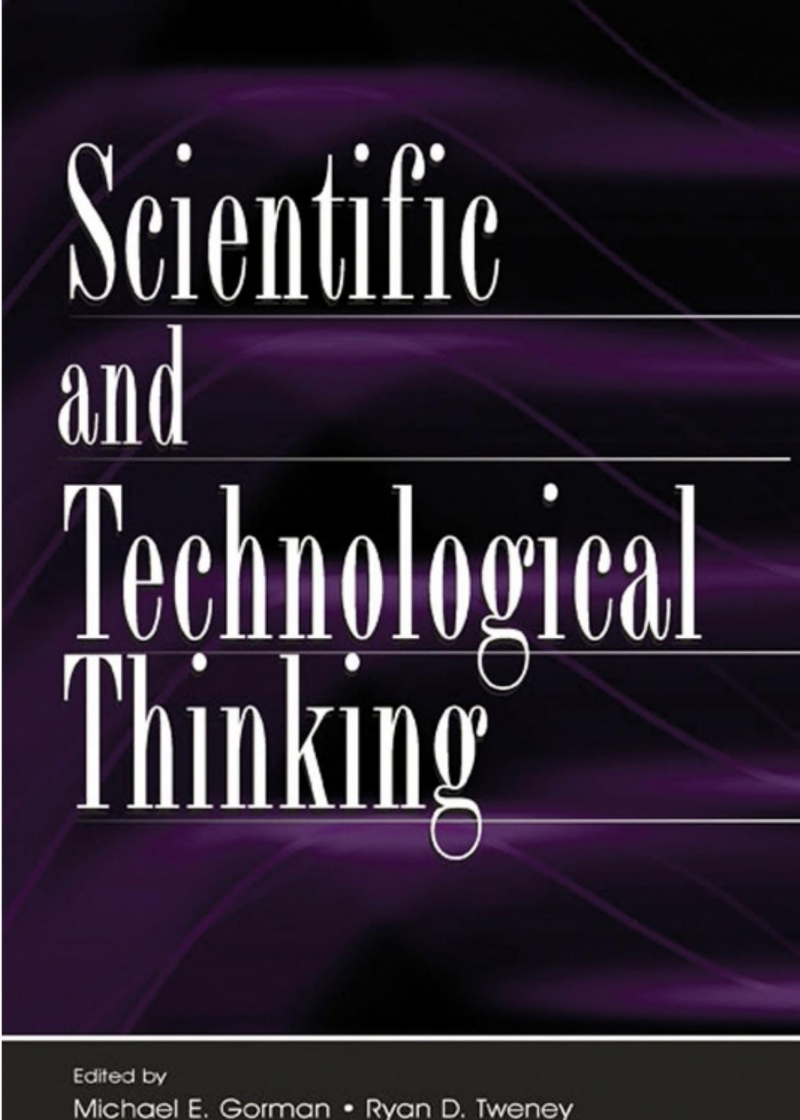

Scientific and Technological Thinking

Edited by

Michael E. Gorman • Ryan D. Tweney

David C. Gooding • Alexandra P. Kincannon

SCIENTIFIC AND TECHNOLOGICAL THINKING

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LEA
2005

LAWRENCE ERLBAUM ASSOCIATES, PUBLISHERS
Mahwah, New Jersey London

Lawrence Erlbaum Associates, Inc., Publishers
10 Industrial Avenue
Mahwah, New Jersey 07430

Cover design by Kathryn Houghtaling Lacey

Library of Congress Cataloging-in-Publication Data

Scientific and technological thinking / edited by Michael E. Gorman
… [et al.].
p. cm.
Includes bibliographical references and index.
ISBN 0-8058-4529-1 (cloth : alk. paper)
1. Creative ability in technology. 2. Creative thinking. I.Gorman,
Michael E., 1952- .
T49.5.N475 2004

Books published by Lawrence Erlbaum Associates are printed on acidfree paper, and their bindings are chosen for strength and durability.
153.3'5—dc22 2003063115
CIP

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Preface

This volume is the product of a workshop on cognitive studies of science and technology that was held at the University of Virginia in March 2001. The goal of the workshop was to assemble a diverse group from a variety of fields, including graduate students and junior and senior faculty, to discuss the latest research and to generate ideas for the future of "Cognitive Studies of Science and Technology." The workshop was made possible through the generous support of the National Science Foundation, the Strategic Institute of the Boston Consulting Group, and the National Collegiate Inventors and Innovators Alliance.

The chapters in this volume (except chap. 14) are authored by workshop participants. They describe recent developments and discuss ongoing issues in the study of the cognitive processes of discovery and invention. Building on our workshop discussions, we have developed a conceptual framework that we hope will help to clarify the current state of the field and to spawn new ideas for future investigation. For readers interested in the original papers and authors, and the lively discussion that occupied much of the workshop, all of this material was recorded live in digital format. It can be shared, with permission of the original participants, via the workshop Web site at http://repo-nt.tcc.virginia.edu/cogwkshop/. For a brief description of our deliberations, see the following chapter: Gorman, M. E., Kincannon, A., and Mehalik, M. M. (2001). Spherical horses and shared toothbrushes: Lessons learned from a workshop on scientific and technological thinking. In K. P. Jantke and A. Shinohara (Eds.), *Discovery science* (pp. 74–86). Berlin, Germany: Springer-Verlag.

1

Editors' Introduction

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At the turn of the 21st century, the most valuable commodity in society is knowledge, particularly new knowledge that may give a culture, a company, or a laboratory an adaptive advantage (Christensen, 1997; Evans & Wurster, 2000; Nonaka & Takeuchi, 1995). Turning knowledge into a commodity poses two dangers. One is to increase the risk that one culture, company, or group can obtain an unfair advantage over others. The other is to impose a one-dimensional, goal-driven view of something that, as the chapters in this volume will show, is subtle, complex, and diverse as to its motivations and applications. To be sure, knowledge about the cognitive processes that lead to discovery and invention can enhance the probability of making valuable new discoveries and inventions. However, if made widely available, this knowledge could ensure that no particular interest group "corners the market" on techno-scientific creativity. It would also facilitate the development of business strategies and social policies based on a genuine understanding of the creative process. Furthermore, through an understanding of principles underlying the cognitive processes related to discovery, educators can use these principles to teach students effective problem-solving strategies as part of their education as future scientists.

A special focus of this volume is an exploration of what fine-grained case studies can tell one about cognitive processes. The case study method is normally associated with sociology and anthropology of science and technology; these disciplines have been skeptical about cognitive explanations. If there is a well-established eliminativism among neuroscientists (Churchland, 1989), there is also a social eliminativism that seeks to replace cognitive accounts with sociological accounts (Woolgar, 1987). In these socio-anthropological case studies, interactions, inscriptions, and actions are made salient. The private mental processes that underlie public behavior have to be inferred, and most anthropologists and sociologists of science are trained to regard these cognitive processes as epiphenomenal. By contrast, intellectual or internalist studies by historians of science go into reasoning processes in detail (Drakes, 1978; Mayr, 1991; Westfall, 1980). Historians of science have produced a large number of studies that describe processes of experimentation, modeling, and theory construction. These studies can be informative about reasoning and inference in relation to declarative knowledge, experiential knowledge and experimental data (Galison, 1997; Gooding, 1990; Principe, 1998), visualization (Rudwick, 1976; Tweney & Gooding, 1991; Wise, 1979), and the dynamics of consensus formation through negotiation and other forms of personal interactions (Rudwick, 1976). However, historians generally have no interest in identifying and theorizing about general as opposed to personal and culture-specific features of creative processes. Thus, very few historical studies have combined historical detail with an interest in general features of the creative process (for exceptions, see Bijker, 1995; Carlson & Gorman, 1990; Giere, 1992; Gooding, 1990; Gruber, 1974; Law, 1987; Miller, 1986; Tweney & Gooding, 1991; Wallace & Gruber, 1989).

Despite the usefulness of fine-grained case studies, cognitive psychologists have traditionally lamented their lack of rigor and control. How can one identify general features, let alone develop general principles of scientific reasoning, from studies of a specific discovery, however detailed they may be? One answer is to develop the sorts of computational models preferred by cognitive scientists (Shrager & Langley, 1990). One classic example of this kind of modeling is, of course, Kulkarni and Simon's (1988) simulation of a historical account by Larry Holmes (1989) dealing with Hans Krebs's discovery of the ornithine cycle (see also Langley, Simon, Bradshaw, & Zytkow, 1987). These models can be abstracted from historical cases as well as current, ethnographic ones. However, it is important to remember that such models are typically designed to suit the representational capabilities of a particular computer language. Models derived from other domains—such as information theory, logic, mathematics, and computability theory—can become procrustean beds, forcing the territory to fit the researcher's preconceived map (Gorman, 1992; Tweney, 1990).

The tension among historical, sociological, and cognitive approaches to the study of science is given thorough treatment in the chapter 2, by Nersessian. She distinguishes between good old-fashioned artificial intelligence, represented by computer programs whose programmers claim they discover scientific laws, and social studies of science and technology, represented by detailed or "thick" descriptions of scientific practice. Proponents of the former approach regard cognition as the manipulation of symbols abstracted from reality; proponents of the latter see science as constructed by social practices, not reducible to individual symbol systems. Nersessian describes a way for cognitive studies of science and technology to move beyond these positions, taking what she calls an environmental perspective that puts cognition in the world as well as in the brain. Ethnography can be informative about cognitive matters as well as sociological ones, as Nersessian's study of biomedical engineering laboratories shows, and historical research helps make the cognitive practices intelligible.

