A Model of "Integrated Scientific Method" and its Application for the Analysis of Instruction

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ABSTRACT:

A model of 'integrated scientific method' (ISM) was constructed as a framework for describing the process of science in terms of activities (formulating a research problem, and inventing and evaluating actions — such as selecting and inventing theories, evaluating theories, designing experiments, and doing experiments — intended to solve the problem) and evaluation criteria (empirical, conceptual, and cultural-personal). Instead of trying to define *the* scientific method, ISM is intended to serve as a flexible framework that — by varying the characteristics of its components, their integrated relationships, and their relative importance — can be used to describe a variety of scientific methods, and a variety of perspectives about what constitutes an accurate portrayal of scientific methods.

This framework is outlined visually and verbally, followed by an elaboration of the framework and my own views about science, and an evaluation of whether ISM can serve as a relatively neutral framework for describing a wide range of science practices and science interpretations.

ISM was used to analyze an innovative, guided inquiry classroom (taught by Susan Johnson, using Genetics Construction Kit software) in which students do simulated scientific research by solving classical genetics problems that require effect-to-cause reasoning and theory revision. The immediate goal of analysis was to examine the 'science experiences' of students, to determine how the 'structure of instruction' provides opportunities for these experiences. Another goal was to test and improve the descriptive and analytical utility of ISM.

In developing ISM, a major objective was to make ISM educationally useful. A concluding discussion includes controversies about "the nature of science" and how to teach it, how instruction can expand opportunities for student experience, and how goal-oriented intentional learning (using ISM) might improve the learning, retention, and transfer of thinking skills. Potential educational applications of ISM could involve its use for instructional analysis or design, or for teaching students in the classroom; or ISM and IDM (a closely related, generalized 'integrated design method') could play valuable roles in a 'wide spiral' curriculum designed for the coordinated teaching of thinking skills, including creativity and critical thinking, across a wide range of subjects.

Chapter 1

An Overview

Introduction

In every field of learning, at every level of education, creativity and critical thinking are essential. These complementary skills are intimately integrated in the problem-solving methods used by scientists. The ability to combine creative and critical thinking in a mutually supportive system, as exemplified by scientists in their pursuit of scientific knowledge, can play a valuable role in education. The practical value of 'scientific thinking skills' is described, by a group of scientists and educators who are trying to improve science education,

> There are certain thinking skills associated with science, mathematics, and technology that young people need to develop during their school years. These are mostly, but not exclusively, mathematical and logical skills that are essential tools for both formal and informal learning and for a lifetime of participation in society as a whole. (Rutherford & Ahlgren, 1990, p. 171)

Because these skills are considered so important, many educators are making a large investment of time, effort, and money, with the goal of developing instructional methods that will be more effective in helping students improve their thinking skills. As a way to contribute to this ongoing effort, an explicit articulation of the problem-solving methods used by scientists seems to have the potential to become a powerful tool for improving education.

My dissertation research attempts to develop this potential more fully by pursuing the following objectives:

1. Construct an integrative model of 'scientific method'.
2. Use this model to analyze the instruction — including both the planned activities and the ways in which these activities are put into action, by teacher and students, in the classroom — that occurs in an innovative, inquiry-oriented science course.

These two objectives are described briefly in the remainder of Chapter 1, and will be discussed more thoroughly in Chapters 2 and 3.

Objective 1

A model of 'integrated scientific method' (ISM) is a *descriptive framework* that can be used to describe the activities of scientists — what they think about, and what they do — during scientific research. The ISM framework is based on my own *knowledge* of science (from personal experience working in research labs, reading about the activities of scientists, and talking with scientists) and on my *analysis* of science. When constructing this framework I have adopted a multidisciplinary approach, selecting and synthesizing existing ideas from scientists and from contemporary scholars who study the history, philosophy, psychology, and sociology of science. Thus, the components of ISM are conventional; it is my expectation that some added value will come from their organization into a coherent, integrated model. The process of development has been guided by two main goals. The first goal is to construct ISM as a framework that can be used to describe scientific activity in a wide variety of contexts, and to express divergent viewpoints about what constitutes an accurate portrayal of scientific method. The second goal is for ISM to be useful as a tool that can improve education by facilitating the analysis and design of instruction, and by helping teachers and students to understand 'the nature of science' and to develop their thinking skills.

The content of ISM is expressed using two educationally useful formats: a visual model that shows how multiple factors enter into the generation, evaluation, and application of scientific theories, and a written commentary describing the substance and interrelationships of these factors. A brief sketch of ISM follows:

Motivated by curiosity and by questions arising from inadequately explained observations, scientists invent one or more theories that, if true, might explain what they have observed. A theory — in association with supplementary theories, and relative to competitive theories — is evaluated using *empirical factors* (comparisons of experimental observations with 'if-then' deductive predictions) and *conceptual factors* (judgments about internal consistency and logical structure, and external consistency with other theories). During the activities of science — which include selecting or inventing theories, evaluating theories, designing experiments, and doing experiments — scientists are also influenced by *cultural-personal factors* that operate in the scientific community and in society as a whole.

As described above, the first goal is to develop ISM as a framework for describing science. But since I agree with the current consensus of scholars that no single 'method' is used by all scientists, I am not trying to discover or define *the* scientific method. Instead, ISM has been constructed as a framework that provides structure yet is flexible. This flexibility — by per-mitting variations in defining the characteristics of different activities and evaluation factors, describing the interrelationships between them, and placing emphasis on them — gives ISM the capability of describing the wide variety of actual scientific practice (as it has occurred in various times and places, in different fields of science) and the wide range of views about how to interpret science and scientists. The second goal, to develop ISM as a framework that is useful for improving education, begins in Objective 1 and continues in Objective 2.

Objective 2

A major goal of many educators is to improve the teaching of thinking skills, including the types of skills that are used by scientists in their pursuit of knowledge. When an instructional program is developed in an effort to achieve this goal, an evaluation of this program, for the purpose of determining the ways in which it contributes to achieving educational objectives, is an important component of *curriculum decisions* and *instructional development.* Evaluation provides essential input for making curriculum decisions about instructional policies (such as deciding whether to continue, discontinue, or modify existing instruction) and also for developing new approaches to instruction.

Reliable knowledge provides a firm foundation for the evaluation that occurs during curriculum decisions and instructional development. One source of knowledge is empirical data that involves learning *outcomes* (what students learn) or instructional *methods* (including what the teacher does, and the activities students are asked to do) or student *actions* (what students do during an instructional activity). Based on these three types of data — which will be called *outcome-data, methods-data,* and *action-data* — an evaluation of instructional effectiveness can be primarily empirical or conceptual.

What I am calling an *empirical evaluation* of instruction occurs by gathering empirical outcome-data. For example, educators might examine students who had participated in the program, to assess their thinking skills and their understanding of the nature of science, and how these were affected by the program. Then this data is used to evaluate the effectiveness of the program.

What I am calling *conceptual evaluation* can occur by using either methods-data or methods-data plus action-data. As an example of a conceptual evaluation, consider an extreme case where the dual objectives of a program are to help students learn about the nature of science and to improve their thinking skills, yet the methods-data indicates that the nature of science is never discussed, nor are thinking strategies, and students are never given an opportunity to engage in scientific problem solving. Even with no outcome-data, it is easy to predict that this program — due to the obvious mismatch between objectives and methods — will not be successful in achieving its objectives.

But compared with this simple example in which there is an extreme mismatch, in most real-life situations the application of conceptual criteria will be more difficult, the meaning of conceptual evaluation will be open to a wider range of interpretations, and the conclusions that are reached will be viewed with caution. For example, conceptual criteria might be useful in defining conditions that are "necessary but not sufficient" for successful instruction — i.e., if a certain condition (such as a good match between methods and objectives) is absent the instruction probably will not be successful, but if this condition is present there is no guarantee of success, because many other factors (besides the specified condition) will influence the outcomes of instruction, and might also be necessary if the instruction is to be effective.

If a conceptual evaluation is to have practical value, it should be based on a deep, accurate understanding of what happens during instruction. This understanding begins with the collection of reliable data for either methods or methods-and-actions, and continues with an interpretation of this data. My claim is that an understanding of instruction can be enhanced by using ISM to analyze instruction. As discussed in the outline for Objective 1, ISM has been constructed as a framework that is useful for describing the integrated structure of scientific methods. It is reasonable to expect that this framework may also be useful for describing the integrated structure of the instruction that occurs when students are learning about scientific methods and are using scientific methods. In this context, ISM would be used as a tool for analyzing instructional activities.

In Objective 2, I tested the usefulness of ISM as a framework for describing the 'content and structure' of instruction, and searched for ways to improve this usefulness. The focus of Objective 2 was a careful examination of an inquiry-oriented science course, using ISM as the primary basis for analysis. During this analysis the main goals were to gain a deeper understanding of the instructional methods used in the course, and to learn more about ISM and its potential educational applications. The connection between these two goals is, roughly, a means-end relationship; during the process of using ISM to analyze and characterize the science course, I tested the analytical utility of ISM, and searched for ways to improve it. Before outlining the methods used for the analysis, however, I will briefly describe the science course.

In a conventional course, students typically learn science as a body of knowledge but not as a process of thinking, and rarely do they have the opportunity to see how research science becomes textbook science. A notable exception is a popular, innovative genetics course taught at Monona Grove High School by Sue Johnson, who in 1990 was named "Wisconsin Biology Teacher of the Year" by the National Association of Biology Teachers, due in large part to her creative work in developing and teaching this course. In her classroom, students experience a wide range of problem-solving activities as they build and test scientific theories and, when necessary, revise these theories. After students have solved several problems that "follow the rules" of a basic Mendelian theory of inheritance, they begin to encounter data that cannot be explained using their initial theory. To solve this new type of problem the students, working in small 'research groups', must recognize the anomalies and revise their existing theory in an effort to develop new theories that can be judged, on the basis of the students' own evaluation criteria, to be capable of satisfactorily explaining the anomalous data.

As these students generate and evaluate theories, they are gaining first-hand experience in the role of research scientists. They also gain second-hand experience in the form of science history, by hearing or reading stories about the adventures of research scientists zealously pursuing their goal of advancing the frontiers of knowledge. A balanced combination that skillfully blends both types of student experience can be used to more effectively simulate the total experience of a scientist actively involved in research. According to educators who have studied this classroom, students often achieve a higher motivation level, improved problem-solving skills, and an appreciation for science as an intellectual activity.

My analysis of this science course occurred in three stages. During a preliminary stage the basic ISM 'descriptive framework' was adapted so it could serve as an 'analytical framework', and a detailed characterization of student activities was constructed. Following this, in the first phase of analysis eleven instructional activities were analyzed to determine, for each activity, the 'science experiences' of students. In a second phase of analysis, these eleven activities (and the experiences they promoted) were compared in an effort to gain a deeper understanding, facilitated by the relational organization of ISM, of how the various classroom activities are functionally related.

The immediate goal of analysis was to construct a multi-faceted representation of the instructional methods, student activities, and student actions in one classroom, with the purpose of gaining a deeper understanding of the functional relationships among various activities and actions, and of the ways in which student actions are related to the actions of scientists. Although this characterization could be used for the type of conceptual evaluation described above, I will not attempt to evaluate the effectiveness of instruction in the classroom.

Viewed from a long-term perspective, the goal is to use this analysis as an opportunity for testing the analytical utility of ISM, gaining a deeper understanding of ISM, and improving its effectiveness as a descriptive framework and as an analytical thinking tool that can be used, by curriculum designers and teachers, for developing instructional methods that will expand the range of learning experiences for students.

Significance of the Research

Science and scientists, due to their characteristics and their importance in modern society, have been studied extensively (and intensively) by many scholars in a broad range of fields: in science, history of science, philosophy, sociology, psychology, and education. The model of 'integrated scientific method' (ISM) developed in this research has three characteristics that distinguish it from work done previously. First, although most work done by 'study of science' scholars makes a clear declaration about at least one aspect of science, ISM is intended to be a relatively neutral "empty framework" that can be filled with details in different ways by different people. Second, although most studies of science focus on a few aspects of scientific methods, the ISM framework is more comprehensive in the scope of the activities and relationships it covers. Third, the visual representation of ISM (in Figure 1) is distinctive in its level of detail depicting the integrated relationships between activities.

By providing a coherent, integrated framework for a broad range of ideas about science, ISM offers a wide-view perspective that should contribute to the improvement of education.[1] This could occur in three ways — by using ISM to facilitate instructional analysis and design, to educate teachers, or to teach students in the classroom.

Compared with conventional science courses, the innovative genetics course that was analyzed gives students an opportunity to experience an unusually wide range of the 'methods of science'. Due to its special characteristics, this course has been studied in four previous doctoral dissertations (Finkel, 1993; Wynne, 1995; Lemberger, 1995; and Johnson, 1996). Each of these researchers focused on the behavior of students who were solving problems. My research differs from these studies, because it is the first to focus on the methods of instruction; it does this with a detailed analysis of each instructional activity and of the relationships between activities. This analysis, based on ISM, has provided an in-depth characterization of the 'structure of instruction' in the

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1. ISM is primarily intended to be a tool for improving education, but it might also be useful in the scholarly study of science by serving as a framework for analyzing the methods used by scientists during historical scientific episodes. The possibility of using ISM for historical/philosophical analysis will not be explicitly discussed in my dissertation, but this potential application may be worth exploring in the future. Although ISM does not introduce major new philosophical concepts (since its components have been borrowed from contemporary scholars), it does provide an integrated structure for synthesizing a wide range of ideas, and this wide-angle perspective might serve a useful function in the scholarly study of science.

classroom, and a deeper understanding of how this structure helps expand the range of students' science experiences, and how these instructional methods might be generalized so they can be used in a wider variety of classroom contexts.

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CHAPTER 2

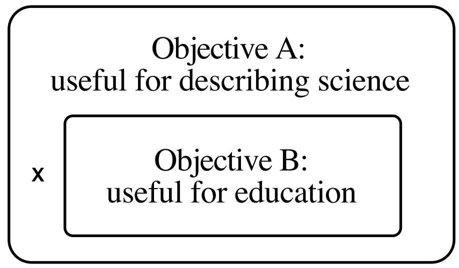
A Model of 'Integrated Scientific Method'

This chapter describes a model of 'integrated scientific method' (ISM). It begins with a discussion of what ISM does and does not intend to accomplish, continues with a description of the ISM framework followed by a detailed elaboration of ISM, and concludes with a discussion of the extent to which ISM has achieved the goals set for it.

2.00: Goals for a Model of ISM

Chapter 1 described two main goals: Objectives 1 and 2. During the process of pursuing Objective 1 (the development of ISM) two additional goals were formulated. Objective A: ISM should be capable of describing a wide variety of scientific practices, and articulating a wide range of viewpoints about what constitutes an accurate portrayal of scientific methods. Objective B: ISM should be a useful tool for contributing to the improvement of education. Figure 2 shows the relationships between these objectives.

Figure 2: Relationships between Objectives A and B

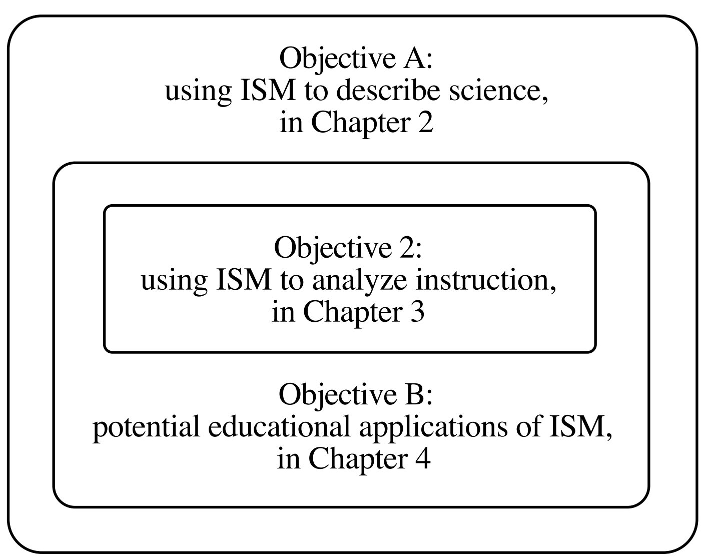


As shown by the x, it would be possible to construct ISM with the goal of achieving only Objective A, with no concern for educational utility. For example, some models of science constructed by philosophers are too esoteric and specialized (intended for use mainly by philosophers) to be useful for educational applications at K-12 or undergraduate levels. Thus, Objective A is not sufficient for achieving Objective B. But I think that A is necessary for B, that ISM must be useful for describing science if it is to be educationally useful.

Figure 3 shows the relationships between dissertation objectives (1 and 2), ISM objectives (A and B), and Chapters 2-4.

Figure 3: Relationships between Objectives (1, 2, A, B)
and between Chapters 2, 3 and 4

Objective 1: construct ISM to achieve Objectives A and B



Objective 1 involves the construction of ISM to achieve Objectives A and B. My descriptions of "science as a process" in terms of ISM, which are the result of trying to achieve Objective A, are the subject of Chapter 2. My analysis of instruction, in an effort to achieve Objective 2, is described in Chapter 3. And some potential educational applications of ISM, which would be the fruit of achieving Objective B, and would include but are not limited to instructional analysis, are discussed in Chapter 4.

My claims for the potential educational utility of ISM assume that students will benefit from learning not just the 'content of science' (as an organized body of concepts and theories intended to explain nature) but also the 'process of science' (that develops and evaluates these concepts and theories). The latter aspect of science education can be considered valuable for either cognitive or cultural reasons — because learning scientific thinking will help students improve their higher-level thinking skills such as creativity and critical thinking, or because it will help them understand the cultural importance of science as an influential institution in modern society, and as a way to generate and evaluate claims for 'scientific knowledge'.

It is by helping students learn the process of science, as they experience this process in first-hand problem solving or second-hand stories or illustrations, that ISM has the potential to be most useful for curriculum developers, teachers, and students. Some educational appli-cations of ISM will be discussed in Chapter 4, following an examination of Objectives A and 2 (using ISM to describe science and analyze instruction) in Chapters 2 and 3.

Objective A must be interpreted carefully, to avoid the implication that it promises to achieve more than is actually claimed. ISM tries to describe the process of science (what scientists do and how science operates) but no description of these "science activities" will ever be considered satisfactory by everyone. It is impossible to construct a universally acceptable description of scientific methods, for two reasons. First, the methods used by scientists change with time and culture, as shown by the empirical evidence of history, and vary from one scientific discipline to another. Second, even when describing the same scientific event, scholars may disagree about what happened and why. Because there is such a wide spectrum of differing interpretations, with irreconcilable perspectives ranging from the charicature of a traditional 'method' with a rigid algorithmic sequence to the anarchistic "anything goes" anti-method of Feyerabend (1975), it would be foolish to try to develop ISM into '*the* scientific method' that will satisfy everyone. Neither does ISM argue for the correctness of any of the competing interpretations. Instead, my goal has been to design ISM as a 'tool for thinking' that can be used to clearly express each of the divergent viewpoints, so the similarities and differences between these perspectives can be more clearly understood and articulated.

To accomplish this, ISM has been constructed using a multidisciplinary approach, selecting existing ideas (from scientists and from contemporary scholars who study the history, philosophy, psychology, and sociology of science) and organizing these ideas into a coherent, integrated model of scientific method. Since ideas are drawn from all parts of the interpretive spectrum, ISM contains the essential concepts that are needed to describe divergent viewpoints.

Based on my experience using ISM, it seems that differing interpretations of science can be explained, at least to a reasonable approximation, in terms of differences in how to describe the *characteristics* of science components (such as activities and evaluative factors), the integrated *relationships* between components, and the *balance* (regarding relative importance) between components or relationships. Following a description of the ISM framework, Section 2.08 will illustrate how this framework is constructed so it can flexibly accommodate a wide range of perspectives about characteristics, relationships, and balances, thus allowing ISM to be used for describing a wide variety of actual scientific practices and a wide range of viewpoints about how to interpret science and scientists.

The ISM Framework

The ISM-diagram (Figure 1, pages 9-10) is a visual representation of the ISM framework.[2] Sections 2.01-2.07 are a complementary verbal description: three sections are for evaluation factors

[2]. Appendix A1 contains an ISM diagram that has not been modified (by making the fonts larger and the margins smaller) to conform to UW-Madison's requirements for a "library copy" of the dissertation.

(empirical, conceptual, and cultural-personal), three are for activities (theory evaluation, theory invention, and experimental design), and one is an overview of problems, cultural influence, and productive thinking. In the descriptions that follow, each element in the ISM diagram is highlighted with **bold print**, and my intended meaning (in the context of ISM) is defined.

In developing ISM, one major challenge was the selection of words and meanings. If everyone used the same terms to describe scientific methods, I would use these terms in ISM. Unfortunately, there is no consistent terminology; instead, there are important terms — especially model, hypothesis, and theory — with many conflicting meanings, and meanings known by many names. Due to this lack of agreement by other scholars, I have been forced to choose among competing alternatives. Despite the linguistic confusion, over which I have no control, in the context of ISM I have tried to use terms consistently, in ways that correspond reasonably well with their common uses by scientists, philosophers, and educators. Temporarily, for the purpose of understanding my descriptions of ISM and scientific methods, it will be useful to accept terms as I define them. Later, the end of Section 2.08 contains a detailed discussion about the challenge of coping with inconsistent terminology, and describes an option that involves a substitution of terms.

2.01: Empirical Factors in Theory Evaluation

Although it isn't necessarily where scientists embark on their studies of nature, this tour of the ISM framework begins by looking at the shaded hypothetico-deductive ‘box’ whose corners are defined by four elements — model and system, predictions and observations — at the right side of the ISM diagram (this box is adapted from Giere, 1991; the ISM definitions of model and hypothesis are also adapted from Giere). It is assumed that there already has been a decision to study a certain area of nature by doing a specific experiment.

In ISM, an **experimental system** (for a controlled experiment or a field study of natural phenomena) is defined as everything involved in an experiment, including what is being studied, what is done to it, and the instruments of observation. Based on a selected **theory** and (as necessary) **supplementary theories** — which include, but are not limited to, theories used to interpret observations — scientists construct a **model** that for a *descriptive theory* is a description of relationships between observable properties, and that for an *explanatory theory* is a simplified representation of the system's *composition* (what it is) and *operation* (what it does). After a model is defined, a 'thought experiment' can be done by asking, "IF this model is true, THEN what will occur?", thereby using **deductive logic** to make **predictions**. When a 'physical experiment' is done with the experimental system, **observation detectors** (and recorders) are used to obtain **observations**.

The dual-parallel shape of the hypothetico-deductive box symbolizes two parallel relationships. The left-side process (done by *mentally* running a theory-based model) parallels the right-side process (done by *physically* running a real-world experimental system). There is a second parallel, of a different type, between the top and bottom of the box. At the top, the **hypothesis** is a claim that the model and system are similar in specified respects and to a specified (or implied) degree of accuracy. At the bottom is a **logical comparison** of predictions (by the model) and observations (of the system); this comparison is used to evaluate the hypothesis, based on the parallel logic (symbolized by the parallel bottom and top of the box) that the **degree of agreement** between predictions and observations is related to the degree of similarity between model and system. Often, statistical analysis is necessary to estimate a degree of agreement. Because a theory can be false even if its predictions agree with observations, it is necessary to supplement the 'agreement' criterion with another criterion, the **degree of predictive contrast**, that asks, "For this experimental system, how much contrast exists between the predictions of this **theory** and the predictions of plausible **alternative theories**?", in an effort to consider the possibility that two or more theories could make the same correct predictions.

Estimates for degrees of agreement and predictive contrast are combined to form an **empirical evaluation of current hypothesis**. This evaluation and the analogous **empirical evaluations of previous hypotheses** (which are based on the same theory as the current hypothesis, and that are considered relevant) are the **empirical factors** that enter into **evaluation of theory**.

2.02: Conceptual Factors in Theory Evaluation

On the left side of the ISM diagram, three arrows go into ‘evaluation of theory’, representing inputs from three types of evaluation factors: empirical, conceptual, and cultural-personal. In ISM the **conceptual factors** (Laudan, 1977) that influence theory evaluation have been split into three categories: **internal consistency** (Are the components of a theory logically compatible with each other?), **logical structure** (Does a theory conform to the preference of scientists regarding the characteristics and interrelationships among theory-components?), and **external relationships** (What are the logical relationships between a theory and other currently accepted scientific theories and cultural-personal theories?).

A theory is a humanly constructed representation intended to describe or explain a set of related phenomena in a specified domain of nature. The function of a ‘predictive theory’ is to describe the relationships among observations, while an *explanatory theory* tries to explain systems (and relationships among observations) by guiding the construction of composition-and-operation models. Often, a theory begins as a descriptive theory, which is then converted into an explanatory theory when a composition-and-operation mechanism is proposed.

In every field there are implicit and explicit constraints on the types of entities, actions, and interactions that should and should not be included in a theory; these constraints can be due to beliefs about ontology (what exists) or utility (what is scientifically useful). For example, a view held by some philosophers, although it is rare (possibly nonexistent) among current scientists, is *empiricism* — the claim that scientific theories should not postulate the existence of entities or interactions that cannot be directly observed. By contrast, empirically based hypothetico-deductive logic allows ‘unobservable’ components in a theory, if this theory makes predictions about observable outcomes. Notice that ‘empirical’ science includes both empiricist science (with only observable components in theories) and non-empiricist science (that also allows unobservable components).

Scientists expect a theory's components to be internally consistent with each other. A theory's internal logical structure can be evaluated using a wide variety of criteria, including a *simplicity* that involves a theory's level of ‘simplification’ (by using a minimum number of components) and ‘systematicity’ (by forming logical connections among components).

Scientists usually want a theory that is both plausible and useful; is easy to learn and use;

makes precise, accurate predictions about observable outcomes; stimulates ideas for experimental or theoretical research; and is a powerful tool for solving problems. *Scientific utility* includes *cognitive utility* (for inspiring and facilitating productive thinking about theory components and their relationships) and *research utility* (for stimulating and guiding theoretical or experimental research projects). The inputs from theory evaluation based on utility are personalized, and will depend on point of view and context, because research goals (and personal goals) vary among scientists, and can change from one context to another.

External relationships between theories can involve an overlapping of domains or a sharing of theory components.

Theories with overlapping domains claim to explain the same systems, so these theories are in direct competition with each other. They can agree or disagree about predictions, and about models (i.e., about the nature of experimental systems). The result of competition may be the triumph of one theory, the rejection of both, or a coexistence in which both are retained because each theory is plausible or scientifically useful in a different way.

Theories with shared components serve to unify the different domains of science they describe; if there is agreement about the characteristics of the shared component (so there is *external consistency)* the theories provide support for each other. But if theories disagree about the characteristics of a component, there is less unity, and the status of one theory (or both) will usually decrease. Theories with shared components can be related by overlapping levels of organization, as in biological theories for molecules, cells, organs, organisms, populations, and ecological systems. Or one theory can be a subset of another; for example, there are many sub-theories within atomic theory. All other factors being equal, scientists are generally impressed with a theory that has a wide *scope*— i.e., a wide domain of applicability.

There is some similarity between the internal structure of a theory (composed of smaller components) and a mega-theory (composed of smaller theories), and many conceptual criteria can be used for either internal structure (within a theory) or external relationships (between theories in a mega-theory). For example, viewing external relationships (between theories) as internal relationships within a not-yet-complete 'grand unified theory' may provide a useful perspective when considering the larger questions of how theories relate to each other and interact to form the structure of a scientific discipline, and how disciplines interact to form the structure of science as a whole.

2.03: Cultural-Personal Factors in Theory Evaluation

During all activities of science, including evaluation, scientists are influenced by **cultural-personal factors**. These factors include "**psychological motives** and **practical concerns**" (such as intellectual curiosity and desires for self esteem, respect from others, financial security, and power), **metaphysical worldviews** (that form a foundation for some criteria used for conceptual evaluation), **ideological principles** (about “the way things should be” in society) and **opinions of** ‘**authorities**’ (acknowledged due to expertise, personality, and/or power).

These five factors interact with each other, and they develop and operate in a complex social context at many levels — in the lives of individuals, in the scientific community, and in society as a whole. In an attempt to describe this complexity, the analysis-and-synthesis framework of ISM includes the characteristics of (and mutual interactions among) *individuals* and a variety of *groups*; *profession-related politics* (occurring primarily within the scientific community) and *societal politics* (involving broader issues in society); and the *institutional structures* of science and society. The term ‘cultural-personal’ implies that both cultural and personal levels (and their interactions) are important, and are intimately connected by mutual interactions, because individuals work and think in the context of a culture, and this culture (including its institutional structure, operations, and politics) is developed by and composed of individual persons.

Cultural-personal factors influence science, are influenced by science. Science affects culture due to its medical and technological applications (with the accompanying effects on health care, lifestyles, and social structures) and because it helps to shape cultural worldviews, concepts, and thinking patterns. Culture influences science in several ways. Society supports research that may lead to desired medical and technological applications. Scientists are more likely to accept a scientific theory that is consistent with their metaphysical and ideological theories; in the ISM diagram, this influence appears as a conceptual factor, **external relationships**...**with cultural-**

personal theories. And when making a decision about theory-evaluation or action-evaluation, scientists often ask a practical question related to personal goals: "How will this decision affect my own personal and professional life?" The influence of self-interest also operates at the level of groups and societies. The influence of culture, on both the *process of science* and the *content of science,* is summarized at the top of the ISM diagram: "Scientific activities... are affected by culturally influenced **thought styles**."

Sometimes cultural-personal influence is the result of a desire for personal consistency in life, for a consistency between ideas, between actions, and between ideas and actions. Since groups are formed by people, principles of personal consistency can be extrapolated (with appropriate modifications) to groups.

In the ISM diagram, three small arrows point from 'evaluation of theory' back toward the three evaluation factors. These arrows show the 'feedback' that occurs when a conclusion about theory status already has been reached and, to achieve personal consistency, there is a tendency to interpret other factors so they also support this conclusion. In this way, each type of evaluation criterion is affected by feedback from the current theory status and from the other evaluation criteria.

2.04: Theory Evaluation

Inputs to **evaluation of theory** come from empirical, conceptual, and cultural-personal factors, with the relative weighting of factors varying from one situation to another. The immediate output of theory evaluation is a **theory status** (Hewson, 1981) — an estimate that reflects scientists' beliefs about the plausibility and/or usefulness of a theory. At any given time, a theory has two types of status: its own *intrinsic status,* and a *relative status* that is defined by the question, "What is the overall appeal of this theory, when all factors are considered, compared with **alternative theories**?" Of course, a change in the status of one theory will affect the relative status for alternative theories; this feedback is indicated by the small arrow pointing into 'alternative theories' from 'theory evaluation'.

Three responses to evaluation are: continue to **retain** this theory with no revisions, for the purpose of *pursuit* [i.e. to serve as a basis for further research] or *acceptance,* possibly with a change of status; **revise** the theory to generate a new theory that can be used in subsequent application, testing and evaluation; **reject** the theory. Or there can be a delay in responding, while other activities are being pursued.

Sometimes there is tension between different evaluation criteria. For example, if empirical agreement is increased by adding a component, this may decrease the simplicity and conceptual adequacy, while the empirical adequacy increases. And there is inherent tension between the conflicting criteria of completeness and simplicity; for most natural systems there is a need to compromise, to seek a useful balance.

It is logically impossible to prove that a theory is either true or false. In addition to the logical limitations of hypothetico-deductive logic (that “if A, then B” and B occurs, this does not prove A), there can be suspicions about ad hoc adjustments if a theory is proposed to fit known data (as in **retroductive inference**), and also about the possibility of biased data collection or circularity between theories and observation-theories, the reliability of inductive generalization, and the influence of cultural-personal factors that are not a part of formal logic. But in their effort to cope with these difficulties, scientists have developed methods (including estimates for predictive contrast, and sophisticated techniques for logical analysis) that encourage them to claim a ‘rationally justified confidence’ for their scientific conclusions, despite the impossibility of proof or disproof.

As a reminder that the outcome of theory evaluation is an educated estimate rather than a claim for certainty, ISM uses a ‘status’ continuum, ranging from very low to very high, to describe the degree of confidence in a theory. To allow an accurate description of theory status, four distinctions are useful.

First, there is *intrinsic status* and *relative status,* described above.

Second is a distinction between pursuit (Laudan, 1977) and acceptance. Scientists can judge a theory to be worthy of *pursuit* (for application and development, to stimulate ideas for new experimental and theoretical research) or as worthy of *acceptance* (of being treated, for purposes of doing science, as if it were true).

Third, there is *utility status* (an estimate of how scientifically useful a theory has been and is now, or may be in the future) and *truth status* (an estimate of the probability that a theory's models correspond to the reality of the systems they are intended to represent). Because a hypothesis claims that a model and system are similar "in specified respects and to a specified (or implied) degree of accuracy," for the same model and system there are many possible hypotheses, and truth status may vary with the strength of a hypothetical claim; a strong claim (of an exact match between model and system) may have lower truth status than a weaker claim.

Fourth, theories can be viewed with a *realist* interpretation (that a theory claims to describe what really occurs in nature) and an *instrumentalist* interpretation (that a theory is just a tool for making predictions and for doing scientific research). An intermediate position, *critical realism,* adopts a *critical epistemology* (willing to be skeptical about the truth status of a particular theory) and *realist goals* (wanting to find the truth). In addition, a critical realist can adjust the relative importance of truth and utility by adopting a personal perspective — which can vary from one theory to another — of where a theory is located on a continuum between realist and instrumentalist interpretations. At one end of the continuum only truth is important, at the other end only utility counts, and in the middle there is a balanced emphasis.

2.05: Theory Invention

Sometimes an existing theory can serve as the basis for constructing a satisfactory model of an experimental system; in this case, **selection of an old theory** is sufficient. However, if there is enough dissatisfaction with old theories, or if a curious scientist wants to explore other possibilities, the **invention of a new theory** will be attempted.

In ISM the term 'invention' is used in a broad sense, to include the process of revising an existing theory by increments, as well as the sudden creation of a totally new theory. In most cases the process of invention involves what in common language is referred to as a process of 'development'.

Theory invention, which is closely related to theory evaluation, is guided by the evaluative constraints imposed by conceptual factors (internal consistency, logical structure, and external relationships with other theories), by cultural-personal factors, and by empirical factors (from one experiment or many).

Empirical guidance occurs in the creative-and-critical process of **retroductive inference** that is inspired and guided by the goal of finding a theory whose predictions will match known observations. In contrast with deductive logic that asks, "If this is the model, then what will the observations be?", retroductive logic asks a reversed question in the past tense, "These were the observations, so what could the model (and theory) have been?" In retroductive logic, observations precede the proposal (by selection or invention) of a theory, while in hypothetico-deductive logic a theory is selected before the observations are known. This timing is the main difference between these two types of inference, so the same logical limitations apply; due to the possibility that plausible alternative theories could also explain the observations, with retroduction (as with hypothetico-deduction) there is only a cautious conclusion: IF system-and-observations, THEN MAYBE model-and-theory. This caution contrasts with the definite conclusion of deductive logic: IF theory-and-model, THEN prediction.

