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Research Problems

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Abstract

To identify and conceptualize research problems in science, philosophers and often scientists rely on classical accounts of problems that focus on intellectual problems often defined in relation to theories. Recently, philosophers have begun to study the structures and functions of research problems not defined in relation to theories. Furthermore, scientists have long pursued research problems often labeled as practical or applied. As yet, no account of problems specifies the description of both so-called intellectual problems and so-called applied problems. This paper proposes a new conceptual framework of problems that accounts both for intellectual aspects of problems and for their practical or applied aspects. I illustrate the account with an example of a recent research project from evolutionary biology, and I indicate further routes by which to develop the account, especially in connection to empirical studies of science.

1 Introduction

At the height of discussions about historicist systems of scientific rationality, philosophers developed influential accounts about the structure of research problems (Nickles [2017]). They defined problems as lacks of knowledge, as anomalies for theories, or as something like hurdles to knowledge (for example, Laudan [1977]; Nickles [1981]). Almost universally, they treated problems as in some way corresponding to questions, for which a common gloss was that problems in science are unanswered questions for which theories provide answers (Laudan [1977]). Science evolved as it solved these intellectual problems. Insofar as philosophers discussed other kinds of problems, they classed them as practical or applied problems and set them aside as either uninteresting or unfit for philosophical study (for example, Kuhn [1962]). While many have adopted the general program of historicizing science, few have lately paid attention to the structure of research problems, with a notable exception of Alan Love's account of problem agendas (Love [2008], [2014]).

More recent philosophy of science indicates that classical accounts of problems can be fruitfully augmented. First, philosophers increasingly describe and study the processes, practices, and reasoning strategies of actual science, including problem-solving (Soler *et al.* [2014]). For example, Nancy Nersessian conceptualizes research laboratories partly as problem-spaces, and with Miles MacLeod she empirically describes how researchers adaptively solve problems (Nersessian [2006]; MacLeod and Nersessian [2016]). Similarly, Love uses his account of problem agendas to show how developmental biologists organize their knowledge around sets of related questions, and not necessarily around overarching theories (Love [2014]). Second, and relatedly, philosophers increasingly study integrative or interdisciplinary research, and they note researchers' professed aims to solve problems that are complex, socially significant, or both (for example, MacLeod and Nagatsu [2018]; Boon and Van Baalen [2019]). In both of these programs, problems have been shown to function in scientific research without being criteria for the evaluation of overarching causal theories. This result outpaces the resources of classical accounts of problems to describe the structures and functions of problems actually operative in science.

Additionally, so-called practical or applied problems have long functioned as motivations and as benchmarks in science. Medical researchers work to solve problems like diseases, traumatic injuries, and chronic ailments or disorders. Conservation biologists work to solve problems like destructions of species and ecosystems. Agriculture researchers work to solve problems of malnutrition or low crop yields. Psychiatric researchers work to lessen the burdens of mental traumas and affective disorders. And so on. These kinds of issues have been institutionalized in many public funding agencies, which often require applicants to demonstrate the public value of their research.[1] Some call for publicly-funded science in particular to solve concrete or applied problems (Sarewitz [2016]), and for research into how values influence people as they characterize and solve of these kinds of problems (for example, Miller *et al.* [2014]).[2] Again, classical accounts of problems are outpaced, as they are unable to characterize these kinds of problems as operative in actual science.

In this paper, I propose a framework of research problems that augments classical accounts for contemporary needs. The framework has three primary parts. First, a 'slogan' provides a rough definition for research problems. It says that problems are situations or states of affairs in which something valued is harmed or is obstructed from reaching an end valued and assigned to it. Next, a 'general model of problem propositions' provides details for describing situations as problems. If the conditions in the model are true of a situation, then the situation is a problem, even if no one is aware that the conditions are met. Third, a 'general agential model of problems' embeds the general model of problem propositions in a theory of action for agents. When agents assert that they believe or accept the conditions of the agential model for a given state of affairs, then they treat the state of affairs as a problem for them.

This framework augments, rather than refutes, classical accounts of problems. It sets aside artificial distinctions between so-called intellectual or pure problems on the one hand, and so-called practical or applied problems on the other (Douglas [2014]). On this unified framework, problems vary in their intellectual and practical dimensions, and few (if any) problems are entirely intellectual or entirely applied. Furthermore, it extends the historicist

[1] Grant writers notoriously struggle to show how their proposed projects demonstrate public value. One reason for such struggles, I suggest, lies in our lack of a sufficiently robust conceptual framework for research problems, and for tying those problems to research questions.

[2] Sarewitz ([2016]) advocates for solving concrete practical problems, and against merely answering intellectual questions posed by scientists, as something between the proper aim of much of science and the best route for scientific innovation. In contrast, I hold more weakly only that solving so-called concrete problems is but one (albeit often powerful) aim of much of science, and that my framework of problems helps conceptualize them.

project of treating problems as objects that can be empirically studied, while also tying them to contemporary discussions about values and practices in science (Laudan [2004]; Douglas [2016]). Finally, by indicating that problems can engender multiple questions, it complements Love's account of problem agendas (Love [2008], [2014]).

This framework has both descriptive and potentially normative functions. For practice-oriented philosophers, it provides a set of tools for identifying and characterizing problems that motivate research, regardless of whether or not those problems are defined in relation to theories or whether or not they are classed as intellectual or applied. While it provides a general conceptual model, it also indicates routes by which to operationalize the model for empirical studies. Because the framework doesn't define problems in relation to theories, it can be used to study research problems in a variety of contexts, including in projects that integrate or cross disciplines, focus on complex phenomena, or are traditionally classed as applied. The more that descriptive studies employ and test the framework to see if it is descriptively accurate for successful projects, the more they might vindicate it for potential normative uses by practicing scientists (Laudan [1990]). For researchers more generally, then, this framework potentially provides a normative template for characterizing research problems during the design, execution, and justification of research projects.[3] That said, the descriptive function is primary here.

My strategy for this paper is as follows. In the subsequent section, I review classical accounts of problems and contemporary discussions of problems in science. Drawing lessons from that review, I next propose a new conceptual framework. Sections 4 and 5 illustrate the framework with a case from evolutionary biology. I conclude by addressing some challenges to my framework and by indicating some routes for fruitfully developing it.

2 Standard Approaches

Below, I review six overlapping and relatively standard approaches for conceptualizing problems in science, and I show their focus on intellectual problems. This review merely describes that

[3] Nickles's influential ([1981]) paper was titled 'What Is a Problem That We May Solve It?', but it provided limited guidance for scientists looking for help in the description of research problems. This paper, with a broader scope of science in view, can be seen as addressing a more focused question: what is a problem such that we may (empirically) describe it?

focus, without trying to explain it, which is a task beyond the scope of this paper. Next, I review more recent discussions of problems in science, from which I distill criteria by which to develop a new framework.

