eutema Technology Management GmbH
Austrian Research Promotion Agency

FET Flagships

Interim Public Report
Analysis of Flagship-like Examples

edited by Erich Prem, eutema

for the study team authors:

Mark Milutinovich (AAAS), Stefan Lasser and Thomas Zergoi (FFG), Erich Prem (eutema)

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Executive Summary

An analysis of previous flagship-like initiatives points to important lessons learned for the design of large-scale research initiatives. The expert consultations performed in the context of this study are generally well aligned with these lessons and result in the following general recommendations.

Flagships are clearly science-driven initiatives with inherent risks, but the *goal achieving* character of flagships is vital. Their scientific mission should be complex, comprehensive and broad, but it must be clear when it is fulfilled. Also, the mission should be easy to communicate. Goals are important for achieving topical alignment and for interdisciplinary integration. Goals are also important to funding agencies, politicians and a broad public.

Flagships are particular as they are focused, long-term initiatives; but it will also be important to create impact, new technologies, and evaluations along the way. It is essential to involve the research community in the shaping of the program and to balance individual researcher goals with those of the initiative.

Flagships will benefit from strong scientific leadership. This leadership also concerns the flagship content, not just management aspects. Leaders act as the glue binding people and projects together; they have to be identified at a very early stage and should take responsibility from the beginning.

The initiatives should benefit from efforts and infrastructure to integrate data between various research groups and possibly from joint or closely networked centres. An environment rewarding integration is important as flagships must be more than collections of research projects.

Flagships should perform scrutiny in regularly evaluating progress towards the goals and taking corrective actions.

Flagships will have to remain open to the participation of small groups or individuals as the origins of creative new ideas.

The integration different ICT fields and the integration of ICT with other scientific disciplines including the humanities is essential. ICT in a flagship is not just at the service of another discipline, however. It must lie at the core with mutually beneficial cooperation for all fields.

Flagships should be managed by a small, possibly multidisciplinary team of top people. They must break down high-level goals to smaller aims and topics. This requires managing scientists, not just administrators. Management and the shape of flagships may be different for different topics. Some form of centre (either virtual or physical) can be useful for achieving integration.

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Introduction

This document summarizes interim results of the FET Flagship study by eutema and its partner FFG and subcontractor AAAS. In the reporting period for this interim report, the focus was on the identification and analysis of previous initiatives, the revision of the flagship concept and criteria. The analysis of previous flagship-like examples was based on desk and internet research, and interviews with experts.

Analysis of comparative experiences, validating the concept

Success factors

An overview of previous flagship-like initiatives resulted in a collection of more than 60 examples. Many of these are large collaborative projects in an academic or a cooperative industry-research setting. A set of six such previous flagship-like examples was subjected to an analysis of success factors including interviews with experts knowledgeable in these flagships. Lessons learned include the necessity to involve the research community in the shaping of the program and to balance individual researcher goals with those of the initiative. It is highly important to clearly define and strategically re-evaluate the goals of such an undertaking; also the structure of the flagship should be very clear. An environment conductive to integration is as important as is leadership which also includes scientific leadership. The initiatives will benefit from efforts and infrastructure to integrate data between various research groups.

Consultation

Results from the consultation phase and other sources as well as interviews with external experts were performed to collect further input for the refinement of concept and criteria and to assess potential topics for flagship initiatives.

The "mission" character of flagships is considered very important. Goals are considered important for alignment (integration), for interdisciplinary integration, but also for funding agencies, politicians and the broad public. Experts warn against oversimplified, narrow, or short-ranged goals. Finding these goals, however, is considered very difficult. For some, the ISTAG topics are very broad and also overlapping.

There is some scepticism that fundamental research breakthroughs are possible via large-scale top-down directed mechanisms and this should be taken into account in the flagship design. The individual scientist is important and breakthroughs often come from small teams, not from large initiatives. Expert views on the desirable time-to-impact vary, also because "impact" means different things to different experts. There is very high agreement between experts that the integration of and with different scientific disciplines is very important or even essential.

Several experts suggest that flagships should be managed by a small multidisciplinary team of top experts (2 – 20 people). They must break down high-level goals to smaller aims and topics. This requires managing scientists, not just administrators. Management may be different for different topics. Most experts believe that some form of centre (either virtual or physical) is useful for achieving integration.

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There is very strong agreement that successful flagships require strong leadership. Leadership should concern the content, not just management aspects. Leaders act as the glue binding people and projects together.

The findings of this report were used in the refinement of the FET Flagship concept.

MAIN RESULTS

1. Collection of previous flagship-like examples

Work first focused on the analysis of previous flagships and the refinement of the flagship concept. The first task aimed at collecting relevant data and information from other flagship-like cases realised in EU und non-EU countries. Particular attention was paid to the US, as some initiatives can be seen as best-practice in relation to large collaborative projects between industry and research using several resources. The "American Association for the Advancement of Science" assisted in the identification of such initiatives, gathering information and analysing the initiatives (in Task 1.2).

As a starting point, an extensive *internet and desk research* of national as well as international sources was performed. This task was pursed in a four-step approach (thus deviating from the original two-steps planning):

1) Collecting short (1-2 page) descriptions of flagship-like initiatives, 13 in the US, 11 international and further 10 potential US examples.

2) A refinement of this list resulted in 28 US-based examples, 14 international examples and 21 further potential US examples.

3) A selection of cases was made for more detailed analysis, resulting in a long list of 36 potential examples and a short list of 23 potential flagship-like initiatives for further study. These examples were described on 1-2 pages each and sorted in four different classes according to estimated suitability for comparison with the FET-F initiative.

4) From this list the study team selected 6 examples.

The initial list of potential flagship-like examples contains the following examples:

US-Based
- Assembling the Tree of Life
- Cancer Biomedical Informatics Grid (caBIG)
- Consortium for the Barcoding of Life (CBOL)
- DARPA Grand Challenge
- Deep Thought/Deep Blue
- Earthscope
- Geoscience Network (GEON/GRID)
- Hubble Space Telescope
- Human Genome Project
- Human Microbiome Project
- iPlant
- Long Term Ecological Network (LTER)

- Man on the Moon Challenge (Apollo Program)
- NASA's Centennial Challenges program
- National Ignition Facility (construction phase)
- National Nanotechnology Initiative (NNI)
- NCEAS - National Center for Ecological Analysis and Synthesis
- NIGMS Protein Structure Initiative
- Pacific Rim Applications and Grid Middleware Assembly (PRAGMA)
- Sematech
- Strategic Computing Initiative
- Strategic Defense Initiative (SDI) or Star Wars
- Superconducting Super Collider
- TeraGrid
- The Accelerated Strategic Computing Initiative (ASCI)
- War on Cancer
- X Prize Foundation

International
- Blue Brain Project
- EU-US RFID Lighthouse pilot projects
- GBIF, the Global Biodiversity Information Facility
- Japan earth simulator
- Large Hadron Collider
- OMII-UK
- Super Kamiokande
- The 5th Generation Computing Initiative
- Virtual Physiological Human Network of Excellence

In addition to this list, several examples of currently starting or recently started initiatives were also identified, e.g. Bionic Eye (Australia), Ambient Assisted Living (EU). But they were considered too young to be studied concerning success factors.

The following cases were considered as examples for further study:

Example	Motivation for selection by the study team
DARPA Challenge	Achieved great impact and attention with relatively small budget. Significantly different from FET flagship concept so far. Might thus be interesting in complementing the concept.
Human Genome Project	Very good impact and clearly goal-driven initiative. Significant ICT research aspects were also addressed.
Large Hadron Collider	Potentially, management aspects would be interesting as this is a truly large-scale initiative and very much basic research (with clear applications concerning for example the ICT systems).
Long-term ecological research network	A very ambitious and goal-driven initiative. Several lessons to learn concerning long-term running and progress evaluation, educational and training aspects, networking of research locations and data acquisition/management for cross-site synthesis.
Strategic Computing Initiative	Large-scale initiative in computer science with mixed success. Three could be lessons to be learned here.
Assembling Tree of Life	Ambitious and large-scale initiative that also includes training goals. The initiative also had a clear vision. Could be interesting, although it may be too early to assess impact.

2. Analysis of success factors of previous flagships

An analysis of the selected cases was performed based on desk and internet research. Interviews with persons knowledgeable in past activities were performed to supplement the internet and desk research activities.

The results were summarized with respect to critical design features and success factors of established flagships. In order to avoid misleading comparisons, the analysis takes into account different contexts and system frameworks into which the flagships are embedded.

Methodology

To prepare a detailed analysis of the flagship-like initiatives, information was collected from numerous sources. When possible, planning documents, funding announcements, evaluation materials, and initiative websites were all utilized. For some initiatives, analyses that overlap with the goals of this task have been conducted. Those sources have also been integrated. Finally, interviews were conducted with people intimately familiar with the initiatives to provide a personal context and reality-check for written documentation.

For the six initiatives, twenty interviews were conducted using a defined protocol to guide the discussion (attached). The protocol was informed by discussions with FFG and Eutema and based on criteria defined by the Report of ISTAG FET Working Group (FET flagships: Transforming ICT for 2020 and beyond).Where possible, there was an attempt to talk with at least one person from the management or funding perspective and one from the research or development perspective. Information gathered from these interviews has been embedded where appropriate in the analyses without ascribing contribution to specific individuals. This structure was established to ensure honest and open discussions during interviews.