A theory-based model of a system is constructed from two sources: a general domain-theory (about all systems in a domain) and a specific system-theory (about the constituents of one experimental system). Therefore, once a model has been proposed, either or both of these theories can be revised in an effort to construct this model and achieve a retroductive match with observations. There are relationships between these theories; in particular, a domain-theory (about all systems in the theory's domain) will often influence a system-theory about one system in this domain.

One possible result of theory proposal (by selection or invention) is the construction or revision of a system-theory. Three other possible results are shown by three arrows, leading to 'theory' and 'supplementary theories' because both are domain-theories that can be used to build models for retroductive inference, and to 'alternative theories' because a new theory that has been proposed (by selection or invention) becomes an alternative that competes with an old theory. Or a new theory may become the 'main' theory while an old theory is called 'alternative' since this labeling depends on context; what scientists consider a main theory in one situation could be alternative or supplementary in other situations.

If there is data from several experiments, retroduction can be made more rigorous by demanding that a theory's predictions must be consistent with all known data. The domain of a theory proposed during this ‘retroductive induction’ will be larger than the domain of each model for an individual experiment. When there are multiple sources of data, a useful invention strategy is to retroductively infer a model for one system, and then apply the principles of this model (i.e., a ‘theory’ from which this model could be derived) to construct models for the other systems, to test this theory's generalizability. Another strategy is to search for an empirical pattern that, once found, can provide the inspiration and guiding constraints for inventing a theory with a composition-and-operation mechanism to explain the pattern.

Invention often begins with the selection of an old (i.e., previously existing) theory that can be revised. One strategy for revision begins with analysis; split a theory into components and play with them by thinking about what might happen if one or more components were modified, reorganized, eliminated, or added. During this process, scientists can assume that some ‘protected’ components are not revisable, or they can explore all possibilities for revision. Another invention strategy is to build a theory on the foundation of a few assumed axiom-components, using the logic of internal consistency.

Sometimes new ideas for building or revising a theory are inspired by studying the components and logical structure of other theories. Maybe a component can be borrowed from another theory. Or the structure of an old theory can be retained (with appropriate modifications, if necessary) while the content of the old components is changed, thereby using analogy to guide the logical structuring of the new theory. And sometimes an existing theory can be generalized, as-is or modified, into a new domain.

Another possibility is mutual analysis-and-synthesis; by carefully comparing the components of two theories, it may be possible to gain a deeper understanding of how the two are related by an overlapping of components or structures. This improved understanding might inspire a revision of either theory, or a synthesis that combines ideas from both theories into a unified theory that is more conceptually coherent and has a wider empirical scope.

2.06: Experimental Design

In ISM an ‘experiment’ is defined broadly to include both *controlled experiments* and *field studies.* Three arrows point toward **experimental design**, showing inputs from ‘evaluation of theory’ (which can motivate and guide design), **gaps in system knowledge** (to be filled by designing experimental systems) and ‘**do thought experiments**...’ (to facilitate the process of design). There is one outgoing arrow, showing that the result of experimental design is a ‘real-world **experimental system**’. This arrow is duplicated at the right side of the diagram, where it again points toward ‘real-world **experimental system**’. When this system is put into action, scientists **do physical experiment with system**.

Sometimes experiments are done just to see what will happen, to gather observations for an empirical database that can be used for interpretation and invention in the future. Often, however, experiments are designed to accomplish a goal. For example, an experiment (or a cluster of related experiments) can be designed to gather information about a theory or a system, to learn about a new experimental technique, to facilitate anomaly resolution, or to serve as a ‘crucial experiment’ that can distinguish between competitive theories.

To facilitate the collection and interpretation of data for each goal, logical strategies are available. For example, to make inferences about a theory or system, retroduction with 3 variables (observations, domain-theory, system-theory) is useful. To ‘calibrate’ a new experimental technique, a scientist can design controlled cross-checking experiments that compare (for the same system) data from the new technique and from a familiar technique. And to design or interpret experimental ‘clusters’ there are logical strategies, such as Mill's Methods for experimental inquiry, for the systematic variation of parameters (individually or in combination) to discover correlations or to establish the effects of various types of ‘controls’. Complementary ‘variations on a theme’ clusters can be planned in advance, or improvised in response to feedback from previous experimental results.

If an experiment produces anomalous results, there are several ways to resolve the anomaly. Perhaps the theory is inadequate, and a new experiment can help localize the anomaly to a faulty theory component, and further experiments can test options for revising or replacing this component. Or the experimental system may not actually be what the scientists planned it to be, and the experiment needs to be re-designed and re-done. Or maybe what really happened is more interesting than what was planned, and an “accidental experiment” or “surprise result” can serve as an opportunity for serendipitous discovery.

Scientists can design *heuristic experiments* with the primary goal of learning about a domain or developing and testing a theory, or they can focus on the design of impressive *demonstrative experiments* that will be useful in persuading others (Grinnell, 1992). For either type of experiment, but especially for demonstration, a useful strategy is to think ahead to questions that will be raised during evaluation. These questions — about issues such as sample size and representativeness if the goal is to generalize from a sample to a larger unexamined population, or the degree of predictive contrast if there are competitive theories, or the consideration of all relevant factors and the adequacy of controls — can be used to probe the current database of systems-and-observations, searching for gaps that can be filled by experimentation.

Often, new opportunities for scientific activity — in both experimenting and theorizing — emerge from a change in the status quo. For example, opportunities for field studies may arise from new events (such as an ozone hole) or new discoveries (of old dinosaur bones,...). A new theory may stimulate experiments designed to test the theory and develop it, or to explore its application for a variety of systems. The inductive generalization of an old theory into a new domain can serve similar purposes. And sometimes new instrumentation technologies or observation techniques open the door to opportunities for new types of experimental systems. When an area of science opens up due to any of these changes, opportunities for research are produced. To creatively take advantage of these opportunities requires an open-minded awareness, to imagine the possibilities.

Mental experiments, done to quickly explore a variety of experimental possibilities, can help scientists decide which experimental systems are worthy of further pursuit. Typically, mental experiments serve as an inexpensive screening process for the design of physical experiments that typically require larger investments of time and money.

Thought-experiments play a key role in three parts of ISM. In each context a prediction is generated from a theory by using deductive logic, but there are essential differences in objectives.

In thought-experiments for experimental design, the divergent objectives of scientists — looking for predicted outcomes that might somehow be interesting or useful — are less clearly defined than in retroductive thought-experiments where, despite a divergent search for theories, the convergent goal is to find a model whose predictions match the known observations. And in a hypothetico-deductive context, mental experiments are even more constrained, being done with one fixed theory and one experimental system. Or thought-experiments can be done for their own sake, to probe the implications of a theory by deductively exploring systems that may be difficult or impossible to attain physically.

2.07: Problem Solving, Thought Styles, and Thinking

A. Problem-Solving Projects

The activities of science usually occur in the context of an effort to solve scientific problems. Generally, *problem solving* can be defined as "an effort to convert an actual current state into a desired future state" or, in abbreviated form, "converting a *NOW-state* into a *GOAL-state.*" The goal of science is knowledge about nature, so the goal of scientific research is an *improved knowledge* about *observations* of nature and *interpretations* of nature. Critical evaluation of the current state of knowledge (regarding a domain, phenomenon, or theory) may lead to recognizing a gap in the current knowledge, and to a characterization, in the imagination, of a potential future state with improved knowledge. A problem is defined by specifying a set of *constraints* on its solution — i.e., by specifying the characteristics of goal-states that would be considered a satisfactory solution (Nickles, 1981).

Formulating a good scientific problem — one that is original, significant, and capable of being solved with the available resources (of time, people, knowledge, equipment, materials, and money) — is an important, challenging part of science. Effective problem formulation is customized to fit the resources of a particular research group.

When scientists decide to actively pursue a solution to a particular science problem, solving this problem becomes the focal point for a *research project.* The movement from problem to project requires ‘action evaluation’ and a decision. Scientists can define the goal of a project in terms of solving a problem, answering a question, or achieving an objective. The essential character of a scientific project is determined by decisions about what to study and how to study it.

In an effort to achieve a problem solution, scientists invent, evaluate, and execute *actions.* Any activity in ISM can be an action that involves *observation* (design and do experiments or field studies, make observations, or learn the observations of others) or *interpretation* (organize data to facilitate pattern recognition, analyze and synthesize, select or invent theories, evaluate theories, or review the interpretations of others).

Action evaluation, to decide which actions to do, is guided by an awareness of the problem; the problem's goal-state can serve as an aiming point to orient the search for a solution. At any given time, the now-state (which is constantly changing) is compared with the goal-state, to search for ‘problem gaps’ (i.e., specific ways in which the now-state and goal-state differ) that can guide the planning of actions designed to close these gaps. The process of action evaluation, which is itself an action, is similar to the process of theory evaluation. Eventually, the now-state may become close enough to the goal-state that it is considered a satisfactory solution. Or the project may be abandoned, at least temporarily, because progress is slow or because there is a decision to work on another project.

Theory evaluation or action evaluation can involve *internally-oriented persuasion* to convince the evaluators (as individuals or as a research group) about a conclusion. If scientists think they have found a solution, there can be *externally-oriented persuasion* to convince others that the proposed solution is worthy of acceptance (as plausible, useful scientific knowledge) or pursuit (to investigate by further research).

Before and during problem formulation, scientists *prepare* by learning the current now-state of knowledge about a selected area of nature, including observations, theories, and experimental techniques. Learning can occur by reading (of textbooks or journal articles), listening (in lectures, discussions, or conversations), and by “learning from experience” during research. Because one scientist can interpret what another observes, sometimes an effective strategy for collecting data is to be a ‘theoretician’ by reading (or hearing) about the experiments of others, for the purpose of gathering observations that can then be interpreted.

The connections between projects, and between actions within a project, can be viewed in the larger context of all scientific research, with different levels of problems and problem-solving activity. During scientific research a *mega-problem* (the attempt by science to understand all of nature) is narrowed to a *problem* (of trying to answer specific questions about one area of nature) and then to *sub-problems* that involve the planning and execution of specific *actions.* Often, there are overlaps, with a group working on many problems and sub-problems simultaneously, or with several groups working on the same problem or on different parts of a 'family' of related problems.

To make it easier to discuss the actions that occur during a project, it is convenient to use a compact '4Ps' terminology, adapted from the terms in a '3Ps' model of science by Peterson & Jungck (1988). The 4Ps are *preparing* (reading,...), *posing* (formulating a problem), *probing* (doing actions to probe the problem and pursue a solution), and *persuading.*

B. Thought Styles

All activities in science, mental and physical, are influenced by the context of culturally-influenced **thought styles** (Grinnell, 1992) that operate at the levels of individuals and communities, and involve both conscious choices and unconscious assumptions. A collective thought style includes the shared beliefs, among a group of scientists, for "what should be done, and how it should be done." Thought styles affect the types of problem-solving projects that are formulated and pursued, theories that are invented and pursued and accepted, experiments that are designed and done, and the techniques and perspectives that are used to interpret data. There are mutual influences between thinking styles and the procedural 'rules of the game' that are developed by a community of scientists to establish and maintain certain types of institutions and reward systems, styles of presentation and argumentation, systems for coordinating the activities of different scientists and groups, attitudes toward competition and cooperation and how to combine them effectively, and relationships between science, technology and society. Decisions about problem-solving projects — decisions that are heavily influenced by thought styles — play a key role in the mutual interactions between science and society by determining the allocation of societal resources (within science) and the returns (to society) that may arise from investments in scientific research.

Thought styles affect the process and content of science in many ways. But this influence is not the same for all science, because thought styles vary from one field of science to another. Thought styles also change with time. Variations in thought styles may be due to: intrinsic differences in the areas of nature being studied, and in the observational techniques available for studying each area; differences, due to self-selection, in the cognitive styles, personalities, values, and metaphysical-ideological beliefs of scientists who choose to enter different fields; and historical contingencies.

C. Mental Operations

The mental operations involved in science — for theory invention and for all other activities — are summarized at the top of the ISM diagram by "motivation and memory, creativity and critical thinking." **Motivation** inspires effort. To do science well, **memory** — with storage in the mind of a scientist, or using ‘external storage’ such as notes or a book or computer file — is not sufficient by itself, but it is necessary to provide raw materials (theories, experimental techniques, known observations,...) for **creativity and critical thinking** which, rather than being separate activities, are aspects of the same ‘productive thinking’ process; the development of useful theories or actions requires a blending of creative and critical thinking. In fact, productive thinking often requires combining different qualities that may seem to be “in tension,” such as creativity and criticality, tradition and innovation, perseverance and flexibility, and viewing a problem (or theory, data set,...) from different perspectives. Fully aware observation can be highly effective in converting available information into recorded data. Following this, an insightful interpretation of observations can harvest more meaning from the raw data.

At its best, productive thinking combines knowledge with creativity and critical thinking. Ideally, an effective, productive scientist will have the ability to be fully creative and fully critical, and will know, based on logic and intuition, what blend of cognitive styles is likely to be productive in each current situation.

AN EVALUATION OF ISM AS A DESCRIPTIVE FRAMEWORK

2.08: Can ISM Describe a Wide Range of Views and Practices?

In constructing ISM there were two main goals, as described in Section 2.00. Objective B was to make ISM useful for education. An evaluation of ISM with respect to this goal will be delayed until Chapters 3 and 4.

Objective A was to make ISM useful for describing science and views of science: if it is designed well, "ISM should be capable of describing a wide variety of scientific practices, and articulating a wide range of viewpoints about what constitutes an accurate portrayal of scientific method." The nine parts of Section 2.08 explain why I think that ISM has achieved Objective A. It begins by making a distinction between the ISM framework and an elaboration of this framework.

A. One Framework, Many Elaborations

The basic ISM *framework* is described visually in the ISM diagram, and verbally in Sections 2.01-2.07. Later, there will be an *elaboration* of this framework in Sections 2.11-2.73, to describe and explain it in more detail, illustrate it with examples, interpret its meaning, and to describe my view of scientific methods. As indicated by the title of this subsection, there is one ISM framework, but my current elaboration is only one of many possible ‘alternative elaborations’ of ISM. With this perspective, ISM — which is the framework, not my elaboration — is more than my own view of science.

B. Can ISM describe a wide range of views?

Objective A contains two sub-goals: to describe science, and to describe views about science. The second sub-goal will be discussed first. I have claimed that ISM is able to "clearly express...divergent viewpoints, so the similarities and differences between these perspectives can be more clearly understood and articulated." But how is it possible for one framework to express divergent views? Because, I have claimed, differing interpretations of science can be described in terms of differences in the *characteristics* of science components, the *relationships* between components, and the *balance* between components.

It may be useful to think of ISM as an 'empty framework' with blanks that can be filled (with characteristics, relationships, and balances) in a wide variety of ways. In addition, in order to express some views it may be necessary to adjust the 'information content' of ISM by simplifying it (perhaps by temporarily eliminating some elements in order to focus attention on the elements that are considered most important) or by supplementing parts of it with ideas from other sources. In constructing ISM I have aimed for an intermediate level of complexity that can be adjusted by simplification or supplementation.

My claims about descriptive flexibility will be illustrated with alternative elaborations of four elements in ISM: external consistency and retroductive inference; cultural-personal factors; and hypothetico-deductive reasoning.

C. External Consistency and Retroductive Inference?

One conceptual factor in the ISM diagram is "external relationships with other scientific theories." Most interpreters of science would use one of these relationships, 'external consistency', as an opportunity to emphasize the importance of constructing a theory so it is consistent with other theories. But this is not the only option. The same factor could be used to introduce the contrary view of Feyerabend (1975) that scientific progress requires free innovation, with theoretical pluralism enhanced by the postulation of new theories that are incompatible with currently accepted theories. Thus, we see the same opportunity — provided by the 'external relationships' element of ISM — used in two very different ways.

Similarly, the 'retroductive inference' element in ISM could be used either to emphasize the importance of inventing new models that are consistent with known observations, or to explain Feyerabend's view that scientists should ignore this constraint by freely using 'counterinduction' to propose models that are currently unsupported by (or even contrary to) the existing evidence, because a currently accepted theory can make it impossible to observe the existence of observations which might falsify that theory and verify alternative theories.

D. Cultural-Personal Influence?

The existence of 'cultural-personal factors' in the ISM diagram might seem to imply that these factors are an important part of science. But this interpretation is not the only option. Someone could call attention to this ISM-element, and use it as an opportunity to express a contrasting view. For example, a teacher using ISM could argue that although some extremists emphasize the rare cases where cultural factors do exert significant influence, in the vast majority of scientific evaluations the effect of cultural factors is minimal. Or there could be an explanation, as suggested by Bauer (1992), of the checks-and-balances that occur in science during the communal process of 'filtering' knowledge claims, and that tend to counteract individual biases. Or there could be an admission that 'cultural bias' is common, accompanied by a claim that this bias is not a part of authentic science, so it should be avoided. This could be the entry point for a discussion about sources of bias, and for warnings about the dangers — because bias is detrimental to objective critical thinking, yet is difficult to avoid, tough to detect, and easy to rationalize — along with practical strategies for detecting bias and minimizing its effects. Or a teacher could take an activist stance with appeals for patriotism or a progressive populism, by deploring the lack of cultural conscience in science and exhorting students to use science for the benefit of the nation or 'the people'.

Compared with my elaboration of cultural-personal factors in Sections 2.31-2.34 and 2.45_{B}, sociologists who advocate a radical Strong Program approach to the study of science (Bloor, 1976, 1991) offer a very different description of characteristics, relationships, and balance. For example, some strong-program sociologists describe *characteristics* that include cultural mechanisms for "creating facts" in the laboratory (Latour & Woolgar, 1979, 1986). This activity produces a *relationship,* which contrasts sharply with the traditional view of science, in which observations — the supposedly firm foundation for empirical evaluation — are caused by culture, not nature. In this way, and many others, there is a change in the *balance* of criteria for theory evaluation, with a dramatic shift toward cultural-personal factors and away from empirical factors. In comparing a Strong Program elaboration with my own elaboration, there are different characteristics, relationships, and balances, and therefore a different view of science, even though the same ISM framework is used for both of these alternative elaborations. It is the differing elaborations, not the framework, that produces the differing views of science.

Another example of approximate neutrality is that the ISM framework does not explicitly express any opinions on multicultural perspectives of science, such as feminist critiques (Rosser, 1989) that science — including its educational practices and institutional structures, profession-related politics, thought styles, and theories — is significantly influenced by gender. But ISM does include a "culturally influenced thought styles" category where these ideas could be discussed, and where a wide variety of opinions could be expressed.

E. Hypothetico-Deductive Reasoning?

Although I think the ISM framework is relatively neutral on most issues, it probably contains a bias against empiricism (a view that scientists should never include ‘non-observable’ entities or actions in their theories) in favor of hypothetico-deductive reasoning, which is the foundation for non-empiricist science.[3] Perhaps there is bias due to the prominent ‘hypothetico-deductive box’ in the ISM diagram, and the discussions of hypothetico-deduction and retroduction in Sections 2.01 and 2.05. This emphasis is intentional; when constructing this part of ISM there were two options — I could clearly explain hypothetico-deductive reasoning, which I consider the essential foundation of modern science, or adopt a weaker treatment that is less biased — and I chose clarity. But the ISM framework, because it includes both descriptive theories (with only observable components) and explanatory theories (with components that are either observable or nonobservable), can be used to express empiricist and non-empiricist views of science.

The preceding paragraph contains the modifiers "probably" and "perhaps" because a perception of bias depends on inferring that the existence of an element (such as external consistency, retroduction, cultural-personal factors, or hypothetico-deduction) is an implicit argument that the element is an essential part of science. I think this is a valid inference, but when interpreting ISM it should be tempered by the explanations offered above, that an ISM-element can be used as an

[3]. Non-empiricist science requires hypothetico-deductive logic, but empiricist science can be done with or without it. And hypothetico-deductive logic can be done for theories with observable and/or unobservable components.

opportunity to express any view, including a view that a particular element (such as external consistency) is not a part of science as it is practiced or as it should be practiced.

F. Analysis and Holism

It might be argued that there is a fundamental incompatibility between the analytical approach of ISM and a holistic view of science. Does the analytical orientation of ISM hinder the expression of a more holistic perspective? This is a legitimate concern.

The overall orientation of ISM — which splits science into component elements, and then considers the integrated relationships between these elements — is 'analysis and synthesis'. This orientation is evident in the total framework, as represented in the ISM diagram, and also in smaller parts of ISM. For example, my approach to cultural-personal factors involves analysis and synthesis, by describing 'the whole' in terms of its constituent elements: five influencing factors, individuals and groups, institutional structures, profession-related politics and societal politics, self-interest and personal consistency, feedback between factors, two-way influences between science and culture, and thought styles.

My main defense for this approach is necessity. If anyone tried to build a complete model of science (or just cultural-personal factors) with full consideration of the possibilities for multi-level, multi-factorial interactions, the complexity would quickly become overwhelming. In an attempt to cope with this complexity, and to balance the tension between completeness and simplicity, I have found analysis-and-synthesis to be a useful tool in constructing ISM. Even though this approach does simplify, and cannot adequately describe the composition and operation of everything in the complex system of science, neither can any other type of model. Each approach, including my own, has advantages and disadvantages. Although rigorous logic cannot be used to justify a splitting of the whole into categories, it can be *useful* to think about categories and their overlaps, simultaneous and sequential relationships, and the variety of actions, interactions, and reactions.

Can holism be explained by using analysis? With a skillful description of relationships, using the ISM framework, many essential characteristics of a holistic view can be described. And even if there were difficulties due to a mismatch of basic styles, this could stimulate a thoughtful

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examination of holistic and analytic/synthetic approaches — to try to understand each approach, and compare them to find their similarities and differences, and their relative advantages in various contexts and for different purposes.

G. Can ISM describe a wide range of science?

The previous subsections have explained how ISM can describe different interpretations of science. In the same ways — by varying the characteristics, relationships, and balancing of elements — ISM can also describe a wide range of science, across fields and across time. For example, I would offer differing descriptions for the typical scientific methods currently used in the fields of molecular biology, paleontology, astronomy, elementary particle physics, psychology, and nutrition. And there would be different descriptions for astronomy in 1500 and in 1997.

Three types of variations — across different interpreters, fields, and times — have been discussed. These variations are combined in the following example. Imagine that three people use ISM to describe four types of science: current biology and physics, and biology and physics in 1850. The result will be twelve ISM-based descriptions. Maybe this type of ISM-based comparison could be used to develop a deeper understanding of variations, across fields (for example, by comparing current biology with current physics, or biology in 1850 with physics in 1850) and across time (by comparing biology now and in 1850, or physics now and in 1850).

Or there could be an ISM-based study of variations that occur within a narrower range of fields and time. For example, in the 1960s and 1970s there was fierce competition between three communities with different theories about the mechanism of oxidative phosphorylation. A comparative analysis of the scientific methods used by these three groups, each taking a different approach to studying the same area of nature, could be facilitated by ISM.

H. Is ISM biased?

In discussing the bias of ISM, it is necessary to distinguish between the ISM framework and my elaboration of this framework. My two main claims are that the ISM framework is relatively neutral, and that although my elaboration is biased this does not affect the neutrality of ISM.

The main bias in my elaboration of ISM, which is situated in the moderate mainstream of current interpretations, is a bias against what I consider to be “extreme” interpretations of science. Although my elaboration contains many original ideas, it will raise relatively few eyebrows or pulse rates among the majority of scientists and ‘study of science’ scholars. This is partly because the views in my elaboration are not very controversial (especially among scientists), and partly because I have tried to accurately describe the most influential current interpretations of science, even when I disagree with these interpretations. But in three sections of the elaboration (2.24, 2.44, 2.45) strong opinions are clearly expressed when I criticize empiricism and the unwarranted stretching of credible views (logical skepticism, cultural-personal influence, and relativism) into extreme views. And my detailed treatment of hypothetico-deductive logic is an implicit argument against empiricism. If one defines ‘bias’ as any expression of any opinion, then it is impossible to avoid bias relative to other opinions. For example, even if a view is in the moderate mainstream of majority opinion, it will differ from other views (such as those referred to as "extreme") so it is therefore biased against these other views.

But none of this really matters. The question of my own impartiality, or lack of it, is irrelevant because another person could write an alternative elaboration that was compatible with the ISM framework, even though it expressed numerous views contrary to my own. Stated in a brief summary, “The views expressed in my elaboration are not necessarily those of the ISM framework.”

In thinking about descriptive flexibility and neutrality, it may be useful to think of ISM as a ‘language’ for describing science. A primary function of language is to allow accurate communication of ideas. A flexible language allows the accurate expression of a wide range of ideas. A neutral language would allow an equally easy, accurate, and influential expression of all ideas. To the extent that it is possible to express some ideas more easily, accurately, and powerfully, the language is biased toward these ideas.

Even though it is *possible* to describe any view in terms of ISM, it is *easier* to describe certain types of views, so these views are favored by the ISM framework. And the mere existence of an element in ISM may be interpreted, whether or not this is justified, as an implicit argument that this element is an important part of science.

When all of these factors are considered, it seems difficult to say anything definite. Perhaps my overall claim should be that ISM (defined as the ISM framework) is biased in some ways, but this bias is weak enough to overcome, thus allowing ISM to be used for clearly expressing a wide range of scientific practices and views about science.

I. Can ISM cope with differences in terminology?

The "Introduction to the ISM Framework" states that "there is no consistent terminology; instead, there are important terms...with many conflicting meanings, and meanings known by many names." This leads to the question, “Can ISM describe the views of a person who uses different terms than those in ISM, or who defines the ISM terms differently?”

There are several possible responses to this challenging question. One approach attempts to solve conflicts by a simple substitution of one term for another. But a more sophisticated approach is needed for the really tough inconsistencies; for these conflicts a “solution” seems impossible, but there may be ways to minimize the confusion. And in any case, difficulties arising from choices about terms are not unique to ISM; because of the widespread inconsistency in terminology, every description of science faces a similar challenge.

Substitution is an easy way to cope with some differences. For example, the same process of logic is known by two common names: retroduction and abduction.[4] There are good reasons to use ‘retroduction’,[5] but if someone wants to replace this term with ‘abduction’ it would cause no significant change in ISM. Another potential candidate for substitution is ‘experimental system’; in ISM this is defined as "everything involved in an experiment" for a reason,[6] but sometimes

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[4]. Almost everyone uses these two terms as synonyms, but as usual there is variation. Ernst McMullin (1992) uses ‘abduction’ to mean the same thing that ‘retroduction’ does in ISM, but he uses ‘retroduction’ to describe a wider-ranging process of thinking that includes “abduction and more.”

[5]. I decided to use ‘retroduction’ in ISM, for two reasons. First, the prefix ‘retro’ is a reminder that this logic is oriented backward in time, to explain what already has occurred. Second, if ISM is to be maximally useful for science education, and if part of this usefulness occurs when teachers encourage children to learn a productive form of creative-and-critical thinking, it seems unwise to call this desirable activity ‘abduction’ — a term whose primary common meaning is the undesirable activity of kidnapping.

[6]. Observations are produced by an experiment that involves everything in the experimental

scientists define a system as only "what is being studied," with "what is done to it, and the instruments of observation" being external to the system. In this case, ‘experimental system’ could be replaced by another term, such as ‘experimental setup’ or ‘a system and its experimental context’; this substitution would not alter the hypothetico-deductive logic used in the ISM framework. Or someone might want to replace theory ‘invention’ with ‘development’ or ‘generation’.

But for three ISM terms — model, hypothesis, and theory — the situation is more complex. Part of the difficulty is inconsistent terminology; these terms and others (such as principle, law, concept, or conjecture) are often used with similar or overlapping meanings, or with contrasting meanings that differ from one definer-of-terms to another. The overall use of these terms lacks both consistency and precision.

The term ‘model’ is used in many ways, so no matter how this term is used there will be conflicts with other definitions. Often it seems to be a synonym for a theory or sub-theory, or for a certain type of theory. Or it can refer to an exact application or (more commonly) a simplified application of a theory for a certain type of system, as when Giere (1988, 1994) defines a theory in terms of "families of models." For example, the theory of Newtonian Mechanics includes many models and families of models, such as gravity-driven motion on an inclined plane with no friction, or with friction, or with friction plus air resistance. Giere thus uses the term ‘model’ in two ways that are different yet compatible: each of the models (that together comprise the theory) can be useful when constructing a model (as this term is used in ISM's hypothetico-deductive box) for certain experimental systems. For example, an ‘inclined plane’ *family of models* is a useful shortcut when applying Newton's theory for a specific system that is a member of the corresponding *family of systems,* while a ‘pendulum’ family of models is useful for theory application within another family of systems. By analogy with the distinction between ‘domain-theories’ and ‘system-theories’ in Section 2.05, there can be ‘domain-models’ (such as the family of inclined plane models that occurs when Newton's theory is applied, in various ways, within the

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system. And to make predictions a model must consider everything, including the instruments of observation.

domain of inclined plane systems) and ‘system-models’ (resulting from the application of Newton's theory, in a certain way, to specific inclined plane systems). And, analogous to the influence of a domain-theory on system-theories, a general domain-model (about systems in a domain) will influence the construction of a specific system-model for one system in its domain.

‘Hypothesis’ has an even wider variety of conflicting meanings. Consistent with Giere (1991), ISM defines a hypothesis as a claim that a theory-based model is similar to a real-world system "in indicated respects and to an implied degree of accuracy. (Giere, 1991, p. 27)" Gibbs & Lawson (1992) define it as "a single proposition intended as a possible explanation...for a specific effect (p. 143)" in contrast to "a theory...intended to explain a broader class of phenomena and consisting of multiple hypotheses and/or general postulates that, taken together, constitute the explanation (p. 149)"; but the authors report with dismay that "a number of textbooks give examples of hypotheses that clearly are predictions, not hypotheses (p. 147)," and that most authors "define theories as hypotheses that have been supported over a long period of time (p. 147)" even though according to the authors' own definitions "the evidence may or may not support a theory (p. 148)" — for example, the Ptolemaic Theory of earth-centered planetary orbits is still considered to be a theory even though it now has low status. Darden (1991, p. 17) describes two meanings for theory — it can be "[a claim that is] hypothetical, not yet proven" or "a well-supported, general, explanatory claim" — and chooses the latter definition to use in her analysis; and she defines a hypothesis as "a proposed alternative to a theoretical component. (p. 18)" Grinnell (1992) usually uses hypothesis in the same way that ISM uses theory (a word he never uses) — for example, he says that scientists "imagine hypotheses to explain these regularities...in observed natural events (p. 1)," and in criticizing the model of theory falsification (Popper, 1963), he says that "according to this model, science progresses through selective falsification of competing hypotheses, ... [and] it takes only one negative result to call a hypothesis into question (p. 40)" — but sometimes this word changes meaning and is a prediction: "[an explicit hypothesis] is the change that I expect to see if I do [a certain experiment]. (p. 25)"

The chaotic state of terminology is captured in the paragraph above. In it, hypothesis is defined — by Giere, Gibbs & Lawson, Darden, and Grinnell, all authors whose work I respect — as a claim about a model for one system, or an explanation for a type of phenomenon, a sub-theory or theory component, a prediction, a new theory with low status, a newly proposed theory component, and a theory. What a wild mix! Generally, however, the difference between hypothesis and theory tends to be defined along two dimensions — scope and certainty.[7] When both factors point toward the same choice of a term there is general agreement, which includes myself and ISM, about definitions: if a proposed explanation has narrow scope and low status, it is a hypothesis; if it has wide scope and high status, it is a theory. With mixed criteria (narrow scope and high status, or wide scope and low status) there is less agreement. Another criterion is age; older explanations tend to be called theories, not hypotheses. For example, the Ptolemaic Theory remains a theory even though it currently has very low status; once a theory, always a theory?

In ISM the distinction between a hypothesis and theory depends only on scope; age doesn't matter; and a hypothesis can have either low status or high status, as can a theory. In ISM (and for Giere) the criterion for scope is clear; a hypothesis refers to one system, while a theory refers to two or more systems. By contrast, the other authors define the distinction with words like "broader" or "general."

I do not claim that my use of 'hypothesis' and 'theory' is the best possible solution for this "terminology problem." The principle advantages of my definitions are logical simplicity and internal consistency. The main disadvantage is that, despite agreement with the use of terms by some philosophers (especially Giere) there is disagreement with other philosophers and with most scientists.

There are significant logical advantages, described below, in using the ISM terms. These advantages carry over to practical concerns. For example, with one non-ISM definition a low-status theory (hypothesis) may eventually become a theory, but before this occurs the hypothesis occupies the same status as 'theory' does in the current ISM. This means that everywhere in the ISM diagram where "theory" appears (15 places) it would have to be replaced by "theory or hypothesis"; similar changes would be needed throughout the text of Chapter 2.

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7. But if a hypothesis is defined as a theory-component (as it is by Gibbs & Lawson, and by Darden), this is not necessarily the same as having narrow scope (which is also used by these authors to define a hypothesis), because a component can have wide scope.

A logical disadvantage of this non-ISM definition is that if "hypothesis" appears in the "theory" oval (on the left side of the diagram) rather than in the hypothetico-deductive box, how does 'hypothesis' enter into 'hypothetico-deductive' reasoning when a theory (not a hypothesis) is used to construct a model and make predictions?

A logical advantage of the ISM definition of theory, which avoids the use of status in defining a theory, is that this lessens the rhetorical value of using the term 'theory' to influence critical thinking. For example, in the quotations cited above Darden describes two ways to define a theory; in one a theory is "hypothetical, not yet proven" while in the other it is "well-supported," so the same word can be used to imply low status or high status! I think it is better to evaluate a proposed explanation (i.e., a theory) based on merit rather than terminology. We can just say "here is a proposed explanation; now we can decide how well supported it seems to be," instead of trying to short-circuit the critical process by saying "it is a theory so it is not proven" or "it is a theory so it is well supported." Another difficulty in drawing a demarcation line between hypothesis and theory is pointed out by Gibbs & Lawson (1992): "Who decides when an hypothesis is supported to the extent that it gets to be called a theory? (p. 148)"

There are many logical and practical reasons to use the definitions I have chosen for ISM; these have overcome the advantages of using a more commonly used definition. This is one of the few places where I am aware of ISM being normative by "prescribing" how things should be done in science, by saying "this way is more logical and practical." This decision was made easier by the fact that no matter which definitions I use in ISM, they will be inconsistent with the many other definitions that are commonly used; it is impossible to be consistent with inconsistency.

2.09: Is ISM a model for 'scientific method'? (Part 1)

A. A Penchant for Patterns?

There is a difference, among scholars who study science, in a tendency to see patterns in the methods used by scientists. For example, consider the complaints (by Laudan, et al, 1986) that even though some of the newer philosophical models are compatible with the 'social influence' interpretation of science favored by many historians,

> Very few historians of science are currently involved in evaluating and improving theories of scientific change. ... Despite the importance of history in these models...there has been only cursory examination by historians of the claims made by these theories and practically no attempt on the part of professional historians to use them in a serious way to inform their interpretations of science. It has been an opportunity missed and, from the point of view of developing a comprehensive theory of science, a responsibility avoided. (p. 152)

In reply to Laudan's accusation of "responsibility avoided," most historians would claim that their history is better when it is not constrained by the presuppositions imposed by a model, when they are free to draw their conclusions from the distinctive qualities of each historical episode.

