International Partnerships in Ice Core Sciences
(IPICS)

The oldest ice core: A 1.5 million year record of climate and greenhouse gases from Antarctica

Science and outline implementation plan
For final approval: May 2008

Executive summary

The international ice core community has, under the auspices of International Partnerships in Ice Core Sciences (IPICS), defined four priority projects for the next decade or more. One of these is to obtain the oldest possible ice core record from Antarctica. An ice core record reaching back to or towards 1.5 Ma ago would be a major step forward in understanding Quaternary climate, and would further our understanding of the relationship between greenhouse gases and climate.

In order to attain the goals, it is essential to carry out a very significant preliminary programme to provide assurance that they can be achieved. In particular, there must be reasonable certainty that at least 1.3 Ma will be achieved, and a series of survey and modelling steps will be required to determine suitable sites. At least two cores will be required in order to provide replication in a regime where flow disturbance is possible. Based on ice thickness, bedrock topography, accumulation rate and basal temperature, the likely search region is a broad area of East Antarctica, with a region south of Dome A provoking particular interest; other areas should also be researched.

After the current planning stage, a broad area survey and a season of shallow drilling are needed in any candidate region. This will provide enough data for a more targeted modelling effort that will provide candidate sites. Multinational consortia would be expected to form in order to tackle each candidate site. Before being confirmed as an "oldest ice" site, each candidate should undergo more detailed local survey and we would also recommend the drilling of a 300 m core to reach the last glacial maximum before heavy equipment is committed. Drilling should not pose any radical new problems, but a new drill fluid will be required, and the likely areas for drilling will be logistically very difficult. A decision will be needed on whether to do significant core processing in the field, or to have a minimal science party in the field and use a dedicated processing centre.

Such a programme will be beyond the capacity of any one nation, and we propose ways of harnessing the efforts at all stages of the project through to analysis and publication, while apportioning credit without excessive bureaucracy. Previous successful ice core projects provide a possible model. However, the details of such a scheme require much further discussion. It seems realistic to design a programme that could deliver oldest ice within about 10 years from initiation. The next step should be for the IPICS SC to agree a framework for such a programme, and to set up a formal "oldest ice" IPICS group to push the implementation of such a plan.

Contents

1. Introduction – the IPICS priority projects

Ice cores have revolutionized our view of the Earth system, and have become a cornerstone of research into climate and biogeochemistry. For example, they provided the first clear evidence that abrupt climate changes have occurred, and they have shown that greenhouse gases and climate have been tightly linked over the last 800,000 years. Ice cores have provided much of our information about how greenhouse gases and other pollutants have increased in recent times, as well as highly resolved information on polar climate variability. Ice core studies have already made a huge contribution to societally-relevant and global-scale issues, such as furthering our understanding of climate change, and by tracking the extent of global pollution.

However, much more still needs to be done, especially to meet the challenge of understanding how the Earth's combined biogeochemical/climate system works, and how it will respond to the change in atmospheric composition currently taking place. Recognising this, all the major ice coring nations have combined to define an agenda for future research.

International Partnerships in Ice Core Sciences (IPICS) is a group of scientists, engineers and logistics experts from the leading laboratories and national operators carrying out ice core science [*Brook and Wolff*, 2006]. It has gained recognition or sponsorship from the IGBP Past Global Changes (PAGES) project, and from the Scientific Committee on Antarctic Research (SCAR) (helping to meet the goals of SCAR's Antarctic Climate Evolution (ACE) Programme). It has also been in discussion with the IUGG's International Association of Cryospheric Sciences (IACS). At the first IPICS meeting, in Washington, DC in 2004, participants identified several high priority international scientific projects to be undertaken over the next decade or more. At the second IPICS meeting, in Brussels, Belgium, in October 2005, these projects were further defined, and routes to implementation were discussed. The 2005 meeting also placed IPICS on a more formal footing. It now has an international steering committee including representatives of 21 nations. It was agreed that the next step is to form planning groups around each of the scientific projects; an additional international group of drillers and engineers has been organized. The third IPICS meeting, held in Vienna in April 2008, endorsed (subject to minor changes) the current document.

The priority projects are:

1. The oldest ice core: A 1.5 million year record of climate and greenhouse gases from Antarctica.

2. The last interglacial and beyond: A northwest Greenland deep ice core drilling project.

3. The IPICS 40,000 year network: a bipolar record of climate forcing and response.

4. The IPICS 2k Array: a network of ice core climate and climate forcing records for the last two millennia

The technical and drilling group are developing plans around the title “Ice core drilling technical challenges”.

Each of these projects has a white paper (available at the IPICS web site at http://www.pages-igbp.org/ipics/index.html) that outlines the scientific requirement and some of the issues that must be solved in order to realise the science goals. This needs to be expanded into a science plan that explains in more detail the scientific rationale and targets behind each project. In some cases, it is also necessary at this stage to define an outline implementation plan that discusses ways of realising the goals and of overcoming the technical and organisational impediments to them. The current document is the science and outline implementation plan for the first project “The oldest ice core: A 1.5 million year record of climate and greenhouse gases from Antarctica”

2. Introduction to the oldest ice project

Ice cores provide information that cannot be obtained from any other paleoclimate archive. They supply unique records of atmospheric composition, including greenhouse gas concentrations; and they provide numerous different environmental parameters representing forcings, amplifiers and responses of the Earth climate system. They are unique in providing all of this information in the same record, allowing us to understand precisely how various elements of the Earth system change with time. If we are to understand the evolution of Earth's climate into its present state, we require ice core data, in combination with indicators of oceanic conditions found in marine sediment and other records, and of land climate and vegetation seen in a range of terrestrial records.

Each time ice cores have extended further back in time, they have revealed new facets of climate dynamics. Early deep Antarctic cores (such as Byrd and the original Dome C core) gave a first indication of how the transition from glacial to interglacial might look. The Vostok ice core in East Antarctica extended the records through a complete climatic cycle and eventually, through 4 climate cycles [*Petit et al.*, 1999] as the core reached an age of 420 ka (1 ka = 1000 years). This core highlighted in particular the close linkage between climate and greenhouse gas concentrations over the last four glacial-interglacial cycles. It was supplemented by the 340-ka record from Dome Fuji [*Watanabe et al.*, 2003] (now extended to some 700 ka), which demonstrates the homogeneity of the basic climate signal over the East Antarctic Plateau. The European Project for Ice Coring in Antarctica (EPICA) core at Dome C has extended the record back to just over 800 ka [*EPICA Community Members*, 2004; *Jouzel et al.*, 2007], and showed that different styles of glacial-interglacial cycle can occur even under superficially similar external forcing.

The Dome C site was selected to have old ice, but not the oldest available ice. Ice is generally believed to have been present continuously in parts of East Antarctica for at least 15 million years [*Zachos et al.*, 2001]. Although basal melting will have removed the very oldest ice in many places, it is reasonable to expect that older ice exists somewhere on the East Antarctic plateau, and that a continuous sequence from the present to ages older than 800 ka BP (BP = before present, present defined as 1950 AD) can be recovered.

2.1. What have we learned from 800,000 years of Antarctic ice core?

The climate of the last 800 ka is dominated by glacial-interglacial cycles with a governing period averaging to about 100 ka (ranging from 80 to 120 ka). This 100 ka period persists throughout the Dome C ice core deuterium record (representing Antarctic temperature) [*EPICA Community Members*, 2004; *Jouzel et al.*, 2007], and is also seen in other parameters measured in ice cores [*Siegenthaler et al.*, 2005; *Wolff et al.*, 2006]. Within each glacial cycle, there is variability at other orbital periods, as well as millennial scale variability, with Antarctic features appearing as counterparts [*EPICA Community Members*, 2006] to the abrupt Dansgaard-Oeschger events seen in Greenland and many other northern hemisphere records.

The covariance of different parameters recording very different parts of the Earth system is extremely striking in the Dome C and other ice core records. For example

(Figure 1), CO_2 mixing ratios [*Siegenthaler et al.*, 2005] show a very strong resemblance to deuterium [*EPICA Community Members*, 2004; *Jouzel et al.*, 2007] (proxy for Antarctic temperature) suggesting that processes in the Southern Ocean, or related to Southern Ocean climate, control atmospheric CO_2 over these timescales. Calcium flux [*Wolff et al.*, 2006] (representing the input of terrestrial dust, mainly from South America) follows the same pattern, with much higher dust fluxes in cold periods.