After reviewing the background materials and completing the interviews, information was synthesized and structured as responses to the criteria used in the interview protocol. While other formats were considered, this structure was chosen to convey the information in a way that maps to the construct of the envisioned FET-Flagship program. The accompanying analyses are not comprehensive. However, it is our hope that each initiative has been analyzed in sufficient depth to elaborate critical design features, challenges, and success factors. A sincere attempt has been made to cite written materials when appropriate. These primary sources undoubtedly contain additional information that might be useful to this project.

In addition to analyzing each initiative, an attempt has been made to identify those aspects that appear to transcend specific initiatives, and may be important for the design and implementation of an array of programs. Again, the sample size is small and it is possible that these cross-cutting aspects may be specific to the selected initiatives only and not indicative of general "best practices." However, they are interesting to note.

The analysis is mostly structured along dimensions also candidates for assessment or selection criteria: ambition, plausibility, structure, integration, and impact.

2.1 Assembling the Tree of Life

The goal of the Assembling the Tree of Life initative is to construct an evolutionary history for all major lineages of life. Established in 2002, the initative was designed to provide a complete tree within 10-15 years.

Ambition

The ambition of the Assembling the Tree of Life (AToL) is to assemble an evolutionary history for the major lineages of the 1.7 million identified current species on Earth.[1] Resolving the Tree of Life has been called "among the most complex scientific problems facing biology and presents challenges much greater than sequencing the human genome."[2]

With whole genome sequencing developing in the 1990's, utilizing comparative genomic approaches to address biological questions became a reality, necessitating an organizational framework for understanding evolutionary relationships.[1] Prior to the start of AToL, less than 70,000 species had been studied in various levels of depth, potentially limiting progress in many areas of scientific research, human health, and environmental management. According to one person involved with AToL, "There used to be twenty taxa. Now there are thousands to tens of thousands taxa, and that number is growing exponentially. This scale means that current methods don't work at all." To meet this challenge requires an extremely inter-disciplinary approach incorporating scientists from fields such as computer science, ecology, engineering, genomics, geology, mathematics, and many others.[3]

Plausibility

The concept and planning of AToL was community driven. The idea for a Tree of Life initiative has roots in a report authored by three international Systematics societies in 1994.[4] The specific AToL initiative developed out of three workshops held in 1999 and 2000. The workshops, sponsored by the US National Science Foundation (NSF), brought together dozens of scientists from universities, foundations, Federal laboratories, and museums that laid out the goals and rationale to achieve a Tree of Life in 10-15 years.[2,5]

Despite these efforts, a well-defined roadmap and milestones for AToL were not developed.[6] AToL was implemented in 2002 through an NSF grant solicitation for projects that addressed the following three goals: increase the number of taxa and data sets with a focus on resolving phylogenetic relationships of large branches of the Tree

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[1] Assembling the Tree of Life, NSF Program Solicitation, NSF-02-074

[2] Assembling the Tree of Life: Harnessing Life's History to Benefit Science and Society (http://ucjeps.berkeley.edu/tol.pdf)

[3] Interview on 3/31/2010

[4] Systematics Agenda 2000 (1994a) *Systematics Agenda 2000: Charting the Biosphere*. New York: Society of Systematics Biologists, American Society of Plant Taxonomists, Willi Hennig Society, Association of Systematics Collections.

[5] Email communication, 4/2/2010

[6] Interview on 4/16/2010

of Life; research and development of tools for computational phylogenetics and phyloinformatics; and outreach and education in comparative phylogenetics and phyloinformatics. To accomplish this, it was suggested that projects should be "ambitious, large scale, and to involve multiple investigators from multiple disciplines, likely from multiple institutions… and to include training, outreach, and dissemination components." Each award could ask for a total of $3 million USD for a duration of five years.[1] Approximately $14 million USD was awarded in the first year, 2002.[7]

Without concrete milestones in place, there is a sense that AToL has gotten a bit off track.[3,6,8] In part, this is due to the inherent challenges involved in this comprehensive initiative. For instance, a unified scientific approach has been difficult, as each branch of the tree contains its own unique challenges (e.g., resolving the prokaryotic tree is complicated by lateral gene transfer). This has resulted in each group making individual choices for project design, including what markers to sequence. In addition, computational tool development is needed to make sense of accumulating data. Consequently, stitching the patchwork together into a comprehensive tree will be a difficult task.[8]

Structure

Research has been funded through a grant mechanism managed by the NSF. A Program Officer at NSF is charged with managing the initiative. Awards are made through a competitive peer review process and range up to $3 million USD, for durations up to 5 years. AToL has awarded approximately $112 million USD since 2002.[6] Continuation of funding for each award is predicated on milestones achieved and progress toward goals of the project. In essence, AToL funds two different types of projects: those oriented toward establishing a tree for organisms; and those developing computational tools to make that possible.[3] Projects funded by AToL are allowed to organize as they see fit. Aside from a periodic investigators meeting every two to three years, there are no formal or informal mechanisms for community interaction in place.[6,8]

Integrations

There are few limitations regarding who can apply for funding from AToL. Usually, applicant teams consist of large groups of investigators, usually from multiple disciplines and institutions.[7] Integration within these teams is driven mainly by common research interests. Each team develops its own functioning structure, with minimal input from AToL leadership.

However, reaching the goals of AToL requires the integration of diverse scientific approaches and information technologies across the different groups of investigators. This is extremely challenging, as each group of organisms presents its own scientific hurdles. Plus, the field is quickly evolving; every new generation of sequencing technology affects phylogenetic analyses. Thus, data management is a huge challenge. Finally, there are sociological challenges when different groups of scientists work together.[3,8]

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[7] Data compiled from the NSF Awards database (http://www.nsf.gov/awardsearch/index.jsp)

[8] Interview on 4/2/2010

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There historically has not been an emphasis on synthesis and coordination with the AToL community.[3,6] One outcome of this is that AToL is now doing salvage work by defining a set of 30 core genes and resequencing them again to stitch the patchwork together. In addition, selection of taxa has not occurred strategically (i.e., projects were not selected with the initiative needs in mind.).In 2008, AToL organized a workshop to bring fresh vision to the initiative.[6] One outcome from this exercise is that the grant solicitation for 2010 has been modified to emphasize integration and involvement of new taxa. Having focused on resolving the phyologenetic relationships among the major taxonomic groups (such as classes, phyla, and even kingdoms), the program is now seeking proposals to study major taxonomic groups that haven't been looked at to date by AToL. It is also looking to fund ambitious ideas for how to integrate the data that has already been completed (analytical synthesis).[9] This represents a significant change of course for AToL. As one researcher described, "NSF used to give out the money and groups would do their own thing. Now, they use the money to drive social engineering."[3]

An additional challenge faced by AToL is that big collaboration projects such as this are controversial in the research community. There will always be a group that feels that the money is being diverted to a few people. This can be managed a bit by how some money is set aside (e.g., single investigator awards, underrepresented groups, etc.), but cannot be eliminated.[7]

Impact

For individual aspects of the initiative, there have been large scientific impacts. For instance, beetle, flowering plants and fungal families have made remarkable progress. However, as mentioned above, integration of different lineages has been slower to be realized. Perhaps the major scientific impact from AToL will be the creation of a substantial infrastructure of information that will enable significant new questions to be asked in numerous fields of science.[3,6]

As an NSF initiative, all proposals must address broader impacts, defined as: How well does the activity advance discovery and understanding while promoting teaching, training, and learning? How well does the proposed activity broaden the participation of underrepresented groups (e.g., gender, ethnicity, disability, geographic, etc.)? To what extent will it enhance the infrastructure for research and education, such as facilities, instrumentation, networks, and partnerships? Will the results be disseminated broadly to enhance scientific and technological understanding? What may be the benefits of the proposed activity to society?[9] Individual projects within AToL have made significant impacts on a local level, engaging students and society in understanding biodiversity.[6] Some of these activities are also having broader impacts though the use of the internet.[10] However, there is not coordination of activities across AToL.

The AToL program, itself, does not impose additional impact criteria or discuss economic impacts. However, it is likely that defining evolutionary relationships will have biomedical, agricultural, and ecological impacts.

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[9] Assembling the Tree of Life, NSF Program Solicitation, NSF-10-513

[10] http://www.youtube.com/watch?v=9R8hpPY_9kY

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Success Factors/Lessons learned

The major success of AToL has been the identification of an important conceptual initiative by a community of scientists. In addition, individual research projects have had significant success. However, progress toward the larger goal of creating a Tree of Life for all identified species has not been as evident. The following are a collection of lessons that seem to emerge.

1) Roadmap or vision; A clear understanding of the whole initiative is needed, including potential pitfalls, challenges, and opportunities. This "vision" can come from either within the research community or be provided from a top-down approach.

2) Oversight is crucial; To ensure progress, identify challenges early, and make adjustments as necessary, requires oversight. This can be provided through periodic internal or external evaluations. AToL did not undergo an evaluation until 2008, making adjustments painful. In addition, drift from mission can be exacerbated by frequent changes in program management personnel.

3) Emphasize coordination from the outset; In large programmatic initiatives, the goal is to have the whole be greater than the sum or its parts. Because research projects were allowed to progress without coordination, AToL is struggling now with integration. Looking back, involving a strategic coordinating body comprised of members of the research community may have assisted integration.[6]

4) Balance the needs of the investigators with needs of the program; The balance will differ depending on the culture of the funding agency and research community. For instance, given the traditionally hands-off management style of the NSF, adjusting AToL through revised solicitation review criteria is appropriate. The risk of imposing too much top-down management in this case would be discouraging the research community from feeling a part of the initiative. The perception could be that the initiative is driven by too few people.

