1 The chance-configuration theory

The psychology of science

With the launching of the Soviet sputnik into space, American psychologists were alerted to the urgency of enlarging our understanding of scientific creativity. J. P. Guilford, in his 1950 Presidential Address before the American Psychological Association, had already called for closer attention to the study of creativity, but current events injected this need with more significance (Golovin 1963). About this time the National Science Foundation sponsored a series of conferences, “The Identification of Creative Scientific Talent,” at the University of Utah, the central papers of which were published in the 1963 volume Scientific Creativity: Its Recognition and Development, edited by Calvin W. Taylor and Frank Barron. We thus had every reason to believe that the discipline was on the threshold of a respectable “psychology of science,” the first comprehensive science of science (see also Maslow 1966; Stevens 1939). But matters progressed little further, and the concerted effort largely paled out within a decade (Singer 1971). By the time that the psychology of science had, for all practical purposes, vanished as a distinct field of inquiry, the sociology of science had taken wing as a scholarly enterprise, joining the already high-flying disciplines of the philosophy of science and the history of science. There were accordingly three “metasciences” dedicated to the scholarly examination of science, two of these humanistic and only one scientific in analytical emphasis—with psychology patently excluded (see Houts 1988). This is not to say that psychologists ignored the subject altogether but only that any efforts were sporadic, inconsequential, or noncumulative (Fisch 1977). Many psychological studies were oriented more toward idiographic case studies than toward the abstraction of nomothetic principles that govern scientific discovery and invention (cf. Simonton 1983c). General laws were applied to specific instances rather than adducing those generalizations from multiple particulars. Howard Gruber’s Darwin on Man (1974) may illustrate this approach at its best.

Nonetheless, in the past few years several psychologists have come to the realization, however delayed, that when opportunity had knocked at the
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door, the discipline was found asleep. Psychological processes permeate all scientific activities, and this is particularly true in regard to creativity and problem solving. The recent coming of age of the cognitive sciences perhaps accelerated the dawning awareness that psychology may have something to contribute beyond what had already been offered by sociology, philosophy, and the history of science (see Faust 1984). In any event, psychologists have again received invitations to attend conferences devoted to the examination of science, especially the study of scientific creativity. Articles by psychologists have become more common in professional journals, including the interdisciplinary journals specializing in science studies, such as *Scientometrics* and *Social Studies of Science*. And books are once again concentrating on the psychological aspects of science (e.g., Faust 1984; Gholsen, Houts, Neimeyer, & Shadish 1988; Jackson & Rushton 1987; Mansfield & Busse 1981; Tweney, Doherty, & Mynatt 1981). It is always dangerous to engage in prophecy, yet it seems that this growing movement may constitute a renaissance of that very psychology of science whose development was arrested two decades ago. At this moment in this miniature narrative, I as an investigator enter the story.

Over the past dozen years or so I have been engaged in research on exceptional personal influence. That is, I have been interested in determining why certain individuals have an inordinate and enduring impact on others in a given domain of achievement. For the most part, although not exclusively, this compelling interest has taken the form of historiometric studies of “geniuses”—of eminent creators and leaders—with much of this work focusing on scientific creativity. Some of the greatest geniuses in science—like Aristotle, Newton, and Einstein—exerted a tremendous and long-term influence not only on their scientific colleagues but on the general intellectual community besides. I have endeavored to understand the personal and social basis for such monumental impact, concentrating especially on the connection between age and achievement, the consequences of political conditions, and the role of the zeitgeist in the generation and acceptance of discoveries and inventions.

In addition to my own empirical and theoretical labors, I have tried to keep abreast of the vast literature on genius, in general, and scientific creativity, in particular. During this research and reading, I have spotted what I consider a consistent theme pervading the phenomenon of outstanding scientific discovery and invention. This theme expanded first into some empirical hypotheses and now can be developed into a full-fledged psychological theory. I style this explanatory and predictive framework the chance-configuration theory. This conception, I maintain, facilitates both the organi-

zation of past research findings and the formation of new research hypotheses regarding scientific creativity.

I shall begin by sketching the chief tenets of the chance-configuration theory. This chapter will conclude by outlining what I consider to be the theory’s explanatory scope. The remainder of this book is largely devoted to an empirical development of the basic ideas presented in this chapter—their enlargement into a comprehensive interpretation of exceptional scientific creativity.

The theory

At the most superficial level, there is little if anything original about the chance-configuration theory. I have always been impressed with Darwin’s theory of evolution by natural selection and have often been fascinated with attempts to apply Darwinian ideas to innovation and sociocultural change. In particular, my own theoretical outlook can be said to have roots in Donald Campbell’s (1960) blind-variation and selective-retention model of creative thought. To some degree, the current theory is an elaboration, albeit with a shift in nomenclature, of Campbell’s ideas—ideas that were recently identified as holding “promise as a possible integrative framework for the psychology of science” (Tweney et al. 1980, p. 405). I shall outline those aspects of Campbell’s model that I find most useful before I present my own rendition.

Campbell’s scheme purports to be rather general, applicable to virtually any variety of knowledge acquisition or environmental adaptation, including biological evolution by natural selection, trial-and-error learning, creative thought, and social evolution (Campbell 1960, 1965). Furthermore, the model has provided the basis for his “evolutionary epistemology” (Campbell 1974a), a descriptive theory of knowledge that has certain affinities with Karl Popper’s philosophy of science (see Schlipp 1974). Although I am in essential sympathy with all of these developments, we need to discuss only that portion of Campbell’s thinking that deals specifically with the creative process and the growth of scientific knowledge. For our purposes, then, Campbell’s position may be summarized as the following three core propositions:

1. The acquisition of new knowledge, the solution of novel problems, requires some means of producing variation. Campbell argues that this variation, to be truly effective, must be fully blind. To count as “blind” the variations must be unrelated to the environmental conditions, including the specific problem, under which the variations are generated, and the varia-
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tions should be unrelated to one another (i.e., feedback from the failure of one variation is not used to formulate the next variation in a series of trials) (Campbell 1960). To the extent that the variations are shaped by the environment, whether past or present, they cannot be considered blind. Of course, many alternative qualifiers might be placed on the variations, such as chance, random, aleatory, fortuitous, and haphazard (Campbell 1974b, p. 147), but Campbell preferred the designation blind, for it retains the notion that the variations do not use any information already given, while at the same time it does not commit the variations to a particular generation mechanism. However, within the specific confines of creativity, I prefer the adjective chance, as will become evident later in this chapter.

2. These heterogeneous variations are subjected to a consistent selection process that winnows out all but those that exhibit adaptive fit (Campbell 1960). In other words, there must exist somewhat stable criteria by which those variations that offer viable solutions to the problem at hand are separated from those that embody no advance and hence are useless. In Darwinian evolution just such a selection procedure is the cornerstone of the theory: Natural selection chooses those genetic variations (whether chance mutations or random assortments of genes) that favor the fit between organism and environment. In scientific discovery, too, variations are judged against a set of criteria; those variations that fail to meet these requirements are weeded out from the body of scientific knowledge.

3. The variations that have been selected must be preserved and reproduced by some mechanism; without such retention a successful variation cannot represent a permanent contribution to adaptive fitness. The chromosomes retain fit variations in biological evolution; memory preserves knowledge acquired through learning; and cultural transmission through socialization and education saves valuable customs and techniques in sociocultural evolution.

Campbell noted the fundamental contradiction between the first and third propositions: Blind variation implies a departure from retained knowledge. A genetic mutation is a shot in the dark that ignores the wisdom contained in parental chromosomes, and thus mutant genes are often lethal; an excessive mutation rate would spell the extinction of a species. At the same time, however, a gene pool totally lacking in variation would be unable to adapt to changing circumstances, with consequences just as fatal to the species' survival; in time the genetically encoded wisdom would convert to foolishness. A comparable process operates on the level of the creative process. Any society has a rich repertoire of skills and concepts that enable its members to survive and prosper, and accordingly the cross-generational preservation and transmission of these adaptive features are a high priority. But without any provision for variation, for creativity, the sociocultural system will eventually stagnate, lose adaptive advantages, and in the end be defeated in the competition with rival systems. In a sense, there is an intrinsic contradiction between preserving the fruits of past creative acts and sowing the seeds for future creative achievements. In regard to scientific creativity, Thomas Kuhn (1963, p. 343) referred to this conflict as an "essential tension," for "very often the successful scientist must simultaneously display the characteristics of the traditionalist and of the iconoclast."

Frequently the solution to this conflict is to place restrictions on the variations, limitations that use a priori or a posteriori information. Most biological variation is limited to recombinations of genes of proven environmental utility, and even then not all combinations are permitted. In trial-and-error learning, not all potential behavior patterns are attempted but, rather, merely a subset that has proved itself useful in the past experience of the species and the individual. Cultural variations, too, are normally not allowed to run rampant; certain types of behavioral combinations, in fact, are outright proscribed as criminal or insane. Consequently, many variations display some "insight" into narrowing the possible trials from the near infinity of conceivable alterations. In any event, in chapter 5 we shall see that the essential tension between variation and retention helps explain why success as a scientist is so often a curvilinear, concave-downward function of key developmental variables. Creative development requires a well-adjusted trade-off between the traditionalist and iconoclast dispositions (Simonton 1987a,c).

Campbell (1960) was willing to admit that his model had been anticipated by many thinkers before him, the latter half of the 19th century being particularly resplendent with philosophers who felt the influence of Darwin's revolutionary ideas. He cited, among many examples, the 1880 essay, "Great Men, Great Thoughts, and the Environment," by William James, which emphatically states that "the relation of the visible environment to the great man is in the main exactly what it is to the 'variation' in the Darwinian philosophy" (p. 445). In particular,

the new conceptions, emotions, and active tendencies which evolve are originally produced in the shape of random images, fancies, accidental outbursts of spontaneous variation in the functional activity of the excessively unstable human brain, which the outer environment simply confirms or refutes, adopts or rejects, preserves or destroys—selects, in short, just as it selects morphological and social variations due to molecular accidents of an analogous sort. (p. 456)

Even though these quotations require qualification to be palatable to modern ears, they do illustrate how Darwinian ideas might be extrapolated to
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creative behavior. Indeed, if anything, the analogy between biological and other forms of evolution or development has been treated too often and taken too seriously over the past century or so—from Herbert Spencer to the present day.

I consequently should emphasize that I do not wish to draw detailed correspondences among various knowledge- acquisition processes. There are many ways that the analogy between biological and sociocultural evolution breaks down, and human information processing, which constitutes a form of individual development, has its own characteristics as well (Campbell 1965, 1986). Even so, the three components of variation, selection, and retention unite all varieties of knowledge acquisition under a single generic form. The chance-configuration theory offered in the following sections clearly falls into this broad class, too. The key ideas of this theory are (1) the chance permutation of mental elements, (2) the formation of configurations, and (3) the communication, social acceptance, and sociocultural preservation of those configurations. It will become evident that I am here offering a truly social-psychological theory of scientific creativity, one that emphasizes both intrapsychic events taking place solely within the individual and interpsychic or interpersonal events depending on social communication and interaction.

**Chance permutations**

We shall begin with the assumption that the creative process entails operations on what I choose to call mental elements. These psychological entities are the fundamental units that can be manipulated in some manner, such as the sensations that we decide to attend to, the emotions that we experience, and the diverse cognitive schemata, ideas, concepts, or recollections that we can retrieve from long-term memory. In scientific creativity, the predominant mental elements are cognitions of some kind, such as facts, principles, relations, rules, laws, formulae, and images. Yet immediate sensations may also play a role in laboratory experimentation and field exploration, and feelings may figure in scientific thought and discourse as well (Mahoney 1976). Sometimes these mental elements can be evoked voluntarily (e.g., the deliberate retrieval of a stored fact from memory); at other times these elements enter mental processing involuntarily (e.g., via a conditioned emotional association). Moreover, these mental elements do not have to be fully conscious, but rather, many enter information processing at the periphery of consciousness. As Einstein observed, what we “call full consciousness is a limit case which can never be fully accomplished” because of the inherent “narrowness of consciousness” (quoted in Hadamard 1945, p. 143).

Whether voluntary or involuntary, conscious or unconscious, these mental elements must be free to enter into various combinations. In fact, according to the theory proposed here, the fundamental generating mechanism in scientific creativity involves the chance permutation of these elements. To clarify what I mean, let me start with the term permutation. I favor this term over the alternative more often employed, namely, combination. In probability theory, combinations are sets of elements that have no particular order, whereas for permutations the elements’ order in the sets is critical to distinguishing among sets. In actual applications, the combinations are frequently more interesting than are the permutations. When calculating the odds of being dealt a royal flush in a card game, for example, the order in which one acquires the ace, king, queen, jack, and ten is immaterial to the chances of obtaining a winning hand. Nonetheless, in other applications the specific order of the elements is crucial, requiring that any given generic combination be separated into its specific permutations. As a case in point, “a mathematical demonstration is not a simple juxtaposition of syllogisms, it is syllogisms placed in a certain order, and the order in which these elements are placed is much more important than the elements themselves” (Poincaré 1921, p. 385). This distinction will become useful later in chapter 6 when we discuss the phenomenon of multiple discovery. Consequently, the term permutation is retained insofar as it connotes that we must discriminate among combinations that, although containing identical elements, differ in how those elements are arranged. This usage permits us to say that a combination can form two or more permutations with the same elements but with the elements assigned distinctive levels of importance or emphasis within each permutation.

The hard part is to define chance. In general, to claim that the permutations are generated by chance is equivalent to saying that each mental element is evoked by a myriad determinants, there being virtually no overlap in the determinants for any pair of elements defining a given permutation. Chance, after all, is a measure of ignorance, a gauge of the situation in which the number of causes is so immense as to defy identification. Though chance implies unpredictability, it does not necessitate total randomness. We do not need to argue that all permutations of a specific set of elements are equiprobable, in contrast with Mendelian genetics. We must merely insist that a large number of potential permutations exist, all with comparably low but nonzero probabilities. Later in chapter 3, when I relate the theory to the cognitive style of creative persons, I shall describe how chance permutations
can come about, at that time drawing on a model originally proposed to explain intuitive thought processes (Simonton 1980a).

**Configuration formation**

At this point we have evidently postulated a process that yields variations. We next must introduce some principle of selection into the theory, for not all chance permutations can or should be retained. In the case of scientific creativity, selection mechanisms operate on both personal and social levels. At this stage in the argument let us focus on one personal, or intrapsychic, criterion: Here we propose that the primary selection procedure is predicated on the fact that chance permutations vary appreciably in stability. On one extreme are transitory juxtapositions of mental elements that lack sufficient coherence to form a stable permutation, so that the permutation process usually continues with little or no pause. These unstable permutations we may call mental aggregates. On the other extreme are permutations whose elements, though brought together by a chance confluence of multiple determinants, seem to hang together in a stable arrangement or patterned whole of interrelated parts. These stable permutations I label configurations. It must be stressed that aggregates and configurations are permutations of mental elements that fall along a continuum from the highly unstable to the highly stable, with many gradations between. Nonetheless, we assume that of the innumerable chance permutations, only the most stable are retained for further information processing, for the greater the stability is, the higher the probability of selection will be. Further, on a subjective plane, the more stable a permutation is, the more attention it will command in consciousness, as the unstable permutations are too fleeting to rise often above unconscious levels of processing. Thus, configurations of elements are selected out from the permutations to be saved for further conscious deliberation.

The crucial requirement, then, is to define configuration. I chose this word advisedly, over the many possible alternatives (schemata, associative fields, constructs, concepts, ideas, matrices, etc.), based on its etymology and common applications. The root of configuration is a Latin word meaning "to shape after some pattern." A configuration is thus a conformation or structural arrangement of entities and implies that the relative disposition of these entities is central to the configuration's identity. In chemistry and physics the relative spatial location of atoms in a molecule is often called a configuration. Likewise in astronomy the characteristic grouping of heavenly bodies is sometimes referred to as a configuration. Finally, in psychology and, most particularly, in Gestalt theory, a configuration is a collection of sensations, emotions, motor patterns, and concepts organized in such fashion that the collection operates as a unit in thought and behavior. Indeed, if a configuration becomes sufficiently refined, it can become a new mental element that can enter into further permutations. That is, if the diverse elements that make up the configuration become strongly connected, they all will become "chunked" so that they function as a single element, taking up less space in limited attention. This process of consolidation is analogous to that when atoms forming a molecule become subordinate to that molecule, which then operates as a unit in physical transformations (e.g., Avogadro's number applies to molecules of gas, not to the separate atoms).

It may seem contradictory to assert that mental elements thrown together by happenstance can unite in a way that prevents disintegration, but we must recognize that what jumbles the elements together is different from what glues them together. The elements themselves contain properties that will determine how well they fit together. The intrinsic attributes of one element may dovetail nicely with other elements, creating a stable unit. Hence, even if two elements are tossed together by haphazard juxtaposition, those elements may stick together because of mutually compatible properties. This event is analogous to a chance encounter that brings two people together who then form a lasting relationship on the basis of similar and complementary interests and values. Or to offer an analogy from chemistry, the hundred or so chemical elements each have characteristics, principally valence, that decide how they will behave in chemical reactions. For example, an atom of sodium tends to give up an electron in order to acquire a complete outer electron shell, whereas chlorine, because it lacks only one electron to finish out its outer shell, tends to take up an electron. Thus, sodium and chlorine atoms are intrinsically compatible elements, the former yielding an electron to the latter so that both can form a stable "molecule" of sodium chloride (NaCl). Therefore, the random impact of gaseous chlorine on solid sodium will corrode the metal into sodium chloride. On the other hand, helium, which already possesses a full outer shell and thus is placed in the column of inert elements on the periodic table, will not combine with either sodium or chlorine, no matter how many random impacts are permitted between the molecules.

Because certain elements have intrinsic affinities for each other, not only can a chance linkage of two elements produce a stable pairing, but large clusters of elements also can spontaneously form highly ordered arrangements out of chaos. Campbell (1974b) offered a striking example of crystal formation, in which under the proper conditions, a dissolved chemical will not precipitate into merely amorphous aggregates but, rather, fine crystals.
A specific crystalline structure is implicit in the ions or molecules leaving solution, and so a more organized spatial pattern is actually more stable than is one less organized, yielding a specific configuration from the mere random collisions of the ions or molecules.

To be sure, this last example is much simpler than what occurs in intellectual matters. It is not always apparent when two distinct mental elements contain a natural affinity, nor is it obvious how these affinities might lead to larger structures, or configurations. At least, such ideational compatibilities are less apparent on a priori grounds and are, rather, discerned retrospectively, on a post hoc basis. For instance, a number of studies have illustrated how specific combinations of philosophical beliefs have been more prone to appear than have others in Western civilization (Simonton 1976c,f). Nominalism, as a case in point, is more likely to be associated with empiricism, mechanistic determinism, and the doctrine of incessant change than with mysticism, monistic idealism, and eternalism, just as hedonistic and utilitarian ethics display a stronger a posteriori linkage with mechanistic materialism, nominalism, and extreme individualism than with monistic idealism, realism, and universalism or statism. Even if some were to argue that these affinities could be justified a priori (see, e.g., Sorokin 1937–1941), such arguments would be precarious at best. As is well known, Kant erred in holding that Euclidean geometry was true a priori, a belief dispelled with the advent of perfectly consistent non-Euclidean geometries that later became the foundation for Einstein’s treatment of space in his general relativity theory.

Configuration acquisition. To appreciate how chance permutations may generate stable collections of elements, we first must note that very few configurations arise in this way. On the contrary, most configurations consist of mental elements that have been connected on either empirical or logical grounds. In particular, these mental givens that provide the material for chance configurations are of two types, a posteriori and a priori configurations (cf. Stevens 1939).

A posteriori configurations establish a correspondence between perceived events and their cognitive representations. If, for example, we have a set of world events $A_1, A_2, \ldots, A_n$ represented by a set of mental elements $A'_1, A'_2, \ldots, A'_n$ and if, in reality, the conditional probability of any one event given any one of the others is much greater than zero, so that $p(A_i/A_j) > 0$ for all $i \neq j$, we can expect the mental elements to be ordered so that the subjective association strengths approximate the objective conditional probabilities (e.g., the rank order of conditional probabilities positively correlates with the rank order of association strengths). That is, in some manner a posteriori configurations are internal images of the world, mental expectations matching the observed cooccurrences of events. There is no need to specify how this fitting of configurations to environmental probabilities is accomplished. Pavlovian (classical) and Skinnerian (operant) conditioning certainly contribute, but cognitive and social learning theories may apply in equal or greater force. For instance, in Piaget’s developmental theory, beginning in the sensorimotor phase, the child constructs schemata that relate sensations and actions into a coherent view of the world, eventually acquiring concepts of objects, causal expectations, and other useful configurations that symbolize how the world works and how the child can work on the world. However, we shall show in chapter 5, which discusses developmental antecedents of creativity, that certain ways of acquiring associations are more conducive to the emergence of an efficacious chance-permutation mechanism.

Unlike a posteriori configurations, which derive from experience, a priori configurations emerge from given conventions. These conventions define a set of mental elements and the rules by which these elements can be combined into a proper order. In arithmetic, algebra, and other forms of mathematics, for instance, the members of a given tradition are provided with rules for the correct manipulation of numbers and abstract symbols, enabling a practitioner to perform long division, solve an equation to express an unknown quantity in terms of known quantities, and to accomplish like tasks. Logic, too, regulates how specific verbal propositions can be combined so that we can detect when a set of statements is consistent or inconsistent. It is, in fact, characteristic of a priori configurations that decisions of rightness or wrongness, truth or falsity, are absolute within a given body of rules; a number either is or is not the square root of another number, and a function either is or is not the integral of a given function. The adequacy of a posteriori configurations, in contrast, is decided on probabilistic grounds, based on the degree of congruence between observation and cognitive representation. This absolute, right-or-wrong nature of a priori configurations is seen not just in mathematics and logic but in linguistic conventions as well. Each language contains rules, such as grammars, that define how phonemes combine into morphemes, how morphemes form words, and how words are arranged into sentences.

When I call such configurations a priori, I do not consider them anything more than mere conventions. Although some rules may represent innate ways of organizing thoughts founded on the evolutionary history of the species, most are likely derived instead from cultural evolution (cf. Campbell 1974b). Hence, even if children initially evolve their own grammars based on an instinctive coordination of linguistic material, eventually
all children end up with the grammars that characterize their particular language community. Likewise, logic and mathematics may overlay primitive, species-specific givens regarding the appropriate approach to structuring cognitions according to culturally provided principles of information-processing etiquette (see also Campbell 1986, 1987).

A priori configurations have one valuable asset relative to a posteriori configurations, namely, that mental elements are combined without any recourse to empirical experience. Even a child can generate sentences never before heard; a logician can produce propositions from a set of premises that are not immediately apparent; and a mathematician can derive equations that are merely implicit in a set of axioms (that may or may not constitute abstractions from experience). Thus in a sense, a priori configurations can be truly "inventive," just as we can claim that chance configurations are fully "imaginative." Even so, the inventiveness of a priori configurations is less extensive than is the imaginativeness of chance configurations. The former is bounded by the conventions that define the acceptable repertoire of elements and rules of permutation, whereas the latter is virtually unlimited, for chance can place in juxtaposition virtually any element, without regard to logic.

We observed earlier that configurations may be "consolidated"; that is, the elements can be so compacted that the configuration as a whole can function as a single unit in mental manipulations. This can occur for a posteriori and a priori configurations as well as for chance configurations. In the case of a posteriori configurations, certain events cooccur so frequently that they become definitive elements of a particular concept (e.g., birds as feathered egg layers). For a priori configurations, particular operations may be refined so that what once was a complicated procedure becomes conveniently simple. For instance, by using algebraic rules and the concept of limits, one can demonstrate how various functions are differentiated, yielding a new set of rules that allow a mathematician to circumvent the original derivations (e.g., except in introductory calculus courses, no one uses limits, with the δε nicety, to take the first derivative of trigonometric, logarithmic/exponential, polynomial, and other standard functions). Once a priori and a posteriori configurations become consolidated sufficiently so that they can be manipulated as mental elements, they can enter the process by which chance permutations emerge. (However, if the configurations are too refined, they will be less able to participate in chance variations, for reasons that will become evident in chapter 3.) Hence, the fortuitous union of two or more established configurations can form a new configuration, a configuration that may later become an element in another configuration still.

