Establishing a Minimum Standard for Collaborative Research in Federal Environmental Agencies

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ABSTRACT

There is a general consensus that—given the magnitude of the challenges facing our nation’s natural resource managers—the rate, efficiency, and effectiveness of linking research to decision making must be enhanced. Many reports have touched on this issue, most of them culminating with the exhortation to “foster more interactions between scientists and users,” but very few documents provide details or assign responsibility to drive the interactions that most agree should happen. As a result, many natural science and engineering programs “talk the talk”—that is, they say they do collaborative research with intended users; however, upon inspection, few of them “walk the walk” by effectively supporting collaboration throughout the research process. Moreover, when called to support transition to application in specific ways, research agencies often balk, most often objecting that research programs cannot afford to take any support away from funding more research. They may also argue that science works best for society when it is freed from concerns related to application. In this paper we will 1) review the cultural conflict that often underlies disagreements about collaborative research, 2) offer details on the basic ingredients required to achieve a minimum standard for collaborative research, 3) suggest an approach for determining the appropriate level of support for collaborative research, given various research goals, and 4) recommend specific steps for motivating scientists and stakeholders to participate in collaborative research.

Keywords: Science technology policy Decision making

INTRODUCTION

Many are concerned that our society is not reaping maximum benefits from federal investments in environmental research. Reports and publications calling for increased linkages between science and decision making are numerous (e.g., National Research Council 1995; Pew Oceans Commission 2003; United States Commission on Ocean Policy 2004; Urban Harbors Institute 2004). From the research sector, reactions to calls for improving the linkage between knowledge and decision making are typified by the following 3 responses: 1) public and slightly vague proclamations to improve transitioning from research to application, 2) claims that the problem is already being effectively addressed, and 3) worries from scientists that this direction foreshadows lower quality science and less money for research.

The discussion is muddled and messy because it involves several contentious questions. These questions include:

- Is there an inherent conflict between collaborative research and high quality science?
- What is required for knowledge to be used effectively to solve environmental problems?
- What is the difference between collaborative research and pseudocollaborative research?
- How does one determine when and how often to use collaborative research?

This paper will strive to answer the questions above and in doing so will make the case that 1) fundamental science does not conflict with use-inspired research, 2) effective linking of knowledge and technology to decision making requires that research problems be framed and addressed jointly by involving both researchers and stakeholders, 3) best practice collaborative research involves the same level of rigor in its protocols as are applied to the methods of physical and life sciences, and 4) the determination of when and to what extent collaborative research should be used is dependent on the extent to which the problem to be addressed involves a human dimension, either in the problem’s creation or management. This determination should be made by a neutral and trained expert in collaborative research. Finally, we will assert that the research program manager—that person who directs how research dollars are spent, around what topics, and with what programmatic requirements—is most able to support collaborative research and motivate the various players who need to be involved. Currently, no one has been explicitly entrusted with the responsibility of driving this process. We think that person should be the program manager.

The outlook for improved collaborative research is positive. Many programs are experimenting with new ways of implementing quality science that have a high chance of being used efficiently by intended audiences. With this article, we hope to crystallize the guiding principles behind these experiments, challenge ourselves to raise the bar for collaborative research, and ultimately evaluate the impacts. As a point of clarification, in this paper we will use the term

* To whom correspondence may be addressed: kalle.matso@unh.edu
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“collaborative research” to denote research that involves scientists interacting with stakeholders within a formal and structured process; this is not to be confused with scientists collaborating with other scientists from different organizations or disciplines.

**IS THERE AN INHERENT CONFLICT BETWEEN COLLABORATIVE RESEARCH AND HIGH QUALITY SCIENCE?**

We contend that no inherent conflict exists, though a cultural conflict remains. It is important to understand that this conflict has existed at least since the time of the classical Greek philosophers and rages on to this day (Flyvbjerg 2001), typically with reductionists on one side of the argument and collaborative research proponents on the other. In the “reductionist” tradition—which has dominated the scientific enterprise throughout history, in particular in the United States in the 20th century—theories that cannot be expressed mathematically are considered 2nd class. Moreover, disciplines that allow themselves to be corrupted by human values are perceived as lacking rigor.

