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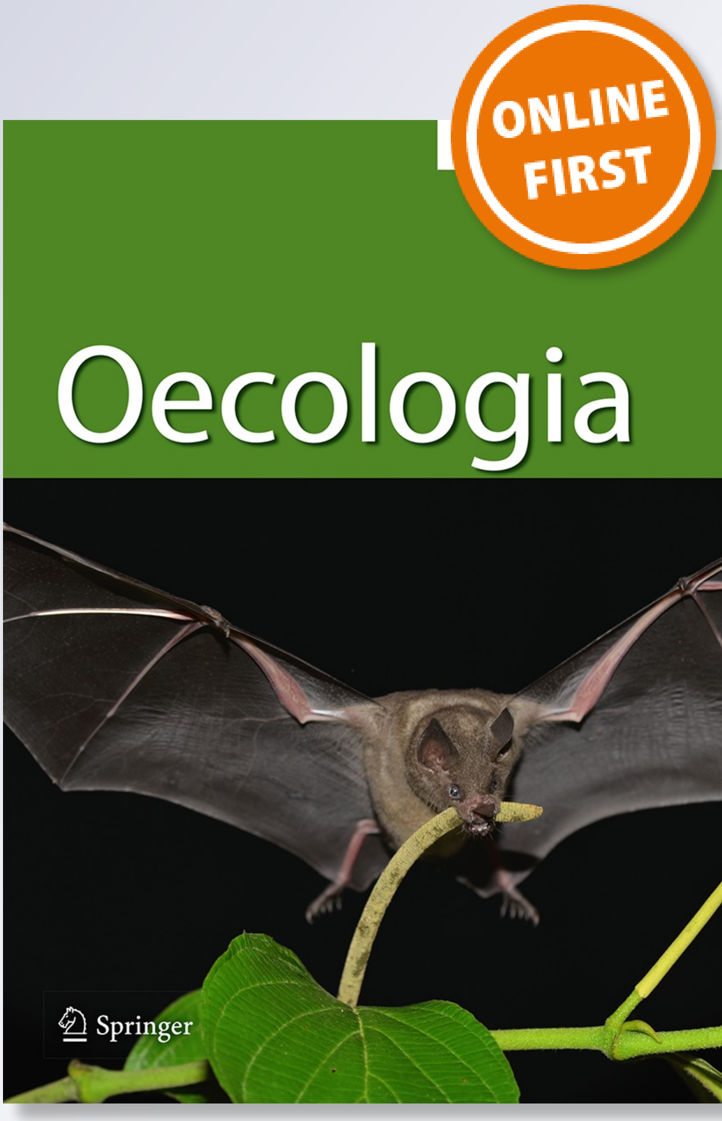

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SPECIAL TOPIC: COORDINATED APPROACHES TO GLOBAL CHANGE RESEARCH

Big questions, big science: meeting the challenges of global ecology

David Schimel · Michael Keller

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Abstract Ecologists are increasingly tackling questions that require significant infrastucture, large experiments, networks of observations, and complex data and computation. Key hypotheses in ecology increasingly require more investment, and larger data sets to be tested than can be collected by a single investigator's or s group of investigator's labs, sustained for longer than a typical grant. Large-scale projects are expensive, so their scientific return on the investment has to justify the opportunity cost-the science foregone because resources were expended on a large project rather than supporting a number of individual projects. In addition, their management must be accountable and efficient in the use of significant resources, requiring the use of formal systems engineering and project management to mitigate risk of failure. Mapping the scientific method into formal project management requires both scientists able to work in the context, and a project implementation team sensitive to the unique requirements of ecology. Sponsoring agencies, under pressure from external and internal forces, experience many pressures that push them towards counterproductive project management but a scientific community aware and experienced in large project science can mitigate these tendencies. For big ecology to result in great science, ecologists must become informed, aware and engaged in the advocacy and governance of large ecological projects.

Keywords Project management · Systems engineering · National Ecological Observatory Network (NEON)

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Introduction and motivation

Ecologists are increasingly concerned with big questions (Schimel et al. 2015b; Kreft and Jetz 2007). Ecologists may seek general theory or transferable management recommendations and require data to extend their knowledge from a small number of detailed case studies to larger regions. Alternately, other ecologists, having discerned patterns in large but sparse data sets, seek confirmation through large experiments, intensive measurements or rigorous sampling (Soranno and Schimel 2014). The challenges of a changing world require observations over time, and detection of changes requires experiments and observations that are dense in time and space. Ecologists interested in the interactions of ecosystems with the earth system need to be able to add up fluxes of carbon, water, energy or other constituents to quantify global budgets. All of these types of advances require adequate, and often very extensive, experimentation and infrastructure. These projects are of extraordinary importance and can lead to breakthroughs, either directly or by providing the *connective tissue* that allows linking separate projects together to produce an unanticipated result. There is an emerging literature in ecology around the conduct of larger programs, but little of it references the disciplines, mechanisms and formalisms required by projects with significant engineering,

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construction, data and computing components (Lindenmayer and Likens 2010; Osmond et al. 2004).