In order to ground cognitive theory in experimental data, psychologists have conducted reasoning experiments, mainly with college students but also with scientists. These experiments allow for controlled comparisons, in which all participants experience the same situation except for a variable of interest that is manipulated. A psychologist might compare the performance of scientists and students on several versions of a task. Consider, for example, a simple task developed by Peter Wason (1960). He showed participants in his study the number triplet "2, 4, 6" and asked them to propose additional triplets in an effort to guess a rule he had in mind. In its original form, the experimenter's rule was always "any three increasing numbers," which proved hard for most participants to find, given that the starting example suggests a much more specific rule (e.g., "three even numbers"). Each triplet can be viewed as an experiment, which participants used to generate and test hypotheses. For Wason, participants seemed to manifest a *confirmation bias* that made it hard to see the need to disconfirm their own hypotheses. Later research suggested a much different set of explanations (see Gorman, 1992, for a review). Tasks such as this one were designed to permit the isolation of variables, such as the effect of possible errors in the experimental results (Gorman, 1989); however, they are not representative of the complexities of actual scientific practice. The idealization of reasoning processes in scientific and technological innovation (Gorman, 1992; Tweney, 1989), and of scientific experiments in computer models (Gooding & Addis, 1999), is a critical limitation of this kind of research.

What makes models powerful and predictive is their selectivity. A good model simplifies the modeled world so as to make it amenable to one's preferred methods of analysis or problem solving. However, selectivity can make one's models limited in scope and, at worst, unrealistic. Insofar as this is a problem, it is often stated as a dichotomy between abstraction and real' ism, as for example by the mathematician James Gleick: “The choice is always the same. You can make your model more complex and more faithful to reality, or you can make it simpler and easier to handle” (Gleick, 1987, p. 278). David Gooding illustrated this problem at a workshop with a joke that could become a central metaphor for science and technology studies:

> A millionaire with a passion for horse racing offered a large prize—enough to buy a few Silicon Graphics machines—to anyone who could predict the outcome of any horse race. Three scientists took up the challenge, a physiologist, a geneticist and a theoretical physicist. One year later the three scientists announced their results. Here’s what each reported:
>
> *The Physiologist*: “I have analysed oxygen uptake, power to weight ratios, dietary intake and metabolic rates, but there are just too many variables. I am unable to predict which horse will win.”
>
> *The Geneticist*: “I have examined blood lines, breeding programs and all the form books, but there are just too many uncertainties. I cannot predict who will win any race.”
>
> *The Physicist*: “I have developed a theoretical model of the dynamics of horse racing, and have used it to write a computer program that will predict the outcome of any horse race to 7 decimal places. I claim the prize. But—there is one proviso. The model is only valid for a perfectly spherical horse moving through a vacuum”[1]

Experimental simulations of scientific reasoning using tasks like Wason’s (1960) 2–4–6 task are abstractions just like the spherical horse: They achieve a high degree of rigor and control over participants’ behavior but leave out many of the factors that play a major role in scientific practice. Gooding (in press) argues that in the history of any scientific field there is a searching back and forth between models and real world complexities, to achieve an *appropriate level of abstraction*—not overly simple, capturing enough to be representative or valid, yet not so complex as to defeat the problem-solving strategies of a domain. Gooding’s chapter for this volume (chap. 9) shows how scientists use visualization in creating models that enable them to negotiate the tension between simplicity and solvability on the one hand and complexity and real world application on the other. He argues that, like any other scientific discipline, cognitive studies of science and technology must find appropriate abstractions with which to describe, investigate, model, and theorize about the phenomena it seeks to explain.

To compare a wide range of experiments and case studies conducted in different problem domains, one needs a general framework that will establish a basis for comparison. As Chris Schunn noted in the workshop on cognitive studies of science and technology that inspired this volume (see Preface), models, taxonomies, and frameworks are like toothbrushes—no one wants to use anyone else’s. In science and technology studies, this has been the equivalent of the “not invented here” syndrome. This usually reflects the methodological norms of a discipline, such as the sociological aversion to cognitive processes, which is reminiscent of behavioral psychology’s rejection of mental processes as unobservables. One strategy for achieving de facto supremacy is to assume, even if one cannot demonstrate it, that one’s own “toothbrush” is superior to any other (Gorman, 1992).

Discovery

Sociological and historical studies of the resolution of scientific controversies have shown that the supremacy of a particular theory, technology, or methodological approach involves negotiation. Because no method is epistemologically neutral, this negotiation often focuses on the validity of the method (s) of establishing facts and of making inferences from them (Galison, 1997). Therefore, rather than promoting the investigative potential of a single method, we advocate approaches that address both Gooding’s problem of abstraction and Schunn’s problem of shareable frameworks. Dunbar and Fugelsang develop one such approach in their contribution (chap. 3) to this volume. This approach combines experiments, modeling and case studies in a complementary manner. They develop the distinction (first made by Bruner, Goodnow, & Austin, 1956) between *in vitro* studies of scientific thinking (which involve abstract tasks like Wason’s [1960] 2–4–6 task) and *in vivo* studies (which involve observing and analyzing scientific practice). Dunbar (1999) used an *in vitro* task to study how participants reasoned about a genetic control mechanism and conducted *in vivo* studies of molecular biology laboratories. In chapter 3, Dunbar and Fugelsang label four more approaches in the same style:

1. *Ex vivo* research, in which a scientist is taken out of her or his laboratory and investigated using *in vitro* research, by presenting problems similar to those he or she would use in his or her research.
2. *In magnetico* research, using techniques such as magnetic resonance imaging to study brain patterns during problem solving, including potentially both *in vitro* and *in vivo* research.
3. *In silico* research, involving computational simulation and modeling of the cognitive processes underlying scientific thinking, including the good old-fashioned artificial intelligence work cited by Nersessian and alternatives.
4. *Sub specie historiae* research, focusing on detailed historical accounts of scientific and technological problem solving. These *in historico* studies can serve as data for *in silico* simulations.

Later chapters offer a variety of examples of *sub specie historiae* and *in vivo* studies, with references to the other types of research noted earlier.

In chapter 4, Klahr takes a framework that he was involved in developing and stretches it in a way that makes it useful for organizing and comparing results across chapters in this volume. His idea is that discovery involve searches in multiple problem spaces (Klahr & Dunbar, 1988; Simon, Langley, & Bradshaw, 1981). For example, a scientist may have a set of possible experiments she might conduct, a set of possible hypotheses that might explain the experimental results, and a set of possible sources of experimental error that might account for discrepancies between hypotheses and results. Klahr adds two other dimensions: (a) whether a space is general or domain specific and (b) whether the search is conducted by individuals, dyads, or teams. This framework leads to interesting questions, such as: Under what circumstances does a mismatch between current hypothesis and current experimental result lead to a search of a space of possible errors in the experiment? and When does it trigger a search for a new hypothesis? Empirical studies can provide specific answers to these questions.

We can now suggest one possible framework to support a comparative analysis of different kinds of study. We can combine Klahr's idea of multiple search spaces with Dunbar's distinction among six types of methodology to produce a multidimensional matrix that allows us to organize and compare the research studies reported in this volume. For example, the studies involving the introduction of error into the 2–4–6 task involve an *in vitro* methodology, three types of general problem spaces, and are conducted on individuals (Gorman, 1989). Tweney and his colleagues have used scientists as experimental participants in a selection task (Tweney & Yachanin, 1985) and in "artificial universe" studies (Mynatt, Doherty, & Tweney, 1978). Similarly, specific computational simulations could be put in the *in silico* row, with the problem spaces they modeled as columns. Historical case studies could be placed in the *sub specie historiae* row, with their problem spaces across the top; these would either be domain specific or, in the case of individuals who move across domains in the course of their careers, person specific. Consider, for example, Herbert Simon, whose career included original contributions to economics, computer science, psychology, and cognitive studies of science. There will, of course, be cases where methods are mixed within the same study, as Dunbar and Fugelsang do when they combine *in magnetico* with *in vitro* techniques.

At this point, gaps in the matrix draw our attention to ways of extending research. These gaps, together with studies we consider relevant but that do not fit the framework, suggest how the framework needs to be developed. They also help identify limitations in the framework itself, as we discuss in chapter 15. Similarly, new studies will add new problem spaces, especially because discovery and invention involve the creation of new problem spaces. Furthermore, as the “thick” historical and ethnographic studies show, much scientific and technological thinking is not conducted “over a defined problem space but across a domain consisting of, say, apparatus, external texts (books, articles, tables, etc.), internal memory (reflecting the scientist’s domain knowledge), and a symbol system that may need to be altered or created, rather than merely rearranged” (Tweney, 2001, p. 154). We emphasize, therefore, that we use problem spaces as an organizational heuristic rather than as an attempt to prescribe an ontology of resources and procedures for discovery.

Trickett, Schunn, and Trafton report in chapter 5 a domain-specific *in vivo* study. They show that, to resolve anomalies, two astronomers and a physicist search both hypothesis and data spaces. The data consist of images of galaxies in the astronomy case and of various representations of the relation between a model and data in the physics case. In both cases, the researchers had to decide which kinds of visualizations worked best; therefore, they searched through problem-specific spaces of visualizations. In both cases, the scientists generated new visualizations and studied existing ones more closely. This study suggests the need to incorporate visualizations into our framework. As Gooding argues in chapter 9, visualizations are used across a wide range of contexts; in particular, they are used to generate phenomenological descriptions, proto-explanations, dynamical models, and in the context of communicating about results. It follows that ways of dealing with different modes of representation should be included in each of the spaces in the Simon-Klahr-Dunbar scheme, in addition to considering that researchers might search a space of possible visualizations on certain kinds of problems.