**Self-organization.** We can now state why some chance permutations are more stable than others are. Sometimes two configurations, of whatever origin, will have similar structures, and so the elements can line up more or less one on one. In other words, an approximate one-to-one correspondence can be fixed between the two configurations so that the elements of one set are mapped onto the elements of the other set. One important and commonplace example of this matching of elements across configurations is one chance permutation's producing an analogy between two hitherto unrelated phenomena (see Poze 1983; cf. Sternberg 1977). When Huygens, Young, Fresnel, or Maxwell saw that light behaved as a wave phenomenon, they established equivalences between two a posteriori configurations, one encoding the diverse behaviors of light in experimental studies, and the other the known behaviors of waves. Thus, different colors correspond to distinct wavelengths, color intensity to wave amplitude, and complex hues to mixtures of various wavelengths and amplitudes. Reflection, refraction, diffraction, and interference are effects with counterparts in both light and wave phenomena. Because so many characteristics of light map successfully onto wave attributes, the junction of these two configurations itself forms a stable configuration (in classical physics).

A second example is the pairing of an a posteriori configuration with a configuration derived a priori. Sometimes a particular mathematical formula will describe relationships between two or more abstract variables that fit the observed relationships between a similar number of concrete variables. The Balmer series illustrates this sort of isomorphism: By means of induction, Balmer found a relatively simple equation that could predict the location of the spectral lines of hydrogen, in which the wavelengths are expressed as a function of integers. We do not know exactly how Balmer arrived at his equation, but some sort of trial-and-error process was involved. Not until Bohr introduced his quantum theory of the atom did the Balmer series receive a theoretical justification (i.e., was subsumed under a larger a priori configuration).

What is gained by merging two or more configurations? One answer is that one configuration is frequently better consolidated than the other is, which is less well defined. A good model of light, for instance, allows us to connect a range of phenomena that before would be deemed isolated facts. We not only link together the various light phenomena but also place the visual spectrum within a single, unidimensional scale (wavelength) that stretches from X-rays and ultraviolet to infrared and radio waves, all with the same set of definitive behaviors (reflection, refraction, diffraction, etc.). Moreover, once we consider light as a wave rather than a particle phenomenon, we are led to specific expectations that, if verified, will ex-
pand our knowledge of light (e.g., whether the velocity of light increases or decreases when passing from air to water). Thus, a tight analogy or model permits us to know more about the world with less work (see also Gentner & Gentner 1983; Kochen & Lansing 1985). The same gain in informational efficiency is even seen in the application of an arbitrary formula to empirical data. It takes less mental space to remember Balmer’s equation than it does to memorize all the wavelengths appearing in the hydrogen spectrum. More generally, the integration of configurations makes us think more economically, for the number of unrelated elements with which we must cope is dramatically reduced. Even if the correspondences are not perfect, there may be a net economical gain if the elements neatly packaged outnumber the elements left unaccounted for. For example, in English we have the orthographic rule “i before e except after c or when sounded as a as in neighbor and weigh.” It is easier to recall this verse and the rare exceptions, such as weird, than to memorize the spelling of all words with a paired e and i.

The gain in information-processing efficiency becomes especially apparent when configurations are united hierarchically, that is, when mental elements are ordered for the optimal retrieval of facts and the anticipation of events. This asset is perhaps most conspicuous in biological taxonomy, in which each living form is assigned to a kingdom, phylum, class, order, family, genus, and species. Knowing how a particular species falls in each category immediately produces relevant data about morphology, physiology, and behavior. Likewise in the case of classical physics, many phenomena can be grouped into particle and wave events, wave phenomena into transverse and longitudinal waves, and so forth. We thus postulate that the human intellect is programmed to self-organize its contents into hierarchical structures in which knowledge is most efficaciously distributed. On the plane of subjective experience, we might even posit that the mind receives subjective rewards, or pleasure, from noticeable enhancements in cognitive order, in which pleasure is merely the marking of an adaptive event, as in the sweetness of carbohydrates. In other words, cognitive events that reduce mental “entropy,” by combining configurations into more comprehensive hierarchical formations, receive intrinsic reinforcement. These subjective rewards or gratifications may be identified with the “peak experiences” characteristic of Maslow’s (1962) “self-actualizers.” Conversely, stimuli, whether facts or ideas, that appear to disrupt or challenge the present order are perceived as obnoxious, provoking defensive displeasure and rejection. Yet however adaptive and pleasurable intellectual organization may be, configurations cannot be ordered into higher-order configurations unless they are sufficiently consolidated. Otherwise each component of a hierar-

The theory of self-organization, which is here presumed to drive the creative process in great science, is not equivalent to the dispassionate quest for truth that is so often thought to energize the scientific enterprise. Epistemologists are fond of abstract schemes by which knowledge propositions can be discerned and demonstrated. Although some scientists have indeed claimed to have been guided by such prescriptions—Charles Darwin gave lip service in his notebooks to Baconian induction (Gruber 1974), and the Nobel laureate John Eccles (1975) maintained that he successfully applied Popperian falsification—the bulk of the evidence suggests that few scientists are so well behaved. On the contrary, a provocative research literature has accumulated on the “confirmation bias” in human reasoning (e.g., Mynatt, Doherty, & Tweney 1977; Wason 1968). When faced with having to infer the principle underlying a sequence of events, people are not inclined to seek out information that contradicts a favored hypothesis but, rather, to gather the most confirmatory evidence that the circumstances will allow. Even when disconfirmatory data unexpectedly appear, they often are ignored as a minor exception to the accepted rule. Not only is there reason to believe that practicing scientists betray this confirmatory bias (Mahoney 1976), but those scientists most admired by their colleagues may also be those most dedicated to proving a cherished hypothesis or theory (Mitroff 1974). The notion of the disinterested investigator calmly committed to pure induction and falsification is a myth (see also Faust 1984).

Self-organization motivates the mind, whenever possible, to assimilate unpleasant facts into the present cognitive order, for to give up one’s intellectual framework willy-nilly simply to accommodate the confusion of random events is to risk expanding psychological disorder, with a corresponding loss in behavioral adaptiveness (Campbell 1986). Indeed, notwithstanding his advocacy of doubt as the foundation of philosophy, Descartes admitted that skepticism should not be extended to practical affairs. And judging by the confirmatory bias he exhibited in grounding his theories on a priori rather than a posteriori principles, Descartes apparently limited the application of doubt in the sphere of scientific practice. Thus, scientists, or at least the greatest among them, appear to be more oriented toward the discovery of regularity, structure, order, harmony, or, in a word, beauty (see, e.g., Dirac 1963; Einstein 1933; Hardy 1940; Poincaré 1921; Wechsler 1978). Although not repudiating the value of truth, creative scientists apparently operate according to the “psychologie” voiced by Keats in The Ode on a Grecian Urn that “Beauty is truth, truth beauty,—that is all! Ye
know on earth, and all ye need to know." This intuitive equivalence will be documented in later chapters.

To summarize, we assume that human information processing is designed to select those chance permutations that significantly augment the efficiency of thought. The stable permutations, or configurations, can then be further consolidated, eventually forming a hierarchy. This process reflects "a fundamental tendency for the stable to replace the unstable" (Ashby 1952, p. vi) in an apparent (but only superficial) violation of the second law of thermodynamics (see also Prigogine & Stengers 1984).

Communication and acceptance

Although we now have the rudiments of a chance-permutation and selective-retention theory, scientific creativity cannot be fully explained without adding three more variation-selection processes. The first is also intrapsychic, but the remaining two are social. Because these represent a temporal sequence of selection processes that occur after the primary creative act, let us outline them in order.

Communication configurations. Once a configuration has been isolated and proved useful in structuring cognition, that discovery will remain an article of personal knowledge only until it is successfully expressed to others. Occasionally the stable permutation is already in suitable form, for the elements being varied may have been linguistic or mathematical. But far more often, further intellectual effort is demanded to articulate the discovery or invention. For example, if the new configuration is a visual image or model of a process, such as a chemical reaction or molecular structure, then that configuration must be linked with a verbal description in which a correspondence is set up between image referent and verbal symbol. In some disciplines, a coherent logical pattern must be imposed as well, especially the translation of the initial idea into mathematical forms. Finally, the sciences have formats for the accepted presentation of findings. These journal styles are arbitrary and artificial inasmuch as they no longer correspond to the simple narration of how a finding came about, as was common for scientific papers published centuries ago (e.g., in Philosophical Transactions).

Hence, this conversion of a configuration from the ineffable to the articulate yields an often more complex communication configuration. On most occasions this conversion process is relatively straightforward, conscious, and deliberate, but at times the isolation of an appropriate vehicle of expression itself mandates a creative synthesis, sometimes even to the point of having to scan through another series of chance permutations. Moreover, when the conversion cannot be perfunctory, the particular communication configuration arrived at may be so arbitrary that something is lost in the translation. A one-to-one correspondence is lacking between the original and articulated configurations, so that one cannot be completely mapped onto the other. Such misfits are especially likely when the structure of the language, logic, or mathematics used fails to parallel that of the new stable permutation. In the case of linguistic articulation, the Whorf-Sapir hypothesis that language determines thought may operate here, albeit deflecting more the communication of knowledge than the knowledge acquisition per se (cf. Whorf 1956). And for mathematical expression, a scientist is sometimes compelled to devise a novel mathematics in order to convey an original thought, as did Newton, Fourier, and Hamilton.

Interpersonal influence. If a workable communication configuration has been devised, it can be made available to other scientists via the standard vehicles of scientific exchange, most commonly an article in a technical journal or a patent application. At this point the selection mechanisms cease to be merely intrapsychic, becoming fully social instead. Specifically, the articulated discovery or invention must succeed in the domain of interpersonal influence; that is, it must be accepted by colleagues in the same discipline. Referees must agree that the offered idea represents an advance worthy of publication, and readers of the published article or monograph must be able to use the communicated configuration to restructure their own thinking habits in approximately the same manner as did the originator. Social acceptance signifies that a community of scientists have found the suggested configuration to be valuable to their own personal endeavors toward self-organization, toward augmented cognitive effectiveness — whether the idea be a key finding that helps fill in a puzzling gap in knowledge, an original technique or instrument that allows variables and relationships to be assessed that hitherto escaped scrutiny, or a novel revolutionary theory that mandates the remaking of thought. In the case of inventions, the offered creation must enable potential users to structure more effectively subsequent planned actions, thereby enhancing the organization of practical behaviors. Several requirements must be met before the creation can achieve acceptance, four perhaps standing out.

1. Each member of the community must have available a similar repertoire of mental elements, such as a shared body of facts, methods, and questions. This shared foundation may include "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" (Kuhn 1970, p. viii).
2. Those mental elements must be in comparative disarray in the minds of potential acceptors, so that there is a need for a more efficient approach to structuring information. Darwin noted in the last chapter of his *Origin of Species* (1860, p. 240): “Although I am fully convinced of the truth of the views given in this volume . . . I by no means expect to convince experienced naturalists whose minds are stocked with a multitude of facts all viewed, during a long course of years, from a point of view directly opposite to mine.” Hence, Kuhn (1970, p. 53) observed how the accumulation of anomalies—findings that cannot be assimilated into a given scientific framework, tradition, or paradigm—prepares the way for scientific revolution. “Discovery begins with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science.”

3. There must be a consensus on the meaning of the linguistic, logical, and mathematical elements making up the communication configuration. This consensus enables each member of the community to reconstruct the original configuration from its social representation. From this requirement arises the difficulties encountered by those compelled to devise a totally new language or nomenclature, even logic, to articulate an original idea.

4. The originator must have successfully translated and communicated the initial conception so as to facilitate the requisite reverse translation by fellow scientists. Anthony Hope may have held, in his *Dolly Dialogues*, that “unless one is a genius, it is best to aim at being intelligible,” yet even a genius, and certainly a scientific one, must aspire to the same goal. The capacity to exert personal influence over others is undermined by unintelligibility, for people are seldom persuaded by those who insist on talking over their heads (Simonton 1985a). Galois and Heaviside are just two of numerous scientists whose ideas were not immediately comprehensible to potential colleagues, thus substantially delaying the time that they could effectively contribute to scientific advance. Thus, even if the idea is couched in understandable terms, the communication configuration must assume a proper form. For instance, in his *On the Nature of Things*, Lucretius could convey scientific concepts in verse; but long before Erasmus Darwin made a similar endeavor when announcing his evolutionary theory, prose, not poetry, became the only acceptable manner of presenting scientific ideas. Also, scientists who dare to publish their thoughts in languages outside the lingua franca of scientific exchange suffer comparable consequences. A final aspect of this fourth requirement entails choosing the best vehicle for publishing theoretical or empirical results. It is no accident that competent scientists today select their journals quite deliberately: Unless their work reaches the best audience for their ideas, the odds of leaving an imprint on science will be small indeed (see Gordon 1984).

These four requirements assume that the innovator’s intellectual constitution is somewhat closer to that of the traditionalist than the iconoclast. That is, the scientific genius must to some extent be representative of others in the same discipline. Both creators and potential acceptors must share a set of mental elements, which are in comparable states of disassembly, and agree on the referents and rules belonging to particular expressive symbols (e.g., nomenclatures and standards of proof or evidence). To the extent that scientists do not represent their colleagues on these matters, their personal impact will diminish. The history of science is replete with instances of great minds who failed to leave an imprint on their times, owing to their failure to put together a passable communication configuration, however correct and significant their ideas were. Indeed, as we shall point out in chapter 6, this lapse in communication is one of the chief causes of multiple discovery and invention. Nonetheless, as long as some communication configuration has been offered, even if only in the guise of an unpublished manuscript or working model, the proffered ideas may be acceptable to a future community that has caught up with the forerunner.

**Sociocultural preservation.** In simple terms, the magnitude of social acceptance is registered by a poll of scientific colleagues. The more fellow scientists who use the communicated configuration to reorganize their thinking, the more adherents the innovation can claim. If almost all scientists in a discipline (or, at least, all productive or influential ones) adopt the new way of looking at experience, then this selection process can be said to be maximally complete. Though this victory is desirable, in revolutionary results or concepts often only a subset of the potential population of acceptors is convinced, in which case the innovation may merely add a new school rather than supplanting the old ones. When this occurs, we have a selection process operating on a higher plane, for the schools will then be competing for adherents. The school that succeeds will be the one that most promotes cognitive efficiency in its membership. In Kuhnian terms, we have rival scientific paradigms competing for the devotion of the forthcoming generations of scientists (Kuhn 1970).

In some sciences, naturally, no one school or paradigm is superior to any other, and accordingly two or more traditions may exist side by side for some time. This “speciation” of a discipline is particularly likely when the amount of data is large and unevenly distributed throughout the community, thereby allowing each school to concentrate on a subset of elements
for synthesis (which is analogous in biological evolution to specializing in a particular ecological niche). This is often the case in the behavioral and earth sciences. As an example, for a long time geology was bifurcated into Neptunists and Plutonists (or Vulcanists), the geographic distribution of adherents in each school roughly corresponding to the prominence of sedimentary versus metamorphic minerals and formations in the lands where the geologists did their field work. Only when geological information became more homogeneously disseminated could these rival systems fuse into a conception like that found in Lyell's *Principles of Geology*. More recently, the willingness to accept the theory of continental drift has been shown to have depended partly on which section of the world the geoscientists did their research training, with those working in South America being the most receptive (Stewart 1986). Likewise, the profusion of theories that currently permeate psychology may in part reflect the uneven distribution of raw facts, with psychologists diverging on the sources of phenomena to which they are exposed—animal versus human, abnormal versus normal, laboratory versus natural, behavioral versus introspective, developmental versus differential, and so forth.

Nonetheless, it is crucial to recognize that even when an original configuration recruits enough adherents to form a school or paradigm, it does not mean that all converts will subscribe to the exact same ideas. On the contrary, because the mental elements to be integrated will almost never be identical within each mind, the subsequent cognitive integration will rarely be equivalent from person to person. Those who accept a scientific innovation assimilate the configuration to match their own personal needs for self-organization. For instance, not all converts to Darwin's theory maintained the same set of theoretical propositions, even if all could be said to define a school in united opposition to the traditional creationist interpretation of the origins of life (Hull, Tessner, & Diamond 1978).

A chance permutation must therefore pass a series of tests before it can become part of what is considered to be general scientific knowledge. Two tests are intrapsychic or personal, and another pair is social. First, the chance permutation must be sufficiently stable to coalesce into an original configuration. Then this configuration must be capable of elaboration into a communication configuration that makes it accessible to other practitioners of the scientific enterprise. Given this articulation, enough colleagues must accept the new configuration by using it, however idiosyncratically, to reorganize their own conceptions of reality. Finally, when there are enough converts to form a new school, this school must win the competition with the other schools, including any handed down from previous generations. In a sense, this selection sequence represents a chaining of separate variation–selection processes. Of all chance permutations, only a small proportion is stable enough to constitute configurations. Of this set of configurations, sometimes only a subset is successfully articulated. And from the resulting variation in available communication configurations, just an elite number will be honored with social acceptance (e.g., only a small percentage of published articles is even cited in subsequent works by others). Should two or more schools form, their variation will enable further selection at the sociocultural level.

**Scope of application**

So far we have introduced such key theoretical concepts as the chance permutation of mental elements; the formation of chance, a priori, and a posteriori configurations; and the creation, social acceptance, and sociocultural survival of communication configurations—all driven by an intrinsic quest for self-organization. Still, the chance-configuration theory is currently just a bare sketch in need of elaboration. To develop the theory further, I shall draw on information published in several disciplines (especially psychology, education, sociology, anthropology, and history) concerning key aspects of scientific creativity: (1) impressionistic evidence concerning the creative process, including both introspections and anecdotes; (2) individual differences in personality, whether cognitive or motivational; (3) productivity, including both cross-sectional variation and longitudinal fluctuations in quantity and quality of contribution; (4) developmental antecedents, such as family background, role models, education, marginality, and the sociocultural milieu; and (5) the phenomenon of multiples, that is, independent and often simultaneous discoveries and inventions. These are the topics of chapters 2 through 6. Chapter 7 then consolidates and applies the chief conclusions.

Before we advance to the empirical development of the theory, however, I must set forth what I consider to be the theory's explanatory scope. Any scientific model or system is necessarily restricted to a subset of all known phenomena (Toulmin 1960). Here we have two fuzzy delimiters.

First, the emphasis is on explaining creativity in the pure or basic sciences, such as mathematics, astronomy, physics, chemistry, and biology. This is not to say that the chance-configuration theory is irrelevant to understanding innovations in the applied sciences, like technology and medicine, but only that little effort will be made to account for any peculiarities inherent in the practical applications of scientific knowledge. Actually, I have argued elsewhere that the chance-configuration theory handles quite well the chief empirical aspects of creativity in the arts and human-
ties, not just in science and technology (Simonton 1987b). Indeed, certain facets of leadership, whether political, military, or economic, might be constructively interpreted, mutatis mutandis, in the same theoretical language. At suitable places throughout this volume, I shall compare and contrast scientific creativity with other guises of creativity, even with leadership. Even so, these discussions will serve largely to clarify the nature of pure science. At the same time, I shall pay little attention to differences among the several disciplines of basic science. Creativity in physics is not identical with that in biology, for instance (Roe 1952a). Nevertheless, all forms of creativity, and especially all forms dedicated to advancing scientific knowledge, share a common basis for discovery and invention. It is this common ground that supports my main theoretical arguments in the following chapters.

Second, I shall stress enlarging our appreciation of scientific creativity in its most remarkable form—that of the genius. This concept is romantic and elusive enough to make difficult a comprehensive and precise definition. A tradition over a century old associates genius with an exceptional intellect, as psychometrically registered by an intelligence quotient, or IQ, at least two standard deviations above the mean (Cattell 1903; Cox 1926; Galton 1869). But even if this position does have advantages, it remains incomplete, for cognitive power means nothing at all in isolation from the drive to use it (Nicholls 1972). “Genius does what it must, and Talent does what it can,” apologized Owen Meredith in his Last Words of a Sensitive Second-Rate Poet. Yet even the inclusion of this motivational side of scientific genius does not suffice, for many profound and energetic minds have labored away without leaving even the tiniest imprint on scientific progress. Ultimately, any meaningful definition of genius—in order to have real-world validity—must include a social component (cf. Brannigan 1981). I conceive creativity as a form of personal influence over others and therein as a special variety of leadership—sociocultural rather than political, military, or economic. Social psychologists frequently define a leader as that group member who exerts more influence in problem solving or decision making than does the typical member of the group (Simonton 1985a). According to this conception, clearly an Aristotle, Newton, or Einstein is a leader par excellence in the community of scientists. The scientific genius exerts a massive effect not just on contemporaries but also on posterity (Albert 1975). In the last analysis, genius is defined by enduring eminence or reputation. “By reputation,” said Francis Galton (1869, p. 33), “I mean the opinion of contemporaries, revised by posterity . . . the reputation of a leader of opinion, of an originator, of a man to whom the world deliberately acknowledges itself largely indebted.”

Scope of application

Eminence, to be sure, is not a perfect indicator of creative greatness. Even Thomas Carlyle, a stalwart proponent of the cult of genius, admitted that fame “is no sure test of merit, but only a probability of such.” Indeed, in later chapters we shall discuss some of the capricious ways that intrinsic capacity is translated into outward success. And we shall dispel certain heroic notions about the attributes of scientific luminaries (see also Weisberg 1986). Still, I chose to concentrate on creative genius in science because I believe that the chance-configuration theory best applies to those who have achieved a truly illustrious and permanent position in the annals of science.
Productivity

The distinguishing characteristic of genius, scientific or otherwise, is immense productivity (Simonton 1984, chap. 5). The common misconception that phenomenal intellects contribute only a handful of selective masterworks, or even a single magnum opus, is plain wrong. Darwin could claim 119 publications at the close of his career, Einstein 248, and, in psychology, Galton 227, Binet 277, James 307, Freud 330, and Maslow 165 (Albert 1975). Edison may be best known for his incandescent light bulb and phonograph, but all told he held 1,093 patents—still the record at the United States Patent Office. Frank Barron (1963, p. 139) commented that there is good reason for believing...that originality is almost habitual with persons who produce a really singular insight. The biography of the inventive genius commonly records a lifetime of original thinking, though only a few ideas survive and are remembered to fame. Voluminous productivity is the rule and not the exception among the individuals who have made some noteworthy contribution.

The chance-configuration theory helps explain three aspects of this feature of scientific creativity: (1) the distinctive cross-sectional distribution of productivity, (2) the relation between age and productive output, and (3) the association between quantity and quality. Moreover, at the close of this chapter I shall show how the theory clarifies three more issues of importance, namely, the Ortega hypothesis, the Yuasa phenomenon, and Planck’s principle.

Cross-sectional distribution

The distribution of output in any scientific endeavor is highly elitist, with a small proportion of the total scientific community accounting for the majority of the published contributions. In a study of disciplines as diverse as linguistics, infantile paralysis, gerontology and geriatrics, geology, and chemistry, Wayne Dennis (1955) found that the top 10% most prolific contributors were responsible for about half of the total work, whereas the bottom 50% least productive can be credited with only about 15% of the contributions (see also Bloom 1963). Indeed, typically around half of those at all active contribute only one work each, meaning that the most prolific contributor is several times more productive than are the least productive contributors (Dennis 1955; see also Davis 1987; Shockley 1957). In psychology, for instance, the most prolific scientist can claim more titles than can 80 colleagues in the lower half of the distribution (Dennis 1954c). And as Dennis (1955) pointed out, these figures may tremendously underestimate the magnitude of elitism, as only those who have contributed at least one item to posterity are counted at all. Sometimes more than half of the young PhDs from a distinguished university may publish nothing beyond their doctoral dissertations (Bloom 1963). There is thus some justification for the prima facie preposterous claim of Cesare Lombroso, in his 1863 book The Man of Genius, that “the appearance of a single great genius is more than equivalent to the birth of a hundred mediocrities.” Two laws have been proposed that describe the empirical distribution.