2.2 DARPA Grand Challenge

The DARPA Grand Challenge was a prize competition for autonomous ground vehicles, funded by the Defense Advanced Research Projects Agency (DARPA), a research organization of the United States Department of Defense. From 2004 to 2007, three competitions were held to identify vehicles that were able to navigate a specific course in a given timeframe. The first two competitions, in 2004 and 2005, were designed to simulate desert environments, while the the third competition was designed to mirror an interactive urban environment.

Ambition

Established in 1958, DAPRA has a long history of pursuing high risk, high reward research and technology. To attract a diverse mix of disciplines and personalities not typically funded by the Department of Defense and accelerate technology development, DARPA began thinking about the idea of prize incentives in the late 1990's.[11] However, they were legally restricted from awarding funds in this way until the United States Congress passed legislation allowing DAPRA to "award up to $10 million in cash prizes, in a fiscal year, to recognize outstanding achievements in basic, advanced, and applied research; technology development; and prototype developments that have the potential for application to the performance of the military mission of the Department of Defense."[12]

At that time, DARPA began looking for a good problem. They settled on autonomous vehicles for the following reasons: 1) a formal goal of the US military is to have one-third of the operational ground combat vehicles be unmanned by 2015; 2) while research and development on specific aspects of autonomous vehicles was improving capabilities, there were few opportunities to test full-system integration; 3) it was possible to design a challenge around the concept that would test autonomous vehicles in ways that reflect real world situations; and 4) DARPA wanted to bring more people into this area. As one interviewee stated, "If you're trying to attract people to spend their own money, they need to have a good head start; everyone has a car in their garage."[11,12]

DARPA struggled internally with the concept of a competition. The perception in competitions is that there can only be one winner. However, since the competition would have strong research and technology components, it would be possible to learn from the successes and failures of all competitors. Another concern was that a competition may scare off institutions from participating. In fact, anecdotally this happened, as some industry companies refused to participate because they feared losing to student teams. Alternatively, the participation of teams from prestigious institutions such as Stanford University and Carnegie Mellon actually provided an incentive for other institutions to participate and try and "knock off the big guns".[11,13]

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[11] Interview on 3/29/2010

[12] Report to Congress: DARPA Prize Authority, Fiscal Year 2005 report in accordance with 10 U.S.C. 2374a

[13] Interview on 3/24/2010

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Plausibility

There was no scientific or technical roadmap for this initiative. Demonstrations of autonomous vehicles had been conducted in the past (e.g., the EUREKA Prometheus Project), but none had involved off-road desert driving. By bringing together diverse teams from industry, academia, and business, the intent was to "develop synergies that would foster new ways of thinking."[14] For the first two challenges, competitors bore the full cost of development. For the third competition, DARPA provided $11 million USD of milestone-based grant support to eleven teams chosen through a competitive process. Those not receiving funds were allowed to participate, but received no DARPA funding for support.

Goals and milestones for the DARPA Grand Challenge were ambitious. The first challenge was announced in 2002, with the goal of completing a 142 mile desert course within 10 hours on March 13, 2004. DARPA believed there would be a winner for the first challenge.[11] From an initial list of 106 interested teams, 25 were chosen to participate in the competition. Selection was based on success at a National Qualification Event held prior to the Grand Challenge event. Of the 25 participants, no vehicle completed the first challenge. In fact, the most successful vehicle completed only 7 miles of the course.[12] However, important steps were made.[11]

Based on perceived progress and the interest and enthusiasm that had been created, a second competition was announced immediately following the conclusion of the first. As an added incentive, the prize for completing the 132 mile desert course in the shortest time was raised to $2 million. This time 195 applications were received, and again 25 were chosen to participate in the competition. To help select the most competitive teams, DARPA implemented a three-stage review process including video review of the vehicle in operation, a site visit by DARPA staff, and evaluation at the National Qualification Event. The second competition was held on October 8, 2005. Stanford Racing won the event, with four additional teams completing the challenge in the allotted time. All but one competitor exceeded the 7 mile distance achieved by the best vehicle in the previous challenge. At the conclusion of the event, DARPA announced that there would be no more autonomous vehicle challenges.[12,15]

However, there was still enthusiasm at DARPA and within the community to continue.[11] A third challenge, focused on the ability of autonomous vehicles to navigate an interactive urban environment, was announced on May 1, 2006.[16] This change was necessary because it brought new people to the competition and focused on a critical aspect for future utility of autonomous vehicles.[13] To assist in development efforts, DARPA ran a parallel program to award eleven grants for $1 million each to teams interested in competing (Track A). This allowed some teams that had used all available resources during the previous two competitions to continue.[11] Those competitors not selected for Track A funding, but still willing to bear the full burden of cost, were also allowed to participate (Track B). For further incentive, prizes were added for second ($1 million) and third ($500,000) place, in addition to the $2 million prize for first place. From the 11 Track A and 89 Track B teams, eleven were chosen to participate in the competition (seven from Track A and four from Track B). Again, DARPA utilized a multiple-step review process for ensuring the progress of funded teams and inclusion

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[14] The DARPA Grand Challenge Commemorative Program (http://www.darpa.mil/grandchallenge04/)

[15] http://www.darpa.mil/grandchallenge05/index.html

[16] http://www.darpa.mil/grandchallenge/index.asp

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of the most competitive teams in the challenge event. The third challenge event took take place November 3, 2007. Six of the eleven teams completed the course in the required time. Of those, five were Track A teams. Based on time and demonstration of ability to follow California traffic laws, Tartan Racing was awarded first prize, Stanford Racing was awarded second prize, and Victor Tango was awarded third prize.[16]

Structure

DARPA developed extensive rules for each of the Grand Challenge events, defining such features as the purpose, eligibility, vehicle requirements, qualification process, and final challenge event. Teams were required to submit technical papers describing their technology to DARPA for public release after the challenge events.

For each of the three competitions, DARPA did provide oversight and guidance in the selection of which competitors to include in the final event. This evolved from selecting participants based on a single qualifying event for the 2004 competition, to a multi-step review process for the 2005 and 2007 events. This was done, in part, due to logistical limitations on the number of participants that could compete in the final event, but also to provide an incentive for progress.[14,15,16]

The third competition varied from the first two in purpose, vehicle requirements, qualification process, and final event. Most changes were due to the strategic decision to focus the third challenge on the ability to navigate in an urban environments, rather than desert. This allowed DARPA to build on gains made during the previous competitions, while building toward a more sophisticated autonomous vehicle. To encourage those who had used their own resources for the previous two competitions to continue their involvement in the initiative, the urban challenge involved awarding grants to a subset of participants. This mechanism was also a pull for new teams to enter the competiton.[11,13,14,15,16]

Perhaps the largest structural component to the three challenges was coordinating logistics of the initiative (i.e., informational meetings, qualifying events, and the final events. This involved a large number of DARPA employees, contractors, and partners.

Integrations

Teams were encouraged to develop their own structure, with minimal requirements from DARPA. For instance, each team must designate a team leader who was a US citizen at least 21 years old. Foreign citizens were allowed to participate as members of teams. Foreign corporations could participate as team sponsors, contributing labor, materials, services, equipment or funds to the team.[14,15,16]

Teams were encouraged to be diverse, with industry and academia working together. This was envisioned to both increase the sophistication of developed vehicles and speed the translation of technology into the market. DARPA claimed no intellectual property rights from competitiors, with all "trade secrets, copyrights, patent rights, and software rights" remaining with the respective teams.

While conflicts arose within teams, the focusing effect of competing before peers was important in minimizing these conflicts.

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Impact

From the perspective of DARPA, the leap in technology sophistication from the first to the least challenge was enormous and could not have been achieved through a more traditional funding mechanism.[11] The long-term impacts are unclear. DARPA traditionally does not invest in sustaining fields of research and technology, instead relying on other more mission-oriented funders to continue where they have left off. For military applications, there appears to be continued streams of funding from other Federal agencies. However, for civilian applications, the main funders appear to be private industry.[13]

Technology transfer has occurred between competing academic teams and industrial partners.[12] In addition, the publication of each vehicle's technical details and availability of data from competitions should continue to drive innovation.

Estimates of the amount of money spent by competitors and sponsors varies, but is considered significantly greater than the amount of available prize money.[11,13] For the entire 2005 competition, DARPA expended only $7.8 million, plus the $2 million prize.[12]

While there was significant financial leverage, the real leverage was in the coherence of a field.[11] The competitions brought non-traditional people into the autonomous vehicle field (e.g., theorists) to work on a practical problem. This has spawned new collaborations within institutions and new collaborations between academic institutions and industry. In addition, the high profile nature of the competitions and challenge had a significant impact on student interest. High school, undergraduate, and graduate students all participated as team members. At least one of the participating teams in the Urban Challenge, did so primarily due to student enthusiasm.[13]

Success Factors/Lessons learned

The DARPA Grand Challenge events were very successful in demonstrating the ability of autonomous ground vehicles to navigate both real-work desert and urban environments. The high profile nature and structure of the competitions encouraged integration of industry, academic, and "garage" participants that led significant synergistic innovations. The following are a collection of lessons that seem to emerge.

1) Competition can be a powerful driver, but also a deterrent. For industry, interest was usually driven by mid-level managers who attempted to talk their bosses into supporting their involvement. Due to the risk of failure, some did not participate. Those who did fell into two classes: 1) those from established companies that didn't worry about being embarrassed; and 2) those from hungry companies that wanted an excuse to demonstrate their ability in front of potential funders. Academic interest was mainly driven by student enthusiasm, though some faculty participated for the opportunity to beat prestigious institutions, such as Stanford. Consequently, involvement was mainly driven by prestige, but also potential financial gain.

