Part V. SOCIOCULTURAL CONTEXT OF PSYCHOLOGICAL SCIENCE

In the last part of Chapter 12 I argued that the low representation of women among great psychologists says more about the sociocultural system than about women per se. The impact of the internal and external milieu now becomes the subject of the three chapters that make up Part V.

Chapter 13. Internal Milieu

To a certain extent the emergence of notable psychologists may depend on intellectual movements or trends within psychology itself. This possibility is examined in terms of the sociocultural phenomena and processes suggested by Kroeber, Comte, Kuhn, Hegel, and Merton. Part of this discussion includes an examination of what this research implies about psychology's status as a scientific enterprise.

"Some are born great, some achieve greatness, and some have greatness thrust upon 'em," wrote William Shakespeare (quoted in Browning, 1986, p. 140).

William James (1880), for one, had this to say about the phenomenon:

> Sporadic great men come everywhere. But for a community to get vibrating through and through with intensely active life, many geniuses coming together and in rapid succession are required. This is why great epochs are so rare, – why the sudden bloom of a Greece, an early Rome, a Renaissance, is such a mystery. Blow must follow blow so fast that no cooling can occur in the intervals. Then the mass of the nation glows incandescent, and may continue to glow by pure inertia long after the originators of its internal movement have passed away. We often hear surprise expressed that in these high tides of human affairs not only the people should be filled with stronger life, but that individual geniuses should seem so exceptionally abundant. This mystery is just about as deep as the time-honored conundrum as to why great rivers flow by great towns. It is true that great public fermentations awaken and adopt many geniuses who in more torpid times would have had no chance to work. But over and above this there must be an exceptional concourse of genius about a time, to make the fermentation begin at all. The unlikeliness of the *concourse* is far greater than the unlikeliness of any particular genius; hence the rarity of these periods and the exceptional aspect which they always wear. (p. 453)

James appears to be operating according to the principle that if the probability of a single genius occurring is 1 out of 10,000, then the probability of a cluster of 10 geniuses occurring would be 1 out of $10{,}000^{10}$ – very low odds indeed.

Yet James may be mistaken, so that the odds of 10 appearing together may be much greater than one genius appearing in isolation.

Their concourse is favored because the milieu beyond the circumscribed world of the individual's psychology is what drives the emergence of genius.

As noted in the introduction to Part V, this milieu may consist of both internal and external factors, and this chapter will concentrate of the former.

Specifically, the chapter will discuss the following sociocultural phenomena:

1. Kroeberian configurations,
2. Comtian progress,
3. Kuhnian transformations,
4. Hegelian dialectics, and
5. Mertonian multiples.

KROEBERIAN CONFIGURATIONS

- Alfred Kroeber's 1944 book *Configurations of Culture Growth* can be considered one of the classics in the historiometric study of genius.
- In one respect, this book appears quite similar to Galton's (1869) *Hereditary Genius*: Both contain long lists of illustrious personalities who had achieved distinction in a diversity of domains.
- Yet on closer examination, some striking differences appear.
    - First, Kroeber's lists are far less ethnocentric than Galton's are. Appreciative of the fact that great accomplishments have originated in all parts of the world, Kroeber devotes considerable attention to Islamic, Hindu, Chinese, Japanese, Southeast Asian, and American civilizations.
    - Second, Kroeber's lists include many anonymous achievements, like the relief sculpture of Ancient Egypt, the Sanskrit *Mahabharata*, and Cambodia's Angkor Wat.
    - Third, Kroeber's chapters are titled in a less individualistic fashion than are Galton's (e.g., "Science," "Painting," "Literature," and "Music" rather than "Men of Science," "Painters," "Literary Men," and "Musicians").
    - Fourth, and most strikingly, all of Kroeber's notables are listed in chronological order, whereas all of Galton's are listed in alphabetical order.
- Nor are these contrasts trivial.
- This last contrast finally betrays the fact that Kroeber was trying to do something very different than was Galton.
    - Kroeber was an eminent cultural anthropologist who had studied under the great Franz Boas.
    - Boas had so emphatically rejected biological interpretations of differences between human groups that the Nazis reacted by rescinding his PhD and burning his books.
    - Kroeber was no less opposed to such nature explanations of cultural differences, and thus conceived *Configurations of Culture Growth* as a direct attack on Galton's genetic determinism.
    - Although "Galton clearly recognized … the difference of genius production between fifth-century Athens and nineteenth-century England," said Kroeber's (1944), "he misinterpreted it by giving the Athenians a hereditary rating as many degrees superior to that of the modern English as these are superior to the African negro" (p. 11).
    - Kroeber maintained that Galton's conclusions are invalid "because there is a powerful factor of 'environment' at work which he ignored in his search for a biological cause" (p. 11).
- Kroeber believed that the most conclusive evidence against Galton's biological determinism was the distinctive manner in which genius clustered into certain times and places.
    - This happened far too quickly to be attributed to changes in the gene pool of the populations producing those geniuses.
    - Therefore, these clusters must represent the impact of some environmental factor that can change rapidly.
    - For Kroeber, this factor must be the sociocultural system.
    - The coming and going of genius within a given civilization merely reflects underlying "configurations of culture growth" – hence the book's title.
    - This environmentalist position also accounted for the distinctive manner in which Kroeber gathered and presented his data.
    - In particular, where Galton listed his geniuses in alphabetical order to emphasize family relationships, Kroeber listed his in chronological order to emphasize the degree of clustering.

Kroeber's (1944) treatment of British science is fairly typical of the results he reported for other creative domains and other human civilizations.

This specific cluster demonstrates two things.

1. When Galton (1874) conducted his survey for *English Men of Science*, he had many distinguished scientists to choose from (see Hilts, 1975, for a list of those whom Galton surveyed).
2. Because Galton was himself embedded in a well-defined scientific cluster, or cultural configuration, Galton's argument about the inheritance of genius is undermined by his very own birth year.

Configurations of scientific genius appear in various specialties besides.

For instance, an inquiry into the historical placement of 242 eminent English botanists found comparable clusters for that subdiscipline (Schneider, 1937). Like Kroeber, this investigator concluded that his data flatly contradicted Galton's genetic determinism.

The data are one thing, their interpretation another. What is the reason why genius clusters so? Below two general types of explanations will be examined.

The first assumes that creative geniuses must have predecessors on which they build their own work. "If I have seen further, it is by standing on the shoulders of giants," said Newton (quoted in *Who Said What When*, 1991, p. 129) – a confession that might be made by all great intellects.

The second type of explanation concentrates on the beneficial effects of having so many contemporaries who are creating ideas within the same field.

Predecessors: Lagged Effects

Kroeber (1944) himself believed that the configurations resulted from cross-generational influences.

Yet when Kroeber tried to specify the reason why this positive association holds, he ended up proposing a process that appears more psychological than cultural.

In particular, Kroeber quoted at length the views of Velleius Paterculus, a Roman historian who two millennia earlier had noticed the clustering of genius:

> For who can marvel sufficiently that the most distinguished minds in each branch of human achievement have happened to adopt the same form of effort, and to have fallen within the same narrow space of time. … A single epoch, and that only of a few years' duration, gave lustre to tragedy through the three men of divine inspiration, Aeschylus, Sophocles, and Euripedes. … The great philosophers, too, received their inspiration from the lips of Socrates … how long did they flourish after the death of Plato and Aristotle? What distinction was there in oratory before Isocrates, or after the time of his disciples and in turn of their pupils? So crowded were they into a brief epoch that there were no two worthy of mention who could not have seen each other. (p. 17)

After giving some additional examples from the history of Roman civilization, Velleius added some speculations:

> Though I frequently search for the reasons why men of similar talents occur exclusively in certain epochs and not only flock to one pursuit but also attain like success, I can never find any of whose truth I am certain, thought I do find some which perhaps seem likely, and particularly the following. Genius is fostered by emulation, and it is now envy, now admiration, which enkindles imitation, and, in the nature of things, that which is cultivated with the highest zeal advances to the highest perfection. (p. 18)

Thus, according to Velleius, the florescence of creative activity is based on the socio-psychological processes of imitation, emulation, admiration, and envy. Each generation endeavors to surpass the achievements of the preceding generation, eventually reaching the heights of a Golden Age.

But why does the civilization recede from that high point? Velleius answered that

> it is difficult to continue at the point of perfection, and naturally that which cannot advance must recede. And as in the beginning we are fired with the ambition to overtake those whom we regard as leaders, so when we have despaired of being able either to surpass or even to equal them, our zeal wanes with our hope; it ceases to follow what it cannot overtake, and abandoning the old field as though pre-empted, it seeks a new one. Passing over that in which we cannot be pre-eminent, we seek for some new object of our effort. (p. 18)

Kroeber styled this process "pattern exhaustion."

Each generation is engaged in conceiving products that work out the implications or potential of a given aesthetic or philosophical system.

Once all the best has been extracted, and perfection reached, subsequent creators are left with the cultural dregs.

F. C. Bartlett (1958), the distinguished British psychologist, has described how this exhaustion process often takes place in scientific research:

> 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 technique 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. (p. 136)

Eventually, creative minds find another domain in which their talents can be better utilized, and a new configuration begins to grow (for indirect evidence, see Marcetti, 1980; Price, 1963).

Apropos of Galton's (1869) claims about the genetic superiority of the Athenian race, Velleius maintained that

> a single city of Attica blossomed with more masterpieces of every kind of eloquence than all the rest of Greece together – to such a degree, in fact, that one would think that although the bodies of the Greek race were distributed among the other states, their intellects were confined within the walls of Athens alone. (p. 18)

Yet for Velleius, and for Kroeber, this clustering of Athenian greatness could be ascribed to the joint agency of personal emulation and cultural exhaustion.

Enough of these speculations – what about scientific tests?

Happily, the conjectures of Velleius and Kroeber have been subjected to empirical scrutiny (Simonton, 1984d).

Because investigation of this subject requires the introduction of a special methodology, and because this methodology will prove useful in both this chapter and chapters 14 and 15, it is worthwhile to devote some space to outlining its principal characteristics.

After that I can return to the questions raised by Velleius and Kroeber.

Generational time-series analysis.

Although Kroeber's (1944) raw data consisted of chronological lists of eminent figures in various domains, he realized that the cultural configurations could often be better conceived in terms of a "generation," which he took to represent one third of a century.

> The individuals making up his lists could then be assigned to that generation in which they attained their "peak of productivity."
>
> Kroeber (1944) called this optimal career age the person's "acme" or "floruit," where age 40 was taken to provide "an unusually sound average estimate" (p. 27).
>
> In accord with Kroeber's basic thesis, genius was not randomly distributed over the generations, but rather appeared to be concentrated in certain periods, while other periods displayed a paucity, if not total absence, of genius.

Neither Ortega nor Kroeber nor any of their predecessors went beyond a fairly qualitative conception of the generation.

> As a consequence, the concept did not lend itself to the kind of precise statistical analysis that Galton (1869) could employ in his *Hereditary Genius*.
>
> Kroeber (1944) himself expressed begrudging admiration of Galton's quantitative analysis, and certainly would have wished to have offered something comparable in his argument on behalf of sociocultural determinism.

Nonetheless, with a few modifications, it is possible to integrate Kroeber's and Ortega's ideas into a methodological strategy that lends itself to a powerful analytical technique, namely time-series analysis. The resulting integration is called "generational time-series analysis" (Simonton, 1984c; cf. Sheldon, 1979, 1980).

This technique may be described as follows:

1. The historical period under consideration is subdivided into consecutive generations. Departing from the tradition, these time units are defined by 20-year intervals, or 5 generations per century. Figure 13.1 shows the corresponding historical slices for the formative period of psychology's history: 1820-1839, 1840-59, 1860-79, 1880-1899, and 1900-1919. Then adopting Kroeber's procedure, a given historical figure is assigned to that 20-year unit in which he or she attained age 40. Individuals who died prior to reaching 40 are still assigned as if they had done so, for reasons that will become clearer shortly. Let us identify all those individuals who have been assigned to this period as "generation *g*."

2. Assuming that the list of famous personalities is sufficiently dense, then there will be a respectable number of persons in most or all generations. As a result, it is possible to speak of the average characteristics of those who compose a particular generation. In particular, it may be said that the average person assigned generation *g*, or any other generation, will be 40 years of age. Furthermore, the typical individual will be around 30 at the beginning of this period and around 50 at the end of this period. The 30-50 age interval corresponds very closely with what was discussed in chapter 4. The first career landmark tends to appear around 30, the last around 50. Hence, the average person is assigned to that generation in which most of his or her most outstanding contributions are likely to have been made. This interval may be styled the individual's *productive period*.

3. If a typical member of generation *g* is 40, then those members will be around 20 in the preceding generation, or what is designated generation *g* - 1. More accurately, the average individual will be between 10 and 30 in this interval. According to this scheme, this interval is labeled the *developmental period* of the individual's life. It is during this phase that the person is most susceptible to various environmental influences, especially role models and mentors, that contribute to the development of a person's creative potential (Simonton, 1997b). In terms of Kroeber's (1944) views, it would be during the developmental period that the imitation and emulation processes would presumably kick in. Hence, the count of distinguished figures at generation *g* - 1 is tantamount to a measure of "role-model availability" for those in generation *g* who are at that time in their developmental period (Simonton, 1984c).

4. As Figure 13.1 makes clear, it is not just generation *g* - 1 that can provide role models for generation *g,* but generation *g* - 2 besides. Instead, they will have entered the third period of their life according to this scheme, namely what has been styled, for lack of a better word, the *consolidative period*. That is, when members of generation g are 20 ± 10, members of generation g - 2 will be 60 ± 10. Consequently, it is likely the luminaries two generations removed from those at generation g will be less effective role models.

