Problems and Questions in Scientific Practice

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Abstract

Philosophers increasingly study how scientists conduct actual scientific projects and the goals they pursue. But as of yet, there are few accounts of goals that can be used to identify different kinds, and specific instances, of goals pursued by scientists. I propose that there are at least four distinct kinds of goals pursued by scientists: ameliorating problems, addressing questions, satisfying values, and achieving epistemic aims. I focus on the first two kinds, and I provide tools to help conceptualize, distinguish, and identify the problems and questions pursued by scientists. This paper illustrates the use of those tools with two examples.[1]

[1] Thanks to Tom Nickles, Rick Creath, and Manfred Laubichler for comments on earlier versions of this paper.

1- Introduction

Philosophers increasingly study how contemporary scientists conduct ongoing scientific projects, often called scientific practice (Ankeny et al. 2011). Philosophers focus on the myriad theories, models, laws, hypotheses, and similar items (here grouped under the term 'scientific products') deployed in those projects (Green 2013; O'Malley et al. 2014). In doing so, they note that scientists construct, select, and deploy scientific products to achieve many different kinds of goals, from better describing phenomena, to explaining it, to addressing issues like deforestation or abnormal embryonic development (Elliott and McKaughan 2014; Love 2013). But as of yet, no one has developed a systematic account of the different kinds of goals actually pursued by scientists.

Such studies have yielded a wealth of new concepts with which to study the goal-directed aspect of much of science. Many accounts use similar terms for different concepts, such as for concepts of goals and aims (Elliott and McKaughan 2014; Potochnik 2015), and for values (Douglas 2013; Brigandt 2015), to name just a few. Older but kindred accounts conflated terms or concepts of aims with those of values (Kuhn 1977; Laudan 1984), or of question-answering with those of problem-solving (Laudan 1977; Hintikka 1981).

That florescence of concepts portends some issues. First, it may prompt philosophers to argue about, and entrench their positions against, concepts or accounts that are in fact compatible with their own. More importantly, it has made difficult at best our ability to compare results across different studies of scientific practice. Without such comparisons, such studies amount to little more than recounts of scientific research, yielding little understanding of the epistemologies of science. We need instead a framework of the myriad goals of scientific practice, combined with a strategy with which to study and systematically compare cases of scientific practice. Here, I focus on the first issue.

To ameliorate the above issue, I propose a framework of the goals of scientific research. The framework knits together many of the concepts proposed by others. I focus on two such goals: solving problems and answering questions. Many have nearly identified the two, but I argue that we more fruitfully study scientific practice if we distinguish them.

In the next section, I sketch the overall framework of goals, while I discuss problems and questions in sections 3 and 4, respectively. In section five I provide some reasons for distinguishing problems from questions, and in section 6 I highlight two research projects that we better understand if we distinguish the questions they address from the problems they ameliorate. I close the paper by forestalling some worries about the overall sketch and about the distinction between problems and questions.

2- Goals of Science

Many accounts of the goals, aims, or ends of science privilege an ultimate goal, such as explanation (Popper 1957), problem solving (Laudan 1977), significant truth (Kitcher 2001), or understanding (Potochnik 2015). Some accounts propose multiple aims, especially focusing on addressing practical issues (Elliott and McKaughan 2014). Critics urge that there are no general aims of science, and that we should instead develop concepts of aims that are localized to researchers (Hardcastle 1999). I propose multiple goals of science focused around research teams. But my account knits together some of the insights of those who proposed ultimate aims.

I propose that contemporary researchers pursue at least four kinds of aims: to solve problems, to answer questions, to achieve epistemic goals (such as describing, explaining, or predicting phenomena), and to satisfy values. When I focus on scientific products instead of on researchers, I use the language of functions for artifacts (Knuuttila 2011; Love 2013; Woodward 2014). Thus, scientific products function to solve problems, answer questions, etc.

Furthermore, over the course of a project, the functions of problems, questions, etc. themselves change. At the beginning of a project, they motivate researchers to act. In the middle part of a project, they constrain the behaviors of researchers. At the end of a project, researchers evaluate the results of their projects and those of other according to those problems, questions, etc.