2.1 Six classical approaches

The first approach treats problems as unmet wants or goals (Simon [1966]; Newell and Simon [1972]), or as unmet theoretical goals such as explaining phenomena or logical consistency (Boas [1937]; Popper [1972]; Agassi [1975]; Hattiangadi [1978], [1979]; Laudan [1977]; Nickles [1980], [1981]). On these accounts, notions of problems run the gamut from being overly broad to overly focusing on purely intellectual problems. If any unmet want is a problem, then unmet frivolous wants count, and the notion of problem collapses into that of wants. Despite that breadth, Simon and Newell focused on a subset of problems and puzzles for which people can specify stepwise procedures for attaining their goals, such as proving a mathematical theorem or programming a computer. They influenced most others to study only the intellectual aspects of problems in science, a program taken to its extreme by Hattiangadi ([1978]), who defined all problems in science as sets of logically inconsistent statements of theories, theorems, phenomena, and possible solutions.

A second approach highlights the historical aspects of problems and their roles in investigation (Dewey [1938]; Kuhn [1962]; Popper [1963], [1972]; Agassi [1975]; Laudan [1977]; Hattiangadi [1979]; Nickles [1981]). This approach tends to treat the evolution of science as in some way related to the set of solved problems, with a field of science evolving as it solves problems or puzzles set for it within an overarching paradigm or tradition. Kuhn influentially set aside problems like curing cancer, which he said aren't assured to have solutions, and therefore can't function as tests of ingenuity (Kuhn [1962]). Laudan argued that solving problems is the aim of science, and like Kuhn he said we could only understand the historical processes of science by studying how scientists solve problems (Laudan [1977]). Laudan conceptualized solutions as claims inferred from theories. For this second approach, the only problems and solutions of interest have logical relations to each other via theories. As with the first approach, the second approach focuses on highly intellectualized problems.

A third approach treats problems either as unanswered questions or as tightly connected to unanswered questions (Boas [1937]; Brown [1975]; Laudan [1977]; Hintikka [1981]; Nickles

[1981]; Agre [1982]; Goldman [1986]). This move enables researchers to explicate problems, problem solving, and solutions with the comparatively well-developed tools from the study of questions. Some don't identify questions with problems, but rather use the former to help define the latter. For instance, Boas said that a problem is a statement of a theory, a statement of a deviation from it, and a posed question (Boas [1937]). Regardless, these accounts all define a problem either as, or partly constituted by, a lack of knowledge or understanding. As with the previous two approaches, this approach reinforces a limited scope on highly intellectualized problems.

A fourth approach treats problems as sets of constraints on solutions (Newell and Simon [1972]; Brown [1975]; Nickles [1978], [1980], [1981], [1988], Haig [1987]). Nickles provided the most detailed account, for which a problem is an abstract object that includes a set of structured constraints that describe a range of possible solutions and a demand for selecting at least one such solution. In the process of investigation, researchers specify, structure, and differentially weight those constraints. Furthermore, researchers often don't specify all constraints, as some remain tacit. Nickles regards theories as in some sense solutions to problems, reinforcing the focus on intellectualized problems as the only ones relevant for science.

A fifth approach focuses on the relations between agents and problems (Agre [1981]; Goldman [1986]). On this approach, a problem is defined partly by a description of a situation and of agents' attitudes in relation to that situation. On Agre's account, which has had more influence in social sciences other than philosophy, agents must be aware of an unanswered question or situation, disvalue it, believe effort is required to specify and solve the problem, believe the situation is solvable, and believe language of solving is appropriate for describing the activities of addressing the situation. Agential accounts like Agre's have the potential to bridge abstract accounts of problems with accounts of the cognitive attitudes and behaviours of those who address problems.

Agre and Goldman focused on intellectual problems constituted partly by unanswered questions. As a result, their accounts struggle with the practical dimensions of problems and with situations in which relevant agents are unaware of problems. For example, if someone has late-stage undiagnosed pancreatic cancer, and doesn't know it, on these accounts that person doesn't have a problem. As soon as she became aware of it, however, she does have a problem. A different route is to say that the person has a problem from the beginning of the cancer, but that the problem can't motivate her behaviour until she learns of it. Accounts in the fifth approach can't pursue that route, but the account I provide below does.

A sixth approach provides taxonomies of problems. Laudan distinguished empirical from conceptual problems, and he sketched subkinds for each (Laudan [1977]). Simon distinguished well-structured problems, for which there are clear tests to determine if something is a solution, from ill structured problems, for which there are not (Simon [1973]). Ill-structured problems were alternately branded as wicked problems and further discussed by Rittel and Webber ([1973]), who influenced many in the social sciences to treat scientific problems as solvable puzzles and distinct from "wicked" societal problems.

2.2 Contemporary discussions

More recent discussions of problems examine the practices and reasoning strategies used in a wider array of sciences. Alan Love ([2008], [2014]) proposes an account of problems that differs in content and function from previous accounts. Love acknowledges that the problems that motivate particular research projects are relatively specific unanswered conceptual or empirical questions. Their specificity, however, is relative to problems in a second sense, which he suggests for some sciences are something like characterizations of general (often process) phenomena (e.g. morphogenesis in developmental biology). Problems in this second sense function as principles by which to organize large swaths of related knowledge that is theory-informed but not theory-unified, especially in didactic tools like textbooks and review articles. They also function as agendas by which to specify more precise research questions. As agendas they have the form of lists of conceptual or empirical questions that can be increasingly specified and interrelated in hierarchies, and they have histories that can be studied to determine how the structure of knowledge has evolved over time, especially in fields that aren't organized around central theories. Love ([2008]) also argues that problem agendas can structure integrative fields like evolutionary developmental biology.

Other recent discussions about problems relate to interdisciplinarity. Many theorists of interdisciplinarity claim that its various forms (for example, multi-, cross-, or trans-disciplinarity) are necessary to address complex problems, especially real-world and global problems such as poverty and climate change (for example, Pohl and Hirsch Hadorn [2007];

Huutoniemi *et al.* [2010]). Philosophers of interdisciplinarity acknowledge those problem-solving aims, with many showing how inherited epistemic tools of science constrain researchers from achieving those aims (Andersen [2016]; O'Rourke *et al.* [2016]; MacLeod and Nagatsu [2018]; Boon and Van Baalen [2019]). One such constraint is about how to conceptualize problems, as theorists often default to problems as unanswered questions (for example, Pohl and Hirsch Hadorn [2008]). In a discussion of this constraint, Jan Schmidt ([2011]) proposes instead that problem-focused (interdisciplinary) research roughly requires knowledge about undesirable states, desirable goal states, barriers from moving from on to the other, and techniques for doing so. Schmidt explicitly calls for further research into how to conceptualize problems so that interdisciplinary research can be more effectively designed and executed.