It was within this reductionist tradition that an engineer named Vannevar Bush rose to become director of the Office of Scientific Research and Development under President Franklin D. Roosevelt. Bush set the post-World War II United States on its trajectory with the 1944 publication of his report, “The Endless Frontier” (Bush 1944). In this report, Bush emphasized the importance of basic research versus research that is considered practical. He warned that, when mixed, applied research would drive out basic research. While none of his claims were wrong, per se, the enduring effect of his report was a distrust of use-inspired science and an almost blind belief in a one-to-one correlation between societal benefit and investment in basic research. In this context, it is not surprising that many in the scientific establishment view the collaborative approach—steeped as it is in applied research and human values—with strong distrust.

Donald Stokes’ “Pasteur’s Quadrant” (1997) details how Bush’s ideas affected the science establishment and continue to do so. Stokes and others—see, especially, Sarewitz and Pielke (2007)—have argued that societal problems demand a broader, more inclusive perspective than that championed by Vannevar Bush. We would like to emphasize an additional point. By advocating for a higher standard for collaborative research, we are not criticizing basic research. We fully admit the enormous societal value that comes from basic research. But we also believe that applied research has a vital role to play in addressing pressing environmental problems. Moreover, by definition, applied research exists within a human context where problems are either created and/or managed by humans. In these cases, the bottom line is how “good” the science is (although it may be an important factor); the bottom line is how effective research dollars are at ameliorating environmental problems. In this context, the next section reviews recent work that examines the factors that contribute to effective linking of research to decision making.

**WHAT IS REQUIRED FOR KNOWLEDGE TO BE USED EFFECTIVELY TO SOLVE ENVIRONMENTAL PROBLEMS?**

The ability of knowledge to address societal problems is related to 1) the quality of the knowledge and 2) the use of that knowledge. In turn, the use of knowledge is related to the “users” knowing about the knowledge, trusting the knowledge, and being able to use the knowledge, given a specific logistical context (e.g., financial limitations, political barriers). Cash et al. (2003), reviewing over 30 different cases from a series of international workshops that took place between 2001 and 2002, concluded that effective transfer of knowledge to users is related to 3 factors: 1) credibility (the extent to which the knowledge is viewed as coming from a credible source), 2) salience (the extent to which the knowledge is viewed as being relevant to the users), and 3) legitimacy (the extent to which the process of framing the questions and carrying out the research is viewed as being respectful to the perspectives of the intended users).

Building on Cash’s efforts, Clark and Holliday (2006) document the results of a workshop involving over 25 program managers for research organizations involved in sustainability, from agriculture to environmental quality. Each program manager was asked to describe the essential qualities necessary to link knowledge to decision making effectively. The results of the workshop can be summarized in terms of the essential ingredient for effective research as well as why this ingredient was so often left out.

The crucial ingredient, the managers agreed, was “collaborative, ongoing user-driven dialogue, including the role of user-producer dialogues, the boundary organizations that facilitate such dialogues, and the importance of user-driven definition [of the problem]” (Clark and Holliday 2006). The barriers, also agreed upon, were risk aversion, disincentives from evaluation systems, human resource constraints, and political uncertainty. This research provides support for the notion that collaboration between researchers and intended users is critical to maximize our return on investment when solving environmental problems is the specified goal. If this is true, it is reasonable to ask 1) what constitutes collaborative research? and 2) does the level of collaboration change with different types of research projects?