2) Designing a meaningful competition is challenging. When designing the goals of the grand challenge competitions, the technical focus was on demonstrating the potential future capabilities of using autonomous ground vehicles for military

purposes. Therefore, simulating the environments where those vehicles could be used was critical in the design of the competitions.

3) Be flexible. DARPA fully believed that there would be a winner of the inaugural Grand Challenge event. However, when that did not occur, they made the decision to hold a follow-on competition rather than discontinue the excercise. The improvement in vehicle performance between the first and second challenge was significant and supported this decision. Rather than continue in the desert environment for the third challenge, DARPA chose to move vehicle development in a new direction, with an interactive urban environment. This presented new challenges to the research community and brought new teams into the competition. To jump-start the new teams, while continuing to build on developments made during the first two competitions, DARPA offered grants to the most competitive teams.

4) High profile competitions can have drawbacks. There are some in the autonomous vehicle community that feel DARPA was too focused on identifying a winning team, after no such team arose during the first challenge. They feel as though some of the more challenging goals were scaled back to ensure success, such that the ultimate designs were not as innovative as they could have been.

2.3 Human Genome Project

The Human Genome Project was designed to identify all the approximately 20,000-25,000 genes in human DNA; determine the sequences of the 3 billion chemical base pairs that make up human DNA; store this information in databases; improve tools for data analysis; transfer related technologies to the private sector; and address the ethical, legal, and social issues that may arise from the project.

Ambition

The Human Genome Project (HGP) has been described as the "equivalent of President Kennedy's call for the mobilization of national resources to land a man on the moon within the decade of the '60s."[17] Basic research in molecular and medical genetics since the 1950's had indicated the importance of genetic material in numerous fields, including human health and agriculture. The advent of recombinant DNA technologies in the 1970's resulted in the ability to isolate and amplify defined fragments of DNA. This, coupled with the development of sequencing technologies in the late 1970's made it possible to envision the sequencing of the human genome.[18] While theoretically possible, the leap to tackling the human genome was immense. For example, the first DNA genome, a 5386 base pair bacteriophage, was sequenced in 1977.[19] The human genome was estimated to be 3 billion base pairs.[17] To accomplish this task would involve a focused multi-disciplinary program involving researchers from around the world.

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[17] The Human Genome Initiative: A Different Type of Research; 1990, vol. 4, pp 1423-4, FASEB
http://www.fasebj.org/cgi/reprint/4/5/1423.pdf)

[18] Understanding Our Genetic Inheritance, The US Human Genome Project: The First Five Years; Fiscal Years 1991-1995
(http://www.ornl.gov/sci/techresources/Human_Genome/project/5yrplan/summary.shtml)

[19] Sanger, F. et al. (1977) Nucleotide sequence of bacteriophage X174 DNA. Nature, 265, 687-695.

Plausibility

Interestingly, interviewees described the goals of the initiative at the time of implementation as "not plausible" and "reckless", but all deemed them necessary.[20,21]

The development of the Human Genome Project was driven by a number of meetings involving the research community and Federal funding agencies. The first serious discussion of the possibility of sequencing the human genome was in 1985 at a meeting conducted by the chancellor of the University of California at Santa Cruz.[22] In 1986, the US Department of Energy (DOE) began funding initial research into human genome mapping and sequencing. The DOE's interest in human genomics was derived from its involvement in understanding health impacts of radiation exposure. They were heavily involved in mutation detection technologies. With recombinant DNA technologies developed n the mid 1970's, it became possible to define reference sequence in the human population.[21] Further discussions took place in 1986 and 1987, culminating in the 1998 US National Academy of Sciences recommendation for initiation of the Human Genome Project. In 1999, the US Congress appropriated funds to the DOE and US National Institutes of Health (NIH) to support research efforts to "determine the structure of complex genomes."[18]

Along with the appropriated funds, came a request for the NIH and DOE to develop strategic planning documents for the Human Genome Project. Thus, in 1990 a five-year strategic plan was published covering 1991-1995. The jointly developed plan laid out specific scientific goals for mapping and sequencing the human genome and model organism genomes, including an emphasis on technology development and informatics. Further, it stressed the importance of training researchers in genomic science. In addition, the plan incorporated research on ethical, legal, and social considerations, anticipating the controversial consequences of the initiative. Finally, the plan described an implementation strategy.[18] In some sense, the development of such a specific strategic plan was not surprising, as the initiative was not hypothesis driven research. The intent was to build an enabling infrastructure.[23] Also, sequencing milestones were relatively easy to measure in real-time, making progress easily measureable.[21]

Do to "unexpected advances in genomic research," a revised five-year strategic plan was developed in 1993, covering 1993-1998. This plan updated the initial five-year goals and added new goals in nearly all areas.[24] Also in 2003, The Sanger Centre, with funding from the Wellcome Trust in the United Kingdom, joined the Human Genome Project. The Sanger Centre's contribution to the Human Genome Project would eventually be second only to the NIH.

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[20] Interview on 4/5/2010

[21] Interview on 3/25/2010

[22] Collins, F. et al (2003) The Human Genome Project: Lessons from Large-Scale Biology. Science, v300(5617), 286 - 290

[23] Interview on 3/29/2010

[24] Revised 5-Year Research Goals of the U.S. Human Genome Project: 1993-1998 (http://www.ornl.gov/sci/techresources/Human_Genome/project/5yrplan/5yrplanrev.shtml)

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In 1998, following a series of DOE and NIH workshops, the third and final strategic plan was released, calling for a draft sequence to be completed in 2001 and the final human sequence to be completed in 2003, two years ahead of schedule.[25]

The year 1998 marked an important year for the Human Genome Project for two other reasons. One, a new sequencing technology using capillary electrophoresis had entered the market, promising increased sequencing capabilities.[21] In addition, an industrial competitor had entered the competition. Celera, founded by Craig Venter, began sequencing the human genome in 1998 using a new technique, whole-genome shotgun sequencing. This created concern about whether the human genomic data would be controlled by the private sector.[22]

This competition has been cited as galvanizing the public consortium of researchers involved in the publicly-funded Human Genome Project. It also correlated with a decision to change the structure of the NIH sequencing centers. To date, the NIH had funded a distributed array of sequencing centers, each tasked with specific parts of the genome. In 1998, the decision was made to concentrate these sequencing efforts at three large sequencing centers and dramatically scale-up.[21] The DOE followed suit in 1999, combining its three sequencing centers into one facility in Walnut Creek, California. Throughout the Human Genome Project, sequencing centers were encouraged to work in collaboration with international sequencing centers. Indeed, one of the reasons credited for advancing so quickly was the involvement of international cooperation, primarily through scientist to scientist interactions.[24]

Consequently in 2001, due in part to competition from private industry, an industrial-scale sequencing effort in the public sector, and international collaboration, a draft sequence of the human genome was released. The full sequence was released in 2003.

Structure

DOE and NIH were funded separately to participate in the Human Genome Project. Both agencies had advisory boards, coordinating committees, and working groups charged with specific aspects of the initiative. In addition, there were cross agency meetings that offered potential for coordination of efforts. The NIH and DOE signed a formal Memorandum of Understanding regarding their joint involvement in the Human Genome Project.[18] Just as important were the informal interactions between sequencing centers. These served to provide both peer pressure regarding progress, but also a conduit for sharing best practices.[21] After the Sanger Centre joined the Human Genome Project, it began coordinating with the DOE and NIH.

The DOE managed its part of the initiative through the Office of Biological and Environmental Research. The NIH established the Office of Human Genome Research in 1988 (directed by James D. Watson) to plan and coordinate NIH genome activities. That office eventually evolved into the National Human Genome Research Institute (NHGRI)). Approximately twenty centers in six countries participated in the Human Genome Project. Their coordination was managed by the The Human Genome Organisation (HUGO).

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[25] Collins, F. et al (1998) New Goals for the US Human Genome Project: 1998-2003. *Science*, v282, 682-689

The DOE initially concentrated its sequencing efforts at three genomic sequencing centers located at federal laboratories: Lawrence Berkeley National Laboratory, Lawrence Livermore National Laboratory, and Los Alamos National Laboratory. The NIH, through peer review, established approximately twenty sequencing centers from 1990-1998. In 1998, NIH made the strategic decision to concentrate the sequencing efforts at three centers and scale-up sequencing efforts considerably. Through a competitive peer review process, the three sequencing centers selected were located at: Washington University in St. Louis, Baylor University, and the Broad Institute (then a facility shared between MIT, Harvard, and the Whitehead Institute). In 1999, the three DOE sequencing centers were combined into a new facility in Walnut Creek, California. In essence, these consolidations allowed the Human Genome Project to take an industrial-scale approach to the sequencing effort.