2.3 Further Directions

The review above establishes that classical accounts of problems universally focus on the intellectual aspects of problems in science, and that there is a contemporary need for accounts of problems that can accommodate practical or applied aspects. It also indicates some directions by which to fashion a new account of problems. I note at least five desiderata for a new account. From the first approach, an account of problems shouldn’t be so broad that it collapses notions of problems into notions of wants or desires. Second, previous accounts need to be augmented to describe the practical or applied aspects of problems. Third, a useful component of problems is a demand or request to address them. Many describe such demands as ones for *solving* problems. But if humans are satisficers, a more apt approach is to detail a demand for *ameliorating* problems, of making them at least partially better or less bad, language I adopt below. Fourth, how can demands for amelioration be justified? Agre usefully indicated that appeals to values help address that question, a route I develop below, and that can help distinguish problems from wants or desires. Finally, to study science, an account of problems should conceptualize problems as historical entities with social contexts, and it should still account for highly intellectualized problems. Below, I satisfy these desiderata with a new framework.

3 A New Framework

From the accounts reviewed above, I note at least four kinds of tools used to conceptualize problems. First are brief slogans to help locate, but not completely specify, reference classes of problems. Second are general and abstract models by which to more precisely specify which items go in those classes (and which don't). Third are accounts by which to specify the relations between agents and the items in a reference class of problems. Fourth are taxonomies to help order the items in the class. Previously, philosophers used at best one or two of those kinds of tools to develop their accounts of problems. For my framework, I use the first three and discuss a role for the fourth. I detail each tool below, and I indicate how they overcome issues endemic to earlier accounts.

3.1 Slogan and reference class

Brief slogans can help locate a class of items, without fully specifying the class. The slogan below does so for the set of problems, outlining the reference class for the term 'problem'.

> Slogan: A problem is a state of affairs or situation in which something valued is harmed or is obstructed from reaching an end both valued and assigned to it.

Though grammatically passive, the slogan enables us, when treating some situations as problems and others as not, to note the relevant roles of agents. If something is harmed, agents must have valued the thing to enable determination of that harm. Similarly, if something is obstructed from reaching an end, agents assigned that end to the thing and valued that end. Consider a boulder on a mesa in the wilderness of Arizona, where the elements erode it. If no one values the boulder, or assigns an end to it that erosion obstructs, then the erosion of the boulder isn't a problem. But if the boulder has rare petroglyphs on it, then many might well describe the erosion as a problem. By doing so, they value the boulder for its petroglyphs, and they treat its erosion as a harm. Situations are problems partly due to the relevant harms, values, ends, and agents who hold those values and assign those ends.

This general approach has several benefits. First, it enables us to capture that people can (and often do) disagree about the extent to which some state of affairs is a problem, and it enables us to diagnose reasons for such disagreements. For instance, loggers and conservationists might agree that owls and the forest those owls live in have value, but disagree about the extent of such value relative to the value of the trees to the logging company, their employees, and their clients. Conservationists value the forest and the owls more than the ends of the logging company and their clients, and the opposite is true of the logging company. For science, the above slogan explicitly connects discussions of problems in science to discussions of values in science (Douglas [2016]). A host of values influence scientists as they conceptualize problems, develop projects to ameliorate those problems, and select criteria by which to evaluate whether or not those problems have been ameliorated.

Second, this approach avoids issues of being too broad or too limited in scope. Recall accounts that identify problems with unmet wants, no matter how frivolous. A chemist might want and lack a helipad on the roof of her lab. But the lack of a helipad isn't a problem until the chemist can show how the absence of a helipad causes some harm to something or obstructs some valued end. While the chemist might struggle to do so, a pathologist at a rural hospital who often needs to transport samples to bigger hospitals elsewhere, might not. My framework overcomes issues of unnecessarily broad scope by relying on distinctions between wants and things valued, invoking the latter notion. On the issue of limited scope, my framework allows for values of things besides answers to questions or new knowledge, and is thus not unduly focused on intellectualized problems.

Third, my framework indicates the often empirical aspects of problems. On some classical accounts, problems are treated as unanswered questions, with little if any empirical work needed to establish the existence of a problem. On my framework, as on other historical accounts, empirical work is often needed to establish that some situation is in fact a problem. Doing so requires showing that something is in fact valued, harmed, and/or obstructed from reaching an end. For instance, if a biochemist works to develop a palliative for the symptom of a disease, some early tasks she might do are to establish the extent to which people have the disease, the extent to which those who have the disease have the symptom, and how much the symptom harms them or prevents them from reaching ends they would otherwise set for themselves. All three of those phenomena can be established only empirically. The situation is not a problem if no one has the disease, if those who have it don't have the symptom, or if the symptom causes them no harm or obstacles to their ends.[4] On many classical accounts, a problem is partly solved when it is clearly specified and related to theories. I'm sympathetic, but I further argue that for many problems, they can't be specified without being empirically established.

3.2 Statements and a general propositional model

Getting clear about how 'problem' functions in sentences and propositions provides several benefits. Doing so provides a simplest case model for locutions about problems, and it enables a more precise definition of the terms in those locutions. Below I detail the general structures of problem statements and of a model of problem propositions.

> Problem Statement: A sentence that describes a situation *X* as a problem. Put differently, a problem statement expresses the proposition '*X* is a problem' and specifies the *X*.

In a problem statement, the *X* that is specified is a situation or state of affairs.[5] For instance: 'the logging of the forest is a problem'. In addition to specifying the state of affairs, the statement implies many things about the state of affairs. Those implications are detailed by a problem proposition.

A general model of problem propositions does several things. It provides a more precise definition of 'problem' than does the brief slogan above, and it provides the fillable semantic schema for any given problem proposition expressed in a statement. I treat instantiations of the schema as individual problem propositions, which collectively specify the class of problems more precisely than does the slogan. The general model of problem propositions also provides a conceptual core around which to construct an agential model of problems, as I later indicate.

[4] This empirical aspect of problems underwrites, for instance, critiques of politicians for regulating states of affairs that aren't problems and of pharmaceutical companies for creating drugs for conditions that aren't problems.

[5] Dewey claimed that problems were situations of indeterminacy independent of peoples' cognitions of those situations, but that spurred people to inquiry (Dewey [1938]). The framework developed in this paper can be seen as an explication of Dewey's suggestive view.