**WHAT IS THE DIFFERENCE BETWEEN COLLABORATIVE RESEARCH AND PSEUDOCOLLABORATIVE RESEARCH?**

Focusing on what constitutes “collaboration” may suggest we are overly obsessed with semantics, arguing one definition over another. In this section, we will make the case that the difference between pseudocolaboration and collaboration is akin to the difference between pseudoscience and science. In both cases, the latter terms refer to a set of core principles that build a foundation for better understanding and interacting with our environment; the former terms denote sloppiness and lack of rigor. It is an absolutely critical distinction. Collaborative research in scientific settings is a discipline unto itself (McNie 2007). As with any discipline, active researchers disagree on the minutia. We suggest that research must have the following 2 ingredients to qualify as “collaborative”: 1) the funding agency supports a formal approach for guiding the interactions between researchers and stakeholders; the approach should be rigorous, supported in the literature, and accepted within the consensus building community, and 2) the funding agency provides a neutral collaborative research expert to help determine the extent of collaboration necessary and to broker communications between researchers and stakeholders, if necessary.
The most often cited example of a formal approach is joint fact finding. Joint fact finding is considered a step in consensus building and has seen most use in resolving specific environmental disputes. However, joint fact finding is flexible and can be adapted to more exploratory research and development projects as well (Ehrmann and Stinson 1999). The essential aspects of joint fact finding are 1) jointly framing the questions to be answered (between researchers and stakeholders), 2) agreeing on analytic methods and data sources to answer questions, and 3) maintaining interactions between researchers and stakeholders throughout the project. Table 1 offers details on joint fact finding in research settings.

There are times when joint fact finding will not be the best approach to use; for example, if the research question has no discernible connection to resource management or public policy. This is one of the reasons that using a collaborative expert is critical. Among other things, the specialist can help determine which formal process is most appropriate, a task that is completely outside the skills set of the program manager, researcher, and stakeholder.

Incorporating a neutral collaborative research expert is a step that makes many researchers and program managers nervous, perhaps because they want to retain control, perhaps because of the cost involved, or because they have had a bad collaborative experience in the past. We wholeheartedly understand these concerns, which emphasize how important it is to acknowledge that collaborative expertise is a discipline that requires rigor and experience, just like engineering, ecology, chemistry, or other components of science. Yet too often collaborative expertise is looked on as something anyone can do, as long as they are assertive and comfortable speaking in public. Just as a scientific project requires highly skilled, proven researchers, so does collaborative research require—at least at the outset—highly skilled, proven specialists.

One critical role of a collaboration expert is to help determine if collaborative research is even warranted at all,

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**Table 1. Outline of joint fact finding (JFF) process—based on materials from the Consensus Building Institute (www.cbuilding.org)**

<table>
<thead>
<tr>
<th>Step</th>
<th>Actions</th>
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| 1 Prepare | Understand how JFF fits into consensus building process  
Document the interests of all relevant stakeholders  
Work with a professional “neutral”  
Convene a JFF |
| 2 Scope | Work with stakeholders to draft roles and responsibilities  
Generate technical questions  
Identify existing information and knowledge gaps  
Advise on methods for dealing with conflicting data and interpretations of facts and forecasts |
| 3 Define analysis methods | Translate general questions into researchable question  
Identify relevant methods of information gathering/analysis and highlight the benefits and disadvantages of each  
Determine the costs and benefits of additional information gathering  
Determine whether proposed studies will enable stakeholders to meet their interests |
| 4 Conduct the study | Undertake the work, checking back with constituents  
Draw on expertise and knowledge of stakeholders  
Review drafts of the final JFF reports |
| 5 Evaluate the results | Use sensitivity analysis to examine the overall significance of scientific assumptions and findings  
Compare findings to the published literature  
Translate findings into possible policy responses  
Clarify remaining uncertainties and appropriate contingent responses  
Determine whether and how JFF results have or have not answered key questions |
| 6 Communicate the results | Jointly present findings to stakeholders  
Scientists communicate JFF results to various constituencies and policy-makers  
Determine if further JFF is necessary |
and if so, to what extent. If the problem is relatively simple and the conceptual differences to be managed are negligible, a specialist might not be required. Or perhaps a specialist will be required to convene the group and establish guidelines, leaving the group to continue on its own after the initial meetings. Whatever the case, a neutral expert should be used to make that initial determination. Program managers lack that expertise and, left to their own devices, will usually not appoint a specialist in order to avoid the added friction.