5. Given a sequence of consecutive generations of sufficient length, the next step is to perform a time-series analysis (Box, Jenkins, & Reinsel, 1994). Specifically, the following equation can be fit to the data (Simonton, 1990d): $y_g = \phi_1 y_{g-1} + \phi_2 y_{g-2} + a_g$ (13.1) This is the equation for what is called second-order autoregression. The data-transformed tabulations of eminent figures at generation g provides the dependent variable y_g, which is regressed on the corresponding tabulations at generation g - 1 and g - 2, y_{g-1} and y_{g-2}, respectively. The autoregressive parameters ϕ_1 and ϕ_2 assess the magnitude of the same two effects. Finally, a_g represents an independent random shock, in a manner identical to the error term in a regular regression equation. If the Velleius-Kroeber interpretation is correct, then $\phi_1 > 0$.

6. That is, the count of eminent figures in generation g should be a positive linear function of the count at generation g - 1. Under most conditions, moreover, $\phi_2 < \phi_1$, and perhaps may even approach zero. In words, predecessors in their productive period should provide role models superior to those in their consolidative period. Finally, but less obviously, when the residuals of the autoregression are closely examined, they should exhibit a random temporal distribution (i.e., "white noise" according to the jargon of the technique). This latter demonstration permits the conclusion that some other stochastic process (viz., a third-order autoregressive or even moving-average model) does not better explicate the generational time series. Instead, the clustering of genius into contiguous generations is totally explained in terms of the autoregressive process that provides the formal representation of the role-modeling effects. The number of geniuses in any given generation would be a simple function of the number of geniuses in the preceding generation who are available for imitation and emulation.

That, in a nutshell, is how generational time-series analysis works.

Of course, there are several complications in the actual procedure.

Usually the raw data must undergo several transformations, such as special operations to remove any secular trends (e.g., linear or exponential).

Moreover, often the generational counts are weighted, so that the more eminent figures in the field provide more points than the less eminent (e.g., Gray, 1958, 1966; Simonton, 1975d, 1988b; Sorokin, 1937-1941).

Generational time-series analysis provides a direct test of whether the clustering of genius can be explained in terms of the cross-generational effects described by Velleius and Kroeber.

DATE

1820-1839	**1840-1859**	**1860-1879**	**1880-1899**	**1900-1919**
	GENERATION ***g* - 2**			
Developmental period (age 20 ± 10)	**Productive period** (age 40 ± 10)	**Consolidative period** (age 60 ± 10)		
		↓		
		GENERATION ***g* - 1**		
	Developmental period (age 20 ± 10)	**Productive period** (age 40 ± 10)	**Consolidative period** (age 60 ± 10)	
		↓		
			GENERATION ***g***	
		Developmental period (age 20 ± 10)	**Productive period** (age 40 ± 10)	**Consolidative period** (age 60 ± 10)

GENERATIONAL PLACEMENT OF SOME GREAT 19TH CENTURY PSYCHOLOGISTS

Fechner 1801-1887, J. Müller 1801-1858, C. Darwin 1809-1882, Bernard 1813-1878, Ludwig 1816-1895, Brown-Séquard 1817-1894, Lotze 1817-1881, Donders 1818-1889, Bain 1818-1903, Du BoisReymond 1818-1896, Brücke 1819-1892

Spencer 1820-1903, Helmholtz 1821-1894, Galton 1822-1911, Liébeault 1823-1904, Broca 1824-1880, Charcot 1825-1893, Aubert 1826-1892, Pflüger 1829-1910, Sechenov 1829-1905, Wundt 1832-1920, Meynert 1833-1892, Dilthey 1833-1911, Hering 1834-1918, C. Lange 1834-1900, Lombroso 1835-1909, Hitzig 1838-1907, Mach 1838-1916, Fritsch 1838-1927, Brentano 1838-1917, Ribot 1839-1916

Bernheim 1840-1919, Le Bon 1841-1931, James 1842-1910, Breuer 1842-1925, Golgi 1843-1926, Avenarius 1843-1896, G. S. Hall 1844-1924, Nietzsche 1844-1900, Emmert 1844-1911, Ladd-Franklin 1847-1930, Stumpf 1848-1936, Pavlov 1849-1936, Ebbinghaus 1850-1909, G. E. Müller 1850-1934, C. L. Morgan 1852-1936, Ramón y Cajal 1852-1934, Féré 1852-1907, Prince 1854-1929, Kraepelin 1856-1926, S. Freud 1956-1939, Bekhterev1857-1927, Coué 1857-1926, Pearson 1857-1939, Binet 1857-1911, Babinski 1857-1932, Sherrington 1857-1952, Ellis 1859-1939, Loeb 1859-1924, Janet 1859-1947, Bergson 1859-1941, Dewey 1859-1952

Role-model availability.

Unfortunately, the history of psychology proper is too short to permit the application of the technique just outlined.

Even with the most liberal definition of the discipline, the units would number only a dozen or so. At five generations per century, it would take a millennium before there would be sufficient degrees of freedom to apply time-series methods.

Nonetheless, the technique has been applied to related domains of achievement that enjoy much longer histories.

With only a few minor exceptions, these studies have supported the first-order autoregressive model (Simonton, 1975d, 1988b, 1992a).

> The results of an inquiry into Chinese civilization are representative (Simonton, 1988b).
>
> This investigation began by compiling a chronological listing of all the major figures in Chinese history from 840 BC to AD 1979.
>
> This compilation incorporated all of the individuals listed in Kroeber's (1944) work as well as thousands more drawn from dozens of histories, chronologies, biographical dictionaries, and encyclopedias.
>
> The 10,160 luminaries so selected were divided into distinctive achievement domains, and then assigned to 141 consecutive 20-year periods.

Of special relevance here were the generational time series constructed for philosophy, mathematics, physical sciences, and the biological sciences – the four groups with the closest affinity with psychology in its own historical development.

> In every single case, the number of eminent figures at generation *g* was a positive function of the number at generation $g - 1$, but not of the number at generation $g - 2$.
>
> The autoregressive parameters (i.e., the ϕ_1s) were as follows: philosophy .50, mathematics .51, physical sciences .38, and biological sciences .29.
>
> These results obtained for the unweighted tabulations, but pretty much the same findings appeared when the counts were weighted according to the differential distinction attained by the various philosophers, mathematicians, and physical and biological scientists.
>
> The only difference was that the autoregressive parameters were often smaller, ranging between .23 for the biological sciences and .38 for the physical sciences.
>
> For both weighted and unweighted generational time series, the first-order parameter was statistically significant and positive while the second-order was not statistically significant and was close to zero.
>
> Finally, but quite importantly, all eight time series became random (i.e., were reduced to white noise) once the effects of the first-order autoregression were extracted.
>
> The clustering of genius could thus be totally explained in terms of the association between two contiguous generations.

Given the foregoing results, it seems highly likely that a similar role-modeling process has played a major role in psychology's history, however short the period in which it has had the opportunity to operate.

Role-modeling effects can take many forms, but the most obvious and direct are the mentor-student or master-disciple relationships discussed in chapter 11.

Hence, the greater is the availability of role models, the higher is the likelihood that these direct relationships can maintain the discipline's vitality.

Although the autoregressive model does such a great job explicating the data, two problems remain to be addressed.

The first, and least critical, is that it fails to specify the scope of the domains to which it applies.

Second, and more critical, is the apparent fact that the autoregressive model has left something out of the Velleius-Kroeber formulation. The model captures the hypothesized impact of imitation, admiration, or emulation, but what about the notion of pattern exhaustion?

Contemporaries: Synchronous Associations

Earlier I said that generational time-series could consist of either unweighted or weighted counts of historical figures.

I also stated that these alternative operational definitions of transhistorical fluctuations in creative activity yield the same basic conclusions.

For both weighted and unweighted measures, the score at generation g is a positive function of the score at g - 1, that is, the time series exhibit first-order autoregression.

This concurrence implies that the greatest figures of history tend to appear in the same generations as do the lesser figures.

Empirical support for this inference was found in a generational time-series analysis of 10,160 notables of Chinese civilization (Simonton, 1988b).

For each domain of achievement these individuals were split into major and minor figures, according to the number of times they were mentioned in various sources.

On the average, the major figures constituted about a third of the total count.

After tabulating the major and minor figures into their separate generational time series, the cross-correlations between the two series were assessed.

That is, the correlations between major and minor figures were calculated for various lags.

In every single domain, the synchronous correlation between the two series was the highest.

The greater was the degree of lag, the smaller the size of the cross-correlation.

Hence, the activity of major creators does not tend to stimulate the activity of minor creators with a delay of one or more generations, nor did the activity of minor creators stimulate the activity major creators after some generational lag in the reverse direction.

Furthermore, the synchronous correlations were all of respectable magnitude.

To cite the statistics most relevant to psychological science, the correlations were .54 for the philosophers, .49 for the mathematicians, .32 for the physical scientists, and .46 for the biological scientists (or .36, .41, .31, and .26, respectively, for detrended data).

What accounts for the generational simultaneity of major and minor figures?

One possibility is simply that the appearances of both great and small are likewise contingent on the availability of role models in the previous generation.

Another explanation might be derived from the equal-odds rule treated back in chapters 3 and 4.

The more individuals who are active in a particular domain, the higher should be the odds that a subset of them might attain true greatness (see, e.g., Lawani, 1986).

These explanations all view extreme greatness as a passive or incidental outcome of the sheer mass of activity in a particular generation.

Yet it could be that the connection between great and small is more dynamic and direct that these interpretations imply.

Once a certain "critical mass" is reached, a "chain reaction" might take place where individuals are inspired to reach higher levels of creativity (see, e.g., Fowler, 1987).

One basis for this belief is the Price Law introduced in chapter 3.

The original formulation of this law was expressed in a provocative manner, namely, that "the total number of scientists goes up as the square, more or less, of the number of good ones" (Price, 1963, p. 53).

The "good ones" are those who collectively account for half of all contributions to the field.

Hence, if the total number of scientists within a given generation and specified field equals k, then half of all work can be attributed to $\sqrt{k}$.

This implies that as k increases, the proportion of good scientists declines.

If there are only 10 working in an area, then about one third will account for half of all the contributions ($\sqrt{10} \approx 3.2$), whereas if the number increased to 100, the productive elite represents only 10% of the whole ($\sqrt{100} = 10$).

In general, as the number of participants increases, the discipline becomes ever more elitist.

The expanded elitism predicted by the Price Law suggests that the members of a given generation are doing something to simulate the creativity of the greatest thinkers of their generation.

> One likely explanation is that the members of a large disciplinary cohort form various kinds of professional relationships that encourage and maintain creative achievement.
>
> This specific linkage was illustrated in a study of 2,026 eminent scientists (Simonton, 1992c; also see Simonton, 1984a).
>
> For each scientist was recorded the number of professional associates of different types, such as collaborators, correspondents, friends, and even rivals.
>
> These measures of professional relationships were then correlated with three criteria of overall achievement, namely lifetime creative output, active career length, and posthumous reputation.
>
> The correlations were uniformly positive and statistically significant (even after introducing controls for potential artifacts).
>
> The more eminent, enduring, and prolific scientists had more professional connections than did their less successful colleagues.

Only as science became increasingly institutionalized, especially in the guise of research laboratories and academic institutions, would collaboration become a major factor in enhancing scientific creativity.

> Similar patterns are seen in psychology (Over, 1982a). In 1949 the average number of authors of papers appearing in APA journals was about 1.5, but by 1979 this mean increased to 2.2.
>
> Nor is this trend restricted to lesser figures in the discipline. For 69 eminent American psychologists active between 1879 and 1967, the correlation between the percentage of works that were coauthored and the year of birth was .49, a very substantial figure.
>
> Corresponding with this increased emphasis on collaboration is a tendency for collaborative research to receive slightly more recognition, including citations (e.g., Ashton & Oppenheim, 1978; Beaver, 1986; Diamond, 1985; Smart & Bayer, 1986).

For example, highly productive university faculty tend to display higher rates of communication with researchers at other institutions (Blackburn, Behymer, & Hall, 1978).

In addition, the more prolific was the researcher, the higher was the probability that he or she came from a large academic department (Blackburn, Behymer, & Hall, 1978).

This latter fact should be integrated with an observation made in chapter 11. Great scientists typically end up being affiliated with distinguished research institutions (e.g., Crane, 1965; Manis, 1951).

A portion of this effect might be ascribed to the number of collegial relationships that are available.

This likelihood is suggested by the results of a study of 180 psychology departments in the United States, Canada, and Great Britain (Endler, Rushton, & Roediger, 1978).

The department's overall reputational rating was highly correlated with the number of fulltime faculty affiliated with the department (also see Helmreich et al., 1981).

Faculty size was also strongly correlated with total departmental publications and citations.

More provocatively, faculty size was positively associated with the mean number of publications, the mean number of citations, and the median number of citations.

Thus, on a per capita basis, the larger departments were more productive and more influential. This enhancement implies the existence of some synergistic process such that the output of the whole is greater than the separate parts.

This departmental impact on personal productivity helps account for a finding also noted in chapter 11, namely that scientists who exhibited upward mobility by moving from less prestigious to more prestigious institutions tend to increase their overall productivity (Allison & Long, 1990; also see Long & McGinnis, 1981).

Yet it is essential to point out that the benefits of such affiliation are not just short term. In the study of 69 eminent American psychologists, affiliation with a distinguished research institution correlated .37 with the total number of cited publications, .33 with the total number of citations, and .27 with the number of citations to the single most influential work (Simonton, 1992b).

All told, the clustering of genius into Kroeberian configurations may have two major sources: lagged and synchronous.

COMTIAN PROGRESS

Kroeber's notion of sociocultural change seems antithetical to the modern concept of human progress – the belief that the history of civilization is a record of constant improvement.

This belief has had many adherents, but among the most forceful was certainly Auguste Comte, the early 19th century French philosopher who founded positivism.

- Comte argued that human progress was not a hypothesis or conjecture, but rather it was an outright law of civilization.
- The history of the human mind consisted of three stages, the theological, and metaphysical, and the positive.
- The last of these three stages represented the culmination of the upward progression, for knowledge would depend solely on reason and observation.
- In a word, human civilization culminated in science.
- At the same time, Comte believed that different domains of knowledge progressed through this sequence of stages at different rates.
- The first to reach the highest state was astronomy, followed by physics, then chemistry, and much later physiology.
- Comte argued, moreover, for the emergence of a new department of positive philosophy, which he christened sociology.