Approximately 5% of the Human Genome Project budget was spent on ethical, law, and societal initiatives (ELSI).[23]

Integrations

The Human Genome Project was a coordinated effort involving academia, Federal laboratories, and to a small extent industry (mainly in technology development). In addition, sequencing centers included mathematicians, physicists, and engineers as well as biologists. The driving force that integrated these potentially disparate groups of people was a common belief in the goals of the initiative. "Everyone came together for the common good, which by and large was to put sequencing data into the public domain so that Celera couldn't get it."[21] Coordination was spectacular. Coordination was managed through frequent formal and informal interactions, such as conference calls, working groups, and technology exchanges.[23]

The Human Genome Project helped fund a publication, "Human Genome News" that reported on genomics and the downstream implications of genomics research. In addition to a newsletter, the "Human Genome News" put together communication and outreach modules for different user populations: teachers, teachers of teachers, business leaders, exhibitions at scientific meetings, etc. These helped to bring the community together around the idea of genomics.[20]

Impact

Scientifically, completion of the human genome was a "game changer", enabling all kinds of new questions that could be asked by researchers. In addition, technologies and techniques developed by the Human Genome Project laid the groundwork for genomics, which is now integrated into every discipline.[20] In addition to the human sequence, the initiative was critical in establishing genome centers. These centers have had long-term impacts both scientifically and through industrial collaborations with instrumentation.[21]

While biomedical impacts have been slow to realize, thee have been big scientific and economic impacts in industrial applications (white biotech).[20]

The Human Genome Project incorporated ethical, legal, and societal implications (ELSI) as a formal part of the initiative. ELSI received ~5% of the total budget to award research grants. ELSI funded research grants in areas such as privacy and

confidentiality, education, fair use, and integration of genomics into clinical practice. While direct impacts from ELSI activities are hard to assess, it was important that ELSI was done. [23]

Organizationally, the Human Genome Project showed that an industrial scale approach to biology can be successful. Also, it was focused on building a tool (the genomic data), showing that not all science needs to be hypothesis driven.

Success Factors/Lessons learned

The Human Genome Project was an enormous success. It captured the imagination of the public and resulted in a completely new way of thinking about and doing biological research. In addition, it facilitated the integration of genomics into numerous fields and spawned numerous subsequent –omic approaches. If that's not enough, the initiative completed its goals ahead of schedule and under budget. The following are a collection of lessons that seem to emerge.

1) Importance of strategic planning; The Human Genome Project fully utilized 5-year strategic plans to inform the conduct of the initiative and measure progress. In addition, the documents were reviewed continuously and updated appropriately.
2) Coordination; an initiative of this scale requires a strong effort in coordination. The Human Genome Project managed this through both a plethora of formal meetings, as well as informal information sharing amongst the community.
3) Balance the needs of the investigators with needs of the program; In the beginning the initiative, NIH and DOE distributed sequencing capabilities at multiple locations. However, when it was realized that a more coordinated effort was need to meet the goals of the program, management restructured the initiative through a competitive peer review process. This shifting balance was crucial to the success the Human Genome Project.
4) International collaboration; Approximately twenty centers in six countries participated in the completion of the human genome. One of the centers, the Sanger Centre was the second largest contributor. Importantly, these international relationships did not develop through formal governmental discussions, but rather through informal scientist-to-scientist collaborations.
5) Clearly defined goals; The definition of a clear goal is critical, as it provides a target for the initiative and rallying point for the community. A correctly chosen target can enable passion, commitment, and focus from the research community. More diffuse projects may be just as interesting, but they are drastically more challenging to measure.

2.4 Large Hadron Collider

The Large Hadron Collider (LHC) is the world's largest and highest-energy particle accelerator, intended to collide opposing particle beams of either protons at an energy of 7 TeV per particle, or lead nuclei at an energy of 574 TeV per nucleus. It is expected that it will address the most fundamental questions of physics, hopefully allowing progress in understanding the deepest laws of nature. The LHC lies in a tunnel 27 kilometres (17 mi) in circumference, as much as 175 metres (570 ft) beneath the Franco-Swiss border near Geneva, Switzerland.

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Ambition

The ambition for a Large Hadron Collider was driven by the particle physics community. During a symposium in Lausanne, Switzerland in 1984 discussions were held about the future of particle physics. It was decided that the next machine should be a hadron collider.[26] The particle physics community is well organized, has a history of successful international collaboration, and is fairly united in common goals, strengthening the call for this infrastructure. From a research perspective, the new accelerator would allow for new questions that couldn't be addressed with available infrastructure.

Subsequent international meetings were held to further develop the idea of a hadron collider and a program was launched to test the technical feasibility of the new accelerator.[27]

In 1986, the US decided to develop its own superconducting accelerator, the Supercolliding Super Conductor. There was minimal international involvement in this project, as international collaborators were expected to help with costs, but were not allowed to contribute to decision making.[28] to pay without representation in decision making Funding was pulled for the SSC in 1993 for various reasons, including being over budget and behind schedule. One estimate indicates an initial estimate for building the SSC was $5.9 billion USD and by the time it was cancelled, it was expected to cost at least $11 billion USD. At the time of being pulled, approximately $2 billion USD had been spent.[29] This example illuminated the potential pitfalls involved with such a large infrastructure investment.

In 1994, a proposal was taken to the European Organization for Nuclear Research (CERN) by the physics community. The governing Council at CERN approved the proposal, but required that non-member collaborators must bare share the cost. Typically, infrastructure costs at CERN were born by the 20 member states. In this case, CERN agreed to pay 80% of collider construction and 20% of detector costs. The main non-member contributors were India, Canada, Russia, Japan and the US (both Japan and the US joined after the failure of SSC).[26, 28]

Plausibility

Construction of the Large Hadron Collider (LHC) was housed in an existing tunnel that previously held the Large Electron–Positron Collider. This helped to keep costs down (the US SSC was digging a new tunnel). In addition to deconstructing the LEP, extensive civil engineering was required to make room for the experiments that would accompany the accelerator. A committee was set-up to determine which experiments should be conducted at LHC. By 1998, four experiments were approved, setting in motion construction. Two subsequent experiments were approved later, bringing the total to six.[27]

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[26] Interview on 3/24/2010

[27] http://lhc-milestones.web.cern.ch/LHC-Milestones/LHCMilestones-en.html

[28] Interview on 4/6/2010

[29] The Demise of the Superconducting Super Collider, Physics in Perspective (PIP), Volume 2, Number 4 / December, 2000)

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Development and installation of the detectors was a massively coordinated effort, managed by CERN, but involving committees and working groups from each of the experiments.[26] Detector development required pushing state of the art technologies used in previous accelerator designs to new levels, a feat that required technical contributions from around the globe. In some the five and half years of construction resulted in hundred's of cutting edge, unique circuitry designs, new materials, and new techniques. [28] The first successful beam was circulated in 2008. The lifespan of the LHC is 20-30 years.

In addition to construction of the accelerator and detectors, LHC had to deal with the massive amounts of data that would be generated and need to be accessible to thousands of researchers in thirty-four countries. To manage this flow of data, a Wordwide LHC Computing Grid was developed.[30]

"Data from the LHC experiments is distributed around the globe, with a primary backup recorded on tape at CERN. After initial processing, this data is distributed to eleven large computer centres – in Canada, France, Germany, Italy, the Netherlands, the Nordic countries, Spain, Taipei, the UK, and two sites in the USA – with sufficient storage capacity for a large fraction of the data, and with round-the-clock support for the computing grid.

These so-called "Tier-1" centres make the data available to over 160 "Tier-2" centres for specific analysis tasks. Individual scientists can then access the LHC data from their home country, using local computer clusters or even individual PCs."[30]

Structure

A major advantage for the LHC was its placement at CERN. CERN has a long history of fostering international research collaborations and had build particle accelerators before. Thus, internal structures and mechanisms were in place for facilitating and coordinating efforts for the LHC. The exception was the creation of an LHC office to oversee progress. The experiments were charged with construction of their own detector arrays. Coordination was maintained through various levels of formal meetings and oversight.[26,28]

There are currently 6 experiments being conducted by international collaborators. The two biggest in number of people are ATLAS and Compact Muon Solenoid (CNS) (2000-3000 people per).

In general, the organization of each project was pretty isolated from CERN control. Each project has a Project Lead, or Spokesperson that is elected by a 2/3 majority of the team (must be approved by CERN). The Spokesperson nominates a Technical Coordinator and Resources Coordinator, who also must be approved by CERN. There is also an Executive Board comprised of ~20 people with specific expertise that helped coordinate and provide oversight for specific aspects of project. Beyond that, there were informal and formal interactions that took place beyond the control of CERN.[28]

For each experiment, the Resources Review Board called all funding agencies around the world to a meeting once or twice a year. Memorandums of Understanding were

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arranged for sharing responsibilities and financial contributions, those these weren't legally binding. From a governments standpoint, the incentives for meeting expectations were: 1) internal pressure from physics community; 2) common goal; 3) the high visibility of the project was an incentive to see it succeed (prestige).

Integrations

The two largest experiments, ATLAS and CMS involve thousands of scientists around the word. For example, CMS involves 38, soon to be 39, countries and approximately 170 different institutions. It is suggested that these large collaborations work for two main reasons: 1) they have enough organization to function without crippling creativity; and 2) people are passionate and committed, willing to sublimate ego to the will of the group.[28]

From all accounts, there have been few challenges in integrating teams. All interviewees pointed to common goals and the cohesiveness of the particle physics community as reasons for this. As evidence, it was stated that "those that build experiments are just as important as those that do analysis. All names will appear on all publications."[26]

Impact

Scientific impacts from the six experiments remain to be seen, as they are just beginning to collect data.

There was a considerable Outreach component to the LHC. CERN member-states have people responsible for exciting government and the people using their own language. So, when the LHC was being developed, it tapped into this existing outreach mechanism.[26]

The main focus was on scientific discoveries. Tech spin-offs are welcome, but not really planned for.[28]

Anecdotally, the high profile nature of the LHC has resulted in huge impacts in the number of student pursuing particle physics.[26]

There are a large number of students working on each of the six experiments.

Success Factors/Lessons learned

While the scientific impacts of the Large Hadron Collider remain to be seen, construction of the accelerator and detectors has been a success. The following are a collection of lessons that seem to emerge.

1) Sense of community; from the beginning discussions of the LHC, through to the present, a sense of community has been a key factor for the success of the initiative.
2) Peer pressure; there we no legal restrictions keeping collaborations intact. Instead, there was a common sense of purpose and the pressure of carrying your own weight.