Can progress be made without a specialist? Of course, but it is likely to be flawed in such cases and certainly not likely to result in an optimal outcome or best practice. In the absence of a neutral, qualified specialist, one of two things usually happens in the context of a research project: 1) nobody fills the vacuum and the collaboration is marked by ineffectiveness and discord, or 2) one of the vested parties (e.g., the head researcher or one of the stakeholder representatives) takes the reins. The process is now being guided by a nonneutral entity with the likely result that other participants are not sufficiently invested in the process or they are dissatisfied due to the perception of inequality of the negotiations. The legitimacy of the process—one of the essential ingredients noted by Cash et al. (2003)—is now seriously compromised.

These two criteria—having an explicit collaboration process and using a neutral specialist to establish the appropriate process—set the stage for a higher standard for collaborative research. Using this as a filter, many research programs that usually lay claim to sponsoring collaborative research would have to withdraw that claim. Having established a minimum standard for collaborative research, we now consider the criteria that should be weighed to determine when and to what scale collaborative research should be applied.

**HOW DOES ONE DETERMINE WHEN AND HOW OFTEN TO USE COLLABORATIVE RESEARCH?**

On this topic, there is less of a consensus in the literature. In discussing joint fact finding, Erhmann and Stinson (1999) note that it is especially useful in situations where there are obvious disputes about information or interpretation of data, or when dealing with parties that have a history of poor relationships. There are many papers that support the idea of collaborative research to resolve complex resource management disputes, such as Beierle and Cayford (2002), Adomakai and Sheate (2004), and McNie (2007). (For an example of a paper challenging collaborative research, see Coglians is 1999). Complex environmental problems have been dubbed by some as being “wicked,” most obviously characterized by their complexity and the involvement of a human values dimension (Patterson 2006). We contend that one of the reasons why a lot of excellent research is not used is because program managers and scientists tend to underestimate how complex—tangled in human values—most resource management problems are. Scientists especially tend to see resource management problems as technical issues only, and not as issues that involve differing values and priorities.

As noted, there is a dearth of examples of collaborative approaches for more exploratory research and development programs. It is necessary, therefore, to use a conceptual model of sorts to answer the question of when to use collaborative research in the less obviously wicked scenarios. We suggest that it is reasonable that collaborative approaches should be considered for any research endeavor for which a specific use and user community is anticipated. If a specific user community can be identified, it is likely that communicating with that community will improve the saliency, credibility, and legitimacy of the science. Moreover, if it is worth communicating with a specific user community, it is worth doing it in a manner that optimizes the legitimacy of the process: again, requiring the use of a formal collaborative approach and some level of collaborative expertise. The exception to this rule would be those cases where it can be shown with a high degree of certainty that the problem and the application of new knowledge or technology are not impacted by the human values dimension. Table 2 helps to make this more tangible.

Among the examples in Table 2, we contend that the first two lack the necessary explicit connection to an identified problem and user community; however, the remaining examples warrant some level of collaborative research. We want to clarify that collaborative research comes in various shapes and sizes. It is not enough to determine simply that collaborative research is appropriate. In a world of finite resources, program managers must decide on the appropriate scale of collaboration.

The best way to approach this task is perhaps to consider the main limitations on collaboration: 1) money, and 2) fears that stakeholder involvement will decrease the quality of the science. Costs are associated with the number of meetings as well as the number of stakeholders involved. Let us first deal with the number of meetings since this is the easier problem. Table 1 presents the essential 6 steps of the joint fact finding process. While it is possible to complete each step with 1 day-long meeting, this would be a tall order. Therefore, program managers should budget for between 9 and 15 meetings throughout the duration of the research endeavor.

