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Disunity in Psychology and Other Sciences: The Network or the Block Universe?

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The nineteenth-century metaphor of a block universe in which science is regarded as a structure consisting of basic building blocks resting on firm foundations is contrasted with the contemporary metaphor of science as a network of relations. The network metaphor challenges the view that one science is more foundational than others and raises questions about whether an all-pervasive unity is desirable or even possible. The unity–disunity issue in psychology and other sciences (with special reference to biology) is discussed with respect to the network and building block metaphors and with respect to three arenas: organizations, methodology, and subject content areas. It is argued that in these three arenas, psychology is no more disunified than biology. There is no basis for the development of a disciplinary inferiority complex based on the belief that the other sciences are unified while psychology remains in the intellectual backwaters of plurality.

Concern about the disunity of psychology is currently very topical and the subject of a large number of articles (e.g., see Green, 1992; Kimble, 1989, 1990; Staats, 1989, 1991, 1993; Viney, 1989; Viney, King, and King, 1992; Wand, 1993). Such concern is undoubtedly fueled partly by the belief that the more mature sciences are unified and coherent disciplines. Staats (1989), for example, has pointed out that "each science undergoes a transition from early disunification to later unification" (p. 143). He notes that this transition has not been understood and that the natural sciences have attained considerable consensus regarding theory, methodology and philosophy. The behavioral sciences, by contrast, are disunified and therefore, "relatively backward" (p. 148). The goal of this paper is to explore unity and disunity in other sciences with a special focus on biology. It will be argued that the unity

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of other sciences may be overestimated resulting in an unfortunate devaluing of the scientific and professional status of psychology.

The Problem of Defining Unity

The term unity literally refers to oneness, or to a simple, single, and consistent totality complete in itself and admitting of no possibility of real separations. If unity is truly attainable, then the separations or disjunctions that are encountered in experience are not real, but mere appearances that will someday be corrected by the discovery of a properly informed, harmonizing and regulative principle. Monistic philosophers believe the world is best understood in terms of a basic uniformity or oneness, but there has always been a problem about what the oneness is. Perhaps the only truths we really know are truths about words, and if that is the case, then unifying principles will most likely be found within a philosophy of nominalism. Materialism, idealism and various mystical philosophies have also offered their disparate visions about the nature of the one and only reality and the keys to unity. "The unity of what?" is central to informed discussions of unity, but historically it has been one of the most divisive questions in philosophy. Schneider (1992) has argued appropriately that unification in psychology must be within a naturalistic context, but even within a thoroughgoing naturalistic context there will still be legitimate debates between classic materialists, emergentists, nominalists, and phenomenologists about the nature of the unity we seek. There is more than one variety of naturalistic philosophy!

Is Theoretical Unity Possible?

In addition to the problem of what the oneness is, there is an even more difficult problem for theoretical unity that has grown out of the work of science itself. Practically, there is real order in the world, but there are also real indeterminations, novelties, surprises, and chaos. The history of twentieth century science is understood partly in terms of discoveries (e.g., see Gleick, 1987) that suggest not just a practical, but a theoretical basis for indetermination and chaos. Such concepts challenge 19th century concepts of a "block universe," a term introduced by Thomas Davidson referring, in the words of Hall (1967), to a "single closed system of interlocking parts in which there is no genuine plurality and no room for alternative possibilities" (p. 363). Even if physicists find that there is a grand unified theory that links the four forces of nature, we will still be unable to write a biography in advance or, in the words of Stephen Hawking (1993), we will still be unable to predict "that Sinead O'Connor will be the top of the hit parade this week, or that Madonna will be on the cover of Cosmopolitan" (p. 128).

Another problem for any all-pervasive theory of unity is encountered in experience itself. There are regions of experience that assimilate with each other, but others are uneasy partners and still others that seem to refuse to assimilate. The aesthetic region for example, has always been of interest to scientists, but what is the proper role of aesthetics in scientific activities? In his book *The Double Helix*, James Watson (1968) spoke of "pretty experiments" and when he and Francis Crick completed work on the DNA molecule, he referred to the structure as "too pretty not to be true" (p. 134). Philosopher Charles Hartshorne (1982) described science as "the search for the hidden beauty of the world" (p. 85). Physicist Paul Dirac argued that "a theorist should prefer beautiful equations to uglier ones that yield closer agreement with experimental data" (see Brush, 1974, p. 1167). Other theorists would undoubtedly disagree with Dirac on the grounds that the equation that makes the best prediction wins and an ugly victory is better than no victory at all. For the foreseeable future, it may be that the role of aesthetics in scientific activities will have to be arbitrary. Though human beings may understand some of the dimensions involved in judgments of beauty, we have little information about the possible connections between aesthetics and truth, or between aesthetics and method.

