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The Psychology of Science: Review and Integration of a Nascent Discipline

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Disciplines that study science are relatively well established in philosophy, history, and sociology. Psychology of science, by comparison, is a late bloomer but has recently shown signs of codification. The authors further this codification by integrating and reviewing the growing literature in the developmental, cognitive, personality, and social psychology of science. Only by integrating the findings from each of these perspectives can the basic questions in the study of scientific behavior be answered: Who becomes a scientist and what role do biology, family, school, and gender play? Are productivity, scientific reasoning, and theory acceptance influenced by age? What thought processes and heuristics lead to successful discovery? What personality characteristics distinguish scientists from nonscientists and eminent from less eminent scientists? Finally, how do intergroup relations and social forces influence scientific behavior? A model that integrates the consensual empirical findings from the psychology of science is proposed.

> Without the addition of a psychological dimension, I believe, it is impossible to appreciate fully the essence of the scientific imagination. And without this appreciation, the origins of science, the emergence of new ideas about natural phenomena, must escape our grasp. Psychology is mandatory if we wish to comprehend the scientific genius as the generator of science. (Simonton, 1988a, p. 200)

It is indisputable that the growth of science and the development of technology have transformed the world, both physically and culturally. For this reason, science ought to be an object of intense psychological study. However, efforts to study science and technology from a psychological perspective are scattered across the disciplines of psychology, and often there is too little communication among those involved. As Mahoney (1979) wrote in the late 1970s, "In terms of behavior patterns, affect, and even some intellectual matters, we know more about alcoholics, Christians, and criminals than we do about the psychology of the scientist" (p. 349). In contrast, other disciplines like philosophy, history, and sociology have spawned clearly identifiable subdisciplines devoted to science studies. We firmly believe that science deserves more attention from psychologists, and one of the goals of this review is to show how the psychology of science has grown from being the amorphous and scattered field that Fisch (1977) and Mahoney (1979) described to a codified, albeit nascent, subdiscipline today. Psychology of science can benefit not only other psychologists, but philosophers, historians, and sociologists also.

The psychology of science applies the empirical methods of psychological investigation to the study of scientific behavior. In other words, it is the empirical study of the cognitive, biological, developmental, personality, and social influences of those individuals who are involved in the enterprise of science or who are simulating scientific problem solving. In this sense, the psychology of science is primarily descriptive, describing actual behavior, rather than prescriptive (describing ideal behavior). Consider the case of Michael Faraday, discoverer of electromagnetic fields. Faraday's discovery can be analyzed from developmental, cognitive, personality, or social psychological perspectives. From a developmental perspective, one could study the shifts in Faraday's

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beliefs, methods, and productivity rates as he grew older. Explicating the cognitive processes (confirmation vs. falsification, analogy and metaphor, ideation, and elaboration) used by Faraday in his integration of electromagnetic fields would shed important light on his discovery. Faraday's personality was important in his rise from bookbinder to fellow of the Royal Society; the same patient, methodical devotion to a task is apparent in the way he bound books, conducted experiments, and kept notebooks detailing results and speculations. Furthermore, the social context from which his discoveries stemmed are critical to understanding his scientific process. He belonged to a religious organization that referred to themselves as *Sandemanians,* and from this group stemmed his faith that the book of nature could be read by anyone who devoted careful time and attention. To understand a Faraday or any other scientist, a psychological perspective is as important and necessary as philosophical, historical, or sociological ones.

A Brief History of the Psychology of Science

No definite date can be given for the birth of the psychology of science. Stevens (1936, 1939) wrote on the psychology of science in the 1930s, but Roe's (1952a, 1952b, 1953) classic work, along with Cattell's (R. Cattell & Drevdahl, 1955), foreshadowed the burst of research on psychological attributes of scientists that occurred in the early 1960s. In general, studies in the 1960s placed a heavy emphasis on creativity in science (Chambers, 1964; Eiduson, 1962; Gough & Woodworth, 1960; Taylor & Barron, 1963). In addition, Maslow published a book in 1966 with the title *The Psychology of Science,* in which he argued for expanding the scope of traditional mechanistic, reductionistic views of science to include a broader, more humanistic and psychological conceptualization of science. Finally, a precursor to the entire discipline of cognitive psychology of science also could be seen in Herbert Simon's chapter on scientific discovery and the psychology of problem solving (Simon, 1966).

During the 1970s, however, there was a decline in research on the psychology of science, and few major works were produced on the topic. One exception was a conceptual article by Singer (1971), who pointed out that although a new "science of science" was a nascent discipline as far back as the 1930s, "some 30 years have passed, and we do not as yet have a developed, self-conscious discipline of a science of science. We are now, however, in a better position to anticipate its arrival" (p. 1010). Another exception from the 1970s was the first major review of the field (Fisch, 1977). Toward the end of the decade, Fisch echoed Singer's concern and opened his review by pointing out the disparate and unsystematic nature of investigations into the psychological attributes of scientists. He concluded his review pessimistically: "Having now reviewed the field, it is lamentably clear that basic concepts are diffuse and contradictory, and rarely become common to several investigations. For this and other reasons, results cannot really be compared, and little scholarly cumulation has resulted" (p. 298). In another review of the literature just 2 years later, Mahoney (1979) reached similarly pessimistic conclusions about the state of the field.

However, since the early to mid 1980s there has been a steady surge in works devoted to the psychological underpinnings of science (Gholson, Shadish, Neimeyer, & Houts, 1989). This surge was so evident that Shadish, Fuller, and Gorman in 1994 could proclaim that the "psychology of science has finally arrived" (p. 3). Furthermore, "substantively, psychological contributions to science studies are increasing in frequency and quality. Sociologically, psychologists are beginning to identify themselves as interested in the topic" (Shadish, Houts, Gholson, & Neimeyer, 1989, p. 1). Granted, many psychologists who study scientists and the scientific process often do so not explicitly from a psychology of science perspective, and one purpose of this review is to make the connection explicit.

Before we review the literature in each substantive area we should note a few caveats. First, our organization is empirical rather than theoretical. We believe that the first stage in establishing a discipline is to demonstrate descriptive consensus. Second, we do not claim that the reviews are exhaustive, but we made every effort to locate as many articles as possible on the relevant topics and extract the general findings. Some topics fall outside of or cut across traditional subdisciplinary boundaries, in particular creativity and productivity, and therefore will be discussed under more than one category. In addition, we have chosen to focus on the four most developed subdisciplines—developmental, cognitive, personality, and social psychologies of science. We could have perhaps included a fifth section on the less developed field of the biological psychology of science. However, we chose to include it in the developmental section because biological and genetic influences have been discussed primarily in relation to gender differences in, and the development of, mathematical ability.

Developmental Psychology of Science

One of the first and most interesting questions that can be addressed by the psychology of science concerns how and why certain individuals become scientists. What is the origin of the necessary talents and skills required to be a scientist? Why do some possess these talents and others not? How do these talents and abilities change and develop with age? These questions are the core focus of the developmental psychology of science. Developmental psychology of science has much overlap with the social psychology of science because children and adolescents are dependent on others (usually parents) for survival. In fact, developmental questions can be placed on a continuum from the relatively nonsocial to the very social. The relatively nonsocial questions include biological and genetic influences of math ability and creative genius, and the moderately social topics include age and productivity, age and receptivity to new discoveries, and gender and science. Finally, the more purely social questions include mentoring and training, family influences, and religious background. Of course, the social continuum is meant only as a heuristic because biology and environment do have mutually reinforcing influences on each other.

Precocity, Giftedness, and Creativity: Influence of Biology and Genetics

No one is born a scientist, but some are born with talents and temperaments that form the foundation for doing science. In some children, these talents and aptitudes are manifested very clearly and very precociously.

Among the more innate aptitudes is mathematics. Some children begin to display incredible mathematical computational and reasoning skills as early as 2 or 3 years old, and by 10 years of age are already performing complex mathematical calculations (Bell, 1937; Kanigel, 1991; Wiener, 1953). The list of historical examples of innate and precocious mathematical genius is long and impressive: Pascal, Newton, Leibniz, Laplace, Gauss, Boole, Wiener, Ramanujan, and Feynman, to name but a few of the truly outstanding examples (Bell, 1937; Gleick, 1992; Kanigel, 1991; Wiener, 1953). With the exception of the Bernoullis, most of these mathematically precocious geniuses came from humble and nonmathematical families (Bell, 1937). Although some researchers have used family lineage evidence to infer genetic influence, familial accumulation per se is irrelevant in any nature–nurture debate (Eysenck, 1988, 1993, 1995). This is so for the simple reason that genetics and environment are inherently confounded within families. Theoretically, if a trait were 100% genetically determined or 100% environmentally determined it would accumulate in families in either case (Eysenck, 1988). If mathematical ability is to some extent genetically determined, then how is it possible to have mathematical genius spring from nonmathematical families? As Bouchard and Segal (1990) argued, such innate genius may demonstrate Lykken's principle of *emergenesis,* which is defined as "the inheritance of a unique configuration of genetic factors that may explain the hereditary transmission of traits that do not appear to run in families" (Bouchard & Segal, 1990, p. 192). In other words, some genetic traits are so complex, and are made up of "a configuration—rather than by a simple sum—of polymorphic genes" (Lykken, McGue, Tellegen, & Bouchard, 1992, p. 1565), that even though they are genetically influenced they are not likely to run in families. There is some evidence that creativity and genius are such complex traits (Lykken et al., 1992; Waller, Bouchard, Lykken, Tellegen, & Blacker, 1993).

There is also direct evidence, however, that nongenius-level mathematical ability is more strongly related in monozygotic than in dizygotic twins, and is therefore at least partially genetically determined (Bouchard & McGue, 1981; Husén, 1960; Loehlin & Nichols, 1976; Scarr & Saltzman, 1982; Vandenberg, 1988). More specifically, these studies have found that heritability estimates [i.e., double the difference between monozygotic and dizygotic correlations: $h^2 = 2(r_{mz} - r_{dz})$], clearly implicate a genetic influence in mathematical ability. Whether variability in math is also attributable to other physiological, neurochemical, or anatomical differences is more debatable, even if the consensus is that these factors do play some role in individual differences in math ability (see Benbow, 1988, and the resulting commentaries in *Behavioral and Brain Sciences* for discussion of potential causes of mathematical ability).

Age and Productivity

How does productivity change with age? One of the oldest of the developmental psychology of science questions concerns whether age affects level of productivity. The question is unique not simply because it has been asked for such a long time, but because its answer is now rather consensually agreed on. There is a relationship between age and productivity in science (and other professions) and it is an inverted U (Bayer & Dutton, 1977; S. Cole, 1979; Dennis, 1956; Diamond, 1986; Horner, Rushton, & Vernon, 1986; Lehman, 1953, 1960, 1962, 1966; Over, 1982, 1989; Simonton, 1984, 1988a, 1988b, 1989, 1991, 1992a; Zuckerman, 1977). Furthermore, once controls are made for different ways of operationalizing output, the curve peaks at the same age (early 40s) for quality and quantity of productivity. However, it does peak somewhat differently for various disciplines (earlier in math and physics, later in biology and geology). This is not to say that the topic of age and productivity has been without controversy. On the contrary; it has been replete with controversy from its inception. In particular, Lehman's seminal work has been the object of frequent criticism and rebuttal (S. Cole, 1979; Dennis, 1956, 1958; Horner et al., 1986; Over, 1989; Zuckerman & Merton, 1972). Granted some of these criticisms are valid and justified, but once many of the controls are made that Lehman failed to make, the result is still an inverted-U relationship (Simonton, 1988b). The peak may be a little flatter and it may occur a little later, but basically every study conducted on the relationship between age and productivity has shown a curvilinear relationship that peaks either in the late 30s or early 40s and then drops off more gradually than it rose.

However, we must point out that age only accounts for a relatively small percentage of the overall variability in productivity (Bayer & Dutton, 1977; S. Cole, 1979; Horner et al., 1986). The work of Horner et al. (1986) illustrates this point. They sampled over 1,000 male research psychologists from four different birth cohorts, and found a curvilinear relationship between age and productivity, with the peak occurring in the early 40s. In this sample, however age accounted for 6.5% of the overall variance in publication rate. In short, it is clear that other individual-difference and social factors (such as early levels of productivity, rewards and honors, and institutional support) have at least as strong if not stronger of a relationship with productivity (S. Cole, 1979; Zuckerman, 1977; Zuckerman & Merton, 1972).

If the description of the relationship between age and productivity is relatively clear and agreed on, its explanation is not. Little theoretical attention has been devoted to the topic. The few attempts at explanation can be divided into two general categories: extrinsic versus intrinsic factors (Simonton, 1988b). The primary candidates for extrinsic theories concern decline in physical health, increase in family and administrative obligations, and unfavorable work conditions, whereas the intrinsic factors are concerned with changes in motivation, experience, intelligence, and creativity.

Empirical evidence does at least partially support the extrinsic theories (Hargens, McCann & Reskin, 1978; Roe, 1972; Simonton, 1977a). Indeed, sociologists have long argued that an extrinsic factor (reward) plays an important role in maintaining high levels of productivity in some and discouraging it in others (J. Cole & S. Cole, 1973; S. Cole, 1979; Merton, 1973; Zuckerman, 1977). In other words, those scientists who produce the most impactful works early in their careers and who are thereby rewarded with tenured jobs at top departments, financial support, and prestigious awards are the ones who are most likely to continue producing. The main problem with this theory is that it cannot explain the single-peak curvilinear relationship between age and productivity in all scientists, not only the most precociously productive.

However, little longitudinal research has been conducted on the intrinsic theories, namely developmental changes in motivation, intelligence, and creativity across the lifespan. The sparse empirical work conducted on change in intelligence across adulthood, however, points to a rather late and small decline (Schaie, 1984), which suggests that age-related declines in productivity may not be a result of a drop in intelligence. The more likely intrinsic candidate, namely motivational decline, has not received much empirical attention, but one longitudinal study has reported a decline in drive in scientists with age: "from the standpoint of satisfaction there is some diminution of the involvement in work—some of the gratifications are beginning to pall and some of the fire, drive, and curiosity is gone" (Eiduson, 1974, p. 408). Theoretically, however, motivational decline has received attention for over 100 years (Beard, 1874). Beard argued that productivity is a function of changes in motivation (enthusiasm) and experience. The young are more enthusiastic and the old are more experienced, and both enthusiasm and experience are linear functions of age (enthusiasm negative and experience positive). Creative achievement is a result of the balance between youthful enthusiasm and the experience of old age, and hence, productivity peaks when these two intrinsic processes overlap (i.e., in the late 30s or early 40s).

Simonton has developed a more complex theoretical model that attempts to predict and explain the age–productivity relationship by focusing on intrinsic factors, namely cognitive components (Simonton, 1984, 1988a, 1988b, 1989, 1991). This model is based on his chance-configuration theory and consists of a few key assumptions: first, each creator starts off with a set amount of creative potential (number of contributions made over a normal, unrestricted life span). Second, the actualization of creative potential can be broken down into two components: ideation and elaboration. *Ideation* is the rate at which potential ideas are expressed, whereas *elaboration* is the rate at which ideas are put into concrete, public form. So as each creator produces a new work she or he "uses up" some creative potential. The rate at which a creator actualizes potential and produces works is a direct function of the two cognitive transformations, ideation and elaboration. To graphically model this relationship, Simonton has developed one of his better known differential equations, with the peak occurring roughly 20 years into one's career and thereafter slowly declining (Simonton, 1984, 1988a, 1989).

Does producing works early predict later levels of productivity? Again, enough work has been conducted on this question to provide a rather consensual answer: yes, early levels of high productivity do regularly foreshadow continued levels of high productivity across one's lifetime (S. Cole, 1979; Dennis, 1954, 1966; Helson & Crutchfield, 1970; Horner et al., 1986; Lehman, 1953; Over, 1982; Reskin, 1977; Roe, 1965; Simonton, 1988b, 1991, 1992a). Those who are prolific early in their careers also tend to continue to be productive for the longest periods of time. For example, Horner et al. (1986) reported that the most prolific group of scientists outpublished medium and low publishers by more than 2 to 1 in the 25–34 age period, and they maintained about a paper-per-year advantage over both groups during each 10 year period until the mid 60 to 70 age period. At this age all three groups dropped to approximately a half a paper per year, but the precocious group still outproduced the other two groups.

As is the case with productivity in general, sociologists tend to explain this phenomenon in terms of the *cumulative advantage* or the *Matthew effect* (S. Cole, 1979; Merton, 1973; Zuckerman & Merton, 1972): Those who publish frequently early in their careers and are therefore rewarded by their peers continue to garner more and more of their share of the resources and continue to outproduce their peers because of the ever increasing supply of financial and social support. Productivity data are inherently positively skewed with one tenth of the scientists producing roughly one half of all of the works (Lotka, 1926; Price, 1963). The rich get richer and the poor get poorer! Furthermore, there is some evidence that quantity of publication matters more than quality of publication when predicting who will receive the most peer recognition and prestigious honors—that is, who will become the most eminent (Feist, 1997).

Compared with older scientists, do younger scientists produce a disproportionate number of high-quality works? Lehman (1953, 1960, 1966) suggested that younger scientists (below age 40) produce most of the highly cited and impactful works. However, Lehman's data did not take absolute number of scientists at each age period into account, and therefore may be biased towards the young simply because there are more young scientists. Over (1989) examined whether older scientists were more likely to produce works of lower quality than younger scientists. He found that although it is true that a disproportionate number of high-quality works come from scientists less than 10 years post-PhD, it is equally true that a disproportionate number of low-quality works come from this age group of scientists. In other words, more high quality works are being produced by younger scientists not because of age but because of the high number of young scientists. The same holds true once longitudinal rather than cross-sectional data are examined (S. Cole, 1979). Longitudinal data are important because they do not confound age and cohort effects the way cross-sectional data do.

Are older scientists more resistant to scientific revolutions than younger ones? Max Planck's experience with resistance to his novel ideas gave him "an opportunity to learn a new fact—a remarkable one, in my opinion: A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (as quoted in Barber, 1961, p. 597). This observation of Planck's, which has come to be called *Planck's principle,* fits well with T. Kuhn's notion of *paradigm shift,* in which the old and new paradigms are so different that they are incommensurable. Similarly, toward the end of the *On the Origin of Species,* Darwin noted that

> A few naturalists, endowed with much flexibility of mind, and who have already begun to doubt on the immutability of species, may be influenced by this volume; but I look with confidence to the future, to young and rising naturalists, who will be able to view both sides of the question with impartiality. (cited in Hull, Tessner, & Diamond, 1978, p. 718)

Darwin's primary defender, T. H. Huxley, went further, arguing that men of science ought to be strangled on their 60th birthday, lest they retard scientific progress (Hull et al., 1978).

Hull et al. (1978) compared the ages of scientists who accepted and rejected the idea that species evolved in the 10 years after publication of *Origin;* the rejecters, on the average, were 10 years older—a statistically and practically significant difference. On the other hand, as with productivity, age accounted for less than 10% of the variance in theory acceptance, and can only provide weak support for Planck's principle. Indeed, Sulloway (1996) has recently published an exhaustive historical analysis of theory acceptance in science and concluded that birth-order accounted for more variance than any other single variable. Furthermore, Messeri (1988) studied age differences in acceptance of plate tectonics after the discovery of sea-floor spreading by Hess and Dietz in the early 1960s. During the period immediately following publication of this new idea, older scientists were significantly more likely to adopt plate tectonics than younger ones, exactly the reverse of what one would expect if Planck's principle were true. Later, after substantial confirmatory data had been disseminated, age no longer played a role in theory acceptance. Finally, Levin, Stephen, and Walker (1995) used logit regression on the data collected by Hull et al. (1978) and divided it into the same time periods used by Messeri. They found no significant relationship between age and theory acceptance and concluded that "No researcher to date has found substantial effects of age on the acceptance of new ideas" (Levin et al., 1995, p. 281).

Gender and Science

One of the more contentious and polemical domains of the psychology of science concerns the role that gender plays in science in general (cf. Keller, 1985) and in scientific and mathematical ability and achievement in particular. The topics of gender and science and gender differences in scientific achievement could in and of themselves be the focus of a review article, and we leave the more exhaustive review of this literature to others. There are three questions that we believe have accumulated enough literature to warrant our attention, and each concerns gender differences: first in mathematical ability, second in productivity, and third in quality of scientific work.

Are there gender differences in mathematical ability? One of the more consistent and robust findings in the gender-difference literature involves mathematical ability, with male participants scoring higher than female participants throughout the distribution of scores (Astin, 1975; Backman, 1972; Benbow, 1988; Benbow

& Stanley, 1980, 1983; Deaux, 1985; Fischbein, 1990; Fox, 1976; Holden, 1987; Keating, 1974; Maccoby & Jacklin, 1974; Moore & Smith, 1987; Stanley, 1988). Both longitudinal and cohort data over the last 20 years suggest that the gender difference is remaining constant at around .50 standard deviation in favor of males. However, as Maccoby and Jacklin (1974) first reported, there are a couple of qualifications to this generalization. First, there is little to no gender difference before adolescence and second, at least up through early adolescence, girls achieve higher grades than boys in math classes.