With regard to the number of stakeholders and technical experts, Susskind (1999) offers a logical method for building the research group. For each step, we will use the 4th example (dealing with sediment capping technology) from Table 2 to make it less abstract: 1) identify a 1st circle of essential participants; these are 1st order and obvious stakeholders who will impact the framing of the problem and whether the knowledge/technology is used (in our example, the 1st circle could include representatives from the US Environmental Protection Agency, the California state regional water quality board, and a citizen’s group such as Surfrider or Save the Bay), 2) identify a 2nd circle of suggested participants; the first circle is asked to identify people who might be able to contribute to (or block) efforts to apply knowledge to the specified problem (in our example, the 2nd circle could include prominent citizens, scientists, politicians, or other groups), and 3) identify any missing actors (pending a final decision to move forward with joint fact finding). The program manager will increase the viability of the endeavor by identifying any missing stakeholders whose absence will affect the utility and application of the knowledge when the research is concluded.

Theoretically, completing each additional step increases the credibility, saliency, and legitimacy of the knowledge produced. It also increases the costs to the research program in order to pay for the convening of meetings and collaborative expertise. Finally, it elongates the planning phase of the research endeavor, which can be frustrating for scientists and program managers (Juliana Birkhoff, Consensus Building Institute, personal communication). However, there is evidence to suggest that, in the long run, collaborative research
saves money by cutting off disputes before rather than after considerable investment (Adomakai and Sheate 2004). Program managers can choose to reduce costs by involving only the 1st circle of stakeholders, which equates to fewer meetings. Most likely, this would lead to a corresponding decrease in the application of the knowledge or technology. This tradeoff will have to be analyzed iteratively and on a case by case basis. In situations that are especially complex, such as the last example in Table 2, which relates to increased incidence of cancer near a paper mill, many would deem it worth the extra time and money to involve all 3 levels of stakeholders. In the example that involves new sediment capping technology (Table 2), there is a more compelling argument for fewer meetings and fewer stakeholders.

In considering these tradeoffs, federal funding programs should also weigh the collateral benefits of collaborative research, such as the increased communication between users and producers, which has been called for in many reports (e.g., Pew Oceans Commission 2003; United States Commission on Ocean Policy 2004; Urban Harbors Institute 2004). Increased communications lead not only to better future research but also to greater use of past and existing research. The benefit is bilateral, so researchers benefit from ideas presented by various stakeholders, including private sector companies, which are often on the cutting edge of innovation and also have a good understanding of market dynamics.

Previously, we noted that many scientists and program managers worry that involving stakeholders through facilitated discussions may decrease the quality of the science. This is a legitimate concern. There is evidence, however, that stakeholder-driven processes actually improve the quality of the science (Beierle and Cayford 2002). With regard to the collaborative expertise component, we do not suggest that this is an automatic panacea for the challenges of linking knowledge to decision making. Poor collaboration happens, as does poor science, so steps must be taken to ensure the highest quality collaborative expertise support.

Another limitation to collaboration is the fact that some researchers simply do not like collaborating, especially with the nonscientific realm. This is a reality, and it is unavoidable. However, program managers who control how money is spent on research can assign responsibility for collaborative research