Trickett et al.’s study (chap. 5) also uses a dyad as a unit of analysis. Two astronomers worked together on identifying and accounting for anomalies in their visualizations. Collaboration was not the focus of the study, however; the same analytic framework was used for the physicist working alone and the astronomers working together. Trickett et al. point out the need for future research to determine whether two or more scientists working together are more likely to notice anomalies than individuals working in relative isolation.

Some of the most successful scientists and inventors kept notebooks, which they used to enhance their problem-solving strategies. Shrager’s chapter, 6, provides a contemporary example of how record keeping can support learning. Shrager describes how he gathered data on the process by which he became a molecular biologist. His is therefore a reflexive *in vivo* study. Shrager kept both a laboratory notebook and a protocol book; he remarked at the workshop (see Preface), “If you lose your lab notebook, you’re hosed.” So, another kind of potential search space is the notes a scientist or inventor keeps on experiments, hypotheses, new designs, and so on—notes that are incredibly valuable when it comes to conducting further research and in establishing priority for an invention or discovery. Gorman and his colleagues have described the way in which Bell made a major improvement on his telephone by searching his own notebook for promising results and discovering one that led to an improved telephone design (Gorman, Mehalik, Carlson, & Oblon, 1993). Tweney and Gooding (1991) showed how Faraday used records of his work to monitor his research stratagems in order to evaluate and refine his research program. However, Shrager also kept a different kind of diary that included his observations on his own learning process. At the end of his chapter, he speculates that this kind of diary might be useful both for science and technology studies scholars and in the education of scientists and engineers.[2]

Notebooks and diaries are more than an external memory aid that scientists and inventors can search; they also create a space of reflection and transformation. Bell, for example, sprinkled his notebook with reflections on his style of invention, gradually realizing that his strength was not in the hands-on component of inventing but in developing the theoretical principles underlying the transmission of speech. For Bell, this “theory” was not a formal hypothesis but a powerful mental model he described and deployed (Gorman, 1997). Similarly, Shrager’s diary includes reflections on how the problem-solving style he has to develop for molecular biology differs from the style he uses in repairing cars.

Faraday’s extensive laboratory investigations are recorded in his laboratory diary, and partial records also survive in the form of material artifacts. These include many instruments and objects with which Faraday “fixed” phenomena. Chapter 7, by Tweney, Mears, and Spitzmüller, is an *in historico* study of Faraday’s investigation of the fine structure of matter. Faraday’s studies of gold colloids and thin gold films have left a rich source of information about his methods and a way of recovering the phenomena that Faraday observed. Faraday designed experiments to test hypotheses, kept extensive notes, and struggled for appropriate ways to produce phenomena that would help him visualize the interaction of light and matter at the atomic level. In other words, he created artifacts in an effort to answer questions about phenomena. These artifacts constitute a space of negotiation that is closely related to the visual space explored by scientists in Trickett et al.’s and Gooding’s chapters, because they also attempted to improve visualizations, just as Faraday struggled to find the right physical representations of his growing understandings. Tweney et al.’s account reveals the way in which Faraday’s private speculations were eventually translated into public documents and artifacts intended to demonstrate phenomena, rather than simply explore them. Note also that Tweney et al.'s replications of Faraday's procedures constitute something like an attempt to merge the *in historico* method with other methods—a kind of real-world version of *in silico* investigations. It is important to emphasize that these replications add a necessarily very personal dimension to the analysis of Faraday's research. This personal dimension is also examined in Gooding's chapter, and it corresponds to the early, exploratory stages of the discovery process, as does Shrager's diary analysis.

Thagard's chapter, 8, provides a useful end-point to a series of chapters devoted to science. He took a list of successful habits for scientists generated by workshop members (see Preface) and compared them with the recommendations of three eminent scientists. In the workshop itself, Thagard recommended that the participants consider a space of questions a scientist might pursue. Finding an interesting problem that one can solve is not a trivial matter, not least because interest and importance do not necessarily go hand in hand with solvability. Answers to many of the questions that Darwin and Faraday articulated near the start of their careers emerged over several decades (Gruber, 1974; Tweney & Gooding, 1991); others eluded them entirely. Harder still is to find a problem that has the potential to make a breakthrough. As Thagard notes, Herb Simon recommended going off the beaten path to find such problems. He had a particular knack for cultivating collaborators as he charged into the unknown. Similarly, James Watson advocated taking risks, but having very bright colleagues and mentors on whom to fall back.

Thagard's chapter highlights the importance of collaboration, an activity partly captured by having a dyadic category in one's evolving framework. However, within this dyadic category there ought to be an analysis of different kinds of collaborations. For example, some stay within a domain, and others stretch across disciplines. In some collaborations the work of each participant is easily distinguished from the other, and in others the whole is greater than the sum of the parts. Many collaborations involve entire teams of researchers, stretching beyond the dyadic category.

In chapter 10, Ippolito uses insights from the literature on scientific thinking to help readers understand Virginia Woolf's development as a writer, including her role in creating a new kind of stream-of-consciousness novel. Woolf kept a diary of reflections on her own process and generalizations about what it took to be a writer. She also fished wherever she went for interesting details about other human beings, and noted them—creating a space of "observations" where she conducted perceptual rehearsal, gaining "practiced familiarity with certain classes of problems" and from which she constructed representations she hoped to share with her readers. The process of creating these representations was rigorous and transformative. As

Woolf (1926, p. 135) wrote, “Life is subjected to a thousand disciplines and exercises. It is curbed; it is killed. It is mixed with this, stiffened with that, brought into contrast with something else; so that when we get our scene ... a year later the surface signs by which we remembered it have disappeared.” Ippolito compares Woolf’s process to the way in which scientists such as Faraday, Newton, Maxwell, and Einstein developed sophisticated representations that led to discoveries.

Invention

Is the construction of the extended narrative of a novel more like an invention than a discovery? Chapters 11–14 are an attempt to understand the kind of thinking that goes into the development of new technologies.

In chapter 11, Bradshaw focuses on the Rocket Boys described by Homer Hickham in his book of the same name. In his earlier work, Bradshaw invoked a problem-space framework to explain why the Wright Brothers succeeded; while their competitors search only through a design space, the Wrights considered both design and function spaces. Bradshaw’s analysis of the Rocket Boys includes a larger array of more specific problem spaces, such as types of fuel, fins, and alternatives for the nozzle geometry. Bradshaw maintains that this decomposition of the design space into more specific problem spaces is one reason the boys succeeded,[3] but they could not test all possible variations arising from the combinations of these multiple factors. Instead, the boys developed a shared mental model of rocket design. This model evolved as they studied sources and interacted with experts, such as their teachers. The boys also took good notes on their experiments and results and found a good division of labor within the team.

Hughes’s chapter, 12, illustrates one of the key problems facing inventors: integrating components that have been shown to work into a larger system that also performs as specified. This is known as the problem of *decomposition*. In his earlier work, Hughes has shown how Edison was an inventor of systems (Hughes, 1977); in his more recent work, Hughes has shown how systems engineers create not just new artifacts but also the systems in which these artifacts will play a role (Hughes, 1998). Hughes’s work suggests the importance of including in one’s framework the extent to which scientists and inventors are focusing on redesigning systems.

Chapter 12 reminds readers that new technological systems evolve through the work of many different actors. These occupy a space in which methods, techniques, meanings, and even the very basis for communication are negotiated: a trading zone (Fujimura, 1992;

