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THE SCIENTIFIC VALUE OF HIGH ENERGY PHYSICS

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Abstract—In this paper I will discuss two aspects of the role of accelerators in the development of modern physical science: first, the increasing prominence of high energy/elementary particle physics in the past two decades, relative to other areas of physics, with suggestions about how the significance and cost of discoveries in different areas of science might be estimated; second, the justification of substantial funding for this kind of research on the grounds that it is 'fundamental' to science, with remarks on the change in judgements of fundamentality from a long-term historical perspective.

I

During the past half-century, accelerators have had an enormous impact on many areas of science and technology. Their best known contribution is to elementary particle physics (see Table 1); but accelerators have also been involved in chemistry, biology, and medicine.[1] The technique of radiation processing of industrial products is now applied to materials valued at more than $1 billion per year.[2] Can we estimate the *scientific* value of a field like high energy physics apart from its applications?

During the last two decades there has been a shift of effort, resources, and interest from molecular, atomic, and nuclear physics toward the study of elementary particles and their interactions, especially at the very high energies made available by accelerators. Since the *results* of this research seem to be more and more abstract and remote from practical applications (although the *technology* of accelerators has many spin-offs), it is very difficult for the non-physicist to evaluate them; even physicists are reluctant to consider seriously how one should weight the importance of their own discoveries relative to those in other sciences.

It should no longer be necessary to make the case for government support of basic research, but it *is* necessary for the scientific community to provide some sensible advice about how the finite amount of money available for research should be spent. In particular, it would be useful to have answers to questions such as the following, in order to make decisions about allocation of research funds:

(1) How important are discoveries in high energy physics compared to those in other areas of science, and how this situation changed in recent years?

(2) What proportion of the discoveries has been made in the United States, and how much of this research has been funded by various government agencies?

(3) Which areas of science have become significantly more or less important in recent years? (i.e. the rate of discovery has markedly increased or decreased)

(4) What is the average cost for funding a discovery in high energy physics and how does this compare with the cost of funding discoveries in other sciences?

In order to answer any of these questions we must obviously have a working definition of 'discovery,' that is, some way of measuring the quality or importance of a particular scientific result. Strictly speaking this can only be done in retrospect; 50 or 100 years afterwards it may become obvious that a discovery was of great importance even though it was not recognized as such when it was first announced. One can make such a determination for previous decades or centuries by consulting systematic works on the history of science, as I have done, for example, for astronomy in the period 1800–1975.[3]

More often we tend to use the award of the Nobel Prize to certify the importance of a scientific discovery. But the number of prizes in a specialized field such as high energy physics is so small that this does not help very much in measuring our progress from one year or even one decade to the next. Moreover, there is always the suspicion that the people who decide on the prize will be biased for or against a particular field of research, and therefore their judgment cannot be used as an objective measure of the importance or progress of that field relative to others.[4]

Although there is no such thing as a completely objective measure of the quality of scientific research, several recent studies have shown that for many purposes it is convenient to use the *citation count*, i.e. the number of times a published article is cited in other articles. There are a number of objections to the validity of the citation count when applied to individual papers or authors, but it seems to be a fairly good indicator of the significance or impact of papers when used statistically.[5] In any case I am not aware of any alternative method that is any more reliable, and the citation method is now much more practical than any other because the necessary data has already been compiled and published by Eugene Garfield in his *Science Citation Index*. Moreover, Garfield has recently summarized the information on physical science articles in a form that is appropriate for our purpose, and enables us to give at least a preliminary answer to some of the questions listed above. If this approach is considered worth pursuing, it could easily be carried out in a much more comprehensive fashion.

