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# What Scientific Thinking Reveals About the Nature of Cognition

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The mental processes underlying scientific thinking and discovery have been investigated by cognitive psychologists, educators, and creativity researchers for over half a century. Despite this wide interest in scientific thinking, the field has been regarded as an intriguing offshoot of cognitive psychology that provides scientific cover stories for theories of analogy, concept acquisition, problem solving, and cognitive development rather than revealing something new about the nature of cognition. In this chapter, I will provide a summary of recent research that we have conducted on scientific thinking and I will argue that this research provides a new perspective on the ways that basic cognitive processes function. Thus, scientific thinking allows us to understand complex cognition and generate new models of basic cognitive processes that have eluded researchers using arbitrary and simple stimuli that are commonly used in psychology experiments. Rather than being an offshoot of mainstream cognitive theories, I will propose that scientific thinking is a paradigmatic example of cognition that reveals the key features of many basic cognitive processes. Recent research on the nature of scientific thinking has implications not only for theories of cognition, but also for both the content and practice of science education.

### **Part I: The In Vivo/In Vitro Approach to Cognition**

One important issue for understanding scientific thinking is to determine an appropriate strategy for discovering the types of reasoning processes that people use when they reason scientifically. The traditional cognitive

approach has been for researchers to introspect on what is important in science, such as concept formation, and then design a task that can be used to investigate this process (e.g., Bruner, Goodnow, & Austin, 1956). Using this approach, researchers interested in scientific thinking have investigated many different issues, such as the forming of concepts, analogical reasoning, and problem solving (for reviews see Dunbar, 1999a; 1999b; Klahr & Simon, 1999). While this approach has been very successful, and we now have many excellent models of these types of cognitive processes, the results of basic research on scientific thinking are somewhat puzzling: Much cognitive research and research on scientific thinking has demonstrated that human beings are prone to making many different types of reasoning errors and possess numerous biases. For example, people tend to consider only one hypothesis at a time, only use analogies that are superficially similar to the problem that they are currently working on, and ignore important information when reasoning causally (Feist & Gorman, 1998; Tweney & Chitwood, 1995; Tweney, Doherty, & Mynatt, 1982). Sadly, informing subjects of these biases does little to improve performance, and even teaching subjects strategies for overcoming them does little to ameliorate these biases either. In fact, even a brief perusal of an introductory cognitive psychology book reveals that much research on human thinking and reasoning shows that thinking is so error-prone that it would appear unlikely that scientists would make any discoveries at all!

How should we interpret these findings? One answer that is frequently offered is that people in cognitive experiments are often novices and that the errors that they make are due to their lack of expertise. Research on expertise has indeed shown that there are many important differences between experts and novices and that these differences in knowledge representation and problem solving strategies are very important (Ericsson & Smith, 1991). Thus, it may be the case that for any type of successful thinking and reasoning to occur people must have expertise in the domain. In standard psychological experiments, subjects are usually non-experts in the tasks given to them. The subjects are often given arbitrary stimuli, no knowledge of a domain, and an arbitrary task to perform. In these contexts, it may not be possible for subjects to use the standard cognitive machinery that they use in their normal lives. Thus, research in cognition may have found out about two extreme cases: knowledge-rich—with experts—and knowledge-lean situations—with novices. The majority of people are probably not in either of these two camps, most of what we reason about, such as why my car will not start, or why my pasta takes so long to boil is dependent upon some knowledge—definitely not that of

an expert, but not arbitrary. This is a well recognized issue in cognitive research and many researchers have formulated different approaches towards generating cognitive theories that are more “ecologically valid.” (Bartlett, 1932; Brunswik, 1943; Cole, 1996; Gibson, 1979; Neisser, 1976). More recently, many researchers have argued that factors of the context in which experiments are conducted are very different in cognitive experiments and naturalistic situations. These researchers have argued that it is important to investigate cognition in natural situations, as results from cognitive experiments are inherently misleading (Lave & Wegener, 1991; Suchman, 1987). Advocates of this approach thus stress the contexts within which reasoning occurs and have recommended that we turn to naturalistic settings and situations to understand human cognition. Furthermore, many advocates of the situated approach have argued that cognitive researchers have ignored the most important aspects of what determines cognition—the environment. Thus, many of the advocates of the situated approach have argued that the experimental approach is bankrupt (see Seifert, 1999 for a summary of the situated approach).

While the situated cognition approach tackles some important problems with traditional cognitive experiments, and very clearly brings into focus many of the problems with the traditional approach, do we need to throw out experimentation entirely? This is an important issue for all educators and cognitive researchers and is particularly poignant for science educators as a large component of what is taught in science classes is through experimentation. In my lab we have developed another approach to understanding human cognition that we argue preserves the best features of the situated and experimental approaches: Rather than using only experiments, or observing only naturalistic situations, it is possible to use both approaches to understanding the same phenomena. An analogy for this can be found in the research practices of current day biologists. In much biological research, scientists conduct both “*in vivo*” and “*in vitro*” research. For example, in HIV research, the researchers investigate parts of the virus in a petri dish, outside of a host organism. This is their *in vitro* research. The same researchers also investigate the virus *in vivo* when it infects a host organism. Using both the *in vivo* and *in vitro* approaches the scientists build a more complete model of the way that the virus functions. Both approaches compliment each other and have profound effects on the research conducted. *In vitro* and *in vivo* approaches can be also used in cognitive research in general and scientific thinking in particular.

What we have been doing in my laboratory for the past decade is to pursue an *in vivo/in vitro* approach to scientific thinking and reasoning.

We have observed and analyzed scientists as they think and reason “live” at laboratory meetings. This is the *in vivo* cognition component of the research. We have then gone back into the cognitive laboratory and conducted experiments on the cognitive processes that we have identified *in vivo*. This is the *in vitro* component of the research. Using this two-pronged approach, we have found that *in vivo* scientists make far less of the reasoning errors that have been identified in the psychological laboratory, and use new reasoning strategies that had not previously been identified. When we have gone back into the psychological laboratory we have been able to replicate what the scientists do. We have also been able to discover some of the reasons why previous researchers have not seen these cognitive processes used in more traditional experiments. Furthermore, by comparing expert scientists with non-expert subjects in our laboratory, we also investigate how important expertise is to some of the reasoning strategies that the scientists use. In this chapter, I will show how we have used this approach to understand aspects of analogical reasoning and causal reasoning. I will then draw some general conclusions on what this approach reveals about the nature of cognition and how these findings can be applied in educational settings.