Neither do we understand the appropriate accommodations between epistemology and ontology. Historically, assumptions about what is real have sometimes dictated methods and vice versa and all too often with consequences that have been disastrous to tolerance and progress (e.g., Trofim Lysenko's Marxist approach to biology in the former Soviet Union, and Aryan approaches to physics in Nazi Germany). To date, there is no intellectual metalevel that specifies in advance the appropriate priorities between these two regions of experience. An all-pervasive unity would presumably clarify the relationships between epistemology and ontology and all other dimensions of experience, but at present, it is absurd to even suggest the possibility of such a clarification.

William James (1907/1975, pp. 65–74) spoke of the difficulties inherent in any formal abstract concept of unity that collects everything in the world into one coherent totality. James saw great value in debate over unity vs. disunity or monism vs. pluralism, but he argued for moderate or measured approaches that break unities down by kinds. He believed that rational analysis will be difficult enough when it is restricted to specific kinds of unity such as methodological unity or linguistic unity. He advised that discussions of grand unity are best left to the mystics. Following James's advice, the present paper will explore unity and disunity in psychology and other sciences in three specific arenas: organizations, methodology, and disciplinary subject matter.

Organizational Unity

One mark of unity and disunity is encountered in social organizations (e.g., societies, academic departments, professional and scientific journals, etc.) that sustain and promote disciplinary interests. In terms of administrative and organizational structures, psychology is as unified in the late 20th century as many of the other sciences. In the typical university, psychology is taught out of a single department, though there are exceptions, especially where graduate programs are concerned. It is interesting to compare psychology, in this regard with biology. When psychology was a fledgling discipline, there was normally only one or two biological science departments listed in university catalogs. One might encounter a single department of biology or a department of botany and a separate department of zoology. By contrast, biology, in large universities in the late 20th century is administered in a large variety of departments that are offshoots from the core discipline. Each department has its own specialized research area, curriculum, and degree programs.

The number of departments offering biological science degrees is especially large in American land-grant universities where there is an emphasis on service and application. In such universities, it is not uncommon to encounter as many as twenty or more separate departments teaching undergraduates and graduates and researching in the various branches of biology. On the plant side, there may be separate and sizable departments of agronomy, botany, forestry, and horticulture. On the animal side, there may be separate departments of anatomy, animal science, fishery and wildlife biology, physiology, and zoology. Other departments, not easily classified on the plant or animal sides but relevant to both may include: biochemistry, cellular and molecular biology, environmental health, food science and nutrition, marine biology, and microbiology. Any examination of the historical development of the organization of biology in typical university catalogs will reveal a steady proliferation of new departments and degree offerings over the past 60 to 70 years.

The organizational diversity of biology is also manifested in the large number of scientific and professional societies that serve the various sub-disciplinary branches of this science. Indeed, there is no organization in biology comparable in size or scope with the American Psychological Association. The diversity of biology in terms of academic departments and professional and scientific organizations does not, of course, imply that biology is disunified. Brožek (1990) makes a helpful distinction between diversity and disunity pointing out, appropriately, that diversity does not imply disunity. Is not biology, for all of its organizational diversity, still a unified and scientifically coherent discipline? The answer to such a question is not necessarily encouraging to those who may wish to look to biology as a model. Evidence of disunity is abundant.

A number of years ago, I served as Director of the Biology Core Curriculum at Colorado State University and in that capacity had first hand opportunity to observe some of the interesting complexities and tensions in relations among the eighteen departments on our campus that were, in one way or another, associated with biology. A core curriculum was deemed necessary because it is obviously not economical for all departments to teach certain core areas such as introduction to biology, ecology, genetics, cellular and molecular biology and developmental biology. Courses in these content areas are commonly regarded as basic to the education of all biology students regardless of their special interests. The problems of administering a core curriculum quickly revealed a host of deep divisions not at all unlike those that beset the field of psychology.