For the purpose of this statement I define a 'discovery' as the research result reported in one of the 100 most frequently cited papers in the physical sciences, published in the year 1976, based on citations during the years 1976–78; or in one of the 100 most frequently cited papers in the physical sciences, published in the 1960s, based on citations during the years 1961–78.[6]

The two most frequently cited papers on high energy physics published in the 1960s were 'Symmetries of baryons and mesons' by Gell-Mann (1962) and 'A model of leptons' by Weinberg (1967); both were theoretical papers closely related to accelerator experiments. The most highly cited paper published in 1976 was 'Observation in e^+e^- annihilation of a narrow state at 1865 MeV/c^2 decaying to $K\pi$ and $K\pi\pi\pi$' by Goldhaber and 39 others. This was a report of an experiment at SPEAR by the SLAC/LBL group, confirming the existence of 'charm'. Each of these papers clearly represents a major scientific discovery by any criterion. On the other hand, if you were to make a list of the most important papers in high energy physics published in the 1960s or in 1976, as judged by researchers, you would undoubtedly find several that do not appear on Garfield's list of the 100 most frequently cited papers.

An example of such a list is given in Table 1. Of the papers published in the 1960s, only 2 out of 4 (theoretical papers by Gell-Mann and Weinberg just mentioned) are identified by the citation count; apparently experimental papers are not cited very frequently after two or three years—if they are important, their results are incorporated into the theoretical and review papers which are more frequently cited by later authors. This suggests that one should not try to use the citation method to compare the significance of theoretical and experimental papers within the same field.

A list of 1977 papers most frequently cited in 1977 and 1978, published by Garfield just after this hearing (*Current Contents*, 28 July, 1980), shows that the announcement of the discovery of the upsilon particle at Fermilab heads the list. So in this case the citation count method is confirmed by the independent assessment presented in Table 1.

To give an idea of overall trends I have divided the

Table 1. U.S. major accomplishments in high energy physics since 1950

1955	Discovery of the antiproton at the Bevatron. Segre and Chamberlain awarded 1959 Nobel Prize.
1956	Parity violation predicted in weak interactions. After confirmation in the Columbia experiment, Lee and Yang awarded 1957 Nobel Prize.
1961	Particle classification scheme for strong interactions [SU(3)] proposed. Quarks predicted. Gell-Mann awarded the 1969 Nobel Prize.
1962	Muon neutrino discovered at the AGS.
1964	CP violating decays observed at the AGS. Means time reversal violation.
1967–75	Theory unifying the weak and electromagnetic forces proposed. Experimental confirmation followed. Weinberg and Glashow in the U.S. awarded the 1979 Nobel Prize.
1968	Scaling in deep inelastic electron proton scattering observed at SLAC. The proton consists of point constituents.
1974	J/Psi discovered independently at the AGS and SPEAR. Confirms force unification and charmed quarks. Ting and Richter shared 1976 Nobel Prize.
1975	A new heavy lepton (tau) discovered at SPEAR. Suggests a fundamental connection between leptons and quarks.
1975	Particle jets observed in electron-positron annihilation at SPEAR. Confirms predictions of quark-gluon structure of hadrons.
1977	Upsilon discovered at Fermilab. Heaviest particle. Means the existence of a new fifth quark.

Compiled by Melvin Month, Department of Energy; selected from 'US Major Accomplishments in High Energy Physics since 1945,' prepared by Division of High Energy Physics, U.S. Department of Energy, Washington, D.C., June 1979.

100 papers in each group into five general categories (see below).

The list of most frequently cited 1977 papers is not completely comparable to the 1976 list because of a change in the way it is presented, but it indicates that papers in high energy physics, elementary particles and field theory make up an even greater proportion of the discoveries in physical sciences. When citations from 1979 to the 1977 papers are included, however, it appears that several papers in geophysics make the list.

It is not possible to tell from the published compilations whether the sizable jump in the number of discoveries in elementary particle physics is characteristic of the past five years or peculiar to 1976 and 1977, but I think this point deserves further investigation.

By looking in more detail at the individual papers on these two lists, we learn that three areas that were well-represented in the 1960s seem to have dropped substantially in significance in 1976: applied mathematics, crystallography, and nuclear physics. Several other areas, in addition to elementary particle experiments, are attracting much more attention in 1976: astrophysics, the Martian atmosphere, field theory, supergravity, phase transitions, lasers and fibre optics, and photochemistry.