Comte's theory of human progress thus implies an internalist history of science.

- Each scientific discipline advances through the theological, metaphysical, and positivist stages according to their intrinsic characteristics.
- Those that deal with more abstract and simple phenomena, such as astronomy, advance to the acme more quickly than those that treat more concrete and complex phenomena, such as sociology.
- To be sure, Comte also argued that the degree of advancement depended on the extent to which each science was contingent on other sciences.
- Astronomers could develop independent of what happened in other disciplines, whereas physiology depended on chemistry, and chemistry on physics.
- Nevertheless, even with this complication, the progress of any given scientific domain is mainly a function of that domain's subject matter and the progress of those other domains of science on which its development depends.

This Comtian philosophy and history of science leads naturally to two sets of research questions that deserve a positivistic response:

1. What evidence is there that various scientific disciplines can be ordered into some hierarchy? Are some sciences closer to the positivistic ideal of integrated logic and fact than are others? If the sciences can be ordered into a hierarchy, where does psychology fit in? Between sociology and physiology, or in some more ignoble position?
2. What evidence is there that any given scientific discipline exhibits progress in a Comtian manner? Better yet, has psychology displayed an upward progression similar to the other sciences? And has psychology arrived at the stage of true positive philosophy, or must it still be considered pre-scientific?

These questions are obviously critical if we wish to comprehend not only the history of psychology, but the scientific status of psychology besides.

Interdisciplinary Hierarchies

I first review the evidence against the existence of a hierarchy of sciences, and then follow with a demonstration that such hierarchies may indeed exist, especially if the supposedly disconfirming data are properly analyzed.

Anti-Comte.
The first systematic attempt to determine whether the various sciences could be ordered into some scientific hierarchy was carried out by a sociologist, Stephen Cole (1983), a representative of the Mertonian school of the sociology of science.
Cole began his inquiry by defining the six interrelated criteria that would be used to decide where any given discipline would be placed in the presumed hierarchy.
At the top would be those sciences that

(a) have well-developed or highly "codified" theories,
(b) quantify their ideas in mathematical language,
(c) obtain high levels of consensus among its practitioners with respect to theory, methods, important problems, and the like,
(d) feature high rates of obsolescence as recent work quickly replaces the old, and
(e) accumulates knowledge at a very rapid pace.

At the bottom would be those that

(a) have few generalizations and low level of codification,
(b) express their key concepts in words,
(c) show little consensus and hence agree little on the worth of any single person's contribution,
(d) retain many references to older, so-called "classical" works that continue to be relevant to current research, and
(e) accumulate knowledge at a very slow pace.

These criteria, while not identical to Comte's, certainly capture the gist of his ideas that pure science is founded in rational empiricism, and that the application of this positivistic approach would contribute to rapid progress in knowledge about the phenomena examined by the domain.
Before S. Cole (1983) could apply these criteria, it was first necessary to make a critical distinction regarding two types of knowledge within any given scientific discipline.

The first type is the *core*, which consists of "fully evaluated and universally accepted ideas which serve as the starting points for graduate education" (p. 111).
The second type is the *research frontier*, which includes "all research currently being conducted" (p. 111) at the leading edge of the discipline.

This distinction was important because S. Cole (1983) found that all scientific disciplines were very similar when it came to what was taking place at their respective research frontiers.
In particular, there were no consistent contrasts with respect to the degree of disciplinary consensus or the rate that new findings and concepts are incorporated into the body of disciplinary knowledge.
Cole's ultimate conclusion was that "in all sciences knowledge at the research frontier is a loosely woven web characterized by substantial levels of disagreement and difficulty in determining which contributions will turn out to be significant" (p. 111).
Evidently, the degree to which a discipline has implemented Comtian positivism does not ameliorate the ambiguities that attend the leading edge of research.
Other investigations appear to endorse Cole's generalization.

For instance, one study showed that the citation practices in the natural sciences, social sciences, and even the arts and humanities differed very little (Barnett, Fink, & Debus, 1989). In all three the citations received by a new publication peaked within two years and then gradually declined, and the shape of the curve was virtually identical for the social and natural sciences.
Another inquiry assessed whether empirical findings in the "hard sciences" were really more cumulative than findings in the "soft sciences" (Hedges, 1987). Using standard statistical methods for comparing the consistency of results in multiple experiments, no difference could be found. In particular, the basic properties of certain elementary particles in high-energy physics were determined no better than various psychological parameters associated with spatial perception and visualization, verbal ability, mathematics achievement, self-concept, student-rating validities, and so forth.

Pro-Comte.

These results notwithstanding, other researchers have offered data that imply a very different conclusion. Back in chapter 11 it was observed that the age at which a scientist most typically receives a major award or honor varies according to the discipline.

These differences might be attributed to placement in the hierarchy of the sciences.

For example, the mean age at which a great scientist becomes a Nobel laureate – physics 49, chemistry 53, and medicine or physiology 55 – corresponds with the degree of codification that characterizes each of the three fields (Shin & Putnam, 1983).

Presumably, in less codified fields it takes longer before a consensus is reached on the merits of a scientist's key contributions.

Note, too, that this order concurs perfectly with Comte's ordering.

The principal drawback to using these statistics as evidence for a scientific hierarchy is that there exists an alternative explanation, namely, interdisciplinary contrasts in the age-productivity curves (see, e.g., Simonton, 1991a).

Two other sources of evidence do not suffer from this objection.

1. The first was a study that examined this question from the standpoint of Leon Festinger's (1954) social comparison theory (Suls & Fletcher, 1983). Briefly put, this theory states that human beings tend to compare themselves with similar others whenever they are uncertain about some belief. If the various sciences differ in the amount of consensus they display with respect to important theories, methods, and substantive issues, then the scientists will correspondingly exhibit distinctive degrees of uncertainty about the merits of their research. The higher is the magnitude of their uncertainty, the stronger will be their desire to consult with colleagues before submitting a paper for publication in the discipline's journals. This consultation will be revealed in the acknowledgment sections of the published articles. Hence, to test this hypothesis, the investigators measured the number of colleagues who were consulted in the journal articles of physics, chemistry, psychology, and sociology. Consistent with prediction, "social scientists were more likely to have consulted with their colleagues than were physical scientists" (p. 575).

2. The second evidence source involves the interdisciplinary variation in a measure called the "theories-to-laws ratio" (Roeckelein, 1997). This measure is based on the relative representation of theories and laws in the textbooks of a discipline. It is specifically defined as the count of theories cited divided by the count of laws cited. Those disciplines that stand at the top of the Comtian hierarchy should have a low theories-to-laws ratio, whereas those at the bottom should have a high ratio. That is, an established science will boast many laws, whereas a struggling science will blush under the profusion of mere theories. This measure was applied to 246 textbooks for five sciences published from 1866 to 1996. The results were fairly consistent with expectation (Roeckelein, 1997b). The average ratios across over a century of textbooks were as follows: physics 0.4:1, chemistry 0.5:1, biology 2.6:1, anthropology 2.8:1, psychology 3.8:1, and sociology 7.3:1. Physics and chemistry clearly come out on top by this criterion, their textbooks containing at least twice as many laws as theories. Biology and anthropology, on the other hand, land a few notches down, as theories outnumber laws by almost three to one. Sociology, moreover, rests at the bottom, with a ratio of more than seven to one. The textbooks of psychology, finally, show a ratio of about four to one, putting our discipline closer to biology and anthropology than to sociology. In the Comtian hierarchy, psychology is more a natural than a social science.

Of course, the above study differs from the rest in that it concentrated on the core rather than the research frontier of each discipline.
That difference alone could explain any discrepancies with those studies that fail to find evidence for a hierarchy of sciences.
Nevertheless, on closer examination, the results are actually not that discrepant. The problem with all of this research is that it tends to address the substantive question piecemeal, one investigator using this criterion and another scientist another criterion.
Moreover, the specific disciplines examined vary from study to study, often in ways that depart significantly from Comte's original conceptions.
Even worse, the various alternative rankings of the sciences are not subjected to any rigorous statistical test of the degree to which they might be in agreement.
Therefore, it is conceivable that a systematic statistical comparison of multiple criteria applied to the same disciplines might demonstrate the presence of a bona fide Comtian hierarchy.

In book I tested the hierarchy, but a more recent study has done this more systematically and extensively.

Simonton, D. K. (2004m). Psychology's status as a scientific discipline: Its empirical placement within an implicit hierarchy of the sciences. *Review of General Psychology*, *8*, 59-67.

Primary Measures

The first set of indicators satisfies three specifications.

First, the measures must have a strong theoretical or empirical connection with the supposed scientific status of a scientific discipline.

Second, the indicators must include assessments on the four disciplines in Simonton's (2002) study, namely, physics, chemistry, psychology, and sociology.

Third, all indicators must be objective rather than subjective.

The following seven indicators met these three standards:

1. *Theories-to-laws ratio* – Roeckelein (1997, Table 2, p. 137) assessed the number of theories and the number of laws mentioned in introductory textbooks in physics, chemistry, psychology, anthropology, and sociology. These counts were then used to compute the ratio of theories to laws, the higher the ratio the more "soft" is the discipline. That is, exact sciences have many more laws in proportion to mere theories. In a sense, scientific status is a function of the ratio of precise facts to vague conjectures.

2. *Consultation rate* – The next criterion was a consultation measure based on Festinger's social comparison theory (Suls & Fletcher, 1983, Table 1, p. 578). According to this theory, when people are uncertain about their beliefs or performance they are more likely to engage in social comparison with similar others. The specific measure was the number of colleagues recognized in the acknowledgment section adjusted for the number of authors. In other words, the measure is independent of the number of collaborators. The higher is this number the greater is the apparent uncertainty about the quality of one's work. This score was available for physics, chemistry, psychology, and sociology.

3. *Obsolescence rate* – Based on the relative frequency of citations to older publications, McDowell (1982) determined the rate at which knowledge becomes obsolete for the disciplines of physics, chemistry, biology, sociology, psychology, history, and English. The specific measure used here was his calculation of the expected publication cost of interrupting a career for just one year (McDowell, 1982, Table 2, p. 757). For example, if the career is interrupted for a single year (e.g., administrative work, parental or health leave), the output of physicists will be cut by about 17% whereas the productivity of psychologists would be cut by about 10% (because physicists would have much more "catching up on the literature" to do before they can resuscitate their careers). This measure was not used in Simonton's (2002) investigation.

4. *Graph prominence* – Cleveland (1984) assessed the extent that graphs appear in articles published in the professional journals, demonstrating that graphs are more extensively used in the "hard" disciplines (see also Smith, Best, Stubbs, Archibald, & Roberson-Nay, in press). The specific disciplines were physics, chemistry, biology, medicine, psychology, economics, and sociology. Although Cleveland (1984) did not aggregate the findings for the disciplines, this aggregation was carried out in Smith et al. (2000). This graph measure is also new to the current investigation (cf. Simonton, 2002).

5. *Early impact rate* – In Table 2 Cole (1983) provided the "proportion of scientists under 35 whose work received more than the mean number of citations for their field" (p. 118). Those fields that incorporate most quickly the work of young scientists are assumed to rank higher in the hierarchy because such disciplines have a stronger consensus about what can be regarded a significant contribution to the field. The disciplines covered were mathematics, physics, chemistry, geology, psychology, and sociology.

6. *Peer evaluation consensus* – Cole's (1983) Table 3 provided data indicating the "consensus on evaluating scientists by field" (p. 120), where 60 scientists per field were rated by colleagues in the same discipline. The consensus was gauged by the mean standard deviation of the ratings, the lower the standard deviation the higher the consensus. The disciplines in this case were physics, chemistry, biochemistry, psychology, and sociology.[1]

7. *Citation concentration* – The "concentration of citations to research articles" was presented in Table 5 of Cole (1983, p. 122). The citations were to journals in mathematics, physics, chemistry, biochemistry, geology, psychology, and sociology. If the citations are all concentrated in a single article, the disciplinary consensus must be very high, scientists concurring on what contributions deserve the status of "citation classics." In contrast, if the citations are more evenly distributed across articles, then the consensus must be minimal. In the case of completely even distribution, in fact, the citations received by articles would not differ from chance expectation.

Because the above seven variables were measured on rather different scales, the raw scores were standardized to z scores ($M = 0$, $SD = 1$). In addition, those variables that were reverse indicators – namely the theories-to-laws ratio, the consultation rate, and the peer evaluation consensus – were inverted by reversing the sign of the standardized scores.

Secondary Measures

The second set of measures all have one thing in common: However many disciplines to which they are applied, they have a missing value for at least one of disciplines in Simonton's (2002) study.

In addition, even though all are relevant to a discipline's scientific status, not all of them are completely objective.

There were five indicators in this group:

1. *Lecture disfluency* – Schachter, Christenfeld, Ravina, and Bilous (1991) determined the rate of filled pauses ("uh," "er," and "um") during classroom lectures for undergraduate courses in mathematics, chemistry, biology, psychology, economics, sociology, political science, philosophy, art history, and English. The higher the number of pause words per minute, the greater is the degree of speech disfluency, which presumably reflects the degree to which a discipline is less formal, structured, and factual. This interpretation is bolstered by the fact that the same set of lecturers did not differ in disfluency when speaking on a common subject. Hence, it is not a matter of the more inarticulate scientists being attracted to the less rigorous disciplines.

2. *Citation immediacy* – Cole (1983, Table 8, p. 126) calculated the extent to which the references in published articles were confined to recent work. In other words, the calculation gauges whether the citations emphasize contemporary research is emphasized over classic studies. Scores on this immediacy factor were available for physics, chemistry, biochemistry, geology, and psychology (but not sociology, and hence its omission from the Simonton, 2002, inquiry).