According to the Price law, if \( k \) represents the total number of contributors to a given field, then \( \sqrt{k} \) will be the predicted number of contributors who will generate half of all contributions (Price 1963, chap. 2). Not only does this law imply a highly skewed distribution, but it also suggests a distribution that becomes ever more elitist as the discipline expands (Simonton 1984d, chap. 5). To illustrate, in a new discipline with only about a dozen investigators, around three, or one-fourth, will account for half of all contributions. But should the discipline grow to 100, the elite will consist of 10, or 10% of the whole field. This expanding hegemony of the few as a domain of inquiry gains practitioners has obtained empirical endorsement (e.g., Zhao & Jiang 1985), although the law probably does not hold for extreme values of \( k \). If a discipline becomes extremely popular, recruiting an active membership of about 10,000, it seems unlikely that a mere 1%, or 100, would so dominate the creative enterprise.

The Lotka law attempts to describe in more detail the shape of this distribution, which is clearly monotonically decreasing at a decelerating rate, yielding a long upper tail (Lotka 1926). If \( n \) is the number of published papers and \( f(n) \) is the number of scientists who publish \( n \) papers, then \( f(n) \) will be inversely proportional to \( n^2 \), where the proportionality constant varies from discipline to discipline. It is interesting that this function is similar to Pareto’s law of income distribution, in which wealth is allotted in inverse proportion to \( n^{-1.5} \) (Price 1963, chap. 2). If a leader is defined as a person whose influence over other group members far exceeds the norm, then clearly the most productive scientists will form an elite as dominating as that of the economic magnates who monopolize a nation’s material resources.

These empirical findings and laws are extremely general, as they hold
equally well for other kinds of creative productivity outside science. Dennis (1955) found the same productive elitism in the publication of secular music and in the books represented in the Library of Congress as he did in the scientific disciplines listed earlier. And the Price and Lotka laws are applicable to creative activities far broader than their authors claimed (Simonton 1984d, 1987b). For example, classical music: About 250 composers account for all the music heard in the repertoire, but a mere 16 can claim credit for creating half of those pieces (16 ≅ V250) (Simonton 1984d, chap. 5; cf. Moles 1958). Bach’s output alone fills 46 volumes (Albert 1975)—enough compositions, reportedly, to keep a copier confined to regular working hours occupied a whole career merely copying the parts out by hand! Furthermore, the comparison between the Lotka and Pareto laws implies that the principle of productive elitism may apply to any domain of achievement, even those requiring leadership rather than creativity per se (Simonton 1984d, chap. 5). Thus the number of battles fought by generals, the number of laws written by legislators, the number of elections entered by politicians, and the amount of monetary assets accumulated by entrepreneurs all may be described by roughly the same skewed cross-sectional distribution.

Although there is some debate about whether the Price law can be derived from the Lotka law (Allison, Price, Griffin, Moravscik, & Stewart 1976), there is very little contention about the general applicability of these laws, at least as rough approximations. And certainly there is no disagreement about the broad form of the empirical distribution, however elitist it may be. The chance-configuration theory offers an explanation for this established fact. But before I present that account, let me briefly review some of the earlier attempts to clarify this phenomenon.

Previous interpretations

Even if all researchers concur on the skewed-right productivity distribution, there will be less consensus on the theoretical foundation for the observed curve. Indeed, at least three conceptually distinct interpretations have been offered, two more psychological and one more sociological in orientation.

Dennis (1954c) himself suggested that the observed distribution represents the upper tail of the normal distribution, a threshold operating to delete the lower portions of that standard distribution (cf. Nicholls 1972). This idea has immediate appeal, not just for its simplicity, but also for its compatibility with a long psychometric tradition—commencing with Francis Galton’s classic Hereditary Genius (1869)—that has conceived exceptional intelligence in terms of the exclusive right-hand portion of the bell-shaped curve (see also Cattell 1903; Simonton 1985a). Yet the Nobel laureate Herbert Simon (1954) indicated that this will not do, because the tail of the productivity distribution is stretched out much farther than we would expect according to the normal curve (cf. Haitun 1983). Either some process must be activated that distorts the distribution in precisely the correct way, or else some entirely distinct mechanism is responsible for generating the skewed distribution. The remaining two interpretations pursue each of these possibilities.

Apropos of the first alternative, another laureate, William Shockley (1957), proposed the following route around the impasse: If we suppose that productivity is the multiplicative product of many factors all normally distributed, then the consequence will be a lognormal distribution, and hence one highly skewed right, with an appropriately elongated upper tail. This interpretation, too, is attractive. It is certainly plausible that several variables are requisite for creativity, that these variables contribute to creativity in a multiplicative manner, and that the variables are distributed normally in the population (Allison 1980b; Simonton 1987c). The main difficulty with Shockley’s solution, however, is that the nature and number of the factors involved are neither theoretically nor empirically specified. Accordingly, his explanation must be considered incomplete.

Perhaps at the present time the majority view is that the cross-sectional distribution is the result of some accumulative advantage process whereby “the rich get richer and the poor get poorer” (e.g., Allison 1980a,b; Allison, Long, & Krauze 1982; Allison & Stewart 1974; Price 1976; Simon 1955). This mechanism is more sociological than psychological, depending as it does on the selective quality of the scientific reward system. Because access to research positions, journal space, grant money, and rewards or honors is severely restricted, those who succeed first obtain an edge that better enables them to succeed later, whereas those who start off on the wrong foot tend to establish a pattern of failure that eventually pushes them out of the competition for kudos. Success breeds further success; failure spawns failure.

Now we cannot really question whether this accumulative advantage (and disadvantage) phenomenon occurs for awards and honors in science. In this domain, what Merton (1968) styled the “Matthew effect” definitely applies. According to Matthew (25:29), “Unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath.” It is equally evident that once certain scientists attain high professional visibility or fame, their opportunities for publication are much better than are those of colleagues yet to establish a
reputation at large. For example, eminent scientists receive more invitations to contribute chapters to books and articles to journals, thereby permitting them to bypass the frequently capricious decisions made by editors and their referees (Rodman & Mancini 1981). And finally, there is little doubt that the principle of accumulative advantage can explain many unique features of scientific influence and acclaim. But what is less apparent, in my mind, is whether this principle can account for the specific cross-sectional distribution of total lifetime output. It is telling that the distribution of publications is less elitist than is the distribution of citations or rewards (Allison 1980b; Allison et al. 1982; Allison & Stewart 1974; Davis 1987), a contrast that implies that some other process may be responsible for the skewed distribution of productivity across scientists, the Matthew effect operating merely to exaggerate this initial inequality yet further (cf. Allison & Stewart 1975; Faia 1975).

Although the first of these three explanations is plainly inadequate, I shall not assert that the second and third are just as wrong. I shall argue only that a fourth interpretation can be derived directly from the chance-configuration theory. Because the theory explains so many other facets of scientific creativity—as indicated in the previous and following chapters as well as the remainder of this chapter—this alternative account may be the most plausible overall.

Present interpretation

Like Shockley and Dennis, I posit that the basis for the productivity distribution is the normal curve, and like Shockley I propose some mechanism by which this curve can be distorted to yield a more skewed result. Let us suppose, in particular, that the number of mental elements available for chance permutations is proportional to an individual's intelligence and hence that this attribute is normally distributed in the population. According to this supposition, the total supply of potential chance permutations of those elements will be characterized by a highly skewed curve with an extremely stretched-out upper tail. This happens because the number of permutations of $n$ items increases as an accelerating (nonlinear, concave upward) function of $n$. Unfortunately, we cannot specify what this function is. If we speak of combinations rather than permutations, there will be $2^n$ ways of taking $n$ elements, a number that is both too large and too small. It is an overestimate inasmuch as it is improbable that all potential sets of $n$ things will be considered (up to and including the set of all things!). And it is an underestimate insofar as $2^n$ refers to combinations not permutations. Therefore, we could use the combinatorial formula for the number of permutations of $n$ elements taken $r$ at a time, where $r$ represents the maximum number of elements that can be considered at any one time. Then $P^n_r = n! / (n + r)!$. The introduction of factorials yields a function that grows at an accelerating pace as a function of $n$. In fact, it probably increases too fast, indeed explosively, even when $n > r$ (for $r$ may be somewhere near Miller's 1956 magical number 7 ± 2). Consequently, I shall offer, as a very rough guess, the more conservative exponential function. That is, if $n$ is the number of mental elements, $e^n$ will be the number of potential chance permutations, where $e$ is the constant 2.718. . . . Even if $n$ is normally distributed in the population of scientists, $e^n$ definitely will not be. Indeed, the distribution will be decidedly lognormal rather than normal, with an immensely long upper tail.

To illustrate this point, I ran a simple Monte Carlo simulation (Simonton 1987b). The computer generated 10,000 random normal deviates to represent the distribution of the quantity of mental elements across individuals. A second set of random scores was created by taking the exponential of the first set. To make the comparisons more concrete, I standardized both sets of numbers to a mean of 100 and a standard deviation of 16, as if they were IQ-like scores, and then I truncated these standardized scores to integer values. The first set of scores, which is taken to reflect the supply of mental elements, ended up with a distribution not all that different from what is normally found for IQ. The minimum was 37, the maximum 155, for a range of 118 points, and the distribution was highly symmetric and normally peaked (skewness = -0.02 and kurtosis = -0.09). The second set of scores, in contrast, had a low of 87, a high of 341, for a spread of 254 points, and the distribution was highly skewed right, featuring a long flat upper tail (skewness 4.20 and kurtosis 31.37). The lowest score was much closer to the mean (100) than was the highest score, and the distribution monotonically decreased at a decelerating rate throughout almost the entire range of scores, making the outcome extremely elitist. The upper tail of the second curve, in fact, is indistinguishable from the distribution usually observed for scientific productivity. The two contrasting curves are shown in Figure 4.1.

Consequently, the chance-configuration theory can explain both the Lotka and Price laws according to the capacity for chance permutations to increase as a nonlinear and accelerating function of the repertoire of free mental elements. Again, although alternative interpretations of this phenomenon exist, the current explanation ensues from a larger theoretical framework that also handles many other data points. In addition, we have every reason to believe that this account fits lifetime output in creative endeavors outside science.
Second, the specific form of the age curve—in particular the points defining the onset, maximum, and magnitude of the postpeak decline in productivity—varies in a predictable fashion from discipline to discipline (also see Cole 1979; Dennis 1966). In the biological sciences, for instance, the early 30s may be the optimal period for contributions to botany and classical descriptions of disease, whereas the late 30s may see more contributions to bacteriology, physiology, pathology, and general medicine. Likewise in the physical sciences, productivity apparently peaks in the late 20s for chemistry, the early 30s for mathematics and physics, and the late 30s for geology and astronomy (Lehman 1953a, chap. 20). The early peak for mathematics and theoretical physics is notorious (cf. Hardy 1940; Moulin 1955; Roe 1972b). This pervasive attitude is revealed in Einstein’s words: “A person who has not made his great contribution to science before the age of thirty will never do so” (quoted in Brodetsky 1942, p. 699). This point is made more emphatically in the little poem of Paul Dirac, who won the Nobel Prize for Physics at the youthful age of 31, for work he had done when only 25:

Age is, of course, a fever chill that every physicist must fear. He’s better dead than living still when once he’s past his thirtieth year.
(quoted in Jungk 1958, p. 27)

These broad conclusions have been corroborated in many other investigations (see, e.g., Bayer & Dutton 1977; Blackburn et al. 1978; Davis 1954; Diemer 1974; Eagly 1974; Fulton & Trow 1974; Lehman 1953b, 1966a,b; Lyons 1968; Zhao & Jiang 1986; Zusne 1976). Nevertheless, Lehman’s research has also been severely criticized on methodological grounds by Dennis (1956a,b, 1958, 1966) and by many sociologists (e.g., Cole 1979; Riley, Johnson, & Foner 1972; Zuckerman & Merton 1972). These critics’ chief argument is that the downward turn in the tables showing aggregate output as a function of age is a mere artifact. First, Lehman is reputed to have failed to weigh the impact on his tabulations of including creators who did not live out their normal life span, thus introducing a spurious decline in the totals (the “compositional fallacy”). In addition, Lehman was accused of not controlling for the changes in competition as a creator’s career advanced and the number of persons entering the same field expanded. Despite these complaints, Lehman’s age curves were largely replicated in subsequent research that used more sophisticated analytical techniques (e.g., Diemer 1974; Horner, Rushton, & Vernon 1986; McDowell 1982; Oromanger 1981; Pelz & Andrews 1976, chap. 11; Simonton 1975a, 1977a, 1980b,d,e, 1984b,f; see also Lehman 1956, 1958, 1960, 1962, 1963, 1966a;
cf. Diamond 1986). Specifically, even though the optimal ages for output may sometimes differ slightly from what Lehman suggested (and often a bit later), the postpeak decline appears even after taking into account differential life span, secular trends in competition, and many other sources of potential methodological artifact. Indeed, there is ample justification for believing that the age curves may be transhistorically and cross-culturally invariant, for they have been replicated on samples of creators who lived in many different nations, even distant civilizations, and in different centuries, even separate millennia (e.g., Lehman 1962, 1963; Simonton 1975a, 1980e).

In my opinion, the primary drawback to Lehman's research, as well as that of his successors, is the near absence of a theoretical scheme for interpreting the key results. Why does the agewise distribution of output assume such a predictable form? For what reason does that form change from discipline to discipline? Lehman (1953a) himself, in the final chapter of his book, offered several post hoc interpretations, with an emphasis on physical health (see also Lehman 1962). Yet the age curves for creative output fail to correspond to those for physical fitness, nor would we expect that people in distinct fields feature comparable contrasts in vigor; and in any event, the decline in productivity materializes even when health is statistically separated out (Simonton 1977a). Over a century ago, Beard (1874) proposed a two-factor theory based on supposed changes in enthusiasm and experience over the course of a career, yet his theory lacks an a priori mathematical formulation, and it fails to handle all key findings (Simonton 1984b). Mumford (1984) put forward an alternative two-factor explanation based on the agewise distribution of two divergent adaptive styles, the accommodating and the controlling, with comparable limitations. Outside psychological accounts, some sociologists have attempted an interpretation based on the doctrine of accumulative advantage (e.g., Cole 1979), whereas some economists have tried to advance an explanation based on investment in "human capital" (e.g., Diamond 1984, 1986; McDowell 1982). Again, for various reasons, these offerings, however noteworthy, are unable to account for the full inventory of established results. More important, we can derive directly from the theory put forward here a model that treats the data comprehensively and precisely.

**Information-processing model**

We shall begin by observing that the chance-configuration theory maintains that creativity, on the intrapsychic level, involves a two-step cognitive process (cf. Wallas 1926). The first step is generating a configuration via the chance-permutation process; the second step converts this chance configuration into a communication configuration suitable for publication. Expressed in the scheme outlined in chapter 3, Step 1 engages more the infracognitive, primary-process associations accessible below the threshold of cognition, whereas Step 2 recruits more the conscious, secondary-process associations freely available at the cognitive level (cf. Suler 1980). In any event, the production of chance configurations depends on the supply of stray mental elements, that is, elements in a relatively disordered state in the mind, whereas the production of communication configurations depends on how many chance configurations have accumulated awaiting fuller articulation. This two-step conception of the chance-configuration theory dovetails nicely with a recent mathematical model of creative productivity (Simonton 1984b). It, too, is predicated on a two-step cognitive process: First, each creator begins with a supply of "creative potential" (i.e., the degree of rich associative interconnections among numerous elements) that, during the course of the creator's career, becomes actualized in the form of "creative ideations" (i.e., chance configurations); second, the ideas produced in the first step are progressively translated into actual "creative contributions" (i.e., communication configurations) for publication in the established disciplinary vehicles.

We can then derive a set of linear differential equations based on two rates. The first corresponds to Step 1 and is called the *ideation rate*, which assumes that a "law of mass action" operates so that the speed at which creative potential is converted into creative ideations is proportional to the size of that potential at a given time. In our theory, the velocity of ideation depends on the mass of still-disorganized mental elements. The more free mental elements there are, the more possible "collisions" there will be, to use Poincaré's molecular analogy, and thus the higher the speed will be at which stable permutations emerge. The second rate corresponds to Step 2 and is called the *elaboration rate*, which concerns the quantity of ideations that await articulation. In our terms, the speed at which communication configurations are generated is proportional to the existing backlog of chance configurations, assuming that creators work on more than one project at the same time (see Hargens 1978; Simon 1974). The solution to these differential equations yields the following equation:

$$p(t) = c(e^{-at} - e^{-bt})$$

(4.1)

Here $p(t)$ is creative productivity (e.g., number of publications) at time $t$; $a$ is the ideation rate; $b$ is the elaboration rate; and $e$ again is the exponential constant. The rate of ideation is assumed to be typically lower than that of elaboration (i.e., $a < b$), a reasonable assumption, as the proportion of
chance permutations that make acceptable initial configurations is far smaller than the proportion of those configurations that can be converted into communication configurations. According to Wallas's (1926) four stages, the combined states of preparation, incubation, and illumination arrive at creative ideas less rapidly than the stage of verification can dispose of them. Or according to the associationist model presented in chapter 3, the speed of processing at the intuitive level is slower than that at the analytical level.

At any rate, the integration constant $c$, which scales the height of the predicted curve, is equal to $abm / (b - a)$, where $m$ is the initial creative potential. That is, $m$ represents the maximum number of contributions that a creator is theoretically capable of producing, given an infinite life span. Previously we affirmed that $m$ should be roughly proportional to $e^n$, where $n$ is the number of mental elements not yet tied down in a hierarchical structure of tightly consolidated configurations.

Significantly, $t$ is taken to represent not chronological age, on which most research concentrates, but, rather, professional or "career age" (cf. Bayer & Dutton 1977; Lyons 1968), for this is an information-processing model in which $t = 0$ at the moment that the ideation (i.e., chance-permutation) process begins. Nonetheless, to validate the model, it is often necessary to translate career age into chronological age equivalents, a reasonable approximation, given that the two alternative measures tend to correlate .87 (Bayer & Dutton 1977; see also Andrews 1979). With that in mind, Figure 4.2 presents a typical age curve according to the model, assuming that $a = 0.04$, $b = 0.05$, and $c = 61$ (i.e., $m = 305$) and that $t = 0$ at chronological age 20.

In general, creative productivity increases rapidly (in a decelerating curve) up to a single peak in the early 40s—with a maximal output of about five contributions per annum—and soon thereafter begins a gradual decline, reaching the zero point asymptotically—yielding an inverted backward-J curve, as found in the empirical literature. Indeed, the average correlation between predicted and observed values is .95, and the equation even predicts data (see Cole 1979; Dennis 1966) claimed to contradict Lehman's age curve! It is significant that the longitudinal function exhibited in Figure 4.2 fits not only the overall form of agewise fluctuations in output but also the fine structure. For instance, a study of the careers of eminent psychologists indicated that the shape of the curve in the beginning is concave downward, in a decelerating curve, and that the closing portion of the curve is characterized by an asymptotic approach to the zero-output level (Simonton 1984b). Further, the model holds that a larger proportion of variance in the longitudinal fluctuations will be explained if the predic-

![Figure 4.2. Predicted productivity as a function of age under typical parameters (from Simonton 1984b).](image)

tions are defined according to career age rather than chronological age (Simonton 1984b). For example, the equation subsumes Zusne's (1976) empirical generalization that the age peak for any given career is located at the harmonic mean of the age of first and last contribution (Simonton 1984b). At the same time, the equation defines the circumstances when we would not expect Zusne's harmonic-mean model to hold, namely, when the values of $a$ and $b$ put the peak in the middle or latter portion of a career.

Nevertheless, the model's most important implications, besides accurately describing the typical age curve, concern interdisciplinary differences in the specific age function and the empirical association among precocity, longevity, and rate of output. After examining each of these subjects, I shall close this discussion of the information-processing model by outlining some useful qualifications.

**Interdisciplinary differences**

Any complete explanation of the age function for creative productivity must provide for the fact that the age curve alters systematically from
practically his whole creative life to complete his *Decline and Fall of the Roman Empire*. To show again how such differences emerge in real data, we can once more use Dennis's (1966) longitudinal tabulations. The parameter estimates are $a = 0.04$ and $b = 0.07$ for poets, $a = 0.02$ and $b = 0.05$ for novelists, and $a = 0.02$ and $b = 0.03$ for historians. These estimated values yield predicted peaks, in terms of career age, of 19, 26, and 40, respectively (cf. Simonton 1975a). As we saw before, small changes in the two information-processing parameters can generate tremendous contrasts in expected productive peaks across disciplines.

Finally, by appropriately altering the two parameters, we can account for any observed historical trends in the optimal age for creative output. As a case in point, the peak age for contributing to science may have increased from around 25 years in 1500 to 37 years in 1960, an increment that cannot be adequately accounted for by corresponding enhancements in life expectancy over the same period (Zhao & Jiang 1986; cf. Roe 1972b). If we assume that the complexity of the problems faced by the scientific community enlarged over this time interval, then the ideation and elaboration rates would be expected to shrink in proportion.

Whatever the specific discipline or era, the same principles probably apply in determining the size of the two key information-processing parameters. If the mental elements entering the chance-permutation process are relatively simple and well consolidated, the configurations can be arrived at quite quickly, so that the creator will soon consume the initial creative potential. But if the elements are more rich and varied, the generation of stable permutations will slow down, and correspondingly more time will be needed to realize the full lifetime potential. Thus, because pure mathematics and theoretical physics deal with highly abstract conceptual entities, the peak age is younger than it is for those disciplines, such as geology, whose central ideas are more complex and concrete. The early productive peaks characteristic of lyric poets may be likewise explained (cf. Simonton 1975a, 1980e). Similarly, in certain fields the proper translation of chance configurations into intelligible and persuasive communication configurations is relatively straightforward, whereas in other fields much time may be consumed in verification, documentation, and calculation before the central idea has acquired presentable clothing. Again, in pure mathematics, once a theorem is conceived and its proof realized, the time necessary to ready a paper for submission to a professional journal may be negligible. As an example, Arthur Cayley, who wrote some 995 mathematical papers in his lifetime, averaged a paper every two weeks! On the other hand, in astronomy, especially before the advent of digital computers, many years were wasted in laborious calculations and observations, considerably delaying
that after a certain point in a career's postpeak portion, the decline in the generation of finished products ceases to be concave downward and instead becomes concave upward (Simonton 1984a). This inflection point occurs at

$$t_i = \frac{1}{b - a} \ln \frac{b^2}{a^2}$$

(4.3)

Thus for the typical case, the inflection appears 45 years into the career, after which the loss in productivity begins to level off. Finally, as we shall discuss later, although the quantity in output may fall in the later years, the proportion of those products that are first-rate does not deteriorate in any way. Hence, a scientist of advanced age has grounds for considerable hope, even in fields in which youth seems to have all the advantages.

**Precocity, longevity, and rate of output**

The cognitive model just developed elucidates more than the longitudinal fluctuations in productivity within careers, for it accounts as well for the cross-sectional intercorrelation among the three independent components of lifetime output. At the beginning of this chapter we observed that scientists vary tremendously in the total number of communication configurations they offer to the world, but we said nothing then about how this distribution emerges in terms of the agewise progression of each scientist's career. To appreciate the time-related foundation for total productivity, we must start by recognizing that there are three principal ways that scientists can attain an impressive lifetime corpus of contributions.

First, prolific scientists can display creative *precocity* by beginning to produce at an exceptionally young age. Lehman (1953a, chap. 12) was among the first investigators to point out the strong connection between the age at which productivity commences and the final count in contributions at the career's close. He demonstrated that this relationship holds for disciplines as diverse as mathematics, physics, and chemistry, in the sciences, and philosophy, literature, painting, and classical music, in the arts and humanities. For mathematics, as an example, the correlation is -.61, signifying that the younger is the age at first contribution, the larger will be the final bibliography of contributions (p. 185). Subsequent investigators have repeatedly corroborated this conspicuous and reliable association: Early productivity is one of the single best predictors of later productivity in all domains of creativity (see, e.g., Blackburn et al. 1978; Clemente 1973; Cole 1979; Davis 1987; Dennis 1954a,b; Helson & Crutchfield 1970; Simonton 1977b). For the most part, major scientists begin productive output sometime in their middle 20s, and truly exceptional scientists may

the time that a novel notion was in the right guise for publication. Indeed, it took Copernicus around 20 years to work out the details for his *On the Revolutions of the Celestial Spheres* (see also Abt 1983).