One of the largest studies ever of mathematical ability was started by Julian Stanley in 1971 at Johns Hopkins University and is titled the Study of Mathematically Precocious Youth. In the tradition of Terman (1925) and Cox (1926), Stanley and his colleagues studied large but select samples of mathematically precocious young people, who they defined as scoring at or above 700 on the SAT–M before age 13 (Stanley, 1988; Stanley, Keating, & Fox, 1974). That this is an extremely selective criterion is beyond dispute: only 4% of male college-bound high school seniors and fewer than 1% of female college-bound high school seniors score 700 or higher on SAT, and Stanley's sample consisted of preadolescents. As both Stanley and Benbow have reported, one of the biggest surprises in collecting these data, however, was the large and consistent gender difference among the extreme scores—ultimately reaching as high as a 12 to 1 ratio in favor of male participants. Furthermore, Benbow's (1988) target article in *Behavioral and Brain Sciences* was commented on by more than 40 experts, and although virtually none of the commentaries took issue with whether a gender difference exists, there was little agreement concerning the potential causes of this gender difference. Indeed, the gender difference raises at least two very important questions: first, how can it be explained, and second do math scores in adolescence actually predict ultimate science and math achievement?

Possible environmental and biological causes for the gender difference in math. Unfortunately, we can only give the briefest overview of this contentious area, and we refer the interested reader to the target article by Benbow (1988) and its commentaries for more detail. More specifically, the author reviewed the evidence for seven of the more common environmental explanations: attitudes toward math, perceived usefulness of math, confidence and self-efficacy, encouragement from parents and teachers, sex-typing, differential course taking, and career and achievement motivation. Benbow found that some of these environmental influences do distinguish male and female participants. For example, girls do like math less, find it less useful for their future goals, and have less confidence in their ability than boys. Furthermore, mathematics is somewhat sex-typed as a "masculine" enterprise, parents and teachers are more encouraging of male than female mathematical achievement, differences in math courses do not explain aptitude differences, and finally male career motivation is more independent of parent or teacher support than female.

However, these findings do not directly address the origin of the difference. As Eysenck (1988) pointed out, the situational findings could result from either genetic or environmental origins. To more directly address the biological explanations, Benbow (1988) offered four possibilities: hemispheric laterality, allergies, hormonal influences, and myopia. For instance, based on a high incidence of left-handedness in the mathematically precocious and in particular the precocious boys, and the greater bilateral or diffuse cognitive functioning of left-handed individuals, Benbow concluded that bilateral or a strong right hemispheric functioning may implicate mathematical ability. Furthermore, prenatal exposure to testosterone has been postulated to influence handedness and immune disorders (cf. Geschwind & Behan, 1982), and therefore could be an indirect influence on mathematical ability. To quote Benbow

> In sum, the above physiological correlates, especially the possibility of prenatal testosterone exposure, lend credence to the view that sex differences in extremely high mathematical reasoning ability may be, in part, physiologically determined (Benbow & Stanley, 1980). Of course, some of the above discussion on physiological correlates is speculative. (1988, p. 182)

Suffice it to say that the physiological explanations were the focus of most of the criticism in the commentaries. However, many criticisms did not take issue with the fact that biological explanations may play a role, but rather that their mechanisms are more complex than, and the evidence is not as solid as, Benbow's presentation.

Predictive validity of mathematical precocity and early scientific achievement. Even with a consistent gender difference, we must still address the predictive validity question, namely to what extent does mathematical precocity translate into becoming a scientist and having an impactful scientific career (Farmer, 1988; Sternberg, 1988b)? Stanley (1988) argued for the predictive value of extreme mathematical precocity:

> these young students seem to have the potential to become the nation's superstars in pure and applied mathematics, computer science, electrical engineering, physics, and other fields that depend heavily on great quantitative aptitude. Quite a few of the 292 [who scored ≥700 on the SAT–M] appear well on the way toward excellence in such fields. (p. 206)

Benbow and her colleagues have presented data showing that precocious ability predicts achievement in high school (Benbow & Minor, 1986; Benbow & Stanley, 1982) and in college (Lubinski & Benbow, 1994). Furthermore, longitudinal research has shown that those who went into math or science careers scored in the 90th percentile on math achievement tests in high school (Wise, Steel, & MacDonald, 1979). But as Farmer (1988) pointed out, only 42% of the male and 22% of the female extremely precocious students went on to choose science or math graduate programs (cf. Benbow, 1988; Benbow & Lubinski, 1993). In short, only 25% of the extremely gifted math sample continue in science and math through graduate school, and even fewer are retained in science and math careers. The retention rate is somewhat higher for those who demonstrate early scientific achievement. Subotnik and Steiner (1992), for instance, reported that by their mid 20s, 81% of the male and 66% of the female Westinghouse Science finalists were still on science-training or science-career tracks.

However, if one chooses a more real-world valid outcome criterion, such as actual creative achievement in math and science, very few of even the extremely talented youth go on to have truly influential careers (Simonton, 1988a; Sternberg, 1988b). The evidence shows that neither grades nor aptitude tests do well at predicting creative achievement in scientific careers (Barron & Harrington, 1981; Gough, 1976; Guilford, 1959; Hudson, 1958; MacKinnon, 1960; Simonton, 1988a; Sternberg, 1988a; Taylor, 1963). Such a lack of predictive validity of aptitude tests is better understood once one takes into account the small relationship between intelligence and creativity (Barron & Harrington, 1981; Getzels, 1987; MacKinnon, 1978; Sternberg, 1986; Wallach, 1970), or in Guilford's terms *convergent* from *divergent thinking* (Guilford, 1987). To solve quickly multiple-choice problems that have known solutions involves convergent or analytical thinking skills, whereas to solve creatively open-ended problems that have no known solutions involves divergent or intuitive thinking skills (Guilford, 1959, 1987; Simonton, 1988a, 1989; Sternberg, 1986). Precocious youth are very intelligent, but not necessarily very creative.

A general conclusion regarding gender differences in math is that boys and men do consistently score higher on mathematical aptitude tests but not on achievement tests or grades. However, the explanation and theoretical accounts for why these differences exist are not yet settled, and currently we can only offer the trite and general conclusion that both biology and environment account for some of the variance. Future empirical work must focus on testing theoretical models and causal factors through quasiexperimental design and multivariate and latent variable analyses if more definitive answers are to be provided.

Do male and female scientists produce works at different rates? Comparing publication rates of men and women has consistently shown that men produce more works than women (J. Cole, 1979, 1987; J. Cole & S. Cole, 1973; Guyer & Fidell, 1973; Helmreich, Spence, Beane, Lucker, & Matthews, 1980; Long, 1992; Pasewark, Fitzgerald, & Sawyer, 1975; Zuckerman & Cole, 1975). This gender difference appears to hold for total number of publications and yearly average (J. Cole, 1987). However, there is some contradictory evidence regarding whether this gender difference increases or decreases across the course of one's career. J. Cole (1987) reported that the gender gap on productivity increases, whereas Long (1992) reported that it decreases over the course of one's career.

One obvious question therefore that begs to be addressed is how to explain the gender difference in total and yearly average publication rates. As with age and productivity, explanations are more contentious and less consensual than the description of the phenomenon. Differences in family obligations, prestige of institution, rank of position, training, and motivation each has been investigated, but with negative or inconsistent results. The intuitively appealing answer that women are hindered by multiple roles of scientist, wife, and mother, and are relegated to marginal departments seems not to have empirical support (J. Cole, 1979, 1987; S. Cole & Zuckerman, 1987). In fact, married women tend to slightly outproduce single women, and women with one or two children tend to outproduce women with no or more than three children (J. Cole, 1979, 1987; S. Cole & Zuckerman, 1987). J. Cole and S. Cole (1973) also presented evidence that gender differences in productivity cannot be explained by differences of institution (college vs. university) or prestige of department. When both of these variables are entered first in a regression equation and thereby held constant, the relationship between gender and productivity still persists.

If prestige of institution and department do not moderate the relationship between gender and productivity, then perhaps other variables do. The most obvious candidate for a moderating variable would be rank of academic position. Perhaps men outproduce women because they are at higher ranks. Guyer and Fidell (1973), however, found gender differences in productivity even when comparing male and female professors at the associate and full ranks, but not at the assistant rank. Moreover, J. Cole (1987) reported that promotion to higher academic rank is one of the few remaining areas of sex discrimination in science. Even holding other variables constant, such as quantity and quality of publication, and career interruptions, women are still less likely to be promoted than men, which could indeed be a factor in their lower productivity.

Is there a gender difference in quality of works produced? With citation counts as the measure of quality and impact, some researchers have found that men receive more citations than women (J. Cole, 1987; J. Cole & S. Cole, 1973; Reskin, 1977). But this may be an artifact of greater number of published papers by men (S. Cole & Zuckerman, 1987). In fact, once number of publications is held constant, women produce works of greater impact than men (Long, 1992; Sonnert, 1995).

Development of Scientific Reasoning

In addition to precocity, age, and gender another contribution of the developmental psychology of science involves examining the cognitive processes that children of different ages use when trying to solve scientific problems. Jean Piaget was the great pioneer in this area. He felt that, in the case of scientific thinking, ontogeny recapitulated phylogeny: the child's development of scientific thought recapitulated the history of science (Piaget & Garcia, 1989). Children, in this view, begin with a kind of Aristotelian view of how the world operates, and—if they reach the highest level of formal operations—end up with a Newtonian or perhaps even an Einsteinian view, one they have internalized, not merely memorized (McCloskey, 1983; Wiser & Carey, 1983). Piaget also made an enormous methodological contribution; he inspired researchers to question children closely, and pose problems for them that would reveal not only what they knew, but how they knew it, and how their knowledge could be changed by additional experimentation.

One fundamental question involves whether children's thought processes are categorically similar or categorically different from those used by adults (Fay, Klahr, & Dunbar, 1990; Klahr, Fay, & Dunbar, 1993; D. Kuhn, 1989; D. Kuhn, Amstel, & O'Loughlin, 1988; Siegler & Liebert, 1975). The heuristic and decision-making movement in cognitive psychology has contributed to the widespread dissemination of the *intuitive scientist* metaphor. In this view, children and nonscientist adults construct cognitive models, evaluate evidence, and modify their conceptualizations of how the world works in a similar but less developed manner to scientists (Brewer & Samarapungavan, 1991; Karmiloff-Smith, 1988). D. Kuhn (1989) and Klahr et al. (1993) argued that although there is some validity to this metaphor, especially in understanding of the world, it is quite misleading and inaccurate when applied to the cognitive processes used by children, novice adults, and scientists. D. Kuhn, for example, reviewed much of the relevant literature and concluded that in terms of thinking like a scientist (i.e., coordinating theory and evidence) neither novice adult nor child is capable of such systematic thinking. When confronted with disconfirming evidence, children often unknowingly distort, selectively make use of the evidence, or unconsciously adjust theory to fit with the evidence. Adult scientists, however, make a clear distinction between theory and evidence and therefore can systematically and consciously modify and manipulate the evidence one piece at a time to see what effect it has and whether the theory needs to be modified.

D. Kuhn's conclusions are in line with the work on searching in two spaces (hypothesis and experiment) by Klahr and his colleagues, in that they both conclude that children fail to distinguish theory from evidence. Klahr, Fay, and Dunbar (1993) performed a fairly sophisticated experiment in order to test whether there are developmental differences in scientific problem solving heuristics. They tested four different groups who varied on age and scientific–technological skill: 3rd graders, 6th graders, community college students with little technical training, and college students with technical training. Results showed rather clearly that under some circumstances children can perform cognitive processes that are similar to adults, but under other circumstances they cannot. When the actual hypotheses are plausible or when the experimental alternatives are few in number children perform similarly to adults. But when the actual hypotheses are implausible or the alternatives are not few in number, adults' performance is categorically superior to children's. Children were not able to consider two alternative hypotheses when the actual hypothesis was implausible, and they stuck with their original plausible but incorrect hypothesis. Adults, on the other hand, were able to search for solutions in two spaces simultaneously, namely hypothesis and experiment.

Mentorships and Training

What role do family members or teachers play in promoting and retaining scientific interests? It has long been assumed that family and school influences are critical to the development of scientific interests. But what does the empirical literature say about these social influences? Eiduson (1962) reported that roughly half of her participants said that some older person was important in their developing and maintaining an interest in science. John-Steiner (1985) eloquently described the importance of apprenticeships and mentorships in the stimulation of creative activity in science and art. Furthermore, Feist (1991) reported that 65% of the elite biological and natural scientists in his sample reported having a significant mentor in high school, and 80% reported having one in graduate school. In high school, mentors tended to be either a teacher (29%) or a parent (26%), whereas in graduate school they were overwhelmingly one's PhD advisor (56%) or another professor (20%). A question raised by these percentages, however, is how they compare to other professions. Whether these figures are unique to the sciences remains to be seen.

Werts and Watley (1972) demonstrated that the family environment can exert a strong influence on choosing science as a career. They reported that college students who won awards and were high achievers in science had fathers who were scientists. Furthermore, a consistent and robust finding from the literature on father's education and occupation is that scientists overwhelmingly come from families of professional occupations and higher education (Berry, 1981; Chambers, 1964; Feist, 1991; Helson & Crutchfield, 1970; Roe, 1952a; Zuckerman, 1977). Either directly or indirectly, having well-educated parents familiar with and interested in science is predictive of an interest in science.

Developing an early interest in science has importance to the extent that it translates into becoming and staying a scientist. Subotnik and her colleagues have collected longitudinal retention data on a sample from finalists to the prestigious Westinghouse Science Talent Search (Subotnik, Duschl, & Selmon, 1993; Subotnik & Steiner, 1992). Five years after being Westinghouse finalists, 94 students took part in a follow-up study that investigated why some stayed in science and others did not. Even with this elite and prestigious group of gifted science students, almost 30% were not pursuing scientific careers just 5 years later. Gender (being female), lack of enthusiastic and motivating high school and college teachers and mentors, and lack of financial support were the strongest discriminators between the two groups. Similar findings on the gender difference in retention of highly precocious math students have been reported by Benbow and her colleagues (Benbow, 1988; Benbow & Lubinski, 1993; Lubinski & Benbow, 1994).

When students complain about lack of

enthusiastic teaching they are no doubt referring to the fact that science often is taught in a manner that emphasizes its "factual" basis and ignores the process of asking questions and discovering solutions. To the extent that only the rational, factual, and objective side of science is taught, children develop the mistaken belief that science is "boring and dry." Feist (1991) reported among eminent scientists a consistent complaint that the "fun" part of doing science was completely absent in primary education of science. Curiosity is a sine qua non of science. Consistently asking, "How does this work? Or, what happens if I do this?" appears to be instrumental to the development of scientific interests, and if curiosity is squelched there is little hope that an otherwise intelligent child will want to pursue a career in science.

Does being trained by an eminent scientist predict obtained eminence? As reported above, having a strong mentor in high school and college does predict staying on and pursuing a scientific career (Subotnik, Duschl, & Selmon, 1993; Subotnik & Steiner, 1992). Having an eminent mentor also appears to be a contributing factor in obtaining eminence (John-Steiner, 1985; Simonton, 1992b; Zuckerman, 1977). This finding has been most clearly demonstrated in Harriet Zuckerman's work with Nobel laureates. One of her strongest findings concerned the cumulative advantage effect of those young scientists who train under the scientific elite (i.e., Nobel prize winners). They produce more at an early stage in their careers, are more likely to produce works of high impact, and are more likely to win the Nobel prize themselves than those who do not train under laureates (Zuckerman, 1977). As Zuckerman and others have argued, however, the causal direction of this influence probably goes both ways: the best young scientists are chosen by the best scientists, which in turn feeds into the cycle of cumulative advantage. Simonton (1992b) also reported that American Psychological Association (APA) presidents were quite likely to have been mentored by an eminent psychologist (i.e., they had an entry in a biographical dictionary).

Birth Order

Beginning with Galton in the 1870s, a number of researchers have reported that compared to nonscientists, scientists are disproportionately first born (e.g., J. Cattell & Brimhall, 1921; Clark & Rice, 1982; Eiduson, 1962; Galton, 1874; Roe, 1952a). However, an even more interesting question arises concerning whether the same holds true for the most creative and eminent scientists. Helson and Crutchfield (1970) found that creative scientists were more likely to be first born than less creative scientists. The largest and most ambitious study to date of birth-order and scientific eminence (Sulloway, 1996) found a curvilinear relationship, with first and last born scientists being the most eminent (cf. Feist, 1991).

Religious Background

Another way family may influence scientific development is through its dominant religious orientation. Many researchers have reported that a disproportionate number of eminent and creative scientists come from Protestant or Jewish families compared with Catholic backgrounds (Chambers, 1964; Datta, 1967; Feist, 1991; Helson & Crutchfield, 1970; Roe, 1952a; Zuckerman, 1977). For example, whereas only 2 to 3% of the American population comes from Jewish backgrounds, the percentage of eminent and elite scientists from Jewish backgrounds ranges from 9% (Roe, 1952a) to 38% (Helson & Crutchfield, 1970). In fact, among the most creative and elite groups of scientists most estimates suggest that 20 to 30% come from Jewish families (Chambers, 1964; Datta, 1967; Feist, 1991; Zuckerman, 1977). Religious background, however, does not tease apart variability due to religious orientation, culture, race, or even genetic influence. Moreover, we must make clear that these data refer to the religious faith of one's family background and upbringing, not one's current behavior. Scientists in general, and eminent scientists in particular, are conspicuous in their rejection of organized religion. The few studies that have asked scientists about their current religious practices have reported an almost complete absence of current religious faith (Chambers, 1965; Feist, 1991; Roe, 1952a; Terman, 1954, 1955).

Conclusions From Developmental Psychology of Science

Among the conclusions that can be drawn about the developmental psychology of science literature (see Table 1) are: (a) some people are born with extreme mathematical talent and genius; (b) there is a curvilinear relationship between age and productivity, with the peak generally occurring in one's late 30s or early 40s; (c) early productivity does predict later productivity; (d) young scientists do not necessarily produce a disproportionate number of high-quality works; (e) older scientists are not more resistant to accepting new theories compared to younger scientists; (f) there are gender differences in mathematical ability, and the etiology of these differences remains unclear, but biological and social factors each appear to play a role; (g) men publish more than women, but whether their work is of higher quality is unclear, and differences in marital status, family size, prestige of department and institution do not account for the gender differences in quantity, but differences in academic rank may; (h) children's thought processes when solving scientific problems are categorically different from novice adults or scientists in that they are not able to coordinate their thinking to simultaneously consider theory–hypothesis and evidence–experiment; (i) enthusiastic teachers and families who value education are critical to the development of an interest in science; (j) having an eminent mentor predicts scientific eminence; (k) effects of birth order on choosing science as a career and producing high-quality works in science are unclear and inconsistent; and finally (l) coming from a Jewish background is related to producing high-quality scientific work.

Cognitive Psychology of Science

Instead of reviewing the entire history of attempts by psychologists to understand scientific thinking (see Campbell, 1989, for a good overview of psychological epistemologies), we look at the current state of the field (primarily from the late 1970s to the present), cite studies that illustrate the major findings, and explicate consistent trends and general principles. To organize this review, we could attempt to categorize research based on its epistemological foundations, as Campbell, Tweney, and others have done. However, it is a virtual guarantee that no two psychologists will agree on these epistemological roots. For example, Tweney (1994; Tweney & Chitwood, 1995) distinguished Kuhnian, Piagetian, Simonian (as in Herbert Simon), and Wasonian (as in Peter Wason) traditions. None of these traditions, however, is mutually exclusive, and in fact Simon's approach to psychology of science is explicitly identified with Kuhn (Bradshaw, Langley, & Simon, 1983), and Wason's with Popper as well as Piaget (Gorman, 1992). De Mey (1992) also shows how the Kuhnian and Piagetian traditions can be tightly linked.

Therefore, we attempt to use methodological rather than theoretical categories to sort studies, focusing more on results than on the epistemologies that lie behind them. In particular, we divide the literature along two lines, namely the nature of the task and the type of participant. The nature of the task involves essentially two types of problems: either abstract tasks that simulate scientific reasoning or actual scientific problems. This category is not a dichotomous variable. Some of the tasks used to simulate scientific reasoning are modeled closely after scientific reasoning, and some of the actual scientific problems resemble those encountered in textbooks rather than in the laboratory. Moreover, studies have used two categories of participants—either novices or experts. The *novice* category of participant is mostly made up of college students of a variety of backgrounds, some of whom may have taken a few science courses, but none of whom are practitioners. *Experts*, on the other hand, are defined as practicing scientists of varying abilities. Again, this is not a dichotomous variable. Novices can range from children to graduate students, and practitioners from scientists at the beginning of their careers to eminent veterans. This way of organizing the literature and its basic findings on cognitive psychology of science are summarized in Table 2.