These large science questions generate a common requirement, data, lots of it collected on scales that require resources outside the range available to even the best-funded single principal investigator or group project. Collecting data on these scales requires technology, logistics, collaboration, management and money. Because large-scale projects are expensive, their scientific return on the investment has to justify the opportunity cost–the research foregone because resources were expended on a large project rather than supporting a number of individual projects. This conflict between the payoff from investment in infrastructure and coordinated data collection versus funding small individual projects is faced by all disciplines, and is increasingly being faced by ecologists, a field populated by scientists who really prefer to be in the woods (or another favorite ecosystem) by themselves!

Every aspect of big ecology is challenging. Sponsors of research have to make extraordinary efforts to support large projects. These projects do not fit into normal budget envelopes, and so have to compete with projects from other disciplines or be prioritized relative to smaller research grants. Some agencies and disciplines have regular budget lines for such activities, but even in that case, long-term maintenance and operations funding is required. While some disciplines have long experience with large projects, terrestrial ecology has had limited support from large facilities or coordinated programs. An exception to this comes from the NASA remote sensing programs, which however involved a relative small number of ecologists in the advocacy, design and implementation phases. As a result, there are relatively few ecologists in either the research or the funding agency communities with deep experience in large project management. It is critical to share experience in the design, implementation and application of big science activities, in the context of ecology and to develop a larger community able to participate in the development and management of big ecology.

This essay builds on the authors' experience in big ecology over several decades. We have served in a number of project roles including as members of executive committees for field campaigns, community modeling activities and satellite missions. We have held leadership roles as PI or project coordinator (ACME: Desai et al. 2014, VEMAP: Schimel et al. 2000), Project Scientist (CSMP: Kiehl and Gent 2004, LBA: Keller et al. 2004) and as Chief Scientist (NEON: Keller et al. 2008), and served in other science team roles on significant projects (FIFE: Sellers and Schimel 1993, EOS: Moore et al. 1996). We have also served as peer reviewers, project reviewers and as NRC committee members on large project-related activities. We base this paper on both positive experiences and lessons learned from our mistakes and failures.

Background, terminology and components

Large science projects, when they reach a certain critical size, tend to accumulate technical management apparatus. Ideally a big project is like any other research project—just bigger!—with a well-formulated question and rigorous methods.

Large-scale projects can be big experiments, with clear goals and hypotheses similar to smaller experiments, but requiring substantial engineering and investment. Other projects provide resources that are common elements of research for an entire area of inquiry. The distinction between large-scale experiments and general-purpose infrastructure is a continuum and not a dichotomy, but the management elements are largely common across the continuum. Building an oceanographic research vessel is an example of infrastructure that can serve a wide range of purposes, but must still meet scientific needs. The most obvious terrestrial equivalent to a ship is a field station, which can support research from genomics to biogeochemistry, and stations too must meet the needs of their researchers. Other large projects are built around very specific hypotheses or questions, and have less of an infrastructural character, but are really very large experiments, such as FACE projects (Norby 2005).

Successful project management for science research and infrastructure matches resources with questions, whether they are very general or highly specific; management cannot be an end in itself. Science leadership defines goals, objectives, observables and products, and evaluates whether the observables and products can adequately address the goals and objectives. In a common model, science leadership is implemented through a Principal Investigator, an individual with knowledge and experience of the large-scale project's goals, a Chief Scientist or Project Scientist, who provides science input to day-to-day decisions, and a science team, funded and engaged individuals who provide specific technical expertise and are available to support more specific decisions. A large project may also have staff scientists, responsible for day-to-day design and implementation of protocols, algorithms and other tasks, and advisory groups to provide independent input.