### **The In Vivo/In Vitro Method**

A key feature of the *in vivo/in vitro* method is that we can investigate a question in a naturalistic situation and then go back into the psychological laboratory and conduct controlled experiments on what has been identified in the naturalistic settings. Here, by observing naturalistic settings, new issues, topics, and ideas can be identified which have not been the focus of traditional experimental approach. Thus, the *in vivo* research can introduce new issues into a field and has the potential to radically shift the types of questions and theories that a field holds. By going from naturalistic settings with a multitude of factors interacting back into the cognitive laboratory, it is possible to avoid the problem of extrapolating from an artificial experimental task that has little to do with what naturally occurs in cognition. Thus, by using the *in vivo* approach it is possible to increase the likelihood that important real-world cognitive phenomena are investigated (see Cole, 1996 for a discussion of similar issues in relation to education).

The most important feature of the *in vivo* method (Dunbar, 1993; 1995; 1997a; 1999c) is that cognition is observed in its naturalistic con-

text. Rather than isolating one small component of cognition, cognition is investigated in a naturally occurring situation; what this means is that the whole gamut of psychological processes are observed. The first step in conducting this type of research is to identify a naturally occurring situation where the topic of interest would occur. In my case, I was seeking to understand the ways that scientists think, reason, and make discoveries. I picked a field in which many discoveries are being made—molecular biology. Many of the brightest and most creative minds in science are attracted to this field, and molecular biology has now taken over the biological and medical sciences as the major way of theorizing and as a set of methodologies. As a consequence, the field of molecular biology is undergoing an immense period of scientific discovery and breakthroughs, making it an ideal domain within which to investigate the scientific discovery process. Having identified molecular biology as a scientific domain, I then identified leading laboratories in the United States that I could investigate. My goal was to investigate the thinking and reasoning strategies that leading scientists use while conducting their research. I consulted with a number of leading scientists, including one Nobel Prize winner, and also conducted an extensive search of the literature. I identified six laboratories at major U. S. universities, headed by scientists with an international reputation. Each scientist had conducted path-breaking research that changed theory, methods, and approaches in their fields. Each scientist was concerned with discovering new biological mechanisms that give fundamental insights into biology. Once I had selected the six labs, I then asked the scientists for permission to investigate them. All of the laboratory directors agreed to participate in the study. I then conducted extensive interviews with the scientists on what their theories and methods were, their current research projects, and what their managerial styles and discovery heuristics were. This interview made it possible for me to select four labs for further investigation.

My initial *in vivo* work thus focused on four labs in the United States. Subsequently we have used the same method for investigating laboratories in Canada and Italy. We have also investigated labs with many women scientists, labs that are all male, and a leading lab that had only women scientists. Thus, we are using the *in vivo* method to explore cognitive, social, cultural, and gender issues in science.

Having identified molecular biology as the domain to investigate, and having selected four labs to investigate, I had to determine what was the best way to investigate scientific discovery in these labs. The first few months were spent visiting the labs every day, chatting with the scientists

and gaining their trust. At this point I was searching for the most naturalistic way of observing the scientific mind at work. What I discovered was that a place where scientists displayed a wide range of reasoning abilities was at the weekly lab meeting. All of the labs have weekly lab meetings that involve the professor who runs the lab, post-doctoral fellows, graduate students, and technicians. The usual lab meeting consists of a scientist presenting her or his latest research. Members of the lab ask questions about the research, propose new experiments, hypotheses and interpretations, often forcing the presenting scientist to reconceptualize his or her ideas. At some meetings totally new concepts are generated and modified by members of the laboratory. Often the senior scientist who runs the laboratory plays a crucial role in the development of new ideas and concepts at the meeting. The scientists' reasoning at lab meetings is often spontaneous and the "live" interactions concern some of the most creative moments in science.

The finding that lab meetings are a key place in which scientific thinking and reasoning takes place is important because the reasoning that occurs at these meetings occurs through presentations and spontaneous interactions. Because the scientists are talking out loud there is an external record of thinking and reasoning. The scientists externalize much of their thinking through interactions with other scientists in the lab. Thus by recording laboratory meetings it is possible to gain access to "live" thinking and reasoning without influencing the way that the scientists think, as might be the case with asking an individual scientist to give a verbal protocol. While it is certainly the case that scientific thinking and reasoning occurs outside the laboratory meeting, what I found is that the laboratory meeting provides a representative cross-section of the cognitive processes that occur in working alone, dyadic interactions, and hallway conversations. I found that the laboratory meetings provide a much more accurate picture of the conceptual life of a laboratory than interviews, lab books, or papers. In fact I found that the scientists were often unable to remember the steps in the development of a particular concept. The laboratory meetings provided a far more veridical and complete record of the evolution of ideas than other sources of information. Having collected data from interviews and lab meetings, I used the laboratory meetings as the core source of data and the interviews and papers as supplemental sources of information.

The particular method that I used to collect data revolved around the discovery that the laboratory meetings are central to the conceptual life of a laboratory. I designed a pre-present-post design for uncovering the

effects of laboratory meetings on the scientists' theories and methods: Prior to a lab meeting I interviewed the scientist to find out what their hypotheses were, what they thought the data meant, and what they were going to do next. Then I recorded the lab meeting (videotape or audiotape). After the lab meeting I interviewed the scientist to determine whether the lab meeting had an effect on their knowledge. I also interviewed the senior scientist on their conceptualization of the research project. This was a cyclical process in which I observed the scientist present work a number of times. In addition to recording laboratory meetings, I conducted interviews with members of the laboratory, was given copies of grant proposals, drafts of papers, attended lectures by the senior scientists, and attended many impromptu meetings. Thus I collected data on all aspects of scientific research with the central focus being the laboratory meetings.<sup>1</sup>

### A Typical Lab Meeting

A typical lab meeting consists of the director of the lab, two to four post-doctoral researchers, three to six graduate students, and two or three technicians (though we do have data from a lab with 22 postdocs!). Often the lab meetings have two components. In one component, the scientists discuss the day-to-day chores of the labs such as ordering reagents, who didn't clean up, and who should do what job to keep the lab running smoothly. This component of a lab meeting lasts approximately ten to fifteen minutes. Then the scientists spend the rest of the meeting discussing research. The most common style is for one of the scientists to present a set of findings. Often they are asked questions about what a finding is, are challenged about their interpretation of the results, and the theoretical interpretation of the results is often discussed. Much of the time the scientists have unexpected findings, ambiguous results, and uninterpretable data. Discussion of these issues are at the core of the lab meetings and can