Simulated Scientific Tasks: Novices

Confirmation bias. The literature using simulated tasks with novices primarily has examined how (quasi-)scientific hypotheses are tested. Moreover, almost all of this literature has focused on confirmation bias or some variation thereof. The impetus for this line of research was Karl Popper's (1959) assertion that science should, and the best science does, progress by falsifying hypotheses, not by proving them right. Wason (1960) decided to find out whether novices could falsify by asking them to determine the rule governing a sequence of number triplets, given that the triplet 2–4–6 was an instance of the rule. Participants proposed additional triples, and the experimenter told them whether each fit the rule. When they felt they were ready, participants would tell the experimenter what they thought the rule was, and the experimenter would tell them whether it was the rule he had in mind. If not, they could continue to propose triples and make guesses. Participants typically proposed triples like 6–8–10 and 10–12–14 and guessed the rule was something like "numbers must go up by twos." In fact it was "all three numbers in the triple must ascend in order of magnitude." Wason took this performance as evidence of a confirmation bias on the part of participants because they found a rule sufficient to explain the first pattern that generated positive instances.

Novices can be trained to seek negative evidence. Gorman and his colleagues (Gorman, 1986; Gorman & Gorman, 1984; Gorman, Stafford, & Gorman, 1987) found that instructions to falsify significantly improved performance on the 2–4–6 and similar tasks. So it appeared that confirmation was a bias that could be combated with education. However, Klayman and Ha (1987) argued that Wason, Gorman, and others had confused positive and negative test heuristics with confirmation and disconfirmation. On the 2–4–6 task, a positive test heuristic involves trying to get triples right if one believes one's hypothesis is correct. If, for example, a participant proposes 2, 6, 4, expecting it to be right if her hypothesis is correct, she is following a positive test heuristic (Klayman, personal communication, May 14, 1997). So she might think the rule was "three even numbers," and if she obtains a "no" response she has obtained disconfirmation using a positive test. If, on the other hand, her hypothesis is "go up by twos" and she proposes 2–6–4, this would be a negative test because if the hypothesis is correct the triple will be incorrect and confirm rather than disconfirm her hypothesis. Therefore, negative tests are not to be equated with disconfirmation. Klayman and Ha argued that seeking positive tests is a good all-purpose heuristic and in most situations is even more likely than a negative test heuristic to produce falsifications (see also Oaksford & Chater, 1994). However, when the target participant's rule is more general than her initial hypothesis, the participant needs to be encouraged to seek negative evidence (Gorman, 1992).

Confirm early, disconfirm late. A group at Bowling Green State University (Mynatt, Doherty, & Tweney, 1977, 1978) developed an artificial universe task that bore more resemblance to science than abstract problems like the 2–4–6. Participants spent about 10 hours on the most complex of these tasks, and none of them discovered the rule. The ones that made the most progress exhibited a kind of confirmation bias, but with one important qualification: confirmation bias is most effectively used only early in the hypothesis testing process. Mynatt, Doherty, and Tweney concluded that confirmation was an effective heuristic early in the inference process; once a participant or scientist had discovered and verified a pattern, then she could switch to the search for disconfirmatory evidence. This heuristic combination of confirmation and disconfirmation also worked on abstract problems like the 2–4–6 task, but its value became most apparent on tasks that more closely simulate scientific problems. Initially the best way to follow this confirmatory heuristic would be to conduct positive tests. One of these positive tests might lead to disconfirmation, but the overall goal would be to discover a pattern and verify that it was consistent enough to form the basis for a hypothesis.

Computational simulation of abstract problems. We use the term *simulation* to refer to the programs in this section because their primary goal is to model, understand, and improve human scientific problem solving. Naturally, this has implications for machine-learning, whose goal is finding ways of making machines solve problems, but an effective problem-solving heuristic for a machine may be very different than one for a human being. Herbert Simon and a group of colleagues at Carnegie-Mellon (Langley, Simon, Bradshaw, & Zykow, 1987) developed a series of computer programs designed to emulate scientific discoveries, for example Kepler's law. The simplest of these, called *BACON,* was given columns of numbers and asked to find a relationship, using heuristics like "if the terms in two adjacent columns increase together, compute their ratio." The relationship turned out to be the numerical equivalent of Kepler's third law.

Qin and Simon (1990) gave the same task to 14 college students. Four of them were able to

Table 1
Summary of Topics and Findings in the Developmental Psychology of Science Literature

Topic	Results	Author(s)
Mathematical ability	Truly outstanding mathematical genius appears to be inborn	Bell, 1937 Kanigel, 1991 Wiener, 1953
	Genetics accounts for some of the variability in math ability	Husén, 1960 Loehlin & Nichols, 1976 Bouchard & McGue, 1981 Scarr & Saltzman, 1982 Vandenberg, 1988
Age and productivity	There is a curvilinear relationship between age and productivity, with the peak generally occurring in one's late 30s or early 40s	Lehman, 1953, 1960, 1962, 1966 Dennis, 1954, 1956 Bayer & Dutton, 1977 Zuckerman, 1977 S. Cole, 1979 Over, 1982, 1989 Simonton, 1984, 1988a, 1988b, 1989 Diamond, 1986 Horner et al., 1986
	Early productivity does predict later productivity	Lehman, 1953 Dennis, 1954 Roe, 1965 Helson & Crutchfield, 1970 Over, 1982 Simonton, 1988b, 1989
Age and quality	Young scientists do not necessarily produce a disproportionate number of high-quality works	S. Cole, 1979 Over, 1989
Age and theory acceptance	Older scientists are not more resistant to accepting new theories compared to younger scientists	Barber, 1961 Hull et al., 1978 Messeri, 1988 Levin et al., 1995
Gender differences in math ability	There are gender differences in mathematical ability, but the etiology of these differences remains unclear; biological and social factors each appear to play a role	Backman, 1972 Keating, 1974 Maccoby & Jacklin, 1974 Astin, 1975 Fox, 1976 Benbow & Stanley, 1980, 1983 Deaux, 1985 Holden, 1987 Moore & Smith, 1987 Benbow, 1988 Stanley, 1988 Fischbein, 1990
Gender and productivity	Men publish more than women; differences in marital status, family size, prestige of department and institution do not account for the gender differences in quantity, but differences in academic rank may	J. Cole & S. Cole, 1973 Guyer & Fidell, 1973 Pasewark et al., 1975 Zuckerman & J. Cole, 1975 J. Cole, 1979, 1987 Helmreich et al., 1980 Long, 1992
Gender and quality	Men are more frequently cited when quantity is not taken into account, but women are more cited when publication total is held constant	J. Cole & S. Cole, 1973 Reskin, 1977 J. Cole, 1979 Helmreich et al., 1980 Long, 1992 Sonnert, 1995
Development of scientific reasoning	Children's thought processes when solving scientific problems are categorically different from novice adults or scientists; they are not able to coordinate their thinking to consider simultaneously theory/hypothesis and evidence/experiment	Siegler & Liebert, 1975 D. Kuhn et al., 1988 D. Kuhn, 1989 Fay et al., 1990 Klahr et al., 1993

Table 1 *(continued)*

Topic	Results	Author(s)
Influence of parents, teachers, and mentors	Enthusiastic teachers and families who value education are important to the development of an interest in science	Roe, 1952a Eiduson, 1962 Chambers, 1964 Helson & Crutchfield, 1970 Werts & Watley, 1972 Zuckerman, 1977 Berry, 1981 John-Steiner, 1985 Feist, 1991 Subotnik & Steiner, 1992 Subotnik et al., 1993
	Having an eminent mentor predicts scientific eminence	Zuckerman, 1977 John-Steiner, 1985
Birth order and scientific eminence	Scientists are more likely to be first borns compared with nonscientists	Galton, 1874 Cattell & Brimhall, 1921 Roe, 1952a Eiduson, 1962 Clark & Rice, 1982
	Creative and eminent scientists are most likely to be either first or last born	Helson & Crutchfield, 1970 Feist, 1991 Sulloway, 1996
Religious background	Coming from a Jewish background is related to producing high-quality scientific work	Roe, 1952a Terman, 1954 Chambers, 1964 Datta, 1967 Helson & Crutchfield, 1970 Zuckerman, 1977 Feist, 1991

emulate the computer's discovery using heuristics similar to BACON's. Although this appears to be a task that models the reasoning of an actual scientific expert, it actually bears more resemblance to the 2–4–6 task. Instead of three columns of numbers, Qin and Simon's participants were given two, and the numerical rule they were seeking happened to correspond to Kepler's Law. Interestingly, students trying to solve this problem relied more on visual representations like scatter plots than the program, which could not use diagrammatic reasoning. (More recent computational simulations have this capacity; see Cheng & Simon, 1995; Larkin & Simon, 1987.)

Searching for two rules rather than one. Searching for two complementary rules rather than one appears to increase successful hypothesis testing on abstract tasks. Tweney, Doherty, and Mynatt, (1981), Gorman, Stafford, and Gorman (1987), and Wharton, Cheng, and Wickens (1993) altered Wason's (1960) task to make it a search for two complementary rules rather than a single rule. They found that this change made it much easier for participants to explore the limits of their hypotheses, thereby facilitating discovery of the target rule. Similarly, Farris and Revlin (1989a, 1989b) argued that many participants who appeared to be following a disconfirmatory strategy were actually searching for positive instances of a counterfactual hypothesis. In effect, participants following a counterfactual strategy were searching for two complementary rules (see Oaksford & Chater, 1994). This counterfactual heuristic would be best applied after the initial positive test phase when the scientist or participant has discerned a pattern and formulated an initial hypothesis. Conceptualizing the problem as a search for two complementary hypotheses focuses participants on exploring the boundaries of both, with the result that they conduct a more thorough search of the problem space. Similarly, Freedman (1995) argued that limits on working memory may explain why novices tend to focus on one hypothesis at a time. Instructing participants to search for multiple hypotheses (i.e., to use strong inference strategies) did in

Table 2
Summary of Topics and Findings in the Cognitive Psychology of Science Literature

Task	Participants	Topic	Results	Author(s)
Simulated	Novices	Hypothesis testing	Hypothesis testing is biased toward confirming evidence	Wason, 1960
			Confirm early/disconfirm late heuristic is most effective strategy	Mynatt, Doherty & Tweney, 1977, 1978
			Success in hypothesis testing is increased by searching for two complementary rules rather than one	Tweney et al., 1981 Gorman, Stafford, & Gorman, 1987 Wharton, Cheng & Wickens, 1993
			College students form commonsense representations of scientific phenomena	McCloskey, 1983
			Instructions to falsify improves performance on hypothesis testing	Gorman & Gorman, 1984
			Confirmation bias is not synonymous with positive-test heuristic	Klayman & Ha, 1987
			Successful hypothesis testing is facilitated by thinking simultaneously in two problem spaces (possible alternatives)	Klahr & Dunbar, 1988 Klahr, Dunbar, & Fay, 1990
			Scientific knowledge structures change by the development of more differentiated and elaborate conceptual nodes	Gholson & Houts, 1989
			Disconfirmatory strategies may be positive instances of counterfactual hypothesis testing	Farris & Revlin, 1989a
			Error encourages hypothesis perseveration	Gorman, 1989, 1992
			Adults tentatively make separation between dual spaces	Brewer & Chinn, 1992
			Adults have metacognitive skills absent in children that allow for better coordination of searches in two spaces	Klahr, Fay, & Dunbar, 1993
			Novices' inability to search in dual space is related to working-memory capacity	Freedman, 1995
		Computer simulation	Novices use heuristics similar to a computer simulation	Langley et al., 1987 Qin & Simon, 1990
	Experts	Hypothesis testing	Scientists are more prone to confirmation bias than ministers	Mahoney, 1977
			In scientists, experiments & hypothesis spaces are fully separated, whereas in children experiment & hypothesis spaces are merged; in novice adults, they are partially separated	Mahoney, 1977 D. Kuhn, 1989
Actual	Novices	Textbook problems	Novices gradually develop metacognitive skills as they learn to solve new problems	Anzai, 1991
			Scientists work on problems in a forward, abstract manner rather than backward and concretely as novices do	Larkin, McDermitt, Simon, & Simon, 1980 Larkin, 1983
			Scientists use informal, qualitative logic (analogous, simpler cases) solve problems	Clement, 1991
			Scientists form abstract representations (1st principles)	Chi, Feltovich, & Glaser, 1981

Table 2 *(continued)*

Task	Participants	Topic	Results	Author(s)
	Experts	Cognitive bias	Scientists are just as prone to cognitive bias as nonscientists	Hanson, 1962 Kruglanski, 1994 Mahoney, 1977, 1979 Faust, 1984 Mahoney & DeMonbreun, 1977
		Analogy	Creating and using analogies facilitates creative insight and scientific discovery	John-Steiner, 1985 De Mey, 1989 Gentner & Jeziorski, 1989 Dunbar, 1995
		Laboratory/field problems	Senior scientists are least likely to show confirmation bias, and in fact exhibit a falsification bias; experts are more likely to modify or discard hypotheses compared with novices; the most successful labs make use of local, domain-specific analogies and heuristics; finally, serendipity plays an important role in discovery	Dunbar, 1995
		Cognitive complexity	Scientists are more complex thinkers than nonscientists	Suedfeld, 1985 Feist, 1994
		Historical case studies	Darwin developed a network of cognitive enterprises	Gruber, 1981, 1989 Tweney, 1985, 1989
			Faraday confirmed early and disconfirmed later	
			Faraday used a large repertoire of hands-on procedures, which influenced his mental models	Gooding, 1985, 1990
			In physics, imagery and metaphor have changed from being perceptually based to being propositionally based	Miller, 1989
			Use of metaphor and analogy is critical to creation of scientific knowledge; conceptual schemes can greatly inhibit the creation of new knowledge	De Mey, 1989
		Computer simulation of historical case studies	Krebs used both general and domain-specific heuristics to discover the ornithine cycle	Kulkarni & Simon, 1988
			Computer programs demonstrate the importance of visual diagrams in scientific discovery	Cheng & Simon, 1995
			Developed a computer simulation program that allows researchers to reconstruct steps of discovery	Gooding & Addis, 1993
		Inventors search in dual space	Wright brothers made use of dual space	Bradshaw, 1992
			Bell made use of mental models and heuristics in the invention of the telephone	Gorman, 1995

fact enhance their ability to test hypotheses successfully.

Replication and error. One of the limitations of most abstract tasks and simulated scientific problems is that they include no possibility of error in the results, even though working scientists struggle constantly to separate patterns from noise (see Gorman, 1992, for an extended discussion). Gorman (1989) told participants that anywhere from 0 to 20% of their results on an abstract task similar to the 2–4–6 might be erroneous (i.e., a trial that was classified as inconsistent with the rule might be consistent and vice versa). Errors would occur at random, as determined by a random-number generator on a calculator. Initially, the error rate was set at 0; participants encountered no actual errors. Gorman found that participants used *replication plus extension* to eliminate the possibility of error: they proposed experiments that were similar to, but not exactly the same as, previous experiments in an effort to replicate the current pattern. This strategy resembles the positive test heuristic recommended by Klayman and Ha.

But replication and replication-plus-extension are costly heuristics: they require a significant investment of time and resources of a laboratory while other competing laboratories may be pursuing novel research (see Gorman, 1992, for a discussion). The cost and complexity of replication can be increased on experimental tasks that incorporate the possibility of error. For example, when participants have to replicate an entire sequence of experiments rather than just a single result, the possibility of error encourages hypothesis perseveration, or a reluctance to discard a hypothesis in the face of occasional disconfirmation. When the possibility of 20% error is converted to actual error, participants had even more difficulty using replication and replication plus extension to combat hypothesis perseveration (Gorman, 1989). Even on very simple artifical tasks, replication alone is not sufficient to isolate and eliminate errors. Obviously, scientists rely on other kinds of checks in addition to replication, for instance refinement of procedures. Future experiments on error should use scientific problems and tasks that simulate them, and also compare the performance of scientists to novices.

Form commonsense representations. Researchers like McCloskey (1983), Clement (1982), and Carey (1992; Wiser & Carey, 1983) have established parallels between the mental models of modern novices and historical figures in the evolution of science. For example, McCloskey found that college students held beliefs about momentum that resembled those of Philoponus (6th century) and Buridan (14th century). These historical analogies suggest that novices form a kind of commonsense representation of scientific phenomena (Anzai, 1991).

Simulated Scientific Tasks: Experts

Metacognitive coordination of search in two spaces. Another perhaps related heuristic involved in successful hypothesis testing is the ability to think simultaneously of two or more *problem spaces,* where this term denotes a set of possible alternatives. Klahr and Dunbar (1988) and Klahr, Dunbar, and Fay (1990) asked participants to learn how a device called a *Big Trak* functions by conducting experiments. They found that it was most useful to think metacognitively in terms of separate problem spaces, where one space contained ideas for possible experiments and another contained space for possible hypotheses. The most successful participants reacted to falsificatory evidence in the experimental space by developing new hypotheses that represented a shift in the way they represented the function of the device, which in turn suggested new areas of the problem space to search for evidence.

As discussed in the developmental psychology of science section above, D. Kuhn (1989) argued that the dual-space model of Klahr and his colleagues helps to account for major differences between child, novice adult, and scientist. In the child, experiment and hypothesis spaces are merged into a single mental model; theory and evidence are adjusted to maintain this representation. In the scientist, theory and evidence are clearly separated. The novice adult falls somewhere between. Brewer and Chinn (1992) have explored the scientific beliefs of novice adults by giving them brief readings on quantum theory or special relativity that made predictions that conflicted with commonsense beliefs about space and time and cause and effect. Some participants simply rejected the new information, resembling those scientists who cling to the old paradigm. Other participants interpreted the answer in terms of existing beliefs, for example, by treating relativistic phenomena as optical illusions. Similar to children, these participants adjusted evidence to fit beliefs. A final group of participants showed at least partial assimilation of the new material: they were able to give an answer that corresponded to what they had read, but they "sure didn't believe it" (1992, p. 70). These participants are beginning to make the separation between hypothesis and evidence, though they do not trust their conclusions. The key, according to D. Kuhn (1989) and Klahr, Fay, and Dunbar (1993) is the development of metacognitive skills that permit delineation of theory and evidence, and a coordinated search in two spaces.

Confirmation bias. Mahoney (1977) compared a small sample of scientists working on the 2–4–6 task to a sample of Protestant ministers and, surprisingly, found that the former were more prone to confirmation bias than the latter. Kruglanski (1994) has argued that scientists are subject to some of the same cognitive biases as nonscientists, including confirmation (see also Faust, 1984; Hanson, 1962; Mahoney, 1977, 1979; Mahoney & DeMonbreun, 1977).

Actual Scientific Problems

Experts work forward. Larkin, McDermitt, Simon, and Simon (1980) used kinematics problems from an elementary physics textbook; experts, of course, solved the problems much more rapidly and with fewer errors. But the critical finding was that more efficient problem solving uses qualitative problem-solving strategies. Similarly, according to Larkin (1983), experts worked forward from the information given, reasoning qualitatively until they arrived at a representation that suggested what set of equations to use. Novices, in contrast, worked backwards from the possible solution, applying equations early in the hopes of finding the values of specific variables.

Experts form abstract representations. In contrast to novices who tend to form commonsense representations, expert scientists form abstract representations of scientific phenomena. Chi, Feltovich, and Glaser (1981) found

> that experts tended to categorize problems into types that are defined by the major physics principles that will be used in solution, whereas novices tend to categorize them into types as defined by the entities contained in the problem statement. (p. 150)

For example, when asked to predict whether a yo-yo on a table will roll to the left or right when one pulls on a string, novices say right based on their commonsense experience with yo-yos, whereas experts classify the problem in terms of momentum and force equilibrium and conclude the yo-yo will move to the left (Anzai, 1991). Once an expert has classified a problem in this way, she can work forward rapidly to a solution (Green & Gilhooly, 1992).

Expert use of analogical reasoning. There is a long literature on the importance of metaphor and analogy in scientific problem solving (Clement, 1989; De Mey, 1992; Holyoak & Thagard, 1995) and even in psychology (Leary, 1990). Clement (1991) compared the way technical experts and novices solved more unusual problems like determining what happens when the width of the coils on a spring is doubled and the suspended weight is held constant. Experts used informal, qualitative reasoning processes; for example, they often constructed an analogous simpler case, for instance, imagining what happens if the coils were replaced by a U-shaped spring of the same length. Then they related the analogy to the case (see John-Steiner, 1985). Analogies are one very important means for arriving at appropriate problem representations. Gentner and Gentner (1983) demonstrated that novices who used a flowing-waters analogy to understand electric circuits formed a mental model that was appropriate for battery problems but not for ones involving resistors. Green and Gilhooly (1992) argued that

> the standard expert-novice contrastive paradigm by requiring use of problems accessible to novices has led to a relative neglect of how experts tackle difficult problems and how experts detect and recover from errors in the face of task difficulty. (p. 67)

One solution to this difficulty is to look at how experts solve difficult, novel problems. Gentner and Jeziorski (1989) compared the way Robert Boyle and Sadi Carnot used analogies to the way alchemists used them and concluded that following certain criteria when using analogies (e.g., avoiding mixed analogies, understanding that analogy is not causation) was the key to distinguishing scientific reasoning from the pseudoscientific. But Carnot and Boyle had different styles of analogical reasoning: the former relied on a single analogy, deriving principles from it, whereas the latter preferred to work with a whole family of analogies. Nersessian (1992) observed that James Clerk Maxwell used analogies iteratively, that is, he constantly modified them to fit his growing understanding of the constraints of the target domain. Alexander Graham Bell deliberately followed the analogy of nature and used the human ear as a mental model for his telephone; like Maxwell, he was able to modify this analogy as he learned more about his target domain (Gorman, 1995).