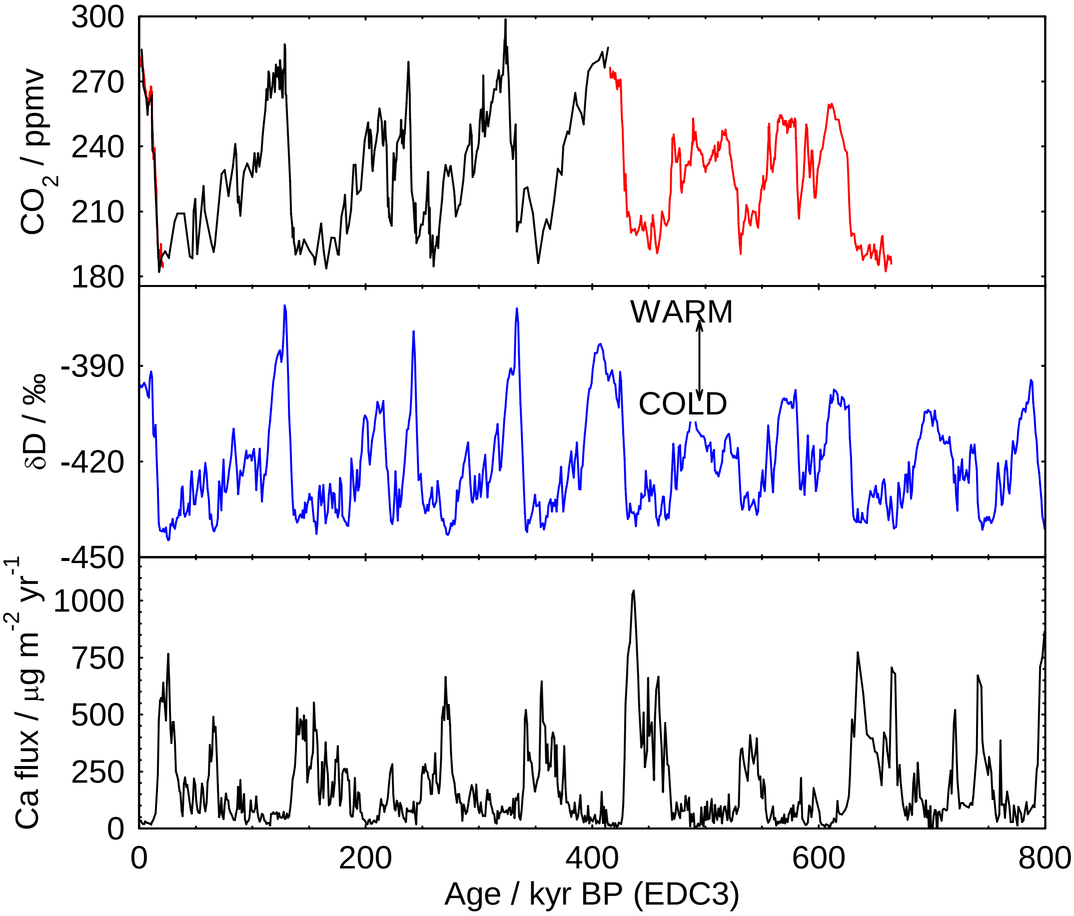


Figure 1: Records of CO_2, deuterium (temperature proxy), and calcium flux (representing terrestrial dust) in the EPICA Dome C ice core (see text for references).

Although interglacial warm periods recurred on average every 100 ka throughout the record, there is a very strong change of style, with a sequence of "weak" interglacials (lower temperatures than the present Holocene period, CO_2 of about 250 ppm before 450 ka BP, and a sequence of "strong" interglacials (warmer maxima than the Holocene, CO_2 280 ppm or more) after 450 ka BP. Although there are subtle differences in the interplay of different components of solar insolation over this time period, it is not yet obvious what factors might have led to these different styles of interglacial. There are also differences in the length and strength of the cold glacial periods, but this is not as obvious in ice core records as in some marine records [Lisiecki and Raymo, 2005]. Study of the new records is starting to give clues about the controls on the sequence of glacial cycles. For example, breakthroughs in providing accurate timescales for ice cores [Kawamura et al., 2007] provide support for the importance of northern summer insolation in triggering warmings, while changes in the amplitude of obliquity (and local Antarctic insolation) might play a role in determining the intensity of interglacials, and particularly the apparent change in amplitude at 450 ka [Jouzel et al., 2007].

2.2. Climate before 800 ka BP

For the time before 800 ka BP, we are mainly reliant on marine sediment cores for information about changing climate. The most commonly-used proxy is the $\delta^{18}O$ of benthic foraminifera in seawater [*Lisiecki and Raymo*, 2005], which combines elements of ice volume and deep ocean temperature. The most obvious feature that deserves explanation is that climate varied on a 40,000 year cycle. This cyclicity was very clearly developed from before 1.5 Ma BP through to 1.2 Ma BP, becoming less clear towards the period known as the Mid-Pleistocene Revolution (MPR), when 100 ka cyclicity became dominant. Another prominent feature of this record is the apparent long term increase in benthic $\delta^{18}O$ from 1.5 million to 800,000 years, presumably reflecting global cooling and/or increase in ice volume.

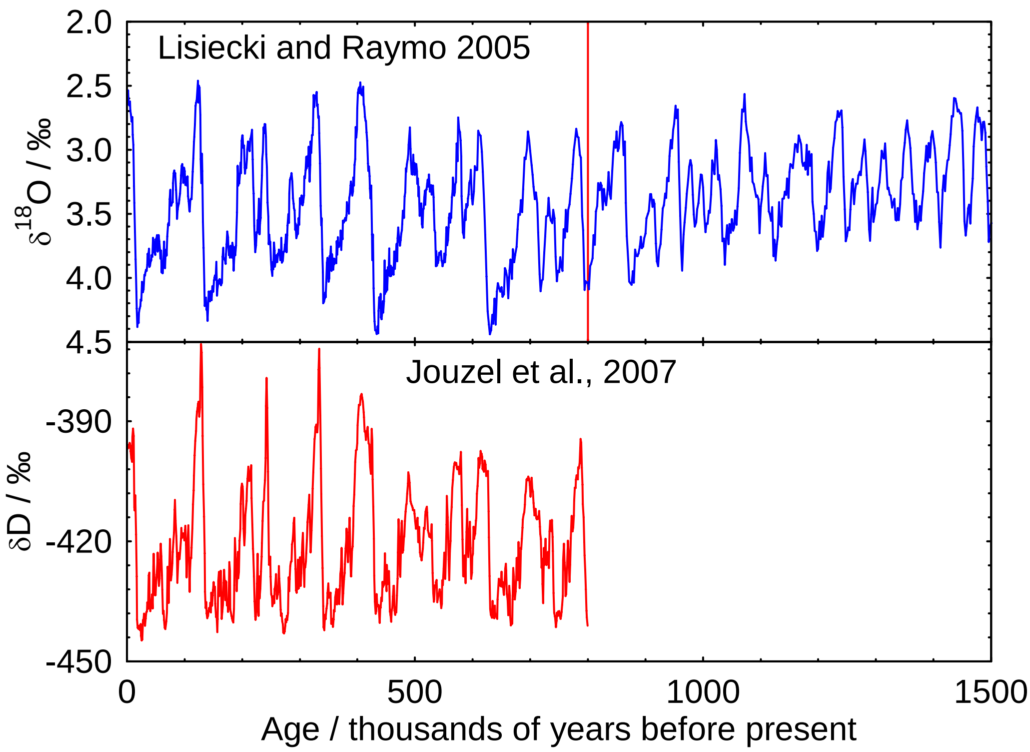


Figure 2. Top: Marine benthic stack showing climate from 1.5 Ma BP to present. The red line shows the age of the Dome C ice core's oldest ice. Bottom: The ice core record so far (from EPICA Dome C).

The ultimate cause of glacial-interglacial cyclicity is generally held to be changes in parameters of Earth's orbit (eccentricity, obliquity and precession), leading to changes in insolation at different latitudes and seasons, with the emphasis often given to the northern high latitudes where ice sheets might nucleate [e.g. *Hays et al.*, 1976]. Both a 40 ka (obliquity) and a 100 ka (eccentricity) period do exist in the orbital cycles, but both components contribute only a minor part of the variability in insolation at high northern latitudes [e.g. *Imbrie et al.*, 1993]. The dominant control on the intensity of summer insolation is precession (with periods close to 20 ka), and therefore neither the 40 ka periodicity of the earlier period, the 100 ka periodicity of the later period, nor the change from one to the other at the MPR are well-understood.