How many of these discoveries could be credited to the United States? There is some ambiguity here because of the free flow of scientists between countries, and the participation of scientists from several countries in several research teams. My estimate from this data is that about 78% of the discoveries in the 1960s were made by people affiliated with institutions in the U.S., and that this proportion increased slightly, to 82%, in 1976. For 1977 it was down to 74%.

Within category I, the proportion of U.S. contributions increased from 20 out of 27 in the 1960s (74%) to 41 out of 43 in 1976 (96%). This change is mainly due to the shift in emphasis to the more expensive elementary-particle experiments within this category.

While these results are based on a single year in the past decade and therefore cannot be relied on very heavily, they do tend to contradict the impression one gets in publications such as *Science Indicators 1978* that the American share of world research in the physical sciences has been declining during the past decade.[8] One can argue that the data base for the *Science Citation Index* tends to favor American publi-

		Number of highly-cited papers in: 1960s	1976
I.	High energy physics, nuclear physics, field theory, elementary particles	27	43
II.	Atomic and molecular physics, solid state physics, statistical physics, lasers and fibre optics	45	30
III.	Astronomy, astrophysics, geophysics, supergravity	5	12
IV.	Applied mathematics	3	0
V.	Chemistry	20	15
	Total	100	100

Here is a more detailed breakdown into subfields of physical science:[7]

	Number of most-cited articles in: 1960s	1976
High energy physics, elementary particles, field theory	16	41
Nuclear physics	11	2
Solid state physics	12	10
Geophysics	4	0
Applied mathematics	3	0
Astronomy, astrophysics, supergravity	1	12
Statistical physics, phase transitions	4	3
Organic chemistry, biochemistry, biophysics	9	5
Inorganic chemistry	3	3
Physical chemistry	8	7
Atomic and molecular physics	28	11
Masers, lasers, fibre optics	1	6
Total	100	100

cations, but I do not think that bias is likely to have increased between the 1960s and 1970s; hence citation analysis probably can give fairly reliable indications of overall shifts between countries.

Finally, we can easily determine the sources of funding of these discoveries since it is customary to acknowledge financial support at the end of every scientific paper. The only uncertainty comes from the fact that many papers list two or more sources of funding. Taking account of fractional-paper support and rounding off the totals, I estimate the following distribution of sponsors for the 82 American discoveries listed among the 100 most frequently cited 1976 publications:

ERDA (predecessor of DOE)	35
NSF	22
NASA	8
DOD	5
HEW (predecessor of HHS)	4
Industrial laboratories (IBM, Bell, Corning, Dupont)	5
Miscellaneous private foundations or no sponsor mentioned	3
Total	82

Within category I, ERDA supported approximately 28 and NSF approximately 13 of the 41 U.S. papers on the 1976 list. The accelerators themselves were all ERDA facilities; out of 19 elementary-particle experiments, 11 were done at Fermilab, 5 at the SLAC/LBL/SPEAR complex, 2 at Brookhaven, and 1 at the Savannah River Plant operated by Dupont for ERDA. (Of the two European elementary particle experiments, one was done at CERN in Switzerland and the other at DORIS/DESY in Germany.)

We can now try to estimate the cost per discovery, on the basis of the support for basic research in physical science provided by these agencies during the two or three years prior to the publication of the results. The following data are provided by NSF's Division of Science Resource Studies:

	FY 1974	FY 1975
ERDA	\$237 million	\$243 million
NSF	119	138
NASA	160	198
DOD	54	52
Other	63	61
Total	\$633 million	\$692 million

(These figures do not include support for mathematics or geophysics.)

As a very rough estimate (using the averages of the above numbers) we then find that each discovery supported by ERDA cost 240/35 = \$7 million; by NSF, 129/22 = \$6 million; by NASA, 179/8 = \$22 million; by DOD, 53/5 = \$11 million.

Because of the different citation practices in different fields, one cannot legitimately compare the number of discoveries in two different fields of science on the basis of data from a single year; only the relative changes over a period of time are meaningful. Therefore we cannot say how expensive it is to fund high energy physics as compared to astronomy or chemistry, on a 'per-discovery' basis, until a more extensive analysis for other years has been done.