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1. While the scientists did request anonymity, it is important to note that all the scientists allowed free access to their laboratories, to interview anyone in the laboratory, attend any meeting, read and keep copies of their grant proposals (including the pink sheets), attend their talks and lectures, and read drafts of their papers. Thus, there was complete access to the day-to-day activities of laboratories. In addition, the laboratories were so cooperative that they frequently phoned me to attend impromptu meetings and discussions within the laboratory, or they would call me to come over when they felt that interesting events were occurring in the lab. Because of the request for anonymity, I have changed any identifying details from the excerpts of lab meetings presented here.

result in major new theories and discoveries. A number of discoveries occurred “live” at these meetings. What is important for cognitive researchers is that the discussions at these meetings are spontaneous, and that the whole range of cognitive and scientific reasoning strategies that scientists use in their day-to-day research can be observed. Another type of lab meeting that we have observed is where more than one lab member will speak. These types of meetings are often very problem oriented—where the members of the lab help solve conceptual, methodological, or analytic problems in a research project.

To provide a flavor of a lab meeting, I will now discuss a meeting where a postdoc presented her latest experiments on a parasite. The lab meeting lasted 90 minutes. The postdoc presented, and four other postdocs, two graduate students, a technician, and the professor who runs the lab were also present. The lab meeting was audiotaped and I had interviewed the presenting postdoc the day before the meeting. This lab meeting is typical in the sense that no earth-shattering event occurred at the meeting. While there were a few meetings where major conceptual changes occurred, these types of meetings are not representative. The excerpts here are representative and show a scientist formulating hypotheses, drawing analogies, having difficulty interpreting data, and designing further experiments. This is the standard approach of most scientific labs. She was conducting research on the ways that the parasite enters the host. Her goal was to identify the genes that are responsible for the parasite entering the host. At the lab meeting she discussed five different research projects. For each project she gave a statement of her goals, briefly mentions the method, and gives the results. Other members of the lab frequently asked questions, gave suggestions, and reasoned about her findings.

The postdoc initially formed hypotheses by making an analogy with malaria and by unpacking the analogy until she has both a specific hypothesis and a specific method for investigating the hypothesis that she mapped over from other peoples’ research on malaria. She initially gave background for her experiment. She identified the mitochondria as an important component of the invasion process:

“The Mitochondria, vary very much in sizes in nuclear membranes. It has not been well characterized in Para...and uh nobody really knows a lot about it. But ah, in malaria I tested. It has been extensively studied. And Uh their genome is unexpectedly complex. You have not only a, a, nucleus, uh information, but has two um, organular genomes. This is a linear one, 8Kb molecules, which contain two protected genes that are

usually found in mitochondrial genomes (she is pointing to a gel). So that people believe that this is a mitochondrial genome. And beside that there is a 40kb molecule which is circular, which contain a very big, uh, cruciform structure... This cruciform structure was in a chloroplast genome. This is one evidence to think that maybe, this come, this is a plastid genome... But this 40Kb molecule, it would be interesting to look for it (in Para). But this 40Kb molecule, it would be interesting to look for it... The problem about malaria is that its very AT rich. So you can't really do some uh analysis, analysis of homology with chloroplast uh genome, because of this very AT rich uh business. That would not be the case for Para and could give a better answer for some of the putative homology."

The postdoc initially attempts to purify her sample. She then conducts experiments probing for a gene. She has the problem of not getting clear data, and tries a number of different ways of cleaning up her experiment:

"I was thinking that it would be better instead of using P50 to use one of the probes from malaria that would be a real positive control because here what I'm supposed to see is some difference between the rap1 and the p50 because the P50s, and if I have a signal for a 40Kb molecule, it would be a positive."

The Director then interjects. "What about using Danny's rap1 or rap2 as a probe. The ones that have these mitochondrial sequences in them?"

Other members of the lab then provide different suggestions for control conditions and the post-doc then moves on to the next project:

"I was just doing some routine tests of infection with Para and uh cos cells and with the yield of Para. And I repeat it, and I dilute them, to be sure that the cos cells will be in really good shape, proliferating, as you know. And all the time I was infecting and I couldn't get really nice lysis. And so I said this is really strange. I mean uh this was just a qualitative experiment. And then I thought that maybe I should quantify the invasion in cos cells. Uh, so this is one of the things that I observed. First of all, uh, the growth. Para enters the cos cells. But they do not form this very typical rosette... But still, I mean the problem for me is that there is some controversy how the Para are synthesized. Either they are growing, growing, and the cell explodes, or they, there is lysis and toxin working to lyse the cells... But it seems to me that the cos cells are resistant to Para. I would say extremely resistant... It's really strange"

She replicated the results, tried using different conditions, but discovered that the cell is not being infected. After seeing a series of findings like this she concluded that it is impossible to infect these types of cells and switches to using a different type of cell. She then described four more lines of experiments. Clearly, the transcripts provide a rich database of different types of cognitive operations that have important implications for both our understanding of the ways that scientists work and how scientists should be educated. The next step was to code these transcripts.

Transcription and coding of the data are a very time consuming process. Transcription of a one-hour audio/videotape generally takes about eight hours: the tapes contain many novel scientific terms, and the speakers are not always clear in their enunciation. All coding is conducted by coding the transcriptions into a computerized database. Multiple coders are used, and independent coders conduct reliability checks. The basic unit of analysis is the statement. A statement is essentially equivalent to a clause or sentence. Statements are the basic unit of analysis as they contain a verb phrase, which in turn contains the core mental operation (proposition or idea) that the presenter is employing at the time. Thus, we treat statements at a meeting in the same way that statements are treated in standard protocol analyses (cf. Ericsson & Simon, 1993): We use the statement to build a representation of scientists' mental operations. One of the reasons that we are able to have a coding scheme of this power and flexibility is because of the MacSHAPA coding and database software that we use (Sanderson, Scott, Johnston, Mainzer, Watanabe, & James, 1993). Using this coding scheme we code a number of important dimensions of on-line scientific thinking and on-line interactions at laboratory meetings. The three main dimensions that we code are the scientists' representation of their research, group interactions, and the scientists' cognitive operations.