The above sketch provides a bit of context for the sections to come. I can't here detail all elements of the sketch, so I focus on two aspects of it: problems and questions. The next two sections provide the machinery with which to conceptualize problems and questions, while the ensuing section illustrates examples in which the machinery is useful.

3- Problems

If we're to take solving problems as a general goal of many scientific projects, we need an account of problems (Nickles 1981). To apply to actual scientific practice, such an account must provide a set of tools with which to identify the problems that scientists pursue, a known but little studied issue (Nickles 1988). The set of tools should include at least the following.

First, the account must specify the kinds of things to which 'problem' refers. Second, the account must provide a general semantical structure for propositions of the form "X is a problem". Third, the account must provide conditions according to which researchers assert the proposition that 'X is a problem'. With those tools, those who study scientific practice can better identify the problems that scientists identify as parts of their projects. I sketch four tools below.

1. Problems:

   Problems are, and the term ‘problem’ refers to, states of affairs or situations in which something valued is harmed or is obstructed from flourishing.

Many who discuss problems in science adopt more restricted views of problems. They view problems merely as troubles faced by theories (Laudan 1977; Hattiangadi 1978; Nickles 1981). That view prompts most to treat problems as questions, a positon I discuss in a later section. My account is more general, noting that scientists often motivate and evaluate their projects by more worldly issues, such as droughts, birth defects, and extinctions.

2. Proposition that “X is a problem”:

   The proposition is an abstract object that includes: a set of propositions that describe a situation, an evaluative proposition that disvalues the situation, an imperative proposition to ameliorate the situation, and a set of propositions that describe constraints on the amelioration.

The above tool is due largely to Thomas Nickles (1981). My version is more general than his in several ways. First, I countenance more kinds of situations as problems than does Nickles. Second, for Nickles, all scientific problems include at least obstructed goals in relation to theories; while my account countenances harms or obstructions to explicit theoretical goals, but also to anything valued. Third, my account explicitly includes propositions to describe states of affairs and an evaluative proposition, while Nickles's account at best lumps those propositions as kinds of constraints on solutions.

3. Problem Statement:

   A problem statement is a sentence that describes a problem. Put differently, a problem statement expresses the proposition "X is a problem" and specifies the X.

A problem statement uses the concept of a problem when it implies all of the propositions that constitute that concept. For instance, the sentence "Deforestation is a problem in Montana" implies, among other things, that deforestation in Montana is dis valuable and that it should be reduced or halted.

Nickles aimed to describe problems as things that could persist across projects and researchers, that could evolve over time, and that themselves could be studied. While he noted that agent-focused tools might complement his semantic account and be useful to study scientific practice, he provided none. The following is one such complementary tool. It provides a set of conditions in which agents can assert a problem statement.

4. Agents (straightforwardly) assert that "X is a problem" only when:
   1. They're conscious or aware of the state of affairs X
   2. They disvalue X
   3. They imply an imperative to ameliorate X
   4. They believe that effort is needed to further specify or to ameliorate X
   5. They believe that a possible strategy of action could ameliorate X

6. They believe that, when pursuing problems, it's appropriate to use the language of ameliorable (ability), ameliorating (process), and ameliorated (result).

The above tool is largely due to Gene Agre (1982). According to it, agents assert that some situation is a problem based on their background knowledge, values, and beliefs, all of which can differ across agents, and the latter two of which can be disputed. Furthermore, it provides a starting point around which those who study scientific practice can develop tools, such as surveys and content analyses, to collect data about the problems invoked and pursued by researchers.

In a further respect, the above tools are more general than nearly all earlier accounts of problems. Earlier accounts describe researchers as solving problems. The above tools instead describe them as ameliorating problems. More than language of "solving", language of "ameliorating" better fits the model of satisficing rationality explicitly invoked by most previous accounts of problems.

4- Questions

If we're to take answering questions as a general goal of many scientific projects, we need an account of questions. There has been substantially more research into questions than into problems, yielding a wide array of topics and accounts (Cross and Roelofsen 2016). Given that array, much less machinery needs to be developed to identify questions pursued in scientific practice. For semantics of questions, I review a standard account familiar to philosophers of science (Belnap and Steel 1976), while for conditions under which agents poses questions, I specify a new account.

