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BIG SCIENCE PROJECTS - WHAT IS IT THAT MAKES SOME A SUCCESS AND OTHERS TO FAIL?

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Abstract

This paper evaluates the driving forces behind big science projects. The paper should be useful to organizations and individuals participating in big science projects or wanting to make business with such projects.

INTRODUCTION

As a participant in many big science projects over the years I developed a checklist that I use to evaluate the probability that a project will be successful. The checklist is simply to help evaluate the conditions for success, and most importantly to find ways to improve those conditions:

1. Facility must be a priority of the science community!
2. Funding agency commitments and strong host role
3. Collaboration leadership enables success of others
4. Establish realistic goals – “Experience over hope”
5. Credibility through openness with transparency
6. Collective ownership of problems & solutions
7. Populate the organization with experience

It is my view that these factors can be used to evaluate any big science project and ideally this evaluation would be done in collaboration among the stakeholders so there is a share view of the both the opportunities an challenges. I provide some examples from projects where I have played a management or leadership role, providing me with some insight into the factors that enabled the projects to succeed, and in one case fail. I end with a short summary of the European Spallation Source project that I currently head as CEO and Director General.

COMPACT IGNITION TOKAMAK

The Compact Ignition Tokamak (CIT) located at the Princeton Plasma Physics Laboratory (PPPL) was a $300M project with a reasonable contingency (20%) planned within the budget, circa 1980’s. The project was started and later cancelled as there were a number of conditions for success that were not in place. The project lacked the right level of priority within the US fusion community. While CIT was argued to be the next step in proving the scientific feasibility of controlled fusion with a tokamak, the project was started in an era when US funding for fusion was declining. This situation created intense competition for funding among the US fusion community and strong opposition to redirecting funds from existing operating facilities into the construction of a new facility. CIT was not a clear priority of the US fusion science community, there was not a strong commitment of the funding agency to overcome this lack of consensus, and finally the host laboratory, PPPL, was also somewhat conflicted. At PPPL there was competition for support between the existing operating facility, the Tokamak Fusion Test Reactor (TFTR), and CIT. Compounding the problem was some delays in making technical decisions at the project level.

The participants in any success or failure will take away lessons learned from the experience. The US fusion community concluded that CIT was not good enough and that a facility with higher performance would be a better choice. The failure of CIT was followed by the initiation of the Burning Plasma Experiment (PBX), promising not only ignition but also sustained burning plasma operations at three times the project cost of CIT. The funding agency did not have the ability to support this facility either and it was subsequently cancelled. We should have built CIT.

RELATIVISTIC HEAVY ION COLLIDER

The Relativistic Heavy Ion Collider (RHIC) at Brookhaven National Laboratory (BNL) was a $600M project with arguably inadequate contingency (10-15%), circa 1990’s. It was established as a priority in the US nuclear physics long-term plans and this priority was sustained during an era when the annual budgets for nuclear physics were relatively flat. The project was given priority in the US funding agency within annual funding limits and it was the highest priority at the host laboratory, with generous resources and support provided by BNL. The project hired key experienced individuals and engaged the US nuclear physics community leadership. The initial cost and schedule goals were somewhat unrealistic, but the evolved into more achievable goals as the project progressed. The older and more experienced people mentored the younger and quite energetic and enthusiastic participants. There were many problems to work through given the very tight budgets and technical challenges, e.g., the partnership with industry to deliver almost a thousand superconducting accelerator magnets. These challenges were addressed without adverse scrutiny or criticism as most of the political focus was on on the Superconducting Super Collider (SSC) project. The RHIC team was able to solve problems in the shadow of SSC. The bottom line is that the RHIC project was successfully completed and continues to deliver on its scientific promise.

Different people working on the same project learned dissimilar lessons on the role and importance of an adequate contingency budget. I learned that a project

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needs a healthy contingency budget so that problems can be solved in the most transparent and efficient manner. Others learned that it is possible to complete a project without an adequate contingency budget. The US Department of Energy Inspector General concluded that we used another 20% of the total cost through indirect subsides etc. The common lesson learned is that some level of contingency budget is needed and clear understanding of risk factors and risk mitigation and treatment plans. Contingency can be used to treat many different risks.