There were intense disagreements and prolonged debate about how the large two-semester introductory course should be taught. Since many of the differences could not be reconciled, a two-track system representing two fundamentally different approaches was installed to satisfy various competing factions. There were also intense differences of opinion about staffing a course in genetics. On one occasion, a member of the Biology Council suggested the name of a faculty member from the College of Forestry as a potential instructor for the genetics course. Another member of the council objected declaring "We don't want any of those 'tree people' teaching genetics; they have no concept of change!" Disagreements over how a core course in genetics should be taught were ultimately irreconcilable, so a multi-track approach to this topic was necessary. The same problem occurred in the case of ecology so a multi-track approach was installed to assure that the guardians of plant interests, animal interests, molar interests, and molecular interests would be satisfied. Biologists still do not understand that the major problems of ecology are psychological!

Like psychology, biology is torn by tensions between basic and applied interests. A faculty member from the Department of Horticulture confessed he had little understanding of what botanists actually do, but "I can tell you this," he confessed, "They don't like plants!" A botany student informed his advisor that he wished to enroll in a course in horticulture; the advisor quipped "So, you wish to enroll in flower arranging 101."

There are also persistent territorial battles in modern biological science disciplines. For example, range scientists and foresters often have very different views regarding the boundaries of a range and the boundaries of a forest. The former may view forests as somewhat incidental islands of trees that occasionally dot the more important landscape of the range while the latter are more likely to view the range as an occasional open area that falls naturally within the jurisdiction of forestry. Figure–ground quarrels between these two disciplines are not unique. There are similar territorial and boundary disputes between other disciplines such as horticulture and botany, and biophysics and biochemistry.

The tensions, philosophical differences, and territorial disputes among biological science disciplines have a long history and are reflected in the organizational development of biology in the United States. In an article on the organization of American biology, Appel (1988) calls attention to the fact that "separate societies for the biological sciences were formed very early" (p.88) and that federation efforts have failed to produce any meaningful unity or comprehensive umbrella. Further, he calls attention to the idea that the lack of organizational unity "complements the internal history of biology by revealing in an accessible manner how the study of the phenomena of living beings was divided into specialties and what the real lines of cleavage were" (Appel, 1988, p. 89). An examination of the *Encyclopedia of Associations* reveals a large number of small specialized societies dedicated to the advancement of a great variety of professional and scientific interests in the biological sciences. From an organizational standpoint, biology has not followed the transition mentioned by Staats from "early disunification to later unification."

In academic institutions, the physical sciences appear to be more tightly organized than biology. Even so, there are discrepancies from catalog to catalog regarding the organizational administration of these more mature sciences. As one of many possible examples, there can be disputes about whether chemical engineering should be housed in chemistry or in engineering. There are clear methodological and substantive differences in chemical engineering depending on where it is located. There are also intense territorial disputes over the proper locus of courses and research facilities in thermodynamics. It is often impossible to settle such disputes in an economical fashion so separate courses, each with different orientations, are offered in several different departments. Although there are unities with respect to fundamental equations, the contexts and applications seem to demand different approaches.

More mature sciences are also sharply divided with respect to the languages they employ in their subdisciplinary areas. Biologists who move from one sub-disciplinary area to another often find that they know the materials in their new area, but must learn new vocabularies. Neither are the divisions between the branches of the so-called natural sciences merely verbal. Any administration of a collegial organization of natural sciences will be beset with profound differences between disciplines in ways of thinking about what science is and how to do it. A "chalk board" discipline such as mathematics is often at odds with "wet lab" disciplines over all kinds of theoretical and practical issues. The mathematician is more likely to think deductively and to emphasize certain proofs while those in "wet lab" disciplines are often inclined to think inductively and to emphasize probabilities. On the practical side, publication rates, for example, between mathematicians and chemists may differ very widely creating enormous tensions regarding ways to measure collegial equity in matters of promotion, tenure, and salaries. A mathematician, discouraged over perceived inequities between the laboratory disciplines and mathematics, confessed he would just as soon see mathematics located in the arts or humanities!