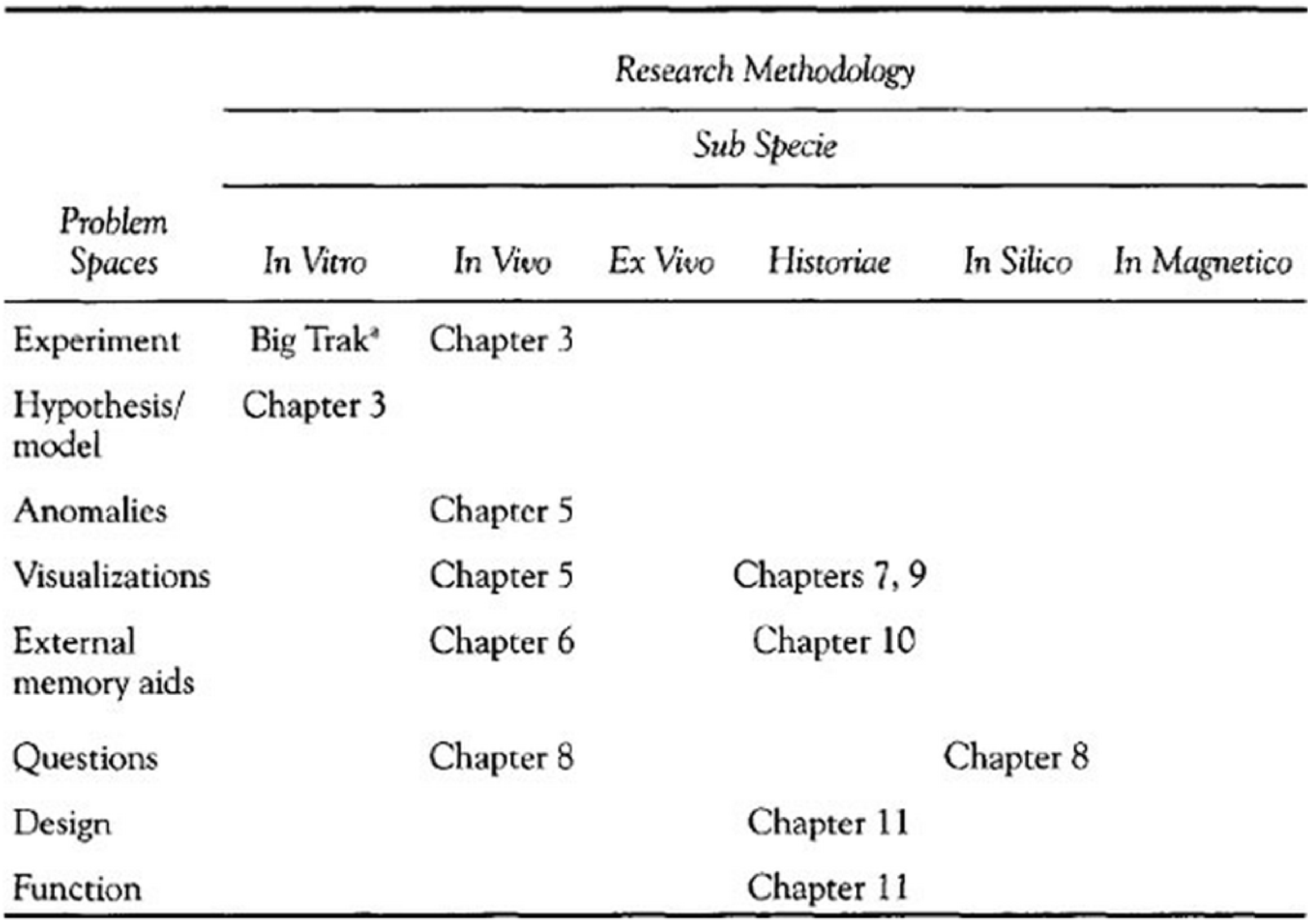
Galison, 1996; Star & Griesemer, 1989). Gorman lays out in chapter 13 a framework for studying and understanding the kinds of trading zones that emerge around technological systems. The effectiveness of the trading zone depends, in part, on the nature of the network linking participants. A network represents both the connectivity or patterns of communication between actors and the power or authority structure. What Gorman calls a *State 1* is a structure dominated by one party or by the viewpoint of a single person or group. A *State 2* is a zone where participants are relatively equal and communicate via a Creole that is compatible with several interpretations and approaches to a problem. A *State 3* occurs when participants develop a shared mental model. Therefore, Gorman suggests that a developed cognitive approach to science and technology should include trading zones, especially when considering research that depends on dyads, teams, and entire systems.

Because one of our goals in this volume is to establish that cognitive studies of science and technology have relevance outside of academia, a representative from industry who also has strong academic credentials has contributed a chapter to this volume. Brad Allenby is the creator of Earth Systems Engineering Management (ESEM), which involves a new way of thinking about environmental issues that incorporates insights from the literature on science and technology studies. Human technological systems have been transforming nature for thousands of years, but the pace has accelerated. Allenby argues in chapter 4 that there is no point in trying to undo these effects; instead, scientists' responsibility is to manage these complex systems intelligently. To succeed, ESEM will require the formation of State 3 networks regarding environmental systems, because the complexity of these systems and their vulnerability to unexpected perturbations require a continuous dialogue among stakeholders. ESEM is one kind of activity that requires searches in multiple spaces at different levels and particular attention to how these spaces combine at the systems level.

Summary

Table 1.1 shows the chapters in this volume organized in a matrix that focuses on research methodology on one axis and problem space on another, in context with a few other seminal studies, shown in italics. The studies in this volume are mostly *in vivo* and *sub specie historiae*, as one of our goals was to demonstrate the value of these approaches in combination with others. Most of the studies described in this book were also domain specific, although at different levels and with different types of specificity. The Big Trak study (Shrager & Klahr, 1986) is noted for comparison as an example of an *in vivo* general problem-solving study not linked to a specific domain.

Table 1.1 Studies in This Volume Classified by Research Methodology and Problem Space



	Research Methodology					
	Sub Specie					
Problem Spaces	*In Vitro*	*In Vivo*	*Ex Vivo*	*Historiae*	*In Silico*	*In Magnetico*
Experiment	Big Trak[a]	Chapter 3				
Hypothesis/ model	Chapter 3					
Anomalies		Chapter 5				
Visualizations		Chapter 5		Chapters 7, 9		
External memory aids		Chapter 6		Chapter 10		
Questions		Chapter 8			Chapter 8	
Design				Chapter 11		
Function				Chapter 11		

[a] Shrager and Klahr (1986).

Table 1.2 shows another dimension on which studies could be classified: whether a group or a system is the primary focus of the analysis. Of course, there could be finer gradations in group size, perhaps following the social evolutionary framework that distinguishes among group, tribal, and larger organizational structures (Caporael, 1997). All these levels of human interaction depend on a close coupling between human and nonhuman actants, as Nersessian, and Tweney et al., and Allenby point out in chapters 2, 7, and 14. The systems level reflects a scale-up in complexity as well as in the number of actors and actants. One could use Gorman's three network states to distinguish among types of trading zones.

Table 1.2 Chapters That Treat Dyads, Teams, or Systems as the Primary Unit of Analysis

Dyad	*Team*	*System*
Trickett et al.'s astronomers	Nersessian's biomedical engineering laboratories	Hughes
Thagard	Dunbar's molecular biology laboratories	Gorman

These tables are meant to be provocative rather than comprehensive, and we invite readers to incorporate additional studies and develop new categories. The point is to find heuristics for comparing cognitive studies of science and technology. In the case of this volume, our heuristic framework highlights the focus on detailed case studies of domain-specific problem solving. In chapter 15, we reconsider this framework and point the way toward future research.

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[1] This anecdote is attributed to Adam Katalsky in *New Scientist*, December 19–26,1992.

[2] Gorman and Shrager are currently exploring these possibilities in a pilot research project involving graduate students entering ananotechnology and systems engineering.

[3] In their use of this strategy, the boys unwittingly followed in the footsteps of Nobel laureate John Bardeen, who was taught decomposition by Eugene Wigner: “The first step was to decompose the problem, either into smaller problems with less scope or into simpler problems that contained the essence of the larger problem” (Hoddeson & Daitch, 2002, p. 54).

2

Interpreting Scientific and Engineering Practices: Integrating the Cognitive, Social, and Cultural Dimensions

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Cognitive studies of science and technology ("cognitive studies") participate in two interdisciplinary fields: (a) cognitive science and (b) science and technology studies (STS). My analysis starts from issues about how cognitive studies are situated with respect to the social and cultural research programs in STS. As we will see, these issues have implications for how cognitive studies are situated within cognitive science as well. Within STS there is a perceived divide between cognitive accounts and social and cultural ("sociocultural"[1]) accounts of knowledge construction, evaluation, and transmission. Sociocultural accounts are dominant and have tended to claim that cognitive factors are inconsequential to interpreting these practices. Scientists are seen as having interests and motivations and as being members of cultures, but cognition remains, in effect, "black boxed." Cognitive studies accounts, for their part, have paid deference to the importance of the social and cultural dimensions of practice but have not, by and large, made these dimensions an integral part of their analyses. The situation has fostered a perception of incompatibility between cognitive and sociocultural accounts. One clear indication of this perception is the now-expired infamous "ten-year moratorium" on cognitive explanations issued first in 1986 by Bruno Latour and Stephen Woolgar (1986, p. 280; Latour, 1987, p. 247), by which time, they claimed, all pertinent aspects of science would be explained in terms of sociocultural factors. Perceptions to the contrary, any such divide is artificial. Producing scientific knowledge requires the kind of sophisticated cognition that only rich social, cultural, and material environments can enable. Thus, the major challenge for interpreting scientific and engineering knowledge-producing practices is to develop accounts that capture the fusion of the social–cognitive—

cultural dimensions in these.

I argue in this chapter that the perception stems not from a fundamental incompatibility between cognitive and sociocultural accounts of science and technology but rather arises from the fact that integration has been hampered by implicit and explicit notions of "cognition" used on both sides of the perceived divide. Implicit echoes of Cartesian dualism underlie the anticognitive stance in sociocultural studies, leading to sociocultural reductionism. On this side, Cartesianism is rejected as untenable but, rather than developing an alternative theory to encompass cognitive explanatory factors, these are rejected outright. Within cognitive studies, these echoes are more explicit in their association with the traditional cognitive science view of cognition connected with GOFAI ("Good Old Fashioned AI" [coined by Haugeland, 1985]). The founding "functionalist" assumption of AI, that has in turn dominated cognitive science, is that thinking or intelligence is an abstractable structure that can be implemented in various media, including computers and humans. Cognitive reductionism identifies cognition with symbol processing that, in humans, takes place within an individual mind. Research in cognitive studies of science supports the position that important aspects of the representational and reasoning practices of scientists and engineers cannot be explained without invoking cognitive structures and processes. However, this large body of research, especially "*in vivo*" (coined by Dunbar, 1995) observational studies and "cognitive-historical" (coined by Nersessian, 1992; see also Nersessian, 1995b) studies, has led equally to recognizing that the social, cultural, and material environments in which science is practiced are critical to understanding scientific cognition (see, e.g., Dunbar 1995; Giere, 1988, 2002; Gooding, 1990; Gorman, 1997; Gorman & Carlson, 1990; Kurz & Tweney, 1998; Nersessian, 1984, 1995, 2002b; Thagard, 2000; Tweney, 1985, 2002). Accommodating these insights requires inserting a third approach to interpreting science and engineering practices—one that can serve as a *via media* in that it is nonreductive. The main purpose of this chapter, and an important part of the agenda for this volume, is to theorize cognition in relation to context or environment.