Even if this cognitive model features two central parameters, in many respects the ideation constant $a$ is the most interesting, for this determines the rate at which creative potential is consumed, that is, the rate at which an intuitive genius is transformed into an analytical genius. We may define the typical "creative half-life" in any discipline as the natural logarithm of 2 divided by $a$. In mathematics, then, the half-life may be around 23 years, meaning that half of the initial creative potential is normally consumed in the production of chance configurations; so if the mathematician begins at, say, age 20, he or she will be midway between intuition and analysis by age 43. In more scholarly endeavors, the half-life may be about 35 years, signifying that a scholar still has half of a creative life ahead at age 55 if the career commenced at age 20. Creativity in other fields may fall between these two extremes. The half-life for creativity in poetry, for instance, may be around 17 years, the same half-life as in the prototypical case presented in Figure 4.2 (cf. Simonton 1984b).

As the Dirac poem quoted earlier suggests, creative persons themselves are all too aware of the loss of creative potential over time. In one study of eminent scholars in several disciplines, only 33% of the physicists with a mean age of 47 thought they had "more important contributions in the future," and only 17% of the mathematicians with a mean age of 53 expressed this hope (Simon 1974). For the social sciences and humanities these figures are 72% in political science, 50% in English, 38% in sociology, and 33% in philosophy (with mean ages of 55, 55, 58, and 64, respectively). Thus with the exception of the political scientists and philosophers, half or more of distinguished scholars believe they are "over the hill" when they reach their 50s.

Because of this pervasive attitude, we should stress that this cognitive model, rather than having pessimistic implications, projects an optimistic picture of the later years of a creator's life. Because we are dealing with a decay curve like that for radioactivity, creative potential is lost at a decelerating rate. Accordingly, in the typical case graphed in Figure 4.2, a creator who works for 60 years can still accomplish more in that last decade than was achieved in the first decade (see, e.g., Fulton & Trow 1974). Furthermore, at the end of that final career decade, about one-quarter of the initial creative potential will remain unrealized. The intuitive genius seldom completely self-organizes into an analytical genius. Moreover, not only will numerous creative ideations remain, but there also will be some backlog of ideations awaiting translation into creative contributions. It is worth noting
begin even earlier (Albert 1975; Helson & Crutchfield 1970; Horner, Rushton, & Vernon 1986; Lehman 1958; Raskin 1936; Roe 1972b). The classic case is Newton's *annus mirabilis*, during which before the age of 24, he had already begun to work out his ideas on universal gravitation, the theory of colors, the calculus (or "fluxions"), the binomial theorem, and the method of infinite series—his chief contributions to science. As this list implies, such precocity is particularly common in mathematics: Abel, Clairaut, Euler, Galois, Gauss, Hamilton, Lagrange, L'Hospital, Maxwell, Monge, Pascal, and Poisson all initiated their careers while still teenagers (Lehman 1953a, pp. 178–183), and mathematics is so much a "young man's game [that] the average age of election to the Royal Society is lowest in mathematics" (Hardy 1940, pp. 70–71). This precocity is also manifested in the fact that notable scientists tend to earn their PhDs—the supposed certificate of graduation into most scientific professions—at a comparatively young age, usually around the mid-20s (Roe 1952a; see also Helson & Crutchfield 1970). For instance, those psychologists who were later honored with the presidency of the American Psychological Association earned their doctorates, on the average, at about age 26 and were already highly visible researchers at around age 28—all this when the mean age for receiving a doctorate in psychology was 31 years of age (Lyons 1968). As this example suggests, the odds that "late bloomers" will establish a scientific reputation are minuscule (cf. Busse & Mansfield 1981). Indeed, even scientists whose careers began at more ordinary ages tend to make only half as many "breakthroughs" as claimed by their precocious colleagues (Zhao & Jiang 1986). In summarizing his own data, Lehman quoted the words of Oliver Wendell Holmes, Jr.: "If you haven't cut your name on the door of fame by the time you've reached 40, you might just as well put up your jackknife" (1953a, pp. 185–186).

Second, productive scientists can exhibit extraordinary creative longevity by continuing to produce until quite late in life. Darwin, Freud, and Einstein, for example, not only started their creative careers in their early 20s, but they all continued to generate ideas for around half a century afterward. Generally, the truly eminent in any endeavor usually have careers spanning over a quarter-century, and careers a half-century long are not uncommon (Albert 1975; Davis 1987; Raskin 1936). The contributions of distinguished creators "are not 'one-shot' affairs. They are productive over many decades, and their most important contributions tend to be spread over many years" (Simon 1974, p. 335). Unlike their less illustrious colleagues, these workers retain an active enthusiasm for research until late in life (Blackburn et al. 1978) and refuse to acknowledge retirement as a limit on their activities. "Even after they have retired from regular academic or other positions, almost all of them continue to work long hours each Jay, most days of the year" (Simon 1974, p. 335). Indeed, the career of the high-caliber scientist is far more likely to be terminated by death or debilitating health than by a loss of intellectual fertility.

Third, prolific lifetime contributors can produce at high rates of output per unit of time. Harriet Zuckerman, in her *The Scientific Elite* (1977), compared Nobel laureates with a matched sample of scientists drawn from *American Men of Science*, finding that the former published at a rate over twice that of the latter: The laureates averaged 3.24 papers per year, the controls 1.48. Because the controls in her study could be clearly viewed as moderately successful scientists, it is not surprising that an inquiry into University of Chicago PhDs obtained an even more dramatic contrast; after a decade interval, the creative scholars yielded a mean of four publications per year, whereas the controls averaged fewer than one publication every two years (Bloom 1963). Those investigators who received the Distinguished Scientific Contribution Award from the American Psychological Association could boast a mean per annum of nearly three publications (Albert 1975). These figures, which are typical (see also Roe 1965), can be better appreciated when we observe that to obtain tenure at a major university in the United States normally requires about two publications per year. Hence, the prolific contributors are generating contributions at rates faster than needed for professional survival and advancement in academe. Indeed, security of employment has no impact whatsoever on the yearly output of those committed to the scientific enterprise (Bridgewater, Walsh, & Walkenbach 1982).

From a mathematical standpoint, these three components are quite distinct and can comprise orthogonal determinants of the final contribution count. If $O$ represents total output at the end of the career, then obviously $O = R(E - S)$, where $R$ is the mean rate of output per time unit, $E$ is the age at which the career ended, and $S$ is the age at which the career started. Clearly, $R$, $E$, and $S$ may exhibit almost any arbitrary correlation, whether positive or negative, without changing their respective relationships with $O$. For example, we cannot rule out mere a priori grounds the possibility that those who begin their careers earlier are disposed to end their careers earlier as well; if precocity were thus associated with "early burnout," the difference between $E$ and $S$ would be constant. Or those who have long careers may feature lower rates of production to compensate. Notwithstanding the mathematical independence of these three components, these three variables are empirically highly correlated with one another. Early productivity is strongly associated with high rates of output over a long career (e.g., Andrews 1979; Blackburn et al. 1978; Clemente 1973; Davis
1987; Dennis 1954a,b; Horner et al. 1986; Lehman 1953a, 1958; Lyons 1968; Roe 1952a; Segal, Busse, & Mansfield 1980; Simonton 1977b; Zuckerman 1977). As a consequence, highly productive scientists commence their careers well ahead of their less prolific associates, and this initial inequality between elite and hoi polloi only expands as their respective careers progress (Allison et al. 1974; Allison & Stewart 1974; Faia 1975; cf. Mulkay 1980). One comprehensive survey of university faculty concluded that

those who will be productive over their full careers are the individuals who start early, receive their degrees when young, and take on the habit of regular output. These individuals are not affected by status changes (promotion, tenure) but rather continue to widen the productivity gap between themselves and their less productive colleagues as time passes. They are discernible early in their careers. (Blackburn et al. 1978, p. 140)

Dennis (1954b) published some instructive documentation of this generalization in an examination of notable 19th-century scientists, including mathematicians, astronomers, physicists, chemists, physiologists, naturalists, and geologists. Using the Catalogue of Scientific Literature, 1800–1900, Dennis tabulated the output in each consecutive career decade from the 20s to the 70s. Although the correlations in productivity were highest for adjacent decades (ranging from .49 to .84), the coefficients remained respectable for decades even far apart. The lowest correlation, namely, that between productivity in the 20s and in the 70s, remained an honorable .33 (see also Horner et al. 1986). In computing these correlation coefficients, Dennis used only those eminent scientists who lived into their 80s. This methodological choice was a wise one, for otherwise the link between precocity and longevity might be seriously underestimated. Data indicate that precocity is negatively related to life expectancy, and needless to say, the latter variable places an inescapable restriction on creative longevity (e.g., Simonton 1977b; Zhao & Jiang 1986). It is probably not so much that precocity causes an early death as that precocity allows a creator to die young and still place a name in the annals of history. As a case in point, Galois did not die in a duel at age 20 because he was an extremely precocious mathematician, but rather he was able to make a lasting contribution to mathematics, despite his tragic early death, because of his having become so prolific so early in his short life (cf. Simonton 1975a).

Significantly, the two-step cognitive model actually predicts a high positive relation among all three components. The source of this prediction is the integration constant $c$ that we specified was a function of $a$, $b$, and $m$. The first two constants, the ideation and elaboration rates, are presumed to be characteristic of a given discipline, as discussed in the preceding section, but $m$, or the maximum number of potential contributions (i.e., configurations), directly gauges creative potential. With $a$ and $b$ fixed, $c$ is directly proportional to $m$ and accordingly stands for a scientist's potential output. Because all age curves are similar for a given field, $c$ describes the height of the curve throughout the career. It immediately follows that the higher the creative potential is, the earlier output will begin, the higher the rate of productive flow, and the later the output will terminate (Simonton 1984b). Expressed in terms of our present theoretical schema, the more imposing is the inventory of potential chance permutations, the higher will be the probability of a scientist's arriving at a viable chance configuration early in his or her career; the higher will be the rate at which such configurations appear per annum; and the longer it will take for all possible configurations to be found.

On the other hand, the model predicts that the peak age for productive output—unlike precocity, longevity, and output rate—is independent of the final lifetime score (Simonton 1984b). That is, the maximum point on the curve shown in Figure 4.2, given in Equation 4.2, is a function solely of $a$ and $b$, which reflect the information-processing needs of a given discipline, whereas creative potential, represented by $m$, is irrelevant. Although no study I know of has specifically tested this prediction, a number of investigators have inadvertently collected data that endorse it. Zusne (1976) showed that the correlation between the eminence of psychologists and the age at which they made their most outstanding contributions—which, as will be seen later, tends to be the optimal age for output—was virtually zero ($r = -.01$). No matter what the psychologist's fame, the maximum on the curve appeared around the 39th year, a point reinforced by data presented by Lehman (1953b) (cf. Horner, Rushton, & Vernon 1986). Zuckerman (1977) gave evidence that despite the higher rate of output exhibited by her laureates relative to her controls, the ratio of output for the two groups remained more or less constant over consecutive decades, something that would naturally follow from the assumption that the age curves were a function merely of the discipline and not of creative potential (see also Blackburn et al. 1978). Further, a cross-cultural and transcultural survey of major figures in world literature suggests that the same prediction holds for the arts as well as the sciences. The peak age for creating a literary masterpiece is determined more by the nature of the composition than by the work's aesthetic success (Simonton 1975a). A similar dependence may hold for classical music (Lehman 1953a, chap. 20; Simonton 1977a,b). Lastly, a biographical analysis of 120 scientists and 123 literary figures from the 19th century found that in both domains eminence was positively associated with precocity but that the "age of greatest production" did not vary with the degree of distinction attained in either endeavor (Raskin 1936).
In any event, this prediction of the model is important, for it counters the claim, represented by the Dirac verse quoted earlier, that phenomenal scientists peak earlier and thus go “over the hill” faster. Because creative potential is associated with precocity, scientific geniuses will have an impact on their chosen fields much earlier than average, but that is the beginning, not the end, of their contributions. Einstein may have begun his career with a bang in 1905, yet he was publishing influential work for decades after that, and the peak of his career probably occurred at the normal time, in his late 30s, with the development of his general relativity theory. To generalize this point further, scientists who receive a Nobel Prize often make contributions subsequently that would earn them another prize if there were enough of them to go around (Zuckerman 1977). In fact, this practice of rewarding the first major contribution of an exceptional scientist may perform the disservice of exaggerating the creativity of youth (cf. Diemer 1974). Ironically, the earlier in their careers that scientists are so honored, the older they are likely to be before their production of prize-deserving work ceases.

To summarize, the chance-configuration theory, via the two-step cognitive model just developed, accounts for the age curves that distinguish various scientific disciplines; the empirical association among precocity, longevity, and rate of output; and the stability of the expected peak productive age with respect to individual differences in creative potential. Furthermore, we have every reason to believe at this time that the theory would be equally adequate in explaining the age-wise distribution of contributions in esthetic endeavors.

**Qualifications and emendations**

To say that these implications arise from human information processing should not be taken as a dogmatic assertion that extrinsic events do not impinge on the intrinsic working out of creative potential. Chance-configuration theory implies, in fact, some of the ways that extraneous factors might interfere. Certainly anything that reduces the time invested in permutation generation would depress productivity, and thus it comes as no surprise that enlarged administrative duties may push productivity below the baseline expectation (Garvey & Tomita 1972; Roe 1972a; Stern 1978) or that poor physical health can have an adverse impact as well (Simonton 1977a). Likewise, increased parental responsibilities may cause creative output to be somewhat less than predicted by the model—a loss of almost one article per year (Hargens, McCann, & Reskin 1978; see also McDowell 1982). Apropos of this last point, Moses Gomberg, a chemist, never married and prohibited his students from marrying until after they finished their graduate work, believing that family life interfered with doing quality science. Furthermore, as we noted in chapter 3, because the chance-permutation process depends on having access to the network of infraconscious associations, any environmental stressor strong enough to raise appreciably the emotional level will inherently undermine creativity (Simonton 1980a). The dominant, high-probability responses will tend to be elicited instead, and these cannot effectively generate chance configurations. Therefore, it is perfectly consistent with the theory propounded here to find that major wars lower the probability of notable advances in science and technology (Price 1978; Roe 1972a; Simonton 1976c, 1980c; cf. Simonton 1976b).

Because the preceding remarks can be neatly integrated into the chance-configuration framework, they constitute more extensions than true qualifications. Nevertheless, other empirical findings apparently do not fit so easily into the given theory. In particular, there is some question concerning the supposed single-peak age curve for productive output. Occasionally investigators have observed a “saddle-shaped” function with two peaks, a noticeable sag appearing in the 40s or a bit later (e.g., Abt 1983; Andrews 1979; Blackburn et al. 1978; Dennis 1966; Diemer 1974; Pelz & Andrews 1976, chap. 10; Roe 1965; Stern 1978). On other occasions the bimodal curve consists of an age peak at the expected location, but with a secondary peak, small but still observable, around retirement age (e.g., Davis 1954; Haefele 1962, pp. 235–236). I can respond to these seeming discrepancies with the following four points.

First, the departures from the hypothesized single-peak function, when found, are seldom major. Higher-order polynomial terms beyond the linear and quadratic usually explain only a small proportion of additional variance, less than 7% (e.g., Bayer & Dutton 1977; Simonton 1977a, 1984b). It is for this reason that the predicted age curve still fits the data well even when some sort of momentary mid-career dip is evident.

Second, several of the reported aggregate tabulations may be guilty of the compositional fallacy warned against earlier; that is, productivity in two distinct disciplines with age peaks far apart may be summed across to yield a combined age curve with a spurious bimodal distribution (Simonton 1984b). For example, if agewise output is aggregated across both pure and applied mathematics, a saddle-shaped tabulation can emerge that fails to describe the real career trends in any of the mathematicians in the sample (cf. Dennis 1966; Stern 1978).

Third, in some cases the multiple peaks may not be artifacts but rather may represent mid-life career changes that entail a switch in the nature of
creative output and hence a modification of information-processing requirements (cf. Peltz & Andrews 1976, chap. 11). For instance, it is common for scientists to shift from an emphasis on original theoretical or experimental research to more scholarly endeavors, such as history and philosophy. Because the age maxima are later for scholarship than for most scientific work—and such scientists have essentially launched a totally new career with a reset professional age—a secondary peak could naturally result. A similar phenomenon can occur in artistic creativity. For example, the age peaks for art songs, symphonies, and operas are separated by many years, and thus a composer who first created songs, then symphonies, and closed with operas could end up with a trimodel agewise distribution for contributions (cf. Lehman 1953a, chap. 20).

Fourth, we have already acknowledged that extrinsic factors may deflect the age curve insofar as they interfere in some way with the chance-permutation process. It is conceivable, therefore, that some supposed saddle-shaped age curves are in truth single-peak functions with a depression imposed by extraneous circumstances. Thus, a sag in the 40s may reveal more about the stresses associated with “mid-life crises” in creators’ personal affairs than about bona fide shifts in the intrinsic ability to generate chance configurations. Or this same depression may result from a scientist’s assuming administrative and “gatekeeping” roles, such as journal editorships or positions whose new responsibilities temporarily disrupt the research routine; scientists tend to assume these roles shortly after they reach their peak productive age (see Diemer 1974; Stern 1978; Zuckermand & Merton 1972; cf. Jernegan 1927). Similarly, the modest resurgence in output sometimes observed toward retirement age may betray changes in the time committed to research when an academic career is winding down (cf. Haefele 1962).

These four points affirm that this theory need not be drastically modified in order to explain the central findings on the connection between age and output. However, I would like to complete our discussion of this issue by introducing two potential amendments to the information-processing model.

First, the conception of creative potential so far given is likely too passive, even stagnant. Creative scientists are presumed to commence their careers with a fixed quantity of potential, conceived as the number of possible chance permutations they can realize, which thereafter is inexorably expended for the remainder of their lives. Yet without modifying the predictions of the model or the tenets of the theory, we can offer a more dynamic, more interactive version of this key idea. Certainly, a sizable portion of a scientist’s creative potential stems from the responses he or she receives from colleagues in the scientific community after the career has already begun. Another researcher, even a stalwart opponent, may publish a theoretical, methodological, or empirical critique of one’s work that implants new ideas, new elements in subsequent ideations. In addition, empirical researchers may receive further support from the hypothesis-disconfirming results they obtain in the laboratory, results that oblige them to rethink cherished ideas. Therefore, we may reformulate creative potential as the total number of conceivable chance permutations, taking into consideration the most probable array of responses from the creator’s environment. Hence, two individuals may begin their careers with equivalent creative potential under the old definition but end up having disparate potential output by the new definition because of disparities in intellectual reactions from fellow scientists and in the magnitude of empirical reality testing. This altered conception of creative potential is now interpersonal and ecological, even sociocultural, rather than strictly psychological, but it does not appreciably alter the model’s consequences. As long as we assume that the odds of receiving such stimulating recharges are more or less randomly distributed throughout the career, the same age curves and cross-sectional correlations will result.

In addition, we must reconsider the form of the predicted age curve in cases of extraordinary creative potential. We have up to this point postulated that the elaboration rate is proportional to the total supply of accumulated creative ideations—that the rate at which communication configurations emerge is proportional to the backlog of chance configurations. This assumption may be reasonable enough for those of low to moderate creative potential, but what about those whose capacity for chance permutations is far out on the right tail of the skewed distribution? Surely there is an upper bound in time and energy available for converting the initial germs into finished products. Accordingly, the most prolific scientists, the geniuses par excellence, will gather “works-in-progress” faster than they can dispose of them as publications. The result is a ceiling on the rate of contribution, an upper limit that will necessarily flatten out and extend the hypothesized age peak into a high plateau. In extreme cases, the peak is virtually nonexistent, as the scientist never succeeds in catching up with his or her imaginative powers and still has so much left undone when biological reality terminates the quest.

Gauss, a phenomenal mathematician, illustrates this possibility. Extremely precocious as well as exceptionally fluent in ideas, Gauss labored feverishly throughout his life to translate all his innovative notions into what he considered publishable form. Apparently he was told one day, while he was deeply enthralled by a problem, that his wife was dying, but
he muttered, "Tell her to wait a moment till I'm through." Whether or not this is true, after he himself died, Gauss left piles of unpublished manuscripts containing ideas that anticipated much of the work of his contemporaries and successors. Thus, in cases of truly unusual scientific genius, the age curve may level off and decline only slightly in the last decades of the career. The predicted curve graphed in Figure 4.2 may be distorted not by extrinsic variables but by the fact that there are only 24 hours in a day.

Quantity and quality

Thus far we have been speaking of productivity rather than creativity per se, although at times I have hedged with the expression "creative productivity." Actually, on both empirical and theoretical grounds, these two concepts can be used almost interchangeably; productivity, or total quantity of output, is intertwined with creativity, or selective quality of output. Those scientists who claim the longest bibliographies also claim the longest lists of notable contributions, and hence the most impressive ultimate fame (Dennis 1954a,b; Helmreich et al. 1980; Segal et al. 1980; cf. Rubin 1978).

This link between quantity and quality is perhaps most thoroughly established in research that uses the Science Citation Index (SCI), a bibliographic tool devised by Eugene Garfield, founder of the Institute for Scientific Information (see Garfield 1979). From the SCI one can tabulate the number of times a scientist's published work is cited in the professional literature, at least as registered in the technical journals. Such citations exhibit two significant relationships. First, the number of citations that a scientist earns is the single most accurate predictor of scientific distinction, as gauged by such rare honors as the Nobel Prize (Ashton & Oppenheim 1978; Clark 1957; Cole & Cole 1967, 1971, 1973; Crandall 1978; Gaston 1978; Myers 1970; Rushton 1984; Rushton & Endler 1979; Simonont 1984h). Scientific acclaim requires that a researcher publish work that colleagues find useful or in some manner find impossible to ignore in their own work. Second, the primary predictor of the citation count is the scientist's total output (Busse & Mansfield 1984; Cole & Cole 1973; Davis 1987; Gaston 1978; Gupta, Gilbert, & Pierce 1983; Helmreich et al. 1980). Overall, the correlation between total productivity and citation counts ranges from .47 to .76, with a coefficient midway between these two values being fairly typical. Even when we restrict our attention to the citations received by the scientist's best work, that count is still a positive function of total output; the correlation between the number of citations of the three most-cited papers and the total number of papers published was .72 for a sample of physicists (Cole & Cole 1967). The connection between quantity and quality holds for larger units of analysis as well: Those academic departments that publish the most receive the most citations (e.g., Endler, Rushton, & Roediger 1978), and those nations that contribute the most total research also tend to make the most notable contributions (e.g., Lawani 1986).

Many critics of citation analysis have pointed out many problems with such bibliometric tabulations. For example, methodological papers usually are cited far more often than are theoretical or empirical papers (e.g., Folly, Hajtman, Nazy, & Ruff 1981; Peritz 1983); research papers become obsolete as their key findings are incorporated into the "common knowledge" of a discipline (e.g., Abt 1983; MacRae 1969; Price 1965); citation rates may be vulnerable to the effects of personal influence on students and colleagues, so that the rates change after the death of a particular investigator (Trimble 1986); papers originating in prestigious institutions may be cited more often even if those papers are equal in quality to papers coming out of less impressive laboratories (e.g., Cole 1970; cf. Stewart 1983); and citations practices make it possible for a scientist "to chalk up high citation counts by simply writing barely publishable papers on fashionable subjects which will then be cited as perfunctory, 'also ran' references" (Moravec & Murugesan 1975, p. 91) - not even considering such methodological difficulties as how to handle multiple authorship (e.g., Ashton & Oppenheim 1978; Lindsay 1980). Despite these complaints, citations remain one of the best objective and quantitative indicators of scientific worth (see also Cole & Cole 1971; Endler 1987; Folly et al. 1981; Rushton 1984; Shadish 1988; Simonton 1984h).