Dunbar (1995), in a cognitive study of molecular biology laboratories, noticed that the least successful of his four laboratories used virtually no analogies, whereas the other three used local analogies to change representations and procedures. A *local analogy* involves drawing on a similar experiment to solve a problem with the current one. The backgrounds of the members of the laboratory that used no analogies were too similar; they all drew on the same knowledge base. Dunbar also noted that expert scientists made more analogies than relative novices because the deep, structural features of a domain were obvious to them and they could therefore map them readily onto other domains.

Experts confirm early and disconfirm late. Tweney (1985, 1989, 1991) used his experimental work on confirmation and disconfirmation to frame and enrich a cognitive account of how Michael Faraday used these strategies. Tweney constructed detailed problem–behavior graphs of Faraday's problem-solving processes. Faraday wrote about the dangers of *inertia of the mind,* by which he meant premature attachment to one's own ideas (see Chamberlain, 1890/1965), but he also argued that it is important to ignore disconfirmatory evidence when one is dealing with a new hypothesis (Tweney, 1989). In general, Faraday followed the confirm-early–disconfirm-late heuristic: confirm until you have a well-corroborated hypothesis, then try to disconfirm it. For example, his initial attempts to use magnets to induce an electric current produced apparent disconfirmations, but he ignored them—a single confirmation was more powerful than half-a-dozen disconfirmations, especially given the high possibility of error in his initial experiments. When he obtained a more powerful magnet, he was able to reduce the level of noise and obtain consistent confirmations. Again, Tweney's work illustrates how experimental studies can provide a framework for analyzing naturalistic situations, which in turn, force alterations in the framework.

Dunbar (1995) focused on laboratory meetings rather than the actual conduct of experiments; therefore, it is impossible to assess whether scientists in his study tried to generate positive or negative tests. When confronted with a disconfirmatory result, however, the scientists typically did one of three things: they either changed a corollary assumption of the current hypothesis, attributed an anomalous result to error, or displayed a falsification bias, discarding results that appeared to confirm a hypothesis. Dunbar speculated that this falsification bias was a protection against airing hypotheses that might later be proved wrong, a frequent experience for the senior scientists.

Dunbar also explicitly compared experts and novices. Experts were more willing to modify or discard hypotheses than novices. Part of this willingness came from the fact that group interaction helped scientists articulate alternate hypotheses. In scientific practice, much of the coordination between hypothesis and evidence goes on in groups. Perhaps that explains the apparent difference between Tweney's and Dunbar's results: Tweney studied a detailed record of Faraday's experiments, and Dunbar focused on laboratory meetings. Here the cognitive psychology of science begins to merge with the social psychology of science (see below)—conceptual change occurs in a group setting.

Experts and metacognitive dual-space search. Like Klahr and Dunbar's (1988) most successful participants, Alexander Graham Bell decided early on that he would focus more on the hypothesis space than on the experimental one. He conducted a small number of experiments and reflected constantly on the relationship between evidence and hypothesis. Similarly, Bradshaw (1992) has argued that the success of the Wright Brothers is due to the fact that they did a coordinated search in function and design spaces.

Experts form a complex network of enterprises. Gruber (1981, 1989) originally expected to rely on the work of historians in his cognitive analysis of Darwin's development of evolutionary theory, but he found that his Piagetian background enabled him to see patterns in Darwin's activities that had eluded historians. He noticed that Darwin's apparently disparate activities fit into a network of enterprises including what Klahr and colleagues would call *observational* and *hypothesis spaces* (Klahr & Dunbar, 1988; Klahr, Dunbar, & Fay, 1990). Darwin's observational space included detailed studies of barnacles, worms, and coral and influenced his work in the evolutionary hypothesis space in ways that are worth tracing in detail. Indeed, such a complex cognitive network appears to be common among professional scientists, especially the most creative ones (Gruber, 1981, 1989).

Computational simulation of historical scientific problem solving. Kulkarni and Simon (1988) used Holmes' (1980) detailed study of the discovery of the ornithine cycle to create a computer simulation that followed Krebs' discovery process as closely as possible. *KEKADA,* as the program was called, relied on a dual-space search and a hierarchy of heuristics to accomplish this goal. The hierarchy included general heuristics that could have been used across a wide range of scientific problems and specific ones limited to the domain of organic chemistry. One of the conclusions from KEKADA is that experts possess both general and domain-specific heuristics, whereas novices are more likely to possess only the more general ones. There were even some heuristics possessed only by Krebs and a few others, including a tissue-slicing technique that greatly facilitated the discovery. Obviously, KEKADA could not simulate the kinds of hands-on skills that play such an important role in discovery.

Shrager and Langley (1990), in an excellent volume on computational simulations of scientific discovery, describe

> two important aspects of intellectual activity—embedding and embodiment—that have significant bearing on science but that have not been addressed by existing computational models. Briefly, science takes place in a world that is occupied by the scientist, by the physical system under study, and by other agents, and this world has indefinite richness of physical structure and constraint. Thus the scientist is an embodied agent embedded in a physical and social world. (p. 15)

This criticism applies to BACON, KEKADA, and a variety of other computational approaches, including Paul Thagard's *ECHO,* a connectionist simulation that embodies Thagard's theory that the scientific hypothesis with the most explanatory coherence wins in disputes. ECHO has been applied to the oxygen–phlogiston debate and the controversy surrounding the extinction of the dinosaurs (Thagard, 1988; Thagard & Nowak, 1990). This simulation is directed more toward testing philosophical norms for settling controversies than emulating the psychological processes of participants.

One important aspect of embodiment is visualization, and here computational simulations are making progress. Cheng and Simon showed that it might have been easier for Huygens and Wren to have discovered the law of conservation of momentum using diagrams rather than deriving it from theory or by data-driven processes similar to those used by BACON (Cheng & Simon, 1992). Cheng then created *HUYGENS,* a more general computational simulation of discovery by one-dimensional diagrams. HUYGENS uses a kind of dual-space search: From given numerical data, HUYGENS switches to a space of diagrams in its search for regularities by looking for patterns in the diagrams. When patterns have been found, the regularities are simply transformed back into equations. The change to diagrammatic representation permits different operators, regularity spotters, and heuristics to be used that are more effective than those used in the direct search of a space of algebraic terms (Cheng & Simon, 1995). Cheng admits that we cannot be sure the real Huygens (the discoverer of conservation of momentum) used this method, but it is historically plausible, and HUYGENS demonstrates that it would have been more efficient than alternatives. Instead of claiming he developed a program that discovers, Cheng argued instead that he provided computational evidence for the importance of using diagrams in scientific discovery, evidence that could be combined with material from other sources (e.g., fine-grained case studies of the way diagrams are used in actual discoveries). Cheng's goal appears to be to provide both a normative account—how diagrams should be used to discover—and a historically plausible one—how diagrams probably were used by Huygens.

Gooding and Addis (1993) took another step in the direction of computational simulations of embodiment by developing a programming environment called *CLARITY* that allows them to simulate Faraday's problem-solving processes. Although CLARITY cannot conduct experiments itself, it allows researchers to incorporate fine-grained details of experimental procedures in an effort to reconstruct an inventor or scientist's path. It also allows the researcher to model the way in which information from articles and interaction with others influenced Faraday's thinking. Instead of making discoveries like BACON and KEKADA, or settling controversies like ECHO, "CLARITY diagrams make hypotheses about inference and learning processes accessible; they can be discussed and criticized more readily than computer code and are therefore open to revision and experimentation in ways that most code-based modeling is not" (Gooding & Addis, 1993, p. 8). In other words, CLARITY serves as an expert assistant, helping scholars understand discovery. Such simulations will need psychologists of science to supply detailed data on human processes in similar domains. It is exactly this sort of rich and detailed data that make simulations like KEKADA, BACON, HUYGENS, and CLARITY so powerful.

Experts are cognitively complex. The study of dispositional cognitive styles, such as integrative complexity, provides a link between cognitive and personality psychology of science (see next section). Integrative complexity is a measure of complexity of thinking and is divided into two components: differentiation and integration (Schroder, Driver, & Streufert, 1967; Tetlock & Suedfeld, 1988). The simple thinker makes relatively few qualifications and sees things in black and white terms. In contrast, the complex thinker not only makes distinctions and qualifications, but integrates into a synthetic whole the opposing points of view. Only two studies have been conducted on integrative complexity in scientists. Suedfeld (1985) reported that the APA presidents not only had the highest complexity means compared to all nonscientist samples, but that the most eminent psychologists gave the most complex presidential addresses. Feist (1994) interviewed a group of eminent scientists and, among other things, had them respond to a set of semistructured questions, which were transcribed and coded on integrative complexity. The mean levels of complexity in these physicists, chemists, and biologists were even higher than those in the Suedfeld study. These eminent scientists were complex thinkers about their research but not about other issues (such as science education).

Conclusions From the Cognitive Psychology of Science

To summarize the consensual findings from the cognitive psychology of science (see Table 2): (a) confirm early, disconfirm late is an effective and successful heuristic in hypothesis testing; (b) searching for solutions in two spaces simultaneously leads to more successful hypothesis testing than searching in one space; (c) creating analogies and using metacognitive skills facilitates successful problem solving; (d) introducing error increases hypothesis perseveration; (e) computer simulations can shed insight on the heuristics involved in scientific discovery; (f) complexity of scientific thought is associated with scientific eminence.

Personality Psychology of Science

As a way of organizing the literature on the personality psychology of science, we categorize it around four fundamental topics, namely consistent personality differences between scientists and nonscientists, consistent personality differences between eminent and less eminent scientists, whether scientists of different theoretical persuasions differ in terms of personality, and finally the directional influence of personality on scientific behavior (see Table 3).

Comparing Personality Characteristics of Scientists to Nonscientists

In 1874 Francis Galton published the first scientific investigation of the psychological characteristics of scientists. Galton collected qualitative self-report data from 180 English men of science and found that they were energetic, were physically healthy, were persevering, had good memories, and were very independent. The study of genius was furthered by J. McKeen Cattell (1910), Terman (1925), and Cox (1926). Under the guidance of Terman and using J. Cattell's eminent sample as participants, Cox carried out the most ambitious, systematic, and most quantitative of the early investigations into genius. Although she did not focus exclusively on scientists, she did report findings broken down by group. Cox found the traits that most clearly distinguished scientists from nonscientific eminent men were the desire to excel, originality, reason, tendency not to be changeable, determination, and neatness and accuracy of work. Since the 1950s, however, more systematic work has focused on personality and scientists, with *scientists* being defined as any sample that consisted of either students of science, engineers, inventors, social scientists, biological scientists, or natural scientists. This body of literature can be summarized with the conclusions discussed in the following sections.

Scientists are more conscientious. The empirical research has revealed a consistent pattern of greater conscientiousness among scientists compared to nonscientists (Albert & Runco, 1995; Bachtold, 1976; Barton & H. Cattell, 1972; Feist & Barron, 1995; Gough, 1987; Ham & Shaughnessy, 1992; Kline & Lapham, 1992; Schaefer, 1969; Udell, Baker, & Albaum, 1976; Wilson & Jackson, 1994). For example, Kline and Lapham (1992) used the Eysenck Personality Questionnaire (EPQ) to measure the personality characteristics of 326 science majors and compared them to 357 art majors. They reported a difference on conscientiousness between the two groups that translates into an effect-size d (i.e., the difference between the two means divided by the average standard deviation; see Cohen, 1988) of 1.59. Given the highly structured and organized nature of scientific investigation, it is not surprising that scientists would have dispositions towards orderliness and conscientiousness.

Scientists are more dominant, achievement oriented, and driven. Scientists are also more ambitious, driven, and dominant in personality than nonscientists (Albert & Runco, 1995; Arvey & Dewhirst, 1976; Bachtold & Werner, 1972; R. Cattell & Drevdahl, 1955; Feist & Barron, 1995; Gough, 1987; Ham & Shaughnessy, 1992; Pearce, 1968; Schaefer, 1969; Udell, Baker, & Albaum, 1976). Dominance and drive appear to be distinguishing characteristics of both female and male scientists. For example, Bachtold and Werner (1972) collected personality data on 116 female biologists and chemists listed in *Who's Who in America* and *Who's Who of American Women* and compared them to female norms. Using Cattell's Sixteen Personality Factor (16PF) as the measure of personality, they found that the women scientists were more dominant, confident, intelligent, radical, and adventurous than women in general. Furthermore, the personality profile of female scientists was quite consistent with that of male scientists (Bachtold & Werner, 1972). Two of the three studies that found scientists to be less dominant than a comparison group was on samples of female scientists being compared to female artists (Bachtold, 1976; Barton & H. Cattell, 1972). The other negative finding on dominance was on a student sample (Scott & Sedlacek, 1975).

Scientists are more independent, introverted, and less sociable. Scientists, relative to nonscientists, do prefer to be alone and are somewhat less social (Arvey & Dewhirst, 1976; Bachtold, 1976; Bachtold & Werner, 1976; Butcher, 1969; R. Cattell & Drevdahl, 1955; Eiduson, 1962; Feist, 1987; Feist & Barron, 1995; Pearce, 1968; Roe, 1952a; Scott & Sedlacek, 1975; Wilson & Jackson, 1994). The recent paper by Wilson and Jackson (1994) is representative. They administered the EPQ to 109 male and 133 female physicists and compared their scores to the population norms. Among the many differences, male physicists were almost one standard deviation ($d = -.94$) and female physicists were approximately three fourths of a standard deviation ($d = -.73$) lower on the Sociable–Unsociable dimension. The only contrary finding was reported by Mohan and Kaur (1993), who reported a mean on Extroversion for a sample of scientists that was higher than the normative mean. However, the Mohan and Kaur sample was from India and therefore cultural differences may be responsible for the negative relationship.

Scientists are emotionally stable and impulse controlled. Compared to nonscientists, scientists tend to be relatively emotionally stable, low on neuroticism, and more likely to control their impulses (Albert & Runco, 1995; Bachtold, 1976; Bamber, Bill, Boyd, & Corbett, 1983; Barton & H. Cattell, 1972; Butcher, 1969; R. Cattell & Drevdahl, 1955; Eiduson, 1962; Feist & Barron, 1995; Gough, 1987; Ham & Shaughnessy, 1992; Mossholder, Dewhirst, & Arvey, 1981; Scott & Sedlacek, 1975; Terman, 1954; Wilson & Jackson, 1994). For example, Cattell and Drevdahl (1955) gave the 16PF to scientists who were classified as primarily researchers, teachers, or administrators. Taking the means on only the researcher subsample ($n = 144$) and comparing them to norms, researchers were more than half a standard deviation higher on impulse control ($d = .54$) and more than two thirds of a standard deviation higher on ego strength ($d = .70$).

Table 3
Summary of Topics and Findings in the Personality Psychology of Science Literature

Topic	Results	Author(s)
Scientists vs. nonscientists	Scientists are more conscientious or orderly	Schaefer, 1969 Barton & Cattell, 1972 Bachtold, 1976 Gough, 1987 Ham & Shaughnessy, 1992 Kline & Lapham, 1992 Wilson & Jackson, 1994 Albert & Runco, 1995 Feist & Barron, 1995
	Scientists are more dominant, driven, or achievement oriented	R. Cattell & Drevdahl, 1955 Pearce, 1968 Schaefer, 1969 Bachtold & Werner, 1972 Scott & Sedlacek, 1975 Arvey & Dewhirst, 1976 Udell, Baker, & Albaum, 1976 Gough, 1987 Ham & Shaughnessy, 1992 Albert & Runco, 1995 Feist & Barron, 1995
	Scientists are more independent and less sociable	Roe, 1952a Cattell & Drevdahl, 1955 Terman, 1955 Eiduson, 1962 Pearce, 1968 Butcher, 1969 Bachtold & Werner, 1972 Scott & Sedlacek, 1975 Arvey & Dewhirst, 1976 Bachtold, 1976 Feist, 1987 Wilson & Jackson, 1994 Feist & Barron, 1995
	Scientists are more emotionally stable or impulse controlled	Roe, 1952a Cattell & Drevdahl, 1955 Eiduson, 1962 Butcher, 1969 Barton & Cattell, 1972 Scott & Sedlacek, 1975 Bachtold, 1976 Mossholder, Dewhirst, & Arvey, 1981 Bamber, Bill, Boyd, & Corbett, 1983 Albert & Runco, 1987 Gough, 1987 Ham & Shaughnessy, 1992 Wilson & Jackson, 1994 Feist & Barron, 1995
Eminent/creative vs. less eminent/creative	Creative scientists are more dominant, arrogant, self-confident, or hostile	Van Zelst & Kerr, 1954 Gough, 1961 Wispe, 1963 Chambers, 1964 Garwood, 1964 Parloff & Datta, 1965 McDermid, 1965 Davids, 1968 Parloff et al., 1968 Shapiro, 1968 Schaefer, 1969 Erickson et al., 1970 Helson & Crutchfield, 1970

Table 3 *(continued)*

Topic	Results	Author(s)
		Gantz et al., 1972
		Lacey & Erickson, 1974
		Rushton, Murray, & Paunonen, 1987
		Helmreich, Spence, & Pred, 1988
		Feist, 1993
	Creative scientists are more autonomous, independent, or introverted	Van Zelst & Kerr, 1954
		Holland, 1961
		Chambers, 1964
		Garwood, 1964
		Parloff & Datta, 1965
		Davids, 1968
		Schaefer, 1969
		Erickson et al., 1970
		Helson & Crutchfield, 1970
		Smithers & Batcock, 1970
		Helson, 1971
		Lacey & Erickson, 1974
		Busse & Mansfield, 1984
		Rushton, Murray, & Paunonen, 1987
		Feist, 1993
		Roco, 1993
		Feist & Barron, 1995
	Creative scientists are more driven, ambitious, or achievement oriented	Van Zelst & Kerr, 1954
		Gough, 1961
		Holland, 1961
		Wispe, 1963
		Chambers, 1964
		Davids, 1968
		Shapiro, 1968
		Schaefer, 1969
		Erickson et al., 1970
		Gantz et al., 1972
		Lacey & Erickson, 1974
		Busse & Mansfield, 1984
		Ikapaahindi, 1987
		Rushton, Murray, & Paunonen, 1987
		Helmreich, Spence, & Pred, 1988
	Creative scientists are more open and flexible in thought or behavior	Van Zelst & Kerr, 1954
		Gough, 1961
		Wispe, 1963
		Garwood, 1964
		Parloff et al., 1968
		Shapiro, 1968
		Schaefer, 1969
		Helson & Crutchfield, 1970
		Helson, 1971
		Rushton et al., 1987
		Roco, 1993
		Feist & Barron, 1995
Theoretical predilection	Personality influences theory creation, acceptance, and orientation	Atwood & Tomkins, 1976
		Johnson et al., 1988
		Hart, 1982
		Royalty & Magoon, 1985
Directional influence	Directional influence between personality and scientific behavior is uncertain	Eiduson, 1974
		Feist, 1993
		Feist & Barron, 1995

Comparing Personality Characteristics of Eminent and Creative to Less Eminent and Creative Scientists

In addition to the general research on the distinctive personality characteristics of scientists, another body of work has focused more specifically on the unique personality characteristics of the most successful, eminent, and creative scientists.

Eminent–creative scientists are more dominant, arrogant, hostile, and self-confident. In the highly competitive world of science, especially "big" science, where the most productive and influential continue to be rewarded with more and more of the resources, success is more likely for those who thrive in competitive environments: the dominant, arrogant, hostile, and self-confident (Chambers, 1964; Davids, 1968; Erickson, Gantz, & Stephenson, 1970; Feist, 1993; Gantz, Erickson, & Stephenson, 1972; Garwood, 1964; Gough, 1961; Helmreich, Spence, and Pred, 1988; Helson & Crutchfield, 1970; Lacey & Erickson, 1974; McDermid, 1965; Parloff & Datta, 1965; Parloff, Datta, Kleman, & Handlon, 1968; Rushton, Murray, & Paunonen, 1987; Schaefer, 1969; Shapiro, 1968; Van Zelst & Kerr, 1954; Wispe, 1963). For example, Van Zelst and Kerr (1954) collected personality self-descriptions from 514 technical and scientific personnel from a research foundation and a university. Holding age constant, they reported significant partial correlations between productivity and the self-description of *argumentative* ($r_p = .23$), *assertive* ($r_p = .22$), and *self-confident* ($r_p = .35$). Similarly, Feist (1993) presented a structural-equations model of scientific eminence in which the path between observer-related hostility and eminence was direct and the path between arrogant working style and eminence was indirect (mediated by productivity) but significant.

