. Also, the shipboard operations and publications of ODP were both, in my opinion, superb, incredibly professional, and unmatched by any other organization or program with which I've been involved in more than 30 years of field research. IODP has followed in these traditions, with diminishing success.

From the perspective of understanding the formation and evolution of oceanic crust and upper mantle, ODP drilling yielded many unexpected and stimulating results, and in so doing created natural focus sites around which synergistic field studies - submersible, seismic, BRIDGE drilling, etc - nucleated, often with no instigation by program directors or workshop directives. Without the JR, its scientific parties, and associated site survey cruises, driven hither and thither by Henry Dick, Jim Natland, Mathilde Cannat and their ilk (plus, OK, gravity studies that had little to do with ODP), little of this would have been accomplished, and we might still think that the entire oceanic crust could be understood via a single Mohole. Where RIDGE supplied global syntheses, breathed life into the US ophiolite research community, and enabled lots of geophysical surveys, ODP yielded amazement, rethinking, and new paradigms for slow spreading crust. Both were phenomenally successful in the 90's and the beginning of this century. Living up to this legacy in the past few years has been problematic for both the drilling and ridge communities.

I understand the political situation, but I am particularly disappointed by the "even-handed" allocation of drilling time between Chikyu and the JR. By putting both platforms onto a 7 or 8 month per year basis, IODP is treating these two as though they are equivalent, in cost and in scientific potential. They are not equivalent! If a riser is not needed, a hole should rarely if ever be drilled with Chikyu, it just costs too much. Obviously, if the two platforms were to be funded at equal levels, the JR would be at sea for 15 months per year, and the Chikyu would rarely leave port, so this is not an option. However, I believe it would be best to let riser and non-riser science compete on a level playing field, use the Chikyu only for highly ranked science that requires a riser, and allocate funds for scientific drilling accordingly.

Engineering projects need their own legs. Chief scientists and their scientific party don't want to put down experimental gear and risk losing a hole, or a few days, with everyone sitting on their hands except for a few engineers. IODP management and international partners should bite the bullet and specifically allocate funding for engineering, including dedicated cruises just for this purpose. Also, testing gear in drill holes on the shoreline should be much more common, and so needs funding. These are small budget items compared to the really big funding challenges that the program faces, but they will really improve drilling technology and success on scientific legs.

Posted by Alberto Malinverno on 2009-03-03 at 16:06

Gas hydrates - A significant ODP/IODP success is the considerable progress that has been made in understanding gas hydrates in continental margin sediments. The detailed observations that can be made by scientific drilling are unique and crucial to understand the heterogeneity of gas hydrate deposits and to estimate local and global amounts. Just

think where we would be without the drilling results from Blake Ridge and the Cascadia margin.

Posted by Anthony Koppers on 2009-03-06 at 19:09

As a solid-Earth researcher I have been most impressed with ODP Leg 197 which showed that hotspots/mantle plumes are NOT stationary in the mantle. In fact ODP Leg 197 showed us that the Hawaiian hotspot has been moving ~15° south between 80 and 50 Ma -- deemed impossible for many decades! This is a result that only could be achieved by drilling the volcanic basement at seamounts in our most famous hotspot trail, yet it has revolutionized our thinking about how the Earth's mantle is convecting. Even though the first results were only published in 2003, it almost immediately entered the Earth Science textbooks as an example how state-of-the-art science (that is what ocean drilling is after all) can quickly and drastically change the status-quo in science thinking!

A6.2 IODP weaknesses

Posted by Richard Norris on 2009-02-05 at 00:48

It is clear that we cannot sustain a system in which we spend more than we did under ODP and get half the drilling (or less!). This is particularly the case if we trade off riserless drilling for riser drilling and then drill no riser programs outside Japan (and do not even do those with some regularity). I think IODP has not been particularly good value for funds spent, not just because of the long delays in remobilizing the JR, but because of the reduced activity of riserless drilling compared to the past. The advent of the more regular use of mission-specific platforms has been the one shining success of IODP. Unfortunately, the top-down "mission" concept, the large backlog of undrilled proposals placed in limbo, and the inept duplication of effort in the repositories have been black spots. I mourn for the program.

Fortunately, we can do something about all these things. The change in leadership, the proposed reduction in management organizations, and the possibility that we may be able to fund a more regular drilling schedule are all positive developments. Drilling has been a massive success in the past and there are no end of critical questions in climate, evolution, and oceanography that can hardly be answered any other way than through drilling.