There have recently been some new ideas about the causes of the 40 ka cycles, suggesting for example that integrated summer insolation at high northern latitudes (which is strong at a period of 40 ka) is the relevant parameter [*Huybers*, 2006], or alternatively that both northern ice sheets and southern (Antarctic) ice varied at precessional frequencies, but that the sea level effects (represented by benthic oxygen isotopes) at these frequencies are in antiphase and cancel out, leaving an apparent 40 ka period [*Raymo et al.*, 2006].

Also obscure is the cause of the dominance of 100 ka cycles in the recent past. Conceptual models have been used to discuss this [e.g. *Paillard and Parrenin*, 2004; *Tziperman et al.*, 2006]. However, underlying such models are as-yet unproven assumptions about internal features of the Earth System. Even if these ideas are correct, they do not readily explain the change from 40 ka to 100 ka periodicity at the MPR; although hypotheses exist, they remain speculative without further data.

The increase in benthic $\delta^{18}O$ from 1.5 million to 800,000 years is the end of a long period of cooling and increasing ice volume over the Cenozoic era. This Cenozoic cooling is often attributed to a long term decrease in atmospheric CO_2, which is inferred from a variety of indirect geochemical proxies. These indirect proxies have large uncertainties, however, and generally are available at quite coarse temporal resolution. The role of CO_2 in the cooling from 1.5 million to 800,000 years is thus uncertain, but potentially quite important for understanding the sensitivity of climate to future changes in atmospheric CO_2.

3. The case for extending the ice core record to 1.5 Ma

Although climate models can reproduce many aspects of today's climate, they do this by using known boundary conditions (such as greenhouse gas concentrations and ice sheet extent) as input. They would not currently be capable of attaining the coupled aspects of that climate using only orbital forcing as input. In other words, we cannot yet model the evolution of climate, and therefore we do not currently understand why we have the climate we do today based only on external forcing. At some deeper level, to understand the current climate and therefore its natural evolution, we need to know:

- Why we are in a period where 100 ka cycles dominate
- Why we are currently in an interglacial, and indeed in one that has the style of the Holocene rather than that of an earlier cooler or warmer interglacial.

Thus understanding what controls glacial-interglacial cycles is THE scientific question of the Quaternary period; it would lead us to an understanding of our current climate and its future evolution in the absence of anthropogenic input. But most importantly it would certainly also lead us to an understanding of the interactions between the components of the Earth System under a wide range of conditions that would greatly improve our ability to predict the effects of anthropogenic interference (in particular in the carbon cycle).

To understand those glacial-interglacial cycles, it would be a huge step forward to understand the reasons for the transition from 40 ka to 100 ka periods, and ice cores have the potential to supply many critical parts to this puzzle. In particular, a core that fully reaches the clear 40 ka cycles would:

- Provide a definite answer about the climate of Antarctica in the earlier period that would allow testing of the hypothesis that 40 ka periodicity is actually a result of cancellation of the sea level effects of northern and southern 20 ka cycles [*Raymo et al.*, 2006]
- Test the related hypothesis that Antarctic ice terminated on land in the earlier period, making its mass balance sensitive to local summer insolation, and that the MPT occurred because the ice reached the coast causing ablation to become dominated by calving [*Raymo et al.*, 2006]
- Show whether the quantitative relationship between temperature and CO_2 extends into the 40 ka world, and whether the style and phasing of terminations and inceptions remains the same
- Directly test the hypothesis that a reducing CO_2 concentration was the factor that shifted thresholds (for example for ice sheet growth and loss) and led to a change to a 100 ka world [*Berger et al.*, 1999]
- Provide further examples to test hypotheses about the triggers for deglaciations [e.g. *Kawamura et al*., 2007]
- Show (through indirect measures including CH_4 concentration) whether Dansgaard-Oeschger events occurred under a 40 ka world, either in glacials or interglacials (thus much better defining the conditions under which the bistability underlying such events is allowed)
- Give numerous extra examples of the relationships between different parts of the Earth system in different parts of parameter space, in particular testing the manifestation in different components of glacials that appear in the marine record to be much weaker than recent ones

Taken together, and related to existing marine and terrestrial records, these new data would provide numerous constraints on proposed mechanisms, provide previously unattainable evidence against which to test models that attempt to reproduce past climate, and define much more closely the different possible behaviours of the coupled Earth system under a range of states but in an Earth that has a geography similar to today's.

A new ice core record extending towards 1.5 Ma BP would be a major step forward in understanding Quaternary climate, in unlocking the mystery of the 40 and 100 ka cycles and furthering our understanding of the relationship between greenhouse gases and climate

4. The scientific challenge

In order to obtain the scientific benefits described above, the goal is

- to obtain a reliable ice core record of climate and biogeochemistry extending through several of the 40,000 year cycles and up to the present

which in practice means that we need to obtain

- a replicated Antarctic ice core record extending at least 1.3 million and preferably 1.5 million years, into the past. The youngest set of clear 40 ka cycles occur between 1.3 and 1.2 million years ago, making that interval a natural minimum target.

We recognise that an alternative means to attain the scientific goals would be to find a setting in which deposition ceased 800,000 years ago. In this case the targeted record would be a continuous record between >1.3 and 0.8 million years ago that could clearly be mapped onto existing records, and the complete climate record would be created as a composite of two or more ice cores, much as is done for tree ring records. This alternative approach has the benefit that the time interval of interest will not be as thinned or as near the bed as in the first case. Areas that are currently "blue ice zones" may be one such setting. Horizontal tracing of radar-identified isochrons from existing ice cores is probably the easiest way to identify such sites. Although we support the search for such sites, we nevertheless feel that the most likely strategy for success in obtaining a reliable record of the period is to search for deep drill sites with a continuous record.

Given the logistic effort that will be involved in obtaining such a core (or cores) it is our judgment that the drilling itself can be justified only after we have satisfied ourselves that there is strong probability that the core to be drilled will be not just older than Dome C, but will actually reach clear 40 ka cycles: hence the requirement for at least 1.3 Ma, but preferably 1.5 Ma, and the strong emphasis in the document on several layers of site selection.

The most interesting (oldest) ice will inevitably be rather close to the bed of the ice sheet, where there is always a danger of flow disturbance. Very-low-accumulation sites, where the oldest ice will perforce be found, can be subject to depositional hiatuses (i.e. "blue ice" intervals), adding to the risk of stratigraphic disturbances. While there are ways to test for the most likely disturbances (e.g., trapped gas analyses), they rely on assumptions that might not hold further back in time. The most conclusive way of testing the integrity of deep ice records is to obtain replicates. It was in this way that the existence of flow disturbances at the base of the GRIP and GISP2 cores was clearly shown, and it is only with the (as yet unpublished) data from Dome Fuji that the integrity of the Dome C record through most of its length has been fully confirmed. We therefore strongly recommend that the aim should be to retrieve at least two ice cores, possibly in different parts of the continent. Given inherent uncertainties about the age and condition of the deepest ice that are bound to persist until drilling is complete, this strategy will also increase the likelihood that one of them will successfully reach the goal.

4.1. Steps to meeting the challenge

We can identify a number of steps required to meet the goal set out in the sections above.

1. Identify the science requirements for the core(s): record length, time resolution, core integrity, environmental parameters, constraints on operating conditions, etc.
2. Carry out survey and preparatory work to identify the site(s) for drilling
3. Pinpoint the exact locations of the drill site(s)
4. Assemble international teams capable of providing the logistics, drilling expertise, and intellectual knowledge to obtain the core(s) and the scientific returns from them.
5. Drill good quality core(s), carry out initial analyses in the field, and transport them to home laboratories.
6. Carry out all the analyses that such a major project requires.
7. Provide and archive data on the parameters measured in the core(s), and write papers describing the new knowledge of the Earth System gained from them.
8. Provide for a long term archive of what would be a completely unique set of paleoenvironmental samples.

Item 1 is more or less completed, although no doubt additional goals will be added as the project takes shape, and details will be refined. It is the task of the remaining part of this document to describe the science in more detail, and to discuss how these steps could be implemented in terms of collaboration, funding, logistic and scientific inputs and outputs.

4.2. Criteria for site selection

The criteria for a suitable site are that it should contain the required number of years in a continuous profile, without significant flow disturbance, with well preserved isotopic, chemical and gas records, and be capable of a good dating methodology. In practice this implies sites with a reasonably high ice thickness, a low snow accumulation rate, and a low ice velocity. This narrows the search to relatively poorly-surveyed areas of central East Antarctica. A rather flat bedrock will assist in ensuring an undisturbed record, particularly if the location is away from an ice dome. Lack of basal melting is likely to ensure that older ice is present, although in some cases melting may also reduce the likelihood of flow disturbance near the bed.