General Propositional Model for ‘*X* is a problem’: The model is an abstract object, the instantiation of which requires at least:

P1. a set of propositions that describe a situation (the *X*), including propositions about harms and/or obstructed ends

P2. a set of propositions listing the agents who have assigned value or ends to items in the state of affairs

P3. an evaluative proposition of disvalue about the situation

P4. an imperative proposition to ameliorate the situation

P5. a set of propositions that describe constraints on the amelioration

A given problem proposition is true when its component propositions are well formed, and when the component propositions in conditions P1, P2, and P5 are true.[6] As discussed earlier, the existence of harms and obstructions described in P1 often need to be empirically established. The same holds for the propositions collected in P2 and P5. Doing so enables us to account for differences in judgements about problems across agents, as in the logging example.

The propositions in P5 can be especially difficult to establish. In practice, many such propositions are or can be empirically verified, while others remain implicit, unverified, and potentially obstructive to developing strategies to ameliorate problems. One heuristic to describe and ultimately ameliorate problems is to identify and root out implicit or ungrounded assumptions collected in P5. Doing so makes for more accurate and explicit problem propositions. For instance, in the logging example, assume that the company calculated an expected profit from the sale of the trees, but didn’t factor in costs for litigation with the conservationists. Once it learns that such litigation is likely and will constrain its budgets, the company revises its profit estimates, which the conservationists hope will prove too tenuous for the company to proceed.

[6] Realists about values might claim that the proposition in condition P3 (and maybe P4) should also be true. They might then explain cases of disagreement about problems as those in which one party asserts a true problem proposition while other parties assert false ones. This realist position has much to recommend it, especially as a tool to potentially describe how actual agents conceptualize their disagreements. For expository purposes, however, I use the weaker version above and remain agnostic about value realism, enabling the overall framework to function alongside a range of philosophical commitments.

The above model is largely inspired by Thomas Nickles's ([1981]) account, on which a problem is an abstract object that includes a set of structured constraints that describe a range of possible solutions and a demand for selecting at least one such solution. Nickles treats the specification of problems as historical processes. In early stages, a description of a problem includes that of a goal, a demand to meet the goal, and constraints that determine the range of admissible solutions. In middle stages, researchers describe more constraints, root out tacit constraints, revise previously held constraints, and thus zero in on an increasingly precise range admissible solutions. In the final stages, the description of constraints indicates precise solution(s).

While I adopt Nickles's broad-stroke historical picture, my account is more general than his in several ways. First, I countenance more kinds of situations as problems than does Nickles, who focuses on obstructed aims in relation to theories. My account countenances harms or obstructions to theoretical aims, but also to anything valued or assigned an end. Second, my account explicitly includes propositions to describe states of affairs, relevant agents, and evaluative propositions, while Nickles's account at best lumps those propositions as kinds of constraints. Finally, I replace discussions of solutions with that of ameliorations.

Nickles's account everywhere treats scientists as agents, but Nickles never applied his account to agents. He aimed partly to account for 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 no such tools. I do so next.

3.3 Agential model

Agential accounts of problems do several things. First, they make explicit the relations between agents and problems such that the latter enter into the consciousnesses of agents and influence their behaviours. Second, they enable empirical studies of such behaviours. But extant agential accounts have limitations. While Agre and Goldman proposed such accounts, they treated problems as either identical with, or partly comprised of, questions. Furthermore, many treat agential accounts as importantly distinct from most other approaches to problems, so agential accounts have had more influence in social science and analytic epistemology than in philosophy of science. The model below avoids those limitations.

Agents (straightforwardly) assert that '*X* is a problem' only when:

A1. They accept or believe propositions that describe the state of affairs *X*

A2. They accept or believe propositions about the value of items in *X*, or about the value of the ends or aims of things in *X*

A3. They accept or believe propositions about relevant harms or obstructions of ends in *X*

A4. They disvalue *X*

A5. They accept an imperative to ameliorate *X*

A6. They accept or believe that effort is needed to further specify or to ameliorate *X*

A7. They accept or believe that a possible strategy of action could ameliorate *X*

A8. They accept or believe propositions that describe constraints on the above effort and strategies

A9. They believe that, when addressing the problem, it's appropriate to use the language of ameliorable (ability), ameliorating (process), and ameliorated (result).

The above model is inspired by Gene Agre's ([1982]) account, which explicitly invokes the roles of agents and of values in the determination of problems, and which specifies cognitive attitudes between agents and propositions about problems. My model goes beyond his in several ways. First, conditions A6, A7, and A9 are modifications of his account that, along with A5, employ language of ameliorating rather than of solving. This difference enables the account above to cover cases in which agents accept (perhaps falsely) that problems cannot be solved. For instance, consider again the case of late-stage cancer, and assume that the patient receives a terminal diagnosis from an oncologist. While neither the patient nor the doctor assert that the cancer is curable, nor that the situation is solvable, they nonetheless accept the potential for effort and strategies to make the problem less bad. For instance, the patient might put her final affairs in order, reconcile with loved ones, and receive palliative care. Standard accounts of problems, which require solutions instead of piecewise ameliorations, struggle to account for such cases, which are easily handled on the account proposed here.

Second, Agre relies on belief as the sole cognitive attitude that relates agents to aspects of problems, while my account sometimes uses the weaker attitude of acceptance. I do so when relating agents to empirically testable propositions. By doing so, my model can better capture cases in which agents accept a problem based on available evidence, but perhaps don't fully believe that the problem exists.

Third, the agential model above explicitly ties to the general propositional model from Section 3.2, while Agre's account is comprised entirely by his set of agential conditions. On the above agential model, conditions A1 and A3 embed P1 from the propositional model, A2 embeds P2, A4 embeds P3, A5 embeds P4, and A8 embeds P5. By doing so, my overall framework can account for situations in which problems exist, even if no one asserts as much, whereas Agre's account cannot. For instance, consider again the case of undiagnosed cancer. For it, an agent has a problem even if she's unaware of the problem. To make sense of such cases, we say that the agent hasn't accepted or believed propositions described in conditions A1, A3, or A6–A8, so she can't straightforwardly assert that her cancer is a problem. That said, the claim 'her undiagnosed cancer is a problem' is true according to the propositional model from Section 3.2.[7]

One of the primary benefits of agential accounts is that they enable empirical studies of problems as they impact people's behaviours and practices. To conduct such studies, social scientists and historians need tools to collect data about the problems people pursue. Such tools include surveys, interview protocols, and content analysis protocols. Those tools require any given concept of 'problem' to be operationalized. The agential conditions provide a touchstone for such operationalizations. In effect, they place the general model of problem propositions in a theory of action for agents, such that the relevant action is assertion. Given that placement, those who study science can develop tools to ask scientists if they think a situation is a problem, and if they are pursuing the amelioration of that problem.[8]

[7] One technical move to establish this is to use generalized propositions for conditions P3 and P4. For instance, if P3 says something like 'For anyone's cancer, Ayesha disvalues it,' then condition P3 can still be met without Ayesha knowing that she has cancer. In this case, the proposition 'Ayesha's undiagnosed cancer is a problem' can be true, even though Ayesha can't straightforwardly assert as much because she doesn't accept or believe that she has cancer as part of A1.