More important, the respectable covariation between quantity and quality can be demonstrated without resorting to citation data. Thus, the total number of published items that could be claimed by a 19th-century scientist correlates .46 with whether that same scientist was honored with an entry in the Encyclopaedia Britannica in the middle of the 20th century (Simonton 1981b); not a single scientist so honored published fewer than seven contributions (Dennis 1954a). Or to gauge success in more contemporary terms, those psychologists who were elected president of the American Psychological Association, became starred scientists in American Men of Science, or were invited to contribute autobiographies to History of Psychology in Biography almost invariably came from that upper crust of researchers in the top 10% in total output (Dennis 1954c). Similarly, mathematicians rated as extremely creative by colleagues published at over triple the pace of a matched group of less highly esteemed mathematicians (Helson & Crutchfield 1970). And total output in high-energy physics correlates .53
with recognition, the last defined as membership in honorary societies, editorships, and other professional acknowledgments of impact (Gaston 1973, p. 56). Even in the realm of obtaining extramural funding for research, the best predictor of having a grant proposal approved is the total number of proposals submitted (Cole, Cole, & Simon 1981). Hence, the positive cross-sectional association between quantity and quality is virtually universal, for it transcends indicator operationalization and unit definition.

After reviewing results such as these, Dennis (1954a, p. 182) concluded:

I submit that the correlation between fame and fecundity may be understood in part in terms of the proposition that the greater the number of pieces of scientific work done by a given man, the more of them will prove to be important... Other things being equal, the greater the number of researches, the greater the likelihood of making an important discovery that will make the finder famous.

Hence "the likelihood of achieving a certain degree of eminence increases with the number of publications. In science, quantity and quality are correlated, although they are not identical" (p. 183). Actually, this idea was already nearly a century old by the time Dennis was writing, for the philosopher Alexander Bain (1855, p. 597) held that "the greatest practical inventions being so much dependent upon chance, the only hope of success is to multiply the chances by multiplying the experiments."

More recently this position was formally expressed as the "constant-probability-of-success model," which simply holds that the odds of making a contribution are a straightforward probabilistic consequence of total output (Simonton 1977a, 1984d, 1985b). Moreover, it has been argued that this model follows as an essential corollary of Campbell's (1960) blind-variation and selective-retention model. Accordingly, the covariation between quality and quantity can be derived from the chance-configuration theory as well. This derivation follows from the assumptions that any given chance configuration is but a random sample of all possible configurations obtainable from a given scientist's creative potential and that any one scientist's reservoir of potential chance configurations is likewise but a sample drawn from the larger pool of conceivable configurations in the larger scientific community. Therefore, whether any particular configuration will prove successful by winning social acceptance will depend in large measure on chance—on the luck of the draw. This tendency, of course, is analogous to what occurs in biological evolution, that those organisms within a species that produce the most offspring have the best odds of contributing to the population's gene pool.

To be sure, there apparently are exceptions regarding the close link between quantity and quality, for the correlation is by no means perfect.

To use the terminology introduced by the Coles, even if the majority of researchers in any discipline fall along the continuum connecting the "silent," who produce little and that of minimal worth, and the "prolific," who generate an impressive bibliography studded with significant contributions, two sorts of "outliers" appear in the graph as well: "perfectionists," who devote all their efforts to a handful of supreme contributions, and "mass producers," who churn out hundreds of worthless items (Cole & Cole 1973). Even so, the exceptions are nothing more than rare departures from a pervasive rule and would be expected anyway, according to theory. The more communication configurations that are made available for social selection, the higher will be the odds that one will earn acceptance, but those odds function only on the average or in the long run. If we compared samples of scientists with equal productivity levels, their contribution levels would not be exactly equal but rather would be probabilistically scattered around some central value. There will always be scientists who, by chance alone, will get a series of lucky hits, whereas other scientists will labor away without recognition in some cul-de-sac of science.

Furthermore, a portion of the scatter around the regression line can be ascribed to individual differences in the efficacy or application of variation generation. Many mass producers subject only a few mental elements to chance permutations, so that an entire corpus offers only a few choices to the selection process. Indeed, some mass producers do not use chance permutations at all but, rather, are engaged in routine research programs that apply unchanging techniques and heuristics to the same problems over and over. On the other hand, many perfectionists generate a few distinct configurations that cannot be considered merely as minor variations on the same theme. Some, such as the mathematician Gauss, apply extremely rigorous standards in determining which chance configurations will be given to the world for social selection, and thus their publications have been selected in advance. Sometimes this discretion is unfortunate, for society is not allowed even to see some potential landmark discoveries; for example, Gauss never gave the scientific community an opportunity to see the non-Euclidean geometry he had been working on.

These qualifications should not blind us to the fact that the proportion of hits to total shots remains more or less constant from one scientist to another. As a case in point, despite the substantial variation in both productivity and citations, the average citation rate per bibliographic item fluctuates relatively little from one scientist to another, and any individual differences fail to correlate with overt success (e.g., Davis 1987; Simonton 1985b). Further, the constant-probability-of-success model, and the chance-configuration theory from which it can be derived, has two additional implications
concerning the nature of scientific creativity—implications that have received empirical endorsements. One consequence concerns how a scientist’s odds of producing an influential work are distributed over the course of a career, and the other concerns the long-term impact of a scientist’s body of work on his or her reputation.

The quantity–quality ratio within careers

The constant-probability-of-success model should apply not merely across careers but within careers as well (Simonton 1984d, chap. 6). That is, those periods in which a scientist is most productive should also be those in which the most exceptional contributions are made, thereby making quality a probabilistic result of quantity. If the sole generator of configurations is a chance-permutation procedure and if that procedure is truly as blind or haphazard as assumed in chapters 1 through 3, then the proportion of successful to failed ideas should not change systematically over a scientist’s career. Again, the situation is analogous to biological evolution: Those periods in an organism’s reproductive life in which the most offspring are produced are more likely, on the average, to be the same periods that produced offspring that survived and reproduced to continue the parental contribution to the gene pool—for organisms cannot through experience learn to improve the adaptive fitness of the genetic combinations they create each breeding season.

There is already ample evidence of this longitudinal extension of the model (e.g., Oromanan 1977; Simonton 1977a, 1984d, chap. 6, 1985b; cf. Lehman 1962). For instance, one study of eminent psychologists (Donald Campbell, curiously, among them) divided their entire output into major and minor items according to the frequency of citation in the professional literature (Simonton 1985b). Both major and minor works tended to covary within any given career even after separating out the overall age trend ($r = .23$). Further, the “quality ratio,” or the ratio of major to total publications per time unit, exhibited no systematic change over time. The ratio neither improves nor deteriorates, nor does it adopt some curvilinear form. Rather, the proportion of successful communication configurations fluctuates randomly throughout the career. Some periods have more hits, others more misses, without any predictability, whether ascribable to time trends or autoregression. This result contradicts the suggestion that the cross-sectional nexus between quantity and quality is a mere consequence of the more prolific scientists’ gradually learning what makes good science—the “practice makes perfect” hypothesis (cf. Lawani 1986). Researchers evidently cannot learn how to improve, by means of maturity and experience, the proportion of hits. At the same time, these data show that the scientist does not usually exhaust the best combinations at the beginning of his or her career, only to peter out as the career drags out (cf. Parnes & Meadow 1963). As we would expect from the assumption that creativity is founded on the haphazard collision of elements, successful combinations—in proportion to unsuccessful combinations—are randomly distributed throughout the career.

Moreover, the constant-probability-of-success model apparently applies just as accurately to artistic careers: One inquiry into the agewise output of classical composers arrived at the same conclusions as found for scientists (Simonton 1977a). In concrete terms, those career periods in which the most masterpieces were created were also mainly those that saw the most potboilers or pièces d’occasion. As was found for scientists, this linkage held whether we examined the overall age trend or the period-to-period fluctuations (in the latter case, $r$’s between .32 and .45). It is as if even the greatest musical creators were unable to separate the grain from the chaff. Indeed, the favorite works of the most applauded composers are not necessarily the same as those most appreciated by audiences today. For example, Beethoven’s personal favorites among his symphonies, quartets, and sonatas are not identical with those that dominate the concert halls and recording studios—such as the Fifth Symphony and the Moonlight Sonata. This inability to identify one’s “best” work also frequently appears in the sciences. In retrospect, Newton wasted a good deal of time on alchemical pursuits that contributed nothing to the advance of science, time that might have been better spent on further investigations in mathematics, mechanics, and optics.

In 1891 Helmholtz, upon reflecting on the similar lack of correspondence between his own appraisal of his work and the assessment of his contemporaries, provided a plausible explanation for this discrepancy:

My colleagues, as well as the public at large, evaluate a scientific or artistic work on the basis of its utility, its instructiveness, or the pleasure which it affords. An author is more inclined to base his evaluation on the labor a work has cost him, and it is but rarely that both kinds of judgment agree. Indeed, we can see from occasional statements of some of the most celebrated men, especially artists, that they assign small value to achievements which seem to us inimitable, compared with others which were difficult for them and yet which appear much less successful to readers and observers. I need only mention Goethe, who once stated to Eckermann that he did not value his poetic works as highly as the work he had done in the theory of color. (Kahl 1971, p. 467)

Part of the disparity between personal effort and social effect may stem from the fact that some chance configurations are more easily transformed
into communication configurations, but this ease of translation may not correlate with the value of the final product. More pertinent, perhaps, is the fact that the published communication configuration seldom conveys the difficulties that the investigator faced in creating it. Indeed, in the same speech Helmholtz later admitted:

But the pride which I might have felt about the final result in these cases was considerably lowered by my consciousness that I had only succeeded in solving such problems after many devious ways, by the gradually increasing generalisation of favourable examples, and by a series of fortunate guesses. I had to compare myself with an Alpine climber, who, not knowing the way, ascends slowly and with toil, and is often compelled to retrace his steps because his progress is stopped; sometimes by reasoning, and sometimes by accident, he hits upon traces of a fresh path, which again leads him a little further; and finally, when he has reached the goal, he finds to his annoyance a royal road on which he might have ridden up if he had been clever enough to find the right starting-point at the outset. In my memoirs I have, of course, not given the reader an account of my wanderings, but I have described the beaten path on which he can now reach the summit without trouble. (Helmholtz 1898, p. 282)

There is theoretical justification for scientists being so silent about their actual paths to their discoveries. The difficulties encountered en route to a solution are only partly connected with the problem's intrinsic complexity, for the road may be arduous or easy largely due to the whims of the chance-permutation procedure, as we documented in chapter 2. By deleting the narration of how an idea was finally reached, scientific colleagues are better able to judge contributions on their merits rather than on the basis of the contributor's luck. Even so, because the contributors themselves remember their hardships and frustrations, they will have very different favorites among their conceptions in comparison with those of their contemporaries. Creators are thus not the best judges of their own work.

But to return to the main point, a valuable implication can be deduced from the fact that the constant-probability-of-success model applies to within-career fluctuations, an implication that enhances our appreciation of how scientific creativity changes with age. The curve given in Figure 4.2 may look discouraging to those researchers already "past their prime." Yet we must repeat that this graph describes aggregate output. If instead we plotted the quality ratio—the proportion of hits to total tries—for single contributions as a function of age, a horizontal line would result. What this zero-slope linear function tells us is that contribution for contribution, or paper for paper, age becomes irrelevant as a predictor of scientific impact. To illustrate, one citational analysis of the fate of single sociological papers in the first decade after publication concluded that "there are no differences in the mean number of citations received by articles written by sociologists of various ages" (Oromaner 1977, p. 383). Hence, if we are reading an individual paper by some scientific colleague, any knowledge we have of the author's age will not help us determine whether that particular contribution will be dubbed "creative."

Admittedly, the colleague's age would, according to this theory, bear some strong connection with total output for a given year, information that would help us predict the odds of a hit for that year. Even so, productive quantity alone serves as the direct cause in this case, whereas age is solely an indirect factor behind quality output. In addition, despite the absence of a systematic change in the ratio of quality to age, that ratio is by no means constant over time but, rather, fluctuates randomly over a career. Consequently, even if we have a single paper written by an older colleague and we know that it represents that scientist's sole offering for that year, we have no special trick for determining whether or not this particular time is one of the author's lucky ones—when the success rate reaches 100%. The chance-configuration theory simply does not justify using age in a prejudicial manner to discount the worth of a scientific publication. And needless to say, this same silence applies equally to the novices whose productivity has yet to reach the peak rate. No matter where a scientist might be placed on the abscissa of Figure 4.2, each prospective contribution must be weighed on its own merits, leaving the author's age off the scales.

The durability of reputation

The constant-probability-of-success model also helps account for the notable durability of scientific reputation over time, even in the hindsight of centuries (Over 1982; Simonton 1984d, chap. 1). It is frequently observed that citation rates are extremely stable, that those authors who receive much attention in one year in the professional literature are largely those often referred to in consecutive years, with correlations typically in the upper .90s (see, e.g., Helmreich, Spence, & Thorbecke 1981; Rushton 1984). What is less often noted is that this reputation normally endures for generations. Notwithstanding occasional exceptions—Mendel offering the classic instance—scientists who attain fame in their own day tend also to be highly regarded by posterity, and the initially obscure remain so. One investigator concluded, for instance, that "in the case of psychology there was no individual who was markedly out of favor in 1903 but markedly in favor in 1966–1970, or vice versa" (Over 1982, p. 60); the "test–retest" reliability, or correlation across nearly three-fourths of a century, was .72. Scientists with enduring reputations are those who have staked their claims to eminence on a respectable body of varied contributions, and accord-
ingly, their status does not rise or fall with the fate of single contributions. If one idea becomes outdated, another has high odds of picking up the slack. Newton's fame rests on far more than the *Principia Mathematica*, Darwin's on much more than the *Origin of Species*; Nobel laureate Max Born said that Einstein "would be one of the greatest theoretical physicists of all times even if he had not written a single line on relativity" (quoted in Hoffman 1972, p. 7). Often the contributions for which scientists are best known today are not identical with those that earned them renown in their own times. Einstein was honored with the Nobel Prize in 1921, 16 years after he began publishing on special relativity and 6 years after his work on general relativity began appearing in the journals. But the prize was not for relativity but, rather, was for his pioneer 1905 paper on the photoelectric effect and certain unspecified "services in theoretical physics."

It is worth pointing out that the same durability of reputation found in the sciences applies equally to the arts and humanities (e.g., Farnsworth 1969; Rosengren 1985; Simonton 1984d, chap. 1). Plato's reputation is not totally dependent on *The Republic*, nor Shakespeare's on just *Hamlet*, nor Michelangelo's solely on the Sistine Chapel ceiling, nor Beethoven's on the Fifth Symphony alone. No matter what creative endeavor we inspect, the stature of the individual creators is far more stable over time than is the status, epistemological or aesthetic, of the separate creative products on which their reputations rest (see also Simonton 1976a).

Nevertheless, if we can expect prolific scientists to enjoy high odds of success, they should also have fairly good opportunities to fail. Not all chance configurations, not even all those selected for conversion into communication configurations, will earn social acceptance, and some will even prove quite controversial. Sometimes an original notion will be controversial because it is revolutionary and thus difficult to assimilate without an exhaustive intellectual reorganization by potential acceptors. But an idea may also provoke debate because it is a spin-off from a mind on an off-hour and is plain wrong. That is, it is a myth to assert that genius is always in the right (Weisberg 1986). For example, Galileo insisted on circular orbits to the point of denying the cosmic reality of comets; Newton dogmatically maintained that an achromatic lens could not possibly be constructed; Darwin compromised his evolutionary theory with the doctrine of pangenesis; and Einstein persevered in advocating a totally deterministc unified theory that ignored the advances made by the Copenhagen school of quantum mechanics. It is virtually impossible to identify a prolific scientist who was right all the time—and some, such as Lord Kelvin (William Thomson), are nearly as famous for their errors as for their triumphs. Likewise, for any productive aesthetic creator we can easily list many compositional failures.

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What W. H. Auden said of poets applies equally to creators in all domains: "The chances are that, in the course of his lifetime, the major poet will write more bad poems than the minor" simply because major poets "write a lot" (quoted in Bennett 1980, p. 15). We may style this tendency the principle of the "constant probability of failure."

This last principle, when coupled with what we noted earlier, again implies that creative scientists, even the highest-caliber geniuses, are not always the best judges of what their successes and failures are among the profusion of ideas that they thrust before the world. If the particular items on which a scientist's reputation is based may change over time, if even the most illustrious scientists can make big mistakes as well as milestones, and if scientists appear unable to improve their success rate with enhanced professional experience, then the expertise underlying scientific creativity seems largely restricted to the cognitive capacity and the motivational wherewithal for generating numerous and diverse intellectual variations. Nonetheless, it would be rash to surmise from this broad principle that productivity is sufficient to achieve success in science or any other creative endeavor. Distinction in any domain depends directly on the social acceptance of a large body of communication configurations, and mere productivity by no means ensures a hit; it only raises the odds. Stated differently, the proximate cause of acclaim is quality, not quantity, and quantity may have solely a probabilistic linkage with quality. Consequently, the direct impact of quantity on fame, after excluding its indirect effect by way of quality, should be tiny if not zero. Perhaps the mass producers may gain a little by impressing their colleagues with their energy and determination, but ultimately the laurels will go to those prolific creators who have persuaded members of the scientific community to restructure their thinking about the phenomena that define the discipline.

To illustrate this point, let me reanalyze some data reported by Cole and Cole (1973, p. 94) that they analyzed differently and, according to this theory, incorrectly. Taking a sample of physicists, the Coles showed that quantity and quality, or total publications and citations, correlated .60, whereas the correlations of these two measures with an index of scientific awards and honors were .45 and .57, respectively. With regard to honors, not only is the association for quantity less than that for quality, but much of this correlation may be the spurious upshot of the other two correlations. Therefore, we must determine what proportion of the variance in awards is determined by quantity, once we have taken out the variance directly attributable, on theoretical grounds, to quality. In other words, we should perform a hierarchical regression analysis (not a simultaneous analysis, as conducted by the Coles), in which quality is entered first as the indepen-
dent variable and then quantity is added. Using this design, quality handles 32% of the variance in scientific kudos, whereas quantity increases this predictive power to only 34%, an almost trivial increment.

The lesson is manifest: Even if it may be more advantageous to be a mass producer than to be merely silent, it is far better still to be prolific in the positive sense. Scientists who labor hard at ballooning their “curriculum vitae” with pages of bibliography may also be prolific producers of high-quality efforts, but the possibility always remains, albeit with smaller odds, that such scientists may have done no more than build up long, almost autistic lists of unread and unappreciated titles. Quantity divorced of quality is indicative of little scientific creativity.

**Discussion**

This theory also has implications for three topics closely related to the issues discussed so far. Although all three of these topics were introduced solely in terms of scientific productivity and influence, I believe that they are more generally applicable, extending to artistic creativity definitely and to leadership possibly. These three topics are the Ortega hypothesis, the Yuasa phenomenon, and Planck’s principle.

**Ortega hypothesis**

In *The Revolt of the Masses* (1957), the Spanish philosopher Ortega y Gasset asserted that it is necessary to insist upon this extraordinary but undeniable fact: experimental science has progressed thanks in great part to the work of men astoundingly mediocre, and even less than mediocre. That is to say, modern science, the root and symbol of our actual civilization, finds a place for the intellectually commonplace man and allows him to work therein with success. (pp. 110–111)

This proclamation offers a very attractive idea insofar as it pictures scientific creativity as far more democratic than the image so far portrayed in this chapter. If Ortega is correct, then the elitism apparent in scientific productivity may be misleading, and even the term scientific genius may prove meaningless from the standpoint of the advancement of science. The scientific edifice is built piece by piece using small bricks mostly laid by undistinguished craftsmen. Hence, anyone with the appropriate training in the trade of science may participate in the construction of even the most impressive monuments (see also Spiller 1929).

Cole and Cole (1972) christened this intriguing notion the “Ortega hypothes” and observed that practicing scientists, not just armchair philosophers, have subscribed to the same conception of scientific creativity. They gave the example of Howard Florey, who shared a Nobel Prize with Fleming and Chain for the development of penicillin. In his last presidential address before the Royal Society, Florey observed that science is rarely advanced by what is known in current jargon as a “break-through,” rather does our increasing knowledge depend on the activity of thousands of our colleagues throughout the world who add small points to what will eventually become a splendid picture much in the same way that the Pointillistes built up their extremely beautiful canvasses. (quoted in Crowther 1968, p. 363)

Newton may have said that he saw farther than his contemporaries because he stood on the shoulders of giants, but according to Ortega and Florey he more probably crouched on the backs of dwarfs—and may have been a dwarf himself!

Fortunately, the Coles subjected this hypothesis to empirical tests (Cole & Cole 1972). Looking at physics, they examined whether the classic contributions (i.e., the most frequently cited papers) leaned heavily on the publications of lesser-known figures in the field. Quite the opposite was found to be the case: Often cited papers tended to cite publications by often cited predecessors—thus reestablishing the elitism inherent in scientific creativity (see also Snizek 1986). Similar results have been obtained for other scientific fields, such as criminology (Green 1981) and sociology (Oromuner 1985). Indeed, if anything, the hard evidence seems to suggest that the mediocre scientists that Ortega so praises are largely ignored by their colleagues. In any given year, around 35% of all existing papers are cited not even once, another 49% just once, 9% twice, 3% three times, 2% four times, 1% five times, and the remaining 1% six times or more (Price 1965). Hence an extremely small proportion of the published papers are extensively used by others in the same discipline. In 1910, James McKee Catell, when listing some of the unanswered questions that must be addressed by a science of science, stated that “we do not know whether progress is in the main due to a large number of faithful workers or to the genius of a few” (p. 634). We now have evidence that this issue has been resolved in favor of the second alternative.

Looking at this problem solely from the standpoint of the chance-configuration theory, Ortega may have captured at best a partial truth. On the one hand, according to the constant-probability-of-success model, the odds of making a notable contribution to science is a function of the total number of attempts. Consequently, highly prolific scientists will tend, on the average, to exert more influence over the thoughts of their colleagues. Science is dominated by those intellectual leaders whom we may rightly call
geniuses, making the enterprise more elitist than democratic, albeit a meritorocratic elitism. On the other hand, as Campbell (1960, p. 393) noted, it is “likely that many important contributions will come from the relatively untalented, undiligent, and uneducated, even though on an average contribution per capita basis, they will contribute much less.” After all, the connection between quantity and quality is far from perfect, with a wide dispersion around the regression line, readily enabling the existence of “outliers” consisting of relatively unproductive but nonetheless influential creators. And by the Lotka and Price laws we know that there is an immense number of mediocre scientists in the world, and so by chance alone a significant number will experience a stroke of phenomenal luck. Even so, these constitute true exceptions to a general rule. The odds are against an average scientist contributing to the advance of science. In this sense, then, the Ortega hypothesis is not just empirically inaccurate but also theoretically implausible.

We must be cautious about drawing policy recommendations from the rejection of the Ortega hypothesis. The Coles implied that the unpraised and unsung scientists of the world might be excluded from the profession without affecting the growth of scientific knowledge (Cole & Cole 1972). The condition of science could even be improved if scarce resources were transferred from the mediocre to the elite scientists. This suggestion has been criticized by those who speculate that members of the scientific elite are merely selected at random from the larger pool of talent and then are showered with citations and honors, not according to intrinsic merit, but by the luck of the draw. This alternative interpretation of the same data has been termed the “Ecclesiastes hypothesis” after the passage (9:11) that asserts, “The race is not to the swift, nor the battle to the strong, neither bread to the wise, nor yet riches to men of understanding, nor yet favor to men of skill; but time and chance happeneth to them all” (Turner & Chubin 1976, 1979).