3. *Anticipation frequency* – Hagstrom (1974, Table 1, p. 3) reported the results of a survey of 1,718 scientists who asked to report whether they had their work had been anticipated by other scientists. The percentage of scientists who had this experience at least once during their career course was gauged for mathematics, physics (combining theoretical and experimental), chemistry, and biology (combining experimental and other). The greater is the frequency of anticipation, the higher the consensus on what are deemed the important and unimportant problems in a discipline.

4. *Age at Nobel Prize* – Stephan and Leven (1993, Table 1, p. 395) provided the median age at which scientists received Nobel prizes in the fields of chemistry, physics, and medicine (from 1901-1992). Using the information provided at the official Nobel Prize site (http://www.nobel.se) the same statistic was obtained for the recipients of the economics prize (from 1969 to 2001). The logic behind including this indicator is the same as the early impact rate measure among the primary predictors. The more codified or paradigmatic a discipline is, the sooner it can recognize when a scientist has made an exceptional contribution to the field.

5. *Rated disciplinary hardness* – Smith et al. (2000) had psychologists rate disciplines on the degree to which they could be considered "hard" versus "soft." The respondents used a 10-point Likert scale, with 10 indicating the highest degree of hardness. Seven disciplines were so rated, namely, physics, chemistry, biology, medicine, psychology, economics, and sociology. Smith et al. (2000) showed that this subjective assessment correlated .97 with Cleveland's (1984) measure of graph use. In addition, the investigators showed that this hardness assessment correlated .94 with an independent measure of paradigm development in various disciplines (Ashar & Shapiro, 1990). This measure, although subjective, was included to determine whether the objective assessments concur with more intuitive attitudes about the relative status of different scientific disciplines.

As before, the above measures were all standardized to a mean of 0 and a standard deviation of 1. Moreover, lecture disfluency was inverted by multiplying by -1.

Generating the Composite Measure from the Primary Indicators

The four disciplines of physics, chemistry, psychology, and sociology were used to calibrate an analytical base line for devising a more comprehensive measure that would apply to a wider range of disciplines.

The first step was to calculate the correlations among the seven measures for just these 4 sciences, the resulting correlations ranging between .63 and .998.

These correlations were then subjected to a principal components analysis.[2]

Only one component had an eigenvalue exceeding unity, and that lone component accounted for 86% of the total variance.

Moreover, the loadings on the first component were uniformly high, ranging from .86 to .99.

The specific loadings were as follows: theories-to-laws ratio .99, consultation rate .99, graph prominence .96, peer evaluation consensus .93, early impact rate .88, citation concentration .87, and obsolescence rate .86.

As a consequence, the standardized scores across all seven measures were averaged to produce a linear composite.

The internal-consistency reliability (Cronbach's alpha) for this composite was .96.

The next step was to extend this linear composite to all disciplines that contained at least one non-missing value on the seven primary indicators.

This was accomplished by simply averaging the standardized scores across all indicators with non-missing values for a given discipline.

This means that a discipline's score on the linear composite may represent anywhere between one and seven scores.

Of course, the expected measurement error will be greater for those disciplines that have more missing values.

The ratings based on a single component criterion would be the least reliable.

In any case, the resulting composite measure was re-standardized to a zero mean and unit standard deviation.

Validating the Composite Measure Using Secondary Indicators

To validate the resulting composite indicator, the scores on the secondary measures were plotted as a function of scores on the composite measure. The outcome is shown in Figure 1, which also gives the lines of best least-squares fit.

Higher scores on the composite are associated with lower lecture disfluency, higher concentration of citations on more recent literature, more frequent experiences of anticipation, the greater youthfulness of Nobel Prize recipients, and higher rated disciplinary hardness.

The corresponding Pearson product-moment correlation coefficients range between .60 and .97, with a median of .88.

This secondary composite correlates .87 (n = 11, p = .0004) with the primary composite, thereby confirming statistically what is so apparent graphically in Figure 1.

The 12 primary and secondary indicators reflect a coherent latent variable on which disciplines can be reliably differentiated.

Ranking Five Disciplines Using the Composite Measure

Because five disciplines have non-missing values on at least three of the primary indicators, it is possible to provide a fairly reliable rankings for this subset of the 13 studied.

Figure 2 shows the outcome.

Physics, chemistry, biology, psychology, and sociology are arrayed according to standardized composite score and rank.

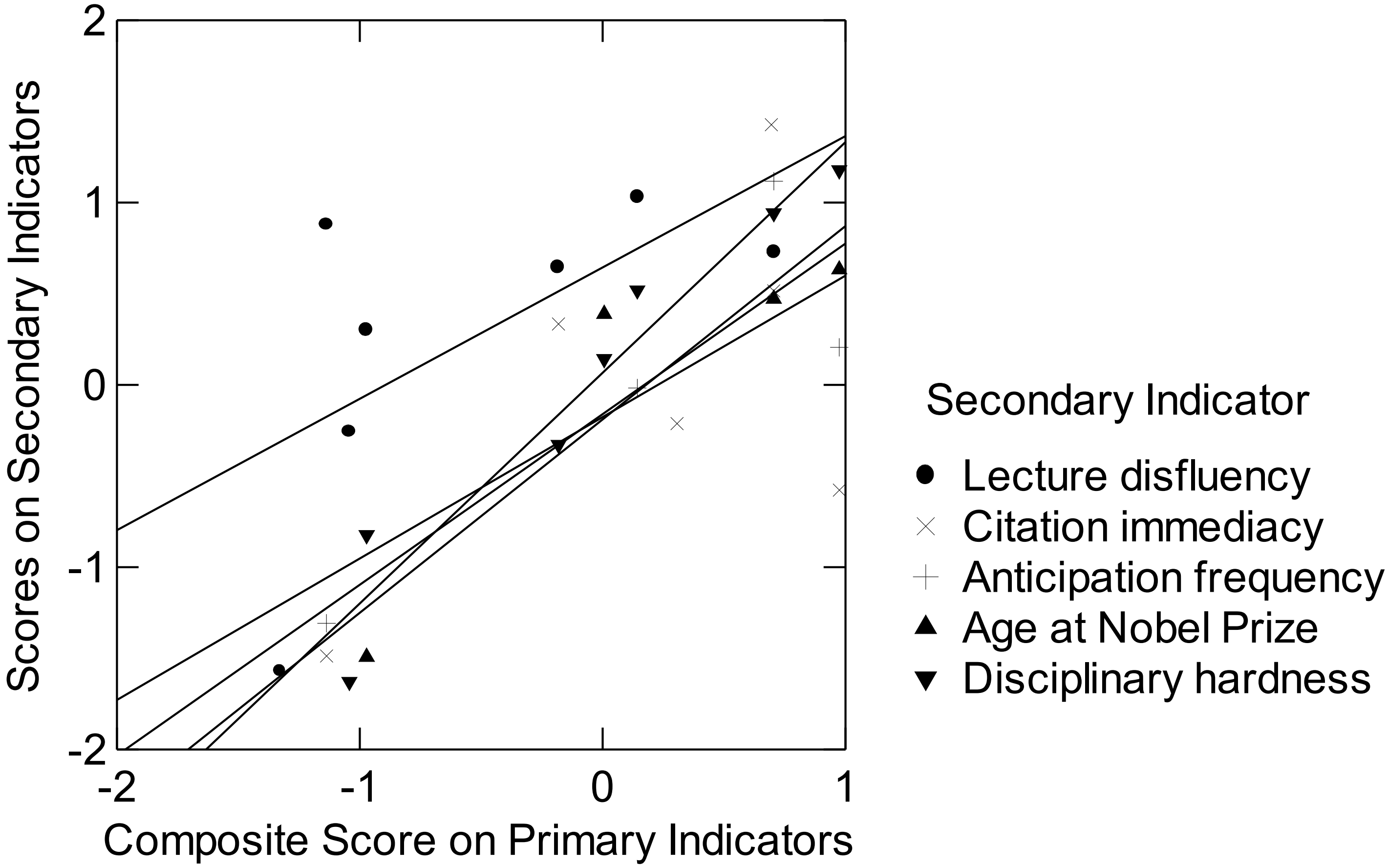
The composite scores for physics, chemistry, psychology, and biology are based on all seven primary indicators, and thus have an internal-consistency reliability of .96.

Because the standard error of measurement is equal to the square root of 1 minus the reliability coefficient, the error for these four disciplines is only 0.2, or just one fifth of a standard deviation.

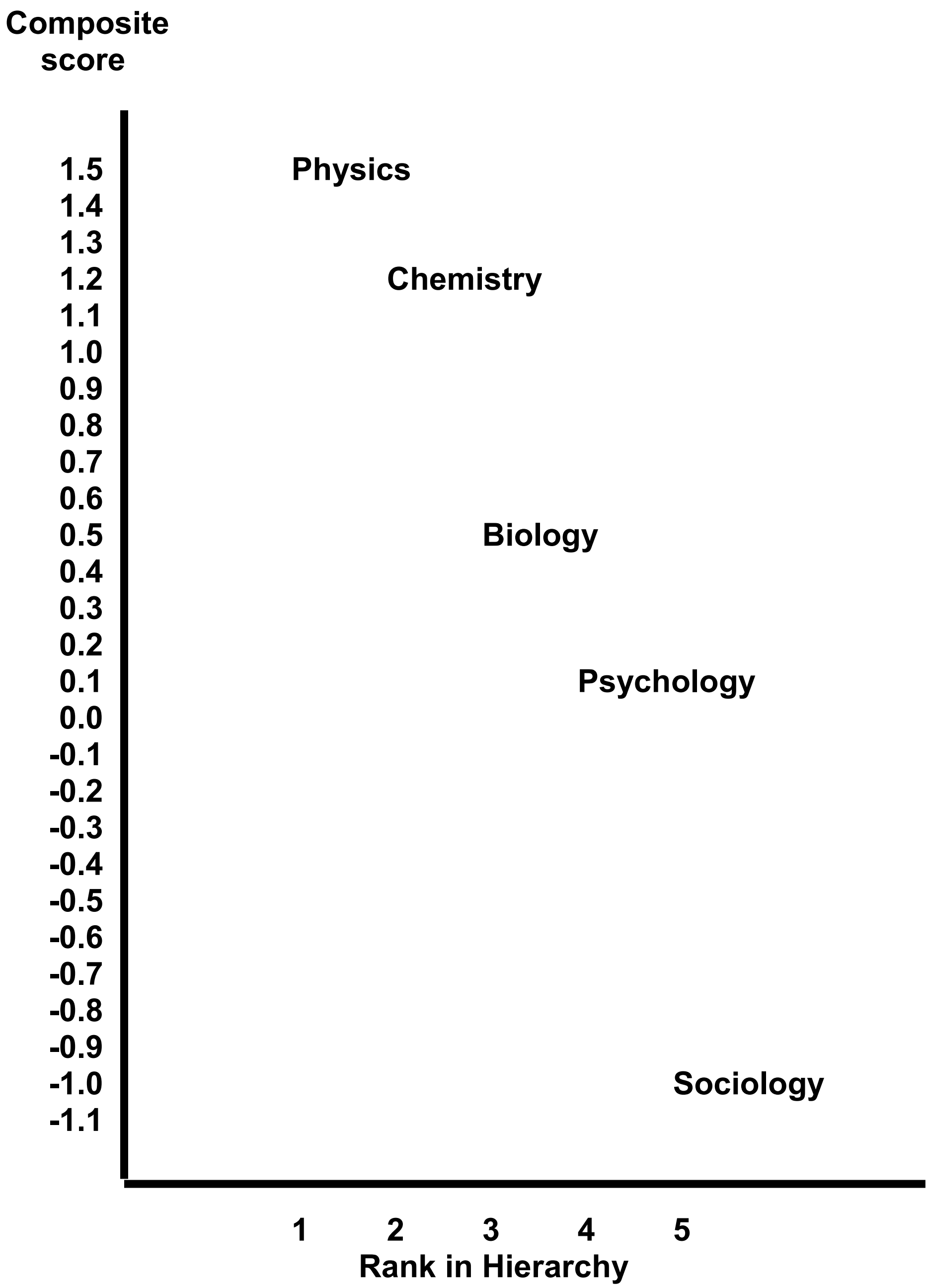
The composite score for biology, in contrast, was based on only 3 indicators, with a reliability of .89.

Even so, the standard error of measurement is still reasonably small, namely 0.3, or about a third of a standard deviation. Consequently, the ordinal placement of these five disciplines is reasonably secure.

It is immediately apparent that the disciplines are ordered in close conformity to the expected hierarchy.



Scores on Secondary Indicators
Composite Score on Primary Indicators
2
1
0
-1
-2
-2
-1
0
1
Secondary Indicator
Lecture disfluency
Citation immediacy
Anticipation frequency
Age at Nobel Prize
Disciplinary hardness


Composite score
1.5
1.4
1.3
1.2
1.1
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0.0
-0.1
-0.2
-0.3
-0.4
-0.5
-0.6
-0.7
-0.8
-0.9
-1.0
-1.1
Physics
Chemistry
Biology
Psychology
Sociology
1
2
3
4
5
Rank in Hierarchy

Intradisciplinary Advancement

If Comte's philosophy is correct, then the positivistic status of a science is not a static phenomenon, but rather it should change over time – at least until its history culminates in pure positivism.

Therefore, even if psychology falls below the natural sciences in the scale of scientific perfection, it should exhibit a transhistorical trend toward reaching the more elevated levels in the hierarchy.

That is, psychology should become more scientific over the course of its history.

This hypothesis might be easily tested using the theories-to-laws ratio which has already been shown to have the highest factor loading on the 6-item principal component that gauges scientific status.

> Besides reporting the overall means across all the textbooks for each science, the investigator also provided separate means for consecutive periods (Roeckelein, 1997).
>
> Moreover, the periods were 1866-1919, 1920-1939, 1940-1959, 1960-1979, and 1980-1996, and thus are tantamount to a generational analysis, except for the more inclusively defined first time interval.
>
> In the case of physics, chemistry, and biology, the theories-to-laws ratio declines over time.
>
> When I calculated the correlations between their scores and the date of the midpoint of the five periods, I obtained the values of -.70, -.16, and -.72, respectively.
>
> Hence, despite their high status of these three sciences in the Comtian hierarchy, there remained some room for improvement, especially in the case of physics and biology.