Eminent–creative scientists are more driven, ambitious, and achievement oriented. Related to their hostility, arrogance, dominance, and self-confidence, the most eminent and creative scientists also tend to be more driven, ambitious, and achievement oriented than their less eminent peers (Busse & Mansfield, 1984; Chambers, 1964; Davids, 1968; Erickson et al., 1970; Gantz et al., 1972; Gough, 1961; Helmreich, Spence, & Pred, 1988; Holland, 1961; Ikpaahindi, 1987; Lacey & Erickson, 1974; Rushton et al., 1987; Schaefer, 1969; Shapiro, 1968; Van Zelst & Kerr, 1954; Wispe, 1963). Busse and Mansfield (1984), for instance, studied the personality characteristics of 196 biologists, 201 chemists, and 171 physicists. Holding age and professional age constant, commitment to work (i.e., "need to concentrate intensively over long periods of time on one's work") was the strongest predictor of productivity (i.e., publication quantity). Of course, drive and ambition are predictive of success in other fields also, but their effect in science are nevertheless important to demonstrate.

Eminent–creative scientists are more autonomous, introverted, and independent. If scientists in general are more aloof, asocial, and introverted than nonscientists, then these characteristics appear to be even more salient for the scientific elite (Busse & Mansfield, 1984; Chambers, 1964; Davids, 1968; Erickson et al., 1970; Garwood, 1964; Helson, 1971; Helson & Crutchfield, 1970; Holland, 1961; Lacey & Erickson, 1974; Parloff & Datta, 1965; Roco, 1993; Rushton, Murray, & Paunonen, 1987; Schaefer, 1969; Smithers & Batcock, 1970; Van Zelst & Kerr, 1954). Chambers (1964) used the 16PF to obtain self-reported personality data from 225 chemists and 213 psychologists. Based on awards and honors, some of these scientists were classified in the creative group, whereas the rest were classified in the less creative group. The creative scientists were .39 of a standard deviation higher than the less creative scientists on self-sufficiency. Furthermore, Helson (1971) compared creative female mathematicians matched on IQ with less creative female mathematicians. Observers blindly rated the former as having more "unconventional thought processes," as being more "rebellious and nonconforming," and as being less likely to judge "self and others in conventional terms" than the latter.

Eminent–creative scientists are more open to experience or flexible in thought and behavior. A final consistent effect of personality on creativity in science is the finding that creative and eminent scientists tend to be more open to experience and more flexible in thought than less creative and eminent scientists (Feist & Barron, 1995; Garwood, 1964; Gough, 1961;

Helson, 1971; Helson & Crutchfield, 1970; Parloff & Datta, 1965; Parloff et al., 1968; Roco, 1993; Schaefer, 1969; Shapiro, 1968; Van Zelst & Kerr, 1954; Wispe, 1963). Many of these findings stem from data on the Flexibility scale (Fe) of the California Psychological Inventory (Feist & Barron, 1995; Garwood, 1964; Gough, 1961; Helson, 1971; Helson & Crutchfield, 1970; Parloff & Datta, 1965). The Fe scale taps into flexibility and adaptability of thought and behavior as well as the preference for change and novelty (Gough, 1987). The few studies that have reported either no effect or a negative effect of flexibility in scientific creativity have been with student samples (Davids, 1968; Smithers & Batcock, 1970).

Personality and Theoretical Predilection

Earlier we reviewed the literature on age and theory acceptance and showed that there is little evidence for Planck's principle (new theories are accepted only once old scientists die and younger ones take control). The work on personality can also shed light on theory acceptance and even theory creation. Or stated as a question, do certain personality styles predispose a scientist to create, accept, and/or reject certain theories? The first work on this question was done by Atwood and Tomkins (1976), who showed through case studies how the personality of the theorist influences his or her theory of personality. More systematic empirical investigations have expanded this work and have demonstrated that personality influences not only theories of personality, but also the theoretical orientation of behavioral scientists and how quantitatively or qualitatively oriented they are (Conway, 1988; Hart, 1982; Johnson, Germer, Efran, & Overton, 1988; Royalty & Magoon, 1985; Zachar & Leong, 1992). However, all of these studies have been with psychologists, so answering the question of whether these results generalize to the biological and natural sciences remains a task for future psychologists of science.

Directional Influence Between Personality and Scientific Behavior

The most pressing question that begs to be addressed from the personality findings is whether these traits are causes or effects of scientific behavior. To put it most simply, do smart, conscientious, introverted, driven, and controlled people become scientists or does science create smart, conscientious, introverted, driven, and controlled people? Out of logical necessity, it would seem very unlikely that any of these characteristics would be nonexistent until one became a scientist, and therefore unlikely that being a scientist actually caused these traits of personality. However, some of them may in fact become more pronounced after being trained as a scientist and after practicing science. As is often the case, however, the model that may best fit the relationship between personality and scientific behavior is probably bidirectional, going from personality to scientific behavior and from scientific behavior to personality. Because one cannot perform experimental designs on either occupational interest or personality, the best methodology one can use to address issues of causality is longitudinal design. However, although longitudinal data are able to address the first two criteria of causality, namely covariation and temporal precedence, they still do not easily address the third and most difficult criterion: ruling out extraneous variable explanations (Rosenthal & Rosnow, 1991).

Of the dozen or so studies that have examined scientific behavior longitudinally (Arvey, Dewhirst, & Brown, 1976; S. Cole, 1979; Diamond, 1986; Eiduson, 1974; Feist & Barron, 1995; Hinrichs, 1972; Horner et al., 1986; Roe, 1965; Root-Bernstein, Bernstein, & Garnier, 1995; Simonton, 1991, 1992a; Subotnik et al., 1993; Terman, 1954), most have focused on questions of age and productivity and only two have looked at personality across time (Eiduson, 1974; Feist & Barron, 1995). Initial results examining the directionality question from the Feist and Barron study show that certain personality traits, such as dominance, may become more pronounced during and after a career in science (Feist & Barron, 1995), suggesting a directional influence from career to personality. However, before one can confirm such an inference, one must rule out alternative variable explanations. For instance, perhaps age and maturation, not scientific careers, leads to this difference in dominance. Lack of research on longitudinal personality change and stability is one of the real shortcomings of the personality psychology of science literature.

Conclusions From the Personality Psychology of Science

In sum, the empirical literature over the last 40 years has revealed rather consistent portraits of the scientific personality, both in comparison with nonscientists and to less creative scientists (see Table 3). Furthermore, personality characteristics appear to be related to which domain of science one is attracted to (i.e., physical vs. biological vs. social science). Results have converged on a description of scientists as more conscientious, driven, introverted, stable, and controlled compared with nonscientists. Moreover, the empirical literature also suggests that creative scientists are more dominant, arrogant, hostile, driven, introverted, and open and flexible than less creative scientists. In addition, personality dispositions appear to influence the kinds of theories behavioral scientists are likely to create or accept. Finally, suggestive work on the extent to which personality is a cause or an effect of scientific behavior needs to be supplemented with more systematic research.

Social Psychology of Science

Science is unquestionably a cognitive activity, and the social–cognitive and attributional perspectives, with their emphasis on cognitive heuristics, biases, and causal explanations, can complement the work we cited earlier on cognitive psychology of science. Science also is unquestionably a highly social activity, with much of the work being done cooperatively or competitively with other research teams. Addressing the social factors involved in science, the field of social psychology of science finds itself in an unusual situation. It is potentially one of the richest and most stimulating areas in the psychology of science, but as yet remains more latent than actual. One can easily apply all of the major social psychological phenomena—social cognition, attribution theory, attitude and attitude change, conformity and social influence and persuasion, and intergroup relations—to the study of science and scientists. However, as yet, much of this work has not been conducted. The province of *social psychology* can be defined as "an attempt to understand and explain how the thought, feeling and behavior of individuals are influenced by the actual, imagined or implied presence of others" (Allport, 1985, p. 3). As Shadish et al. (1994) noted, substituting *individuals* with *scientists* in Allport's quotation creates a good working definition of the social psychology of science. Social psychology of science may not be as well-developed as the developmental, cognitive, or personality psychologies of science, but the recent book *The Social Psychology of Science* (Shadish & Fuller, 1994) suggests the field is on the verge of blossoming. Here, some of the main figures in social psychology have begun to produce work that is directly relevant to the social psychology of science, and are starting to make the connection quite explicitly. In what follows we review some of the main contributions to the social psychology of science literature. Because of this dichotomy between actual and potential, our review of social psychology of science is divided into extant and potential–proposed topics of investigation.

Extant Social Psychology of Science Literature

Rosenthal's (1976, 1994) work on experimenter, observer, interpreter, and expectancy effects is without a doubt one of the more persuasive and powerful bodies of literature relevant to the psychology of science in general and a social psychology of science in particular. For example, research on experimenter effects has demonstrated that participants' responses can be influenced by the personality, attractiveness, attire, or gender of the experimenter (Barnes & Rosenthal, 1985). In addition, *observer effects* occur when systematic error exists in observations of raw data, whereas *interpreter effects* exist when there is systematic error in the interpretation of data. Observer and interpreter effects may be quite perceptual and cognitive in nature, but they also have very clear social ramifications. Finally, expectancy effects are concerned with "how the investigator's expectation can come to serve as a self-fulfilling prophecy" (Rosenthal & Rosnow, 1991, p. 129). That a researcher's prior expectations can affect the observations, final results, and interpretations of research has been demonstrated not only when the participants are humans (Rosenthal & Fode, 1963a; Rosenthal & Jacobson, 1992;

Stanton & Baker, 1942), but also when they are animals (Cordaro & Ison, 1963; Rosenthal & Fode, 1963b). The history of the physical, biological, and behavioral sciences is replete with examples of observational and interpretational disagreements about data. More than once, junior researchers have lost jobs or were delegated to obscure jobs for disagreements with senior colleagues over observations. In fact, often these differences are cast off as simply being a result of error on the junior researcher's part. Granted, this may be the case in some instances, but there are many known instances for which history has shown these simply to be honest differences in observation (Rosenthal, 1976).

One contribution the social psychology of science has begun to make is to shed light on these experimenter and observer effects, which will increase the understanding of the social–cognitive processes involved in the creation and development of scientific knowledge. In Kruglanski's (1994) words,

> cognitive and motivational biases that influence scientific conclusions are fundamentally inevitable and are an integral part of how all knowledge is acquired. Rather than regarding them as impediments to truth, it may be more practical to take them into account to improve the quality and persuasiveness of one's research. (p. 211)

It would not be an exaggeration to say that the whole field of experimenter effects could be categorized as a subdiscipline under social and cognitive psychology of science. Indeed, this body of work provides a prototypic example of how social psychology has much to offer science studies and implicitly has been doing so for years.

Another key figure in the social psychology of science is Dean Simonton, whose work has more explicitly explored how social structures influence the creation and maintenance of science. Theoretically, Simonton's chance-configuration model (1988a, 1989) provides an explanation for how an individual scientist's conceptual configurations and insights develop, are articulated, are communicated, are accepted or rejected, become influential, and potentially develop into a school of similar-thinking individuals; how those who produce the most ideas are most likely to wield wide-ranging influence by their high-quality work; and how individual differences and social factors contribute to the essential tension between traditional knowledge and revolutionary, not yet accepted knowledge (see T. Kuhn, 1970). Empirically, Simonton has shown through analysis of historical and archival data how mentors and role models (see also Subotnik et al., 1993; Subotnik & Steiner, 1992) war, and political upheaval or stability influence creative output in science. Using cross-lagged panel designs, Simonton (1975, 1976a, 1976b, 1980) has examined the causal influence of war on scientific productivity. For instance, in examining the influence of war on productivity in seven European countries from 1500 to 1900, Simonton reported that war had a significant influence on productivity rather than the other way around, but the influences were complex and inconsistent across country (1976a). Finally, Simonton (1988a) has reported that often the most creative contributions come from those who know two different cultures, suggesting that exposure to multiple cultural frames of reference is important for creative productivity in science.

Shadish (1989) has written about the importance of a psychological perspective in the evaluation of quality in science; quality evaluations are at the heart of the scientific enterprise. Such evaluations and their criteria and measurement are what determines who gets which job, who gets tenure, who gets which grants, and who gets which awards and honors (Feist, 1997). Science is a competitive enterprise and resources (read, reward and recognition) are scarce. Of course, the question of quality in science immediately raises a few other critical—and social psychological—questions: Whose perceptions should be used to evaluate quality? What criteria are used? How is it decided how to weigh the various criteria? Are these criteria and evaluations fair or biased against particular individuals or groups of individuals? Until recently, philosophy, history, and sociology may have been the disciplines most likely to address these questions, but as Shadish wrote

> Why should we think that psychology offers an important perspective on our understanding of science quality? The reason is this: The perception of quality in science probably exercises an inordinate amount of influence in scientific reward systems, and perception is largely a psychological variable. (p. 407)

Social negotiations and self-presentation tactics involved with promoting one's own career clearly play a role in influencing the perceptions of the "powers that be." Few would deny this. The real question then becomes how much of a role does self-presentation play in career success? The cynic may say a major role, whereas the more naive may say no role. Rather than leave the question to one's predilection towards cynicism or gullibility, we argue that the question is fundamentally an empirical one and therefore should be examined empirically.

One of the so-called objective measures that Shadish argues is important in quality evaluation is citation analysis; the importance of a particular scientist's opus easily and fairly reliably can be measured by counting the number of times her or his works are cited by peers. Most frequently used by sociologists, citation analysis seldom has examined the cognitive and psychological reasons authors have for citing any particular paper. Shadish, Tolliver, Gray, and Gupta (1995), however, have used surveys to address the citation-analysis issue for a large sample of psychologists. They found that oft-cited works were considered exemplars, seen as higher in quality, published longer ago, and often used as sources of methods or designs. Interestingly and unexpectedly, frequently cited articles were also perceived to be less creative. It is not clear, however, why psychologists believe highly cited papers are high in quality but low in creativity, especially because Sternberg and Gordeeva (1996) reported that papers were highly cited because they were novel. The latter also found that importance of theoretical contribution and whether papers generated research were rated as the most important reasons psychologists gave for citing a paper. Clearly, additional research is needed in understanding the explicit and implicit reasons that scientists cite works.

There is a long and distinguished literature on group processes in social psychology, and yet only recently has any of it focused on variables and tasks involved in science. For example, work on small-group processes in science has made use of experimental methods and provided some insight into differences between individuals and groups working on scientific problems (Gorman, 1986; Gorman, Gorman, Latta, & Cunningham, 1984). Gorman and his colleagues found that *interacting groups* (i.e., those whose members interacted directly) on a scientific-reasoning task performed no better than the best individual in a *coacting group* (i.e., those whose members work separately but were informed of other members' hypotheses), and disconfirmatory instructions were superior to confirmatory except when there is possibility of error in the data. These findings were replicated with individual participants (Gorman, 1989; Gorman & Gorman, 1984), suggesting that groups perform about as well as the best of an equal number of individuals on these scientific reasoning tasks. Steiner (1972) called such tasks *eureka problems,* and proposed that groups would only perform better than the best of an equal number of individuals on divisible problems. Modern research teams succeed in part because they divide labor effectively among participants with different skills and resources. For instance, Gholson and Houts (1989) reported that coacting groups were more prone to confirmation bias than interacting groups. More experimental work needs to be done comparing group and individual performance on divisible tasks that simulate aspects of scientific reasoning.

Potential Social Psychology of Science Literature

Possible historical case studies. Because relatively little actual empirical work has been carried out on the social psychology of science, a few researchers have outlined how various methods could be applied in investigating social elements in science. Shadish et al. (1994), for example, outlined a simulated experimental paradigm that would allow one to investigate issues raised by the case study of the Devonian controversy in geology and controversies over the existence of canals on Mars (Gorman, 1992). In this case, the discovery of the Devonian period in geological history was not the product of a single individual; rather, it emerged out of a mix of cooperative and competitive interactions among a group of geologists. Attribution theory (Kelley, 1967) could easily be applied to help explain how and why one particular "discoverer" managed to get most of the credit. In this particular case, the external attribution comes from the consensual perspective of the community of geologists awarding Murchison the label *discoverer,* the end result of a process of social negotiations. Social psychology of science can help unpack these negotiations.

In addition, the Murchison case study can provide insight into the role of minority influence on majority opinion in science. The literature on social influence suggests that a unanimous majority can cause a minority of one to conform to an erroneous position on an unambiguous perceptual task (Asch, 1956). However, a consistent, determined minority also can influence the judgments of a majority in an ambiguous perceptual task (Moscovici & Nemeth, 1974). Moscovici takes the view that minority influence forces the majority to look more closely at the stimuli that are the focus of argument. For example, in the beginning, Murchison's was a novel, minority view, but there was no consistent majority to oppose it. But Murchison was a persuasive scientist and his consistent, determined arguments fueled a close study of those aspects of the data that he thought were particularly important. Gradually, Murchison's position became the majority view.

Possible experimental paradigms. How can these minority influence processes be studied experimentally? One could study the circumstances under which a minority can force a majority to look more carefully at the data on a scientific-simulation task such as the artificial universe used by Mynatt et al. (1978), in which participants shot particles at shapes on a screen in order to discover laws governing their motion. Such experiments could be conducted by (a) manipulating task ambiguity by introducing different levels of error; (b) using a confederate to play the role of minority member and varying the style of argument that she or he uses (Rosenwein, 1994); (c) manipulating the credibility of the minority-group members, perhaps by presenting them as having had previous success with a similar task; and (d) looking at minority influence across generations (cf. Jacobs & Campbell, 1961), in which members of an original group are replaced one-by-one and each new member can consider the minority's arguments anew. In addition, Gorman and Rosenwein (1995) have proposed a possible quasiexperiment in which groups of individual participants try to solve problems that mimic scientific reasoning in a multifaceted environment that simulates the social negotiations found in scientific communities.

Conclusions From a Social Psychology of Science

Social psychology has recently begun to actualize its vast potential to shed light on critical issues confronting science studies. Minority influence, attitudes and attitude change, persuasion, and small-group processes have begun to build up at least a small, albeit it somewhat disconnected literature. Attributional processes, decision making, and conformity each have nearly nonexistent literatures in science studies, and therefore have the most potential. Not only does social psychology of science have important methodological contributions to make, but also may make fundamental theoretical and substantive contributions. Only social psychology of science combines an emphasis on the individual with a social context, and this places social psychology of science in a very strong position to shed light on how individual scientists influence and are influenced by the complex social network in which they work.

General Conclusions and Discussion

The psychology of science is still in its infancy. There is still much that is not known about the development, thought processes, personalities, motives, and social factors involved in scientific behavior. However, there are clear signs that the field is solidifying and developing an autonomous sense of identity. Much of this growth in fact has been stimulated by confrontations with critics from philosophy, sociology, history, and even within psychology.