The starting point for any search is our existing knowledge of bedrock topography, ice sheet thickness and snow accumulation rate. We present recent compilations of the first two of these properties in Figs. 3 and 4. Recent snow accumulation maps are also available (Arthern et al., 2006), and essentially show low accumulation rates at inland, high-altitude sites. The bedrock map shows areas of substantial bedrock topography that would be best avoided, but the apparently very smooth areas are in fact diagnostic of a lack of data in large regions.

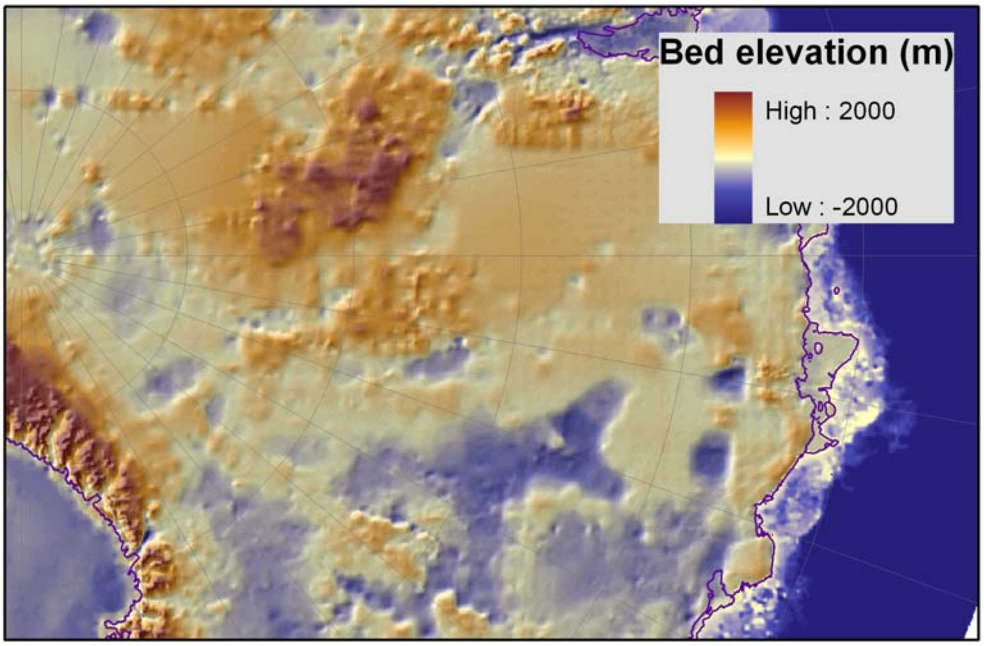


Figure 3. A map of the bedrock under the East Antarctic ice sheet (taken from the BEDMAP compilation (Lythe and Vaughan, 2001)). Apparently smooth regions are mainly areas with no data!

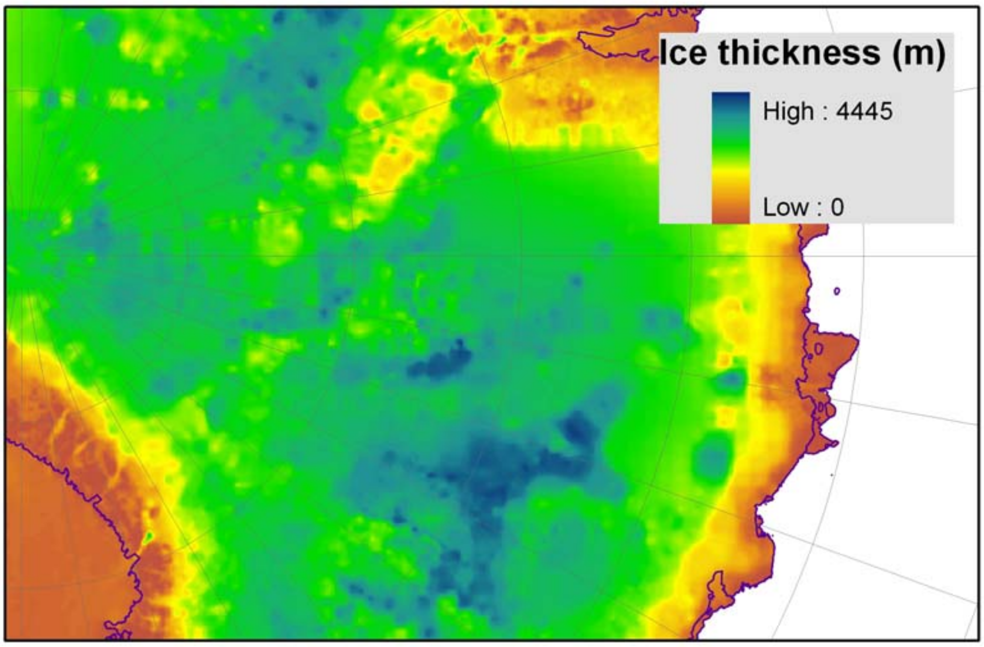


Figure 4. A map of the ice thickness over the East Antarctic plateau (taken from the BEDMAP compilation (Lythe and Vaughan, 2001)).

A current ice sheet model has already been used to estimate where old ice might be found.

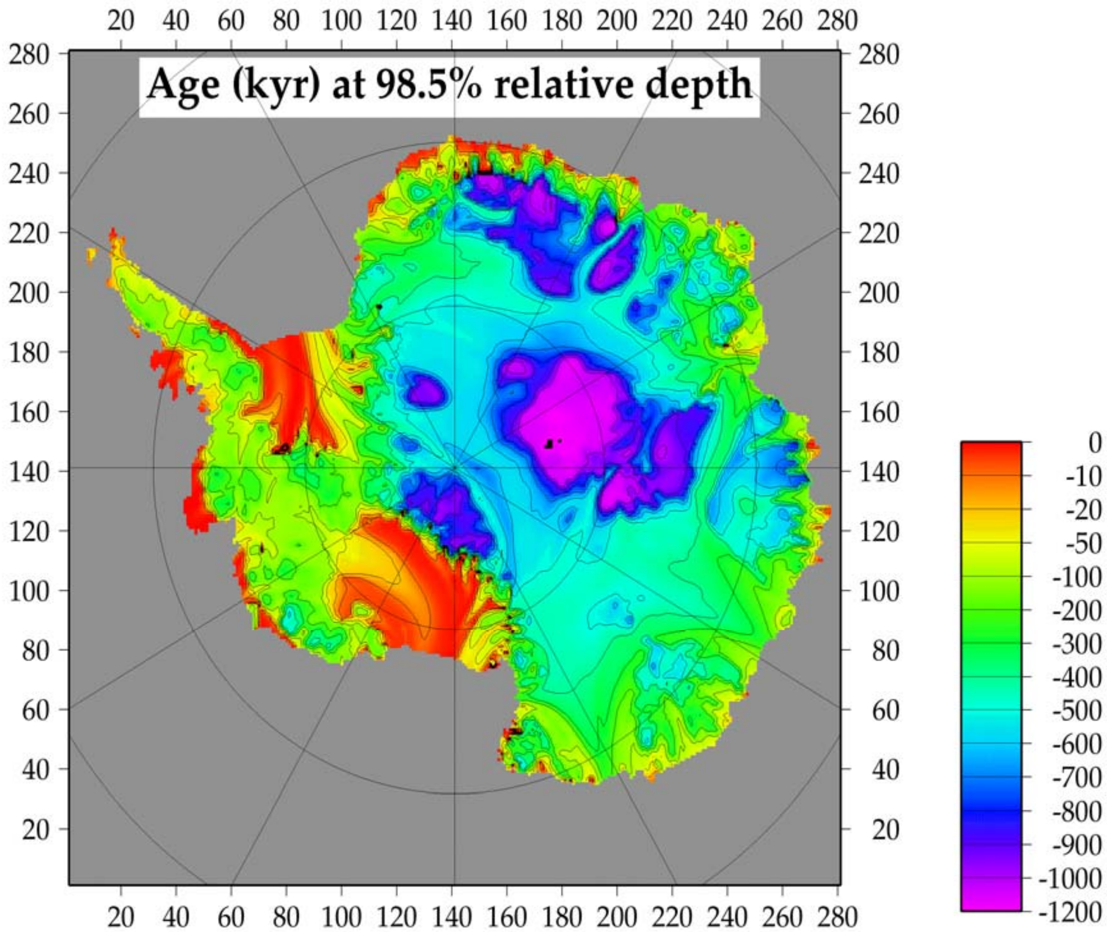


Figure 5. Estimated location of sites with oldest ice, based on knowledge as of 2005, courtesy of Philippe Huybrechts. Contours are age (in ka BP) at 98.5% depth (typically 50 m above the bed).