3) CERN as a key enabler; The speed and efficiency with which this initiative took shape was dependent on CERN.
4) International collaboration; essential to all aspects of the initiative.

2.5 Long Term Ecological Research Network

The Long Term Ecological Network (LTER) is a collaborative effort investigating ecological processes over long temporal and broad spatial scales. The goals of LTER are to understand a diverse array of ecosystems at multiple spatial and temporal scales; create general knowledge through long-term, interdisciplinary research, synthesis of information, and development of theory; inform the LTER and broader scientific community by creating well designed and well documented databases; create a legacy of well-designed and documented long-term observations, experiments, and archives of samples and specimens for future generations; promote training, teaching, and learning about long-term ecological research and the Earth's ecosystems, and to educate a new generation of scientists; and reach out to the broader scientific community, natural resource managers, policymakers, and the general public by providing decision support, information, recommendations and the knowledge and capability to address complex environmental challenges.

Ambition

Ecological research programs in the United States evolved from an emphasis on short term observations in single systems to more collaborative, multi-disciplinary efforts to understand complex problems. However, the ecological community realized that the current funding mechanisms forced researchers to focus on short-term questions.[31] The Long Term Ecological Research Network (LTER) was the first funding program to focus explicitly on long-term, large-scale ecological phenomena.[32]

Plausibility

LTER developed through a bottom-up demand from the ecological sciences community. A series of workshops were held in the late 1970's which led to a proposal to the US National Science Foundation (NSF) to start the LTER program. At the time, typical NSF grants were three years in duration. To enable long-term questions, LTER was designed to award 6 year grants.[26] Progress would be monitored by site visits every three years, with sites renewed on a competitive basis. If a site didn't meet expectations at the end of funding cycle, it would be placed on probation for 2 years, after which it could re-compete for a 4 year award.

The original intent of LTER was to fund individual research sites to collect long-term data in five core areas:

1. Pattern and control of primary production;

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[31] Interview 4/7/2010

[32] Integrative Science for Society and Environment: A Strategic Research Initiative
(http://www.lternet.edu/decadalplan/)

2. Spatial and temporal distribution of populations selected to represent trophic structure;

3. Pattern and control of organic matter accumulation in surface layers and sediments;

4. Patterns of inorganic inputs and movements of nutrients through soils, groundwater and surface waters;

5. Patterns and frequency of site disturbances.[33]

In 1980, LTER was launched with the funding of six sites, with expansion to occur based on available funds.[34] The sites were selected through a peer review process managed by the NSF, essentially looking at the quality of the proposed science and people. The initiative progressed in this manner in the subsequent years, slowly adding sites to the network.

During an external review of the initiative in 1993, LTER was challenged to broaden its "scope, scale, and integrative power…through collaborative interactions among sites and researchers." A subsequent external review in 2002 acknowledged the successful research efforts being done at the LTER sites, but reiterated the call for the now twenty-four LTER sites to collaborate and challenged the community to engage in "synthesis science."[35]

The new vision of LTER as a coordinated network was, and continues to be, challenging to implement. While individual sites were reviewed every 6 years, review criteria historically did not reward collaboration or integration. Sites were reviewed on the merit of what was being done at their sites only. In addition, NSF never insisted on a common data collection, analysis, and informatics structure for the LTER sites. Thus, each site was allowed to design projects and collect data in their own way, resulting in idiosyncrasies.[29] Finally, there were no portfolio balancing decisions in deciding which sites to fund; they were chosen based strictly on individually merit. Consequently, there were no sites in the significant ecosystems, limiting the types of synthesis questions that could be addressed.

To address these problems and others, the 2002 external review panel challenged the LTER community and NSF to co-develop a strategic plan for the next decade of LTER. Since then, NSF has modified the criteria it uses to review LTER to reward sites that participate in science larger than their site and incorporate informatics activities.[26] In addition, biodiversity has been added to the core areas already being measured by LTER sites and attempts have been made to include the social sciences.[36] Finally, the LTER Network Office has been tasked with coordinating cyberinfrastructure, establishing data sharing policies, providing standards, and other activities to facilitate development of a functioning collaboratory.[26]

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[33] http://www.lternet.edu/overview/

[34] Interview of 3/26/2010

[35] Long Term Ecological Research Program Twenty-Year Review, National Science Foundation (2002)

[36] Interview 3/30/2010

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Structure

LTER sites are funded through a grant mechanism managed by the NSF. A Program Officer at NSF is charged with managing the initiative. Awards are made through a competitive peer review process for durations of 6 years. There are currently twenty-six LTER sites ranging from "Alaska to Antarctica and from the Caribbean to French Polynesia and including agricultural lands, alpine tundra, barrier islands, coastal lagoons, cold and hot deserts, coral reefs, estuaries, forests, freshwater wetlands, grasslands, kelp forests, lakes, open ocean, savannas, streams, and urban landscapes."[28] Each site is reviewed every three years, with continuation of funding predicated on milestones achieved and progress toward goals of the project. From its original budget of approximately $600,000 USD per year, LTER is now operating with a budget of approximately $32 million USD per year, with one-third of the money comes from the Bio Directorate and the rest coming from other Directorates at NSF.[26]

LTER sites are allowed to organize as they see fit. Likewise, the structure of the LTER community is decidedly bottom-up. Previously, a committee of the whole (26 people) made decisions at a once a year meeting. Now, there is a more formal structure with an elected Chair, Science Council, Coordinating Committee, and Executive Board. "The Network is governed by an elected Chair and an Executive Board comprised of nine rotating site representatives and one member selected to provide expertise on information management. Eight Standing Commmittees (Climate, Education, Graduate Students, Information Management, International, Network Information System, Publications, and Social Science) support and inform the governance process. The Science Council, with a representative from each site, establishes the scientific direction and vision of the LTER Network. The Science Council reserves ultimate authority for decisions affecting the Network, The Network research agenda is supported by a coordinated program of information management that involves data managers from each site, common metadata standards, and a centralized information architecture that provides access to site data."[28]

A LTER Network Office, separately competed and funded by NSF, was established in 1983 with the original task of managing travel money for the LTER community. It used to be co-located with the Chair, but that is no longer the case. The Network Office has evolved to support the LTER Network through a number of activities and reports to the Executive Board, NSF, and it's home institution. It has some autonomy, but is held to goals and milestones. One of the main tasks is to coordinate cyberinfrastructure, establish data sharing policies, provide standards, etc. In addition, the Network Office is implementing a strategic communication plan to be run out of the Network Office. This is in response to internal and external communication challenges faced by the LTER sites. For example, it's unclear how many people are actually in the network. Also, the sites are overextended and answering to too many constituencies. There will now be a Public Information Officer at the Network Office.[31]

Approximately thirty countries (including the US) are members of International LTER (ILTER). The ILTER was conceived by personalities within LTER at NSF who dedicated effort to talking with countries about setting up their own LTER programs. The main rationale for this global LTER community was the realization that ecosystem problems don't respect political boundaries.

Integrations

As described above, an integrated LTER collaboratory was not the original intent of the initiative. However, the ability to address pressing ecological questions increasingly dictated a coordinated effort, necessitating change. This has put LTER at a disadvantage and it has had to do a lot of catching up. For one, there has been a fundamental struggle between the rights of the individual research sites and the will of the community.[29] Also, because of the individual nature of the original LTER sites, data was often not collected or archived in ways that were easily accessible or useable by other researchers.

In response to these challenges, a bottom-up planning effort was initiated in 2007 to figure out how to function as an integrated network. This led to significant changes. In 2007, LTER established an Integrative Science for Society and Environment (ISSE) framework that is intended to be used when planning research. ISSE is meant to shift sites from just measuring 5 core areas to going beyond ecological studies to connect with social sciences. While the LTER community is still fairly autonomous from NSF, there have been informal efforts toward standardization. Some of that has been led by the Network Office, which is charged with logistics and coordination of the LTER network (e.g., planning annual meetings), standardization of data management and archiving, and encouraging synthesis through funding streams.[31]

Impact

The long-term nature and survival of LTER, represents recognition that the focus of science can be changed to match questions that are important.

Individually, the LTER sites have had tremendous scientific impacts. In addition, because they often involve team members from management agencies (e.g., the Forest Service), LTER sites have had impacts on local policy, as well as international policy (e.g., Antarctic LTER site Palmer Station is located on the most rapidly warming site on Earth).

Economically, LTER sites have been successful in leveraging their core funding from NSF, with an average leverage of 3 to 10 times the initial investment.

Because of the long-term focus of LTER sites, there has been an added emphasis on training the next generation of ecological researchers. In addition to training opportunities for undergraduate and graduate students, LTER created "Schoolyard LTER", aimed at providing educational outreach to K-12.

Success Factors/Lessons learned

The Long Term Ecological Research Network has been in existence since 1980. During this time, it has weathered changes in the number of sites involved, evolving complexities of research questions that can be addressed, and a shift from distributed individual sites to a true collaborative network. These changes have not been easy and some are still ongoing. The following are a collection of lessons that seem to emerge.
1) Information management architecture should be in place prior to data collection;
2) Provide tools and incentives for collaboration; it is often not enough to hope that collaborations will take place.

3) Importance of timely external evaluation. Ten years between external guidance is too long. Too many things can drift too far to allow for quick solutions.
4) Balance individual creativity with goals of the program; For LTER, this balance skewed too far toward individual creativity. The result has been an uphill battle to correct this drift.
5) Establish clear research questions and direction that unite the community from the beginning.