Questions are abstract objects, like propositions, posed by agents. For their most basic semantic structure, elementary questions include a set of possible alternatives and propositions that indicates how many of the distinct alternatives the agent seeks (Belnap and Steel 1976). An interrogative statement is a sentence that expresses a question, just as a problem statement expresses a problem proposition.

For a question to possibly have an answer, it presupposes a proposition that describes a state of affairs in which the agent asking the question lacks information. Agents pose questions under at least the following conditions.

5. Agents (straightforwardly) pose a question only when:
    1. They're aware of their epistemic state of lacking information
    2. They disvalue that state
    3. They imply an imperative to ameliorate that state
    4. They believe that effort is needs to formulate the question or to provide the information to address it
    5. They believe that possible information could ameliorate their epistemic state, and that they could identify that information if presented it (Hintikka 1981).

6. They believe that, when pursuing questions, it's appropriate to use the language of addressable (ability), addressing (process), and addressed (result).[2]

There are many parallels between the above accounts of problems and questions, and I model the conditions for posing questions on Agre's conditions for asserting problem propositions. Such similarities between the accounts partly explains why so many philosophers have given the appearance of equating the two. But for studying science, we most fruitfully treat problems states of affairs and questions as abstract objects.[3]

5- Distinguishing Problems from Questions

Among those who study the role of problems in science, many seemingly identify problems with (sets of) unanswered questions and the practice of problem solving with that of questions answering (Laudan 1977, Hintikka 1981, Goldman 1986, Love 2008). The tools in the previous sections enable us to distinguish problems from questions, and to distinguish the practice of ameliorating problems from that of addressing questions.

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[2] Language of 'addressing questions' replaces that of 'answering questions' to better fit with the satisficing model of rationality.

[3] Insofar as one ontologizes states of affairs as themselves abstract objects, then my argument is instead that problems have logical or abstract structure that is distinct from that of questions. Thus, the set of problems doesn't overlap with the set of questions.

There are several reasons why it is fruitful to distinguish questions from problems when we study research projects. First, the tools described above enable us to charitably interpret previous accounts of problems such their insights are still highly relevant to the study of scientific practice. Given those tools, questions presuppose a kind of problem proposition, and the practice of addressing questions is a subkind of the practice of ameliorating problems. Given a question, we might establish some translation rule to move between the question and a specific one of its presuppositions and back again.

For instance, if I ask "Why do leafblowers make as much noise as they do?", I presuppose that I lack that information, a situation I disvalue. If we treat lacking information as a situation in which something valued (here knowledge, information, or understanding) is obstructed, then we can class the situation as an (intellectualist) problem. Given appropriately developed translation procedures, we could infer "I lack information about what causes leafblowers to make as much as they do" from the above question. And given a proposition that describes an agent's epistemic state of lacking information, we could infer a question from that proposition. Briefly put, a question presupposes an intellectualist problem proposition, and with the right translation procedures, we could move back and forth between the two.

But we cannot develop such translation procedures for problems that aren't epistemic states of lacking information. Such problems engender many questions. If an agent asserts that leafblowers are noisy, we can't infer that the agent doesn't know why they are noisy. She may know perfectly well, and she may be developing a noise damper. Rather, the problem engenders many questions: How noisy are they? For whom are they noisy? Where are they noisiest? When are they nosiest? What is their range of noise? How can we dampen the noise? Why do people use leaf blowers? Etc. For every answered question, people gain information with which they can ameliorate the problem.

Older accounts of problems and questions captured an aspect of many problems pursued in scientific practice. In such cases, researchers disvalue their situations of lacking knowledge, situations that can be expressed either as problem propositions or as presuppositions to questions. The distinctions I offer enable us to maintain most of the usefulness of those accounts, such as rough distinctions between conceptual and empirical problems (Laudan 1977), the abstract structure of problem propositions (Nickles 1981), and an interrogative account of scientific discovery (Hintikka 1981).

There's a second general reason for why it's fruitful to distinguish problems from questions as I do above. The distinction enables us to conceptualize, beyond the epistemic-state or intellectualist problems presupposed by questions, a larger variety of problems that researchers pursue and that give significance to their research projects. Such worldly problems are aspects of projects that comprise a vast expanse of scientific practice, including the study of fault lines near cities, extinctions, the physical decay of art, and disease, to name just a few. Often ignored by those who study science, such research has the potential to reveal not only how researchers design and conduct research, but also how they make trade-offs between competing values (Elliott and McKaughan 2014).