Using this coding scheme we have investigated the roles of analogy, experimental design, group reasoning, induction by generalization, and unexpected findings in science. For example, in analyzing the lab meeting mentioned above, we coded each condition in the experiment (experimental or control), type of result (expected, or unexpected) and how the data from the experiment were treated (replicated, extended, abandoned, dismissed). In her first project there were seven different conditions, ten in her second project, eight in the third, eight in the fourth, and four in the fifth. Overall, there were 21 unexpected and 16 predicted findings. We also coded the types of social interactions (such as suggestion, elaboration, challenge, etc.), and whether certain types of social interactions are

related to certain types of cognitive operations. We coded the types of causal chains that she made. Finally, we coded the use of analogy, particularly looking at the triggering conditions for specific types of analogy use. She used 12 analogies, generalized over sets of findings eight times, and gave 12 causal explanations for sets of findings and predicted results in future experiments. The results of the initial analyses can be seen in a number of publications (Dunbar 1995; 1997a; 1999a), however this data analysis is still an ongoing process! Rather than reiterating the results of these findings, I will use these results as a vehicle for arguing that by investigating scientific thinking we discover more about the nature of cognition. Furthermore I will argue that by using the *in vivo/in vitro* approach we discover new aspects of cognition that have not been seen using the traditional cognitive approaches.

## **Part II: In Vivo/In Vitro Analyses of Analogy and Causal Reasoning**

### Analogical Reasoning

One of the most recent major discoveries in science, that of three planets circling around the star Upsilon Andromedae, 44 light-years away, was based on analogy. Scientists have frequently postulated that there must be another solar system similar to ours and have spent decades searching for this analogous system. On April 15, 1999 scientists announced the discovery of an analogous solar system and immediately began mapping features of our solar system onto the newly discovered solar system. This type of thinking by analogy is frequent in science and in all aspects of human thinking. Because of its importance, cognitive researchers have spent over twenty years delineating how people make analogies and have identified some of the specific cognitive processes that make it possible to draw analogies (see Gentner, Holyoak, & Kokinov, in press for a summary of recent work on analogy). Hundreds of experiments on analogy have been conducted and numerous different models of analogical reasoning have been proposed.

Researchers on analogy distinguish between two main components of an analogy—the source and the target. The source is the concept that the person is familiar with. In the case of the solar system analogy above, our solar system is the source. The target is the other concept that features of the source are mapped onto. In the case of the solar system analogy, the

target is the new solar system in Andromedae Upsilon. What is interesting about this work on analogy is that one of the most consistent findings in the literature is that unless people are given hints or extensive training, they will not see the relationship between a source and a target unless the source and target share superficial features. When the source and target share only deeper structural or relational features and have no superficial similarities, subjects miss the analogical similarity and have great difficulty in solving analogically similar problems. Many different experiments and different experimental approaches have demonstrated a reliance on superficial features in using analogies (Forbus, Gentner, & Law 1996; Holyoak & Thagard, 1997b). However, when we look at the use of analogy outside the experimental context, what we find is that people often use analogies where the source and target share only deep structural features. This is paradoxical, and I have referred to this difference as the “analogical paradox” (Dunbar, in press).

We have used the *in vivo/in vitro* approach to understanding the analogical paradox. Our *in vivo* research on the use of analogy in science has shown that scientists frequently use analogy (Dunbar 1993; 1995; 1997b; 1999c). We found that scientists use anywhere from three to 15 analogies in a one hour laboratory meeting (see Dunbar, 1999a). What we also found is that the majority of analogies that scientists use are from the domain that they are working in. Thus molecular biologists and immunologists use analogies from the biological or immunological domains and not from domains such as economics, astrophysics, or personal finances. For example, in the meeting discussed in the previous section, one of the postdocs at the meeting drew an analogy between the experiments that she has conducted and the experiments that the presenter conducted. She noted that she had conducted a similar experiment at the same temperature as the presenter, and had also obtained strange results. She then said that she had changed the temperature and the experiment then worked. Thus she mapped over the feature of temperature from her experiment (the source) onto the presenter’s experiment (the target). Both source and the target are highly similar here. At first glance, these findings are consistent with the results of the experimental research—many of the analogies that the scientists used had superficial features in common between the source and the target. However, the picture of analogy is slightly more complicated than this. What I found was that when scientists were using analogy to fix experimental problems, the sources and targets did indeed share superficial features.

When scientists switched goals from fixing experiments to formulating an hypothesis analogy use also changed. I found that the distance between the source and the target increased and that the superficial features in common between the source and target also decreased. In this situation, the scientists used structural and relational features to make an analogy between a source and a target. For example, when a postdoc in one of the labs initially obtains an unexpected finding, he makes analogies to other experiments in the lab. However, when he obtains a series of unexpected findings he draws an analogy to another type of bacterium *B subtilis*:

“*B subtilis* may have a del B fusion protein which may mean that it can both make and degrade at the same time, in the same protein. Certainly something that would be consistent with this data. If the data repeats, it would be that maybe GN156 is a mutant in both or something that controls both and that if you were increasing the activity because we are increasing the amount of clexon and that cell can't even degrade it. So that's when we seeing this huge increase. And that's one possible explanation. Um there's no evidence. I have no real data for this, um it might be something to, to sort of keep in mind.”

Over 25% of the analogies that the scientists proposed were based on relational features like this rather than superficial features. Furthermore, the few analogies that the scientists used that were from non-biological domains were used to explain a concept or idea to another person. Thus I hypothesized that when scientists are attempting to explain an idea to others, they may use sources that are from very different domains (Blanchette & Dunbar, 1997; submitted; Dunbar, 1993; 1995; 1997; 1999). Christian Schunn has recently collected data consistent with this hypothesis and found that psychologists are more likely to use more distant analogies in colloquia than at their weekly laboratory meetings (Saner & Schunn, 1999). Overall, what these data show is that while scientists use many analogies that share superficial features between the source and the target, they can and do produce analogies that are based on deeper structural or relational sets of features. Analogy use appears to be flexible and change with the goals of the analogizer. This result provides new information on analogy use that is somewhat inconsistent with the idea that people are *a priori* biased to use superficial features when making analogies.