2.6 Strategic Computing Initiative

The U.S. government's Strategic Computing Initiative funded research into advanced computer hardware and artificial intelligence from 1983 to 1993. The initiative was was funded by the Defense Advanced Research Projects Agency (DARPA), a research organization of the United States Department of Defense, and designed to support all the various projects that were required to develop machine intelligence in a ten year time frame, from chip design and manufacture, computer architecture to artificial intelligence software.

Ambition

The Strategic Computing initative (SC) focused on a number of different areas: parallel computer architectures, AI applications (e.g., Natural Language Processing) that were deemed particularly ripe by those within DARPA. While each of the areas showed promise, the goal of SC was to integrate them into a program demonstrating machine intelligence. In addition to specific technology areas, there was a focus on infrastructure support, getting state-of-the-art computers into the community. Other more mission oriented areas of DARPA were brought in to transition research accomplishments to products. Autonomous vehicles, the Pilot Associate, and Navy system were all targeted technologies.[37] SC was envisioned to be a 10 year $1 billion program [38]

Plausibility

It was the strong belief of some within DARPA that the component areas of research had reached a point where, with substantial effort, funding, coordination, major advances in machine intelligence could be realized. Planning and implementation of SC was driven by a small number of individuals at DARPA who developed a conceptual vision of SC. However, specific plans for how to implement SC and integrate the different aspects of the plan were not developed. In addition, because the effort was rooted in basic science, there were only conceptual milestones. Consequently, SC meant different things to different people, resulting in multiple objectives and reasons for supporting depending on your particular frame of reference.[37]

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[37] Interview on 1/1/2010

[38] Roland and Shiman, Strategic Computing: DARPA and the Quest for Machine Intelligence, 1983-1993

Structure

SC was not terribly structured. For most of its existence, it was run out of a specific office at DARPA. However, various parts of the initiative were embedded throughout DARPA. The idea was to utilize strong Program Mangers at DARPA, whose role it was to know the research community in enough detail to propose project areas. Through a competitive process, grants or contracts (for industry) would be awarded in those project areas. Thus, the real vision for SC resided in the Program Managers.

Integrations

As a major goal of SC was the transfer of technology, the initiative provided funds to researchers in industry and academia. It's been estimated that approximately 50% of all SC funding went to industry.

Impact

The following impacts were mentioned in the interviews:

1) Transition of knowledge from labs to industry
2) Jumpstarted the internet
3) Established parallel computing
4) Moved AI speech understanding, image understanding, and expert systems forward

Success Factors/Lessons learned

The following are a collection of lessons that seem to emerge.
1) Clearly define goals; SC meant different things to different people.
2) Sense of community; when asked what the main driver was for researchers to participate in SC, the answer was "money" Without a sense of community, each part of SC was isolated, limiting integration.

3. Summary of lessons learned

In addition to analyzing each initiative, an attempt was made to identify those principles that appear to transcend specific initiatives. Indeed, despite the fact that the six initiatives came from diverse fields of science, vary in complexity, and cover the last thirty years, similar themes appeared in multiple initiatives. These cross-cutting lessons may be important for the design and implementation of an array of programs and are summarized below.

1. Engage the research community in shaping the program to achieve a sense of shared ownership and identity.

Generating and maintaining a community of researchers is essential to the longevity of an initiative. This community will serve to drive the scientific accomplishments of the flagship initiative, developing and implementing innovative research activities. In addition, they will serve as ambassadors for the flagship initiative, sharing their accomplishments and enthusiasm with colleagues and society. Both will impact the success of the initiative. Indeed, key success factors that were indicated for the Human Genome Project and Large Hadron Collider were the unity of purpose displayed by the involved community of researchers and passion for the initiatives. Another example is the Long-Term Ecological Research Network, which has benefited greatly from a committed community of ecological researchers for nearly thirty years.

Interestingly, all three initiatives were developed through extensive engagements with their respective research communities. Planning and feasibility workshops were used to develop reasonable research directions and expectations. In addition, all three initiatives involved members of the research community in the formal management structure, helping to maintain this connection. Alternatively, Strategic Computing developed from a more directed approach with minimal input from the research community in the design and implementation of the initiative. The result was a research community that viewed the initiative primarily as a source of financial support, making the initiative vulnerable. Consequently, changes in management structures and key personnel resulted in shifting initiative goals and identities. While individual components of the initiative were quite successful, Strategic Computing as a program eventually drifted into the background.

An additional mechanism for involving the research community in shaping the program is through periodic external evaluation and guidance. External evaluation and guidance provides an unbiased check of progress, while identifying challenges and opportunities that may not be apparent to those on the "inside" of the initiative. Both Assembling the Tree of Life and the Long-Term Ecological Research Network suffered from a lack of such external perspective for too long. Now each is undergoing a painful process of correction. For several initiatives, the extension of the collaboration to an international level was an important success factor.

2. Clearly define the unifying goal. Evaluate progress periodically.

Clearly defined goals for flagship initiatives are essential to ensure that decisions are made strategically and progress can be assessed. Equally important to establishing

goals and milestones is implementing a process for evaluation progress toward them. These evaluations should be used to chart progress, identify challenges and point to opportunities for the initiative.

The Human Genome Project utilized a series of 5-year strategic plans to reach its goals and was constantly evaluating progress. When new DNA sequencing technologies entered the marketplace, the Human Genome Project re-evaluated their strategic plan and adjusted milestones accordingly. Assembling the Tree of Life and the Long Term Ecological Research Network were implemented with goals in mind, but no milestones or roadmaps with which to chart progress. In addition, both failed to utilize external evaluation until significant drift from the goals of the initiative had taken place. It is conceivable that more frequent use of evaluation could have saved both of these initiatives significant pain. Strategic Computing had a formal strategic plan when implemented that addressed specific long-term technology goals. In addition, the plan had described areas of research that needed to be advanced in order to achieve these technologies. However, it was never described how the areas of research would be integrated to achieve the technologies or alternative plans if research areas did not advance as expected. Both of these omissions haunted Strategic Computing during its existence.

3. Short-term individual research agendas must be aligned towards the overall long-term goal in a flexible way.

While involvement of the research community in shaping a long-term initiative is essential, it may not be sufficient to achieve programmatic goals. The bulk of the examples analyzed in this report engaged researchers through the use of grant funding. While the length of the awards differed for each initiative, in general they were short in comparison to the length of the initiative. On the one hand, this continually brings fresh ideas to the initiative and challenges project to remain competitive. On the other hand, short-term projects tend to focus on near-term objectives. Consequently, there is a balance that must be achieved between the near-term objectives of the funded research projects and the long-term goals of the initiative. If weighed too heavily toward individual researcher goals, the initiative may resemble a collection of projects rather than a cohesive program. As both Assembling the Tree of Life and the Long-Term Ecological Research Network learned, without some guidance from the top, drift can occur that may not be easy to correct. However, there are also risks associated with weighing too heavily toward programmatic goals. Without the intellectual freedom to pursue interesting questions, researchers may abandon the initiative.

Defining an appropriate balance is, of course, challenging. It relies on understanding the cultures of the specific research and management communities involved. In addition, the balance may shift depending on multiple variables, such as the "age" of the initiative, or in response to evaluations or external stimuli. Assembling the Tree of Life and the Long-Term Ecological Research Network underwent shifts of balance in response to a realization that they had drifted away from programmatic goals. The Human Genome Project made a strategic decision to shift from a distributed network of DNA sequencing centers to a focused, scaled-up approach in response to competition. In each case, the shift was accomplished through use of a mechanism that matched the cultures of the research and management communities.

4. The structure of the initiative must reflect the needs and characteristics of both the Flagship goal and of the participating entities.

While an identifiable leader is important, the complexity of flagship-like initiatives often demands a more comprehensive structure. This becomes especially important when integration between disparate parts of the initiative is required. From analysis of the six flagship-like initiatives, there was no apparent "best" structure that could be defined. They ranged from a minimal structure for Assembling the Tree of Life, where individual grants are awarded by a Program Officer at the US National Science Foundation, to a structure for the Large Hadron Collider that involves integration of input from CERN, the individual experiments, and the dozens of countries involved. Other structures showed different combinations of working groups, executive boards, and steering committees to accomplish decision making. Rather than a specific structure, what seemed to be important was the clarity with which one existed. As a counter example, Strategic Computing was implemented without a plan for integrating research being performed in specific technical areas (e.g. artificial intelligence and computer architecture). In addition, management of the initiative was distributed throughout the funding agency with formal plans for coordination. Consequently, integration was sporadic.

A significant competitive element as a part of the structure is a recurring success requisite. Flagships might even be conceived as 'advanced funding agencies' rewarding the best ideas.

5. Creation of an environment conducive to integration is indispensable in enabling efficient collaboration.

The success of flagship-like initiatives is impacted by their ability to integrate the many different partners involved, such that the whole of the initiative is greater than the sum of its parts. This is complicated by the involvement of different research cultures, disciplines, expectations, and personalities. If not done properly, the result is a collection of successful individual projects at best, and a fractured and unproductive program at worst. While integration may occur in the absence of specific action, for most initiatives it may be necessary to put resources and energy into creating an environment for integration. In response to the realization that integration between funded research projects was not occurring to sufficient levels, both Assembling the Tree of Life and the Long-Term Ecological Research Network have recently modified the way they select research projects to reward those with goals of integration or synergy. Previously, projects were selected based primarily on their scientific merit. Integration can also be assisted by providing conduits for interaction. For example, an extensive IT infrastructure was put in place for the Large Hadron Collider experiments. Each of the experiments involves hundreds or thousands of researchers distributed around the world. The common thread that connects the researchers is data, which are centrally collected and distributed throughout the network. Another mechanism to stimulate integration is the use of working groups that cross boundaries (e.g., interdisciplinary or academia/industry). The Human Genome Project used this mechanism to coordinate activities being managed by the US Department of Energy, US National Institutes of Health, and Wellcome Trust-funded Sanger Centre. An additional mechanism is to present a challenge that necessitates the collaboration of disparate groups of scientists. This approach was utilized by DARPA for both the Grand Challenge and Strategic Computing. In both cases, specific research and

development goals were presented in ways that encouraged academic and industry collaboration to assist in the transfer of research results to the marketplace.