Yet the generational changes are strikingly different for the other three sciences, all of which have positive trends.

> In particular, the correlations between the scores and the date are .88 for sociology and .90 for anthropology and psychology!
>
> Even worse, the coefficient of .90 is statistically significant at the .05 level, the small sample size notwithstanding, so that the trend cannot be dismissed as mere chance fluctuation.
>
> Hence, despite the high theories-to-laws ratios already exhibited by psychology, its status by this criterion has been getting worse, not better.

Does this mean that the great psychologists of today are less likely to be great scientists than in the discipline's early years?

Before this depressing conclusion is reached, it is first necessary to examine more indicators than just the theories-to-laws ratio.

The main problem with this measure is that it concentrates on how the discipline is represented in its introductory textbooks.

Yet, in line with S. Cole's (1983) distinction between the core and the research frontier, what goes on in these textbooks may differ appreciably from what is taking place in the original research of the field.

If there has been any improvement in psychology's status as a scientific enterprise, it may be more evident in the articles that published in the discipline's most prestigious journals.

Hence, content analyses of the research literature may reveal some amount of Comtian progress.

The content analytical studies that would help us address this question fall into two groups.

> The first performs the content analysis using the subjective evaluations of real human beings, while
>
> the second executes the content analysis using computer programs designed to evaluate text.

Subjective (human) content analyses.
Back in 1940, Jerome Bruner and Gordon Allport scrutinized psychology's progress from this standpoint of its research literature.

The study was titled "Fifty Years of Change in American Psychology," in which they specifically inspected the "entire periodical output of the 'leading' psychological journals for every tenth year beginning in 1888 and ending in 1938" (p. 757).

The specific journals were identified by asking 30 members of the American Psychological Association to rate 50 different periodicals. Bruner and Allport then selected the 14 journals that came out on top of the ratings.

These included *Psychological Review*, *American Journal of Psychology*, *Journal of Experimental Psychology*, *Journal of Comparative Psychology*, *Journal of Abnormal and Social Psychology*, *Journal of Educational Psychology*, *Journal of Applied Psychology*, and *Psychological Bulletin*, in which their article was itself published.

Altogether, they examined the contents of 1,627 articles over the half century.

The articles were all scored on 32 different categories.

Several of the categories are directly germane to the degree to which psychological research manifests the positivistic ideal.

Furthermore, without exception, the trend in the representation of these categories is always in the direction that would be expected from a psychology that was growing ever more scientific.

For instance, in line with the significance of quantification in the definition of genuine science, Bruner and Allport (1940) noted that "quantitatively, the most striking change in 50 years is the great increase in the use of statistical aids in psychological research" (p. 766).

Even though their own data analysis was not sufficiently quantitative to specify the magnitude of this trend, it is easy to calculate the appropriate statistic from their tables. The correlation between the use of statistics and the year of publication is .93, which is statistically significant at conventional levels despite the small number of periods covered ($N = 5$, $p = .018$).

In concrete terms, the percentage increased from around 2% in the late 19th century to around 44% in the 1920s and 1930s.

Other trends with significant positive correlations are the use of nonverbal methods (rather than introspection) to study the higher mental processes ($r = .99$, $p < .001$) and what Bruner and Allport called "methodological positivism" ($r = .88$, $p = .048$), a catchall category that included discussion of operational definitions, formal analytical techniques in the field, and the conceptual status of the discipline's concepts.

On the other hand, those categories that displayed negative trends in the psychological literature were those that indicate the field's movement away from what Comte might consider pre-scientific notions. An example is the use of single-case studies, such as "case histories, biographies, autobiographies, diaries, etc. that attempt to obtain and *understanding* of the total personality in its *milieu*" (p. 761, italics in original). The correlation for this category is -.83 ($p = .083$).

Speaking of Table 8.1, it would seem relevant to the issue to hand to determine how scores on these six dimensions have changed from Fechner to Estes.

It turns out that the birth year of these 54 eminent psychologists correlates .39 with the objectivistic versus subjectivistic score, -.41 with the static versus dynamic score, and .31 with the exogenist versus endogenist score (all $p < .05$; Simonton, 2000b).

In other words, these psychologists have placed increasingly more emphasis on observable behavior, motivation and emotion, and environmental determinants.

Objective (computer) content analyses.

The effort that Bruner expended on assessing 1,627 articles on 32 categories was truly prodigious. Happily, an alternative exists that is more efficient and more objective at the same time: computerized content analysis.

Colin Martindale (1990) implemented the first application of this method.

His particular focus was stylistic changes in the prose in which psychology articles are written.

He began by drawing an extensive sample of prose from the *American Journal of Psychology* from 1887 to 1987, taking 10 articles at random every fifth year.

Only genuine articles were chosen, excluding obituaries, book reviews, and other miscellaneous publications.

Martindale then took the first 20 lines from each article, which amounted to about 200 words per article, or nearly 50,000 words in total.

He then used a computer to calculate the Composite Variability Index, an objective assessment of the linguistic complexity of the writing.

This measure incorporates such indicators as mean word length, variation in phrase length, the number of word associates, variation in word frequency, and hapax legomena (percentage of words occurring only once in the text).

Overall, scores on the Composite Variability Index declined over the century covered by his data. The only exception was a slight increase in the early 20th century (when Titchener edited the journal). Martindale interpreted the downward trend as follows:

> It is reasonable to suppose that the prose has simplified as the ideas to be communicated have become more complex. The layman would find many of the earlier articles good reading – not merely because they are by writers such as William James, but mainly because the cognitive load is light and the topics are interesting. The later articles are difficult going. They are written for specialists. The topics are still interesting, but the layman can't even figure out what they are. The authors assume that you know stuff that you don't know. The style, though, is extremely simple. (p. 361)

It may be a sign of scientific progress when a discipline's practitioners publish articles that hardly anyone can comprehend – but are otherwise easy to read!

Another approach to computerized content analysis was illustrated in chapter 5, where I described a study of 69 eminent American psychologists (Simonton, 1992b).

Instead of sampling the main text, whole titles were used.

The titles were not confined to articles appearing in a specific journal, nor even just journal articles, but all major publications, as listed given in the bibliographic entries of R. I. Watson's (1974) *Eminent Contributors to Psychology*.

Using Martindale's (1975, 1990) Regressive Imagery Dictionary (RID), the titles were scored for the presence of primary and secondary process imagery.

At that time it was recorded that primary process content in a psychologist's titles was negatively correlated with his or her long-term impact on the field, as assessed by contemporary citations.

But what has not been reported yet is how primary and secondary process changed over the years represented by these 69 psychologists.

It may come as a surprise, but the presence of primary process exhibited no secular trend.

Even so, secondary process imagery increased prominently over time, with a correlation of .30 ($p < .05$) between the score and the psychologist's birth year.

Because secondary process thinking stresses ideas that are objective, logical, realistic, and articulate, this outcome fits nicely with the assumption that psychology has progressively moved up the Comtian scale of science.

Hence, the results of objective (computer) content analyses appear to corroborate the inferences drawn from subjective (human) content analyses. Why the theories-to-laws ratio indicated contrary conclusions may tell us more about publishing trends in psychology textbooks than about what is happening at the research frontier of the field.

KUHNIAN TRANSFORMATIONS

Fascinating though the preceding results may be, the whole question of a discipline's scientific advancement may suffer from a fatal flaw.

According to Comte's internalist theory, the history of science should always move forward.

Over time, each science moves ever closer to the positivistic ideal.

Yet not all internalist theories of scientific change share this belief in inevitable progress.

The most outstanding example is the theory of advocated by Thomas Kuhn (1970) in his seminal *The Structure of Scientific Revolutions*.

The core concept in Kuhn's theory is the *paradigm*. This he takes to mean

> some accepted examples of actual scientific practice – examples which include law, theory, application, and instrumentation together – provide models from which spring particular coherent traditions of scientific research. These are the traditions which the historian describes under such rubrics as "Ptolemaic astronomy" (or "Copernican"), "Aristotelian dynamics" (or "Newtonian"), "corpuscular optics" (or "wave optics"), and so on. The study of paradigms, including many that are far more specialized than those named illustratively above, is what mainly prepares the student for membership in the particular scientific community with which he will later practice. … Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular research tradition. (pp. 10-11)

When a discipline has a well-established paradigm, it can practice "normal science," in which its members engage in "puzzle-solving research."

Because all members share the same paradigm, the discipline is not divided into schools.

This contrasts greatly with those sciences that still remain in their preparadigmatic phase.

Such disciplines will usually feature two or more contending schools, owing to lack of any consensus on the preferred theories, methods, and problems.

Furthermore, the best that such sciences can accomplish is the accumulation of more or less random facts – facts that lack theoretical context or even agreed-upon significance.

Thus far, Kuhn's theory appears hierarchical, sciences falling into preparadigmatic and paradigmatic categories, the latter representing the true sciences.

Yet what prevents Kuhn's scheme from being progressive like Comte's is the conception of *anomalies*.

An anomaly is a problem that is deemed important and yet which somehow cannot be readily solved within the given paradigm.

Occasionally, a solution arrives so that the anomaly never poses a strong threat to the paradigm.

The resolution of the anomaly became one of the paradigm's triumphs.

In contrast, other anomalies are never successfully solved within the paradigm.

If such an anomaly cannot be dismissed as unimportant, and if joined by additional anomalies the discipline enters a state of *crisis*.

The consensus breaks down, the paradigm's constraints are relaxed, contending theories appear, and the community of practitioners experiences a growing malaise.

Hence, to a certain degree, the science retreats to its preparadigmatic period, a retrogression that has no Comtian counterpart.

However, with a little luck, a new paradigm may emerge that handles all the major phenomena treated by the old paradigm as well as explains the anomalous findings.

Although Kuhn's theory has proven very influential, it has also provoked considerable debate, and many have offered alternative theories of scientific change (e.g., Lakatos, 1978; Laudan, 1977). Furthermore, many have questioned whether the Kuhnian account is applicable to psychology's own history (Gholson & Barker, 1985; Peterson, 1981). This question can be broken to two parts.

It first must be asked whether psychology can be considered a paradigmatic normal science.

Next comes the issue of whether the discipline's history has undergone scientific revolutions.

Paradigms

Is it possible to establish whether psychology could be considered paradigmatic in a Kuhnian sense?

In one respect, this question has already been addressed when we examined psychology's status in the Comtian hierarchy of the sciences.

Most of the criteria given in Table 13.1 concern the magnitude of consensus shown by the four disciplines.

Such consensus can be adopted as an indicator of the degree to which all practitioners within the field subscribe to a unifying paradigm.

By this standard, psychology would have to be considered less paradigmatic than physics or chemistry, but more paradigmatic than sociology.

Table 13.1

Four Sciences Rated on Six Criteria and the Composite Rating on the Comtian Hierarchy of Sciences

	Criterion						
Science	**1**	**2**	**3**	**4**	**5**	**6**	**Rating**
Physics	**0.79**	**0.73**	**0.33**	**1.29**	**1.45**	**0.90**	**0.92**
Chemistry	**0.76**	**0.80**	**0.91**	**0.26**	**-0.48**	**0.13**	**0.40**
Psychology	**-0.24**	**-0.18**	**0.18**	**-0.60**	**-0.78**	**0.39**	**-0.21**
Sociology	**-1.31**	**-1.34**	**-1.42**	**-0.95**	**-0.19**	**-1.42**	**-1.11**

***Note*. All criterion measures were standardized to *z* scores from the statistics published in several distinct sources:**
1 = theories-to-laws ratio (from Roeckelein, 1997; based 23 textbooks for physics, 20 for chemistry, 136 for psychology, and 22 for sociology);
2 = consultation measure based on Festinger's social comparison theory (Suls & Fletcher, 1983, Table 1; viz. the number of persons acknowledged adjusted for the number of authors);
3 = the "proportion of scientists under 35 whose work received more than the mean number of citations for their field" (Cole, 1983, p. 118; i.e., those fields which incorporate most quickly the work of young scientists are assumed to rank higher in the hierarchy);
4 = the "consensus on evaluating scientists by field" (Cole, 1983, p. 120), where 60 scientists per field were rated by colleagues in the same discipline (the consensus was gauged by the mean standard deviation of the ratings);
5 = the consensus gauged by asking scientists to mention those who "have contributed the most in past two decades" (S. Cole, 1983, p. 120; the specific index is the percentage of "mentions received by 5 most mentioned names");
6 = the "concentration of citations to research articles" (Cole, 1983, p. 122; using the Gini coefficient).

It must be admitted, however, that this criterion in only indirect.

Furthermore, even if consensus is accepted as a rough indicator, it can be argued that it makes no sense to speak of whether an entire science is paradigmatic or not.

Some subdisciplines may be guided by strong paradigms, whereas others may remain preparadigmatic.

This intradisciplinary variation is apparent in Table 5 of S. Cole's (1983) study, which was used for criterion 6 of Table 13.1.

The figures given in that column are actually averages, based on the Gini coefficients that were calculated for the leading journals of the corresponding discipline.

Naturally, there was considerable dispersion around each mean.

In the case of psychology, the coefficients ranged from 0.05 to .29, a spread that overlaps considerably with that of chemistry (0.06-0.27) and physics (0.06-0.35).

Indeed, "the psychology journal with the highest Gini coefficient, *Journal of the Experimental Analysis of Behavior*, had a higher coefficient than any journal in chemistry, geology, or mathematics" (p. 121).

It is perhaps a bit strange that the psychology journal with the highest Gini coefficient was represented the Skinnerian paradigm.

At the time that this coefficient was calculated, in the late 1970s, radical behaviorism was already facing a major challenge by a newfangled psychology, a psychology that billed itself as the science of the mind, not just behavior.