Integrating What Is Known: A Theoretical Structural-Equation Model

The overall findings from each of the four psychology of science subdisciplines (summaries of which are found at the end of each section) can be integrated into a theoretical structural model (see Figure 1). This model is a combination and generalization of the path analyses of Helmreich et al. (1980) and Mansfield and Busse (1981) and the structural models proposed by Feist (1993), Reynolds and Walberg (1992), and Simonton (1977b). The content of each of the latent and measured

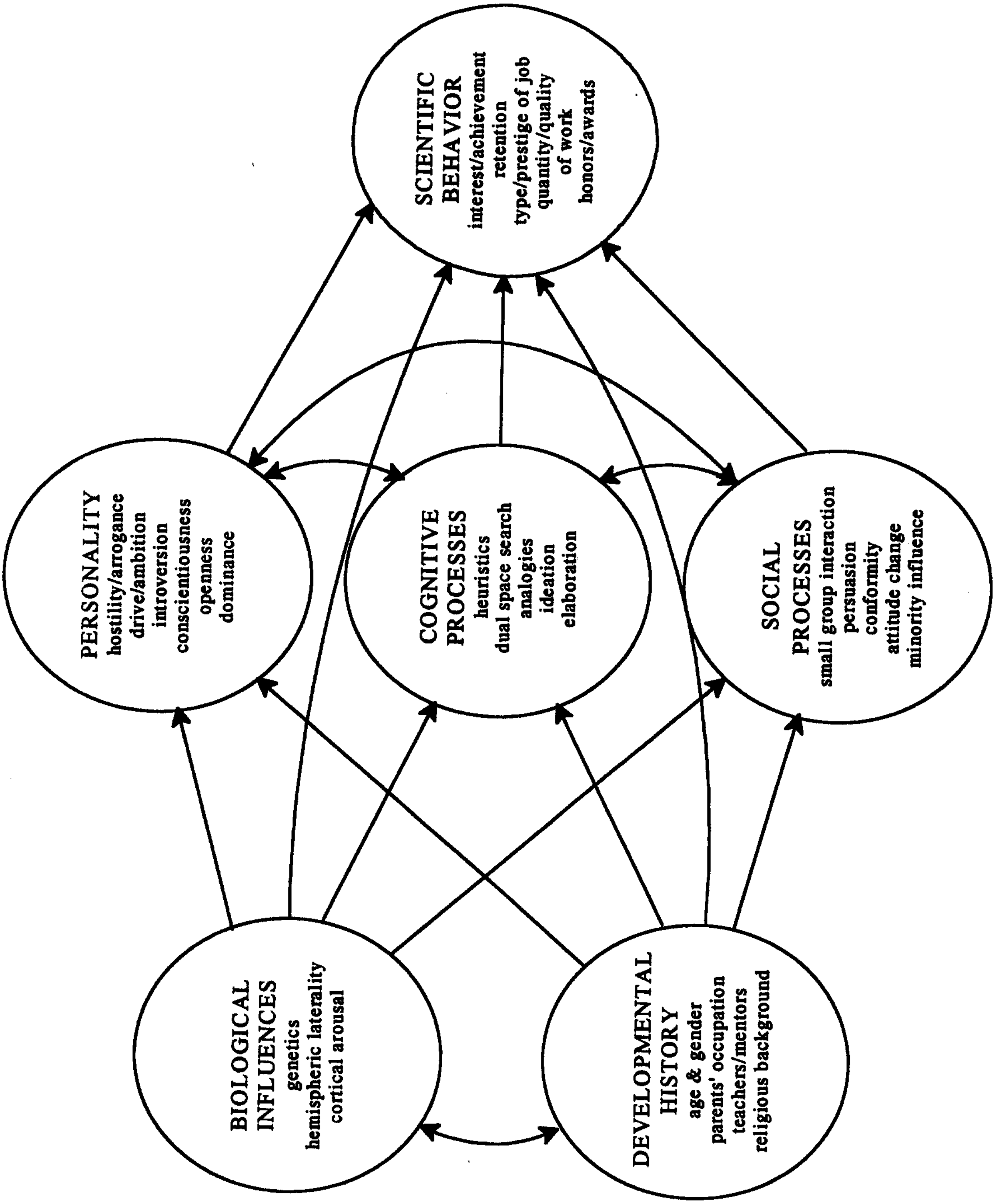

Figure 1. Theoretical structural equation model: Integration of findings in the psychology of science

variables[1] and direction of their structural paths are based on actual empirical findings, with the exception of the social influences latent variable. The logic of the model is based on temporal precedence and purported causal influence. The first structures that develop are the biological and developmental characteristics, which, we propose, have direct influences on the personality, cognitive, and social processes. Biological and developmental effects are also hypothesized to have significant indirect influences on scientific behavior. Personality and cognitive and social processes, in turn, are hypothesized to be the most direct influences on scientific behavior (i.e., the development of an interest in science, school achievement and retention, obtaining a job in science, producing works of varying quality, and receiving honors and awards for one's achievements). One may argue that the model is so general that most any behavior could be placed in the outcome. Indeed, that is precisely to point: scientific behavior is like any other set of complex, integrated behaviors and can be examined from each of the psychological subdisciplines. We call this a theoretical structural model because it has yet to be tested directly, even though it is based on empirical findings from several different studies. We consider this model to be tentative and open to modification, and indeed, we hope that it will stimulate and inspire researchers in the field to test it directly.

What Is Not Known and Prescriptions for the Future

The recent past has made clear what is known about the psychology of science, but an equally critical question, and even more important for the future, is what is not known (Houts, 1989). Much more work is needed on the biological and physiological predictors of scientific aptitude and talent, as well as on the motivational changes with age. Gender differences in math performance need further exploration and empirical scrutiny. The development of scientific thinking needs to be examined longitudinally rather than cross-sectionally, and the role that parents, teachers, and religious orientation play in the development and maintenance of an interest in science must receive more attention. Furthermore, psychologists can provide more knowledge about what happens when scientists tackle unfamiliar problems, especially those that arise on the border of disciplines. In more general terms, very little is known about how scientists solve actual laboratory problems (see Dunbar, 1995).

Convergence in describing the scientific personality is clearly an important step forward, but a critical and difficult issue remains: how to develop a cogent theoretical explanation for the dynamic relationship between the personalities of scientists and their science. Understanding that differences between scientists and nonscientists and between creative and less creative scientists exist reveals very little about why those differences exist or how they developed. The next phase of personality research on scientists must focus on developmental and directional issues. Such questions can only be addressed if longitudinal research is conducted that begins in childhood and continues up through retirement.

Another relatively ignored domain of psychology of science is the motivation to do science. Why do people become scientists and why do they continue to do science? The small area of research that has addressed motivation in scientists was mainly conducted in the 1950s and 1960s (Chambers, 1964; Eiduson, 1962; Maddi, 1965; McClelland, 1962; Roe, 1952a). McClelland (1962), for example, reported that creative physical scientists are unusually hard working, even obsessed; they avoid, and are disturbed by complex human emotions, especially aggression; and they develop early in life a strong interest in the analysis of the structure of things. Although only few have done research on motivation, one of the common and consistent findings is that the most creative and eminent scientists are the most persistent, hard working, and driven (Amabile, 1996; Chambers, 1964; Eiduson, 1962; Maddi, 1965; Mansfield & Busse, 1981; McClelland, 1962; Roe, 1952a; Simon, 1974; Zuckerman, 1977). In addition, some have explored intrinsic and extrinsic motivation and found that in general the most creative scientists tend to be driven more by internal than external reward (Amabile, 1996; Feist, 1993).

[1] For ease of presentation, we present the measured variables inside the latent variables, rather than as rectangles outside the latent variables.

Importance of Psychology of Science for Other Metasciences

Because of the recent increase in activity in the psychology of science, there are now signs that the more mature metascientific disciplines are taking notice of the psychology of science and even wanting to cooperate on problems rather than hold fast to their *disciplinocentrism* (i.e., the belief in the superiority of one's own discipline and the uselessness of others; Feist, 1995). For example, the epistemological question of what scientific knowledge is stands to gain much from the recent developments in cognitive science and artificial intelligence (Gholson & Houts, 1989; Gorman, 1992; Miller, 1989; Simon, Langley, & Bradshaw, 1981; Tweney, 1989; Tweney et al., 1981). Indeed, an increasing number of contemporary philosophers are developing the discipline of natural epistemology (Fuller, 1988, 1993; Giere, 1988; Gooding, 1990; Heyes, 1989; Thagard, 1988), and they directly acknowledge the value of psychology in addressing metascientific questions.

But psychology of science is still a relatively unrecognized specialty, unlike history, philosophy, and sociology of science. It is often ignored in debates about the nature of science. For example, a recent book written by a scientist and a mathematician (Gross & Levitt, 1994) applauded those metascience disciplines—primarily history and philosophy of science—that reinforce the view that science is a rational enterprise. They attacked sociology of science, feminist science studies, and other metascientific movements that question or undermine this view. Psychology of science was not mentioned at all, nor has it played any role in the ensuing controversy, though psychology of science could contribute much to this debate. Psychology of science could transcend and illuminate these debates by contributing empirical research that seeks neither to undermine nor sanctify science. Studies on how science relates to other human activities (i.e., how the thinking of expert scientists differs from novices, and what sorts of personalities and which kinds of group interaction are most likely to lead to success in science), are most likely to advance theory.

Future Prospects for a Psychology of Science

So how can psychology of science acquire a louder voice, both within and outside of psychology? It must follow the route of the other metascientific disciplines, which have formed their own organizations and journals. If any discipline is to establish itself as a legitimate, viable, and healthy field, it must ultimately reach the last of three stages of progress (Mullins, 1973): Stage 1, individual scientists working on similar problems in isolation; Stage 2, explicit identification with a field that is attracting colleagues to it; and Stage 3, the establishment of conferences, journals, and departments.

Without doubt, psychology of science has reached Stage 1, whereby individual scientists work on similar problems. This began in earnest in the 1950s. The field is now moving into Stage 2, with more and more psychologists identifying themselves as psychologists of science. The field is clearly not at Stage 3, although conferences may indeed appear in the near future. Currently there are occasional panels at various conferences, but these cannot substitute for a regular conference that brings psychologists of science together. Further, a journal devoted to the psychology of science is still not on the horizon. Finally, psychology of science needs to become a legitimate area of inquiry within university psychology departments. Departments at Carnegie-Mellon, Bowling Green, and University of Memphis are pioneers in this direction, but without a journal and a regular conference, it is likely that psychology of science will remain an avocation even at these institutions.

Importance of Psychology of Science

We believe the empirical findings and conceptual contributions of a psychology of science are important for at least five reasons:

1. Psychology simply cannot afford to ignore one of the most important human activities, one that has transformed the very world. Granted, the consequences of such knowledge are not uniformly constructive and positive. Combined with the ability to understand and create comes the ability to annihilate and destroy. This is all

the more reason to understand the psychology behind science and scientific knowledge, and in particular how it is created, communicated, and applied to new technologies. Having a citizenry that is ignorant of these processes and therefore unable to evaluate the end product of research can only lead to misguided and misunderstood attempts to control and regulate science. To the extent that politicians follow the will of the people and control the amount of money devoted to basic and applied research, a public is needed that has more than a superficial understanding of science and why it is important.

2. The other metasciences—philosophy, history, sociology (and more recently, anthropology)—are trying to supply their own answers to psychological questions concerning conceptual change, theory choice, motives, and personal styles of scientists. There is no doubt that psychology has the conceptual and theoretical artillery to attack precisely these questions.

3. A better psychological understanding of science is already leading to improvements in pedagogy, both for those who will become scientists and for those who need to understand science in order to be informed citizens. If the goals of having an informed adult populace regarding science are to be met, then psychologists of science need to know how to teach it in ways that unleash children's natural curiosity about how the world works. Furthermore, the psychology of science can contribute greatly to the understanding of how children conceptualize the physical world, and this is precisely what the Piagetian and neo-Piagetian literatures have done (Inhelder & Piaget, 1958; Klahr & Robinson, 1981; Klahr et al., 1993; D. Kuhn, 1989). Having accurate and sophisticated cognitive models of children's understanding of the natural world is absolutely necessary in understanding the development from child to adult scientific knowledge (cf. Klahr & Robinson, 1981).

4. Along similar applied lines, by understanding the actual psychological processes behind science, and in particular the best science, perhaps a psychology of science can have a loud and clear voice about selection criteria for potential graduate students and faculty. It is becoming increasingly evident that purely cognitive and intellectual skills are but one of many important predictors of successful careers, including science (Goleman, 1995; Sternberg, 1986, 1988a; Sternberg, Wagner, Williams, & Horvath, 1995). To the extent that gatekeepers continue to use selection criteria that measure but one part of what leads to doing good science (i.e., entrance exams and IQ tests), identifying and retaining scientific talent will remain poor (Gardner, 1993; Subotnik et al., 1993).

5. Studying the scientist will force psychological theories into an important new domain, leading to changes in psychological concepts. In fact, Simonton (1990, 1995) argued that the psychology of science offers an ideal and rich field for testing general psychological theory. We agree with Singer (1971):

> Psychology can make important practical contributions to the progress of science. The philosophy of science has generated many fundamental questions about scientific behavior which can be translated into research problems for the psychologist. A psychology of science, which seeks to understand the psychological nature of science and of knowing in general, is a rich and fascinating enterprise. (p. 1014)

Psychology of science will also encourage collaboration among psychologists from various subareas, helping the field achieve coherence rather than continued fragmentation. Staats (1991) has long argued for the necessity of a unified discipline of psychology if the field is to mature. The complete and fully developed psychology of science must by definition include developmental, biological, clinical, cognitive, personality, and social perspectives. Collaboration and cooperation among the subdisciplines is required. It would be no small feat and of no small import if the psychology of science could become a model for the parent discipline on how to combine resources and study science from a unified perspective.

References

Albert, R., & Runco, M. (1987). The possible different personality dispositions of scientists and non-scientists. In D. N. Jackson & J. P. Rushton (Eds.), *Scientific Excellence* (pp. 67–97). Beverly Hills, CA: Sage.

Albert, R., & Runco, M. (1995). [Personality dispositions of scientists and non-scientists]. Unpublished raw data.

Allport, G. W. (1985). The historical background of social psychology. In G. Lindzey & E. Aronson

(Eds.), *Handbook of social psychology* (3rd ed., Vol. 1, pp. 1–46) Hillsdale, NJ: Erlbaum.
Amabile, T. (1996). *Creativity in context.* New York: Westview.
Anzai, Y. (1991). Learning and use of representations for physics expertise. In K. A. Ericsson & J. Smith (Eds.), *Toward a general theory of expertise.* Cambridge, England: Cambridge University Press.
Arvey, R. D., & Dewhirst, H. D. (1976). Goal-setting attributes, personality variables, and job satisfaction. *Journal of Vocational Behavior, 9,* 179–189.
Arvey, R. D., Dewhirst, H., & Brown, E. M. (1976). A longitudinal study of the impact of changes in goal setting on employee satisfaction. *Personnel Psychology, 31,* 596–608.
Asch, S. E. (1956). Studies of independence and conformity. A minority of one against a unanimous majority. *Psychological Monographs, 70*(9, Whole No. 16).
Astin, H. S. (1975). Sex differences in mathematical and scientific precocity. *Journal of Special Education, 9,* 79–91.
Atwood, G. E., & Tomkins, S. S. (1976). On subjectivity of personality theory. *Journal of the History of the Behavioral Sciences, 12,* 166–177.
Bachtold, L. M. (1976). Personality characteristics of women of distinction. *Psychology of Women Quarterly, 1,* 70–78.
Bachtold, L. M., & Werner, E. E. (1972). Personality characteristics of women scientists. *Psychological Reports, 31,* 391–396.
Backman, M. E. (1972). Patterns of mental abilities: Ethnic, socioeconomic, and sex differences. *American Educational Research Journal, 9,* 1–12.
Bamber, J. H., Bill, J. M., Boyd, F. E., & Corbett, W. D. (1983). In two minds—arts and science differences at sixth-form level. *British Journal of Educational Psychological, 53,* 222–233.
Barber, B. (1961). Resistance by scientists to scientific discovery. *Science, 134,* 596–602.
Barnes, M. L., & Rosenthal, R. (1985). Interpersonal effects of experimenter attractiveness, attire, and gender. *Journal of Personality and Social Psychology, 48,* 435–446.
Barron, F., & Harrington, D. (1981). Creativity, intelligence, and personality. *Annual Review of Psychology, 32,* 439–476.
Barton, K., & Cattell, H. (1972). Personality characteristics of female psychology, science and art majors. *Psychological Reports, 31,* 807–813.
Bayer, A. E., & Dutton, J. E. (1977). Career age and research—professional activities of academic scientists: Tests of alternative non-linear models and some implications for higher education faculty policies. *Journal of Higher Education, 48,* 259–282.
Beard, G. M. (1874). *Legal responsibilities in old age.* New York: Russell Sage.
Bell, E. T. (1937). *Men of mathematics.* New York: Simon & Shuster.
Benbow, C. P. (1988). Sex differences in mathematical reasoning ability in intellectually talented preadolescents: Their nature, effects, and possible causes. *Behavioral and Brain Sciences, 11,* 169–183.
Benbow, C. P., & Lubinski, D. (1993). Psychological profiles of the mathematically talented. Some sex differences and evidence supporting their biological basis. In G. R. Rock & K. Ackrill (Eds.), *The origins and development of high ability* (pp. 44–66). Chichester, England: Wiley & Sons.
Benbow, C. P., & Minor, L. L. (1986). Mathematically talented students and achievement in the high school sciences. *American Educational Research Journal, 23,* 425–436.
Benbow, C. P., & Stanley, J. C. (1980). Sex differences in mathematical ability: Fact or artifact? *Science, 210,* 1262–1264.
Benbow, C. P., & Stanley, J. C. (1982). Consequences in high school and college of sex differences in mathematical reasoning ability: A longitudinal perspective. *American Educational Research Journal, 19,* 598–622.
Benbow, C. P., & Stanley, J. C. (1983). Sex differences in mathematical ability: More facts. *Science, 222,* 1029–1031.
Berry, C. (1981). The Nobel scientists and the origins of scientific achievement. *British Journal of Sociology, 32,* 381–391.
Bouchard, T. J., Jr., & McGue, M. (1981). Familial studies of intelligence: A review. *Science, 212,* 1055–1059.
Bouchard, T. J., Jr., & Segal, N. L. (1990). Advanced mathematical reasoning ability: A behavioral perspective. *Behavioral and Brain Sciences, 13,* 191–192.
Bradshaw, G. F. (1992). The airplane and the logic of invention. In R. N. Giere (Ed.), *Cognitive models of science* (pp. 239–250). Minneapolis: University of Minnesota Press.
Bradshaw, G. F., Langley, P. W., & Simon, H. A. (1983). Studying scientific discovery by computer simulation. *Science, 222,* 971–975.
Brewer, W. F., & Chinn, C. A. (1992). Entrenched beliefs, inconsistent information, and knowledge change. In L. Birnbaum (Ed.), *Proceedings of 1991 international conferences on the learning sciences* (pp. 67–73). Charlottesville, VA: Association for the Advancement of Computing in Education.
Brewer, W. F., & Samarapungavan, A. (1991). Child theories vs. scientific theories: Differences in reasoning or differences in knowledge? In R. R. Hoffman & D. S. Palermo (Eds.), *Cognition and the symbolic processes: Applied and ecological perspectives* (pp. 209–232). Hillsdale, NJ: Erlbaum.
Busse, T. V., & Mansfield, R. S. (1984). Selected

personality traits and achievement in male scientists. *The Journal of Psychology, 116,* 117–131.

Butcher, H. J. (1969). The structure of abilities, interests and personality in 1,000 Scottish school children. *British Journal of Educational Psychology, 39,* 154–165.

Campbell, D. T. (1989). Fragments of the fragile history of psychological epistemology and theory of science. In B. Gholson, W. R. Shadish, Jr., R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science* (pp. 21–46). Cambridge, England: Cambridge University Press.

Carey, S. (1992). The origin and evolution of everyday concepts. In R. N. Giere (Ed.), *Cognitive models of science* (pp. 89–128). Minneapolis: University of Minnesota Press.

Cattell, J. M. (1910). A further statistical study of American men of science. *Science, 32,* 633–648.

Cattell, J. M., & Brimhall, D. R. (1921). *American Men of Science* (3rd ed.). Garrison, NY: Science Press.

Cattell, R. B., & Drevdahl, J. E. (1955). A comparison of the personality profile (16PF) of eminent researchers with that of eminent teachers and administrators, and the general population. *British Journal of Psychology, 46,* 248–261.

Chamberlain, T. C. (1965). The method of multiple working hypotheses. *Science, 148,* 754–759. (Original work published 1890)

Chambers, J. A. (1964). Relating personality and biographical factors to scientific creativity. *Psychological Monographs: General and Applied, 78,* 1–20.

Chambers, J. A. (1965). Comments. *Science, 147,* 67.

Cheng, P. C.-H., & Simon, H. (1992). The right representation for discovery: Finding the conservation of momentum. In D. Sleeman & P. Edwards (Eds.), *Machine learning: Proceedings of the Ninth International Conference* (pp. 62–71). San Mateo, CA: Morgan Kaufmann.

Cheng, P. C.-H., & Simon, H. A. (1995). Scientific discovery and creative reasoning with diagrams. In S. M. Smith, T. B. Ward, & R. A. Finke (Eds.), *The creative cognition approach* (pp. 205–228). Cambridge, MA: MIT Press.

Chi, M. T. H., Feltovich, P. J., & Glaser, R. (1981). Categorization and representation of physics problems by experts and novices. *Cognitive Science, 5,* 121–152.

Clark, R. D., & Rice, G. A. (1982). Family constellations and eminence: The birth-orders of Nobel prize winners. *Journal of Psychology, 110,* 281–287.

Clement, J. (1982). Students' preconceptions in introductory mechanics. *American Journal of Physics, 50,* 66–71.

Clement, J. (1989). Learning via model construction and criticism: Protocol evidence on sources of creativity in science. In J. A. Glover, R. R. Ronning, & C. R. Reynolds (Eds.), *Handbook of creativity: Perspectives on individual differences* (pp. 341–381). New York: Plenum.

Clement, J. (1991). Experts and science students: The use of analogies, extreme cases, and physical intuition. In J. F. Voss, D. N. Perkins, & J. W. Segal (Eds.), *Informal reasoning and education* (pp. 345–362). Hillsdale, NJ: Erlbaum.

Cohen, J. (1988). *Statistical power analysis for behavioral sciences* (2nd ed.). Hillsdale, NJ: Erlbaum.