One route to attaining integration is to reconceptualize "cognition" by moving the boundaries of representation and processing beyond the individual so as to view scientific and engineering thinking as a complex system encompassing cognitive, social, cultural, and material aspects of practice. This direction is being pursued for accounts of mundane cognition in contemporary cognitive science, where proponents of such accounts refer to them as *embodied* and *embedded*. These accounts challenge central assumptions of GOFAI, and so the research is creating controversy within the field of cognitive science. To date, it has played little role in either cognitive or sociocultural studies of science. Accounts within this emergent research paradigm, which I call *environmental perspectives*, seek to provide explanations of cognition that give substantial roles to bodily and sociocultural factors. Advocates of environmental perspectives argue that the traditional symbol-processing view has mistaken the properties of a complex *cognitive system*, comprising both the individual and the environment, for the properties of an individual mind. They aim to develop an analytical framework in which cognitive processes are not separated from the contexts and activities in which cognition occurs. In this chapter I argue that a promising path to integration of cognitive and sociocultural dimensions of scientific and engineering practices lies in developing studies that both use the research of environmental perspectives on the social–cognitive–cultural nexus and contribute to its development.

The Cartesian Roots of Cognitive and Social Reductionism in STS

What, besides a penchant for rhetorical flourish, could explain such a pronouncement as the 10-year moratorium? One can agree that scientists are human in that they have interests, motivations, and sociocultural loci in conducting research. However, they also have sophisticated cognitive capabilities that historical records and contemporary practices provide strong evidence that they use in doing science. The roots of the position expressed in the 10-year moratorium pronouncement are complex in 20th-century intellectual history in that they arise as a reaction against a mix of issues, including the history of ideas approach to the history of science, the internal-external distinction in history and in sociology of science, the perceived hegemony of philosophical accounts of scientific knowledge, and the logicist “rules and representations” account of thinking of GOFAI analyses of science in early cognitive science. My concern here is with the Cartesian thread that runs through all of these.

The vision of early cognitive studies of science grew out of Herbert Simon’s (Simon, Langley, & Bradshaw, 1981) important idea that scientific discovery involves problem-solving processes that are not different in kind from the problem-solving processes used in mundane circumstances. Coupled with the functionalist assumption of GOFAI, this insight led to attempts to abstract problem solving heuristics, and implement them in AI “scientific discovery” programs capable of making important scientific discoveries, such as was claimed for Kepler’s laws (Langley, Simon, Bradshaw, & Zytkow, 1987) and the Krebs cycle (Kulkarni & Simon 1988). Those who dismiss cognitive explanations countered that when one studies, for example, the practices of high energy particle physicists, knowledge is produced not by what goes on in the mind of a solitary problem solver but by a "network" (Latour, 1987) or "mangle" (Pickering, 1995) of humans, machines, social arrangements, and cultures. Most researchers in contemporary cognitive studies would agree. Discovery programs are post hoc reconstructions. Once a solution is known, there are other ways to derive it. Once the data are known, a discovery program using good heuristics, such as BACON, can derive Kepler's laws. Later programs, such as KEKADA, used significant historical research (Holmes, 1980) to build systems that use many of the heuristics employed by Krebs, and, in this case, novel possible routes to the answer were also "discovered." However, what is missing from these computational accounts are the constructive processes of knowledge development, which are much more complex than simply using the appropriate heuristics. Why someone decides to collect such data, how data are selected as salient, what kinds of experimental devices and instruments are used and constructed for collection and analysis and how these are manipulated, how serendipity can play a role, and so forth, are all critical to constructing the knowledge that makes for a so-called "scientific discovery." However, discovery programs make up only a small fraction of the research in cognitive studies. The nonreductive nature of the social, cultural, and material environment is clear and agreed on in numerous cognitive studies accounts, such as those referenced earlier.

In my own research on Maxwell and the construction of the field concept, for example, I have repeatedly argued that even if one focuses on Maxwell's reasoning processes it matters a great deal to understanding how he derived the mathematical equations that Maxwell was trained in the Scottish geometrical (physical and visual) approach to using mathematics; was trained in Cambridge, England, as a mathematical physicist; was located in a milieu that valued Faraday's theoretical speculations as well as his experimental results, and included teachers and colleagues such as Thomson and his penchant for analogical models; and that he was located in Victorian Britain where, among other factors, there was widespread cultural fascination with machines and mechanisms (Crosbie Smith & Wise, 1989; Davies, 2000; Nersessian, 1984, 1992, 2002b; Siegel, 1991). These sociocultural factors, taken together with cognitive factors, help to explain the nature of the theoretical, experimental, and mathematical knowledge and the methodological practices with which Maxwell formulated the problem and approached its solution. They are reflected in Maxwell's reasoning through mechanical models in deriving the equations, and one cannot understand his construction of these equations without taking these factors into account. Continental physicists working on electromagnetism at the time, such as Ampere, used quite different practices and drew from fundamentally different theoretical assumptions and mathematical and physical representational structures (see, e.g., Hoffman, 1996). Differences in sociocultural factors figure into why members of these communities were not able to derive the field equations. However, one also cannot explain the practices of either community without taking human cognition into account.

Why, then, are cognitive accounts that underscore the importance of sociocultural dimensions not seen as compatible with, or complementary to, sociocultural accounts? One likely issue is that many, though not all, of the cognitive analyses have individual scientists and inventors at their focus. These individuals, though, are conceived as engaging in a sociocultural activity. A Maxwell wrestling alone in his study with a problem is still engaged in a sociocultural process that includes the factors discussed earlier. To find the root of the conflict one needs to consider the issue of what notions of cognition inform the cognitive and the sociocultural sides of the debate.

Cognitive Reductionism

I will begin with the cognitive side, because these accounts make explicit use of cognitive science research. Cognitive studies accounts have been constructed largely without directly challenging the assumptions underlying the traditional cognitive science view of cognition, and this view contains vestiges of a Cartesian mind–body dualism. To connect this analysis with the discussion of environmental perspectives presented in the ENVIRONMENTAL PERSPECTIVES ON COGNITION section, it is useful to focus on the assumptions of the traditional view that are highlighted by these critics. On the traditional view, the cognitive system comprises the *representations* internal to an individual mind and the internal computational *processes* that operate on these. On the functionalist assumption of that view, thinking is "disembodied" in that it is independent of the medium in which it is implemented. Also, although the environment is represented in the content of thinking through being represented in memory, cognitive processing is independent of the social, cultural, and material environment, and thus cognition is not "embedded." Recently, these founding assumptions of cognitive science were reiterated and elaborated on by Alonso Vera and Herbert Simon (1993) in response to criticisms arising from within cognitive science.

In their article, Vera and Simon (1993) argued that the characterization of the traditional view by its critics, as outlined earlier, is a caricature, or at least rests on a misunderstanding of the original claims. They contended that the traditional view does not deny the importance of embodiment and socio-cultural context to cognition —indeed, Simon's (1981, pp. 63–66) early "parable of the ant" recognizes that the complexity in the ant's behavior arises from acting in the environment. Rather, the claim is that what is important about the environment for thinking processes is abstracted through perception and represented in memory by the symbols generated by the cognitive system. The unit of analysis in studying cognition is a “physical symbol system” (see also Simon & Newell, 1972). A physical symbol system has a memory capable of storing and retaining symbols and symbol structures and a set of information processes that form structures as a function of sensory stimuli. In humans, and any natural or artificial physical symbol system with sensory receptors and motor action, sensory stimuli produce symbol structures that cause motor actions and modify symbol structures in memory. Thus, a physical symbol system can interact with the environment by (a) receiving sensory stimuli from it and converting these into symbol structures in memory and (b) acting upon it in ways determined by the symbol structures it produces, such as motor symbols. Perceptual and motor processes connect symbol systems with the environment and provide the semantics for the symbols. Clearly, then, Vera and Simon claimed, cognition is embodied and embedded but also takes place within the individual physical symbol system.

Granting the subtleties of Vera and Simon’s (1993) rearticulation of the traditional view, one can see that it still complies with the Cartesian characterization. First, cognition is independent of the medium in which is it implemented. The physical nature of the patterns that constitute symbols is irrelevant. The processing algorithms are media independent. It makes no difference whether the medium is silicon or organic or anything else. So, ‘mind’ and ‘medium’ are independent categories. Second, the social and cultural environments in which cognition occurs are treated as abstract content on which cognitive processes operate. These dimensions are examined only as sociocultural knowledge residing inside the mind of a human individual or internal to other physical symbol systems.

Sociocultural Reductionism

Turning now to sociocultural studies, the conception of cognition that pervades this side of the perceived divide is largely implicit. It rests on folk notions that are uninformed by research in cognitive science, or even just in psychology. The best way to understand why these accounts reject the explanatory significance of factors pertaining *to* human cognition is to see the rejection as stemming from a tacit understanding of cognition that also retains vestiges of Cartesian dualism. The mind–body, individual–social, and internal-external dichotomies associated with Cartesianism are all in play on the sociocultural side as well, only this time they provide justification for rejecting cognitive explanatory factors—that is, rejecting these distinctions provides the grounds for rejecting cognitive explanations.