My conclusion is that discoveries in physics published in 1976 cost about \$7 million each, assuming that a discovery is defined as a result reported in one of the 100 most frequently cited papers in physical science. This number is arbitrary in the sense that it might be reduced to \$3.5 million, for example, if one changed the definition to include the 200 most cited papers; but it does suggest the possibility of coming to a more definite conclusion by extending the analysis to other years and looking at changes over time.

It does not appear that high energy physics discoveries (funded primarily by ERDA) are significantly more expensive than those in other areas of physics. If 1976 is typical of the recent past, we are getting substantial dividends now from our past investments in this field.

II

Section I indicates that high energy physics is still a highly successful research field as judged by physicists. But the question of whether the field should continue to enjoy a high level of support relative to other sciences remains to be considered. We could probably show by the same methods that some area of biology or psychology is equally successful as judged by citations in the journals of life science or behavioural science, and in addition, its discoveries are more comprehensible, less expensive, and promise more immediate practical benefits.

In their attempts to justify funding for larger and larger accelerators, physicists argue that the study of elementary particles is so *fundamental* that it should be supported even if there were no immediate prospect of practical benefits to society. For example, R. P. Shutt of Brookhaven National Laboratory wrote in 1971:[9]

> "Without particle physics, physics would have no true frontier, and in a larger sense, science would have no frontier...no truly new principles or laws of nature could be discovered any longer."

Recently this viewpoint has been stated as follows, in the words of science writer Nigel Calder:[10]

> "Physics was always the master-science. The behaviour of matter and energy, which was its theme, underlay all action in the world. In time astronomy, chemistry, geology, and even biology became extensions of physics. Moreover, its discoveries found ready application, whether in calculating the tides, creating television or releasing nuclear energy. For better or worse, physics made a noise in the world, but the abiding reason for its special status was that it posed the deepest questions to nature."

These quotations probably reflect the opinions of many physicists. But physics was not always the 'master science', and the study of elementary particles was not always the most fundamental kind of research; it only became so in the first half of the present century. Twentieth-century atomic physics, with the help of accelerators, *earned* the status of 'most fundamental science'. It is only by going back in history to a time when the search for elementary particles was *not* considered the most fundamental goal of science that we can appreciate the magnitude of that achievement, and at the same time recognize that some other science may in the future become the most fundamental.

Looking back to the origins of modern science in the 16th and 17th centuries we find that *astronomy* was considered the most fundamental science. This was the legacy of ancient Greek science which postulated that the heavenly bodies are perfect, move eternally in circular paths, and exhibit the true harmony of the universe, whereas everything in the terrestrial sphere is messy and complicated. As Copernicus wrote in his book, *On the Revolutions of the Heavenly Spheres*, which initiated modern science:[11]

> "Among the many and varied literary and artistic studies upon which the natural talents of man are nourished, I think that those above all should be embraced and pursued with the most loving care which have to do with things that are very beautiful and very worthy of knowledge. Such studies are those which deal with the godlike circular movements of the world and the course of the stars, their magnitudes, distances, rising and settings, and the causes of the other appearances in the heavens; and which finally explicate the whole form."

In the 17th century, Galileo and Newton proposed new laws of mechanics which were confirmed in astronomy and applied also to the terrestrial sphere. Throughout the 18th century, astronomy maintained its high status as calculations of planetary and lunar motion became so accurate that they could be used in navigation. The last great triumph of Newtonian astronomical theory occurred in 1846 when deviations of the planet Uranus from its predicted path were used to locate a previously unsuspected planet, Neptune.

Early in the 19th century planetary astronomy, though still the most respected branch of science, seemed to offer little hope for further advances except through tedious numerical calculation. Chemistry and geology began to attract the attention of those who preferred a younger science in which major discoveries could still be made without excessive mathematical labor. Both were coming to be regarded as 'fundamental' in the sense that they dealt with important problems and provided a firm basis for advances in other sciences.