Methodological Unity

From the beginnings of the modern period there have been continuing and heated disputes about the nature of scientific method. Many of the methodological issues that separated pioneers in the philosophy of science are still with us in the latter part of the 20th century. Methodological issues are played out in the works of philosophers of science such as Feyerabend (1975), Kitcher (1982), Kuhn (1970), Popper (1959) and among scientists in the practical daily routine of their work. Tensions among molecular biologists and field biologists are remarkably comparable to tensions between physiological psychologists and social psychologists. In all scientific disciplines, one encounters methodological disputes that have their origin in the tensions between molar and molecular methodologies. A physical chemist told me that some of the techniques of the biochemists seemed bizarre and incomprehensible to him, but then went on to confess that anything more complex than a single electron made him nervous. By contrast, biochemists may question the relevance of the more elegant studies published by physical chemists.

Psychologists employ a great variety of methodologies including controlled wet lab experiments, correlational studies, field studies, and phenomenological investigations. But is such methodological diversity any different than the methodological diversity one encounters in the more mature sciences? Indeed, the range of methodologies in the mature sciences could make psychology look reasonably unified. The naturalistic observational techniques of the astronomer, the manipulative laboratory experiments of the organic chemist, the rational–deductive techniques of the abstract algebraist or the astronomer, the non-invasive descriptive methods of the field biologist, the quasi-naturalistic modeling techniques of engineers, the statistical techniques of the meteorologist, and the quasi-historical methods of the paleontologist, to name just a few, bespeak of enormous methodological diversity and such diversity increases over time with the proliferation of new mathematical and statistical tools, new observational instruments, new tests, and expanded modeling and memorial functions provided by the computer. From an epistemic standpoint, diversity, plurality, and even some anarchy are the life blood of a science while too much unity results in stagnation or intellectual death.

The history of psychology provides numerous examples of failed systems often based on one grand but rigid and uncompromising method. Perhaps Staats is correct that there are some meaningful ways in which the growth of unity is a mark of a maturing science, but such a contention could hardly apply to the epistemological or methodological dimensions of science because there is always something new and unpredicted to be seen that is rendered visible *only* by new methods and techniques.

Recent work in the philosophy of science also suggests that methodology, understood pluralistically, is more adequate as a way of understanding the work of the scientist. In his book, *The Limits of Science*, Nobel laureate P.B. Medawar (1984) argues that there is no one scientific method and that "no procedure of discovery can be logically scripted" (p. 51). That same sentiment has been set forth vigorously by many scientists and scholars, (e.g., see Bridgman, 1955; Brush, 1974). Hildebrand (1957), a former president of the American Chemical Association, challenged the idea that there is *one* scientific method. He argued that the task of the scientist is to use ingenuity to solve problems. "If [we] do not have the key for the lock, [we] must not hesitate to pick it, to climb through a window, or even kick in a panel" (p. 26). In his book *Against Method*, Paul Feyerabend (1975) argues that "The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research — there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other" (p. 23). More recently, Dupré (1993) in his book *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*, has argued that "there are surely paths to knowledge very different from those sanctioned by the leading scientific academies" (p. 10). Suspicious, of any one grand unifying method, Dupré argues for the more modest goal of "a catalog of epistemic virtues" (p. 11).

There are many legitimate ways of thinking under the scientific umbrella. Scientists think inductively, deductively, nomothetically, idiographically, correlationally, and causally in the material, efficient, and formal senses of the term cause. Rychlak (1994) has also argued for a more pluralistic approach to causality that includes a legitimate place for a human teleology. We are also increasingly aware of the pervasive influence of conscious or unconscious moral influences in scientific thinking. Indeed, it is questionable whether the data of ecology or even psychology can be separated from moral and aesthetic considerations. There are many unities to be found within the methodologies of the sciences and the discovery of such unities is useful, but it is unlikely that there is any kind of discernible pervasive or grand methodological unity in the sciences. Psychology takes its place alongside all the rest of the sciences productively attacking a great variety of problems while employing a great range of strategies, tools, methods, and intellectual structures. There is no basis for the development of a disciplinary complex based on the belief that other sciences enjoy methodological unity while we remain in the epistemic backwaters of plurality. In fact, from a methodological standpoint, we are no more pluralistic than the biological sciences. Indeed, biologists struggle with all the same methodological issues we debate (e.g., molar vs. molecular approaches, the problem of reductionism, the field vs. the laboratory, correlation vs. causation, observer bias, etc.)