As Latour (1999) argued, a cognitive explanation is tantamount to maintaining the epistemological position that the source of knowledge is ideas internal to the mind, where “mind” is a ghostly presence in a physical vessel. Cognitive explanations are cast out in a reactionary response to seeing dualism and GOFAI as providing the only possible ways of understanding ‘mind’ and ‘cognition.’ Reductionism is thus taken in the other direction. Socio-cultural studies replace cognitive reductionism with sociocultural reductionism. Banishing cognitive explanatory factors amounts to “throwing out the baby with the bath water.”

First, cognition is thrown out because it is identified with internal mental processes. Second, there is a disconnect between cognition and behavior. Actions are seen as resulting from the social, cultural, and material environments in which they occur, and from motivations and interests, which are customarily considered noncognitive factors. Cognition is “black boxed” and not part of the explanatory mix in analyzing knowledge construction. Third, the individual is held to be the wrong unit of analysis. In the “actor network,” agency is not located specifically in humans. All actors—human and artifactual—are on equal footing. Cognition is rejected as an explanatory category because, traditionally, it belongs to individuals conceived as loci of solitary mental processing, independent of cultures and communities. These are all indications that an implicit belief that Cartesianism is “the only game in town” underlies sociocultural reductionism.

Rapprochement

Vestiges of Cartesianism on both sides of the divide in STS have been serving to create it. On the one hand, the traditional GOFAI account has not received explicit challenge from researchers in cognitive studies of science and engineering. On the other hand, a Cartesian conception of cognition serves as a basis for rejecting the relevance of cognitive explanatory factors by sociocultural studies. What is needed, instead, is a way of theorizing the cognitive, social, and cultural aspects of practice in relation to one another. Progress toward an integrative account is being hampered by assumptions from which research on both sides of the divide, in fact, points away. On the one side, the best way of reading the cumulative results of observational and cognitive-historical research in cognitive studies is as providing a challenge to the notion that the social, cultural, and material worlds of practice can be reduced to a few parameters in a traditional account of cognition. On the other side, the moratorium has ended. Indeed, even Latour (1999) has made good on his original promise (Latour 1987, p. 247) to “turn to the mind” if anything remained to be explained after the 10-year period. He has turned to the mind in order to discuss the relativism and realism debate in the “science wars,” but what he says primatology, and neuroscience to argue his case. One aspect of this account reinforces the notion that not all cognitive processing need be of internal representations. External representations are indispensable in complex human thinking, and their development has been central to the processes of cultural transmission. Donald's analysis of the evolutionary emergence of distinctively human representational systems starts from the significance *of mimesis*—or re-creation, such as using the body to represent an idea of the motion of an airplane—in the developments of such external representations as painting and drawing (40,000 years ago), writing (6,000 years ago) and phonetic alphabets (4,000 years ago). He argues for a distributed notion of memory as a symbiosis of internal and external representation on the basis of changes in the visuo–spatial architecture of human cognition that came about with the development of external representation. On this account, affordances and constraints in the environment are, *ab initio*, part of cognitive processing.

Research into the relations between culture and cognition, together with neuroscience research into cognitive development, can be construed as moving beyond the old nature–nurture debate and developing an *interactionist* approach. It attempts to provide an account of how evolutionary endowment and sociocultural context act together to shape human cognitive development. In support of this conception, neuroscience studies of the impact of sociocultural deprivation, enrichment, and trauma on brain structure and processes lead to a conception of the brain as possessing significant cortical plasticity and as a structure whose development takes place in response to the sociocultural environment as well as to genetic inheritance and biological evolution (see, e.g., Elman et al., 1998; van der Kolk, McFarlane, & Weisaeth, 1996).

Finally, in so connecting cognition and culture, this body of research indicates that human cognition should display both universal and culturally specific characteristics. Tomasello (1999, pp. 161–163) discussed some of the universal learning abilities, such as those connected with language learning; these include the ability to understand communicative intentions, to use role reversal to reproduce linguistic symbols and constructions, and to use linguistic symbols for contrasting and sharing perspectives in discourse interactions. Recent investigations by Richard Nisbett and his colleagues (Nisbett et al., 2001) provide evidence of culturally specific features of cognition. Their research examined learning, reasoning, problem solving, representation, and decision making for such features. This research was also inspired by the substantial body of historical scholarship that maintains that there were systematic cultural differences between ancient Greek and Chinese societies, especially concerning what Nisbett et al. (2001) call the "sense of personal *agency*" (p. 292). Nisbett et al. hypothesized that these kinds of differences between so-called Eastern and Western cultures, broadly characterized as holistic versus analytic thinking (p. 293), should be detectable in a wide range of cognitive processes, such as categorization, memory, covariation detection, and problem solving.

The comparative contemporary cultures in Nisbett et al.'s (2001) study are those whose development has been influenced either by ancient China (China, Japan, and Korea) or by ancient Greece (western Europe and North America). In a series of experiments with participants from east Asian and Western cultures, and participants whose families had changed cultural location, Nisbett et al. examined explanations, problem solving, and argument evaluation. Some significant systematic differences were found along the five dimensions they identified in the ancient cultures (in the order Eastern vs. Western): (a) focusing on continuity versus on discreteness, (b) focusing on field versus on object, (c) using relations and similarities versus using categories and rules, (d) using dialectics in reasoning versus using logical inference from assumptions and first principles, and (e) using experienced-based knowledge in explanations versus using abstract analysis. Although Nisbett et al.'s grouping of very diverse cultures into such gross categories as "Eastern" and "Western" is problematic, the general results are intriguing and promise to lead to further research into the issue of culturally specific features of cognition.

Environmental Perspectives and the Integration Problem

Situating the problem of interpreting scientific and engineering practices with respect to the framework provided by environmental perspectives on cognition affords the possibility of analyzing the practices from the outset as bearing the imprint of human cognitive development, the imprint of the sociocultural histories of the localities in which science is practiced, and the imprint of the wider societies in which science and technology develop. The implications of the growing body of environmental-perspectives research for the project of constructing integrative accounts of knowledge-producing practices in science and engineering are extensive. Working them out in detail is beyond the scope of any one chapter. One approach to exploring the implications would be to recast some of the analyses in the literatures of both cognitive studies and sociocultural studies of science and engineering in light of it. Here, for example, I am thinking of such research as by Cetina, Galison, Giere, Gooding, Gorman, Latour, Rheinberger, Tweney, and myself, cited earlier.

Another approach would be to undertake new research projects that aim from the outset at integration. In the next section I offer my current research project on interpreting knowledge-producing practices in biomedical engineering research laboratories as an exemplar of an integrative approach. This project combines ethnographic studies with cognitive-historical analyses to examine reasoning and representational practices. My colleagues and I are examining these research practices at all of the levels of analysis noted by Greeno (1998) for situated cognitive systems: at the level of researchers as individual, embodied, social, tool-using agents; at the level of groups of researchers; at the level of the material and conceptual artifacts of the context of laboratory activities; and at various combinations of these.

Research Laboratories as Evolving Distributed Cognitive Systems

Science and engineering research laboratories are prime locations for studying the social–cognitive–cultural nexus in knowledge-producing practices. Extensive STS research has established that laboratory practices are located in rich social, cultural, and material environments. However, these practices make use of sophisticated cognition in addressing research problems. In this section I discuss some features of my current research project that has among its aims the interpretation of reasoning and representational practices used in problem solving in biomedical engineering (BME) laboratories. The research both appropriates and contributes to research within the environmental perspectives discussed in the previous section. My colleagues and I do not adopt or apply any particular theory but rather use a cross-section of that thinking about the nature of cognition as a means of framing our investigation into these research practices. We are influenced also by research on both sides of the supposed divide in STS. As a contribution to STS, specifically, we aim to develop analyses of the creation of BME knowledge in which the cognitive and the sociocultural dimensions are integrated analytically from the outset. Our focus is on the cognitive practices, but we analyze cognition in BME laboratories as situated in localized reasoning and representational practices. This is collaborative research that would not be possible without an interdisciplinary team.[3] The case study has been underway for less than 2 years, so the analysis presented here is preliminary. Nevertheless, it provides a useful exemplar of how integration might be achieved.

We have begun working in multiple sites, but here I discuss a specific tissue engineering laboratory, Laboratory A, that has as its ultimate objective the eventual development of artificial blood vessels. The daily research is directed toward solving problems that are smaller pieces of that grand objective. Our aim is to develop an understanding of (a) the nature of reasoning and problem solving in the laboratory;

(b) the kinds of representations, tools, forms of discourse, and activities used in creating and using knowledge; (c) how these support the ongoing research practices; and (d) the nature of the challenges faced by new researchers as they are apprenticed to the work of the laboratory.

We conceive of and examine the problem-solving activities in Laboratory A as *situated* and *distributed*. These activities are situated in that they lie in localized interactions among humans and among humans and technological artifacts. They are distributed in that they take place across systems of humans and artifacts. BME is an *interdisdpline* in that melding of knowledge and practices from more than one discipline occurs continually, and significantly new ways of thinking and working are emerging. Most important for our purposes is that innovation in technology and laboratory practices happens frequently, and learning, development, and change in researchers are constant features of the laboratory environment. Thus, we characterize the laboratory as comprising "evolving distributed cognitive systems." The characterization of the cognitive systems as *evolving* adds a novel dimension to the existing literature on distributed cognition, which by and large has not examined these kinds of creative activities.