6. Implement data management plan prior to data acquisition

While this was touched on briefly in (6) above, the importance of data management cannot be overemphasized. Flagship-like initiatives involve diverse researchers generating data that will be used to move toward established goals. Achieving those goals demands integrating data between various research groups. Thus, data needs to be of sufficiently high quality, consistent, and available. Assembling the Tree of Life is now struggling with how to integrate phylogenetic data from its taxa-specific groups to generate a unified tree of life. The Long-Term Ecological Research Network was originally envisioned as distinct research nodes and left to collect data as they saw fit. As the sophistication of ecological research questions evolved to be more collaborative, the Network was not able to easily adapt due to a failure to implement a Network data management plan.

7. Strong leadership is fundamental

Planning, implementing, and managing initiatives of the scale envisioned for the FET-Flagships is challenging. The person or people charged with these tasks must have sufficient vision to understand the complexities involved in a big-budget, long-term initiative. In addition they must have credibility with the research community, be skilled communicators, and savvy in dealing with the political and societal attention drawn to such initiatives. The Human Genome Project and construction of the Large Hadron Collider were helped enormously by their respective leadership. Importantly, leadership for these two projects was relatively stable and when change occurred it was managed as seamlessly as possible. This helped ensure a consistent vision for the initiative. In contrast, Strategic Computing underwent numerous changes in leadership. Each change brought a different perspective and identity to the initiative, such that a firm definition for the initiative was challenging. Assembling the Tree of Life has also struggled with consistent leadership, due in part to the use of short-term rotator assignments within the US National Science Foundation to fill the leadership role.

8) The formulation of an explicit initial hypothesis is not always necessary in large-scale initiatives that are productive and successful.

Neither construction of the Large Hadron Collider nor the Human Genome Project was hypothesis driven. Instead, they were essentially large investments designed to enable a plethora of research questions. As such, they will likely have impacts for decades to come. DARPA's Grand Challenge was also not hypothesis-driven. Instead, it focused on bringing new ideas into autonomous vehicle design and demonstrating a specific technology capability that would move a whole field of research forward. Its innovative use of a prize mechanism resulted in significant advances at a minimum of cost to the funding agency.

4. Consultation (interviews summary)

In the following, we summarize the main results from the interviews with experts during the consultation task. The interviews focused on the following three aspects:

-   The general flagship concept
-   The subject areas for a flagship
-   The management of a flagship

Nearly all interviews were phone interviews and for all but two experts the following questionnaire was used:

-   Concept
    -   What is your opinion on the aspect of "goal orientation "?
    -   What do you think about the ambition to create breakthroughs?
    -   How would you relate the different impacts (scientific, technological, economic?)
    -   What should be the time-to-impact?
-   Subject area
    -   Which areas are particularly promising?
    -   How important is the integration of different disciplines?
-   Management
    -   How should such an initiative be managed?
    -   Centre or projects?
    -   How important is leadership?
    -   What do you think about costs (of 100m/year for 10 years)?

The experts interviewed for this task were from the following organizations:

-   Chalmers University, Sweden
-   College de France
-   EPFL, Switzerland
-   Linköping University, Sweden
-   National ICT Australia
-   Open University, United Kingdom
-   Technical University of Vienna, Austria
-   TU Munich, Germany
-   University of Bielefeld, Germany
-   University of Antwerp, The Netherlands

The following is the summary of these interviews.

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On the general flagship concept

- Mission and goal orientation

    o The "mission" character of flagships is considered very important. Goals are considered important for alignment (integration), for interdisciplinary integration, but also for funding agencies, politicians and the broad public: "It needs to be something where the media around the world would talk about it and it can be explained to mom and dad" and "my grandmother should be able to understand it." But some experts would accept that the goal is understandable at the level of students.

    o Although applicability of the results at some stage is important, experts warn that goals must not be oversimplified, too narrow, or too short-ranged. The objective should be such that it cannot be achieved by any other means.

    o Finding the goals is considered very difficult. For some, the ISTAG topics do not yet have the desired level of goal-orientation such as "putting a man on the moon". They are very broad and also overlapping. It is also necessary to check what is available by other means (e.g. non-ICT) to realize the goal. Something that is purely technology-driven should be avoided.

    o A few experts think that it is too difficult to formulate a project for such a long time-scale during which key scientists may also change.

    o Some experts say that such a large initiative needs to be a success. It must not fail, as it could do serious damage to the whole field, in fact to science as a whole.

    o An important goal is to overcome fragmentation through single projects; flagships can create focal points for the whole community.

- Ambition to create breakthroughs

    o ICT still has lot of potential for breakthroughs and it is necessary to aim at breakthroughs; although they cannot be planned. Some of the five topics are in areas were breakthroughs have been sought for decades and great leaps are more likely than real breakthroughs.

    o A few experts pointed out that fundamental research breakthroughs may not be optimally supported via large-scale top-down directed mechanisms and this should be taken into account in the flagship design. Particularly in fundamental research areas, the individual scientist is important and breakthroughs often come from small teams, not from large initiatives.

    o There should be room for creative thinking as bright ideas may not flourish well in a large organisational framework.

- Relation between different impacts (scientific, technological, economic, social)
    o Experts have different views on the desirable type of impact of flagships.
    o For most experts, there is a strong emphasis on putting science first. Impacts on technology, economy and society will follow.

- o Others argue that it is important to include social and economic objectives, although they should not drive the flagship.
- o It also depends on the topic, i.e. on the kind of flagship.

- Desirable time-to-impact
  - o Expert views vary also because "impact" means different things to different experts. Generally it also depends on the topic.
  - o For some, impacts should be visible already after 1-2 years within science and after four years getting closer to the societal objectives should be visible (e.g. robot sub-systems such as sensors etc. could be ready). In ICT quick technology transfer is possible. For others, the impact should be in 5 to 10 years. For some, impact can be in 20 years, with some spin off results after 5 years, or just in 20 years.
  - o Generally, many argue for a 10-years perspective but request some impact before that.

- Integration of different disciplines
  - o There is very high agreement between experts that the integration of and with different scientific disciplines is very important or even essential. Some experts argue that breakthroughs arise at the intersection of disciplines other argue that ICT is approaching maturity and requires inspiration from other fields.
  - o A danger lies in the fact that ICT can contribute to other fields without progress in ICT itself. But also technologies may require mixing.

On management

- How should such an initiative be managed?
  - o Management is the main challenge. Risk assessment is very difficult.
  - o Some experts emphasize that flagship management needs to analyse the field and react in a flexible way, e.g. by creating calls. Calls are good to see what ideas people have. A flagship basically must be an advanced funding agency.
  - o Several experts suggest that flagships should be managed by a small multidisciplinary team of top experts (2 – 20 people). They must break down high-level goals to smaller aims and topics. This requires managing scientists, not just administrators. Large-scale coordination is required as nobody has a perfect overview.
  - o Management may be different for different topics. Scientific aspects are important from the start, not just management and organisational issues.
  - o Some experts point out that a mission-driven project includes non-scientific activities, technical work, demonstration etc. which requires more coordination than purely scientific work.
  - o Flagships cannot be micro-managed, they need a large amount of freedom and control.
  - o A research cannot agenda cannot be written for 10 years. Control functions are necessary, annually and every 3-4 years.

- o It is important to avoid mere collections of projects without interaction. Large projects are not enough. Also, cooperation between projects is necessary.
- o Creating a new organisation may help. Mere coordination is not enough.
- o In a large flagship, nearly everyone will be involved. This makes evaluation and review difficult.

- Centre versus projects
  - o Most experts believe that some form of centre (or "nucleus") is useful for achieving integration. But there is no uniform view on this aspect and for some it depends on the topic.
  - o If centres are created, existing top places in Europe must be included.
  - o Difficulties when creating centres are that opting for a single place is difficult politically, also for some areas, it is not clear where a centre should be located. Experts may not be flexible enough to move to a new centre.
  - o Teams can be formed to fill a centre, but it is dangerous to first create a centre and then "fill" it. A stepwise approach could start with integrating projects as a first step.

- Leadership
  - o There is very strong agreement that successful flagships require strong leadership. Leadership should concern the content, not just management. Leaders act as the glue binding people together. Interdisciplinary research suggests that the scientific basis cannot rely on 1 person, even if high-level. So a steering board etc. is required who jointly develop scientific visions and directions.
  - o Leaders are people in which you have confidence. The leaders should be willing to work for others and trust in researchers.
  - o Only few experts regard scientific and practical leadership as different things.

- Spending 100m/year for 10 years

  - o Some experts suggest starting small and focusing on training first. It is advisable to also invest in people as the flagships will attract a considerable fraction of the researchers in a field.
  - o Similarly, many experts raise the question of sustainability and what should happen to the researchers after the end of the funding period. This needs to be addressed early.
  - o Some experts are concerned that spending large budgets on a single topic is not an easy task and an approach using smaller budget chunks may be required. For example, one should think in terms of clusters of IPs, spending money in chunks of 10-20 million.
  - o Experts also point out that significant budget may be required for research infrastructure for some flagship topics. This could benefit the whole research community in a field.