Large science projects usually include two disciplines, systems engineering and project management, less familiar to ecologists and university scientists in general. Systems engineering is an interdisciplinary field that focuses on how to design and manage complex systems. Systems engineering ensures that all dimensions of a project or system are considered, and integrated into a whole. In a science project, systems engineering formalizes the scientific process of ensuring a priori that the data collected are sufficient to test the proposed hypotheses and while it involves a great deal of new terminology and process, it is nearly

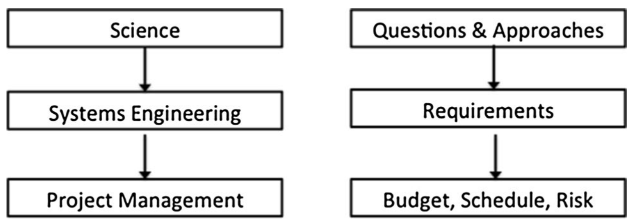

Fig. 1 The idealized elements of translating a scientific question into a project that answers the question. The *left-hand column* shows the flow from science into implementation, while the *right-hand column* shows the steps required. Systems engineering translates the science questions into specific requirements to answer the questions, while project management translates meeting those requirements into cost, schedule, and risk

equivalent to what scientists do for themselves on a smaller scale. Systems engineers also deal with process, the formal translation of questions into requirements that can then guide implementation details, the reviews to ensure that the process remains on track, and the quantification of risk to enable managing cost risk.

The second discipline is project management. Project management is a discipline that on large projects translates the systems engineering framework (derived from the scientific requirements) into actionable and discrete tasks, the implementation of those tasks, and the monitoring of cost, schedule and risk. Project management is also usually responsible, together with financial managers, for reporting and accountability of the project schedule and budget to the sponsor—a responsibility not to be underestimated when large resources are in play.

In concept, the flow from science questions, to system requirements, to budget, schedule, and risk is a simple linear process (Fig. 1). In the real world, the flow does not really work this way. Grand ideas usually translate into excessive costs, impossible schedules, and/or unacceptable risks. The art (and joy and pain) of designing a large science project is the iterative process that retains the kernel of the grand idea and translates it into a set of requirements fitting a reasonable cost-schedule-risk envelope (Fig. 2). This iterative process starts at the high level and works its way through project details sometimes bouncing back up to high levels when problems are uncovered.

Regardless of how beautifully a project is designed, the sponsor plays a crucial role. The sponsor is responsible for securing and retaining funds, usually in a government context. The sponsor, if these funds are of a magnitude large enough to be visible in a larger (agency, or legislative) context is subject to additional pressures that are real, serious and often orthogonal to scientific issues! Ideally, the team (agency personnel, project leadership) works together to ensure the science requirements are met, while satisfying the real-world requirements associated with the

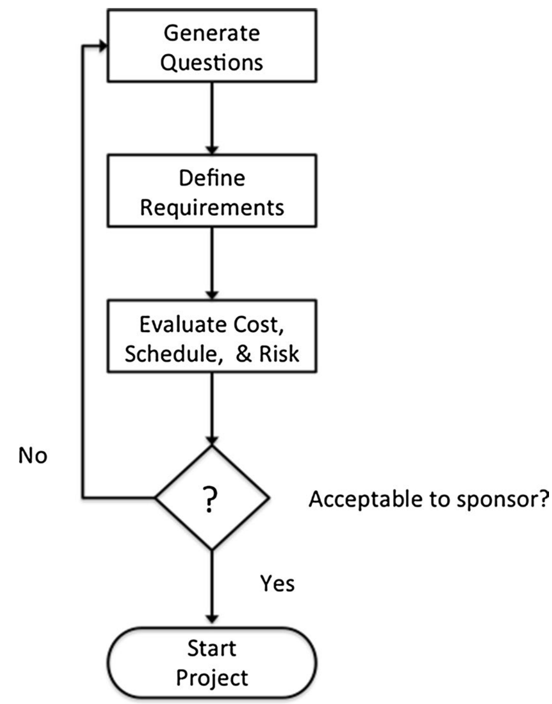

Fig. 2 The real-world iteration of project definition (Fig. 1) against resource constraints and sponsor tolerance for risk. Most projects begin with a planned scope that exceeds available resources or contains unanticipated risk, which emerges during scientific and engineering analysis. At the same time, it is common to discover that questions can be answered, or answered with acceptable uncertainty, with less expensive approaches. When successful, this iteration should identify the maximum science for the resources available. Alternately, this process may reveal that the question cannot be addressed with the resources available, and the funds are better invested in a different question

stewardship of large amounts of science funding—funds for which there are always many other worthy contenders.

Design

As noted above, the design of a large science project begins with a goal and question or questions. Goals may be quite specific, or broader, as in the National Ecological Observatory Network (NEON) Grand Challenge framework, designed to guide science over a 30 year project lifetime (see Table 1). An environmental science project is distinct from a monitoring system designed for environmental management. In the latter case, requirements are met if sufficient information is produced to trigger management actions as in the case of lake acidity measures or weather forecasts. Monitoring systems may support scientific research opportunistically but are not designed to detect unknown phenomena and enable scientific discovery.