Does all this imply that the psychology had witnessed a bona fide Kuhnian paradigm shift?

Revolutions

The history of psychology often appears to contain examples of scientific change that have the superficial appearance of scientific revolutions.

At the same time, historians often put forward generalizations hinting that psychology's historical development may pursue patterns that depart from those put forward by Kuhn's (1970) theory.

That scientific change may operate differently in psychology than in the paradigmatic sciences is also suggested by an impressive and detailed philosophical analysis of authentic conceptual revolutions (Thagard, 1992).

The author began by scrutinizing the logic of the major scientific revolutions led by Copernicus, Newton, Lavoisier, Darwin, Einstein, Wegener, and others.

Frequently this analysis was conducted via a computational model – the program ECHO – that was designed to gauge any theory's "explanatory coherence."

The treatment of these secure cases thus complete, the investigator allotted a whole chapter to the question "Revolutions in Psychology?"

His informed conclusion, based on the case studies and the computer model, was forthright:

> While psychology has seen much conceptual change in this century, with the replacement of the introspectionist and commonsense conceptual system by behaviorism, and the sublation of behaviorism by cognitivism, it has not had revolutions of the sort so important in the natural sciences. Behaviorism and cognitivism involved abundant conceptual change, including concept deletions and conceptual reorganization involving kind-relations. But they are best characterized as approaches rather than theories, and their ascent depended more on estimates of future explanatory coherence than on evaluation of the explanatory coherence of specific theories. (Thagard, 1992, p. 245)

The affirmation that there took place no so-called "cognitive revolution" has been seconded by others as well.

For instance, it has been argued that "the move from behaviorism to cognitivism is best represented in terms of replacement of (operationally defined) 'intervening variables' by genuine 'hypothetical constructs' possessing cognitive 'surplus meaning" and that this replacement actually "continued a cognitive tradition that can be traced back to the 1920s" (Greenwood, 1999, p. 1). There was no "Kuhnian paradigm shift" whatsoever.

These assertions are based on conceptual analyses rather than empirical inquiries.

Hence, it would be valuable to ask whether the facts support these inferences.

Two distinct approaches have emerged to address this question empirically:

citation and

content analyses.

Citation analysis.
Kuhn (1970) himself suggested an empirical approach to testing whether revolutions indeed took place within a given scientific domain.
When a scientific revolution takes place, and the new paradigm displaces the old, Kuhn conjectured that this must leave an impact on what is published in the field.
"One such effect – a shift in the distribution of the technical literature cited in the footnotes to research reports – ought to be studied as a possible index to the occurrence of revolutions" (p. ix).
It took 20 years before Kuhn's suggestion was specifically applied to the history of psychology.
The application took the form of two successive articles that appeared in the *American Psychologist*.

- The first article tested the "Kuhnian displacement thesis" by gauging the citations received by the leading journals in three rival schools of psychological thought (Friman, Allen, Kerwin, & Larzelere, 1993). Psychoanalysis was represented by the *Psychoanalytic Quarterly*, *Journal of American Psychoanalytic Association*, *Contemporary Psychoanalysis*, and *International Journal of Psychoanalysis*; behaviorism by *Behavior Research and Therapy*, *Journal of Experimental Analysis of Behavior*, *Behavior Therapy*, and *Journal of Applied Behavior Analysis*; and cognitivism by *Cognitive Psychology*, *Cognition*, *Journal of Experimental Psychology: Learning, Memory, and Cognition*, and *Memory & Cognition*. The *Social Science Journal Citation Record* was then used to determine the impact these representative journals were having on the research literature between 1979 and 1988. Despite the definition of multiple indicators (citation number, impact factor, immediacy index, etc.), the same general conclusions obtained. Although there usually appeared an increase in citations to articles published in the cognitive psychology journals, and although citations to psychoanalytic journals were often lower than those to the other two, neither behaviorism nor psychoanalysis exhibited any sign of undergoing Kuhnian displacement. At least over the period studied, the citation trends for the latter two schools were fairly flat. The so-called "cognitive revolution" entailed merely the increase in research on human cognition, without any corresponding decline in the scholarly activity displayed by the older schools. Scientific change in psychology consists in the accumulation of additional psychologies rather than paradigm shifts!
- A better alternative might be to look at the relative representation of the three schools in the core journals of psychology. To be concrete, how has the representation of psychoanalysis, behaviorism, and cognitive psychology changed in the articles appearing in *American Psychologist*, *Annual Review of Psychology*, *Psychological Bulletin*, and *Psychological Review*? The critics backed up their remarks by publishing a study that carried out their recommended procedure, with additional improvements (Robins, Gosling, & Craik, 1999; also see Robins, Gosling, & Craik, 1998). For good measure, the authors added a fourth brand of psychology, the neurosciences. Changes in the differential impact of these four psychologies were assessed three distinct ways: (a) the percentage of articles appearing in flagship publications that contain keywords relevant to the psychoanalytic, behavioral, cognitive, and neuroscientific schools; (b) the percentage of dissertations that contain the specified set of keywords for each school; and (c) the total number of annual citations by the four flagship publications to the articles that appeared in the core journals of psychoanalytic, behavioral, cognitive, and neuroscientific psychologies. The time span of the analysis varied according to the specific criterion, but the annual time series could begin as early as 1950 and end as late as 1998. Whatever the details, the results were fairly consistent across the alternative indicators. Psychoanalysis over the interval has been mostly ignored in mainstream psychology, and the neurosciences have made only the smallest impression, despite the substantial increase in neuroscientific research (and its growing citation in general scientific publications like *Science*). More significant, cognitive psychology began an ascent in the early 1960s while behaviorism began a descent in the late 1960s and early 1970s.

Although this follow-up investigation has drawn its share of criticisms (e.g., Martens, 2000), I think its main empirical conclusion survives unscathed: the behaviorist school has yielded ground to cognitive psychology in mainstream psychological science (Robins, Gosling, & Craik, 2000). This trend is certainly consistent with Kuhn's (1970) displacement thesis.

Content analysis.

Already in this chapter I have discussed Martindale's (1990) application of computerized content analysis to the text published in the *American Journal of Psychology*.

The usefulness of applying Martindale's RID has been illustrated in the content analyses of the publication titles of 69 eminent American psychologists (Simonton, 1992b).

Martindale (1990) himself has subjected psychological publications to this same assessment technique.

In the first place, the *American Journal of Psychology* text (which he assessed on the Composite Variability Index) was also gauged on primordial content.

According to his trend analysis, "primordial content increased during the behaviorist paradigm shift and declined once the paradigm was established" (p. 363).

Martindale then examined another sample of text extracted from *Psychological Review* from 1895 to 1985, using the same sampling strategy as for *AJP*.

Here he found that "primordial content fell throughout the behaviorist era and began to rise with the introduction of the cognitive paradigm" (p. 365).

Finally, to get a better idea of what was happening to behaviorism, Martindale content analyzed the *Journal of the Experimental Analysis of Behavior*, randomly sampling 10 articles from every 2 years from 1958 to 1986.

In this more specialized case, "primordial content declined during the atheoretical paradigm and began to increase with the shift to the theoretical paradigm" (p. 366).

Why was Martindale so interested in the transhistorical trends in the primordial (or primary-process) content in these journals?

The reason is that he had already demonstrated, over a series of studies that appeared since 1975, that fluctuations in primordial content were associated with stylistic changes in the arts, especially in such literary forms as poetry and fiction (e.g., Martindale, 1975, 1990).

This association is based on the role that primary process plays in creative thought, the constraints imposed by a given artistic style, and the constant drive toward increased originality that artistic creators must face.

Furthermore, Martindale wanted to show that something comparable is associated to the supposed paradigm shifts undergone by his own discipline.

In fact, as is very obvious from the quotations just given, Martindale felt free to interpret his empirical results in Kuhnian terms.

Accordingly, his content analyses might be said to reinforce the earlier evidence regarding Kuhn's displacement thesis.

Specifically, both citation and content analyses appear to indicate the real existence of a "cognitive revolution."

Yet is this inference really justified?

One objection that might be raised is that the trends in primordial content for the *American Journal of Psychology* and *Psychological Review* are not in complete agreement.

Martindale (1990) explained the discrepancy in terms of changes in the aims of the latter journal, which transformed from a vehicle for empirical research to a place to present new theory.

Even more crucial is the Martindale's demonstration that fluctuations in primordial content are associated with stylistic changes.

Might it not then seem justifiable to assert that the trends observed in *AJP* and *PR* reflect not paradigm shifts, in a Kuhnian sense, but rather merely mirror transformations in research *styles*?

Most serious poets do not write Elizabethan sonnets anymore, and most mainstream psychologists have lost interest in describing the finest details about operant conditioning.

Despite the highly paradigmatic nature of the research published in the *Journal for the Experimental Analysis of Behavior*, behaviorists did not seem to conjure up those anomalies that, according to Kuhnian theory, would only find resolution with the coming of cognitive science.

Cognitive psychologists did not incorporate behaviorism into a more comprehensive paradigm, but simply turned to topics that have fascinated psychologists ever since the days of Wundt.

HEGELIAN DIALECTICS

The putative cognitive revolution may have another interpretation besides it being a shift in either paradigm or fashion.

Instead, the advent of cognitive psychology may represent a pendulum swing, as is often said to occur in the history of ideas.

At the close of chapter 8 we saw how the long-term impact of 54 great psychologists was contingent on their having advocated extremist positions on the theoretical and methodological issues that divide the discipline (Simonton, 2000b).

Certainly such advocacy must have provoked considerable criticism in their own day – criticism that may be healthy for the discipline but unwelcome by the recipient.

In fact, this historical sequence of introspectionism → behaviorism → cognitivism has very much the appearance of the Hegel's dialectic process of thesis → antithesis → synthesis.

The thesis that psychologists can study the mind introduces certain contradictions which motivates the emergence of the antithesis, the notion that psychologists cannot do so and still be a science.

The synthesis, cognitive psychology, brings the discipline back around to a mental science, but not without incorporating certain features introduced by the behaviorists.

The pendulum has not swung back completely to its original position.

Whether cognitive psychology will generate its internal conflicts that will stimulate a behavioristic revival remains to be seen.

Superficially, the hypothesized Hegelian movement seems quite similar to what was envisioned in Kuhn's (1970) theory.

Nevertheless, despite the commonalities between the Hegelian and Kuhnian schemes, they are far from equivalent.

The more critical contrast is that Kuhn's concept of paradigm is both more complex and more rigorous than what is required for the Hegelian dialectic.

That is, a paradigmatic "thesis" contains a logically interconnected collection of theory, method, and substantive issues.

Although the Hegelian dialectic thus seems to provide a handy tool for the interpretation of historical change, it also appears to contradict what was said earlier about Kroeberian configurations.

The clustering of genius in contiguous generations was then interpreted in terms of a social influence process involving some combination of imitation, admiration, and emulation.

Yet if the notables in generation g are using the notables in generation g - 1 as role models, that would seem to imply some degree of continuity in their ideas.

Or speaking more generally, why doesn't each generation hold views diametrically opposed to its predecessors?

This question can be given an empirical answer.

The answer is based on a secondary analysis of extensive data published by the sociologist Pitirim A. Sorokin (1937-1941).

Because these data are so crucial to the current issue, and because these data will be used extensively in the following two chapters as well, I must pause a little to describe what the data measure and how they were collected.

Sorokin's Generational Assessments of European Intellectual History

About the same time that Kroeber (1944) was working on his *Configurations of Culture Growth*, Sorokin (1937-1941) was writing his magnum opus, the four-volume *Social and Cultural Dynamics*.

The main purpose of this work was to develop a theory of sociocultural change that shall receive due attention in chapter 14.

Yet being as much an empiricist as a theorist, Sorokin devoted a considerable amount of this work to the collection of data that he thought would demonstrate his thesis (Ford, Richard, & Talbutt, 1996).

The empirical documentation that is of most interest here is that found in Volume 2, which has the subtitle "Fluctuations in Systems of Truth, Ethics, and Law."

For this volume Sorokin and his research collaborators attempted to gauge the transhistorical changes in various philosophical beliefs from the ancient Greeks to the first two decades of the 20th century.

Data collection began by compiling a list of over 2,000 thinkers where were active between 580 BC and AD 1920 – basically from Thales to Husserl.

The next step was to rate all of these thinkers on a 1-12 scale that gauged the magnitude of their influence in Western civilization.

Table 13.2 shows the scores received by some of the thinkers in this sample that also have some prominence in the history of psychology.

Table 13.2

Comparative Influence of Representative Western Thinkers According to Sorokin (1937-1941)

Score	Name
12	Plato, Aristotle, Plotinus, Thomas Aquinas, Kant
10	Augustine
9	Socrates, Leibniz, Newton, Nietzsche
8	Pythagoras, Protagoras, Democritus, Epicurus, Lucretius, Origen, Erigena, Albertus Magnus, Duns Scotus, William of Occam, Copernicus, G. Bruno, Galileo, Kepler, Descartes, Hobbes, Spinoza, Locke, Berkeley, Hume, Rousseau, Fichte, Schelling, Hegel, Goethe, Gauss, Schopenhauer, Comte, J. S. Mill, Spencer, Hartmann, C. Darwin, Marx, Maxwell, Bergson
7	Heraclitus, Parmenides, Theophrastus, Galen, Anselm, F. Bacon, Pascal, Gassendi, Malebranche, Wolff, Vico, Voltaire, Herbart, Fechner, Renouvier, Bain, Wundt, Ribot, W. James, Lipps
6	Empedocles, Aristippus, Pyrrho, Epictetus, Marcus Aurelius, Boethius, R. Bacon, Petrarch, T. More, Machiavelli, Vives, Montaigne, Bayle, Buffon, Cuvier, Diderot, Lessing, Condillac, Herder, Condorcet, J. Bentham, Malthus, J. Mill, Purkinje, Helmholtz, Galton, Haeckel, Jevons, Mach, Avenarius, Pavlov
5	Anaximander, Xenophaes, Zeno of Elea, Raymond Lully, Erasmus, Mersenne, La Mettrie, Carlyle, Lotze, Quételet, Bernard, DuBois Reymond, J. Royce, Baldwin
4	Alcuin, Abélard, Grosseteste, Pico della Mirandola, Paracelsus, Harvey, Gilbert, Hartley, Reid, B. Franklin, E. Darwin, Pestalozzi, D. Stewart, Cabanis, Coleridge, Boole, F. Brentano, Delboeuf, Lombroso, Ladd, Romanes, Binet, Pearson, Durkheim, Kirkegaard, Münsterberg
3	Ptolemy, Hypatia, Rosellinus, T. Brown, Dilthey
2	Anaximenes, Leucippus, Alcmaeon, Buridan, Leonardo da Vinci
1	Arete

***Note*. The ratings come from several distinct appendices in Sorokin (1937-1941), taking only those names considered important in the history of psychology. Sorokin did not publish the ratings for those in his sample who were still living at the time the study began. This group included such luminaries as Stumpf, Dewey, Janet, S. Freud, Husserl, Külpe, Stern, and Jung.**

As Sorokin admitted, different scholars might quibble with the placement of this or that figure.
Even so, four considerations should ameliorate any complaints.