Third, the distinction I propose enables the creation of tools to identify the problems and questions pursued by researchers. Insofar as we study science empirically, we need empirical tools to gather data about the aspects of research projects and how those aspects evolve over time.

For instance, the two sets of conditions, one for asserting that something is a problem and the other for posing a question, provide foundations from which to create questionnaires or surveys of researchers about the problems and questions they pursue, foundations that are themselves revisable in light of disconfirming evidence. Similarly, the abstract structures of problem propositions and of questions provide foundations from which to create content analyses of documents such as research papers, grant applications, lab notebooks, etc.

Fourth, the distinction between questions and problems enables the study of how those problems and questions function differently over the course of a given research project, and how they influence each other. Both can motivate scientists to design and conduct a project. But while questions focus research activity and function as criteria by which to select methods, problems often function as external justification for a course of action. Further, problems often engender or raise many questions, while the information used to address questions often ameliorates many distinct problems.

A team might be motivated to conduct their project because of one problem, but may invoke a different problem when applying for funding. It may ignore both of those problems and invoke still a different problem when reporting its results and convincing others to use its scientific products. Problems are not, however, mere rhetorical devices, as much of science evolves by using the same results or products to ameliorate different problems, and many products are evaluated by how well they ameliorate problems, and by how many problems they ameliorate.

Finally, the distinction between questions and answers enables more refined studies not only into how science is or should be evaluated and conducted, but also into how it is or should be designed. In that sense, the distinction is foundational in a general study of the conceptual foundations of research design, a field scarcely touched by philosophers of science, and one ripe for further study.

6- Examples

I describe two examples that highlight the above tools and the distinction between questions and problems. The first example straightforwardly fits my account, while the latter, while prima fascia challenging to my account and more amenable to older accounts, also fits it.

Example 1

In Death Valley in California and Nevada, there are many species of pupfish isolated from each other in streams and water holes. One species lives only in Devils Hole, a seemingly bottomless, hot, and geothermal hole only a few meters in width and breadth that sustains little life. Devils Hole pupfish are distinct from pupfish in sister species in that they lack pectoral fins. The pupfish eat mostly the small amounts of algae that grow in Devils Hole, and when shade ceaselessly occludes the hole for two months every year, algae can't grow and the pupfish population crashes. While the population rarely numbers more than a few hundred, during the shady season it has been recorded at barely a few dozen.

The Devils Hole species is one of the most endangered on the planet. The US federal government has explicitly valued and protected not only the species, but also the hole and the water in which it lives (Cappaert v. United States 1976). The US Fish and Wildlife Service, among other federal organizations, pursues several efforts to conserve the species.

In the framework of problems proposed in earlier sections, the population of Devils Hole pupfish is the thing valued, and its constant threat of extinction provides a problem at least to the

Fish and Wildlife Service tasked with conserving it. That problem has engendered many questions about the pupfish.

A recent team to study the pupfish was led by Christopher Martin, a specialist in the speciation of small fishes at the University of North Carolina, Chapel Hill. Motivated by the conservation problem facing the Fish and Wildlife Service, the team primarily asked: How long had the Devils Hole species lived in Devils Hole? But it also noted a cluster of related questions (Martin et al. 2016, 3). Did pupfish colonize the hole just once? If so, how did their populations avoid inbreeding depression and extinction?

To answer those questions, the team sampled DNA from preserved Devils Hole pupfish and compared it to DNA data from nearby sister species. They used that information to build several scientific products, including a metric of genetic diversity in and between species, a dated phylogenetic tree of the species, a DNA mutation rate, and a time-range in which the Devils Hole pupfish diverged from its sister species.

Given those tools, the team answered it primary question by inferring that the current species of Devils Hole pupfish colonized Devils Hole within the last three hundred years, a surprisingly recent event. And given geological record of inundations in Death Valley, the team concluded that the current species of Devils Hole pupfish may be just the most recent species to colonize the hole. They also found evidence of gene flow between the Devils Hole population and sister species, though kilometers of desert often separated those populations. Such gene flow could stave off inbreeding depression, though mechanisms are needed to explain how genetic material somehow traversed kilometers.