Compared with their practice in the first half of the 20th century, since around 1960 many philosophers have been more interested in constructing and evaluating their models of science based on what science actually is, not what they think it logically should be. This trend toward a more empirically based philosophy of science, using observations of science, both past and present, has made philosophy more historical. One indication of this trend is that philosophers would express disappointment, as in the quotation above, that historians have not contributed more often, and more enthusiastically, to a joint endeavor between philosophy and history.

Why are historians not more eager to use the models proposed by philosophers, or to construct their own models? One reason is that historians tend to be suspicious of claims about regularly occurring patterns in science, and of attempts to describe these patterns in models. One might say that ‘thought styles’ differ in history and in philosophy (or sociology), with less of a "penchant for patterns" in history. Due to this difference, even though historians definitely have ‘views’ about science, they are usually less interested in constructing ‘models’ than are philosophers and sociologists.

B. Skeptics about Methods

Some critics claim that the methods of science are too variable and complex to fit into a coherent universal model. A few scholars acknowledge these difficulties but nevertheless try to develop a model. But the majority of writers simply discuss their selected aspect(s) of science, without trying to construct a method, or to criticize any attempt to do this as futile. Questions about scientific methods are rarely the topic of direct discussion; instead, differing perspectives are more likely to be manifested in the structuring of scholarly works.

People differ in their tendency to see patterns in events. As discussed above, when historians of science study a particular episode they search for patterns and causal relationships, but they are reluctant to generalize these patterns to other situations. In other words, most historians tend to doubt the existence of *regularly occurring* patterns. This skepticism is shared by some scholars in other fields, including science education. For example, an important work in recent science education, *Science for All Americans* (Rutherford and Ahlgren, 1989), discusses the methods of science but never mentions a ‘scientific method’; the authors do not explain why this term is avoided. Sometimes there are explicit criticisms of attempts to define patterns in the methods of science. One of the most outspoken critics was Feyerabend (1975) who criticized any attempt to define scientific method, claiming that "there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at one time or another," so the only principle that always applies is "anything goes."

There are several reasons for skepticism about scientific method. Some anti-method arguments focus on disagreements among scholars who study science; if these experts cannot agree on what the methods of science are, then we should not pretend there is a method. And the empirical evidence of history shows that the methods used by scientists change with time and with culture, and vary from one discipline to another. Also, there has been a backlash against overly logical, method-oriented approaches, such as logical empiricism, that exaggerate the level of objectivity in science and that do not acknowledge the creativity involved in scientific activities, especially in experimental design and theory invention.

These criticisms have served a valuable function during the process of constructing ISM, because they provide guidance on what to avoid. But the goal that is being criticized — of trying to develop a universally applicable algorithm for doing scientific research — is not the goal of ISM, so I just nod my head in agreement and continue with the work of developing a model that can minimize the difficulties the skeptics point out.

C. Does ISM try to describe a ‘method’ in science?

The answer depends on a definition of ‘method’. If this means a rigid sequence of steps or a view that all science is the same, there is no method. But with a broader definition of method, the answer is yes.[8] However, this “yes” is really the answer a different question, after conversion from singular to plural: “Are there methods in science?” When the goal shifts from finding ‘a method’ to finding ‘methods’ that are “variations on a basic theme,” the search becomes easier and more productive because there is a closer match between the goal and the reality of actual scientific practice.

Because the methods of science are flexible in their application, a model of scientific method should be flexible. Therefore, ISM does not try to define a single method for all science. But ISM can be used to describe commonly occurring patterns, such as a cycle where observations are used for evaluation that is used to design experiments which produce observations, and the cycle begins again; during cycles of experimentation, empirical knowledge of a domain increases, and often there is a ‘successive approximations’ approach to theory development by revision. Scientists can begin at any point in the cycle. This cycle of observation-and-interpretation can be described using the input/output arrows of ISM: “observations → logical comparison → empirical evaluation → theory evaluation → experimental design → doing experiment → observations → ...(continued cycling).” Or an extra step can be added for the retroductive selection (or invention) of a theory: “observations → retroductive selection → prediction → logical comparison...”

ISM can also be used to describe and analyze complexities of timing, with overlapping and interconnected activities, iterative cycles within cycles, multiple branchings, and so on. But even though there are some patterns in the sequencing of activities, ISM should be viewed, not as a rigorous flowchart of scientific activity, but as a roadmap that shows some possibilities for creative wandering.

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[8]. Similarly, a definition of ‘no method’ must be broad, if a claim that “there is no method” is to be evaluated as being credible. If a “no method” claim is taken literally and rigorously, as a statement of total anarchy, there can be no relationships, causal or correlational, between any pair of actions, with no repeating patterns of any kind. But this view (which could perhaps be described by removing all arrows from the ISM diagram?) is not what is intended by claims that “there is no method,” and it would not be accurate to criticize anti-method views on the basis of this strawman.

Due to the broad definition of ‘methods’ described above, instead of calling ISM an “integrated scientific method” it would be more accurate to call it a “framework for describing some typical relationships between activities that are often used in science.” But ‘ffdstrbataouis’ is too cumbersome for comfort, so I will continue to use ‘ISM’ as a convenient abbreviation.

D. Is ISM a Model?

The answer depends on a definition of ‘model’. According to some definitions, but not others, the answer is yes.

A ‘domain-model’ for an inclined plane, as described in the last part of Section 2.08, is an empty framework with blanks to fill with details (by asking “what is the angle of the plane? coefficient of friction? shape of the object? value of “GM/r^2” in “$F_{gravity} = GMm/r^2$”? mass of the object? frontal surface area of the object? coefficient of air resistance?”) for a specific inclined plane system. Similarly, ISM is an empty framework with blanks to be filled with details (for the characteristics, relationships, and balances of components) for a specific science situation, so in this way ISM behaves like a domain-model. For these two types of models, some functional relationships are roughly analogous; a general domain-model for an inclined plane (provided by application of Newton's theory) makes it easier to construct a system-model for a specific inclined plane system, and a general domain-model for “science actions” (provided by the ISM framework) makes it easier to construct a system-model for a specific “science action” system.

But the analogical relationships become less clear when the ISM framework is used to describe a particular view of science. For example, when ISM is used to describe the views of Feyerabend, are these views the ‘system’ that is being described? Or does this ISM-based description become a new framework, competitive with the original framework, that can function as a domain-model to be used for describing a specific “science action” system such as an episode in the history of science? Or, to be internally consistent with the definitions of model and theory used in ISM, should I consider the ISM framework (or an ISM elaboration) to be a theory of scientific methods, rather than a model of scientific methods? Or is it something else?

This section has ended with a question, symbolizing the unfinished state of the evaluation,

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which continues when Section 2.9 reconsiders the same question, "Is ISM a model for 'scientific method'?"

AN ISM ELABORATION

ISM is the general *framework* described earlier, not the personal *elaboration* of ISM that follows in Sections 2.11-2.73. Although my elaboration discusses topics from the framework in more detail, it is not intended to be an exhaustive treatment, but is only a summary of ideas that have been discussed in much greater detail by other scholars, who have written lengthy chapters (and whole books) about topics that I describe in one short section.

The following elaboration assumes that the reader is familiar with the entire ISM framework as background knowledge, and the numbering systems run parallel: 2.1 covers the same topics as 2.01 but in more detail, 2.2 elaborates 2.02, and so on.

A Numbering System for References: Because ISM is an *integrated* model of science, when describing one aspect of science it is often necessary to refer to another aspect that is discussed in another section. To make these references more precise, so that instead of referring the reader to a broad "Chapter 2" the search can be narrowed to a small area, a logical numbering system is being used. There are several levels of numbering. At one level, Section 2.1 contains all sections that begin with 2.1, so it includes 2.11 and 2.12. At another level, Section 2.11 includes an introduction (2.11_i) and five subsections, 2.11_A to 2.11_E. This numbering system also helps to show the logical organization of concepts in ISM.

2.1: Empirical Factors in Theory Evaluation

The topic of Section 2.1 — theory evaluation based on empirical observations, by using hypothetico-deductive logic — is often considered the foundation of scientific method. I agree. But science includes this and much more; evaluation based on empirical data is not the only factor to influence theory evaluation, and theory evaluation is not the only activity in the process of science.

2.11: System and Model, Predictions and Observations

The following elaboration assumes that during problem formulation, there already has been the selection of an area of nature to study, and the selection (or invention) of a theory about this area.

A. Theories

In ISM, a **theory** (or **supplementary theory**, or **alternative theory**) is broadly defined as any humanly constructed representation whose purpose is to describe or explain phenomena in a specified domain of nature. For the purpose of scientific utility in making an empirical evaluation, the most important functional characteristic of a theory is its ability to make predictions that can be compared with observations.

B. Experimental System

In ISM, a "real-world **experimental system**" is defined as everything involved in an experiment, including what is being studied, what is done to it, and the instruments of observation.[9] For example, in an experimental system of crystalline DNA irradiated by x-rays, the system includes the x-ray source, DNA, x-ray detector/recorder, plus the physical context (such as the nuts and bolts and plates that are used to fix the spatial positioning of the source-DNA-detector). In ISM, 'experimental systems' are defined broadly to include both *controlled experiments* (such as the x-ray/DNA experiment just described) and *field studies* of natural phenomena (e.g., starlight

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9. This differs from a definition sometimes used by scientists, when they think of a system as only "what is being studied," with "what is done to it, and the instruments of observation" being external to the system. With this second definition, in an experiment with "DNA, x-rays, and recorder," the system is only the DNA; everything else is an "experimental context" designed to study the system. The main advantage of the ISM definition is logical utility; in order to obtain predictions, a deductive thought-experiment must include everything in the experiment, including "what is being done to the DNA" and the instruments used to record observations.

being observed with a telescope, or an earthquake observed with a seismograph and by examination of damaged buildings).

C. Theory-Based Model of Experimental System

Scientists can use a selected **theory** and (as necessary) **supplementary theories** to construct a theory-based **model** for an experimental system. Two types of theories, explanatory and descriptive, can be used to construct models.

With an *explanatory theory,* a model is a simplified representation of the system's *composition* (what it is) and *operation* (what it does). The composition of a model includes its parts and, in some systems, their organization into larger structures. A model's operation includes the actions of parts (or structures) and the interactions between parts (or structures).

With a *descriptive theory,* a model is a description of observable properties and their relationships. This type of model can include components for composition and operation, but a complete composition-and-operation description of the system is not considered to be a necessary function of the theory, as long as the model can make predictions about the observations that will occur when the experiment is run.

In ISM an *experimental system* is defined as "everything involved in an experiment." But scientists do not include "everything" when they construct the model that is their simplified characterization of the actual system. Instead, they must decide what to include and exclude. For example, according to the theory of General Relativity the gravitational pull of Pluto will affect the x-rays in an experimental system designed to study DNA, but in this context the effect of Pluto is considered negligible, so scientists will choose to ignore this trivial factor. In fact, it is doubtful that a 'Pluto factor' will even be proposed; it will thus be implicitly rejected by omission, rather than explicitly rejected by conscious decision.

Because x-rays are an important part of the x-ray/DNA system described earlier, when scientists construct a model of this system they use theories about x-rays. These theories are used to interpret the interactions of x-rays with the DNA and also with the detector. An 'x-ray observation theory' will postulate a causal relationship between x-rays and a photograph that is used to detect-and-record experimental observations of x-rays. And an ‘x-ray diffraction theory’ will claim a causal relationship between a certain type of 3-dimensional solid structure and a certain type of x-ray pattern in a photograph. A useful description of the relationships between theory and observation is provided by Shapere (1982), who analyzes an "observation situation" as a three-stage process in which information is released by a source, is transmitted, and is received by a receptor, with scientists interpreting this information according to their corresponding theories of the source, the transmission process, and the receptor.

In ISM, ‘theories of observation’ are generally included among the supplementary theories. In addition, any other theories that are required to construct a system-model, such as theories about the interactions between x-rays and solids, are defined as supplementary theories. In attaching the label ‘supplementary’ there is an assumption about the main goals of scientists who are designing, doing, and interpreting an experiment. For example, in the context of the early 1950s when “DNA chasers” were trying to invent and test a theory for DNA structure, this DNA theory would be considered the ‘main theory’ (or, in ISM terminology, the ‘theory’) while theories about x-rays (including their generation, transmission, interaction with DNA, and detection) would be ‘supplementary theories’. But in an earlier stage of x-ray experimentation, there were similar experiments but with different assumptions (the structure of a substance was assumed to be known, based on previous experimenting and theorizing) and different goals (to develop a theory for determining the structure of a solid from its x-ray patterns). At this time scientists were using the same types of theories as in the 1950s, but in describing this earlier problem-solving context these theories are labeled differently; an x-ray theory is the main theory, while a theory about the solid's structure is supplementary. Thus, whether a theory that is used for model construction is called a ‘theory’ or a ‘supplementary theory’ depends on context; and while there are usually good reasons for assigning these labels, such decisions are to some extent arbitrary.

D. Model-Based Predictions

After a model is defined, a **thought-experiment** can be done by asking, “IF this model is true, THEN what will occur?”, thereby using **deductive logic** to make **predictions** about the experimental observations. The process of prediction can be done in various ways — by logical deduction, by calculation, by "running the model" mentally or in a computer simulation, or by generalizing based on the results of previous experiments — with the choice of method depending on the nature of the model, which in turn depends on the nature of the SYSTEM and theory.

Another perspective on the process of prediction is that a theory is being combined with a characterization of the *initial system-conditions,* to predict the *final system-conditions.*

E. Experimental Observations

When a **physical experiment** is done with the experimental system, **observation detectors** (and recorders) are used to obtain **observations**. Data can be collected more than once during an experiment. With an x-ray/DNA system, for example, acts of observation can occur early, to establish and measure the initial conditions (such as x-ray wavelength, geometry of the source-DNA-detector setup, and so on) that characterize the experimental system, and are required to make predictions. Observations can also occur later, to observe an outcome (in this system it is a photograph) that is labeled "observations" in ISM.

2.12: Hypothetico-Deductive Logic

The hypothetico-deductive part of the ISM diagram is drawn in the shape of a box (adapted from Giere, 1991) because this dual-parallel shape symbolizes two parallel relationships. First, the left-side process (done mentally to produce predictions) parallels the right-side process (done physically to produce observations). A different type of parallel exists between the top and bottom of the box. At the top, the *hypothesis* (using the definition of Giere, 1991) is a claim that the model and system are similar, in specified respects and to a specified (or implied) degree of accuracy. At the bottom is a logical comparison of the left-side deductive predictions (by the model) and right-side observations (of the system). *Hypothetico-deductive reasoning* gets its name by combining 'hypothetico' (at the top of the box) with 'deductive' (the left side of the box), and it occurs when logical comparison (at the bottom) is used to evaluate a hypothesis (at the top), based on the 'parallel logic' that the degree of agreement between **predictions** and **observations** may be related to the degree of similarity between **model** and **system**.

The remainder of Section 2.1 will discuss: 'degree of agreement'; the limitations of hypothetico-deductive logic, and why these limitations indicate the need for 'degree of contrast' as a second criterion; and how empirical evaluations are affected by multiple sources of data.

A. Degree of Agreement

In the context of formal logic, a 'deductive' inference implies certainty. But in the hypothetico-deductive logic used in science, deductive inference often involves uncertainties and a need for probabilistic predictions. When making observations there are also sources of uncertainty, such as 'random errors' in measurements. Although a quick, common sense comparison is sometimes enough to make it obvious that predictions and observations do or do not agree, often the use of sophisticated techniques of data analysis — which take into account the statistical nature of predictions and observations — will help to produce a more reliable estimate for the degree of agreement between predictions and observations.

For example, classical genetics uses 'statistical deduction' to predict that, for a trait governed by simple dominance, a mating between two heterozygous organisms will produce offspring in which 25% have the recessive variation of the trait. So, if 4 of 20 offspring have the recessive variation, is there agreement or not? In an effort to form a more reliable estimate for degree of agreement when considering this type of question, scientists use statistical data analysis, and the answer they seek is a probabilistic estimate, not a simple yes or no. A variety of analytical techniques are available, including an attempt to answer the obvious question, "How closely do the observations match the predictions?", plus appraisals of sample size, and more. The use of statistics varies between fields of science, and from one research project to another.

B. Degree of Predictive Contrast

In hypothetico-deductive logic, one criterion for evaluating a hypothesis is a logical comparison of the agreement between predictions and observations. But even if the degree of agreement is high, this does not necessarily provide strong support for the theory. Why? Because the support provided by empirical data is limited by the possibility that two or more models might make the same correct predictions. Briefly summarized, the principle is that when a theory makes a prediction that "if T, then P" and P is observed, this does not prove T is true. For example, consider a theory that Chicago is in Wisconsin, which produces the deductive prediction that "if Chicago is in Wisconsin, then Chicago is in the United States." When a geographer confirms that Chicago is in the U.S., does this confirm the theory? No, because alternative theories, such as "Chicago is in Illinois" and "Chicago is in Iowa," make the same correct prediction.

In an effort to cope with this logical limitation, Giere (1991, p. 38) suggests that an estimate for degree of agreement should be followed by the question, "Was the prediction likely to agree with the data even if the model under consideration does not provide a good fit to the real world?"

Alternatively, this question can be framed in terms of a "Surprise Principle" (Sober, 1991) by estimating the degree to which the observations would be 'surprising' if the model was not a good representation of the system. To estimate the degree of surprise, or to answer the equivalent question posed by Giere, requires a consideration of how likely it is that plausible alternative theories might make the same correct predictions about the experimental system.

A third perspective is to focus on the experimental design by asking "To what extent did the experiment provide a crucial test that can discriminate between the **theory** and plausible **alternative theories**?" For example (Sober, 1991), asking John to lift a hat as an experiment to test a theory that "John is an Olympic weightlifter" yields a high degree of agreement (because the theory predicts he can lift the hat, and he does) but a low degree of surprise (because plausible alternative theories, such as "John is a man with average strength, not an Olympic weightlifter," predict the same result), so this experiment is inconclusive and offers little support for the Olympic Weightlifter theory despite its correct prediction.

Each of these perspectives — would it agree anyway, is there surprise, and is it a crucial experiment — is a different way to ask the same question. In ISM this question is viewed from a fourth perspective by asking, "What is the degree of contrast between the predictions of this theory and the predictions of plausible alternative theories?" For a particular experiment, a comparison of the predictions made by competitive theories can be used to estimate a **degree of predictive contrast** that supplements the **degree of agreement** criterion, in order to estimate the validity of inferring that an agreement (between prediction and observation) indicates a similarity (between model and system). A fifth perspective provides an informal yet effective way to think about predictive contrast, by asking the simple, intuitive question: Should an agreement between predictions and observations elicit a response of “So what?” or “Wow!” ?

All five perspectives could be used to interpret any situation involving an experimental system and two or more competitive theories. For example, as a critical test for an Olympic Weightlifter theory (OW) and its consequent models, a “lifting the hat” experiment should elicit a response of “so what” because the result was likely to occur even if the OW-theory was wrong, so the result would not be surprising even if OW was wrong. This experiment does not provide a crucial test to discriminate between OW and its competitors, because there is a low degree of contrast between the predictions of OW and the predictions of plausible alternative theories.

Degree of predictive contrast is a counterbalance to the logical principle that T is not proved when the prediction is that “if T, then P,” and P occurs. The truth-status of T increases when it is difficult to imagine any other theory, except T, that would also correctly predict P. Of course, an apparent lack of alternative explanations could be illusory, due to a lack of imagination, but scientists usually assume that a high degree of predictive contrast increases the justifiable confidence in a claim that there is a connection between a prediction-observation agreement and a model-system similarity.

In the example above, high agreement provided little support for a “weightlifter” theory, due to low predictive contrast. But if in another experiment the man lifts a world record weight, there would be a high degree of predictive contrast, which would support a claim for the apparent uniqueness of the predictive capabilities of the weightlifter theory.

C. Two Evaluation Criteria, and Multiple Sources of Data

There are two major criteria for empirical evaluation: degree of agreement and degree of predictive contrast. The criterion that is most straightforward, although it often requires the use of sophisticated statistical logic, is agreement. But this criterion should be combined with predictive contrast, when making any claim that a high degree of agreement lends support to a hypothesis about a model.

But a scientific theory is never based on the results of one experiment, and an empirical evaluation can combine evaluations (for both agreement and predictive contrast) from all relevant experiments and hypotheses. Therefore, in the ISM diagram the ‘empirical factors’ box contains both **empirical evaluation of current hypothesis** and **empirical evaluations of previous hypotheses**. The question of what is "relevant" depends on the status of a theory and the goals of the evaluators. For a theory that already is developed, relevant hypotheses could include all models that have resulted from an application of this theory to experimental systems in the entire domain claimed by the theory. In most situations, however, for practical reasons the evaluators will decide to focus on a subset of selected hypotheses that are judged to be especially relevant for achieving a specific goal. For a theory that is in the process of being developed, relevant hypotheses will include those for any experimental system that scientists think may be helpful in testing or revising the new theory. For an existing theory whose domain is being widened by generalization, relevant hypotheses include all systems that are being considered as candidates for possible inclusion in a widened domain. In this case, scientists ask the question, “Does a system accurately represent other systems in the wider domain?”, and try to answer it by using thought-experiments.

In each problem-solving context, scientists must decide which hypotheses to include in empirical evaluation. Making these decisions effectively, in a way that contributes to achieving the goals of the scientists, is an art. The art and logic of empirical evaluation by using multiple sources of data will be continued in Section 2.52_E.

2.2: Conceptual Factors in Theory Evaluation

On the left side of the ISM diagram, three arrows go into ‘evaluation of theory’, representing inputs from three types of evaluation factors: empirical, conceptual, and cultural-personal. ISM follows Laudan (1977) in making a distinction between empirical factors and **conceptual factors**, and between conceptual factors that are *internal* and *external.* Internal conceptual factors (**internal consistency** and **logical structure**, in the ISM diagram) involve the characteristics and interrelationships of a theory's own components, while external conceptual factors are the **external**

relationships between components of one theory and components of another theory. There is a discussion of internal factors in Sections 2.21-2.26, and external factors in Sections 2.27-2.28.

2.21: Simplicity and Internal Consistency

In ISM a **theory** is a humanly constructed representation, intended to describe or explain a set of related phenomena in a specified domain of nature. A theory is built using *components* — i.e., propositions that can be used to describe empirical patterns or to construct composition-and-operation models for a system's *composition* (what it is) and *operation* (what it does). The internal **logical structure** of a theory involves the characteristics of its components, and the logical relationships among these components.

To illustrate logical structure, Darden (1991) compares two theories that claim to explain the same domain of known data; T_1 contains an independent theory component for every data point in the domain, while T_2 contains only a few logically interlinked components. Even if both theories have the same empirical adequacy, most scientists will prefer T_2 due to its logical structure. The next two subsections discuss the property of *simplicity,* which I will describe in terms of *simplification* and *systematicity.*

A. Simplification

Even though a complete model of a real-world experimental system would have to include everything in the universe, a more useful model is obtained by constructing a *simplified representation* that includes only the 'relevant' entities and interactions, and ignores everything whose effect on the outcome is considered negligible.

When scientists construct a model for a system of x-rays interacting with DNA, for example, they will ignore (implicitly, without even considering the possibility) the bending of x-rays that is caused by the gravitational pull of Pluto. A more realistic example occurs when scientists make an explicit decision, in applying Newton's laws of motion to the fall of a ball, to ignore air resistance and the change in gravitational force as the ball changes altitude.

One strategy for constructing a *family of models* (Giere, 1988) that are "variations on a basic theme" is to begin with a stripped-down model as a first approximation, and then make adjustments. For example, a simplified model applying Newton's Laws to a falling object might ignore the effects of air resistance and altitude change. For some purposes this simplified model is sufficient. And if scientists want a model that is more complete and accurate, they can include one or more factors that previously were ignored. The inclusion of different factors produces a family of models with varying degrees of completeness, each useful for a different situation and objective.

B. Systematicity

Systematicity refers to the logical relationships among the components of a theory. If one component is not logically connected to other components in a theory, this isolated component is considered an *ad hoc* appendage, and its inclusion makes the theory less systematic and less simple, and thus less desirable. For example, if scientists perceive T_1 (described above) as an inelegant patchwork of components, many of which appear to be ad hoc appendages because they are not logically related to other components and have no apparent function except to achieve empirical agreement with 'old data', there is likely to be a suspicion that T_1 is not a credible theory and that it will not predict 'new data'.

C. Simplicity

To evaluate simplification and systematicity, and to combine these into a scientifically useful estimate for the *simplicity* of a theory, is a subtle task that often requires a deep understanding of a theory and its domain, and sophisticated analysis:

> The comparative simplicity of two theories can only be established by careful examination of the assumptions introduced in the various explanations they provide. As has often been remarked, simplicity is very complex. Thagard (1988, p. 84)

Darden (1991) examines her own example of "T_1 versus T_2" and suggests that it can be useful to view the same structural characteristics from different perspectives: in terms of the systematicity of logical interconnections and the degree of ad hocness, or (because the components of T_2 are more general than the narrowly specialized components of T_1) the generality of theory components. Another way to describe component generality is to say (if T_1 and T_2 both explain the same domain

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of phenomena, but T_2 does this with fewer components) that T_2 is more 'efficient' because it does more explanatory work per component.

D. Internal Consistency

For obvious reasons, *internal consistency* — a logical agreement among a theory's own components — is a highly valued characteristic. Typically, it is detrimental to the status of a theory if its components are not logically related to each other, but it is even worse if components are related by logical inconsistency. The independence of components (with no relationships) weakens systematicity, but inconsistency among components (with bad relationships) is the ultimate non-systematicity.

2.22: Conflicting Criteria

A primary feature of ISM is the multiple inputs for theory evaluation; in theory evaluation there is a consideration of both empirical and conceptual factors, as discussed in this section, and also cultural-personal factors, with the relative weighting of factors varying from one situation to another. The combining of multiple evaluation criteria is assumed, usually without discussion, when evaluation factors are discussed in Sections 2.1-2.3. An explicit discussion of the interactions between multiple evaluation criteria begins in the following two subsections, and continues in Section 2.45_A .

A. Simplification versus Completeness and Empirical Adequacy

In scientific theories, completeness is often sacrificed to achieve simplicity, because simplicity is usually a virtue. But not always. It is only one of many evaluation criteria, and often there is a tension between conflicting criteria. For example, within a famous statement of simplicity known as Occam's Razor — "entities should not be multiplied, except from necessity" — there is tension between a desire for simplification ("entities should not be multiplied") and the modifying phrase ("except from necessity"). One type of necessity is motivated by a consideration of empirical adequacy. Although there may be some value in 'ontological economy' — the principle that in a scientific theory the number of ontological entities should be minimized — there is also a good reason to include additional entities in a theory if this produces a revised theory that can more adequately explain observations.

When scientific theories are invented, evaluated, and applied, there may be a tension between the simultaneous demands for simplicity and empirical adequacy. Usually, but not always, a theory that is more complete (and thus less simple) can be more empirically adequate. The value of simplicity (in ontology or structure) varies from one situation to another, and in some situations other criteria are more important.

For example, if a bowling ball is dropped from a height of 5 meters, air resistance can be ignored unless one needs extremely accurate predictions. But when a tennis ball is dropped from 50 meters, predictions are significantly inaccurate if air resistance is ignored. And a project to send a rocket to the moon will fail if scientists ignore either air resistance or the variation of gravity with altitude. In comparing these situations, there are two major variables: the weighting of factors (which depends on objectives), and degrees of predictive contrast. Weighting of factors: for the moon rocket the demand for empirical accuracy is more important than the advantages of conceptual simplicity, but for most bowling ball scenarios the opposite is true. Predictive contrast: for the rocket there is a high degree of predictive contrast between alternative theories (one theory with air resistance and gravity variations, the other without) and the complex theory makes predictions that are more precise and accurate, but for the bowling ball there is a low degree of predictive contrast between these theories, so empirical evaluation does not significantly favor either model.

Because utility depends on context, if there is a family of models (such as the family described in Section 2.21_A) there will be a wider range of options to choose from, to meet the needs of each situation.

B. Ad Hocness versus Inventive Revision

A newly added component may be regarded as ad hoc if there is suspicion that its main function is to achieve empirical agreement with known data. But adding components to an old theory, using retroductive logic to achieve empirical adequacy, is often an effective technique for *invention by revision.* This invention strategy may or may not be effective, depending on the situation and whether the strategy is used skillfully. Therefore, accusations of "ad hoc" should not be made too quickly or without careful analysis:

> It may not be easy to find good methods for distinguishing between illegitimate ad hoc additions to the theory and good, newly added theoretical components. It may take some time to explore the consequences of adding a new theoretical component, to see if it aids in making new predictions, and to see if it can be systematically connected to other theoretical components. Thus, judgment about ad hocness may require hindsight analysis. (Darden, 1991; p. 264)

2.23: Constraints on Theory-Components

One criterion for evaluating logical structure involves decisions about components, about the types of entities (for composition) and actions and interactions (for operation) that should be included in a theory's models, and that should not be included. For example, prior to 1609 when Kepler introduced elliptical planetary orbits, it was widely believed that in astronomical theories all motions should in circles with constant speed. This belief played a role in motivating Copernicus:

> In both *De Revolutionibus* and the *Commentariolus* Copernicus attacks the Ptolemaic astronomy not because in it the sun moves rather than the earth, but because Ptolemy has not strictly adhered to the precept that all celestial motions must be explained only by uniform circular motions or combinations of such circular motions. ... Ptolemy had recognized that an accurate representation of planetary motion necessitated the abandoning of uniform circular motion, and he boldly introduced what was later called an 'equant', [a point in space] from which nonuniform motion along an arc would appear uniform. ... Copernicus considered the use of an equant to be a violation of fundamental principles,...[and] apparently believed that one of his major achievements in astronomy was to restore the principle of uniform circular motion. ... It has been generally believed that Copernicus's insistence on uniform circular motion is part of a philosophical or metaphysical dogma going back to Plato. (Cohen, 1985; pp 112-113)

In every field there are constraints, both implicit and explicit, on the types of components that should (and should not) be included in theories. Justification for these constraints can be based on appeals to *ontology* (by asking "Does it exist?") or *utility* (by asking "Will it be useful for doing science?"). For example, an insistence on uniform circular motion could be based on the ontological belief that celestial bodies never move in noncircular motion, or on the utilitarian rationale that using noncircular motions makes it more difficult to do calculations. And when behaviorist psychologists refused to accept unobservable ‘thinking’ as a component in their theories, this was due mainly to utility (because they believed that if theories included thinking, this would not inspire the gathering of reliable experimental data, and it would encourage theoretical speculations that could not be empirically tested) rather than ontology (because they believed that thinking does not occur).

2.24: Description and Explanation

In ISM there are two types of theories, descriptive and explanatory. Some differences between description and explanation are discussed in this section.

A. Is there an explanation for gravity?

Often, science moves from description to explanation. This can be seen in the history of theories to “describe and explain” the movement of planets. Following the publication of the Copernican model in 1543, in the late 1500s Tycho Brahe used innovative techniques and instrumentation to collect precise data describing the positions and motions of planets; his measurements were far superior to data collected earlier. In the early 1600s Johannes Kepler — impressed by the precision of Brahe's data, and confident of its accuracy — struggled for years to retroductively invent a descriptive theory that could accurately predict this empirical data. In the late 1600s Isaac Newton constructed an explanatory theory that, among its many accomplishments, mathematically derived Kepler's theory by using only a few general postulates about force and motion.

This brief history illustrates three levels of description-and-explanation. Brahe's data was intended to be a literal description (no more, no less) of planetary positions at various times. Kepler's theory was a mathematical representation of the planetary positions as a function of time; his theory accurately described the ‘what’ of planetary motion, but not the ‘why’. Newton, by showing how Kepler's mathematical descriptions fit naturally into a general framework of force and motion, formulated a scientific explanation for planetary motion. But this explanation assumes the existence of gravitational force with certain properties, and Newton could not explain gravity.

Most scientists, including Newton himself, have considered Newton's theory of gravity to be incomplete. His theory-based model for gravitational force contains compositional entities (bodies with mass) and causal interactions (each body exerts an attractive force on the other) but it does not specify a satisfactory mechanism for the interactions that cause the force, so it is incomplete. More than a century later, there was an attempt to resolve the conceptually troubling action-at-a-distance implication of Newton's theory by postulating an intermediary 'field', by analogy to the field concept that was introduced by Faraday for electricity and magnetism. In 1915, more than two centuries after Newton, Einstein's theory of General Relativity (based on an equivalence between the observable effects produced by gravity and acceleration) described gravitational force in terms of a 'curvature of space' that is produced by mass. More recently, dating from the late 1940s, relativistic quantum field theories (based on mass-energy interconvertibility and wave-particle duality) postulate that each basic type of force — gravitational, electromagnetic, weak, and strong — is caused by the exchange of 'virtual quanta' between interacting particles. In this theory, electrical force is due to an exchange of photons, while gravitational force is caused by an exchange of gravitons. But gravitons have never been observed, so their existence is purely speculative, based on analogy with other quanta (such as photons) that, according to 'theories of observation' used by modern scientists, have been observed.

So do gravitons provide an explanation for gravity? And is the logical structure of quantum field theory, which includes a mechanism for force, an improvement over the structure of Newton's model, which is incomplete because it lacks a comparable mechanism? This type of question is a topic of lively debate, especially among philosophers, and most books on philosophy of science contain a detailed discussion about different views on what constitutes a valid 'scientific explanation'. Those who think that science should try to construct models that provide a complete explanation, in an effort to gain a deeper understanding of nature, will criticize Newtonian theory as incomplete due to its inadequate explanations for the interactions that cause gravitational force.

B. Empiricism

An 'empiricist' believes that scientific theories should not postulate the existence of unobservable entities or interactions. A strict empiricist will applaud Newton's "$F_{gravity} = GMm/r^2$" as an empirical generalization that is reliable and approximately accurate, as a descriptive theory that does not postulate unobservable entities such as fields, curved space, or gravitons.

It is important to distinguish between two similar terms: 'empiricist' is not the same as 'empirical'. Empirical data and empirical evaluations are used in all science, whether or not it is based on empiricist philosophy. A harsh critique of empiricism is offered in Appendix A22, to supplement the comments in the following paragraph.