Posted by Mitch Lyle on 2009-02-12 at 12:48

We do have successful drilling, just not a lot of it. The big challenge will be to get through a sufficient number of programs by renewal time to easily justify 10 more years.

The idea that IODP must be managed through one central management structure has proved to be a failure. If anything, the lack of success for any one operator to do drilling seems to lower the bar for the other drilling operators. There is a distressing lack of coordination among the programs, and too much platform for what the respective funding agencies are willing to spend.

Posted by Jamie Austin on 2009-02-16 at 14:35

I take Mitch's point about central management, but when the IODP Planning Subcommittee (IPSC) put the blueprint together for IODP in 1999-2001, we all felt strongly that some form of central manager was critical to the success of a complex multi-platform program. Such an entity would always see the program as a "whole", advocate for it as a fundamental (albeit expensive) scientific activity without favor towards any national facility or infrastructure, and also gently nudge the international scientific community (SAS, users,...) to be accountable for the science proposed and carried out. Without getting into the specifics of what has gone wrong with the program's current management scheme, I for one still believe that such a central entity is crucial to the success of IODP.

Posted by Mitch Lyle on 2009-02-18 at 12:22

This is an important point to discuss--I can see the importance of a central organization as a synthesizer, planner, and advocate, as well as performance evaluator and communicator. What do we need to change to get to this type of management structure?

Posted by James Channell on 2009-02-18 at 15:39

Asking the question: "what's should be the prime organizational priority at IODP?" at two IODP workshops in Germany last fall (amongst groups of IODP advocates mainly from Europe), I got the same reply: "Get the JR on a full-time drilling program".

There is a wide appreciation that Chikyu and Mission-specific platforms are not the "bread and butter" of the IODP community in the US or elsewhere. The program has been severely compromised and the expense of a proven drilling facility (JR) at a time when the science provided by the JR has such societal and scientific relevance.

This view is echoed in the report of the Ad-Hoc Committee of IODP-MI (Dec. 2008) that lists "adequate funding for continuous 12 month operation of the JR" as the number one priority.

Posted by Chris Harrison on 2009-02-20 at 16:24

Assuming that the prime concern is to have the JR run continuously, it seems logical that if an integrated program is to be considered for the next period of time, then there has to be adequate funding for all of the platforms. This probably means that other countries or groups of countries are going to have to contribute a greater percentage of the costs than is currently happening.

Posted by Ken Miller on 2009-02-27 at 15:11

The IODP has completed and posted a summary of its climate successes. <http://www.iodp.org/trc/> I suggest you look this over (I was on the committee, but Hans Christen did a very good job in writing this up).

As to central management, Mitch's post points out a contradiction, "central management failed [implication is that it is not needed]" BUT "There is a distressing lack of coordination among the programs." But who will do the coordination if not IODP-MI? A fear is that the lead agencies would do this. They won't (though some would like them to). IODP-MI is an organization that has been set up to manage the unmanageable. I

suggest it ain't goin' nowhere folks. The best we can hope for is that the regime change in IODP-MI together with the first operation of 3 platforms results in more coordination. Some things at IODP-MI (like OTC) work very well.

I think the discussion needs to go back to the future, doc. What is the role of the SAS? I think it is pretty darn important. As many people participate and gain from participation in the SAS as do sail. I think scientists directing the program (SAS) as opposed to scientists in admin positions is a sine qua non (admins run, scientists direct). So I think of discussions of a dramatic cutback in the SAS should be squashed.

What will the role of missions be in the future? Though they took a hit in the first round, I suspect that missions must be part of the post 2013 program. But we cannot loose the random great idea/proposal.

Finally, I think the theme of the program should be a palineque, DRILL, BABY DRILL. Putting core on deck should be the first priority. We tend to platinum plate our science around these cores. The real legacy of the program are the cores. Getting the JR back to 12 months per year should be the highest priority. If it took closing IODP-MI, releasing the SAS, cutting everything else to the bone, I would advocate that: give me core or give me death.

Posted by Craig Fulthorpe on 2009-02-27 at 17:41

I couldn't agree more with the need to get the JR drilling 12 months per year. Ken isn't the only person to have said it within the forums and it seems vital to the long-term success of IODP. However, I don't really see an intensive focus on this effort, unless things are happening behind the scenes. Is the current part-time model even sustainable in the long run with or without industry support? What will it take to get back to 12-month scientific operations and how can we get there? How can we in the scientific community help?