### Table 2. Research examples illustrating spectrum of specificity with regard to the anticipated user community

<table>
<thead>
<tr>
<th>Research Example</th>
<th>Funding Source</th>
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<tbody>
<tr>
<td><strong>Chemically Selective Displacers for Protein Purification</strong></td>
<td>National Science Foundation</td>
</tr>
<tr>
<td>Engineering research aimed at increasing the selectivity of ion exchange systems.</td>
<td></td>
</tr>
<tr>
<td>A variety of general uses are purported, especially in cellular regulation. No</td>
<td></td>
</tr>
<tr>
<td>specific societal problem is addressed, however.</td>
<td></td>
</tr>
<tr>
<td><strong>Ecological Genomics of Drought Adaptation in the Monkeyflower Plant</strong></td>
<td>National Science Foundation</td>
</tr>
<tr>
<td>Fundamental research with implications for better understanding impacts of</td>
<td></td>
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<tr>
<td>climate change.</td>
<td></td>
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<tr>
<td><strong>Distribution and Phototransformation of Nonpoint Source Agricultural Pesticides</strong></td>
<td>Cooperative Institute for Coastal and Estuarine Environmental Technology</td>
</tr>
<tr>
<td>in Freshwater and Estuarine Wetlands at Two Reserves in Ohio and Florida</td>
<td></td>
</tr>
<tr>
<td>Fundamental research with implications for understanding and managing pesticide</td>
<td></td>
</tr>
<tr>
<td>pollution, but not on pending management decisions.</td>
<td></td>
</tr>
<tr>
<td><strong>Hydrogen-Enhanced Remediation of Capped and Natural Sediments</strong></td>
<td>Cooperative Institute for Coastal and Estuarine Environmental Technology</td>
</tr>
<tr>
<td>A mix of fundamental and engineering research—at the proof of concept stage—</td>
<td></td>
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<tr>
<td>with direct application to sediment management at a site in San Diego, CA.</td>
<td></td>
</tr>
<tr>
<td><strong>Biosolids Research Summit</strong></td>
<td>Water Environment Research Foundation</td>
</tr>
<tr>
<td>Develop an agenda to address research gaps identified by the National Research</td>
<td></td>
</tr>
<tr>
<td>Council. General concern was how practices relating to the application of</td>
<td></td>
</tr>
<tr>
<td>biosolids in agricultural context may impact human health.</td>
<td></td>
</tr>
<tr>
<td><strong>The Northern Oxford County Coalition: Four Maine Towns Tackle a Public Health</strong></td>
<td>US Environmental Protection Agency and Maine Department of Environmental</td>
</tr>
<tr>
<td>Mystery</td>
<td>Protection</td>
</tr>
<tr>
<td>Citizens from a group of towns in Maine were concerned that they were</td>
<td></td>
</tr>
<tr>
<td>experiencing uncommonly high rates of cancer, and suspected that the phenomenon</td>
<td></td>
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<tr>
<td>was related to a local paper mill (McKearnan and Field 1999).</td>
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</table>
and thus motivate the various players who need to be involved.

**PROGRAM MANAGERS: INCENTIVES FOR PROVIDING INCENTIVES**

Earlier, we noted an National Research Council-supported workshop involving over 25 program managers from various research sectors. The impetus for the workshop came from 3 previous meetings related to science policy and sustainability, all of which pointed to the pivotal role played by the program manager (Clark and Holliday 2006). It is logical that linking disparate entities is more a part of the program manager’s job than anybody else’s. The careers of researchers do not depend on their interactions with decision makers. While some researchers may have an interest in interacting with stakeholders, these activities are not part of their incentive program, which is mostly based on publication record. For the most part, the same goes for decision makers. Interactions with scientists are not necessarily an expected part of their day-to-day activities. If the finger of responsibility naturally points to anyone in the research enterprise, it points to those people who set up the research, who distribute funds and thereby have the ability to provide incentives to both researchers and stakeholders to participate in activities that are not specifically part of their job descriptions. If program managers are not doing this sufficiently, we need to ask why and what needs to happen to shift the paradigm.

There are many reasons why program managers are not as active in supporting a higher standard for collaborative research (Clark and Holliday 2006). Many program managers and institute directors are themselves scientists who were educated under the traditional paradigm in which scientist contact with stakeholders was not necessarily considered virtuous. Also, as we have stated, there is a lack of agreement on the definition and potential benefits of collaborative research. Regardless of the cause, steps should be taken to encourage program managers to prioritize linking knowledge to decision making, at least within those federal organizations with an explicit coupling to specified problems. Because people respond to performance objectives that are measured and rewarded, agencies should create reward structures that encourage program managers to sustain collaborations between knowledge producers and knowledge users. Again, since these are not one-size-fits-all problems, agency heads should avoid overly prescriptive, cookie cutter approaches. Rather, they should agree on a set of principles and then provide a reward structure for creativity in embodying those principles. It is important to note that the accountability referred to here is very different from the accountability activities of the Office of Management and Budget, which evaluates overall program outcomes. Instead, we are suggesting that principles of collaborative research and effectively linking knowledge to use be explicitly integrated into person-by-person job performance reviews.