Chemistry was a fundamental science as long as its 'elements'—hydrogen, oxygen, iron, etc.—were thought to be qualitatively different kinds of matter. Chemistry (not physics) seemed to offer the best route toward understanding the atomic structure of matter, and stimulated research in the areas of heat, electricity, agriculture and nutrition. Physics, which was later to incorporate heat and electricity as subfields, did not even exist as a coherent science in its modern sense, including atomic physics, until the last half of the 19th century. Later the discovery of nuclear transmutation showed that one chemical element can be changed into another; they are not in fact 'elementary' but can all be built up from hydrogen. The development of quantum mechanics showed that chemical bonds and reactions can be explained in terms of the physical properties of atoms. Thus chemistry by 1930 was no longer fundamental but reducible to a branch of physics.

Geology at the beginning of the 19th century was also a fundamental science. It dealt not only with the present structure of the entire earth, but with its past history; it was the only subject that attempted to describe changes through long periods of time, and even challenged theology for the right to consider the creation and age of the earth. It was fundamental also in the sense that it provided the basis for another science—paleontology, and later evolutionary biology.

By 1900 geology had greatly contracted its domain: the origin of the earth belonged to astronomy, and most of the inside was the province of a new science, seismology. But, more significantly, geology's claim to be able to establish a time scale for the development of the earth's crust had been demolished by physics. In the 1860s, the British physicist Lord Kelvin became intensely interested in the age of the Earth. He asserted on the one hand that this problem was one of the most important in science, but on the other hand that the geological theories and methods previously applied to it were unacceptable because they conflicted with basic principles of physics (especially those involving heat).

Kelvin's dictum was, in effect, that whenever physics and geology disagree, geology must give way because physics is more fundamental. The geologists, intimidated by Kelvin's prestige and mathematical formulas, accepted his dictum and as a result lost confidence in the value of their own methods—even after 1900, when Kelvin's results on the age of the Earth were found (by other physicists) to be wrong.[12] I find this episode very interesting because it shows quite clearly that physics was becoming more fundamental than another science through a direct confrontation.[13]

The high status of physics in the 20th century is of course primarily due to its own revolutionary successes—Einstein's theory of relativity, Rutherford's experiments on the atomic nucleus, and the quantum theory of Planck, Bohr, Heisenberg and Schroedinger. Soon after quantum mechanics had elucidated the electronic structure of the atom, the accelerator provided 'the key to a new world of phenomena, the world of the nucleus' as Lawrence and Livingston predicted in 1932.[14] Among many important results obtained with accelerators, I will mention only one that reinforced the fundamental character of atomic physics: the discovery (or 'creation') of the antiproton in the Bevatron at Berkeley, by a team led by Emilio Segre and Owen Chamberlain.[15] This experiment confirmed a general principle of relativistic quantum mechanics (first proposed by Dirac) that every elementary particle has an anti-particle, so the world is symmetrical with respect to positive and negative charges.

By the 1960s, elementary particle physics had become the most prestigious and lavishly supported area of science, and in many respects it retains that status today. Let us now ask: what might replace elementary particle physics as the most 'fundamental' area of science?

The most obvious candidate would seem to be cosmology—the study of the development and structure of the universe as a whole—which has enjoyed a spectacular revival in the last two decades. Physicists do not seem to feel threatened by this development, in fact they welcome it since the most advanced cosmological theories and speculations involve large doses of relativity, quantum mechanics and even elementary-particle physics. But let us go further and consider what kind of change might replace the search for the most elementary particle with something radically different that would challenge the current philosophy and priorities of physicists.

In order to answer that question we have to recognize that *atomism*—the assumption that matter can be analyzed in terms of elementary particles—is a basic part of the scientific worldview that has prevailed in Western Europe and America since the 17th century. The previous worldview was 'organic' or 'holistic', emphasizing the *relations* between parts of a system rather than their separate structures. There have been periodic revivals of this worldview as reactions against the 'materialist' or 'mechanistic' tendency of Western science since the time of Newton. So far all of these reactions have failed because they have attempted the impossible task of repealing the advances made by science during the past four centuries.[16] When holism gains ground it is not by a frontal assault on science but by leading science in new directions. Here are some examples.