As one of the founders of modern science and as a founder of modern empiricism, Francis Bacon (1620/1960) noted that "the art of discovery may advance as discoveries advance" (p. 120). Bacon's statement was prophetic and revealed his understanding that the scientific umbrella must not be fixed or static, rather it must adapt to changes in our understanding of the world and to changes in perspective. Methodological unity is all too easily transformed into methodological purity. Purity may arguably be a virtue in some belief systems but it could be mischievous in any epistemology that aspires to be open and responsive to the real complexities of the world.

Unity of Subject Matter

There has been a marked proliferation of substantive content areas in all the sciences. For example, the advent of new topical areas such as molecular biology and ecology have forever changed the intellectual landscape of biology. The current topical content areas in astronomy including such esoterica as black holes, dark matter, quasars and pulsars bear little relationship to the content areas of astronomy as that discipline was known in the early part of this century. The history of chemistry is partly a history of remarkable changes in the periodic system of the elements and consequent development of needs for new subdisciplinary areas: e.g., organic chemistry in the 19th century and biochemistry in the 20th century. The proliferation of new topical areas, based on the discovery of new realities from the subatomic world to the macro world, has challenged the magnificent unity of the Newtonian world view with its emphasis on material and efficient causation, universal determinism, and absolute space and time. In terms of subject matter, or content areas, the sciences seem to grow in the direction of greater pluralism. Such growth does not, of course, rule out the possibility that ultimately, each new area will somehow make perfect connections with all other areas.

The history of psychology, like the history of the other sciences, is a history of the proliferation of new substantive content areas. One way to understand the early systems of psychology is to grasp the importance of attempts to define and unify the field around foundational topical subject matter areas. Some of the early systems, for example, viewed the senses as the foundational subject matter of the new discipline. The senses were thought to be the windows to the mind, and the focus of research and theory was on structures, capacities, limits, and functions of sensory systems. With the advent of American functionalism, the emphasis was on adaptation and habit, but with radical behaviorism there was a new emphasis on movement as the foundational concept in psychology. Indeed, Albert P. Weiss (1924) argued that "the term existence is only a synonym for movement" (p. 40). But psychologists, like other scientists, have discovered that there are many real things in their world and that no one thing is sufficiently robust to be foundational to all the rest.

The belief that the sciences rest on firm foundations with physics as the most fundamental has been seriously challenged in recent years. For example, theoretical physicist, Fritjof Capra (1988) has argued that the metaphors of "basic building blocks" or "firm foundations" are no longer adequate. He argues that in the emerging new paradigm, scientific knowledge is viewed as "a network of concepts and models, in which no part is any more fundamental than the others . . . . Since there are no foundations in the network, the phenomena described by physics are not any more fundamental than those described, for example, by biology or psychology" (p. 148). Capra's reasoning, applied within the field of psychology, suggests that there is no one content area, or vantage point that is foundational to all the rest. Thus, the phenomena of each of the sub-disciplinary areas such as clinical psychology, social psychology, neuropsychology and industrial psychology have their appropriate applications and contexts. What is regarded as "foundational," if such a concept is useful at all, is context dependent and pragmatic. For some kinds of problems, for example, it will be useful to treat the phenomena of social psychology as "foundational," but for other kinds of problems, it will be more useful to treat the phenomena of neuropsychology as "foundational."

The metaphor of scientific work as a network of relations has strong implications for older notions of reductionism. If one thing is foundational to all things, and if causality is unidirectional (from small to large), then simplifying theories are a legitimate major goal in scientific work. But if, in the words of Dupré (1993), there is an "equal reality and causal efficacy of objects both large and small" (p. 7), then simplifying theories will be forced to play a more modest role in scientific work. The emphasis on a basic parity and importance of both large and small objects lends legitimacy to all levels of scientific activity and deepens sensitivities to the positive and negative implications of reductionism. In a thoughtful article on the problem of reductionism in biology, Kincaid (1990) has argued that molecular biology cannot be reduced to biochemistry. There are realizations and functions that are inevitably lost in reductionism. Further, reducing efforts destroy sensitivities to the contexts in which more molar operations are realized.