The constant-probability-of-success model introduces a viewpoint somewhat more moderate than either of these two extremes. Let us posit that this model applies not just to individual creative products but to creative persons besides. Then an increase in the total number of scientists should correspondingly increase the number of scientists of quality. Even if according to the Price law, “the total number of scientists goes up as the square, more or less, of the number of good ones” (Price 1963, p. 53), we can still argue that reducing the number of mediocre scientists will not have a beneficial effect. To be sure, it seems reasonable to doubt the utility of these unsuccessful researchers for the whole scientific enterprise. But these supposed failures are actually performing an extremely useful if thankless task—they are being “unlucky.”

Discussion

This is where the Ecclesiastes hypothesis takes an ironic twist. Just as we cannot have winners of a race without having losers too, so we cannot really have successes in the absence of failures. What is required, according to the current theory, are many variations that are open to sociocultural selection. It would be dangerous, therefore, to decide to truncate artificially this individual variation just because some variations seem now to be unadaptive. Even though scientific reputation is largely consistent over historical time, there are enough exceptions to make the wise wary. Today’s mass producers and nearly silent may turn out to be tomorrow’s prolific and perfectionist scientists. Gregor Mendel is everyone’s favorite example. Although there is some debate about how much Mendel was ignored by the 19th-century scientific community (Zirkle 1964), there is no doubt that his prominence in modern science well exceeds what he enjoyed in his own lifetime. Furthermore, scientists do not have to be vindicated posthumously in order to leave an impression on the advance of science, for some investigators succeed by failing. It is intriguing to note that many citations to other people’s works are actually negative, criticizing or rejecting a conclusion posed in the cited reference (Moravcsik & Murugesan 1975). Unsuccessful researchers may document their scientific blind alleys by more informal means, too, such as professional conferences and graduate seminars. Of course, scientific failures become nonentities rather than celebrities, yet that does not lessen their role in the overall progression of science.

Hence, rejection of the Ortega hypothesis does not mean that we can deny resources to all but the currently fashionable élite without thereby retarding the advance of science. Rather, we should encourage all of those who have the intellectual and dispositional markings of scientific creators. Florey believed, despite his adherence to the Ortega hypothesis, that scientists may vary in creative potential, for “one thing that distinguishes the first-rate scientist from those absorbed in routine is that he very dearly loves to make discoveries—he burns to find out something new and he wants to be the first to do so” (quoted in Crowther 1968, p. 362). By distributing resources to all first-rate scientists without regard to outward success we give posterity the idealational diversity necessary for optimal sociocultural evolution.

Yuasa phenomenon

At the close of the preceding section we assumed that the constant-probability-of-success model is just as applicable to individual scientists as it is to separate scientific contributions. In both instances quality is a probabilistic function of quantity; the greater the number of scientists is, the
better the chance will be that great scientists will be found among them. Actually, there already is supporting evidence on this point, not merely for science, but also for all creative activities (Simonton 1984d). Looking at transhistorical fluctuations in the number of creators per generation within a nation, culture, or civilization, we repeatedly discover that both prominent and obscure figures are contemporaries (e.g., Simonton 1974, 1975c; Sorokin & Merton 1935). Two aspects of these generational fluctuations attract our attention here. First, creators of all grades are not randomly distributed over historical time but, rather, cluster together, forming periods of potent activity (in both quantity and quality) separated by usually much longer periods of creative stagnation. This waxing and waning is not cyclical, only aperiodic, but nevertheless quite real. In regard to civilizations, Kroeber (1944) styled these historical gatherings of creators as “configurations.” Second, the creative zenith of one nation (or civilization) normally does not correspond to the high point of another nation (or civilization). It is almost as if distinct cultural traditions take turns experiencing a golden age in a particular creative enterprise. However, it sometimes appears as if the flourishing of activity in one nation or civilization may stimulate a subsequent surge in a nearby nation or civilization. The center of creative activity accordingly may shift across both space and time, as the torch is passed on.

Yuasa (1974) was interested in this phenomenon in the case of science (see also Schneider 1937). In particular, he wanted to determine how and why the center of scientific activity in Western civilization has shifted from one nation to another since the Renaissance. Using archival data, including both a chronological table of scientific achievements and a biographical dictionary of outstanding scientists, Yuasa defined the “scientific prosperity” in which a nation was a major force in science “as the period in which the percentage of scientific achievements of a country exceeds 25% of that in the entire world in the same period” (p. 81). Under this definition the center of scientific activity has shifted as follows:

1540–1610 Italy (Florence, Venice, Padua): Galileo
1660–1730 England (London): Newton
1770–1830 France (Paris): Lavoisier, Laplace
1810–1920 Germany (Berlin): Gauss, Liebig, Helmholtz
1920– United States (New England, California): Hubble, Lawrence

It is apparent that the main current of scientific activity lingers in a given center for an average of nearly 80 years, a point that Yuasa noted.

Although one may quibble over specific dates in Yuasa’s periodization, his conclusions were predicated on the application of a precise definition to archival data of demonstrated reliability (see, e.g., Simonton 1976b; but also see Nye 1984). Thus, we should take his scheme seriously enough to ask: Why does one nation become a scientific center only to yield ground after a mere handful of generations to a rival? Yuasa’s hypothesis (1974) was that “there seem to be necessary relations between political revolution and scientific revolution” (p. 95). To make his case, Yuasa analyzed the political foundation of French scientific prosperity. From 1750 to 1800 the French social structure was in a state of “anomy,” as indicated by such events as the 1751–1772 Encyclopédie of Diderot and d’Alembert, the 1755 Discourse on the Origin of Inequality of Rousseau, and, most conspicuously, the 1789–1795 French Revolution that toppled the monarchy and aristocracy. These events helped create the conditions most conducive to the development of French scientific genius. Although Yuasa’s discussion of this subject is strictly qualitative, historiometric inquiries partly support his speculations, as we shall discuss more fully in chapter 5.

But once the proper milieu is established for stimulating scientific growth and a nation becomes a hub of activity, a decline begins. Yuasa never fully explained why this happens, but we can offer a suggestion based on the present theory. Kroeber (1944), when discussing why cultural configurations come and go, referred to the phenomenon of “pattern exhaustion.” That is, each cultural tradition commences its creative activities with a given pattern or paradigm that is developed to its fullest by successive generations of creators. A high point is reached after which the potentialities of the finite set of cultural premises lose their capacity to conjure up new ideas, and so the creative ebb and flow fades away after the splendor of the golden age. In science, the operation of this exhaustion process takes place when, in Bartlett’s (1958, p. 136) words,

a mass of routine thinking belonging to an immediately preceding phase of original work has come near to wearing itself out by exploiting a limited range of techniques to establish more and more minute and specialized detail. A stage has been reached in which finding out further details adds little or nothing to what is known already in the way of opening up unexplored relations.

To illustrate, toward the close of the last century, the physics section of the catalog of the University of Chicago—the department then chaired by Michelson—suggested that the laws of nature were so well established that little further research remained to be done except to determine to more decimal places the various fundamental constants—this on the eve of the quantum and relativity revolutions!

The remarkable fact about Kroeber’s configurations, as well as Yuasa’s centers, is that the timewise distribution is quite similar to the curve shown in Figure 4.2. Creativity tends to increase rather rapidly to a single peak
and afterwards trail off slowly but usually irreversibly. To be sure, the initial portion of the curve tends to adopt a form like the logistic function, indicating exponential growth limited by an upper bound (Crane 1972, p. 172; Mulkay, p. 18; Price 1963, chap. 1). Even so, could the same process be responsible for both careers and centers? Let us suppose that each cultural tradition begins its creative life with a collection of cultural traits that, as mental elements, can then be recombined in diverse permutations to generate a large but finite number of configurations. Each creator in the tradition acquires a sample of this population of available elements, subjects them to chance permutations, and feeds the resulting communication configurations back into the system, there to become new elements to enter into permutations executed by subsequent generations of creators. If the creative potential of the sociocultural system is sufficiently high at the outset, these givens will at first encourage this activity, which will attain an acme and maybe even grant the system momentary status as a center of creative prosperity. Nevertheless, the number of worthwhile configurations yet to be derived from the initial cultural elements will invariably decline, causing a drop in output, and eventually the system will recede into the second rank and lower.

Analogies between entities at two levels of analysis are always risky, and so I do not wish to push the foregoing too far. We must not forget that there is no “group mind” that performs the chance permutations. Rather, the creative process dwells solely within individual members of the group. Accordingly, we may speak of the aggregate-level curve representing the ups and downs in creative activity as a sum of the individual-level curves representing the rise and fall in career output for the numerous careers spanning many generations. This modification in the account permits a logistic curve to describe the growth of a scientific field, even though the productivity age functions retain the same shape shown in Figure 4.2. Further, even this watered-down correspondence between the two levels implies that the analogy can be extended one step farther. Just as we expect productivity to ascend and descend with personal age, according to Equation 4.1, so we expect that the entrance into and exit from dominance enjoyed by a given sociocultural system would reflect a proportional shift in the age structure of its membership. In fact, Yuasa (1974) maintained that “one of the main causes in the decline of scientific activity appears to be the aging of the scientific community” (p. 102); “scientific activity will decrease gradually by the aging of the group of scientists” (p. 97). A follow-up investigation conducted by Zhao and Jiang (1985) provided additional evidence of this Yuasa phenomenon. Specifically they showed that “a country’s science will decline if the average age of its outstanding scientists surpasses the border age of 50” (pp. 65–66), an age cutoff chosen because “the mean value of the terminal age at which the world’s 1,228 noted scientists made their last contribution is exactly 50” (p. 66; cf. Fulton & Trow 1974; Lehman 1958; Raskin 1936). Yuasa may have failed to distinguish symptom from cause, for it seems more likely that the upward shift in mean age reflects the failure to recruit youthful replacements in the beginning ranks. Such an interpretation makes the Yuasa phenomenon a mere tautology, and thus Yuasa’s proposed explanation begs the question. Nonetheless, any movement in the age spectrum away from the optimal age toward older mean ages should signal that a country has entered the downward path from glory.

Yuasa (1974, p. 100) was not afraid to indulge in prophecy, saying that “if this pattern holds true also for the U.S.A., then its scientific prosperity, which began in 1920, will end in the year 2000.” Zhao and Jiang (1985) over a decade later apparently concurred. It is impossible at this time to prove this forecast right or wrong, but it cannot be simply dismissed as the envious and wishful proclamations of foreigners (Japanese and Chinese); even in the United States some people have expressed concern. Mark Oromanian, for instance, published an essay in 1981 ominously entitled “The Quality of Scientific Scholarship and the ‘Graying’ of the Academic Profession.” Besides agreeing with the prediction that the output of truly significant scientific work may decline as the mean age of American scientists continues to grow older, Oromanian warned that the upward shift in age structure may make U.S. scientists, as a group, less receptive to innovative ideas. Without this openness, American science may soon be bypassed by the next scientific revolution and thereby lose its current world hegemony. Oromanian predicated this inference on Planck’s principle.

Planck’s principle

As a scientist’s career advances, his or her mind moves along the cognitive continuum from intuitive to analytical genius. Besides expending creative potential in the process of self-organization, the scientist is also progressively changing his or her receptiveness to alternative ways of organizing mental elements. This means that during a career, a creative person should become increasingly unsympathetic to alternative approaches to structuring the same information. As a consequence, a truly revolutionary restructuring of knowledge may not be as well received. The younger members of the discipline, who have yet to finish organizing their scientific experience, should then display superior receptiveness to new ideas. Charles Darwin (1860, p. 240), in a passage that occurs a bit later than the
quotation in chapter 1 from the *Origin*, said that he looked “with confidence to the future, — to the young and rising naturalists, who will be able to view both sides of the question with impartiality.” Or as Max Planck (1949, pp. 33–34) put it, “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.”

Kuhn (1970) cited this idea as a constraint on scientific revolution, and various sociologists have agreed (e.g., Gouldner 1970, p. 377; Hagstrom 1965, p. 283). Barber (1961, p. 601) concluded his essay on the “resistance by scientists to scientific discovery” with the generalization that as a scientist gets older he is more likely to be restricted to innovation by his substantive and methodological preconceptions and by his other cultural accumulations; he is more likely to have high professional standing, to have specialized interests, to be a member or official of an established organization, and to be associated with a “school.”

There are many anecdotes that show how, with age, once-innovative scientists gradually become stalwart opponents of new ideas: Liebig strongly attacked the biological theory of fermentation advanced by Pasteur, his junior by almost 20 years; Magendie objected to the use of ether as an anesthetic; and Lord Rayleigh could not accept the quantum theory advanced by Planck. Some noted scientists have even admitted to the process by which a flexible and open-minded intellect becomes, with time, committed to only a single point of view. For instance, Sigmund Freud confessed in his *Civilization and Its Discontents*, written in 1929 at age 73 and toward the end of his life and career, that “the conceptions I have summarized here I first put forward only tentatively, but in the course of time they have won such a hold over me that I can no longer think in any other way” (p. 790).

More significant than such anecdotes and testimonials is the fact that Planck’s principle has been endorsed by some empirical research. Hull, Tessner, and Diamond (1978) showed that age bore some relation to the probability of accepting Darwin’s theory, and Diamond (1980) demonstrated a comparable effect for the acceptance of cliometrics by historians. In both instances, around 6% of the variance in innovation acceptance may be attributed to the age of the potential acceptor. This principle seems to apply even to domains beyond pure science. For example, one inquiry into the “relationship between age and innovative behavior” indicated that “younger farmers were more likely than their older counterparts to adopt new agricultural practices” (Green, Rich, & Nesman 1985, p. 255). And in business enterprises, age is negatively correlated with managers’ willingness to take risks (Vroom & Pahl 1971). Although I do not know of any specific research on the question, it is possible that Planck’s principle intervenes in the dissemination of new ideas in the arts as well. Yesterday’s artistic innovators may become tomorrow’s defenders of the aesthetic status quo. Supporting this conjecture, scores on the Barron–Welsh Art Scale, which, as noted in chapter 3, record the preference for complexity, tend to decrease with age (Alpaugh & Birren 1977).

I must stress, nonetheless, that the size of the effect is not large enough to preclude many exceptions to the rule. Around 94% of the variance in the disposition to accept a novel concept has nothing to do with a person’s age. Indeed, the chance-configuration theory leads us to expect that the reality underlying Planck’s principle is far more complex than first meets the eye. Three considerations stand out in particular:

1. In chapter 1 we enumerated several constraints on the social acceptance of an offered communication configuration, constraints that have relatively little, if anything, to do with age. On the contrary, these factors mainly concern how representative the scientist is of the larger scientific community. Are the same mental elements being subjected to permutations? Are these elements in comparable states of cognitive disarray? Has the chance configuration been successfully encoded in a symbolic form that encourages an accurate decoding by potential acceptors? Answers to these questions may not necessarily entail considerations of age (see also Mahoney 1976).

2. If the decline in openness to the originality of others is truly coupled with the corresponding decline in one’s own creative potential, then the age-wise curve describing Planck’s principle should be governed by the same function that, according to the theory, describes the loss in creative potential with increased age. This latter function is defined by the simple formula

\[ x = ke^{-at} \]  

(4.4)

where \( x \) is the creative potential at time \( t \) (i.e., professional age), \( a \) is the ideation rate of Equation 4.1, and \( k \) is an integration constant (Simonton 1984b). If \( x \) at \( t = 0 \) is assumed to be the total number of potential contributions (e.g., publications), then \( k = m \), where \( m \) is the maximum number of contributions possible in an unlimited life span. The curve defined by this equation is shown in Figure 4.3. The corresponding curve depicting how openness to new ideas varies as a function of age should parallel that shown in this figure (cf. Alpaugh & Birren 1977).

3. In calculating the relevance of Planck’s principle, we must consider the initial level of creative potential at \( t = 0 \), or at the career onset. Many members of the younger generation (owing to factors that I shall discuss in the next chapter on developmental antecedents) will already have moved
Summary

We have examined how the chance-configuration theory elucidates the three most significant observations regarding scientific productivity. First, the theory accounts for the extremely skewed cross-sectional distribution: The upper tail of the normal curve for intellectual ability is highly distorted because of the accelerating increase in potential configurations as a function of the number of mental elements available for chance permutations. Second, the theory handles various aspects of the connection between productivity and a scientist's age. Specifically, the theory (1) yields an equation that precisely predicts the shape of the longitudinal curve, including the concave-downward onset, decelerating termination, and single peak; (2) explains interdisciplinary differences in the typical career course in terms of the information-processing demands of each discipline and also introduces the useful notion of the average creative half-life for a given discipline; (3) explains the predictive superiority of professional or career age over chronological age by emphasizing the intrinsic working out of creative potential via the chance-permutation process; and (4) makes theoretically necessary the empirical association among precocity, longevity, and productivity rate (as well as the dependence of the peak age on the discipline and not on the creative potential). Third and last, the chance-configuration theory subsumes the constant-probability-of-success model.

far along toward analytical genius, whereas many members of the older generation, owing to an impressive initial creative potential, will yet have a long way to travel before attaining maximal cognitive order. If the probability of accepting a novel idea is directly proportional to \( x \) in Equation 4.2, then it will also be proportional to \( m \), or the given creative potential. We consequently will have a family of curves such as that shown in Figure 4.3, each with a different intercept on the vertical axis (i.e., \( x = k = m \) at \( t = 0 \)). It is this theoretical complication that permits highly creative scientists to appreciate the latest advances in the discipline even when they are old. To illustrate, Charles Lyell, though a dozen years older than Darwin, was still coordinating paleontological material when evolution by natural selection was announced. Not only did Lyell come around to Darwin's point of view, but he stood amazed that the idea had somehow escaped him.

The interpretation just derived from the chance-configuration theory clearly implies a number of empirically testable propositions. First, professional or career age is more important than is chronological age to determining the force of Planck's principle in a particular case. As an example, a scientist new to the field, even if of mature age, may be more responsive to the most imaginative suggestions. Moreover, when receptiveness to revolutionary ideas is plotted as a function of (professional) age, the resulting graph should not be linear but rather curvilinear—specifically, concave upward, as in Figure 4.3. This signifies that most of the change in openness takes place in the earlier portion of a career; indeed, half of the receptiveness will be lost in the same number of years as defines the half-life of creative potential [i.e., \( (\ln 2) / a \)]. Finally, any estimate of the variance accounted by Planck's principle must allow for individual differences in creative potential (e.g., as evidenced by a potential accepors productivity rate relative to that of others in the same discipline). Much of the longitudinal variation in receptiveness is probably swamped by the considerably larger cross-sectional variation.

When we recognize that all investigations published to date have used chronological age rather than professional age, have been based on linear statistics rather than testing for nonlinear functions, and have made no attempt to control for individual differences in intrinsic openness to the creativity of others, it is reasonable to conclude that the current estimates of the impact of Planck's principle may actually be biased downward.
of the linkage between quantity and quality, a positive (albeit probabilistic) association that has been empirically demonstrated to hold both within and across careers.

After we established the theoretical basis for these core facets of productivity, we treated three subsidiary issues, namely, the Ortega hypothesis, the Yuasa phenomenon, and Planck's principle. We concluded that (1) despite the inherent elitism of scientific influence, the constant-probability-of-success model applies as well to individuals as to products, and therefore lesser scientists are not irrelevant to scientific advance; (2) the changing age structure of a scientific community provides a clue to the group's probable status as a center of science; and (3) the increased resistance to new ideas as a scientist ages is directly related to that scientist's progress in self-organization and is accordingly also related to initial creative potential. In addition, because the phenomenon of productivity operates no differently in the arts than in the sciences, the theory's explanatory and predictive successes may be quite broad and may even be applicable to domains of achievement that stress leadership more than creativity.

The unifying construct underlying most of the theoretical discussion of this chapter is the concept of creative potential. Consequently, our task in the next chapter is to examine the factors that contribute to the development of this potential.

5 Developmental antecedents

How does one acquire that creative potential that leads to a long and productive career as a scientist? Mach (1896, p. 171) again provided the start of an explanation when he said that "if the psychical life is subjected to the incessant influences of a powerful and rich experience, then every representative element in the mind is connected with so many others that the actual and natural course of the thoughts is easily influenced and determined by insignificant circumstances, which accidentally are decisive." The mental elements of a scientific notable are profusely interconnected because the mind has been exposed to a supply of enriching experiences. The intellect is to some degree empirical; simple and predictable environments generate mental counterparts, whereas complex and unpredictable environments yield the opposite and thereby induce the need for the self-organization that is the hallmark of creative potential.

A sizable literature on creative development has accumulated that demonstrates the importance of diversified, enriching environments to individuals who attain eminence (e.g., Goertzel & Goertzel 1962; Goertzel, Goertzel, & Goertzel 1978; Simonton 1987a,c; Walberg et al. 1980; cf. Howe 1982). One noticeable drawback of this research, however, is that the diverse routes to achieving distinction are frequently lumped together: Leaders often rub shoulders with creators in the samples, and when creators are treated separately, artists and scientists are not always segregated. In addition, gaps in the literature on the development of scientific luminaries sometimes force us to extrapolate from investigations of other forms of outstanding creativity. All in all, the difficulties here are much the same as those we encountered in the chapter on personality. Nevertheless, I think that the chance-configuration theory is sufficiently well articulated at this point that we can tease out the applicable research findings. We thus shall review how creative potential in science may have five developmental antecedents, namely, family background, role models, formal education, marginality, and zeitgeist (cf. Fisch 1977).
Family background

The early childhood of the future scientific innovator is characterized by events or circumstances that should raise the eventual effectiveness of any chance-permutation mechanism. Anne Roe (1952b, p. 22) summarized the childhood of the “average eminent scientist” in this way:

He was the first-born child of a middle-class family, the son of a professional man. He is likely to have been a sick child or to have lost a parent at an early age. He has a very high IQ and in boyhood began to do a great deal of reading. He tended to feel lonely and “different” and to be shy and aloof from his classmates. (See also Eiduson 1962)

We shall concentrate here on three core aspects of this description, namely, parental loss, birth order, and cultural enrichment (see also Goertzel & Goertzel 1962; Goertzel et al. 1978; Walberg et al. 1980).

Parental loss and orphanhood

Exceptionally achieving individuals in virtually every human endeavor are more likely to have lost a parent, and especially both, relative to any reasonable baseline (Albert 1971; Eiduson 1962; Eisenstadt 1978; Goertzel et al. 1978; Illingworth & Illingworth 1969; Martindale 1972; Walberg et al. 1980). Lenin was a teenager when his father died; Napoleon was around 15 when he lost his father; and Beethoven’s mother died when he was 16, his father when he was 18. To show that these are not isolated instances, Eisenstadt (1978) examined 699 eminent personalities (about 14% of whom were scientists) from almost all eras and nationalities: 61% lost a parent before age 31, 52% before 26, and 45% before 21. Albert (1971) looked at the geniuses, both creators and leaders, who qualified for membership in the Cox (1926) sample and discovered that parental loss was characteristic of between 22 and 31%. Another investigation using a slightly overlapping sample of famous persons from all walks of life found that almost one-third of them had lost their fathers early in life (Walberg et al. 1980). This “orphanhood effect” has been most consistently demonstrated for literary creators: Martindale (1972) observed the absence of the father in 30% of a sample of poets, and more dramatically, Brown (1968) noted that 55% of his sample of writers had lost a parent before age 15.

This same effect may hold for distinguished scientists as well: Newton’s father died before Newton was even born, and though not nearly so dramatic, Boyle, Huygens, Lavoisier, Count Rumford, Lord Kelvin, Maxwell, and Marie Curie all lost a parent early in their lives (Price 1963, p. 109).