In high energy physics itself the proliferation of 'elementary' particles has thrown doubt on the assumption that any of today's particles is really elementary. The most likely prospect is the quark. But it is apparently impossible to pry one quark loose from the others within a larger particle such as a proton, and study it separately. Thus attention has shifted to the *forces* between particles. The currently-fashionable goal of finding a unified theory of all forces, from which particles would be *derived* as secondary entities, seems to be an admission that the search for the most elementary particle has been given lower priority if not abandoned.[17]

If the primary goal of physics is to find a unified theory of all forces, it is by no means obvious that this goal can be reached only by doing experiments at higher and higher energies. I see an interesting historical analogy with the discovery of the relation between electricity and magnetism by Oersted in 1820: he was so firmly convinced, for metaphysical reasons, of the unity of electricity and magnetism within a holistic worldview that he managed to abandon the restrictive preconceptions of Newtonian science to do a very simple experiment with a current and a compass.[10] It was a new kind of arrangement of the apparatus, rather than simply increasing the strength of the charge or the magnet, that led to success.

Putting the emphasis on the unification of forces rather than the discovery of smaller particles might actually strengthen the argument for the fundamentality of high energy physics, since many more areas of science depend on these forces than depend on the properties of the elusive quark. In any case one can argue that if a science deserves to be called fundamental and is to be supported primarily for that reason, its funding might be reviewed by an advisory panel including representatives from those neighboring areas of science that are supposed to be based on it. Such a panel should be able to explain to the public the significance of the discoveries that have been made and the hypotheses that are to be tested.[19]

An even more radical breakdown of the mechanistic preconceptions of modern Western science is suggested by some interpretations of quantum theory. Perhaps phenomena at the atomic level do not have independent reality apart from human observation, or perhaps we cannot make a measurement on an atomic particle in the laboratory without in some way disturbing every other particle in the universe. The experiments currently being done to test these possibilities might produce a more fundamental change in our view of the world than any of the more expensive high-energy physics research now being proposed.[20]

An extension of this line of thought is the 'anthropic principle': the postulate that the physical properties of the universe must allow the evolution of intelligent life that will observe and thereby confer reality on it. In this way one can explain why certain physical constants, such as the 'fine structure constant' (a ratio involving Planck's constant and speed of light) have the values they do: if they were very much different, intelligent life could not have evolved to measure them.[21] These are now just wild speculations, disdained by most scientists even though they come from highly respected physicists like John Wheeler. If they are taken more seriously in the future we might have to elevate biology and psychology to the status of 'fundamental science' now enjoyed primarily by physics.

To summarize: during the past 50 years, quests with U.S. accelerators helped to establish high energy physics as the most fundamental area of science. But this status was earned in a particular historical situation, and is not inherent in the nature of science. Leaders of the scientific community should be sensitive to the changing connections among the sciences brought about by new discoveries and insights. Even if they cannot always agree on priorities, discussions at this level (rather than on the technical 'needs' of each specialty) should help Congress and the public to make reasonable decisions on the allocation of resources.