On this map (Fig. 5), the implication is that suitable sites are likely to be limited to areas in purple; however, this is only a very rough first estimate, because the detailed bedrock and flow information needed to create an accurate model do not yet exist. The smaller areas near the coast consist mainly of areas abutting mountains, where flow is likely to be complex. For this reason the larger area around Dome A might be considered more likely to yield suitable sites, and we are particularly anxious to see this area surveyed. We do not rule out the possibility that suitable sites might be found outside this region, and we encourage further work to assess this. It must be emphasised that the information on both ice thickness and accumulation rate in this area is currently extremely sparse; the search area is likely to narrow considerably when good data become available.

The data that are required to better define the search area, and to refine models, are:

- Ice thickness, elevation and basal topography
- Ice velocity
- Surface accumulation rate
- Temperature, including estimates of basal temperature

- Internal radar layers, that would allow us to follow deep layers of known age from other sites, and to assess the likelihood of flow disturbance near the bed

While some of the data are already available from satellite remote sensing data sets, the critical data on bedrock topography/ice thickness, and on radar internal layers can only be obtained by a significant survey effort using a high quality radar, capable of reaching the bed and of resolving internal layering. An aircraft survey seems essential to cover the area required, although ground traverses will offer a very useful addition of detailed information. The survey should cover the entire search area, and should certainly include linking lines to Dome C, Vostok and Dome Fuji. The survey spacing can be relatively coarse at first pass, allowing us to narrow to much smaller areas for a more detailed survey at a later time. The data from such a survey will supply data for the first two and last bullet above (for example ice velocity can be derived from the data).

A small network of shallow ice cores will be required to determine the accumulation rate, although it is possible that radar internal layers may be used to interpolate between relatively widely spaced cores. 10 m ice temperatures measured at the same locations will provide the surface temperatures needed for further modelling. Cores going back through a small number of known volcanic eruptions will give the most certain estimates of accumulation rate. For this purpose, even 10 metre cores will likely reach the Tambora (1815) eruption, while to reach the characteristic 1259 series of volcanic peaks, cores of 30 m depth would be sufficient. Use of downhole instruments might provide additional information or allow more sites to be surveyed without the need to transport unfeasible lengths of core.

In summary, the requirements for narrowing down the site selection are:

1. A (one-season) radar survey at a minimum spacing of 50 km across the whole region to determine ice thickness and to obtain a map of the geometry of internal layers
2. A series of 30 m cores (including temperature measurements) from which volcanic horizons can be located (perhaps by in-situ or laboratory ECM or DEP). Relatively few (9?) cores will be needed to provide the input data for models.

4.3. Narrowing down the site selection

Once the data described above have been obtained, it should be possible to do a far more accurate model study to determine likely locations for deep drilling. If necessary, models adapted to the local area will have to be created and used to be sure that we are doing the best possible job at this stage. The models should be used to estimate and interpolate in order to provide for any given location:

- Basal temperature and temperature profile through the ice sheet
- Estimated age-depth profile (assuming a climate history scaled to that of Dome C, and beyond 800 ka BP, the marine record)
- Estimate of ice flow and upstream influences

With this information, and at this stage it should be possible to pinpoint one or more candidate sites; however if necessary further iterations of survey and modelling should be completed before proceeding to a narrower and more detailed survey of candidate sites.

Because the cost of drilling at such sites is likely to be very high, a further round of site selection and confirmation studies should be carried out at each candidate site before deep drilling is commenced.

- Firstly a much more detailed radar survey should be completed to ensure that the local bedrock holds no unfavourable features
- Present-day measurements of meteorology and atmospheric chemistry should be commenced to improve the prospects of interpretation of the core
- A core back to at least the last glacial maximum (LGM) should be drilled to ensure that the predictions of the age-depth and temperature profiles are at least adhered to up until that depth; since the LGM should be found at a depth of order 300 m, this work can be carried out without committing any of the expensive logistics implied by a deep drilling operation
- If the technology at the time allows it, a rapid access hole could be taken to a much greater depth, either to confirm the basal temperature, or to obtain a limited section of ice from deep down to confirm that the predicted timescale is correct. However, it will have to be assessed at the time whether the cost of doing this additional step outweighs the risk of drilling a core that does not attain our goal.

4.4. Drilling and field analysis of the core

Once the site(s) have been fully determined, it will be necessary to set up a deep drilling operation at what will certainly be a remote location. However, while the logistic requirements of establishing and maintaining a camp will be challenging (and discussed later in the implementation section), the actual technical challenges of obtaining the core probably throw up few new challenges.

The ice depth is unlikely to be greater that those achieved already in several projects (such as EPICA Dome C and DML, Vostok, Dome Fuji, and indeed the inland WAIS site currently being started). The ice temperature near the surface may be a few degrees colder than encountered so far, but should not pose any qualitatively new difficulties; for most of the ice depth, the drill will be operating in a familiar temperature range. Warm basal ice has already been encountered in a number of drillings; in the best case, it will not be encountered in this one. In other words, several drill designs capable of achieving bedrock at the sites we expect to identify already exist. [For example, the recently completed US DISC drill was built to a -60˚C specification in anticipation of the "Oldest Ice" project]

One difficulty to be overcome is to find a suitable drilling fluid. The densifier used at EPICA and WAIS Divide will not, as far as we are aware, be legally obtainable by the time of drilling, while the new fluid planned for the Greenland NEEM drilling will not be suitable at the temperatures we expect. A new drilling fluid should therefore be researched and identified during the site selection phase of this project, and this is an important task for the drilling group. Given that the most valuable ice will be thinned to a relatively narrow depth range, the drilling group should also further consider whether replicate drilling of the deeper sections has become a viable option by the time of drilling.

Decisions will have to be made about how much science to do in the field. Projects such as GRIP, GISP2, NorthGRIP and Dome C have included a significant amount of in-situ field science. However, this greatly adds to the logistic burden, and the route of taking most of the core to home laboratories and processing it in dedicated sites such as NICL (Denver) or AWI (Bremerhaven) has also been used successfully. It is our suspicion that the most practical route will be to have a minimal science team in the field – perhaps one person to log the core and monitor its quality, one to carry out initial electrical measurements on uncut core, and one who might cut a section off each core to preserve as a safety-net archive left in storage at the drilling site. If this is the case, then one or more processing laboratories will have to be identified; if there are two cores they could be processed in different laboratories, but with comparable procedures. High-quality measurements (e.g. temperature) in the borehole will also be needed after drilling is completed.

4.5. Analyses of the cores

With such a valuable resource, all possible analyses should be carried out on the core. However, just to identify a minimal set that would be essential on any core, we list the obvious analyses and the reason for them, below.

Analysis	**Sample reason (there are others in each case!)**	**Minimum resolution**
Water isotopes (^{18}O, D, ^{17}O)	Basic climate record, plus additional information	Few cm
CO_2	Major climate parameter; Possible role in MPR	Metres (less near bed)
CH_4	Proxy for DO events	Metres (less near bed)
ECM, DEP	Initial survey of ice	cm or less
Crystal size, fabric	Ice properties for modelling	Irregular
Mechanical properties	Ice properties for modelling	Irregular
Other trace gases	Atmospheric chemistry	Metres (less near bed)
Ice inorganic chemistry	Terrestrial and marine environment	Few decimetres; few cm in places
^{10}Be	Dating (esp magnetic reversals)	Metres; decimetres in places
O_2/N_2, total air content	Dating	Millennial
^{18}O and ^{15}N in air	Climate, biogeochemistry and chronology	Metre
Isotopes of gases	Understanding biogeochemical cycling	Few metres
New chemistry (isotopes, organics, heavy noble gases...)	Additional environmental information	Irregular

A more refined set of analytes can be decided for each core, to include sample numbers, methods and resolution, once the sites have been chosen and the science teams have formed.

4.6. Data and publication

For such an important resource, it will be essential that all parties involved agree upon a protocol in which they deposit data at the earliest possible date in international databases (e.g. WDC in Boulder, Pangaea), and that there should be pre-publication data exchange between groups within the project. We suggest that data should be deposited publicly on publication, or 12 months after a dataset is completed, whichever is earliest. Papers should be written and published freely, collaborating wherever possible with experts outside the ice core arena. We discuss possible publication protocols later, in the implementation section.