Is scientists' ability to produce analogies based on structural features a product of expertise? The hypothesis here might be that as expertise in a domain increases, knowledge of underlying structural relations also

increases. This means that experts can seek other sources that have similar underlying structures and hence produce analogies that have few superficial features in common between the source and the target. While, expertise is certainly an important part of the story, we were concerned that there might be more than expertise involved in scientists' use of analogy. Thus, switching to an *in vitro* approach to analogy, Isabelle Blanchette and I went back into the psychological laboratory and conducted more experiments on analogical reasoning (Blanchette & Dunbar 1998; in press). Our main concern was that, in most experiments on analogy, subjects are given the source and the targets. Often the subjects must select a source from a number of sources provided by the experimenter. Generally, subjects choose sources based on superficial features. When we looked at scientists and at other naturalistic situations such as politicians using analogies, we saw one key difference between psychology experiments and naturalistic settings: In the naturalistic settings the scientists and politicians generated their own analogies. If subjects in a psychology experiment were also asked to generate their own analogies, would they use superficial features, or structural features? If the standard view on analogy is correct, then people who are not experts in a domain are *a priori* constrained to using superficial features, then we should not see the use of deep structural features. In fact we found the opposite. Over 80% of the analogies produced by subjects were based on deep structural features and not on superficial features (see Blanchette & Dunbar, in press). When we changed the task to one of choosing an analogy rather than generating an analogy, we obtained the standard results that previous researchers have found. We found that if subjects were asked to choose between analogies that were based on either superficial or structural features, the subjects choose sources that were based on superficial features 80% of the time.

What the combined results of our research using the *in vivo/in vitro* methodology suggests is that analogy is a more complex process than had previously been imagined. People can, and do, use analogies based on structural or relational features, but that the task used with people must be appropriate. This finding has important implications for educational research. As the previous experimental work on analogy implied, unless a person was an expert in a domain, he or she would rely on superficial features when using analogy. If this was the case, then using analogy to make someone more expert in a domain will be an uphill battle as people would be caught in a vicious circle (if you need to be an expert to use analogy, how can analogy be used to make you an expert?). However, our research

indicates that if subjects generate their own analogies, they can find important structural features and relations in a concept. Some recent work on the use of analogy in educational settings is consistent with our findings. For example, Wolfe, Duncan, and Cummins (1999) found that generating analogies to the concept of geologic time allows students to discover underlying structural features of the concept. The results of our work on analogical reasoning indicate that scientific thinking can be used as a vehicle to understand important cognitive processes such as analogy. By investigating analogy use both *in vivo* and *in vitro* it is possible to discover fundamental components of this cognitive process.

### Causal Reasoning

Scientists must constantly reason about causes and effects. They must design experiments where a particular manipulation is expected to cause a particular effect. Often they must discover a cause for a particular effect when they obtain an unexpected result. One of the problems with science is that experiments do not always turn out the way that we expect. Sometimes data is obtained that is due to error; other times, scientists interpret data erroneously—mistakenly assuming that a particular cause generated a certain effect. How do scientists in particular, and how does science in general, deal with these types of errors? Put another way, much of dealing with error is a way of deciding what is the cause of a particular effect. Dealing with error is therefore a type of causal reasoning that scientists must constantly grapple with.

Before tackling causal reasoning in science let us consider what cognitive research has demonstrated about causal reasoning. Much research on causal reasoning over the past forty years has demonstrated that people have a variety of biases when attempting to discover the cause of a particular effect. For example, in research where people have been presented with scenarios with a cause being present or absent and an effect occurring or not occurring, it has been demonstrated repeatedly that people focus on the joint presence of the cause and the effect and do not consider situations where the effect does not occur and the cause is present (Ahn & Bailenson, 1996; Cheng 1999; Einhorn & Hogarth 1986). For example, when subjects are given scenarios such as: eight out of ten people who have the CLA gene get cancer, and that six out ten people who do not have the CLA gene get cancer, people will ignore the six out of ten and assume that the CLA gene does cause cancer (this is a stripped down ver-

sion of this type of task, but shows that people are ignoring relevant information). In other words, people ignore much relevant information when evaluating whether a potential cause really is relevant. Subjects also seem to favor simplicity over complexity, with uncausal explanations being favored over multicausal explanations. While it is plausible to argue that people have biases for preferring simple explanations (see Spellman, 1996) and have to attend to small amounts of information, when deciding on a cause, it may be the case that as with analogical reasoning, these types of causal reasoning biases are not as prevalent or as in-built as the research literature would lead us to believe. Thus, using the same approach that we used with analogical reasoning, we will first of all investigate causal reasoning by scientists *in vivo* and then further investigate causal reasoning by subjects in our lab using an *in vitro* approach.

When scientists suspect that a particular result might be due to error they attempt to isolate the cause of the error. Furthermore, when designing experiments, scientists anticipate potential types of errors and build specific types of controls into their experiments that help them determine whether an unusual result is due to error or some other process. In this section, I will first of all describe our findings on scientists reactions to results that are potentially due to error, then I will describe the ways that scientists design experiments and how the threat of error is constantly evaluated by scientists.

#### Is it Due to Error? Dealing With the Unexpected

Here, I will focus on scientists' use of unexpected findings at these meetings (see also Dunbar, 1999c). What we have found is that when both scientists and subjects in psychology experiments obtain an unexpected finding their first reaction is to blame it on some sort of error or mistake in the experiment. This is not an isolated phenomenon. Unexpected findings are very important in science. When I looked at the types of findings that the scientists made, I found that over 50% of the findings were unexpected and that these scientists had evolved a number of important strategies for dealing with such findings. The scientists initially attribute the unexpected findings to some sort of methodological error. For example, one postdoc said, "So it may be something funny like that. Or it may be something funny about, you know, the cytoscale and Northern under cos cells." Once they have obtained an unexpected result, the scientists either replicate the experiment, change aspects of the experiment, or change the

experimental protocol entirely. Thus, causal reasoning strategy number one for an unexpected finding is to “blame the method.”

What is interesting about the “blame the method” is that this strategy has a number of interesting sub-components that are highly regular. The first sub-component of “blame the method” is one of looking at the unexpected findings and comparing them to those obtained in their control conditions. They use a “check controls” strategy. By comparing their unexpected results to their controls they can determine whether the result was due to some sort of error, or was due to the discovery of some new type of process. Another aspect of this reaction to unexpected findings is that the director of the laboratory has seen many of the straightforward errors before and will draw analogies to other experiments conducted in the lab. Thus, another strategy that scientists use in dealing with potential error is to point out the similarity of a particular experimental result to a result that has previously been obtained. This is the “find similar result” strategy. The analogy to a previous example often contains a solution. If the type of error has been encountered before, the scientists have probably used a method to solve the problem that can also be used to overcome the current error. This use of analogies to similar situations is highly frequent. Scientists’ initial reactions to an unexpected finding is that it is due to error. Only after repeated examples of similar unexpected findings will the scientists start to propose new theories and hypotheses.