Without the discipline of a question and hypotheses, there is no basis for prioritizing and making resource

Table 1 NEON grand challenges

Drivers of ecological change	Responses to ecological change
Climate change	Biogeochemistry
Land use change	Biodiversity
Invasive species	Ecohydrology
	Infectious disease

The NEON grand challenges, a set of science areas broad enough for a 30-year agenda but specific enough that they define what can be in the program, and what lies outside it

decisions. Agreeing on a question creates conflict because different scientists have different interests. But without a compelling question, there is little basis for a design (see Lindenmeyer and Likens 2010 for an excellent and lengthy discussion of this issue). Goals or grand challenges are crucial because, while they do not uniquely define the project, they create a boundary between what the project will and will not address (Table 1).

Once a goal is agreed to, specific questions, hypotheses or predictions that must be tested to answer the question are identified. This is an entirely familiar process to researchers. While managing a large project requires adhering to as much formalism as possible, science is a human endeavor and scientists have knowledge from experience that is impossible to prove within the formal framework. However, experience teaches us that well-designed experiments often pay off for far more than just the question they were designed around. This is not magic, but reflects the value of careful design, and the ability to quantify uncertainty in a well-designed study (enabling inference and quantification of uncertainty, so that even if a design is not optimal, it is usable). There are many examples of this, from applications of satellite instruments like LANDSAT or MODIS to questions that were not even envisioned when they were built, to the fascinating new questions asked of large-scale experimental manipulations like those at the Cedar Creek LTER site (Tilman et al. 2006).

Questions take a project one step closer to a unique definition, because once a question is posed, the team can begin to define what it would take to answer the question. Just as with a goal, this may not lead to a unique answer. This is a crucial point: the high-level challenges and questions help define what a project will not address, but the design process is needed to determine what can adequately address these questions. There is no magic formula for uniquely defining a project from its goals and questions. For example, if we want to quantify the carbon budget of a forest, we could use eddy covariance and measure CO_2 exchange, or we could measure carbon stocks over time. However, we can exclude and evaluate options. For example, if our goal is to measure the carbon budget of a forest using measurements of carbon stocks, we know it is necessary but insufficient to measure only aboveground biomass, and irrelevant to describe pollination ecology of a particular species (although that could be useful ancillary data). The design process then determines an adequate and cost-effective way of answering the question and the designers must choose between the available options.

The questions used to frame the NASA's Earth Observing System and NSF's NEON projects are shown in Table 2. These questions do not uniquely define these programs but they provide a framework and a bit more detail than the goals alone. Most importantly, they provide a framework for identifying what is NOT included in the project and so help define scope. Once questions are defined, measurements adequate to answer them must be identified. In the case of the NEON questions (Table 2), the meaning of "continental-scale ecology" is defined by the goals (Table 1) as measurements of biogeochemistry, biodiversity, ecohydrology and infectious disease. The NEON community then had to iterate specific measurements in each category sufficient to define change, a process that required

Table 2 Example science questions for two large science projects

NASA's Earth Observing System Science Questions (Moore et al. 1991)	The National Ecological Observatory's Science Questions (Schimel et al. 2011)
How are water and energy cycle changing?	What are the impacts of climate change on continental-scale ecology?
How are the oceans changing	What are the impacts of land use change on continental-scale ecology
How is tropospheric chemistry changing?	What are the effects of invasive species on continental-scale ecology?
How are land surface hydrology and ecology changing?	What are the interactive effects of climate, land use and invasives on continental-scale ecology?
How is stratospheric chemistry changing?	How do the transport and mobility of energy, matter and organisms affect continental-scale ecology?
How is solid earth changing?	

The high-level science questions for two big environmental science projects. Such questions further refine the needed measurements, and focus the effort. While science questions do not uniquely define what will be measured, they aid the design process but defining what the project is not responsible for several years, hundreds of scientists and significant statistical analysis (Schimel et al. 2011).

The hypotheses and questions, and the data needed to answer them form the basis for system requirements (Fig. 1). Requirements define the minimum performance a system must achieve in order to meet the project goals. Requirements must be realistic, and considerable experience shows that the more investment and care put into requirements, the more likely is project success. The discipline of translating scientific inquiry into requirements is difficult, and in some cases, must be approximate, and is always easier for incremental than transformative research. However, the definition and then the maintenance of requirements as projects progress and challenges arise is the most important and difficult responsibility of a project's scientific leadership. Without strong, requirements-based leadership, a project can only succeed by chance (NRC 2006). Numerous reports emphasize the risk to project performance that arises from inadequate cost estimation (NRC 2005). Definition and adherence to science requirements is essential for cost definition and control (NRC 2006, 2010).