Cole, J. R. (1979). *Fair science: Women in the scientific community.* New York: Free Press.

Cole, J. R. (1987). Women in science. In D. Jackson & P. Rushton (Eds.), *Scientific Excellence* (pp. 359–375). Beverly Hills, CA: Sage.

Cole, J. R., & Cole, S. (1973). *Social stratification in science.* Chicago: The University of Chicago Press.

Cole, S. (1979). Age and scientific performance. *American Journal of Sociology, 84,* 958–977.

Cole, S., & Zuckerman, H. (1987). Marriage, motherhood, and research performance in science. *Scientific American, 256,* 119–125.

Conway, J. B. (1988). Differences among clinical psychologists: Scientists, practitioners, and science-practitioners. *Professional Psychology: Research and Practice, 19,* 642–655.

Cordaro, L., & Ison, J. R. (1963). Observer bias in classical conditioning of the planarian. *Psychological Reports, 13,* 787–789.

Cox, C. (1926). *Genetic studies of genius: Vol. II. The early mental traits of three hundred geniuses.* Stanford, CA: Stanford University Press.

Datta, L. E. (1967). Family religious background and early scientific creativity. *American Sociological Review, 32,* 626–635.

Davids, A. (1968). Psychological characteristics of high school male and female potential scientists in comparison with academic underachievers. *Psychology in the Schools, 3,* 79–87.

Deaux, K. (1985). Sex and gender. *Annual Review of Psychology, 36,* 49–81.

De Mey, M. (1989). Cognitive paradigms and the psychology of science. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science: Contributions to metascience* (pp. 275–295). Cambridge, England: Cambridge University Press.

De Mey, M. (1992). *The cognitive paradigm.* Chicago: University of Chicago Press.

Dennis, W. (1954). Predicting scientific productivity in later decades from records of earlier decades. *Journal of Gerontology, 9,* 465–467.

Dennis, W. (1956). Age and productivity among scientists. *Science, 123,* 724–725.

Dennis, W. (1958). The age decrement in outstanding scientific contributions: Fact or artifact? *American Psychologist, 13,* 457–460.

Dennis, W. (1966). Creative productivity between the ages of 20 and 80 years. *Journal of Gerontology, 21,* 1–8.

Diamond, A. M. (1986). The life-cycle research productivity of mathematicians and scientists. *Journal of Gerontology, 41,* 520–525.

Dunbar, K. (1995). How scientists really reason: Scientific reasoning in real-world laboratories. In R. J. Sternberg & J. Davidson (Eds.), *The nature of insight* (pp. 365–396). Cambridge, MA: MIT Press.

Eiduson, B. T. (1962). *Scientists: Their psychological world.* New York: Basic Books.

Eiduson, B. T. (1974). 10 year longitudinal Rorschachs on research scientists. *Journal of Personality Assessment, 38,* 405–410.

Erickson, C., Gantz, B. S., & Stephenson, R. W. (1970). Logical and construct validation of a short-form biographical inventory predictor of scientific creativity. In *Proceedings of the Annual Convention of the American Psychological Association, 5*(Pt. 1), 151–152.

Eysenck, H. J. (1988). O tempora, o mores! *Behavioral and Brain Sciences, 11,* 189–190.

Eysenck, H. J. (1993). Creativity and personality: Suggestions for a theory. *Psychological Inquiry, 4,* 147–178.

Eysenck, H. J. (1995). *Genius: The natural history of creativity.* Cambridge, England: Cambridge University Press.

Farmer, H. S. (1988). Predicting who our future scientists and mathematicians will be. *Behavioral and Brain Sciences, 11,* 190–191.

Farris, H., & Revlin, R. (1989a). The discovery process: A counterfactual strategy. *Social Studies of Science, 19,* 497–513.

Farris, H., & Revlin, R. (1989b). Sensible reasoning in two tasks: Rule discovery and hypothesis evaluation. *Memory & Cognition, 17*(2), 221–232.

Faust, D. (1984). *The limits of scientific reasoning.* Minneapolis: University of Minnesota Press.

Fay, A. L., Klahr, D., & Dunbar, K. (1990). Are there developmental milestones in scientific reasoning? In *Proceedings of the Twelfth Annual Conference of Cognitive Science Society* (pp. 333–339). Hillsdale, NJ: Erlbaum.

Feist, G. J. (1987). *The emotional and cognitive aspects and personality dimensions of artistic and scientific creativity.* Unpublished manuscript. University of California, Berkeley.

Feist, G. J. (1991). *The psychology of science: Personality, cognitive, motivational and working styles of eminent and less eminent scientists.* Unpublished doctoral dissertation, University of California, Berkeley.

Feist, G. J. (1993). A structural model of scientific eminence. *Psychological Science, 4,* 366–371.

Feist, G. J. (1994). Personality and working style predictors of integrative complexity: A study of scientists' thinking about research and teaching. *Journal of Personality and Social Psychology, 67,* 474–484.

Feist, G. J. (1995). Psychology of science and history of psychology: Putting behavioral generalizations to the test. *Psychological Inquiry, 6,* 119–123.

Feist, G. J. (1997). Quantity, impact, and depth of research as influences on scientific eminence: Is quantity most important? *Creativity Research Journal, 10,* 325–335.

Feist, G. J., & Barron, F. (1995, October). *Do hostile and arrogant scientists become eminent or are eminent scientists likely to become hostile and arrogant?* Paper presented at the annual conference of Society for Social Studies of Science, Charlottesville, VA.

Fisch, R. (1977). Psychology of science. In I. Spiegel-Rösing & D. de Solla Price (Eds.), *Science, technology, and society: A cross disciplinary perspective* (277–318). London: Sage.

Fischbein, S. (1990). Biosocial influences on sex differences for ability and achievement test results as well as marks at school. *Intelligence, 14,* 127–139.

Fox, L. H. (1976). Sex differences in mathematical precocity: Bridging the gap. In D. P. Keating (Ed.), *Intellectual talent: Research and development* (pp. 32–54). Baltimore: Johns Hopkins University Press.

Freedman, E. (1995, October). *Working memory and testing multiple hypotheses.* Paper presented at Annual Convention of the Society for Social Studies of Science, Charlottesville, VA.

Fuller, S. (1988). *Social epistemology.* Bloomington, IN: Indiana University Press.

Fuller, S. (1993). *Philosophy of science and its discontents* (2nd ed.). New York: Guilford.

Galton, F. (1874). *English men of science.* London: Macmillan.

Gantz, B. S., Erickson, C. O., & Stephenson, R. W. (1972). Some determinants of promotion in a research and development population. In *Proceedings of Annual Convention of the American Psychological Association, 7*(Pt. 1), 451–452.

Gardner, H. (1993). *Multiple intelligences: The theory in practice.* New York: Basic Books.

Garwood, D. S. (1964). Personality factors related to creativity in young scientists. *Journal of Abnormal and Social Psychology, 68,* 413–419.

Gentner, D., & Gentner, G. R. (1983). Flowing waters or teeming crowds: Mental models of electricity. In D. Gentner & A. L. Stevens (Eds.), *Mental models* (pp. 99–129). Hillsdale, NJ: Erlbaum.

Gentner, D., & Jeziorski, M. (1989). Historical shifts in the use of analogy in science. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science: Contributions to metascience* (pp. 296–325). Cambridge, England: Cambridge University Press.

Geschwind, N., & Behan, P. (1982). Left-handedness: Association with immune disease, migraine, and developmental learning disorder. *Proceedings of the National Academy of Sciences, 79,* 5097–5100.

Getzels, J. W. (1987). Creativity, intelligence, and problem finding: Retrospect and prospect. In S. G. Isaksen (Ed.), *Frontiers of creativity research* (pp. 88–102). Buffalo, NY: Bearly.

Gholson, B., & Houts, A. C. (1989). Toward a cognitive psychology of science. *Social Epistemology, 3,* 107–127.

Gholson, B., Shadish, W. R., Neimeyer, R. A., & Houts, A. C. (Eds.). (1989). *The psychology of science: Contributions to metascience.* Cambridge, England: Cambridge University Press.

Giere, R. N. (1988). *Explaining science: A cognitive approach.* Chicago: University of Chicago Press.

Gleick, J. (1992). *Genius: Richard Feynman and modern physics.* New York: Pantheon.

Goleman, D. (1995). *Emotional intelligence.* New York: Bantam Books.

Gooding, D. C. (1985). In nature's school: Faraday as an experimentalist. In D. Gooding & F. James (Eds.), *Faraday rediscovered: Essays on the life and work of Michael Faraday: 1791–1867.* New York: Stockton Press.

Gooding, D. C. (1990). *Experiment on the making of meaning: Human agency in scientific observation and experiment.* Dordrecht, Netherlands: Kluwer Academic Publishers.

Gooding, D. C., & Addis, T. R. (1993). *Modelling Faraday's experiments with visual functional programming 1: Models, methods and examples* (Working Paper No. Joint Research Councils' Initiative on Cognitive Science & Human Computer Interaction Special Project Grant #9107137). Bath, England: University of Bath.

Gorman, M. E. (1986). How the possibility of error affects falsification on a task that models scientific problem-solving. *British Journal of Psychology, 77,* 85–96.

Gorman, M. E. (1989). Error, falsification and scientific inference: An experimental investigation. *Quarterly Journal of Experimental Psychology, 41,* 385–412.

Gorman, M. E. (1992). *Simulating science: Heuristics, mental models and technoscientific thinking.* Bloomington: Indiana University Press.

Gorman, M. E. (1995). Confirmation, disconfirmation and invention: The case of Alexander Graham Bell and the telephone. *Thinking and Reasoning, 1*(7), 31–53.

Gorman, M. E., & Gorman, M. E. (1984). A Comparison of disconfirmatory, confirmatory and a control strategy on Wason's 2-4-6 task. *Quarterly Journal of Experimental Psychology, 36,* 629–648.

Gorman, M. E., & Rosenwein, R. (1995). Simulating social epistemology. *Social Epistemology, 9*(1), 71–79.

Gorman, M. E., Stafford, A., & Gorman, M. E. (1987). Disconfirmation and dual hypotheses on a more difficult version of Wason's 2-4-6 task. *Quarterly Journal of Experimental Psychology, 39,* 1–28.

Gorman, M. E., Gorman, M. E., Latta, R. M., & Cunningham, G. (1984). How disconfirmatory, confirmatory and combined strategies affect group problem-solving. *British Journal of Psychology, 75,* 65–79.

Gough, H. G. (1961, February). *A personality sketch of the creative research scientist.* Paper presented at 5th Annual Conference on Personnel and Industrial Relations Research, Los Angeles, CA.

Gough, H. G. (1976). What happens to creative medical students. *Journal of Medical Education, 51,* 461–467.

Gough, H. G. (1987). *California Psychological Inventory Manual.* Palo Alto, CA: Consulting Psychologists Press.

Gough, H. G., & Woodworth, D. G. (1960). Stylistic variations among professional research scientists. *Journal of Psychology, 49,* 87–98.

Green, A. J. K., & Gilhooly, K. J. (1992). Empirical advances in expertise research. In M. T. Keane & K. J. Gilhooly (Eds.), *Advances in the psychology of thinking* (pp. 45–70). London: Harvester and Wheatchief.

Gross, P. R., & Levitt, N. (1994). *Higher superstition: The academic left and its quarrels with science.* Baltimore: Johns Hopkins Press.

Gruber, H. E. (1981). *Darwin on man: A psychological study of scientific creativity.* Chicago: University of Chicago Press.

Gruber, H. E. (1989). Networks of enterprise in creative scientific thought. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science: Contributions to metascience* (pp. 246–266). Cambridge, England: Cambridge University Press.

Guilford, J. P. (1959). Traits of creativity. In H. H. Anderson (Ed.), *Creativity and its cultivation* (pp. 142–161). New York: Harper.

Guilford, J. P. (1987). A review of a quarter century of progress. In S. G. Isaksen (Ed.), *Frontiers of creativity research* (pp. 45–61). Buffalo, NY: Bearly.

Guyer, L., & Fidell, L. (1973). Publications of men and women psychologists: Do women publish less? *American Psychologist, 28,* 157–160.

Ham, S., & Shaughnessy, M. F. (1992). Personality and scientific promise. *Psychological Reports, 70,* 971–975.

Hanson, N. R. (1962). Scientists and logicians: A confrontation. *Science, 138,* 1311–1313.

Hargens, L. L., McCann, J. C., & Reskin, B. F. (1978). Productivity and reproductivity: Fertility and professional achievement among research scientists. *Social Forces, 57,* 154–163.

Hart, J. J. (1982). Psychology of the scientists: XLVI: Correlation between theoretical orientation in psychology and personality type. *Psychological Reports, 50,* 795–801.

Helmreich, R. L., Spence, J. T., Beane, W. E., Lucker, G. W., & Matthews, K. A. (1980). Making it in academic psychology: Demographic and personality correlates of attainment. *Journal of Personality and Social Psychology, 39,* 896–908.

Helmreich, R. L., Spence, J. T., & Pred, R. S. (1988). Making it without losing it: Type A, achievement motivation and scientific attainment revisited. *Personality and Social Psychology Bulletin, 14,* 495–504.

Helson, R. (1971). Women mathematicians and the creative personality. *Journal of Consulting and Clinical Psychology, 36,* 210–220.

Helson, R., & Crutchfield, R. S. (1970). Mathematicians: The creative researcher and the average PhD. *Journal of Consulting and Clinical Psychology, 34,* 250–257.

Heyes, C. M. (1989). Uneasy chapters in the relationship between psychology and epistemology. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science: Contributions to metascience* (pp. 115–137). Cambridge, England: Cambridge University Press.

Hinrichs, J. R. (1972). Value adaptation of new PhDs to academic and industrial environments: A comparative longitudinal study. *Personnel Psychology, 25,* 545–565.

Holden, C. (1987). Female math anxiety on the wane. *Science, 236,* 600–601.

Holland, J. (1961, June). *The assessment and prediction of creative performance of high aptitude youth.* Paper presented at the 4th University of Utah Conference on the Identification of Creative Scientific Talent, Salt Lake City, UT.

Holmes, F. L. (1980). Hans Krebs and the discovery of the ornithine cycle. *Federation Proceedings, 39,* 216–225.

Holyoak, K. J., & Thagard, P. (1995). *Mental leaps.* Cambridge, MA: MIT Press.

Horner, K. L., Rushton, J. P., & Vernon, P. A. (1986). Relation between aging and research productivity of academic psychologists. *Psychology and Aging, 4,* 319–324.

Houts, A. (1989). Contributions of the psychology of science to metascience: A call for explorers. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science* (pp. 47–88). Cambridge, England: University of Cambridge Press.

Hudson, L. (1958). Undergraduate academic record of Fellows of the Royal Society. *Nature, 182,* 1326.

Hull, D. L., Tessner, P. D., & Diamond, A. M. (1978). Planck's principle: Do younger scientists accept new scientific ideas with greater alacrity than older scientists? *Social Studies of Science, 202,* 717–723.

Husén, T. (1960). Abilities of twins. *Scandinavian Journal of Psychology, 1,* 25–35.

Ikpaahindi, L. (1987). The relationship between the needs for achievement, affiliation, power, and scientific productivity among Nigerian veterinary surgeons. *The Journal of Social Psychology, 127,* 535–537.

Inhelder, B., & Piaget, J. (1958). *The growth of logical thinking from childhood to adolescence.* New York: Basic Books.

Jacobs, R. C., & Campbell, D. T. (1961). The perpetuation of an arbitrary tradition through several generations of a laboratory microculture. *Journal of Abnormal and Social Psychology, 83,* 649–658.

Johnson, J. A., Germer, C. K., Efran, J. S., & Overton, W. F. (1988). Personality as the basis for theoretical predilections. *Journal of Personality and Social Psychology, 55,* 824–835.

John-Steiner, V. (1985). *Notebooks of the mind.* Albuquerque: University of New Mexico Press.

Kanigel, R. (1991). *The man who knew infinity: A life of the genius Ramanujan.* New York: Scribner's & Sons.

Karlsson, J. (1991). *Genetics of human mentality.* New York: Praeger.

Karmiloff-Smith, A. (1988). A child is a theoretician, not an inductivist. *Mind and Language, 3,* 183–195.

Keating, D. P. (1974). The study of mathematically precocious youth. In J. C. Stanley, D. P. Keating, & L. H. Fox (Eds.), *Mathematical talent: Discovery, description, and development* (pp. 23–46). Baltimore: Johns Hopkins University Press.

Keller, E. F. (1985). *Reflections on gender and science.* New Haven, CT: Yale University Press.

Kelley, H. H. (1967). Attribution theory in social psychology. In D. L. Vine (Ed.), *Nebraska symposium on motivation* (pp. 192–240). Lincoln: University of Nebraska Press.

Klahr, D., & Dunbar, K. (1988). Dual space search during scientific reasoning. *Cognitive Science, 12,* 1–48.

Klahr, D., Dunbar, K., & Fay, A. L. (1990). Designing good experiments to test bad hypotheses. In J. Shrager & P. Langley (Eds.), *Computational models of discovery and theory formation* (pp. 355–402). San Mateo, CA: Morgan Kaufmann Publishers, Inc.

Klahr, D., Fay, A. L., & Dunbar, K. (1993). Heuristics for scientific experimentation: A developmental study. *Cognitive Psychology, 25,* 111–146.

Klahr, D., & Robinson, M. (1981). Formal assessment of problem-solving and planning processes in preschool children. *Cognitive Psychology, 13,* 113–148.

Klayman, J., & Ha, Y.-W. (1987). Confirmation, disconfirmation and information in hypothesis testing. *Psychological Review, 94,* 211–228.

Kline, P., & Lapham, S. L. (1992). Personality and faculty in British universities. *Personality and Individual Differences, 13,* 855–857.

Kruglanski, A. W. (1994). The social-cognitive bases of scientific knowledge. In W. R. Shadish & S. Fuller (Eds.), *The social psychology of science* (pp. 197–213). New York: Guilford.

Kuhn, D. (1989). Children and adults as intuitive scientists. *Psychological Review, 96*(4), 674–689.

Kuhn, D., Amstel, E., & O'Loughlin, M. (1988). *The development of scientific thinking skills.* New York: Academic Press.

Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.

Kulkarni, D., & Simon, H. A. (1988). The processes of scientific discovery: The strategies of experimentation. *Cognitive Science, 12,* 139–175.

Lacey, L. A., & Erickson, C. E. (1974). Psychology of the scientist: XXXI: Discriminability of a creativity scale for the Adjective Check List among scientists and engineers. *Psychological Reports, 34,* 755–758.

Langley, P., Simon, H. A., Bradshaw, G. L., & Zykow, J. M. (1987). *Scientific discovery: Computational explorations of the creative processes.* Cambridge, MA: MIT Press.

Larkin, J. (1983). The role of problem representation in physics. In D. Gentner & Stevens, A. L. (Ed.), *Mental models* (pp. 75–98). Hillsdale, NJ: Lawrence Erlbaum Associates.

Larkin, J. H., McDermitt, J., Simon, D. P., & Simon, H. A. (1980). Expert and novice performance in solving physics problems. *Science, 208,* 1335–1342.

Larkin, J. H., & Simon, H. A. (1987). Why a diagram is (sometimes) worth ten thousand words. *Cognitive Science, 11,* 65–99.

Leary, D. E. (Ed.). (1990). *Metaphors in the history of psychology.* New York: Cambridge University Press.

Lehman, H. C. (1953). *Age and achievement.* Princeton, NJ: Princeton University Press.

Lehman, H. C. (1960). The age decrement in outstanding scientific creativity. *American Psychologist, 15,* 128–134.

Lehman, H. C. (1962). The creative production rates of present versus past generations of scientists. *Journal of Gerontology, 17,* 409–417.

Lehman, H. C. (1966). The psychologist's most creative years. *American Psychologist, 21,* 363–369.

Levin, S. G., Stephen, P. E., & Walker, M. B. (1995). Planck's principle revisited: A special note. *Social Studies of Science, 25,* 275–283.

Loehlin, J. C., & Nichols, R. C. (1976). *Heredity, environment, and personality: A study of 850 sets of twins.* Austin, TX: University of Texas Press.

Long, J. S. (1992). Measures of sex differences in scientific productivity. *Social Forces, 71,* 159–178.

Lotka, A. J. (1926). The frequency distribution of scientific productivity. *Journal of the Washington Academy of Sciences, 16,* 317–323.

Lubinski, D., & Benbow, C. P. (1994). The study of mathematically precocious youth: The first three decades of a planned 50-year study of intellectual talent. In R. F. Subotnik & K. D. Arnold (Eds.), *Beyond Terman: Longitudinal studies of giftedness and talent* (pp. 255–281). Norwood, NJ: Ablex.

Lykken, D. T., McGue, M., Tellegen, A., & Bouchard, T. J., Jr. (1992). Emergenesis: Genetic traits that may not run in families. *American Psychologist, 47,* 1565–1577.

Maccoby, E. E., & Jacklin, C. N. (1974). *The psychology of sex differences.* Stanford, CA: Stanford University Press.