Stepping back to the issue of causal reasoning, we can see that a key component of the scientists’ causal reasoning is that they make comparisons to controls. Unlike subjects in psychology experiments, the scientists are attending to many different results and making comparisons between different conditions. While we have no statistical data on differences between expert and novice scientists, it appears that the expert scientists tend to pay more attention to the control conditions than the novice scientists and often ask the novice scientists to include more controls in their experiments. Thus, the use of these controls may be a function of expertise.

Given that controls are an important aspect of the scientists’ reasoning about unexpected results, Lisa Baker and I decided to investigate the use of controls in the design of experiments (Baker & Dunbar, 1996; in press). Scientists design experiments that have both experimental conditions and control conditions. The experimental conditions usually consist of a manipulation of the variable that the scientist is interested in. For example if a scientist thinks that a particular protein is responsible for a cell having a particular function, the scientist might add this protein to

cells and to see if the cells gain this function. However, the scientists also have numerous control conditions in their experiments. One type of control that they use is “known standard controls” (Baker & Dunbar, 1996). These “known standard controls” consist of conditions that have previously been used and validated and are standardized. These known controls are important for comparisons. If an unexpected finding is obtained in the experimental condition, the scientists can compare the unexpected result with a known control. If the known controls are also giving an unexpected result, then there is a problem with the method, and it is not some new exciting finding. What these controls do is to ensure that the technique really works and can allow the scientist to conclude that they are conducting the experiment in an appropriate manner. Another type of control is a “baseline control” in which something is taken away from the experiment or not added to an experiment. When we analyzed the types of experiments that scientists conducted we found that “known standard controls” were particularly important in controlling for error. When scientists were designing experiments and worrying about error they frequently included different types of “known standard controls.”

Following our initial analyses we decided to further probe the use of control conditions and the relationship of controls to error (Baker & Dunbar, in press). We analyzed the design of experiments at four meetings in two Canadian immunology labs. We coded the goal that the scientist had (testing an hypothesis, or anticipating error) and we also coded the type of control conditions used (“baseline control” or “known standard control”). As can be seen from Table 1, what we found was that most “baseline controls” were proposed when testing an hypothesis, and most “known standard controls” were used when ruling out potential errors in the use of the techniques. This result is interesting as it shows that the use of controls, particularly “known standard controls” is one way that scientists attempt to determine whether errors have occurred in their experiments. One important point to note is that these controls are put into the experiment before the experiment is conducted. The scientists are attempting to minimize error before they conduct an experiment. In other words, the scientists are making sure that they have the right information present when reasoning causally about their results.

**Table 1.** Control conditions and their goals used by immunologists *in vivo* at four lab meetings. Reprinted from Baker and Dunbar (in press).

Goal	TYPE OF CONTROL	
	Baseline	Known Standard
Test Hypothesis	7	4
Possible Error	2	7

**Table 2.** Control conditions and their goals used by immunology and developmental biology students *in vitro* (N=60). Reprinted from Baker and Dunbar (in press).

Goal	TYPE OF CONTROL	
	Baseline	Known Standard
Test Hypothesis	31	4
Possible Error	5	28

Having found that scientists spend much of their time both anticipating and dealing with error, we decided to go back into the cognitive laboratory and conduct our own *in vitro* experiments on the ways that scientists deal with error (Baker & Dunbar, in press). In one experiment, we asked immunology and molecular biology students to design experiments that would test a hypothesis regarding a particular gene function. Once the students had designed their experiments we told them that the professor who wanted the experiment done was afraid that there was an error in the experiment. That is, initially we gave the students the goal of testing a hypothesis and then we switched the students to the goal of worrying about the possibility of error in their experiments. Would the science students, like the scientists, use “baseline controls” when testing an hypothesis and “known standard controls” when anticipating error? As can be seen from Table 2, the answer is yes. We found that both immunology and developmental biology students used “baseline controls” when testing hypotheses and “known standard controls” when anticipating error. These results indicate that using “known standard controls” is a strategy that scientists use for anticipating error and interpreting results.

Given that we had seen the use of the “known standard controls” strategy by scientists and science students, we next turned to the issue of, “How general is this strategy?” That is, is this a general reasoning strategy, or something that only domain experts would know? Put another way, would non-science students also use this strategy? We gave non-science students simplified versions of the same problems that we gave to the science students. What we found was quite different from what we saw with the science students. The non-science students did not use “known controls” on the science problems. Thus, the strategy of using “known controls” appears to be one that scientists have acquired as a result of their training. However, they did use baseline controls. These results indicate that one important thing that scientists know is the appropriate type of control to use in experiments. Scientists know that “known controls” are appropriate for finding errors in their experiments and “baseline controls” are appropriate for testing hypotheses. Most psychological treatments of control conditions, and most educational curricula dealing with experimental design, do not distinguish between these two types of controls. One possible reason for this is that in most simple experiments, known controls are not necessary. We have hypothesized that in the complex experiments that molecular biologists and immunologists conduct, there are many different steps in the experiment, and many places in which an experiment can provide erroneous results. To check these steps it is necessary to use many different types of “known controls.” The implications for education are that if the goal is to train students to conduct complicated experimental work, it is necessary to also educate the students to use both “baseline controls” and “known controls.”

What does this research reveal about causal reasoning in science? Clearly causal reasoning is a very complex process and we have investigated only a very small aspect of this process. However, what we have found is that both subjects in our *in vitro* experiments and scientists in our *in vivo* work are highly aware of the use of controls, particularly baseline controls. Unlike the subjects in many causal reasoning experiments, both scientists and subjects can and do consider situations other than the joint presence of a cause and an effect. What is also clear from this work is that expertise also has an effect on causal reasoning. On the one hand, baseline controls are the types of controls that both expert scientists and novices are aware of. It may even be the case that children are also aware of these types of controls (e.g., Sodian, Zaitchik, & Carey, 1988). On the

other hand, known controls appear to be the result of training in a particular domain.