First, Sorokin was able to recruit raters who were professional philosophers of considerable standing in their own right, making the ratings extremely well informed.

Second, his raters implemented highly objective criteria, such as the number of monographs written about each thinker.

Third, the ratings concern the impact on the Western philosophical tradition, and for this reason certain great scientists or writers may appear misplaced (e.g., Newton and Shakespeare).

Fourth and foremost, Sorokin's ratings correlated very highly with alternative assessments of these same thinkers (Simonton, 1976f).

In chapter 3 I introduced the concept of Galton's *G*, the latent variable that underlies the reputation of historical figures. Sorokin's assessment of influence boasts a factor loading of .73 on a 10-indicator measure of Galton's *G* for these 2,012 thinkers, a loading exceeded by only two other measures (Simonton, 1991c).

Sorokin's purpose behind calculating these scores was not to assess individual differences anyway.
Instead, these scores were merely used to create weighted aggregated measures of the representation of various philosophical positions throughout the history of Western thought.
Hence, when Sorokin tabulated his large sample of eminent thinkers into consecutive 20-year periods (according to when each thinker was active), he counted each individual in proportion to their influence score.
He thus obtained generational time series that registered the fluctuations in all the issues and beliefs that have dominated Western intellectual history since its inception.
But what were these philosophical ideas in the first place?
There were many, each designed to address some particular philosophical question.

There were seven issues altogether, each with two or more potential responses.

The seven issues and their possible answers are as follows:

1. *Where does knowledge come from?* The possible answers are: (a) *empiricism* (knowledge through the sense organs; e.g., the Epicureans and the British Empiricists); (b) *rationalism* (knowledge via logic and reason; e.g., Plato and the Neo-Platonists); (c) *mysticism* (knowledge through revelation, intuition, or divine inspiration; e.g., Emerson and Bergson); (d) *skepticism* (knowledge unattainable; e.g., Protagoras and Pyrrho of Elis); (e) *fideism* (knowledge only through a "will to believe" or some "as if" type faith; e.g., the Stoics, Pascal, and William James); and (f) *criticism* (knowledge transcendental, according to Kant and the Kantians).
2. *Is the world fundamentally material or spiritual?* The main answers to this classic ontological question are the following: (a) *mechanistic materialism* (soulless or lifeless matter the sole basis; e.g., the Greek atomists and Epicureans, Hobbes, Pavlov, and J. B. Watson); (b) *hylozoism* (matter the sole basis, but it has some lifelike properties as derivatives; e.g., Thales, William of Occam, La Mettrie, and Diderot); (c) *monistic idealism* (unified spirit or mind the sole basis; e.g., Parmenides, Spinoza, Hegel, and Goethe); and (d) *pluralistic idealism* (multiple spiritual or mental entities the basis; e.g., Pythagoras, Plutarch, Hypatia, Leibniz, and Fichte).
3. *Is reality eternal or is it in constant flux?* This question concentrates on another facet of the world, namely whether change is real or only apparent. The two extreme positions on this issue are: (a) *eternalism* (reality founded in immutable Being; e.g., Parmenides, Pascal, and Schopenhauer); and (b) *temporalism* (reality founded in ever-changing Becoming or progress and evolution; e.g., Heraclitus, Hume, and J. S. Mill).
4. *Where do abstract ideas come from?* What is the relation between the universal and particular, between abstractions (e.g., "dog") and concrete instances (e.g., "Captain, my Australian shepherd"). The three solutions to this problem: (a) *nominalism* (universals only names given by language as labels for particulars; e.g., Protagoras, the Epicureans, Roger Bacon, Machiavelli, and Nietzsche); (b) *realism* (universals actually real, of which particulars are mere facsimiles or appearances; e.g., Pythagoras, Plato, Plotinus, Augustine, Thomas Aquinas, and Swedenborg); and (c) *conceptualism* (universals only mental constructs derived from particulars; e.g., Empedocles, Seneca, Kant, and Renouvier).
5. *Does the individual or society have primacy?* This a question that has considerable disciplinary importance, for it pits "psychological reductionism" against "sociological reductionism." Anyway, the two main answers are: (a) *singularism* (only the individual person exists and acts; e.g., Epicurus, Lucretius, La Mettrie, Rousseau, and Nietzsche); and (b) *universalism* (society takes primacy over the individual, as in statism and collectivism; e.g., Plato, Albertus Magnus, Raymond Lully, Vico, and Hegel).
6. *Is everything determined or do human beings exercise free will?* This question has two main responses: (a) *determinism* (everything caused, whether by fate or by cause-effect sequences governed by natural laws; e.g., Democritus, Marcus Aurelius, Hobbes, Spinoza, Hartley, and Marx); and (b) *indeterminism* (at least some free will or volition exists, at least in human beings; e.g., Aristotle, Augustine, Alcuin, Abélard, Erasmus, and Kant).
7. *What are the foundations of morality?* There are three major ethical systems according to Sorokin (1937-1941): (a) the *ethics of happiness* (hedonism, eudaemonism, utilitarianism, or any other morality that uses pleasure as the criterion of good; e.g., Democritus and the Epicureans, Machiavelli, Locke, Spencer, and Freud); (b) the *ethics of principles* (moral criteria based on abstract and universal principles; e.g., Pythagoras, Plato, Aristotle, Wolff, Kant, and Schelling); and (c) the *ethics of love* (moral criteria founded on altruism and charity; e.g., Origen, Erigena, Rousseau, Herder, and Comte).

In sum, Sorokin (1937-1941) has compiled two or more generational time series for each of seven issues, each time series recording a weighted index of the representation of a particular answer to a given issue.

Time-Series Analyses of Sorokin's Generational Measures

Not only do philosophical geniuses cluster together in contiguous generations, as Kroeber (1944) and others have shown (Simonton, 1988b, 1997d), but also the representatives of particular philosophical positions tend to congregate in adjacent generations as well (Klingemann, Mohler, & Weber, 1982; Simonton, 1976g, 1978b).

Thus, the coming and going of various intellectual movements take so many generations to be realized that the history of ideas tends to be described by quasi-cyclic trends (Simonton, 1978b).

One foundation for this transhistorical continuity is that the weighted count of thinkers advocating a particular position in generation *g* is a function of the weighted count of thinkers hold the same positions in generation *g* - 1.

This autoregressive dependency is apparent in the sizeable autocorrelations for all generational time series (Simonton, 1976g).

For example, nominalism, realism, and conceptualism have coefficients of .81, .75, and .45, respectively.

The latter is the smallest cross-generational autocorrelation of any philosophical position, while the largest is the .89 for empiricism.

From the standpoint of a scientific history of psychology, the comparatively low autocorrelation for conceptualism must be considered provocative.

Because conceptualists believe that abstract ideas are constructions of the human mind, it is essentially a psychological doctrine, in contrast to linguistic nominalism and idealistic realism.

Yet the data provide a basis for comprehending the comparatively low transhistorical stability of this position (Simonton, 1978b).

Conceptualism is a stance that is strongly influenced by a large number of other philosophical positions.

Specifically, the representation of conceptualism in generation *g* is a positive function of the representation of the following positions in generation *g* - 1: empiricism, skepticism, criticism, materialism, temporalism, singularism, and the ethics of happiness.

As valuable as the foregoing results may be, something appears to be missing.

The original purpose behind entering this topic was to discover if their occurred any Hegelian pendulum shifts in the history of ideas.

Is there any evidence that some thesis can induce the emergence of an antithesis?

Generational time-series analysis of Sorokin's (1927-1941) data have come up with one interesting example that comes close to fullfilling this requirement.

Certain intellectual movements appear to have had a polarizing effect on the history of Western thought.

In particular, suppose that generation *g* - 1 has an a burst of philosophical thought advocating empiricism, materialism, temporalism, nominalism, singularism, determinism, and the ethics of happiness?

Then in generation *g* two opposing repercussions occur simultaneously (Simonton, 1978b).

First, there appears a florescence of thinkers advocating either skepticism or criticism.

The human capacity to know directly the real world is thus denied.

Second, there emerges a contemporary surge in thinkers advancing fideism. Rather than give up on knowing anything, the fideist relies on faith alone, which cannot be undermined by arguments about the unreliability of the senses or the fallibility of reason.

William James e.g.

MERTONIAN MULTIPLES

- Several researchers have attempted to compile extensive lists of such phenomena.
  - The first published list contained 148 cases (Ogburn & Thomas, 1922), but later this count was extended to 264 (Merton, 1961b) and later still to 579 (Simonton, 1979).
  - Table 13.2 provides some examples that have some relevance to the history of psychology, either because they concern significant contributions to the discipline or because they involved notable figures in the field.
  - Although the number of reported instances is not large in psychology proper, the phenomenon cannot be considered totally exceptional either.
  - Certainly the occurrence is frequent enough to demand some explanation.
  - Below I present the traditional interpretation of these historical events, after which I will present some objections to that interpretation.
  - I conclude with a discussion of an alternative explanation that does a far better job of handling the empirical details of the multiples phenomenon.

The Traditional Interpretation: Deterministic Zeitgeist

- When Kroeber's (1944) *Configurations of Culture Growth* was discussed earlier in this chapter, I noted that Kroeber's intent was to disprove Galton's (1869) genetic theory of genius.
- However, the 1944 book was not the first time Kroeber had argued against Galton's position.
    - The first attack came in an article entitled "The Superorganic" that Kroeber had published in a 1917 issue of *American Anthropologist*.
    - A critical part of Kroeber's argument in this essay depends on the phenomenon just described.
    - Besides listing over two dozen cases of independent discoveries or inventions, Kroeber makes much out of the near simultaneity of so many of them.
    - Mendelian genetics "was discovered in 1900 because it could have been discovered only then, and because it infallibly must have been discovered then," claimed Kroeber (1917, p. 199).
    - This event was not only inevitable, but inevitable at a narrowly demarcated moment in the history of science.
- Kroeber's position was developed by subsequent anthropologists and sociologists (e.g., Lamb & Easton, 1984; L. White, 1949).
- Of special significance are the ideas of Robert K. Merton (1961a, 1961b), the founder of the Mertonian school of the sociology of science.
    - In Merton's own words, "discoveries and inventions become virtually inevitable (1) as prerequisite kinds of knowledge accumulate in man's cultural store; (2) as the attention of a sufficient number of investigators is focused on a problem – by emerging social needs, or by developments internal to the particular science, or by both" (Merton, 1961a, p. 306).
    - Moreover, he deemed the phenomenon of sufficient importance to provide it with a name: *multiples*, to be distinguished from "singletons" that were the products of a single mind (Merton, 1961b).
- Merton then went on to argue that multiples are actually more typical than singletons in the history of science.
    - To make this case, Merton (1961b) gathered a collection of 264 multiples to study their specific properties.
    - On the basis of this study and other observations, Merton concluded that the singletons, not the multiples, constitute "the residual cases, requiring special examination," this because "all scientific discoveries are in principle multiples, including those that on the surface appear to be singletons" (p. 477).
    - Almost as conclusive proof of this claim, Merton (1961b) observed that the discovery of multiples was itself multiply discovered.

E.G. Boring etc.

Objections to Sociocultural Determinism

Although Kroeber, Merton, and Boring were all convinced that multiples undermine the individual as an agent of scientific advance, that "universalist" inference may go well beyond both logic and data.

It is very possible that despite the prima facie plausibility of the Zeitgeist explanation, some other process is actually better able to explicate all the complexities of the phenomenon.

In fact, there are many notables in the history of psychology who failed to draw the same conclusions from the occurrence.

Two prominent examples are Charles Darwin and his cousin Francis Galton.

I wish to go one step further and argue that the evidence on behalf of the Zeitgeist theory is far more tenuous than most scholars realize, whether they be anthropologists, sociologists, historians, or psychologists.

Specifically, sociocultural determinism fails to deal adequately with the following four issues:

- Generic versus specific categories,
- independent versus antecedent events,
- simultaneous discoveries versus rediscoveries, and
- necessary versus necessary and sufficient causes.

Universal or particular?
Although the number of multiple discoveries looks rather impressive, the specific cases do not always bear up under scrutiny.
Rather, the lists of putative multiples include many clear illustrations of "a failure to distinguish between the genus and the individual" (Schmookler 1966, p. 191).

Independent or antecedent?
The long list of multiples suffer from another liability, namely that the separate contributions fail to satisfy the criterion that that the products be independent.
Far too often those cited as independent contributors were actually influenced by one or more of the other parties to the duplicate.
Making this objection all the more potent, one scientist may influence another without there being any awareness of an intellectual debt.

Simultaneous or rediscovered?
Unlike independence, simultaneity is not an essential requirement for two or more products to be categorized as a multiple.
Even so, a clue concerning the nature of the phenomenon may be found in the fact that supposed multiples are seldom simultaneous in any strict sense.
In Merton's (1961b) study of 264 multiples, only 20% took place even within a one-year interval.
In contrast, fully 34% of the multiples required at least a decade to elapse before the duplications ceased.
This frequent temporal hiatus raises two doubts, one empirical and the other theoretical.