MacKinnon, D. W. (1960). The highly effective individual. *Teachers College Record, 61,* 367–378.

MacKinnon, D. W. (1978). *In search of human effectiveness.* Buffalo, NY: Bearly.

Maddi, S. (1965). Motivational aspects of creativity. *Journal of Personality, 33,* 338–347.

Mahoney, M. J. (1977). Publication prejudices: An experimental study of confirmatory bias in the peer review system. *Cognitive Therapy and Research, 1,* 161–175.

Mahoney, M. J. (1979). Psychology of the scientist: An evaluative review. *Social Studies of Science, 9,* 349–375.

Mahoney, M. J., & DeMonbreun, B. J. (1977). Psychology of the scientist: An analysis of problem solving bias. *Cognitive Therapy and Research, 1,* 229–238.

Mansfield, R. S., & Busse, T. V. (1981). *The psychology of creativity and discovery: Scientists and their work.* Chicago: Nelson-Hall.

Maslow, A. (1966). *The psychology of science.* New York: Harper & Row.

McClelland, D. (1962). On the psychodynamics of creative physical scientists. In H. Gruber, G. Terrell, & M. Wertheimer (Eds.), *Contemporary approaches to creative thinking* (pp. 141–174). New York: Atherton.

McCloskey, M. (1983). Naive theories of motion. In D. Gentner & A. L. Stevens (Eds.), *Mental models* (pp. 299–324). Hillsdale, NJ: Lawrence Erlbaum Associates.

McDermid, C. D. (1965). Some correlates of creativity in engineering personnel. *Journal of Applied Psychology, 49,* 14–19.

Merton, R. K. (1973). *The sociology of science: Theoretical and empirical investigations.* Chicago: University of Chicago Press.

Messeri, P. (1988). Age differences in the reception of new scientific theory: The case of plate tectonics theory. *Social Studies of Science, 18,* 91–112.

Miller, A. (1989). Imagery, metaphor, and physical reality. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science* (pp. 326–341). Cambridge, England: Cambridge University Press.

Mohan, J., & Kaur, M. (1993). Adjustment, personal intelligence, values, and SES of male–female university research scholars. *Social Science International, 9,* 39–50.

Moore, E. G. J., & Smith, A. W. (1987). Sex and ethnic group differences in mathematics achievement: Results from the National Longitudinal Study. *Journal for Research in Mathematics Education, 18,* 25–36.

Moscovici, S., & Nemeth, C. (1974). Social influence II: Minority influence. In C. Nemeth (Ed.), *Social psychology: Classic and contemporary integrations* (pp. 217–249). Chicago: Rand McNally.

Mossholder, K. W., Dewhirst, H. D., & Arvey, R. D. (1981). Vocational interest and personality differences between development and research personnel: A field study. *Journal of Vocational Behavior, 19,* 233–243.

Mullins, N. (1973). *Theories and theory groups in contemporary American sociology.* New York: Harper and Row.

Mynatt, C. R., Doherty, M. E., & Tweney, R. D. (1977). Confirmation bias in simulated research environment: An experimental study of scientific inference. *Quarterly Journal of Experimental Psychology, 29,* 85–95.

Mynatt, C. R., Doherty, M. E., & Tweney, R. D. (1978). Consequences of confirmation and disconfirmation in a simulated research environment. *Quarterly Journal of Experimental Psychology, 30,* 395–406.

Nersessian, N. J. (1992). How do scientists think? Capturing the dynamics of conceptual change in science. In R. N. Giere (Ed.), *Cognitive models of science* (pp. 3–44). Minneapolis: University of Minnesota Press.

Oaksford, M., & Chater, N. (1994). Another look at eliminative and enumerative behaviour in a conceptual task. *The European Journal of Cognitive Psychology, 6,* 149–169.

Over, R. (1982). Is age a good predictor of research productivity? *Australian Psychologist, 17,* 129–139.

Over, R. (1989). Age and scholarly impact. *Psychology and Aging, 4,* 222–225.

Parloff, M. B., & Datta, L. (1965). Personality characteristics of the potentially creative scientist. *Science and Psychoanalysis, 8,* 91–105.

Parloff, M. B., Datta, L., Kleman, M., & Handlon, J. H. (1968). Personality characteristics which differentiate creative male adolescents and adults. *Journal of Personality, 36,* 528–552.

Pasewark, R. A., Fitzgerald, B. J., & Sawyer, R. N. (1975). Psychology of the scientist: XXXII. God at the synapse: Research activities of clinical, experimental, and physiological psychologists. *Psychological Reports, 36,* 671–674.

Pearce, C. (1968). Creativity in young science students. *Exceptional Children, 35,* 121–126.

Piaget, J., & Garcia, R. (1989). *Psychogenesis and the history of science.* New York: Columbia University Press.

Popper, K. (1959). *The logic of scientific discovery.* New York: Science Editions.

Price, D. (1963). *Little science, big science.* New York: Columbia University Press.

Qin, Y., & Simon, H. A. (1990). Laboratory replication of scientific discovery processes. *Cognitive Science, 14,* 281–312.

Reskin, B. F. (1977). Scientific productivity and the reward structure of science. *American Sociological Review, 42,* 491–504.

Reynolds, A. J., & Walberg, H. J. (1992). A structural model of science achievement and attitude: An extension to high school. *Journal of Educational Psychology, 84,* 371–382.

Roco, M. (1993). Creative personalities about creative personality in science. *Revue Roumaine de Psychologie [Romanian Review of Psychology], 37,* 27–36.

Roe, A. (1952a). *The making of a scientist.* New York: Dodd, Mead.

Roe, A. (1952b). A psychologist examines 64 eminent scientists. *Scientific American, 187,* 21–25.

Roe, A. (1953). A psychological study of eminent psychologists and anthropologists, and a comparison with biological and physical scientists. *Psychological Monographs: General and Applied, 67,* 1–55.

Roe, A. (1965). Changes in scientific activities with age. *Science, 150,* 313–318.

Roe, A. (1972). Maintenance of creative output through the years. In C. W. Taylor (Ed.), *Climate for creativity* (pp. 167–191). New York: Pergamon.

Root-Bernstein, R. S., Bernstein, M., & Garnier, H. (1995). Correlations between avocations, scientific style, work habits, and professional impact of scientists. *Creativity Research Journal, 8,* 115–137.

Rosenthal, R. (1976). *Experimenter effects in behavioral research* (enlarged edition). New York: Irvington.

Rosenthal, R. (1994). On being one's own case study: Experimenter effects in behavioral research—30 years later. In W. Shadish & S. Fuller (Eds.), *The social psychology of science*. New York: Guilford.

Rosenthal, R., & Fode, K. L. (1963a). Psychology of the scientist: V: Three experiments in experimenter bias. *Psychological Reports, 12,* 491–511.

Rosenthal, R., & Fode, K. L. (1963b). The effect of experimenter bias on the performance of the albino rat. *Behavioral Science, 8,* 183–189.

Rosenthal, R., & Jacobson, L. (1992). *Pygmalion in the classroom* (enlarged edition). New York: Irvington.

Rosenthal, R., & Rosnow, R. L. (1991). *Essentials of behavioral research: Methods and data analysis* (2nd ed.). New York: McGraw-Hill.

Rosenwein, R. (1994). Social influence in science: Agreement and dissent in achieving scientific consensus. In W. R. Shadish & S. Fuller (Eds.), *The social psychology of science* (pp. 262–285). New York: Guilford.

Royalty, G. M., & Magoon, T. M. (1985). Correlates of scholarly productivity among counseling psychologists. *Journal of Counseling Psychology, 32,* 458–461.

Rushton, J. P., Murray, H. G., & Paunonen, S. V. (1987). Personality characteristics associated with high research productivity. In D. Jackson & J. P. Rushton (Eds.), *Scientific excellence* (pp. 129–148). Beverly Hills, CA: Sage.

Scarr, S., & Saltzman, L. C. (1982). Genetics and intelligence. In R. J. Sternberg (Ed.), *Handbook of human intelligence* (pp. 792–816). Cambridge, England: Cambridge University Press.

Schaefer, C. E. (1969). The self-concept of creative adolescents. *The Journal of Psychology, 72,* 233–242.

Schaie, K. W. (1984). Midlife influences upon intellectual functioning in old age. *International Journal of Behavior Development, 7,* 463–478.

Schroder, H. M., Driver, M. J., & Streufert, S. (1967). *Human information processing*. New York: Holt, Reinhart, & Winston.

Scott, N. A., & Sedlacek, W. E. (1975). Personality differentiation and prediction of persistence in physical science and engineering. *Journal of Vocational Behavior, 6,* 205–216.

Shadish, W. R. (1989). The perception and evaluation of quality in science. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science: Contributions to metascience* (pp. 383–426). Cambridge, England: Cambridge University Press.

Shadish, W. R., & Fuller, S. (Eds.). (1994). *Social psychology of science*. New York: Guilford.

Shadish, W. R., Fuller, S., & Gorman, M. E. (1994). Social psychology of science: A conceptual and empirical research program. In W. R. Shadish & S. Fuller (Eds.), *Social psychology of science* (pp. 3–123). New York: Guilford Press.

Shadish, W. R., Houts, A. C., Gholson, B., & Neimeyer, R. A. (1989). The psychology of science: An introduction. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science: Contributions to metascience* (pp. 1–16). Cambridge, England: Cambridge University Press.

Shadish, W. R., Tolliver, D., Gray, M., & Gupta, S. K. (1995). Author judgements about works they cite: Three studies from psychology journals. *Social Studies of Science, 25*(3), 477–498.

Shapiro, R. J. (1968). Creative research scientists. *Psychologia Africana Monograph Supplement, No. 4* (180).

Shrager, J., & Langley, P. (1990). *Computational models of scientific discovery and theory formation*. San Mateo, CA: Morgan Kaufmann Publishers, Inc.

Siegler, R. S., & Liebert, R. M. (1975). Acquisition of formal scientific reasoning by 10- and 13-year olds: Designing a factorial experiment. *Developmental Psychology, 10,* 401–402.

Simon, H. A. (1966). Scientific discovery and the psychology of problem solving. In R. Colodny (Ed.), *Mind and cosmos* (22–40). Pittsburgh, PA: University of Pittsburgh Press.

Simon, H. A. (1974). The work habits of eminent scientists. *Sociology of Work and Occupations, 1,* 327–335.

Simon, H. L., Langley, P. W., & Bradshaw, G. (1981). Scientific discovery as problem solving. *Syntheses, 47,* 1–27.

Simonton, D. K. (1975). Invention and discovery among the sciences: A p-technique factor analysis. *Journal of Vocational Behavior, 7,* 275–281.

Simonton, D. K. (1976a). The causal relation between war and scientific discovery. *Journal of Cross-Cultural Psychology, 7,* 133–144.

Simonton, D. K. (1976b). Interdisciplinary and military determinants of scientific productivity: A cross-lagged correlation analysis. *Journal of Vocational Behavior, 9,* 53–62.

Simonton, D. K. (1977a). Creative productivity, age, and stress: A biographical time-series analysis of 10 classical composers. *Journal of Personality and Social Psychology, 35,* 791–804.

Simonton, D. K. (1977b). Eminence, creativity, and geographic marginality: A recursive structural equation model. *Journal of Personality and Social Psychology, 35,* 805–816.

Simonton, D. K. (1980). Techno-scientific activity and war: A yearly time-series analysis, 1500–1903 A.D. *Scientometrics, 2,* 251–255.

Simonton, D. K. (1984). Creative productivity and age: A mathematical model based on a two-step cognitive process. *Developmental Review, 4,* 77–111.

Simonton, D. K. (1988a). *Scientific genius: A psychology of science.* Cambridge, England: Cambridge University Press.

Simonton, D. K. (1988b). Age and outstanding achievement: What do we know after a century of research? *Psychological Bulletin, 104,* 251–267.

Simonton, D. K. (1989). Chance-configuration theory of scientific creativity. In B. Gholson, W. R. Shadish, R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science* (pp. 170–213). Cambridge, England: Cambridge University Press.

Simonton, D. K. (1990). *Psychology, science, and history: A treatise on their convergence.* New Haven, CT: Yale University Press.

Simonton, D. K. (1991). Career landmarks in science: Individual differences and interdisciplinary contrasts. *Developmental Psychology, 27,* 119–130.

Simonton, D. K. (1992a). The social context of career success and course for 2,026 scientists and inventors. *Personality and Social Psychology Bulletin, 18,* 452–463.

Simonton, D. K. (1992b). Leaders in American psychology, 1879–1967: Career development, creative output and professional achievement. *Journal of Personality and Social Psychology, 62,* 5–17.

Simonton, D. K. (1995). Behavioral laws in histories of psychology: Psychological science, metascience, and the psychology of science. *Psychological Inquiry, 6,* 89–114.

Singer, B. F. (1971). Toward a psychology of science. *American Psychologist, 26,* 1010–1015.

Smithers, A. G., & Batcock, A. (1970). Success and failure among social scientists and health scientists at a technological university. *British Journal of Educational Psychology, 40,* 144–153.

Sonnert, G. (1995). What makes a good scientist?: Determinants of peer evaluation among biologists. *Social Studies of Science, 25,* 35–55.

Staats, A. (1991). Unified positivism and unification psychology. *American Psychologist, 46,* 899–912.

Stanley, J. C. (1988). Some characteristics of SMPY's "700-800 on SAT-M before age 13 group": Youths who reason *extremely* well mathematically. *Gifted Child Quarterly, 32,* 205–209.

Stanley, J. C., Keating, D. P., & Fox, L. H. (Eds.). (1974). *Mathematical talent: Discovery, description, and development.* Baltimore: Johns Hopkins University Press.

Stanton, F., & Baker, K. H. (1942). Interviewer bias and the recall of incompletely learned materials. *Sociometry, 5,* 123–134.

Steiner, I. D. (1972). *Group process and productivity.* New York: Academic Press.

Sternberg, R. J. (1986). *The triarchic mind: A new theory of human intelligence.* New York: Viking Penguin Inc.

Sternberg, R. J. (1988a). A three-facet model of creativity. In R. J. Sternberg (Ed.), *The nature of creativity* (pp. 125–147). Cambridge, England: Cambridge University Press.

Sternberg, R. J. (1988b). The male/female difference is there: Should we care? *Behavioral and Brain Sciences, 11,* 210–211.

Sternberg, R. J., & Gordeeva, T. (1996). The anatomy of impact: What makes an article influential. *Psychological Science, 7,* 69–75.

Sternberg, R. J., Wagner, R. K., Williams, W. M., & Horvath, J. A. (1995). Testing common sense. *American Psychologist, 50,* 912–927.

Stevens, S. S. (1936). Psychology: The propaedeutic science. *Philosophy of Science, 3,* 90–104.

Stevens, S. S. (1939). Psychology and the science of science. *Psychological Bulletin, 36,* 221–263.

Subotnik, R. F., Duschl, R. A., & Selmon, E. H. (1993). Retention and attrition of science talent: A longitudinal study of Westinghouse science talent search winners. *International Journal of Science Education, 15,* 61–72.

Subotnik, R. F., & Steiner, C. L. (1992). Adult manifestations of adolescent talent in science. *Roeper Review, 15,* 164–169.

Suedfeld, P. (1985). APA presidential addresses: The relation of integrative complexity to historical, professional and personal factors. *Journal of Personality and Social Psychology, 49,* 1634–1651.

Sulloway, F. (1996). *Born to rebel: Birth order, family dynamics, and creative lives.* New York: Pantheon.

Taylor, D. W. (1963). Variables related to creativity and productivity among men in two research laboratories. In C. W. Taylor & F. Barron (Eds.), *Scientific creativity: Its recognition and development* (pp. 228–250). New York: Wiley & Sons.

Taylor, C. W., & Barron, F. (1963). *Scientific creativity: Its recognition and development.* New York: John Wiley & Sons.

Terman, L. M. (1925). *Genetic studies of genius: Vol. 1. Mental and physical traits of a thousand gifted children.* Palo Alto, CA: Stanford University Press.

Terman, L. M. (1954). Scientists and nonscientists in a group of 800 men. *Psychological Monographs, 68,* (7, Whole No. 378).

Terman, L. M. (1955). Are scientists different? *Scientific American, 192,* 25–29.

Tetlock, P. E., & Suedfeld, P. (1988). Integrative complexity coding of verbal behavior. In C. Antaki (Ed.), *Lay explanation* (pp. 43–59). Beverly Hills, CA: Sage.

Thagard, P. (1988). *Computational philosophy of science.* Cambridge, MA: MIT Press.

Thagard, P., & Nowak, G. (1990). The conceptual structure of the geological revolution. In J. Shrager

& P. Langley (Ed.), *Computational models of scientific discovery and theory formation* (pp. 27–72). San Mateo, CA: Morgan Kaufmann Publishers, Inc.

Tweney, R. D. (1985). Faraday's discovery of induction: A cognitive approach. In D. Gooding & F. James (Eds.), *Faraday rediscovered: Essays on the life and work of Michael Faraday: 1791–1867.* New York: Stockton Press.

Tweney, R. D. (1989). A framework for the cognitive psychology of science. In B. Gholson, W. R. Shadish, Jr., R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of Science* (pp. 342–366). Cambridge, MA: Cambridge University Press.

Tweney, R. D. (1991). Faraday's notebooks: The active organization of creative science. *Physics Education, 26,* 301–306.

Tweney, R. D. (1994). Towards a cognitive psychology of science. *Mexican Journal of Behavioral Analysis, 20,* 35–46.

Tweney, R. D., & Chitwood, S. T. (1995). Scientific reasoning. In S. E. Newstead & S. St. B. T. Evans (Eds.), *Perspectives on thinking and reasoning: Essays in honour of Peter Wason* (pp. 241–260). Hove, England: Erlbaum.

Tweney, R. D., Doherty, M. E., & Mynatt, C. R. (Eds.). (1981). *On scientific thinking.* New York: Columbia University Press.

Udell, G. G., Baker, K. G., & Albaum, G. S. (1976). Creativity: Necessary, but not sufficient. *Journal of Creative Behavior, 10,* 92–103.

Vandenberg, S. G. (1988). Could these sex differences be due to genes? *Behavioral and Brain Sciences, 11,* 212–214.

Van Zelst, R. H., & Kerr, W. A. (1954). Personality self-assessment of scientific and technical personnel. *Journal of Applied Psychology, 38,* 145–147.

Vernon, P. E. (1989). The nature–nurture problem in creativity. In A. Glover, R. R. Ronning, & C. R. Reynolds (Eds.), *Handbook of creativity: Perspectives on individual differences* (pp. 93–110). New York: Plenum Press.

Wallach, M. A. (1970). Creativity. In P. H. Mussen (Ed.), *Manual of child psychology* (pp. 1211–1272). New York: Wiley & Sons.

Waller, N. G., Bouchard, T. J., Lykken, D. T., Tellegen, A., & Blacker, D. M. (1993). Creativity, heritability, familiality: Which word does not belong? *Psychological Inquiry, 4,* 235–237.

Wason, P. C. (1960). On the failure to eliminate hypotheses in a conceptual task. *Quarterly Journal of Experimental Psychology, 12,* 129–140.

Werts, C. E., & Watley, D. J. (1972). Paternal influence on talent development. *Journal of Counseling Psychology, 19,* 367–373.

Wharton, C. M., Cheng, P. W., & Wickens, T. D. (1993). Hypothesis-testing strategies: Why two goals are better than one. *Quarterly Journal of Experimental Psychology, 46*(4), 743–758.

Wiener, N. (1953). *Ex-prodigy: My childhood and youth.* Cambridge, MA: MIT Press.

Wilson, G. D., & Jackson, C. (1994). The personality of physicists. *Personality and Individual Differences, 16,* 187–189.

Wise, L. L., Steel, L., & McDonald, C. (1979). *Origins and career consequences of sex differences in high school mathematics achievement.* Washington, DC: American Institute for Research.

Wiser, M., & Carey, S. (1983). When heat and temperature were one. In D. Gentner & A. L. Stevens (Eds.), *Mental models* (pp. 267–297). Hillsdale, NJ: Lawrence Erlbaum Associates.

Wispe, L. G. (1963). Traits of eminent American psychologists. *Science, 141,* 1256–1261.

Zachar, P., & Leong, F. T. L. (1992). A problem of personality: Scientist and practitioner differences in psychology. *Journal of Personality, 60,* 667–677.

Zuckerman, H. (1977). *Scientific elite.* New York: Free Press.

Zuckerman, H., & Cole, J. R. (1975). Women in American science, *Minerva, 13,* 82–102.

Zuckerman, H., & Merton, R. K. (1972). Age, aging, and age structure. In M. W. Riley, M. Johnson, & A. Foner (Eds.), *Aging and Society, Vol. 3.* (292–356). New York: Russell Sage Foundation.

Zuckerman, M. (1994). *Behavioral expressions and biosocial bases of sensation seeking.* New York: Cambridge University Press.

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