The results of the research reported here indicate that a number of new strategies that scientists use that have not been previously tackled in the education of scientists. For example, while there has been much work on experimental design, the tasks that subjects have been given to design have been modeled after the traditional psychology experiment, such as seeing if people can design factorial experiments. Thus, typically, we have asked people to design experiments that are similar to the ones that we conduct in our own cognitive experiments. While this has been a good approach that has important theoretical and educational implications, we must be constantly aware that we are training future scientists in many different disciplines, and that these disciplines often use methods and experiments in ways that are radically different from the classic psychology experiment. For example, many biology and immunology experiments have 12 to 15 conditions in them, with four or five control conditions. Students learn to use these different types of controls by the classic apprenticeship method, rather than by being formally taught these methods. Clearly control conditions are an area that could be much widened and could be incorporated into the scientific education curriculum.

One possible interpretation of these types of results is that as the data on causal reasoning were collected from molecular biology laboratories, the results are only applicable to these types of sciences. However, a look at recent research in psychology indicates that psychologists also use baseline controls and known standard controls in their experiments. These types of conditions are now present in PET and fMRI investigations of brain function. Scientists using these technologies frequently use all these different types of controls. Given that many sciences are becoming increasingly more complex, I expect the use of different types of controls will be an important part of future curricula.

#### **Part IV: Conclusion**

What can we learn about cognition by looking at scientific thinking? First, we must consider what we know from standard research on cognition. Much progress has been made over the last twenty years in our understanding of both analogical and causal reasoning. Many important and detailed models of these cognitive processes have been made. However, research has often painted a bleak picture of the human capacity to think

and reason. By comparing the results of *in vivo* and *in vitro* investigations of these cognitive processes, we can now ask the question of how representative of human cognitive capacities these studies are and also begin to articulate some reasons for the somewhat different results that have been obtained. Most models of analogical and causal reasoning have been constructed based upon experiments that use very simple and arbitrary stimuli. Furthermore, the domains that the subjects must reason about are usually novel and unknown. By conducting experiments using these types of stimuli it is possible to discover important features of cognition, but when stimuli are made more realistic, less arbitrary, and subjects can use some of their day to day knowledge, then additional, and different reasoning strategies are used and some of the cognitive biases identified in previous research begin to disappear. We have seen these other reasoning strategies in both non-experimental contexts and in experimental contexts. Thus, by using a knowledge-rich domain such as science, it is possible to discover new basic facts about cognition. It may also be the case that the types of reasoning that we see in these knowledge-rich domains are more representative of the basic cognitive abilities of human beings than the stripped down tasks used in much cognitive research.

One possible interpretation of the research reported in this chapter is that while conducting research using *in vivo/in vitro* methods may uncover aspects of cognition that have not been observed using the more traditional methods, the traditional framework will remain intact. The implication is that by tying *in vivo* research to theories and models of traditional cognitive research we are merely forcing *in vivo* findings into a pre-existing closed framework that will vitiate the *in vivo* research of any of its real power. While this is a potential limitation of some applications of the *in vivo/in vitro* method, current and future applications of the method point to at least three possible outcomes of the use of it. These three types of outcomes of the method are the three forms of conceptual change most frequently mentioned in the literature on science itself (Carey, 1992; Chi, 1992). First, the findings of some *in vivo/in vitro* investigations may be consistent with what has been found using traditional cognitive research. Here, the results of the research confirm what is already known. Second, *in vivo/in vitro* research may extend traditional cognitive findings into new areas and domains. New concepts, models and theories will be added to the extant cognitive framework. Third, the *in vivo/in vitro* method has the potential—because it is an open system not under the control of the researcher—to radically restructure our understanding of cognition (see also Cole, 1996). The important point here is

that the *in vivo* component of the research is not under the control of the experimenter. This allows many different factors to play a role—factors that traditional experimental work seeks to exclude. However, by using the *in vivo* method, new factors are introduced into the *in vitro* research that can change the nature of *in vitro* research itself.

#### Implications of the *In Vivo/In Vitro* Findings for Science Education

Research on scientific thinking, reasoning, and discovery is of paramount importance to science education. Recent work on science education (e.g., Bransford, Brown, & Cocking 1999; a forthcoming special issue of the journal *Applied Developmental Psychology* on cognitive approaches to education) shows that the field is in a great period of transition. One of the key components of contemporary teaching of scientists, particularly at science education beyond the undergraduate degree, has been the apprenticeship method. Students work with their advisor and learn the methods and theories by participating in research projects in a lab. Using this method of training, some students become excellent researchers and rise to the top of their fields. Unfortunately, many students fall by the wayside, and are unable to learn this way. Clearly, there are many reasons that students do not make it through the system. One of the reasons is that the apprenticeship method is *ad hoc*. What the research that we have conducted shows is that there are definite strategies that scientists use. These strategies such as “blame the method,” followed by “check the controls,” and “find similar results” could easily be taught to students in both undergraduate and graduate curricula. Another place where *in vivo/in vitro* research can be applied is in the area of experimental design. The use of baseline and “known standard” controls is essential to the design of complex experiments in many disciplines and is a key component of reasoning about all experimental findings. Finally, other findings on the types of social interactions that are conducive to scientific discovery (Dunbar, in press b) could also be incorporated into the scientific curriculum. The results of *in vivo* research on scientific thinking have the potential to radically change the undergraduate science curriculum and make the training of graduate students and postdocs less *ad hoc* and more principled.

## Acknowledgements

I thank the Facoltà di Psicologia at the Università San Raffaele in Milan for providing the facilities for writing this chapter. In particular Professor Massimo Piattelli-Palmarini for the invitation to Milan. I would also like to thank the editors of this volume for their astute comments on an earlier version of this chapter. Finally, I thank the scientists who have graciously allowed us to investigate them, and the numerous colleagues and students for assistance on this complex project. Research reported in this paper was funded by The Natural Science and Engineering council of Canada, SSHRC, The Spencer Foundation, and the U.S. Office of Naval Research.