Empiricism is considered a legitimate perspective in philosophy, but it is rare among scientists, who welcome a wide variety of ways to describe and explain, ranging from literal descriptions (such as Brahe's data) to empirical generalizations (Kepler's laws) to incomplete models (Newton's theory) to more-complete models (quantum field theory) or a radical reformulation of basic concepts (general relativity). When there is a choice between restrictions and freedom, scientists usually choose freedom. Contrary to the restrictions of empiricists, scientists practice science the way they feel is most effective, and most modern theories do include unobservable entities (such as photons and electrons) and interactions (such as electrical fields and forces) among their essential components. Although scientists welcome a theory that is limited to a description of empirical patterns, usually this type of theory is seen as a temporary stage along the path to a more complete theory that may or may not include 'unobservables' as components. This feeling, that "we're not there yet" when there is only a descriptive theory for an empirical pattern, contrasts with the empiricist view that this should be the logical ending point for science.

C. Theories (descriptive and explanatory) in ISM

In ISM there are two types of theories (and models): a *descriptive theory* describes an empirical pattern and the domain of systems for which it is claimed to be valid, while an *explanatory theory* describes a pattern and also explains it by describing composition-and-operation models for systems in the claimed domain. The ISM framework is compatible with any type of scientific theory — whether it is descriptive, explanatory, or has some characteristics of each — because, as discussed above, many types of theory play an important role in science.

2.25: Cognitive Utility

When comparing theories, one can focus on either plausibility or utility, by asking "Which theory is a more accurate representation of nature?" or "Which theory is more useful?" This section, and the next, will focus on the second question by examining two aspects of scientific utility: cognitive utility and research utility.

A. Theory Structure and Cognitive Structure

Differences in theory structure can produce differences in cognitive structuring and problem-solving utility, and will affect the harmony between a theory and the thinking styles — due to heredity, personal experience, and cultural influence — of a scientist or a scientific community. If two theories differ in logical structure, evaluation will be influenced by scientists' affinity for the structure that more closely matches their preferred styles of thinking.

Even for the same theory, representations can differ. For example, in a theory of astronomy a phenomenon might be symbolically represented by words (the earth orbits the sun in an approximately elliptical orbit), with simplified concepts (e.g., with the earth and sun as point masses, and the ellipse as a circle), by a visual representation (a diagram depicting the objects and their orbits), or by an equation ($T^2 = Mm/R^2$) where objects and actions are symbolized by mathematical terms.

More generally, the theory of Newtonian classical mechanics can be described with simple algebra (as in most introductory courses), by using calculus, or with a variety of advanced mathematical techniques such as 'Hamiltonians' or tensor analysis; and each type of mathematical formulation can be supplemented by a variety of visual and verbal explanations, and illustrative examples. Similarly, quantum mechanics theory can be formulated in two logically equivalent ways: as particle mechanics by using matrix algebra (this was done in 1925 by Werner Heisenberg and collaborators, especially Max Born), and as wave mechanics by using wave equations (this was done in 1926 by Erwin Schrodinger).

But even if two formulations of a theory are logically equivalent, differing representations — whether this is done by changing the representation mode (such as verbal descriptions, visual diagrams, mathematical equations, or math/visual graphs) or by changing the expression within a particular mode — will change the way the theory is perceived and used. Different representations can produce different mental models and different approaches to solving problems. Often, the cognitive utility of a model depends on the problem-solving context. For example, an algebraic version of classical mechanics may be the easiest way to solve a simple problem, while a formulation using Hamiltonians will be more effective for solving a complex astronomy problem involving the mutually influenced motions of three celestial bodies.

Two formulations of the same theory will always differ in their *ease of translation* into mental models — i.e., in how easily they are learned. And even if both formulations initially have the same logical content, after translation the mental models thus produced will differ in logical content. The mental models will differ in *accuracy* — i.e., in how closely the models correspond to the natural system they are intended to represent. There also will be a difference in cognitive organization[10] and in *cognitive utility* between the two mental models, and thus between the two theory formulations from which they were formed.

If there is a similarity between the structuring of scientific theories (studied by philosophers) and mental models (studied by psychologists), as claimed by those who advocate a synthesis in the areas where philosophy of science and cognitive psychology overlap, and if there is a link between mental models and thinking skills, then the overall connection is "scientific theory → scientific model → mental model → thinking skill (and cognitive utility)."

B. Personal Thinking Styles and Communal Thought Styles

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[10]. A personal example illustrates the importance of integrative organization in the representation of ideas. One of my favorite chemistry textbooks (Davies, 1972) is a small paperback, written at college freshman level, that does a masterful job of showing relationships among a wide range of theoretical perspectives: the book begins with simple quantum mechanics and the resulting distributions of energy levels, then relates these ideas to the concepts of enthalpy, entropy, and free energy, and these to dynamic equilibrium and equilibrium constants. When I read this book I was a college graduate with a degree in chemistry, who had read (and mastered at a fairly high level) many chemistry books, including some intended to present the same subjects at a "higher" level. But, compared with Davies, none of the other textbook authors helped me to produce such a highly integrated mental model for a wide range of essential chemistry concepts. When concepts that I already knew were organized in a different way, the result was increased understanding and a higher level of cognitive utility.

The preceding subsection focused on the individual. But communities also make judgments about scientific utility. Thought styles, which are influenced by the cultural context in which science operates, affect every aspect of science, including preferences for certain types of theory structures. These preferences, which affect (and are affected by) the personal thinking styles of individual scientists, include the selection of criteria that are used to evaluate cognitive utility, research utility, and other conceptual qualitites.

A particular type of theory formulation begins as an internal representation in the mind of an individual, is eventually translated into an external representation (such as an explanation in a textbook) that belongs to the community as a whole, and is then translated back into an internal representation for another individual. During the first translation there is ample opportunity for the logical and aesthetic tastes of a community to affect the style and form of the external representation, as it is manifested in features such as the blending of modes (verbal, visual, mathematical,...), the degree of simplification, and the balance between abstractions and concrete illustrations or analogies. During the second translation an individual's thinking style, which is influenced by community thought styles, will affect the type of mental model that is formed.

For example, if the dominant personalities and authorities in a scientific community strongly preferred theories with only a mathematical representation, and if members of the community could not (or would not) invent and use verbal labels for mathematical terms, then "$Fd = mv_f^2/2 - mv_i^2/2$" might never have become the "work-energy theorem", which occurred when the math terms 'Fd' and '$mv^2/2$' were defined as the verbal terms 'work' and 'kinetic energy'. When both types of terms are available, and there is the option of thinking and communicating by using either a mathematical or verbal expression, or both, the cognitive possibilities are greater than if either form of expression had never been invented.

Thought styles, especially the cultural-personal factors of psychological motives and practical concerns, exert a powerful influence not just on cognitive utility, but also on research utility, which is the topic of the following section.

2.26: Research Utility

Sections 2.25 and 2.26 describe two aspects of *scientific utility,* usefulness for cognition and for research, that are related. In particular, cognitive utility plays an important role in making a theory useful for doing research.

A. Acceptance and Pursuit

Even when a theory has weaknesses and, based on evaluations of plausibility and/or usefulness, it is not yet worthy of *acceptance* (of being treated as if it were true), scientists can rationally view this theory as worthy of *pursuit* (as worthy of exploration and development by further research) if it shows promise for stimulating ideas to do new experimental or theoretical research:

> [Sometimes] scientists have investigated and pursued theories or research traditions which were patently less acceptable, less worthy of belief, than their rivals. Indeed, *the emergence of virtually every new research tradition occurs under just such circumstances. ... A scientist can often be working alternately in two different, and even mutually inconsistent research traditions.* (Laudan, 1977, p. 110, emphasis in original)

Making a distinction between acceptance and pursuit (Laudan, 1977) is useful when thinking about scientific utility. Laudan suggests that when scientists judge whether a theory is worthy of pursuit, instead of just looking at the 'momentary adequacy', they study the 'rate of progress'. If a theory has low status but is improving quickly, or even if it merely shows signs of potential improvement, its development may be pursued. Such a theory would, in the language of ISM, have a low status for acceptance, but a high status for pursuit.

B. Evaluation Criteria for Immature Theories

Darden (1991) suggests that it may be scientifically useful to evaluate mature and immature theories differently, and describes some ways in which the criteria for acceptance and pursuit differ. In a mature theory, scientists typically want a theory with components that are clearly defined and logically consistent. But in an immature theory that is being developed, there are advantages to temporarily relaxing expectations for clarity and consistency:

> Discovery is not necessarily an instantaneous process, producing a complete new theory in a blinding flash of insight. Instead, theories may be constructed incrementally, often beginning with vague ideas that are successively refined and modified. ... A decision to use the strategy of beginning with a vague idea, thus, would dictate that the criterion of *clarity* should not be imposed too early in the process of theory development. ... During theory construction, the

> requirement of internal consistency may not be constantly at the forefront of attention. ... Working out the logical relations between components may require some period of time. And it may even be useful to consider generating hypotheses inconsistent with some other component; maybe the other component is the problematic one. (Darden, 1991; pp. 256, 260, 258)

An immature theory that is worthy of pursuit needs development, but it shows enough promise to be considered worth the effort. In order to facilitate a search for promising theories, Darden suggests that strategies for the pursuit of theory development should include, in addition to a temporary tolerance for structural or logical weakness,

> [the adoption of] forward-looking strategies for assessing the promise of a theory for future work. ... [For example], if a theory is *easily extendable,* then it can easily accommodate changes and extensions to explain new domain items. ... [And] *fruitfulness* is a measure of the theory's fertility in suggesting new experiments or ideas for its further development. (Darden, 1991, p. 264)

C. Ideas for Experimental Design

A new theory can be fruitful in promoting research by offering a new theoretical perspective on the composition-and-operation of experimental systems, and also by inspiring ideas for new experimental systems or techniques. The stimulation of ideas for experimental design, by taking advantage of a change in theory or technology, is discussed in Section 2.62.

Of course, fruitfulness is not limited to the pursuit phase. Even after a theory is generally accepted by scientists there may be plenty of opportunities for experimenting (to explore the theory's application for new systems) and theorizing (to develop the theory's logical structure). But sometimes the opportunities for exciting research are more plentiful with a new theory.

Usually, to inspire experimentation a theory must make predictions about observable outcomes. If theory components are unobservable they cannot be experimentally tested by direct observation, but they can be indirectly tested if they make predictions about observable properties of an experimental system. These predictions fulfill the practical requirement of hypothetico-deductive logic, the need for predictions to compare with observations, that is the motivation for the evaluation criterion of *testability.* Testability is essential if a theory is to have scientific utility, and it is essential for scientific evaluations of a theory's plausibility, but testability is not necessarily related to whether or not a theory really is true.

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D. How a 'False Model' can be Useful

As discussed earlier, even if a theory is not suitable for acceptance, it may be useful for pursuit. Wimsatt (1987) discusses many of the ways that a "false model" can be scientifically useful. Even if a model is wrong, it may inspire the design of interesting experiments. Or it may stimulate new ways of thinking that lead to the careful examination and revision of another theory; if this other theory initially had high status, it might have been too easily accepted "as it was" if there had been no stimulation of critical thinking due to the false model. A false model may stimulate a search for empirical patterns in data. Or it may serve as a starting point; by continually refining and revising a false model, perhaps a better model can be developed. Or,

> Two false models may be used to define the extremes of a continuum of cases in which the real case is presumed to lie, but for which the more realistic intermediate models are too complex to analyze or the information available is too incomplete to guide their construction or to determine a choice between them. In defining these extremes, the 'limiting' models specify a property of which the real case is supposed to have an intermediate value. (Wimsatt, 1987, p. 9)

E. Useful Functions of Simplification

Many of Wimsatt's explanations about utility involve a model that is false due to an incomplete description of components for entities, actions, or interactions. In this situation the erroneous predictions of a model can be analyzed to gather clues about the effects of simplification. A simplified model can serve as a template, as a 'control' for determining the effects of components that have been omitted or oversimplified.

For example, consider several models that vary in their characterization of the 'damping force' for a pendulum. In order to learn more about this force, scientists can design a series of experimental systems, and for each system they can compare each model's predictions with the observations and with predictions made by other models. This approach may provide valuable information about the characteristics of the damping force and its effects on the pendulum's motion. A similar example is the Castle-Hardy-Weinberg Model for population genetics, which assumes a type of *idealized* system that never occurs in nature. But,

> For many purposes, the Castle-Hardy-Weinberg predictions are the most useful when they don't come true. ... These departures themselves are interesting and can sometimes tell us such

> things as how much mutation, natural selection, or plain random change may be going on. These, of course, are the primary forces of evolution. The only way evolution can proceed is through such events; thus a population *out of equilibrium* is immediately interesting. (Wallace, King & Sanders, 1986, pp. 368-369)

In this case, deviations from the model's predictions indicate possibilities for evolutionary change in the population.

Simplification can occur due to ignorance or intention. Sometimes, especially early in the process of theory development, scientists just don't know all of the relevant entities, actions, and interactions. Often, however, scientists make a conscious choice to intentionally simplify a model in order to more effectively achieve their goals. Although scientists recognize that simplification reduces a model's completeness, whatever loss occurs (and it may not be much) must be balanced against the benefits.

Often, an important benefit of simplification is that it allows the mathematization of a theory. For example, Galileo's mathematical treatment of physics was possible only because he did not try to satisfy a previously assumed constraint — that there should be exact agreement between observations and a theory's predictions. This approach to theorizing caused a deep conflict, due to a disagreement about fundamental conceptual factors, between Galileo's science, with its hypothetical idealized systems, and the real-world systems of empiricist Aristotelian science. Described in terms of problem solving, Galileo and the Aristotelians disagreed about whether exact empirical agreement was a necessary constraint for defining an adequate solution. Because Galileo's theories did not satisfy the constraints they had defined, some scientists claimed that his theories were not an adequate scientific explanation:

> Galileo acknowledged that events do not always correspond to his theory. ... [Aristotelian scientists such as] Del Monte and others repeatedly pointed out that actual pendulums do not behave as Galileo maintained. Galileo never tired of saying that *ideal* pendulums would obey the mathematically derived rules. Del Monte retorted that physics was to be about this world, not an imaginary mathematical world. Opposition to the mathematizing of physics was a deeply held Aristotelian, and more generally empiricist, conviction. (Matthews, 1994, pp. 116-117)

In a footnote Matthews explains that "this claim needs to be nuanced" because some Aristotelian science, such as astronomy, did use mathematics, but there were important differences between Galileo and the traditionalists regarding the relationships between mathematical representations and physical phenomena, and the scientific legitimacy of idealized models. Newton continued the Galilean strategy of representing complex physical objects with idealized concepts and mathematical variables. For example, an actual Sun and Earth become idealized point masses, represented by M and m in the equation for gravitational force, 'GMm/R^2'. Similarly, a real action — the approximately elliptical orbit — becomes an idealized circle represented by 'R' in an equation for orbiting time, $T^2 = 4\pi^2 R^3/GM$.

Because scientists do want their theories to describe the real world, oversimplified Newtonian models can be modified by adding "correction factors" to increase the models' accuracy and (for some purposes) their practical utility:

> [Idealization] was the precondition for Newton's masterful mathematical analysis of planetary motion. In the light of refined measurements and observations, the initial assumptions can be altered and the picture of planetary motion made more realistic or concrete. Eventually, the idealizations allow a person to step out of a rocket and walk on the moon's surface. (Matthews, 1994, p. 212)

An increase in cognitive utility, by making a model easier to learn and use, is a common goal of simplification. In building models that will be used by humans with limited cognitive capacities, there is a tension between the conflicting requirements of completeness and simplicity. It is easier for our minds to cope with a model that is simpler than a complex reality. But for models in which predicting or data processing will be done by computers, there is a change in capacities for memory storage and computing speed, so the level and nature of 'useful complexity' will change. High-speed computers can allow the use of models — for numerical analysis of data, or for doing thought-experiment simulations (of weather, ecology, business,...) — that would be too complicated and difficult if computations had to be done by a person.

If the goal is effective communication for the purpose of education or persuasion, sometimes simpler is better. And if the goal is improved understanding of a system, a simpler model may serve a valuable function by focusing attention on those features of a system that are considered to be especially important.

Sometimes additional components do not make a model more complete or accurate because, although a model can be over-simplified by omitting relevant factors that should be included, it also can be over-complicated by including spurious factors that should be omitted. Due to the latter possibility, sometimes the simplification of a complex model will produce a model that is better at making accurate predictions for new experimental systems, as explained by Forster & Sober (1994).

2.27: External Consistency

Sections 2.21-2.26 discussed the internal characteristics of a theory. In Sections 2.27 and 2.28 the focus turns to external relationships between theories.

A. Overlapping Domains and Shared Components

The **external relationships** between scientific theories can be defined along two dimensions: the overlap between domains, and the sharing of theory components. If two theories never make claims about the same real-world systems, their domains do not overlap; if, in addition, the two theories do not share any components for their models, then these theories are *independent.* But if there is an overlapping of domains or a sharing of components, or both, there will be external relationships.

If the domains of two theories overlap, with both theories trying to describe the same systems and explain the same observations, these are 'alternative theories' in competition with each other. The relationships between these theories can be viewed empirically (by comparing their predictions) or conceptually (by comparing their components). When the domains of two theories do not overlap, there is no direct competition and no empirical comparison, but there will be a conceptual relationship if these theories contain similar types of components.

The next three subsections examine external consistency in two types of situations: when there is an overlap between domains, or a sharing of components.

B. A Shared Domain, with Competitive Theories

If two theories with overlapping domains construct different models for the same real-world experimental system, these are *alternative theories* in competition with each other, whether or not they differ in empirical predictions about the system. In this competition, the intensity of conflict increases if there is a large overlap of domains, or a large difference in components for models, or a high degree of contrast in predictions.

One example of intense conflict was the competition, beginning in 1961, to explain the phenomenon of oxidative phosphorylation in mitochondria. In 1960 a 'chemical intermediate' explanation was widely accepted by scientists. Even though a chemical intermediate had never been found, its eventual discovery was confidently assumed, and this theory "was never thought to be a hypothesis; it was considered an established fact of science. (Wallace, et al, 1986; p 140)" But in 1961 Peter Mitchell proposed an alternative theory based on a principle of 'chemiosmosis'. Later, a third competitor, 'energy transduction', entered the battle, and for more than a decade these competitive theories — and their loyal defenders — were involved in heated controversy. Eventually, chemiosmotic theory was declared the winner, and in 1978 Mitchell was awarded the Nobel Prize in chemistry.

However, the resolution of conflict does not always require the declaration of a clear winner. For example,

> In the late nineteenth century, natural selection and isolation were viewed as rival explanations for the origin of new species; the evolutionary synthesis showed that the two processes were compatible and could be combined to explain the splitting of one gene pool into two. (Darden, 1991, p. 269)

Of course, a declaration that "both factors are important, and both contribute to causing speciation" is not the end of questions or inquiry. Scientists can still analyze a particular evolutionary episode in an effort to determine the details of the roles played by each factor. And they can debate the relative importance of each factor in long-term evolutionary scenarios involving many species. And there can be an effort to develop theories that more effectively combine both of these factors and their actions and interactions.

A different type of coexistence occurs with two theories that are simplified applications of quantum mechanics. Valence Bond theory and Molecular Orbital theory each use a different type of approximation in order to more easily apply the core principles of quantum mechanics for the construction of models to describe the characteristics of molecules. Each approach has advantages, and the choice of a preferred theory depends on the situation — the type of molecule being studied, the objectives, and the abilities, experience, and thinking styles of the scientists. Or perhaps both theories can be used. In many ways they are complementary descriptions, as in “The Blind Men and the Elephant,” with each theory providing its own valuable perspective. This type of coexistence (where two methods for theory application use the same theory to construct models that are simplified in different ways) contrasts with the coexistence in speciation (where selection and isolation are potential co-contributors in causing a phenomenon) and with the non-coexistence in oxidative phosphorylation (where one theory has vanquished its former competitors).

C. A Shared Component (with inconsistency) in Different Domains

The preceding subsection describes direct competition, with different models being constructed for the same system. In this subsection the same “type of component” occurs in models that are constructed for different systems.

Even if two theories do not claim the same domain, there is conflict if both theories contain the same type of component but disagree about the characteristics of this component. For example, in the late 1800s theories in three fields — geology, biology, and thermodynamics — contained the same type of component, for the age of the earth. In an effort to describe geological observations, advocates of slow-acting ‘uniformitarian’ geological processes (Bowler, 1989, pp. 134-141) constructed theories that required, as an essential component, an earth that is very old. Similarly, the slow-acting processes postulated by Darwinian evolution required biological theories with an old earth as an essential component (Bowler, pp. 199-208). But the theory of thermodynamics, applied to geology by Lord William Kelvin in 1868, required a relatively young earth in order to construct a plausible model that adequately explained the available observations:

> All geologists [at the time of Kelvin's theorizing] accepted the fact that volcanic activity resulted from high temperatures in the earth's interior. But...if the earth is hot now, it must have been hotter in the past and have gradually cooled down to its present state. If we trace the cooling far enough back in time, we must come to a point where the whole planet was a mass of molten rock, as one would expect it to have begun according to the nebular hypothesis. In 1868, Kelvin showed by detailed calculations that the...age of the earth could not be more than about a hundred million years, far less than the time required for Darwinian evolution [or uniformitarian geology]. (Bowler, p. 206)

Describing the difficulties caused by these inter-theory conflicts, Laudan (1977, p. 57) says that "it was generally perceived that these conceptual problems, until resolved, raised strong doubts about the problem-solving efficacy of a wide range of scientific theories." When two or more theories are in conflict, there is a conceptual difficulty for all of the theories, but especially for those in which scientists have less confidence. In the conflict of thermodynamics versus geology-and-evolution, the authority of Kelvin and physics prevailed. Thermodynamics was affected very little, but:

> So great was Kelvin's prestige and the apparent strength of his argument that most geologists began to rework their theories to incorporate much faster rates of change than those postulated by Lyell. The retreat of the geologists [influenced evolutionary biologists, and]...by the end of the century, a number of alternative evolutionary mechanisms had been suggested, at least in part to show how the process could have advanced more quickly than by natural selection. Kelvin's arguments did not impede the growth of evolutionism, but they certainly contributed to the declining popularity of the selection mechanism in the late nineteenth century. (Bowler, pp. 206-207)

Judged from the viewpoint of 1997, none of the 1868 theories were actually in serious conflict with each other. Instead, the conflict was caused by a faulty characterization of an experimental system, due to the absence of essential information. In 1903 the discovery of radioactive decay — which provides a large source of energy to counteract the earth's cooling — added another component to the characterization of the 'experimental system' used for thermodynamic calculations of the earth's age. Using a newly revised system-theory and the unchanged theory of thermodynamics, the new calculation predicted that the earth is much older, consistent with the 1868 theories of geology and biology. With this new conclusion, scientists studying geology and evolution were released from the constraints that had been imposed on them by theories (and scientists) from outside their own fields.

D. A Shared Component (with consistency) in Different Domains

As described above, in the late 1800s a disagreement between theory components (an old earth for geology and biology, but a young earth for physics) was a cause for concern in all three fields. Conversely, agreement about shared components can lend support to theories or their components. For example, a wide variety of currently accepted theories postulate, as one of their components, time intervals of long duration. During these long periods of time, physical processes occur, and these processes are necessary for empirical adequacy in explaining observations; if the time-component is changed from a long time to a shorter time (such as the 10,000 years suggested by 'young earth' creationists) the result will be models that make erroneous predictions. Theories containing an old-earth component span a wide range, with domains that include ancient fossil reefs, sedimentary rock formations (with vertical changes), seafloor spreading (with horizontal changes) and continental drift, magnetic reversals, radioactive dating, genetic 'molecular clocks', paleontology, formation and evolution of stars, estimates of astronomical distances, and cosmology.

In a wide variety of theories, the same type of component (for amount of time) always has the same general value — the amount of time is very long. This provides support for the shared component — an old earth (or an old universe) — and this support increases because an old earth is an essential component of many theories that in other ways, such as the domains they claim and the other components they use, are relatively independent. This independence makes it less likely — compared with a situation where two theories are closely related and share many essential components, or where the plausibility of each theory depends on the plausibility of the other theory — that suspicions of circular reasoning are justified. Of course, the relationships that do exist between the 'old earth' theories can be considered when evaluating the amount of circularity in the support claimed for the shared component.

E. Component or Conclusion?

Is the postulated age of the earth, which Kelvin inferred by solving a thermodynamics equation, a conclusion or a component? Because it is part of a model for "the way the world is," the earth's age is a component. But it is also the conclusion that occurs when an equation is solved. Kelvin's equation for the earth's cooling, stripped of details in a simplified version, is "$T_f - T_i = (\Delta T/\Delta t)\ \Delta t$", with four variables: the final and initial temperatures, rate of temperature change, and time. In a strict hypothetico-deductive scheme, a theory is combined with initial system-conditions, to deductively predict the final system-conditions. But when using Kelvin's equation it is difficult to define the distinctions between observation and theory, because each input (for final temperature, initial temperature, and rate of temperature change) is constructed by combining observations with theoretical interpretations. And the conclusion, in solving for Δt, is a retroductive inference about one component of a model for the earth, rather than a prediction about what should be observed.

So is it a component and also a conclusion? Yes, it can be either, depending on what is assumed and what is being inferred. Similar analyses could be formulated to show how the earth's age can be viewed as either a component or conclusion in the 1868 theories of geology and biology.

F. Conceptual or Empirical?

Another question involves the relationship between external consistency and empirical adequacy. In the 1500s the astronomical theory of Copernicus had to explain two major phenomena: the movement of celestial objects with respect to each other, and a rotation of the entire sky (including the sun, moon, planets, and stars) every 24 hours. To explain the first and second phenomena, respectively, Copernicus postulated sun-centered orbits and a rotating earth. Each of these postulates conflicted with the currently dominant Aristotelian theories of celestial and terrestrial physics, thus producing a major conceptual problem. But there is also an empirical problem. In the early 1600s, many decades after the Copernican theory was introduced, there still was no ‘Copernican physics’, so a Copernican prediction had to be made by combining Copernican astronomy with Aristotelian physics. For the Copernican postulate of a rotating earth, this combination predicted that objects should fly away from the surface of a rapidly rotating earth, and that airborne objects (such as clouds, birds, and projectiles) should appear to move with respect to the earth that is moving beneath them. But none of these phenomena were observed, so there was empirical anomaly.

Why does the difficulty appear to be both conceptual or empirical? Because, like most theories, Aristotelian Physics (AP) was constructed so its predictions would usually agree with the known data. Therefore, if a new theory conflicted with AP (to cause a conceptual problem) there would also be a conflict with data (causing an empirical problem) unless the new theory and AP were empirically equivalent, with no predictive contrast.

The pre-Copernican combination of “Ptolemaic Astronomy + AP” made predictions that generally were correct. When only one part of this combination was changed, “Copernican

Astronomy + AP" made incorrect predictions. If the AP was assumed to be correct, then the new astronomy must be causing the incorrect predictions. Eventually, however, the conclusion was that the faulty part of this new combination was not the astronomy, but the physics. In the 1630s, Galileo developed an alternative theory of physics (including explanations of relative motion) that, when combined with a rotating earth, made predictions that matched observations, and the major empirical difficulty was solved. The conceptual conflict with AP remained, but this became less of a problem as the status of Aristotelian Physics faded during the 1600s.

2.28: External Connections

In each example of Section 2.27 there was a connection between theories due to a domain overlap or a shared component. Section 2.28 will examine different types of connections between theories, and the process of trying to create connections between theories.

A. Levels of Organization

Related theories with shared components can differ in their level of organization, and in the function of a shared component within each theory. For example, biological phenomena are studied at a number of different levels — molecules, cells, tissues, organs, organisms, populations, ecological systems — and each level shares components with other levels. Cells, which at one level are models constructed from smaller molecular components, can function as components in models for the larger tissues, organs, or organisms that serve as the focus for other levels. Or, in a theory of structural biochemistry an enzyme might be a model (with attention focused on the enzyme's structural composition) that is built from atomic components and their bonding interactions, while in a theory of physiological biochemistry this enzyme (but now with the focus on its operations, on its chemical actions and interactions) would be a component used to build a model.

B. Theories with Wide Scope

Another type of relationship occurs when one theory is a subset of another theory, as with DNA structure and atomic theory. During the development of a theory for DNA structure, scientists assumed the constraint that DNA must conform to the known characteristics of the atoms (C, H, O, N, P,...) and molecules (deoxyribose, cytosine,...) from which it is constructed. When Watson and Crick experimented with different types of physical scale models, they tried to be creative, yet they worked within the constraints defined by the components of atomic theory (such as atom sizes, bond lengths, bond angles, and characteristics of hydrogen bonding) that were relevant for their models. And when describing their theory in a 900-word paper (Watson and Crick, 1953) they assumed atomic theory as a foundation that did not need to be explained or defended; they merely described how atomic theory could be applied, in this specific situation, to explain the structure of DNA.

There is nothing wrong with a narrow-scope theory about DNA structure, but many scientists want science to eventually construct 'simple and unified' theories with wide scope, such as atomic theory. Newton was applauded for showing that the same laws of motion (and the same gravitational force) operate in a wide domain that includes apparently unrelated phenomena such as an apple falling from a tree and the moon orbiting our earth, thus unifying the fields of terrestrial and celestial mechanics. And compared with a conjunction of two independent theories, one for electromagnetic forces and another for weak forces, a unified electro-weak theory is considered more elegant and impressive due to its wide scope and simplifying unity.

C. External Relationships viewed as Internal Relationships

Viewing external relationships as internal relationships within a not-yet-complete 'grand unified theory' may provide a useful perspective when considering the larger questions of how theories relate to each other and interact to form the structure of a scientific discipline, and how disciplines interact to form the structure of science as a whole.

By analogy with a theory composed of smaller components, a unified mega-theory is composed of smaller theories. Just as there are internal relationships between components that comprise a theory, by analogy there are internal relationships between theories that comprise a mega-theory. But these "between theory" relationships, which are internal when viewed from the perspective of a

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mega-theory, are external when viewed from the perspective of the theories. In this way it is possible to view ‘external relationships’ as internal relationships.

The major assumption here is that it is difficult to draw distinct boundaries between various levels of theorizing — such as components, sub-theories, theories, and mega-theories. Therefore, in many situations, but not all, the same types of relationships that exist between two lower levels (such as components and sub-theories) will also exist between higher levels (such as sub-theories and theories, or theories and mega-theories). I have found the analogy between internal and external relationships to be useful for thinking about the connections between different levels of theorizing. At a minimum, this perspective has prevented me from becoming too comfortable with the labels ‘internal’ and ‘external’; since these simple labels no longer seem sufficient, there is a tendency for thinking to become less dichotomous, and this often stimulates a more flexible and careful consideration of what is really involved in each relationship.

D. Is a ‘grand unified theory’ a worthwhile goal?

Probably not. In the typical reductionist dream of a grand unified ‘Theory of Everything’, physics will form the ultimate foundation that explains chemistry, which in turn is able to explain biology, and — moving up through the levels of molecules and cells to the whole organism and beyond — psychology can be explained in terms of biological processes, and the sociology of complex communities in terms of individual psychologies. Due to the chains connecting each succeeding level, everything ultimately will be explained in terms of physics. For logical reasons, as discussed by O'Hear (1989), this reductionistic dream is almost certainly impossible.

It is doubtful whether constructing a Theory of Everything is even a worthy goal, especially in terms of scientific utility. At the present time, in most fields, most scientists will perform more useful research if they are not working directly on constructing a mega-theory that will connect all levels of science. But making connections at low and intermediate levels of theorizing can be practical and important.

E. Progressing from Description to Explanation

One step toward unification, which is also practical at the level of everyday science, occurs when an explanatory theory provides principles to explain a previously known empirical pattern. For example, Newton's physics explained the earlier descriptive theory of Kepler, regarding the elliptical orbits of planets. Another descriptive theory, the Ideal Gas Law (with $PV=nRT$) has been explained by deriving it using Newtonian statistical mechanics. And the structure of the Periodic Table, which was originally derived in the late 1800s by using inductive analysis of empirical data for chemical reactivities, with no credible theoretical mechanism to explain it, was later derived from a few fundamental principles of quantum mechanics. Explaining the Periodic Table was not even the original motivation for developing a theory of quantum mechanics, but when the derivation was discovered it was a pleasant surprise that provided support for the newly developed theory. And because quantum mechanics also explained many other phenomena, over a wide range of domains, it has served as a powerful unifying theory.

F. Unification as Consilience with Simplicity

A useful concept for thinking about the scope of a theory is *consilience,* which measures the size of a theory's domain. As originally conceived by William Whewell, consilience is a sophisticated measure of the number of 'classes of facts' explained by a theory, not just the number of 'facts'. Making this distinction often requires a sophisticated knowledge of a domain:

> We are not concerned with the explanation of a horde of trivial facts from the same class. ... What matters is not the sheer number of facts explained, but their variety and relative importance. Assessment of variety presupposes that an inquirer have rich background knowledge about the kinds of things that the facts are about, requiring a detailed conceptual organization. (Thagard, 1988, p. 81)

Usually it is desirable to increase the consilience of a theory, but this is less impressive when it is done by sacrificing simplicity. An extreme example of achieving consilience by using ad hoc components was described in Section 2.21; theory T_1 achieves consilience over a large domain by having an independent theory component for every data point in the domain. But stringing together a loose collection of unrelated components and calling it a theory is not a way to construct a simple consilient theory, and scientists will not be impressed by this pseudo-unification. There is too much "wiggle room," and each extra component will be viewed as a new 'fudge factor' tacked onto a weak theory.

By contrast, an extremely impressive unification was Newton's postulate that the same gravitational force, governed by the same principles, operates in such widely divergent systems as a falling apple and an orbiting moon. Newton's bold step achieved a huge increase in consilience without any decrease in simplicity. This combination of consilience (a wide-scope domain that encompasses many classes of facts) and simplicity (with simplification by using few components, and systematicity by forming logical connections among components) is typical of a useful unification. But simplicity is not the only virtue, and sometimes it is not a virtue at all, so the characteristics of each situation must be considered when evaluating the value of an attempted unification.

G. A Narrowing of Domains

Sometimes, instead of seeking a wider scope, the best strategy is to decrease the size of the domain claimed for a theory. For example, in 1900 when Mendel's theory of genetics was rediscovered, it was assumed that a theory of Mendelian Dominance applied to all traits for all organisms. But further experimentation showed that for some traits the predictions made by this theory were incorrect. Scientists resolved these anomalies, not by revising their theory, but by redefining its scope in order to place the troublesome observations outside the domain of dominance. Their initial wide-scope theory was thus modified into a sub-theory with a narrower scope, and other sub-theories were invented for parts of the original domain not adequately described by dominance. Eventually, these sub-theories were combined to construct an overall mega-theory of genetics that, compared with the initial theory of dominance, had the same wide scope, with greater empirical adequacy but less simplicity.

Section 2.27_A, in discussing relationships between alternate theories, described three types of evaluation conclusions: one theory wins, or both are causal factors, or each provides a useful perspective. In the latter two cases there is peaceful coexistence between alternate theories. A third type of coexistence occurs in the genetics example above, with sub-theories that are in competition (because they describe the same type of phenomena) “splitting up” the domain claimed by a mega-theory that contains both sub-theories as components; each sub-theory has its own sub-domain, consisting of those systems in which the sub-theory is valid, within the larger domain of the mega-theory.