Whether a research program operates via a competitive grants process or via internal mechanisms, the principles discussed in this paper can and should be integrated into the various stages of the research endeavor. Not all projects will involve stakeholders at the same level, or at all, in fact. But program managers should be able to respond—based on clear principles and direction from agency heads—to general questions as to how they incorporated collaborative research objectives into each research stage, and why. Some examples of these types of questions are:

- At the problem definition stage, what processes did you use to involve stakeholders and why?
- How many stakeholders were involved and why?
- What steps were taken to create an egalitarian exchange?
- What steps did you take to foster cocreation of research plans and evaluative benchmarks?
- What steps did you take to make sure that nontechnical issues related to application were taken into account in the review process? Why?
- What processes were used to ensure continuous involvement of stakeholders in the research execution? How were adaptive management principles embraced so that unexpected results were handled in an agreed upon manner?
- What steps were taken to evaluate the data or technology in a collaborative manner?
- What processes were used to make the link between the research results and relevant resource management policies?
- To what extent were private sector entities involved throughout the process? Why?
- What steps were taken to encourage communication of results to stakeholders?

Note that these questions are not meant to be exhaustive. Rather, they suggest the tone of the questions, which are meant to challenge the program manager to use his or her creativity to act in accordance with agreed upon principles.

**CONCLUSION**

Theories concerning the relationship between research program design and impact must be evaluated so that the theory can be ground truthed and modified in an iterative fashion. Although work has been done in this area (e.g., Yin and Moore 1988; Beierle and Cayford 2002; Karl et al. 2007), much more is required. Unfortunately, program managers in mission-driven agencies tend to be experts in their particular natural science, not evaluation research. Therefore, there is a great deal of confusion and little consensus on what metrics to use and how to implement them (Feller and Gamota 2007). We suggest that experts from the evaluation and planning disciplines must be consulted in the presence of program managers and stakeholders in order to gain increased consensus on this critical area.

Meanwhile, without intervention, the debate will continue to cycle and evolve, with maverick program managers—supported by daring champions at higher levels—spreading creative ideas through word of mouth and, in some cases, through well-disseminated publications. This is the bottom-up paradigm. Alternatively, an infusion of high-profile involvement from the top could be very beneficial, especially if done in a collaborative manner. This action could take numerous forms. For example, various agencies could pool resources to support a review of existing programs that currently embrace collaborative research principles. Part of the review would be to ensure that some evaluative processes were built into these endeavors in order to inform other programs. In addition, several of the most pressing problems facing our nation could be identified as pilot candidates for a full-fledged effort involving collaborative research as we have defined it in this article. An effort of this magnitude would
necessarily involve evaluative benchmarks built into it, along with the support—so often lacking—to monitor and report out on effectiveness in reaching specified benchmarks.

In this article, we have defined a minimum standard for collaborative research and suggested guiding principles for deciding when collaborative research is called for, and at what levels (i.e., number of stakeholders, frequency of meetings). We believe program managers need to be properly motivated by their superiors to seek and implement creative approaches to collaborative research, since they have the ability to convene the various parties that ought to be involved. Lastly, we have pointed out opportunities for collecting data with which to test these theories. Our hope is that this paper—along with others—will elevate an important discussion and contribute to an incremental convergence around a group of guiding principles for analyzing how and when to implement collaborative research. The phrase “gradual and incremental” may lack drama, but history shows that overly hasty as well as overly prescriptive policies can be counterproductive. Moreover, given the scale of resources—both natural and monetary—that are affected by this issue, even an incremental improvement, if supported by broad consensus, would be highly significant.

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REFERENCES