4.7. Education, outreach and policy

Ice core projects have traditionally been easy to publicise, and attractive targets for educators, because they comprise exciting locations, tricky logistical and technical challenges, and outstanding paradigm-changing science. Previous ice core projects have been very successful in achieving international news coverage, and their findings have quickly found their way into authoritative assessment reports and educational literature. The project to retrieve the oldest possible ice will certainly be iconic, and can be used as a flagship project for international collaboration, which has an obvious technical goal as well as an important science story to tell.

The fact that ice this old will never have been examined before gives the project an exploration aspect, and raises the possibility of major surprises being found (e.g., traces of a nearby supernova, asteroid impact, etc.) that lend themselves to wide public interest.

5. Implementation

Some general aspects of implementing the above plan are already included above; on the other hand it would not be appropriate at this stage to reach the level of defining which nations should do which tasks, except in a few exceptional cases. However, we can discuss some of the principles that will allow such a project to be carried out.

Firstly, we regard the project as consisting of an ensemble of all the stages listed above from conception through survey and site selection, to camp construction, drilling, analysis and publication, education and outreach. Each of these activities requires effort and resource, and each of them contributes to the success of the whole "Oldest ice" project. We take it as axiomatic that no single country can carry out all these tasks, and therefore an international collaboration will be required. However, it is also helpful to break out each task and to discuss what it involves. As a final step we will discuss possible models for apportioning input and output to the overall project.

5.1. Conception and planning

This document already forms part of the planning of the project. IPICS has undertaken this role, representing all its members. A sub-group has already been formed to write this plan, and the entire IPICS SC will be asked to endorse it. At some stage an SC specific to this project will be appropriate to ensure that each of the different activities involved in the project is being carried out according to the plan. The composition should include those IPICS nations that are contributing significantly to one or more of the activities. However, its exact composition and powers can only be determined when the method of collaboration has been settled (see later).

5.2. Large-scale radar survey

One of the major components of the pre-site survey is clearly a major airborne geophysical survey, with radio-echo sounding as its most important element. Ground-based survey, although insufficient by itself, would be a valuable adjunct. In particular, the traverses planned under the IPY TASTE-IDEA project (including for example the Italian/French/Russian traverse from Talos Dome, via Dome C, Vostok and Dome B to Dome A, as well as US-Norwegian and Japanese-Swedish traverses started in 2007/08) will provide a significant dataset for this project.

The magnitude of the effort required for the airborne survey clearly depends on the spacing of flight lines. Purely for illustrative purposes, we estimate that a survey over the main candidate area (coloured purple, around Dome A, on Figure 3) at 50 km spacing, would require around 14000 km of flight lines (to include linking lines to Dome Fuji, Vostok, South Pole and Dome C). Such a survey is certainly possible, in a single season, with geophysically-equipped aircraft such as the Twin Otters of BAS (UK) and USAP (USA), or the planned German BGR Basler, provided suitable fuel depots can be established.

At the time of writing, we are aware of plans for such a field season (tentatively in 2008/09) to include survey work for both the oldest ice project and the Gamburtsev mountains project (under the umbrella of AGAP (Antarctica's Gamburtsev Province)). The oldest ice project would only be interested in the southern half of the planned flight lines; the northern half is likely to be too mountainous to be suitable. Again, purely for illustrative purposes, the flight lines that have been discussed by AGAP are shown in Figure 4. Such a survey would certainly fulfil the needs of our IPICS oldest ice survey in the region covered. Similar surveys (such as the proposed ICECAP survey further east) would fulfil a similar role for those areas.

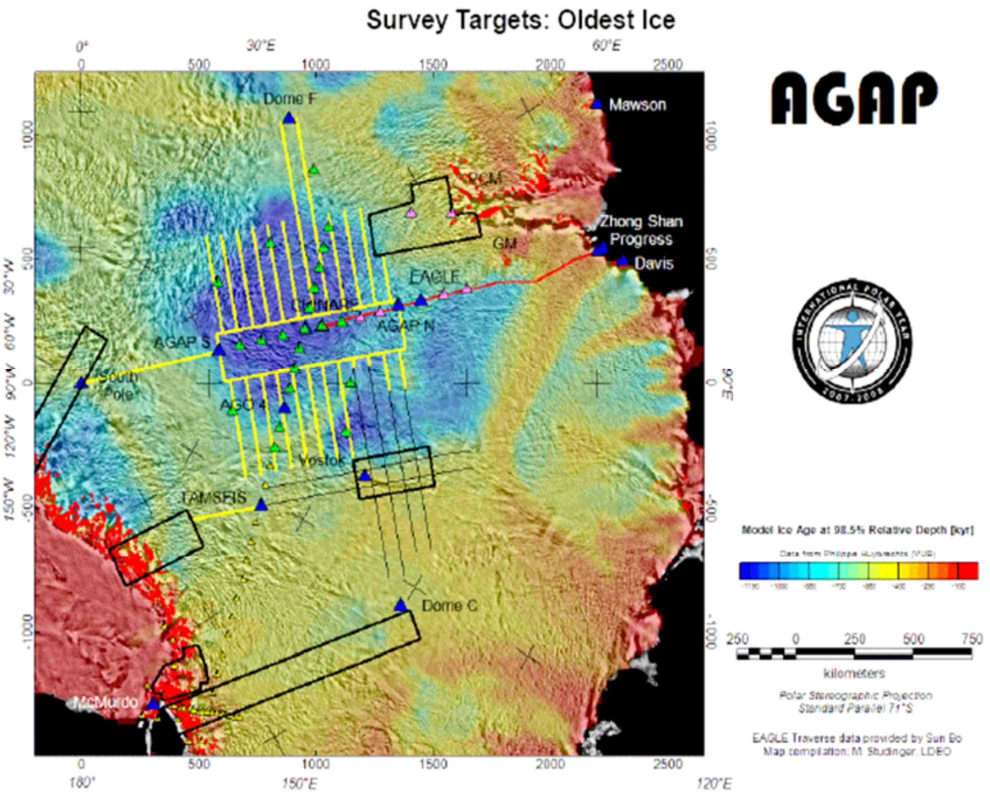


Figure 6. Possible flight lines for the AGAP survey, superimposed on the Huybrechts map of where oldest ice might be found.

While we are anxious to see the "purple area" covered by a good survey, we emphasise again that, in order to find two replicate sites, we encourage exploration of other candidate regions as contributions to IPICS. For example, under its latest plans, part of the ICECAP proposal would fly out of Casey station and target the Aurora Subglacial Basin, an area with particularly thick ice between Dome C and Law Dome.

5.3. Network of 30 m cores

The main aim of drilling a network of cores in the same search area would be to establish some ground truth for surface accumulation rates and surface temperatures in the search region. A 3x3 network of 9 cores would be sufficient: modelling could be used to interpolate between these fixed points in the next stage of site selection. Drilling to 10 m depth would require about 2 hours on site; however if we want to have a reliable 10 m temperature it would make sense to spend one day at each site, which would allow a more useful 30 m core. The core could be analysed by ECM and/or DEP on site, and core edge samples could be collected for ^{18}O analysis. Obviously the option exists to bring back more core and complete further analyses, but these are not essential for the pre-site survey.

We believe it might be impractical to carry out such a project by air, since it is unlikely that pilots would be happy for their aircraft to stay on site at these cold sites for the time required for each core. This therefore implies that a tractor traverse is required. With 9 sites taking one day each, and a total traverse length of order 3000 km, a season is required for a small team to complete this work. Again some depoting of fuel is likely to be required in advance.

One site contributing to this network of cores has already been drilled: we are aware that a team at Dome A recently recovered a shallow core through the firn (Xu et al., 2007). They find a modern accumulation rate of 0.023 m water equiv./yr, similar to other central Antarctic sites, but estimate a significantly lower mean Holocene accumulation rate in the region.

5.4. Narrowing the site selection through modelling

With the additional information from the radar and shallow core surveys, it should be possible to improve considerably the predictions of where oldest ice might be found. Ideally an ice sheet model would be adapted specially for this purpose, although existing models would also allow the site search to be refined. The outcome of modelling work at this stage should be the identification of two or more "spots" where oldest ice criteria are most likely to be realised. For each spot there should be a prediction of the age-depth and temperature profiles, as well as any information about upstream flow (assuming the site is not on the dome).