Investigating and interpreting the cognitive systems in the laboratory has required innovation, too, on the part of our group of researchers studying the laboratory. To date, ethnography has been the primary method for investigating situated cognitive practices in distributed systems. As a method it does not, however, suffice to capture the critical *historical* dimension of the research laboratory: the evolution of technology, researchers, and problem situations over time that are central in interpreting the practices. To capture the evolving dimension of the laboratory, we have developed a mixed-method approach that includes both ethnography and cognitive-historical analysis.

A Mixed-Method Approach to Investigating Evolving Distributed Cognitive Systems

None of the conceptions of distributed cognition in the current literature account for systems that have an evolving nature. In Hutchins's (1995) studies of distributed cognition in work environments—for instance, the cockpit of an airplane or on board a ship—the problem-solving situations change in time. The problems faced, for example, by the pilot change as she is in the process of landing the plane or bringing a ship into the harbor. However, the nature of the technology and the knowledge that the pilot and crew bring to bear in those processes are, by and large, stable. Even though the technological artifacts have a history within the field of navigation, such as the ones Hutchins documented for the instruments aboard a ship, these do not change in the day-to-day problem-solving processes on board. Thus, these kinds of cognitive systems are dynamic but largely *synchronic*. In contrast, we are studying cognition in innovative, creative settings, where artifacts and understandings are undergoing change over time. The cognitive systems of the BME research laboratory are, thus, dynamic and *diachronic*. Although there are loci of stability, during problem-solving processes the components of the systems undergo development and change over time. The technology and the researchers have evolving, *relational* trajectories that must be factored into understanding the cognitive system at any point in time. To capture the evolving dimension of the case study we have been conducting both cognitive–historical analyses of the problems, technology, models, and humans involved in the research and ethnographic analyses of the day-to-day practices in the laboratory.

Ethnographic analysis seeks to uncover the situated activities, tools, and interpretive frameworks used in an environment that support the work and the ongoing meaning-making of a community. Ethnography of science and engineering practices aims to describe and interpret the relations between observed practices and the social, cultural, and material contexts in which they occur. Our ethnographic study of the BME laboratory develops traces of transient and stable arrangements of the components of the cognitive systems, such as evidenced in laboratory routines, the organization of the workspace, the artifacts in use, and the social organization of the laboratory at a particular point in time, as they unfold in the daily research activities and ground those activities. Ethnographic studies of situated sociocultural practices of science and engineering are abundant in STS (see, e.g., Bucciarelli, 1994; Latour & Woolgar, 1986; Lynch, 1985). However, studies that focus on situated *cognitive* practices are few in number in either STS or in cognitive science. Furthermore, existing observational (Dunbar, 1995) and ethnographic studies (see, e.g., Goodwin, 1995; Hall, Stevens, & Torralba, in press; Ochs & Jacoby, 1997) of scientific cognition lack attention to the historical dimension that we find important to our case study.

Cognitive–historical analysis enables one to follow trajectories of the human and technological components of a cognitive system on multiple levels, including their physical shaping and reshaping in response to problems, their changing contributions to the models that are developed in the laboratory and the wider community, and the nature of the concepts that are at play in the research activity at any particular time.[4] As with other cognitive-historical analyses, we use the customary range of historical records to recover how the representational, methodological, and reasoning practices have been developed and used by the BME researchers. The practices can be examined over time spans of varying length, ranging from shorter that comprise people, technology, techniques, knowledge resources (e.g., articles, books, artifacts, the Internet), problems, and relationships. Construed in this way, the notion of "problem space" takes on an expanded meaning from that customarily used in the traditional cognitive science characterization of problem solving as a search through an *internally* represented problem space. Here the problem space comprises both. Researchers and artifacts move back and forth between the wider community and the physical space of the laboratory. Thus the problem space has permeable boundaries.

For instance, among the most notable and recent artifacts (initiated in 1996) in Laboratory A are the tubular- shaped, bioengineered cell-seeded vascular grafts, locally called *constructs* (see Fig. 2.2). These are physical models of native blood vessels engineered to eventually function as viable implants for the human vascular system. The endothelial cells the laboratory uses in seeding constructs are obtained by researchers traveling to a distant medical school and bringing them into the problem space of the laboratory. On occasion, the constructs or substrates of constructs travel with laboratory members to places outside of the laboratory. Recently, for example, one of the graduate students has been taking substrates of constructs to a laboratory at a nearby medical school that has the elaborate instrumentation to perform certain kinds of genetic analysis (microarrays). This line of research is dependent on resources that are currently available only outside Laboratory A in the literal, spatial sense. The information produced in this locale is brought into the problem space of the laboratory by the researcher and figures in the further problem- solving activities of the laboratory.

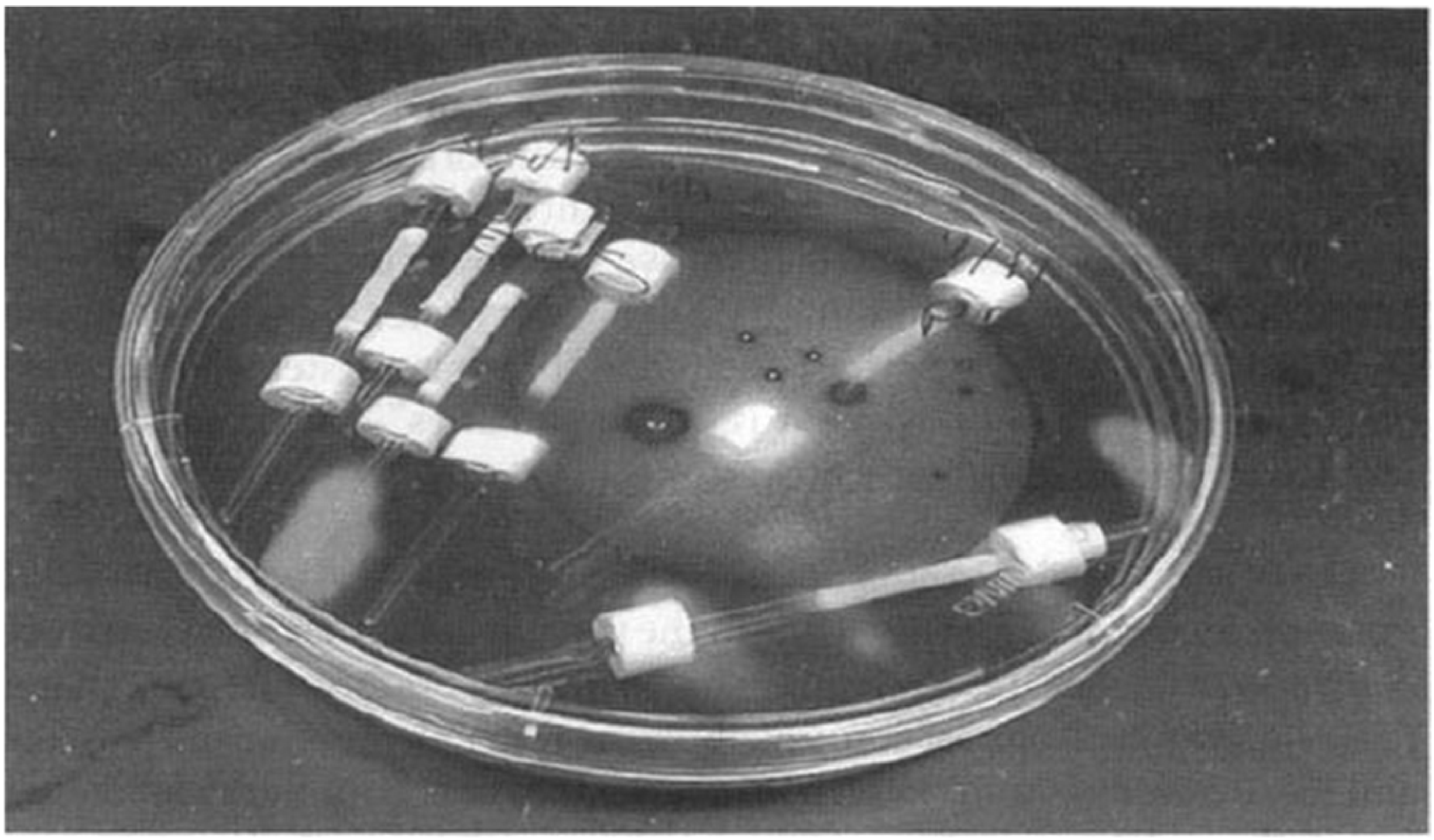

Fig. 2.2. Photograph of a Dish of Vascular Constructs.

Following Hutchins (1995), my colleagues and I analyze the cognitive processes implicated in a problem-solving episode as residing in a *cognitive system* comprising both one or more researchers and the *cognitive artifacts* involved in the episode (see also Norman, 1991). In line with his analysis, a *cognitive system* is understood to be sociotechnical in nature, and *cognitive artifacts* are material media possessing the cognitive properties of generating, manipulating, or propagating representations.[5] So, right from the outset, the systems within the laboratory are analyzed as social-cognitive-cultural in nature. Determining the cognitive artifacts within any cognitive system involves issues of agency and intention that are pressing questions for cognitive science research, both in the development of the theoretical foundations of distributed cognition and in relation to a specific case study. On our analysis, not all parts of the cognitive system are equal. Only the researchers have agency and intentions, which enable the cognitive activities of specific artifacts.