Newtonian Physics is another theory whose inital domain has been narrowed. Early in this century the magnificent theory of Newton, originally claimed to be valid for every system in the universe, was shown to be incorrect for some systems. The limitations were discovered in two phases; first, in 1905 the theory of special relativity showed that the classical mechanics of Newton was not valid for objects moving at high speed; later, in 1925 the theory of quantum mechanics showed that classical mechanics was not valid for objects with small mass, such as electrons. For each of these new theories, classical mechanics could be derived as a special case; within the domain where classical mechanics was approximately valid, its predictions were duplicated by special relativity (for slow objects) and by quantum mechanics (for high-mass objects). But the reverse was not true; classical mechanics made incorrect predictions for fast objects and low-mass objects.

Quantum mechanics is currently considered valid for all systems, but its ‘predictive domain’ is narrowed in an interesting way. For some questions, the theory's answer is that “I refuse to answer the question” or “the answer cannot be known.” But a noncommittal response of “no comment” is better than answers that are confidently clear yet wrong, such as those offered by the earlier Bohr Model. Some of the non-answers offered by quantum mechanics, which imply that there are limits to human knowledge, may be frustrating, but if that is the way nature is, then it is better for scientists to admit this (in their theories) and to say “sorry, we just don't know that, and we probably never will.”

2.3: Cultural-Personal Factors

During all activities of science, including theory evaluation, scientists are influenced by **cultural-personal factors** that operate in the lives of individuals, in the scientific community, and in society as a whole.

2.31: Five Types of Influences

In the ISM diagram, cultural-personal influence is described in terms of five types of influencing factors: psychological motives, practical concerns, metaphysical worldviews, ideological principles, and opinions of authorities.

A. Psychological Motives and Practical Concerns

For most scientists, a powerful motivating force is curiosity about “how things work” and a taste for intellectual stimulation. The joy of science is captured in the following excerpts from letters between two scientists involved in the development of quantum mechanics: Max Planck (who opened the quantum era in 1900) and Erwin Schrodinger (who formulated a quantum mechanics theory in 1926).

> [Planck, in a letter to Schrodinger, says] ‘I am reading your paper in the way a curious child eagerly listens to the solution of a riddle with which he has struggled for a long time, and I rejoice over the beauties that my eye discovers.’ [Schrodinger replies by agreeing that] ‘everything resolves itself with unbelievable simplicity and unbelievable beauty, everything turns out exactly as one would wish, in a perfectly straightforward manner, all by itself and without forcing.’

In addition to a drive to satisfy intellectual curiosity, other typical **psychological motives** and **practical concerns** include desires for self esteem, respect from others, a sense of belonging, financial security, and power. In a scientist's career, these desires may be manifested as efforts to achieve personal and social satisfaction and professional success by forming intellectual alliances with colleagues and by seeking status, power and rewards in the form of publications, grant money, employment, promotions, and honors.

B. Metaphysical Worldviews and Ideological Principles

Metaphysical worldviews form a foundation for conceptual factors such as the criteria, based on beliefs about ontology or scientific utility, for the types of entities and interactions that should be used in theories. Two examples, described earlier, are the preference by early astronomers (such as Copernicus) for using constant-speed circular motions in describing celestial motions, and the preference by behaviorist psychologists for using only observable behaviors in their theories.

Metaphysics can also influence logical structure. For example, Darden (1991) suggests that a metaphysical worldview in which nature is simple and unified may lead to a preference for scientific theories that are simple and unified.

In science, a common metaphysical assumption is empirical consistency, with ‘reproducibility’ as an expectation that the same experimental system will always produces the same observations. However, in science a definition of "the same" is usually subject to statistical interpretations due to both practical and theoretical considerations.

Metaphysical worldviews can be nonreligious, or based on religious principles that are theistic, nontheistic, or atheistic. Everyone has a worldview, which does not cease to exist if it is ignored or denied. For example, to the extent that ‘empiricists’ (also called ‘positivists’) who try to prohibit unobservables in theories are motivated by a futile effort to produce a science without metaphysics, they are motivated by their own metaphysical worldviews.

Another evaluative influence is **ideological principles** that are based on subjective values and on political goals for “the way things should be” in society. These principles span a wide range of concerns, including socioeconomic structures, gender issues, race relations, social philosophies and customs, religions, morality, equality, freedom, and justice. Among scholars who study science there is a wide range of views about the extent to which ideological principles (and other cultural-personal factors) influence the process and content of science. This controversy will be examined in Sections 2.45_B and 4.23.

C. Opinions of Authorities

Evaluation by scientists is also affected by **opinions of** ‘**authorities**’. The quotation marks are a reminder that a perception of authority is in the eye of the beholder. A perception of authority can be due to an acknowledgment of expertise, a response to a dominant personality, and/or involvement in a power relationship. Authority that is based at least partly on power occurs in scientists' relationships with employers, tenure committees, cliques of colleagues, professional organizations, journal editors and referees, publishers, grant reviewers, and politicians who vote on funding for science.

2.32: The Social-Institutional Context of Cultural-Personal Factors

In an ISM model, the five cultural-personal factors (psychological, practical, metaphysical, ideological, and authoritative) supplement and interact with each other. They also develop and operate in a social context, at the levels of *individuals* and *groups,* in a complex environment where each individual is involved in a variety of groups — familial, recreational, professional, political — either formally or informally, consciously or unconsciously, by accident or choice. In this social environment, individuals interact with other individuals; individuals interact, as members or nonmembers, with groups; and groups interact with other groups. All of this occurs in *institutional structures* for science and society, with *profession-related politics* (occurring primarily in the scientific community and operating at many levels) interacting with *societal politics* (that is mainly concerned with broader issues in society). The term 'cultural-personal' implies that both cultural and personal levels (and their interactions) should be included in any attempt to understand and describe the context of science, because individuals work and think in the context of a culture, and this culture is constructed by and composed of individual persons.

There is a close relationship between cognition, social-institutional context, and the development and operation of cultural-personal factors. Three of the five cultural-personal factors — psychological motivations, practical concerns, and authority — are significantly influenced by the social and institutional interactions that constitute the reward system of a scientific community. In fact, in many ways the context can be considered a causal mechanism that is partially responsible for producing the factors. For example, a desire for respect is intrinsic in humans, existing independently of a particular institutional structure, but the situational contexts that stimulate this desire — and the responses (the strategies and actions used to gain respect) that are motivated by these situations — do depend on the social-institutional structure. One important aspect of this structure is its effects on the ways in which authority is created and manifested, especially when power relationships are involved.

Metaphysical worldviews and ideological principles also develop in a social context, and are influenced by the culture of a society and scientific community. And all of these influences are mutual. Individuals (and their motivations, concerns, worldviews, and principles) are affected by their cultural context, and individuals can play an important role in forming the structures and operations of their institutions.

2.33: Mutual Interactions between Science and Culture

What are the results of mutual interactions between science and society? How does science affect culture, and how does culture affect science? The most obvious effect of science has been its medical and technological applications, with the accompanying effects on health care, lifestyles, and social structures. But science also influences culture, in many modern societies, by playing a major role in shaping cultural worldviews, concepts, and thinking patterns. Sometimes this occurs by the gradual, unorchestrated diffusion of ideas from science into the culture. At other times, however, there is a conscious effort, by scientists or nonscientists, to use 'the authority of science' for rhetorical purposes, to claim that scientific theories and evidence support a particular belief system or political program.

But for ISM, which is mainly concerned with the operation of science, the important question is, "How does culture affect science?" Some 'culture → science' influence occurs as a result of the 'science → culture' influence described above. If society wants to obtain certain types of science-based medical or technological applications, this will influence the types of scientific research the society supports with its resources. Another type of influence occurs because scientists are more likely to accept a scientific theory that is consistent with the metaphysical and ideological theories they already have accepted; in the ISM diagram, this influence appears as a conceptual factor, **external relationships**...**with cultural-personal theories**.

A dramatic example of cultural influence is the control of Russian biology, from the 1930s into the 1960s, by the 'ideologically correct' theories and research programs of Lysenko, supported by the power of the Soviet government. This example illustrates the effect of "self interest by government" on a grand scale. But self interest also exerts influence science on a small-scale, personal level. When a theory (or a proposed research project) is evaluated, a rational scientist will be influenced by the common-sense question, "How will the result of evaluations, by myself and others, affect my own personal and professional life?" For example, sometimes a scientist has publicly taken sides on an issue and there is ego involvement with a competitive desire to "win the debate"; or time and money has been invested in a theory or project, and there will be higher payoffs, both practical and psychological, if there is a favorable evaluation by the scientific community. In these situations, when there has been a substantial investment of personal resources, a rational scientist, motivated partly by cultural-personal concerns, will usually try to exert 'authority' to influence the process and result of evaluation.

In the case of Lysenko there was an obvious, consciously planned interference with the operation of science, but cultural influences are usually not so obvious. A more subtle influence is exerted by the 'assumed' ideas of a culture (especially the culture of a scientific community), because these assumptions, along with explicitly formulated ideas and values, form the foundation for the way scientists think when they develop theories, evaluate theories, and plan their research programs. The pervasive influence of culture on the *process of science* is summarized at the top of the ISM diagram: "Scientific activities...are affected by culturally influenced **thought styles**." The *content of science* is also affected by thought styles. Section 2.72 discusses thought styles: their characteristics, their effects on the process and content of science, and their variations across different fields, and changes with time.

2.34: Personal Consistency

Sometimes cultural-personal influence can be interpreted as the result of a desire for personal consistency in life. According to the theory of *cognitive dissonance* (Festinger, 1956), if there is a conflict between ideas, between actions, or between thoughts and actions, this inconsistency produces an unpleasant dissonance, and a person will be motivated to take action aimed at reducing the dissonance. In the overall context of a scientist's life, which includes science and much more, a scientist will seek consistency between the science and non-science aspects of life. Since groups are formed by people, the principles of personal consistency can be extrapolated (with appropriate modifications, and with caution) beyond the individual level, to groups that are small or large, including societies and governments.

For example, during the period when the research program of Lysenko dominated Russian biology, the Soviets wanted consistency between their ideological beliefs and scientific beliefs, for reasons of both plausibility and utility.

Plausibility: If the metaphysical/ideological theories of Marx (and Marxists) are believed to be true, and a scientific theory is true, these theories should agree with each other. According to principles of logic, a belief in Marxist theory should increase the status of all theories that are consistent with Marx. And because it is based on the psychological application of this logical principle, a theory of cognitive dissonance will also predict this same increase in status.

Utility: Because there was consistency between all aspects of public policy (including science policy and the agricultural policy it influenced, and the underlying ideological principles of the government), 'political utility' increased[11] because a policy tends to gain popular support if there is a belief that the policy is based on (and is consistent with) true principles. And if, as described earlier, science "plays a major role in shaping cultural...thinking patterns," the government wanted to insure that a "shaping of ideas by science" would support their own ideological principles and political policies.

Similar interpretations of behavior can be applied to other levels of social structure, such as individual scientists and the research groups they form. If cultural-personal theories and scientific theories are perceived to be consistent, both will increase in perceived plausibility, and the cultural theories will increase in political utility. In addition, a scientific theory will have increased scientific utility — such as serving as the basis for research projects that receive funding — if it is consistent with the cultural theories of those who make decisions about funding.

In the ISM diagram, three large arrows point toward 'evaluation of theory' from the three evaluation factors (empirical, conceptual, cultural-personal), and three small arrows point back the other way. These small arrows show 'feedback'. If a conclusion about theory status already has been reached based on some factors, then to minimize cognitive dissonance there is a tendency to interpret other factors in a way that will support this conclusion. If a conclusion is fairly certain

[11]. At least the hope was that political utility would increase, although in Soviet society the importance of "popular support" is questionable, and the agricultural policy (based on the ideologically influenced science) resulted in a reduction of food production.

(with theory status that is either very high or very low), there will be a greater tendency to gather support for this conclusion in every way possible. Therefore, each evaluation criterion is affected by feedback from the current status of the theory, and from the other evaluation criteria. A change in current theory status will also affect the relative status of ‘alternative theories’; this feedback is indicated by a small arrow.

2.4: Theory Evaluation

Because theory evaluation is so closely connected to other scientific activities, many of the major concepts of evaluation are discussed in other sections: 2.1-2.3 describe three types of factors that serve as inputs for evaluation, 2.5 and 2.6 discuss ways in which evaluation is used for theory proposal and experimental design, and 2.7 describes how evaluation guides research and is influenced by ‘thought styles’ in a scientific community. This section will focus on the immediate results of evaluation, shown in the ISM diagram: an estimate of ‘theory status’, and a decision to retain, revise, or reject a theory.

2.41: Intrinsic Status and Relative Status

Inputs for **evaluation of theory** come from empirical, conceptual, and cultural-personal factors, with the relative weighting of these factors varying from one situation to another. The output of theory evaluation is an estimate of **theory status** that reflects current beliefs about the plausibility and/or usefulness of a theory. It is useful to distinguish between two types of status: intrinsic and relative. A theory has its own *intrinsic status,* which is an estimate of the theory's plausibility (defined by how closely it is believed to approximate the true composition-and-operation of nature) and/or its utility in facilitating the solution of scientific problems. And if science is viewed as a search for the best theory — whether "the best" is defined as the most plausible or the most useful — there is implied competition, so each theory also has a *relative status* that is defined by the question, “What is the overall appeal of this theory, when all factors are considered, compared with **alternative theories**?”

A theory's intrinsic status and relative status tend to be correlated. Usually, when one increases,

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so does the other. Logically, intrinsic status should influence relative status. The reverse is not necessarily true, but for a variety of reasons (including some based on logic, and some not) the intrinsic status of a theory tends to be influenced by the presence or absence of alternative theories. However, a theory can have low intrinsic status even if it seems to be the best choice among all known competitors, if scientists' appraisal of their current theories is that “none of the above” is likely to be true or, in the long run, scientifically useful. For example, before publication of the famous ‘double helix’ paper (Watson and Crick, 1953) the honest answer of scientists, regarding the detailed structure of DNA, was that “we don't know.” After April 1953, however, among the most knowledgeable scientists this skepticism quickly changed to a confident claim that “the correct structure is a double helix.” In 1953 the double helix theory attained high intrinsic status and relative status, but before 1953 all theories about DNA structure had low intrinsic status, even though the best of these would, by default, have high relative status as “the best of the bad theories.”

2.42: Responses to Theory Evaluation

The immediate output of theory evaluation is an estimate of theory status. Following this, three possible responses are: 1) continue to **retain** the theory as it is, with no revisions, for the purpose of *pursuit* [to serve as a basis for research that applies and possibly develops the theory] and/or *acceptance* [as a proposed explanation, for being treated as if it were true], with a status that is increased or decreased or unchanged; 2) **revise** this theory to generate a new theory that can be used in subsequent application, testing and evaluation; or 3) **reject** the theory.

In the ISM diagram, these are the only three responses. In real science, however, the response (or lack of it) often would be more accurately described as a *delay* with a deferred conclusion. For example, imagine that a deferred conclusion leads to temporarily ignoring one theory while other options are being pursued. If this theory is eventually revived for pursuit or acceptance, then in hindsight we could say either that it was being retained (with no development or application) during the interim period, or that it was being tentatively rejected with the option of possible reversal in the future. But if this theory is never revived, then when it was being ignored temporarily it was actually being rejected. In the future we can look back on an original conclusion and attach labels of ‘retaining’ or ‘rejecting’, but when the initial decision to “ignore it for awhile” is made this really is only a delayed decision, rather than a firm commitment to retain or reject.

Sometimes there is no conscious effort to reach a conclusion because there is no apparent need to decide; this produces, by default, a delay with no action one way or another. On the other hand, some sort of decision (and action) may be required even though a careful consideration of the evaluation factors indicates that only a conclusion of “inconclusive” is warranted. In this uncomfortable situation, a wise approach is:

> If use of the model in question is relevant to some practical decision that needs to be made, the problem, then, is to make that decision in a manner that takes proper account of the uncertainty as to whether the corresponding theoretical hypothesis is true. (Giere, 1991, p. 34)

2.43: Truth Status and Utility Status

As a reminder that the outcome of theory evaluation is an educated estimate rather than certainty, in ISM the concept of ‘status’ (Hewson, 1981) — with a status-continuum ranging from extremely low to extremely high — is used to describe the degree of confidence in a theory. When there is enough support for or against a theory, its status may reach a subjective threshold level (either very high or very low) where scientists decide to accept or reject. But thinking is not limited to this binary yes-or-no mode, because there is also an option to think in terms of a continuously varying status.

To allow an accurate description of theory status, four distinctions are useful. First, there is a difference, discussed in Section 2.41, between *intrinsic status* and *relative status.* Second, there is a difference, discussed in Section 2.26_A , between judging a theory to be worthy of *acceptance* (of being treated as if it were true, for purposes of doing science) or *pursuit* (for application and development, to stimulate ideas for new experimental and theoretical research).

Third, it is useful to make a distinction between truth status and utility status. In ISM, *truth status* is an estimate of the similarity between a theoretical composition-and-operation model of a system and the true composition-and-operation of this system in nature. In doing this, I assume a ‘correspondence’ definition of truth, that a theory is true if it corresponds to what actually exists.

Usually, ‘truth status’ is referred to as ‘plausibility’ (instead of the misleading term ‘truthfulness’) to clarify my intended meaning of truth status as “a human estimate for the probability of truth” rather than a claim for a certainty of knowledge about truth. *Utility status* is an estimate of the overall utility of a theory, including scientific utility for cognition and research and, if utility is defined more broadly, cultural-personal utility.

A fourth distinction is between a *realist* interpretation of status (in which a theory's function is to describe what really happens in nature, so it should be judged on this basis) and an *instrumentalist* interpretation (in which a theory is only intended to be useful as a tool for making predictions, or for stimulating and solving scientific research problems, so it should be judged on this basis).

In real life, a range of views is encompassed by those who advocate a realist interpretation, and there is a range of instrumentalist views. But even though there are no ‘pure’ realists or instrumentalists, it is possible to imagine the following thought experiment in which two scientists — a pure realist and a pure instrumentalist, who are so pure that they will consider either only truth or only utility — are combined into one scientist with a more eclectic perspective, who is willing to consider both truth and utility. This third perspective, which I call *critical realism,* is based on a definition by Hodson (1986) that has been modified for use in ISM. A critical realist adopts a *critical epistemology* (willing to be skeptical about the truth status of a particular theory) and *realist goals* (wanting to find the truth), and defines a theory's ‘status’ by combining ‘truth status’ with ‘utility status’. In addition, a critical realist can adjust the relative importance of truth and utility by a personal estimate — which can vary from one theory to another — of where a theory is located on a continuum between realist and instrumentalist interpretations.[12] At one end of the continuum

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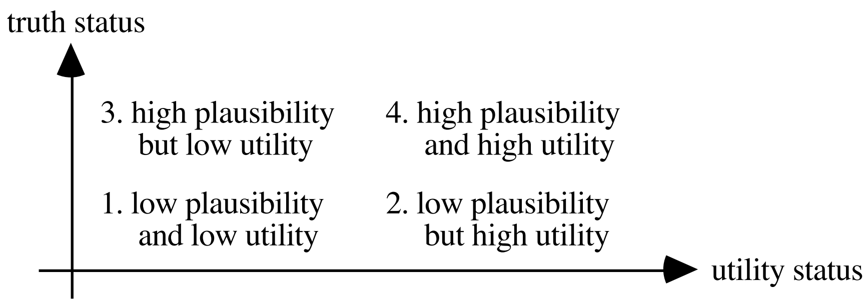
[12]. As often occurs, it is difficult to choose an appropriate term. ‘Instrumentalism’ usually refers to a view that theories are only devices to make predictions, not claims for truth. This is different than (but related to) the concept that theories should be judged on overall utility rather than plausibility. Both of these views are defended by a "pragmatist" in a book that is written as a four-person dialogue written by Laudan (1990), but this term is less appealing for use in ISM because ‘pragmatist’ philosophy is commonly associated with the philosophical views of Peirce and James, and (later) Dewey, which are broader than its intended meaning in ISM.

only truth is important, at the other end only utility counts, and in the middle there is a balanced emphasis.

Consider the four ‘extreme’ combinations of truth status and utility status; in Figure 4 these are numbered 1 to 4. One thoroughly unimpressive theory is viewed as neither plausible nor useful. A scientist may doubt that a second theory is true, yet be confident in evaluating it as high in scientific utility. For a third theory, the conclusion is that while this theory is likely to be true, it is not very scientifically useful. A fourth theory is considered both plausible and useful.

Figure 4: Four Possibilities for combining Truth Status with Utility Status



These four combinations can stimulate thinking about the interpretation of evaluation: If there is a correlation between estimates of truth and utility, combinations 1 and 4 will be more common than 2 and 3. For a pure instrumentalist, 2 and 4 have equal status, and both are better than 1 or 3. For a pure realist, 3 and 4 have equal status, and are better than 1 or 2. With an equal weighting of realist and instrumentalist emphasis, 4 is the best, 1 is the worst, while 2 and 3 are roughly equal. But since perspective can change from one theory to another, it is possible for 2, 3 and 4 to have equal status if a scientist cares only about the utility status of 2 and the truth status of 3.

Another ISM concept that is useful for stimulating flexible, careful thinking is the definition (based on Giere, 1991) of *hypothesis* as "a claim that the model and system are similar, in specified respects and to a specified degree of accuracy." This definition allows different hypotheses to be framed for the same theory-based model of a system. The strongest hypothesis would claim that there is an exact correspondence between all model-components and system-components, while a weaker hypothesis might claim that there is only an approximate correspondence, or that there is a correspondence (exact or approximate) for some components but not for all. If a theory is judged to have only a moderately high truth status, the uncompromising claims of a 'strong hypothesis' will be viewed with suspicion, even though scientists might agree with the diluted claims of a 'weak hypothesis'.

Since the ISM definition of hypothesis is framed in terms of claims about the similarity between a model and a system, it is closely related to claims about 'truth status'. But a similar type of variable-strength hypothesis could be oriented toward an instrumentalist interpretation — for example, with an alternative definition of hypothesis as a claim that "predictions and observations will be similar, in specified respects and to a specified degree of accuracy," or as a claim that a theory will be "scientifically useful, in specified ways and to a specified extent."

Some terms used in this section (realism and instrumentalism, acceptance and pursuit) are commonly used in philosophy, while others (intrinsic status and relative status, truth status and utility status) have been defined by myself. None of these terms are common in the language of scientists, but I think the *concepts* are common in the *thinking* of scientists. I have found all of these concepts useful for analyzing scientific thinking, and other study-of-science scholars may also find them useful. The main function of these terms is to avoid implying a need for choosing between dichotomous alternatives, as implied by terms such as accept or reject, verify or falsify. And when language does not force thinking into narrow dichotomous channels, flexible thinking is encouraged. These terms also allow a more accurate description of scientific methods and thinking, when describing a specific situation or when making a generalization.

2.44: The Limits of Logic, and Rationally Justified Confidence

In the field of science studies, there is controversy about the extent to which the reliability of scientific knowledge is challenged by the limitations of logical inference, and by the influence of cultural-personal factors. These issues have sparked many heated debates between advocates of apparently irreconcilable opinions. There are no answers that will be acceptable to everyone, and I will not try to provide answers. Instead, Sections 2.44 and 2.45 briefly discuss major issues, and express opinions.

Disclaimers: First, and most important, these sections are limited in depth and scope; they attempt to say something about controversial issues, but not everything. Second, my own opinions are clearly expressed in these sections, more so than in most of my elaboration of ISM, so remember that the views expressed in this elaboration are my own, and are not necessarily those of the ISM framework.

Section 2.44 will discuss some ways in which three types of limitations (for hypothetico-deduction, observation, and induction) affect the reliability of scientific knowledge.

A. Limitations of Hypothetico-Deductive Logic

One way to summarize the limitations of logic is a principle of 'underdetermination' which states that it is impossible, using any type of logic, to prove that a theory is either true or false. In addition, there can be suspicions about the reliability of observations. These 'logical skepticism' arguments, challenging the validity of scientific knowledge, are discussed in this section, along with counter-arguments.

If observations agree with a theory's predictions, skeptics correctly point out that this does not prove the theory is true, because another theory — including one that has not yet been invented, and maybe never will be — might also predict the same observations, and might be a better explanation. When a theory makes a prediction that "if T, then P" and P is observed, this does not prove T is true. And when a theory is invented using retroductive logic, which is a variation of hypothetico-deductive logic that is subject to the same epistemological limitations, an additional reason for caution is that a theory is being constructed so it will fit old data, so empirical agreement can be obtained by ad hoc patchwork.

Compared with the task of producing empirical support for a theory, it is generally considered easier to gather convincing evidence that a theory is inadequate. Yet it still is impossible to logically prove a theory is false, because if there is *anomaly* for a theory (with a low degree of agreement between predictions and observations), the disagreement could be due to any of the many elements that contribute to the predictions, the observations, and their comparison. Erroneous predictions could be caused by an inadequate theory or supplementary theory, or by a characterization of the experimental system that is inaccurate or incomplete, or by misapplying theories to construct a model, or using faulty deductive logic to make a prediction. But perhaps it is the observations that are not reliable, due to poor experimental design or sloppy technique; or maybe there was defective equipment, such as an observation detector that did not function as expected. Or the logic used in comparing the predictions and observations may be deficient, and this has produced an estimated degree of agreement that is inappropriately low.

There are many possibilities for the cause of anomaly, and each can be illustrated with examples from the history of science. A rigorous logical analysis leads to the skeptical conclusion that anomaly cannot ever be localized to any of these possibilities. But according to Shapere (1982, p. 516), "What this shows is that formal logic does not exhaust what counts as reasoning in science." Scientists are quite willing to use "reasoning that goes beyond formal logic" to cope with a complex situation and to make educated estimates — based on their confidence in each factor that affects the predictions, observations, and comparison — about where the anomaly is likely to be located.

Similarly, according to formal logic a theory can never be proved true. But sometimes a theory consistently predicts old and new data for a wide variety of experimental systems, even though the combined empirical constraints (for all experiments) are so demanding that it seems unlikely any alternative theory could also satisfy them. Thus, for example, few scientists doubt the double-helix structure of DNA, despite valid logical arguments that this theory is underdetermined by the data. Even though it is logically impossible to prove that any theory is true, scientists can have a *rationally justified confidence* that a particular theory is true, or that it makes reliable predictions and is scientifically useful.

Modern science has given up the quest for epistemological certainty, and is willing to settle for a high degree of plausibility. Scientists rarely worry about skeptical challenges such as "Can you be certain the sun will rise tomorrow?" (argued by Hume), or "How do you know it isn't all a dream?" (asked by Descartes), or "Can you prove that scientific theories of 1997 are closer to the truth than theories of 1497?" (a challenge by modern relativists). When it comes to theory evaluation, instead of asking “What can be proved using formal logic?”, it is more practical for scientists to ask “What is a good way to bet?”

Consistent with the lack of certainty in science, in ISM the concept of *status* uses a continuum to estimate the degree of confidence in a theory. In addition to ‘status’ and the response options (to retain, revise, reject, or delay) there are a variety of modifying concepts to describe the result of evaluation: intrinsic status and relative status, pursuit and acceptance, truth status and utility status, realism and instrumentalism, and variable-strength hypotheses.

B. Limitations of Observations

Another option for skeptics is to attack the foundation of empirical science by claiming that data collection is inevitably biased, and that because observations are ‘theory laden’ they are unreliable and involve circular logic. But for each potential difficulty (discussed at the •'s below), scientists have ways to cope (discussed in each set of brackets below).

Why is data collection biased? • During experimental design, scientists decide what to study and how to study it, and this decision determines the type of data that will be collected. { Design determines the *type* of data, but nature determines the data. And the bias in experimental design can be analyzed in order to either consider it during interpretation, or reduce it in a re-design. } • Biased data collection can occur by a human who has expectations for what is worth seeing or what will occur, or who hopes that certain results will occur. { This is a valid concern that varies with the situation. In a medical experiment there will be little concern if an observation arises from reading a digital thermometer. But if subjective assessments of patients' symptoms are required, scientists often use a ‘double blind’ experimental design to minimize errors due to observational bias at a conscious or unconscious level. }

Why are ‘theory-laden observations’ a cause for concern? • Observations depend on theories, and theories are uncertain, so this lack of reliability transfers to our observations. { As discussed above, scientists can have a ‘rationally justified confidence’ about the truth or reliability of theories, including ‘theories of observation’ such as “theories of the source, the transmission process, and the receptor” (Shapere, 1982). } • Theory-based interpretations always occur during observations.

{ Yes, but usually there are limits on the range of observations. As noted by Strike (1987), although in the early 1600s Galileo and an Aristotelian scientist would "see" a pendulum differently, neither would see a giraffe. } • There is a possibility of circular logic, if theories are used to interpret observations that are used to support theories. { If the 'theory' being evaluated is closely related to an 'observation-theory' used in an experiment, with overlapping domains and many shared assumptions and theory components, concerns about circularity are justified. But if a theory and observation-theory are relatively independent, there will be minimal circularity. Shapere (1982, pp. 514-516) discusses logically sophisticated methods for analyzing observation situations and observation theories, and how scientists use these methods to check for circularity and reliability. }

C. Limitations on Inductive Logic

Skeptics can claim that: • Induction based on observations is unreliable because observations are unreliable. { Concerns about the reliability of observations are discussed above. } • When a shaky observational foundation is extended by inductive generalization it becomes even more uncertain. For example, David Hume asked "Can you be certain the sun will rise tomorrow, based on inductive generalization from its behavior in the past?" According to formal logic the answer is "no." { But science assumes the answer is "yes, unless there is a reason for it not to rise." Even though induction cannot be proved, scientists consider it "a good way to bet"; instead of adopting a total skepticism, they use statistical logic and statistical conclusions. They also try to understand the distinction between inductions that involve different types of systems and properties. For example, most scientists would be more confident about a generalization that "all pure NaCl is white," compared with the analogous claim that "all swans are white." Philosophers explain the relative difference in confidence in terms of differences in 'lawlike' and 'accidental' character, and there are logical reasons to believe that the whiteness of NaCl has more lawlike character. }

Popper (1963) emphasizes the asymmetry between verification and falsification. If based on a theory there is a deductive prediction that "if T, then O" and O occurs, this does not prove T is true, but if O does not occur this does prove T is false. But skeptics even question the possibility of falsification, due to the type of arguments discussed earlier in "Limitations of Hypothetico-Deductive Logic." Another reason for the impossibility of proof or disproof comes from the statistical nature of some predictions or theory-based claims. For example, Grinnell (1992) discusses differences in logic, which are agreed on by skeptics and non-skeptics, between the claim that "All X are Y" which can be falsified but not verified, and "Some X are Y" which can be verified but not falsified, and "90% of X are Y" which cannot be verified or falsified. While most scientists will agree with these conclusions about what can and cannot be proved, skeptics will challenge the possibility that claims "can be verified" or "can be verified." And, on the non-skeptical side, for claims that "cannot be verified" or "cannot be falsified" scientists can nevertheless develop a 'rationally justified confidence' by using statistically based inductive logic. But skeptics will question whether this confidence is justified.

D. Potential Problems and Actual Problems

Logical skepticism is based on sound principles. A critical thinker should be aware of the technical and practical limitations of observations, and of logic that is hypothetico-deductive, retroductive, or inductive. But although some skepticism is good, "too much of this good thing" — without sufficient balance from critical thinking and common sense — can be detrimental to science and rationality. Scientists have developed ways to cope with the concerns of skeptics, so that in most situations the skeptics' *potential problems* do not seem to be a significant *actual problem* for science.

One characteristic of extreme 'logical skepticism' is a tendency to be unable (or unwilling) to distinguish between potential problems and actual problems. Much of what I call "silly skepticism" is characterized by proposing extreme solutions for problems that don't really seem to be problems, by proposing "cures for which there is no adequate disease. (Fodor, 1986)"

2.45: Conflicts and Controversies

The question of conflicting interpretations began in the preceding section, which discussed skeptical arguments and counter-arguments. This section continues with themes of conflict. In the first subsection the conflict is between evaluation criteria. This is followed by a discussion of topics that are hotly debated among scholars: the characteristics of cultural-personal factors, and their effects on the process and content of science. Finally, there is a discussion of realism and instrumentalism, which differ in their answer to the question of whether science aims at (or succeeds at) finding the truth.

A. Empirical Factors and Conceptual Factors

There can be conflicts between empirical factors and conceptual factors, as discussed in Section 2.22. For example, when a theory is simplified (which is usually considered a desirable conceptual factor) the accuracy of its predictions may decrease (which is undesirable according to empirical criteria). Or consider the statement from Section 2.12_C that an estimate of predictive contrast requires a consideration of how likely it is that "plausible alternative theories" might make the same predictions. The word "plausible" indicates that empirical adequacy is not the only relevant factor. To illustrate the need for combining empirical and conceptual criteria, Sober (1991, p. 31) tells a story about explaining an observation (of "a strange rumbling sound in the attic") with a theory ("gremlins bowling in the attic") that is empirically adequate yet conceptually implausible.

Conflicts also occur between conceptual criteria. For example, scientists usually want a theory to be complete (with all important components included) and simple (with no extraneous or spurious components), but there is an inherent conflict between completeness and simplicity. And there are varying degrees of preference for unified theories with wide scope; in some fields but not others, and for some scientists but not others, a unified wide-scope theory is highly valued, relative to other criteria. Another potential source of conceptual conflict is the empiricist claim (that theories should not include unobservable components for entities, actions or interactions) versus explanatory adequacy.

Interaction between empirical factors occurs when there is data from several sources. Scientists want a theory to agree with all known data, but sometimes in order to obtain agreement with one data source, it is necessary to sacrifice empirical adequacy with respect to another source, so a decision about “what is most important” may be necessary.

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B. Relativism

When taken to an extreme, relativism claims that any idea is as well founded, and deserving of belief, as any other idea. Relativism regarding scientific theories is affected by judgments about the balance between empirical factors and cultural-personal factors, and about the validity of empirical evaluation. If cultural-personal influence is strong relative to empirical evaluations, with ideas being determined mainly by culture, then in a different culture the result of evaluation would be different. Relativism is strengthened if the input from empirical factors is weakened, if logical skepticism challenges the credibility (and even the possibility) of culturally-independent empirical 'reality checks' that might compete with cultural influences. Although a philosophy of relativism can be based entirely on 'logical skepticism', usually this is combined with a heavy emphasis on the influence of cultural-personal factors.

In recent decades, relativism has become surprisingly popular among some scholars. One factor in the rise of relativism was the popular, controversial book, *The Structure of Scientific Revolutions* (Kuhn, 1962), which emphasized the central role played by non-logical factors in the revolutionary overthrow of one paradigm by another. These ideas helped inspire a wave of anti-rationalist intellectual activity that pushed the boundaries of relativism far beyond the original claims of Kuhn. One group contributing to the outward push of boundaries, the 'strong program' in the sociology of scientific knowledge (Bloor, 1976; 1991), focused on the ways in which cultural-personal factors affect the *content* of science. This is more controversial than a claim about the *process* of science, which is generally agreed to be influenced by social factors. Scholars in the strong program usually espouse a radical relativism, sometimes to the point of claiming that the content of scientific theories is influenced more by culture than by nature.