Kincaid also notes that attempts to reduce molecular biology to biochemistry often result in explanations that "presuppose biological facts rather than eliminate them" (p. 577). Psychologists can resonate to Kincaid's arguments about the problem of reductionism. Attempts, in our field, for example, to reduce reflective awareness to physiological processes, result in explanations that presuppose the very molar awareness that has supposedly been unpacked and eliminated. Long ago, the neo-Platonic philosopher Plotinus argued that the "soul is not in the universe, on the contrary the universe is in the soul" (Plotinus, trans. 1956, p. 411). The new, more pluralistic philosophy of science suggests that the soul is indeed in the universe and that the universe is also in the soul. There are many real things requiring many equally legitimate levels of inquiry. Thus, psychology qua psychology is enfranchised along with every other scientific discipline that has surfaced naturally out of human attempts to understand the multitude of real things encountered in experience.

Psychology, like biology, deals with a great range of content areas and the various areas do not always blend immediately into each other. For example, in psychology, a phenomenological study of taste hedonics may seem foreign to a study of response extinction as a function of a partial reinforcement schedule or to a specific dream content. In biology, the study of a mechanism by which messenger RNA is transported from the nucleus of a cell to the cytoplasm, may seem very remote from an investigation of the social behavior of a killer whale. An advantage of the network metaphor of science is that it affords a place for privileging a host of perspectives (e.g., physical, chemical, physiological, social, cognitive, emotional, etc.) without the demand that one ultimate bottom-line science be foundational to all the rest and without the demand that everything in the network be hardwired to every other thing. Dupré (1993), in arguing for ontological pluralism, contends that "there are many equally legitimate ways of dividing the world into kinds" (p. 6). He notes the problems associated with dimorphic classifications of gender, and the arbitrary nature of species classifications. The history of classifications of emotional disorders also reflects the problems of establishing unambiguous kinds.

Nonconductors and disjunctions are accommodated in the network metaphor, but it also encourages the quest for connections and for unifying concepts. Evolution is an example of an important unifying concept in the biological sciences (perhaps, no less so in the psychological sciences) though evolution as a field of study is beset with misunderstandings (e.g., see Gould, 1995) and heated disputes over practical and theoretical issues (see Depew and Weber, 1985; Rosenberg, 1994, pp. 57–83). The concept of a network of relations is especially friendly to the quest for modest single-domain unities (e.g., linguistic unity, unity of purpose, axiological unity, etc.) but it is not unfriendly to the search for deeper and more pervasive unities. It is possible that such unities will be more difficult to establish in psychology than in biology because real disjunctions in psychology may turn out to be more radical than those in biology. Psychologists, however, should not underestimate the multitude of very real disjunctions in the biological sciences or the reductive problems in those sciences caused by the difficulties of establishing type identities from one level of scientific organization to the next. Many of the disunities in psychology are not unique.

The network metaphor of the sciences, compared with the older building block metaphor, is more likely to nurture new developments such as chaos theory which, as noted by Barton (1994), has been poorly understood by psychologists. The network concept thus places fewer constraints upon empirical and theoretical work by encouraging scientific investigation without overly restrictive a priori concerns about how each new thing will fit in with the whole. Islands of scientific work may be welcomed in open and tolerant ways though there be little more than limited immediate potential for building bridges to other islands or to the larger body of work.

The history of science, from the standpoint of organization, methodology, and content may well be diagnostic for the future of psychology. We will inevitably develop a far greater range of content areas (some immediately compatible with each other and some not immediately compatible), and a host of new methodologies that will open up new and unanticipated vistas. Like biology, psychological studies will undoubtedly be administered out of a greater range of academic departments and scientific and professional organizations all devoted to the many specific topical content areas that represent the increasingly diverse and sometimes disunified dimensions of the field. If that is our direction in the future, we will not be unique, we will be sharing something in common with the development of other sciences.

References

Appel, T.A. (1988). Organizing biology: The American Society of Naturalists and its "affiliated societies," 1883–1923. In R. Rainger, K.R. Benson, and J. Maienschein (Eds.), *The American development of biology* (pp. 87–120). Philadelphia: University of Pennsylvania Press.

Bacon, F. (1960). *The new organon and related writings* (F.H. Anderson, Ed.). New York: Liberal Arts Press. (Original work published 1620)

Barton, S. (1994). Chaos, self-organization, and psychology. *American Psychologist*, *49*, 5–14.

Bridgman, P.W. (1955). *Reflections of a physicist*. New York: Philosophical Library.