[8] Those who use such tools reconstruct the model A1–A9 above for scientists, who themselves may not have consciously put the pieces of the model together, but who nonetheless exhibit the cognitive behaviours listed.

3.4 Taxonomies

The development and use of taxonomies of problems is an aspect of scientific practice. I don't insist upon particular taxonomies for my overall framework of problems, but I do provide general roles for them as theoretically grounded or as heuristics. I treat taxonomies as providing the dimensions of individual problems, not eternal partitions within the reference class of all problems.

A taxonomy has a theoretical justification when it explicitly ties the criteria for distinctions to the set of propositions that describe constraints on the ameliorations of the problems. To see an instance of how this works, divide a reference class of problems so that you have two kinds of problems: problems_1 and problems_2. Define the kinds such that all problems_1 are similar to each other because, for them to be ameliorated, they require conditions A, B, and C to be met; problems_2 are similar to each other because, for them to be ameliorated, they require conditions L, M, and N to be met; and neither A, B, or C are identical to L, M, or N. In this way, we have distinguished the two kinds of problems by appeal to constraints on their ameliorations, and if the constraints are in fact constraints, we have a justification for the distinction.

More commonly, taxonomies function as heuristics. I treat heuristics, following Wimsatt ([2007]), as algorithmic tools used by satisficing agents to transform intractable issues into tractable ones. Because heuristics involve false or imperfect models of the world, they can fail in systematic ways. Agents use heuristic taxonomies of problems at least to help them roughly sort reference classes of problems, search through those sorts, and identify potential practices for ameliorating those problems that have proven successful elsewhere. For instance, one could employ a distinction between problems pursued in all of science between 1950–85 and those problems pursued in projects funded by the US government between 1975–99. Such a distinction might be useful, for instance, if one is researching how US federal support for scientific research changed after the 1980 election of Ronald Reagan as US president. But the distinction distinguishes problems only by historical features, not by structure, and will likely matter little beyond the context of that particular investigation.

3.5 A framework of problems

The tools above jointly provide a conceptual framework of problems (Figure 1). The slogan provides a shorthand for talking about the framework, and it roughly delimits the reference class of problems. Problem statements express specific problem propositions, which collectively and more precisely specify the reference class. The general model of problem propositions provides a schema for structuring those propositions. It also provides the conceptual kernel embedded in an agential model of problems. Given the agential model, we can operationalize the concept of problems and develop tools, such as surveys and interview protocols, to collect data about which problems people identify and pursue. Finally, with taxonomies of problems, we at least heuristically partition the class of problems to better organize it for our ends.

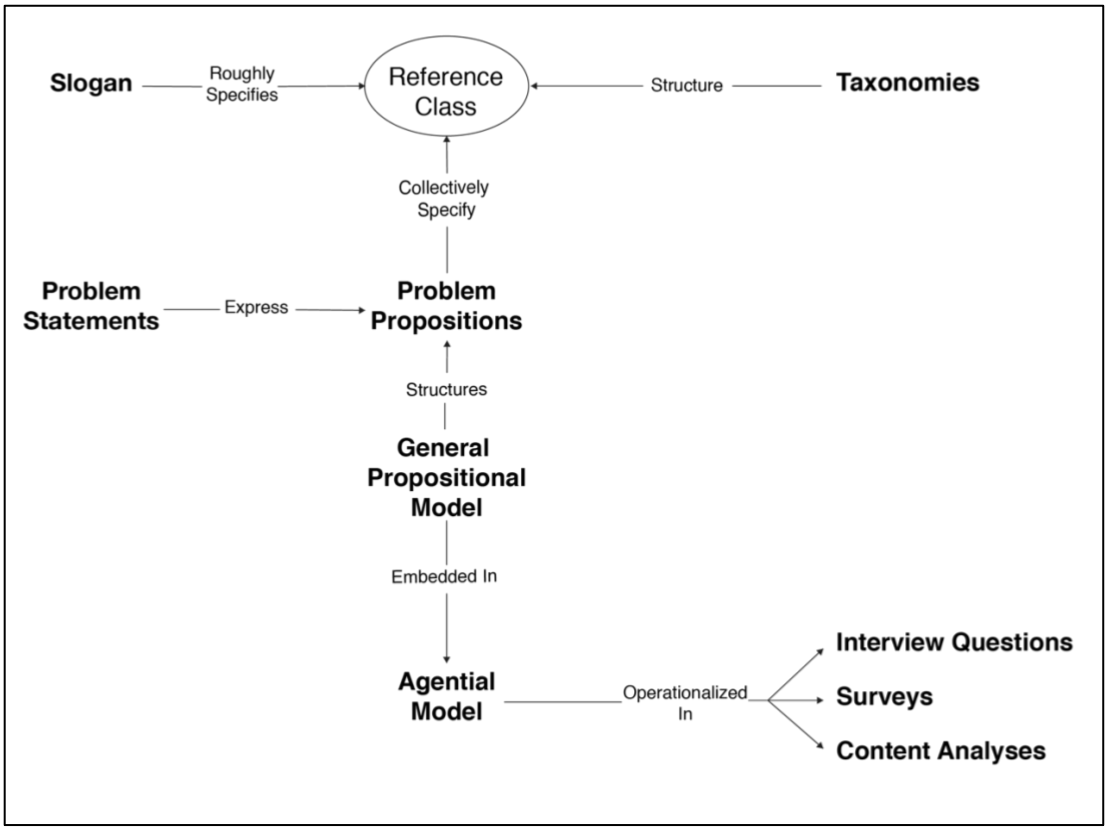

Figure 1. A Conceptual Framework of Research Problems.

4 Case Study

I illustrate the framework of problems above with an example from evolutionary biology. I use this example because, at first glance, it seems as if the research team in the example pursues problems that have only intellectual dimensions, as they explicitly report and address four questions. In the next section, I show how to transform those and other research questions into problem statements, thus preserving the tradition of treating research questions as problems. In this section, however, I use my framework to reveal the underlying practical dimensions that contextualize the team's research.

In Death Valley on the border of California and Nevada, many species of pupfish live isolated from each other in streams and water holes. One species, *Cyprinodon diabolis*, lives only in Devils Hole, a geothermal cavity only a few meters wide at its mouth, but that plunges into a chasm more than 90 meters deep that is too hot to sustain much life. *Diabolis* are blue or brownish, shorter than a pinky finger, and lack pectoral fins, which distinguishes them from sister species. The pupfish eat mostly the algae that grows at the mouth of Devils Hole, and when shade ceaselessly occludes the hole for two months every year, the 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 (Brown [2017]).