Management of scope, responding to budget reductions, unforeseen cost increases or unexpected risk, are amongst the most challenging dimensions of large project management. However, just as most PIs revise their research plans as they begin to implement the experiment they proposed, most large projects go through a certain number of scope changes, either during the design process or as implementation reveals unexpected difficulties (Fig. 2). For example, during the NEON design process, a traditional ecosystem ecology measurement that had long been planned presented unexpected challenges when scaled to the entire network. Soil-atmosphere respiration is a key ecosystem flux for carbon that arises from the combined activities of belowground organisms including roots, the rhizosphere, and the broad community of decomposers. This measurement is so common that automated commercial-off-the-shelf (COTS) systems were available for purchase although the initial cost estimate for commercial systems was relatively high when implemented across the full observatory design. Even the refined commercial systems require substantial on-site maintenance (for example to remove fallen twigs and leaves that prevent chamber closure) and are difficult or impossible to operate in the snow. Removal of chambers in the autumn and replacement in the spring was an additional and meaningful operational cost. The requirement for maintenance translates into high operations costs and risks. Based on the estimated costs and risks, a redundant, albeit less thoroughly tested approach based on estimation of fluxes from soil CO_2 profiles was retained while chambers were discarded. This type of decision-making is routine in large projects but requires the collaboration of all disciplines (science, project management, systems engineering) to succeed.

A larger example of a scope change is reported in Moore et al. (1991), describing the redesign of NASA's Earth Observing System following a series of budget revisions. The redesign was led by the mission's science team (called the Payload Panel), supported by the instrument principal investigators and coordinated by mission project management and systems engineering teams. The process began by reviewing the science goals and key observations, and prioritizing them, based in part on contemporary climate uncertainties defined by the Intergovernmental Panel on Climate Change. Instruments were grouped against those science goals and evaluated both for their separate and synergistic contributions. This process of definition and grouping is described in a series of tables in the Moore et al. (1991) article. This information, in conjunction with cost and schedule information provided by project management was used to prioritize instruments, and design a mission profile that fit within the cost envelope. This required a detailed knowledge of the science questions and a clear understanding of what each measurement contributed. One instrument, a hyperspectral imager, was deferred, even though it would measure important properties, because its field of view was so limited that the amount of data collected would be modest, and obtained at high cost. A second instrument, a synthetic aperture radar, was also deferred because it required a separate satellite platform and had very high costs. The eventual solution preserved most of the missions' science requirements while significantly reducing cost and cost risk on several challenging sensors.

Certain aspects of design are distinctive to ecological projects and less familiar to other disciplines. In ecological studies, requirements for accuracy and precision are often largely determined by the statistical sampling and not by the intrinsic accuracy and precision of an instrument, although there are exceptions (for example in flux or isotopic measurements, e.g., Loescher et al. 2006). Ecologists are masters of sampling design at relatively small scales (a site: 100 m^2 to 10 km^2 or so) but have relied mostly on ad hoc networks to address larger scales (but see Keller et al. 2008). Increasingly the literature shows that relying on even very large sets of ad hoc measurements is problematic. For example Saatchi et al. (2015) show that even large numbers of non-random forest inventory plots produce systematically biased estimates and Schimel et al. (2015a, b) shows that biased global sampling contaminates estimates of carbon fluxes, carbon stocks and plant functional diversity. A major aspect of the design of large-scale ecological studies must be based on analysis and modeling of existing data to build a viable sampling design, rather than assembly bottom-up and hopeful reliance on the central limit theorem—an approach that may work but is not ensured (Jung et al. 2009). Sampling designs define where measurements

Table 3 Example mission requirements

Information product	Mission requirements
Forest biomass (above ground)	20 % accuracy
	100–300 m resolution/16 looks
	2 Biomass maps/year
	Polarimetric interferometric mode
	Global coverage of forests
Forest disturbance	Maps of disturbed area with 10 % classification accuracy100 m resolution/16 looks
	1–2 Forest disturbance maps every 2 months
	Global coverage

A representative set of high-level ecological mission requirements. These requirements are the outcome of a design and formulation activity and define the extent, accuracy and precision of measurements to answer specific questions

European Space Agency BIOMASS mission requirements. BIOMASS is a P-Band synthetic aperture radar whose goal is improved understanding and quantification of the land contribution to the global carbon cycle and whose objectives are to quantify aboveground biomass, forest disturbance and inundation, all factors influencing the land carbon cycle http://esamultimedia.esa.int/docs/EarthObservation/BIOMASS_sheet_130611.pdf

are made, how many measurements are made, replication of manipulations and other crucial aspects of determining whether a question can be answered, and the domain of ecological variation within which that answer will be valid, all key criteria for the scientific value of a study.