Birth order

In his English Men of Science (1874), Francis Galton was possibly the first investigator to maintain that firstborn and only sons are located in the ranks of the great in proportions well in excess of chance expectation. Thirty years later, Havelock Ellis (1904) showed that the prominence of the firstborns extends to other domains of achievement as well. At the present time, after over a century of research on this topic, the primacy of the firstborn child appears assured, not merely in science, but in almost all endeavors (e.g., Goertzel et al. 1978). In Roe’s (1952a) classic study, 39 of her 64 distinguished scientists were firstborn (and 15 of these were only children), and 13 were second-born children in the family, leaving just a dozen to fill out the remaining ordinal positions up to the two seventh borns. Furthermore, Roe revealed that of the 25 who were not firstborn children, 5 were oldest sons, 2 had an older sibling who had died in infancy.
or early childhood, and many of the remainder were separated on the average of five years from their older sibling. Other recent investigations have replicated this preponderance: Firstborns usually represent half or more of the community of active scientists (Chambers 1964; Eidson 1962; Gupta et al. 1983; Helmreich et al. 1980; cf. Datta 1968; West 1960). Significantly, too, the firstborns, relative to their later-born colleagues, have higher citation rates in the literature (Helmreich et al. 1980) and higher creativity ratings from experts (Helson & Crutchfield 1970). The birth-order advantage may even be stronger in the sciences than in the arts (Bliss 1970; Clark & Rice 1982; Eisenman 1964; cf. Schubert, Wagner, & Schubert 1977).

What is the meaning of this birth-order effect? We know that both intelligence and educational attainment are empirically associated with birth order (see, e.g., Adams 1972; Altus 1966), two variables that are clearly relevant to scientific achievement (cf. Schachter 1963). The fundamental factor, however, may be how birth order affects intellectual growth. According to Zajonc's (1976) "confluence model" of intellectual development, the ordinal position in the family determines the amount of environmental stimulation available during childhood. The firstborn child is exposed to social interactions defined by mature adults, whereas the later-born children are raised in an environment increasingly diluted by the rather more immature minds of older siblings. If valid, the confluence model would explain the preeminence of firstborns in science, for, as we discussed in chapter 3, an exceptional intelligence is likely necessary for the collection of richly interconnected mental elements (for further discussion, see Simonton 1987c). Furthermore, Zajonc's model explains why the later borns who do attain distinction will follow the pattern suggested in Roe's (1952a) investigation. That is, successful later borns will tend either to have lost an older sibling early or to have a much older sibling, with both situations lessening the intellectual disadvantage. The model explains, too, why eventual eminence in life is linked with early exposure to a great many adults (Walberg et al. 1980).

Nonetheless, the confluence model remains controversial, with some research confirming the logic and the predictions (e.g., Bernbaum, Markus, & Zajonc 1982; Zajonc 1983, 1986) and other research opposing this provocative idea (e.g., Brackbill & Nichols 1982; Galbraith 1982a,b). Some social scientists speculate that birth order is more germane to personality development than to intellectual growth (e.g., Albert 1980; Eisenman 1987; Stewart 1977). A few investigators have even challenged the conclusion that the birth-order effect exists at all (e.g., Schooller 1972). Although we cannot review here all the theoretical and methodological issues behind such de-bates (cf. Simonton 1987c), birth order does appear to be correlated with success as a scientist and this correlation may be due to the advantages that accrue to the firstborn child during intellectual maturation. This conclusion would hold even if it were demonstrated that it is simply because the firstborn child, and especially the firstborn son, is the beneficiary of special family privileges, such as early independence training and full financial support in college and university education. The birth-order phenomenon hence may be linked with the third and last family-background factor (see also McCurdy 1960).

**Cultural enrichment**

The household environment of a prospective creative talent is replete with intellectually and culturally stimulating materials (Goertzel & Goertzel 1962; Goertzel et al. 1978; Howe 1982; Schaefer & Anastasi 1968; Walberg et al. 1980). Parents are far more likely to have intellectual and cultural interests, as revealed by respectable home libraries, magazine subscriptions, and artistic or mechanical hobbies. These leisure activities appear to rub off on their children, for quite early in life future achievers acquire numerous stimulating hobbies, including omnivorous reading. Indeed, George Bernard Shaw may have proclaimed that "reading rots the mind," but omnivorous reading in childhood and adolescence correlates positively with ultimate adulthood success (Simonton 1984d, chap. 4; see also Emery & Czikszentmihalyi 1982; Schaefer & Anastasi 1968; Smith, Albright, Glennon, & Owens 1961; Taylor 1963). Moreover, parents of achievers are more likely to permit their children to explore actively the environment (Walberg et al. 1980). This picture of a stimulating home environment holds for prospective scientists as well as for those attaining distinction in other domains of creativity or leadership (Roe 1952a; Schaefer & Anastasi 1968).

The family context that promotes creative development also has a few more properties that may be related to cultural enrichment. Although socioeconomic class is not always correlated with eventual achievement, it is true that noteworthy persons are particularly likely to come from the middle or professional classes (Raskin 1936; Simonton 1976a). This also holds in the case of science (Berry 1981; Chambers 1964; Eidson 1962; Elliott 1975; Helson & Crutchfield 1970; Moulin 1955; Raskin 1936; Taylor 1963; West 1961). Over half of the distinguished researchers that Roe (1952a) studied were the sons of professional men; only 2 out of the 64 had fathers who were skilled laborers, and not a single one had a father who was an unskilled laborer. Roe's group also tended to avoid institutional
family background variables and achievement can be ascribed to genetic endowment, the background variables at the very minimum serve as clues to which individuals are likely to have the intellectual capacity for creative science. At this point I am inclined to endorse a mixed account, in which intellectually bright parents may have comparably brilliant children (see, e.g., Simonton 1983b), but appreciable environmental input is required to convert this intrinsic intellectual power into creative potential (Simonton 1987c). To reiterate what chapter 3 stated, intelligence is a necessary, though not sufficient, condition for a repertoire of richly interassociated mental elements that can freely enter chance permutations.

Role models

Adulthood achievement in any given domain of creativity or leadership depends on the availability of role models during the early, formative years of a person’s life (e.g., Simonton 1978a, 1987c; Walberg et al. 1980). Using generational time-series analysis (Simonton 1984c), the odds of an eminent figure emerging in generation $g$ has been shown to increase with the number of illustrious figures in the same endeavor in generations $g - 1$ and $g - 2$, an autoregressive function that apparently holds for both creativity (Simonton 1975e) and leadership (Simonton 1983b). Another study showed that two-thirds or more of a sample of eminent personalities were exposed, in early development, to significant adults who worked in the field in which the eminence was later attained and to adults who themselves claimed some distinction in their chosen field (Walberg et al. 1980). Moreover, these role models can be either impersonal “paragons” who are admired at a distance or personal “mentors” who affect the emerging genius far more directly (e.g., Simonton 1984a). According to Erikson’s (1951) theory of psychosocial development, adolescence may be the best period in a person’s life for acquiring “models of leadership,” for it is during this developmental interval that a person’s identity is said to be established. In any case, the data collected to date support the generalization that somewhere between the teens and the 20s a young developing creator may be most susceptible to role-modeling effects.

Empirical research on scientific creativity has indicated that science likely operates no differently. Time series of aggregate scientific creativity display respectable autocorrelations that may betray the dependence of each generation on its predecessor (Simonton 1974, 1976c). On the individual level, the prospects for achieving acclaim appear to be enhanced by an apprenticeship under successful practitioners of science (Crane 1965; Goldstein 1979; Segal et al. 1980). For instance, the odds of winning the
Role models

older than their apprentices, yet not much less than 10 years older (see, e.g., Gupta et al. 1983; Zuckerman 1977, p. 119). So if apprentices are in their middle 20s, their mentors should be somewhere between 35 and 45 so as to maximize any beneficial effect.

From the perspective of chance-configuration theory, the equivocal influence of role models, whether paragons or mentors, illustrates the “essential tension” required for the scientific excellence of which Kuhn (1963) spoke (see chapter 1). On the one hand, the scientist must to some extent be a traditionalist, mastering the repertoire of problems, techniques, and standards. As noted before, whether or not an offered communication configuration proves successful in the competition for colleagues’ attention hinges on whether the originator’s mind contains the same body of mental elements, has those elements in a comparable condition of disorganization, and can effectively translate the initial chance configuration so that it can be understood by fellow members of the discipline. Insofar as identification with accomplished predecessors, especially distinguished mentors, provides this sort of expertise, some degree of exposure to suitable role models must be developmentally healthy.

On the other hand, to be truly influential, particularly to become a revolutionary who transforms the foundations of a science, demands that the scientist be an iconoclast. Otherwise, severe constraints will be placed on the chance-permutation process. First, the repertoire of mental elements that enter into the combinatory play will be more specialized, and the opportunity for truly novel permutations will be correspondingly truncated. Furthermore, the interconnections among these elements will be more limited, and what few linkages remain may be more firm, consequent undermining the basis for generating chance associations (an effect particularly likely if the disciple has studied under a single master sufficiently advanced in self-organization that most ideas have found their proper place in the intellectual hierarchy). In the extreme case of outright imitation, the protégés or admirers will be subjecting virtually the identical set of elements to permutations as had been recombined by their mentors or paragons, producing disciples who can advance science solely by, in effect, extending the careers of their masters beyond biological limitations.

This tension between traditionalists and iconoclasts implies a curvilinear, inverted-U curve between the magnitude of a role model’s influence on a young scientist and that scientist’s ultimate contribution to science. That is, the peak of the function represents the optimal trade-off point between these two conflicting forces. Just such a curve has been demonstrated to hold for artistic creativity (Simonton 1984a). The differential fame of 772 Western painters and sculptors was predicted by the formula

Nobel Prize for scientific work are better for those who studied under past Nobel laureates (Zuckerman 1977). Indeed, it is this intergenerational linkage that is partly responsible for the clustering of creators into those Kroeberian “configurations” that we mentioned when discussing the Yuasa phenomenon in chapter 4 (Simonton 1981a; cf. Gray 1961).

Nonetheless, role models can have a negative rather than a purely positive consequence for creative development (Simonton 1976e,f, 1977b, 1984a), a possibility that has not received sufficient attention in the empirical work on scientific creativity. Even if the availability of role models improves the prospects of creative precocity (Simonton 1977b), for example, the long-term impact on lifetime productivity can be negative, perhaps because of the excessive imitation of models (Simonton 1987c). A fine line divides emulating one’s predecessors and eventually surpassing them, and merely imitating them as virtually infallible models. Newton may have seen farther than all the rest by standing on the shoulders of giants, yet many of Newton’s contemporaries (especially the Cartesian) were quite content to lie obscured in the shadows of giants. There are evidently at least three ways to augment the utility of models while concomitantly diminishing any tendency toward debilitating imitation (cf. Simonton 1984a).

1. The more models there are, the harder it will be to imitate any one. Thus, it is better to be the protégé of several mentors, and it is equally advantageous to depend on more than one standard of excellence.

2. In the instance of paragons, if one must admire just one or two predecessors, more benefit accrues from modeling oneself after those who are temporally most distant. The more historically removed the source of inspiration is, the less likely it is that the source can provide an exact template for guiding contemporary creativity. Einstein exemplified both of these patterns: The walls of his study contained the portraits of just three predecessors—Newton, Faraday, and Maxwell—whose careers terminated long before Einstein began his own (Maxwell, the closest in age, died the very year that Einstein was born).

3. In the case of mentors, it is probably more advantageous to study with a scientist still in the prime of his or her career rather than with someone in the later stages of self-organization (cf. Simonton 1984d, chap. 2). We proposed in previous chapters that over a career, the process of self-organization converts an intuitive genius into an analytical genius. A student studying under an intuitive genius will be exposed to a larger number of unresolved issues. In addition, mentors who have yet to organize themselves into analytical geniuses will be more accepting of original ideas generated by their protégés, in line with Planck’s principle. In concrete terms, it seems optimal for the masters to be not much more than 20 years
Developmental antecedents

\[ A = 16 + 12C + 0.15G + 0.23CG + 0.001G^2 - 0.001CG^2 \] (5.1)

where \( A \) is the artist's eminence, \( C \) is the number of paragons admired by the given artist, and \( G \) is the average age gap between that artist and those paragons. If we take the partial derivative of \( A \) with respect to \( G \) for Equation 5.1, set the result equal to zero, and solve for \( G \), we will get

\[ G = \frac{75 + 115C}{C - 1} \] (5.2)

This equation indicates the optimal age gap between creator and paragons, given a certain number of paragons. To illustrate, when rounded off, the average number of paragons across all artists sampled yields \( C = 3 \) as the typical case. By substitution, then \( G = 210 \) years. In different terms, among those artists who modeled themselves after three predecessors, the most famous were those whose models had been born over two centuries earlier. In line with the notion of a trade-off, when \( C \) increases, \( G \) decreases. Thus, the most paragons, or standards of excellence, claimed for any artist was 17 which, when put into Equation 5.2, yields a peak at 127 years. In contrast, if an artist has fewer sources of inspiration, then they must be historically more distant so as to avoid the temptation of imitation. In fact, in the extreme situation, when an artist relies on only one paragon from the past, Equation 5.1 degenerates into the linear equation \( A = 28 + 0.38G \), and consequently, Equation 5.1 becomes undefined. The more extensive the age gap is, the better it will be. Figure 5.1 graphs the curve for the typical situation of three paragons.

Unhappily, few researchers have tried to indicate the existence of a more complex and ambivalent link between role models and scientific distinction, like that in the study of artistic creators. Sheldon (1979, 1980) proposed a complicated mathematical model of mentor–protégé influences that allows for adverse effects, but his empirical tests of these “hierarchical cybernets,” although confirmatory, are rather oblique. Another investigation found that serving as a research assistant in graduate school was correlated negatively with the number of citations later received as a scientist, a correlation that has more than one interpretation (Segal et al. 1980; cf. Chambers 1964). Nevertheless, we have provided a theoretical rationale for believing that results similar to those found in the arts may be found in the sciences too, albeit the constants in the equations would probably differ. Assuming that scientific creativity demands more emphasis on traditionalism, whereas artistic creativity requires more iconoclasm, fewer paragons and shorter age gaps may prove more desirable in the sciences. Einstein's scientific idols may be close to the ideal. Despite our present lack of evidence on this point, the results reported in the next section seem corroborative, for a similar curvilinear relationship is found for the relation between creativity and formal education.

Formal education

Einstein is frequently cited to support the opinion that the development of scientific creativity is deterred by traditional education. At one time Einstein condemned education methods with the words: “One had to cram all this stuff into one’s mind for the examinations, whether one liked it or not. This coercion had such a deterring effect on me that, after I passed the final examination, I found the consideration of any scientific problems distasteful to me for an entire year” (quoted in Hoffman 1972, p. 31). On another occasion, and on a broader scale, Einstein affirmed that

it is, in fact, nothing short of a miracle that the modern methods of instruction have not yet entirely strangled the holy curiosity of inquiry; for this delicate little plant, aside from stimulation, stands mostly in need of freedom; without this it goes to wrack and ruin without fail. It is a very grave mistake to think that the enjoyment of seeing and searching can be promoted by means of coercion and a sense of duty. (quoted in Schilpp 1951, p. 17)
To appraise the developmental repercussions of formal education we must address two questions: First, is there any correlation between how well a student performs academically and that student's eventual scientific accomplishments in later life? Second, what is the functional relationship between the level of formal education attained and the prospects for scientific success in maturity? After discussing these issues, I shall outline an integrative scheme based on the theory.

**Scholastic success**

Investigators have consistently shown that creativity is not necessarily correlated with high marks in school and college, such as registered by the grade point average (Baird 1968; Bednar & Parker 1965; Guilford 1959). This null effect has led some to infer that the educational system—whether primary or secondary or higher education—is not really designed with the talented or gifted student in mind (Bentley 1966; Haddon & Lytton 1968; Mahoney 1976, chap. 3; cf. Hudson 1966). High-IQ students (i.e., analytical geniuses) are often preferred to high-creativity students (i.e., intuitive geniuses) (Getzels & Jackson 1963; Hasan & Butcher 1966). That is, the capacity to master and organize the lessons as given is preferred to the ability to challenge conventional wisdom by speculating on alternatives to standard viewpoints. As a consequence, future creative individuals may not always be motivated to perform well in class work, becoming instead underachievers whose scholastic merits are uneven. MacKinnon (1962, p. 494) concluded from his studies of creative architects that in college “they were unwilling to accept anything on the mere say-so of their instructors. Nothing was to be accepted on faith or because it had behind it the voice of authority. Such matters might be accepted, but only after the student on his own had demonstrated their validity to himself.”

Although granting that scholastic performance is definitely more germane to the development of scientific creativity, as compared with other kinds of creativity (Chambers 1964; Hudson 1966; Schaefer & Anastasi 1968), it remains true that high grades are not necessarily conducive to adulthood achievement in science (Gaston 1973, p. 51; Hoyt 1965; MacKinnon 1960; Razik 1967; Taylor, Smith, & Ghiselin 1963; Taylor 1963). Minkowsky said of his former pupil that “in his student days Einstein had been a lazy dog. He never bothered about mathematics at all” (quoted in Seelig 1956, p. 28). An investigation of fellows of the Royal Society indicated that their academic records as undergraduates were generally poor and certainly not superior to those scientists who failed to be elected fellows (Hudson 1958). Evidently, “much of our educational system seems designed to discourage any attempt at finding things out for oneself, but makes learning things others have found out, or think they have, the major goal. . . . Once a student has learned that he can find things out for himself, though, bad pedagogy is probably only an irritant” (Roe 1952a, p. 82). Before we consider these anecdotes, data, and speculations evidence that scholastic success dams a student as being devoid of scientific potential, we should weigh the following five facts:

1. Whether or not a scientifically talented student does well in formal course work is contingent in part on the structure and orientation of the instructional system (Torrance 1962). For example, colleges tend to produce fewer natural scientists if they emphasize conforming to social expectations, full involvement in collegiate activities, and, especially, dogmatic religious worship (Thistlethwaite 1963). A free-thinking iconoclast would clearly suffer under such oppressive circumstances, to the detriment of scholastic effort.

2. Regardless of the general atmosphere of the college or university, certain styles of teaching may encourage scientific talent to pursue formal requirements. In Roe's (1952a) study of eminent scientists, the best-liked teachers were those who let their students pursue personal interests, such as extensive outside reading (see also Schaefer & Anastasi 1968). For the most part, “the teachers who stimulate scientists tend to be non-directive in their teaching methods” (Thistlethwaite 1963, p. 275).

3. According to one criterion of scholastic accomplishment, at least, future scientific contributors do tend to distinguish themselves. As we briefly noted in chapter 4, those most likely to contribute to science as professionals usually earn their doctorates at a relatively early age—one of the most consistent predictors of scientific productivity and acclaim (Blackburn et al. 1978; Eagly 1974; Harmon 1963; Lyons 1968). Ironically, this relation may explain why prospective innovators may not do well by more formal standards, for they may focus more on pursuing their scientific curiosity than on winning respect from their teachers. Bloom (1963, p. 258), who studied the postdoctoral careers of University of Chicago PhDs, put it this way:

An individual who comes to a university with problems that he is really interested in, with some notion of himself as a research worker or scholar, and who is able to resist the student role of doing things because they are required or because he is told to do them is likely to be a most productive individual in his post graduate career.

4. It also has been repeatedly demonstrated that the most productive and most frequently cited scientists have usually completed their undergraduate and graduate education at the more prestigious institutions of
higher learning (Blackburn et al. 1978; Helmreich et al. 1980; Zuckerman 1977). This correlation may be interpreted as showing that entrance into the scientific elite requires attendance at elite institutions that provide the proper training, and yet it is no less plausible that this association represents a spurious effect. Those youths who hold the most promise of creative potential may select the better schools in the first place, as part of their quest for intellectual stimulation. If superior colleges and universities can also be presumed to offer more rigorous academic programs, it follows that just such a choice implies a lower grade point average, because of the higher standard of demonstrated competence in class work. To support this conjecture we mention the fact that the mean IQ for undergraduates ranges from 108 for the least selective institutions to 132 for the most selective, a spread of well over one standard deviation (Cronbach 1960).

5. Aside from the preceding considerations, the correlation between scholastic success and adulthood achievement is not necessarily negative, although it can be (Baird, 1968; Taylor et al. 1963; cf. Chambers 1964; Owens 1969; Taylor & Ellison 1967). For every notable scientist whose academic performance was noticeably below par, another scientist of roughly equivalent distinction can be named whose accomplishments in school would be a source of pride. Einstein may have been a mediocre student, as was evidently Charles Darwin, but Marie Curie, Sigmund Freud, and Max Planck were brilliant in their formal course work.

Before we can integrate these points into a larger framework, we first must discuss the connection between achieved eminence and the height a student climbs on the educational ladder of ever higher degrees.

Level of formal training

Because in this era of “big science,” researchers almost always must have advanced degrees to obtain research positions—whether in academia, industry, or government—it is easy to assume that the process of earning a PhD actually nurtures creative potential. Yet this inference overlooks that many landmark contributions to science were contributed by investigators who lacked respectable academic credentials. Einstein did not obtain his doctorate by attending graduate seminars and working in the laboratories of professors, but rather he obtained his degree while engaged full time at a Swiss patent office. He once complained to a friend that “I shall not become a Ph.D. . . . The whole comedy has become a bore to me” (quoted in Hoffman 1972, p. 55), yet in the critical year of 1905 Einstein wrote four papers, the least important of which he sent off to the University of Zurich, which accepted the paper as a doctoral thesis (after having rejected a nearly identical paper four years earlier). Considering that his other papers of that year included the revolutionary papers on the photoelectric effect and the special relativity theory, it is clear that obtaining a doctorate contributed absolutely nothing to Einstein’s capacity for doing creative science. Nor is Einstein atypical. Eminent scientists generally begin contributing to the store of objective knowledge before obtaining their doctorate (see, e.g., Nelson & Crutchfield 1970; Roe 1972b).

This last tendency takes us to a more central generalization: A series of studies using diverse subject pools and operational definitions actually unearthed evidence that creativity may be a curvilinear, inverted-U function of the level of formal education (Simonton 1976a, 1981c, 1983a, 1984d, chap. 4; cf. Senter 1986; Taylor & Ellison 1967; Taylor et al. 1963). As educational level rises, the probability of achieved eminence in a creative endeavor also increases up to a certain optimum and thereafter declines so that further formal training diminishes the odds of achieving the highest eminence. For most historical periods and creative disciplines, the turnaround point appears somewhere between the junior and senior years of undergraduate education (Simonton 1983a). To illustrate, Figure 5.2 presents the curves observed for the 192 creators (and 109 leaders) who comprised the Cox (1926) sample. Considering that creativity is inversely related to dogmatism (i.e., idealistic inflexibility), it is also pertinent to report that there is a
mirror-image function for the relationship between formal educational level and dogmatism, as assessed for 33 past presidents of the United States. This reflection can be readily seen in Figure 5.3.

However, in the sciences this peak has, in recent years, moved toward the middle of graduate education (Simonton 1984d, chap. 4). That is, for scientists active in the 20th century, advanced training in graduate school appears to contribute to the development of creative potential (cf. Simonton 1986b). Even so, no shift in the optimal level of formal education has been spotted for creativity in the arts and humanities, the high point still appearing somewhere between the junior and senior years of undergraduate training (cf. Terman 1954).

As implied earlier, such nonmonotonic functions suggest that the essential tension that mediates the impact of role models also moderates the repercussion of formal education. On the one side, the traditionalist aspect of scientific creativity is reinforced by extensive formal training. Education, after all, is society's method of preserving and passing down to future generations the cultural variations that have had adaptive value in the past history of sociocultural evolution. The scientist-to-be must acquire the basic tools and concepts of the discipline in order to arrive at new communication configurations that enable social acceptance, for reasons given earlier.

Formal education

On the other side, the iconoclastic facet of scientific creativity—the capacity to produce genuinely original chance configurations—obviously requires that the young scientist not be excessively socialized into a single, narrow-minded way of associating ideas. In this case, informal instruction, especially self-education, is far more conducive to creative development than is the highly formal, even rigid inculcation of cultural dogmas (cf. Haddon & Lytton 1968). Hence, the peak of the curve expressing achievement as a function of formal education may reveal the point at which the iconoclastic component begins to be sacrificed for the traditionalist component. Gains in the ability to compose comprehensible communication configurations cannot compensate for the losses in the capacity for generating chance configurations that depart significantly from the cultural givens.