5.5. Confirming the sites through further survey work and intermediate drilling

At this point in the process, we would have "candidate" drill sites, and the next stage is to carry out "due diligence" tests to confirm that the chosen sites are likely to yield what we hope for. This would most likely be the point at which it would make sense for consortia of IPICS nations to form with the intention of testing and (if positive) drilling each chosen site. Our thinking is that different groups might form around each site, each with their own mix of capabilities in drilling and analysis, and each with their own logistic plans for achieving their own drilling.

However the next stage is to be achieved, it would clearly be necessary, as with any serious drilling, to carry out a more detailed local radar and accumulation/temperature survey to confirm the characteristics of the site. As outlined in the science plan, we also believe that a 300 m core should be drilled, reaching the LGM, to confirm the characteristics of the site (both age-depth and temperature profile). At each candidate site, a single season should be sufficient to carry out a radar survey and shallow coring within a few tens of km of the candidate site, as well as a 300 m core (requiring only a small head of lubricating drill fluid).

We have also mooted the possibility of testing deeper ice using a rapid access hole. We do not feel confident at this stage that this is feasible. However, if it becomes feasible to do this in a further single season, then it could be a worthwhile investment before committing the heavy logistics needed for the deep drilling.

The data from the detailed survey and LGM core must of course be processed and assessed before any serious investment in infrastructure at the drill sites is undertaken. We envisage that the IPICS oldest ice steering committee would only give a site the official status of "IPICS oldest ice confirmed site" when all these stages have been successfully completed.

5.6. The drill camps

For each (maximum 2) candidate site, if the detailed survey confirms its suitability and it becomes a "confirmed site", a drill camp must be established. Assuming that the field science is minimal, each camp would likely have to accommodate of order 9 drillers, 4 scientists, and logistic personnel (at minimum generator engineer, cook, communications expert, vehicle mechanic, and support personnel). Thus a camp of minimum 20 people is needed, although different consortia may choose to build a more ambitious infrastructure. The main point is that we are looking at temporary, summer-only, camps suitable for a minimum staff.

Such camps are likely to require considerable planning, and some kind of environmental impact assessment. At minimum a central building with generators, kitchen and dining area, a drilling area with workshop facilities, and a small science area, as well as sleeping and hygiene areas, would be needed. This is likely to require at least one season to construct. Because of the remote area, it will not be trivial to supply the building materials, fuel etc., and a considerable logistic effort will be needed to establish each site.

5.7. Drilling the cores

Each consortium is likely to choose its own drill design, probably based on one of those that has already been successful. A pilot hole with casing will be required, and might be drilled in the camp building season. Drilling fluid will certainly be required (major logistic input). Based on past experience, we can envisage that, barring problems, the drilling will require (after the pilot hole is complete) 3 seasons of 10 weeks each to reach bedrock. A first follow-up season of borehole measurements can be combined with dismantling the camp.

5.8. Processing and analysing the cores

The science plan assumes that 75% of every core will be transported to a processing centre. It is therefore assumed that each consortium will include access to such a facility, although of course it would be possible to negotiate that both (if two) cores use the same facilities. Analysis of all the components required in the science plan will require several years' work. Again we assume that each consortium will organise all its own analyses; however at this stage an overarching IPICS oldest ice SC should again play a role in ensuring collaboration and intercomparison between the cores, and common science meetings to discuss the interpretation of the data. A publication policy decided in advance will be essential, and forms part of the discussion in the next section.

Table 1. An ambitious version of the timetable for the oldest ice project.

Period	**Activity**
2007	Complete science and outline implementation plan
Spring 2008	IPICS SC to endorse plan
2008-09 season	Airborne survey of target area
2009-10 season	Shallow ice core traverse in target area
2010	Identify candidate sites
2010-11 season	Detailed survey and 300 m core at candidate sites
2011	IPICS oldest ice SC to approve confirmed sites
2012-13 season	Build camp structure and perhaps drill pilot hole
2013-16 seasons	Drill core
2013-	Analyse and publish
2017	Flagship publication of up to 1.5 Ma records

5.9. Education and outreach

Each component of the project will no doubt have its own efforts in this area. However, it will benefit everyone to include an overall IPICS oldest ice branding in their materials. The project is bound to excite major interest and if possible, media participation (whether direct or through new communication tools) should be included in each part of the project. It would also be valuable to keep a historical record of the development of the project, with the goal of producing a book or other output about how such an ambitious and international task was achieved.

6. Collaboration, inputs and outputs

Deep ice coring projects already have a strong history of collaboration, starting with pioneering projects in Greenland and Antarctica in the 1960's through 1990's, culminating for example in the EPICA project involving 10 European nations, and the NGRIP project, involving collaboration across 3 continents. The IPICS oldest ice project has some additional complexity because it likely involves even more nations, and an even more difficult location logistically than previous projects. However, at heart it is of similar scope to EPICA, or (GISP2 + GRIP), involving two cores and pre-site survey work.

A number of models exist for collaborations of this kind in the geosciences. Consortia such as IODP and ANDRILL have consisted of quite formal agreements, while EPICA and NGRIP have been much less formal. In the case of IODP, the whole consortium rests on the use of a single piece of equipment (the drilling ship). This is different to the ice coring case, where the drill itself is not the main issue: designs are very valuable but are already quite freely shared, and further copies are not prohibitively expensive. The main cost in this case is the logistic access to the drilling sites, including the survey work, and analysis of the cores, which may be twice as long (in time) as existing ice cores.

Because they have been very successful, and involved around half the IPICS nations, our first suggestion would be to adapt the EPICA/NGRIP models to the new situation. We realise this may lead to an unfamiliar mode of collaboration that may conflict with existing practice in some nations, particularly in terms of how research funds are committed to projects, and compromises to accommodate this may have to be discussed as the model develops.

The philosophy behind any successful collaboration is obviously that each partner should contribute what they are best equipped to do (be that cash, logistics, equipment, experience, or intellect); and at some level the rewards should be shared in a similar proportion to the input. In the case of a science project the reward comes ultimately in the form of names on papers, and the associated recognition. However, with limited exceptions, it is not reasonable to determine the authorship of papers in advance of the intellectual input to them so reward is more appropriately represented at an intermediate stage by access to samples, or by granting leadership of a particular area of science.

The level of entity at which contribution and reward should be judged is probably the national one: that is, a logistic contribution from a national agency buys "credit" for the scientists of that nation, irrespective of their institutional affiliation: it is for the nation to determine any internal bartering. Nations (e.g. within Europe) might choose to pool their contributions and credits if this suits them.

In order to avoid complications, we propose that the components leading up to the drillings should be treated for most purposes as independent projects: that is, they will justify themselves to funding agencies as contributing to IPICS oldest ice, but they will publish as independent tasks. However they will also earn "credit" for the "entities" contributing to them to take part in the deep drillings themselves.

For the drillings themselves, again each drilling can be considered as a largely independent exercise: each will have its own planning office, and each will have its own science plan that allocates analyses to different science groups; most likely consortia will be formed around various topics as in previous projects. However, since it is access to core samples and allocation of analytical or intellectual leadership that we are treating as "reward" in the project, notional "credits" whether from the other drilling or from site selection activities, should be transferable.

It is not easy (and probably not desirable) to assess credits in absolute financial terms for a number of reasons:

- an activity such as airborne survey can be costed; however, the benefit is not only to the oldest ice project (for example the proposed AGAP surveys will provide data for many other purposes than finding oldest ice) and therefore the full value should not be credited
- a number of components can be provided equally well in different ways, and it would be inappropriate to credit more to someone who has done it the expensive way
- the cost of providing services is different in different countries and national systems, but the benefit to the project is the same.

For these reasons we suggest that a simple qualitative or semi-quantitative system of credit should be used, in which each part of the overall project is assessed as having a notional value, and the different groups contributing to that can share the value. And when allocating "benefits" (which has to be done when participation in analytical consortia, and leadership of science topic writing, is agreed), the contributions should only roughly be borne in mind, and not used rigidly.

To illustrate this (but the details are for later discussion), we give an example of how the notional credit might work. We might choose for example to consider that, for the overall project:

- Conception and planning is worth 2%
- Airborne survey 10%
- Ground survey 5%
- Modelling and site selection 3%
- Each drilling then has 40% to use, which might be allocated through a conventional costing, or through a similar semi-quantitative basis.

The point here is only that the countries that contributed to the airborne survey (for example) are already considered to have made a modest contribution (not exaggerated because they have already obtained some publication benefit from the survey itself) to the drilling, and therefore to have some rights to the benefits of drilling. We emphasise again that the numbers above are supposed only to give an example of the method; it will be for negotiation involving national agencies as well as scientists to determine the actual rules once real activities become clearer.