While there is a strong correlation between a heavy emphasis on cultural factors and relativism, there is no necessary link. For example, David Hull, a prominent philosopher, thinks that reliable content can emerge from a chaotic process:

> Although objective knowledge through bias and commitment sounds as paradoxical as bombs for peace, I agree that the existence and ultimate rationality of science can be explained in terms of bias, jealousy, and irrationality. (Hull, 1988, p. 32)

Hull describes how this can occur; so does Bauer (1992), who claims that during a communal ‘filtering’ process the non-objective behavior of individuals (or groups) tends to cancel, thus producing a result that is more objective than the objectivity of the individual scientists.

In addition to this view (of cultural influence without relativism), it is possible for relativism to be based mainly on logical skepticism, without an appeal to strong cultural influence. Or, instead of reaching relativism as a conclusion, a preference for relativism can come first, with the logical skepticism and cultural factors then enlisted as support.

This section will not discuss relativism in detail. Briefly stated, my opinion (based on the principle used earlier, that without balance “too much of a good thing” can be detrimental) is that extreme relativism is the result of taking useful ideas — such as critical thinking, and an awareness of cultural-personal factors — and stretching them to the point where they lose credibility, and are detrimental to science and society. Section 4.23, entitled "Should Scientific Method be X-Rated?", asks "What is the most accurate description of science, and what educational approach is most beneficial for students?", and discusses the educational disadvantages of extreme views, and the responsibility of educators. A “strong critique of the Strong Program” continues in Appendix A24, which also discusses motivations for relativism.

When evaluating the more extreme interpretations of science, it helps to have tools that encourage flexible, critical, accurate thinking. Two useful analytical tools, idealization and range diagrams, are discussed in Appendix A25. Hopefully, these tools will facilitate an examination of the ways that science is (and is not) influenced by cultural-personal factors, and will help avoid the need for dichotomous generalizations such as “no cultural influence” or “all cultural influence.” The logical principle for the first tool is that an *idealization* is an oversimplified model that can be used to estimate the effects of a component (such as cultural-personal influence) that has been intentionally omitted from the model. The second type of analytical tool, *range diagrams,* can be useful in determining how accurately a sample represents a larger population, and in deciding what types of conclusions can be drawn about a population based on a small sample of case studies. For example, when studying the mutual influence between societal politics and science, different conclusions will result from studying a sociobiologist (this field can be very politicized) or a benzene chemist (very little societal politics is happening here). Each of these experiences is a part of science, so drawing a general conclusion based on either sample would be misleading.

C. Realism and Instrumentalism

One response to the impossibility of proof is to interpret theories as explanations that make claims, not for probable truth, but for scientific usefulness. This 'instrumentalist' position is sketched in Section 2.43, which provides an overview of the main issues in a realist-instrumentalist debate, and describes some useful concepts such as truth status and utility status, critical realism, and variable-strength hypotheses. Appendix A23 is a more detailed discussion, including a more detailed discussion of the basic issues, plus a "million dollar wager" on scientific progress, and a critique of strange ideas about "creating reality." The current subsection only covers a few of the main ideas.

One argument against realism uses "the boy who cried wolf" logic: many theories already have been abandoned; can you prove that our current theories will not meet the same fate? Another argument is that, since the truth of a theory can never be known, realism is an unrealizable goal (i.e., there is no way of knowing whether it has been achieved), so this goal should not be held by rational scientists. On the other side, in my opinion the most impressive argument for realism is that it would seem miraculous, beyond any reasonable odds, if current theories can make accurate predictions, even if none of these theories actually correspond (not even approximately) to what is happening in nature.

Another argument — which is important if a high value is placed on describing scientific practice as it really is, and not just prescribing how it should be according to philosophers — is that most scientists want to "find the truth" about nature. The concept of critical realism — combining a critical epistemology (willing to be skeptical when it seems appropriate) and realist goals (wanting to find the truth), with a flexible attitude toward the balancing of utility and plausibility — seems to describe a common perspective among scientists.

Regarding two possible definitions of truth (as correspondence or consensus), consider the change in scientific theories about the solar system between 1500 and 1700. Using a correspondence definition, I interpret this as a change in theory (from earth-centered orbits to sun-centered orbits) while the truth (of the planets' actual motion) did not change. But if truth is defined more broadly to include a 'scientific truth' that is defined by consensus, then 'the truth' changed in this 200-year period when the consensus of the scientific community changed. To me, this 'consensus' definition seems foolish and unwise. I think it is more accurate to say that what changed was our theories (or beliefs, opinions,...) or maybe, depending on how it is defined, our knowledge. With a consensus definition of truth, there is no word to unambiguously describe the concept of "the way things really are." A consensus definition of truth seems to be an effort to remove the concept of truth (or reality) from our language, similar to the "newspeak" strategy (in George Orwell's *1984)* where language is modified to make it difficult to think (and impossible to communicate) in unapproved ways. But with a correspondence definition of truth, both concepts can be clearly and unambiguously expressed; 'truth' (or reality) has one meaning, and 'theory' (or belief,...) has another, and the relationships between truth and theory can be rationally discussed.

Although I have strong views about this definition, I recognize that other views exist among scholars. For example, Frederick Grinnell, a scientist who has earned my respect due to his interpretations of science that I have found to be generally insightful, accurate, and useful, nevertheless adopts a consensus definition of truth:

> The ideal goal of science is inclusive knowledge. Potentially, this is a truth that everyone could verify. ... What defines truth in this context is intersubjective validity. The scientific attitude aims toward a consensus view of reality. (Grinnell, 1992, pp. 47, 137)

Grinnell describes "inclusive knowledge" and "intersubjective validity" as goals of science (I agree) but then defines this as "truth" rather than the more accurately descriptive "intersubjectively validated observations" or "theories validated as scientific knowledge by consensus." Similarly, "a consensus view [by a particular scientific community] of reality" is defined as "truth." Basically, Grinnell is defining a "view of reality" as "truth" instead of what it really is, a "theory." As discussed above, this definition (a consensus theory = truth) weakens our language by making it difficult to describe an essential concept (what exists and occurs = truth). On the other hand, a correspondence definition makes it possible to clearly discuss both concepts: theory and truth.

2.5: Theory Selection and Invention

Section 2.5 will discuss: the similarities and differences between theory selection and theory invention; an important type of logic that is used for selection-or-invention; and strategies for invention that focus on the use of empirical or conceptual logic.

A reminder about terminology: In ISM the process of 'theory invention' is defined to include the revision of an existing theory, and the revised theory will be called a 'new theory' even though it is not totally new. With this broad definition, 'invention' includes the small-scale theory development that is common in the everyday practice of science, and also the major conceptual revolutions that, although rare, are important when they do occur.

2.51: Selection and Invention

When an existing theory seems to serve as a satisfactory basis for explaining a type of phenomenon or for constructing a model of an experimental system, **selection of an old theory** is usually the choice of scientists. But satisfaction is not an all-or-none state of mind, as emphasized by ISM's concept of a 'theory status' continuum for the result of theory evaluation. The presence of some dissatisfaction (minor or major), combined with creative curiosity (for exploring other possibilities by asking "What would nature be like if...") and personal ambition (to be known as "the inventor"), can provide motivation for attempting the **invention of a new theory**.

There are many similarities between selection and invention. In selection a theory is proposed from memory, with invention a theory is proposed from imagination. But if invention is primarily a process of developing and revising existing ideas, in a complex blending of memory-and-imagination, there is a close cognitive connection between selection and invention. A solid foundation of theory knowledge is essential for selection, and is also needed for invention by revision. A deep knowledge of theories will involve whole theories and the external relationships that integrate these theories into unified mega-theories, and also (moving toward smaller levels) the conceptual components and logical structure that are used to construct each theory. This theory knowledge can exist before a scientist is aware of experimental data, or an awareness of unexplained data can motivate a 'literature search' for one or more theories that can be used, as-is or with revision, to explain what already has been observed.

In addition to the close cognitive connections between selection and invention, after a theory has been proposed there are consequences — such as evaluating the theory, using it to guide experimental design, and so on — that are similar whether the theory was proposed by selection or invention. The selection/invention connection is indicated in the ISM diagram by a single, unified 'selection-or-invention' oval that has a dashed line separating selection and invention, with one arrow going into the oval and one set of arrows emerging from it. The purpose of this single-oval symbolism is to emphasize that, despite their differences, selection and invention are in many ways similar, in both process and consequence. In the following discussions, I will sometimes use 'proposal' to refer to 'selection and/or invention'.

2.52: Retroductive Logic and Empirically Inspired Invention

This section first describes retroductive inference by comparing it with deductive logic and hypothetico-deductive inference, focusing on similarities and differences in timing, purpose, and logical limitations. This is followed by a discussion of invention strategies that are motivated and guided by empirical data.

A. Timing

Theory proposal is guided by empirical evaluation through the creative-and-critical process of **retroductive inference** (also called abductive inference) that is inspired and constrained by known observations. In contrast with deductive logic that asks, "If this is the model, then what will the observations be?", retroductive logic — which uses deductive logic but supplements it with other types of reasoning — asks a reversed question in the past tense, "These were the observations, so what could the model have been?"

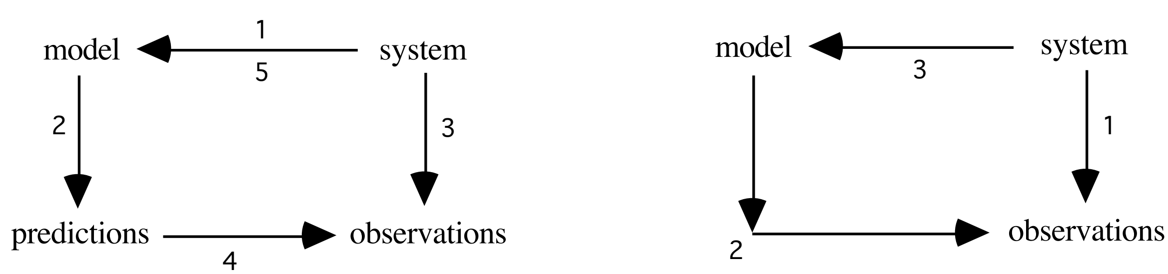
The reasoning that occurs in hypothetico-deductive logic and retroductive logic is similar, but there are important differences in timing, purpose, and generality. The difference in timing is summarized in Figure 5. In the process symbolized by elements of the 'hypothetico-deductive box', the usual timing is: first do the top [a theory-based characterization of an experimental system produces a model: 'system → model'], then do the left [model → predictions] and right [

system → observations], bottom [are predictions ≈ observations?] and top [is model ≈ system?]. These five steps are numbered 1-5 in Figure 5-A. When the same elements are interpreted as a 'retroduction box' in Figure 5-B, the order is different:
1) right [system → observations], 2) left-and-bottom [model → predictions; are predictions ≈ observations?], and 3) top [is model ≈ system?]. In the left-and-bottom process, which is the essence of retroductive inference, thought-experiments are repeated over and over, each time "trying out" a different model that is being non-deductively proposed (by selection or invention) with the goal of producing deductive predictions that match the known observations.

Figure 5: Timing Differences for Hypothetico-Deductive Logic and Retroductive Logic.

Figure 5-A: sequence for **hypothetico-deductive logic**

Figure 5-B: sequence for **retroductive logic**



B. Purpose

These differences in timing are related to a difference of purpose. In a hypothetico-deductive context a theory is proposed before the experiment is done, with the goal of evaluating the theory and/or developing a deeper understanding of the experimental system. By contrast, retroductive reasoning is useful when, after an experiment is over, scientists are not sure about the best way to interpret what has happened. In this context of uncertainty, scientists search for a theory (either old or new, by selection or invention) that will help them make sense of what they have observed. But in the final step of both hypothetico-deduction and retroduction the same type of inference occurs — to answer the question, "is model ≈ system?" — so there is a similarity of purpose in providing information for the empirical evaluation of a theory. In the ISM diagram the input-arrow that enters "empirical evaluation of current hypothesis" from the 'hypothetico-deduction box' could just as easily come from a 'retroduction box' because, as shown in Figure 5, the same elements are involved in both types of inference.

C. Logical Limitations

The logical limitations of hypothetico-deduction also apply to retroduction, because both types of inference try to answer the same question — "is model ≈ system?" — by comparing predictions with observations. But even if these agree, this does not prove a model is true, because it is possible that other models might make the same correct predictions. Therefore, only a cautious conclusion (if system-and-observations, *then maybe* model) is logically justified when using either hypothetico-deductive or retroductive inference. According to formal logic, both types of inference are identical, but with retroduction there should be more concern about the possibility of using ad hoc adjustments to achieve a match between predictions and known observations. This concern applies to retro-selection, and even more so for retro-invention. But skepticism can be overdone, and even though it is impossible to prove a theory is either true or false, scientists can feel a 'rationally justified confidence' in their conclusion about a particular theory.

D. Invention of a Domain-Theory or System-Theory

A theory-based model of a system is constructed from two sources: a general domain-theory (about the characteristics of all systems in a domain) and a specific system-theory (about the characteristics of one experimental system). When these two sources combine in deductive prediction, the overall process is "domain-theory + system-theory → model → prediction" and, if the intermediate model is eliminated, "domain-theory + system-theory → prediction." This 3-variable logic can be compared with an equation that has 3 variables. If an equation is "A + B = C" and if we know any two of the three mathematical variables, then we can find the third. Similarly, if "domain-theory + system-theory → prediction" and if we know two of the three logical variables, we can find the third.

This analogy, although useful, is limited because even though all of the mathematical variables

(A, B, C) are similar in function, the logical variables are not. If there is a precisely characterized model, constructed from a domain-theory and system-theory, deduction will produce a logically correct prediction: if domain-theory and system-theory, then prediction. But with retroductive logic there is not a single logically correct result. We can only claim that "if prediction, then maybe model" and, moving one step further back, "if predictions, then maybe domain-theory and system-theory." But these cautious conclusions are a limitation of retroductive inference, and are not related to the use of a 3-variable perspective.

A theory inventor can explore various combinations of "domain-theory + system-theory," because a change in either of these will allow (and require) a change in the other, in order to generate predictions that match the observations. While analyzing these combinations, an inventor may discover some ways in which a domain-theory and system-theory are not independent. In particular, a domain-theory (about all systems in the theory's domain) will usually influence a system-theory about one system in this domain.

An interesting example of retro-inventing a system-theory was the postulation of Neptune. In the mid-1800s, data from planetary motions did not match the predictions of a domain-theory, Newtonian Physics. By assuming the domain-theory was valid, scientists retroductively calculated that if the system contained an extra planet, with a specified mass and location, the predictions would match the observations. Motivated by this newly invented system-theory, astronomers searched in the specified location for the extra planet, and discovered Neptune. Later, in an effort to resolve the anomalous motion of Mercury, scientists tried this same strategy by postulating an extra planet, Vulcan, between Mercury and the Sun. But this time there was no extra planet; instead, the domain-theory (Newtonian physics) was at fault, and eventually it was superseded by Einstein's theory of general relativity. These examples illustrate two ways to resolve an empirical anomaly, by revising either of the two theoretical foundations that are used for constructing a model. There was a revision of the system-theory (with Neptune) and the domain-theory (for Mercury).

In another example, described earlier, the 1903 discovery of radioactivity caused a revision of a system-theory for the earth's interior geology. This revised system-theory, combined with observations (of the earth's temperature) and a domain-theory (thermodynamics), required a revision in another theory-component (the earth's age), thereby settling an inter-field conflict that began in 1868.

The overall result of retroduction is the selection or invention of a system-theory and domain-theory, because these are the main inputs used in constructing a model for making predictions. A domain-theory may result from combining one or more theories, so in the ISM diagram there are arrows leading from the selection-invention oval to 'theory' and 'supplementary theories'. A third arrow points to 'alternative theories' because a revised theory becomes an alternative that competes with the original unrevised theory. Or the original theory might become an 'alternative', since labeling depends on context; what scientists consider a main theory in one situation could be alternative or supplementary in other situations.

E. Multiple Empirical Constraints and Retroductive Induction

Retroductive logic is used to obtain a match between one model and the observations from one experimental system. But retroduction is also an important part of the inductive generalizing process that converts a newly invented model (for one system) into a general theory (for all systems in a domain), or that expands the scope of the domain for which an existing theory is claimed to be valid.

If there is data from several experiments, the empirical constraints on retroduction can be made more rigorous by demanding that a theory's predictions must be consistent with all known data. One way to view this multiply constrained process is that two or more hypotheses, each for a specific experimental system, are being combined-and-generalized to form a theory that encompasses a wider range of systems. The general theory thus has a wider scope than any of the individual hypotheses. In fact, this process of *retroductive induction* usually produces a theory that has a wider domain than all of the systems combined, when the general theory is assumed to apply to a whole 'class of systems', only some of which are included among the smaller sample of systems for which there is available data.

Retroductive induction for multiple systems converts specific models into a general theory. But in hypothetico-deduction, a general theory is applied to construct a theory-based model for one system. A summary: the 'models → theory' process in retro-induction is a movement from specific to general, while the 'theory → model' process in hypothetico-deduction moves from general to specific.

Retro-induction can also widen the scope of an existing theory, when this theory is retroductively selected for application to a system outside the originally claimed domain of the theory.

As a strategy for coping with the complexity of multiple constraints, a theory can be tested for one system at a time, in succession. For example, a scientist might invent a model that fits the data for System #1, and then apply the principles of this model (i.e., a 'theory' from which this model could be derived) to construct models for Systems #2 and #3. If necessary this process can be repeated to find a theory that constructs satisfactory models for all known systems. The process can end at this point, or a variation of this theory (or maybe a new type of theory) can also be tested by applying it to make models and predictions for all known systems. Or new experiments can be designed to test the existing theory(s). Or thought-experiments can be run, to mentally test potential inductive generalizations.

Or a scientist can use creative methods of data interpretation to stimulate the recognition of an apparent pattern. After an empirical pattern is found, this may provide the inspiration and guiding constraints for inventing a theory with a composition-and-operation mechanism that can explain the pattern, thus converting a descriptive theory into an explanatory theory. While searching for patterns, a scientist can try to imagine new ways to see the data, or to interpret its meaning. Logical strategies for thinking about multiple experiments, such as Mill's Methods of inquiry, can be useful for pattern recognition and theory invention.

2.53: Conceptually Inspired Invention

Although C.S. Peirce (in the 1800s) and Aristotle (much earlier) studied theory invention, most philosophers have made a distinction between invention and evaluation, between the contexts of 'discovery' and 'justification', and have focused their attention on evaluation. Recently, however, many philosophers (such as Hanson, 1958; Darden, 1991) have begun to explore the process of invention and the close relationships between invention and evaluation. Haig (1987) describes a "hypothetico-retroductive inferential" method that includes “the process of discovery” in scientific method. Invention and evaluation are both used in retroduction, with empirical evaluation acting as a motivation and guide for invention, and invention producing the idea being evaluated; it is impossible to say where one process ends and the other begins, or which comes first, as in the classic ‘chicken and egg’ puzzle.

In science the selection or invention of theories is subject to multiple evaluative constraints. Empirical evaluation is important, but scientists also check for adequacy with respect to cultural-personal factors and conceptual criteria: internal consistency, logical structure, and external relationships with other theories. The ISM diagram summarizes these multiple constraints by stating that "selection and invention are guided by all evaluation factors: cultural-personal, conceptual, and (as in retroductive inference...) empirical." The remainder of this section examines the use of conceptual factors in theory invention, to supplement the use of empirically guided retroductive invention.

A. Analysis-and-Revision

Invention often begins with the selection of an old (i.e., previously existing) theory to serve as a first approximation that can be revised to develop a new version of the old theory. One strategy for revising theories begins with analysis; split a theory into components and play with them by thinking about what might happen if components (for composition or operation) are modified, added or eliminated, or are reorganized to form a new structural pattern with new interactions. Wimsatt and Darden describe this technique:

> [When] a model...and the experimental and heuristic tools we have for analyzing it are structured in such a way that we can localize its errors and attribute them to some parts, aspects, assumptions, or subcomponents of the model,... then ‘piecemeal engineering’ can improve the model by modifying its offending parts. (Wimsatt, 1987, p. 8)

> One way of resolving an anomaly is to localize one or more components that may be failing and consider what type of component could replace it to resolve the anomaly. (Darden, 1991, p. 250)

Based on analysis of the activities of classical geneticists, Darden formulates a number of "strategies for producing new ideas":

> [One strategy is] manipulating a symbolic representation. Any use of a model falls under this general strategy, either mental models, diagrammatic representations, scale models, computer simulations, or formal systems of equations. The important feature uniting these is that they all stand in a relation of representation to the natural system being investigated. ... The symbolic representation serves as a substitute for the natural system. (Darden, 1991, p. 255)

According to Lakatos (1970), theory development is typically constrained by an assumption that a "hard core" of essential theory-components should not be changed, so an inventor's focus can be narrowed to the "protective belt" of auxiliary components that are devised and revised to protect the hard core. Usually this narrowing of focus is productive, especially in the short term. But occasionally a revision of the hard core is useful. In searching for ideas, it may be helpful to examine each evaluation constraint, even in the hard core, and ask whether it really is necessary. By relaxing mental blocks about “the way things must be” it may become easier to see theory components or data patterns in a new way, to imagine new possibilities. Or it may be productive to combine this analytical perspective with a more holistic view of the theory, or to shift the mode of thinking from analytical to holistic.

B. Internal Consistency

One guiding constraint for theory invention is a desire to maintain logical consistency within a theory. Sometimes a theory is constructed, using the logic of internal consistency, by building on the foundation of a few assumed axiomatic components. In mathematics, an obvious example is Euclid's geometry. An example from science is Einstein's theory of Special Relativity; after postulating that two things are constant (physical laws in uniformly moving reference frames, and the observed speed of light), logical consistency — which Einstein explored with mental experiments — makes it necessary that some things (length, time, velocity, mass,...) will be relative while other things (proper time, rest mass,...) are invariant. A similar strategy was used in the subsequent invention of General Relativity when, with the help of his friend Marcel Grossmann who was an expert mathematician, Einstein combined his empirically based physical intuitions with the powerful mathematical techniques of multidimensional non-Euclidean geometry and tensor calculus that already had been developed in the 1800s by Bernhard Riemann and Gregorio Ricci, respectively.

Although empirical factors played a role in selecting the initial axioms for Einstein's theories, once these axioms were fixed each theory was developed using conceptual factors such as logical consistency. Responding to an empirical verification of General Relativity's predictions about the bending of light rays by gravity, although Einstein was elated he responded with confidence in his conceptual criteria, saying that the empirical support did not surprise him because his theory was "too beautiful to be false."

C. External Relationships

A variety of idea-generating strategies are possible.[13] For example, instead of focusing on one theory, as in the preceding subsections, perhaps new ideas can be inspired by redirecting the search outward, by studying the components and logical structure of other theories.

During the invention of one theory, a careful study of the components in other theories may result in the concrete theory-to-theory connection that occurs when a scientist borrows a useful component from an existing theory and incorporates this component into a new theory. In this way the shared components become generalized into a wider domain, and systematic connections between theories are established.

Or, instead of sharing components, the structure of an old theory can be retained while the components are changed. This occurs when the logical structure of a new theory is based on analogy to the structure of an old theory, with the new theory being constructed by making appropriate structural adaptations and substitutions of content.

Another possibility is mutual analysis-and-synthesis. Scientists can carefully compare the components of two theories, to gain a deeper understanding by seeing the characteristics of each theory more clearly, and to see how the two theories are related by an overlapping of components

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[13]. A more detailed description of how scientists use conceptual relationships (especially external relationships) is provided by Darden (1991, pp. 244-255), with idea-producing strategies that include: using analogy, invoking a theory type, using theory interrelations, and moving to another level of theory organization.

or logical structures. Perhaps this improved understanding will inspire a revision of one theory, with or without a borrowing or analogizing of components from the other theory. Or there may be a synthesis that combines ideas from the two theories into a unified theory that is more conceptually coherent and has a wider empirical scope.

Sometimes a knowledge of theories in other areas will lead to the recognition that an existing theory (from another area) can be extended, as-is or with appropriate modification, into a domain being studied by a scientist. Of course, this is selection rather than invention, but it still "brings something new" into theorizing in the domain. And the process of selection can be similar, both logically and psychologically, to the process of invention, especially if selection requires the flexible, open-minded perception of a connection between domains that previously were not seen as connected.

2.6: Experimental Design

In the ISM diagram, three arrows point toward **experimental design**, showing inputs from 'evaluation of theory' (since this often provides motivation and guidance for design), **gaps in system knowledge** (these gaps can be filled by designing experimental systems) and '**do thought experiments**...' (these are an efficient way to do the process of design). There is one outgoing arrow, showing that the result of experimental design is a 'real-world **experimental system**'. This outgoing arrow is duplicated at the right side of the diagram; here it again points toward 'real-world **experimental system**' (in the hypothetico-deductive box); when the design is put into action, scientists **do physical experiment with system**.

In ISM an 'experiment' is defined broadly to include *controlled experiments* and *field studies.* In a field study a scientist has little or no control over the naturally occurring phenomenon being studied (such as a dinosaur fossil or an earthquake) but there is some control over how to collect data (where to dig for fossils, and how to make observations and perform controlled experiments on the fossils that are found; or what type of seismographic equipment to use and where to place it, and what post-quake fieldwork to do) and how to analyze the data.

This section examines a variety of ways that the pursuit of scientific goals can motivate and guide the designing of experiments (in Section 2.61), the creation of new opportunities for research (in 2.62), and the use of thought-experiments (in 2.63).

2.61: Goal-Directed Experimental Design

A. Knowledge about Theories and Experimental Systems

Two types of relationships exist between theory and experiment. An experiment can affect a theory, as in empirical evaluation and retroductive selection or invention. Or the roles can reverse, with a theory influencing the selection or invention of experiments. The second type of relationship is discussed in this subsection.

Theory evaluation can provide essential input for experimental design; this influence is indicated by an arrow in the ISM diagram. When scientists evaluate a theory, they may discover three types of ‘trouble spots’ to investigate by experimentation. First, if there is anomaly, the goal of a new experiment may be to localize the source of anomaly, or to test options for theory revision. Second, if there is low predictive contrast, perhaps a newly designed ‘crucial experiment’ can discriminate between the competitive theories. Third, if there is a conceptual difficulty, this can inspire the design of an experiment to learn more about the problematic aspect of the theory.

When scientists design an experiment, they can be primarily interested in either a theory or the experimental system. Some logical relationships between a theory and system are described in Section 2.52_D, in a 3-variable view of retroduction. When a domain-theory (claimed to be valid for all systems in a domain) and a system-theory (for what is in a specific system) are assumed to be independent, there are two retroductive options. If scientists interpret the results of an experiment by assuming they know the system-theory, their inferences involve speculations about the domain-theory. But if they assume the domain-theory is known, their inferences are about the system-theory.

As described above, the purpose of this 3-variable perspective is to interpret the results of an experiment after it has been done. But it is also useful for understanding experimental design. In the same way that inference can involve either a domain-theory or system-theory, so can the goal for what can be learned from an experiment, which will affect how an experiment is designed. For example, scientists might assume that a domain-theory (about one property of a chemical system) is known, and based on this knowledge they design a series of experiments for the purpose of developing system-theories that characterize this property for a series of chemical systems. But there is a different goal when scientists use a familiar chemical system and assume they have an accurate system-theory (about a number of chemical properties that are well characterized due to the application of existing domain-theories) in order to design an experiment that will let them develop a new domain-theory about a chemical property that is not described by the old domain-theories.

Often, however, both types of knowledge increase during experimentation. Consider a situation where scientists assume a physiological domain-theory in biology, and use this theory to guide the design of a series of experiments with different species, in order to learn more about each species. While they are learning about these systems, they may also gain knowledge about the domain-theory; perhaps it needs to be revised for some species or for all species; or they may convince themselves about the truth of a claim (that the same unrevised theory can be generalized to fit all of the species being studied) that previously had been only an assumption.

B. Gathering Data in Early Stages of Development

The preceding subsection assumed that either a system-theory or domain-theory is known. But sometimes there is a knowledge gap that is both empirical and theoretical — with not enough empirical data about systems in a domain, and with no satisfactory theory to guide experimental design. In this situation, experiments can be done just to see what will happen, to gather additional observations about the domain, thus broadening the empirical database for interpretations and inventions in the future.

A state of “dually inadequate” knowledge is especially common in the early stages of developing a theory in an underexplored domain. For example, in the early 1800s atomic theory was still being developed, and chemists were also uncertain about the nature of their experimental systems, such as whether in the electrolysis experiment, “water → hydrogen + oxygen,” the hydrogen was H or H_2, the oxygen was O or O_2, and the water was HO or HO_2 or H_2O.

In a slightly later stage, a theory has been developed but its status is still being debated, and there are gaps in the knowledge of its application to systems in its claimed domain. In this stage, if experiments are done in an underexplored sector of the domain — and if the theory is thus found to be empirically adequate for a wider variety of systems within its claimed domain — this provides support for the theory. And, of course, scientists will learn more about systems in the underexplored sector.

C. Strategies and Principles for Experimental Design

Whether experimental design is or is not guided by pre-existing theory, it can be guided by logic. In any context, logical strategies for design, such as the systematic variation of parameters (individually or in combinations) to discover correlations or to establish the effects of various types of 'controls', can be useful for designing clusters of experiments to generate data that is especially informative. An example of such a strategy is 'Mill's Methods' for experimental inquiry. Complementary 'variations on a theme' experiments can be planned in advance, or improvised in response to feedback from previous experimental results. Well designed clusters of experiments can be useful in any context, whether the primary goal is to invent a new theory, test an existing theory, or learn more about the systems.

During induction a theory (either descriptive or explanatory) is generalized into an unexamined part of a domain. With inductive logic, part of the inference process is the generalizing of observations from a small sample to a larger population. In making this logical leap, besides statistical considerations such as sample size, the central question is whether the sample accurately represents the population as a whole. These basic principles — of statistical adequacy and sampling accuracy — apply whether the observations are made during field studies or controlled experiments.

In addition to these general principles, each area of science has its own principles for designing experiments. For example, in certain types of medical or social science experiments, there should be design features (as described by Borg & Gall, 1989) such as 'blind' observations and interpretations, and controls for psycho-physical ‘placebo effects’ and for motivational factors such as the Hawthorne Effect, John Henry Effect, and Pygmalion Effect.

D. Knowledge of Experimental Techniques

In addition to seeking an improved knowledge of theories and systems, scientists can search for an improved knowledge of experimental techniques. One example is the technique of x-ray diffraction, used for the purpose of determining the structure of molecules such as DNA. In the early days of x-ray experiments, the major goal was to determine the correlations between x-ray observations and molecular structure, with system variables such as x-ray wavelength, width and intensity of beam, angle of incidence, sample preparation and thickness, and type of detector. In other words, the goal was to learn more about the experimental technique.

In pursuing knowledge about a new technique, a powerful strategy is to design controlled cross-checking experiments in which the same system is probed with a known technique and a new technique, thus generating two data sets that can be compared in order to ‘calibrate’ the new technique. For example, if a familiar technique records numerical data of “40.0, 50.0, 60.0, 70.0, 80.0” for five states of a system, and a new technique measures these states as “54.4, 61.2, 67.1, 72.2, 76.8” we can infer that a “new 54.4” corresponds to an “old 40.0,” and so on.

A similar approach uses 3-variable logic retroductive logic. Consider the early days of x-ray experiments, when a system-theory (the 3-dimensional structure of molecules in a sample) was assumed to be known, based on previous experimenting and theorizing. Scientists studied the correlations between this structure, the observations, and the experimental variables, with the goal of developing a domain-theory for the x-ray technique. After this theory was developed, the strategy could be reversed. A domain-theory (for x-ray technique) was assumed to be known, and x-ray observations were used to infer a system-theory (about molecular structure). This latter strategy was employed by Watson and Crick in 1953, when x-ray observations helped them retroductively infer a structure for DNA.

E. Anomaly Resolution

If predictions do not agree with observations, two possible causes for the anomaly are an inadequate system-theory or domain-theory. In either case, maybe a new experiment can be designed with the goal of localizing the anomaly to a faulty theory-component, and further experiments can test options for revising or replacing this component.

A third possible cause of anomaly is misleading observations. For example, in an experimental system that includes an electrical circuit and a voltage meter used as an observation detector, an inaccurate meter might read 4.1 Volts when the actual voltage is 5.7 Volts. If the observation of "4.1 Volts" is assumed to be accurate, scientists may try to revise a domain-theory (or system-theory) for the circuit, even though it "doesn't need fixing." But if there are good reasons to suspect that the model for the circuit is accurate, scientists can turn their attention to "troubleshooting" the entire experimental system in an effort to find the cause of anomaly. After the faulty meter is discovered, and the system-theory is revised to include "a voltage meter that reads 28% low," the predictions of this revised model will match the observations. Or the faulty meter can be replaced by a meter that produces accurate observations.

Another type of anomaly occurs when scientists are surprised, not by a disagreement between current observations and deductive predictions, but by a difference between current observations and previous observations of similar (or apparently identical) experiments. The surprise arises due to a metaphysically based assumption of reproducibility — an expectation that the same experimental system should always produce the same results.

In searching for the source of anomaly, a scientist can check for statistical agreement (is a result outside the statistically expected range?), for errors (are the electrical devices plugged in?), and for more esoteric causes (is the anomaly a phenomenon that with further study may lead to a Nobel Prize, or at least a publication?). The logic used in anomaly resolution is similar to the methods used by a detective or by an expert troubleshooter such as an automechanic searching for trouble in an engine, or a physician trying to find what has gone wrong in a patient's body. Of course, the usual problem-solving strategy is to "find what is wrong and fix it." After the cause of anomaly is found, a scientist can redesign the experimental system, or fix it so it runs the way it was originally designed. Or maybe what actually happened is more interesting than what was planned, as in the unexpected occurrence of penicillin or Teflon, and the anomaly is an opportunity for serendipitous discovery.

F. Predictive Contrast and Crucial Experiments

When predictions and observations do not agree, there is anomaly. Sometimes, however, there is agreement, but with too many theories. If there is not enough 'predictive contrast' for a theory because in previous experiments an alternative theory predicted the same results, a sensible strategy is to design a more discriminating 'crucial experiment' whose outcome will lend clear support to one competitor or the other. To design a crucial experiment that will help distinguish between competitive theories, scientists can run thought-experiments to quickly check a variety of potential experimental systems, searching for contrasting predictions.