The European Space agency has developed a mission to observe forest biomass from space, using a radar instrument called BIOMASS (Table 3). BIOMASS is specifically designed to reduce the sort of uncertainty described in Saatchi et al. (2015), as can be seen in its mission requirements, which are the next level of detail after scientific goals and questions have been identified. For example, issues of sample adequacy are addressed by very specific requirements for spatial resolution, accuracy and coverage (Table 3). The mission requirements for BIOMASS are sufficient to show that the experiment can significantly reduce current uncertainty, addressing the quantification goal, and are sufficiently accurate to test hypotheses about the role of disturbance and biomass levels, amongst others.

While the accuracy of ecological measurements is often not the most limiting aspect of uncertainty, inconsistency among measurements made at different times or places can cripple subsequent analysis. Ensuring consistency through calibration and validation is essential to linking disparate measurements in time and space to enable data analysis over broader scales than in the context of a single investigator's project. Consistency is critical for the legacy of a data set and its use in detecting trends in time or space, an issue of growing concern. Referring back to Table 3, above, high-level mission requirements like 20 % accuracy imply lower-level requirements for calibration and validation sufficient to achieve and document that accuracy. Ensuring consistency across large regions-continents to the globe-and over years or decades may require huge effort and dedicated scientific expertise (Keeling 1998; Harris 2010) and is not a familiar effort for many ecologists. For many ecological measurements the standards and protocols are not in place in the community to even enable calibration and validation, and this is an important challenge.

Implementation

The implementation of a large project requires a wide range of skills and disciplines, whose exact composition of course depends on the nature of the project and will be entirely different between an organismal sampling network, a sensor network or a remote sensing satellite. Despite this, there are some consistent challenges that most projects face. Most projects must confront mismatch between the desired scope and the available resources. Generally, the scope is larger than the resources support and so scientific and project leadership must iterate changes to science scope against the resource requirements of each potential configuration (Fig. 2). Balancing scope against available resources requires a deep understanding of the project requirements, how they arise from the underlying scientific questions, and how the questions arose in the scientific community. This requires scientific leadership that both understands the science and is realistic about resources. The flip side is that project management must acknowledge the scientific requirements and collaborate to trace requirements to cost, so that collaborative decisions can be made when scope must be modified.

Project management and science leadership must have a strong and balanced relationship. The dialogue whether about existential issues or minor modifications is intrinsically conflictual, and while a collegial and cooperative decision-making style is effective, the best outcomes arise from the creative tension between scientific leadership's insight into the research and project management's insights into resources and constraints. In this dialectic, systems engineering enables the dialogue by ensuring that the connections between all the aspects of the complex project are understood, so that scientific and resource impacts can be linked. Systems engineering also acts as a referee, and sets the rules of such a discussion.

Good project management requires not only good process and formalism to provide quantitative information to support decisions; it also requires attention to human relationships, the people issues. An excessive focus on process rather than outcomes provides the appearance of accountability and oversight, without adding significant value to the project and is considered a significant risk (NRC 2006).

Risk management requires clear communication and well-informed decision-making: process supports but does not replace commitment and insight. This depends more on trust, open communication and values than formal reporting mechanisms.

The various leaders of the project team (management, science, systems engineering) must be peers when they debate difficult and contentious issues such as project scope reductions, because these decisions must respect the scientific and the financial integrity of the project. Project leadership must have a balanced knowledge of their project and the ability and openness to learn quickly. If project leadership cannot resolve conflict, then the entire team fails and corrective action may be required because the first requirement of this team is an ability to make decisions based on their different spheres of expertise.

Many sponsors and project managers feel that scientific communities, which are egalitarian, somewhat anarchic and often self-serving, cannot make hard decisions. Funders, under pressure from budget and oversight groups, increasingly must also respond to pressures regarding management of large and expensive project. In their efforts to reduce risk, they adopt practices that may increase the risk of failure. Perceived risk often produces counterproductive activities that seem justified by a pressing problem, but increases long-term risk: the best counter to this is a clear statement of best practice as a positive goal (NRC 2005).

Despite the inevitable tension between the various parties to a large project, every successful principal investigator has had to make draconic decisions, and in fact, scientific communities, which own project requirements can make difficult decisions if well-led and have the unique knowledge, when supported by project management, systems engineering and engineering to identify where requirements may be altered while retaining the maximum scientific value. While scientists can be unruly and undisciplined—anathema to project management—they are also realists and capable of very creative and difficult decisions when well-led and well-informed.