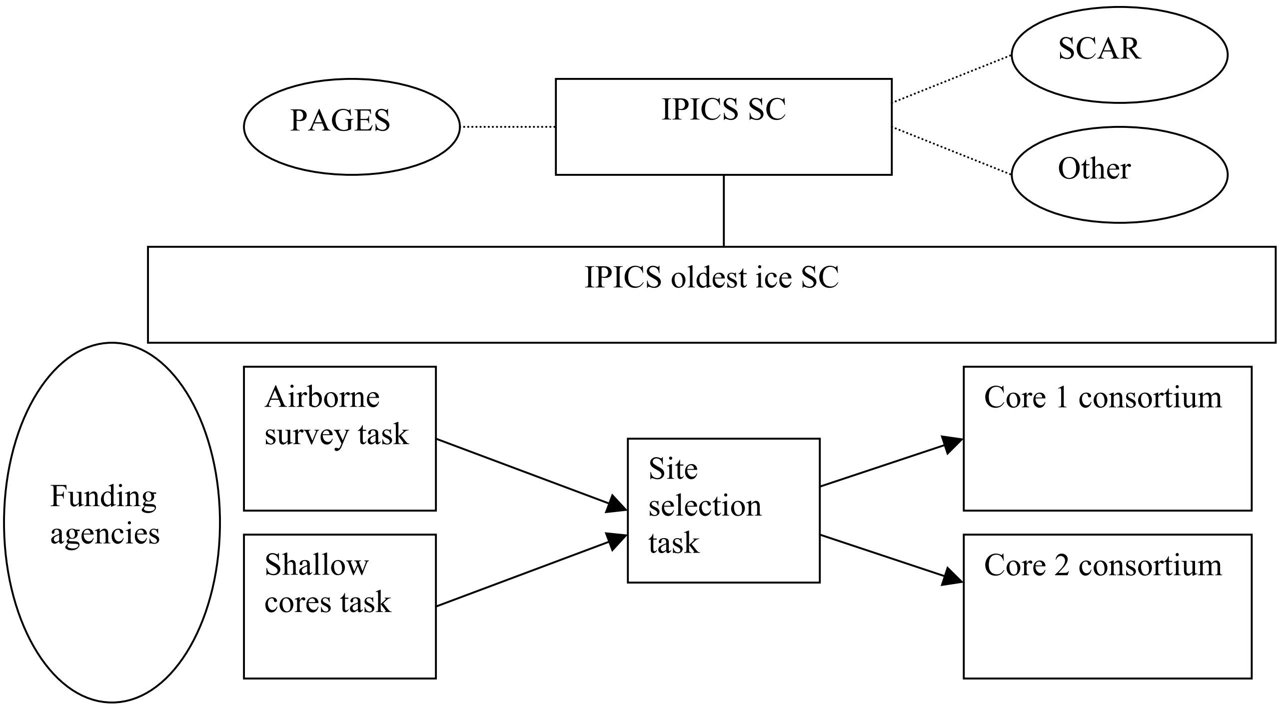
Looking now at the "reward side", country A may have contributed a significant part to the ground survey and site selection, but only modestly (1-2%) to the logistic pot at one drilling site. Overall they still may have accrued notional credit of 5-10%, and therefore would be justified in asking for major access to samples and intellectual leadership in one or more consortia.

A formal publications policy requires further thought but some principles we consider valuable from previous projects are:

- At least one major paper from each drilling should be considered "community papers" with all contributing nations represented
- The authorship for other papers should reflect input to the paper, but with an inclusive philosophy

Clearly much more discussion is needed to refine any model for collaboration. Our points here are (1) that in order to make a start, we should recognise the principle that site selection tasks buy credit in the drilling itself, and (2) that we should operate a flexible system of notional credit that allows those contributing most to benefit most but without stifling intellectual input from people whose nations have contributed less. We recognise that some funding agencies may prefer a much more formal and quantitative system than is proposed here. However we believe this might lead to endless and fruitless dispute, and we emphasise that variants of the system proposed here have worked in previous projects.

Schematic for the main tasks of IPICS oldest ice



7. Next steps

This plan has been written by the sub-group of the IPICS SC that was asked to look at "oldest ice". The next step is for it to be agreed by the IPICS SC. After that (or perhaps simultaneously with it) two things are needed: (1) a project SC is needed to take it forward, (2) commitments or intentions are needed from potential participating nations.

The list below describes the different tasks for which commitments are needed:

Task	Type of commitment
Conception/planning	Oldest ice steering committee to be formed
Airborne survey	Field campaign
Shallow core survey	Field campaign
Site selection	Modelling and assessment exercise
Drill developments	Fluid selection, other developments
Drill site 1	Consortium formation for logistics, drilling and analysis
Drill site 2	Consortium formation for logistics, drilling and analysis
Communication	Web site development, history, etc.

Version:

This version was prepared after comments by the IPICS SC meeting in April 2008.
This version is dated May 2008.

References

Arthern, R. J., D. P. Winebrenner, and D. G. Vaughan (2006), Antarctic snow accumulation mapped using polarization of 4.3-cm wavelength microwave emission, *J. Geophys. Res.*, *111*, D06107, doi:10.1029/2004JD005667.

Berger, A., et al. (1999), Modelling northern hemisphere ice volume over the last 3 Ma, *Quat. Sci. Rev.*, *18*, 1-11.

Brook, E., and E. W. Wolff (2006), The future of ice core science, *EOS Trans.*, *87*, 39.

EPICA Community Members (2004), Eight glacial cycles from an Antarctic ice core, *Nature*, *429*, 623-628.

EPICA Community Members (2006), One-to-one hemispheric coupling of millennial polar climate variability during the last glacial, *Nature*, *444*, 195-198.

Hays, J. D., et al. (1976), Variations in the earth's orbit: Pacemaker of the ice ages, *Science*, *194*, 1121-1132.

Huybers, P. (2006), Early Pleistocene glacial cycles and the integrated summer insolation forcing, *Science*, *313*, 508-511.

Imbrie, J., et al. (1993), On the structure and origin of major glaciation cycles. 2. the 100,000-year cycle, *Paleoceanography*, *8*, 699-735.

Jouzel, J., et al. (2007), Orbital and millennial Antarctic climate variability over the last 800 000 years, *Science*, *317*, 793-796.

Kawamura, K., et al. (2007), Northern Hemisphere forcing of climatic cycles over the past 360,000 years implied by accurately dated Antarctic ice cores, *Nature, 448,* 912-916.

Lisiecki, L. E., and M. E. Raymo (2005), A Pliocene-Pleistocene stack of 57 globally distributed benthic delta O-18 records, *Paleoceanography*, *20*, PA1003; doi:1010.1029/2004PA001071.

Lythe, M. B., and D. G. Vaughan (2001), BEDMAP: A new ice thickness and subglacial topographic model of Antarctica, *J. Geophys. Res.*, *106*, 11335-11351.

Paillard, D., and F. Parrenin (2004), The Antarctic ice sheet and the triggering of deglaciations, *Earth planet. Sci. Lett.*, *227*, 263-271.

Petit, J. R., et al. (1999), Climate and atmospheric history of the past 420,000 years from the Vostok ice core, Antarctica, *Nature*, *399*, 429-436.

Raymo, M. E., et al. (2006), Plio-pleistocene ice volume, Antarctic climate, and the global delta O-18 record, *Science*, *313*, 492-495.

Siegenthaler, U., et al. (2005), Stable carbon cycle-climate relationship during the late Pleistocene, *Science, 310*, 1313-1317.

Tziperman, E., et al. (2006), Consequences of pacing the Pleistocene 100 kyr ice ages by nonlinear phase locking to Milankovitch forcing, *Paleoceanography*, *21*, PA4206, doi:4210.1029/2005PA001241.

Watanabe, O., et al. (2003), Homogeneous climate variability across East Antarctica over the past three glacial cycles, *Nature*, *422*, 509-512.

Wolff, E. W., et al. (2006), Southern Ocean sea-ice extent, productivity and iron flux over the past eight glacial cycles, *Nature*, *440*, 491-496.

Xu JZ, Hou SG, Ren JW, et al. (2007), Insoluble dust in a new core from Dome Argus, central East Antarctica, *Journal of Glaciology, 53*, 154-156.

Zachos, J., et al. (2001), Trends, rhythms, and aberrations in global climate 65 Ma to present, *Science*, *292*, 686-693.