As an example of an initially inconclusive situation, with low predictive contrast, consider a liquid that conducts electricity well. One possible explanation is that the liquid is a solution of NaCl in water. With this theory there is good agreement — because theories of chemistry (involving NaCl, water, dissolving, ions, and conductivity) predict that a solution of NaCl in water will conduct electricity — but this retroductive inference is uncertain due to low predictive contrast, because for many other types of solutions (such as water with HCl, NaOH, or KBr; or methanol with NaCl) there is also a prediction of high conductivity. In an effort to eliminate alternative theories from consideration, a scientist could design other experiments, such as testing for acidity or basicity (to distinguish between neutral NaCl, acidic HCl, and basic NaOH), observing the flame color (to distinguish the Na of NaCl from the K of KBr), determining the density, flammability or odor (to distinguish between different solvents), and so on. These additional experiments would either support the NaCl/water theory or weaken it, but could not prove it true. In this example the scientist assumes the adequacy of domain-theories (involving ions,...), and is evaluating the status of alternative system-theories. In other situations, however, the status of one or more domain-theories could be the focus of evaluation.

G. Heuristic Experiments and Demonstration Experiments

Two types of experiments, differing in their objectives, are described by Grinnell (1992). Early in the exploration of a domain or a theory, to learn about the domain or to develop and test a theory, scientists design *heuristic experiments.* Following this initial exploratory stage of learning and self-persuasion, the goal can shift toward the design of impressive *demonstrative experiments* that will be useful in persuading others about a theory (for a domain or system) by clearly highlighting its strengths or weaknesses.

Whether there is an attempt to strengthen or weaken a theory depends on whether scientists think this theory is plausible-and-useful. It may also depend on whether scientists *hope* a theory will be judged plausible-and-useful; these hopes are due to cultural-personal factors, and occur when there is personal investment (practical, psychological, metaphysical, or ideological) in a theory's success or failure. Despite the philosophical claims made by Popper (1963), scientists rarely try to falsify a theory that is the utilitarian foundation of their work, or in which they have a personal investment, due partly to psychological and practical factors. For example, a scientist who has developed a theory usually hopes it will be supported by experimental data, not falsified. On the other hand, scientists who have less investment, or who hope this theory will be discredited, may actively try to falsify it.

For either type of experiment, heuristic or demonstrative, but especially for demonstration, a useful strategy is to think ahead to questions that will be raised during evaluation. These questions — about sample size and representativeness, random errors and systematic errors, adequacy of controls, predictive contrast with respect to alternative theories, consideration of all relevant factors, and so on — can be used to probe the current database of systems-and-observations, searching for gaps that should be filled by experimentation. In all phases of research, one should be creatively critical, but when searching for demonstration experiments (and arguments) it is wise to be brutally critical, to be at least as tough as one's critics will be. A useful strategy is to imagine how things will appear from the perspective of an outsider who has not been involved in the research, to imagine the toughest questions and challenges, and then to answer these questions.

Often a heuristic experiment that is informative will also be effective for demonstration. For example, a crucial experiment that can distinguish between plausible alternatives is useful in any context. But there can be significant differences in the motivation of scientists when they are designing an experiment; are they mainly interested in learning or persuading? For example, are scientists trying to design a crucial experiment mainly to convince themselves, or to convince others? Do they want to increase a sample size due to their own doubts, or because this will be more persuasive in a paper they plan to publish?

Usually, however, the two goals will produce different experiments. For example, will scientists run a novel experiment because they are curious about what will happen, or a familiar experiment whose outcome is predictable, after refining it to "clean up the loose ends" so it will become a more impressive demonstration of what they already know?

As knowledge increases and motivation changes, typically there is a shift in the way experimental design is influenced by theory. In an early heuristic phase, a theory and its evaluation may not provide much guidance, and the little that does occur may be relatively unfocused. But in a later demonstration phase, there is enough knowledge (of theory and domain) that the guidance can be more focused and precise.

H. Experiments in Problem-Solving Projects

In ISM a *scientific problem-solving project* is defined as an attempt to achieve a goal of improved knowledge about observations and interpretations of nature. An important aspect of experimental design is its function in the challenging task of formulating a good scientific problem — one that is original, significant, and capable of being solved in a reasonable amount of time with the available people, knowledge, equipment, materials and money — and solving this problem.

Effective problem formulation is customized to fit the expertise and resources of a particular research group. For example, if members of one group are expert at theorizing about a certain molecule, they may use a wide variety of experimental techniques (plus reading and listening) to gather information about their molecule. Another group, whose members have the expertise to do a difficult experimental technique, and who own the expensive machine required to do it, may search for a wide variety of molecules they can study with their technique.

During work on a research project, cultural-personal factors such as "psychological motives and practical concerns" play a role in the choice of which actions to pursue. These factors (and strategies for the optimal attainment of conflicting goals) can affect the balance between heuristic and demonstrative experiments:

> The more time that an investigator spends on demonstrative experiments, the less new information will be learned from heuristic experiments. If, however, the demonstrative experiments are not convincing enough, then the work may not be publishable. Finding an appropriate balance between these two goals is a recurrent difficulty for every investigator. (Grinnell, 1992, pp. 74-75)

So far, this discussion has not challenged an implicit assumption that the only way for a scientist to collect observations is to do an experiment. But one scientist can interpret what another observes. Sometimes an effective way to do research is to vicariously "design and do" experiments by reading (or hearing) about the work of others, for the purpose of gathering observations that can then be interpreted. This strategy won a Nobel Prize for James Watson and Francis Crick. They never did any productive DNA experimental work themselves, but they did gather useful observations from other scientists: x-ray diffraction photographs (from Rosalind Franklin), data about DNA's water content (also from Franklin), data about the ratios of base pairs (from Erwin Chargaff), and information about the chemistry and structure of DNA bases (from Jerry Donohue). Then they interpreted this information by analyzing it with thought-experiments and by playing with physical models for the atoms and molecules that form DNA, and they retroductively invented a theory for DNA structure. Even though they did no experimental design, a similar function was performed by their decisions about gathering, and paying close attention to, certain types of observations.

2.62: Taking Advantage of Opportunities

Often, new opportunities for scientific activity — in both experimenting and theorizing — emerge from a change in the status quo. A newly invented theory can stimulate experiments with a variety of goals: to test the theory and, when necessary, to revise and develop it; to explore its application for a variety of systems within (and perhaps beyond) its claimed domain; to calculate the value of physical constants in the theory, such as the G in Newton's gravity force, GMm/r^2; or to illuminate the relationships between this new theory and the older theories that previously had been used to describe systems in the domain of the new theory. Similarly, when an old theory is inductively generalized into a new domain, this can inspire experiments to explore the ways in which the domain can be understood more completely by using the theory.

New experimental systems can be produced by new events (the eruption of a volcano, the development of an ozone hole,...), by new discoveries (of old geological formations, old dinosaur bones,...), or by the discovery of a new type of phenomenon (such as radioactivity in 1896, or quasars in 1960). The new experiments can include field studies of the natural phenomena, and controlled experiments such as the labwork that is done to study dinosaur bones.

Sometimes new instrumentation technologies or observation techniques open the door to opportunities for designing new types of experimental systems. When this occurs the scientists' focus of interest can be on learning more about an existing theory or domain by using the new experimental tool, or on learning more about the tool. For example, controlled cross-checking experiments can be used to interpret and calibrate the new technique. Scientists can design their own instruments, or they can use technology that was developed mainly for other purposes, or they can provide motivation for the development of new observational technologies by making known their wishlist along with a promise that a market will exist for the new technologies. Or old technologies can be used in a new way, such as setting up the Hubble Telescope on a satellite in outer space, above the optically distorting atmosphere of the earth.

Because of the interactions between different elements of science — including instrumentation, experimenting, theorizing, and defining domains — a change in one element will affect other elements, producing a ‘wave’ effect. Thus, the ultimate effects of a change may extend far beyond the small area that is directly affected by the change.

When an area of science opens up due to any of the changes described above, for awhile this area is highly fertile and the possibilities for research are numerous. For example, Humphrey Davy, by effectively using the newly developed experimental technique of electrolysis, discovered 7 elements in 1807 and 1808. Of course, in science (as in the rest of life) it helps to be lucky, to be in the right place at the right time, but to take advantage of opportunity a person must be prepared. As Louis Pasteur was fond of saying, "Chance favors the prepared mind." Many other scientists were working in the early 1800s, yet it was Davy who had the most success in using the new technique for discovery.

To creatively take advantage of a temporary window of opportunity, an open-minded awareness (to perceive the possibilities) and speed (to pursue possibilities before they vanish due to the work of other scientists) are often essential, and sometimes a scientist must be willing to take risks.

2.63: Thought Experiments

A. Thought Experiments and Physical Experiments

In the ISM diagram, one input for experimental design is **thought-experiments** that are done "with theory-based models for a variety of potential experimental systems." Mental experiments, done to quickly explore a wide variety of experimental possibilities ranging from conventional techniques to daring innovations, can help scientists decide which experimental systems are worthy of further pursuit. As an essential part of a cost-effective strategy, mental experiments serve as a quick-and-cheap preliminary screening process that can facilitate the effective designing and selection of physical experiments that typically require much larger investments of time and money.

Usually, thought-experiments are a prelude to physical experiments. But occasionally thought-experiments are done for their own sake, to probe the implications of a theory by deductively exploring systems that may be difficult or impossible to attain physically. One famous example is the use of imaginary ‘speed of light’ rocket ships by Einstein during his development of relativity theory. In these mental experiments, instead of comparing predictions with empirical observations, as in a physical experiment, there is an exploration of theoretical empirical possibilities.

B. Four Types of Thought-Experiments

Thought-experiments play a key role in three parts of ISM: for hypothetico-deductive logic, retroductive logic, and experimental design; in addition, as discussed above, there can be a deductive exploration of exotic systems. In each context a prediction is generated from a theory-based model by using deductive logic, but there are essential differences in constraints, timing, goals, and assumptions about what is known.

In a hypothetico-deductive context the mental experiments are tightly constrained, being done with one experimental system and one theory. A theory is selected before the experiment is done. Technically, all *pre*dictions should be made before an experiment is done, but in reality a prediction is often made after the observations are known, and usually this is considered acceptable unless there are reasons to suspect ad hoc adjustments. The usual goal is to evaluate the theory. It is assumed that the theory and system are known; these are used to construct a model and make predictions.

In a retroductive context there is still one experimental system, but now a theory is not proposed (by selection or invention) until *after* the experiment has been done and the observations are known. There is a *divergent search* for theories, with many being "tried out" one at a time, but the *convergent goal* is to find a model whose predictions match the known observations. The observations are known; and if it is assumed that one theory (either the domain-theory or system-theory) is known, then the third variable (which is the other theory) can be inferred, with a cautious "if..., then maybe..." conclusion.

In the context of experimental design, scientists usually use thought-experiments to explore the experimental options that are available with one theory. But it is possible to do thought-experiments with two or more theories for the same system, comparing predictions in an effort to design a crucial experiment. In any case, the *divergent goal* is to *divergently search* for experimental systems that might in some way be interesting or useful. In deciding what might be interesting or useful, cultural-personal and conceptual factors play a major role. Each thought-experiment is constrained by the assumption of a specified theory and system, but the nature of these constraints will vary as different possibilities are examined. The main objective of these thought-experiments is to function as a quick, cheap way to facilitate the design of physical experiments that require a larger investment of time and money.

Even if there is no intention of doing the corresponding physical experiment, sometimes thought-experiments are done for their own sake, for the sole purpose of mentally exploring the characteristics of an experimental system or the logical implications of a theory. In these mental experiments there are no physical constraints, so the only limits are those imposed by the imagination. And there are no practical constraints, except for the time invested in designing and running the mental experiments.

2.7: Problem Solving, Thought Styles, and Thinking

Three important aspects of science — problem-solving projects, thought styles, and mental operations — are described in the upper-left corner of the ISM diagram. These three topics are discussed in Sections 2.71-2.73.

2.71: Problem Solving in Science

A. Problems

The activities of science, described in ISM, occur in the context of an effort to solve scientific problems. The common meaning of ‘problem’ implies that something is wrong and it needs fixing. Consistent with this meaning, a general definition of *problem solving* is “converting an *actual current state* into a *desired future state*” or, in abbreviated form, “converting a *NOW-state* into a *GOAL-state.*” For example, an everyday experience of problem solving occurs when a now-state (with a car that won't run) is converted into a more desirable goal-state (with the car running well).

The goal of science is knowledge about nature, and the goal of scientific research is to improve this knowledge, so *scientific problem solving* is an attempt to progress from a current NOW-state of knowledge (consisting of observations about nature, and interpretations of nature) to an improved GOAL-state of knowledge in the future.

Critical evaluation of a current state of knowledge (regarding a selected domain, phenomenon, or theory) may lead to the recognition of a weakness in this knowledge structure, and to a characterization, in the imagination, of a potential future state with improved knowledge. Nickles (1981) suggests that a problem is defined by specifying a set of *constraints* on its solution — i.e., by specifying the characteristics of a goal-state that would be considered a satisfactory solution.

The gap between the perceived now-state and a proposed goal-state (or a class of goal-states) defines a *science problem.*

B. Problem-Solving Actions

In an effort to achieve a solution for a problem, scientists invent, evaluate, and execute research 'actions'. An action can be any of the major activities in ISM — theory selection or invention, theory evaluation, thought-experiments, experimental design, or doing experiments. These actions can be grouped in two basic categories: *observation* (design and do experiments or field studies, make observations, or learn the observations of others) and *interpretation* (organize data to facilitate pattern recognition, analyze and synthesize, use algorithms and heuristics, select or invent theories, evaluate theories, or review the interpretations of others).

C. Problem-Solving Projects

When scientists decide to actively pursue a solution to a science problem, the goal of solving this problem becomes the focal point for a *research project.* The movement from problem to project requires evaluation and a decision. Just because a problem can be formulated, this does not necessarily mean that its solution should be pursued. Members of a research group must evaluate the potential benefits of a problem-solving project, compared with other alternatives, and ask "Why should we do this?" in order to persuade themselves, either yes or no, whether it is likely to be a wise investment of their time.

In deciding whether a problem solution should be pursued, an important consideration is the existence of actions that may lead to a solution. In other words, is there hope? Are there good reasons to expect that a solution can be constructed? A decision about whether a problem can be solved may or may not be preceded by the planning of specific actions. Sometimes a project begins with an assumption that a problem is solvable, combined with a decision to solve the problem, and later there is a detailed planning of actions. But in other situations a preliminary planning of actions precedes a decision to commit resources to the active pursuit of a solution. In this latter case, a decision to pursue a solution will depend on the quality of the plans for action, and an estimate of the likelihood for success. Because the amounts of preliminary action-planning vary from one project to another, it can be useful to define a project as either "a problem plus a decision to pursue a solution," or as "a problem and the problem-solving actions that are planned, plus a decision to pursue this plan of action."

With either perspective, my definition of a problem differs from that of Nickles (1981, p. 109) who states that "a problem consists of all the conditions or constraints on the solution plus the demand that the solution...be found." Nickles' definition of a problem (constraints plus demand) corresponds to my definition of a project. I think it is useful to separate a problem from a corresponding project because this makes it easier to discuss the actual practice of science, where problems can be formulated even if their solution is not actively pursued. And by making a distinction between problem and project, the definition of a problem is closer to its common meaning, which makes it easier to discuss 'problems' with a wide range of people, with straightforward simplicity and without being misunderstood.

A problem-solving view of research can be useful even though scientists do not always think of their project as an effort to solve a problem. Some scientists view a project as asking a question, and then trying to answer it. Or a project can be viewed as an effort to achieve an objective. Although it is possible to view a research project in different ways — such as the pursuit of a solution, answer, or objective — the following discussion, for reasons of logical and linguistic simplicity, will be framed in terms of problems and solutions.

D. Action Evaluation

Action evaluation, oriented toward seeking a solution, serves as a guidance system for the planning of research actions. Effective action evaluation is guided by an awareness of the problem, because the problem's goal-state can serve as an aiming point to orient the search for a solution. The evaluator tries to understand the constantly changing now-state so this can be compared with the goal-state, in order to search for 'problem gaps' (i.e., specific ways in which the now-state and goal-state differ) that can guide the planning of actions designed to close these gaps.

One important action is action evaluation, done by pausing to get oriented and to think carefully about what should be done next, or maybe to construct a long-term plan for actions in the future. In many ways, action evaluation is similar to theory evaluation. For both processes, there is input from multiple factors, competition between alternatives, and selection or invention. One major difference is that action evaluation usually results in action, but theory evaluation does not.

The first stage of action evaluation, which compares the current now-state with the goal-state, can be viewed as an evaluation of potential solutions. As the project continues, usually the now-state becomes more similar to the goal-state, and eventually scientists may achieve a solution that can be evaluated as satisfactory, based on criteria defined by the problem constraints. Or they may decide that the project should be abandoned, at least temporarily, because progress toward a solution is slow, or because despite satisfactory progress there is a decision that working on another project is likely to be even more productive.

E. Private Evaluation and Public Evaluation

Evaluation (of an action or a theory) can be private or public. In *private evaluation,* the goal of scientists is to convince themselves (as individuals or as members of a cooperative research group) about an evaluation conclusion for an action or theory. *Public evaluation* is similar, but now the main goal is to convince others, outside the research group, about an evaluation. With externally-oriented persuasion regarding action evaluation, the goal might be to convince others that a proposed research project is worthy of support. With externally-oriented persuasion regarding a theory, the goal could be a favorable evaluation, by others, of the extent to which a proposed theory is worthy of either *acceptance* (as plausible, useful scientific knowledge) or *pursuit* (to investigate by further research).

F. Preparation

Before and during their formulation of a problem, scientists prepare by learning the current now-state of knowledge about a selected area of nature, including theories and experimental techniques. Early in the career of a scientist, as a student, typically most of this preparation comes from ‘direct learning’ by reading books and listening to teachers, with supplementation by 'discovery learning' that involves first-hand experience in observation and interpretation. Later, when a scientist is actively involved in research, typically there is a shift toward an increased reliance on the discovery learning that occurs during research, but some direct learning still occurs by reading and listening. When a scientist becomes more intellectually mature, less knowledge is accepted solely due to a trust in authority, because there is an increase in the ability and willingness to think critically. In both types of learning, direct and discovery, there is an active construction of concepts in the mind of the learner.

As suggested by Salomon & Perkins (1989), the use of knowledge can be viewed from two perspectives: backward-reaching and forward-reaching. Scientists can reach backward in time, to use at the present time what they have learned from their past experiences in reading, listening, and researching. Or scientists can focus on learning from their current experiences, because they are looking forward to potential uses of this knowledge in the future.

G. Levels of Problem Solving

The connections between projects, and between activities within one project, can be viewed in the larger context of all scientific research. This context is complex, with many groups working on a wide range of interconnected projects over long periods of time. As a way to cope with this complexity, it can be useful to think in terms of a simplified model with different levels of problems and problem-solving activity. In this model, during scientific research a *mega-problem* (the attempt by science to understand all of nature) is narrowed to a *problem* (of trying to answer specific questions about one area of nature) and then to *sub-problems* that involve the planning and execution of problem-solving *actions.*

H. A 3Ps Model of Science

A *3Ps* model of science (Peterson & Jungck, 1988) interprets scientific problem solving in terms of *posing, probing,* and *persuasion.* A brief summary of the 3Ps is that scientists pose a problem, then probe the problem in an effort to solve it, and try to persuade themselves and others that their solution is satisfactory. This simple model, which portrays the overall flow of scientific research, was initially proposed for the main purpose of influencing science education. In this role it has stimulated a great deal of productive thinking about science and science education, thereby attracting many enthusiastic advocates, including myself. Some educational implications of the 3Ps model will be discussed in Sections 3.12_B and 3.44. Although the basic model is outlined in the initial '3Ps' paper (Peterson & Jungck, 1988), the nature of each P is not precisely defined, nor is the distinction between the Ps. In my own work, I have tried to more clearly characterize and distinguish the 3Ps, to explore their functioning in the process of science, and to examine (and often construct) the relationships between the 3Ps and ISM.

Although the ideas in my most recent problem-solving model (in Subsections A-G) have come mainly from other sources,[14] this model is designed to be compatible with the 3Ps model and its terminology. For example, 'problem formulation' (in ISM) is posing (in 3Ps), 'pursuit of a problem solution' is probing, and 'private evaluation and public evaluation' is persuasion. But 'preparation' has no counterpart in 3Ps. By comparing the length of terms, it is easy to see one advantage of a 3Ps terminology. Another advantage is intrinsic clarity; the intended meaning is accurately expressed in the common meaning for each 3Ps term. In the remainder of this dissertation the ISM model for problem solving will usually be expressed using a 4Ps terminology with four P-terms: prepare, pose, probe, and persuade.

I. A Basic Theme with Variations

A complete understanding of scientific problem solving would require an integrated analysis of all relationships within individual projects and across families of related projects, for groups in all scientific fields, for all times. This monumental mega-problem will not be pursued in my dissertation, but the remainder of Section 2.71 will sketch an outline of the mega-problem. This

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[14]. The ISM model of problem solving comes mainly from my own thinking, beginning with ideas in a "Strategies for Problem Solving" booklet (Rusbult, 1978) that were stimulated and nourished by concepts from a variety of sources, including books on study skills (Pauk, 1974; Voeks, 1973), creativity (McKim, 1972), and problem solving (Adams, 1974). My ideas continued to develop in the 1980s and 1990s, stimulated and guided by my experience as a learner and teacher, and have been expressed in numerous handouts written for my students, and in "Physics: Tools for Problem Solving" (Rusbult, 1989).

sketch begins, in the following paragraph, with a simple description of a simple project, in terms of concepts from Subsections A-G, using 4Ps terminology.

Preliminary preparation (to learn about now-states of knowledge) leads to posing a problem (by selecting an area to study, finding a weakness in the now-state, and defining the constraints on acceptable goal-states). After deciding what to study, scientists can think about how to study it and/or whether to study it. If scientists decide to pursue a solution for a problem, this becomes the focus for a project. In an effort to solve the problem, scientists invent, evaluate, and execute probing activities (to observe and interpret). Probing often involves recurring cycles of observation-and-interpretation: interpretations (of previous observations) are used to design experiments which produce observations that are used in further interpretation, and the cycle begins again; during each cycle there can be an increase in knowledge for both observations and interpretations, as well as a preparation for future cycles. Probing actions, motivated and guided by an awareness of the problem and the gap between a constantly changing now-state and a goal-state of improved knowledge, are done to reduce the gap until a satisfactory solution has been achieved. At all stages of a project there is internally-oriented persuasion about the state of knowledge and "what to do next"; eventually there can be externally-oriented persuasion to convince others about the value of a problem solution.

This is a simple version. The next two sections examine two ways in which a problem becomes more complicated: by overlaps in time, and overlaps of levels.

J. Interactions between Stages and Activities

When analyzing a research project, the 4Ps can be viewed as 4 activities or 4 stages. The interactions between activities and stages, with each type of activity occurring during each stage, are described in detail by Rusbult (1994), and are outlined below.

Persuading activity begins in the posing stage. If problem constraints are chosen so they conform to the evaluation criteria of the dominant scientific community, a solution that satisfies these constraints is more likely to be accepted by other scientists. And if a research group, as a result of action evaluation, persuades themselves that a problem solution should be pursued, they may try to persuade others, such as a grant-funding agency, that the project is worthy of support.

And persuasion during the final persuading stage of a current project can affect the posing stage of projects in the future, which are more likely to be supported if a current project, and its people and their problem-solving approach, are considered successful.

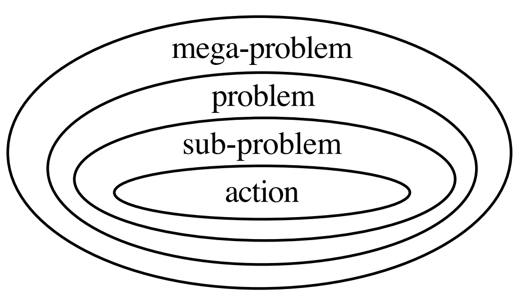
The posing activity for a future project can begin during any stage of a current project, whenever there is an idea for a 'spinoff' project based on a new problem. Similarly, during the probing stage there can be plans for immediate actions to probe the current problem, or delayed actions to probe a different problem in the future.

During the posing stage, often there is some preliminary planning of actions to solve the problem. Later, during the probing stage these plans are modified and supplemented by improvised planning, done in response to the constantly changing now-state. Finally, during the persuading stage, after a solution apparently has been achieved, there should be a rigorous self-critical evaluation of one's own arguments in support of the proposed solution; this close scrutiny often leads to a recognition of gaps in support, and to the planning of additional probing activities for observation or interpretation.

K. Interactions between Levels of Problem Solving

The earlier description of "Levels of Problem Solving," with a mega-problem being narrowed to a problem, sub-problem, and action, can be symbolized visually:

Figure 6: Relationships between Levels of Problem Solving



This diagram shows four levels, but a greater number of levels can exist. At every level, scientific research involves setting goals (for a problem) and pursuing these goals (for a solution).

Sometimes the roles of goal-setting and goal-pursuing overlap. For example, viewed from the perspective of the ‘problem’, a whole ‘sub-problem’ (including its formulation and solution) is an action that contributes to solving the ‘problem’. But a ‘sub-problem’ can also be viewed as a problem that is being formulated and solved by doing ‘actions’. Thus, a sub-problem can function as either an action or a problem. These two relationships are depicted visually, above; the ‘sub-problem oval’ is contained within a ‘problem oval’, and it contains an ‘action oval’. This lack of consistency in terminology (and function) is not a logical difficulty for a multi-level model of research, because the role in a relationship — and thus the term that describes the role — varies with context. But if the relational context of an action is clearly defined, the role of the action (and the term used to describe it) is also clearly defined. A familiar analogy is that a woman can correctly be called a daughter, sister, mother, cousin, niece, or aunt, depending on the relational context being considered.

Many types of interactions occur. There are relationships within a level, such as a need to coordinate all sub-problems that are part of solving the same problem. At any given time a typical research group will be working on several problems and sub-problems; due to connections between these problems, an action may contribute to solving more than one problem. There are sequential connections between successive projects; work on a current project may inspire ideas for a future project, while results from an earlier project are being written-and-revised for publication. By developing variations on a research theme, a ‘family’ of related projects can be produced.

Some of the most important interactions involve knowledge. During a current project, scientists can search backward for what they have learned from their past work, or forward to how they may be able to use what is being learned now, or ‘sideways’ for possibilities of sharing knowledge among concurrent research projects. Newly learned knowledge that is used in several contexts can include observations, experimental techniques, or knowledge involving the interpretation of systems or theories. Learning that occurs during research will help the group that does the research, in their current and future projects. And if a group's work is published, their experience can help other scientists learn.

2.72: Thought Styles

All activities of science operate in the context of culturally influenced 'thought styles'. This section describes what thought styles are, and how they affect the process and content of science.

A: Definitions

As described by Grinnell (1992), a cell biologist with an insider's view of science, a scientist's *thought style* involves a system of concepts, developed from prior experience, about nature and research science. In a community, many components of individual thought styles are shared:

> Every group is characterized by certain accepted attitudes regarding the way in which the activities of the group should be carried out. ... These attitudes constitute the operating 'paradigm' of the group. ... Beliefs and goals held in common by individual investigators...[constitute a] shared knowledge that includes assumptions about various aspects of science including methodological approaches, observations, accepted hypotheses, and important problems requiring further investigation. These shared assumptions and beliefs make up the prevailing thought style of the collective, and they include a definition of what it means to do research. (Grinnell, 1992, pp. 47-48)

A collective 'thought style' refers to scientists' ideas about nature and about the observation-and-interpretation procedures of science. These ideas are related, in some ways, to the social and institutional structures within which they develop and operate. But even though many ideas are shared in a scientific community, some aspects of a thought style vary from one individual to another. And each person is involved with many groups (at different levels) that interact with each other in the complex social/institutional structure of science. If there was an effort to consider all of the relevant factors and interactions in detail, the possibilities for description and analysis would become overwhelming. Therefore, to avoid becoming lost in details, the following sections will focus on a broad overview of thought styles, and the ways in which they influence the activities of science.

B. Effects on Experiments and Theories, Goals and Procedural Styles

Thought styles affect the process and content of science. All activities in science occur in an operating context that includes culturally influenced thought styles at the levels of individuals and communities, involves both conscious choices and unconscious assumptions, and produces effects that span a wide range from the artistic taste that defines a theory's 'elegance' to the hard-nosed pragmatism of deciding whether a project to develop a theory or explore a domain is worth the resources it would require. A thought style will influence — and when viewed from another perspective, is comprised by — the problem-posing and problem-solving strategies of individuals and groups.

The thought style of a scientific community will tend to favor the design of certain types of observation and interpretation. Guided by their thought styles for example, scientists may prefer either controlled experiments or field studies, and data collection that is qualitative or quantitative; they may choose to use (or avoid) particular techniques for data analysis; and they will develop expectations for the logical connections between experimenting and theorizing.

An intellectual environment will favor the invention, pursuit and acceptance of certain types of theories. Some of this influence arises from the design of experiments, which determines what is studied and how, and the types of data thus collected. Another mechanism for influence is the construction and selection of criteria for theory evaluation. For example, thought styles can exert a strong influence on conceptual factors, such as preferences for the types of components used in theories, the balance between completeness and simplicity, the value of unified wide-scope theories, and the importance of (and definitions of) scientific utility in promoting cognition and research.

Thought styles (and thus experiments and theories,...) will influence, and will be influenced by, the goals of science — such as whether the main goal of research projects should be to improve the state of observations or interpretations, whether the main function of theories should be empirical description or mechanistic explanation, and whether science should focus on understanding nature or controlling nature — and the choice of ontological and metaphysical assumptions, such as the current consensus to prohibit the postulation of unobservable 'supernatural entities' in scientific theories about what occurs in nature.

And there will be mutual influences between thought styles and the procedural 'rules of the game' that are developed by a community of scientists to establish and maintain certain types of institutions and reward systems, styles of presentation and argumentation, preferences for project proposals with comprehensive "know every step in advance" preliminary planning versus casual "steer as you go" improvisational serendipity, systems for coordinating the activities of different scientists and groups, attitudes toward competition and cooperation and how to combine them effectively, and relationships between science, technology and society. For the everyday practice of science, an especially important factor is the relationships between scientists working in a research group. Later, in Section 3.47_B there is a detailed description of a very effective working environment; a brief summary is that "the individual abilities complemented each other, providing a cohesiveness...that allowed the work to progress with astounding rapidity. (Allen, 1978)"

One type of procedural strategy involves decisions about "who does what" during research. Although it is possible for one scientist to do all the activities in ISM, this is not necessary because scientists usually work cooperatively in each research group (with people working on different parts of a problem) and in a field as a whole (with groups working on different parts of the overall mega-problem). With a 'division of labor', individuals or groups can specialize in certain types of activities. One division is between experimentalists who generate observations, and theorists who focus on interpretation. But most scientists do some of both, with the balance depending on the requirements of a particular research project and on the abilities and preferences of colleagues. For example, in one research group a scientist may be the best available experimentalist so she does this work, while in another group (or in the same group but working on a different project) she would do less experimental design and would be more involved with interpretation in the role of a theorist.

The metaphor of *conceptual ecology* (Toulmin, 1972) offers an interesting perspective on the effects of thought styles, based on analogy between biological and conceptual environments. In much the same way that the environmental characteristics of an ecological niche will affect the natural selection that occurs within its bounds, the intellectual characteristics of individuals — and of the dominant thought styles in the communities they establish and within which they operate — will favor the development and maintenance of certain types of ideas (about theories, experiments, goals, procedures,...) rather than others.

A thought style will tend to favor the production of certain types of observation-and-interpretation knowledge rather than other types. This influence may be difficult to perceive because the ideas in a thought style are often unconsciously assumed as "the way things are done" rather than being explicitly stated. But these ideas exist nevertheless, and they affect the process and content of science.

C. Two Metaphors: a Puzzle and a Filter

A useful perspective on science is provided by Bauer (1992) in a book, *The Myth of the Scientific Method,* that challenges the misconception of an objective, algorithmic method. Instead, Bauer thinks the process of science is more accurately described by two metaphors: a puzzle and a filter.

To explain why "modern science began when cooperation among scientists became widespread and systematic (p. 43)," Bauer compares science to solving a *puzzle.* In this metaphor (from Polanyi, 1962) scientists are assembling a jigsaw puzzle of knowledge about nature, with the semi-finished puzzle out in the open for all to see. When one scientist fits a piece into the puzzle, or modifies a piece already in place, others respond to this change by thinking about the next step that then becomes possible. In this way the independent activities of many scientists are coordinated so they blend together and form a structured whole: "a series of independent initiatives are organized to a joint achievement by mutually adjusting themselves at every successive stage to the situation created by all the others who are acting likewise. (Polanyi, 1962)"

This portrait of science is supplemented by the metaphor of a *filter,* to describe the process in which semi-reliable work of scientists on the frontiers of research, which Bauer describes in a way reminiscent of the "anything goes" anti-method anarchy of Feyerabend (1975), is refined into the generally reliable body of knowledge that appears in textbooks. In science, filtering occurs in a perpetual process of self-correction, as individual inadequacies and errors are filtered through the sieve of public accountability by collaborators and colleagues, journal editors and referees, and by the community of scientists who read journal articles, listen to conference presentations, and evaluate what they read and hear. During this process it is probable, but not guaranteed, that much of the effect of biased self-interest by one individual or group will be offset by the actions of other groups. The overall result of filtering is that ‘textbook knowledge’ is generally more reliable than ‘research knowledge’ at the frontiers, and that the objectivity of science as a whole is greater than the objectivity of its individual participants.

Bauer claims that two benefits of the filtering process are a reduction in the effects of bias by individual scientists, and a protection against pseudoscience. But a byproduct of filtering, not directly acknowledged by Bauer, is that the collective evaluations of a scientific community introduce a 'community bias' into the process and content of science. The filtering process serves to reduce individual bias while it introduces a bias toward the dominant thought styles of the community.

The puzzle and filter metaphors seem to offer alternative ways to visualize posing and persuading, respectively. • Posing: Individual scientists and research groups, as they see what other scientists are doing with the 'puzzle' of scientific knowledge, watch for gaps that need to be filled, and they make posing decisions about their research, about how to narrow a mega-problem (understanding all of nature) into a smaller problem where an investment of their time and resources is likely to be productive. • Persuasion: A 'filter' can be a useful way to view the overall process of scientific persuasion, including its institutional procedures. The result of filtering is knowledge that can be used for preparation. Research specialists in a field will want to learn from the minimally filtered 'frontier knowledge' in their own field, while scientists in related fields may wait for a later stage (such as review papers that interpret and summarize the research in a field), and students will learn mainly the knowledge that has survived the filtering process long enough to be included in textbooks.

D. Problem Posing

Problem posing requires selecting an area to study, forming perceptions about the current state of knowledge in this area, and defining a desired state of knowledge in the future. The thought style of a scientific community will affect all of these actions — selecting an area, perceiving the current situation, and defining a desirable goal — which determine the questions that are asked, and influence the answers that are constructed.

Problem posing is important within science, and it plays a key role in the mutual interactions between science and society by influencing both of the main ways that science affects culture.

First, posing affects the investment of societal resources (people, time, and money) and the returns (in medical-technological applications and improved knowledge) that may arise from these investments. Second, the questions asked by science, and the constraints on how these questions are answered, will help to shape cultural worldviews, concepts, and thinking patterns.