NOTES

1. A good though somewhat outdated review of these applications may be found in the book edited by L. C. L. Yuan, *Elementary Particles: Science, Technology, and Society* (Academic Press, New York, 1971). For a recent, brief overview of elementary particle physics see the first three pages of D. A. Bromley's 'Physics', *Science* **209**, 110–121 (1980).
2. J. Silverman: 'Basic concepts of radiation processing' and 'Current status of radiation processing', *Radiat. Phys. Chem.* **9**, 1–15 (1977); **14**, 17–21 (1979).
3. 'Looking up: The rise of astronomy in America,' *American Studies* **20** (2), 41–67 (1979).
4. See Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (Free Press/Macmillan, New York, 1977); H. Inhaber and K. Przednowek, 'Quality of Research and the Nobel Prize', *Social Studies of Science* **5**, 33–50 (1976).
5. J. R. Cole and S. Cole, *Social Stratification in Science* (University of Chicago Press, 1973); F. Narin *et al.*, *Evaluative Bibliometrics: The Use of Publication and Citation Analysis in the Evaluation of Scientific Activity* (Computer Horizons, Cherry Hill, N. J., 1976); Eugene Garfield, *Essays of an Information Scientist*, 2 vols. (ISI Press, Philadelphia, 1977) and *Citation Indexing, Its Theory and Application in Science, Technology and Humanities* (Wiley, New York, 1979); E. Garfield, M. V. Malin and H. Small, 'Citation data as science indicators', in Y. Elkana *et al.* (Editors) *Toward a Metric of Science*, pp. 179–207 (Wiley, New York, 1978).
6. E. Garfield, 'The 1976 articles most cited in 1976 and 1977. 2. Physical Sciences,' *Current Contents* **19** (17), 5–16 (1979), and 'Most-cited articles of the 1960s. 1. Physical Sciences,' *ibid.* **19** (21), 5–15 (1979). A somewhat similar analysis for 1972 chemistry publications may be found in 'Report of the National Science Board to the Subcommittee on Science, Research and Technology of the Committee on Science and Technology, U.S. House of Representatives, regarding peer review procedures at the National Science Foundation,' November 1977, pp. II–25f.
7. I have changed the classification of some of the articles in Garfield's lists as follows. For 1960s papers, the one by Kadanoff *et al.* has been moved from 'molecular physics' to 'statistical physics,' and the one by Fisher has been moved from 'physical chemistry' to 'statistical physics.' For the 1976 list, Kirkpatrick's paper in solid state physics and the two papers listed under 'field theory in solid state physics' have all been moved to 'statistical physics'; the paper by Letokhov and Moore has been moved from 'atomic and molecular physics' to 'lasers and fibre optics'; both papers in 'chemical physics' have been moved to 'atomic and molecular physics.'
8. In physics, U.S. articles declined from 33% of the world literature in 1973 to 30% in 1977. During this period the 'citation ratio' remained about 1.40 or 1.41, i.e. U.S. articles were cited about 40% more than their share of the literature. *Science Indicators 1978, Report of the National Science Board 1979*, pp. 150, 152 (National Science Foundation, Washington, D.C., 1979). While recognizing the utility of citation counts as a measure of the influence of research publications, this report gives no hint of the extent of American domination of specialized fields such as high energy physics, revealed by Garfield's listings of 'most-cited articles.' For a general discussion and critique of the 'science indicators' project see Y. Elkana *et al.* (Editors), *Toward a Metric of Science: The Advent of Science Indicators* (Wiley, New York, 1978).
9. Quoted from p. 31 of his article 'Science, Physics, and Particle Physics,' in Yuan, *op. cit.* (note 1), 11–48.

M. J. Moravcsik presents a well-reasoned analysis of the concept of 'fundamentaity' in his article 'A refinement of extrinsic criteria for scientific choice,' *Research Policy* **3**, 88–97 (1974).