A recent team to study *diabolis* was led by Christopher Martin, an expert in fish speciation at the University of North Carolina, Chapel Hill. The team published their initial results in 2016, and they pursued several questions (Martin *et al.* [2016], p. 3). How long has the *diabolis* lived in Devils Hole? Have pupfish colonized the hole only once thousands of years ago? If so, how did their populations avoid inbreeding depression and extinction? Did phenotypic plasticity enable *diabolis* to adapt to the hot environment of Devils Hole?

To answer those questions, the team did several things common in evolutionary biology. They sampled DNA from preserved specimens of *diabolis* and compared it to DNA from nearby sister species. They used that information to build several scientific products, including measures of genetic diversity in and between pupfish species, a phylogenetic tree of the species with estimated dates of their speciations, a DNA mutation rate, and a time-range in which *diabolis* diverged from its sister species. They also bred *diabolis* with fishes from sister species, and they took photos of hybrids' pelvic fins (and the lack thereof).

Given those products, the team answered its first question by inferring that *diabolis* colonized Devils Hole within the last 850 years (Martin and Turner [2018]), a surprisingly recent event. And given the geological record of floods in Death Valley, the team concluded that *diabolis* may be just the most recent species to colonize the hole. They also found evidence of gene flow between *diabolis* and sister species, though kilometers of desert often separated those populations. Such gene flow could stave off inbreeding depression, a feature common to small populations like that of *diabolis*, though mechanisms like floods are needed to explain how genetic material somehow traversed the desert between otherwise unconnected waters. Finally, phenotypic plasticity could have enabled *diabolis* to adapt to the heat and limited resources of Devils Hole, as breeding experiments indicated that pupfish lost their pelvic fins in response to warm temperatures.[9]

The questions and their answers provide only some of the rationale for Martin's project.[10] One of the co-authors on the published report, Lee Simons, indicates the broader context. Simons was a senior fish biologist for the US Fish and Wildlife Service in Nevada. The service has a clear mandate to conserve *diabolis*, one of the most endangered species on the planet, and all three branches of the US federal government have afforded some protection to the species and its habitat (Brown [2017]).

Simons collaborated with Martin's team for several reasons. First, without Simons, Martin's team would have had a tougher route to get the genetic materials and fish specimens needed to conduct their study. Second, Simons and others in the Fish and Wildlife Service need the information generated from research like Martin's to help them figure out how best to execute their conservation mandate. Third, the threat of extinction provided a motivation by which Martin and his colleagues could divert some of their time and resources away from their other projects, which focus on fish in other parts of the world, to study *diabolis* evolution.

I use the framework of problems provided above to precisely characterize the broader problem that partly motivated Martin's team to ask their four questions and to conduct their research.

Problem Statement: The high risk of *diabolis* extinction is a problem.

9 Those answers, especially about the age of the species, are hotly debated among evolutionary biologists. A team led by İsmail K. Sağlam at the University of California, Davis, argues that *diabolis* originated roughly at the same time as Devils Hole itself, roughly 60 thousand years ago (Sağlam *et al.* [2016], [2018]).

10 To illuminate the further rationale, we might establish how those questions fit into a disciplinary problem agenda in the sense of Love ([2008], [2014]). Another route is to show how the questions relate to issues traditionally labeled as practical or applied. Both routes complement each other. I pursue only the second route here to show that my framework can account for the applied aspects of problems, and I acknowledge that the first route remains fruitful, but beyond the scope of this paper.

Problem Proposition:

1. Set of propositions describing the situation.
    a. *Diabolis* exists only in Devils Hole, in Nevada.
    b. Its population is small, numbering between a few hundred and a few dozen, depending on the time of year.
    c. For the past decade, the highest population numbers in a given year are three to four times smaller than those numbers in years from previous decades. So the population is shrinking.
    d. *Diabolis* is listed as endangered by the US Federal Government.
    e. *Diabolis* faces a high risk of extinction.
    f. The shrinking of the population harms the species by creating population bottlenecks that limit genetic diversity and increasing the risk of extinction.
    g. If *diabolis* goes extinct, it will be irretrievably harmed.
    h. If *diabolis* goes extinct, its ecosystem could be irretrievably harmed.
2. Set of propositions describing the things valued or assigned ends, and the agents who do the valuing or assigning.
    a. The US federal government, via all three of its branches, values *diabolis*, the Devils Hole habitat, and the surrounding ecosystem.
3. Evaluative proposition of disvalue about the situation.
    a. The US federal government disvalues the high risk of *diabolis* extinction and the harm such an extinction would cause to its ecosystem.
4. Imperative propositions to ameliorate the situation.
    a. *Diabolis* should be conserved, and its population should be returned to more stable numbers.
5. Propositions describing constraints on amelioration.
    a. *Diabolis* lives in a hot environment with little oxygen in the water.
    b. Individual *diabolis* live about a year.
    c. *Diabolis* eggs are difficult to collect and manage artificially.

d. *Diabolis* eggs often grow funguses and bacterial infections outside of Devils Hole.
e. Conserving the species will cost money, time, and effort.
f. Conserving the species will prevent those resources from being used for other efforts.
g. Etc.

Agential Model: In this example, agents include at least each of the three branches of the US federal government, the US Fish and Wildlife Service and especially their branches in Nevada and California—which have been tasked with conserving the species, and individual employees of those branches. The agential model could be specified for each of those agents, but here I focus on the Nevada Fish and Wildlife branch office, which most directly manages *diabolis*. The Nevada office created and still follows a species preservation plan as part of a more general plan to conserve Devils Hole and its surrounding Ash Meadows region (Seda [1990]), and the office regularly reports its activities (Brown [2017]). From those sources, the office makes explicit that it accepts propositions about the state of affairs describe above, about value for *diabolis*, about disvalue for the high risk of extinction, and about constraints to efforts for amelioration. The agency also holds that effort is needed to ameliorate the situation, that a strategy can help, and that it's appropriate to talk about ameliorating the problem.

Ultimately, Martin's team made several suggestions about *diabolis* conservation based on their results. In doing so, the team directly addressed the conservation problem facing the Fish and Wildlife office. 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. Martin's team suggested that the federal government, in valuing *diabolis*, might also value and conserve the potential for colonization and genetic transfer into and out of Devils Hole (Martin *et al.* [2016]; Martin and Turner [2018]). Whether or not anyone employs those suggestions remains to be seen. Doing so would require a new approach to the species conservation plan. Regardless, Martin's suggestions make sense only in light of the conservation problem described above, and not in light of the evolutionary questions explicitly invoked in their 2016 report.