Overly top-down and authoritarian management creates an environment for catastrophic failure, especially when combined with mutual distrust. One of the most important examples is the "Challenger Effect", named for the circumstances surrounding the loss of the space shuttle Challenger. Investigations after the tragedy showed key information was not effectively shared as a result of management culture, both between NASA and its contractor and within NASA's Center management (Vaughn 1996). In a stressful management situation, there is a natural human tendency to implement hierarchical and highly accountable management structures. A risk in hierarchical systems is that subordinates fear passing bad news up the management hierarchy and for management to discount new and contrary information. Adversarial relationships between a project and its sponsor can likewise impede the discussion of critical information. This is especially serious if management or the sponsor perceives bad news as failure and is punitive or critical. Richard Feynman concluded that the Challenger disaster resulted from "engineers and managers who were not communicating effectively" (Feynman 1988).

While the Challenger incident was an engineering failure, cultural elements contributed that apply equally to science projects. The Challenger Accident Investigation Report found that "cultural mandates for business-like efficiency, production pressures, allegiance to hierarchy, and rule-following" were significant contributory factors to the incident (Vaughn 1996). All of these cultural attributes can easily develop, and in context, can seem reasonable. However, the outcome of these factors is pressure against open communication, flexibility and initiative. Because the cultural mandates listed above are common in organizational settings, it follows that active measures must be taken to enhance communication across organizational levels, have in place adequate independent review to ensure that "production pressures" and the desire for "business-like efficiency" do not undermine effective decision-making (Presidential Commission 1986).

Good project management encourages communication, rewards junior staff for voicing concerns and encourages managers to accept concerns as a positive development. Sponsors should similarly view problems identified by the project in a positive light, since problems identified early are almost always easier and cheaper to correct. When project participants fear to report challenges, problems grow, costs grow and failure is more likely. While a chain of accountability is crucial, flatter organizations with open communication are far less likely to conceal problems and risk serious failure. Successful management of complex, multidisciplinary projects comes from a commitment by peer leaders in the key disciplines to success and conflict resolution, rather than an organizational chart and chain of command that appears to enable executive decision making (see Fig. 3 for examples of alternative project organizational structures).

Transparency to the user community is an important method of avoiding risk: while dealing with criticism from the community of potential users may be painful, is often time-consuming, and can even be disruptive, transparency and responsiveness leads to long-term support and enthusiasm for a project. This is crucial for sustaining support of a project once built. Again, it is better to identify issues early, have time to investigate and respond, than to be caught short late in a project where not only are problems more serious, but the perception of concealment erodes trust. Trust is a crucial commodity when a project depends