Our approach to better understanding such issues is to focus on the technology used in experimentation. During a research meeting with the laboratory members, including the PI, we asked them to sort the material artifacts in the laboratory according to categories of their own devising and rank the importance of the various pieces to their research. Their classification in terms of "devices," "instruments," and "equipment" is represented in Table 2.1. Much to the surprise of the PI, the newer PhD students initially wanted to rank some of the equipment, such as the pipette, as the most important for their research, whereas for the PI and the more senior researchers deemed the devices the laboratory engineers for simulation purposes as most important to the research. Additional ethnographic observations have led us to formulate working definitions of the categories used by Laboratory A's researchers. *Devices* are engineered facsimiles that serve as *in vitro* models and sites of simulation;[6] *instruments* generate measured output in visual, quantitative, or graphical form; and *equipment* assists with manual or mental labor.

Table 2.1 Sorting of Laboratory Artifacts by the Laboratory Members

order to improve their overall mechanical properties. The researchers call this process *mechanical conditioning*—or, as one researcher put it, "exercising the cells." This preferably is done at an early stage of the formation of the construct, shortly after seeding the cells onto a prepared tubular silicon sleeve. *In vivo,* arterial wall motion is conditioned on pulsatile blood flow. With the bioreactor, though, which consists of a rectangular reservoir containing a fluid medium (blood-mimicking fluid) in which the tubular constructs are immersed and connected to inlet and outlet ports off the walls of the reservoir, "fluid doesn't actually move," as one laboratory member put it, "which is somewhat different from the actual, uh, you know, real life situation that flows." The sleeves are inflated with pressurized culture medium, under pneumatic control (produced by an air pump). The medium functions as an incompressible fluid, similar to blood. By pressurizing the medium within the sleeves, the diameter of the silicon sleeve is changed, producing strain on the cells, similar to that experienced *in vivo*. The bioreactor is thus a functional model of pulsatile blood flow, and needs to be understood by the researcher as such.

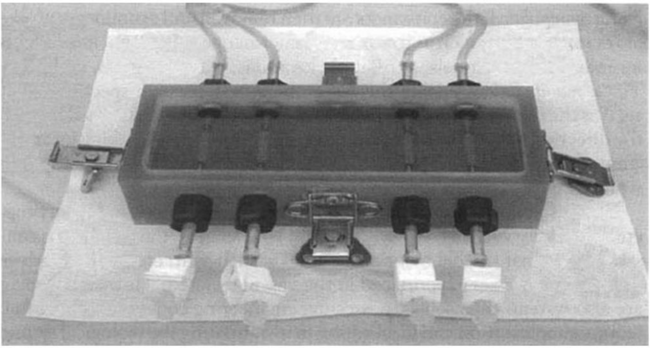

Fig. 2.3. Photograph of a Bioreactor.

Distributed Mental Modeling. Significant to our reconceiving the internal-external distinction is that the problem space comprises mental models and physical artifacts together with a repertoire of activities in which simulative model-based reasoning assumes a central place. Many instances of model-based reasoning in science and engineering use external representations that are constructed during the reasoning process, such as diagrams, sketches, and physical models. In line with the discussion of such representations in the ENVIRONMENTAL PERSPECTIVES ON COGNITION section, these can be seen to provide constraints and affordances essential to problem solving that augment those available in whatever internal representations are used by the reasoner during the process. In this way, "cognitive capabilities" are understood to encompass more than "natural" capabilities. The devices used in Laboratory A are physical models used in the problem solving. Within the cognitive systems in the laboratory, then, devices instantiate part of the current community model of the phenomena and allow simulation and manipulation. The intent of the simulation is to create new situations *in vitro* that parallel potential *in vivo* situations.

One researcher we interviewed called the processes of constructing and manipulating these *in vitro* sites "putting a thought into the bench top and seeing whether it works or not." These instantiated "thoughts" allow researchers to perform controlled simulations of an *in vivo* context—for example, of the local forces at work in the artery. The "bench top," as one researcher explained, is not the flat table surface but comprises all the locales where experimentation takes place. In previous research, I (Nersessian, 1999, 2002a) have characterized the reasoning involved in simulative model-based reasoning as a form of dynamic mental modeling, possibly using iconic representations. There the focus was on thought experiments, and that analysis used the notion of a mental model in the traditional manner as referring to an internal object of thought. In the current research, I am expanding the notion of simulating a mental model to comprise both what are customarily held to be the internal thought processes of the human agent and the processing of the external device. Simulative model-based reasoning involves a process of coconstructing the "internal" researcher models of the phenomena and of the device and the "external" model that is the device, each incomplete. Understood in this way, simulating the mental model would consist of processing information both in memory and in the environment; that is, the mental modeling process is distributed in the cognitive system.[8]

Cognitive Partnerships. Our account of the distributed cognitive systems in the laboratory characterizes cognition in terms of the lived relationships among the components of these systems, people, and artifacts. In Laboratory A these relationships develop in significant ways for the individual laboratory members and for the community as a whole. Newcomers to the laboratory, who are seeking to find their place in the evolving system, initially encounter the cognitive artifacts as materially circumscribed objects. For example, one new undergraduate who was about to use the *mechanical tester*, an instrument for testing the strength of the constructs (see Fig. 2.4), responded to our query about the technology she was going to use in her research project:

participation.

The cognitive partnerships transform both researcher and artifact. A researcher who some months earlier was a newcomer and who saw the artifacts as just many kinds of machines and objects piled on shelves and on the bench top now can see a device as an *in vitro* site for "putting a thought [his/ her thought] into the bench top and seeing whether it works or not." During the problem-solving processes involved in instantiating a thought and seeing if it works, devices are re-engineered, as exemplified above with the flow loop. Re-engineering is possible because the researcher with a developed partnership appropriates and participates in the history of a device. A senior PhD researcher, at that point in time, considered the "resident expert" on the bioreactor, was able easily to reconstruct some of his lived relationship with it and some of its history within this laboratory:

> I: Do you sometimes go back and make modifications? Does that mean you have some generations of this?
>
> … Uh yes I do. The first generation and the second generation or an off-shoot I guess of the first generation. Well the first one I made was to do mechanical loading and perfusion. And then we realized that perfusion was a much more intricate problem than we had—or interesting thing to look at—than we had guessed. And so we decided okay we will make a bioreactor that just does perfusion on a smaller scale, doesn't take up much space, can be used more easily, can have a larger number of replicates, and so I came up with this idea.

He continued by pulling down previous versions of bioreactor (made by earlier researchers as well) and explaining the modifications and problems for which design changes were made. His account suggests a developed partnership.

Furthermore, in developed partnerships potential device transformations can be envisioned, as with one undergraduate research scholar we interviewed about the bioreactor:

> A16: I wish we could accomplish—would be to actually suture the actual construct in there somehow. To find a way not to use the silicon sleeve…. That would really be neat. Um, simply because the silicon sleeves add the next level of doubt. They're—they are a variable thing that we use, they're not always 100% consistent. Um the construct itself is not actually seeing the pressure that the sleeve does. And because of that you know, it doesn't actually see a—a pressure. It feels the distension but it doesn't really feel the pressure. It doesn't have to withstand the pressure. That's the whole idea of the sleeve. And so, um, I think that it would provide a little bit more realism to it. And uh, because that also, a surgeon would actually want to suture the construct into a patient. And um, because of that you're also mimicking the patient as well—if you actually have the construct in the path. I think another thing is to actually have the flow because um, so this flow wouldn't be

> important with just the sleeve in there. But if you had the construct in contact with the—with the liquid that's on the inside, you could actually start to flow media through there.

In this case an undergraduate student has been transformed over the course of several semesters to a BME researcher, contributing to immediate research goals; who transforms artifacts in his immediate research; who understands the outstanding problems and objectives; and who can envision how a device might change from a functional model to a model more closely paralleling the *in vivo* situation to push the research along. At this point in evolution, thinking is taking place through the cognitive partnering of the researcher and device. In their established form, relationships with artifacts entail cognitive partnerships that live in *interlocking* models performing internally as well as externally.

Implications of the Exemplar for Integration

Our approach to interpreting the knowledge-producing practices in the laboratory contributes to the project of developing means of interpreting cognitive, social, and cultural dimensions of practice in relation to one another. By starting from the perspective that cognition is embedded in complex environments, the laboratory's innovative problem-solving practices are interpreted as social–cognitive–cultural from the outset. The mixed methodology enables both thick descriptions of specific systems and hypotheses about "the nature of cognition in human activity" that go beyond the specifics of the laboratory under study. Consider the outline of our analysis of the flow loop. It is a major cognitive tool developed and used in the model-based reasoning in this laboratory. It is a significant cultural artifact, originating in the research program of the PI and then passed down through generations[9] of researchers, enabling each to build on the research of others, while sometimes being re-engineered as an artifact in the service of model-based reasoning. It is a locus for social interaction, such as that involved in learning and didactical interaction between mentor and apprentice. At one point it served as the vehicle for initiation into the community of practice, although at present cell culturing serves this purpose, because the problem situation has evolved, and now the flow loop is no longer the only experimental device. On the one hand, the histories of the lived relations among the flow loop and researchers can be developed into thick social–cognitive–cultural descriptions of the evolving systems of the laboratory. On the other hand, understanding the role of the flow loop as a device—a cognitive artifact for performing simulative model-based reasoning in the problem-solving activities within the distributed cognitive systems of the laboratory— leads to hypotheses about the