5 Problems and Questions

In this section, I show how to preserve within my framework the treatment of unanswered questions as research problems, which is undeniably a part of science. There has been substantially more research into questions than into problems, yielding a wide array of topics and accounts (Cross and Roelofsen [2018]). I here rely on a standard account familiar to philosophers of science (Belnap and Steel [1976]).

Belnap and Steel distinguish questions from interrogative statements. They treat questions as abstract semantic objects posed by agents and that, in their most elementary forms, include a set of possible alternative answers and requests or propositions that indicates how many of the distinct alternatives the agent seeks. Belnap and Steel treat interrogative statements as bits of language that express questions. This distinction is mirrored in my framework of problems, which distinguishes problem statements from problem propositions, and for which the former express the latter.

Two lessons emerge from those distinctions. First, on my framework problems aren't identical to questions. Problems are states of affairs, while questions are abstract semantic objects. There is a category distinction between them.[11] While there are relations worth studying between problems and questions, identity isn't one of them. Second, a framework of questions could be built with the same kinds of pieces used in my framework of problems: a slogan, abstract semantic objects (general model of questions), linguistic expressions (interrogatives), agential models, and taxonomies. That task is beyond the scope of this paper.

Given those lessons, I sketch two different kinds of relations between problems and questions. The first is a semantic relation. Given appropriate translation procedures, we can translate a given interrogative statement into a problem statement. But we cannot necessarily translate any given problem statement into a single interrogative statement. To see why, consider the following example.

Martin's team asked: how long has *diabolis* lived in Devils Hole? For a (straightforwardly posed) question to possibly have an answer, it presupposes a proposition that

[11] Insofar as one ontologizes states of affairs as themselves abstract objects, then my claim is instead that problems have logical or abstract structure distinct from that of questions. Thus, the set of problems doesn't overlap with the set of questions, though the set of problem propositions overlaps with the set of questions, as discussed later.

describes a situation in which the agent asking the question lacks information.[12] If we treat lacking information as a situation in which something valued (for instance knowledge, information, or understanding) is obstructed, then we can class the situation as a problem of highly intellectual dimension. We can specify the following problem proposition:

Problem Proposition:

1. Set of propositions describing the situation.
    a. *Diabolis* exists only in Devils Hole, in Nevada.
    b. No one knows how long the species has lived there.
2. Set of propositions describing the things valued or assigned ends, and the agents who do the valuing or assigning.
    a. The US federal government and Martin's team value knowledge about the age of *diabolis*.
3. Evaluative proposition of disvalue about the situation.
    a. The US federal government and Martin's team disvalue the lack of that knowledge.
4. Imperative propositions to ameliorate the situation.
    a. That knowledge should be established.
5. Propositions describing constraints on amelioration.
    a. Establishing the knowledge will cost money, time, and manpower.
    b. The age could be anything from 95 to 60,000 years-old.
    c. The age must account for the length of the speciation process.
    d. The knowledge will provide an estimate of the true age.
    e. Etc.

In situations like the one above, we can translate the interrogative statement into a problem statement and *vice versa*. The problem proposition above has the same semantic content as is captured in Martin's question. Because both statements express the same content, we can translate "how long has *diabolis* lived in Devils hole?" into a the problem statement "the lack of knowledge about the age of *diabolis* is a problem (of high intellectual dimension)" and *vice*

[12] Questions are straightforwardly posed when they are not merely rhetorical or didactic devices.

versa. So although problems are not identical with questions, there is a way to translate between interrogative statements and some problem statements. In this way, my framework of problems preserves a role for treating research questions as problems.

Not all problem statements, however, translate neatly into single interrogative statements, which indicates at least one further kind of relation between problems and questions. In many cases, a single specified problem leads to many questions. Consider the problem described in the previous section: the high risk of *diabolis* extinction. Given this problem, Martin's team pursued at least four distinct questions. And Martin's team pursued just one of hundreds of research projects related to *diabolis* that have been designed, conducted, and reported in scientific literature in the past ninety years or so. So unlike the situation of a highly specified intellectual problem, in which there is a one-one relation between problem statements and interrogative statements, with other types of problems, there is a one-many relation from problems to interrogative statements. What is this one-many relation?

While there may be many kinds of such relations, I focus on one. The relation is one of 'engendering' or 'motivating', and it is most easily seen in the research designs or rationales of actual research projects. In a research project, a single problem (but not necessarily only a single problem) can engender multiple questions (which themselves can be recast as problems of high intellectual dimension). As researchers conduct their projects, they answer those questions, and the information in the answers can be used partly to ameliorate the motivating problem (Figure 2).

Martin's case illustrates how answers to questions can affect how people address a problem. When his team answered their four questions, they found that *diabolis* was likely less than a thousand years-old and that there had been some gene flow between *diabolis* and its nearby sister species. These findings prompted the team to suggest that the problem adopted by the US federal government might be miscast. Rather than valuing only *diabolis* itself, the team suggested that perhaps another thing to value is the possibility of gene flow in and out of Devils Hole. If so, then a revised or additional problem statement would include threats to such possibilities. Martin's case reinforces that problems have histories and genealogies, and that some of their historical structure is constituted by relations of engendering to sets of questions. Such relations should be better studied and specified, a task beyond the scope of this paper.