US CONTRIBUTION TO THE LHC CONSTRUCTION

The US Large Hadron Collider (USLHC) construction project was budgeted at $530M with substantial contingency (20 - 40% depending on the subproject), circa late 1990's – early 2000's. The US LHC effort was unquestionably the highest priority within the US high-energy physics community. The US investment in LHC was established following the cancellation of the SSC and the US community re-establishing priorities with full engagement in particle physics at the energy frontier at the top of the list. The annual funding profile was established by the CERN-US international agreement and there was priority at the host laboratories and universities. The support framework was in place and the experienced people took leadership roles. This highly experienced and motivated leadership team strove to maximize the value of US deliverables to the LHC program within a fixed total budget. The original goals for deliverables were realistic the relatively high level of the initial contingency permitted the eventual inclusion of additional deliverables. The international effort to realize the LHC was exemplary in many ways, in particular the willingness of all partners to rally together to solve problems and overcome challenges quickly.

The US funding agencies were motivated to succeed for many reasons, including the LHC scientific mission and the need to establish credibility on big science projects given the SSC debacle. The US LHC contingency story can be reduced to one project principle – under promise and over deliver. The establishment of realistic goals combined with strong collective leadership provided a solid foundation for success.

IceCube

This construction of the IceCube Neutrino Observatory project was budget at about $300M and had a reasonable contingency of 22%, circa early 2000's. National studies confirmed the priority assuming funding from a new initiative on big science project funding by the US National Science Foundation (NSF). The funding was provided from the NSF Major Research Equipment and Facility Construction (MREFC) account providing a stable annual funding plan based on the technical schedule requirements. The scientific requirements were set by the international collaboration of particle astrophysicist and the responsibility for delivering the project with this collaboration was with the University of Wisconsin-Madison (UW). The IceCube project was a priority for UW and for support from the NSF's Antarctic program including the South Pole Station. There were unique challenges due to seasonal installation that must be completed within the three month long austral summers. Learning from previous experience there was a strong effort to establish realistic goals and to provide an incentive for adding scope if cost and schedule performance permitted. The scope of the project was eventually increased about 20% relative to the baseline scope through use of some of the contingency budget and additional contributions from partners. Figure 1 shows the history of the IceCube project contingency. The figure notes points during the project when the contingency budget was allocated for major in scope costs or to add scope. The IceCube project benefited from strong collective leadership among the stakeholders.

The lessons learned from the IceCube project experience are many but there are a few that stand out in my mind when planning future big science projects. Invest early in defining the relative roles and responsibilities among the participants, set realistic goals, understand the unique characteristics or challenges of the project (South Pole location and support culture), and emphasize schedule performance to reduce cost and increase scope.