With its scientific products and answers to their questions, the team recommended some strategies to conserve the Devils Hole pupfish, or to ameliorate the problem facing the Fish and

Wildlife Service. The team suggested that the current species may be only the most recent in a series of species that have colonized Devils Hole, evolved, and gone extinct. To conserve pupfish in Devils Hole, if not the current species, fish and wildlife managers should preserve the possibility for genetic information, and perhaps new organisms, to flow to and from Devils Hole. Whether or not anyone employs those suggestions to ameliorate the problem of potential extinction of the pupfish, or of life in Devils Hole, remains to be seen.

Example 2

Not all research projects fit my account as nicely as the previous case. When many scientists describe their projects, they often don't identify questions or worldly problems as motivation or justification for their projects. They often invoke intellectualist problems, such as a lack of understanding of a phenomenon, or trouble for a theory. Such cases are those traditionally studied by those who study problem solving in science. But as my account conserves and clarifies previous accounts, it can still handle such cases.

In the late 1960s, Eric Davidson partnered with Roy Britten to develop a project about gene regulation. They noted evidence that genes yielded products that regulated how other genes made products, and ultimately how cells differentiate. Davidson and Britten valued knowledge of how genes control cell differentiation. And the problem, as they came to state, was that researchers knew little about those mechanisms in which genes regulate each other, and they had little theory in which to describe such mechanisms or figure them out (Britten and Davidson 1969).

To address that problem, Britten and Davidson proposed a set of scientific products, which included theoretical concepts and a general model of a gene battery, according to which gene products within a cell interacted with other genes in the cell to differentiate the cell into a given type. A few years later, they moved to Caltech to collaborate regularly. They pursued major grants together, and over time, their project and its motivating problems evolved. They helped to establish many instances in which genes regulated other genes in sea urchins.

But by the late 1990s, the theory had evolved enough that Davidson and an army of colleagues could pose a new problem. No one had provided a relatively complete example of a gene regulatory network, as the batteries had been renamed, according to which researchers could manipulate gene regulation and precisely predict the effects on a major developmental process. Davidson was aware of that situation, disvalued it as a challenge to his and Britten's theory, and exercised a considerable amount of effort to ameliorate it via a complex strategy (Davidson et al. 2002).

To address that problem, Davidson's team focused on the specification of endomesoderm cells in early sea urchin embryos, some of which develop into the distinctive juvenile skeletons found in many sea urchin larvae. The team systematically perturbed the expression of dozens of genes. Ultimately, they constructed a model of all the genes and their regulatory connections required to turn early sea urchin cells into endomesoderm cells, and ultimately juvenile skeletons. Given a model that ameliorated, but didn't completely solve, the above problem, Davidson's research evolved as he continued to refine motivating problems, from needing more detail on the endomesoderm network (Peter and Davidson 2010) to not knowing how it had evolved (Hinman et al. 2007).

7- Conclusion

In closing, I forestall some worries about the above sketch of problems and questions. First, while it characterizes problems and questions, is says little about solutions and answers, or about how researchers go about finding them. So the above sketches are incomplete. That point is right, and while a more thorough study of those topics is beyond the scope of this paper, it is ripe for future research.

Second, my examples draw evidence only from the published reports of scientists, reports that are known not to capture how their authors actually reasoned during the life of their research projects. As such, the above examples may systematically mislead readers about the problems and questions actually pursued in the projects, which the authors rationally reconstruct in their reports. To an extent, that point is also right. But it misses at least two larger issues.

First, within any given research project, the functions of problems and questions change over the life of the project. In early stages, they motivate researchers to act, in later stages they constrain the behaviors that researchers perform, and in still later stages they justify the project, its results, and its products to other scientists. Here, I focus my examples only on the late stage in which researchers publish their results. Fuller case histories, however, would identify the problems and questions, and how they changed, throughout the life of the projects.

Second, while scientists reconstruct their projects in their research reports, that practice is a worthwhile object of study. Those who study them should be mindful that such reconstructions sacrifice historical accuracy and have rhetorical functions. But those reconstructions, and the practice of making them, provide a window through which those who study science can piece together the rationales for projects, results, and scientific products that scientists find convincing. Problems and questions are often distinct aspects of those rationales.

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