General interpretation

To understand the theoretical meaning of the findings reviewed in the preceding two sections, we shall consider the following five points (cf. Simonton 1984d, chap. 4):

1. A person's native intellectual ability may have to be weighed when assessing the effects of both scholastic prowess and formal educational level. Let us suppose that intrinsic intelligence is correlated not only with the number of associations among mental elements that can be stored and retrieved but also with the relative speed with which such associations can be acquired. In other words, perhaps we can take somewhat literally the assertion that intelligent students are "quick" and unintelligent students are "slow." This supposition is consistent with the conception of IQ as the ratio of mental to chronological age (i.e., intelligence is associated with quicker intellectual advance). Then, highly intelligent students can complete their school work with minimal effort, leaving ample time to pursue those extracurricular activities that may better support the development of an interconnected collection of ideas. Accordingly, if we first match students on inherent information-processing power, then we should expect a negative link between scholastic performance and creative potential. Holding intelligence constant, the requirements of formal education will directly compete with enriching self-education.

2. Even in controlling for intellectual ability, we must assume that the competition is really between educational demands and self-instruction. For example, some schools, colleges, and universities support special programs for the "gifted" that enable the student to earn academic credit for self-initiated courses of inquiry, thus making the education informal rather than formal (cf. Haddon & Lytton 1968; Mahoney 1976, chap. 3). More
significantly, there are many ways of becoming a poor student besides spending too much time on outside intellectual and cultural interests—such as having to cope with extraordinary economic, emotional, or social difficulties. Only when scholastic success is forced to give way to the demands of self-education, not these extraneous obstacles to study, does the negative correlation obtain between performance and creative potential (cf. Richards, Holland, & Lutz 1967). As noted earlier, gifted children and adolescents do seem dedicated to the independent pursuit of knowledge, as evinced by extensive independent reading and numerous intellectual hobbies (Goertzl & Goertzl 1962; Goertzl et al. 1978; Schaefer & Anastasi 1968; Segal et al. 1980; Walberg et al. 1980). As graduate students, particularly, creative scientists—in contrast with their unproductive peers—tend to devote far more time on research and study, over 50 hours per week (Chambers 1964). By one estimate, indeed, a full decade of preparation must transpire before a person’s knowledge base becomes sufficiently rich to support creative output (Hayes 1981, pp. 209–214; Simon 1986). This intrinsically motivated mastery of a discipline is a sine qua non of success. “No man should dream of solving a great problem unless he is so thoroughly saturated with his subject that everything else sinks into comparative insignificance,” said Mach (1896, pp. 170–171).

3. As one proceeds up the academic ladder, the effort that must be expended to achieve the maximal scholastic success, the highest grades, enlarges as well (cf. Taylor & Ellison 1967). The magnitude of competition from fellow students becomes more conspicuous. For instance, the average IQ of high school graduates in the United States is 110, an intellectual level that would give a student only a 50–50 chance of graduating from college and almost no chance of obtaining a PhD. For the mean IQ of a college graduate is 120 and that of a PhD 130 (Cronbach 1960). Aside from this intensified competition, the volume and difficulty of the material to be mastered become ever more demanding. Consequently, even keeping intelligence constant and presuming the intrinsic motivation to seek out intellectual stimulation, the correlation between scholastic and scientific achievement should become more negative when advancing from primary through graduate education (see, e.g., Owens 1969).

4. Also to be taken into account is that there is more than one way to make a lasting contribution to science. First, one can be an **advancer**, a practitioner of what Kuhn (1970) styled “normal science.” Advancers concentrate on “three classes of problems—determination of significant facts, matching facts with theory, and articulation of theory” (p. 34) in order to extend the explanatory power and precision of the prevailing paradigm. Because an advancer is thus more on the traditionalist side of the scale, both high scholastic success and full progression along the formal education sequence are most beneficial. Robert Oppenheimer was an extremely remarkable student, but his contribution to science was limited to developing and enlarging the established views of his mentors and predecessors. Second, one can be a **revolutionary** scientist, which then places a bigger premium on becoming a more daring iconoclast, thus forcing the correlation between scholastic performance and attainment to become more negative and the function between formal educational level and final accomplishment more obviously nonmonotonic, concave downward. A revolutionary scientist must allow the chance-permutation process far more freedom in order to produce an original yet effective integration of scientific knowledge. Einstein, for instance, allowed himself to cut across traditional boundaries in classical physics, pondering simultaneously the phenomena behind both Newtonian mechanics and Maxwell’s electromagnetic equations (Holton 1971–1972). Einstein took as fundamental problems what his predecessors took for granted as unquestionable premises.

5. Because revolutionaries—compared with advances—are usually accorded more acclaim in posterity’s eyes, one wonders why everyone with any ambition does not opt for the former path to posthumous glory. Herein emerges the final consideration, namely, the current state of the scientific zeitgeist. If the times are not ripe for a scientific revolution—if the elements necessary for an adaptive synthesis are not available and if the thoughts of the scientific community are sufficiently well structured around the traditional paradigm—it is improbable that chance configurations of revolutionary import can materialize, or, if they do, that the scientific world will be sympathetic to any attempt to fix what does not seem to require fixing. For instance, until such Kuhnian anomalies as black body radiation, the photoelectric effect, and the Michelson–Morley interferometer experiment became widely disseminated and debated, the zeitgeist was not ready for the revolutionary concepts of quantum and relativity theory. Hence one cannot always become a college dropout with the rational expectation of revamping the very core of science, for the times may hand all the creative opportunities to the advancers. Young scientists who strike out on novel paths despite the unfavorable setting can hope only to become mere precursors: a **precursor** genius is a creator who follows an independent path rather than operating within the confines of the accepted paradigm but who, as a consequence, can often offer only incomplete anticipations of ideas that later will shake the foundations of science under the aegis of more notable names. Charles Darwin had several such precursors, the most striking among them, perhaps, being Patrick Matthew (Eiseley 1958, chap. 5; Zirkle 1941). Needless to say, precursors seldom earn as much credit as do
advancers in the annals of science, and uncountable potential precursors miss the mark altogether, becoming thereby merely eccentrics to contemporaries and nonentities to posterity.

To sum up, theoretical considerations suggest that Einstein's condemnation of education was far too simplistic. To gauge fairly the developmental influence on scientific creativity, we must know a student's native intelligence, the degree of personal involvement in self-education, the level of formal education presently demanding limited information-processing resources, whether that student aspires to revolutionizing rather than advancing science, and whether the zeitgeist is appropriately prepared for a scientific transformation rather than elaboration. These factors affect the optimal degree of scholastic success and formal educational level by determining the best trade-off between the traditionalist quest for immediately acceptable communication configurations and the iconoclastic search for original chance configurations.

Marginality

Campbell (1960, p. 391), in documenting his own model, noted that "persons who have been uprooted from traditional culture, or who have been thoroughly exposed to two or more cultures, seem to have an advantage in the range of hypotheses they are apt to consider, and through this means, in the frequency of creative innovation." To support this conclusion, Campbell cited such classic essays as Veblin's 1919 paper on the conspicuous intellectual prominence of Jews in Western civilization and Park's 1928 discussion of the "marginal man." This notion is compatible, too, with Toynbee's (1946) theory of a "creative minority" who advances human civilization through a process of "withdrawal and return." The Goertzels offered more contemporary endorsement by pointing to the respectable percentage of first- and second-generation immigrants among distinguished 20th-century personalities (Goertzel et al. 1978). A comparable advantage of immigration may hold for scientists as well, at least for mathematicians (Helson & Crutchfield 1970). Individuals raised in one culture but living in another are blessed with a heterogeneous array of mental elements, permitting combinatory variations unavailable to those who reside solely in one cultural world. The disproportional representation among notable scientists of those with Jewish backgrounds illustrates this point (Berry 1981; Hayes 1981, pp. 227-229).

Sociocultural marginality is not the only variety of marginality that may nurture the development of creative potential, because professional marginality can have much the same effect. Significant contributions to a given endeavor are frequently made by those who either switched fields or were self-taught (Hughes 1958), a circumstance that apparently holds specifically for science. Koestler (1964, p. 230) proclaimed that "all decisive advances in the history of scientific thought can be described in terms of mental cross-fertilization between different disciplines." Likewise, Bartlett (1958, p. 98) observed that "it has often happened that critical stages for advance are reached when what has been called one body of knowledge can be brought into close and effective relationship with what has been treated as a different, and a largely or wholly independent, scientific discipline." And finally, Kuhn (1970, p. 90) elaborated this point by asserting almost always the men who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigm they change. . . . [F]or obviously these are the men who, being little committed by prior practice to the traditional rules of normal science, are particularly likely to see that these rules no longer define a playable game and to conceive another set that can replace them.

In any event, a scientist exposed to more than one discipline can combine elements in a truly unique fashion, chancing upon original configurations that may revolutionize one or more disciplines.

There are many anecdotes illustrating the positive impact of professional marginality. As Bartlett noted, scientists will often be successful in one field precisely because they introduce ideas, techniques, or habits of thought that are practically standard in another domain. For example, Landsteiner's previous background in chemistry likely facilitated his isolation of blood groups, Kekulé's early ambition to become an architect may have nourished a thinking style conducive to his conceiving the structural basis of organic chemistry, and Helmholtz specifically acknowledged that his invention of the ophthalmoscope arose because he was obliged to pursue a medical career when his primary fascination was with physics, especially optics. At other times, in line with Kuhn's statement, the chief asset to be gained from professional marginality is an ignorance that allows the chance permutations to proceed a fresh. Bessemeir explained his success in devising an industrial process for steel production in this way: "I had an immense advantage over many others dealing with the problem inasmuch as I had no fixed ideas derived from long established practice to control and bias my mind, and did not suffer from the general belief that whatever is, is right" (quoted in Beveridge 1957, p. 5). Likewise, when chemist Pasteur was urged by his former teacher Dumas to tackle the disease that threatened the French silk industry, his protest "But I never worked with silkworms" was countered with "So much the better" (Asimov 1982, p. 423). Pasteur then advanced from one domain of ignorance to another, eventu-
ally propounding the germ theory of disease and introducing inoculation for rabies, among other feats that founded the basis for modern medicine.

Unfortunately, even if these stories make the concept plausible, little empirical work has been done to assess directly the role of professional marginality. Gieryn and Hirsh (1983) tried to gauge the association between marginality and innovation in X-ray astronomy. Even though they concluded that marginality bore little connection to innovativeness, a closer inspection of their data indicated that approximately 20% of the variation in innovation could be explained by their seven indicators of professional marginality (Simonton 1984a). In addition, Stewart (1986) conducted a quantitative inquiry into the early reception of Alfred Wegener’s continental drift theory that found that (1) opponents were more likely to have published numerous articles in mainstream geology and that (2) proponents were more likely to have been scientists who were in some discipline other than the geosciences. Both of these results are in line with the hypothesized impact of professional marginality. So enough evidence exists to advise further research on this interesting issue. Nevertheless, the discussion offered earlier in this chapter also cautions future investigators to test for curvilinear as well as linear functions. When we examined the repercussions of role models and formal education on the development of creative potential, we surmised that the necessary compromise between traditionalism and iconoclasm implies a single-peaked, concave-downward curve. Plotting innovativeness as a function of professional marginality may yield parallel graphs. This possibility may apply to sociocultural marginality as well.

Thus, whatever the theoretical plausibility, there is little quantitative evidence for the conclusion that marginality, whether sociocultural or professional, enhances creative potential. Even so, some research has been carried out on yet a third kind of marginality, namely, geographic marginality, but this has been shown to have an adverse impact on creative development. That is, those creators born and raised far away from the cultural center of their day may face an uphill struggle for recognition, at least in aesthetic fields (Simonton 1977b, 1984a, 1986a). Although we cannot say for certain whether this negative relationship holds for the scientific enterprise equally well, the fact that the best scientists tend to study at the elite institutions (presumably at the current centers of creative activity) could be cited as possible support (see, e.g., Crane 1965; Eagly 1974; Zuckerman 1977). Also supportive is the finding that metropolitan areas produce a larger percentage per capita of distinguished scientists than do rural regions (Berry 1981). Further, the chance-configuration theory leads us to expect congruence. So-called provincials are likely deprived of the diversified and stimulating environments to be found in the capitals of civilization, with corresponding losses in the creative potential that can be developed (see Elliott 1975). Hence, further empirical studies on geographic marginality, focused on its linkage with scientific creativity, are needed.

**Zeitgeist**

In the section on formal education, we recognized how the scientific “spirit of the times” may mediate the developmental consequence of instruction. Several generational analyses have revealed, too, that the more broadly defined zeitgeist exerts an independent influence on creative development (Simonton 1978a). What transpires in generation $g$ establishes the environmental context for the development of creators in generation $g + 1$ (Simonton 1984c). These cross-generational effects are of two primary types, sociocultural and political.

**Sociocultural effects**

The creative activity of one generation affects the probable creative output of the next in many ways. The impact of role-model availability that we discussed previously is one example. Because the quantity and quality of creative scientists depend on the supply of potential mentors and models in the preceding two generations, scientific geniuses tend to cluster together in time and space, as described by Kroebber (1944) and Yuasa (1974). Not only may the prior generation determine the opportunities for achievement in the following generation, but the content of the predecessors’ contributions also may direct the course taken by their successors. For example, in the history of ideas, the prevalence of certain philosophical beliefs in one generation leads to the emergence of other beliefs in the succeeding generation, whether as reaction or as development (Simonton 1978c). The advent of empiricism tends to stimulate the appearance two decades later of materialism, individualism, conceptualism (i.e., universals are only mental constructs), and skepticism—intellectual trends that favor scientific growth (cf. Simonton 1976c). In turn, the prominence of empiricist thinkers in one generation has an ideational origin in the previous generation’s preoccupation with nominalism, or the view that universals are but names.

Although many more examples can be given of intergenerational effects in the history of ideas, the main point is that the intellectual fascinations of one generation of intellectuals can inspire the ideas of the next generation. Thus the epistemological notion that immediate sensory experience is the source of all knowledge, or empiricism, becomes a natural component of
other philosophical constructions, such as the awareness that knowledge ensues from a material process of physical stimuli striking physiological sense organs, that we as individuals are responsible for acquiring knowledge rather than receiving ideas more directly and infallibly via revelation, that abstractions are only mental creations used to coordinate the diversity of stimuli, and that our ability to know is vulnerable to the imperfections of sensation, as illustrated by the perceptual illusions (Simonton 1978c).

More directly relevant to understanding scientific innovation, however, are those studies that suggest how the discoveries and inventions of one discipline may insert the raw materials for contributions in neighboring disciplines after a one-generation delay (Simonton 1976e). The most interesting case is perhaps the finding that the chances for creative progress in biology are enhanced whenever the immediately preceding generations took major strides in medicine, geology, and chemistry. Specifically, the number of major discoveries in biology is a positive function of the number of landmark contributions that emerged about a quarter of a century earlier in these three disciplines. The development of creative potential in a future biologist is evidently nourished by the addition of new interdisciplinary ideas that enter into later chance permutations. The tremendous impression made by Lyell's *Principles of Geology* on the young naturalist on the Beagle, Charles Darwin, is one of numerous examples. Intergenerational effects such as these may equally represent at the aggregate level the role of professional marginality that supposedly operates at the individual level. The marginal scientists may be precisely the ones who carry over the key findings of one field into another field when those principles, facts, or techniques have interdisciplinary implications.

I confess that all these investigations are exploratory, and thus the generalizations are tenuous. But my remarks should stimulate more confirmatory research showing how one generation creates the mental elements used by the subsequent generation in its creative integrations.

**Political effects**

Far more intriguing than the sociocultural effects, in my view, are the instances in which the political milieu in generation g shapes the path taken by creativity in generation g + 1. It has been often speculated that nationalism is positively linked with creative development, whereas the emergence of large empires is antithetical to later creative fertility. Kroebner (1944) noted how rare it is for suppressed nationalities to make monumental contributions to human culture; Toynbee (1946) observed that the creative activity of a given civilization is negatively related to the expansion of "universal states"; and Sorokin (1947) said that many nationalities reach the climax of their creativity right after their liberation from foreign domination. This historical generalization may be called the "Danielsky law," after the naturalist and philosophical historian who promulgated it as his "second law of the dynamics of great cultures," namely, that "in order for civilization of a potentially creative group to be conceived and developed, the group and its subgroups must be politically independent" (quoted in Sorokin 1947, p. 543).

These theoretical speculations have been confirmed empirically, for creativity in all domains, science included, increases whenever a multicultural civilization area is fragmented into a large number of sovereign nations, the growth of empire states signaling the decline of cultural innovation (Naroll et al. 1971; Schaffer, Babu, & Rao 1977; Simonton 1975e, 1976f; cf. Ting 1986). Such political fragmentation apparently augments cultural and ideological diversity, a condition via the influx of more variation in available elements that promotes the emergence of individuals with exceptional creative potential (Simonton 1976d). Political fragmentation also favors an ideological zeitgeist that may better shelter the developing scientist, namely, empiricism, materialism, nominalism, evolutionism, and individualism characteristics, it may be argued, that give more free reign to an inquiring mind (Simonton 1976g). There are data that support the frequent conjecture that the collapse of controls during periods of political upheaval prepares the ground for fruitful creativity later (e.g., Barnett 1953). For example, revolts and rebellions directed against the hegemony of empires tend to increase creativity after a one-generation lag, an effect likely due to the resurgence of cultural heterogeneity (i.e., nationalism) against the homogenizing impositions of imperial systems (Simonton 1975e). And civil disturbances generally tend to mix the cultural broth, thereby resuscitating the zeitgeist most friendly to creative growth (Simonton 1976f, 1976g).

In the preceding examples, political events may contribute to creative development by increasing the diversity of mental elements likely to enter chance permutations, in much the same way as does sociocultural and professional marginality. Nonetheless, not all violent or dramatic events in the political sphere have this pleasant outcome. Wars between states, for instance, tend to produce an ideological zeitgeist that may not welcome innovation (Simonton 1976f), and creativity is unlikely to come forth after a political system crumbles into total anarchy, as registered by military revolts, dynastic conflicts, political assassinations, coups d'état, and other examples of chaos among the power elite (Simonton 1975e, 1976f).

It is difficult to subsume these particular findings under the present theoretical scheme, but the essential tension between traditionalism and
iconoclasm may once more offer an interpretative clue. On the one hand, wartime propaganda and patriotism may excessively reinforce traditionalism at the expense of iconoclasm; for instance, war indeed discourages the emergence of individualism and empiricism (Simonton 1976g). On the other hand, an era of political instability may instill in the forthcoming generation a debilitating iconoclasm, a distrust of tradition that may verge on nihilism. It is telling that the more rational and systematic endeavors, like science and philosophy, are more inhibited by political anarchy than are artistic activities, like painting and sculpture, for which iconoclasm is presumably more desirable (Simonton 1975e,f).

Conclusion

The goal of this chapter was to review some of the chief developmental factors that may contribute to the acquisition of creative potential. In the broadest terms, it should be clear that the several sets of developmental antecedents—family background, role models, formal education, marginality, and zeitgeist—influence the extent to which the child, adolescent, and young adult will possess an unusual number of profusely interassociated mental elements (Simonton 1987a,c). Though a few of these influences, such as birth order, may operate by augmenting the raw intellectual capacity for storing elements, most variables probably function as stimuli that nurture the cognitive style requisite for generating chance permutations. These inputs favor the emergence of an intuitive genius over an analytical genius. The intuitive genius can effectively appear only in a diversified and unconventional context, such as that nourished by parental loss, marginality, and a sociocultural milieu full of varied and novel events. Naturally, this picture contains a few complexities.

First, some of the observed relationships between environmental diversity and creative development may be the spurious result of genotype-environment effects. Thus, highly gifted parents may have highly gifted children and concomitantly construct a home environment filled with cultural stimuli that in fact make no direct contribution to their children's intellectual growth. And when talented youths reach a suitable age, they may actively shape their environments to be consistent with their genotypic disposition. This could include the selection of role models or educational experiences so as to produce the mere illusion of an environmental effect. Nevertheless, because the genetic interpretation can take us only so far in integrating all the data on creative development, at some point explanations in terms of nurture rather than nature become more plausible. It is certainly difficult to order all the results on parental loss, birth order, role models, education, marginality, and the sociocultural context to march under the banner of inherited personality.

Another complication regards the frequent intrusion of curvilinear functions. The research on role models and formal education, in particular, has come forth with nonmonotonic, concave-downward curves that indicate an ambivalence in how these developmental antecedents impinge on creative potential. These functions were interpreted as the essential tension between the iconoclasm necessary for uninhibited chance permutations and the traditionalism required to optimize the odds that any resulting configurations will earn social acceptance and thus prove influential. Furthermore, we predicted that similar curvilinear functions may be found elsewhere as well, as in the operation of professional marginality. In fact, we can speculate that such single-peak functions are more ubiquitous than so far indicated by the literature. As a case in point, we surmised that events that disrupt stereotyped socialization practices, such as parental loss, assist the development of creative potential, a point also repeatedly found in the research (e.g., Goertzel & Goertzel 1962; Simonton 1987e). Even so, we can readily imagine a family situation so troublesome, so chaotic, that entirely iconoclastic offspring emerge who, as adults, are mere nihilistic "rebels without a cause." Indeed, juvenile delinquents and suicidal depressed patients are two groups with incidences of orphanhood comparable to those observed for eminent personalities (Eisenstadt 1978; cf. Crook & Eliot 1980). Consequently, future research may find it profitable to calculate composite scores made up of the many ways of interjecting complexity and variety into cognitive development and then to test for curvilinearity when estimating the relationship between these aggregated scores and achieved eminence. Nonmonotonic functions may be found, too, for the sociocultural and political variables. A central point made by Arnold Toynbee in his Study of History (1946) is that the creative growth of a civilization requires a response to challenge, yet that challenge must lie at the "golden mean" between excessive adversity that arrests further advance and the easy life that encourages a culture to lapse into complacency. Although the few tests of this hypothesis have failed to confirm it (e.g., Narroll et al. 1971; Simonton 1975e; Ting 1986), the conjecture is well worth pursuing in additional inquiries (Hagen 1962).

A final nicety is related to the preceding: Any nonlinear equation expressing creative achievement as a function of a developmental variable would, according to theory, exhibit a single peak, yet the precise location of that optimal point is contingent on the specific endeavor under discussion. The best compromise between traditionalism and iconoclasm varies according to the nature of the contribution. Thus artistic creativity, which
6 Multiple discovery and invention

The zeitgeist interpretation of multiples

In the last chapter we discussed how the sociocultural and political milieu may sway the course of creative development. We have evidence, too, that the zeitgeist can guide the adulthood realization of that creative potential. In fact, the situational context has been shown to affect the manifestation of all kinds of historical impact, whether political and military leadership (e.g., Simonton 1979b, 1980b, 1981c, 1984g, 1986e, 1987e) or the diverse guises of creativity (e.g., Simonton 1976f, 1980e, 1984a). In the case of scientific creativity, discoveries and inventions are more apt to appear in particular political, ideological, and cultural settings. For example, scientific advances are more likely to appear under peacetime, as compared with wartime, conditions (Simonton 1976e, 1980c) and under a cultural ideology that favors empiricism, materialism, nominalism, individualism, and determinism rather than mysticism, idealism, realism, statism, and the doctrine of free will (Simonton 1976c; cf. Sorokin 1937–1941). Economic prosperity may also contribute to the growth of scientific knowledge and the acquisition of technological expertise (cf. Rainoff 1929; Schmookler 1966, chap. 6). In addition, different extrinsic conditions may encourage distinctive types of creativity (Simonton 1975e), and various subdisciplines within science may be nurtured by a characteristic set of sociocultural circumstances (Simonton 1975d).

Nonetheless, one phenomenon has frequently been cited as the single best proof of the zeitgeist's participation—multiple discovery and invention. This phenomenon occurs whenever two or more scientists, working independently and often simultaneously, make the exact same contribution to science. Classic illustrations are the devising of calculus by Newton and Leibniz, the prediction of the planet Neptune by J. C. Adams and Leverrier, the formulation of the law of the conservation of energy by Mayer, Helmholtz, and Joule, the production of oxygen by Scheele and Priestley, the proposal of a theory of evolution through natural selection by Darwin and Wallace, the introduction of ether anesthesia in surgery by Long and Morton, and the invention of the telephone by Bell and Gray. Investigators