- On the empirical side, the longer the delay separating two or more hypothesized duplicate discoveries, the more hazardous is the supposition that they satisfy the essential criterion of independence.
  On the theoretical side, the very existence of these rediscoveries – even when truly independent – must call into question the explanatory power of sociocultural determinism.

The rejection is most likely to take place when a discovery's "implications cannot be connected by a series of simple logical steps to canonical, or generally accepted, knowledge" (Stent, 1972, p. 84). The idea is then *premature*, reducing the unfortunate anticipator to the status of a precursor genius.

Inevitable or eventual?
Sociocultural determinism does not adequately distinguish between necessary causes and causes that are necessary and sufficient.
The occurrence of long-delayed rediscoveries implies that we must take care to distinguish between necessary and sufficient determinants of a creative product.
A necessary cause is one that supplies a prerequisite for another event to happen.
Admittedly, those who believe in the inexorable advance of science might still argue that all discoveries will eventually appear once the requisite groundwork has been laid.
Yet to say that something will eventually see the light of day is a far cry from claiming the inevitability of its birth at a precise point in time.
Furthermore, even when we can hold that a specific discovery will happen eventually, that does not necessitate that the events will unfold in a predetermined pattern.

The Modern Interpretation: Stochastic Genius

I have just shown that the empirical evidence on behalf of the Zeitgeist or sociocultural determinist explanation suffers from an excessive use of generic categories to define multiples and from a tendency to overlook the essential criterion of true independence.

I have also indicated how the traditional theory has problems handling the phenomenon of rediscovery, and fails to distinguish between necessary and necessary and sufficient causes – between eventuality and inevitability.

If this theory were the only one available, then it still might be necessary to retain it, at least as a working hypothesis.

Yet I have shown in a series of empirical analyses and logical arguments that the multiples phenomenon can be explicated without recourse to sociocultural determinism (Simonton, 1987b, 1999b).

The Zeitgeist still plays a role, but a much more limited one.

Just as importantly, the alternative explanation is more firmly grounded in what psychologists have learned about the creative process, creative productivity, and the creative product, as reviewed in chapters 3-6. This explanation goes as follows.

- During the developmental period, the future scientist acquires a large inventory of facts, concepts, techniques, themes, and questions that provide the foundation for his or her creative potential. This inventory comes largely from the sociocultural milieu, especially as represented by formal education and available role models, but it is supplemented by various experiences that are unique to each scientist.
- Once creative potential is established, and the productive period begins, the material that makes up this inventory is subjected to the creative process, as described at length in chapter 6. In line with Donald Campbell's (1960) variation-selection model of creativity, the scientist enters a process of generating various combinations of the facts, concepts, techniques, themes, and questions that constitute his or her distinctive repertoire. Yet another preemptive factor is even more critical: when some other scientist comes up with the same combination, or at least one that is recognizably similar.

Once immediate consequence of the above model should be made explicit: It can account for rediscovery multiples like Mendelian genetics.

Because each scientist's creative potential is a mix of shared and idiosyncratic ideas, the ideational combinations that scientists generate will likewise consist of a heterogeneous collection.

The larger the proportion of idiosyncratic ideas that a creative product contains, the smaller the number of colleagues who will be able to appreciate its merits (Csikszentmihaly, 1990).

Mendel's interest in breeding peas, his willingness to quantity his observations and to calculate probabilities, and his fascination with hybridization as a mechanism for Darwinian evolution was far too peculiar for his papers to receive wide attention in his day (Olby, 1979).

On the other side of the coin, this model helps us appreciate why multiples will seldom consist of exact duplicates.

The ideational combinations produced by each scientist will probably always incorporate a few components that are idiosyncratic to that scientist.

Those personal elements are evident even in the exact sciences, where one would think objectivity would filter out anything distinctive.

These interpretations are qualitative rather than quantitative. Yet the explanatory power of this alternative model comes from its quantitative predictions (Simonton, 1987b, 1999b).

The model makes specific and precise predictions with respect to three critical aspects of independent discoveries:

multiple participation,
multiple grades, and
temporal separation.

Who generates the duplicates?
In chapter 3 it was made quite evident that scientists exhibit considerable cross-sectional variation in lifetime creative output.
Corresponding individual differences exist in the number of times a particular scientist has inadvertently duplicated the efforts of some other scientist.
The stochastic-genius model can easily explicate these differences (Simonton, 1988d, 1999b).
In particular, the model leads to two predictions:

1. The greater the number of scientists working within a given domain, the higher the likelihood that that those scientists will participate in one or more multiple discoveries. If there are dozens of creators all subjecting the same subset of ideas to combinatorial variation, then the odds of arriving at a duplicate variant will be very great. In contrast, a scientist who works in isolation, and thus avoids the “hot topics” of the day, will be less prone to duplicate the variants produced by others.

2. The greater a scientist’s lifetime productivity, the higher the likelihood that he or she will participate in one or more multiple discoveries. After all, those individuals who create more ideational combinations are more likely to duplicate the combinations of others.

Empirical studies endorse both of these predictions (Hagstrom, 1974; Simonton, 1979).

For instance, one study of 1,718 mathematicians, physicists, chemists, and biologists found that those who published the most were most likely to have had their work anticipated by other researchers (Hagstrom, 1974).
Moreover, those who worked in popular research areas were also more likely to experience anticipation. These confirmatory results are strengthened all the more by the stochastic models that have been developed to explain two main aspects about the multiples themselves.

How many duplicates are there?
Some multiples have more participants than others do.
It is also apparent from the published lists of multiples that some grades may be more frequent than others.

In the broadest terms, the higher is the grade, the lower is the frequency.
That frequency tends to decline with increased grade is clear not only in Table 13.3, but also in Table 13.4, which provides the tabulations for three different collections of multiples (Merton, 1961b; Ogburn & Thomas, 1922; Simonton, 1979).
The highest grade ever claimed was grade 9, or a nonet, but this is very rare. In contrast, grade-2 multiples are the most common, followed by grade 3, then grade 4, and so on.

Table 13.3
Some Putative Instances of Multiple Discoveries and Inventions
Microscope: Johannides (1610?); Drebbel (1610?); Galileo (1610?).
Logarithms: Bürgi (1620); Napier and Briggs (1614).
Context theory of meaning: Berkeley (1709); Titchener (1909).
Animal electricity: Sultzer (1768); Cotuguo (1786); Galvani (1791).
Calculus: Newton (1671); Leibniz (1676).
Oxygen: Scheele (1774); Priestley (1774).
Color Theory: Young (1801); Helmholtz (1856-66).
Principle of least squares: Gauss (1809); Legendre (1806).
Evolution by natural selection: W. C. Wells (1813); P. Matthew (1831); C. Darwin (1844); Wallace (1858).
Purkinje effect: M. Klotz (1816); Purkinje (1825).
Unconscious motivation and repression: Schopenhauer (1819); S. Freud (1895).
Term "objective psychology": Purkinje (1827); H. Spencer (1855).
Energy conservation: J. R. von Mayer (1843); Helmholtz (1847); Joule (1847); Colding (1847); Thomson (1847).
Emmert's Law: Schopenhauer? (1815); Séguin (1854); Lubinoff (1858); Zehender (1856); Emmert (1881).
Ophthalmoscope: Anagnostakis (1854); Helmholtz (1851); C. Babbage (1847).
Genetic laws: Mendel (1865); De Vries (1900); Correns (1900); Tschermak (1900).
Spinal nerve root functions: C. Bell (1811); Magendie (1822).
Theory of emotions: W. James (1884); Lange (1885).
Positivist basis for introspection: Mach (1886); Avenarius (1888-90).
The unconsciousness mind in psychopathology: Janet (1889); S. Freud (1895).
Mutation theory: Korschinsky (1899); De Vries (1900).
Classical conditioning: Pavlov (1902?); Twitmyer (1904).
Behaviorism: Piéron (1908); J. B. Watson (1913).
***Note*. The above list concentrates on those putative multiples that are of special relevance to the history of psychology and closely allied disciplines.**

Table 13.4

Observed Multiple Grades and Predicted Poisson Values for Three Data Sets

Grade	Ogburn-Thomas (1922) Observed	Predicted	Merton (1961b) Observed	Predicted	Simonton (1979) Observed	Predicted
0	–	132	–	159	–	1,361
1	–	158	–	223	–	1,088
2	90	95	179	156	449	435
3	36	38	51	73	104	116
4	9	11	17	26	18	23
5	7	3	6	7	7	4
6	2	1	8	2	0	0
7	2	0	1	0	0	0
8	1	0	0	0	1	0
9	1	0	2	0	0	0
μ	1.2		1.4		0.8	

***Note*. The predicted frequencies derived from Equation 13.1, using the corresponding μ (Simonton, 1986g). Table adapted from "Stochastic Models of Multiple Discovery," by D. K. Simonton, 1986, *Czechoslovak Journal of Physics*, *B 36*, p. 139. Copyright by the Czechoslovak Academy of Science.**

From the combinatorial theory we can predict the specific shape of the probability distribution.
In the first place, because the creative process is more or less random, a large number of variants must be generated before a useful variant survives.

In other words, the probability of success is relatively small. There are many trials and many errors.

Concomitantly, any given discipline will consist of a fairly large number of creators independently subjecting roughly the same subset of ideas to the combinatorial process.

Thus, the low probability of success for any one individual is somewhat compensated by the large number of participants.

Essentially, this is a form of parallel processing where each creator is blindly generating ideational combinations, but where the discipline has "safety in numbers."

Because of this redundancy, the odds will be enhanced that many of the potentially useful combinations will be found by at least one member of the field.

At the same time, this same redundancy will permit a certain number of multiples to emerge, even if the creators are truly working independently of each other.

By chance alone, there will appear multiples of grade 2, 3, 4, and so forth up to the sole grade-9 multiple.

Given these conditions, the predicted probabilities of occurrence for multiple grades must be closely approximated by what is called the Poisson distribution, which is given by the formula:

$$P(i) = \mu^i e^{-\mu} / i! \quad (13.2)$$

Here $P(i)$ gives the probability of getting a multiple of grade i, e is the exponential constant (as seen in Equation 4.1), μ is the mean (and variance), and $i!$ is i factorial (i.e., $i! = 1 \times 2 \times 3 \times \ldots \times i$).

The Poisson distribution accurately describes the occurrence of events when the number of trials is extremely large but the probability of success extremely low.

This feature emerges from its derivation from the binomial distribution, with the parameter n (the number of trials) approaching infinity and p (the probability of a success) approaching zero – yielding the mean $\mu = np$.

Research has repeatedly shown that this distribution does an excellent job of predicting the observed frequencies of events when those events are so unlikely to happen that they can only happen because there are so many attempts (e.g., the number of Prussian cavalry officers killed by horse kicks in a given period of time).

The same predictive success holds for multiples as well.

This success is apparent in Table 13.4, which presents the fit for the three data sets.
The discrepancies between observed and predicted scores are so small that they can be attributed to statistical error (as demonstrated by the appropriate χ^2 tests; Simonton, 1978a, 1979).
In addition, the same close fit between observed and predicted distributions is found when the multiples are separated by discipline (Simonton, 1978a).
It must be realized that the traditional explanation for multiples cannot accommodate these findings.

What is the time separation between duplicates?

As already noted, sociocultural determinists like Kroeber (1917) placed a great deal of emphasis on the near simultaneity of so many multiples.

Yet according to the stochastic-genius model, multiples are almost compelled to be simultaneous for the multiple to happen at all.

> The dissemination of discoveries often takes time, especially if the information must go across barriers of language or discipline.
>
> Nonetheless, as time advances, the likelihood increases that knowledge of a discovery will diffuse throughout the scientific community, and thereby lower the probability of some scientist duplicating the original contribution.
>
> Those scientists who never hear of the discovery will probably belong to disciplines so remote from the discovery's domain that they would not have the ability to participate in a multiple anyway.
>
> Hence, with sufficient lapse of time, all potential claimants to a multiple will move to other scientific problems that remain unsolved.

It is easy to construct stochastic models that make allowances for this communication process (Brannigan & Wanner, 1983a; Simonton, 1986c, 1986d).

> These "contagion" models operate much like the Poisson model in that the ideational combinations are randomly generated.
>
> But one further constraint is added: The longer the amount of time that elapses after the first appearance of a published combination, the lower becomes the likelihood of a duplicate discovery.
>
> Knowledge of the innovation probabilistically but inevitably disseminates so as to preempt others from continuing further on the same project.
>
> Models based on this constraint still predict the comparative frequencies of the multiple grades, but at the same time the models accurately predict how many years will elapse before duplicates can no longer appear (Brannigan & Wanner, 1983a).
>
> Just as low-grade multiples will be more common than high-grade multiples, so will short temporal separations be more likely than long separations (Simonton, 1986c).

Because these more complex stochastic models still operate according to an underlying combinatory mechanism, this interpretation would seem to have more explanatory power than sociocultural determinism.

This is especially true given how Zeitgeist theory provides no a priori means of accommodating multiples with ample temporal separation.

Furthermore, a stochastic model with a contagion component predicts that the temporal separation of multiples should be decreasing and the average grade should be declining.

The basis for these predictions is the simple observation that the communication process in science has become ever more efficient, with the advent of scientific journals, conferences, preprints, and, most recently, the Internet.

Confirming both of these predictions is an empirical study that demonstrated "that the mean number of scientists involved in multiples has been declining, and that the time interval separating independent reports has been approaching zero" (Brannigan & Wanner, 1983b, p. 135).

Although the sociocultural milieu might claim some credit for the increased simultaneity of multiples, it is difficult to see how it would explain the decline in multiple grade.

Even worse, Zeitgeist theory cannot accommodate the highly skewed distribution of multiple grades, nor does it make any distinctive predictions about how multiples are distributed across scientists.