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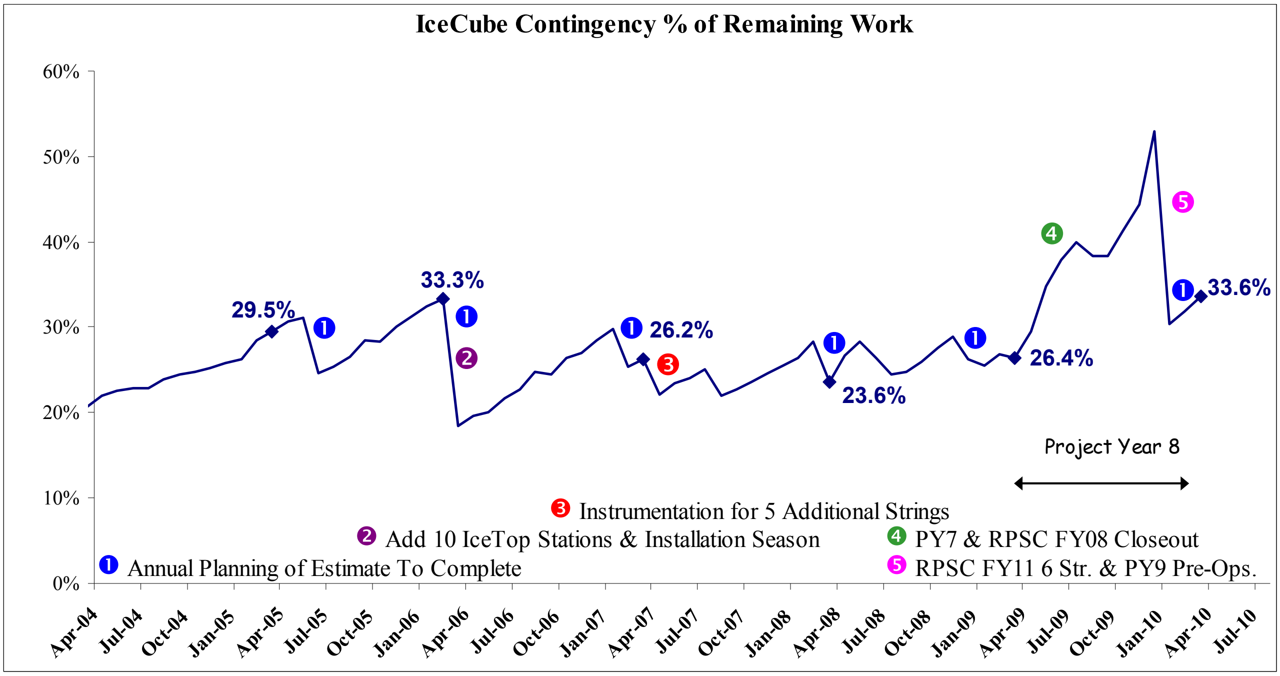

Figure 1: Ice Cube contingency in percentage of remaining work. Note how the contingency enabled additional deliverables.

EUROPEAN SPALLATION SOURCE

ESS is a partnership of 17 European nations committed to the goal of collectively building and operating the world's leading facility for research using neutrons by the second quarter of the 21st century. It will be the world most powerful spallation source with highest flux and real time data acquisition. The host countries, Sweden and Denmark, have established ESS as a priority within their national funding plans and intend to provide new funding added to avoid competition with other national projects. The OECD already in the 90s established the construction of three regional (US, Asia and Europe) MW spallation sources as a top priority for the field and this has since then been re-iterated in national roadmaps in the funding countries and in the European Commission ESFRI (European Strategy Forum on Research Infrastructures) roadmap. Key staff positions are being recruited from all over the world for the project and a comprehensive review of the project is helping to establish a realistic budget and schedule from an initially unconstrained but technically limited scenario. The project will migrate from a publically owned company (host states) to a European Research Infrastructure Consortium (ERIC) that should ensure a strong collective ownership by the ESS ERIC members. The construction funding commitment for ESS was recently secured and a general agreement on principles for sharing responsibilities for operations funding are in place. All of the success factor checklist items are relevant to the ESS and the stakeholders are working together to ensure that the conditions are in place to increase the probability for success of the ESS construction project. There are some unique factors, e.g., the green field organization and site, and common challenges, e.g., the large fraction of in-kind deliverables and number of participating countries. ESS is committed to being open and transparent with our challenges and issues and will work together with our stakeholders so that we share together in our future success.