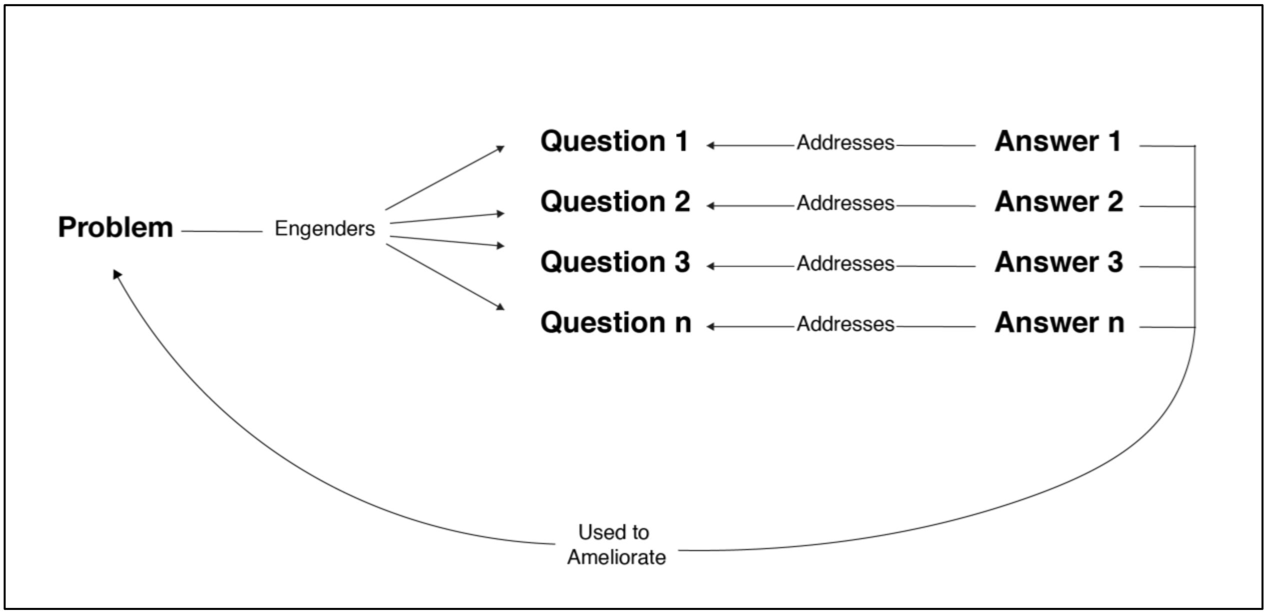

Figure 2. Relations between a problem and questions in a research project.

This relation of engendering indicates a path to augment Love's ([2008], [2014]) account of problem agendas. As discussed in Section 2.2, Love focuses on problems in two senses. First, following Nickles and Laudan, Love treats problems as unanswered questions. Second, following practicing biologists, he treats problems as something like characterizations of general (often process) phenomena. Scientists sometimes use problems in the second sense to organize problems in the first sense (similar to Figure 2), and to order domain-specific knowledge without reliance on overarching theories. We can study how the contents and structures of those agendas change to understand the evolution of science. If we use the account of problems proposed in this paper, then problems with high applied or practical dimensions can be used to describe how scientists structure research agendas in particular projects or in many more general domains, including interdisciplinary ones. In such contexts, problems provide a means to organize knowledge and practices when an overarching theory cannot do so, and they provide potential justifications for pursuing particular research questions. Answers to those questions can be used to ameliorate situations in which something valued is harmed or is obstructed from reaching an end valued and assigned to it.

6 Conclusion

In this paper I propose a framework according to which a problem is a state of affairs in which something valued is harmed or is obstructed from reaching an end that is valued and assigned to it. The framework includes several items, including a brief slogan, a general model of problem propositions, and a general agential model of problems. I illustrate the framework with an example from evolutionary biology.

The framework satisfies five desiderata listed in Section 2.3. First, it distinguishes problems from frivolous unmet wants by defining the former in relation to values. Second, and similarly, it accounts for so-called practical or applied problems by conceptualizing problems in relation to anything valued. Third, the framework includes demands to 'ameliorate' problems, rather than more restricted demands to 'solve' problems, thus accounting for a broader array of actions that satisficing agents use to address problems. Fourth, it provides tools to propose, describe, or evaluate potential justifications for (competing) ameliorations by indicating the role of values in the structure of problems. Finally, it augments, rather than replaces, earlier accounts of problems by accounting for highly intellectualized problems and by treating problems as historical entities. Ultimately, the framework provides a set of tools by which to better identify problems and to study their roles in the conduct of science.

In closing, I forestall a criticism about the above framework, and I indicate some routes by which to further develop the framework. Some may argue that my case study could be misleading because it draws evidence only from published reports that have complex relations to how their authors actually reasoned during the life of their research project. A team might be motivated to conduct a project because of one problem, invoke a different problem when applying for funding, and invoke still a third when reporting its results and convincing others to use their scientific products. As such, scientists might systematically mislead readers of their rationally reconstructed research reports about the problems and questions actually pursued in their project.

There are a couple of responses to this criticism. 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 middle stages they constrain the behaviours of researchers, and in later stages their existences partly justify projects and results to other scientists. I focus my example only on the late stage in which researchers publish their results, and a fuller case study could identify further problems and questions and how they changed throughout the life of the project. Nonetheless, my framework provides the tools needed to conduct those fuller case studies. Second, while scientists reconstruct their projects in their research reports, that practice is itself a worthwhile object of study (Holmes [1987]). Those who conduct such studies should be mindful that those reconstructions sacrifice historical accuracy and have rhetorical functions. But those reconstructions, and the practices of making them, provide a window through which those who study science can piece together the rationales for the projects, results, and scientific products that scientists find convincing. Problems and their descriptions are aspects of those rationales, and my framework helps us better identify and articulate those aspects.

For further development, my framework provides tools for articulating the structure of ameliorations and processes of ameliorating problems. Many have worked on problem solving, with Newell and Simon ([1972]) and MacLeod and Nersessian ([2016]) providing good examples about how further studies could proceed to identify problem-ameliorating practices. An initial step could be to treat ameliorations as states of affairs in which the things valued are harmed or obstructed less, when compared to a temporally earlier problems. These ameliorations are intentional when achieved partly through the execution of a plan or strategy designed to address the problem. By showing how problems are ameliorated, rather than solved, philosophers and historians might explain the historical persistence of some research problems, especially those the motivate larger research agendas.

Next, the framework of problems developed here has the potential to aid other philosophical programs. By embedding a general propositional model in an agential model, the account provides a means by which to operationalize the problem concept in instruments—surveys, interview questions, etc.—to collect data about the problems identified and pursued in actual research projects. Put differently, this framework provides an example of how to connect abstract tools in general philosophy of science (for example, Nickles [1981]) with more recent efforts for empirical philosophy of science (for example, Osbeck and Nersessian [2015]), a method that may generalize to other philosophical topics.

Furthermore, while my focus has been on establishing a descriptive tool, the framework might serve two normative functions. There is more to investigate about how agents who vary in their conceptions of a specific problem situation can nonetheless coordinate their efforts to ameliorate that situation in ways satisfactory to them all. The framework developed here can help identify those aspects of problems about which agents in such situations agree and disagree, about which kinds of disagreements forestall shared action, and about which can be laid aside to enable shared action. As such, it provides a potential tool for helping to negotiate shared action despite competing values, as in cases of sustainability interventions (Miller *et al.* [2014]).

Finally, the framework has the potential to inform those who design, execute, and evaluate research proposals and results. The framework can be seen as an explication of the concept of problem and of its relations to questions. Those designing research projects or proposing them to funding agencies might use the explication to indicate what they mean by ‘research problems’ and how they intend to relate those problems to research questions. This might yield statements of research aims that enable clearer evaluations of a project’s worth, especially for early stage projects (Haufe [2013]). Furthermore, when paired with an account of ameliorations, it might provide a criterion for evaluating the success or failure of individual projects and of longer-term programs (Douglas [2014]).

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