## References

- Ahn, W., & Bailenson, J. (1996). Causal attribution as a search for underlying mechanisms: An explanation of the conjunction fallacy and the discounting principle. *Cognitive Psychology*, 31, 82-123.
- Baker, L. M., & Dunbar, K. (in press). Experimental design heuristics for scientific discovery: The use of “baseline” and “known standard controls.” *International Journal of Computer Studies*.
- Baker, L. M., & Dunbar, K. (1996). Constraints on the experimental design process in real-world science. In *Proceedings of the eighteenth annual meeting of Cognitive Science Society*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Bartlett, F. C. (1932). *Remembering*. Cambridge, UK: Cambridge University Press.
- Blanchette, I., & Dunbar, K. (in press). How analogies are generated: The roles of structural and superficial similarity. *Memory & Cognition*.
- Bransford, J. D., Brown, A. L., & Cocking, R. (1999). *How people learn: Brain, mind, experience, and school*. Washington, DC: National Academy Press.
- Bruner, J. S., Goodnow, J. J., & Austin, G.A. (1956). *A study of thinking*. New York: Wiley.
- Brunswik, E. (1943). Organismic achievement and environmental probability. *Psychological Review*, 50, 255-272.
- Carey, S. (1992). Conceptual change within and across ontological categories: Examples from learning and discovery in science. In R. N. Giere (Ed.), *Minnesota studies in the philosophy of science: Vol. 15: Cognitive models of science*. Minneapolis, MN: University of Minnesota Press.
- Cheng, P. (1999). Causal reasoning. In R.A. Wilson & F. Keil (Eds.), *The MIT encyclopedia of the cognitive sciences*. Cambridge, MA: MIT Press.
- Chi, M. T. H. (1992). Conceptual change within and across ontological categories: Examples from learning and discovery in science. In R. N. Giere

- (Ed.), *Minnesota studies in the philosophy of science: Vol. 15: Cognitive models of science*. Minneapolis: University of Minnesota Press.
- Cole, M. (1996). *Cultural psychology: A once and future discipline*. Cambridge, MA: Harvard University Press.
- Cole, M. (1999). Ecological validity. In R.A. Wilson & F. Keil (Eds.), *The MIT encyclopedia of the cognitive sciences*. Cambridge, MA: MIT Press.
- Dunbar, K. (in press). The analogical paradox: Why analogy is so easy in naturalistic settings, yet so difficult in the psychology laboratory. In D. Gentner, K. Holyoak, & B. Kokinov (Eds.), *Analogy: Perspectives from cognitive science*. Cambridge, MA: MIT press.
- Dunbar, K. (1999a). Science. In M. Runco & S. Pritzker (Eds.), *The encyclopedia of creativity*. San Diego, CA: Academic Press.
- Dunbar, K. (1999b). Cognitive development and scientific thinking. In R.A. Wilson & F. Keil (Eds.), *The MIT encyclopedia of the cognitive sciences*. Cambridge, MA: MIT Press.
- Dunbar, K. (1997a). "On-line" inductive reasoning in scientific laboratories: What it reveals about the nature of induction and scientific discovery. *Proceedings of the Nineteenth Annual Meeting of the Cognitive Science Society*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Dunbar, K. (1999c). The scientist *in vivo*: How scientists think and reason in the laboratory. To appear in Magnani, L., Nersessian, N., & Thagard, P. (Eds.), *Model-based reasoning in scientific discovery*. New York: Plenum Press
- Dunbar, K. (1997b). How scientists think: Online creativity and conceptual change in science. In T.B. Ward, S.M. Smith, & S.Vaid (Eds.), *Conceptual structures and processes: Emergence, discovery and change*. Washington, DC: APA Press.
- Dunbar, K. (1995). How scientists really reason: Scientific reasoning in real-world laboratories. In R. J. Sterberg & J. E. Davidson (Eds.) *The Nature of Insight*. Cambridge, MA: MIT Press.
- Dunbar, K. (1993, March). *In vivo cognition: Knowledge representation and change in real-world scientific laboratories*. Paper presented at the Society for Research in Child Development. New Orleans, LA.
- Einhorn, H. J., & Hogarth, R. M. (1986). Judging probable cause. *Psychological Bulletin*, 99, 3-19.
- Ericsson, K. A., & Simon, H. A. (1993). *Protocol analysis: Verbal reports as data*. Cambridge, MA: MIT Press (Revised Edition).
- Ericsson, K. A., & Smith, J. (1991). *Toward a general theory of expertise: Prospects and limits*. Cambridge, UK: Cambridge University Press.
- Feist, S., & Gorman, M. (1998). The psychology of science: Review and integration of a nascent discipline. *Review of General Psychology*, 2(1), 3-47.

- Forbus, K., Gentner, D., & Law, K. (1995). MAC/FAC: A model of similarity-based retrieval. *Cognitive Science*, 19, 141-205.
- Holyoak, K. J., & Thagard, P. (1997). The analogical mind. *American Psychologist*, 52, 35-44.
- Gibson, J. J. (1979). *The ecological approach to visual perception*. Boston, MA: Houghton-Mifflin.
- Klahr, D., & Simon, H. A. (1999). Studies of scientific discovery: Complementary approaches and convergent findings. *Psychological Bulletin*, 125(5), 524-543.
- Lave, J., & Wenger, E. (1991). *Situated learning: Legitimate peripheral participation*. Cambridge, UK: Cambridge University Press.
- Neisser, U. (1976). *Cognition and reality: Principles and implications of cognitive psychology*. San Francisco, CA: W.H. Freeman.
- Sanderson, P. M., Scott, J. J. P., Johnston, T., Mainzer, J., Watanabe, L. M., & James, J. M. (1993). MacSHAPA and the enterprise of exploratory sequential data analysis. Unpublished manuscript.
- Saner, L., & Schunn, C. D. (1999). Analogies out of the blue: When history seems to retell itself. *Proceedings of the Twenty-First Annual Conference of the Cognitive Science Society*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Seifert, C. (1999). Situated cognition and learning. In R. A. Wilson, and F. C. Keil (Eds.). *The MIT encyclopedia of the cognitive sciences*. Cambridge, MA: MIT Press (pp. 767-769).
- Sodian, B., Zaitchik, D., & Carey, S. (1991). Young children's differentiation of hypothetical beliefs from evidence. *Child Development*, 62, 753-766.
- Spellman, B. A. (1996). Acting as intuitive scientists: Contingency judgments are made while controlling for alternative potential causes. *Psychological Science*, 7(6), 337-342.
- Suchman, L. (1987). *Plans and situated actions: The problem of human-machine communication*. Cambridge, UK: Cambridge University Press.
- Tweney, R. D., & Chitwood, S. T. (1995). Scientific reasoning. In S. E. Newstead & J. S. B. T. Evans (Eds.), *Perspectives on thinking and reasoning: Essays in honour of Peter Wason*. Hillsdale, NJ: Lawrence Erlbaum Associates.
- Tweney, R. D., Doherty, M. E., & Mynatt, C. R. (Eds.) (1982). *On scientific thinking*. New York: Columbia University Press.
- Wolfe, C. R., Duncan, S. C., & Cummins R. H. (1999). The effects of generating analogies on the mental representation of geologic time. Manuscript submitted for publication.