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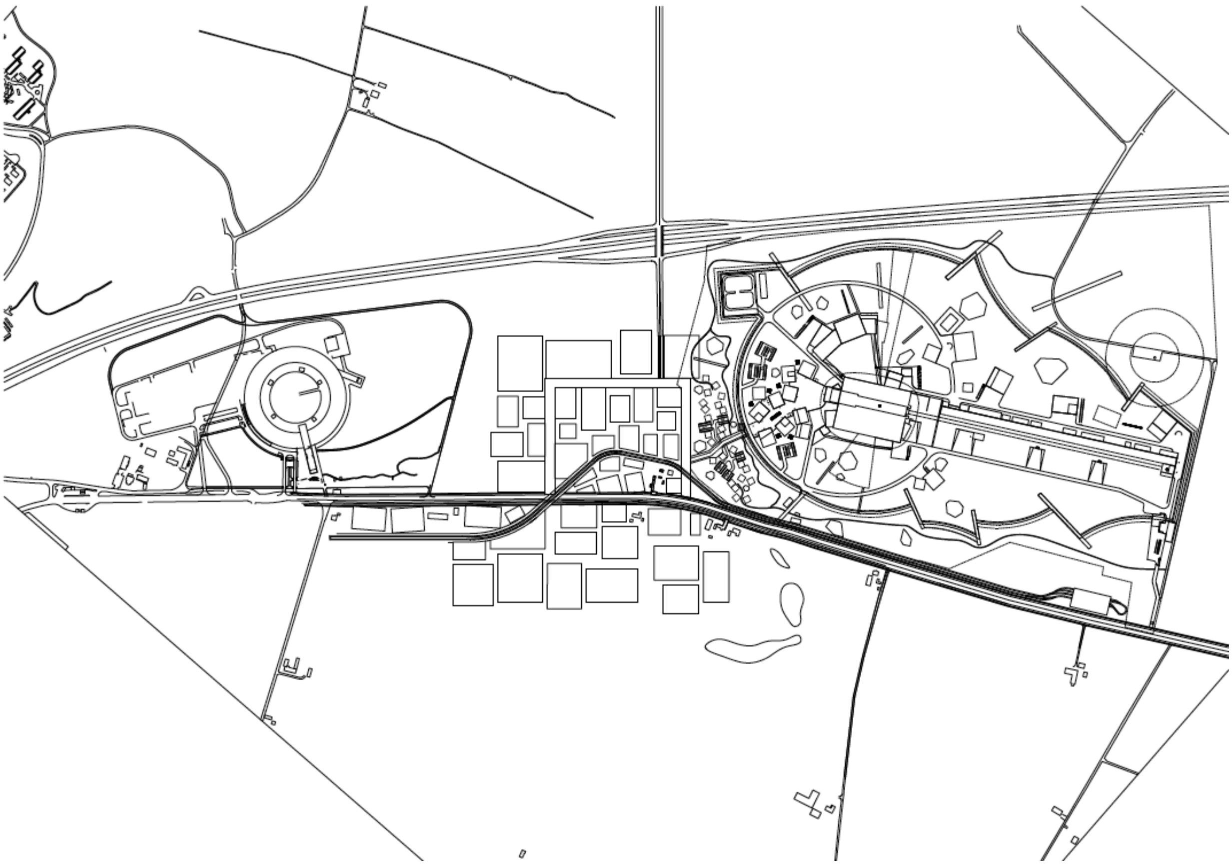

Figure 2: The European Spallation source site with the Swedish national synchrotron light laboratory MAX-IV laboratory to the right and space for a science park to be constructed between the facilities. Additional funding for ESS has enabled the national project to progress in advance of ESS and it will take first light to instruments in 2016.

CONCLUSIONS

To summarize my experience of successful projects I want to come back to my initial success factors. First and foremost, the project (the facility) has to be a priority of the science community! It needs Funding agency commitment, clarity of roles and responsibilities and a strong host role as an equal partner with the funding agencies. It must populate the organization with high quality people – recruit experience. The Project & Collaboration Leadership must make timely decisions and seek consensus whenever possible. It should serve as an umbrella for the team so they can focus on their jobs and should manage expectations and communicate plans and results. The project must search to understand the project – determine characteristics that are common to other large projects and those that are unique. It must establish realistic project goals (experience over hope) and maintain credibility with stakeholders through openness and transparency. Finally, it must seek collective ownership of problems and solutions. Only then can in my experience a big science project succeed.