Brožek, J. (1990). Disunity versus diversity. *American Psychologist*, *45*, 983.

Brush, S.G. (1974). Should the history of science be rated X? *Science*, *183*, 1164–1172.

Capra, F. (1988). The role of physics in the current change of paradigms. In R.F. Kitchener (Ed.), *The world view of contemporary physics* (pp. 144–155). New York: State University of New York Press.

Depew, D.J., and Weber, B.H. (Eds.). (1985). *Evolution at a crossroads: The new biology and the new philosophy of science*. Cambridge: The MIT Press.

Dupré, J. (1993). *The disorder of things: Metaphysical foundations of the disunity of science.* Cambridge, Massachusetts: Harvard University Press.
Feyerabend, P.K. (1975). *Against method.* London: NLB.
Gleick, J. (1987). *Chaos: Making a new science.* New York: Viking Penguin Inc.
Gould, S.J. (1995). Ladders and cones: Constraining evolution by canonical icons. In R.B. Silvers (Ed.), *Hidden histories of science* (pp. 37–67). New York: NYREV, Inc.
Green, C.D. (1992). Is unified positivism the answer to psychology's disunity. *American Psychologist, 47,* 1057–1058.
Hall, R. (1967). Monism and pluralism. In P. Edwards (Ed.), *The encyclopedia of philosophy, 5,* (pp. 363–365). New York: Macmillan Publishing Co. Inc.
Hartshorne, C. (1982). Science as the search for the hidden beauty of the world. In D.W. Curtin (Ed.), *The aesthetic dimension of science: 1980 Nobel Conference* (pp. 85–106). New York: Philosophical Library.
Hawking, S. (1993). *Black holes and baby universes and other essays.* New York: Bantam Books.
Hildebrand, J.H. (1957). *Science in the making.* New York: Columbia University Press.
James, W. (1975). *Pragmatism.* Cambridge, Massachusetts: Harvard University Press. (Original work published 1907)
Kimble, G.A. (1989). Psychology from the standpoint of a generalist. *American Psychologist, 44,* 491–499.
Kimble, G.A. (1990). To be or ought to be? That is the question. *American Psychologist, 45,* 559–560.
Kincaid, H. (1990). Molecular biology and the unity of science. *Philosophy of Science, 57,* 575–593.
Kitcher, P. (1982). *Abusing science.* Cambridge, Massachusetts: MIT Press.
Kuhn, T.S. (1970). *The structure of scientific revolutions* (second edition). Chicago: Chicago University Press.
Medawar, P.B. (1984). *The limits of science.* New York: Harper and Row.
Plotinus. (1956). *The enneads* [S. MacKenna, Trans.]. London: Faber & Faber.
Popper, K.R. (1959). *The logic of scientific discovery.* London: Hutchinson & Co.
Rosenberg, A. (1994). *Instrumental biology, or the disunity of science.* Chicago: The University of Chicago Press.
Rychlak, J.F. (1994). *Logical learning theory.* Lincoln, Nebraska: University of Nebraska Press.
Schneider, S.M. (1992). Can this marriage be saved? *American Psychologist, 47,* 1055–1057.
Staats, A.W., (1989). Unificationism: Philosophy for the modern disunified science of psychology. *Philosophical Psychology, 2,* 143–164.
Staats, A.W. (1991). Unified positivism and unification psychology: Fad or new field? *American Psychologist, 46,* 899–912.
Staats, A.W. (1993). Redefinitions of the science–practice relationship without a framework for unity will fail. *American Psychologist, 48,* 58–59.
Viney, W. (1989). The cyclops and the twelve-eyed toad: William James and the unity–disunity problem in psychology. *American Psychologist, 44,* 1261–1265.
Viney, W., King, C.L., and King, D.B. (1992). William James on the advantages of a pluralistic psychology. In M. E. Donnelly (Ed.), *Reinterpreting the legacy of William James* (pp. 91–100). Washington, D.C.: American Psychological Association.
Wand, B. (1993). The unity of the discipline: A challenge to the profession. *Canadian Psychology, 34,* 124–134.
Watson, J.D. (1968). *The double helix.* New York: New American Library.
Weiss, A.P. (1924). Behaviorism and behavior I. *Psychological Review, 31,* 32–50.