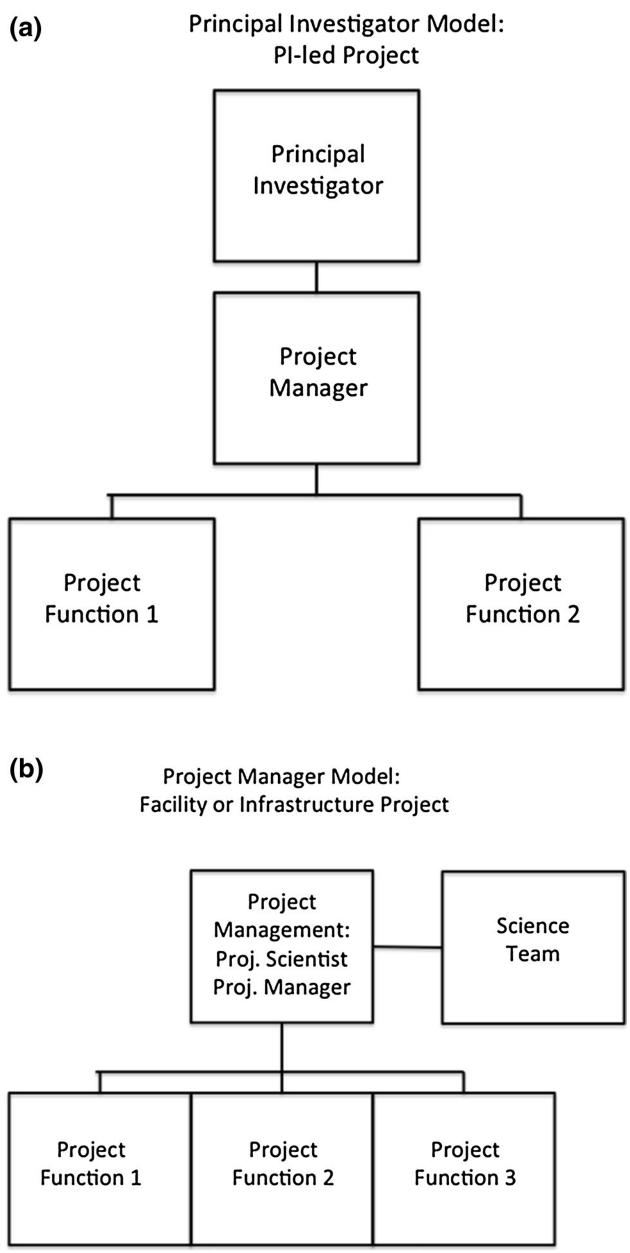

Fig. 3 Two project management models. **a** Is typical for a competitively selected project, often of intermediate size. **b** Is more common for large, complex or infrastructure projects where a high degree of management experience is needed because of cost and complexity. In **b**, the role of the science team is to define and manage the science requirements and to ensure that scope changes are consistent with scientific outcomes. Science teams are often competitively selected and funded independently by the sponsor to ensure independence

for its long-term execution on its scientific community's support. Transparency is difficult for many sponsors for whom control of information becomes part of strategy for managing political or bureaucratic risks, or who represent organizations whose size, specialization and social distance between members makes sharing information difficult (What Vaughn 1996 calls "structural secrecy"), but this tendency should be resisted to the extent possible.

Lessons learned

Big projects are a big deal and scientific communities, when they identify challenges which warrant the investment of significant resources, must take the stewardship of these resources with the utmost seriousness. Sponsoring agencies must respect the commitment of the scientific community and understand their interests in a project. The scientific community, when asking and receiving politically visible sums of money must likewise respect the difficult political and bureaucratic environment their sponsors experience, and appreciate the behind-the-scenes and perhaps of necessity invisible efforts made on their behalf!

Leadership and collaboration across the scientific, technical and enabling disciplines within a project are crucial. While it is easy to adopt a rigid and hierarchical model to ensure control, this maximizes the risk of serious failure due to staff protecting information to assert control, concealing risks out of fear of punishment and eroding trust with stakeholder communities. While micromanagement is a natural response to perceived risk, it almost always causes delay and increased cost (NRC 2005). Management by fear, often confused by all parties as *being tough* in addition to reducing morale, creates very real project risks and increases the possibility of failure.

Requirements definition is the step in a project where the adequacy of the proposed study to meet its scientific goals is assessed. Good requirements lead to good science, are the foundation of accurate cost estimation and allow scope adjustments. Requirements are the bridge between science and all of the other project disciplines. Ownership of the requirements must rest with the science function, which must have sufficient independence, knowledge, expertise and authority to maintain, and if need be, negotiate changing requirements with other project groups. Project management owns the resources and has the responsibility to remain within budget and schedule. Systems engineering must own the process for changing requirements to enable science return on investment and total resources available to be reconciled. Ecologists must learn the skills to work within this formalism.

Project management and scientific leadership are by design in conflict, and this can lead to the best outcome through healthy debate. Enabling this debate requires underlying respect between the parties, clear rules and accurate information resulting from strong systems engineering. The leadership team must recognize that failure to achieve a decision will result in consequences for the entire leadership team. If the leaders are not peers, and so equally accountable for failure, then the motivation to reach a constructive decision is lessened.

Ecological uncertainty is often dominated by statistical sampling design in time and space, rather than by measurement accuracy and precision, and so differs from the dominant components of uncertainty faced in other scientific fields. The broad spatial scales, and long time scales of ecological change also impose challenging requirements for standardization and calibration to enable comparative analysis over vast areas and time spans. Ecological projects must adequately address the twin design issues of sampling and standardization across diverse landscapes.

The big questions of ecology, biogeography, biodiversity, the carbon cycle, the movement of invasive species, the functioning of whole watersheds, and the science broadly underlying sustainability increasingly require more data than a typical lab can collect on their own. This implies infrastructure, informatics, calibration and standardization and a degree of effort sometimes unfamiliar and perhaps even unwelcome to ecologists. This has implications for culture, rewards and training for ecologists. While many of the recommendations in this paper are commonsensical, they are not universally accepted and are easily abandoned in the stressful and pressured environment that inevitably accompanies large projects.

Perhaps the single hardest lesson to learn in the execution of large projects is a certain tolerance of risk and failure. Large projects entail risk as they involve doing things never done before, and often at scales never before attempted. Large projects always involve some degree of failure and the skill of the team shows in how they recover from these failures. The Hubble telescope correction is an example of this, and the recent re-boot of the Kepler mission. While project management and systems engineering controls risk, too much effort to avoid risk and failure, as documented above can lead to catastrophic failure. Despite all of these challenges, large investments are required for the success of ecology, and to enable sustaining ecosystems in a changing world.

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