10. Quoted by J. E. Leiss, Associate Director for High Energy and Nuclear Physics of the Department of Energy, in his presentation to the Energy Research and Production Subcommittee of the House Science and Technology Committee, February 26, 1980. See Nigel Calder, *The Key to the Universe*, p. 14 (Viking, New York, 1977).
11. N. Copernicus, *De Revolutionibus Orbium Coelestium* (1543); quoted from the translation by C.G. Wallis, in *Great Books of the Western World*, Vol. 16, p. 510.
12. J. D. Burchfield, *Lord Kelvin and the Age of the Earth* (Science History Publications, New York, 1975). The greatly diminished vitality of geology in the first part of the 20th century, due to many other factors as well as Kelvin's attacks, is shown not only by the scarcity of major discoveries in that period but also by morbid features of its published literature—the turgid style of language, immense bibliographies, delays in publication of new results, etc. See the fascinating book by Henry Menard (now Director of the U.S. Geological Survey), *Science: Growth and Change* Harvard University Press, Cambridge, Mass. 1971) which shows how one can detect the health or decay of a scientific discipline by looking at various quantitative indicatiors. Another phenomenon which may be more difficult to establish except on an anecdotal basis is the tendency of the best younger scientists to more into a field they consider 'fundamental' and desert a less prestigious field where they might have been able to make a major contribution. I can recall from my own days as a graduate student in the 1950s that the best students in physics were not encouraged to go into geophysics, even though with hindsight I can see that those were the days when the 'revolution in the earth sciences' leading to plate tectonics was just beginning, and geophysics has since become much more respectable.
13. S. G. Brush, 'Planetary Science: From Underground to Underdog,' *Scientia* **113**, 771–87 (1978). The fact that physics is considered more fundamental than geology affects our perception of the history of science in previous centuries. For example, it is generally believed that Americans were indifferent to basic science in the 19th century, since they accomplished relatively little in physics; but in fact they were quite active and successful in the fundamental sciences of the time—astronomy, the earth sciences and some areas of biology. See D. J. Kevles, J. L. Sturchio and P. T. Carroll, 'The Sciences in America, Circa 1880,' *Science* **209**, 27–32 (1980); S. G. Brush, 'Looking up': The rise of astronomy in America,' *American Studies* **20**, 41–67 (1979).
14. E. O. Lawrence and M. S. Livingston, 'Production of high speed ions,' *Phys. Rev.* [2] **42**, 20–35 (1935), reprinted in H. A. Boorse and L. Motz (Editors) *The World of the Atom*, p. 1390 (Basic Book, New York, 1966).
15. See Boorse and Motz., *op. cit.*, Chapter 82, for discussion of this experiment and reprint of the original report in *Nature* (1956).
16. Carolyn Merchant presents a provocative discussion of the change from holism to mechanism from the viewpoint of modern feminism and ecology in her recent book, *The Death of Nature: Women, Ecology, and the Scientific Revolution* (Harper and Row, San Francisco, 1980). I have reviewed the various 'romantic' movements in relation to developments in science since 1800 in my book *The Temperature of History: Phases of Science and Culture in the Nineteenth Century* (Burt Franklin, New York, 1978) and my article 'The chimerical cat: Philosophy of quantum mechanics in historical perspective,' *Social Studies of Science*, in press.
17. This is stated most explicitly on page 33 of Steven Weinberg's article, 'The search for unity: Notes for a history of quantum field theory,' *Daedalus* **106**, 17–35 (1977). Bromley's article, cited in note 1, illustrates rather nicely the continuing tension between the conviction that particles like quarks must exist even though they can never be detected, and the desire for a 'grand unification' of forces which will lead to observable consequences in high energy experiments.
18. R. C. Stauffer, 'Speculation and experiment in the background of Oersted's discovery of electromagnetis,' *Isis* **48**, 33–50 (1957).
19. M. J. Moravcsik proposed such a panel for somewhat different reasons, to deal with a perceived 'crisis' in high energy physics: see 'The crisis in particle physics,' *Research Policy* **6**, 78–107 (1977). In my view interdisciplinary panels should be a regular part of the funding review process in all areas of science: physicists would have to justify their major projects to chemists and vice versa.
20. B. d'Espagnat, 'The Quantum Theory and Reality,' *Scient. Am.* **241** (9), 158–81 (1979); A. Shimony, 'Metaphysical problems in the foundations of quantum mechanics,' *Int. Phil. Q.* **18**, 3–17 (1978).
21. J. A. Wheeler, 'The universe as home for man, ' *Am. Scient.* **62**, 683–91 (1974); 'Genesis and observership,' in *Proc. 5th Int. Congr. Logic, Methodology and Philosophy of Science* (edited by R. E. Butts and J. Hintikka, Part 2, pp. 3–33 (Reidel, Boston, 1977).
22. This paper is the text of a statement to the Subcommittee on Energy Research and Production, Committee on Science and Technology, U.S. House of Representatives, at the hearing on "Quests with U.S. Accelerators... 50 years," July 23, 1980. I thank Dr Henry Small of the Institute for Scientific Information for supplying information on citation counts. My research has been supported by the History and Philosophy of Science Program of the National Science Foundation.