Posted by Andy Fisher on 2009-03-07 at 13:23

I'll take exception to one aspect of Ken's cogent post: excellent science involves drilling and casing and coring and experiments. It is *not* just about coring. This is a trap the program has fallen into repeatedly, linking success to miles of core recovered. To study a process you have to test the process. Exploring the record can help, but understanding active processes as they occur today requires a different mindset. The ISP for IODP claimed to recognize this, but the record has indicated reluctance to embrace non-coring science. Make no mistake, there is a LOT to be learned from coring, but coring recovery can not be the primary metric for success. Getting excellent science done should be the first priority. Sometimes this will mean collecting no core.

Posted by Ken Miller on 2009-03-07 at 13:28

Andy is right that science involves more than cores. To a certain extent, though, cores remain as a primary legacy to the generation coming behind. But I think we can agree on the mantra, drill baby drill...whether for cores, logs, or corks.

Posted by Anthony Koppers on 2009-03-06 at 19:20

What I think hasn't been working is getting ocean drilling results to the front burner in our society. Not many people on the street and students in our high and middle schools know about ocean drilling and the scientific fruits of our labor, yet everybody knows about missions to the Moon and upcoming missions to Mars! We are exploring the unknowns (and there are many as is evident from the CHART workshop) in the deep of our oceans using an amazing but expensive tool. It is clear that in the remainder of the IODP program and in the next phase of ocean drilling we have to emphasize to society why ocean drilling is important, even if we are using it as a tool to carry out basic science with little direct societal relevance. Going to the Moon has little societal relevance, yet we can get an entire nation excited about the prospect of picking up rocks on the Moon or Mars!

Posted by Andy Fisher on 2009-03-07 at 13:19

That's exactly right. A few years back I was walking through DFW, saw USA Today on display in every shop. Above the fold was this headline: "Hubble Telescope: 10 years, 10 amazing discoveries." The 10 photos that accompanied this headline were banal - grainy images of galaxies, stars, etc. I'm not knocking astronomy or the Hubble Telescope, these discoveries were very important. But somehow NASA managed to "sell" the use of a highly esoteric instrument to study processes that have absolutely zero impact on the lives of 99.99% of the American public. Surely we can do better than we have done with scientific ocean drilling.

Posted by Christina Ravelo on 2009-03-08 at 18:06

I am all for IODP putting more effort and resources into outreach and education. It can only help raise the visibility of the drilling related science, it is our obligation to let the public know how their money is being spent, and it is part of a cycle that mutually grows the program, advances science, and benefits society. But, remember that NASA spends more on education and outreach than we do on our entire drilling program, and that defense aerospace industry (e.g., Lockheed, NASA's biggest contractor) spends 10's of millions of dollars each year lobbying congress. Last year they spent over \$50M lobbying congress. So, while I think we can learn lessons from NASA in terms of 'selling' science that does not impact the lives of the American public, I think we also have to listen to the funding agencies and the lobbyists that advocate for Ocean Science research on the hill on our behalf - and they seem to be telling us quite clearly that societal relevance is more important now than it was in the past. This is not to say that IODP should not be mainly about supporting basic research, it is simply saying that we may have to elevate societal relevance when we package and pitch and 'sell' the program to funding decision-makers and the public in a way that we have not done in the past.

Posted by Susanne Straub on 2009-03-08 at 18:25

I agree on all points - especially given the change on public awareness towards global climate change and the recent political change, it is an excellent moment to emphasize more than ever the links between (basic) science and societal relevance

Posted by Anthony Koppers on 2009-03-09 at 15:56

When I wrote the initial comment of course I realized that NASA is the big player here and has lots of resources (to squander), yet we (that is the program and the scientists)

have to start thinking about outreach in different and innovative ways. For example, the IODP research vessels could have a tele-presence with HD-TV and internet feeds to the shore and through that also to classrooms or any 8 PM news broadcast. IODP could start producing its own 5 min documentaries to be disseminated via YouTube or a special iTunes PodCast Classroom Channel on Ocean Drilling. There are many more ways to reach the larger public these days, most of them using modern internet means, yet they do not require tens of millions of dollars, just a more focused and determined effort ...