E. Conflicts in Problem Posing

The basic goal of science, to improve our knowledge of nature, is simple. But the process of pursuing this goal is complex. The possibilities for pursuit are essentially open-ended, and numerous options are available. But due to limited resources, many options must be eliminated; therefore, posing requires some tough decisions.

Posing decisions are influenced by cultural-personal factors (psychological, practical, metaphysical, ideological, authoritative) that operate partly through thought styles. Decisions involve values, and depend on criteria such as weighing the relative value of short-term and long-term rewards, or comparing expected outcomes that are small-but-probable versus large-yet-unlikely, in deciding between ‘applied’ and ‘pure’ research, or in deciding between projects within each of these broad categories. Evaluators of posing must ask “What knowledge is best for society?” and then, based on this answer, specific questions such as “Which research is best for improving this knowledge?” or (more cynically), “Which research will tend to produce answers that we consider beneficial for society?” And, to add another perspective, in many biological and medical experiments there is a cost in suffering and death for animals, and we can ask whether this cost is worth the potential benefits for humans.

Decisions about posing can be made locally by an individual or a research group. Or they may involve larger institutions and many people: decisions by grant reviewers are obviously important, and their effect is direct; less direct yet still important are the actions of taxpayers, voters, and the politicians and administrators who determine the allocation of resources in government, education, and industry; votes are also cast by purchasers of products that are developed by science-based technology; and journal editors and manuscript reviewers exert influence by setting trends for what is considered important research.

During the decision-making process that determines what will be done and who will be supported, there are conflicts between science and other segments of society, and between different groups within science. Often there is a zero-sum competition, with one losing what another gains. For example, a university must decide how to allocate its limited resources to different departments, both scientific and nonscientific. Different groups compete for government funding. And in a corporation there may be arguments between the marketing department and the scientists (and engineers) regarding how much money should be spent for advertising and for research-and-development. In addition to a competition for money, different fields (and groups within each field) compete for recruiting the most talented students and colleagues.

Conflicts can be caused by differences in values, in estimates of expected outcomes, or in who will reap the benefits. Each individual and group will have opinions about what types of science, and which specific projects, are most worthy of pursuit. Motivations for posing a project (or supporting it) span a wide range, from intellectual curiosity to societal improvement to career advancement. Typically, one criterion involves asking which projects are most likely to result in personal gain, and all interested parties, including scientists, will try to steer the trends of science in the direction of their own expertise and investments, where they will be most highly rewarded. In addition to this self-interest factor, many scientists are motivated by a sincere conviction that the area they have chosen to study is the most interesting and important part of science, the epitome of science at its best, and should therefore be supported.

Conflicts can occur between fields or within a field. In either case there will be disagreements about problem posing — about which areas are most worthy of study, and what questions should be asked, and about the characteristics of desirable goal-states. Many different problems are possible. And even after a problem has been posed, there may be many approaches to bridging the gap between the now-state and goal-state; thus, many different projects can be proposed in an effort to solve the same problem. One group may focus on interpreting what already is known (about a domain, phenomenon, or theory) from previous experiments, another group wants to design a new experiment to gather new observations, while a third and fourth group decide to probe the problem with a different interpretive approach and a different type of experiment, respectively.

A fascinating example of an intra-field dispute, with radically different approaches to solving the same problem, occurred in the 1960s and 1970s when the development of a theory for oxidative phosphorylation was being vigorously pursued. During this period there were three major competitive theories: chemical intermediates, chemiosmosis, and energy transduction. Advocates of each theory built their own communities, each with its base of support from colleagues and institutions, and each with its own 'thought style' with differing assumptions and preferences regarding theories, experimental techniques, and criteria for empirical and conceptual evaluation. All aspects of science — including posing with its crucial question of which projects were most worthy of support — were hotly debated due to the conflicting perspectives and the corresponding differences in self-interest and in evaluations about the plausibility and utility of each theory.

F. Preparation, Probing, and Persuasion

The two preceding subsections discussed one of the 4Ps. But what about the other Ps?

Preparation. There are mutual influences between thought styles and two ways to learn. First, the formal education of those who will become scientists in the future is affected by the thought styles of current scientists and educators; in this way, current science education helps to shape scientific thought styles in the future. Second, another type of preparation — what current scientists learn from their research experience, to use in future research — will depend on their styles of thought.

Probing. Both basic types of probing activities, observation and interpretation, will be heavily influenced by thought styles, as described earlier.

Persuasion. For effective persuasion, arguments should be framed in the structure of current knowledge (because an important factor in persuasion is whether ideas are explained in a way that makes sense to the readers or listeners), with an acceptable style of presentation, in a way that will be convincing when judged by the standards of the evaluators, by carefully considering all factors — empirical, conceptual, and cultural-personal — that may influence the evaluation process at the levels of individuals and communities. Doing all of these things skillfully requires a good working knowledge of the thought styles in a scientific culture.

G: Variety

Thought styles affect the process of science in many ways, but this influence varies because thought styles vary from one field of science to another. For example, the methodology of chemistry emphasizes controlled experiments, while geology and astronomy (or paleontology,...) depend mainly on observations from field studies. Another example is that experiments in medicine or social science, which typically use a relatively small number of subjects, must be interpreted using a sophisticated analysis of sampling and statistics, by contrast with the statistical simplicity of chemistry experiments that involve a huge number of molecules. Even within the same field, styles of science can vary with geographical region and culture, and from one research group to another; and within each group there will be differences between individuals.

These variations could be caused by a variety of contributing factors, including 1) intrinsic differences in the areas of nature being studied; 2) differences in the observational techniques available for studying each area; 3) differences, due to self selection, in the cognitive styles, personalities, values, and metaphysical-ideological beliefs of scientists who choose to enter different fields; and 4) historical contingencies.

H: Conformity

Although there is some variation in thought styles, there are also many shared ideas. This section examines some motivations and mechanisms for conformity.

A tendency to conform begins during education, when students are taught the currently accepted theories and methods. If a student continues the process of becoming a mature scientist, eventually this education will include work in a research lab, where "most investigators welcome these scientific ‘offspring’ to their laboratories; this is the means whereby the continuity of science is established, and there is a certain sense of immortality in passing down one's thought style. (Grinnell, 1992, p. 65)" This process seems analogous to natural selection, as mature scientists try to pass on the methods they prefer. If students learn a method and use it, the method has survived into a new generation. Some theories and methods are not emphasized by the senior researchers, or are not used by students, and these methods may not survive into the next generation. Of course, new ideas and methods are continually introduced into science. For example, in a continuation of the passage quoted above, Grinnell describes how "scientifically adventuresome" graduate students can help a senior investigator learn new methods.[15]

Problem-solving effectiveness is one reason for a method (or any other component of a thought style) to survive. Another reason is tradition; even if a method is not especially effective, if it has been used, and is being used, it may become a habit that continues in the future. There can also be a ‘personal consistency’ motivation; a scientist who uses a method would like to think this is a wise decision, and when others also use this method there is a reassurance that reduces doubts and dissonance.

Professional success — such as getting employment, grants, or publications — depends on decisions by a community that evaluates according to its own criteria; this is another reason to conform to a majority or a powerful minority. But in science one accepted criterion is creativity, so the community expects a balance between conformity and innovation, a “disciplined rebellion” that works creatively within an established framework. Finding an effective balance depends on the abilities and personality of a scientist, and on the situation; sometimes innovation is necessary because traditional approaches are not working well, but in other cases it is wise to heed the adage, “if it ain't broke, don't fix it.” In any case, the prevailing consensus is often a good starting point for innovative invention. And effective persuasion usually involves framing arguments within the structure of current ideas and in accordance with the current thought style, even if the argument is a proposal to change some of the currently accepted ideas.

According to Feyerabend (1975), science operates at its best when the atmosphere of a scientific community encourages wild innovation; there should be a state of theoretical pluralism with constant competition between a multitude of conflicting theories. In a community that adopts this thought style, conformity to the accepted standards would require an attitude of rebellious creative anarchy, with actions to match.[16] Although scholarly communities rarely live up to

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[15]. By stretching the natural selection analogy, this introduction of new ideas would be analogous to introducing new genes into a gene pool by the migration of new individuals into a population. And invention-by-revision would be analogous to mutation.

[16]. Is this a Liar's Paradox, with conformity to rebellion?

Feyerabend's ideal of free anarchy, usually some innovation is expected. Therefore, a scientifically useful education should help a student learn how to work with disciplined creativity. This education might be accomplished, for example, by combining a foundation of traditional content-knowledge with the process-knowledge (including skills and attitude) that is needed to become a creative developer of new observation-and-interpretation knowledge.

So far, a collective thought style has been treated as if it were a pre-established “given” that an individual can only respond to. But communities are composed of individuals, and there are mutual interactions between people and their cultural context. Rosenberg (1988) explains that "institutional forms have developed in a kind of symbiosis with the structuring and informing role of career-oriented individuals. (p. 568)" Because of this, a community and its members try to develop an operating context (of thought styles and institutional structures) that works for the benefit of its members. Hull (1988) claims that with the conventional reward structure of science, individuals who operate in their own best interests will usually contribute to the overall goals of the scientific community.

I: Change

Thought styles vary across science at any given time, and also change across time. Logically, it seems that there can be two types of driving force for a change in thought style: change can occur in response to a changing context, or it can be due mainly to individuals who have a new view of how things should be done.

An interesting transformation occurs when variation across fields combines with change across time. For example, when there is a newly important overlapping of domains between two fields that had been relatively independent, for awhile the thought styles from the two fields may clash. Eventually, however, some scientists will find a way to synthesize the best of both styles into an eclectic mix that will be accepted by some (but not all) scientists from the two fields who work in the area of overlap. Sometimes new fields of science arise from this type of synthesis.

Near the end of the previous section is a statement that "a community and its members try to develop an operating context...that works for the benefit of its members." But within a community there will always be members with differing situations and objectives, and the current thought style will benefit some members more than others. Some of the disadvantaged members may decide to create a better context for themselves by trying to change the thought styles and reward systems of their community.

Used creatively, these two principles — change due to context and individuals — seem flexible enough to explain any change, especially if "context" is defined broadly. But being so flexible, do they really explain anything? Maybe not, but they can be useful for thinking about change because they stimulate questions such as “Was there a change in context, was there a response, and what was the effect?”, or “What was the status quo, who was dissatisfied with it and why, what did they do about it, and what was the effect?” Of course, these two broad principles do not form a ‘model’ for change, but (as explained above) this is not my objective.

But a model that is useful for describing and interpreting changes in science is proposed by Laudan (1984), whose "reticulated model of scientific rationality" is based on the mutual interactions between the *goals, theories,* and *methods* of scientists. When a dissonance develops between any of these, in order to maintain logical consistency there will be a motivation (as in the "personal consistency" factor discussed in Section 2.34) to adjust the others to bring them all back into agreement. Therefore, a change in one of these (goals, theories, or methods) can lead to a change in the others.

For example, conceptual criteria are formulated and adopted by people, and can be changed by people. In 1600 noncircular motion in theories of astronomy was considered inappropriate, but in 1700 it was acceptable. What caused this change? The theories of Kepler and Newton. First, Kepler formulated a description of planetary motions with orbits that were elliptical, not circular. Later, Newton provided a theoretical explanation for Kepler's elliptical orbits by showing how they can be derived by combining his own laws of motion and principle of universal gravitation. For a wide range of reasons, scientists considered these theories — which postulated noncircular celestial motions — to be successful, both empirically and conceptually, so the previous prohibition of noncircular motions was abandoned. In this case the standard portrait of science was reversed. Instead of using permanently existing criteria to evaluate proposed theories, already-accepted theories were used to evaluate and revise the evaluation criteria.

Laudan (1977, 1984) describes a similar situation, with conflict between two beliefs, but this time the resolving of dissonance resulted in a more significant change, a change in the fundamental epistemological foundations of science. Some early interpretations of Newton's methods claimed that he rigidly adhered to building theories by inductive generalization from observations, and refused to indulge in hypothetical speculation. Although these claims are disputed by most modern analyses, they were influential in the early 1700s, and the apparently Newtonian methods were adopted by scientists who tried to continue Newton's development of empiricist theories (with core components derived directly from experience), and philosophers developed empiricist theories of knowledge. But by the 1750s it was becoming apparent that many of the most successful theories, in a variety of fields, depended on the postulation of unobservable entities. There was a conflict between these theories of science and the explicitly empiricist goals of science. Rather than give up their non-empiricist theories,

> They sought to legitimate the aim of understanding the visible world by means of postu-lating an invisible world whose behavior was causally responsible for what we do observe. ... To make good on their proposed aims, they had to develop a new methodology of science,...the hypothetico-deductive method. Such a method allowed for the legitimacy of hypotheses referring to theoretical entities, just so long as a broad range of correct observational claims could be derived from such hypotheses. (Laudan, 1984; p. 57)

Variation and change are a part of science, and the study of methodological diversity and transformation can be fascinating and informative. But these characteristics of science should be viewed in proper perspective. It is important to balance a recognition of differences with an understanding of similarities, with an appreciation of the extent to which differences can be explained as "variations on a theme" — as variations on the basic methods shared by all scientists.

2.73: Motivation and Memory, Creativity and Critical Thinking

Science is done by individual scientists, even though it occurs in the context of a community. A productive idea can be stimulated, critiqued and nurtured by colleagues, but an idea always begins in the mind of an individual. The process of productive thinking is the focus of this section.

The mental operations involved in scientific activities are summarized in the ISM diagram by "**motivation and memory**, **creativity and critical thinking**." These mental operations, which in actual use are often blended so thoroughly that it is difficult to distinguish them, are discussed in the next three subsections. To provide a concrete focal point for discussion, most of my examples will involve theory invention, but similar principles apply to other aspects of scientific thinking, especially for the analogous activity of inventing actions.

A. Motivation

Motivation inspires effort. For a scientist, the main motivating factors are a curiosity about "how things work in nature" and a taste for intellectual stimulation, along with psychological motives and practical concerns that include desires for self esteem, respect from others, financial security, and power.

The invention of a major theory is relatively rare in science, but when this does occur it is very important for the career of an individual scientist and for the progress of science as a whole. More common, however, are small-scale inventions such as selecting an existing theory and revising it with minor modifications, or extending its domain to include new systems.

Often, necessity is the mother of invention. For example, Newton invented a theory of calculus because it was needed to fill a gap in the logical structure of his theory for celestial mechanics. His immediate practical goal was finding a method to show that the gravitational force produced by (or acting on) a spherically symmetric object is exactly the same as if all of the object's mass was concentrated at a point in the center of the sphere. Calculus did show this, which enabled Newton's theory of physics to make easy calculations for planets, such as the earth and moon, whose distribution of mass was approximately spherically symmetric.

Conversely, an absence of perceived need can hinder invention. For example, there are clear benefits in doing science with more than one theory, because with competition there probably will be a more lively phase of research pursuit with better testing and more experiments designed to falsify a theory, and a more objective evaluation with less danger of prematurely accepting a theory because "it's all we have." But despite these benefits, if a scientist already has one theory he (or she) usually will not try to invent an alternative. Based on a careful study of research in classical genetics, Darden (1991) describes the usual problem-solving strategy:

> A single scientist usually proposed one alternative and began testing predictions from it; other scientists did likewise. Yet, by using more thorough methods of hypothesis generation, one scientist might have systematically generated the alternatives that were proposed by others, as well as the ones I proposed. (p. 268)

B. Memory

Although for the activities of science it is not sufficient, memory is *necessary* to provide raw materials (theories and exemplars, analogies and metaphors; systems and observations, experimental techniques and problem-solving heuristics,...) for processing by creative, critical thinking. The process of theory invention is nourished by ideas from a wide variety of sources. Often, an especially valuable resource is an existing theory that claims to describe systems of the type being studied. Even if such a theory has been evaluated as “not entirely satisfactory as-is,” perhaps it can be modified to construct a better theory, or maybe an examination of its flaws will provide clues for what to avoid, and for what to do instead.

To build the solid foundation of knowledge that is almost always required for productive research, scientists prepare by learning the currently accepted ‘state of the art’ knowledge about theories and experimental methods. In the early stages of a scientist's career, as a student, there is some lab experience, but in conventional education most knowledge is learned from textbooks and teachers, and is accepted due to the ‘authority’ of these sources, based on trust. Later, when a scientist is doing research, much of the learning comes from this research and from other scientists via articles and books, lectures and discussions, letters and conversations; in this stage, trust in authority decreases while critical thinking increases.

During previous eras, and especially in the modern ‘information age’, the knowledge needed by a scientist can come from a variety of sources. To be used for mental operations, to stimulate and guide the process of thinking, knowledge must be in the mind of the scientist, in the ‘working memory’. But there are several ways to get knowledge into the mind; knowledge can be retrieved from internal storage in the scientists' long-term memory, or it can be retrieved from external storage in the scientists' notes, in articles or books, in computer memory (locally or on the internet), or from the memory of colleagues.

C. Creativity and Critical Thinking

These two essential aspects of productive scientific thinking are discussed in the same subsection because they are closely related:

> The two types of thinking are not opposites; they complement each other and even share many attributes. ... Distinguishing clearly between them is impossible because all good thinking involves both quality assessment and the production of novelty. ... [We should] avoid implying that critical thinking and creative thinking are opposite ends of the spectrum. Instead,...[we should] understand that highly creative thinking is often highly critical, and vice versa. (Marzano, et al, 1988; pp. 17-18)

For example, invention and evaluation (roughly analogous to being creative and critical) are intimately connected, with evaluation acting as a motivation and guide for invention, and invention producing the idea being evaluated.

In defining creativity, Perkins (1984) emphasizes the criterion of productivity:

> Creative thinking is thinking patterned in a way that leads to creative results. ... The ultimate criterion for creativity is output. We call a person creative when that person consistently gets creative results, meaning, roughly speaking, original and otherwise appropriate results by the criteria of the domain in question. (pp. 18-19)

Many strategies for productive thinking are discussed in my elaboration of ISM. Guidelines for critical thinking appear throughout Chapter 2. Strategies for creativity are less frequent, but they do exist. For example, Sections 2.52-2.53 contain many suggestions for invention, such as splitting a theory into components and playing with them by thinking about “what might happen if...”, or using creative data analysis and logical strategies (such as Mill's Methods) with the goal of imagining new ways to see and interpret data.

In one strategy for productive thinking, the principle is to allow both creativity and criticality to operate freely. Although effective invention requires a blending of creative and critical thinking, being overly critical, especially in the early stages of invention, may stifle creativity. Therefore, instruction designed to enhance creativity often uses a technique of ‘brainstorm and edit’ — during an initial brainstorming phase, critical restraints are minimized (perhaps by experimenting with the critical-creative balance in various ways) to encourage a totally free creativity in generating lots of ideas; in a later editing phase, these ideas can be critically checked for plausibility or utility.

During the brainstorming phase, inventors can afford to think freely because they have the security of knowing that their wild ideas will not be acted on prematurely before these ideas have been critically evaluated during the editing phase that follows.

When searching for ideas, it may be helpful to examine each evaluation constraint and ask whether it is necessary, and why. By relaxing non-essential constraints on a problem solution, it may become easier to see data patterns or theory components in a new way, to imagine new possibilities. An ability to view a situation from new perspectives is essential for creativity. But sometimes a knowledge of "the way things have to be" can hinder creativity. The following passage describes a problem, and suggests a strategy for solving it:

> Human 'theories of the world' are essential to our learning and making sense of the world. However, there is a curious paradox about schemata. Just as they are the basis of human perception and understanding, so too are they 'blinders' to interpretations that fall outside their scope. ... Creativity involves the ability to go beyond the schema normally used to approach a problem — to 'go beyond the lines' — and reframe the problem so it might appear in a different light. Characteristically, the creative person has the ability to look at a problem from one frame of reference or schema and then consciously shift to another frame of reference, giving a completely new perspective. This process continues until the person has viewed the problem from many different perspectives. (Marzano, et al, 1988, p. 26)

A tension between tradition and innovation is at the heart of productive thinking. Sometimes new ideas are needed, but more often the application of old ideas is the key to success. Productive work in science usually involves the selection and application of an old theory, not the invention of a new theory. Old actions can also be used effectively. For example, when a new organic compound is discovered (in nature) or synthesized (in the lab), instead of inventing new experiments it may be more productive to use a standard methodology consisting of experiments that in the past have been useful for exploring the properties of new compounds.

There is a similar tension between the virtues of tenacious hard work and the wisdom to stop wasting time on an approach that isn't working and probably never will. This is eloquently portrayed by Robert McKim (1972) in his book, *Experiences in Visual Thinking:*

> When should you abandon one strategy to try another? When is perseverance a virtue, and when is flexibility? Sometimes dogged persistence in the use of a single strategy yields a solution: despite frustration and fatigue, the thinker rattles the same key in the door until it opens. On the other hand, it may simply be the wrong key. When staying with one strategy pays off, we call it 'perseverance'; when it does not, we call it 'stubborn inflexibility'. Genius is often associated with the ability to persevere or, in Edison's terms, to perspire. Creativity is also linked to the ability to be flexible. Clearly, we are facing a paradox. Perseverance and

flexibility are opposites that together form an important unity. (p. 165)

Much of the discussion above has focused on the invention and evaluation of theories. But similar cognitive processes are important in other activities of science, especially for the analogous process of inventing and evaluating actions. One of the most important actions in science is to recognize an opportunity and take advantage of it, whether this involves observation or interpretation. The imaginative use of available observation detectors — either mechanical or human, for controlled experiments or planned field studies, for expected or unexpected results — can be highly effective in converting available information into recorded data. Following this, an insightful interpretation of observations can harvest more meaning from the raw data. Sherlock Holmes, with his alert awareness, keen observations, and clever interpretations, provides a fictional illustration of the benefits arising from an effective gathering-and-processing of all available information. Of course, being alertly aware and clever are also valuable assets for a real scientist.

AN EVALUATION OF ISM AS A DESCRIPTIVE FRAMEWORK

Sections 2.8 and 2.9 are a continuation of the discussion begun in 2.08 and 2.09, regarding the extent to which ISM has achieved Objective A by being able to describe a wide range of scientific methods and perspectives about scientific methods.

2.8: Other Views of Scientific Method

The ISM framework and my ISM elaboration are only two of many views of science, which span a wide range of interpretations. This section discusses one relationship between the ISM framework and other views of science.[17]

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[17]. Appendix B11 summarizes a few of the many current views of science — those proposed by Kuhn (1962, 1970), Lakatos (1970), Laudan (1977), and Laudan (1984), and a brief discussion of the response to these models.

2.81: Alternative Elaborations and Borrowed Ideas

The main relationship between ISM and other views of science is one of actual or potential inclusion; a wide range of ideas either has been incorporated into ISM, or could be expressed using ISM. In constructing the ISM framework and elaborating it, some concepts and terms have been borrowed from (or inspired by, or supported by) other scholars. The major sources of ideas are cited in the text. In the following summary, sources are divided into those used in the ISM framework, those used in my elaboration, and those not used despite their relevance.

I have found some concepts (or terms) so useful that they are incorporated into the ISM framework. These include the hypothetico-deductive box (adapted from Giere, 1991), definitions of model and hypothesis (Giere, 1991), internal and external conceptual factors (Laudan, 1977), pursuit (Laudan, 1977), status (Hewson, 1981), heuristic and demonstrative experiments (Grinnell, 1992), constraints (Nickles, 1981), posing, probing and persuasion (Peterson & Jungck, 1988), and thought styles (Grinnell, 1992).

Other ideas are not used in the framework, but are used in my elaboration. These include the analysis of observational theories by Shapere (1982), cognitive dissonance (Festinger, 1956), hard-core components (Lakatos, 1970), a puzzle (Polanyi, 1962) and filter (Bauer, 1992), and a model for reticulated change (Laudan, 1984); Darden (1991) has been the source of many ideas.

In some cases I have intentionally avoided terms that could have been used. For example, ISM uses ‘supplementary theory’ instead of auxiliary theory (Lakatos, 1970), and ‘thought styles’ instead of paradigm (Kuhn, 1962, 1970), and in describing the structure of scientific research I have not used the terms ‘research programme’ (Lakatos, 1970) or ‘research tradition’ (Laudan, 1977). These terms have been avoided mainly to minimize confusion, so there will be no implication that I am using a term in the same way the original author used it, and no need to explain the difference in my usage. An additional reason for not using ‘paradigm’ is that I want to avoid an assumed connection between ISM and the radical views associated with Kuhn.

In other cases, however, I am using a borrowed term even though in ISM it is used in a slightly different way. For example, the ISM framework uses ‘conceptual factors’ instead of conceptual problems (as in Laudan, 1977), and I have modified the hypothetico-deductive box (from Giere,

1991) in minor ways that do not affect its basic meaning. My elaboration uses hard-core theory components in the context of small-scale theory revision, instead of hard-core theories in the context of a large-scale research programme (as in Lakatos, 1970). And I add an additional P to the 3Ps model (of Peterson & Jungck, 1988) to form a '4Ps' variation. In each of these cases, my use of a term is relatively close to the original use, so I feel comfortable with the modification, in contrast with cases where I have avoided using a term.

2.9: Is ISM a model for 'scientific method'? (Part 2)

As indicated in the title, this section is the second part of a discussion that began in Section 2.09. The continuing discussion begins with a consideration of four functions for a model of science — to describe, predict, explain, and prescribe.

2.91: Description, Prediction, Explanation, Prescription

A. Description

Section 2.08, in response to the question "Can ISM Describe a Wide Range of Views and Practices?", expressed most of what I wanted say about the use of ISM for description, so this section will focus on the possibility of using ISM to perform three other functions of a model.

B. Prediction

My claim is that although ISM is satisfactory as a framework to *describe* what happens in science, its ability to *predict* what will happen is limited. But limitations in predictive adequacy are not limited to ISM, because it is difficult for anyone to construct a model of scientific method that can make predictions that are both precise and accurate. Generally, with an increase in precision (in the specificity of predictions) there is a decrease in accuracy.

Part of the difficulty in prediction is that the timing of scientific activities varies from one situation to another. Due to this variability, a rigid 'method' that specifies an invariable sequence of activities (which allows precise predictions) will often produce inaccurate predictions.

A 'theory' about scientific method would contain principles for the strategies that scientists use in certain types of problem-solving situations. A person could use this theory, given an experimental system (i.e., a problem-solving situation in science that is defined by a known now-state and goal-state), to predict what the scientists will do next. Although this type of prediction is a possibility, such a theory would encounter the same difficulties that occur with other theories in social science. The main difficulty is the overwhelming complexity. In the domain of 'scientific practice' there is a wide variety of complex now-states and goal-states, and thus a wide range of problem situations formed by combining them. The now-state is constantly changing, and for each problem situation there are many options for actions, guided by a variety of problem-solving strategies, which vary with the field, research group, and individual. In addition, there often seems to be an inherent 'free will' sort of randomness in behavior; in a particular situation an individual may choose to do either Action A or Action B or..., and there may be no practical way of knowing which will occur, just as there is no practical way to predict the outcome of a typical coin flip. When combined, complexity and apparent randomness form a 'historical contingency' in which, if the same type of field experiment (involving people) occurs four times, there may be four different outcomes. In response to this complexity and contingency, the usual response of social scientists is to say, "Instead of just giving up, let's try to do the best we can." And that has been my response.

With the help of a model of science, a person can make probabilistic predictions about what *might* occur in a specific science situation, although there cannot be deductively certain predictions about what *will* occur. Usually, the more a person knows about an initial situation and the strategies and actions that typically are used in this type of situation, the more likely it is that a subsequent prediction will be precise and accurate. My claim is that ISM can contribute to an improved understanding of initial situations and of problem-solving strategies and actions, and this can lead to an improvement in predictions.

Notice that I am not comparing ISM with other models of science. My claim is merely that an ISM model is better than no model. Even though I think ISM is useful, I do not think it is superior to all other models in every way. For example, the model of Thagard (1992) — which includes a computational model that can be run with a computer program, and is different than ISM in form, function, and intent — is more useful than ISM, for some purposes. But in other ways, for other purposes, ISM is more useful. The same sort of indistinct conclusion, that "for some purposes this is better, but for other purposes that is better," would result from comparing ISM with a variety of alternative models for science.

Although ISM can be used to make predictions, I think it will be most useful as a descriptive framework, used for the purpose of understanding what already has happened, and for describing "what usually happens" in science. ISM can be used to describe commonly occurring patterns such as a cycle of observation-interpretation-experimentation, and to analyze complexities of timing such as overlapping and interconnected activities, and ambiguities such as the "which came first" chicken-or-egg puzzle involving theory invention and evaluation. When the framework of ISM is filled with details about an episode from the recent or distant past, the relational structure of the framework can facilitate analysis that constructs an integrated picture of this episode. And a comparison of ISM-based analyses for different episodes, to search for differences and for generalizable similarities, could facilitate the construction of general models for science.

C. Explanation

The question of "what constitutes an explanation" is complex and is a topic of lively debate. But according to one common criterion there is an explanation when an ISM-based description postulates a causal mechanism for the relationships among actions. For example, analysis based on ISM may lead to an explanation that in a certain situation a new theory was invented because scientists used known observations and retroductive inference, constrained by conceptual assumptions, to revise an old theory. Or a person using ISM may explain that certain observations were made because theory evaluation revealed a knowledge gap, which motivated scientists to design an experiment (with the help of thought-experiments to test various options) and then do the experiment.

The two examples above involve activities (retroductive inference and experimental design) whose integrated relationships are well defined by the ISM framework. However, ISM is not as useful in providing an integrated structure for elements (such as thought styles and cognitive activities) whose relationships are not clearly defined by the ISM framework, or for elements (such as cultural-personal factors and problem-solving projects) with complex relationships that are only described in general terms by the ISM framework. For these elements, which are described in more detail in other, more specialized models, my justifiably modest claims are only that the ISM framework is compatible with the specialized models, which can be viewed as a detailed "blowup" elaboration for a part of ISM, and which could be used to supplement ISM and increase its information content.

D. Prescription

For any model of science, we can ask whether the goal is to be *descriptive* (to describe how science *is* done) or *prescriptive* (to tell scientists how it *should be* done). My answer is that the ISM framework is primarily intended to be descriptive. On the other hand, an ISM elaboration, by myself or by others, could be intentionally prescriptive or not, depending on the goals of the elaborator. But in another context, the ISM framework is intended to be prescriptive when this means making suggestions to students (not scientists) about how science should be done; this is one way to achieve Objective B by contributing to the improvement of education.

When any model of science is used to teach students in the classroom there will be a normative influence on students. If students perceive a model to be a picture of either "what science is" or "what science should be and how scientists should behave," and if this picture influences students to behave in the way that is described, then the model has performed a prescriptive function. In fact, the possibility of promoting a change in student behavior is a major motivation for including any model for "the nature of science" in a curriculum. Therefore, when selecting a model of science to be used for teaching students in the classroom, an important practical question is, "What type of model is most beneficial for students?" This question will be discussed in Section 4.23.

2.92: Is ISM a model for a method?

This section reviews some explanations and arguments from Sections 2.08-2.09 and 2.91. A good starting point is an admission that a response to this question depends on definitions of model and method. Due to differing definitions and penchants for patterns, scholars who study science disagree about whether there is a method in science, and whether (and how) a model for the process of science can be constructed.

In my opinion, there is enough patterning in the process of science to justify a claim for a method that consists of variations on the basic themes contained in the ISM framework. These variations can be conceptualized as differences in the characteristics, integrated relationships, and balancing of components in ISM. According to this definition, ISM does describe some 'methods' for science.

But is ISM a model? As discussed in Section 2.09_D, ISM can function as a general domain-model that facilitates the construction of a specific system-model, analogous to the functioning of a scientific domain-model. The preceding subsection examines four potential functions of a model: description, prediction, explanation, and prescription. ISM was designed with the intention of performing only one of these functions: description. I am confident that the ISM framework fulfills its objective as a model that can be used to describe a wide range of science and views about science. I think the other three functions are less essential, but they seem to be done reasonably well by ISM, anyway. Prediction? It is difficult for any model to predict any aspect of human behavior in its glorious complexity, but I think ISM could be helpful in making improved predictions. Explanation? If this is defined as description with a causal mechanism, then ISM does propose explanations. Prescription? Although ISM is not intended to be a prescriptive framework for practicing scientists, it could perform this function for students; in fact, constructing ISM as a "useful tool for contributing to the improvement of education" was one of the two main goals for ISM.

Section 2.08 illustrates how ISM is a relatively neutral descriptive framework that can be used to describe a variety of contrasting perspectives by adjusting the characteristics, relationships, and balances of ISM components.[18] Perhaps I just lack imagination, but I cannot imagine any

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[18]. The diverse perspectives expressed using ISM include: external consistency or conflicting theories, and empirically constrained retroduction or freewheeling counterinduction; views of cultural-personal influence as being significant or trivial, as being an inherent part of science

reasonable perspective — one that scholars would take seriously, or that teachers and students should consider — that could not be described using the ISM framework.

But this flexibility leads to a question: "If ISM can be adjusted to say everything, does it really say anything?" Or, with an alternative formulation, "If a model of science makes minimal claims about the nature of science, how can it be interesting or useful?" This question, which provides an important counterbalance for concerns about bias, raises a valid concern. In contrast to models that make strong claims about science — such as the 'strong program' claim for the cultural determination of scientific knowledge — the ISM framework makes no radical statements about science. And although my elaboration of ISM occasionally criticizes other views, the ISM framework has no harsh words for other models of science. By comparison, Lakatos (1970) did criticize the earlier models of Kuhn (1962) and Popper (1934/1959, 1963) in order to clarify the ways in which his own model differed from previous views, and to argue for the advantages of his model. But does a model have to make controversial claims, in order to be useful?

In discussing this question, it will be helpful to consider the ways that scholars who study science differ in their preferences for finding patterns and making models. For example, a typical historian is less likely to do either, compared with a philosopher. The ISM framework combines aspects of both approaches. ISM does attempt to find regularly occurring patterns and to construct a general model, as do philosophers. But since ISM begins as an empty framework that can be filled with details for a specific situation, it is compatible with a historian's claim that history can be more accurate when it is customized for each historical episode, when it is minimally constrained by the presuppositions imposed by a model.

Because the ISM framework is 'empty' it does not impose a theory about "how things are" before the evidence is examined. Due to its relative neutrality the empty framework, when it is filled with details for an individual episode, could serve a useful function in the construction of a more accurate portrait of what happened, how it happened, and why. But ISM is not just empty; it is also a 'framework', and its organized structure could facilitate the process of finding relational

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or an appendix that is neither needed nor wanted, as an undesirable bias or a valuable social conscience, as described in my own elaboration or by radical 'strong program' sociologists; and empiricism or non-empiricism.

patterns in the episode. Then, by comparing patterns from many episodes, using ISM as a common basis for analysis, it might be easier to recognize regularly occurring patterns that would contribute to constructing a deeper understanding of the process of science.

More important than potential "study of science" applications, however, are the potential educational applications of ISM, whether or not it is considered to be a "model for a method." Chapters 3 and 4 will explore some possibilities for using ISM to improve education.